Revisiting the effects of minimum wages in developing countries

Evidence from a particular policy change in Honduras[*]

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Abstract

This paper exploits a recent policy change in Honduras to study the effects of the minimum wage in a dual labor market. Using a large individual data set, I propose using this policy as a natural experiment to obtain estimates of its impact on formal and informal workers' earnings. The main results indicate positive effects for formal workers and find significant evidence of a short-lived lighthouse effect on informal employees. In distributive terms, the minimum wage markedly reduced poverty among the employed since the increase mostly affected low earning formal workers and higher paid informal workers; suggesting that this last group constitutes the beneficiaries of the spillover.

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1   Introduction

Beginning with Stigler (1946), there has been extensive research on how labor markets react to the minimum wage (MW). In a competitive market, the effects of raising the minimum wage are unambiguous: average wages increase and employment falls. However, developing country labor markets such as those in Latin America tend to be more complex (Behrman, 1999). These settings are usually characterized by a labor market with two sectors: a covered formal sector entitled to the MW and an uncovered informal sector which is not. In this dual setting, economic theory dictates that a higher minimum wage increases (decreases) average wages and generates employment losses (gains) in the covered (uncovered) sector.

However, even in these markets, the minimum wage sometimes has unexpected consequences (Neumark and Wascher, 2008). Existing evidence has found several discrepancies between how labor markets are expected to react to changing minimum wages and their actual response. One such instance is referred to as the *lighthouse effect*, whereby raising the minimum has positive spillovers on uncovered workers' earnings (Boeri et al., 2011).

This paper aims to contribute further evidence to the discussion on minimum wages in developing countries by exploiting a policy change implemented in Honduras during 2009. This reform increased the real value of the minimum wage by 75% and completely changed its payment structure. I argue here that this policy was constitutes a viable natural experiment which allows quantifying its impact using difference in difference methods.

To carry out this task, I assembled a unique data set from the *Encuesta Permanente de Hogares de Propósitos Múltiples* (EPHPM) household surveys for 2006-2011. This large individual-level data set is further augmented with official minimum wage values at the industry firm-size level contained in the decrees from the Ministry of Labor and Social Security (STSS) and aggregate macroeconomic information from the Honduran Central Bank (BCH); creating a unique informational source for a sample of workers before and after the policy change.

The main findings are in line with previous evidence which finds positive effects of the minimum wage on both formal and informal workers' earnings and are robust to different definitions of labor informality. Immediately after introduction of the policy, average earnings did not increase significantly more for workers in the formal sector when compared to informal employees. Nonetheless, this result masks a more interesting finding. Observed separately, formal earnings rose by 22.8 percentage points, and informal earnings by 20.1 p.p.; implying a significant lighthouse effect. However, this spillover is short-lived, since

subsequent adjustment of the labor market shows a large reduction in informal earnings in line with the wage predictions from the dual labor market model.

Additional results indicate no differences in impact by gender but do find that some employees benefited more from the policy: mainly middle-aged and skilled formal workers. In distributive terms, the minimum wage increase affected mostly low earning formal workers and higher paid informal workers, suggesting that this last group constitutes the recipients of the lighthouse effect. Furthermore, the policy significantly reduced poverty by almost 17 percentage points.

One interesting implication from these estimates is that while the change in wage floors was substantially large, the benefits of this increase seem to outweigh its costs. For instance, the policy raised the minimum wage from 54% of the basic consumption bundle to 88%, allowing many workers to increase their living standards. Moreover, the policy was equity-enhancing since it mostly benefited salaried workers in the low and middle portions of the earnings distribution. Also, it generated a marked reduction in poverty rates. Finally, descriptive evidence suggests that the number of destroyed jobs is small, since neither formal or informal employment dropped substantially after the new minimum wage was implemented. However, further research in this last direction is necessary to fully determine the policy's scope.

The remainder of this study is organized as follows. Section 2 briefly reviews minimum wage theory. Section 3 surveys the existing empirical literature in Latin America. Section 4 describes the institutional context before and after the 2009 policy and previous minimum wage studies in Honduras. Section 5 presents the data, relevant definitions and outlines the econometric framework used to obtain the results in Section 6. Finally, Section 7 concludes and suggests directions for future research.

2   Theoretical predictions of minimum wage effects

The minimum wage is a labor market institution which grants covered workers a wage floor, effectively setting a lower bound to the wage distribution.[1] Nevertheless, although most countries have a legislated MW, its relevance hinges on certain characteristics (Boeri, 2012). This section briefly reviews the main theoretical frameworks used to study the minimum wage; focusing on imperfect markets, incomplete coverage and heterogeneous

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[1]A summary of other labor market institutions may be found in Blau and Kahn (1999) and Freeman (2009).

workers.[2] Notwithstanding, it is essential to begin with the simplest model.

The traditional model of minimum wage effects is set in a purely competitive labor market, with complete coverage and homogeneous workers (see Figure A.1 in the Appendix). In this setting, the equilibrium wage (w^*) is the marginal product paid to each identical employee. The labor market regulator chooses the minimum wage (w^{min}) below, equal to or above the equilibrium wage. In the first and second cases, the MW would not have an impact on the wage distribution or employment, since equilibrium would lead to market clearing at w^*. However, if the wage floor is binding ($w^{min} > w^*$) there will be a level of employment loss (to E^{min}) which depends on the elasticity of labor demand. In particular, unemployment will rise because of a displacement effect: workers whose marginal product is below w^{min} lose their jobs. Additionally, the rise in pay attracts new workers raising labor supply. Since this is a one sector model, all individuals who lose their jobs or enter the labor market are relegated to unemployment. Therefore, in this case, the effects of introducing a higher MW are unambiguous: average wages increase and employment falls.

However, most labor markets are not this simplistic. In reality, perfect competition is seldom achieved, coverage is far from complete and workers are different. These attributes change the nature of the previous model and its predictions.

First, consider a departure from the competitive setting.[3] One of the widely studied cases in labor economics is when employers are pure monopsonists (Dickens et al., 1999, Manning, 2003a,b). These firms maximize profits by choosing the wages they pay at the point where marginal revenue equals marginal cost. As Figure A.2 in the Appendix shows, this leads to a wage (w^{nc}) and employment level (E^{nc}) which are lower than the competitive equilibrium under the basic model.

In this situation, there are a number of potential effects of the MW. For instance, if $w^{nc} < w^{min} < w^*$ (as in the Figure), then a binding wage floor will have a positive effect on both wages and employment. Nonetheless, if the minimum is set higher than the non-competitive and equilibrium wages, then employment losses are expected. In fact, if the minimum is raised high enough, the level of employment under the MW might be lower than in an unregulated pure monopsony. Nevertheless, this model suggests that negative employment effects are not the only potential outcome of minimum wage hikes.

Another source of ambiguity surfaces when there is incomplete coverage. The usual

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[2]A more extensive treatment of the concepts described here may be found in Fields (1990), Brown (1999), Manning (2003a) and Boeri and van Ours (2008).

[3]Non-competitive labor markets have sparked substantial theoretical interest, as shown by the special volume of the Journal of Labor Economics edited by Alan Manning in 2010.

case corresponds to a dual labor market where one fraction of the workforce is covered by labor market institutions while another remains uncovered. Perhaps the most widely applied conceptual framework in this context is that by Harris and Todaro (1970).

This model begins by clearly identifying the two types of workers: covered or formal and uncovered or informal.[4] Since the MW only applies to the first sector, it is best to begin the analysis there. Upon setting a binding minimum, wages for covered workers increase and a number of them lose their jobs (similar to the competitive case). These displaced workers are faced with a choice: they may either queue in unemployment for another formal job or migrate to the informal sector. Those who decide to move increase labor supply in the informal job market, lowering the equilibrium wage in that sector. In summary, a higher minimum wage would increase (decrease) wages and generate employment losses (gains) in the covered (uncovered) sector as Figure A.3 in the Appendix shows.

Finally, it is important to address worker heterogeneity. In the models surveyed in this section, the unspoken assumption has been that workers are identical and that minimum wage effects are applicable only to some employees, usually low earning, unskilled or younger workers. However, this may not be the case since some countries use the MW as a benchmark or numeraire to set other wages. Hence, the overall effect of the MW is a balance of sorts between the impact on direct recipients and spillovers to those whose outcomes are indirectly affected (Brown, 1999).

These three aspects, *noncompetitive markets, incomplete coverage and worker heterogeneity*, are distinguishing characteristics of developing country labor markets (Behrman, 1999). From the above discussion, it becomes unclear how to carry out predictions without a detailed background on the particular case being studied. For this purpose, it is essential to review the existing evidence, both at the regional and country level.

3   Literature Review

The theoretical framework in the previous section outlined a number of potential effects of the minimum wage directly tied to the labor market under study. This uncertainty has been confirmed in empirical research; which has found positive, negative and even no effect of the MW on wages and employment as Brown (1999), Lemos (2007) and Neumark and Wascher (2008) find in their comprehensive reviews.

The shorter review here highlights some of the most recent work on minimum wages

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[4]I use covered/formal and uncovered/informal interchangeably throughout the remainder of the paper.

in Latin America focusing on the aspects mentioned in the end of Section 2. In general, the empirical literature on MWs in Latin America may be separated into two strands: descriptive cross-country analyses and individual case studies.

The first group includes contributions from Márquez and Pagés (1998), Maloney and Mendez (2004) and Boeri et al. (2008), who analyze regional trends in MWs and present a number of findings. First, they emphasize that contrary to developed nations, there tend to be multiple minimum wages in Latin America. Second, they note that while its relevance varies by country, between 10-30% of salaried workers bunch around the legislated minimum.[5] Additionally, wage floors represent approximately 20-60% of average wages and between 30-90% of the median. This last point suggests a highly binding MW in the region, and implies that changing wage floors should be expected to have a strong impact on labor market outcomes.[6]

Additionally, these studies take particular note of the region's growing informal sector. Today, Latin American countries have an estimated level of labor informality between 30-75 percent of all workers according to Gasparini and Tornarolli (2009), a fraction which has been increasing in the past twenty years.[7] In fact, some of the previous research argues that the binding nature of the MW and other labor market frictions may be a determinant of this rise in informality (Carneiro, 2002, Cox Edwards, 2007, Comola and Mello, 2011).

These generalized findings have influenced the second strand of studies which aim to estimate the causal effect of the MW in specific countries. In this direction, the most documented case is Brazil, with contributions by Neri et al. (2000), Fajnzylber (2001), Carneiro (2002) and Lemos (2009). The consensus among these studies is that the MW leads to large wage increases and modest employment losses while also serving as a tool for redistribution.

It was this line of work that initially found evidence of the *lighthouse effect*, whereby informal workers' wages are positively affected by minimum wage increases. Since then, this result has also been noted in other contexts and has led to an increased interest in this somewhat 'unexpected' consequence (Boeri et al., 2011). Among the studies which observe this phenomenon, they explain this result by: the broad redistributive nature of the MW (Lustig and McLeod, 1997), the linkages of labor and goods markets (Fiszbein,

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[5]The bandwidth is 0.80-1.2 times the minimum wage.

[6]In fact, minimum wage effects in the Latin American context may be considered a lower bound since enforcement is low (Grimshaw and Miozzo, 2003), and any increase in policing is expected to further enlarge their effects (Almeida and Carneiro, 2009).

[7]These estimates correspond to a social security definition, which measures whether workers have employer provided retirement coverage.

1992), the use of the MW as a benchmark (Maloney and Mendez, 2004) and heterogeneity of workers within sectors (Fields, 1990).

Other important case studies include Mexico and Colombia. Bell (1997), for instance, finds no effect of MWs in the first country, mostly due to the eroding nature of the wage floor since the 1980s. Ensuing studies have since found evidence of a modest employment loss in Mexico (Villareal and Samaniego, 1997, Feliciano, 1998, Kaplan and Pérez Arce Novaro, 2006), but also attribute the small effect to the same reason as Bell. More recently, Bosch and Manacorda (2010) find that the most important consequence of MWs in Mexico was its contribution to increase earnings inequality. For Colombia, Bell also finds positive effects on wages and negative employment effects ranging between 2-12 percent. These results are confirmed in more recent studies (Maloney and Mendez, 2004, Arango and Pachón, 2007) who find a substantial effect on wages, their distribution and employment in Colombia.[8]

The available evidence also includes studies for Chile, Costa Rica and Nicaragua. For the first, Corbo (1981), Paldam and Riveros (1987) and Montenegro and Pagés (2004) all find large and significant effects of the minimum wage in line with theoretical predictions of the dual sector model. However, these studies find no evidence of lighthouse effects in Chile. This result is also found in Costa Rica and Nicaragua, in studies by Gindling and Terrell (2005, 2007) and Alaniz et al. (2011), respectively.

Complementary to these findings, most of these studies have also addressed heterogeneous impact of the MW by assessing the distributional impact on earnings and welfare (Kristensen and Cunningham, 2006). In general, most of these studies find that minimum wages compress the earnings distribution, lowering inequality and reducing the proportion of the working poor. However, some researchers argue that wage floors are not the most efficient way to promote redistribution from a cost-benefit perspective (Lustig and McLeod, 1997) and as mentioned, there is some evidence that MWs actually increase inequality. Hence, in Latin America it seems that the minimum wage does not only affect the lowest earning workers, but has broader effects.

In summary, existing evidence suggests that Latin American labor markets are prone to larger effects from increases in the minimum wage, since they tend to be more binding. In turn, this effect may be exacerbated by the high level of informality in the region, reflecting the relevance of the dual labor market model as a starting point. Finally, there seems to be a somewhat instilled notion of positive effects on wages and their distribution for formal workers (and varied effects for informal workers). The sign and magnitude of all these

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[8]In comparison to the US, the effects are twice as large.

effects will depend not only on the labor market, but its institutions and the dynamics between formal and informal sectors, which tend to be overlooked in most minimum wage studies as Boeri (2012) notes. For this purpose, it is ideal to fully describe the Honduran case to pave the way for the ensuing analysis.

4   Background

4.1   Context and institutional setting

In Honduras, the minimum wage was first instituted in 1971 and has since become a standard component of labor legislation under the responsibility of the General Directorate of Wages (DGS, in Spanish).[9] Since then, adjustments to the minimum have usually been set on a yearly basis through a tripartite commission involving government, business and worker representatives. However, negotiations generally reach an impasse due to inability of agreement between the three parties. In such cases, the law contemplates that a final decision must be taken by the executive branch of the government. For example, 5 of the last 7 increases have been unilaterally determined.[10]

The MW in Honduras has several distinctive characteristics. First, there are multiple minimum wages. In fact, as many as 36 different wage floors have co-existed. Usually, these have varied by industry, region and firm size. Second, the minimum applies legally to formal salaried employees in the private sector, although a number of other wages use the MW as a benchmark (e.g. doctors, teachers and most public employees). Third, they represent a large fraction of average earnings. Previous estimates indicate that the ratio of the MW to average earnings is no less than 0.4; and that even while this ratio fell somewhat during the 1990s it has not suffered significant erosion (Gindling and Terrell, 2009). Finally, the average yearly increase throughout the past ten years has been on par with the inflation rate (reaching at most 12%), implying that adjustments are not typically large.

These attributes remained mostly unchanged until 2009. In this year, setting the minimum wage was far from business as usual. Upon a month of talks between the three negotiating parties with no agreement, the new minimum wage was established unilaterally by president Manuel Zelaya in a controversial political move. In fact, some researchers

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[9]The minimum wage and the DGS were created by Decree 103, published on April 30th in *La Gaceta*, the official newspaper of the Honduran government.

[10]This includes the 2012 increase, set on January 17th of that year by decree STSS-001-2012.

suggest that this policy was one of the determining factors which led to a political crisis in June of 2009, where the president was at the center of the first recorded *coup d'état* in Latin America in over thirty years.[11]

The main attribute of the 2009 minimum wage increase was its magnitude. Monthly earnings were to rise 75 percent in real terms, more than five times the highest average increase in the previous decade. However, it should be noted that while the increase was large, it actually took the purchasing power of the minimum from 54% of the basic consumption bundle to 88% (Cordero, 2009). In dollar terms, this elevated the MW from approximately US$ 204 to 339 a month (in 2005 purchasing power parity terms). To put this into perspective, consider that Honduras is the third poorest country in Latin America and average incomes were approximately US$ 283 in 2008.

Secondly, president Zelaya also modified the structure of the minimum wage, simplifying the system from multiple values by industry and firm size (approximately 22 at the time) to two regional floors (urban and rural). Finally, its implementation was immediate; the announcement was made official on December 28th, 2008 and became effective on the first day of the new calendar year.

Notably, even after president Zelaya had been removed from office, the minimum wage policy remained unchanged. Analysts believe that this occurred to avoid large political costs for the interim government and the successive elected officials from reneging the increase. Therefore, after this one time event, MW setting in Honduras returned to normal in terms of negotiation, the magnitude of the increases (tied to inflation) and its industry-specific structure.

In particular, this background places the 2009 MW policy change as an exogenous shock to the labor market due to its unexpected nature, its rapid implementation and most importantly, the magnitude and scope of its proposed changes. In Section 6, I provide evidence that this assumption holds for the available data.

4.2 Previous minimum wage studies in Honduras

To my knowledge, two studies have analyzed MWs and their relationship to labor market outcomes in Honduras. The first, Gindling and Terrell (2009), is mainly focused on obtaining estimates of the elasticity of earnings and employment to the MW using industry-level

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[11]The full range of political implications of this policy and other government actions, however, lie beyond the scope of this paper which only aims to quantify the economic effects of the measure. For more on the political crisis, see Aguilar (2009) and Dominguez Avila (2009).

panel data for 1990-2004. The study takes full account of the described context, controlling for the existence of multiple minimums and the formal/informal divide by observing differences between small firms which they define as informal and medium-large firms which are taken as the formal sector. Their main results provide elasticity estimates of earnings and employment to the MW of 0.29 and -0.46, respectively.

An important discussion in their study regards the results for covered and uncovered employees. In particular, the elasticities mentioned above apply to private salaried workers in medium and large firms.[12] For robustness, a number of definitions of the uncovered sector are tested, using salaried workers in small firms and self-employed workers as control groups, for which they find no effects. They do find spillovers on public sector employees, but as mentioned previously, those wages are indexed to the MW.

One fairly substantial limitation of the Gindling and Terrell strategy is their definition of covered/uncovered, which may not actually capture the distinction between formal and informal sectors. Particularly, the view taken by Gindling and Terrell is associated to the so-called *productive* definition of informality, which uses firm size as a proxy for an individual's job quality. However, although this definition has been used extensively in the past, it is now widely agreed to be both theoretically weak and empirically difficult to implement (Gasparini and Tornarolli, 2009).

As an alternative, a *social security* definition has been favored in more recent work. Although not without its problems (mostly empirical), the notion that a formal job is one where the worker has labor protection or social benefits from their employment seems more robust.[13] Unfortunately, the surveys used by these authors had no way to implement this approach, since workers were only asked about employer mandated benefits beginning in 2005.

The second available study for Honduras was also carried out by the same authors, with the difference that the main results concern the distributional impact of the MW (Gindling and Terrell, 2010). Specifically, the authors focus on two outcomes: poverty and inequality. Their findings indicate that MWs moderately reduce inequality and poverty. For the latter the elasticity to the MW is approximately -0.22. They explain that this modest effect emanates from the low percentage of poor workers in the covered sector,

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[12]The distinction between large and small firms used by the authors is the official definition of the Honduran government, which classifies firms as small if they employ 15 full-time workers or less. Large firms are those which employ at least 16 full-time workers.

[13]The main empirical issue is that most household surveys do not collect the necessary information to classify workers using a social security criterion. See Gasparini and Tornarolli (2009) for more on this matter.

while there is no evidence of effects on better-off workers.

Therefore, there remain unaddressed issues with respect to the MW in Honduras. First, at the time of writing, there remain no studies which assess the impact of the 2009 MW increase. Second, while the panel nature of the Gindling and Terrell data is an asset in many ways, it also implies a loss of individual level information. Finally, and most importantly, the definition of labor informality used in previous work is far from ideal. In the Honduran context, this takes more relevance since firm size is one of the factors used to set the MW. Overlooking the correlation of the productive definition with MW setting and other unobservable characteristics may be cause for significant bias. In this study, I aim to contribute in these areas and outline other directions for future research.

5 Data and Empirical strategy

5.1 Data and definitions

For this study, I constructed an augmented data set based on individual microdata from the *Encuesta Permanente de Hogares de Propósitos Múltiples* (EPHPM). The EPHPM is a nationally representative household survey conducted twice a year (in May and September) by the National Statistics Institute; and contains information on demographics, education, employment and income for the Honduran population.[14]

From this source, I assemble a pool of repeated cross-sections which encompasses three pre-policy periods (2006, 2007 and 2008) and the same number of post-policy periods (2009, 2010 and 2011). To ensure maximum comparability, all variables were defined identically in all surveys.[15]

Also for sake of comparability, I use the May data for each year, except in 2008 where I use the September round. In particular, the May 2008 survey represents an outlier, since it overestimates incomes and differs from the previous and following surveys in other important characteristics.[16] Moreover, since the surveys have not changed dramatically and the estimates all include time effects, the results will not be substantially affected. Furthermore, this decision also provides advantages since I am able to measure outcomes in September 2008 and May 2009; which aids in isolating the immediate effect of the

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[14]For a brief history of household surveys in Honduras, see Ham (2011).

[15]This is usually a problem when survey instruments are undergoing continual changes. However, the EPHPM has remained fairly stable with only minor changes since 2001.

[16]A comparative analysis between the May and September 2008 survey is available upon request.

minimum wage policy.

This survey data is further augmented with two additional sources of information. On the one hand, using the official MW tables in the decrees from the Ministry of Labor and Social Security (STSS), I impute the corresponding industry firm-size minimum wage for each worker using ISIC industry codes and reported firm size. On the other hand, macroeconomic data on prices and industry GDP from the Honduran Central Bank (BCH) is included for each survey year (in the case of prices, I use the monthly consumer price index for the month of data recollection).

Table 1 lists the surveys and valid MW decrees at the time of fieldwork. During the observed period, most changes in legislation became effective on January 1st of the respective calendar year, except during 2010, where the respective increase became valid in September. Hence, in the data, the 2009 scheme was still applicable at the time when fieldwork for the 2010 survey was undertaken.

To isolate the effect of the 2009 minimum wage policy on workers, I focus on adult salaried workers (aged 25-65) living in urban areas.[17] The following are excluded from the sample: *maquila* workers and public employees who are under a different wage scheme and employees who belong to a union which in Honduras represent a small fraction of private-sector salaried workers (approximately 0.84%). Finally, I do not consider workers who earn more than three times the MW, roughly corresponding to the 90th percentile in each industry firm-size's distribution.

This leaves $N = 16,848$ individuals with complete data on earnings, demographics and employment characteristics for 2006-2011; which I classify as either formal or informal using a social security criterion. In particular, the definitions exploit a survey question that asks salaried workers what employer mandated benefits they receive. These benefits are listed in Table A.1 in the Appendix.

I consider several definitions based on these responses. First, I identify formal workers as those who are entitled to at least one of the listed benefits and informal workers as those who do not receive any. However, while not lawfully required, it may be likely that informal jobs grant some of these benefits (e.g. vacations or bonuses). So, I use an alternative definition that considers formal workers as those employees that are entitled to *pension benefits, severance pay, overtime pay and accident insurance.*[18]

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[17]Rural areas are omitted because the proportion of salaried workers is low, approximately 32% over the period. Moreover, enforcement tends to be much less strict in this sector than in urban or metropolitan areas.

[18]Naturally, these benefits are not mutually exclusive. So it is possible that a worker with right to

As an additional robustness check I also employ a third definition, based on whether individuals are employed under contract. Note however, that this last definition is correlated with formality but does not necessarily capture the same concept. One issue is that since the survey is conducted on individual workers and not firms, the employee may not be certain whether the employer is accountable for these contracts, which is the key idea behind defining formality in this manner (Henley et al., 2009). Hence, there is no way to ensure that these contracts are enforceable. Additionally, having a written contract is not a guarantee of receiving social protection or employer benefits, since there may be formal workers with no contract and informal workers with contracts. In fact, before the policy, 23% of formal workers did not have a contract while 11.5% of informal workers did. Nonetheless, despite these problems, this definition remains preferable to the productive definition of informality.

Finally, a natural caveat with household surveys is that earnings are self-reported and thus subject to measurement error and top coding, which has led researchers to prefer administrative data. However, in Honduras, administrative records would not be helpful to the purposes of this study since most informal employment is unregistered. Top coding is not a significant issue here since the aim of this study is to observe workers in the lower and middle percentiles of the earnings distribution. Finally, measurement error in the outcome does not affect the consistency of the estimates but would lower precision. In the case that this error may be large, however, the estimates presented here may be considered as lower bound effects.

5.2   Empirical Strategy

The main identification strategy of this study is that the 2009 minimum wage increase in Honduras represents a natural experiment in labor policy with formal workers constituting the treatment group and informal workers as the control group.

While not the ideal experimental conditions since assignment to formal and informal jobs is non-random and the groups are markedly different (Maloney, 1998, 2004); the key assumption is that in absence of the policy the outcome trends between formal and informal workers would evolve similarly. For this purpose, the estimation framework presented here directly incorporates a way to test this assumption.

In particular, I estimate the earnings effect of the 2009 new minimum wage policy

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vacations or bonuses also receives other benefits. Therefore, for the second definition I reclassify as informal those workers who receive vacation rights and bonuses but no additional benefits.

(NMW for short) using the following linear reduced form:

$$y_{ijt} = \alpha F_{ijt} + \beta(F_{ijt} \times \delta_t) + \delta_t + \mu_j + \eta MW_{jt} + \gamma X_{ijt} + u_{ijt} \tag{1}$$

where y_{ijt} represents the logarithm of real monthly earnings for individual i employed in industry firm-size j at time t. F_{ijt} is an indicator function which identifies whether the worker is formal and thus α controls for pre-existing differences between treatment and control groups. δ_t are binary indicators for each time period which capture survey specific aggregate trends (2006 is the omitted year). The main parameter of interest, β, accompanies the interaction between the treatment indicator and the year dummies; and provides an estimate of the effect of minimum wage changes on formal workers' earnings. In particular, I prefer using this strategy as opposed to a pre/post framework since it provides a direct test for the validity of the estimates by simultaneously testing for parallel trends between the groups prior to the NMW policy.[19]

Additionally, Equation (1) also includes fixed effects for each industry-firm size cell (μ_j) and the logarithm of the real minimum wage (MW_{jt}) to capture the elasticity of earnings to changes in the wage floor. To control for other differences and increase precision I also include a matrix of individual covariates X_{ijt} which contains a constant, a binary indicator if the worker is male, years of education, labor market experience (and its square) as well as a dummy variable equal to unity if the worker resides in a metropolitan area. Finally, u_{ijt} is a mean zero error term assumed to be independent of all right hand side variables.

Moreover, since the analysis also aims to look at effects for different worker types, a natural extension of Equation (1) to account for heterogeneous effects of the NMW policy for a worker belonging to a category g of a particular group is:

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\begin{aligned}
y_{ijt} =& \alpha F_{ijt} + \phi(F_{ijt} \times \kappa_g) + \theta(post \times \kappa_g) + \lambda^g(F_{ijt} \times post \times \kappa_g) \\
& + \kappa_g + \psi post + \delta_t + \mu_j + \eta MW_{jt} + \gamma X_{ijt} + \nu_{ijt}
\end{aligned}
\tag{2}
$$

where for simplicity, I use a pre/post framework since the parallel trends assumption will have already been tested and the number of categories used in some of the groups is large. Here, *post* is an indicator variable identifying the period after exposure (2009-2011), κ_g represents the group categories, which in this study are: gender (male indicator), age

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[19]Specifically, this approach conducts placebo regressions directly in the main estimates. To test the parallel trends assumption, I look at the individual β's for 2007 and 2008 and F-statistics testing the joint significance of these coefficients in pre-policy years for the different definitions of the treatment.

groups (8 categories), educational level (4 categories) and earnings deciles. Thus, for each of these groups, the coefficient (λ^g) on the triple interaction captures the differential effect of minimum wage changes on formal workers belonging to category g of each grouping. All remaining variables are the same as in Equation (1).

In what follows, I estimate both equations by OLS on the selected sample using the provided population weights in each survey. The standard errors from the regressions are corrected for heteroskedasticity and are clustered by j since minimum wage setting in Honduras varies by industry firm-size. This adjustment prevents bias from serial correlation and provides more efficient estimators (Bertrand et al., 2004, Donald and Lang, 2007).

6   Results

6.1   Testing the identification assumption

To test the identification strategy, I provide evidence that the policy change is most likely driving the observed changes and justify the choice of treatment and control groups by analyzing MW coverage and conducting control experiments on pre-policy outcomes. If these tests support the main hypothesis, then the methods outlined in the previous Section will capture the causal effect of the 2009 NMW policy on the selected outcomes.

The first caveat in this setting is related to the atypical size of the increase. A plausible concern is that extenuating circumstances in Honduras' macroeconomic performance led to the implementation of the policy. However, aggregate indicators from the Central Bank and labor market indicators from the EPHPM surveys do not support this hypothesis as Table 2 shows. Honduras grew steadily between 4-6 percent and averaged an inflation rate of 8.2% before the policy. Note that even though inflation in the year prior to the policy was above average (11.4%), the observed increase was similar to behavior during the 1990s where the MW did not change as drastically. Moreover, Cordero (2009) notes that this particular rise was due to unusually high international commodity prices which drove up inflation in Honduras and its neighboring countries.[20]

Another worrisome fact is the 2009 recession, where real GDP fell by 2.1%. Previous evidence suggests this was due to two main factors: the global financial crisis (Fernandez et al., 2009) and the political turmoil related to the coup in June 2009. However, although both are considered to be significant to the downturn, there remains no decomposition of

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[20]The inflation rate increased 28% in Costa Rica, 12% in El Salvador and 6% in Guatemala.

their effect.

In our current setting, a significant problem would stem from the possibility that financial and political crises affected formal and informal workers differently. However, this seems unlikely since both were exogenous shocks which had a generalized impact. On the one hand, while the financial crisis led to a loss of international funding, Honduran financial institutions remained solid and measures were taken to mitigate its effects with no substantial costs to workers (Cordero, 2009). On the other hand, political instability is not a large concern in the data since for the first year after the policy implementation the survey was conducted before the coup and for the subsequent year, the new democratically elected government was in power. Therefore, these potential confounders would not be expected to affect either group more than the other in the surveys.

Finally, Table 2 shows that labor market indicators present no evidence of structural changes in participation, employment and unemployment rates during the last decade.[21] There is some evidence of an increase in unemployment after the policy, but how much is directly caused by the NMW remains an area for further research.

Did the policy cause a change in individual outcomes? Table 3 presents the evolution of monthly minimum wages, earnings and employment to answer this question. Clearly, the estimates suggest large differences before and after 2009 in average earnings. While monthly pay increased around 4% in real terms during the pre-policy period, earnings grew 27% directly after the policy, about 8 times the pre-policy rate.

This trend is more evident when analyzing the ratios of the MW to mean and median earnings. The calculated ratios show that the MW is particularly binding in Honduras. Specifically, MWs represented 0.71 of average earnings and a slightly higher proportion with respect to median earnings (0.77) before the policy. The introduction of the NMW took these proportions on par with the median. Thus, for an unemployed worker obtaining a minimum wage job, the gain in income was similar to receiving a transfer of the median level of earnings in Honduras. Surprisingly, formal and informal employment does not drop after policy implementation. There seems to be some decrease in the ensuing years, but there seems to be no differing trend between the groups.

While the above evidence is striking, it remains incomplete. In what follows, I answer two remaining issues with respect to the identification strategy. On the one hand, I observe whether formal workers are effectively 'treated' by examining MW coverage between the groups. On the other hand, I test the assumption that informal workers are viable controls.

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[21]The statistics were calculated from the May rounds of the EPHPM for 2001-2011, except for 2008 where estimates correspond to the September round.

First, are formal workers covered and informal workers uncovered? As an initial indicator, Table 4 quantifies the amount of individuals in the sample who earn less than the MW, exactly the minimum and above the MW before and after policy implementation.[22] The results also include mean tests for changes in these proportions in pre and post policy periods.

The findings reveal that approximately 90 percent of formal workers earned at least the minimum wage in the pre-policy period (with 13.5% earning exactly the MW). After the introduction of the NMW, the proportion of formal workers at the minimum almost tripled and a small fraction fell below the new wage floor. Surprisingly, 58% of informal workers earned at least the MW before the policy. After 2009, the proportion at the minimum remained statistically unchanged, with the majority of workers earning above the MW falling to the lowest category and leaving only 12.2% with earnings above the minimum. These findings suggest that minimum wages in Honduras seem to bind for workers in the formal sector, but not for informal employees.

Figure 1 also explores coverage using kernel density plots of the log difference between earnings and the MW, following Gindling and Terrell (2009). Panel 1a presents formal and panel 1b informal earning distributions before and after the policy change. In this setting, complete compliance would be denoted by a truncated distribution at zero (which is set to equal the minimum wage). The results confirm the findings in Table 4 that formal workers tend to earn the MW or higher. As found beforehand, a mass of informal workers was bunched at the minimum before 2009 but there seems to be no evidence of concentration around this value upon the introduction of the NMW.

This coverage analysis presents two relevant findings to keep in consideration. On the one hand, there exist a number of formal workers who earn less than the MW, implying imperfect coverage even within this sector. On the other hand, a non-zero proportion of informal workers earn the MW or higher after the policy change, hinting at the potential for spillovers in this sector.

So far, the evidence suggests that formal workers were affected by the 2009 NMW policy and informal workers were not. A natural idea would be to compare both groups before and after the policy change. However, assignment to formal and informal jobs is far from random since the groups are markedly dissimilar, a fact argued extensively in Maloney (1998, 2004), Perry (2007) and Moreno (2007). In fact, a balance analysis of pre-policy covariates presented in Table 5 finds significant differences in demographic, educational

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[22]Some degree of error is permitted. The ranges are defined as follows: earning less than the minimum [<0.90MW]; exactly the minimum [0.90MW,1.10MW]; and earning more than the minimum [>1.10MW].

and labor characteristics between groups.

Nevertheless, these differences do not rule out the potential of the strategy to identify the effect of the 2009 NMW policy. In order for the proposed identification assumption to hold, the relevant test is for no differential trends in outcomes between the groups prior to the policy. Due to the unique data set constructed for this study which contains information on several pre-policy years, this is a relatively straightforward test.

To begin, Figure 2 plots the trends in earnings by group across the timeframe. It is clear that formal workers earn more than informal workers, and at first glance it seems that the earnings gap is stable before the policy. However, after the introduction of the NMW (denoted by the vertical line), there is a clear break in the trend showing a more than average increase for formal workers. Note, however, that there also seems to be growth in informal earnings, but this gain seems to be reversed in the ensuing years.

While this graph is intuitive, we may formally test the assumption of no differential trends using two-by-two tables and mean tests. Table 6 shows unconditional difference in difference estimates for the logarithm of real monthly earnings between formal and informal workers. Panel A compares years where there was no exposure, Panel B shows results directly after implementation and Panel C shows post-policy means.

The main findings from this exercise are the control experiments in Panel A. These results (difference in differences estimate insignificant and close to zero) provide suggestive evidence that the results in Panels B and C are not driven by inappropriate identification assumptions and that there are no differential trends in earnings between formal and informal workers before the intervention. Thus, panels B and C provide a naive estimate of the policy effect on formal sector earnings.

These calculations indicate that immediately after the introduction of the NMW, average earnings did not increase significantly more for workers in the formal sector when compared to informal employees. Nonetheless, closer inspection of the results in Panel B shows that this masks an interesting finding. Table 6 implies that formal earnings rose by 22.8 percentage points and informal earnings by 20.1 p.p directly after policy implementation. Therefore, informal workers gained almost as much as formal workers in terms of earnings. Since no substantial events occurred between September 2008 and May 2009, we may interpret this as a direct consequence of the MW policy (or in a less optimistic case, as a very precisely estimated upper bound). This result provides evidence of a significant and substantial lighthouse effect. However, the estimates also indicate that this spillover is short-lived, since subsequent adjustment of the labor market shows a large reduction in

informal earnings, in line with the wage predictions from the dual labor market model.

Note that even though the estimates in Table 6 provide a quantification of the effect of the 2009 NMW policy on formal earnings, these simple calculations do not account for other observable differences. This is easily incorporated using the regression framework presented in Section 5 which only requires that there are no significant changes in the covariate distribution within formal and informal groups over time. Table 7 shows that covariates are balanced inside the groups.

6.2   Earning effects of the 2009 NMW Policy

Table 8 presents the regression results for the impact of the 2009 NMW using all three formality definitions and the elasticity coefficient (η).[23] Columns (1), (3) and (5) correspond to unconditional results and the remaining specifications include the covariates mentioned in Section 5, which are henceforth referred to as the full specification.

For the first definition, the control experiments are insignificantly different from zero for all pre-policy years. Moreover, a joint significance test accepts the null hypothesis that formal and informal workers had parallel trends in earnings before the policy. As in the previous calculations, I find no immediate impact after the introduction of the NMW, which is likely driven by a significant lighthouse effect. However, formal employees benefit significantly more after some adjustment has taken place, with the coefficient for program impact showing an increase of 23.1 percentage points with no controls and a somewhat smaller 19.5 p.p. when including covariates. By the end of the observed period, the effects of the policy dissipate, indicating a short-run effect of the wage floor as expected by theory (Neumark and Wascher, 2008). A joint test for the significance of the post-policy coefficients indicates that the policy had a non-zero effect on formal earnings with probability 0.001.

Table 8 also includes the estimated elasticity to the MW. For the full specification, this estimate is 0.367 which is strongly significant and although somewhat higher than previous estimates, seems fairly consistent with the value of 0.29 estimated by Gindling and Terrell (2009).

The implications of these results are twofold. On the one hand, in contexts with high informality, an assessment of the impact of the MW may be confounded because of the presence of significant spillovers. As mentioned beforehand, an upper bound estimate of this lighthouse effects is approximately 20.1 percentage points. Therefore, more refined

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[23]The full difference in differences regression results are available upon request.

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empirical strategies are required to more precisely estimate this parameter. On the other hand, the findings indicate that while spillovers are important, labor markets do eventually adjust. In this case, Table 6 shows that informal workers benefit at first but their earnings decrease sharply one year later. This is consistent with the wage predictions in the dual labor market model, which expect the equilibrium wage to fall in the uncovered sector after a minimum wage hike.

The remainder of Table 8 presents the same results for the two alternative definitions of labor informality. For the first (columns 3-4), I reclassified workers who receive only vacation rights and bonuses as informal, since these may also be granted in uncovered jobs. In general, the main results are mostly unchanged, although the point estimates are somewhat lower. However, all the significance results provide the same conclusions..

The third definition contemplates a formal worker as an individual employed under contract. Previously, I mentioned the potential issues with this measure, which mostly stem from the inability to determine whether these contracts are enforceable. In particular, the estimates seem somewhat similar to the results using the social security criterion, but contain identification problems. First, we cannot reject the existence of differential trends in earnings before implementation, which invalidates any estimates of program impact. Second, while the coefficients of program impact are similar to those from the second definition, a joint significance test cannot reject that the policy had no effect. Therefore, while illustrative, these results suggest that written contracts are not a consistent way to define formality, at least from household surveys.

Another robustness check for the above results comes from identifying workers who receive in-kind payment. According to legislation, employers may pay these individuals up to 20% less than the MW (if they provide housing). Therefore, even though the estimates above use only monetary income as the dependent variable, it may be that surveyed individuals overreport their earnings to include this additional income. In the defined sample, $N = 3223$ workers receive non-pecuniary payment. Table 9 presents regression estimates for the full specification and the three definitions of informality. The findings reinforce the main conclusions drawn from Table 8, although the point estimates are lower (the gain for formal workers is 15.7 percentage points for the preferred definition). Therefore, the earnings gain of formal workers from the policy is between 15.7-19.5 percentage points.

The final three columns of Table 9 address another potential concern. Due to the higher MW, employees may decide to change their labor supply to work more hours and hence, raise their earnings. This might drive the observed effect and bias the previous estimates.

For this reason, I explore whether there was a significant change in the number of hours worked using the full specification. The results for all definitions show that the groups had parallel trends in hours worked before the policy, and find no significant change in labor supply in the intensive margin, confirming that the earnings increase estimated beforehand is associated solely to the policy and not to other behavioral aspects.

6.3 Differential impact of the 2009 NMW policy

In what follows, I use the full specification including in-kind earners to estimate Equation (2) and assess whether there are differential effects on some formal workers. Specifically, I classify individuals by gender, age, education and deciles.[24] The findings indicate no differential impact between males and females, with an estimated coefficient of 0.014 (s.e. 0.03). For ease of presentation, the results for the remaining groupings are shown in Figure 3 which plots the difference in differences estimates and their 95% confidence interval.

First, I divide the sample into eight broad groups separated by 5-year bands. Panel 3a shows that formal workers who are younger (25-34) and those close to retirement (60-65) did not benefit from the policy since their estimated coefficient is not statistically different than zero. From the results, it seems that the observed increase in formal earnings responded to the policy's effect on middle-aged workers (35-54).

Next, panel 3b observes impact across educational levels. Ex ante, we would expect that most workers affected by minimum wages should be unskilled. However, somewhat surprisingly, the observed effect is significant at all levels and actually increases with education. The estimates indicate that the policy effect is 8, 10 and 20 percentage points for formal workers with complete primary, secondary and college; respectively.

Finally, panel 3c presents results across deciles to observe where the policy's beneficiaries are located on the earnings distribution. From the estimates, it is clear that the policy did not have a substantial effect on the lowest and highest earning workers; with most of the positive effect closer to the median. However, all the coefficients are insignificant at the chosen significance level. One potential issue with this framework is that the definition of deciles pools both formal and informal workers and then partitions them into ten income groups. An alternative would be to calculate an individual's ranking respect to their particular group and quantify the growth rate in formal and informal earnings.[25]

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[24]Deciles are defined in terms of the entire distribution (pooling formal and informal workers) in each year.

[25]This is analog to calculating growth incidence curves but using earnings instead of household income. See Ravallion and Chen (2003).

6.4 A closer look at the distributive impact of the 2009 NMW

Figure 4 plots the growth in earnings by percentiles. On average, formal earnings grew about 30.7 percent after the policy and informal earnings by 11.6 percent.[26] However, this was not a uniform increase. In fact, most of the formal sector growth occurred below the median, but there is also evidence of increases for those at the right tail. This last fact responds to two factors. On the one hand, I am observing those who earn less than 3 times the MW, which does not reflect a gain from actual top earners. On the other hand, this may be a direct consequence of the previous finding that skilled formal workers seemed to benefit significantly more from the policy.

In contrast, the informal distribution shows a relatively flat growth profile until the median, where a large earnings increase was experienced by higher earning informal workers. This evidence is suggestive that the observed lighthouse effect may be driven by this particular subset of informal employees. It is a possibility that the jobs and pay for higher earning workers in the uncovered sector may be more akin to low earning formal workers than to informals with low wages. Testing this proposition and characterizing these groups is certainly an interesting and highly relevant direction for future research.

Finally, Table 10 presents estimates of Equation (1) changing the dependent variable to an indicator function equal to 1 if the worker resides in a household classified as poor according to the official poverty lines.[27] In the background section, I mentioned that Honduras is the third poorest country in Latin America. Therefore, it is not surprising that in 2006, approximately 53% of formal workers in the sample lived in a poor household. In turn, 75% of informal employees were in poverty.

The effects of the policy show a marked improvement in these poverty rates, since the proportion of working poor in the formal sector fell by 6.4 percentage points directly after implementation and an additional 11 p.p. one year later. From the baseline estimates, this implies that the fraction of employees living in poor households fell from 53 percent to 36, a substantial drop. The finding is robust to the second alternative definition and shows no effect for the contract definition. Finally, note that the estimated elasticity of the poverty rate to the MW is -0.23; which is almost identical to the estimate of -0.22 presented in

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[26]Note that the difference, 19.1 percentage points, is fairly close to the estimated effect in Table 8 for the full specification.

[27]In particular, there are two measurements of poverty in Honduras: extreme and moderate. Since extreme poverty is mostly rural, I select the second concept which classifies as deprived those households whose per capita income is below an augmented basic goods bundle and includes those in extreme poverty. For more on the definition of poverty in Honduras see Ham (2011).

Gindling and Terrell (2010).

These findings suggest that the policy had a benign distributive effect, since it mostly affected workers at the lower end of the distribution and substantially reduced poverty. If we also recall that the employment trends in Table 3 show that employment did not fall substantially in either sector and that while unemployment increased, its rise was not as large as may be expected; this suggests that the benefits of the 2009 NMW policy seem to outweigh its costs. Nevertheless, to convincingly make this assertion it is key to develop a strategy to quantify the employment effects and perform a more traditional cost-benefit analysis to determine the policy's full scope.

7   Conclusion

This paper contributed further evidence on minimum wages in developing countries by exploiting a policy change implemented in Honduras during 2009. This reform increased the real value of the minimum wage by 75% and completely changed its payment structure. Using a large individual data set, I use this policy change as a natural experiment to obtain estimates of the impact of the minimum wage on formal and informal workers' earnings.

The main results are in line with previous evidence which finds positive effects of the minimum wage on both formal and informal workers' earnings and are robust to different definitions of labor informality and defined samples. Immediately after introduction of the policy, average earnings did not increase significantly more for workers in the formal sector when compared to informal employees. Nonetheless, this result masks a more interesting finding. Observed separately, formal earnings rose by 22.8 percentage points, and informal earnings by 20.1 p.p.; implying a substantial lighthouse effect. However, this spillover is short-lived, since subsequent adjustment of the labor market shows a large reduction in informal earnings in line with the wage predictions from the dual labor market model.

Additional results which examine differential impact of the policy indicate no differences by gender and find that some employees benefited more from the policy, mainly middle-aged and skilled formal workers. In distributive terms, the minimum wage increase affected mostly low earning formal workers and higher paid informal workers, suggesting that this last grouping constitutes the beneficiaries of the lighthouse effect. Furthermore, the fraction of formal employees living in poor households fell from 53 to 36 percent, a substantial drop.

One interesting implication from these estimates is that while the change in wage floors was substantial, the benefits of this increase in Honduras seem to outweigh its costs. For

instance, the policy took the minimum wage from 54% of the basic consumption bundle to 88%, allowing many workers to increase their living standards. Moreover, the policy mostly benefited salaried workers in the low and middle portions of the earnings distribution, improving equity. Also, it generated a marked reduction in poverty rates. Finally, descriptive evidence suggests that the number of destroyed jobs is small, since neither formal or informal employment dropped substantially after the new minimum wage was implemented. However, further research in these directions is necessary to fully determine the full implications of the measure.

Future work in the direction of this study may help pinpoint the workings of labor markets characterized by a high degree of informality. By devising both theoretical and empirical frameworks, we may be able to measure and understand how the minimum wage and other labor market institutions affect this growing segment of the workforce in Latin America. Undoubtedly, a first step is to pursue a strategy to more precisely estimate the lighthouse effect and who benefits from it. In terms of policy, particular findings for each country may be essential to design complementary labor market policies to extend the benefits and counter the trade-offs derived from minimum wage changes.

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Table 1
Data sources and valid MW legislation at time of surveys

EPHPM Survey	Valid MW Decree at Survey	Agreed or set unilaterally?	Effective on:	Number of MWs
May 2006	027-STSS-06	Agreed	January 1, 2006	22
May 2007	STSS-041-07	Set unilaterally	January 1, 2007	22
September 2008	STSS-258-STSS-07	Set unilaterally	January 1, 2008	22
May 2009	STSS-374-STSS-08	Set unilaterally	January 1, 2009	2
May 2010	STSS-374-STSS-08	Set unilaterally	January 1, 2009	2
May 2011	STSS-223-2011	Set unilaterally	January 1, 2011	36

Source: National Statistics Institute (INE) and Ministry of Labor and Social Security (STSS).
Notes: The decrees are publicly available on the DGS website located at:
http://www.trabajo.gob.hn/organizacion/dgt-1/direccion-general-de-salarios/decretos

Table 2
Selected macroeconomic and labor force indicators (2001-2011)

Year	GDP growth (real)	Inflation (in %)	Labor force participation	Employment rate	Unemployment rate	Labor informality
2001	2.7	9.6	68.2	63.6	6.7	n.a.
2002	3.8	7.7	66.5	62.3	6.2	n.a.
2003	4.5	7.7	65.9	60.8	7.7	n.a.
2004	6.2	8.2	65.9	60.7	7.8	n.a.
2005	6.1	8.8	68.4	63.7	6.9	n.a.
2006	6.6	5.6	65.6	62.2	5.3	25.1
2007	6.2	6.9	63.8	61.2	4.1	25.9
2008	4.2	11.4	65.7	63.0	4.1	24.7
2009	-2.1	6.1	65.8	62.6	5.0	26.6
2010	2.8	4.4	66.1	61.8	6.5	27.8
2011	3.6	7.7	64.3	59.8	7.0	29.8

Source: Honduran Central Bank and EPHPM surveys.
Notes: Labor market indicators are for adult workers (25-65). Informality rate is for all salaried workers in the country (public and private). All statistics from EPHPM are weighted.

Table 3
Minimum wage, earnings and employment trends

	Pre			Post		
	2006	2007	2008	2009	2010	2011
Legislated Monthly MW	1,390.02	1,452.75	1,505.10	2,632.84	2,522.94	2,599.52
Monthly Earnings	1,960.53	2,027.75	2,134.62	2,719.87	2,598.73	2,576.66
Formal	2,239.33	2,310.92	2,471.26	3,163.95	3,141.03	2,985.30
Informal	1,518.17	1,612.63	1,564.69	1,919.37	1,666.71	1,817.50
MW/Average earnings	0.71	0.72	0.71	0.97	0.97	1.01
MW/Median earnings	0.77	0.80	0.77	1.00	1.00	1.02
MW/Formal sector earnings	0.62	0.63	0.61	0.83	0.80	0.87
MW/Informal sector earnings	0.92	0.90	0.96	1.37	1.51	1.43
Employment (# workers)	204,902	217,955	244,414	286,477	277,820	207,611
Formal	126,149	130,112	154,640	184,050	174,119	137,034
Informal	78,753	87,843	89,774	102,427	103,701	70,577

Source: Own calculations from EPHPM surveys.
Notes: All monetary values deflated at December 1999 prices using the CPI of the survey month from the BCH.

Table 4
Minimum wage coverage by group (Pre and post policy)

Worker type	Earns:	Pre 2006-2008	Post 2009-2011	Difference
	Less than MW	0.104	0.241	**0.137**
		(0.021)	(0.018)	(0.039)***
Formal	MW	0.135	0.388	**0.253**
		(0.012)	(0.010)	(0.022)***
	More than MW	0.761	0.371	**-0.390**
		(0.021)	(0.017)	(0.038)***
	Less than MW	0.421	0.708	**0.287**
		(0.022)	(0.020)	(0.042)***
Informal	MW	0.139	0.170	**0.030**
		(0.013)	(0.012)	(0.025)
	More than MW	0.439	0.122	**-0.317**
		(0.018)	(0.017)	(0.035)***

Source: Own calculations from EPHPM surveys.
Clustered standard errors at the industry firm-size level in parentheses.
* Significant at 10%; ** 5%; *** 1%
Notes: Mean tests control for industry firm-size fixed effects. Less than MW comprises earnings
in the interval [<0.90MW]; MW between [0.90MW,1.10MW] and More than MW [>1.10MW].

Table 5

Balance in covariates between formal and informal workers

Pre-policy

Variable	Formal N=6,453		Informal N=3,739		Difference
	Mean	(SD)	Mean	(SD)	[T-Statistic]
Males	0.73	(0.49)	0.65	(0.40)	-3.02
Years of education	5.71	(4.03)	7.79	(3.58)	17.54
Experience (in years)	3.04	(5.53)	4.80	(5.25)	7.91
Experience squared (in years)	35.34	(135.35)	54.71	(134.66)	3.99
Lives in Metropolitan Areas	0.38	(0.49)	0.56	(0.47)	11.22
Agriculture, Fishing and Hunting	0.08	(0.18)	0.08	(0.35)	0.00
Mining	0.00	(0.04)	0.00	(0.05)	-0.98
Manufacturing	0.18	(0.42)	0.18	(0.32)	1.09
Utilities	0.00	(0.09)	0.01	(0.06)	0.90
Construction	0.15	(0.19)	0.14	(0.46)	-1.24
Commerce, Hotels and Restaurants	0.27	(0.47)	0.30	(0.41)	1.07
Transport, Storage and Communications	0.08	(0.24)	0.05	(0.25)	-1.14
Banking, Financial and Real Estate Services	0.09	(0.35)	0.11	(0.18)	1.18
Communal, Social and Personal Services	0.15	(0.36)	0.12	(0.30)	-0.99

Source: Own calculations from EPHPM surveys.
Notes: Mean tests control for industry firm-size fixed effects and t-statistics are based on clustered standard errors.

Table 6
Changes in log earnings by group and year

	Log Real Monthly Earnings		
	$t=0$	$t=1$	Difference
Panel A: Pre-policy period			
2006-2007			
Observations	*6,565*		
Formal	7.629	7.640	0.011
	(0.030)	(0.032)	(0.100)
Informal	7.152	7.188	0.036
	(0.050)	(0.039)	(0.056)
Difference	0.477	0.452	**-0.025**
	(0.114)***	(0.056)***	**(0.054)**
2007-2008			
Observations	*6,936*		
Formal	7.702	7.630	-0.072
	(0.026)	(0.015)	(0.042)
Informal	7.249	7.192	-0.058
	(0.031)	(0.028)	(0.019)***
Difference	0.453	0.438	**-0.015**
	(0.085)***	(0.019)***	**(0.028)**
Panel B: Introduction of NMW policy			
2008-2009			
Observations	*7,636*		
Formal	7.666	7.894	0.228
	(0.013)	(0.016)	(0.030)***
Informal	7.257	7.458	0.201
	(0.024)	(0.023)	(0.013)***
Difference	0.409	0.436	**0.027**
	(0.059)***	(0.013)***	**(0.019)**
Panel C: Post-policy adjustment			
2009-2010			
Observations	*5,240*		
Formal	7.884	7.896	0.012
	(0.012)	(0.020)	(0.079)
Informal	7.462	7.287	-0.175
	(0.027)	(0.033)	(0.038)***
Difference	0.422	0.609	**0.187**
	(0.061)***	(0.038)***	**(0.043)***
2010-2011			
Observations	*2,122*		
Formal	7.886	7.850	-0.036
	(0.016)	(0.027)	(0.175)
Informal	7.282	7.390	0.108
	(0.049)	(0.058)	(0.086)
Difference	0.604	0.460	**-0.144**
	(0.107)***	(0.086)***	**(0.091)**

Source: Own calculations from EPHPM surveys.
Clustered standard errors at the industry firm-size level in parentheses.
* Significant at 10%; ** 5%; *** 1%
Notes: All means and differences calculated by regression controlling for industry firm-size fixed effects.

Table 7
Balance in covariates for formal and informal workers across time

	Formal				Informal			
	Pre N=6,453		Post N=3,963		Pre N=3,739		Post N=2,168	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Dependent variable								
Log Real Monthly Earnings	7.68	(0.42)	7.95	(0.45)	7.15	(0.73)	7.26	(0.77)
Covariates								
Males	0.61	(0.49)	0.62	(0.49)	0.80	(0.40)	0.77	(0.42)
Years of education	8.12	(4.03)	9.17	(4.35)	5.17	(3.58)	6.00	(3.79)
Experience (in years)	4.85	(5.53)	5.52	(6.10)	2.96	(5.25)	3.55	(6.06)
Experience squared (in years)	54.11	(135.35)	67.63	(160.58)	36.30	(134.66)	49.37	(178.35)
Lives in Metropolitan Areas	0.59	(0.49)	0.60	(0.49)	0.34	(0.47)	0.33	(0.47)
Agriculture, Fishing and Hunting	0.03	(0.18)	0.03	(0.18)	0.15	(0.35)	0.16	(0.36)
Mining	0.00	(0.04)	0.00	(0.04)	0.00	(0.05)	0.00	(0.07)
Manufacturing	0.22	(0.42)	0.20	(0.40)	0.12	(0.32)	0.14	(0.34)
Utilities	0.01	(0.09)	0.01	(0.09)	0.00	(0.06)	0.00	(0.04)
Construction	0.04	(0.19)	0.02	(0.15)	0.31	(0.46)	0.21	(0.41)
Commerce, Hotels and Restaurants	0.34	(0.47)	0.39	(0.49)	0.22	(0.41)	0.28	(0.45)
Transport, Storage and Communications	0.06	(0.24)	0.07	(0.25)	0.07	(0.25)	0.07	(0.26)
Banking, Financial and Real Estate Services	0.15	(0.35)	0.15	(0.36)	0.04	(0.18)	0.05	(0.21)
Communal, Social and Personal Services	0.15	(0.36)	0.12	(0.32)	0.10	(0.30)	0.09	(0.28)

Source: Own calculations from EPHPM surveys.

Table 8
Earning effects of the 2009 NMW policy in Honduras

	Definition 1		Definition 2		Definition 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Elasticity to the MW	0.476	0.367	0.486	0.372	0.533	0.425
	$(0.123)^{***}$	$(0.101)^{***}$	$(0.116)^{***}$	$(0.093)^{***}$	$(0.124)^{***}$	$(0.096)^{***}$
Panel A: Pre-policy						
2007	0.009	0.018	-0.021	-0.012	-0.007	-0.011
	(0.049)	(0.043)	(0.046)	(0.040)	(0.051)	(0.037)
2008	-0.043	-0.025	-0.088	-0.060	-0.077	-0.080
	(0.043)	(0.042)	(0.051)	(0.046)	$(0.043)^{*}$	$(0.033)^{**}$
Panel B: Introduction of NMW						
2009	0.048	0.047	0.003	0.009	0.088	0.063
	(0.042)	(0.040)	(0.040)	(0.044)	$(0.039)^{**}$	$(0.030)^{**}$
Panel C: Post-Policy Adjustment						
2010	0.231	0.195	0.181	0.139	0.177	0.139
	$(0.051)^{***}$	$(0.047)^{***}$	$(0.059)^{***}$	$(0.053)^{**}$	$(0.076)^{**}$	$(0.057)^{**}$
2011	0.034	0.061	0.009	0.033	0.044	0.032
	(0.082)	(0.077)	(0.064)	(0.056)	(0.083)	(0.073)
Hypothesis tests						
H_0: $\beta^{2007} = \beta^{2008} = 0$						
F-Statistic	1.72	1.00	1.74	1.03	2.42	3.57
p-value	0.209	0.388	0.206	0.378	0.119	0.051
H_0: $\beta^{2009} = \beta^{2010} = \beta^{2011} = 0$						
F-Statistic	8.05	7.15	7.45	6.53	2.05	2.12
p-value	0.001	0.003	0.002	0.004	0.145	0.135
Industry-firm size effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and employment controls	No	Yes	No	Yes	No	Yes
Observations	16,323	16,323	16,323	16,323	16,443	16,443
R^2	0.31	0.40	0.29	0.39	0.28	0.39

Source: Own calculations from EPHPM surveys.
Clustered standard errors at the industry firm-size level in parentheses.
* Significant at 10%; ** 5%; *** 1%
Notes: The specifications correspond to different definition of treatment and control (see Table A.1). In particular, (1) and (2) use the primary definition of formal/informal; (3) and (4) use the restricted definition and (5) and (6) use contract/no contract as treatment and controls, respectively.

Table 9
Additional robustness checks

|	Log Earnings (no inkind earners)			Hours worked		
	Definition 1	Definition 2	Definition 3	Definition 1	Definition 2	Definition 3
Elasticity to the MW	0.365	0.360	0.421	1.661	2.162	2.707
	(0.128)**	(0.123)***	(0.124)***	(2.132)	(2.124)	(2.573)
Panel A: Pre-policy						
2007	0.007	-0.011	-0.021	-0.845	-1.491	-0.494
	(0.051)	(0.043)	(0.039)	(1.078)	(1.231)	(1.428)
2008	-0.033	-0.052	-0.079	0.966	-0.789	-0.569
	(0.044)	(0.045)	(0.034)**	(0.785)	(1.004)	(1.503)
Panel B: Introduction of NMW						
2009	0.033	-0.003	0.056	-0.451	-1.436	-1.515
	(0.044)	(0.040)	(0.032)*	(0.821)	(1.089)	(1.235)
Panel C: Post-Policy Adjustment						
2010	0.157	0.106	0.113	1.114	-0.278	0.040
	(0.043)***	(0.048)**	(0.064)*	(1.413)	-1.254	(1.762)
2011	0.069	0.064	0.029	-0.434	-1.088	-0.262
	(0.089)	(0.063)	(0.081)	(1.191)	(1.374)	(1.983)
Hypothesis tests						
$H_0 : \beta^{2007} = \beta^{2008} = 0$						
F-Statistic	0.89	0.77	3.00	2.12	0.76	0.08
p-value	0.429	0.479	0.077	0.151	0.481	0.921
$H_0 : \beta^{2009} = \beta^{2010} = \beta^{2011} = 0$						
F-Statistic	6.08	4.05	1.31	0.53	0.60	2.00
p-value	0.005	0.024	0.305	0.669	0.621	0.152
Observations	13,182	13,182	13,304	16,171	16,171	16,288
R^2	0.41	0.40	0.40	0.11	0.10	0.10

Source: Own calculations from EPHPM surveys.

Clustered standard errors at the industry firm-size level in parentheses.

* Significant at 10%; ** 5%; *** 1%

Notes: The specifications correspond to the full specification with covariates in Table 8.

Table 10

Poverty effects of the 2009 NMW policy in Honduras

	Definition 1	Definition 2	Definition 3
Elasticity to the MW	-0.230	-0.225	-0.207
	(0.047)***	(0.045)***	(0.052)***
Panel A: Pre-policy			
2007	0.000	0.007	0.009
	(0.026)	(0.022)	(0.024)
2008	-0.009	-0.004	0.008
	(0.029)	(0.025)	(0.031)
Panel B: Introduction of NMW			
2009	-0.064	-0.058	-0.043
	(0.029)**	(0.027)**	(0.026)
Panel C: Post-Policy Adjustment			
2010	-0.110	-0.087	-0.009
	(0.034)***	(0.032)**	(0.038)
2011	-0.043	-0.034	0.009
	(0.040)	(0.044)	(0.041)
Hypothesis tests			
H_0: $\beta^{2007} = \beta^{2008} = 0$			
F-Statistic	0.09	0.21	0.07
p-value	0.917	0.816	0.934
H_0: $\beta^{2009} = \beta^{2010} = \beta^{2011} = 0$			
F-Statistic	4.03	2.77	1.20
p-value	0.025	0.073	0.339
Observations	16,323	16,323	16,443
R^2	0.18	0.18	0.18

Source: Own calculations from EPHPM surveys.

Clustered standard errors at the industry firm-size level in parentheses.

* Significant at 10%; ** 5%; *** 1%

Notes: The specifications correspond to the full specification with covariates in Table 8. In particular, (1) uses the primary definition of formal/informal; (2) the restricted definition and (3) uses contract/no contract as treatment and controls, respectively.

Figure 1
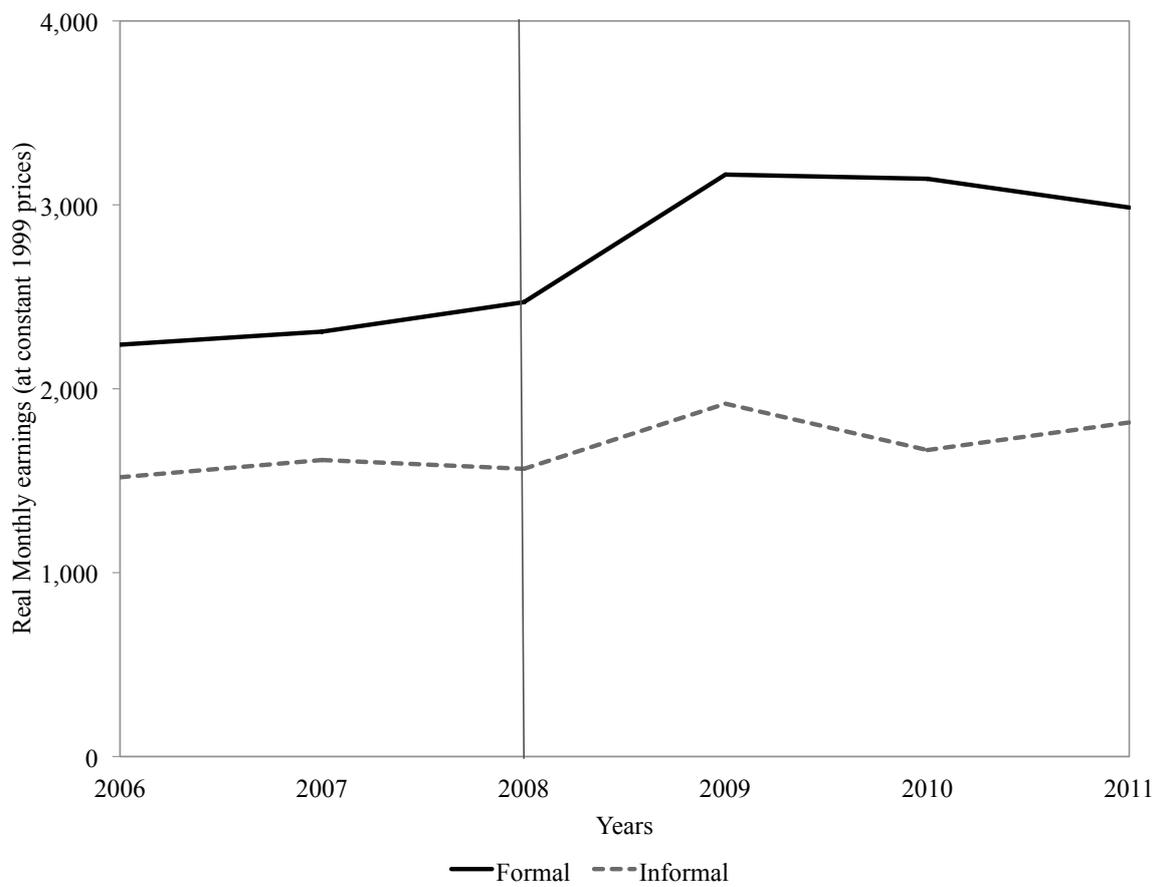**Compliance with the minimum wage: Formal and informal workers**
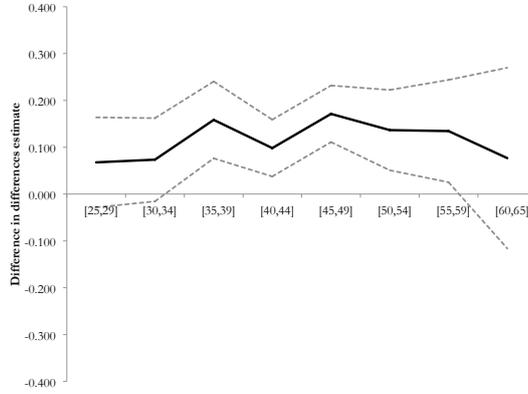

(a) Formal

(b) Informal

Figure 2
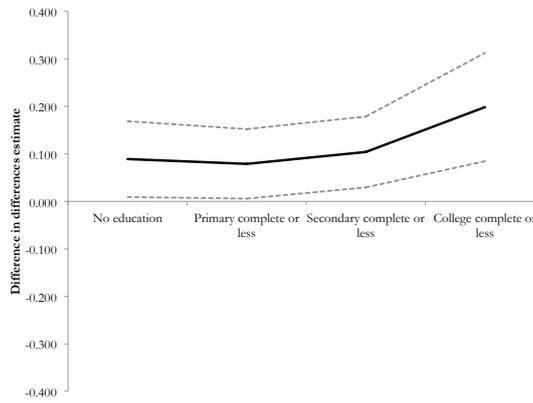**Earning trends for formal and informal workers**
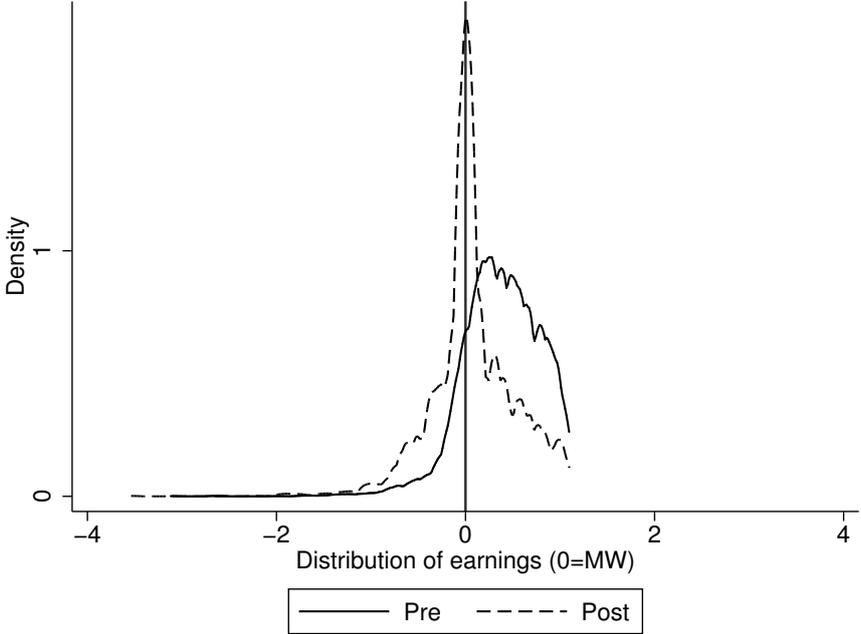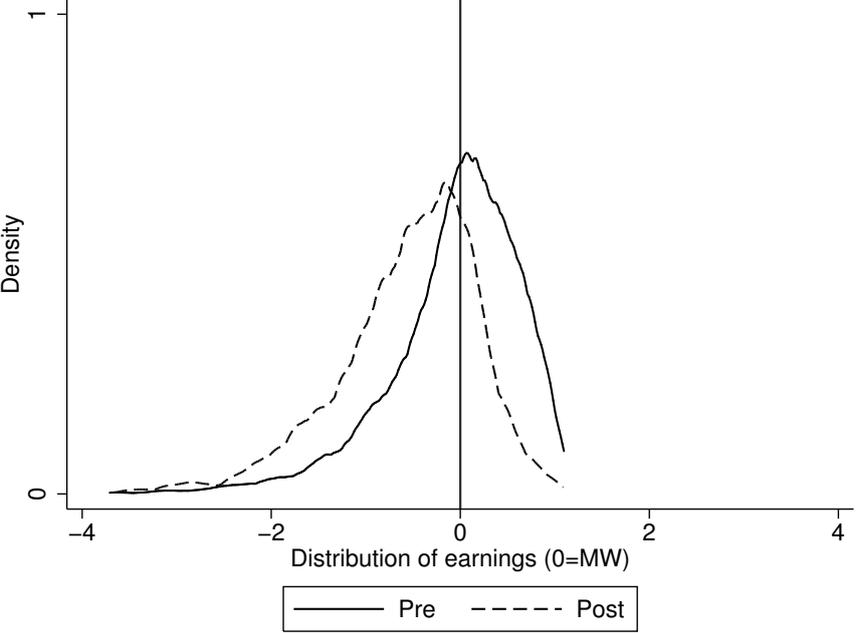
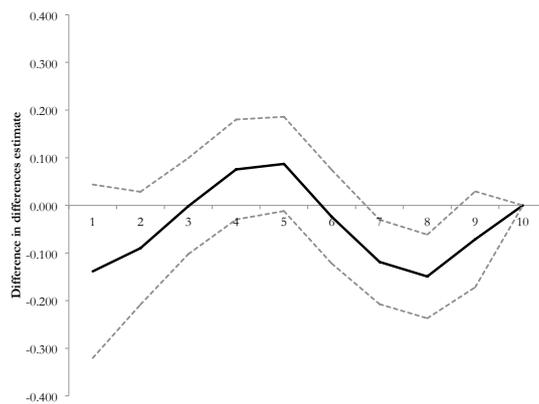Source: Own calculations based on EPHPM data.

Figure 3
Heterogeneous effects of the 2009 NMW policy in Honduras
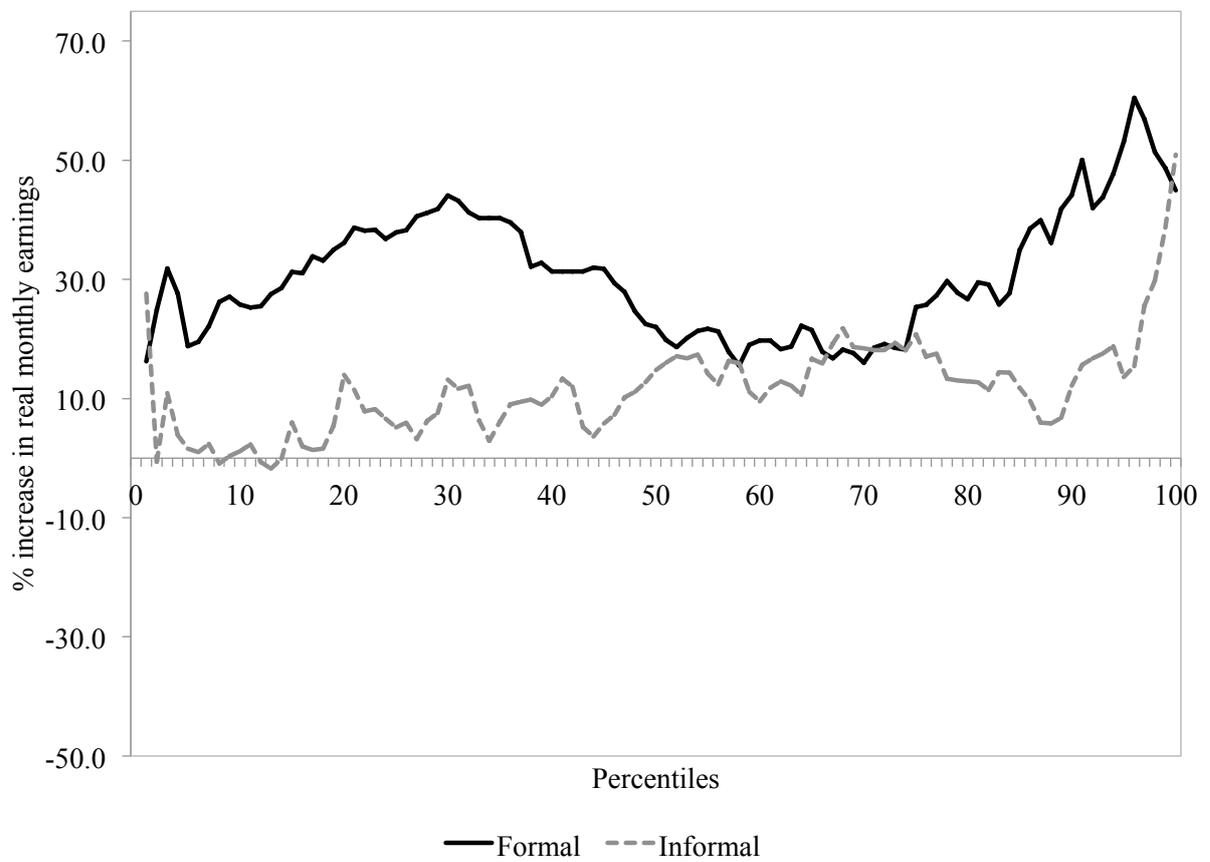

(a) Age groups

(b) Education levels

(c) Earnings deciles

Source: Own calculations based on EPHPM data.
95% confidence intervals in dotted lines

40

Figure 4
**Growth in earnings by percentile**
Pre and post policy

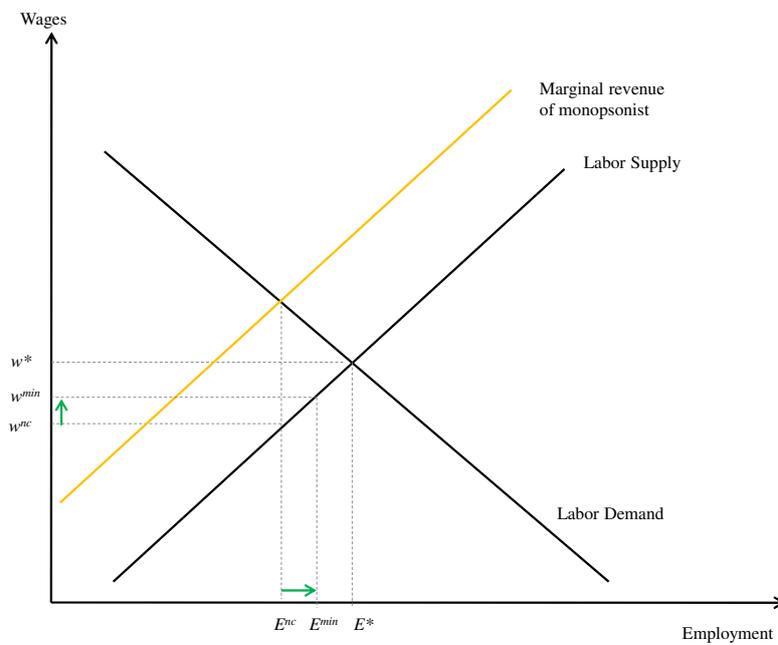Source: Own calculations based on EPHPM data.

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Appendix

Table A.1
Formality definitions

Formality definitions			
Definition 1		Definition 2	Definition 3
Formal		*Formal*	*Formal*
Pension	Accident insurance	Pension	Contract
Severance pay	Christmas Bonus (13th month)	Severance pay	
Paid vacations	Mid-year Bonus (14th month)	Overtime pay	
Overtime pay		Accident insurance	
Informal		*Informal*	*Informal*
No benefits		No benefits	No contract
		Paid vacations	
		Christmas Bonus (13th month)	
		Mid-year Bonus (14th month)	

Figure A.1
Effect of binding MWs in a competitive labor market
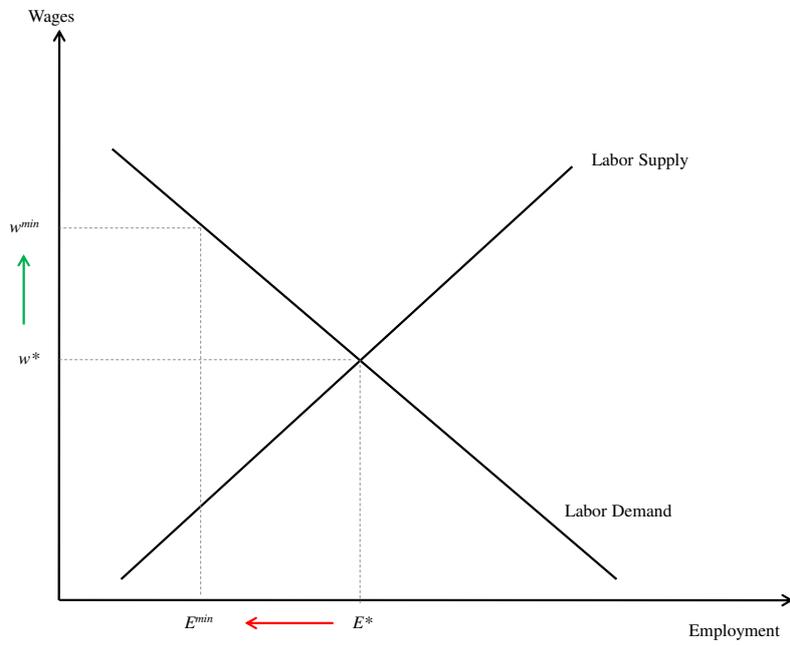
Figure A.2
Effect of binding MWs in a non-competitive labor market

Figure A.3
Effect of binding MWs in a dual labor market

Formal sector

Informal sector

Labor Supply

Labor Supply

Labor Demand

Labor Demand

Wages

Wages

Employment

Employment

w^{min}

w_f^*

w_i^*

w_i^{**}

E^{min} — E_f^*

$E_i^* \to E_i^{min}$