PRELIMINARY—PLEASE DO NOT QUOTE

The Consequences of Increased Enforcement of Legal Minimum Wages in a Developing Country: An evaluation of the impact of the *Campaña Nacional de Salarios Mínimos* in Costa Rica

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June, 2012

I. Introduction

Non-compliance with labor protection legislation such as the legal minimum wage is wide-spread in many developing economies. Almost all recent studies of legal minimum wages in developing countries have found a high degree of non-compliance (for example, see Lemos, 2004; Harrison and Scorse, 2004; Strobl and Walsh, 2001; and Cunningham, 2007). Costa Rica is no exception; our previous studies have found that over 30% of workers legally covered by minimum wage legislation in Costa Rica actually earn less than the legal minimum (Gindling and Trejos, 2010; Gindling and Terrell, 1995, 2005 and 2007). Whether or not minimum wages are complied with will affect whether minimum wages have the impacts proponents expect. For example, some researchers have argued that non-compliance with minimum wage legislation reduces the impact that minimum wages might have on inequality and poverty (i.e. Cunningham 2007).

Despite the prevalence and importance of non-compliance with minimum wage legislation, there have been relatively few empirical studies of the impact of non-compliance on labor market outcomes in developing economies (exceptions include the studies reviewed in the next section). In this paper, we contribute to this sparse literature by evaluating the impact of a comprehensive program designed by the Costa Rican Ministry of Labor to increase compliance with minimum wage legislation. The National Campaign for Minimum Wages, begun in August 2010, (1) enacted a well-funded publicity campaign to "create a level of consciousness among employers and workers regarding the importance of complying with the minimum wage", (2) encouraged workers to denounce employers who pay less than the minimum wage (to support this the Ministry expanded a call center with a call-in complaint line: 1-800-TRABAJO), and (3) increased labor inspections targeting minimum wage violations. This is the first time that we know of that a Latin American government has implemented such a comprehensive plan to reduce non-compliance with minimum wages. As such, it provides a unique opportunity to study the impact of increased enforcement of minimum wages on compliance, wages, employment, whether employers pay into the Social Security system, poverty and gender equity.

Previous published papers have identified the impact of increased enforcement of minimum wages by comparing regional differences in labor inspections to regional differences in compliance, informality, and other labor market outcomes. The authors of these studies point out, in the words of Ronconi (2010), "There are two main challenges to estimating the causal effect of enforcement on compliance. First, adequate measures for both variables are not easily available...A second challenge is that a potential simultaneous relation between enforcement and compliance complicates identification. On the one hand, firms' propensities to comply with regulations depend on the probability of being penalized, and, on the other hand, public enforcement agencies' resources are likely to be affected by the extent of compliance." (pp. 719-720). These two problems are addressed in our study. First, the National Campaign for Minimum Wages provides a clear and precise break where enforcement increased substantially; we can use this break as a natural experiment to measure the causal impact of increased enforcement. Second, as we show later in this paper, the National Campaign for Minimum Wages was not an endogenous response to increased violations of minimum wage laws, but rather was a policy change that occurred because a new president and administration recognized publicly for the first time that non-compliance with minimum wage legislation is a problem in Costa Rica.

II. Literature Review

Basu, Chau and Kanbur (2010) develop a theoretical model where the labor market can be characterized by imperfect competition, imperfect enforcement of minimum wage laws, and imperfect commitment on the part of government inspectors. Within this framework the impact of legal minimum wages is complex. For example, Basu, Chau and Kanbur (2010) show that, depending on the degree of compliance and the structure of the labor market, higher minimum wages can result in increases, decreases or no significant changes in wages, employment or informality. They also show that the impact of increased enforcement on wages, employment and informality is ambiguous—all might increase, decrease or stay the same depending on labor market structure and labor supply and labor demand elasticities.

Aghion, Algan and Cahuc (2010) develop a theoretical model where rigidly enforced legal minimum wages can reduce the trust between workers and employers, and therefore reduce scope for cooperation between workers and firms, discouraging social investments in labor relations and unionization. Within this framework, it is possible that increased commitment to enforce minimum wages can lead to both reduced wages and reduced employment.

This recent theoretical research suggests that, under different sets of reasonable assumptions, increased enforcement of legal minimum wages has ambiguous effects on labor market outcomes. Whether increased enforcement of minimum wages has a positive or negative impact

on wages, employment, informality and poverty is, therefore, an empirical question. Next we review the recent empirical literature on the impact of increased enforcement of labor market protection legislation in developing economies.

Ronconi (2008) evaluates the impact of increased labor inspections in Argentina on minimum wages, maximum hours, paid vacation time, annual extra monthly wage and two components of social security: workers compensation insurance and health insurance. Ronconi constructs a province-level panel data set with annual observations for the period 1995-2003. The measure of enforcement used is the annual number of labor inspections per capita by province. Regressions are estimated where the dependent variables are measures of the level of compliance with the above labor regulations in each province and year, and the independent variables include the number of labor inspections per capita, labor market and population characteristics, and province-level fixed effects. To address the possible simultaneous relationship between non-compliance and increased enforcement, election cycles (years) are used as an instrumental variable for the number of labor inspections per capita. Ronconi (2008) finds that increased enforcement caused increased compliance with minimum wage legislation and maximum legal working hours. On the other hand, increased enforcement did not have a statistically significant impact on compliance with social security regulations.

Almeida and Carneiro (2011) evaluate the impact of differential enforcement of labor regulations across cities in Brazil on the proportion of workers who are in the formal sector, informal sector, self-employed and non-employed, and on wages in the formal and informal sectors. The measure of enforcement used is the interaction between the distance of the city from the nearest enforcement office and the number of inspectors in that office. An increase in the value of this interaction variable indicates an increase in enforcement. Almeida and Carneiro show that this interaction variable is correlated with an increased number of inspections, but argue that it is less likely to suffer from an endogenous relationship with non-compliance. To further control for the possibility of a simultaneous relationship between enforcement and non-compliance, Almeida and Carneiro include, as a control variable, the outcome variable in an earlier period when "enforcement was a less important activity" (p.3). They conclude that in cities with more enforcement of labor regulations there is more formal employment, more non-employment and less self-employment. They also find evidence that increased enforcement causes an increase in wages at the bottom and a decrease in wages at the top of the formal wage distribution.

Almeida and Susanli (2011) use a large firm level data set across 63 countries to examine the impact of firing regulations and enforcement on firm size. They regress firm size on an interaction between measures of *de jure* firing costs and enforcement of labor laws. Other firm level characteristics are used as control variables. The address the issue of simultaneous causality between enforcement and non-compliance they include sector and country fixed effects. They find evidence that more stringently enforced firing regulations reduce average

employment in firms.[1] Almeida and Carneiro (2009) come to a similar conclusion using firm-level data on firm characteristics and city-level enforcement of labor regulations in Brazil. To address the simultaneity between non-compliance and inspections, Almeida and Carneiro (2009) use distance from the firm to the nearest inspection office as an instrument for the probability of inspection (number of inspections per 100 firms). They conclude that stricter enforcement of labor regulations reduce firm size.

III. The *Campaña Nacional de Salarios Mínimos*

On August 9, 2010 the *Campaña Nacional de Salarios Mínimos* (National Campaign for Minimum Wages) was announced jointly by Costa Rican President Laura Chinchilla and Minister of Labor Sandra Piszk with much fanfare and press attention.[2] The explicit purpose of the Campaign is to improve compliance with minimum wage legislation. There were three broad mechanisms presented to achieve this goal: (1) publicity to "create a level of consciousness among employers and workers regarding the importance of complying with the minimum wage"; (2) encourage workers to denounce employers who pay less than the minimum wage (to support this the Ministry expanded a call center with a call-in complaint line: 1-800-TRABAJO); and (3) more labor inspections targeting minimum wage violations. Coincident with these three mechanisms, the Ministry of Labor implemented a new computer-based information system to keep track of violations of labor laws (the *Sistema Electronico de Casos*).

The Campaign was partly a response to published research that showed high levels of non-compliance with minimum wages in Costa Rica and a public campaign to confront this issue by the director of the *Estado de la Nación* program, Miquel Guitierrez (Arias, 2011).[3] At the same time, non-compliance with labor regulations also gained visibility because of the CAFTA-DR trade negotiations. Representatives of U.S. labor interests have long argued that non-compliance with labor regulations was widespread in Central America, and this was an issue in the CAFTA-DR negotiations. In response to pressure from U.S. labor interests, the U.S. Labor Department funded and financed a program titled *Cumple y Gana*, administered by the non-governmental FUNPADEM (*Fundacion para la Paz y Democracia*). The *Cumple y Gana* program was designed to strengthen the capabilities of Central American governments to carry out effective labor inspections. While the *Cumple y Gana* program did not focus exclusively on minimum

[1] Almeida and Aterido (2011) use the same firm-level data set as Almeida and Susanli (2011), and a similar empirical technique, to examine the relationship between enforcement of labor regulations and on-the-job training. They find evidence that more stringently enforced labor regulations increase the probability of on-the-job training.

[2] One of the authors, Juan Diego Trejos, has been appointed a special advisor to the Ministry of Labor charged with assisting the Directorate of Inspection in implementing and evaluating the Campaign.

[3] This includes research by the authors of this paper (Gindling andTerrell, 1995, 2005 and 2007; Gindling and Trejos, 2010), as well as studies by the *Estado de la Nación* (2009) and the Costa Rican Ministry of Labor.

wages, improving inspections for minimum wage compliance was an important component and goal. Finally, a new Costa Rican President and Minister of Labor took office in May 2010. President Laura Chinchilla came to power, in part, on a law-and-order platform. Increased enforcement across the board of existing laws fit into this platform, and increased enforcement of minimum wage legislation played well with the working class and labor segment of her *Liberación Nacional* party. One interesting aspect of this background is that the campaign to increase compliance with minimum wages came into existence because of research results and politics, and not because of any clear increase in non-compliance in Costa Rica. In terms of our empirical analysis, this helps address the simultaneity/endogeneity problem that occurs in other studies that use regional variation in inspections as the strategy to identify the impact of increased enforcement (i.e. Ronconi, 2008).

The first component of the campaign, the publicity campaign, began with the joint announcement on August 9, 2010 by the Minister of Labor and the President, with both wearing t-shirts listing the 1-800-TRABAJO telephone number of the call center. The announcement of the initiation of the campaign appeared widely in both the national and international press. The publicity campaign, which continues today, has included announcements in the press, 1800 prime time radio commercials, $1500 spent on television commercials, several web sites, over 130 billboards at bus stops and other public places, posters at work places, over 30,000 pamphlets, and widely distributed t-shirts (see figure 1). In the most public display, both teams in a semifinal match for the national professional soccer league returned to the field at half time sporting the t-shirts (there is a minimum wage for professional soccer players, and prior to the semi-final match several newspapers ran stories about some professional soccer players being paid less than the minimum wage). In addition, once each month during the Campaign, the Minister of Labor has held a press conference where she has highlighted and described the increased inspections, and presented information to show that the campaign is having an effect. The press conferences have always been followed by stories in the major Costa Rican newspapers about the campaign. The press conferences keep the attention of the public on the need to reduce non-compliance and build public support for the campaign.

The campaign also developed a call center and toll-free number to answer questions about labor legislation and to receive complaints (*denuncias*) from workers about firms violating labor regulations. The telephone number, 1-800-TRABAJO, was made effective in August of 2010. Before the campaign, to register a complaint (*denuncias*), workers had to go in person to a regional office of the Ministry of Labor. Now, a complaint can be registered anonymously by phone, and each complaint results in an inspection by the Ministry of Labor. Between August 1, 2010 and June 30, 2011 there were 77,816 calls, resulting in 988 complaints; 768 (78%) complaints were about minimum wages. In addition to complaints, the information aspect of the call center is important—workers can call to ask what their minimum wage should be, given

their job, something that is not easy because of the complex structure of minimum wages in Costa Rica (see Appendix 2).

The campaign also included an increase in inspections targeted towards minimum wage violations. Before discussing the way in which inspections were increased, it is necessary to describe the process of labor inspections. Labor inspections begin with an initial visit by an inspector. Initial visit by an inspector can be because of a complaint (*denuncia*) or at the discretion of the inspector (inspectors are responsible for a geographic region). Inspections can be a full inspection that checks for any labor code violation, or an inspection focused on one or a few violations. Inspectors interview workers and view payroll records. In small firms inspectors interview all workers, in large firms inspectors interview a sample of workers. Full inspections look for violations of any part of the labor code, including: minimum wages, overtime pay, correct payroll records (*comprobante de pago*), Social Security payments, Work Risk insurance payments, emergency exits and other parts of the safety code, mandated maternity leave, holidays, work week violations, *aguinaldo* (13^{th} month pay), etc. If a violation is found, a second visit is carried out within 30 days from when the violation is recorded. If the firm is no longer violating the labor regulation, then nothing further happens (no fines or other sanctions). [4] If firms are still violating the labor regulation, then the labor inspector refers the case to the Labor Tribunals. Labor inspectors cannot impose fines or sanctions, only the Labor Tribunals can do this.

Beginning August 2010 and continuing throughout 2011, the Ministry of Labor increased the number of inspections focused on minimum wage violations. Inspectors in these focused inspections also checked overtime pay and payroll records. Targeted (focused) inspections do not explicitly looking for any other labor code violations. The increase in the number of focused inspections was accompanied by a limited increase in number of inspectors and resources available to the Direction of Inspection: the budget of Direction of Inspection increased 27% in real terms, the number of inspectors went from 90 to 101, the fleet of cars available to inspectors went from 11 to 22, and inspectors were given 64 new laptop computers.

The increase in resources for new inspections was limited. In our interviews with inspectors and supervisors at the Direction of Inspections, we heard many complaints from inspectors about the lack of resources—for example, there are no maps showing the locations of firms, so that inspectors have to rely on memory in order to locate firms. In addition, resources were shifted away from full inspections in favor of a focus on targeted inspection of minimum wage

[4] Inspections for violations of Social Security are carried out separately. Social Security inspectors have more resources and can impose sanctions (including fines; up to closing down a firm). If a Social Security inspector finds a violation of any other part of the labor code they are not required to inform the Ministry of Labor. On the other hand, if a Ministry of Labor inspector finds a violation of Social Security legislation, they are required to inform the Social Security inspectors.

violations. Inspectors told us that the campaign for minimum wages resulted in fewer violations of other labor regulation violations being found because of fewer full inspections. Further, inspectors were under pressure to increase the number of firms inspected for minimum wage violations, so that they spent less time per inspection (which they could as long as they focused only on minimum wages) and tended to focus on regions, sectors and industries where there were many firms within a small area. According to what inspectors told us, effectively this implied fewer inspections of agricultural and rural firms; and more inspections of small firms (which took less time), especially in commerce in urban areas (where many firms exist close together) and near the inspection offices (so that transportation time and costs were minimized).[5]

Table 1 shows that the total number of inspections increased in 2010 and 2011, but that the number of full inspections fell as the number of inspections focused on minimum wages increased. The increase in focused inspections resulted in an increase in the number of firms found to be in violation of minimum wage laws. On the other hand, as the number of full inspections fell, so did the number of firms found to be violating other (non-minimum wage) labor law violations. Focused inspections were concentrated in commerce (5654 inspections), with a smaller yet substantial number in services (1048 inspections) and manufacturing (945 inspections). On the other hand, there were few inspections in agriculture (400), transportation (97) or construction (107). This is consistent with what we heard from inspectors. Table 2 illustrates the point that non-compliance with minimum wages is substantial in all firms, and especially in small firms. Minimum wage violations were most common in agriculture (44.5% of firms inspected were found in violation), commerce (43% of firms inspected were found in violation) and manufacturing (40%). Table 3 illustrates that a high percent of firms found in violation in the first inspection were found to have complied with minimum wages by the second inspection. This may indicate that inspections were successful. However, there were no further follow-up inspections beyond the second inspection, so it is not clear if firms remained in compliance after the second inspection. Firms found to still be in violation after the second inspection were referred to labor tribunals for possible sanctions. Labor tribunals are very slow, and sanctions generally include only the requirement to pay back wages owed. The low sanctions imply that being discovered to have violated minimum wage legislation through a labor inspection imposes very few monetary costs on firms. It was suggested to us that the biggest cost to firms of being found in violation of minimum wage laws is in terms of their reputations with the public in general.

[5] Several other reforms were proposed but not yet implemented, such as: to allow Ministry of Labor inspectors to levy fines for violations; an accord that requires Social Security inspectors to tell the Ministry of Labor if they find a minimum wage violation; and publishing names of firms that violate minimum wage law. In fact, the Supreme Court ruled that the Ministry of Labor was required to publish the names of firms which had violated labor regulations—but the Ministry has so far not complied, under extreme pressure from the Chamber of Commerce (which is clearly worried about the reputation of its members);

IV. Data

The next step in our analysis is an evaluation of whether the *Campaña Nacional de Salarios Mínimos* had an impact on compliance, wages, employment, formality, mandated non-wage benefits (such as Social Security) and voluntary non-wage benefits (such as pension contributions and payments in kind). The basic methodology we use is regression discontinuity. In the regression discontinuity approach we compare workers who, prior to the Campaign, earned just below the minimum wage with workers who, prior to the Campaign, earned just above the minimum wage. If successful, the Campaign should increase the wages of those who were earning below the minimum wage but will have no impact on the wages of those who were already earning at or above the minimum wage. To conduct this analysis, we therefore need data on the wages and other personal and labor market characteristics of workers before the Campaign (begun in August, 2010) and data on these same workers after the introduction of the campaign. That is, we need panel or longitudinal data, where we observe the same individuals both before and after the campaign.

The data we use in this analysis is a panel data set of individuals constructed from two yearly Costa Rica National Household Surveys, one conducted in July 2010 (just before the campaign), the other conducted in July 2011 (after the campaign had been active for some time). The National Household Surveys are conducted each year by the Costa Rican National Statistics Institute and use a rotating sample design whereby interviewers in 2011 returned to approximately 75% of the households interviewed in 2010. In both 2010 and 2011 interviewers recorded a code identifying the address of each household, as well as recording the name of each household member. Working with the Statistics Institute, we used this information to construct a panel data set of households (who remained at the same address between 2010 and 2011) and individuals (who remained with the same households between 2010 and 2011).

Using the minimum wage decrees of January 2010 and January 2011 we assigned each worker a minimum wage in 2010 and 2011. Costa Rica has a complex minimum wage system where different minimum wages apply to workers in different occupations and skill levels (see Appendix 2). Combining information on occupations and minimum wages from the Ministry of Labor with data on the occupations, skill level and education level of workers from our surveys, we assign each worker a minimum wage in 2010 and 2011.[6]

Table 4 presents some descriptive statistics of the worker-level panel data set, which is what we use in our analysis. Summary statistics are presented for all workers, and for two sub-samples of the data: workers with earnings within 10% of the minimum wage, and workers within 15% of

[6] An example of a Costa Rican minimum wage decree is presented in Appendix 2. As a robustness check, we will also compare wages to the lowest minimum wage in each year (the Ministry of Labor calls this the *minimum minimorum*). When press reports specify a number for the minimum wage, the *minimum minimorum* is the minimum wage most often reported.

the minimum wage. Within these three subsamples, the data is further divided in to those who earn above and below the minimum wage. The Regression Discontinuity design assumes that workers just above and just below the minimum wage are similar. Note that in table 2, the differences between the characteristics of the sample of workers below and above the minimum wage become smaller as we narrow the sample to a narrower band around the minimum wage.

V. Regression Discontinuity Methodology

The primary methodology we use to evaluate the impact of the Campaign for Minimum Wages is Regression Discontinuity (RD). RD designs are appropriate where treatment is determined by whether an observed "assignment" or "forcing" variable falls below or exceeds a known cutoff point. In our case, whether a worker receives the "treatment" (is affected by the Campaign for Minimum Wages) depends on whether the worker is earning less than the minimum wage prior to the introduction of the Campaign (in 2010). Workers who initially earned above the minimum wage are not affected by the Campaign and therefore do not receive the treatment. RD designs compare outcomes for those workers who have values of the "assignment" variable just below the cutoff with those workers who are just above the cutoff. In our case, the assignment variable is the difference between the wage received by the worker in 2010 and the minimum wage that applies to that worker in 2010 (the horizontal axis in figure 2). The cutoff point is where this variable is equal to zero; that is, where the wage is equal to the minimum wage. If the assignment variable is below the cutoff, this indicates that that worker is earning below the minimum wage, and should be affected by the Campaign (that is, that worker is part of the treatment group). If the assignment variable is above the cutoff, this indicates that the worker will not be affected by the Campaign, and is therefore part of the untreated or control group.

In our study we compare labor market outcomes for workers who, before the Campaign, were earning just below the minimum wage to the outcomes for those earning just above the minimum wage. The RD design assumes that workers earning just above the minimum wage are similar to those earning just below the minimum wage in all observable and unobservable ways except for the treatment. In appendix 1 we present the results of tests of the assumptions underlying the RD design.

The first labor market outcome we look at is the change in wages, which will indicate whether or not compliance with minimum wage legislation increased after the Campaign. To study the impact of the Campaign on wages and compliance, we compare the change in wages between 2010 and 2011 for workers earning above and below the minimum wage in 2010 (before the Campaign began). If the Campaign was effective in causing employers to pay the minimum wage to those previously earning below the minimum wage, then we should see the wages of those initially earning below the minimum wage increase in 2011 by more than the wages of

those who were earning at or above the minimum wage. This would be evidence that the Campaign increased compliance with minimum wage legislation.

One advantage of the RD design is that it can be used to illustrate the impact of the treatment graphically. Figure 2 shows how this might work. In both panels of figure 2, the workers earning just below the minimum wage in 2010 increase by more than the wages of workers who had been earning just above the minimum wage. The RD estimate of the impact of the Campaign on wages is the difference in the wage change at the cutoff point. Using a non-parametric estimation technique and data on the wages of private sector employees in 2010 and 2011, we construct figures like those in figure 2 in order to see whether there is a "jump" in the relationship at the cutoff point. Non-parametric estimation allows us to capture any possible non-linear relationship between the outcome variable (the wage change) and the assignment variable (the difference between the wage and minimum wage), such as that shown in the second panel of figure 2.

We also present more formal regression discontinuity estimates of the treatment effect. Let Y_i = the outcome (i.e. the change in wages for worker i from before to after the Campaign: $LogW_{i,2011} - LogW_{i,2010}$). The gap between the reported wage and minimum wage for each worker i in 2010 (before the Campaign) is the assignment variable ($LogW_{i,2010}$-$LogMW_{i,2010}$). Let D_i=1 if worker i earns less than the minimum wage in 2010, and D_i=0 if the worker earns more than the minimum wage in 2010. Assuming that, except for the jump at the cutoff point (where $LogW_{i,2010}$=$LogMW_{i,2010}$), the relationship between the assignment variable ($LogW_{i,2011} - LogW_{i,2010}$) and outcome variable ($Y_i$, i.e. $LogW_{i,2011} - LogW_{i,2010}$) is linear, as in panel A of figure 2, a simple way to estimate the treatment effect, τ, is by estimating the following linear regression (Lee and Lemieux, 2009):

$$Y_i = \alpha_l + \tau D_i + \beta\,(LogW_{i,t-1} - LogMW_{i,t-1}) + \varepsilon_i \qquad \text{(EQ 1)}$$

However, if the relationship is non-linear, then equation (1) will not provide a good estimate of the RD effect. One way to incorporate a non-linear relationship between the assignment variable and outcome variable is to model that relationship as a higher order polynomial. In this paper we present estimates using a very flexible functional form using a 5^{th} order polynomial specification. Another way to address this issue is through the estimates of local linear regressions. In the local linear regression one estimates a linear regression using only data near the cutoff point, and allow the slope of the curve to differ to the left and right of the cutoff point. The local linear regression technique is equivalent to estimating two linear regressions: one using data just below the cutoff point and the other using data just above the cutoff point:

$$Y_i = \alpha_l + \beta_l\,(LogW_{i,2010} - LogMW_{i,2010}) + \varepsilon_i \text{ where } LogMW_{i,t2010} - h \le LogW_{i,2010} < LogMW_{i,t2010} \qquad \text{(EQ 2a)}$$

$$Y_i = \alpha_r + \beta_r\,(LogW_{i,2010} - LogMW_{i,2010}) + \varepsilon_i \text{ where } LogMW_{i,2010} \le LogW_{i,2010} \le LogMW_{i,2010} + h \qquad \text{(EQ 2b)}$$

Or equivalently

$$Y_i = \alpha_l + \tau D_i + \beta_l (LogW_{i,2010} - LogMW_{i,2010}) + (\beta_r - \beta_l) D_i (LogW_{i,2010} - LogMW_{i,2010}) + \varepsilon_i \quad \text{(EQ 3)}$$
$$\text{if } LogMW_{i,2010} - h \leq LogW_{i,2010} < LogMW_{i,2010} + h$$

where i = worker, Y_i = the outcome variable (i.e. $LogW_{i,2011} - LogW_{i,2010}$), $LogW_{i,2010}$ = log of the wage of worker i in 2010; $LogMW_{i,2010}$ = the log of the legal minimum wage in 2010, and ε_i = random error. h is called the bandwidth; this is the local area around the cutoff point from which we draw the data to estimate the local linear regressions. We estimate equation 3, where τ is the RD estimate of the treatment effect.

One difficulty with the local linear regression approach is how to choose the appropriate bandwidth; the bandwidth must be small enough to reasonably expect the relationship between the assignment variable and outcome variable to be linear but wide enough so that there are sufficient observations within the bandwidth to confidently estimate the regression parameters. We report estimates for a variety of bandwidths to show that our estimates are not sensitive to the bandwidth chosen. We choose our range of bandwidths in two ways. First, through an ad-hoc examination of a non-parametric estimate of the relationship between the assignment variable and outcome variable. Second, by using the technique derived in Imbens and Kalyanaraman (2009) that minimizes the mean square error of the difference between the predicted and actual outcome variables.

VI. Results: The Impact on Compliance and Wages

We first examine if there is evidence that the Campaign for Minimum Wages increased compliance with legal minimum wage legislation in Costa Rica. That is, the first outcome variable we look at is the change in wages for worker i between 2010 and 2011. If the wages of workers who were earning below the minimum wage increased by more than the wages of those who were earning above the minimum wage, this would be evidence that the Campaign increased compliance with minimum wage legislation.

(i) Descriptive statistics

If the Campaign for Minimum Wages increased compliance with minimum wage legislation, we would expect to see the proportion of workers earning less than the minimum wage to fall from 2010 to 2011, and the proportion of workers earning equal to the minimum wage to increase.

Table 5 presents the proportion of full-time private sector employees who earn below, at, and above the minimum wage in 2010 and 2011. We consider only private sector employees because they are the workers who are legally covered by minimum wage legislation in Costa Rica; wages

for public sector workers are set based on a different set of institutionally-determined wages, and self employed workers are not covered by minimum wage legislation because they cannot be forced to pay themselves the minimum wage. We limit this part of our analysis to full-time workers (36 hours per week or more) for two reasons. First, most minimum wages are specified in the government decrees as monthly earnings for full-time workers, and it is not entirely clear how or when minimum wages apply to part-time workers. Second, because of the potential measurement error estimating part-time wages that can occur because the data report monthly earnings but weekly hours worked. To take potential measurement error into account, we look at a band of 5% around the minimum wage. That is, table 5 actually reports the proportion of workers earning below 95% of the minimum wage, from 95% to 105% of the minimum wage, and above 105% of the minimum wage.

Table 5 shows that the proportion of workers earning below the minimum wage falls from 2010 to 2011, while the proportion of workers earning within 5% of the minimum wage increases. The proportion of workers earning above the minimum wage does not change between 2010 and 2011. This evidence is consistent with the hypothesis that the Campaign for Minimum Wages increased compliance with minimum wage legislation. That is, table 5 suggests that between 2010 and 2011 some workers who were earning below the minimum wage had their wages increased to the minimum wage.

To further explore whether we can identify an impact of the Campaign for Minimum Wages by examining changes in the distribution of wages between 2010 and 2011, figure 3 presents kernel density estimates of the distribution of the log of wage minus the log minimum wage for each private sector employee. In figure 3 a zero indicates that the worker is earning the minimum wage applicable to that workers occupation, skill level and education; a negative number indicates that the worker is earning below the minimum wage; and a positive number indicates that the worker is earning above the minimum wage. In both 2010 and 2011, there is a spike in the distribution of wages at the minimum wage, but the spike is bigger in 2011. This illustrates that the proportion of workers earning at the minimum wage increased from 2010 to 2011. There also appears to be fewer people earning below the minimum wage in 2011 compared to 2010, while the distribution of workers earning above the minimum wage is similar in 2010 and 2011. This evidence is consistent with the hypothesis that many workers earning below the minimum wage before the Campaign had their wages increased to the minimum wage after the Campaign, but that the Campaign did not affect the distribution of wages for those workers whose wages were already above the minimum wage.

The regression discontinuity estimates presented in the next subsection compare how wages changed from before and after the campaign for those who earned below the minimum wage in 2010 compared to those who earned above the minimum wage. Table 6 presents the changes in mean log monthly earnings for workers who were below and above 1%, 5% and 10% of the minimum wage in 2010. Table 6 shows that, after the implementation of the Campaign for

Minimum Wages, the earnings of those who had earned below the minimum wage increased approximately 10% more than the earnings of those who had earned above the minimum wage. The evidence, too, is consistent with the hypothesis that the Campaign caused the wages of those below the minimum wage to increase, implying increased compliance with minimum wages.

(ii) Regression Discontinuity Estimates

Figure 4 presents a graphical representation of the regression discontinuity estimate of the impact of the Campaign for Minimum Wages (this is an estimate, using our data, similar to the stylized figure 2). Figure 4 is consistent with the hypothesis that the Campaign increased compliance with minimum wages for those workers who had been earning just below the minimum wage. Figure 4 shows that when the Campaign was implemented, earnings increased more for those who had been earning below the minimum wage when compared to earnings changes for those who had been earning above the minimum wage. Over the entire sample, there is a trend where earnings increased more for those whose earnings are lowest (relative to the minimum wage). However, the earnings of those earning just below the minimum wage increased more than the overall trend would suggest, while the earnings of those just above the minimum wage increased by less. That is, there is a clear discontinuity at the cutoff point—workers earning just below the minimum wage before the Campaign saw their wages increase by more than those earning just above the minimum wage. Again, this is evidence that the Campaign increased compliance with minimum wages in Costa Rica. The evidence from Figure 4 suggests that the Campaign had the biggest impact on the wages of those who were earning within 15% of the minimum wage. There is a clear change in the pattern of the relationship between wages in 2010 and wage changes for workers earning from 0 to 15% below the minimum wage; for these workers wages increase by more than the overall pattern would suggest.

Table 7 presents local linear RD estimates of the impact of the Campaign on wage changes, estimated for a variety of bandwidths[7]. Specifically, we consider bandwidths suggested by Figure 4: there appears to be a linear relationship between the assignment variable and the outcome variable for values of the assignment variable that are around 10-15% above and below the cutoff point. Thus we present estimates using 10% and 15% bandwidths. We also present estimates using the range of "optimal" bandwidths suggested by Imbens and Kalyanaraman (2009).[8] Finally, table 7 also presents regression discontinuity estimates based on a flexible 5th order polynomial regression (using all of the observations). All of these estimates are consistent,

[7] Equations 1 through 2 are local linear regressions with rectangular kernels while 4 through 6 are local linear regressions with triangular kernels.

[8] The bandwidth used in equations (6) in table 7 use the bandwidth that minimizes the mean squared error of the difference between the predicted outcome of the regression and the actual outcome. However, Imbens and Kalyanaraman (2009) note that because this is based on the entire sample, it may be an overestimate of the truly optimal bandwidth. They also suggest using ½ of this bandwidth might be more appropriate. In table 7, we present estimates using this "optimal" bandwidth (.41), half (.21) and our preferred bandwidth (.15).

and suggest that the Campaign for Minimum Wages caused the earnings of those earning just below the minimum wage to increase by about 10 percent more than the increase in the wages of those earning just above the minimum wage. These estimates are statistically significant at the 10% level in almost all specifications, and significant at the 5% level for the majority of specifications.

Table 8 adds covariates to the local linear regression (age, gender, firm size, education, whether the worker belongs to a union or solidarity organization, and a set of dummy variables indicating the industry sector). Including covariates may increase the precision of the regression discontinuity estimates (Lee and Lemieux, 2009). In our case, adding these covariates to our RD regressions does change the regression discontinuity of the estimates of the impact of the Campaign. The RD estimates however remain high ranging from 8 to 10 percent and are still statistically significant (except for the .10 and 0.41 bandwidth).

To test which groups were most affected by the Campaign for Minimum Wages, we estimate the impact separately for different sub-sets of the population. Table 9[9] presents RD estimates of the impact of the Campaign separately for: men vs. women; youth (15-24) vs. older workers; and secondary school graduates vs. less education. These results suggest that the Campaign for Minimum Wages had a larger positive wage impact on women and youth. On average, the campaign increased the mean earnings of women by 14 to 27%, while the impact on the mean earnings of men was only 5%-8% (and statistically insignificant). This result is probably a reflection of the fact that women were much more likely to earn less than the minimum wage in 2010 compared to men (see table 4). The estimated impact of the campaign on the mean earnings of youth is 9%-18%, which is higher than the estimated impact on the wages of older workers. Interestingly, the Campaign had a bigger positive impact on the earnings of secondary school graduates than it did on the earnings of those who did not complete secondary school.

(iii) Testing for a placebo effect: does the RD technique find an impact on the wages of the self-employed?

In practice, self-employed workers are not covered by minimum wage legislation; self-employed workers cannot be forced to pay themselves the minimum wage. Therefore, we should not see the wages of self-employed workers who had been earning below the minimum wage before the campaign increase by more than the wages of the self-employed who had been earning above the minimum wage. If we find that this is happening among the self-employed as well as among private sector employees, then it is likely that the result presented in the last subsection is not due to the Campaign for Minimum Wages, but is due to some other phenomenon that affects both private sector employees and the self-employed. To test for this possible "placebo" effect, we

[9] It should be noted that these estimates do not include control variables. The RD estimates decrease after adding covariates to regressiosn 1 throught 3 and become statistically insignificant (except for the polynomial regression).

re-do our Regression Discontinuity analysis of the impact of the Campaign using data from the self-employed rather than private sector employees.

Figure 5 presents kernel density estimates of the Log Wage minus Log Minimum Wage for full-time self-employed workers in 2010 and 2011. Overall, the distributions for 2010 and 2011 look very similar. As in the distribution of paid employees, there is a large proportion of self-employed workers who report earning below the legal minimum wage. Unlike in a similar figure using data for paid employees, we do not see a larger spike in the distribution of self-employed earnings at the minimum wage in 2011 compared to 2010. If anything, the spike at the minimum wage is larger in 2010 than in 2011. This evidence is consistent with the hypothesis that the Campaign had no impact on the distribution of earnings among self-employed workers.

Figure 6 shows that when the Campaign for Minimum Wages was implemented, earnings did not increase any faster for those who had been earning just below the minimum wage when compared to those who had been earning just above the minimum wage. There is no discontinuity at the cutoff point, which again suggests that the Campaign had no impact on the earnings of self-employed workers.

Table 10 presents the results of the RD regressions. Table 10 suggests that the Campaign for Minimum Wages did not have a statistically significant impact on the wages of self-employed workers who had been earning below the minimum wage prior to the Campaign.[10] This provides additional evidence to strengthen our conclusion that the Campaign had a positive causal impact on the wages of private sector employees (the covered sector) who had been earning below the minimum wage before the Campaign.

VII. Results: The Impact on Employment

In the last section, we concluded that the Campaign for Minimum Wages increased compliance with minimum wage legislation. That is, the Campaign caused employers who had been paying less than the minimum wage to some workers to increase the wages of these workers to the legal minimum. If the marginal productivity of minimum wage workers is less than the legal minimum wage, then the Campaign may have also caused some employers to fire the now-more-costly workers to whom they had been paying less than the minimum wage. To test for this possibility we conduct a regression discontinuity analysis similar to that in the last section, but replace the outcome variable with a measure of whether or not workers kept their jobs between 2010 and 2011.

Figure 7 presents a graph of the relationship between whether or not the worker was earning below the minimum wage in 2010 and the probability that a full-time private sector employee in

[10] These results also hold when we add covariates to the RD regressions for the self-employed.

2010 is still a full-time employee in 2011. Over the entire sample, full-time private sector employees with earnings below the minimum wage in 2010 were less likely to remain full-time employees in 2011. However, figure 7 presents no clear evidence that this was true for those earning just below vs. just above the minimum wage in 2010. That is, there is no clear discontinuity. This suggests that the Campaign had no impact on the probability that a worker remained employed.

Tables 11 and 12 present regression discontinuity estimates of the impact of the Campaign for Minimum Wages on the probability that a private sector worker remained employed from 2010 to 2011. The outcome variable in these regressions is a dummy variable that is one if worker i remains a full-time employee between 2010 and 2010, and zero if not. Table 11 presents the basic RD estimates; table 12 adds covariates to the regressions. The results of all RD estimates are consistent: workers earning less than the minimum wage in 2010 were not less likely to remain employed in 2011. The RD estimates are positive (although not statistically significant), which would suggest, if anything, that workers who had been earning less than the minimum were more likely to remain employed than workers who had been earning more than the minimum wage. That is, we find no support for the hypothesis that the Campaign for Minimum Wages resulted in employers firing the now-more-costly workers to whom they had been paying less than the minimum wage

While we found no evidence that employers fired workers because they were forced to pay sub-minimum wage workers more, it is still possible that the employment opportunities for this group declined because of the Campaign. That is, employers may have reduced new hires of workers with sub-minimum wage productivity. To provide some evidence on this possibility, the next step in our analysis is to test whether the Campaign led to reduced hiring of sub-minimum wage self-employed workers.

VIII. Conclusions and Extensions

In August 2010 the Costa Rican government implemented a comprehensive program to increase compliance with legal minimum wages in Costa Rica: the Campaign for Minimum Wages. We analyze a panel data set with data on workers from before the Campaign began (July, 2010) and after the Campaign had been in operation for some time (July, 2011). To evaluate the impact of the Campaign, we use a regression discontinuity approach, which compares what happed to workers who before the campaign had been earning below the minimum wage to those who before the Campaign had been earning above the minimum wage. We find evidence that the Campaign led to an increase in compliance with minimum wage laws in Costa Rica; the mean earnings of those earning less than the minimum wage in 2010 increased by approximately 10% more than the earnings of those who had been earning more than the minimum wage. We also

find evidence that the Campaign led to a larger mean increase in the wages of women and younger workers. Further, we find no evidence that the Campaign had a negative impact on the employment of workers whose wages were increased.

This version of our paper is preliminary. By the time of the workshop on September, 2012, we will have done significantly more work than is presented in this preliminary version. In addition to refining our estimates of the impact of the Campaign for Minimum Wages on wages and employment, we also plan to test for an impact of the campaign on the following: whether an employer provides mandated non-wage benefits (such as contributions to Social Security); whether the employer provides voluntary non-wage benefits (such as pension contributions and payments in kind); and whether there is an impact on the proportion of the work force in the informal sector.

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Appendix 1: Testing key assumptions of the regression discontinuity approach

A key assumption of the regression discontinuity approach is that workers just below the cutoff are not systematically different from workers just above the cutoff, except for the fact that they have been treated (Imbens & Lemieux, 2008; McCrary, 2008; Van der klaauw, 2008). In our context, this assumption would be violated if there are non-random differences in the observable or unobservable characteristics between workers earning just below vs. just above the minimum wage in 2010. This could occur if employers intentionally choose which sub-minimum wage worker gets a wage below the legal minimum wage and which gets a wage above the legal minimum wage, or if workers have control over who earns above or below the minimum wage. According to the regression discontinuity literature, knowledge and manipulation of treatment assignment rules may generate unexpected behavioral responses from employees or employers (Imbens & Lemieux, 2008; McCrary, 2008; Van der klaauw, 2008). Either can invalidate the comparability of workers near the threshold because of sorting around the cut off, where those below the minimum wage might differ on average from those just above.

There might be reasons to believe that workers might have been sorted on unobserved characteristics such as innate ability, power to negotiate, knowledge of minimum wage laws and so on. If this holds, the key identifying assumption behind the RD design will be violated and the results will be subject to selection bias. The key question is whether this is a *precise* or *imprecise* manipulation of the assignment variable. Under imperfect "imprecise" manipulation, the continuity assumption holds (Lee, 2008) due to some random element as to who gets bumped above the minimum wage and who does not.

McCrary (2008)[11] developed a formal test of manipulation based on an intuition that density to the right of the cut-off of the assignment variable will be considerably different than to the left of the cut-off under complete manipulation of the assignment variable. Lee and Lemieux (2009) show examples of densities of forcing variable under three conditions; complete, imprecise and precise manipulation. Under imprecise manipulation, believed to be the case in our RD design, if workers or employers cannot precisely control the assignment variable, then the expected density of the assignment variable should be continuous around the discontinuity point as shown in

[11] In practice, McCrary's test is executed in two steps as follows: the first step involves plotting a very under smoothed histogram. The bins for the histogram are defined so that no bin includes points both to the left and right of the discontinuity point. The second step is a local linear regression of the histogram separately on either side of the cutoff. The midpoints of the histogram bins are treated as a regressor in the regression, and the normalized counts of the number of observations into each bin are treated as an outcome variable. Finally, the discontinuity at the cutoff is then estimated as the log difference in height on the intercept.

figure A1. That is, figure A1 suggests that our data pass the McCrary test.

Finally, McCrary (2008) strongly points out that such a density test is only informative if manipulation is monotonic, where the treatment induces agents (workers and employers) to change the assignment variable in one direction only and that the absence of a discontinuity in the density of the assignment variable is neither a necessary nor a sufficient condition for valid inference (McCrary, 2008 p.5).

Another validity test is to examine the comparability of worker on either side of the threshold. Ideally, workers earning just below the minimum wage should on average have similar observed and unobserved characteristics to those earning just above the minimum wage.

We can plot the average of the covariates against the assignment variables on either side of the threshold and see if a discontinuity exits. An alternative method is to use the covariates as the dependent variable and run a regression discontinuity using the assignment variable as the independent variable (Imbens & Lemieux, 2007; van der Klaauw, 2008; Lee & Lemieux, 2009) (Table A1).

We find sharp discontinuities in the distribution of several worker characteristics around the cutoff threshold. This includes discontinuities in the variables gender, educational level and industry type. For example, female workers are more likely to earn below the minimum wage (that is, there is a higher concentration of female workers below the cut-off than above. Similarly, less educated workers are more likely to earn below the minimum wage. These results suggest that women and less educated workers are likely to benefit the most from the treatment. Finally, there is evidence that it is more probable that workers just below the minimum wage are in industries were wages are below the minimum (construction, manufacturing, agriculture and manufacturing.[12] Thus, the treatment (the campaign) is confounded with relative observed worker characteristics that might be associated with the earlier finding of sorting close to the cut-off and might undermine the reliability of the RD design.

In principle, the regression-discontinuity design conditional on worker covariates (i.e. educational level, gender and industry) should reduce the problem of sorting. In the body of this paper we present RD estimates that include these variables as covariates, as well as RD estimates of the impact of the campaign for workers with each of these characteristics (i.e. by gender and education level). The results of all our estimates are consistent and show that the Campaign did have a positive impact on wages and compliance. However, if additional unobserved factors

[12] Interestingly, assignment to above or below the minimum wage does not depend on whether the worker is in a small firm or is a member of a union or solidarity organization.

such as skills jointly determine the likelihood of earnings at or above the minimum wage, then sorting might persist and can invalidate the underlying RD identification assumptions, and straightforward RD estimates would be subject to selection bias.

Appendix 2: Minimum Wage Decree First Semester of 2010; in effect January 1 2010

Decreto de Salarios Mínimos Nº 36292-MTSS
I Semestre del 2011

(Gaceta 238 del 8 de diciembre del 2010, rige a partir del 1o de enero del 2011)

LA PRESIDENTA DE LA REPÚBLICA

Y LA MINISTRA DE TRABAJO Y SEGURIDAD SOCIAL

En el ejercicio de las potestades conferidas en el artículo 140, incisos 3) y 18) de la Constitución Política y la Ley Nº 832 del 4 de noviembre de 1949 y sus reformas.

Considerando:

1º—Que el Consejo Nacional de Salarios, en el ejercicio de las potestades conferidas en la Ley Nº 832 del 4 de noviembre de 1949 y el Reglamento a dicha ley que fue publicado mediante el Decreto Nº 25619 y su reformas, en Acta Nº 5127 del 21 de octubre de 2010, aprobó la respectiva determinación de Salarios Mínimos que han de regir a partir del 1º enero del año 2011. **Por tanto**;

DECRETAN:

Fijación de salarios mínimos para el sector privado
que regirán a partir del 1º de enero de 2011

Artículo 1º—Fíjanse los salarios mínimos que regirán en todo el país a partir del 1º de enero del 2011:

1a- Agricultura (Por jornada ordinaria)

Agricultura, (Subsectores: Agrícola, Ganadero, Silvícola, Pesquero), explotación de minas y canteras, industrias manufactureras, construcción, electricidad, comercio, turismo, servicios, transportes y almacenamientos.

Trabajadores no calificados	¢7.383,17
Trabajadores semicalificados	¢8.038,71
Trabajadores calificados	¢8.193,77
Trabajadores especializados	¢9.820,13

A los trabajadores que realicen labores ya reconocidas como pesadas, insalubres o peligrosas y las que llegasen a ser determinadas como tales por el organismo competente, se les fijará un salario por hora equivalente a la sexta parte del salario fijado por jornada para el trabajador no calificado.

Las ocupaciones en Pesca y Transporte acuático, cuando impliquen imposibilidad para el trabajador de regresar al lugar de partida inicial al finalizar su jornada ordinaria, tienen derecho a la alimentación.

1b- Genéricos (por mes)

Trabajadores no calificados	¢220.345,47
Trabajadores semicalificados	¢237.353,28
Trabajadores calificados	¢249.725,23
Técnicos medios de educación diversificada	¢268.376,40
Trabajadores especializados	¢287.599,41
Técnicos de educación superior	¢330.744,39
Diplomados de educación superior	¢357.216,28
Bachilleres universitarios	¢405.167,82
Licenciados universitarios	¢486.218,04

En todo caso en que por disposición legal o administrativa se pida al trabajador determinado título académico de los aquí incluidos, se le debe pagar el salario mínimo correspondiente, excepto si las tareas que desempeña están catalogadas en una categoría ocupacional superior de cualquier capítulo salarial de este Decreto, en cuyo caso regirá el salario de esa categoría y no el correspondiente al título académico.

Los salarios para profesionales aquí incluidos rigen para aquellos trabajadores debidamente incorporados y autorizados por el Colegio Profesional respectivo, con excepción de los trabajadores y profesionales en enfermería, quienes se rigen en esta materia por la Ley Nº 7085 del 20 de octubre de 1987 y su Reglamento.

Los profesionales contratados en las condiciones señaladas en los dos párrafos anteriores, que estén sujetos a disponibilidad, bajo los límites señalados en el artículo 143 del Código de Trabajo, tendrán derecho a percibir un 23% adicional sobre el salario mínimo estipulado según su grado académico de Bachilleres o Licenciados universitarios.

1c- Relativo a fijaciones específicas

Recolectores de café (por cajuela)	¢709,51
Recolectores de coyol (por kilo)	¢23,33
Servicio doméstico (por mes)	¢131.907,04
Trabajadores de especialización superior[1]	¢15.381,02
Periodistas contratados como tales (incluye el 23%en razón de su disponibilidad) (por mes)	¢598.825,18

Estibadores: ¢1,02 por caja de banano ¢63,30 por tonelada ¢269,97 por movimiento

Los portaloneros y los wincheros devengan un salario mínimo de un 10% más de estas tarifas.

Taxistas en participación, el 30% de las entradas brutas del vehículo. En caso de que no funcione o se interrumpa el sistema en participación, el salario no podrá ser menor de ocho mil novecientos setenta y dos colones con noventa y ocho céntimos (¢8.972,98) por jornada ordinaria.

Agentes vendedores de cerveza, el 2.45% sobre la venta, considerando únicamente el valor neto del líquido. Circuladores de periódicos, el 15% del valor de los periódicos de edición diaria que distribuyan o vendan.

Artículo 2º—Por todo trabajo no cubierto por las disposiciones del artículo 1º—de este Decreto, todo patrono pagará un salario no menor al de Trabajador no Calificado del inciso 1a del Artículo 1º—de este Decreto.

Artículo 3º—Los salarios mínimos fijados en este Decreto, son referidos a la jornada ordinaria de acuerdo con lo estipulado en el Capítulo Segundo del Título Tercero del Código de Trabajo, con excepción de aquellos casos en los que se indique específicamente que están referidos a otra unidad de medida.

Cuando el salario esté fijado por hora, ese valor se entiende referido a la hora ordinaria diurna. Para las jornadas mixta y nocturna, se harán las equivalencias correspondientes, a efecto de que siempre resulten iguales los salarios por las respectivas jornadas ordinarias.

Artículo 4º—El título “Genéricos”, cubre a las ocupaciones indicadas bajo este título, en todas las actividades, con excepción de aquellas ocupaciones que estén especificadas bajo otros títulos. Los salarios estipulados bajo cada título cubren a los trabajadores del proceso a que se refiere el título respectivo, y no a los trabajadores incluidos bajo el título Genéricos.

Para la correcta ubicación de las ocupaciones de las distintas categorías salariales de los Títulos de los Capítulos del Decreto de Salarios, se deberá aplicar lo establecido en los Perfiles Ocupacionales, que fueron aprobados por el Consejo Nacional de Salarios y publicados en *La Gaceta* Nº 233 de 5 de diciembre 2000.

Artículo 5º—Este Decreto no modifica los salarios que, en virtud de contratos individuales de trabajo o convenios colectivos, sean superiores a los aquí indicados.

Artículo 6º—Los salarios por trabajos que se ejecuten por pieza, a destajo o por tarea o a domicilio, ya sea en lugares propiedad del empleador o bien en el domicilio del trabajador, no podrán ser inferiores a la suma que el trabajador hubiera devengado laborando normalmente durante las jornadas ordinarias y de acuerdo con los mínimos de salarios establecidos en el

Artículo 7º—Regulación de formas de pago: si el salario se paga por semana, se debe de pagar por 6 días, excepto en comercio en que siempre se deben pagar 7 días semanales en virtud del artículo 152 del Código de Trabajo. Si el salario se paga por quincena comprende el pago de 15 días, o de 30 días si se paga por mes, indistintamente de la actividad que se trate. Los salarios determinados en forma mensual en este Decreto, indican que es el monto total que debe ganar el trabajador, y si se paga por semana, siempre que la actividad no sea comercial, el salario mensual debe dividirse entre 26 y multiplicarse por los días efectivamente trabajados.

Artículo 8º—Rige a partir del 1º de enero del 2011.

Dado en la Presidencia de la República, a los veintiocho días del mes de octubre del dos mil diez.

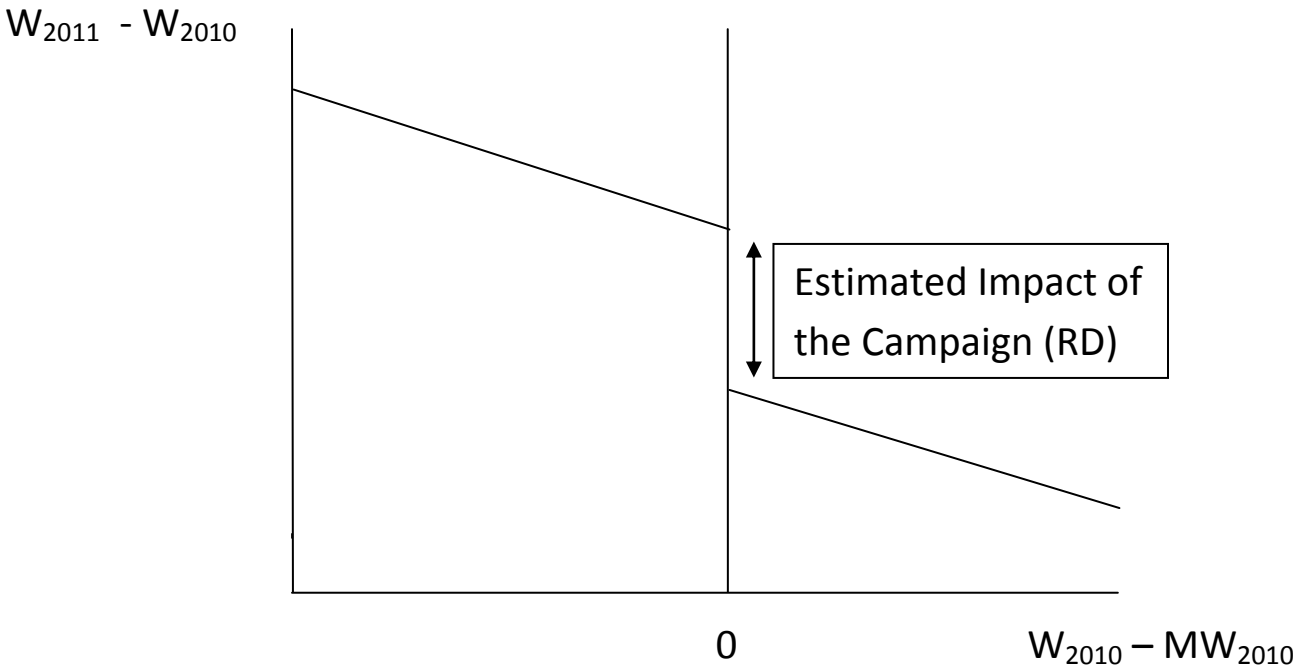
LAURA CHINCHILLA MIRANDA.—La Ministra de Trabajo y Seguridad Social, Sandra Piszk Feinzilber.—1 vez.—O. C. Nº 9988.—Solicitud Nº 38708.—C-93500.—(D-36292-IN20100101886).

Publicado en la Gaceta No. 238 del 8 de diciembre del 2010.

Figure 1: Components of the publicity campaign illustrated on a Ministry of Labor web site

Figure 2: Potential impact of the Campaign on wages and compliance.

Panel A: Assuming a linear relationship between the assignment variable and outcome variable:



Panel B: Assuming a non-linear relationship between the assignment variable and outcome variable:

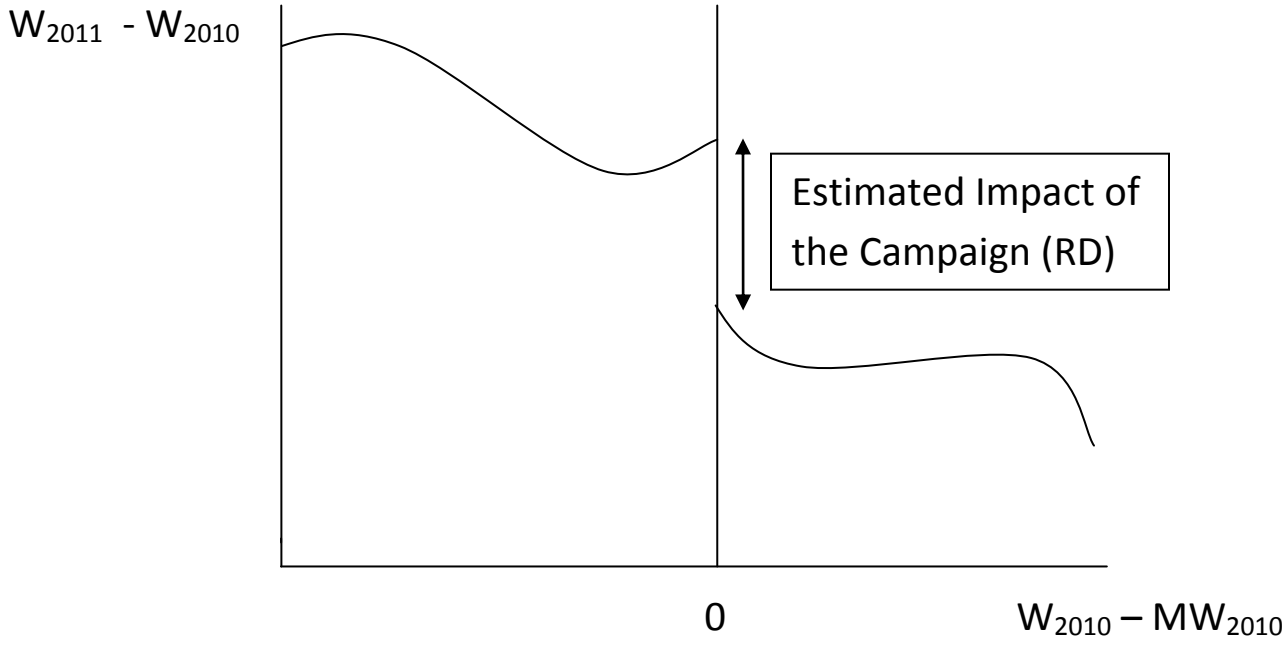


Figure 3: Kernel density estimates of the distribution of the log of monthly earnings relative to the legal minimum wage, for full-time private sector employees, 2010 and 2011

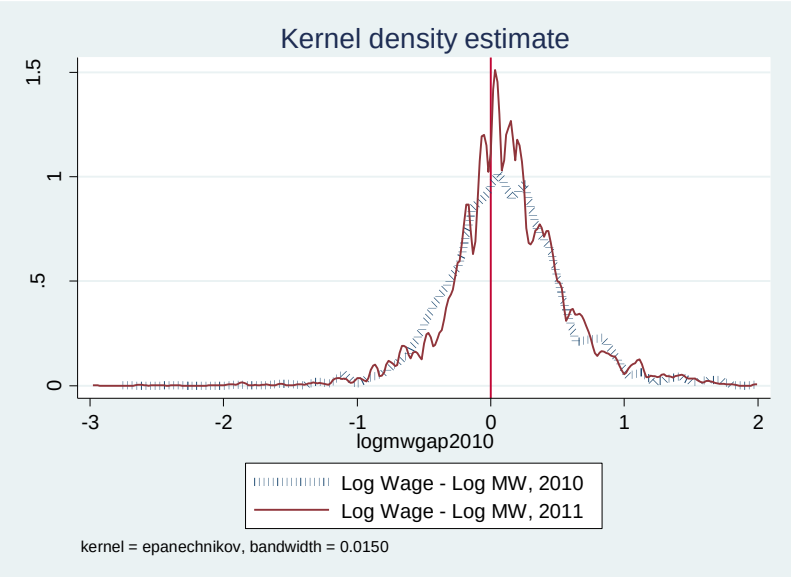
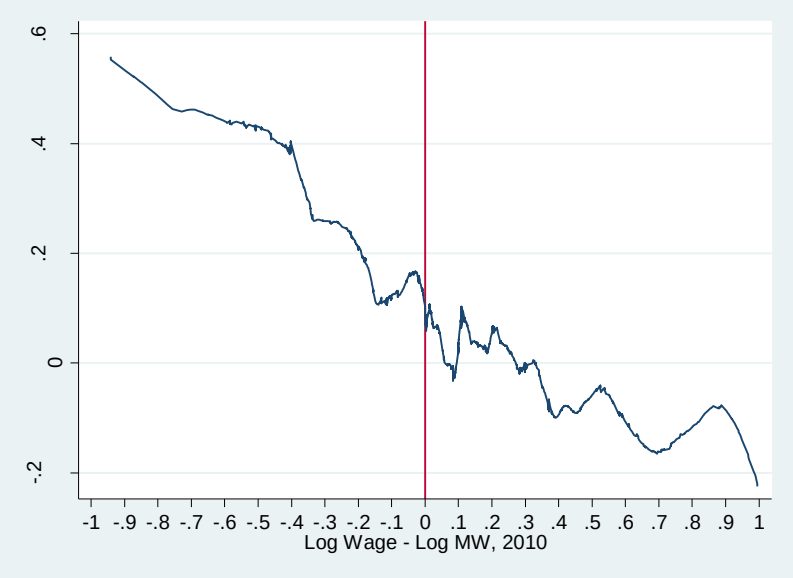
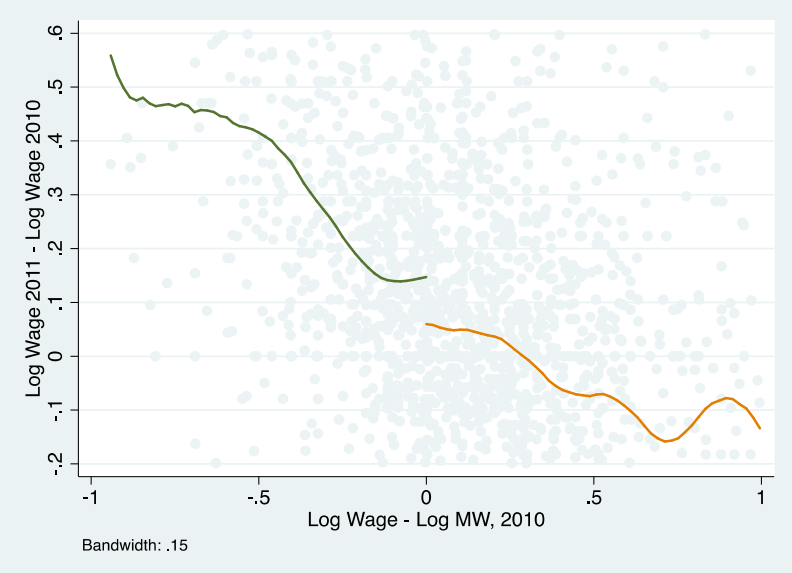


Figure 4a: Graphical representation of regression discontinuity estimate of impact on wages, full-time private sector employees in 2010 and 2011



Note: Graph was created using locally weighted regression smoothing (the Stata lowess command), bandwidth=0.07.

Figure 4b: Regression discontinuity estimate of impact on wages, full-time private sector employees in 2010 and 2011



Estimated discontinuity= .095, standard error= .046

Figure 5a: Placebo/falsification Test: kernel density estimates of the distribution of the log (earnings)-log(earnings) in 2010 and 2011, for self-employed workers

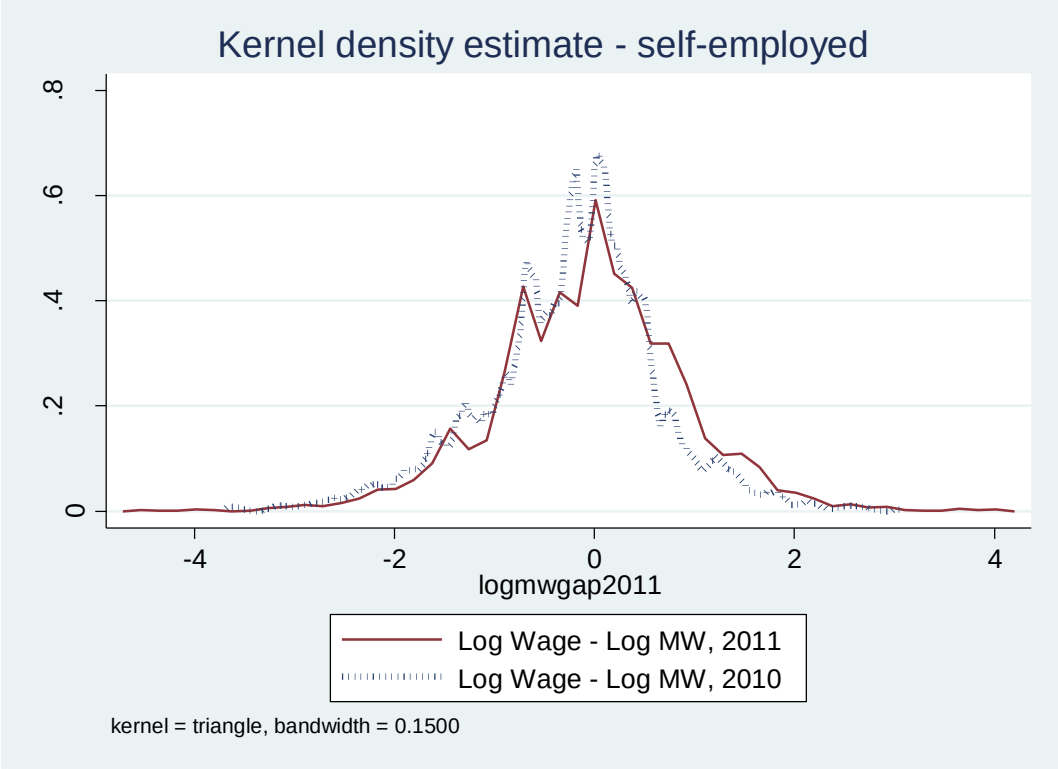
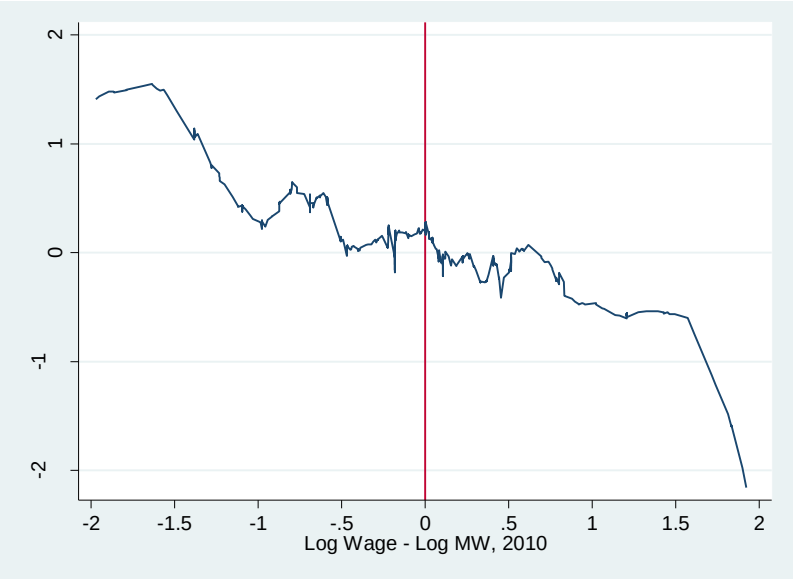
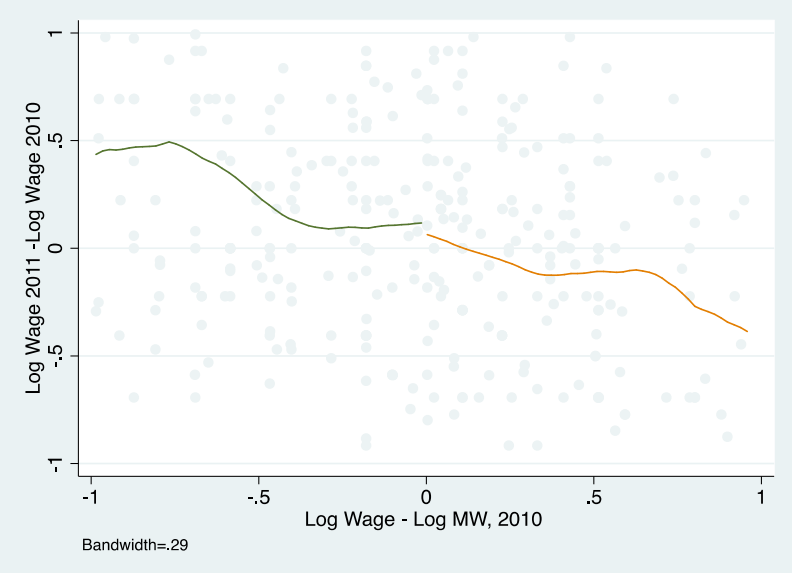


Figure 6a: Graphical representation of regression discontinuity estimate of impact on wages, self-employed workers in 2010 and 2011



Note: Graph was created using locally weighted regression smoothing (the Stata lowess command), bandwidth=0.07.

Figure 6b: Placebo/falsification Test: Regression discontinuity estimate of impact on wages, for self-employed workers



Estimated discontinuity= .017, standard error= .215

Figure 7: Graphical representation of regression discontinuity estimate of impact on the probability that a full-time private employee in 2010 remains a full-time employee in 2011

Note: Graph was created using locally weighted regression smoothing (the Stata lowess command), bandwidth=0.13.

Figure 7b: Regression discontinuity estimate of impact on the probability that a full-time private employee in 2010 remains a full-time employee in 2011

Estimated discontinuity= .029, standard error= .049

Figure A1: Density of the assignment variable (Log Wage – Log MW, 2010)

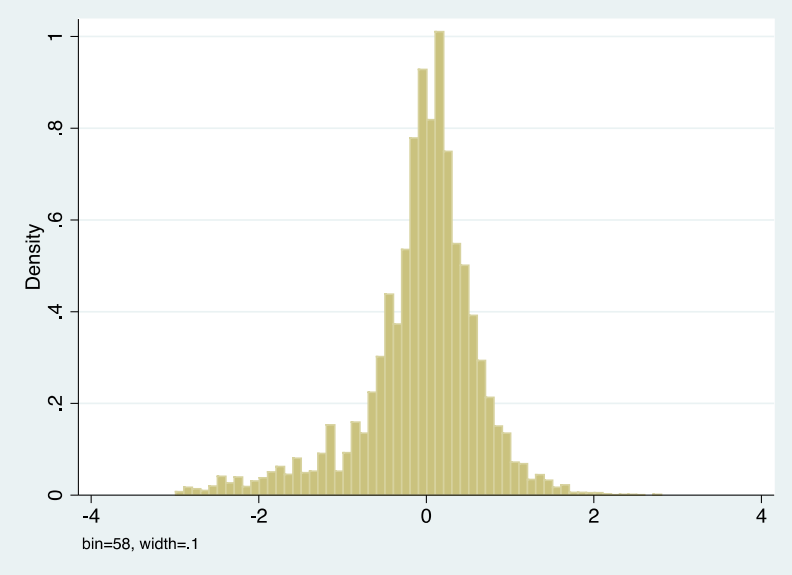


Table 1: Number of inspections, 2005-2011

	2005	2006	2007	2008	2009	2010	2011 (Jan. 1 - June 30)
Total First Inspections	6,717	5,888	6,815	8,673	7,415	9,512	4,250
Full inspections (Not targeted)	6,717	5,888	6,815	8,673	7,415	4,313	1,185
Official	4,599	4,304	4,987	6,874	5,556	3,080	909
Result of complaint	2,118	1,584	1,485	1,799	1,185	1,233	276
No data	0	0	343	0	674	0	0
Minimum Wage targeted (CSM)	0	0	0	0	0	5,199	3,065
Official	0	0	0	0	0	5,199	3,004
Result of complaint	0	0	0	0	0	0	61
Total Second Inspections	4,525	4,080	3,015	3,652	4,061	4,493	1,927
Full inspections (Not targeted)	4,525	4,080	3,015	3,652	4,061	3,148	706
Minimum Wage targeted (CSM)	0	0	0	0	0	1,345	1,221
Total Inspection	11,242	9,968	9,830	12,325	11,476	14,005	6,177

CSM: Campaña Nacional del Salario Mínimo

Source: DNI. Unidad de Gestión.

Table 2: Results of initial inspections targeted on minimum wages; August 1, 2010 to June 30, 2011

Type of Firm	Total number of firms inspected	No violation	Minimum wage violation found	% of firms with MW violations
Microfims (1-5 workers)	6,111	3,527	2,584	42.3
Small firms (6-30 workers)	1,604	947	657	41.0
Medium sized firms (31-100 workers)	237	162	75	31.6
Large firms (more than 100 workers)	93	65	28	30.1
Ignorado	206	156	50	24.3
Total	**8,251**	**4,857**	**3,394**	**41.1**
Agriculture	400	222	178	44.5
Manufacturing	945	566	379	40.1
Construction	107	80	27	25.2
Commerce	5,654	3,239	2,415	42.7
Transport	97	70	27	27.8
Services	1,048	680	368	35.1
Total	**8,251**	**4,857**	**3,394**	**41.1**

Table 3: Results of second inspections, 2055-2011

	2005	2007	2010	2011 (Jan. 1 – June 30)
Total number of second visits	**4351**	**3015**	**4493**	**1927**
Second visits (full inspections--not targeted)	**4351**	**3015**	**3148**	**706**
Complied	63.3%	63.5%	NA	77.4%
Not complied	19.7%	19.4%	-	13.2%
Not applicable	17.1%	17.1%	-	8.3%
Second visits (minimum wage targeted--CSM	**NA**	**NA**	**1345**	**1221**
Complied	0	0	80.4%	74.7%
Not complied	0	0	8.2%	10.7%
Not applicable	0	0	11.3%	14.7%

Note: not applicable implies that either the worker or firm was no longer present.

Table 4: Descriptive statistics – Group means and group differences, above and below the minimum wage, for different sub-samples of workers, 2010

Variables	Full sample			Within 10% of MW			Within 15% of MW		
	Below MW	Above MW	Difference	10% below MW	10% above MW	Difference	15% below MW	15% above MW	Difference
Demographics									
Mean Age	35.19	36.55	1.35	34.79	34.71	-0.08	34.47	34.62	0.15
Proportion Male	0.57	0.70	0.13	0.60	0.68	0.08***	0.62	0.68	0.06***
Mean Years of schooling	7.80	9.86	2.06***	8.40	8.48	0.08	8.41	8.55	0.14
Firm									
Proportion in Small Firms	0.51	0.20	-0.32	0.27	0.24	-0.03	0.27	0.29	0.02
Proportion Union Membership	0.03	0.11	0.07	0.05	0.07	0.02***	0.07	0.06	-0.01
Industry (proportion in each)									
Commerce	0.20	0.19	-0.01**	0.23	0.20	-0.03*	0.22	0.22	0.00
Finance	0.06	0.08	0.03	0.09	0.07	-0.02	0.09	0.07	-0.02**
Service	0.33	0.35	0.02**	0.27	0.24	-0.03	0.27	0.24	-0.02
Manufacturing	0.09	0.11	0.02	0.11	0.12	0.01	0.11	0.11	0.00
Construction	0.04	0.06	0.02	0.03	0.08	0.05***	0.04	0.08	0.04***
Agriculture	0.25	0.15	-0.10	0.22	0.23	0.01	0.24	0.23	-0.01
Transportation	0.03	0.06	0.03***	0.04	0.06	0.02	0.04	0.05	0.01
Number of Observations	5,495	6,250	--	853	1,158	--	1,417	1,778	--

Note: ***Indicate statistical significance at the 1% level. **For the 5% level.
*For the 10% level. Standard errors are shown in parentheses.
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Table 5: Percent of full-time private sector employees earning below, at and above the minimum wage, July 2010 and July 2011

Year	Below 95% of MW	At MW	Above 105% of MW
2010	32.0	11.6	56.4
2011	30.6	13.2	56.2

Table 6: Change between 2010 and 2011 in the log of mean monthly earnings, for those who were full-time private sector employees in both July 2010 and July 2011; comparing those who earned below the minimum wage in 2010 with those who earned above the minimum wage in 2012

	Within 1% of MW in 2010	Within 5% of MW in 2010	Within 10% of MW in 2010
Below MW in 2010	0.150	0.164	0.142
(Standard deviation)	*(0.268)*	*(0.269)*	*(0.278)*
At or Above MW in 2010	0.051	0.066	0.052
(Standard deviation)	*(0.385)*	*(0.317)*	*(0.374)*
Change in earnings, 2010-2011	0.099	0.098	0.090

Table 7: Regression discontinuity estimates of the effect of minimum wage enforcement on wages for those who were employed full-time in the private sector in 2010 and remained full-time private sector employees in 2011

	Parametric			Non-parametric		
Bandwidth (+/-)	(+/- 0.10) (1)	(+/- 0.15) (2)	Polynomial (5th order) (3)	(+/- 0.15) (4)	(+/- 0.21) (5)	(+/- 0.41) (6)
RD effect	0.085*	0.101**	0.116**	0.095**	0.088**	0.05
	(0.051)	(0.043)	(0.045)	(0.046)	(0.041)	(0.032)
R^2	0.032	0.029	0.2	--	--	--
Number of Observations	428	680	2015	--	--	--

Notes: [1.] The optimal bandwidths were chosen based on the technique described in Imbens and Kalyanaraman (2009).

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level. Standard errors are shown in parentheses.

Table 8: Regression discontinuity estimates of the effect of minimum wage enforcement on wages for those who were employed in the private sector in 2010 and remained private sector employees or became public sector employees in 2011

	Parametric			Non-parametric		
Bandwidth (+/-)	(+/-.10)	(+/-.15)	Polynomial (5th order)	(+/-.15)	(+/-.21)	(+/-.41)
RD effect	0.055	0.077*	0.100**	0.095**	0.088**	0.050
	(0.055)	(0.043)	(0.044)	(0.046)	(0.041)	(0.032)
Age	0.000	0.000	0.000	-0.175	0.414	0.406
	(0.001)	(0.001)	(0.001)	(1.916)	(1.700)	(1.282)
Sex	-0.083*	-0.071**	0.049**	0.209***	0.174***	0.081*
	(0.045)	(0.029)	(0.020)	(0.068)	(0.060)	(0.046)
Years of Education	0.015***	0.011***	0.005*	1.015*	0.877*	0.219
	(0.005)	(0.004)	(0.003)	(0.574)	(0.519)	(0.411)
Small firm	-0.002	0.004	-0.062***	-0.081	-0.081*	-0.056
	(0.047)	(0.031)	(0.021)	(0.052)	(0.047)	(0.038)
Union Membership	0.093*	0.125***	0.141***	0.051	0.058	0.038
	(0.052)	(0.041)	(0.028)	(0.046)	(0.040)	(0.031)
Industry Dummies	YES	YES	YES	YES	YES	YES
R^2	0.128	0.078	0.231	--	--	--
Number of Observations	428	680	2,015	--	--	--

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level. Standard errors are shown in parentheses. Regressions include 6 industry covariates; agriculture, commerce, finance, transportation, services and manufacturing.

Table 9: For different groups: regression discontinuity estimates of the effect of minimum wage enforcement on wages for those who were employed in the private sector in 2010 and remained private sector employees or became public sector employees in 2011

	Parametric			Non-parametric		
Bandwidth (+/-)	(+/-.10)	(+/-.15)	Polynomial (5th order)	(+/-.15)	(+/-.21)	(+/-.41)
Female	0.208*	0.230**	0.272***	0.212*	0.209**	0.136**
	(0.123)	(0.102)	(0.101)	(0.110)	(0.098)	(0.068)
Male	0.053	0.061	0.086	0.063	0.053	0.023
	(0.059)	(0.050)	(0.054)	(0.053)	(0.047)	(0.037)
Youth (15-24 years old)	0.181*	0.128	0.157*	0.156*	0.139*	0.091
	(0.100)	(0.080)	(0.092)	(0.088)	(0.077)	(0.057)
25 years and older	0.057	0.0943*	0.113**	0.075	0.073	0.036
	(0.059)	(0.050)	(0.053)	(0.054)	(0.048)	(0.037)
Secondary graduate	0.258*	0.181	0.221**	0.215	0.203*	0.120
	(0.148)	(0.115)	(0.105)	(0.135)	(0.114)	(0.074)
Less than secondary graduate	0.033	0.069	0.0895*	0.054	0.047	0.021
	(0.052)	(0.045)	(0.051)	(0.047)	(0.042)	(0.035)

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level. Standard errors are shown in parentheses.

Table 10: Placebo/Falsification Test: Regression discontinuity estimates of the effect of minimum wage enforcement on wages for those who are self-employed in 2011

	Parametric			Non-parametric		
Bandwidth (+/-)	(+/- 0.10)	(+/- 0.15)	Polynomial (5th order)	(+/- 0.29)	(+/- 0.58)	(+/- 1.16)
RD effect	-0.056	-0.015	-0.031	0.017	0.035	-0.059
	(0.341)	(0.276)	(0.205)	(0.215)	(0.159)	(0.131)
R^2	0.021	0.02	0.315	--	--	--
Number of Observations	57	95	502	--	--	--

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level.

Table 11: Regression discontinuity estimates of the effect of minimum wage enforcement on whether full-time private sector employees in 2010 remained employed full-time employees in 2011

	Parametric			Non-parametric		
Bandwidth (+/-)	(+/- 0.10)	(+/- 0.15)	Polynomial (5th order)	(+/- 0.15)	(+/- 0.31)	(+/- 0.62)
RD effect	0.031	0.019	0.036	0.029	0.030	0.008
	(0.056)	(0.048)	(0.046)	(0.049)	(0.038)	(0.029)
R^2	0.001	0.001	0.022	--	--	--
Number of Observations	589	944	2,773	--	--	--

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level.

Table 12: Regression discontinuity estimates of the effect of minimum wage enforcement on whether full-time private sector employees in 2010 remained employed full-time employees in 2011, adding covariates

		Parametric		Non-parametric
Bandwidth (+/-)	(+/-.10)	(+/-.15)	Polynomial (5th order)	(+/-.31)
RD effect	0.005	0.013	0.028	0.030
	(0.05)	(0.05)	(0.04)	(0.038)
Age	0.001	0.000	-0.001	1.495
	(0.00)	(0.00)	(0.00)	(1.252)
Sex	-0.077*	-0.092***	-0.0887***	0.082*
	(0.04)	(0.03)	(0.02)	(0.045)
Years of Education	0.000	-0.003	0.001	0.104
	(0.01)	(0.00)	(0.00)	(0.398)
Small firm	-0.160***	-0.120***	-0.133***	0.030
	(0.05)	(0.03)	(0.02)	(0.042)
Union Membership	0.140***	0.169***	0.123***	0.025
	(0.03)	(0.02)	(0.01)	(0.027)
Industry Dummies	YES	YES	YES	YES
R^2	0.086	0.063	0.08	--
Number of Observations	589	944	2,773	--

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level. Standard errors are shown in parentheses. Regressions include 6 industry covariates; agriculture, commerce, finance, transportation, services and manufacturing.

Table A1: Regression discontinuities in potential covariates

Bandwidth (-/+)	(0.15)	(0.21)	(0.41)
Age	0.779	0.671	0.302
	(0.866)	*(0.769)*	*(0.579)*
Gender (Male=1)	-0.192***	-0.167***	-0.086***
	(0.034)	*(0.030)*	*(0.023)*
Years of Education	0.385**	0.385**	0.385**
	(0.192)	*(0.192)*	*(0.192)*
Small Firm	(0.002)	(0.008)	(0.019)
	-0.032	*-0.029*	*-0.022*
Union Membership	0.013	0.004	0.000
	(0.017)	*(0.014)*	*(0.011)*
Commerce	0.031	0.028	0.006
	(0.029)	*(0.026)*	*(0.020)*
Finance	0.057***	0.053***	0.035***
	(0.021)	*(0.018)*	*(0.014)*
Construction	-0.092***	-0.084***	-0.059***
	(0.017)	*(0.015)*	*(0.011)*
Manufacturing	-0.057**	-0.040**	-0.01
	(0.022)	*(0.020)*	*(0.015)*
Services	0.166***	0.142***	0.072***
	(0.030)	*(0.027)*	*(0.021)*
Transportation	-0.046**	-0.035**	-0.020*
	(0.016)	*(0.015)*	*(0.011)*
Agriculture	-0.060*	-0.061**	-0.023
	(0.031)	*(0.027)*	*(0.020)*

***Indicate statistical significance at the 1% level. **For the 5% level. *For the 10% level. Standard errors are shown in parentheses.