WORKING PAPER

Effects of domestic worker legislation reform in Brazil

Joana Simões de Melo Costa, Institute for Applied Economic Research (Ipea)
Ana Luiza Neves de Holanda Barbosa, Institute for Applied Ecomomic Research (Ipea)
Guilherme Hirata, IDados – Alfa e Beto Institute

international policy centre for inclusive growth

EFFECTS OF DOMESTIC WORKER LEGISLATION REFORM IN BRAZIL[1]

Joana Simões de Melo Costa, [2] Ana Luiza Neves de Holanda Barbosa[3]
and Guilherme Hirata[4]

This study investigates the impact of the extensions of labour rights (as foreseen in Constitutional Amendment 72 of 2 April 2013) over the formalisation, working hours and wages of Brazilian domestic workers. We also aim to identify whether this new legislation has resulted in the shift of domestic workers to unemployment or other labour status. The methodology combines the difference-in-differences approach with propensity score reweighting. The analysis is carried out with microdata from the National Household Sample Survey (Pesquisa Nacional por Amostra de Domicílios—PNAD) from 2012 through 2014. Our results suggest that this legislation increased the number of formal jobs, reduced the amount of working hours and had no effect on the wages of monthly domestic workers.

Keywords: domestic work, formal sector, working hours.
JEL Codes: J2, J38, J4.

1 INTRODUCTION

In Brazil, approximately 16 per cent of employedwomen (more than 6 million people) held jobs as domestic workers in 2014. This occupation is historically associated with poor working conditions, such as lack of legal registration (informality), low wages and high weekly working hours. It is also a unique occupation because, until 2015, it was not governed by the same labour rules as all other private occupations. Other private occupations are subject to the Consolidated Labour Laws from 1943. Instead, labour rules for paid domestic work were set by a specific law (Law No. 5,859) in 1972 and by the 1988 Federal Constitution. The reason for this distinct treatment is the nature of the job performed by domestic workers, usually at the employer's home and very close to their family. In fact, some of these workers actually live in the employer's household, as evidenced by several household surveys conducted in the country.

This paper analyses the impact of the reform of legislation for domestic workers on their working conditions. In April 2013, the House of Representatives approved an Amendment to the Constitution (*Emenda Constitucional—EC—*72) that guarantees labour rights that other employees already had access to. It should be noted that some of the rights contained in the Amendment still depend on specific legislation, which means that they did not come into force immediately after promulgation. Right after April 2013, the only practical effects of the legislative change were the limitation of working hours (up to eight hours a day and 44 hours a week) and the obligation to pay overtime. In June 2015, the Amendment was sanctioned as a supplementary law (*Lei Complementar*, *LC* 150/2015) by the President.[5]

Despite the notable increase in female labour force participation over recent decades, gender inequality still persists in the Brazilian labour market. In addition to higher levels of informal employment among women, they receive lower salaries, on average, than men. Data from the 2014 National Household Sample Survey (*Pesquisa Nacional por Amostra de Domicílios—PNAD*) show that 29 per cent of men and 37 per cent of women who are employees hold an informal job, while the salary for men was approximately 35 per cent higher than for women. Given that paid domestic work is mostly a female occupation (more than 92 per cent of domestic workers are women) and that more than 6 million people work in this sector, the Constitutional Amendment may have an impact on gender inequality as a whole at the national level, not only in terms of wage inequality but also in terms of working conditions.

To evaluate the effects of the Constitutional Amendment, we investigate whether the reform had any impact on the working hours, informality and wages of domestic workers. The empirical strategy follows a difference-in-differences (DID) approach. We use data from *PNAD* waves from 2012 to 2014. This is an annual sample survey with a reference period in September. Therefore, we have data for a period of six months before the approval of the Constitutional Amendment, and for six and 18 months after the reform.

The *PNAD* has a repeated cross-sectional design. Therefore, we do not follow the same individuals throughout the period. To minimise the possibility of fixed unobserved differences in characteristics between domestic workers and the control group determining the outcomes, we also implement a propensity score technique. Following Abadie (2005), we weight the estimating equation by the probability of an individual being a domestic worker (the propensity score), a strategy known as inverse probability weighting (IPW). The idea is to give more weight to the control group observations that are most similar to the treatment group in terms of observable characteristics. The advantage of IPW over other matching techniques is the efficiency gain due to the fact that the entire sample is used in the estimation process.

There are two 'types' of domestic workers in Brazil: the *mensalista* (monthly worker) and the *diarista* (daily worker). The former usually works for the same household the entire month and receives a monthly wage; this is the most common arrangement for all jobs in Brazil. The *diarista* usually works for two or more households during the week, spending one or two days in each household, and receives daily payments. It is possible to observe a *diarista* working for six different households in a single week.

The main difference between these types of domestic workers is the employment relationship as defined by law. Law 5859/1972 defines the domestic worker as the "one who provides continuous and non-profit services to a person or family at their home". Therefore, continuity is a key element for the characterisation of the domestic employment relationship the law refers to, which is not present in the services provided by a daily worker. It is worth noting that the monthly worker, despite having an employment relationship, does not necessarily hold

a formal job. In fact, only 40 per cent of them do so (see Table 1). There are several ways to distinguish between the formal and informal private sector in Brazil.[6] Here we identify domestic workers who are formally registered and who possess a 'signed work card' (*carteira de trabalho e previdência social*). This work card is an official document that entitles workers to benefits such as social security and unemployment insurance. In the case of daily workers, although some of them are legally registered, it is not mandatory for the employer to sign the work card. To have access to social security, daily workers have to contribute to it themselves.

Therefore, it is important to keep in mind that the effect of the Constitutional Amendment may be very distinct for the two types of domestic workers. Considering the legal definition, it is possible that daily workers are not affected at all, since the Amendment might not be applicable to this group. For this reason, the analysis undertaken by this paper regards monthly and daily workers separately.

Every federal labour law may affect all workers. While the Amendment does not have an immediate, direct impact on manufacturing workers, it may well affect the extensive margin decision of individuals holding jobs in that sector. Assuming that workers are flexible, when the working conditions of a specific job improve, it might attract workers from different sectors who potentially have different characteristics. Thus, we may incorrectly attribute an impact to the Amendment when in fact the cause of the change in outcome was the change in composition of the treatment group. Conversely, due to the rigidity of the labour market and personal resistance to change sector due to preferences, it is expected that a job-specific law will take some time to be assimilated by workers from other sectors and have more general effects. Nevertheless, we perform several estimations considering different control groups, aiming to verify the robustness of our results. Our preferred control group is the one comprising service workers occupied as employees (who are not supervisors). This definition comes from the Brazilian Classification of Household Occupations (*Classificação Brasileira de Ocupações Domiciliar—CBO-Domiciliar*), codes 511 to 519.

The results indicate that the Constitutional Amendment increased the probability of *mensalistas* having their work card signed, which means that domestic workers gained more access to social security. We highlight that there is no mention of legalisation in the Amendment, since it was already illegal to have an employment relationship without duly signing the worker's work card. Because the Amendment generated a heated debate in Brazilian society, this result probably comes from employers trying to avoid penalties resulting from litigation against them by their employees. We also identify a decrease in the weekly working hours for the same group. Adjusting the working hours of domestic workers was probably the first step taken by the employer, particularly for those whose employees live in their household. Regarding wages, we identify no impact for *mensalistas*. On the one hand, one could argue that this result comes as no surprise, since the hours of work decreased. On the other hand, if informal workers receive lower wages than formal ones, the increase in the formality rate would raise the mean wage, but this did not happen.

We also investigate whether the Amendment had general effects on the labour market. There is no clear evidence that it increased the probability of being a domestic worker. However, the probability of being a daily worker among domestic workers increased. In addition, there has been an increase in the probability of being unemployed. Therefore, it is possible that the reduction in informality among *mensalistas* is a consequence of migration to informal jobs or to unemployment rather than an increase in the absolute number of legally registered workers.

Empirical evidence regarding the role of institutions and the informal sector is very extensive in Brazil.[7] One of the strands of this literature points out that the contractual rigidity and labour legislation costs are two of the main reasons for the existence of high levels of informality in the Brazilian labour market (Amadeo and Camargo 1996; Barros et al. 1999; Goldberg and Pavcnik 2003; Ulyssea 2010). There are also some studies that investigate the impact of particular labour market institutions such as the enforcement of labour regulations and the potential impacts of formalisation policies targeting micro-entrepreneurs (Almeida and Carneiro 2012; Monteiro and Assunção 2012; Corseuil, Neri, and Ulyssea 2014). Regarding domestic work, however, the literature is very scarce. To the best of our knowledge, Theodoro and Scorzafave (2011) is the only paper to analyse the effects of changes to labour legislation in Brazil. The authors analyse the impact of the law[8] that allowed employers of domestic workers to deduct from their income tax the amount spent on the domestic worker's social security contributions, which represents a reduction in labour costs. The results are inconclusive, since some estimates have shown positive effects on legalisation, while others were not significant.

Our paper speaks to a broader literature on labour legislation reforms, where the leading case is the effect of a minimum wage on several outcomes in developed and developing countries. Examples include: Ashenfelter and Smith (1979) on employers' compliance; Newmark and Wascher (1995) on school enrolment and employment of teenagers; Bell (1997) on employment in formal and informal sectors in Mexico and Colombia; and Lee (1999) on wage inequality in the USA. For Brazil, most of the evidence is related to the effect of the minimum wage on formal employment and wage inequality (Brito et al. 2016; Corseuil and Foguel 2015; Corseuil et al. 2015a; Foguel et al. 2014; Broecke and Vandeweyer 2014). In addition to minimum wage analysis, the literature is smaller but no less important.[9] Fallon and Lucas (1993) report a reduction in employment in India and Zimbabwe after increasing firing rigidities. Besley and Burgess (2004), analysing labour regulation in India, point out that states which amended federal regulations in favour of workers (stricter regulation) experienced lower employment and productivity rates in the formal sector, but higher output rates in the informal sector.

Specifically regarding international evidence about domestic workers, Dinkelman and Ranchhold (2012) found a positive impact on wages and on the share of domestic workers with formal contracts after the introduction of a minimum wage in South Africa. In a similar context, a wage increase was also found by Gudibande and Jacob (2015) in India, but only in the short term. Wong (2015) estimates impacts on labour outcomes of the employer social security enrolment mandate for domestic workers in Ecuador. The Ecuadorian case is similar to the Brazilian one, as the enforcement of employer (generally, heads of households) social security mandates originating from a law implemented in 1964 only started in 2008. Based on the DID methodology, Wong's results show evidence of negative impacts on domestic workers' wages and hours worked.[10]

Following this introduction, this paper comprises five sections. The next section presents some considerations about legislation that is relevant to domestic workers. The third section presents the data, performs a brief analysis of the domestic employment profile in Brazil during the 2000s and presents a descriptive analysis. The fourth section describes the empirical strategy used to assess the impact of Constitutional Amendment 72 on the working conditions of domestic workers. The fifth section presents and discusses the results. Finally, the last section is dedicated to concluding remarks.

2 INSTITUTIONAL BACKGROUND OF DOMESTIC WORK IN BRAZIL

In 1943, when the Consolidated Labour Laws were established, domestic work was already an important occupation in Brazilian labour market (Pinheiro et al. 2012; Fraga 2010). Yet paid domestic work was not among the occupations that the laws intended to regulate. This type of job was not subject to formal regulation until December 1972, when Law No. 5859 was sanctioned. This act made the signing of the work card mandatory, guaranteed the right to paid annual leave of 20 working days and provided access to social security services and benefits.

Significant changes regarding the rights of domestic workers were introduced by the 1988 Federal Constitution. In addition to the rights listed in the 1972 legislation, the following rights were extended to domestic workers: i) minimum wage; ii) wage irreducibility; iii) 13th month of annual salary; iv) paid weekly rest; v) paid annual leave (with a third of the wage as an extra payment); vi) maternity leave of 120 days; vii) paternity leave; viii) notice period proportional to tenure; and ix) retirement. In addition, access to the *Fundo de Garantia por Tempo de Serviço* (*FGTS*—Time of Service Guarantee Fund)[11] and unemployment insurance became optional.[12]

In 2006, among other changes, Law No. 11324 gave the employer of domestic workers the option to deduct the required contribution to social security from their income taxes. The purpose of this measure was to reduce informality by reducing the costs of employing domestic workers. Finally, to conform to Convention 182 of the International Labour Organization (ILO), which dealt with the eradication of child labour, Decree No. 6481 from 2008 banned paid domestic work for children and adolescents under 18 years old. In 2011, Law No. 10208 made the collection of *FGTS* optional and guaranteed workers the right to receive unemployment insurance (of at least one minimum wage) for three months in case of dismissal (Domingues and Souza 2012).

Constitutional Amendment 72, approved by the Brazilian National Congress in April 2013, was the outcome of the proposed Constitutional Amendment 66 (PEC 66/2012), which became widely known as the 'domestic workers PEC'. Article 7 of the 1988 Federal Constitution, which reinforced the exceptional nature of domestic work, was changed by the Amendment to include labour rights such as working days of no more than eight hours and 44-hour working weeks, and overtime payment no less than 50 per cent of the normal hourly wage. These rights became compulsory when the Amendment was published. Other rights included in the Amendment still depend on specific regulation: *FGTS*, compensation in the event of dismissal without just cause, unemployment insurance, bonus for nightly work shifts, and insurance against work-related accidents, among others. The Amendment was sanctioned on 1 June 2015, two years after its publication, with only two minor vetoes.[13]

3 DATA AND DESCRIPTIVE ANALYSIS

3.1 DATA

We use data from the *PNAD*, an annual nationally representative survey conducted by the Brazilian Census Bureau (*Instituto Brasileiro de Geografia e Estatística*—IBGE) since 1981. The survey provides information on demographic, labour, fertility, socio-economic and household characteristics for all interviewees. We use the data from the 2012–2014 waves. As Constitutional Amendment 72 was published in April 2013, we have information for one

period before the reform and two periods afterwards. As the *PNAD* goes into the field in September, the information collected relates to six months prior, and six and 18 months after the Amendment. Because more than 92 per cent of domestic workers are women, our sample includes women only, aged 18 or over, living in urban areas.

Due to the definition of paid domestic work established by Law No. 5859 from 1972 ("continuous and non-profit services provided to a person or household"), it is important to distinguish between two categories of domestic workers: the *mensalista* (monthly worker) and the *diarista* (daily worker). The *PNAD* questionnaire allows for this separation because it has specific questions for domestic workers. There is information on how many times a week and to how many different households the worker provides domestic services. Given these two variables, we classify the domestic workers as follows. The *mensalista* works at a single household for three days or more during a working week. In this case, it is mandatory for the employer to sign the work card. All other possible combinations of these two variables define a *diarista*. Thus, a worker who has only one employer but provides services up to two days a week is considered a *diarista*.

It worth noting that there is no information on the number of days worked and the number of employers a domestic worker has in other surveys conducted in Brazil. Specifically, there is no such information in the *Pesquisa Mensal de Emprego* (*PME*—Monthly Employment Survey), the only survey conducted in the country that follows the same individuals across several waves.[14]

3.2 PAID DOMESTIC WORK IN BRAZIL—2000S

Before going through the sample we use in the empirical analysis, we will take a look at the evolution of our main outcomes over the past decade. The figures below use the 2001–2014 waves of the *PNAD*.

Figure 1 shows the evolution of the share of *mensalistas* and *diaristas* with formal labour contracts. We can see an upward trend for monthly workers (dotted line) throughout the period, with a sharp increase between 2012 and 2013 (7 percentage points). For the daily workers (continuous line), the proportion is stable. For the sake of comparison, we also depict the same proportion for other employees. The share of workers with formal contracts for this group is almost twice that of the *mensalistas*. The proportion for other employees also increased during the period, but there is no path-breaking at the end.

Figure 2 presents the weekly working hours for the same three categories of employees. While the length of the working day has been reduced for *mensalistas* (dotted line) and *diaristas* (continuous line), it has remained constant for other employees (dashed line). For the latter, this is expected, since 40-hour working weeks are the most common labour contract in the country. In practice, this number may vary a little, but, due to the self-reported nature of the survey, the standard answer would be 40 hours even for informal workers.

FIGURE 1

Percentage of workers with signed work card, 2001–2014 (*mensalistas*, *diaristas* and other employees)
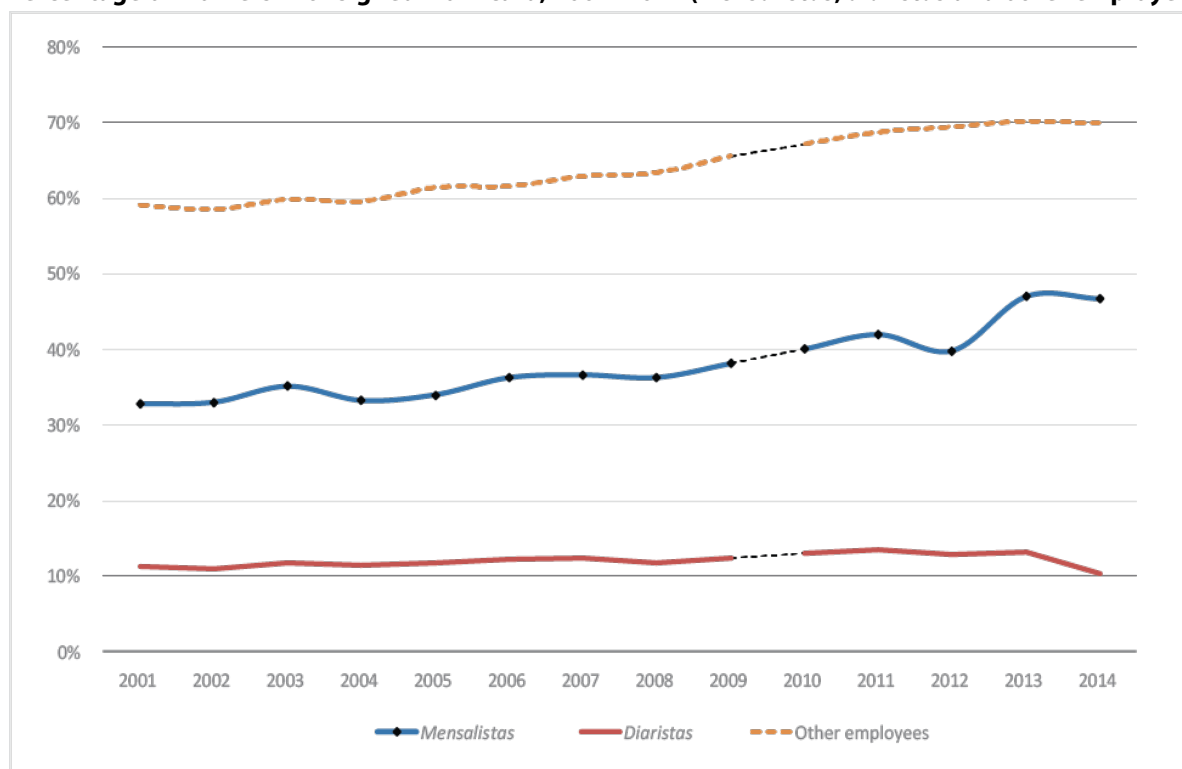
Source: PNAD. Authors' elaboration. There was no PNAD in 2010 (a census year).

FIGURE 2

Average weekly working hours, 2001–2014 *(mensalistas*, *diaristas* and other employees)

Source: PNAD. Authors' elaboration. There was no PNAD in 2010 (a census year).

Until 2012, the evolution of the real hourly wage is similar for all categories. According to Figure 3, it started increasing in 2004, and, after stabilising during 2008–2009, an increase is observed, larger than what was previously seen. From 2012 to 2013, the real hourly wage for *diar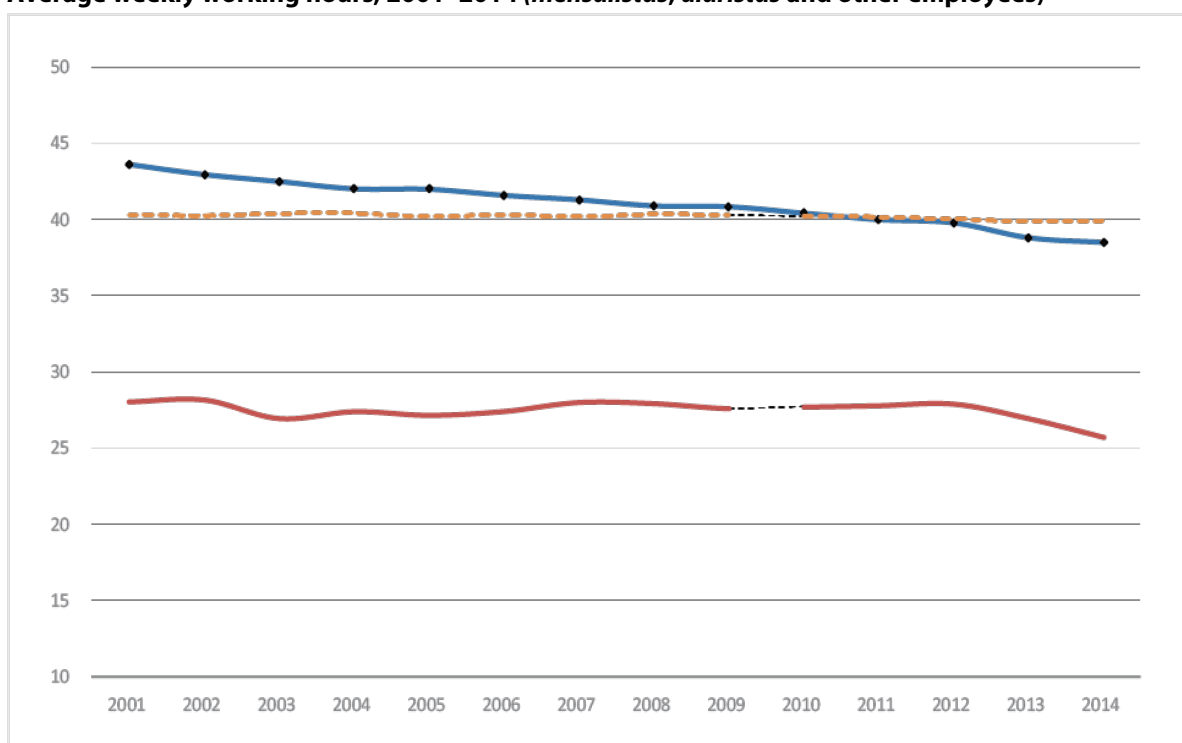istas* (dashed line) remained stable, and there was a slight reduction in the following year. Regarding the other categories, we observe an increase in wages from 2012 to 2013 and a decrease from 2013 to 2014. Again, there is an important difference between daily and monthly workers. In addition to working less, daily workers are earning more. Part of this wage premium works as compensation for not having access to the same labour rights guaranteed by law that the *mensalistas* have. Conversely, as discussed earlier, only a third of *mensalistas* have their work card signed. For those who do not, the situation is even worse, since they are paid less than a *diarista* and do not have access to social security (unless they contribute to it themselves).

These figures highlight the difference between domestic workers and other employees. Paid domestic work plays a leading role in the absorption of less-skilled women with no work experience, although both the age and average education levels of domestic workers have risen over the past few years (Melo 2000; Fraga 2010). It is also an occupation featuring low pay and high informality rates, as just described.

FIGURE 3

Real hourly wage (BRL), 2001–2014 (*mensalistas*, *diaristas*, and other employees)
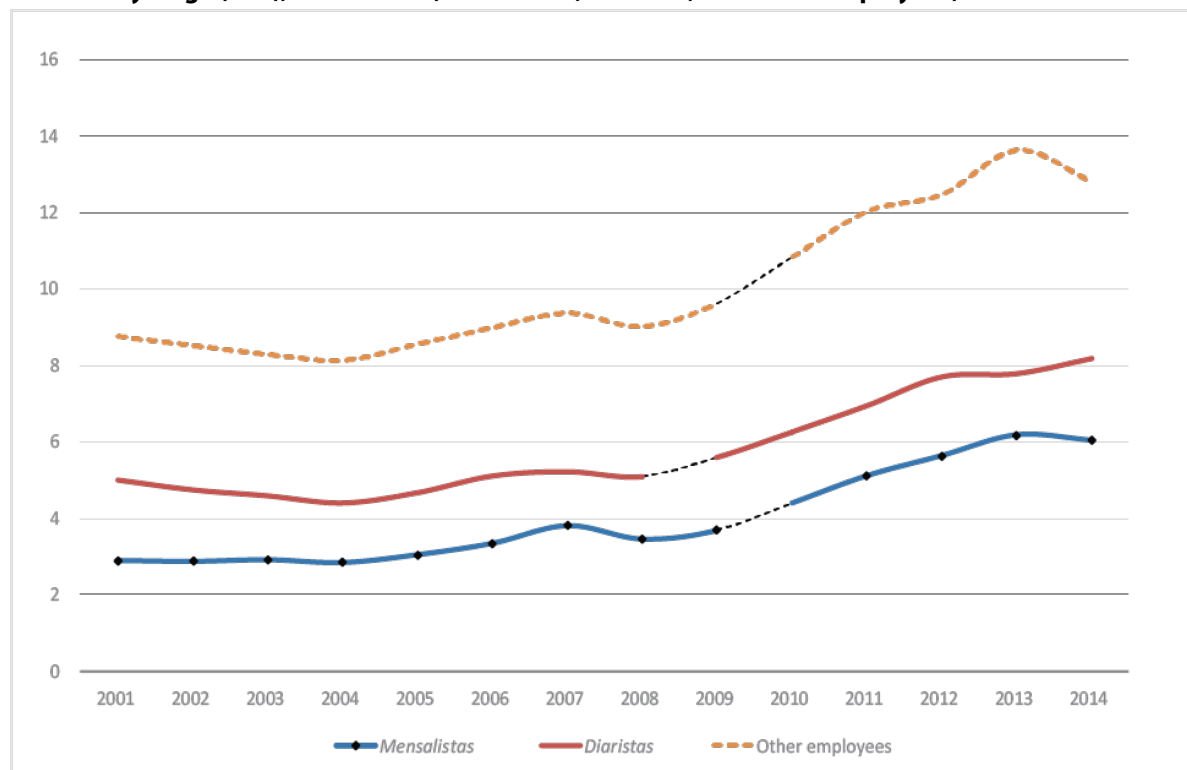
Source: PNAD. Authors' elaboration. There was no PNAD in 2010 (a census year).

The evidence presented in this section complements previous findings on the characteristics of domestic workers. Domestic workers who are married and have children present the highest rates of informality (Perry et al. 2007). In addition, those who had their first job as a domestic worker have a higher probability of remaining in the same occupation in comparison to those who did not start as a domestic worker (Saito and Souza 2008). Finally, it is also worth noting that as a close substitute for household production, paid domestic work

may enable women with higher opportunity costs to change their usage of time and labour supply decisions (Cortés and Pan 2013; Cortés and Tessada 2011; Chan 2006; Suen 1994).

3.3 DESCRIPTIVE ANALYSIS (2012–2014)

The empirical strategy described in the next section uses data from the 2012–2014 waves of the *PNAD*. Our sample includes women aged 18 or older, living in urban areas. In the main specifications, we use employees only. Unless expressly noted, this is the domain we cover. However, in a few exercises, we also consider unemployed women and those outside the labour force (as specified on a case-by-case basis).

Table 3.1 presents the main characteristics of the sample. We have four groups represented in the columns, and for each group we have the average of the variable indicated in the first column for 2012. Column 1 shows the characteristics of all domestic workers taken as a whole. It is observed that the typical domestic worker is 41 years old, has a low educational level (more than half of the sample have not completed primary education), is non-white (more than 60 per cent are black or brown—*pardos*) and lives outside metropolitan areas.

TABLE 1

Characterisation of workers in the domestic and services sector (2012)

	Domestic worker	*Mensalista* (monthly worker)	*Diarista* (daily worker)	Service workers (CBO 511 to 519)
Per cent work card signed	0.30	0.41	0.13	0.68
	(0.46)	(0.49)	(0.34)	(0.47)
weekly working hours	35.55	39.91	28.63	40.83
	(14.91)	(13.17)	(14.89)	(10.80)
hourly wage (BRL)	6.58	5.80	7.81	6.51
	(16.97)	(16.38)	(17.79)	(14.49)
Age	41.54	40.93	42.50	37.87
	(11.55)	(11.68)	(11.28)	(11.37)
Per cent no schooling	0.09	0.08	0.09	0.04
	(0.28)	(0.28)	(0.29)	(0.20)
Per cent 1–5 years of schooling	0.27	0.26	0.29	0.16
	(0.45)	(0.44)	(0.45)	(0.37)
Per cent 6–8 years of schooling	0.21	0.20	0.21	0.17
	(0.40)	(0.40)	(0.40)	(0.37)
Per cent 9–11 years of schooling	0.22	0.22	0.22	0.24
	(0.41)	(0.42)	(0.41)	(0.43)
Per cent high school (at least)	0.21	0.22	0.20	0.39
	(0.41)	(0.42)	(0.40)	(0.49)
Per cent black	0.14	0.14	0.13	0.12
	(0.35)	(0.35)	(0.34)	(0.33)
Per cent brown (*pardo*)	0.49	0.50	0.47	0.46
	(0.50)	(0.50)	(0.50)	(0.50)
Per cent metropolitan region	0.37	0.37	0.38	0.40
	(0.48)	(0.48)	(0.49)	(0.49)
Observations	9639	5966	3673	6151

Source: Authors' elaboration.

Comparing monthly (column 2) and daily (column 3) domestic workers, one does not observe any major sociodemographic differences. The differences appear in the characteristics related to employment, as previously discussed. A typical daily worker earns a higher hourly wage and works fewer hours per week. In addition, as expected, there is a large difference in the proportion of workers with signed work cards.

In the last column, we present the characteristics of women who hold jobs in occupations classified as 'service sector workers' (*trabalhadores dos serviços*) according to the Brazilian *CBO-Domiciliar*. Most comparison groups used in the empirical analysis stem from this group of workers. On average, this group is better educated and younger than domestic workers. In addition, there is a higher proportion living in metropolitan areas. In terms of outcomes, the most striking difference is the proportion of workers with a formal contract: almost 70 per cent of these workers have their work card signed.

4 EMPIRICAL STRATEGY

To analyse the impact of *EC* 72, our first approach uses a before-and-after estimator. We run the following OLS regression on the sample of domestic workers only:

$$y_{it} = \alpha + \beta_1 D_{1,it} + \beta_2 D_{2,it} + X_{it}\gamma + \varepsilon_{it} \qquad (1)$$

where y_{it} is the outcome of interest to the individual i at time t; $D_{1,it}$ is a dummy that takes value 1 if $t = 2013$ (i.e. six months after the reform); $D_{2,it}$ is a dummy that takes value 1 if $t = 2014$ (i.e. 18 months after the reform); and X_{it} is a vector of control variables (dummy variables for age ranges, years of schooling, race, states and metropolitan area). The β_1 and β_2 coefficients are expected to be different from zero if Constitutional Amendment 72 has affected the working conditions of domestic workers.

The outcomes are a dummy for the possession of signed work card, weekly working hours and real hourly wage in 2013 Brazilian Reais (BRL). At first, it is expected that the 0 coefficients are positive for work card and hourly wage, and negative for the working day. This would mean that *EC* 72 is contributing to bring domestic workers' rights to the same level as others. In that case, the employer would sign the worker's work card, stop paying wages below the minimum established by law and avoid overtime by domestic workers.

The before-and-after estimator provides an overview of what happened to the group directly affected by the reform. Nevertheless, it is possible that the estimated parameters are describing effects other than the ones caused by the reform. In the case of work cards, for example, it is possible that the percentage of workers with formal contracts has increased simply because the economy grew in the period, decreasing informality across all occupational groups. Therefore, we have to take into account the counterfactual possibility—i.e. what would have happened to domestic workers if *EC* 72 had not existed.

Our main empirical strategy follows a DID approach. Ideally, we would like to observe the same individual with and without the advent of *EC* 72. Since that proposition is impossible, we use a control group to substitute for the counterfactual and observe what would have happened to domestic workers in the absence of the Amendment. This is a valid strategy under the assumption that the counterfactual would have followed a time trend parallel to the control group. If, for

example, the differences that determine labour choice are not time-invariant, differences between treatment and control after the Amendment might be incorrectly attributed to reform.

EC 72 aims specifically to affect domestic workers. However, by affecting the working conditions of one particular category of workers, it could also affect the labour market decisions of individuals who are not domestic workers. If this were the case, *EC* 72 would affect the composition of the treatment and control groups, which might change the trends of the groups in a different way, violating the main assumptions of the DID strategy. However, while the Amendment would affect the target category almost immediately, it would certainly require some time for other workers to start taking new information into account and decide how to proceed in their labour decisions.

The DID model is estimated by the following equation, where the subscript i has been omitted:

$$y_t = \alpha + \beta_1 D_{1,t} + \beta_2 D_{2,t} + \beta_3 T_t + \beta_4 D_{1,t} T_{it} + \beta_5 D_{2,t} T_t + X_t \gamma + \varepsilon_t \tag{2}$$

where $T_t = 1$ if the individual belongs to the treatment group in period t, and $T_t = 0$, if otherwise. The other variables are the same as presented in equation 1. The interaction term parameters β_4 and β_5 recover the impact of the reform six and 18 months later.

The parallel trend does not require the control group to have similar characteristics to the treatment group. However, different characteristics may end up leading to different trends. To minimise this potential problem, we follow Abadie (2005) by combining the DID approach with IPW, a matching technique. It consists of using the propensity score to reweight the control group distribution to make it more comparable to the treatment group. By propensity score (ps_i) we mean the probability of an individual belonging to the treatment group. Formally, this probability is estimated through a Probit model, given by:

$$ps_i(X) = Prob(T_i = 1|X) = \Phi(X\delta) \tag{3}$$

where Φ is the normal cumulative distribution function, and X is the vector of observable characteristics of workers, the same described in equations (1) and (2). The propensity score is estimated using the sample of the first period of analysis, before the Constitutional Amendment. We calculate ps_i for other periods using the estimated coefficients of equation 3. With the IPW procedure, equation 2 is weighted by:

$$w_{it} = T_{it} + (1 - T_{it}) \times ps_i \times (1 - ps_i)^{-1}. \tag{4}$$

That is, for the control group ($T_{it} = 0$), the greater the propensity score, the greater its weight. For the treatment group ($T_{it} = 1$), the weight is always equal to 1.[15] By using this weighting scheme, the estimation of equation (2) allows one to recover the effect of *EC* 72 on domestic workers —i.e. the average treatment effect on the treated (ATT).

Reweighting observations using the propensity score is one of the possible techniques than can be implemented when using propensity score to match treatment and control groups. As such, it relies on two important assumptions. The first is the conditional independence assumption (CIA). It is assumed that, conditional on the observed variables, selection into treatment is exogenous. The CIA is essential for the method to be valid, but it is not directly testable. One way to indirectly verify the validity of the method is to check whether the observable characteristics are balanced. Thus, the satisfaction of the following condition is evaluated:

$$E[X|ps, T = 1] = E[X|ps, T = 0]. \tag{5}$$

That is, conditional on propensity score, on average, there should be no difference in observable characteristics between treated and control groups.

The other assumption is termed of 'common support', which means that, conditional on observed variables, the probability of being untreated is less than 1, an assumption that guarantees the existence of a control group. Formally, we assume that $P(D = 1|X) < 1$ with probability 1.[16]

4.1 TREATMENT AND CONTROL GROUPS

EC 72 intended to expand the labour rights of domestic workers. As a consequence, these workers are the most obvious treatment group. However, important differences between the two types of domestic workers were highlighted in previous sections. To carefully evaluate the impact of the amendment, we are going to implement the empirical strategy using different treatment groups.

According to Brazilian jurisprudence, continuous work is characterised only for those domestic employees who work at least three days per week in the same household. Therefore, domestic labour law does not apply to *diaristas*. In this case, we expect to find effects of *EC* 72 only for *mensalistas*, so we are going to estimate its impact on this group of workers separately.

However, daily workers are obviously domestic workers as well. Thus, it would be misleading not to investigate whether there was any impact of *EC* 72 on them, even though we have reasons to believe that there was not. As econometricians, we are not able to observe how employers of daily workers are interpreting the Amendment. It is possible that they have adjusted their labour relationship with the *diarista* to avoid sanctions they believe they would face if they violate labour laws. Regardless, one can see the analysis for daily workers as a robustness exercise, given that this group should not be affected by the reform.

The main comparison group for all three treatment groups includes women working as employees in occupations classified as 'services workers' according to the *CBO-Domiciliar* (codes 511 to 519), excluding supervisory jobs. These women hold jobs as cooks, waitresses and room maids, among others. The idea is to compose the control group with workers in occupations that may be considered close substitutes to domestic work.

Due to concerns about sample selection issues when constructing the control group, we consider two additional control groups. The first one includes all employees, regardless of the nature of occupation, while the second one includes workers in relatively similar occupations. Among the service workers, we include workers in administrative services (codes 411 to 424), sellers and other trade workers (521 to 524) and blue collar workers (711 to 992, except supervisors). The results using these control groups are going to be part of our robustness checks.

In addition, we run a specification where the treatment group is composed of monthly workers and the control group is composed of daily workers. If *EC* 72 had no impact on *diaristas* due to the fact that it does not address this group of workers, then the *diaristas* would certainly be a good control group. On the other hand, we should keep in mind that these occupations are substitutes; therefore, *EC* 72 could affect the allocation of workers between these two types of work over a short period of time.

4.2 BALANCING TESTS

As mentioned above, the propensity score is estimated for the first period of analysis (2012). This prevents the likelihood of domestic workers being defined as such according to characteristics defined after the reform. For each available specification—i.e. for each analysis of treatment and control groups—we estimate the propensity score, calculate the weights and test condition (5).

To test condition (5), we perform mean tests between groups for the variables used in the estimation of the propensity score with and without weighing observations. Table A.1 in the appendix reports the p-value of these tests for the specification where the *mensalistas* are the treatment group.[17] It is observed that, whatever the control group is composed of, there is no statistical difference between the averages of most variables after the propensity score reweighting. The results for other specifications are similar. We interpret these results as evidence of no violation of the CIA assumption.

Another way to look at this result is through the density of the propensity score of treatment and control groups before and after reweighting the sample. Figure 4 shows the propensity score distributions of the treatment and control groups for the specification where the treatment group is the monthly worker and the control group is the service worker.[18] As can be seen, after reweighting observations, we end up with very similar distributions. This result adds up to the mean tests as evidence of good balancing properties.

FIGURE 4

Weighted and reweighted propensity score distributions
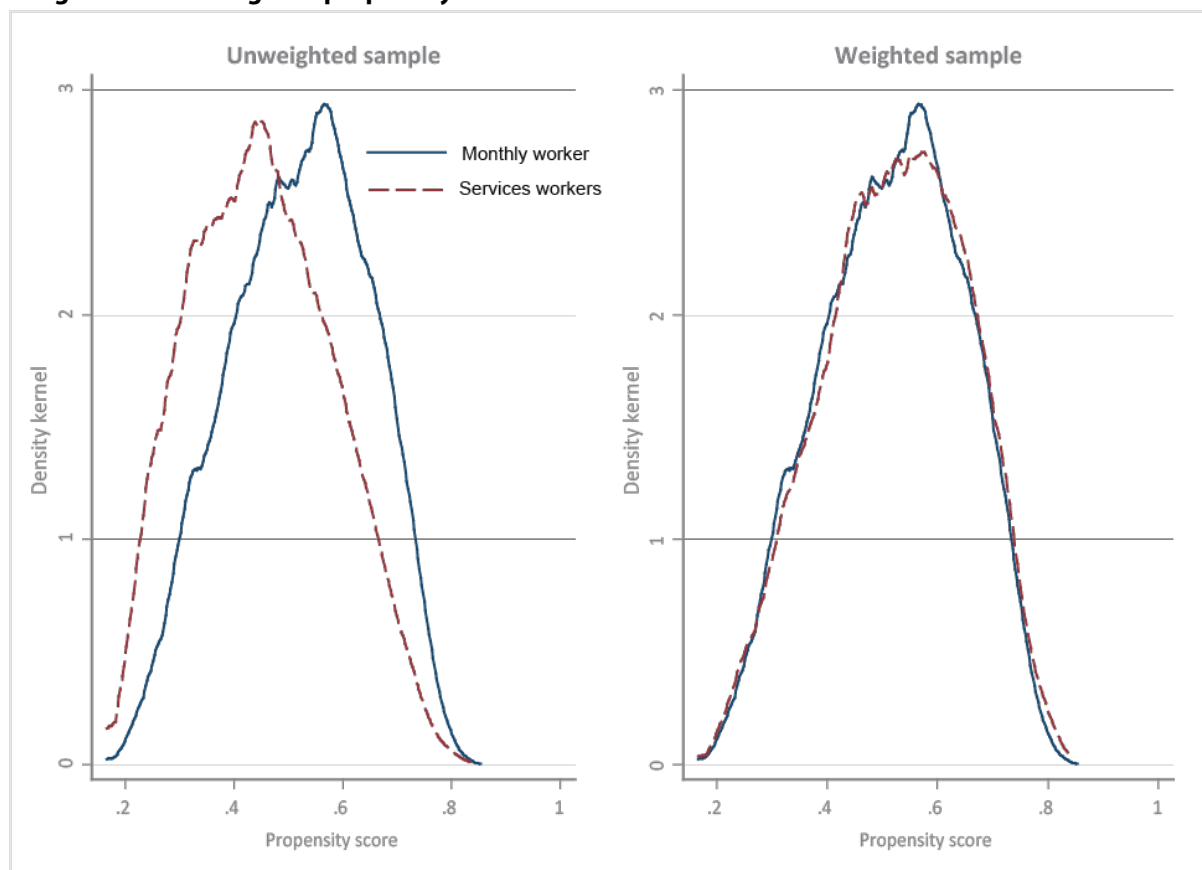
Source: Authors' elaboration.

5 RESULTS

5.1 BEFORE AND AFTER

Table 2 reports the estimation results of equation (1) for the before-and-after analysis, which uses the sample of domestic workers only. Column 1 includes all domestic workers, while the other two columns refer to monthly and daily workers. We report only the coefficients for the time dummies. The 2013 and 2014 dummies refer to six and 18 months after the Constitutional Amendment, respectively. The table includes the results for the three variables of interest: work card signed, weekly working hours and real hourly wage.

According to this methodology, we find an increase of 3.6 percentage points in the formalisation of domestic workers six months after the approval of *EC* 72. This impact is reduced by 1.7 percentage points in the following 12 months (column 1), but it remains statistically significant. The effect on monthly workers is even higher (column 3). One can see that the probability of having the work card signed increased by 6.3 percentage points, and remained stable in the following 12 months.

TABLE 2

Before-and-after estimation: effects of *EC* 72 on formalisation, hours worked and hourly wage

Time dummies	Domestic workers	Monthly workers	Daily workers
	(1)	(2)	(3)
	Probability of having work card signed		
2013	0.036***	0.006	0.063***
	(0.007)	(0.008)	(0.009)
2014	0.019***	-0.023***	0.063***
	(0.007)	(0.008)	(0.009)
	Weekly hours worked		
2013	-1.277***	-1.159***	-1.019***
	(0.212)	(0.337)	(0.236)
2014	-2.057***	-2.219***	-1.352***
	(0.213)	(0.335)	(0.238)
	Real hourly wage		
2013	0.281	-0.032	0.431
	(0.227)	(0.342)	(0.289)
2014	0.422*	0.507	0.248
	(0.227)	(0.340)	(0.291)

Note: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. OLS regressions. Dependent variables: dummy for possession of work card signed, hours worked per week and hourly wage in 2013 BRL. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

Another piece of evidence in favour of the hypothesis that the new law would have contributed to the immediate increase in the formalisation of monthly workers is the fact that we do not identify the same phenomenon occurring among daily workers (column 2). Given that the automatic establishment of an employment relationship occurs only in the case of monthly workers, a positive impact of the change in legislation on daily workers could not be expected. There is, however, a reduction in the probability of formalisation of daily workers between September 2013 and September 2014. This is why the impact on all domestic workers is lower for the 18-month period.

Table 2 also indicates that there would have been a reduction in the weekly working hours and an increase in the hourly wage (in this last case, only 18 months after the Amendment). Nevertheless, contrary to what we found in the formalisation outcome, these results were also observed for daily workers. Thus, these changes could also be related to a previous trend that had been occurring to all domestic workers regardless of the approval of *EC* 72 in 2013.

This multivariate analysis, although a bit more refined than the simple description of the outcomes, is not sufficient to establish a causal relationship between the Constitutional Amendment and the working conditions of domestic workers, as we highlighted in the methodology section. It is important to have a comparison group representing what would be the counterfactual. To understand the effect of the change in legislation, it is necessary to consider what would have happened to the domestic workers in its absence. Therefore, in the next section, we will use the group of workers in service occupations to build the domestic worker counterfactual. We assume that what happened to this group during the study period would have occurred to the domestic workers if the Constitutional Amendment had not been approved.

5.2 DIFFERENCE-IN-DIFFERENCES

To assess the causal effect of *EC* 72, we estimate a DID model considering the sample reweighted by the propensity score. The reweighting procedure we implement allows us to recover the average treatment effect on the treated parameter. The results are presented in the next three tables. The treatment and comparison groups are indicated at the top of each column of each table. The treatment groups are the same ones as in the before-and-after model. Remember that we use service workers to construct our control groups. In column 1 of Table 3, for example, the treatment group consists of all domestic workers, and the control group is composed of all services workers (occupational groups according to the *CBO-Domiciliar*, codes 511–519) except supervisors.

Table 3 reports the results for the possession of a signed work card. We are not able to identify a significant difference between the formalisation path of domestic workers and service workers (column 1). This means that the result we previously found in the before-and-after analysis was misleading—i.e. considering all domestic workers, formalisation would have increased even in the absence of *EC* 72.

Our main interest, however, lies with monthly workers. Column 2 shows that there was a statistically significant increase of 4 percentage points in the probability of monthly workers having their work card signed. This represents an increase of 10 per cent in the formalisation rate of monthly workers (in 2012, 41 per cent had a formal contract). Since *EC* 72 did not necessarily aim to encourage formalisation, this increase is an important effect and was also found 18 months after the implementation of the Amendment.

Column 3 shows why we were unable to find a significant impact for all domestic workers. Six months after the approval of *EC* 72, we found a negative—though not statistically significant—impact on the formalisation of daily workers. In addition, we found a reduction of 3.3 percentage points in the formalisation of daily workers 18 months after the Amendment, which compensated for the positive impact on monthly workers. This result will be discussed further in the next section.

When considering other comparison groups for monthly workers, the results (columns 4 and 5) also point to a positive change in their likelihood of having a formal contract. A point of note is that the quantitative effects for all cases considering monthly

workers are close to each other, six and 18 months after the Amendment. Therefore, our results are robust regarding the choice of the control group.

TABLE 3
DID estimation: effect of *EC* 72 on the possession of a signed work card

	Domestic workers X Service workers	Monthly workers X Service workers	Daily workers X Service workers	Monthly workers X Other employees	Monthlyworkers X Other similar employees
	(1)	(2)	(3)	(4)	(5)
2013 X (Treated = 1)	0.004	0.040***	-0.013	0.043***	0.048***
	(0.010)	(0.012)	(0.011)	(0.010)	(0.010)
2014 X (Treated = 1)	-0.014	0.040***	-0.033***	0.045***	0.047***
	(0.010)	(0.012)	(0.011)	(0.010)	(0.010)
Treated	-0.402***	-0.238***	-0.560***	-0.251***	-0.272***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
2013	0.020***	0.023***	0.020***	0.021***	0.017**
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
2014	0.017**	0.022***	0.012	0.020***	0.019***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
Observations	47204	35879	30088	48411	51038
R-squared	0.239	0.156	0.379	0.164	0.168

Notes*:* Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. WLS regressions, where the weights are based on the propensity score. Dependent variable: dummy for possession of signed work card. Treatment dummy indicated at the top of each column. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

Table 4 presents the results for weekly working hours. The results suggest that the Constitutional Amendment reduced the amount of working hours of monthly workers by 37 minutes six months after the Amendment, rising to one hour 12 months later (column 2). Using other comparison groups, the quantitative effects are significant and, in fact, even higher (columns 4 and 5), which reinforces the main result.

When comparing daily workers to other workers employed in service occupations, we note that, at first, it is not possible to identify a variation in weekly working hours. However, in the 18-month period after the approval of *EC* 72, there was a reduction of about two hours (column 3). This time, the effect on daily workers goes in the same direction as the ones found for monthly workers, which contributes to the negative and significant impact on all domestic workers.

The reduction in working hours is one of the expected effects, since the Constitutional Amendment defined working hours as up to eight hours a day and 44 hours a week. The estimated impact on monthly workers (a reduction of one hour in a working week) represents a reduction of 2.5 per cent in average weekly working hours. This impact may seem low quantitatively, but it is worth noting that, according to our descriptive analysis, the working-hour limit is not binding, since the average is lower than the 44-hour limit.

In the case of working hours, one could argue that the Amendment would be enforced only for the monthly workers with a formal contract. To verify the implications of this possibility, we performed an exercise restricting our sample to workers who have their work card signed (column 6), a sample we have not used before. The impact is almost twice what was observed before (column 2) six months after the Amendment, and it is still 40 per cent higher 18 months after. This increases our confidence that *EC* 72 was effective in causing a reduction in the average number of working hours for domestic workers.

TABLE 4

DID estimation: effect of *EC* 72 on weekly working hours

	Domestic workers X Service workers	Monthly workers X Service workers	Daily workers X Service workers	Monthly workers X Other employees	Monthly workers X Other similar employees	Formal monthly workers X Other formal service workers
	(1)	(2)	(3)	(4)	(5)	(6)
2013 X (Treated = 1)	-0.878***	-0.622**	-0.551	-0.736***	-0.650***	-1.222***
	(0.309)	(0.306)	(0.358)	(0.259)	(0.250)	(0.326)
2014 X (Treated = 1)	-2.018***	-0.972***	-2.078***	-1.038***	-1.157***	-1.356***
	(0.307)	(0.306)	(0.358)	(0.260)	(0.251)	(0.327)
Treated	-6.785***	-0.544**	-12.022***	-0.625***	-0.838***	0.497**
	(0.220)	(0.217)	(0.256)	(0.183)	(0.177)	(0.232)
2013	-0.416*	-0.379*	-0.356	-0.324*	-0.375**	-0.042
	(0.237)	(0.216)	(0.243)	(0.182)	(0.177)	(0.234)
2014	-0.242	-0.309	-0.080	-0.143	-0.107	-0.160
	(0.234)	(0.213)	(0.241)	(0.181)	(0.175)	(0.231)
Observations	47204	35879	30088	48411	51038	20175
R-squared	0.110	0.016	0.234	0.016	0.017	0.017

Notes: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. WLS regressions, where the weights are based on the propensity score. Dependent variable: weekly working hours. Treatment dummy indicated at the top of each column. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

Table 5 presents the results regarding hourly wage. According to them, there was no change in the wage path due to the introduction of the new labour rules. In general, the results are barely significant and show no consistency regarding the direction of the effect. After 18 months, no coefficient is statistically significant.

TABLE 5

DID estimation: effect of *EC* 72 on real hourly wage

	Domestic workers X Service workers	Monthly workers X Service workers	Daily workers X Service workers	Monthly workers X Other employees	Monthly workers X Other similar employees	Formal monthly workers X Other formal service workers
	(1)	(2)	(3)	(4)	(5)	(6)
2013 X (Treated = 1)	-0.666*	-0.665	-0.865*	-1.123***	-0.685**	-0.529
	(0.367)	(0.431)	(0.456)	(0.401)	(0.345)	(0.655)
2014 X (Treated = 1)	0.330	-0.167	0.556	-0.272	0.041	-0.845
	(0.363)	(0.431)	(0.456)	(0.402)	(0.345)	(0.655)
Treated	0.486*	-0.323	1.342***	-0.681**	-0.437*	0.543
	(0.261)	(0.305)	(0.326)	(0.283)	(0.243)	(0.466)
2013	1.008***	1.103***	0.845***	1.244***	1.036***	0.845*
	(0.282)	(0.304)	(0.309)	(0.282)	(0.243)	(0.471)
2014	0.357	0.415	0.221	0.238	0.205	0.545
	(0.277)	(0.299)	(0.307)	(0.280)	(0.240)	(0.464)
Observations	46387	35236	29575	47443	50079	19853
R-squared	0.015	0.014	0.015	0.016	0.015	0.011

Notes: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. WLS regressions, where the weights are based on the propensity score. Dependent variable: hourly wage in 2013 BRL. Treatment dummy indicated at the top of each column. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

The effect on the hourly wage can be interpreted as a 'synthesis' of the impacts of *EC* 72 on the working conditions of domestic workers. The increase in hourly wage was expected through at least one of three mechanisms. The first one is a direct mechanism via overtime pay, since this is one of the subjects the Amendment specifically refers to. The other two mechanisms are indirect ones; the hourly wage could have increased due to a reduction in non-overtime working hours, which has indeed occurred. It could also be a result of formalisation. On average, the wage of an informal worker is less than the minimum wage. Therefore, if a given worker has a signed work card, it is likely that they have received a wage increase.

Because an increase in formalisation and a reduction in working hours were found, it is somewhat odd that there is such a lack of consistency about the effects of *EC* 72 on hourly wage. In the next section we further investigate the impact of the Amendment by implementing robustness check exercises. Specifically, we investigate the role of previous trends and the movement of workers among positions in the labour market. The idea is to enhance our empirical evidence to better understand and interpret our main results.

5.3 DISCUSSION

Robustness check: do the observed effects also occur before the 2013 Constitutional Amendment?

The results so far suggest that the approval of *EC* 72 resulted in an increased likelihood of monthly workers having a formal contract and in a reduction in their weekly working hours. If the decreases in informality and working hours observed for *mensalistas* are in fact being caused by the Amendment, we should not observe these effects in the period before its approval. Therefore, as a robustness check we apply the same methodology (DID weight by propensity score) to the period 2011–2012.

TABLE 6

Falsification test (previous trend): impact for the period before approval of *EC* 72 (2011–2012)

	Domestic workers X Service workers	Monthly workers X Service workers	Daily workers X Service workers	Monthly workers X Other employees	Monthly workers X Other similar employees
	(1)	(2)	(3)	(4)	(5)
	Probability of having work card signed				
2012 X (Treated = 1)	-0.006	-0.013	-0.005	-0.020**	-0.017*
	(0.010)	(0.012)	(0.011)	(0.010)	(0.010)
	Weekly working hours				
2012 X (Treated = 1)	0.489	0.110	0.673*	0.098	0.127
	(0.318)	(0.315)	(0.377)	(0.263)	(0.256)
	Hourly wage				
2012 X (Treated = 1)	0.524	0.796**	0.464	0.789**	0.936***
	(0.319)	(0.361)	(0.406)	(0.319)	(0.306)

Notes: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. WLS regressions, where the weights are based on the propensity scores. Dependent variables: dummy for possession of work card signed, hours worked per week, and hourly wage in 2013 BRL. Treatment dummy indicated at the top of each column. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

Table 6 presents the results of this exercise. The columns are the same as presented in Section 5.2. This time, however, we present the interaction coefficient only, which represents

the 'impact' of the treatment on each outcome of interest. As can be seen, by comparing the paths of monthly domestic workers and workers in service occupations, monthly workers do not exhibit the previous trends of higher formality or lower work hours when compared to other comparison groups. The point estimates suggest the contrary, although most of the results are not statistically significant.

What are the mechanisms underlying higher formality rates and lower working hours?

The approval of more labour rights for domestic workers implies an increase in the penalty for employers who do not sign the worker's work card, in cases of litigation by their employees. Therefore, employers could decide to sign the work card and reduce working hours, to avoid a more significant punishment. Another possible strategy for the employer would be not to employ a domestic worker or to substitute a *mensalista* for a *diarista*. Either decision would contribute to increase formality and reduce working hours among monthly domestic workers.

To shed some light on the issue, Table 7 presents the change in the probabilities of being in several labour market positions, both before and after the approval of *EC* 72. The sample considered here is all economically active women (except column 8, which represents only domestic workers). Each column label represents the position considered as an outcome variable (*dummy*). The coefficients associated with each year refer to the variation relative to 2012, since that year was omitted. Note that to analyse the changes between 2011 and 2012, we have to consider the opposite sign of the 2011 variable coefficient.

Table 7 shows that the probability of being a domestic worker decreases during 2011–2012 and 2012–2014, but not between 2012 and 2013 (column 1). The same trajectory is observed when considering only *mensalistas* (column 2). The probability of being a monthly domestic worker reduced by 0.7 percentage points between 2012 and 2014, but this could be either due to persistence of a previous trend or due to the legal reform.

TABLE 7

Probability of being in specific labour market positions

	Domestic worker	Monthly worker	Daily worker	Occupied	Occupied as employee	Employee in similar occupations	Service worker	Daily worker, conditional on being a domestic worker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Time dummies								
2014	-0.005***	-0.007***	0.002**	-0.009***	-0.001	-0.005**	0.002	0.026***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.006)
2013	0.001	-0.002	0.003**	-0.005***	0.002	-0.001	-0.002	0.017***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.006)
2011	0.007***	0.003**	0.004***	-0.009***	-0.016***	-0.012***	-0.005***	0.003
	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.006)
Observations	264139	264139	264139	264139	264139	264139	264139	46394
R-squared	0.137	0.073	0.057	0.039	0.110	0.061	0.029	0.019

Notes: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. OLS regressions. Dependent variables: dummy for position in occupation indicated at the top of each column. Reported coefficients refer to the time dummies. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies. Columns (1) to (7) samples include women in the labour force. Column (8) includes only domestic workers.

Source: Authors' elaboration.

On the other hand, the probability of being a daily domestic worker increased by 0.2 percentage points between 2012 and 2014 (column 3). Even though the variation is less than 1 percentage point, this scenario of fewer *mensalistas* and more *diaristas* suggests that, at least to

some extent, there has been a transition from monthly to daily workers after the Constitutional Amendment. This hypothesis is corroborated by the increase of 2.6 percentage points in the probability of a domestic worker being a *diarista*.

There is no increase in the probability of having other types of occupation (columns 5, 6 and 7) after 2012. This suggests that there is no shift from *mensalistas* to occupations other than *diaristas*. In addition, there is a decrease of 0.9 percentage points in the probability of being employed (column 4) between 2012 and 2014. Hence, it is also possible that monthly domestic workers are transitioning to unemployment.

Table 8 presents another empirical analysis aiming at understanding the mechanisms underlying higher formality rates and lower working hours among *mensalistas*. This analysis focuses only on women with less than secondary schooling. We built groups according to levels of schooling (no schooling, incomplete first primary cycle, incomplete second primary cycle, complete primary cycle or more), age (18–29, 30–39, 40–49, 50–60), race (non-whites and whites) and geographical areas (states and metropolitan areas). For each group and each year, we computed the proportion of the employed, unemployed, economically inactive, formal workers, informal workers, *diaristas* and those employed in service occupations. Then, we ran weighted least squares regressions to investigate what happens to the groups that had a higher rate of *mensalistas* in 2012.

TABLE 8

Pseudo cohort analysis: effects of *EC* 72 on changes in labour market indicators

	Per cent occupied	Per cent unemployed	Per cent outside labour force	Per cent with formal contract	Per cent without formal contract	Per cent daily workers	Per cent in other services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 2012–2014							
2014 X (% *mensalistas* in 2012)	-0.168*	-0.008	0.175*	-0.019	0.019	0.032	0.125*
	(0.090)	(0.036)	(0.097)	(0.091)	(0.091)	(0.039)	(0.074)
2013 X (% *mensalistas* in 2012)	-0.307***	0.112***	0.195*	-0.098	0.098	0.065*	-0.002
	(0.102)	(0.036)	(0.104)	(0.096)	(0.096)	(0.037)	(0.086)
% *mensalistas* in 2012	0.283***	-0.022	-0.261***	0.018	-0.018	0.045	-0.255***
	(0.076)	(0.027)	(0.081)	(0.073)	(0.073)	(0.028)	(0.061)
2014	0.019**	0.009**	-0.028***	0.009	-0.009	0.001	-0.008
	(0.009)	(0.004)	(0.010)	(0.010)	(0.010)	(0.003)	(0.008)
2013	0.019*	-0.005	-0.014	0.014	-0.014	-0.002	-0.002
	(0.011)	(0.004)	(0.011)	(0.011)	(0.011)	(0.003)	(0.010)
Observations	3438	3438	3438	3438	3438	3438	3438
R-squared	0.714	0.667	0.755	0.819	0.819	0.510	0.833
Painel B: 2011–2012							
2012 X (% *mensalistas* in 2011)	-0.201*	0.004	0.198*	-0.032	0.032	0.016	0.232**
	(0.109)	(0.034)	(0.116)	(0.117)	(0.117)	(0.038)	(0.101)
% *mensalistas* in 2011	0.250***	-0.013	-0.236***	-0.014	0.014	0.068**	-0.334***
	(0.084)	(0.027)	(0.087)	(0.091)	(0.091)	(0.030)	(0.074)
2012	0.020	-0.008**	-0.012	0.010	-0.010	-0.005*	-0.005
	(0.012)	(0.004)	(0.013)	(0.013)	(0.013)	(0.003)	(0.012)
Observations	2295	2295	2295	2295	2295	2295	2295
R-squared	0.727	0.671	0.767	0.821	0.821	0.515	0.838

Notes: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. WLS regressions, where the weights are the size of cohorts. Dependent variables: proportion for each group indicated at the top of each column. All regressions include controls for age, schooling, race, metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

Results in Table 8 (panel A) indicate that the higher the proportion of *mensalistas* in 2012, the higher the proportion of the unemployed, economically inactive, *diaristas* and those employed in other service occupations after *EC* 72. This suggests that *mensalistas* are moving to these other positions. An equivalent analysis was conducted for 2011–2012 (Table 8, panel B). The increase in the proportion of the unemployed and *diaristas* are the only trends that were not already occurring before the approval of the Amendment. Therefore, this analysis also suggests that monthly domestic workers could be transitioning to unemployment or to daily worker positions.

Finally, we present a third empirical analysis to investigate whether there has been a change in the composition of *mensalistas* and other labour market positions. The previous methodology of DID weighted by propensity scores is applied considering worker characteristics as outcomes. The result allows us to investigate whether there has been a change in the characteristics among the treatment and control groups considered. If the Constitutional Amendment did not have an effect on transitions among different occupations, we expect to see no change in workers' characteristics.

Table 9 presents the results of this analysis for schooling, age and race. The outcomes indicate that there was a change in the age trajectory of *mensalistas* when compared to employees in service occupations (columns 4, 5 and 6), *diaristas* (columns 10, 11 and 12), all employees (columns 13, 14 and 15) and employees in similar occupations (columns 16, 17 and 18). This change in the age trajectory reinforces the hypothesis that there has been a significant transition from *mensalistas* to other labour positions as a consequence of the Amendment.

In sum, we found evidence that *mensalistas* would be transitioning to other labour status such as *diaristas* or unemployed. Nonetheless, the changes do not seem large enough to imply that this is the main reason to explain higher formalisation rates among monthly domestic workers after *EC* 72. One important caveat of our analysis is that our data are not longitudinal. If we could observe the same individuals throughout time, we could conduct more robust evaluations to examine the mechanisms underlying the process of increasing formalisation and decreasing working hours.

TABLE 9

Composition effects: impact of *EC* 72 on characteristics of domestic workers

	Domestic workers X Service workers			Monthly workers X Service workers			Daily workers X Service workers			Monthly workers X Other employees			Monthly workers X Other similar employees		
	Schooling	Age	White	Schooling	Age	White	Schooling	Age	White	Schooling	Age	White	Schooling	Age	White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(13)	(14)	(15)	(16)	(17)	(18)
2013 X (treated = 1)	-0.054	0.186	-0.016	0.009	0.114	-0.017	-0.072	0.148	-0.011	0.053	0.145	-0.005	0.062	0.220	-0.005
	(0.078)	(0.241)	(0.010)	(0.089)	(0.275)	(0.012)	(0.095)	(0.293)	(0.013)	(0.073)	(0.233)	(0.010)	(0.073)	(0.225)	(0.010)
2014 X (treated = 1)	-0.096	0.584**	-0.015	0.024	0.746***	-0.014	-0.116	0.235	-0.011	0.051	0.653***	-0.006	0.086	0.778***	0.000
	(0.077)	(0.237)	(0.010)	(0.088)	(0.273)	(0.012)	(0.093)	(0.288)	(0.013)	(0.072)	(0.232)	(0.010)	(0.073)	(0.224)	(0.010)
treated = 1	-0.645***	1.008***	-0.005	-0.053	-0.267	0.002	-1.116***	2.264***	-0.014	-0.205***	-0.455***	0.022***	-0.112**	-0.604***	0.010
	(0.055)	(0.170)	(0.007)	(0.062)	(0.193)	(0.008)	(0.068)	(0.209)	(0.009)	(0.051)	(0.163)	(0.007)	(0.051)	(0.158)	(0.007)
2013	0.231***	0.323*	0.016*	0.221***	0.355*	0.016*	0.241***	0.262	0.015*	0.134***	0.375**	-0.000	0.149***	0.227	0.002
	(0.060)	(0.185)	(0.008)	(0.063)	(0.194)	(0.008)	(0.064)	(0.198)	(0.009)	(0.051)	(0.164)	(0.007)	(0.052)	(0.159)	(0.007)
2014	0.335***	0.761***	-0.004	0.330***	0.746***	-0.002	0.324***	0.777***	-0.004	0.223***	0.829***	-0.017**	0.219***	0.723***	-0.018***
	(0.058)	(0.181)	(0.008)	(0.061)	(0.190)	(0.008)	(0.063)	(0.193)	(0.009)	(0.050)	(0.161)	(0.007)	(0.051)	(0.156)	(0.007)
Observations	47204	47204	47204	35879	35879	35879	30088	30088	30088	48411	48411	48411	51038	51038	51038
R-squared	0.158	0.199	0.117	0.130	0.182	0.110	0.171	0.198	0.124	0.075	0.174	0.107	0.079	0.177	0.101

Notes: Standard errors in parenthesis. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. WLS regressions, where the weights are based on the propensity score.
Dependent variables: years of schooling, age and a dummy for white. Treatment dummy indicated at the top of each column. All regressions include controls for age, schooling, and race (except when the dependent variable is the respective control variable), metropolitan regions and 26 state dummies.

Source: Authors' elaboration.

6 CONCLUDING REMARKS

Despite significant changes in its configuration in recent years, paid domestic work remains one of the main occupations of Brazilian women. In particular, paid domestic service plays a leading role in the absorption of less-skilled and -experienced women. This occupation works as a point of entry to the labour market for young migrants from rural to urban areas and is characterised by low pay and high informality rates. Given these characteristics, the role of paid domestic work in Brazilian families and the uniqueness of its labour legislation, further analysis of domestic work in Brazil is extremely relevant.

This paper investigated the effects of Constitutional Amendment 72, which regulated the labour rights of domestic workers in Brazil, on the working conditions of these workers. In particular, we investigated whether the Amendment caused impacts on formalisation rates, working hours and wages of domestic workers. In addition, we analysed the extent to which it affected the likelihood of a woman becoming a domestic worker. Our methodology is based on a strategy that combines the DID methodology with IPW sampling.

The results show that the new legislation had different impacts on monthly workers (*mensalistas*) and daily workers (*diaristas*). Regarding monthly workers, there was an increase in formalisation rates and a reduction in working hours. However, there was no observed effect on wages. For daily workers, no effect was found on any of these indicators, as expected. We also estimated an increased likelihood of a domestic worker being a daily worker, and a change in the characteristics of monthly workers in comparison to daily workers, which could be related to the substitution of daily workers for monthly workers by employers. Thus, it is not possible to determine whether the increased formalisation of monthly workers was due to a shift from informality to formality among *mensalistas* or if there was a change in the composition of monthly workers.

Although not all changes included in the Amendment came into effect immediately after its approval, we believe it is possible to assess whether the proposed changes produced any impact on the working conditions of domestic workers. *EC* 72, which became popularly known as the 'domestic workers PEC', opened up a broad debate in society, and the changes regarding domestic workers' labour rights were broadly disseminated at the time. Still, it is possible that the Amendment will have a delayed effect, since employers and employees may need more time not only to adapt to the new rules but also to obtain more complete information. A longer-term analysis is welcome, and will enable the effects of these changes to be captured more accurately.

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NOTES

5. Both instruments (*EC* 72 and *LC* 150) ratify the mission to effectively improve the working conditions of domestic workers, to which many countries—in particular, developing ones—are committing themselves under the lead of the International Labour Organization and its Domestic Workers Convention No. 189 and Recommendation 201 supplementing it (UN Women 2013; ILO 2012).

6. For instance, Hirata and Machado (2010) define informal workers as those who are self-employed or micro-entrepreneurs.

7. Ulyssea (2006) presents a comprehensive survey of the determinants of institutional performance on levels of informality. For a more recent analysis about institutions and sectoral choices of firms and workers, see Ulyssea (2014).

8. Law No. 11324 from 2006: <http://www.planalto.gov.br/ccivil_03/_ato2004-2006/2006/lei/l11324.htm>.

9. It is worth noting the theoretical study by Summers (1989), which put forward a framework for the supply and demand for labour to analyse the wage and employment impacts of mandated benefits. This framework was later developed and applied in a study for the USA by Grueber and Krueger (1991) and for other developed countries by Freeman (2009). For Latin American evidence, see Heckman and Pagés (2003).

10. Based on a framework similar to the one adopted in Wong (2015) and in this study, Thurston (1997) and Buchmueller et al. (2011) investigate the impact of legislation regarding the compulsory mandates related to social security benefits in Hawaii. Both studies find negative wage effects, but no effect on hours worked by industrial workers.

11. Every month, employers from the private sector have to deposit 8 per cent of the worker's monthly wage into a bank account controlled by the government. The worker has access to the money in specific situations, as in the case of dismissal or illness.

12. Law No. 10208 of 23 March 2001: <http://www.planalto.gov.br/ccivil_03/leis/LEIS_2001/L10208.htm>.

13. The first vetoes the possibility of a 12-hour working day followed by 36 hours of leave. The second one deals with situations that may or may not characterise a reason for dismissal without just cause.

14. The PME was phased out in February 2016. It was replaced by the *PNAD-Contínua* (Continuous PNAD), which does not provide information about number of days or households attended by domestic workers.

15. It should be noted that we are dealing with repeated cross-sections, not panel data. Therefore, we have to consider the difference in the sample size throughout the years. Following Abadie (2005), we weight observations for each year by $1/f_t$, where f_t is the proportion of observations of year t. Finally, we also have to take into account the *PNAD* sampling weights. Accordingly, the w_{it}/f_t is multiplied by the sampling weights.

16. Because we are interested in the ATT, an additional assumption is needed: $P(D = 1) > 0$.

17. To save space, we do not report other mean tests. Those results are available on request.

18. To save space again, we do not report other density comparisons. They are available on request.