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# Minimum Wages for Domestic Workers : Impact Evaluation of the Indian Experience \*

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## Abstract

The paper explores the labor market effect of minimum wage legislations in the informal sector for a developing country. The paper conducts an impact evaluation of the minimum wage legislation for domestic workers introduced in four states in India over the period of 2004-2012. Combining matching procedures with difference-in-difference, the paper estimates both the short-run and the long-run impact of the legislation on real wages and employment opportunities. Results show a positive impact of the legislation on real wages in the short-run, with no significant impact in the long-run. Further, the legislation did not seem to have had any impact on the extensive margin in terms of employment opportunities or the probability of being employed as a domestic worker over the entire period. Available evidence, in line with theoretical predictions, point towards a weak enforcement of the legislation as the driving factor of observed results.

**Keywords:** wages; minimum wages; domestic workers; unemployment; informal sector

**JEL Classification numbers:** J16 J31 J33 J38

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# 1 Introduction

The impact of minimum wage legislations on employment and wages has been the subject of an extensive empirical work.<sup>1</sup> The body of empirical literature on the general impact of minimum wage policy primarily relies on firm or sectoral level data.<sup>2</sup> While the majority of existing studies focus on the impact of minimum wage laws in developed countries, there is an emerging focus on developing countries exploring similar empirical questions. Most of these studies show mixed evidence on the impact of minimum wage laws. The findings suggest that while minimum wage laws tend to increase wages at the intensive margin, the impact on employment at the extensive margin is unclear with results showing positive, neutral or a negative impact.

There is an established body of theoretical literature trying to predict the general impact of a minimum wage law. Most theories predict a uniform negative effect of binding minimum wages on employment in competitive labor markets and a non linear relationship between minimum wages and employment in monopsonistic and oligopsonist models (Stigler, 1946; Bhaskar et al., 2002; Basu et al., 2010; Coffey et al., 2015; Morten, 2016). Basu et al. (2010) develop an incentive compatible equilibrium model which predicts the response of employment to a minimum wage rise as either positive, negative or subdued given the level of minimum wage and enforcement.<sup>3</sup> Hence summarizing both theoretical and empirical work across the general literature on minimum wages we observe that in perfect labor markets, a) there is generally a positive impact of minimum wage law on wages without accounting for enforcement, b) a negative impact on employment without accounting for enforcement and c) ambiguous impact on employment when we account for enforcement. Overall the broad takeaway from existing work on minimum wages is the following. The impact of minimum wages will depend on the market structure, enforcement rates and the gap between existing wages and minimum wages. We use the existing theoretical literature to analyze mechanisms that might drive observed empirical results in the context of a vulnerable informal sector like domestic work.

More than 60% of the global employment arise from informal sector (ILO, 2018). The empirical literature on the the impact of minimum wages for this sector in developing countries while

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<sup>1</sup>See Card and Krueger (1994, 1998); Ashenfelter and Smith (1979); Neumark and Wascher (1995a,b); Bell (1997); Lang and Kahn (1998); Michael et al. (1999); Martin (2001); Carneiro (2001); Fajnzylber (2001); Strobl and Walsh (2003); Lemos et al. (2004); Machin and Wilson (2004); Neumark et al. (2006); Almeida and Carneiro (2005, 2007); Gindling and Terrell (2007); Dube et al. (2007); Ronconi (2008); Lemos (2009); Gindling and Terrell (2009); Dube et al. (2010); Almeida and Carneiro (2011); Khamis (2013); Giuliano (2013); Hohberg and Lay (2015); Dolton et al. (2015); Broecke et al. (2017); Menon and Rodgers (2017).

<sup>2</sup>While some of the literature focus on compliance rates (Ashenfelter and Smith, 1979), others focus on the intensive (in terms of actual wage increase) and extensive impacts (changes in employment status) of the legislation (Neumark and Wascher, 1992; Card and Krueger, 1994; Neumark and Wascher, 1995a,b; Card and Krueger, 1998; Michael et al., 1999; Dube et al., 2007, 2010; Neumark et al., 2013).

<sup>3</sup>Other theoretical work have highlighted the potential distributional effects of minimum wage legislation (Lang and Kahn, 1998).

still relatively small, is an emerging area of empirical research. Most of these studies focus on Latin American countries using firm or sectoral level data (Bell, 1997; Carneiro, 2001; Fajnzylber, 2001; Maloney and Mendez, 2003; Maloney, 2004; Lemos et al., 2004; Almeida and Carneiro, 2005; Neumark et al., 2006; Almeida and Carneiro, 2007; Gindling and Terrell, 2007; Ronconi, 2008; Lemos, 2009; Gindling and Terrell, 2009; Almeida and Carneiro, 2011; Khamis, 2013; Broecke et al., 2017; Menon and Rodgers, 2017). The main findings are the following. First, minimum wage legislations or revisions compressed the wage distribution for both the informal and formal labor market. Second, minimum wage legislations or revisions seem to have had a mixed effect on employment outcomes. Some studies point to negative employment effects of varying degree in both sectors, while other studies have pointed out to a positive employment effect in informal sectors and a negative employment effect in formal sectors. Still, other studies have pointed to null or negligible employment effects across both informal and formal labor markets.<sup>4</sup> The variation in these effects seem to be related to the level of minimum wages and the degree of informality. In low minimum wage settings with high degree of informality the null or negligible employment effects seem to persist across different countries (Broecke et al., 2017). Now most of the aforementioned studies define informality in a broad manner. Some of the broad definitions of informality are i) self employed, ii) as those uncovered by the legislation, iii) as those not providing a labor contract, iv) firms which employ less than a certain number of workers, v) as those with lower wages and vi) those with a lack of access or rights to pension. None of these definitions really pinpoint to actual source of informality or the vulnerability of the groups involved.<sup>5</sup>

Maloney (2004) using survey and labor market data from a variety of Latin American countries show that majority of workers in these broadly defined informal categories are voluntarily seeking jobs due to the following reasons : i) they might be earning relatively more in these sectors, and ii) it is more convenient for them to work in informal sectors due to education, skill or access to finance constraints for formal work. He further highlights that vulnerable groups such as female workers are more likely to work in informal sectors voluntarily despite available higher opportunity costs in formal sectors.<sup>6</sup> Therefore in order to accurately evaluate the impact of minimum wage legislations for vulnerable groups in developing countries it is important to focus on specific sectors that can unbundle the source of informality. One such sector is the domestic work sector which refers to employment within private households where there is little possibility of market driven

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<sup>4</sup>See Broecke et al. (2017) for a comprehensive review on the effects of minimum wages on employment in emerging economies.

<sup>5</sup>When it comes to vulnerability of workers most of the existing studies define it as those who have relatively low skills or education, or those who are relatively young or those who are women.

<sup>6</sup>For example, the disproportionate representation of women in informal self employment can be explained by the flexibility of the sector to balance women's productive (employment) and reproductive (homecare) roles (Maloney, 2004). In other words once a woman goes married it could be more easier for her to take care of her children by exploiting the flexibility of being self employed and not as an employee in a more formal sector.

social protection for workers unless the state directly intervenes through legislation. Further this sector in many developing countries are dominated by vulnerable groups such as young or female workers who have little bargaining power (D'souza, 2010). This sector is especially under-studied within the broad empirical literature on minimum wages. There are only a handful of studies confined to few geographical areas such as the USA and South Africa (Mattila, 1973; Dinkelman and Ranchhod, 2012; Bhorat et al., 2013).<sup>7</sup> The empirical literature on the impact of minimum wage laws for precise informal sectors such as domestic work, is limited, largely owing to paucity of data and a lack of minimum wage legislations. Hence there is a need to expand minimum wage studies for domestic workers in other developing countries where they constitute a large proportion of informal employment.

The paper tries to fill this gap by evaluating the labor market impact of minimum wage legislation in the domestic work sector for a large developing country. The geographic focus of this paper is India. India in general has been relatively understudied within the minimum wage literature with hardly any published study on the labor market impact of minimum wages across both formal and informal sectors (Broecke et al., 2017).<sup>8</sup>

India makes an interesting case for the following reasons. Firstly, according to National Domestic Worker's Movement in India, the number of domestic workers in India range from 4.2 million to 50 million, implying this is a relatively large sector within India.<sup>9</sup> In our sample for the year 2012 approximately 1.35 % of the total employed workers are engaged in domestic work. Secondly, domestic household workers are one of the few occupational groups not yet covered by the national minimum wage law in India despite a lot of debate regarding the same at the national level. In 2011, the ILO passed a convention on decent work for domestic workers but India has not yet ratified this convention.<sup>10</sup> One of the primary reasons for the non inclusion of the domestic services sector in India under the National Minimum wage act of 1948 has been the fact that it is a very personalized (informal) service within a private household. Thirdly, after renewed struggle by domestic workers, a few states in India have included domestic services under their list of sectors covered by minimum wage laws over the past decade. There is currently a proposal about instituting a National Policy for Domestic Workers, which includes the right to minimum wage legislation for all domestic workers. Hence, this study is very timely.

The existing empirical literature on the impact of minimum wage legislation for domestic

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<sup>7</sup>The main findings so far suggest i) private households are very quick to find substitutes as the relative costs of domestic workers increase due to minimum wages, ii) on average minimum wage laws increase wages for domestic workers even in the absence of full compliance and iii) there is little to no significant impact on employment for domestic workers.

<sup>8</sup>The only published study is a recent paper by Menon and Rodgers (2017). The paper looks at the national impact of minimum wages in formal sectors and find little to no impact on labor market outcomes.

<sup>9</sup><http://ndwm.org/domestic-workers/>

<sup>10</sup>ILO Website-<http://www.ilo.org/>

workers in India are mostly qualitative (see Section 3). This qualitative evidence primarily focuses on how the minimum wages were notified and implemented in each of the four states in India. The findings based on case-analysis at the micro level suggests that the minimum wage laws for domestic workers were found wanting in their construction, implementation and effectiveness vis-à-vis other work sectors (Neetha, 2009, 2013c,a; Eluri and Singh, 2013).<sup>11</sup> Hence there is a need to concretely quantify the impact of the minimum wage laws for domestic workers in India. Studying impact of any legislation on informal sector is challenging. Most importantly, there are limited number of surveys that systematically account for informal sector workers. The informal nature of the sector implies that very often there are no written contracts or formal records of transactions that can be exploited for analytical work. In this paper, we overcome some of these limitations by identifying precise informal sector workers, employed in domestic work in India from the national sample surveys and collect information pertaining to their employment. By following the cohort of domestic workers over repeated cross-sectional surveys we try to delineate the impact of minimum wages legislation in four states in India compared to untreated states.

The paper studies the short and long run impact of the minimum wage legislation for domestic workers introduced across four states in India. The paper utilizes four rounds of a cross-sectional national sample survey or NSS (1999, 2004, 2009 and 2012 rounds). Within the NSS surveys, there is an indicator under the enterprise type which identifies ‘private households employing maid servant, watchman, cook, etc.’ All workers engaged in this category can be safely classified as domestic workers. This study exploits state level heterogeneity in minimum wage legislation across four states in India.<sup>12</sup> Our identification strategy combines matching with difference in difference estimations. The matching of domestic workers within treatment states with their counter parts in control states, both before and after treatment period, on a set of observable characteristics help us to create suitable counterfactuals. The paper then analyzes the impact of the minimum wage laws on both the intensive (wages) and extensive margins (employment opportunity).

Based on the data reported by domestic workers, the paper finds a positive impact of the legislation on real wages in the short-run between, 2004 and 2009, while this impact seems to attenuate by 2012.<sup>13</sup> There seems to be no impact of the legislation at the extensive margin on employment opportunities or on the probability of being employed as a domestic sector worker in both the short and the long-run. However in the short run, the magnitude of impact on real wages seems to be large indicating a 30 percent increase in mean real wages compared to control states. The results are consistent for different specifications and for a number of robustness and sensitivity

<sup>11</sup>These studies also highlight the challenges faced in the effective implementation of minimum wage laws in India.

<sup>12</sup> Karnataka was the first to institute minimum wages for domestic workers in 2004, followed by Bihar and Andhra Pradesh in 2007 and Rajasthan in 2008.

<sup>13</sup>The long run refers to the entire period between 2004 and 2012.

checks (see Sections 6). In explaining the mechanism that drives the results, we conduct a variety of tests to rule out supply-side factors. We attribute poor law enforcement and weak monitoring mechanism that accompanied the law as the key factor driving the observed results in terms of wages and unemployment. This is in line with theoretical models presented in [Basu et al. \(2010\)](#), which predict null employment effect along with positive impact on wages under the context of low enforcement rate. Hence, the key policy message of our paper is that highly vulnerable informal workers need a lot more than just an ‘announcement effect’ of minimum wage legislation in order to make a real positive impact on their livelihoods as determined by their wages and employment opportunities (see Section 5.5).

The paper contributes to the literature in the following manner. First, the study attempts to quantify the effect of minimum wage laws for domestic workers on both real wages and employment, and contributes to the emerging literature studying the labour effects of minimum wages in the informal sector. Second, the study adds to the nascent literature studying the impact of minimum wages on the informal sector in India. While 86 % of employment take place in the informal sector, there is virtually no research on the impact of minimum wages in India ([Broecke et al., 2017](#)).<sup>14</sup> The only domestic work literature that exists in India are qualitative literature, ([Neetha, 2009, 2013c,a; Eluri and Singh, 2013](#)) which we exploit to set the context of treatment and delineate the mechanisms driving the results.

The rest of the paper is divided into four sections. Section 2 will give the theoretical motivation informing our analysis. Section 3 will discuss the domestic work sector in India and describe the implementation of minimum wages. Section 4.1 will describe the empirical methodology used in the paper. Section 5 will provide our main results. It starts with exploring the short-run impact of the legislation on wages and unemployment between 2004 and 2009. Following which it discusses the impact of the legislation on wages and employment in the long run over 2004 and 2012. Then it studies the impact of the treatment intensity on wages and unemployment, and proceeds to discuss the impact of treatment on the extensive margins in terms of probability of being employed as a domestic worker. Section 5.5 discusses potential mechanisms that could drive the observed results. Section 6 will describe some robustness checks and sensitivity analysis for the key results. Some of the key robustness checks conducted involve verifying parallel trends assumption, common support, ensuring lack of treatment effect in a placebo sector, conducting tests separately for male and female sample, accounting for distributional changes between pre and post treatment samples.

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<sup>14</sup>To the best of our knowledge, there are only two papers on India analyzing the impact of minimum wages under varying degrees of enforcement on the low wage construction sector ([Soundararajan, 2013, 2018](#)).



## 2 Theoretical Motivation

The effects of the minimum wage legislation depends on the labor market structure and enforcement rates. There are two contrasting predictions. First, if the labor market is perfectly competitive one would expect an increase in wages and a decline in employment (Stigler, 1946). Second, in imperfect markets with monopsony, employers might have a wage setting power unlike competitive markets allowing them to pay wages to a worker below his or her value of marginal product (Stigler, 1946).<sup>15</sup> Hence introduction of a minimum wage legislation depending on how large is the difference with prevailing labor market wages could lead to an increase in employment. In other words given the degree of monopsonistic power of the employer to set wages, a relatively low minimum wage *vis-à-vis* value of marginal product of the worker, the employer can pay the minimum wage and still employ more workers until the minimum wage is equal to the value of marginal product. The increase in employment can vary from no loss, negative or positive gains depending upon the characteristics of the targeted work sector such as the degree of informality or vulnerability.

In developing countries, low paying occupations such as the domestic work sector are less likely to be competitive and more likely to be monopsonistic where employers are more likely to have an influence in setting wages due to i) low skill requirements for the work involved, or ii) high costs involved in worker migration to high wage areas (Coffey et al., 2015; Morten, 2016).<sup>16</sup> This means any effective wage or employment outcomes from minimum wage legislation targeting the domestic work sector depends on the degree of enforcement which pushes employers to comply with the minimum wage rate. There are two possible outcomes in such a case.

First, if the minimum wage introduced is low and below a given threshold and there exists perfect enforcement in its introduction then the wage increase should lower the ratio of the marginal cost of labor to the minimum wage, increasing employment in the new equilibrium. However a high minimum wage above the threshold would induce an increase in the ratio and hence reduce employment in the new equilibrium. This suggests an inverted V shaped relationship between employment and the minimum wage distributions.<sup>17</sup> By construction the binding nature of the enforcement should lead to an increase in minimum wage under the new equilibrium.

Second, Basu et al. (2010) demonstrates that  $\lambda$ , (where  $0 < \lambda < 1$ ) or the enforcement rate (the

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<sup>15</sup>Here monopsony generally represents various types of frictions leading to finitely elastic labor supply stemming from different worker preferences or specific human capital requirements of the firm (Bhaskar et al., 2002; Alan, 2011).

<sup>16</sup>Monopsony in the context of the domestic work sector does not mean only one buyer. It refers to monopsonistic competition where employer's market power persists despite competition with other employers given a large number of employers with free entry (Bhaskar et al., 2002).

<sup>17</sup>See Soundararajan (2018) for a review of the theoretical relationship between minimum wages and employment in low wage - imperfect markets as demonstrated in Basu et al. (2010).



probability of the employer being inspected and any violation being caught) determines how the minimum wage affects the expected marginal cost and not the true marginal cost. The expected marginal cost is the weighted average of the minimum wage and the true marginal cost, where the weight is given by a monotonically increasing function of  $\lambda$  as an interaction with  $\theta$  (Basu et al., 2010).<sup>18</sup> This leads to two key observations in the presence of a low minimum wage given a threshold (Basu et al., 2010): i) a high  $\lambda$  would lower the ratio of the expected marginal cost relative to the true marginal cost and hence increase employment for a wide range along the minimum wage distribution; ii) if  $\lambda$  is low, then the expected marginal cost would be lower than the true marginal cost for a relatively smaller range of the minimum wage distribution. In other words, if enforcement is low we should observe negative or null employment effects along with an increase in wages more likely at the lower end of the minimum wage distribution.<sup>19</sup>

In this paper, we will employ these predictions to partly unravel the mechanisms that drive the key empirical results. Further, the paper provides evidence to support the theoretical predictions contained in Basu et al. (2010).

### 3 Details of the Treatment

The Minimum Wages Act of India, 1948 legally grants a minimum wage for workers in activities listed in the “employment schedule” of the government, maintained by both the central and state governments (Soundararajan, 2013). Domestic work is not part of this schedule maintained by the central government, hence the lack of minimum wages for domestic workers at the national level in India.

The lack of legal recognition of domestic work is not unique to India. ILO reports that many countries either explicitly exclude domestic work from the ambit of labor legislation or make very little provisions to address the needs of domestic workers (D’souza, 2010). Some key features of domestic work differentiating it from other similar low skilled informal occupations are the following (D’souza, 2010), i) the employment relationship is invisible or in other words it is often undeclared and has no written contract involved; ii) there is an unequal balance of power between employer and employee; iii) a lack of precise job description and iv) the expectation on the part of the employer to be available at all times.

India has not yet ratified the ILO’s Domestic Workers Convention, 2011 (No.189) which came into force in 2013. This convention which became a binding international law from September 5th 2013 extends basic labor rights to domestic workers around the globe. In India, the main

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<sup>18</sup> $\theta$  indicates the efficiency of the judicial process and government bureaucracy (Basu et al., 2010).

<sup>19</sup>If enforcement is high we should observe positive employment effects along with an increase in wages less likely at the lower end of the minimum wage distribution (Basu et al., 2010).

cited reasons for the exclusion of domestic workers from the employment schedule are, namely (Neetha, 2009; Armacost, 1994), a) potential large scale loss of employment opportunities of domestic workers, b) the difficulty in enforcing the laws protecting the minimum wage of the workers due to location of work being private space and c) the informal employment relation between the domestic worker and the private household.

However, partially due to mounting pressure of organizations working for the cause of domestic workers, state governments have started taking measures to improve the condition of domestic workers. By 2012, a total of seven states notified minimum wages for domestic workers, namely, Karnataka, Kerala, Andhra Pradesh (AP), Bihar, Rajasthan, Jharkhand and Odisha.<sup>20</sup> Of these states we focus our analysis on Karnataka, which was the first to institute minimum wages for domestic workers in 2004 followed by Bihar and Andhra Pradesh in 2007 and Rajasthan in 2008. See figure 1 for a depiction of these treatment states.<sup>21</sup>

*insert figure 1 here*

Among these four states, it was only in Karnataka and Andhra Pradesh, that unions and organizations working among domestic workers played a key role in bringing the sector under the purview of the Act. The implementation of minimum wage laws in Rajasthan and Bihar were more of a technocratic exercise and the exact reason for their implementation remains unknown (Neetha, 2013b). All these states define domestic work in terms of tasks undertaken and the tasks are listed in their respective minimum wage notification. The tasks listed in the notification vary from state to state but in general they include cooking, cleaning and basic care work.

The notifications on the minimum wages were not rooted in the employment relationship which governs this sector but rather in the societal understanding of domestic work (Neetha, 2013c). For example, within the legislation domestic work is as non technical and an unskilled occupation. Further, minimum wage rates are either fixed for an hour or a day as opposed to a monthly calculation. In the three treatment states (AP, Rajasthan and Bihar), all tasks have the

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<sup>20</sup>There are three more states namely Tamil Nadu, Maharashtra and Assam that have included domestic work in their respective state list of scheduled employment in 2007, 2008 and 2013 respectively but have not fixed any wage rate. Chhattisgarh (2013), Jharkhand (2011) and Kerala (2011) have also extended Rashtriya Swasthya Bima Yojana (RSBY), a health insurance scheme to domestic workers as the first step in widening the coverage of welfare schemes to this occupational group to domestic workers.

<sup>21</sup>We exclude Kerala, Jharkhand and Odisha as treatment from our analysis due to several reasons. Kerala witnessed an informal norm of minimum wage standards for domestic workers as early as in 1989 despite the official minimum wage being notified only in 2010 making Kerala a unique case when compared to the treatment states in our analysis. Hence we exclude Kerala entirely from our analysis. In Jharkhand the minimum wage law was instituted only in 2010 and hence is used only in the control group until 2010 and dropped for later period. Until 2012 Odisha was in the process of including domestic workers in the list of scheduled employment and a minimum wage was fixed only after 2012. Hence, Odisha remains in the control group in our analysis. Our survey data pertains to 1999, 2004, 2009 and 2012, hence we do not have two post-treatment survey rounds for these states. Therefore, we also exclude Jharkhand from the control group in our long run analysis.

same minimum wage while Karnataka has two separate wage rates for housework with and without child care, with the latter commanding a higher rate. Finally, appendix table A-1 shows that compared to the minimum wages for similar work outside private households, domestic workers get a much lower minimum wage for the same job. This is true across the four states. This uniformity resonates with the societal devaluation of house-work as a non productive work in an unproductive work-space (Sankaran, 2013). Table 1 summarizes the variations in the legislation among these four states.

*insert table 1 here*

## 4 Data and Empirical Strategy

### 4.1 Empirical Strategy

Four states instituted minimum wage laws for domestic workers namely: Karnataka, Bihar, Andhra Pradesh, and Rajasthan instituted minimum wages between 2004 and 2009.<sup>22</sup> We have two rounds of the national sample survey (55th (1999) and 61st(2004) rounds) before the law was instituted and two rounds of the survey (66th (2009) and 68th (2012) rounds) after the law came into effect in these four states. In the absence of nationally comparable panel data that includes both treatment and control states during this period, we have to make do with the repeated cross section data available through these surveys. The empirical strategies described below help address some of the key limitations of repeated cross-sectional data to some extent.

Our primary estimation strategy is a difference-in-difference approach for our outcome variables between the treatment and control groups. While the treatment is at the state level the data is at the individual-district level corresponding to each state. The following difference-in-difference empirical model is adopted.

$$y_{idst} = \alpha_0 + \alpha_1 * Post_t + \alpha_2 * treatment_{dst} + \alpha_3 * treatment_{dst} * Post_t + \gamma * X_{idst} + \epsilon$$

$y_{idst}$  corresponds to different outcome variables of interest at the individual level located in a given district  $d$ -state  $s$  pair in time  $t$ ,  $Post_t$  is a dummy indicating 1 for the post treatment period.  $treatment_{dst}$  refers to the district in the treated state in time  $t$ .  $\alpha_3$  gives the difference-in-difference estimator for the outcome variable of interest.

<sup>22</sup>In Karnataka, domestic work was removed from the scheduled list under the Minimum Wages Act in the state of Karnataka in 1993, a year after it was first included. Though the minimum wages act was notified in 2004, it is only in June 2005, Karnataka again brought ‘domestic work’ back into the schedule (Neetha, 2009). So for the purpose of this study the treatment took place in Karnataka in June 2005.

$X_{idst}$  represents different individual and household covariates. It includes age, sex, marital status, education level, land owned by household and a dummy variable distinguishing between higher and lower caste categories. These variables should capture all the relevant vulnerabilities associated with an informal sector such as domestic work related to gender, youth, low education or skills in the literature (Menon and Rodgers, 2017). In addition, we also use the interactions between these covariates in some of the specifications. The reason for conducting regressions with interactions is that some of these interactions will have very significant impact on determining the wage rates. For instance, gender and caste interaction term will help capture any additional discrimination faced by women of lower caste, compared to their higher caste counterparts. Various economic, social and cultural dimensions enters the calculation of wages for domestic sector workers (Sengupta and Sen, 2013) and these interaction terms help to capture several of such factors.

The paper also adopts a combination of matching and difference-in-difference approach to estimate the impact of the minimum wage law legislation in these states. For domestic workers in the treatment states (those that instituted minimum wage legislations), we find counterparts in the same states (treatment states) and non-treatment states, both before and after treatment through a matching procedure. The difference in difference of the outcome variables between these treatment and control group members before and after the treatment, gives us a measure of the impact of the minimum wage legislation.

The main advantage of combining the matching and difference-in-difference methods (MDID) is to weaken the underlying assumptions of these two methods used separately. MDID adopts a common trends assumption - had the treated remain untreated they would have experienced the same change in outcomes as that of the control groups (Blundell and Dias, 2009; Blundell et al., 2004). Let  $t$  and  $t'$  be the time period before and after treatment respectively,  $X$ , the observable characteristics,  $D = 1$  indicates selection for treatment.  $Y_0$  represents the outcome variable in the untreated state. So the underlying parallel trends assumption of MDID and our basic difference-in-difference estimation strategy translates to the following.

$$E(Y_{0t} - Y_{0t'} | X, D = 1) = E(Y_{0t} - Y_{0t'} | X, D = 0)$$

In addition to the parallel trends assumption, MDID also require the following common support assumption.

$$P(D_{it'} = 1 | X, t) < 1$$

With repeated cross section data (like in our case), we need to ensure that the treatment group members have comparable counterparts in all the three control groups namely - the treated

before treatment, untreated both before and after treatment. By estimating treatment effect on the treated via matching in the region of common support we are able to reduce the bias due to non-overlapping support. The primary common support is determined where all observations with a propensity score smaller than the minimum and larger than the maximum in opposite group were deleted using the minima and maxima criterion.<sup>23</sup>

Heckman et al. (1997) cites some additional advantages of MDID. It reduces the bias induced by different questionnaires likely to be used for treatment and control groups as well as the bias caused by differing local market characteristics of the control and treatment groups.<sup>24</sup>

The matching estimator of the treatment effect,  $\alpha^{MDID}$ , can be represented as (Blundell and Dias, 2009; Heckman et al., 1997) :

$$\alpha^{MDID} = \sum_{i \in T_1} \{ [y_{it'} - \sum_{j \in C_1} \varpi_{ijt'}^C y_{it'}] - [\sum_{j \in T_0} \varpi_{ijt}^T y_{it} - \sum_{j \in C_0} \varpi_{ijt}^C y_{it}] \} \omega_i$$

where  $(T_0, T_1, C_0, C_1)$  denotes the treatment and control groups before and after the treatment and  $\varpi_{ijt}^G$  represents the weight attributed to individual  $j$  in group  $G$  and time  $t$  when compared with the treated individual  $i$ ,  $\omega_i$  accounts for the re-weighting that reconstructs the outcome distribution for the treated sample.

Further, we use entropy balancing (represented as ebalance in our estimation tables) as described in Hainmueller (2012) to supplement the matching and DiD estimations. Entropy balancing process ensures balancing of first three moments of covariates between the treatment and control groups. This is achieved through a data pre-processing methodology that relies on maximum entropy re-weighting scheme that calibrates unit weights so that the re-weighted treatment and control groups have balanced moments in terms of all covariates. Hainmueller (2012) finds that this procedure can greatly reduce model dependence of estimation of treatment effect.

In executing the MDID approach, the paper uses seven matching algorithms, each with its own merit and limitation, as summarised in Caliendo and Kopeinig (2008). The algorithms used are kernel density matching, nearest neighbor matching (with and without replacement), nearest neighbor caliper matching (with and without replacement), multiple nearest neighbor with replacement and multiple nearest neighbor caliper matching. Kernel density matching is a non-parametric matching which uses almost all members of the control group, using appropriate weighting, to create a counter-factual. While nearest neighbor selects the member from the control group who is closest to each treatment observation in terms of propensity scores. However, there is a risk that the nearest neighbor is in fact quite far in terms of propensity scores and it can lead

<sup>23</sup>This estimator is inherently different from that of an experimental RCT setup, as we estimate the treatment effect on treated who falls in the region of common support.

<sup>24</sup>These could be potentially important source of bias as exemplified by Heckman et al. (1998). In our case, both treatment and control group members were instituted from the same survey questionnaires making such source of bias unlikely.

to bad matches. To correct for this, caliper matching allows to impose a tolerance level on the distance (in this paper we use a tolerance level of .01) between the matched pairs. In all these cases, matching ‘with replacement’ refers to the case when the observations from control group can be used multiple times to match with treated observations, while, ‘without replacement’ means this is not permitted. Employing these different matching algorithms provide robustness checks to the MDID approach. While we will show the results from all the seven matching estimators when it comes to our main results in sections 5.1 and 5.2, we prefer kernel matching as our primary matching technique whenever we employ MDID approach in subsequent sections including those discussing mechanisms and robustness checks. This is because given the fact that we have repeated cross-sectional data, this approach gives us a greater degree of freedom as the entire distribution in both treatment and control groups are in play.<sup>25</sup>

In addition to the above estimations of impact of treatment. We also study the impact of treatment intensity on the key outcome variables. In the minimum wage literature, the treatment intensity is most commonly quantified in terms of wage gap (WG), which is the difference between the newly instituted minimum wages and the median wages in the pre-treatment period.

$$WG_s = \log(\text{Minimumwage}_s) - \log(\text{Median}(\text{wage}_{s,2004}))$$

The median weekly wages for treatment states appear to be substantially lower than the instituted minimum wage rates.<sup>26</sup>

We adapt the combination of difference-in-difference and matching methodology described in earlier sections to take into account the impact of wage gap on the outcome variables using the following empirical model.

$$y_{idst} = \alpha_0 + \alpha_1 * Post_t + \alpha_2 * WG_{dst} + \alpha_3 * WG_{dst} * Post_t + \gamma * X_{idst} + \epsilon$$

Similar to the earlier equations,  $\alpha_3$  gives us the difference-in-difference estimate of the impact of treatment intensity on our outcome variables. If  $\alpha_3$  is positive and significant it implies that the states with higher treatment intensity (WG) registered higher increase in outcome variable.

## 4.2 Data

This paper uses the 55th, 61st, 66th and 68th rounds of National Sample Survey (NSS) of India, which corresponds to 1999, 2004, 2009, 2012 respectively. All the four treatment states notified

<sup>25</sup>Unless explicitly stated, whenever an MDID result is displayed in the paper it should be assumed to be from a kernel density matching approach.

<sup>26</sup>We calculated the weekly minimum wages using 8 hours work per day and 7 work days per week. To construct the wage gap, the median weekly wages of each state for the pre-treatment year 2004 were used.

minimum wages between 2004 and 2009. This provides two survey years prior to the treatment and two surveys post treatment.

The survey is at the household level and covers the whole of the Indian Union except (i) interior villages of Nagaland situated beyond five kilo meters of the bus route and (ii) villages in Andaman and Nicobar Islands which remain inaccessible throughout the year. The sample size at state level is in proportion to its population and at least 16 first stage sampling units in both rural and urban areas of each stage are included. This allows us to study impact of public policies at state level.

### 4.3 Sample selection and Key variables

How do we identify domestic workers from NSS surveys? Within the NSS surveys, there is an indicator under the enterprise type which identifies ‘private households employing maid servant, watchman, cook, etc’. All workers engaged in this category can be safely classified as domestic workers. The wage rates and other details of the workers are reported by the workers themselves, hence, we can cast aside any reasons for potential exaggeration of reported wages. The wage rate, is the total weekly wages received (both cash and in kind) in the previous week. We convert nominal weekly wages into real wages based on 2004 price levels.<sup>27</sup> Hence our main outcome variable is log real wage. Less than 3 % of the total data in our sample recorded zero values, hence these observations get removed from our analysis when the log values of wages are considered.

We also study the impact of the legislation on unemployment. For the unemployment variable, approximate number of months without work in the previous year is used as the measure. Few caveats about this variable. We only know the record of unemployment of people who are currently employed in domestic work. So if due to legislation some workers got unemployed and they continued to be unemployed when the survey was conducted, we will not be able to track them. Despite this limitation, the variable does give a sense of direction in employment opportunities.

The matching procedure is done based on the following covariates gender, age, marital status, dummy for lower caste (whether belongs to scheduled caste (SC), scheduled tribe (ST), or other backward classes (OBC)), level of education (illiterate, just literate, primary, secondary or post-secondary education), and land owned by the household.

We restrict our analysis on the impact of the minimum wage law for domestic workers to urban areas in the state of Karnataka, Andhra Pradesh, Bihar and Rajasthan.<sup>28</sup>

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<sup>27</sup>We use the food price index of the urban industrial workers provided by the Reserve Bank of India to convert nominal wages to real wages.

<sup>28</sup>We do not include rural areas for the following reasons, (i) domestic work is primarily an urban phenomenon, (ii) due to the problems of enforceability of minimum wage in rural areas, (iii) the National Rural Employment Guarantee Scheme (NREGS), which guarantees 100 days of employment in rural areas, was introduced in many of the districts within these states since 2006. The scheme would have had some impact on domestic workers in these rural areas as



Let us go through an overview of summary statistics. Table 2 compares the sample means for all the Indian States before and after treatment.

*insert table 2 here*

For the key dependent variables namely (log) real wages and months unemployed we observe the following namely, i) mean real wages sharply increase for all states in the post law period compared to pre-law period and the difference is significantly different from zero and ii) mean months unemployed seem to be significantly declining between post law and pre law period indicating an increase in employment for domestic workers. Now table A-2 in the appendix which compares the means before and after treatment only for the treatment states. Again focusing on our key dependent variables we observe that while the mean real wages increase significantly in post law period, the months unemployed decline significantly in post law period again indicating an increase in employment for domestic workers after the treatment.

From the summary statistics one can conclude that there was an increase in mean real wages and decline in the mean months unemployed after treatment in treatment states. However these summary facts despite being suggestive does not account for the heterogeneity in the impact of the law across the treated and untreated states in our sample. Our attempt in the rest of the paper is to isolate the impact of the domestic wage legislation through the observed changes in the outcome variables (wages and unemployment) and see if the increase in wages or decline in unemployment in the treated states is significantly different from that of comparable members in a control group both in the long and the short run.

Now comparing the means before and after the law for our covariates we observe the following i) when we include all states, barring the type of caste the individual belonged to and marital status, all the covariates are significantly different from each other; ii) in the case of treatment states, except for ownership of land and marital status, all variables show significant changes in their mean values before and after treatment; iii) the majority of domestic workers are illiterate and belong to lower castes; iv) the proportion of females (which is coded as 0) is substantially higher than the males in our domestic worker sample both in treatment states and otherwise; and v) the mean age of domestic workers in our sample (both for treated and the non treated states) seem to be around 35 years before treatment and 37 years after treatment.<sup>29</sup> In section 6, we show a test to illustrate that the results we observe is not driven by the changes in these observable characteristics of workers pre and post treatment.

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they were most likely to enroll in NREGS work simultaneously. Hence by including rural areas we run the risk of not being able to distinguish between the impact of NREGS from that of the minimum wage legislation. In rural areas, domestic workers also do agricultural work and some of their tasks cannot be distinguished from agricultural laborers and sometimes they are paid mostly in kind, making the wage measures increasingly prone to such errors.

<sup>29</sup> See tables 2 and A-2.

## 4.4 Balancing and Sensitivity Tests

As our estimation relies on propensity score matching, it is important to verify that balancing conditions are satisfied by the data and ensure the reliability of estimations. This section conducts a set of balancing tests (Girma and Görg, 2007).

The first balancing test checks whether the standardised differences or bias between the covariates used for matching is lower than an accepted threshold value. The standardised differences is essentially the difference in means between the values of households in treated states and the matched observations in control states scaled by the average variances. Following Rosenbaum and Rubin (1985), if the threshold value of the bias reduction is less than 20 then we have not achieved adequate balancing between the treatment and matched-control groups. Secondly, for each covariate we check if the difference between treatment and matched-control means are significantly different from zero using a simple t-test. Thirdly, we conduct Hotelling's t-test to check whether the cross-sample differences in covariates for treatment and control groups are jointly significant. Fourthly, we employ a regression framework for each covariate and estimate the equation given below as described in Smith and Todd (2005).  $P(\hat{X})$  denotes the estimated propensity score and  $D$  denotes the treatment dummy. Then for each covariate included in the matching algorithm, the regression function (quartic in  $P(\hat{X})$ ) is estimated. If the propensity score satisfies the balancing condition, then the treatment status (or  $D$ ) should not convey any additional significant information about the covariates and we should expect all the  $\gamma_s$  to be jointly statistically insignificant (Smith and Todd, 2005).

$$Covariate = \beta_0 + \sum_{k=1}^4 \beta_k P(\hat{X})^k + \sum_{k=1}^4 \gamma_k D P(\hat{X})^k + \epsilon$$

The results of the balancing tests for the covariates in the paper are given in table 3. Across the four tests, the balancing conditions are satisfied. The t-test are insignificant for each of the covariates. The bias reduction is more than the threshold value for each of the covariates. The Hotelling's t-test does not reject the joint null hypothesis of similarity of means of covariates between the treatment and matched-control groups (t-squared value of 1.06; p-value of 0.99). The regression test proposed by Smith and Todd (2005) also supports the balancing result as all the interaction terms with the treatment dummy are jointly insignificant for each of the covariates. We also check the sensitivity of propensity score balancing between treatment and matched controls by using various re-weighting schemes laid out in DiNardo et al. (1996); Nichols et al. (2008), followed by joint hotelling t-tests.<sup>30</sup> The results support a balanced sample across both the treatment and control groups. We further check the robustness of matching estimates to minor changes

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<sup>30</sup>Results will be presented on request.

in the propensity score model (Dehejia, 2005). The matching estimates emerge robust to minor changes in the propensity score model.

*insert table 3 here*

## 5 Results

### 5.1 Aggregate Results : Short-Run Impacts on wages and unemployment

We analyze the impact of the minimum wage legislation for domestic workers on two main outcome variables: log real wages and unemployment. For both these outcome variables we study the impact using three approaches explained earlier : normal difference-in-difference, entropy balancing (ebalance), matching with diff-in-diff (MDID) with different matching procedures including kernel density matching, nearest neighbor matching (with and without replacement), multiple nearest neighbor matching with three neighbours, caliper nearest neighbor matching (with and without replacement).

Table 4 provides the estimations for normal difference-in-difference (DiD) estimations and entropy balanced (ebalance) DiD estimations for all four treatment states. We find statistically significant positive impact under both ordinary DiD and entropy balanced procedures on real log wages in these treatment states.

The covariates affect the real wages in the expected direction. For example, belonging to lower caste implied you earned lower wages, while more educated workers received higher wages compared to less educated counterparts. Estimations show that many of these interactions are significant in determining the bargaining power and hence the wage levels of domestic sector workers.<sup>31</sup> The positive impact of minimum wage legislation holds even in this specification which includes all interactions between relevant covariates. For all these specifications we find a significant and positive impact of the minimum wage legislation on wages. The magnitude of the impact is also very similar.

*insert table 4 here*

We next employ the MDID approach to study the impact of minimum wage legislation on real wages for domestic sector workers, using various matching algorithms discussed in section 4.1. We match the treatment group members with counterparts from these states in the pre-treatment phase and we follow the same procedure for control group members. The matching is

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<sup>31</sup>We do not discuss the signs and significance of these covariates and their interactions in this paper as they are not part of the key results. The detailed results of their impact can be provided upon request.

carried out on all the covariates used in the normal DiD approach. Since we have a repeated cross sectional data, this procedure ensures that difference in difference is estimated using matching of counterparts across different survey rounds, using a set of observable characteristics. Table 5 gives the result of the MDID estimation where we cluster the standard errors at state level. Similar to the case of entropy balancing and DiD, we find a significant positive impact of minimum wage legislation on weekly real wages. The results also hold for short run MDID estimates where the standard errors are clustered and interactions between all the covariates on which the data is matched are included as controls.<sup>32</sup> We observe that there is a significant positive impact of minimum wage legislation on weekly real wages. The magnitude of the impact ranges from 0.32 to 0.48, all significant at 1 % level, slightly higher than the estimates of DiD estimation in table 4.

*insert table 5 here*

Figure 2 below gives the distribution of log real weekly wages before and after treatment for both the treatment and control states. We clearly observe a rightward shift in the distribution of log real wages in the treatment states which is significantly larger than any observable shift in the distribution of the control group.

*insert figure 2 here*

We conduct a similar analysis for our second outcome variable of interest- the measure of unemployment. This measure of unemployment records the number of months each surveyed individual was unemployed during the past one year from the survey date. We find no statistically significant impact of the minimum wage legislation on the unemployment measure using both the DiD and MDID approaches. This suggests that in those states where minimum wages were instituted, there is no evidence of any significant change to the employment opportunities of workers compared to the control group states. We adopted the same estimation strategies that were used for testing the impact on real wages and the results are shown in tables 6 and 7.

*insert tables 6 and 7 here*

## 5.2 Aggregate Results : Long-Run Impacts

The NSSO round for the year 2012 enables us to study whether the positive impact of the legislation on wages persisted in treatment states in the long-run, i.e., for the period 2004 to 2012.

Table 8 provide the normal diff-in-diff and e-balance estimations of the long-run impact of the minimum wage legislation on real wages. We find that the treatment impact has reduced in

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<sup>32</sup>Tables not included in the paper. It can be provided on request.

magnitude and is statistically insignificant in the long run. We also use the matching with diff-in-diff and arrive at the exact same conclusion, as shown in table 9. The results suggests that the impact of minimum wage legislation in these four treatment states seems to have worn off by 2012.

*insert tables 8 and 9 here*

We repeat the long-run analysis with unemployment measure as the outcome variable in table 10 and 11. Similar to the short-run case, we find no evidence of impact on the unemployment measure. Hence, it rules out the possibility that the lack of significant impact on real wages in the long-run was due to changes in employment options for workers. For instance, if workers had experienced higher unemployment rates following the introduction of the minimum wage legislation, it would have led them to negotiate for lower wages with their employers, which in turn would have reduced the impact of minimum wage legislation on wages. However, employment opportunities seem to have not responded to the minimum wage legislation in either the short-run or in long-run.

*insert tables 10 and 11 here*

### 5.3 Intensity of Treatment

As explained in the earlier section, the minimum wages set by the various treatment states differed in terms of hourly rates, the method of calculation and the tasks included. Naturally, the difference between the pre-existing wages and the stipulated minimum wages, which defines the treatment intensity, also differed across these states. In this section we further analyze the impact of treatment intensity on our outcome variables. The treatment intensity is most commonly quantified in terms of wage gap (WG), which is the difference between the minimum wages and the median wages in the pre-treatment period, as explained in section 4.1.

*insert tables 12 and 13 here*

Table 12 and 13 gives the wage gap estimates using both the normal and entropy balanced DiD in the short-run and long-run, respectively. We find that the intensity of treatment led to a positive and statistically significant increase in log real wages for treatment states in the short-run. Higher the wage gap in the treatment states, higher were the increase in real wages. But this impact seems to attenuate in the long-run. This result holds with the model specification after including all covariates and their interactions as well. Similar to the earlier case, there seems to be no impact on the unemployment measure.<sup>33</sup>

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<sup>33</sup>The regression results for the unemployment measure is not included.

However an important point to note is that minimum wages in each of the four treatment states have been updated since their respective state governments issued notification for minimum wages to domestic workers for the first time. In the preceding analysis we used the minimum wages as updated until September 2012. While the use of the 2012 minimum wages is relevant for the long run intensity of treatment (as in Table 13), we re-run our short run intensity of treatment by creating wage gap variable for each of the treatment states with minimum wages notified until the year 2010.<sup>34</sup> We also run specifications by including state dummies and interactions (both individually and together). The short run impact remains positive and significant as before. Furthermore using the 2010 updated minimum wages in order to calculate the long run intensity of treatment does not change the results.<sup>35</sup>

## 5.4 Impact at the Extensive Margin

In this sub-section we try to identify whether the minimum wage legislation had any impact at the extensive margin. The question is did the minimum wage legislation increase the probability of being employed as a domestic worker in these treatment states? To study this impact at the extensive margin, we develop a dummy variable that indicates if the worker is a domestic sector worker or not. As a comparator group, we restrict our sample to unskilled job categories that could provide job alternatives to potential domestic worker, namely people employed in hotels, restaurants and construction. We treat the dummy indicator variable as the outcome and apply a linear probability model, within the normal and entropy balanced DiD frameworks described earlier. We conduct our analysis for both the short-run and long-run time frame described above.

Table 14 and 15 provide the results. It seems that there was a secular decline in the overall probability of being employed as a domestic worker in India since 2004, denoted by the negative and significant coefficient of the Post variable. This could be because of opening up of other employment opportunities in the urban centers. However, our treatment effect is statistically insignificant implying that the minimum wage legislation did not affect the probability of being employed as a domestic worker in the treatment states. Hence, there seems to be no evidence of significant treatment impact at the extensive margin. Considering that the legislation had a limited short-term impact on wages in treatment states, this does not come as a surprise. In South Africa, [Dinkelman and Ranchhod \(2012\)](#) find that despite strong positive impact on wages of domestic sector workers, there was no evidence of any impact at the extensive margin following the minimum wage legislation. Our conclusions seem to resonate with the South African evidence in this respect. We also conduct an extension of this analysis to determine if the treatment did have any

<sup>34</sup>The 2010 minimum wages were recovered from the Labor Bureau of India.

<sup>35</sup>Results can be presented on request.

impact on the probability of being employed in a close substitute sector for the domestic worker. We selected workers in hotels and restaurants to be this sector as their tasks and even work timings are comparable to that of domestic workers. We conduct an extensive margin analysis on the probability of being employed in hotel and restaurants. We find no treatment spill over to this sector at the extensive margin. The results of these placebo extensive margin analysis are included in appendix tables [A-5](#) and [A-6](#).

*insert tables [14](#) and [15](#) here*

## 5.5 Possible Mechanisms

This section discusses the possible mechanisms that might help explain the observed results in the short run and long run. The estimations suggest that there was a strong impact on real wages in the short-run but this impact disappears in the long run. There are two potential mechanisms that can drive the result, firstly, supply side adjustments, where in which, the higher minimum wages in treatment states led to increased supply of domestic sector workers in these states, leading to gradual disappearance of differential trends in wages between treatment and control states.<sup>36</sup> Secondly, the lack of enforcement of the legislation leading to a gradual disappearance of the positive announcement effect of the legislation on wages in the short-run.

To uncover which of the potential mechanisms were at play we conduct a distance analysis, where in which we classify the districts within the control groups according to their distance from the treatment states. Those control groups that fall within a 250 km distance of treatment groups (from the state borders) were called ‘near controls’, those further than this distance are classified as ‘far controls’. The left panel of figure [A-10](#) maps the aforementioned distance classification. While this is an arbitrary boundary limit, the results also hold with a non-arbitrary near and far control grouping based on contiguous boundaries. Refer to section [6](#).

*insert figure [3](#) here*

Figure [3](#), traces the movement of estimated mean wages in treatment groups and distance classified control groups in 2004 (61st round), 2009 (66th round) and 2012 (68th round). As shown by the previous empirical analyses, the mean estimated wage of the treatment group for

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<sup>36</sup>Short term migration, often seasonal, is a common consumption smoothing mechanism adopted by the poor as a response to the lack of rural earning opportunities during the agricultural off season ([Khandker, 2012](#); [Bryan and Morten, 2015](#); [Morten, 2016](#)). From section [2](#) one could conceive that in imperfect monopsonistic markets such as the domestic work sector when low range minimum wage legislation is introduced, short term migration in response to the minimum wage legislation might positively increase the supply of workers to levels allowing for the positive labor supply to attenuate any initial increase in wages even under high rates of enforcement of the minimum wage legislation over time. At low rates of enforcement the attenuation effect on wages should be faster over time. Similar to wages one might also expect significant negative employment effects over time under both low and high rates of enforcement.



the 66th round significantly rises relative to the real wages in the control groups when compared to the pre-treatment 61st round. The movement of the estimated mean wages in the treatment relative to the control group classified by distance between the 66th and the 68th round shines light on the potential mechanism driving the lack of a long-run impact from the minimum wage law. The figure clearly shows that the wages in all the control groups are on the rise between 2009 and 2012, while the mean wages in the treatment group seem to have stagnated.

Figure 4 plots the movement of average months unemployed, which shows that the trends in the estimated measure of unemployment were similar across the treatment, near and far controls without any significant impact from the minimum wage law.

*insert figure 4 here*

The disappearance of the positive impact on treated wages in the long-run can be due to two potential mechanisms. First, the significant short-run effect on wages observed in 2009 for the treatment states had a significant impact on the labour supply in the control groups which led them to adjust wages upwards in order to equalize wages with treatment states in the long-run. Second, the legislation was not effectively implemented or enforced in the treatment states and as a result the wages in the treatment states stagnated in the long-run, following a short-term surge after the announcement of minimum wages, while the wages in the control groups gradually caught up with the treated states by 2012 given the rigidities associated with the informality of the domestic work sector. The gradual catch up of wages in the control group relative to the treatment group might be a spill over ‘numeraire’ effect of the minimum wage announcement in the treated states (Maloney, 2004).

It is difficult to delineate between these two potential mechanisms. However, evidence from the previous section revealed that there was no impact of the legislation at the extensive margin on the employment probability of domestic workers. Also, there was no impact of the legislation on the unemployment rates of workers. This seem to reduce the possibility that any significant supply-side adjustments might have taken place as a result of the legislation. Further figure 5, plots the movement of actual log real wages in domestic work versus close substitute or placebo sector consisting of hotel and restaurants workers in both the treatment and control groups. We selected workers in hotels and restaurants to be the placebo sector as their tasks and even work timings are comparable to that of domestic workers. In the placebo sector we observe that in post treatment rounds (66th and 68th rounds), there seems to be a secular increase in real wages when compared to the pre-treatment round (61st round). This however is not the case with domestic work where the actual wages seem to be stagnating between 2009 and 2012 similar to figure 3. It seems that the domestic work sector in treatment states seem to have missed out on that increase

relative to the controls characterized by distance. These observations help to eliminate the first potential channel.<sup>37</sup> We also estimate the average log real wages for the placebo sector using the MDID framework and the results are similar to the actual increase in real wages as seen in figure 5.<sup>38</sup>

*insert figure 5 here*

Difference-in-difference estimations provided in appendix table A-8 shows a negative and significant differential impact of the law on real wages of domestic workers in the treated states relative to the control groups between 2009 and 2012.<sup>39</sup> So why domestic workers in the treatment states not experience this secular increase in wages relative to the control states between 2009 and 2012?

Following the announcement of minimum wages in treatment states, often by Ministers, gave the legislation a significant media attention. In fact, a survey conducted among employers in treatment states found that Media reports, newspapers and TV, were cited as the main sources of information on minimum wages Neetha (2013a). This might have led to wage adjustment in treatment states as captured by short-run analysis (between 2004-2009). However, plenty of qualitative evidence from the treatment states seem to support that there was very weak monitoring and implementation of the legislation (Neetha, 2013c). Firstly, as shown earlier the average minimum wage in each of the treatment state as seen in table 1 is at the lower end of the minimum wage distribution. Second, Neetha (2013a) provides ample qualitative evidence on weak enforcement of legislation from all the four treatment states. Some of the key impediments to the enforcement that were identified through interactions with labour ministry officials, domestic workers and union representatives ranged from the lack of awareness about the provisions within the legislation among workers and employers to restrictions on labour inspections at private households. The weak enforcement of the legislation most likely incentivized employers not to update the wages beyond 2009 allowing wages to stagnate in treatment states.

As illustrated in section 2 a plausible explanation for the same can be found in Basu et al. (2010) which shows that turning a blind eye to a minimum wage legislation by the very same government which passed it can be an equilibrium phenomenon with ex post credibility in a model

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<sup>37</sup>Coffey et al. (2015) using a unique survey data on short term migration characteristics in India show i) migration is seasonal depending upon agricultural productivity, ii) 99% of trips recorded trips were less than 4 months long with the median value being 30 days, iii) over 80% of the migrants are employed in construction, agricultural labor or factory work. This gives further credence that the labor supply in domestic work sector was not likely to have been affected by short term rural to urban seasonal migration.

<sup>38</sup>Figure for the estimated placebo log real wages under the 'near control' distance classification can be presented on request.

<sup>39</sup>We also run MDID estimates and find the similar negative differential impact on real wages between 2009 and 2012 for treatment states relative to the control states.

of minimum wage policy with imperfect competition, imperfect enforcement and imperfect commitment. The result indicates that since credible enforcement most likely requires costly ex post transfer of income from employers to workers, a government concerned only with pareto efficiency but not with distribution is unable to credibly elicit efficiency improvements via a minimum wage reform. This theoretical conclusion is in line with our results. Our explanation, supported by the theoretical model in [Basu et al. \(2010\)](#), focuses on the failure in the enforcement of the law. Interestingly, congruent to these theoretical implication, in Karnataka and Andhra Pradesh, the minimum wage notification for domestic sector workers came even with the explicit guideline that employers are exempted from maintaining records of employment ([Neetha, 2013c](#)). This seems like a possible intervention by Governments to pacify the employers of domestic workers.

Our results are not at all startling looking at the implementation mechanism that accompanied these legislation. In all the treatment states complaint redressal and monitoring mechanisms were virtually non-existent. As a result, the impact of legislation seems to have waned off completely by 2012, with little effective updating or no proper implementation of wages.

## 6 Robustness Checks

As explained in the section on estimation methodology, the difference-in-difference approach strongly relies on the assumption of parallel trends, even when we combine the same with matching methodology. Since we have two survey rounds prior to treatment, pertaining to 1999 and 2004 respectively, we test the assumption of parallel trends. The methodology of testing for parallel trends is straight forward. We assume that year 2004 is the post-treatment phase and 1999 as the pre-treatment phase.<sup>40</sup> In order for parallel trends to hold, our estimate of treatment effect should not be statistically significant. These estimations are included in the appendix tables [A-7](#). We find that parallel trends assumption holds for both real wages and unemployment measures as the treatment effect was not significant.

The second assumption that MDID relies on is that of common support, which translates to the fact that there is an adequate overlap in the characteristics of treated and control group units to find sufficient matches. We test this assumption for all our MDID estimations and we find that common support assumption holds in terms of each of our covariates and their interactions for both our outcome variables. In addition, in all the MDID estimations, the sensitivity of the common support region was checked. We use two methods to accurately determine the region of common support. First all observations with a propensity score smaller than the minimum and larger than

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<sup>40</sup>We also plot the distribution of log real wages in 1999 and do not find any significant distributional differences between the treatment and control states. Figure can be presented on request.

the maximum in the opposite group were deleted using the minima and maxima criterion. This is the primary method operationalized earlier. Second, following Heckman et al. (1998), 5% of the observations with low density values were trimmed. As a further robustness check we also trimmed 10% of the observations with low density values, and following (Black and Smith, 2004; Mizala et al., 2011), we define a thick support region defined by  $0.33 < p(\hat{x}) < 0.67$ . The results do not change.<sup>41</sup> In addition, in all the MDID estimations, the balancing of the covariates between the treatment and control groups were verified using the t-test.<sup>42</sup>

In order to ensure that our results are not driven by any one particular state, we remove one treatment state at a time and re-run our analysis and we find that our results holds in all these specifications. In Karnataka the treatment took place in 2005, hence by the time we record the treatment impact in 2009, around four years have passed since the introduction of the legislation in Karnataka. While in other states the treatment impact is recorded within a much shorter time frame. When we remove Karnataka from our sample and re-run the short run analysis the magnitude of the impact reduces slightly. Hence, the net short-run impact on wages has already started to wane by 2009.<sup>43</sup>

We conduct further robustness check by running the exact difference-in-difference estimations used to derive our main results for a placebo sector. As stated in section 5.5 we select workers in hotels and restaurants to be the placebo sector as their tasks and even work timings are comparable to that of domestic workers. If our main results hold, the difference-in-difference analysis of the outcome variables (wages and unemployment) should not show any significant results for this placebo sector. We conduct the normal diff-in-diff, ebalance and matching analysis and find that there is no statistically significant treatment effect in the placebo sector. The diff-in-diff and ebalance results of this robustness check is provided in table A-3 and A-4 in the appendix.<sup>44</sup> This implies that the treatment effect was not capturing any macro-level shock that affected all similar sectors in the treatment states during the post-treatment period. This imparts further confidence in our identification strategy's ability to isolate the impact of the treatment on domestic sector workers.

In order to develop a more comparable control group, we further restrict our control groups to untreated states that share borders with treatment states and then re-run the analysis. The diff-in-diff and ebalance results of this robustness check provided in table A-9 and A-10 in the appendix clearly show that after restricting the control group to the untreated border states, we see a significant treatment effect but with similar signs and magnitude on log real wages in the

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<sup>41</sup>The results can be presented on request.

<sup>42</sup>The result can be presented on request.

<sup>43</sup>For brevity, results of these robustness checks are not included here and would be made available upon request.

<sup>44</sup>The MDID results will be presented on request.

short run and no significant effect in the long run.<sup>45</sup> The increase in wages in the short run or  $\alpha_3$  estimate including the MDID results ranges between 0.26 to 0.33, which is similar to the increase in our main results.

Domestic work sector in India is dominated by female workers, so we conducted a sub-sample analysis for female workers. Our results remain similar in magnitude and in significance level for this sub-group.<sup>46</sup> Unemployment for long duration could indicate lower abilities which might translate to lower earnings potential. In order to purge our analysis from such impacts, we removed the workers who were unemployed for more than 6 months in the past year from our sample and repeated the analysis. We find that our results hold even after removing these outliers.<sup>47</sup>

Implementation capacity of states could be different, which could hamper the implementation of any law. We looked at the NREGS implementation performance reports of the four treatment states. Andhra Pradesh and Rajasthan emerge as top performers in terms of NREGS implementation, with Karnataka and Bihar trailing behind (Breman and Varinder, 2012). So we clubbed together the two top-performers and repeated the analysis exclusively for these two treatment states. We found that our results did not vary from the main findings, suggesting that the minimum wage law implementation did not vary significantly in these two states.<sup>48</sup> Further, using migration statistics from India, Clément and Papp (2016) show that the migration effect of NREGS on urban areas affected mostly the male, short-term migrants who engaged in mostly casual unskilled activities especially in construction and manufacturing sectors. So the impact of differential implementation rates of NREGS during the initial phase of its implementation, on domestic work sector dominated by women who tend to migrate for long-term, would be limited.

We follow the approach taken by Dinkelman and Ranchhod (2012) to show that the shift in observable characteristics of the domestic workers do not account for the significant shift in wages between 2004 and 2009. Following the same methodology as in Dinkelman and Ranchhod (2012) appendix 2, figures 6 shows that despite re-weighting the pre-treatment data with the post-treatment distributional characteristics, the rightward shift in wages after treatment persists in the short-run.

*insert figure 6 here*

Furthermore, to control for any differential impact due to scale of cities, we estimate MDID short run estimates with city dummies and the interaction terms as additional covariates (with clustered standard errors). We create dummies for 10 big metros or cities of India namely Mumbai,

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<sup>45</sup>The MDID results will be presented on request.

<sup>46</sup>We could not conduct a similar sub-group analysis for male workers due to insufficient observations in some states. For brevity, results of these robustness checks are not included here and would be made available upon request.

<sup>47</sup>The results can be made available on request.

<sup>48</sup>The results can be made available on request.

Delhi, Kolkata, Chennai, Bangalore, Hyderabad, Ahmedabad, Pune, Surat and Jaipur and include it as additional controls in our regressions along with interaction controls. The key results hold even after addition of these controls.<sup>49</sup>

Finally, within the distance analysis provided in section 5.5 the near controls were defined as those belonging to a 250 km radius of treatment states. We modify the definition of near controls to include, a) those districts in non-treatment states with contiguous boundaries to the treatment states and b) those districts in non-treatment states with contiguous boundaries to non-treated neighboring districts in a). All the remaining districts in the non-treated states are classified as far controls. The right panel of figure A-10 maps the new distance classification. Graphs A-7, A-8 and A-9 in the appendix plots figures 3,4 and 5, respectively with the modified definition of control groups (near controls are now labeled as 'nbr' or neighbor controls). The results are robust to change in the definition of far and near controls.

## 7 Discussion and Implications for Future Research

The main conclusion derived from the above analysis is that minimum wage legislation for domestic sector workers seems to have had a significant positive impact in terms of real wages in the short-run (by 2009), while no impact was detected in the long-run (by 2012).

The estimated impact on log real wages is around the magnitude of 0.3, which translates to a 30 percent increase in real wages approximately (in 2004 prices). This is a substantial increase in the short run. However this impact of minimum wage legislation becomes rather limited in the long run as the short run increase in wages are not sustained after 2009. In fact in treatment states, the domestic sector wages seem to have missed the secular rise in wages observed in a close informal sector such as hotel and restaurants across both treatment and control states. The paper rules out the case of a supply-side adjustment as the driving factor for this change and points at the direction of weak implementation or enforcement of the legislation.

The paper also throws light on the question of how these minimum wages stood in comparison to the wage levels that existed in these states before the implementation of the minimum wage legislation. In our analysis of treatment intensity, we attempted to answer this question. We found out that the pre-existing minimum wages were considerably lower than that of the introduced minimum wages in the treatment states. Hence, if the minimum wage legislation was implemented properly, we would have observed a still greater impact on the outcome variable of real wages. Further, the median weekly wages in post-treatment period in treatment states were considerably lower compared to the stipulated minimum wages.

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<sup>49</sup> The result tables can be presented on request.



Our study finds that legislation alone cannot make much difference to the lives of domestic workers. A strong, transparent and efficient monitoring mechanism is crucial. The majority of domestic workers operate on an informal basis mostly without a work contract or any kind of formal agreement on wages or regarding number of holidays per year. [Sengupta and Sen \(2013\)](#) point to the fact that the minimum wage laws protect the employers interests compared to that of the domestic workers as the employers do not have to maintain registers on wages given nor create contracts with the domestic workers. This certainly complicates the monitoring and enforcement mechanism in this sector. Perhaps minimum wage legislations should also make it compulsory to issue formal contracts to domestic workers. Stronger formalization efforts need to accompany these minimum wage legislation to increase the probability of enforcement. Formalizing the overall rights of the domestic workers beyond just the minimum wage is also important. This was the approach taken in Brazil, where minimum wage legislation for domestic workers was followed up by efforts to increase formalization of the sector. Beginning with guaranteeing fundamental labor rights enjoyed by workers in other sectors to domestic workers, registration of labor cards of domestic workers was made mandatory. Subsequent to these efforts in Brazil, between 2001 and 2009 the number of formalized domestic workers increased by 30% in Brazil ([Gomes and Puig, 2013](#)). Another possible intervention to increase enforcement of minimum wage legislation is through providing incentives for employers to comply with the legislation. Incentives in terms of tax credits are used in Belgium and France to increase compliance to regulations in domestic sector employment ([D'souza, 2010](#)).

Placement agencies that recruit workers from rural areas to be placed in urban households are emerging as key players in this largely informal domestic work sector. Regulating the work of these agencies by instituting strict guidelines on the need for maintaining records, formal contracts for hired candidates could be a way of improving the effective implementation of the minimum wage legislation. Further, the Government of India has initiated a new universal identification scheme in terms of issuing biometric id cards (called Aadhar cards) that are attached to bank accounts of all citizens. In future, the payment of domestic sector wages to the workers' bank accounts could be made mandatory, opening up further avenues to formalize this sector.

In terms of future research, the impact of NREGS need to be explored. NREGS is an all India scheme which was expanded to all districts in India before our post-treatment period, hence it is reasonable to assume that most urban areas would have felt the reduction in flow of rural migrants in a more or less uniform manner.<sup>50</sup> On a related note, the impact of minimum wage legislation on migration within India needs to be studied. It needs to be researched if differing migration rates played any role in the stagnation of wages in treatment states. But considering the

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<sup>50</sup>However, this effect needs to be verified through appropriate future research and data.



insignificant impact of minimum wage legislation at extensive margin and on unemployment, it is safe to assume this effect to be minimal. Another pointer for future research is the impact of union membership of worker on wage bargaining.

## 8 Conclusion

This paper conducts an impact evaluation of the minimum wage legislation for domestic workers in urban areas of four Indian states. It tests the impact of the legislation at both intensive (real wages) and extensive (employment opportunity) margins in both short-run and in long-run. We find that the minimum wage legislation had positive and statistically significant impact on real wages in the short-run. The paper also finds a similar impact in terms of the intensity of the treatment itself, measured by wage gap. Higher wage gap resulted in higher treatment impact in the short-run. However, the strong positive impact on real wages seems to disappear by 2012 in these four states. In the case of employment opportunities and the probability of being employed as a domestic worker, the legislation seems to have had no impact in both short and long run. Qualitative evidence from the ground-level analysis of these treatment states, finds that the legislation was not accompanied by any strong enforcement and monitoring mechanisms, which we attribute as the key factor driving the results. As a result, the wages in treatment states stagnated over the years, after the initial surge following the announcement of minimum wages, while in the control groups the wages gradually rose to catch-up with wages in treatment states.

## Results : Tables and Figures

Figure 1: Map of India-Treatment and Control States

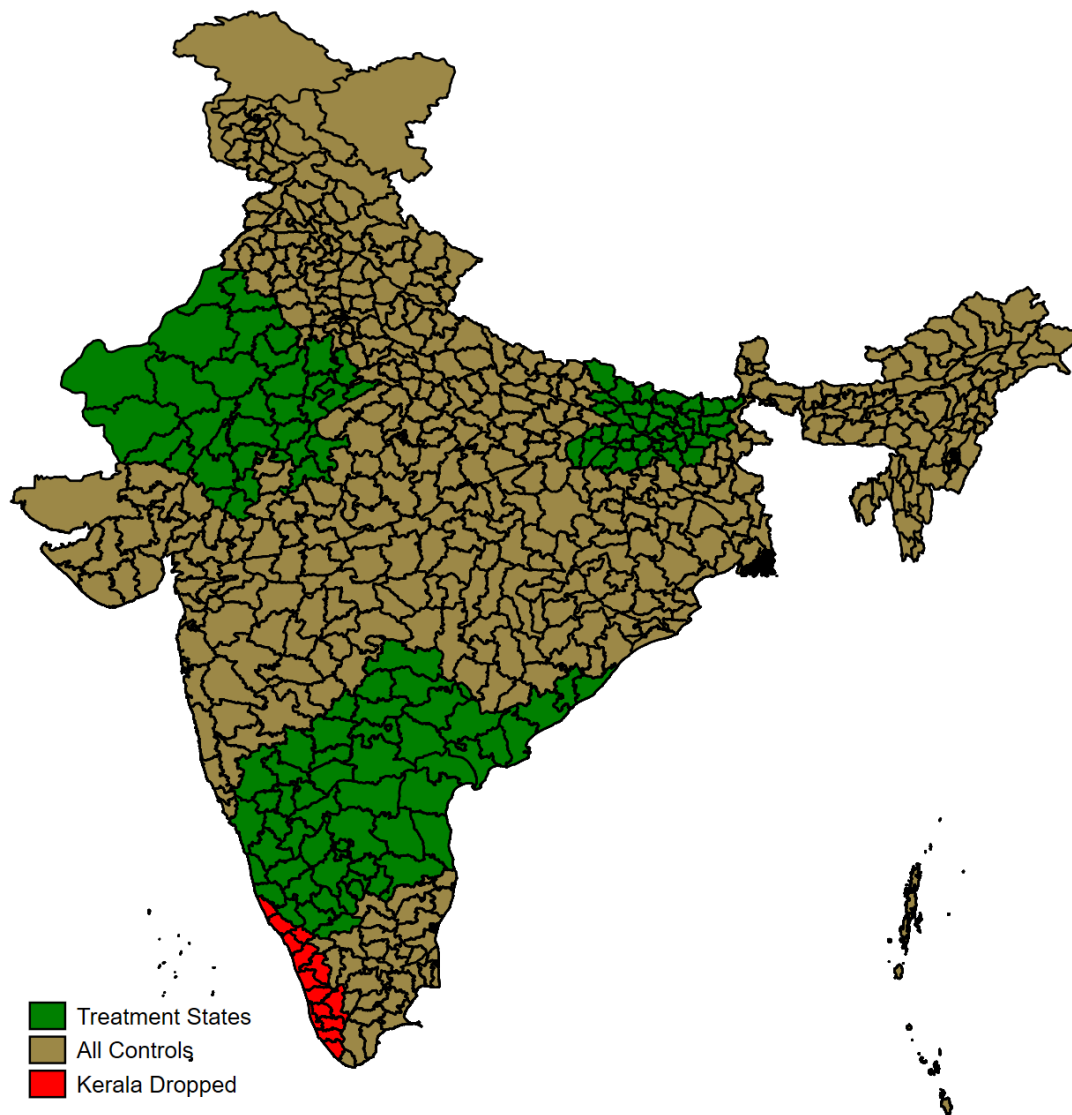


Table 1: Summary of Domestic Minimum Wage legislation (between treatment states)

State	Year of Introduction	Average Wage	Same wage for all tasks	Strong Unions
Karnataka	2004	165.58	no	yes
Andhra Pradesh	2008	173.85	yes	yes
Bihar	2007	144.62	yes	no
Rajasthan	2007	144.73	yes	no

Notes : All minimum wages is for 8 hours day rate in INR

The domestic minimum wages are for all household tasks including child care

The figures are updated as of December 2012

Table 2: Summary Statistics: Full Sample (pre and post Treatment Periods)

	Pre N	Post N	Pre Mean(s.d)	Post mean(s.d)	Post-Pre(s.e)	p-value
Dependent Variables						
Real Wages	2368	2266	302.09 (295.40)	427.59 (485.53)	125.50 (11.75)	0.00***
Log Real Wages	2259	2228	5.49 (.72)	5.78 (.76)	.28 (.020)	0.00***
Months unemployed	2367	1937	.51 (1.9)	.29 (1.12)	-.22 (.04)	0.00***
Covariates						
Sex	2368	2266	.78 (.40)	.66 (.47)	.12 (.012)	0.00***
Age	2368	2266	35.54 (13.62)	37.38 (12.94)	1.84 (.38)	0.00***
Marriage	2368	2266	2.05 (.74)	2.04 (.68)	.007 (.02)	0.72
Lower caste	2364	2266	.68 (.46)	.67 (.46)	.01 (.01)	0.19
Log land owned	1268	1271	2.11 (1.81)	2.18 (1.88)	0.14 (.0701)	0.05*
Illiterate	2368	2266	.52 (.49)	.40 (.49)	-.12 (.014)	0.00***
Just literate	2368	2266	.03 (.18)	.01 (.10)	-.02 (.004)	0.00***
Primary	2368	2266	.25 (.43)	.31 (.46)	.06 (.013)	0.00***
Middle	2368	2266	.15 (.36)	.22 (.41)	0.06 (.01)	0.00***
Higher	2368	2266	.02 (.15)	.04 (.19)	.02 (.005)	0.001***

Standard deviation and standard errors reported in parentheses

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels

Table 3: Summary of Balancing Test Results

Covariates	Mean (treated)	Mean (control)	% bias	% bias reduction	Regression test p-value	T - Test p-value
Sex	.7517	.753	-0.4	86.9	0.255	0.964
Age	37.078	37.095	-0.1	98.4	0.813	0.988
Marriage	2.1327	2.1257	1.0	95.2	0.649	0.908
Lower caste	.77891	.77626	0.6	97.9	0.697	0.939
Log-Land Owned	2.3442	2.2627	4.1	73.2	0.119	0.632
Illiterate	.63265	.61634	3.3	92.0	0.396	0.683
Just Literate	.02721	.02801	-0.5	50.1	0.472	0.953
Primary	.15986	.17116	-2.7	93.1	0.895	0.713
Middle	.15306	.15746	-1.2	88.5	0.473	0.883
Higher	.02721	.02703	0.1	81.3	0.780	0.989

Standard deviation and standard errors reported in parentheses

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels

Table 4: Short Run : Impact on Real Wages

Outcome Variable :	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
Estimation :	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-0.03 (0.05)	-0.05 (0.05)	-0.03 (0.04)	-0.03 (.04)
Treatment states	-0.06 (0.05)	-0.07 (0.05)	-0.07 (0.04)	-0.07 (.04)
Post*treatment states	0.29*** (0.08)	0.27*** (0.08)	0.30*** (0.07)	0.29*** (.07)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1862	1862	1861	1861
$R^2$	0.24	0.28	0.28	0.31

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 5: Short Run: Estimates of the DiD effect for different matching algorithms

Matching Technique	Treatment Effect on Log-Real Wages
Kernel Density	0.32 *** (0.11)
Nearest Neighbor (with replacement)	0.43*** (0.14)
Nearest Neighbor (without replacement)	0.48*** (0.11)
Multiple Nearest Neighbor matching with replacement	0.45*** (0.12)
Multiple Nearest Neighbor matching (caliper .01)	0.42*** (0.11)
Nearest Neighbor (caliper .01, no replacement)	0.43*** (0.12)
Nearest Neighbor (caliper .01, with replacement)	0.38** (0.15)

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 6: Short Run : Impact on Unemployment

Outcome Variable :	Unemployment	Unemployment	Unemployment	Unemployment
Estimation :	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-0.30*** (0.09)	-0.29*** (0.10)	-0.34*** (0.08)	-0.35*** (.09)
Treatment states	-0.11 (0.20)	-0.06 (0.21)	-0.11 (0.16)	-0.14 (.16)
Post*treatment states	-0.06 (0.21)	-0.06 (0.23)	-0.01 (0.18)	-0.01 (.20)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1929	1929	1929	1929
R2	0.01	0.02	0.01	0.04

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 7: Short Run: Estimates of the DiD effect for different matching techniques

Matching Technique	Treatment effect on Unemployment
Kernel Density	-0.42 (0.41)
Nearest Neighbor (with replacement)	-0.66 (0.52)
Nearest Neighbor (without replacement)	-0.44 (0.30)
Multiple Nearest Neighbor matching with replacement	-0.39 (0.42)
Multiple Nearest Neighbor matching (caliper .01)	-0.43 (0.47)
Nearest Neighbor (caliper .01, no replacement)	-0.59 (0.35)
Nearest Neighbor (caliper .01, with replacement)	-0.69 (0.55)

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 8: Long Run: DiD and E-balance estimations

Outcome Variable :	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
Estimation :	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	0.27*** (0.04)	0.25*** (0.04)	0.27*** (0.04)	0.26*** (.04)
Treatment states	-0.05 (0.05)	-0.07 (0.04)	-0.06 (0.04)	-0.06 (.04)
Post*treatment states	0.07 (0.08)	0.10 (0.07)	0.09 (0.08)	0.10 (.08)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1779	1779	1779	1779
R2	0.27	0.31	0.29	0.33

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 9: Long Run: Estimates of the DiD effect for different matching techniques

Matching Technique	Treatment effect on real wages
Kernel Density	0.15 (0.11)
Nearest Neighbor (with replacement)	0.14 (0.16)
Nearest Neighbor (without replacement)	0.20 (0.16)
Multiple Nearest Neighbor matching with replacement	0.16 (0.15)
Multiple Nearest Neighbor matching (caliper .01)	0.26 (0.16)
Nearest Neighbor (caliper .01, no replacement)	0.20 (0.16)
Nearest Neighbor (caliper .01, with replacement)	0.14 (0.16)

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 10: Long Run: DiD and E-balance estimations

Outcome Variable :	Unemployment	Unemployment	Unemployment	Unemployment
Estimation :	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-0.16 (0.10)	-0.16 (0.10)	-0.22* (0.11)	-0.32** (.12)
Treatment states	-0.09 (0.20)	-0.10 (0.21)	-0.12 (0.16)	-0.14 (.17)
Post*treatment states	-0.17 (0.16)	-0.19 (0.17)	-0.14 (0.21)	-0.06 (.21)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1676	1676	1675	1675
R2	0.01	0.02	0.01	0.03

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).



Table 11: Long Run: Estimates of the DiD effect for different matching techniques

Matching Technique	Treatment effect on unemployment
Kernel Density	- 0.35 (0.29)
Nearest Neighbor (with replacement)	-0.05 (0.35)
Nearest Neighbor (without replacement)	-0.05 (0.35)
Multiple Nearest Neighbor matching with replacement	-0.05 (0.35)
Multiple Nearest Neighbor matching (caliper .01)	-0.05 (0.35)
Nearest Neighbor (caliper .01, no replacement)	-0.05 (0.35)
Nearest Neighbor (caliper .01, with replacement)	-0.05 (0.35)

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 12: Short Run : Intensity of Treatment

Outcome Variable :	Log Real Wages DiD	Log Real Wages DiD	Log Real Wages E-balance & DiD	Log Real Wages E-balance & DiD
Post	-0.02 (0.05)	-0.05 (0.05)	-0.01 (0.04)	-0.00 (0.04)
Wage Gap	-0.05* (0.04)	-0.05** (0.04)	-0.05** (0.04)	-0.05** (0.03)
Post*Wage Gap	0.21** (0.08)	0.20** (0.08)	0.20*** (0.06)	0.20*** (0.06)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1862	1862	1862	1862
R2	0.24	0.28	0.27	0.31

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 13: Long Run : Intensity of Treatment

<b>Outcome Variable :</b>	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	.27*** (.04)	.25*** (0.04)	.27*** (.04)	.26*** (0.04)
Wage Gap	-0.04 (0.04)	-.05 (0.03)	-.04 (0.03)	-0.05 (0.03)
Post*Wage Gap	0.04 (0.06)	0.07 (0.05)	0.05 (0.06)	.07 (0.06)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1798	1798	1798	1798
R2	0.27	0.31	0.29	0.33

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table 14: Short Run : Treatment Impact at the Extensive Margin

<b>Outcome Variable : Probability of Employment as Domestic Worker</b>				
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-.03*** (.007)	-.02*** (.007)	-.03*** (.006)	-.03*** (.006)
Treatment states	-.02 (.01)	-.03* (.01)	-.02** (.009)	-.02** (.009)
Post*treatment states	-.01 (.01)	-.01 (.01)	-.01 (.01)	-.01 (.01)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	12269	12269	12269	12269
R2	0.30	0.33	0.22	0.24

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Figure 2: Kernel Density Plot of Log Real Wage Distribution in Treatment and Control States

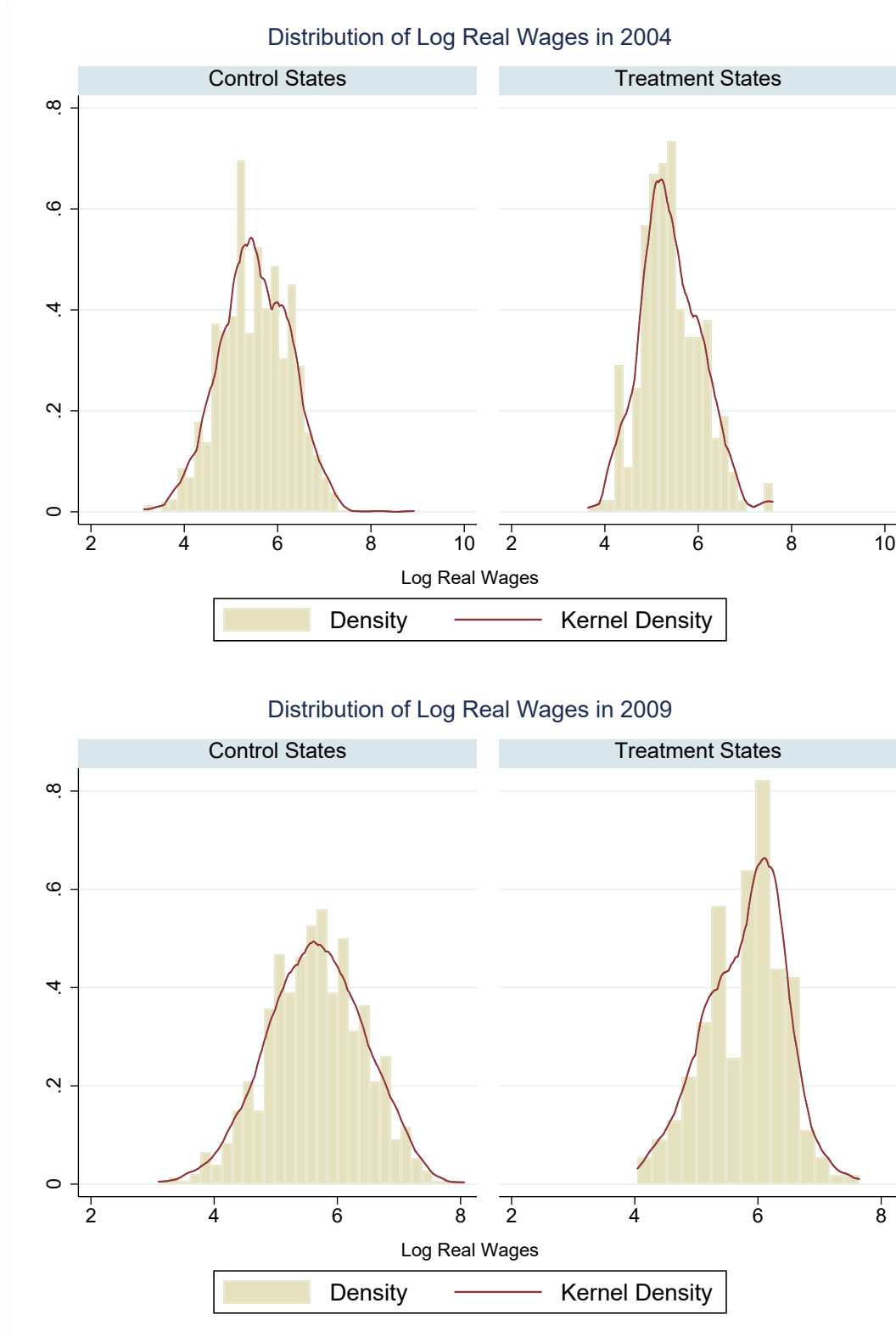


Table 15: Long Run : Treatment Impact at the Extensive Margin

<b>Outcome Variable : Probability of Employment as Domestic Worker</b>				
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	.28*** (.02)	.27*** (.02)	.30*** (.01)	.29*** (.01)
Treatment states	-.03* (0.01)	-.03 (.01)	-.05*** (.01)	-.05*** (.01)
Post*treatment states	.01 (.06)	.02 (.07)	.006 (.02)	.01 (.02)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	7446	7446	7445	7446
R2	0.42	0.45	0.50	0.53

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Figure 3: Distance analysis for Real Wages

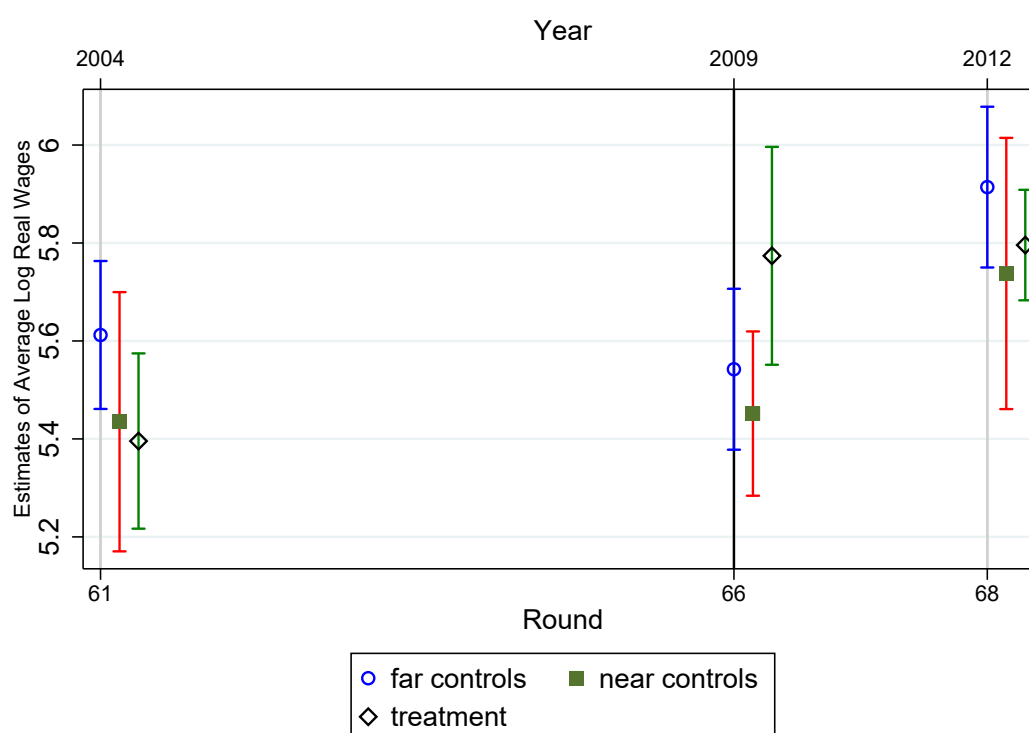


Figure 4: Distance analysis for Months Unemployed

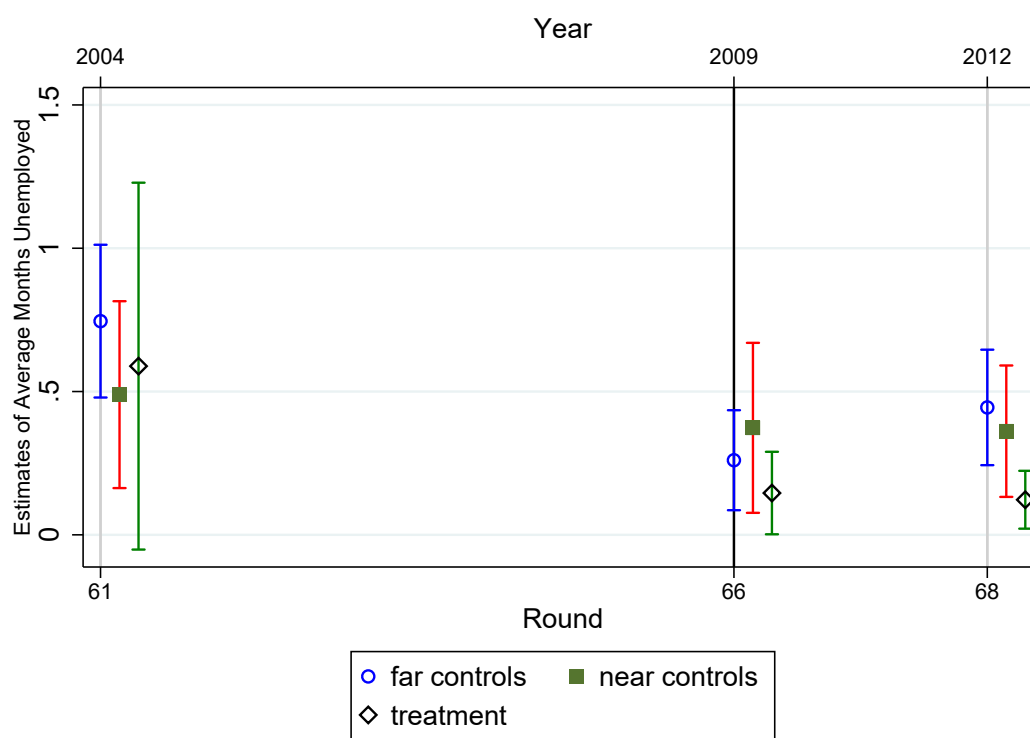


Figure 5: Log Real Wages in Domestic vs Placebo Sector

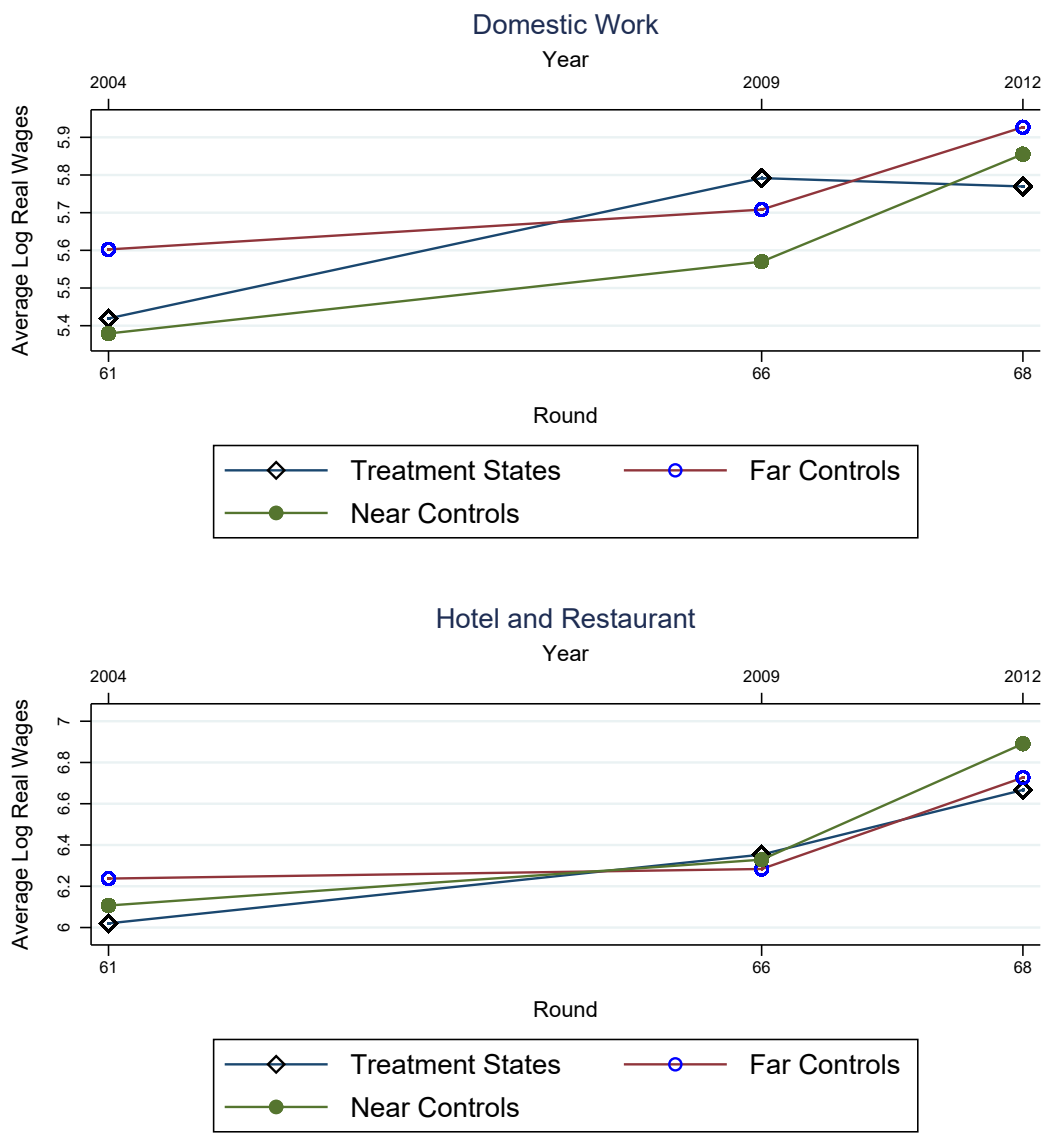
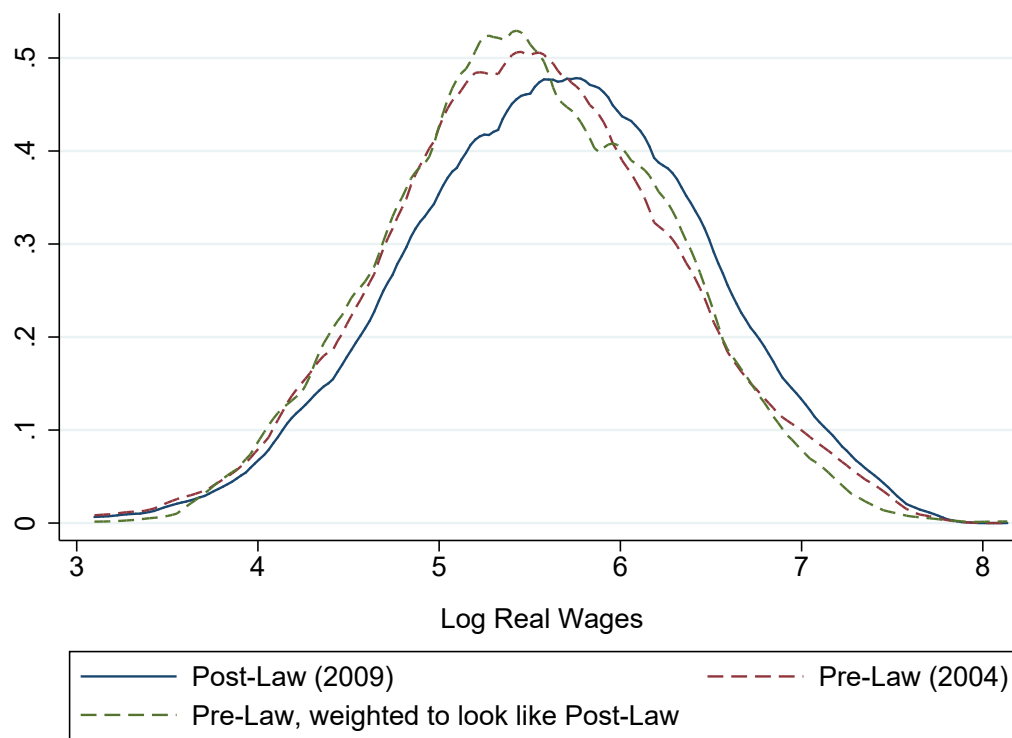


Figure 6: Re-weighted Wage Distributions



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## Online Appendix

Table A-1: Comparing Domestic Minimum wages to Other Informal Sectors between Treatment States (in Indian Rupees)

Minimum Wages			
States	Domestic sector (DMW)	Hotel sector (HMW)	Ratio of DMW to HMW
Karnataka	165.58	196.70	0.84
Andhra Pradesh	173.85	229.83	0.75
Bihar	144.62	157	0.92
Rajasthan	144.73	147	0.98

Notes : All minimum wages is for 8 hrs daily rate

The domestic minimum wages are for all household tasks including childcare

The figures are updated as of December 2012

The minimum wage for Sweeping and Cleaning Workers in Karnataka is an inter-state average

Table A-2: Summary Statistics: Comparing Sample Means for Treatment States (pre and post treatment period)

	Pre N	Post N	Pre Mean(s.d)	Post mean(s.d)	Post-Pre(s.e)	p-value
Dependent Variables						
Real Wages	488	467	277.04 (240.67)	402.99 (284.54)	125.95 (17.02)	0.00***
Log RealWages	476	465	5.41 (.65)	5.78 (.68)	0.37 (.04)	0.00***
Monthsunemployed	488	410	.30 (1.57)	.12 (.62)	-.18 (.082)	0.02**
Covariates						
Sex	488	467	.20 (.40)	.32 (.47)	.12 (.028)	0.00***
Age	488	467	34.78 (13.11)	37.70 (12.42)	2.92 (.82)	0.00***
Marriage	488	467	.60 (.48)	.65 (.475)	.05 (.03)	0.10
Lower caste	488	467	.75 (.37)	.85 (.416)	.09 (.02)	0.00**
LogLand Owned	200	216	2.24 (2.10)	2.26 ( 2.015)	.02 (.201)	0.93
Illiterate	488	467	.68 (.46)	.54 (.49)	-.14 (.031)	0.00***
Just Literate	488	467	.02 (.14)	.01 (.103)	-.01 (.008)	0.22
Primary	488	467	.157 (.36)	.21 (.409)	.054 (.025)	0.030**
Middle	488	467	.12 (.33)	.20 (.407)	.08 (.024)	0.00***
Higher	488	467	.004 (.06)	.02 (.15)	.016 (.007)	0.00***
Standard deviation and standard errors reported in parentheses						
* , **, *** indicate significance at 10, 5 and 1 % significance levels						

Table A-3: Short Run : Impact on Real Wages in Placebo Sector

<b>Outcome Variable :</b>	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-0.01 (0.04)	0.05 (0.04)	-0.04 (0.06)	.008 (.06)
Treatment states	0.04 (0.06)	0.13 (0.05)	.02 (0.07)	0.1* (.06)
Post*treatment states	0.09 (0.2)	-0.03 (0.18)	0.1 (0.12)	0.2 (.1)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	824	824	825	824
$R^2$	0.31	0.41	0.3	0.4

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-4: Long Run : Impact on Real Wages in Placebo Sector

<b>Outcome Variable :</b>	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	.3** (0.14)	.3** (0.15)	.5*** (0.18)	.5*** (.15)
Treatment states	0.04 (0.06)	0.2** (0.04)	.07 (0.1)	0.1* (.06)
Post*treatment states	0.07 (0.15)	-.02 (0.17)	-.09 (0.21)	-.18 (.19)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	526	526	526	526
$R^2$	0.4	0.5	0.4	0.5

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-5: Short Run : Treatment Impact at the Extensive Margin in Placebo Sector

<b>Outcome Variable : Probability of Employment in Placebo Sector</b>				
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	.003 (.004)	.003 (.003)	.002 (.002)	.002 (.002)
Treatment states	.007 (.006)	.007 (.006)	.007* (.004)	.007* (.004)
Post*treatment states	-.004 (.007)	-.004 (.007)	-.003 (.005)	-.003 (.005)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	41857	41857	41857	41857
R <sup>2</sup>	0.002	0.003	0.002	0.01

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-6: Long Run : Treatment Impact at the Extensive Margin in Placebo Sector

<b>Outcome Variable : Probability of Employment in Placebo Sector</b>				
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-.04*** (.005)	-.04*** (.005)	-.04*** (.002)	-.04*** (.002)
Treatment states	.007 (0.006)	.007 (.006)	.007* (.004)	.007 (.004)
Post*treatment states	-.006 (.006)	-.006 (.006)	-.005 (.004)	-.006 (.004)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	40366	40366	40366	40366
R <sup>2</sup>	0.02	0.02	0.03	0.03

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-7: Test for Parallel Trends : 1999 and 2004 Survey Rounds

<b>Outcome Variable :</b>	Log Real Wages	Unemployment	Log Real Wages	Unemployment
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-.08 (0.08)	.002 (.16)	.001 (0.04)	.04 (.11)
Treatment states	-.15* (0.07)	-.3* (.15)	-.1 (0.06)	-.29*** (.11)
Post*treatment states	.09 (0.08)	.18 (.28)	.01 (0.08)	0.2 (.2)
Covariates	Yes	Yes	Yes	Yes
N	2129	1907	2129	1907
R <sup>2</sup>	0.28	0.007	0.29	0.01

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-8: DiD and E-balance estimations : 2009-2012

<b>Outcome Variable :</b>	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	0.31*** (0.02)	0.32*** (0.02)	0.34*** (0.05)	0.35*** (.04)
Treatment states	.20** (0.09)	0.19** (0.08)	0.22** (0.06)	0.22** (.06)
Post*treatment states	-.19** (0.09)	-0.18 (0.08)	-.22** (0.09)	-0.22* (.09)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1225	1225	1224	1225
R <sup>2</sup>	0.28	0.33	0.32	0.36

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).



Table A-9: Short Run : Impact on Real Wages while Restricting the Control group to Border States

<b>Outcome Variable :</b>	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	-.02 (.06)	-.04 (.06)	-.02 (.05)	-.02 (.05)
Treatment states	-.05 (.06)	-.05 (.05)	-.13** (.06)	-.06 (.05)
Post*treatment states	.27*** (.08)	.26*** (.9)	.33*** (.09)	.29*** (.08)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1627	1627	1627	1627
R <sup>2</sup>	0.24	0.28	0.32	0.33

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-10: Long Run : Impact on Real Wages while Restricting the Control group to Border States

<b>Outcome Variable :</b>	Log Real Wages	Log Real Wages	Log Real Wages	Log Real Wages
<b>Estimation :</b>	DiD	DiD	E-balance & DiD	E-balance & DiD
Post	.28 *** (.05)	.27*** (.05)	.27*** (.05)	.27*** (.05)
Treatment states	-.04 (.06)	-.05 (.05)	-.08 (.06)	-.06 (.05)
Post*treatment states	.06 (.09)	.09 (.07)	.1 (.08)	.1 (.09)
Covariates	Yes	Yes	Yes	Yes
Interactions	No	Yes	No	Yes
N	1572	1572	1572	1572
R <sup>2</sup>	0.27	0.31	0.33	0.35

DiD indicates the difference-in-difference estimate of the outcome variable.

Standard errors clustered at State level in parentheses. Number of clusters is 33.

\*, \*\*, \*\*\* indicate significance at 10, 5 and 1 % significance levels.

Treatment states refer to Andhra Pradesh (AP), Bihar, Karnataka and Rajasthan.

Non-treated states refer to remaining states of India, excluding Kerala and Jharkhand.

Covariates refer to age, sex, marital status, education level (illiterate, just literate, primary, secondary, middle or higher), land owned by household and caste category (high or low).

Table A-11: Comparing Sample Means between Treated and Untreated States from 2009 to 2012

	Treated	Untreated	T- Mean(s.d)	Un- Mean(s.d)	Diff (s.e)	p-value
Dependent Variables						
Real Wages (2009)	228	914	397 ( 258.08)	377 (325.71)	20 (23.2)	0.38
Real Wages (2012)	239	885	409 ( 308.1)	493 ( 666.7)	-84 (44.36)	0.058*

Standard deviation and standard errors reported in parentheses  
 \*, \*\*, \*\*\* indicate significance at 10, 5 and 1% significance levels  
*T- Mean* refers to the mean in the treated states.  
*Un-Mean* refers to mean in the untreated states  
*Real wages 09* refers to real wages in 2009  
*Real wages 12* refers to real wages in 2012

Figure A-7: Distance analysis for Real Wages

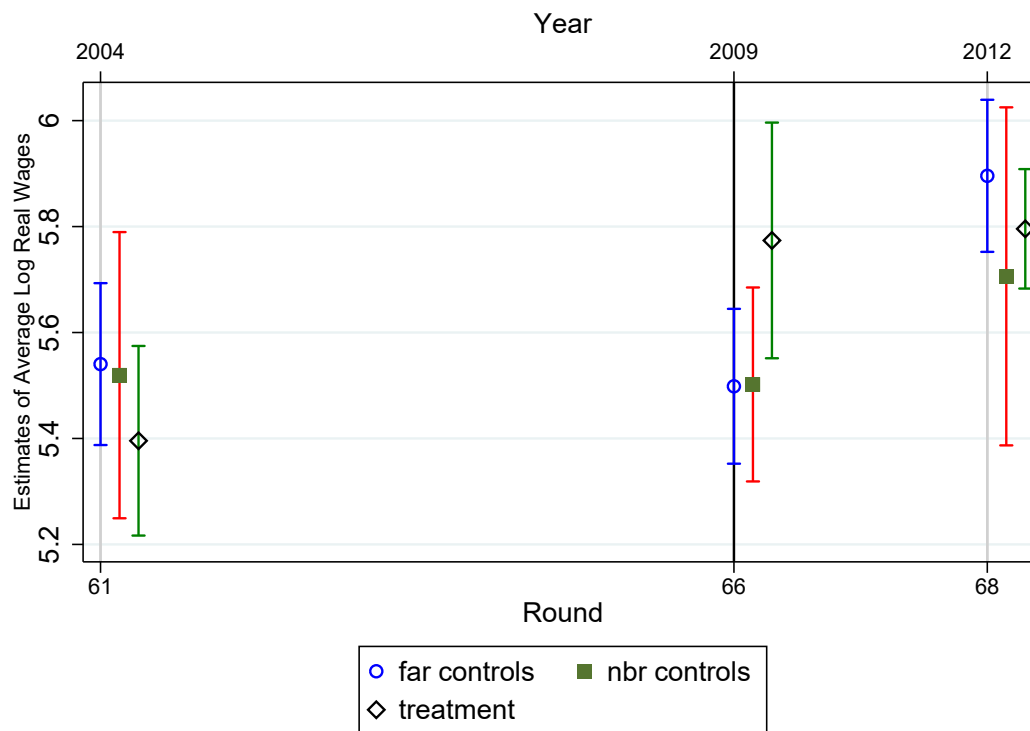


Figure A-8: Distance analysis for Months Unemployed

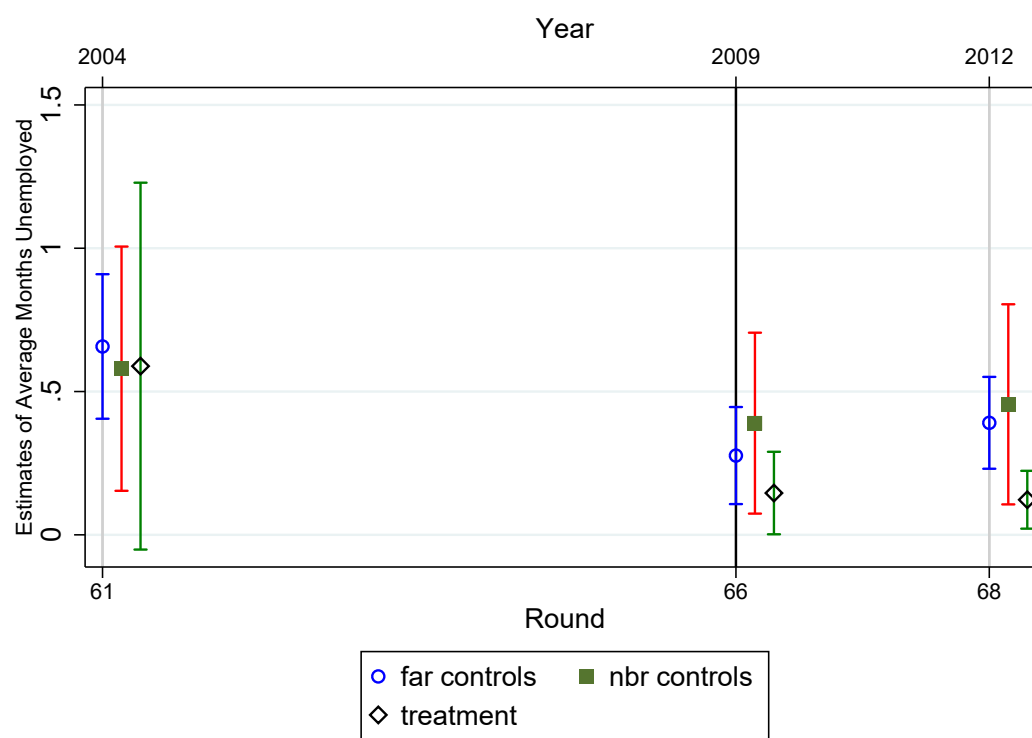


Figure A-9: Log Real Wages in Domestic vs Placebo sector

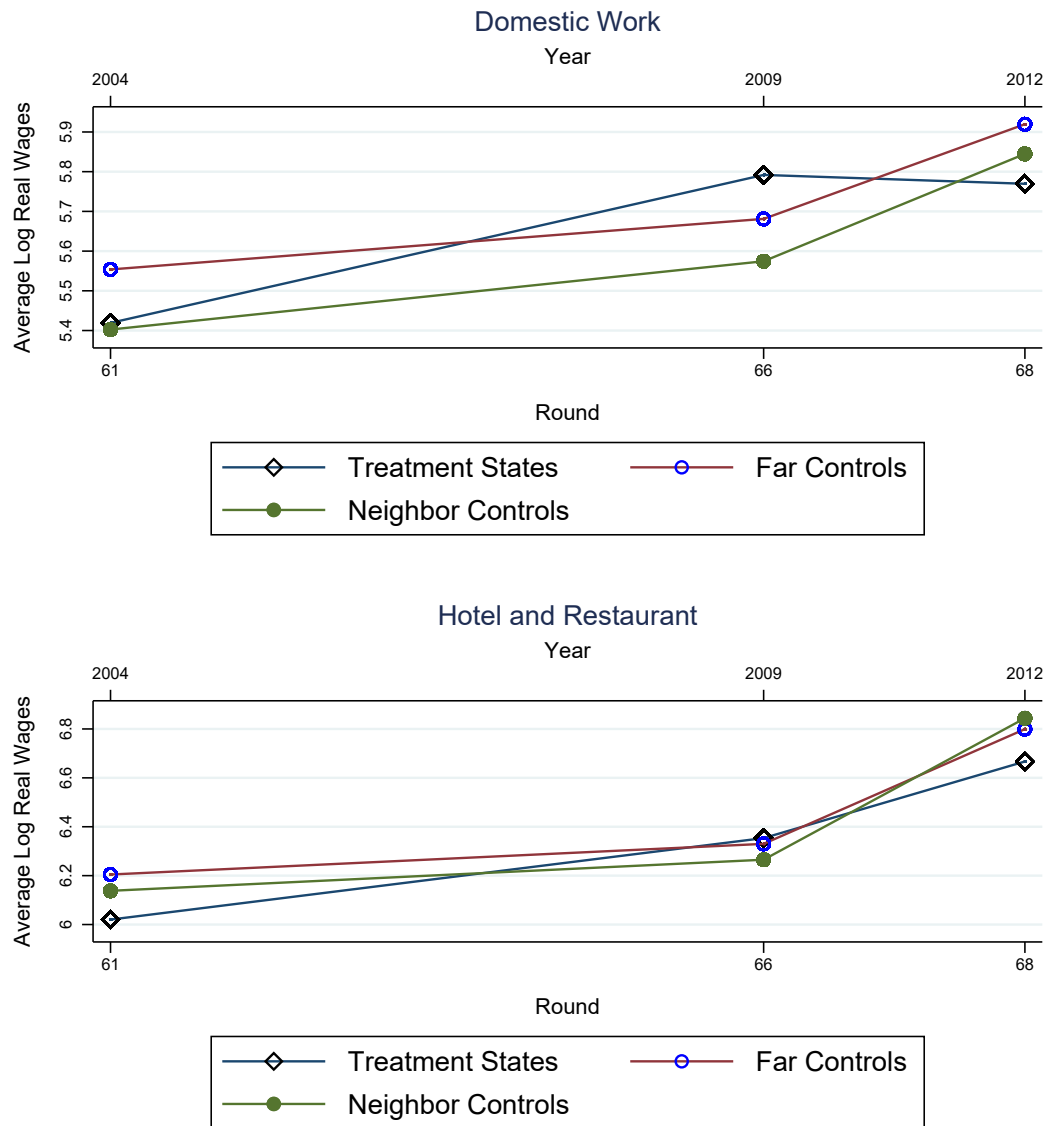


Figure A-10:  
Near Controls vs Neighbor Controls Classification

