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Minimum wage impacts on wages and hours worked of low-income workers in Ecuador

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Abstract

The minimum wage policy in Ecuador aims to raise the real income of low-wage workers. We analyze the effects of the January 2012 increase in minimum wages on wages and hours worked of low-wage workers. Individuals may select themselves into the occupations of the groups of workers who are covered by the minimum wage legislation, or into those who are not. We apply a difference-in-differences estimation as an identification strategy to account for selection on unobservables. We construct individual panel data from a household panel.

The main results suggest a significant and positive effect of the minimum wage increase on the wages of affected workers, increasing their wages by 0.41% to 0.48% for each 1% increase in minimum wages, relative to the earnings of unaffected workers. Results from hours worked highlight several variables that should be accounted for to find significant and sensible estimations that differentiate between full time work and other heterogeneous effects on the treated group.

JEL: J21; J23; J38.

Keywords: minimum wage, difference-in-difference, hours worked, panel data, Ecuador.

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I. Introduction

Theoretically and empirically, the emphasis of the minimum wage literature has been on dis-employment impacts. More recently, there has been interest in impacts throughout the whole distribution of earnings and wages. However, given the characteristics of Latin American countries with a high percentage of low-earnings workers and a high percentage of informal workers such as Ecuador, the choice when facing higher minimum wages may not be unemployment, and also the adjustment may not be at the extensive margin – on jobs – but at the intensive margin – on hours. Thus, studying the impacts of minimum wages on the wages of low earners is perhaps more relevant. Thus, the main research question we seek to answer is what are the effects of the increase in the minimum wage in January 2012 on wages and hours worked by low wage workers in Ecuador? When answering this overarching question the purpose of our study is to address the following two issues: (i) Beyond their direct impacts, minimum wage policies may have (earnings, wage) spillover effects on the covered (by the minimum wage legislation) high-income wage workers and on non-covered wage groups, and (ii) minimum wage policies may affect different types of covered low-wage workers differently given the potential issue of noncompliance in this region.

Our estimation strategy applies a difference-in-differences (DID) method to control for potential self-selection bias on unobservables – a recognized source of endogeneity but rarely addressed (due to the lack of suitable panel data) in previous relevant studies. In other words, the identification strategy of minimum wage impacts on wages and hours relies on the DID model; it removes individual unobserved effects – that are assumed to be constant over time – which could be correlated with the minimum wage treatment, or in other words, with the treatment's endogeneity concerns. This study uses panel data for December 2011 and December 2012 at the individual level constructed from a panel of households using a matching algorithm. For identification purposes the study also takes advantage of the so argued exogenous (to labor markets or to the economy) variation in the complex wage structure of sectorial minimum wages in Ecuador. We also discuss another source of potential bias: attrition by the panel and regression approach. Furthermore, we discuss intensity effects, analyzed in some empirical studies through the application of a wage gap or indicator variables.

There are multiple minimum wages in Ecuador, according to a structure that depends on industry and occupation – the so-called sectoral minimum wages (SMS, from its acronym in Spanish). There is also a basic unified minimum wage (SBU in Spanish) applied in January of each year, which provides a floor for the rest of the wage structure. In some years, the increase in the referential SBU has been well beyond inflation or productivity growth in Ecuador (Ecuador is a dollarized economy that adopted the US dollar as its own currency in January 2000). It is also important to note that the structure of the SMS points to changes in the number of minimum wages over time (at times reducing the number of SMS, at times increasing it). In addition, the increase in minimum wages within and by sectorial commissions has been very different, and by no means justified by developments in their labor markets, but perhaps by other (political) considerations (such as incoming or recently past elections). Table 1 shows the evolution of the SBU, inflation rate, and a measure of productivity growth (non-oil real GDP divided by the economically active population) for 2006 to 2014. This table shows that the annual increments in the SBU (again, the referential wage for the rest of the categories) may be –for years that coincide with, or are right after key elections– above the inflation rate, and what is suggested by productivity growth. This minimum wage setting, and its evolution, although complex in practice, should be helpful for identification purposes.

Our research uses several specifications to identify minimum wage effects that take advantage of the, we argue, exogenous variation by industry and occupation of the SMS, and apply it to the survey data available for Ecuador, although our study cannot construct long panels of individuals, only short ones. This study avoids focusing on narrow industries; instead, the present study takes advantage of the complex structure and policy of changes in the number of SMS as well as –what we argue independent (from productivity or economic reasons)– increases throughout its wage structure to identify minimum wage impacts. Lastly, the study accounts for substitution effects within different skill groups, in particular on low-wage workers, who as suggested in the literature, may be the most harmed or benefited by minimum wage increases.[1] These least-skilled and low-wage individuals may include some youth. It may also include women with low level, and even no formal education (such as domestic workers).

Table 2 suggests three issues worth noticing. Firstly that the average wages for women and young salaried workers are lower than those of male wage workers.[2] Secondly, this table suggests that minimum wages may be binding: the national minimum wage is equal to or a bit higher than the mean wage, in particular for the total, and certainly for male workers. However, the mean may be affected by extreme values. A comparison with the median is a less sensitive measure to extreme values in the upper tail, or to compression in the lower tail by the minimum wage (Maloney & Nuñez 2004). The minimum wage is above, but close to, the median. However, as pointed out by Maloney and Nuñez, there are several reasons why standardizing by the first moment is not sufficient for assessing whether the minimum wage is binding. Thirdly and last of all, Table 2 shows that the minimum wage is well above the wages for those in the 10th percentile of the wage distribution, which might suggest a problem of noncompliance affecting some types of jobs or firms.

To enforce compliance in Ecuador the government has implemented labor inspections at firms' premises, and penalty fees for firms that do not comply with minimum wages and other labor rights. The inspections usually take place in large firms, and therefore, we expect noncompliance to be an issue in small, rather than large firms. However, even in large firms, considered formal, there may be some informal firms (that is, firms that do not comply with labor and tax regulations). Nonetheless, the degree of noncompliance in the informal sector should be relatively low in Ecuador. Back in the 1990s, Morrisson (1993) (as cited in Strobl and Walsh 2003) found that the degree of noncompliance in the informal sector in Ecuador was 11 %. This figure should be lower nowadays as law enforcement has improved and costs of detection have fallen. We try to account for noncompliance effects on wages and hours by applying heterogeneous effect indicators by group of affected wage workers. Except for impacts on the youth, the literature rarely distinguishes differential impacts of minimum wage workers by type of affected individuals.

Thus, the study contributes to the literature of impacts of a minimum wage increase in several aspects. We provide evidence of the effects on hours, an issue scarcely addressed in the literature. To do so we discuss the importance of taking into account intensity effects or the difference between the actual wage and the future higher minimum wage. We address potential issues of endogeneity, self-

[1] These considerations are highlighted by Neumark and Wascher in their 2006 review of the so-called new minimum wage research.

[2] Note that Table 2 shows the inverse of the minimum wage standardized by the mean wage (as well as by the median wage, and the wage at the 10th percentile).

selection bias (in the treatment) and attrition bias (due to the panel and regression approach), rarely accounted for in the empirical literature, but that could prove important for identification purposes. Our focus on a developing country such as Ecuador, leads us to address additional issues, namely spillover effects and noncompliance issues. We rely on the heterogeneous effect literature to address these issues.

The main results suggest that the minimum wage increase has a significant and positive effect on wages of low-income wage workers ("affected"), increasing their wages by 0.41% to 0.48% for each 1% increase in the minimum wage (after controlling for indirect effects) –relative to the change in unaffected workers' earnings. The results also show that wage effects are felt differently by different types of workers. There are some negative wage effects that reduce (but do not eliminate) the positive impacts on income of female wage workers, and there are additional positive effects on the income of agricultural wage workers. The effects on female workers may point towards a problem of noncompliance. However, we also need to analyze the impacts on hours, as some workers may be earning less because they work fewer hours after the minimum wage increases. We find a significant increase in hours worked by affected workers who are not full-time workers relative to the hours worked by unaffected workers. However, for individuals working full time, the significant impacts on hours are almost nil. In general, we find no significant indirect impacts or heterogeneous effects on hours worked. However, when we account for the intensity effect of the wage gap variable (the difference between the next-period minimum wage and the current-period income), we find a positive and significant effect on hours worked for young workers, which is in agreement to previous results in the literature. The only significant and negative impact on hours worked we identify, that effectively reduces hours worked, is for female affected workers –relative to unaffected workers' hours. The latter may explain the positive, but smaller changes that we find in wages of female workers. Thus, the adjustment to a minimum wage increase may have come through the intensive margin for female workers.

The rest of the study is organized as follows: the next section presents a review of the relevant literature. Section 3 explains the methodology and data used. Section 4 summarizes the results of minimum wage effects on wages and hours worked. Section 5 presents conclusions and policy implications.

II. Literature review

Minimum wage effects on wages and employment have been vastly studied before (although heavily focused on employment), providing estimations mostly in the context of developed countries. As reported in Brown, Gilroy and Kohen (1982), early empirical evidence suggests a negative impact of a 10% increase in minimum wages on teenage employment, ranging from -1% to -3%, as teenagers are one of the most studied groups in the empirical literature (given the coverage and level of the minimum wage in developed countries); the effect on young adults (20-24 year old) was smaller but still negative; and, the impact on employment of low-wage workers in agriculture and manufacturing industries was also negative. Later on, the evidence suggested that impacts on employment were smaller (Brown, 1999), or that there may even be non-negative effects on employment (see for instance, Card, 1992; Card & Krueger, 1994, 2000; Dickens, Machin & Manning 1999; Machin, Manning & Rahman 2003), all while addressing identification issues, as the latter has been a source of contentious debates in this literature.[3] A basic theory guiding these empirical results in developed countries is the perfect competitive model, or if suitable, an imperfect competition setting where negative impacts on employment of a minimum wage are not necessarily the final outcome.[4]

Nonetheless, recent discussion points to evidence of a dis-employment effect of minimum wages in particular for least-skilled groups of workers such as some teenagers (Neumark & Wascher, 2007). The main point of discussion is the soundness of the identification strategy applied by the studies who support non-negative employment effects, as these studies apply standard panel data techniques that exploit regional variation in the minimum wages, but do not account for spatial heterogeneity (Neumark, Salas & Wascher 2013). Given that in the economies under study there is usually only one minimum wage, researchers have to rely on some source of variation (other than time and the minimum wage itself) such as spatial variation.

For developing countries, like those in Latin America and the Caribbean (LAC), there are also other issues to deal with, as coverage of minimum wages may be even broader and its "bite" larger than in developed countries, and such impacts on employment may be expected to be bigger.[5] In addition, in developing countries there may be issues of compliance which may be lower in some industries or groups of workers, or types of firms than those in developed countries. The impacts of minimum wages also depend on how large or small the increase in minimum wage might be, and according to Terrell and Almeida (2008), in LAC, the minimum wages tend to be set at relatively high levels, which might explain the dis-employment effects among the low-skilled, low-wage workers[6]. Yet, another factor that may impinge on minimum wages effects on employment is how low or high the inflation might be (Lemos, 2004). Many LAC countries have had high inflation rates, but unlike these countries, in Ecuador, the inflation rate has been low in the last few years, thanks to its regime of dollarization, while the increases in minimum wages have been generous.

In the developing country setting, the two-sector model has been used to guide the expected results. This model predicts that increases in the minimum wages should lead to increases in wages and reductions in employment in the covered (and formal) sector, but decreases in earnings in the non-covered (or informal) sector since displaced covered-sector workers may go to the non-covered (or informal) sector to find jobs (for a discussion of the expected two-sector model`s results in a LAC context see Terrell & Almeida, 2008; Lemos, 2004; and Lemos, 2009). However, even this theoretical setting might not help to predict labor market outcomes of minimum wage increases if other events that have characterized LAC countries take place, such as periods of high inflation, or employment protection laws, or if the labor market is not really segmented but instead also exhibits features of a competitive integrated labor market (Lemos, 2009).

Nonetheless, the separation between groups of workers, covered and non-covered, and within the covered on formal and informal sectors is a needed conceptual setting applied in the empirical studies of minimum wage impacts for LAC. Thus, in our study we follow this separation and

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[3] See Neumark and Walsch (2007) for both a summary of the debate in the new minimum wage literature and for a summary of the empirical evidence of minimum wages impacts on employment in developed countries.
[4] The theory may provide more complex models, but such conceptual settings are not necessarily well suited to apply to or are not applied in empirical estimations (see Brown, 1982).
[5] See ILO (2007), pages 25-26, for a table with minimum wage coverage in LAC.
[6] According to Lemos (2004), the unemployment effects in LAC are stronger than those in developed countries: "a 10% increase in the minimum wages decreases employment up to 12% across the available studies, … this is substantially larger than the U.S. employment effect" (p. 222). As pointed out by Lemos, while the effects could be stronger in LAC, care should be taken with their magnitude, as there are only a few studies per country in LAC, and those estimates present a high variance among them, which Lemos attributes to substantial institutional differences.

define covered workers as those workers subject to the minimum wage legislation –in general these are private sector workers–, and non-covered as those not subject to such legislation –namely, public sector workers and the self-employed. Informal workers are alternatively, covered workers (i) whose employers may not be fulfilling obligations such as social security enrollment, or (ii) employed by small firms with 10 or less than 10 employees with no accounting and no tax records (as it is usually applied by the National Institute of Statistics and Census in Ecuador).[7] For more on the measuring of the Informal Economy in Latin America and the Caribbean see Vuletin (2008).

As stressed in the literature (see, for instance, Terrell & Almeida, 2008; Neumark & Wascher, 2006), the effects of minimum wages should be most likely felt by those in the lower tail of the initial distribution of wages –leading to a wage compression effect when analyzing wage impacts[8]. We follow such literature to provide evidence on whether wages of covered workers in the lowest tail of the skill or wage distribution in Ecuador (presumably low-educated females and youth and/or some other low-earnings workers) should be the ones most significantly affected (positively) by changes in minimum wages. Other groups might also be affected by minimum wages; that is, spillover effects on wages of the covered workers in the higher end of the wage distribution, or in earnings of non-covered or informal sector workers).

In several LAC countries the minimum wages affect the wages of the informal sector positively, both at the minimum wage and at multiples of the minimum wage (Cunningham, 2007). Gindling and Terrell (2007), using a time-series cross-section approach, found that the public sector emulates minimum wage increases in its wage structure when it is not formally covered. Evidence on the impacts of minimum wage on employment in the non-covered sector is unclear (Terrell & Almeida, 2008); some find positive employment effects (Carneiro & Corseuil, 2001; and Lemos, 2009 for Brazil), others find negative ones (Fajnzylber 2001 for Brazil). When defined as self-employed, there are also mixed findings; small positive effects in Costa Rica, but no significant effect in Honduras (Gindling & Terrell 2007, 2009). Dis-employment effects in the public sector appear to be insignificant or small (Lemos, 2009 for Brazil, Gindling &Terrell, 2007 for Costa Rica).

Studies that address the impacts of minimum wages on hours are fewer. We highlight Zavodny's (2000) study in the USA as she investigated the impacts of minimum wage increases on hours worked by young workers constructing a panel at the individual level, while addressing endogeneity. We follow this author when building our individual panel from a household panel, and applying difference-in-difference estimations. However, she did not address sample selection concerns due to attrition bias that may have arisen in her data and estimation methodology, or when restricting the sample only to those workers who remained employed. Her study also highlights the role of the wage gap in capturing minimum wage effects, and by using a sample of teens who remained employed this author found a positive effect of the wage gap on affected workers' hours (relative to the change in unaffected workers' hours). Zavodny defines the wage gap in level terms: wage gap =

[7] Strictly speaking, in Ecuador, workers in small firms should earn the minimum wage, that is, they should be considered covered workers (although it is only since 2010 that the law states that *all* private workers should earn the minimum wage). However, the definitions above acknowledge the fact that for small firms with no tax and/or no accounting records, the minimum wage enforcement might be rather non-existent. So this group of workers may be considered informal despite being –at least in theory– covered.

[8] Lemos (2004), when studying the minimum wage effects on formal and informal sectors in Brazil, discusses the wage compression effect as a decrease in the wage gap of the 90th and the 10th percentile wages. According to this author, the wage compression effect extends higher in the informal sector wage distribution. Her study also provides a list of other studies with empirical evidence on wage compression effects for Latin American countries. See Lemos (2002) for references on this topic for developed countries.

$MW_{t+1} - W_t$, and also applies it to the wage regressions –which calls for concern about the validity of her results of the wage estimations since the dependent variable and the wage gap are mechanically correlated because both have the wage at t.

Concerning compliance, the empirical evidence on noncompliance is still scant for developing countries, despite being a key issue for minimum wage policies in these types of countries. For instance, Strobl and Walsh (2003) found evidence of noncompliance in small firms in Trinidad and Tobago. As noted in the introduction, compliance should be improving in Ecuador, due to controls implemented by the government, in particular in large firms. We aim to account for noncompliance using heterogeneous effects on the affected wage workers' estimations for both wages and hours worked.

There is only one previous study to our knowledge that addresses the employment and wage impacts of minimum wages in Ecuador. Canelas (2014) uses province-level data and applies standard equations found in the so-called new minimum wage literature (e.g. Card & Krueger, 1994), that rely on geographical variation to identify employment and average wage impacts of increases in the basic unified minimum wage (that is, her study does not use the sectoral minimum wages) on formal and informal workers. Canelas found positive or no evidence of employment costs of a minimum wage increase. Besides some data issues,[9] the results may lack the proper identification; it is not clear whether the variation in minimum wages across provinces used in her study are due to price variation or other province-level or time-level shocks.[10] These results and concerns show that more studies on the wage and employment impacts of minimum wage policies in Ecuador are needed.

In summary, the evidence for LAC has lately focused on employment effects of minimum wages as earlier evidence concluded that there are strong wage compression effects in LAC, stronger than those in developed countries (Lemos, 2004), whereby increases in minimum wages increase the wages of low-wage workers. Among these low-wage workers there are some women and young workers. However, the question of what the overall employment effects are and whether there are positive or negative minimum wage effects on earnings and wages of noncovered (and informal) workers remains unanswered. This is because evidence is limited and inconclusive, and this is due to the fact that the effects are difficult to quantify (Terrell & Almeida, 2008). Unlike in several minimum wage studies on developed countries, most of the studies on minimum wage effects in LAC do not use panel data at the individual level. Therefore, these studies cannot control for self-selection bias that may be expected for workers. Our study not only uses panel data at the individual level, but also takes advantage of exogenous variation in the complex structure of minimum wages in Ecuador. We also address impacts on hours, another potentially important margin of adjustment in labor markets that has been scarcely studied, while accounting for spillovers, intensity, and heterogeneous effects of a minimum wage increase.

[9] There is a break in the survey data used by Canelas (2014) because the definition of employment in that survey changed in 2007; that is, there is one definition of employment before September 2007 and another in and after September 2007, as the questions that defined employed versus unemployed in the survey data were changed.

[10] Assuming the choice of the period does not pose the challenges discussed above, another modeling issue in her employment equation is the lack of a term to control for differences in economic trend by territory. This issue has been discussed lately in the literature (see, for instance, Neumark et al. 2013).

III. Methodology and data

A minimum wage increase can be seen as a treatment whereby workers covered by the minimum wage legislation –and effectively affected– receive the treatment (the treated group), and other workers do not (the non-treated, comparison or control group). The results of the comparison of outcomes between treated and control groups of workers may be marred by self-selection bias on the basis of non-observable traits (Menezes-Filho, Mendes & Almeida, 2002; Carneiro & Henley, 2001). Carneiro and Henley (2001) also found evidence that workers chose the informal sector based on their comparative advantages (at least in Brazil), or on the basis of observables ones. The pre- and post-minimum wage increase data available, allows us to use the difference-in-differences (DID) approach for estimating the causal effects of such policy change (the so-called treatment) on earnings and hours worked of low earners while controlling for self-selection based on unobservables that are assumed constant over time. Although, as previously discussed in the literature review, employment effects might be important –in particular for low-wage workers– we leave aside any employment effects of the minimum wage increase and study only earnings and hours effects, conditional on continued employment.

This research design is based on comparing both groups (the affected and the non-affected by the treatment) and controlling for confounding variables (that is, variables that are related to the treatment and the potential outcomes). As pointed out by Lechner (2010), DID has the advantage of allowing for heterogeneous effects of the treatment across population members, and as the data suggest, this policy change neither affects all individuals at the same time, nor in the same way (see Table 2). The latter will also allow us to explore issues of noncompliance.

Our study provides a methodological contribution as we address issues of indirect effects, noncompliance and informality that characterize Latin American labor markets. We address endogeneity issues that may be present in the DID estimation that have not been accounted for in previous studies that use similar panel data. These endogeneity issues may arise due to (i) potential attrition bias, resulting from panel construction and DID cross sectional estimation, and (ii) the use of intensity effects (the wage gap for the case of wage estimations).

3.1. Identification issues

The identification of the causal effect of the treatment rests on the idea that the treated and the non-treated group are subject to the same time trends, and that the treatment has had no effects on the pre-treatment period, which allows us to remove the effects of confounding factors when comparing the post-treatment outcomes of the treated and non-treated. Applying this idea to our study, we focus on mean changes of the outcome variables (wages or earnings and hours worked) for the non-treated during the periods before (December 2011) and after (December 2012). We then add them to the pre-treatment mean level of the outcome variable for the workers subject to (and affected by) the January 2012 minimum wage changes, in order to get the mean outcome these treated workers would have experienced had they not been subjected to the minimum wage increase.[11]

[11] Some studies on the minimum wage impacts on earnings have focused on the distribution of the outcome variable, in particular when addressing the income inequality effects of minimum wages. Our goal is to study the impact of the minimum wage increase on low-wage earners, thus we do not make efforts to estimate or recover quantile treatment effects or minimum wage effects over the entire income distribution.

Therefore, the DID estimation rests on a key assumption: in the absence of the treatment, differences in the expected potential (non-treatment) outcomes between the two groups (treated and control) are constant overtime. This implies that the covariates X should be selected so that they control for all variables that would lead to differential time trends (Lechner, 2010). However, as we examine a particular minimum wage increase (that of January 2012) using a two-period data panel, we cannot deal with, nor can we control for time trends. Nonetheless, Appendix 1 presents a group comparison section by examining time trends of the outcome variables for workers subject to the minimum wage legislation, and those not subject to such legislation. This comparison supports the assumption that, in general, in the pre-treatment period differences in outcomes (wages or hours) of the two groups were constant.

Thus, if the common trend assumption holds, any deviation of the trend of the observed outcomes of the treated from the trend of the observed outcome of the control group (once proper control variables are accounted for) will be explained by the effect of the treatment.

By using DID with panel data, and panel data at the individual level in particular, we are controlling for time-constant individual-level confounding factors that are additively separable from the remaining part of the conditional expectations (Lechner, 2010). To the extent that the entrepreneurial spirit of the person, their managerial drive, their personality, and other non-observables –that we are assuming do not change over time– influence both their selection into treatment and their potential outcomes, the use of DID with an individual panel allows us to remove such endogeneity. In the Latin American context, the literature review reveals that one concern in the estimations of the impacts of a minimum wage increase is whether individuals select themselves as wage workers (treated), or as self-employed (non-treated), based on non-observable factors such as the ones mentioned; furthermore, the same can be said about the earnings that individuals may be receiving, and those innate treats may determine their earnings. Thus, as pointed out by Lechner "we can allow for selection into treatment based on unobservable variables that also influence potential outcomes as along as their impact is constant over time" (2010, p. 189).

It is important to acknowledge that the DID approach with the panel data that we are using does not resolve endogeneity problems when there is selection on non-observables that change over time. If other unobservables, such as soft skills that improve over time (due, for instance, to some training received by the individual), positively influence the selection and outcomes, the results may be over estimating the impact of the treatment. Our panel data does not allow us to control for such changes over time, if they happen.

We could have used cross sectional data and added fixed individual effects, instead of the treatment group dummy in a DID regression (and all time constant covariates X) which should lead to the same estimate (Angrist & Pischke, 2009). However, this is a rather restrictive specification and the precision of the estimator may change.

Alternatively, matching estimation based on conditioning on pre-treatment outcomes is also feasible; and, although matching does not require common trends, it assumes that conditional on pre-treatment outcomes confounding unobservables are irrelevant (Lechner, 2010). In our study, we assume that confounding unobservables (that are constant over time), as the ones previously mentioned, are important factors to control for using panel data with DID. In any case, the literature presents both, discussions that favor matching compared to DID in panel data (Imbens & Wooldridge,

2009) as well as discussions that favor DID to matching as the latter may be biased (Chabé-Ferret, 2010).

It is also assumed that the treatment does not influence the components of the set of covariates X.[12] In our study, this implies that the minimum wage increase does not affect the earnings (or hours worked) of the comparison group. However, as previously discussed in the literature, the minimum wage increase may affect the earnings of those who are not covered (otherwise known as the lighthouse effect for informal workers or the self-employed, or even the wages of higher-wage workers). Although it may be unlikely that a minimum wage increase affects the wages of government employees as these workers have their own track of wages in Ecuador, or of informal workers working in some small firms (which may have compliance issues).

To the extent that the earnings of the comparison group were affected (e.g., increased) by the increase in the minimum wages, and we would be including such endogenous variable, if we found a positive effect of the minimum wage increase on the wages of the treated, this positive effect would be under-estimated (and just the opposite if the minimum wage increase would affect negatively the earnings of the control group). That is, conditioning on an endogenous variable as if we were estimating only that part of the causal effect that had not already been captured by the particular endogenous variable (Lechner, 2010). However, given the potential for spillovers of the minimum wage increase into other groups, in particular of uncovered low earners, we attempt to account for such indirect effects in our regressions (see equations and discussion on indirect effects below).

We also aim to find intensity effects of the minimum wage increase on low-income workers (both covered and not covered by the minimum wage legislation) that could be affected by the minimum wage increase, and we do so only for the regressions on hours worked as we will later present.

3.2. Group definition and comparability

DID boils down to having a proper treatment group and a proper control group, meaning two groups with common trends in the pre-treatment period. Appendix 1 provides a summary of the comparability of the two groups in the survey data.

We define the treatment group as workers covered by the minimum wage legislation (private-sector wage workers) and effectively affected. The latter means that the wage of the salaried worker is low enough. The literature has defined a private-sector wage worker that in the base year earns at least the base-year minimum wage but less than the next-period minimum wage as affected or bound (Zavodny, 2000; Currie & Fallick, 1996)[13]: However, in Ecuador –and perhaps reflecting either a delay in compliance or a downward wage bias against certain groups of workers such as women and youth (see Table 2), or less than full-time working hours (or a combination of all these factors)– many workers may earn, in the base year, less than the base-year minimum wage (and not at least), thus we cannot apply the same definition of affected or bound worker used in studies conducted in developed countries (Zavodny, 2000; Currie & Fallick, 1996). So, as an alternative, we define those private-sector

[12] According to Lechner (2008), the assumption that the components of X are not influenced by the treatment is too strong. It should suffice to rule out that any influence of the treatment on X does not affect the potential outcomes.
[13] Currie and Fallick (1996) also added the condition that the worker was not working in the state or local public sectors, in agriculture, or in domestic service, as such a definition was applicable by the time their study was conducted for the U.S.

wage workers that in the base year earn less than the next-year minimum wage (MW_{t+1}) as treated or affected workers.

Affected: private-sector wage worker whose wage at time t (W_t) is $W_t < MW_{t+1}$

The control group includes workers not covered by the minimum wage legislation in Ecuador: government employees, self-employed, and business owners. We also include high-earning workers (whose income or wage at time t is greater than or equal to the minimum wage at t+1) and who are covered but may not be effectively affected by the minimum wage increase. Table 3 summarizes descriptive statistics that characterize both affected and control workers in our panel data.

3.3. Model specification

The basic DID model to be estimated is[14] :

For wages (and hours).

(1a) $\Delta\, ln\, w_i = \beta_o + \beta_1 X_i + \beta_{2i}\,(affected_{\,i}) + \beta_3 indirect\_a_i + \beta_4\, indirect\_b_i + \beta_5 indirect\_c_i\ [omitted] + \theta_m + \delta_o + \rho_r + \eta_i$

In addition, only for hours.-

(1b) $\Delta\, ln\, h_i = \beta_o + \beta_1 X_i + \beta_{2i}\,(affected_{\,i}) * Wagegap_i + \beta_3 indirect\_a_i * Wagegap_i + \beta_4\, indirect\_b_i + \beta_5 indirect\_c_i + \theta_m + \delta_o + \rho_r + \eta_i$

And, for both specifications above:

(1') $\beta_{2i} = \beta_2 + \Upsilon_j z_{ji}$

Where, i, m, o, and r index individual, industry, occupation, and region, respectively.

The dependent variable for the regression on wages (hours) effects is the change in the logarithm of the monthly real wage (weekly hours worked in the principal job) of individual i between the two periods under analysis.[15] We use real terms deflating the wage or income using the consumer price index (CPI) of the corresponding year (2011 or 2012).

The coefficient β_{2i} is the DID term that should capture the differences in wages (hours) between the treatment and the control group in the post minimum wage increase period.

We also use a measure of the intensity effects that aim to capture the extent to which the increase in the minimum affects a worker, known as the wage gap in the literature (see Currie & Fallick, 1996; Zavodny, 2000). Thus, the wage gap is the difference between the real minimum wage at time t+1 and real earnings at time t in logarithm.

Wage gap = $\ln (MW_{t+1} / W_t)$, if year is 2012

In other words, the wage gap helps reflect the relative effect in the hours of the low-income affected workers who remain employed as it is expected that the degree of impact of the minimum wage may depend on how far below from the minimum wage the earnings of such workers are.

[14] In all equations below, the time index is omitted for the sake of simplicity.
[15] For self-employed or business owners we used the monthly income recorded.

However, unlike Zavodny (2000), and Currie and Fallick (1996), we only apply the wage gap to the hours worked estimation since applying this wage gap to the wage estimation may bring in some spurious results (due to the mechanical correlation between the dependent variable and the wage gap variable). In the hours estimation, we apply the wage gap not only to the affected workers, but also to unaffected low-earning workers so as to capture indirect effects of the minimum wage increase on some informal workers. As in Zavodny, we do not apply the wage gap for high-income individuals.

When defining this independent variable of interest (wage gap) we use the sectorial minimum wages as the minimum wage variable, which vary by industry and occupation, also in real terms deflated using the CPI. As discussed in the introduction, we argue that the increase in the many sector/occupation minimum wages has been determined by factors other than the economy and labor market indicators. For example, the timing of the election and demand for votes can be seen as an indicator of the size of the minimum wages increase.

The indirect effects in the equations above refer to: (i) individuals in the control group whose earnings are below the next-period minimum wage (indirect_a) and, as such, their indirect effect dummy is multiplied times the wage gap variable to capture the intensity effect of the minimum wage increase –again, only for the impacts on hours; (ii) individuals in the control group whose earnings are above or equal to the next-period minimum wage (indirect_b); and (iii) treated individuals whose wages are above the next-period minimum wage (indirect_c) (see Figure 1).

Figure 1. "Affected" and indirect effects

	Covered Private-sector wage workers	Uncovered Gov. employees, self-employed, business owners
w(t) < MW(t+1)	**affected**	indirect_a
w(t) >= MW(t+1)	indirect_c	indirect_b

Note: We distinguish *covered* by the minimum wage legislations from *affected* by the minimum wage: our treatment group ("affected") are workers who are covered *and* affected by the minimum wage. Our control group are workers who are either covered but not affected (indirect_6c), or not covered by minimum wage (indirect_6a, indirect_6b).

The minimum wage effects may depend on the degree of the employers' compliance. If non-compliance is an issue, we would expect the effect of minimum wages on the wages of wage workers effectively affected to be lowered, with respect to the case of issues of noncompliance not being accounted for. Such heterogeneous effects could be captured by specifying a worker specific minimum wage effect parameter, or ,in other words, by saying that the coefficient of the minimum wage variable is indeed a linear function of other variables (dummy) that specify j different types of workers. Thus, we add interaction effects to the treated variable with dummy variables for each of the following types of workers (assumed to be related with noncompliance) –in separate regressions: (i) women, (ii) youth, (iii) those who work for firms with 10 or fewer workers (small firms), or (iv) who work for firms that do not hold accounting records, or (v) who work for firms with no tax records, (vi) domestic workers, and (vii) agricultural workers.[16]

Equation (1') formalizes the heterogeneous effects due to potential noncompliance by adding the j worker-specific term in our coefficient of interest (in both equations 1a and 1b), that vary according to the i individual in our data. Only in the case of the hours-worked regressions do we multiply these heterogeneous effects by the intensity effect or wage gap.

X_{ijt} controls for worker characteristics such as experience. The estimates may be sensitive to controlling for industry, occupation, and region effects, and according to Lemos (2007), modelling region effects helps with the identification of the minimum wage impacts. Thus, we also add industry (θ_m) (at the 1 digit level of the ISIC rev. 3 classification) and occupation (δ_o) (at the 1 digit level of the CIOU occupation classification) effects, as well as a dummy for the sierra region (ρ_r) for year two.

For the regressions we use a sample that is restricted to individuals who are employed (and have wages or earnings) during both periods. We apply the Heckman two-step to account for any sample selection bias in the case of the cross sectional estimating equations (1a) and (1b).

We have to deal with potential sample selection and attrition bias, as some participants may drop out (are not matched in the panel, or become unemployed) in the second period. Although our two-period data is a constructed panel at the individual level, our regression data is not. The wage and hours regressions are a cross section in which the dependent variable is a first difference of both time periods, and the attrition leads to missing (cross sectional) observations for the dependent variable. If some of the dropouts are systematically different from those who stay in the sample, there may be a potential threat of bias, in particular, if the remaining sample becomes different from the original sample (see, for instance, Miller & Hollist, 2007). Thus, we apply a two-step procedure proposed by Heckman (Heckman, 1976, 1979).[17] Miller and Hollist summarize this procedure: in brief, the first step of the procedure estimates a probit model where the binary regressor is equal to 1 when the wage difference is observed (and "0" when missing) and renders an outcome variable called λ (mills lambda or inverse mills ratio). In the second step, the λ value of each observation is included as an explanatory variable into the larger data set (panel and non-panel) and then included in the analysis of interest.[18] If the sample selection bias is not significant (that is, if the estimated coefficient of the Inverse Mills Ratio is not significant) we can also apply OLS, in which case we apply robust errors, and we can also estimate using the population weights.[19]

[16] As Table 2 shows, female and young workers may in fact have received less than the minimum wage (assuming they all worked full time). Small firms or firms with no accounting or tax records may be noncompliant with minimum wage legislation. The enforcement of the minimum wage may be difficult for domestic and agricultural workers.

[17] The control variables in the *selection* equation include experience squared, a dummy for females, a dummy for youth, and education (in years). We previously tested that education is not significant to explain the changes in the real wages so we can introduce it as an exclusion restriction in the selection equation.

[18] Thanks to participants at the University of Laval seminar for pointing out this issue. As it is known in the literature, the Heckman two-step estimator is a limited information maximum likelihood estimator that requires normality only of the error term in the selection equation as well as linearity of the conditional expectation of the outcome equation error term conditional on the selection equation one (Montes-Rojas, 2008). However, the literature also points out that if there is joint normality of the error terms, the two-step is still consistent but no longer efficient. Thus, we also computed the Heckman maximum likelihood estimates (MLE). Although it is believed that bivariate normality is a much stronger assumption, which is in general rejected (Montes-Rojas, 2008), and if we only have univariate normality, the two-step remains consistent while the (full information) MLE are not.

[19] We also estimated bootstrapped standard errors when estimating equations (1a) and (1b) with a Heckman two step. According to Freedman (1984), bootstrapping standard errors allows for the model to be tested against its own assumptions (concerning the errors term structure). With affected and bootstrapped standard errors we obtained the same results in terms of the significance of the coefficients as with no bootstrapped errors; thus, the tables with results for affected are with no bootstrapped errors.

We also account for noncompliance adding to the treatment variable the previously discussed interaction effects for women, youth, individuals working in small firms or in firms with no tax records or in firms with no accounting records, or if the worker is a domestic or agricultural worker. Thus, we interact our treated variable with each of these seven groups adding, one at a time, in different regressions, an interaction term (with the treated variable) using dummies that equal one if the individual is a woman, or her age is between 15-24 years old, or if she is a domestic worker, or if she is an agricultural worker, of if she works for a firm that is small in size (less than 10 workers), or has no accounting records, or has no tax records. Recall that the treatment variable is multiplied by the wage gap in the estimation of hours effects. If non-compliance is an issue, we expect it to have significant positive or negative coefficients of the interaction term, so that it increases or decreases the minimum wage effect on hours of the treated group.

The effects of the minimum wage increase on hours may be ambiguous: it depends on the following: (i) whether employers have fixed costs of employment (hours of workers who remain employed may increase); (ii) how employers view hours worked (if as another factor, then employers not only may reduce workers but also hours worked by the workers still employed); (iii) whether there are costs associated to firing workers (in which case average hours, not employment, may fall), for instance, see Zavodny (2000) and Gindling and Terrell (2007).

3.4. Data

We use the Ecuadorian household survey data (ENEMDU) which is a 2x2x2 household panel, whereby the same households appear in two consecutive quarters, then leave for two quarters only to reappear again for another two consecutive quarters. It is conducted every quarter of each year, and the December and June issues have data for both urban and rural areas. Given that the increase in the minimum wage takes place in January, an option for a relevant panel data period is December (the before period) and March (the after period) of two consecutive years (for urban areas only, and at most 50% of the households would be in the panel). We could also construct a panel with one December (before) and one December (after) periods which covers all areas in the country and where 100% of households may be in the panel. That is, we can only construct short-run periods of panel data which limits our ability to control for long trends in the estimation of such a data setting. If differences in trends between the two groups (treatment and control) occur due to factors other than the minimum wage, the estimation will be invalid or biased (Gertler, Martinez, Premand, Rawling & Verneersch, 2011). Due to data limitations (we only have data on sectorial minimum wages starting in 2011) we chose December 2011 to December 2012, and thus analyze the impacts of the January 2012 minimum wage increase.

To construct a panel at the individual level, we apply a matching procedure in which, within each household, we match age (with a gap of 0 to 2 years between the two periods), gender (exact match), education (with a gap of 0 to 2 years between the two periods), and race (with some corrections for probable recording errors) (see Zavodny, 2000). Appendix 2 (Table A1) shows some descriptive statistics of the matched (panel) and not-matched (non-panel) individuals of the age of interest (15 to 70 years old in the second period). These statistics are similar in the two groups (panel and non-panel workers in the age of interest) showing that the panel constructed at the individual level can be seen as representative of the population that it is drawn from. See Appendix 2 for additional details on the matching procedure.

IV. Application and results

4.1. Wage effects

The main results suggest that the minimum wage increase had a positive and significant impact on the wages and income of low earners (see Table 4). In particular, the increase in the minimum wage had a positive effect on those salaried workers who remained employed after the minimum wage increase, and who in the base year earned less than the next period minimum wage, ranging from 0.41% to 0.48% for each 1% increase in the minimum wage (after controlling for indirect effects) –relative to the change in the control workers' earnings. There were also some positive spillover effects of the minimum wage increase on the income of low-earners in the control group (indirect_a), similar in size to the wage effects in the treatment group. However, there may have been some negative effects, albeit much smaller (about a fourth in size of the direct effects), of the minimum wage increase on the high-income individuals in the control group (indirect_b; the high-wage workers, indirect_c, constitute the omitted category). The control group, as explained in the methodology section, included government employees, self-employed, and business owners, all of whom were not included in the minimum wage legislation.

All wage effects of the minimum wage increase in Table 4 resulted from applying the Heckman two-step procedure to account for potential sample selection bias and attrition. The DID model, that is apparent in the definition of the dependent variable (differences in wage or hours), removes individual unobserved effects (that are constant over time) and allows for the identification of the minimum wage impacts; in other words, it deals with the treatment's endogeneity concern (self-selection bias, as there are variables that affect participation in the labor force). However, as we apply our matching algorithm to construct a two-period panel at the individual level, and as we have a cross sectional regression (by construction of the dependent variable), we may have ended up with a non-random sample of workers (sample selection bias). If this were the case, then OLS on the sample panel of workers would be biased and inconsistent; that is, if sample selectivity existed then the OLS coefficients might not be applicable to all workers (working and non-working). This is a concern that has not been addressed in the previous literature that uses similar types of data. Thus, as explained in the methodology section, we applied the Heckman two-step procedure to account for sample selection bias, if any. Table 4 shows that the coefficient of the Inverse Mills Ratio is negative, but only significant at 10% in two cases (in the regressions that control for firms that did not have tax records or accounting records). A significant and negative coefficient in the Inverse Mills Ratio suggests that the error terms in selection and outcome equations are negatively correlated, so unobserved factors that make participation more likely tend to be associated with lower wages. However, when bootstrapping errors for these two regressions, the coefficient of the Inverse Mills Ratio turns no significant. When the Inverse Mills Ratio is not significant we can apply OLS (with robust errors to account for cluster effects in our survey data). In doing so, we obtained similar coefficient results as in the Heckman two-step estimations, thus we omitted these additional OLS results in Table 4 (although they are available upon request).

Heterogeneous effects on wages by group of workers

Table 4 also shows the results of the heterogeneous effects of the treatment by type of wage worker. Only for women and agricultural workers did we obtain significant results: the coefficient for women's wage impact is negative, reducing the positive effect of the minimum wage on their income (-0.138) in about a third of the treatment effect (0.482). The coefficient for the agricultural workers is positive, increasing the total positive effect of the minimum wage in their wages (0.413) by 0.125.

4.2. Effects on hours worked

In Table 5a we present the effects of minimum wage increase on hours worked for our treatment group (affected).[20] Results suggest that a 1% increase in the minimum wage may have led to an increase in hours worked ranging from 0.056% to 0.069% for each percent of minimum wage increase –relative to the change in control workers' hours– when accounting for indirect effects. Except for half of the cases for low earners in the control group, the spillover effects of the minimum wage increase on hours worked by low or high earners in the control group are in general not significant. Similarly, for heterogeneous effects by type of wage workers in the treatment group, only for workers in firms with no accounting records did we find a marginal increase on hours worked (of 0.045% for each 1% increase in the minimum wage that was significant at 10%). We estimated these regressions by applying a Heckman two step and obtaining Inverse Mills Ratios that were not significant suggesting that there was no attrition bias.

Applying wage gap to the estimation of hours effects

However, when studying the effects on hours of a minimum wage increase, a factor that might have influenced the results is how far the current wage of the individual was from the next-period minimum wage, the so-called wage gap (Currie & Fallick, 1996; Zavodny, 2000). We expect that the farther the wage of the worker from the minimum wage, the stronger the effect on the hours worked. When applying interaction to the treated with the wage, we found a positive and significant effect on the treated of a minimum wage increase. The interaction term captures all the impacts, which was stronger than the effect on hours worked without the wage gap. The coefficients on the interaction term between the treatment and the wage gap ranged from 0.179 to 0.199 for each 1% increase in the minimum wage (after controlling for indirect effects) –relative to the change in the hours worked by the control group.

Results also suggest positive spillover effects of the wage gap on hours worked for the low earners in the control group (indirect_a) from 0.096 to 0.100. This is about half the size of the effect on the hours worked by the treated.

For subgroups of workers (noncompliance effects), we only found a significant and positive effect on hours worked for the youth. This result for the youth is in agreement with the results from Zavodny (2000) who found a positive and significant effect of the wage gap –when minimum wage increases– on treated workers' hours relative to the change in control workers' hours. This author analyzed the effect of the early 1980s and early 1990s minimum wage increase on hours worked for teenagers aged 16-19 in the US, restricting her sample to individuals who were employed. However,

[20] As in the case of wage effects, when studying the effects on hours worked, we applied a Heckman two step. The estimated coefficients for the inverse mills ratio were not significant, thus we also applied OLS (with sample estimation, with robust errors estimation, and with population weights). These estimations yielded similar results for the value and significance of the coefficients as the results obtained with the Heckman two-step estimator. We have not included these additional estimations in Table 5, part (a).

her study did not address potential sample selection bias due to panel attrition. As in our study, her DID approach tries to account for any self-selectivity bias into treatment (endogeneity).[21]

4.3. Robustness checks

Accounting for full-time jobs: Wage effects

Given that the definition of affected includes all private wage workers who in one period one earned less than the period-two minimum wage, it could be that some workers earned less because they worked fewer hours than those expected to earn a full salary, and when a minimum wage increases, the wage effects may be different depending on how many hours the individual works. We counted how many workers (sample and population) work between 30 to 50 hours, which would make them entitled to a full salary.[22] Out of the 2,087 observations equal to 1 in the affected, 69% worked between 30 to 50 hours per week (similarly, when we accounted for the population weights); that is, most treatment individuals were full-time workers. As mentioned in the methodology, to determine whether there were any differences in the minimum wage impacts for those working between 30 to 50 hours per week, we introduced an additional interaction term to our treatment for individuals working in that range of hours. The coefficients of this additional interaction term were negative and significant for the wage regressions. The results for the total impacts of the minimum wage increase on wages of full-time workers remained positive and significant. However, the negative coefficients of the additional term that controls for full-time workers means that the impacts of the minimum wage increase on wages was reduced (by about a 0.05% for each 1% increase in the minimum wage) for full-time wage workers, whereas for not-full time workers, the wage impact ranged from 0.558% to 0.636% for each percent increase in the minimum wage (again, relative to the change in the control workers' earnings, see Table 6). As in the previous results for the wage effects, we also found positive spillover effects for low earners and negative indirect effects for high earners in the control group. Concerning heterogeneous effects, there were still some negative impacts on women's wage (that reduced, but did not eliminate the positive wage effects of the minimum wage increase) and positive wage impacts on agricultural workers. However, we also found negative impacts of the minimum wage increase on domestic workers' wages, although the total impact is still positive.

Accounting for full-time jobs: Effects on hours

In Table 5 (part a), results suggest that a minimum wage increase led to an increase in hours worked ranging from 0.056% to 0.069% for each percent of minimum wage increase. However, we expected that the incentives on hours worked brought about by a minimum wage increase would be different for full-time workers and for non-full time workers. It should be more difficult for full time workers to increase hours worked (even if their employers see those hours worked as a fixed cost) – or to reduce their hours (even if their employers see their hours worked as another factor, or even if there were high costs of firing). To identify whether the increase in hours worked by full-time workers in the treatment group held, we introduced a dummy for full-time workers.

[21] It remains to see if the Zavodny study would obtain different results and if it would correct for any panel attrition –if attrition bias were significant. Note that Tables 5, part (a) and part (b) in our study indicate that panel attrition bias is not significant, thus we expected (and obtained; see previous footnote) no difference in the significance of results when we did not apply the correction (e.g., applying just OLS).

[22] The "normal" duration of a work week, according to the Ecuadorian law, is 40 hours in Ecuador.

As for the wage regressions, the coefficients of this additional interaction term for full-time workers were negative and significant. These terms almost outweighed the effect of the treatment on the affected workers. Thus, the impacts of the minimum wage increase on hours worked for full-time wage workers affected by the minimum wage remained positive and significant –albeit much smaller (almost nil) than previously accounted for. The results in Table 7 (part a), also show that the impacts on hours worked of non-full time affected workers were much stronger than previously measured. We also obtained positive and significant effects on hours worked by low earners in the control group. As in Table 5 (part a), where heterogeneous effects by type of treatment workers were almost absent (none, except for a marginal positive effect on hours of workers from firms with no accounting records), Table 7 (part a) shows no significant impacts on hours worked by type of affected workers. However, as in Table 5 (part b), the heterogeneous effects on hours appeared when we introduced intensity effects using the wage gap variable.

Full-time jobs – Applying wage gap to the estimation of hours effects

As previously carried out for the wage regressions, we added an interaction term to the wage gap for those working 30 to 50 hours to the hours regression, and we found that women working 30 to 50 hours per week were the ones adjusting their work hours down. This result may explain why treated women's earnings may have grown less than other treated workers, as found in the previous section (although female treated workers ended up with higher wages, the increase was less than the increase in wages experienced by treated male workers).

Table 7 (part b), also shows that for affected individuals working 30 to 50 hours per week, there may not have been any appreciable effects on hours worked in total; the positive and significant effect of the wage gap practically cancelled out the significant and negative effect of the interaction term of the wage gap with the 30 to 50 hours' dummy. The affected individuals who worked less than 30 hours or more than 50 hours per week were the ones who experienced a significant increase in their hours worked for every 1% in the wage gap, relative to the change in control workers' hours. And, it is still the case that low earners in the control group (indirect_a) also experienced an increase in hours worked for each percent of difference in the wage gap. Estimations in Table 7b apply the Heckman two-step procedure to test for sample selection bias. In general, we found no evidence of this bias. Thus, when estimating by (the more efficient) OLS, the results show, in general, similar coefficients, although in the net there were in some cases of significant and positive (but very small) effects on hours for the affected workers, relative to the unaffected ones working 30 to 50 hours per week.

Addressing duplicates in the panel - wage effects and hours effects

A source of potential concern for the robustness of the results comes from the way the data was constructed. As explained in the data section and Appendix 2, in order to construct panel data at the individual level from the household panel, during the matching process we had to deal with duplicates of individual observations, and we did so by losing some duplicate observations. Alternatively, we into account the duplicated observations by implementing a weighting scheme whereby the weight was simply 1/n where n is the number of duplicates, so we averaged the value of the repeated observations of our outcome variables used (wages and earnings, and hours worked). As the problem of duplicate observations within the panel of interest is not widespread, we did not expect to find any noticeable differences in the estimation results. Table 8 (part a) for wages and Table 8 (part b) for hours worked, using the new weighted database, show that the coefficient results were almost the same as the corresponding regressions that did not average over the duplicate observations (compared to results in Table 4, and 5 (part b), respectively).

V. Conclusions and policy implications

We estimated the effects of the minimum wage increase in January 2012 on wages and hours worked of low earners in Ecuador. A minimum wage increase can be seen as a treatment whereby those workers covered by the legislation –and effectively affected– receive the treatment (treated group), and other workers did not receive the treatment (control group). In Ecuador, all private-sector wage workers are covered by the minimum wage legislation; public sector or government employees are not covered, and by definition, neither are self-employed workers or business owners. Private wage workers affected by the minimum wage increase may have different characteristics from comparison groups due to non-observables like motivation and connections, among others, and observables like education, ethnicity and gender (we assumed these differences were constant over time). We addressed self-selection bias due to non-observables by applying difference-in-difference as an identification strategy using individual-level panel data constructed from a household panel. Our identification assumption also relied on the so-argued exogenous increase in the basic unified minimum wage and the exogenous variation in the minimum wages across industry and occupations.

This paper contributes to previous research in different ways. We studied the impacts of a minimum wage increase on wages and hours worked, the latter of which is scarcely addressed in the literature. Our focus on the effects of the minimum wage increase in a developing country such as Ecuador, led us to address additional issues, namely spillover effects on other groups of workers (uncovered workers and covered with high earnings), and noncompliance issues. We relied on the heterogeneous effect literature to address these issues. We also addressed potential sample selection bias for panel attrition.

The main results suggest that the minimum wage increase had a significant and positive effect on wages of low-income wage workers (affected), increasing their wages by 0.41% to 0.48% for each 1% increase in the minimum wage (after controlling for indirect effects), relative to the change in the unaffected workers' earnings. However, it may also have had negative effects on the income of high-earners thus suggesting an income-compression effect of the 2012 increase in sectorial minimum wages whereby employers may have tried to offset the cost of a minimum wage increase by freezing or even reducing the wages of higher income employees (see Lemos, 2002, 2004). The results also show that wage effects were felt differently by different types of workers. There were some negative wage effects that reduced (but did not eliminate) the positive impacts on income of female wage workers, and there were additional positive effects on the income of agricultural wage workers. These results on wages hold even when we controlled for workers working 30 to 50 hours per week. However, we also found negative impacts of the minimum wage increase on domestic workers' wages after controlling for full-time workers that reduced, without eliminating, the final increase in their wages.

These effects may suggest a problem of noncompliance. However, we also needed to analyze the impacts on hours, as some workers may have earned less because they worked fewer hours after the minimum wage increase.

As with the minimum wage effects on wages, the impact of the minimum wage increase on hours was also analyzed with indirect effects and differentiated impacts by type of worker. We found a significant increase in hours worked by affected workers relative to the hours worked by unaffected workers, although, in general, we did not find any significant indirect impacts or heterogeneous effects on hours worked. However, when we accounted for the intensity effect of the wage gap variable (the difference between the next-period minimum wage and the current-period income), we found a positive and significant effect on hours worked for young workers –which is in agreement with previous results in the literature. When accounting for these intensity effects, we also found spillover effects on hours for low-income earners in the control group; for this control group there was a significant and positive effect of the wage gap on their hours worked. However, when we controlled for those working between 30 to 50 hours per week, we found smaller impacts on hours worked by affected individuals working between 30 to 50 hours per week (full-time wage workers), but a larger positive and significant effect on hours of affected workers working either less than 30 hours or more than 50 hours. The only significant and negative impact on hours worked we found (that effectively reduces hours worked) was for female affected workers –relative to unaffected workers' hours. The latter may explain the positive, but smaller changes in wages of female workers that we find in the first set of estimations. Thus, the adjustment to a minimum wage increase may have come through the intensive margin for female workers.

There is a need for further work in analyzing the employment effects of increases in minimum wages. In a country where there is a high percentage of informal individuals (refer to the definition of informal worker indicated in the literature section), the relevant choice in working status may not be between just employed and unemployed, as there may be some positions in informal employment. In this case, we would need to disentangle the different choices or different working statuses to estimate the employment effects of a minimum wage increase. We leave this for future research. As recent literature has emphasized, there are also impacts of a minimum wage increase throughout the income distribution. We also leave this analysis for future work.

The differentiated impacts, on wages and hours worked, for certain groups of workers (female wage workers, wage workers of firms with no accounting records, and domestic workers) suggest venues for policy intervention. In particular, if female workers are unwillingly accepting a reduction in work hours, and thus wages (that are still growing with a minimum wage increase, but by less than the growth experienced by other affected groups of workers), and similarly for hours worked by domestic workers (in which case compliance with the labor legislation may be the problem), then policy makers may need to devise mechanisms that control and enforce the fulfillment of the hours worked and the minimum wage. If female workers willingly work less hours then there may not be a problem with compliance, and no policy action to recommend. These outcomes happened during a period of high growth in this economy, and it remains to be studied if during a downturn (like the current period of a bust in commodity prices for Ecuador) the minimum wage increases may exacerbate unemployment effects and reverse the gains achieved on reductions in income inequality (see Table 1). The government should be aware of such trade-offs and measure any negative impacts of the minimum wage policy on different groups to devise offset measures of such cost for these groups of workers. Our study should be considered just the tip of the iceberg of studies that should account for such effects and measures.

In the context of high-growth years, as the ones observed in this paper in Ecuador, we can conclude that increases in minimum wages were influential in boosting incomes and reducing inequality, but the question remains whether these results would continue in the current downturn era for Ecuador (as a bust in commodity prices sets in), in other words, would a policy of minimum wage increases in the downturn exacerbate unemployment effects and reverse the gains? Additional studies on the impacts of minimum wage increases are needed to address these questions concerning distributional impacts of minimum wage policies in Ecuador.

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Tables

Table 1. Basic unified minimum wage in Ecuador, 2006-2014

Year	Basic Unified Minimum Wage (BUMW) (US$)	Election [1] year under current government	Real GDP Growth (%) (2007=100)	Inflation rate[2] (%)	Non-oil Sectors Real GDP / PEA, Growth rate[3] (%)	Nominal BUMW, Growth rate (%)	Real BUMW[4] (US$) (2004=100)	Real BUMW Growth rate (%)	GINI
2006	160	n.a.	4.4	3.3	-	6.67	151.74	3.26	0.54
2007	170	n.a.	2.2	2.28	-	6.25	157.63	3.88	0.55
2008	200	yes	6.4	8.39	6.37	17.65	171.08	8.53	0.51
2009	218	yes	0.6	5.2	-1.83	9.00	177.32	3.65	0.50
2010	240	no	3.5	3.56	5.76	10.09	188.52	6.31	0.50
2011	264	no	7.9	4.47	6.07	10.00	198.49	5.29	0.47
2012	292	no	5.2	5.11	3.63	10.61	208.88	5.24	0.48
2013	318	yes	4.6	2.73	0.52	8.90	221.45	6.02	0.49
2014	340	no	3.7	3.59	3.75	6.92	228.57	3.21	0.47

Sources: Wages taken from Official Registry of the Republic of Ecuador, Inflation rate, GINI, and PEA taken from the National Institute of Statistics and Census (INEC), and GDP from the Central Bank of Ecuador.

Notes: 1.- n.a.= not applicable. In 2008 there was a referendum to vote for the approval or disapproval of the new Constitution backed by the current government; in 2009 and 2013 presidential elections took place. **2.-** Inflation rate corresponds to the monthly average inflation rate. Real minimum wage is calculated using the monthly average CPI per year (2004=100). **3.-** Economically active population (PEA, by its acronym in Spanish) corresponds to 15-64 year -old who worked at least one hour in the reference week, or even if they did not work, they had a job (employed), or those who were unemployed but were available for work and were seeking for a job (unemployed). PEA data corresponds to December of each year, except for 2014, that corresponds to June 2014. 4.- BUMW corresponds to the basic unified minimum wage or "salario básico unificado", the floor wage for the sectorial minimum wages that is increased in January o each year; see its real growth rate in the last column of this table.

Table 2. Average wages in relation to national minimum wage and share of wage workers

	2009		2010		2011		2012		2013	
Total										
Population	*2 737 418*	*100%*	*2 747 022*	*100%*	*2 706 757*	*100%*	*2 789 210*	*100%*	*3 158 232*	*100%*
mean	208,35	96%	222,27	93%	230,09	87%	241,42	83%	266,45	84%
median	162,68	75%	188,52	79%	198,48	75%	208,88	72%	221,45	70%
p10	68,33	31%	78,55	33%	90,22	34%	93	32%	104,46	33%
Male										
Population	*1 842 820*	*67%*	*1 878 190*	*68%*	*1 853 254*	*68%*	*1 883 319*	*68%*	*2 146 894*	*68%*
mean	218	100%	228,61	95%	235,24	89%	244,13	84%	274,11	86%
median	177,32	81%	188,52	79%	198,48	75%	211,03	72%	221,45	70%
p10	81,34	37%	94,26	39%	90,22	34%	107,3	37%	104,46	33%
Female										
Population	*894 597*	*33%*	*868 832*	*32%*	*853 502*	*32%*	*905 892*	*32%*	*1 011 338*	*32%*
mean	188,45	86%	208,57	87%	218,91	83%	235,78	81%	250,19	79%
median	162,68	75%	188,52	79%	195,47	74%	208,88	72%	217,97	69%
p10	65,07	30%	78,55	33%	90,22	34%	85,84	29%	83,57	26%
Youth (15 - 24 yr)										
Population	*750 454*	*27%*	*700 751*	*26%*	*623 540*	*23%*	*609 429*	*22%*	*682 711*	*22%*
mean	153,01	70%	165,08	69%	178,37	68%	190,88	65%	204,65	64%
median	146,41	67%	170,45	71%	180,44	68%	200,3	69%	203,34	64%
p10	65,07	30%	72,26	30%	75,18	28%	78,69	27%	83,57	26%
Monthly Min. Wage (US$)	218		240		264		292		318	

Source: Own construction using survey data (ENEMDU) from the National Institute of Statistics and Census (INEC).

Table 3. Descriptive statistics of panel data: treatment and control groups

	Affected	Control group	Total panel of interest
Percent affected by increases in real minimum wage (with respecto to total in panel of interest)	29.9%	n.a.	n.a.
Average real "wage gap" if affected	48.1%	n.a.	n.a.
Average real income 2012 (second) year	US$ 178.9	307.5	n.a.
Average nominal income 2012 (second) year	US$ 250.2	429.9	n.a.
Average hours per week	41.5	42.3	41.8
Average age in (second) year	36.2	43.5	40.7
Percentage female	33.1%	34.5%	35.9%
Percentage young (15 - 24yrs)	24.3%	7.1%	13.5%
Sample size	2,087	4,899	7,625
Population size (based on first year)	447,698	1,047,204	1,624,422

Source: Own calculations using the panel data created using the household survey data (ENEMDU) for December 2011 and December 2012.

Notes: n.a. = not applicable. Panel of interest are individuals in the panel with ages between 15 and 70-year old in the second year.

Table 4. Wage effects of minimum wages

Dependent variable: change in real wages or income, in logarithm
Treatment group: "affected"

	(0)	(1)	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)
affected	0.253 ***	0.443 ***	0.482 ***	0.450 ***	0.469 ***	0.470 ***	0.467 ***	0.449 ***	0.413 ***
	(0.024)	(0.034)	(0.035)	(0.033)	(0.055)	(0.045)	(0.046)	(0.034)	(0.038)
indirect_a		0.484 ***	0.476 ***	0.480 ***	0.558 ***	0.569 ***	0.571 ***	0.490 ***	0.489 ***
		(0.057)	-0.057	(0.049)	(0.063)	(0.064)	-0.064	(0.057)	(0.059)
indirect_b		-0.100 ***	-0.107 ***	-0.100 ***	-0.119 ***	-0.116 ***	-0.115 ***	-0.099 ***	-0.107 ***
		(0.032)	-0.032	(0.031)	(0.044)	(0.045)	(0.045)	(0.032)	(0.032)
int_affected*fem			-0.138 ***						
			(0.039)						
int_affected*you				-0.037					
				(0.041)					
int_affected*fsi					0.017				
					(0.048)				
int_affected*nor						0.057			
						(0.045)			
int_affected*nac							0.065		
							(0.045)		
int_affected*mai								-0.066	
								(0.077)	
int_affected*agr									0.125 ***
									(0.046)
Constant	0.057	-0.026	-0.033	-0.025	-0.056	-0.050	-0.047	-0.033	-0.027
	(0.056)	(0.046)	(0.046)	(0.045)	(0.059)	(0.062)	(0.062)	(0.048)	(0.046)
Inverse Mills Ratio	-0.162	-0.101	-0.066	-0.086	-0.273	-0.311 *	-0.318 *	-0.125	-0.111
	(0.175)	(0.182)	(0.181)	(0.153)	(0.185)	(0.189)	(0.190)	(0.183)	(0.190)
Number of observations	6864	6864	6864	6864	5397	5271	5271	6864	6864
Chi2	155.2	403	416.2	434.3	390.5	374.5	378.3	405.4	435.2

Notes: Standard errors in parenthesis. *** Significant at 1%. ** Significant at 5%. * Significant at 10%.

All wage regressions include controls for experience, industry, occupation, and region -as stated in the text. Variables for experience, industry and occupation are in most cases significant at the 1% level. The number of observations in the treatment group "affected" is 2087 (see Table 3). All regressions apply Heckman 2-step procedure. The Inverse Mills Ratio is not significant in most cases, and significant at 10 percent for both the regressions that try to control for firms with no accounting records (int_affected*nac) and the regressions for firms with not tax registration (int_affected*nor). Given that the Inverse Mills Ratio is not significant, we could estimate the regressions applying OLS; we did so (sample, sample with robust errors, and population weighted) and obtain similar coefficient results.

Table 5. Effects on hours worked

Dependent variable: change in weekly hours of work, in logarithm
Treatment group: "affected"

Part (a)	**(0)**	**(1)**	**(1a)**	**(2a)**	**(3a)**	**(4a)**	**(5a)**	**(6a)**	**(7a)**
affected	0.046 ***	0.063 ***	0.056 ***	0.067 ***	0.058	0.069 ***	0.061 **	0.063 ***	0.0584 ***
	(0.0130)	(0.019)	(0.020)	(0.019)	(0.031)	(0.025)	(0.026)	(0.019)	(0.022)
indirect_a		0.037	0.040	0.057 **	0.066 *	0.073 **	0.074 **	0.038	0.046
		(0.032)	(0.032)	(0.028)	(0.036)	(0.036)	(0.036)	(0.032)	(0.033)
indirect_b		-0.009	-0.007	-0.004	-0.011	-0.008	-0.008	-0.008	-0.008
		(0.018)	(0.018)	(0.018)	(0.025)	(0.025)	(0.025)	(0.018)	(0.018)
int_affected*fem			0.026						
			(0.022)						
int_affected*you				0.015					
				(0.023)					
int_affected*fsi					0.021				
					(0.028)				
int_affected*nor						0.019			
						(0.025)			
int_affected*nac							0.045 *		
							(0.025)		
int_affected*mai								0.012	
								(0.044)	
int_affected*agr									0.0316
									(0.026)
Constant	0.003	-0.018	-0.016	-0.012	-0.007	-0.002	0.001	-0.016	-0.015
	(0.031)	(0.026)	(0.026)	(0.026)	(0.034)	(0.035)	(0.035)	(0.027)	(0.026)
Inverse Mills Ratio	0.010	0.107	0.096	0.031	0.022	-0.001	-0.004	0.103	0.0752
	(0.095)	(0.103)	(0.102)	(0.086)	(0.106)	(0.106)	(0.107)	(0.103)	(0.107)
Number of observations	6843	6843	6843	6843	5380	5254	5254	6843	6843
Chi2	21.57	29.22	30.95	32.82	30.95	31.69	35.27	29.44	33.03

Part (b). Adding intensity effect: "wage gap"

	(0)	**(1)**	**(1a)**	**(2a)**	**(3a)**	**(4a)**	**(5a)**	**(6a)**	**(7a)**
affected	-0.043 **	-0.026	-0.027	-0.028	-0.042	-0.041	-0.042	-0.027	-0.028
	(0.018)	(0.024)	(0.024)	(0.023)	(0.028)	(0.028)	(0.028)	(0.024)	(0.024)
wagegap	0.056 ***	-0.008	-0.008	-0.009	-0.009	-0.010	-0.010	-0.008	-0.009
	(0.011)	(0.020)	(0.020)	(0.019)	(0.025)	(0.025)	(0.025)	(0.020)	(0.020)
int_affected*wagegap	0.131 ***	0.194 ***	0.199 ***	0.179 ***	0.189 **	0.196 ***	0.185 ***	0.195 ***	0.195 ***
	(0.021)	(0.026)	(0.028)	(0.027)	(0.057)	(0.035)	(0.036)	(0.026)	(0.029)
int_indirect_a*wagegap		0.096 ***	0.098 ***	0.099 ***	0.099 **	0.099 ***	0.100 ***	0.097 ***	0.0987 ***
		(0.028)	(0.028)	(0.027)	(0.033)	(0.033)	(0.033)	(0.028)	(0.028)
indirect_b		-0.013	-0.014	-0.015	-0.027	-0.027	-0.028	-0.013	-0.014
		(0.024)	(0.024)	(0.023)	(0.028)	(0.028)	(0.028)	(0.024)	(0.024)
indirect_c		-0.013	-0.014	-0.016	-0.031	-0.032	-0.033	-0.014	-0.015
		(0.027)	(0.027)	(0.025)	(0.031)	(0.031)	(0.031)	(0.027)	(0.027)
int_affected*wagegap*fem			-0.012						
			(0.027)						
int_affected*wagegap*you				0.071 **					
				(0.031)					
int_affected*wagegap*fsi					-0.001				
					(0.049)				
int_affected*wagegap*nor						-0.023			
						(0.031)			
int_affected*wagegap*nac							0.004		
							(0.031)		
int_affected*wagegap*mai								-0.013	
								(0.050)	
int_affected*wagegap*agr									-0.001
									(0.028)
Constant	0.053 *	0.028	0.030	0.031	0.056	0.056	0.059	0.028	0.0312
	(0.031)	(0.036)	(0.036)	(0.034)	(0.042)	(0.043)	(0.043)	(0.036)	(0.036)
Inverse Mills Ratio	-0.046	-0.062	-0.072	-0.093	-0.137	-0.140	-0.146	-0.070	-0.08
	(0.119)	(0.122)	(0.121)	(0.102)	(0.120)	(0.120)	(0.119)	(0.122)	(0.121)
Number of observations	6601	6601	6601	6601	5172	5051	5051	6601	6601
Chi2	178.05	186.41	186.7	195.24	141.04	133.71	133.51	186.52	186.68

Notes: Standard errors in parenthesis. *** Significant at 1%. ** Significant at 5%. * Significant at 10%.
All wage regressions include controls for experience, industry, occupation, and region. The number of observations in the treatment group "affected" is 2087 (see Table 3). All regressions apply Heckman 2-step procedure. The Inverse Mills

Ratio is not significant in all cases. Given that the Inverse Mills Ratio is not significant, we could estimate the regressions applying OLS and obtain similar coefficient results.

Table 6. Wage effects of minimum wages

Dependent variable: change in real wages or income, in logarithm
Treatment group: "affected"
With interaction term for those working between 30 to 50 hours per week in the first year

	(0)	(1)	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)
affected	0.384 ***	0.587 ***	0.636 ***	0.589 ***	0.633 ***	0.618 ***	0.619 ***	0.596 ***	0.5579 ***
	(0.038)	(0.042)	(0.044)	(0.041)	(0.065)	(0.056)	(0.057)	(0.042)	(0.045)
int_affected*h3050	-0.191 ***	-0.191 ***	-0.198 ***	-0.191 ***	-0.209 ***	-0.199 ***	-0.199 ***	-0.194 ***	-0.189 ***
	(0.039)	(0.038)	(0.038)	(0.038)	(0.045)	(0.048)	(0.048)	(0.038)	(0.038)
indirect_a		0.534 ***	0.526 ***	0.510 ***	0.605 ***	0.610 ***	0.615 ***	-0.089	0.5401 ***
		(0.057)	(0.057)	(0.048)	(0.063)	(0.063)	(0.063)	(0.078)	(0.058)
indirect_b		-0.084 **	-0.091 ***	-0.090 ***	-0.094 **	-0.093 **	-0.091 *	0.540 ***	-0.091 ***
		(0.033)	-0.032	(0.032)	(0.046)	(0.046)	(0.046)	(0.057)	(0.033)
int_affected*fem			-0.146 ***						
			(0.0390)						
int_affected*you				-0.036					
				(0.041)					
int_affected*fsi					0.001				
					(0.050)				
int_affected*nor						0.038			
						(0.047)			
int_affected*nac							0.036		
							(0.047)		
int_affected*mai								-0.083 **	
								(0.033)	
int_affected*agr									0.1183 ***
									(0.044)
Constant	0.058	-0.001	-0.007	-0.007	-0.032	-0.030	-0.027	-0.010	-0.002
	(0.053)	(0.046)	(0.046)	(0.045)	(0.059)	(0.062)	(0.062)	(0.048)	(0.046)
Inverse Mills Ratio	-0.147	-0.321	-0.282	-0.223	-0.488 **	-0.510 **	-0.529 ***	-0.345 *	-0.336
	(0.177)	(0.203)	(0.201)	(0.168)	(0.202)	(0.203)	(0.204)	(0.203)	(0.208)
Number of observations	6747	6747	6747	6747	5296	5172	5172	6747	6747
Chi2	155.33	511.11	528.73	541.28	451.33	432.92	434.66	511.42	531.62

Notes: Standard errors in parenthesis. *** Significant at 1%. ** Significant at 5%. * Significant at 10%.

All wage regressions include controls for experience, industry, occupation, and region -as stated in the text. Variables for experience, industry and occupation are in most cases significant at the 1% level. The number of observations in the treatment group "affected" is 2087 (see Table 3). All regressions apply Heckman 2-step procedure. The Inverse Mills Ratio is not significant in most cases. When that the Inverse Mills Ratio is not significant, we can estimate the regressions applying OLS.

Table 7. Effects on hours worked

Dependent variable: change in weekly hours of work, in logarithm
Treatment group: "affected"
With interaction term for those working between 30 to 50 hours per week in the first year

Part (a)	(0)	(1)	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)
affected	0.175 ***	0.202 ***	0.197 ***	0.203 ***	0.220 ***	0.225 ***	0.219 ***	0.203 ***	0.1992 ***
	(0.021)	(0.023)	(0.024)	(0.023)	(0.036)	(0.031)	(0.031)	(0.024)	(0.025)
int_affected*h3050	-0.182 ***	-0.181 ***	-0.181 ***	-0.182 ***	-0.208 ***	-0.204 ***	-0.203 ***	-0.182 ***	-0.181 ***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.025)	(0.026)	(0.026)	(0.021)	(0.021)
indirect_a		0.055 *	0.057 *	0.069 **	0.088 **	0.093 ***	0.096 ***	0.057 *	0.0641 **
		(0.032)	(0.032)	(0.027)	(0.035)	(0.035)	(0.035)	(0.032)	(0.032)
indirect_b		-0.004	-0.003	-0.001	-0.003	0.000	0.001	-0.004	-0.003
		(0.018)	(0.018)	(0.018)	(0.025)	(0.025)	(0.025)	(0.018)	(0.019)
int_affected*fem			0.017						
			(0.022)						
int_affected*you				0.013					
				(0.023)					
int_affected*fsi					0.013				
					(0.027)				
int_affected*nor						0.014			
						(0.026)			
int_affected*nac							0.032		
							(0.026)		
int_affected*mai								-0.012	
								(0.043)	
int_affected*agr									0.0199
									(0.025)
Constant	0.017	-0.006	-0.005	-0.002	0.008	0.009	0.012	-0.007	-0.003
	(0.029)	(0.026)	(0.026)	(0.026)	(0.034)	(0.035)	(0.035)	(0.027)	(0.026)
Inverse Mills Ratio	-0.032	0.043	0.034	-0.013	-0.065	-0.083	-0.095	0.036	0.0088
	(0.096)	(0.114)	(0.113)	(0.095)	(0.114)	(0.114)	(0.114)	(0.114)	(0.117)
Number of observations	6747	6747	6747	6747	5296	5172	5172	6747	6747
Chi2	92.42	110.22	111.16	113.26	105.27	97.94	100.53	110.36	112.54

Part (b). Adding intensity effect: "wage gap" and with interaction term for those working 30-50 hours per week in the first year

	(0)	(1)	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)
affected	-0.012	0.006	0.003	0.001	-0.005	-0.008	-0.007	0.005	0.0041
	(0.018)	(0.024)	(0.024)	(0.023)	(0.028)	(0.028)	(0.028)	(0.024)	(0.024)
wagegap	0.055 ***	-0.008	-0.008	-0.010	-0.009	-0.010	-0.010	-0.009	-0.009
	(0.011)	(0.019)	(0.019)	(0.019)	(0.025)	(0.025)	(0.025)	(0.019)	(0.019)
int_affected*wagegap	0.256 ***	0.319 ***	0.345 ***	0.309 ***	0.299 ***	0.302 ***	0.295 ***	0.321 ***	0.3137 ***
	(0.023)	(0.028)	(0.030)	(0.029)	(0.057)	(0.037)	(0.038)	(0.028)	(0.030)
int_affected*wagegap*h305	-0.311 ***	-0.310 ***	-0.318 ***	-0.308 ***	-0.308 ***	-0.301 ***	-0.301 ***	-0.311 ***	-0.311 ***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.029)	(0.031)	(0.031)	(0.026)	(0.026)
int_indirect_a*wagegap		0.096 ***	0.097 ***	0.102 ***	0.097 ***	0.100 ***	0.100 ***	0.097 ***	0.0989 ***
		(0.028)	(0.028)	(0.027)	(0.033)	(0.033)	(0.033)	(0.028)	(0.028)
indirect_b		-0.012	-0.014	-0.015	-0.026	-0.027	-0.027	-0.013	-0.014
		(0.024)	(0.024)	(0.023)	(0.027)	(0.028)	(0.028)	(0.024)	(0.024)
indirect_c		-0.012	-0.014	-0.017	-0.027	-0.032	-0.032	-0.013	-0.015
		(0.026)	(0.026)	(0.025)	(0.031)	(0.031)	(0.031)	(0.026)	(0.026)
int_affected*wagegap*fem			-0.058 **						
			(0.027)						
int_affected*wagegap*you				0.047					
				(0.031)					
int_affected*wagegap*fsi					0.013				
					(0.049)				
int_affected*wagegap*nor						0.016			
						(0.031)			
int_affected*wagegap*nac							0.027		
							(0.031)		
int_affected*wagegap*mai								-0.026	
								(0.049)	
int_affected*wagegap*agr									0.0139
									(0.027)
Constant	0.055 *	0.028	0.030	0.035	0.054	0.057	0.057	0.028	0.0326
	(0.031)	(0.036)	(0.036)	(0.033)	(0.042)	(0.043)	(0.043)	(0.036)	(0.036)
Inverse Mills Ratio	-0.056	-0.065	-0.078	-0.111	-0.123	-0.148	-0.146	-0.074	-0.088
	(0.117)	(0.120)	(0.120)	(0.101)	(0.118)	(0.118)	(0.118)	(0.120)	(0.120)
Number of observations	6601	6601	6601	6601	5172	5051	5051	6601	6601
Chi2	326.31	335.73	339.59	339.62	256.75	232.38	233.34	335.75	335.64

Notes: Standard errors in parenthesis. *** Significant at 1%. ** Significant at 5%. * Significant at 10%.
All wage regressions include controls for experience, industry, occupation, and region. The number of observations in the treatment group "affected" is 2087 (see Table 3). All regressions apply Heckman 2-step procedure. The Inverse Mills

Ratio is not significant in all cases. Given that the Inverse Mills Ratio is not significant, we could estimate the regressions applying OLS and obtain similar coefficient results.

Table 8. Wage and hours effects of minimum wages, using weighted database

Part (a), Dependent variable is the change in real wages or income, in logarithm
Part (b), Dependent variable is change in weekly hours of work, in logarithm. This part applies the intensity effect variable ("wage gap")
Treatment group: "affected"

Part (a)	**(0)**	**(1)**	**(1a)**	**(2a)**	**(3a)**	**(4a)**	**(5a)**	**(6a)**	**(7a)**
affected	0.252 ***	0.440 ***	0.480 ***	0.447 ***	0.468 ***	0.470 ***	0.466 ***	0.447 ***	0.4102 ***
	(0.024)	(0.034)	(0.036)	(0.033)	(0.055)	(0.045)	(0.046)	(0.034)	(0.038)
indirect_a		0.483 ***	0.475 ***	0.477 ***	0.559 ***	0.569 ***	0.571 ***	0.489 ***	0.4874 ***
		(0.057)	(0.057)	(0.049)	(0.063)	(0.064)	(0.064)	(0.057)	(0.059)
indirect_b		-0.101 ***	-0.108 ***	-0.102 ***	-0.119 ***	-0.116 ***	-0.116 ***	-0.100 ***	-0.109 ***
		(0.032)	(0.032)	(0.031)	(0.044)	(0.045)	(0.045)	(0.032)	(0.032)
int_affected*fem			-0.136 ***						
			(0.038)						
int_affected*you				-0.040					
				(0.041)					
int_affected*fsi					0.018				
					(0.048)				
int_affected*nor						0.059			
						(0.045)			
int_affected*nac							0.067		
							(0.044)		
int_affected*mai								-0.065	
								(0.077)	
int_affected*agr									0.1277 ***
									(0.045)
Constant	0.056	-0.026	-0.032	-0.026	-0.055	-0.049	-0.046	-0.032	-0.027
	(0.056)	(0.046)	(0.046)	(0.045)	(0.059)	(0.062)	(0.062)	(0.048)	(0.046)
Inverse Mills Ratio	-0.157	-0.097	-0.063	-0.072	-0.270	-0.308	-0.315 *	-0.121	-0.105
	(0.175)	(0.182)	(0.181)	(0.152)	(0.185)	(0.189)	(0.189)	(0.183)	(0.190)
Number of observations	6865	6865	6865	6865	5397	5271	5271	6865	6865
Chi2	154.26	402.28	415.29	432.95	391.14	375.39	379.21	404.68	435.02
Part (b): Hours effects	**(0)**	**(1)**	**(1a)**	**(2a)**	**(3a)**	**(4a)**	**(5a)**	**(6a)**	**(7a)**
affected	-0.042 **	-0.026	-0.028	-0.028	-0.042	-0.042	-0.043	-0.027	-0.028
	(0.018)	(0.024)	(0.024)	(0.023)	(0.028)	(0.028)	(0.028)	(0.024)	(0.024)
wagegap	0.056 ***	-0.007	-0.008	-0.008	-0.009	-0.009	-0.010	-0.008	-0.008
	(0.011)	(0.020)	(0.020)	(0.019)	(0.025)	(0.025)	(0.025)	(0.020)	(0.020)
int_affected*wagegap	0.130 ***	0.192 ***	0.197 ***	0.177 ***	0.185 ***	0.195 ***	0.184 ***	0.194 ***	0.1932 ***
	(0.021)	(0.026)	(0.028)	(0.027)	(0.057)	(0.035)	(0.036)	(0.026)	(0.029)
int_indirect_a*wagegap		0.095 ***	0.096 ***	0.098 ***	0.097	0.097 ***	0.098 ***	0.096 ***	0.0971 ***
		(0.028)	(0.028)	(0.027)	(0.033)	(0.033)	(0.033)	(0.028)	(0.028)
indirect_b		-0.014	-0.015	-0.016	-0.029	-0.029	-0.029	-0.015	-0.016
		(0.024)	(0.024)	(0.023)	(0.028)	(0.028)	(0.028)	(0.024)	(0.024)
indirect_c		-0.014	-0.015	-0.017	-0.032	-0.033	-0.034	-0.015	-0.016
		(0.027)	(0.027)	(0.025)	(0.031)	(0.031)	(0.031)	(0.027)	(0.027)
int_affected*wagegap*fem			-0.011						
			(0.027)						
int_affected*wagegap*you				0.070 **					
				(0.031)					
int_affected*wagegap*fsi					0.002				
					(0.049)				
int_affected*wagegap*nor						-0.023			
						(0.031)			
int_affected*wagegap*nac							0.005		
							(0.031)		
int_affected*wagegap*mai								-0.012	
								(0.050)	
int_affected*wagegap*agr									-2E-04
									(0.028)
Constant	0.055 *	0.030	0.032	0.033	0.059	0.059	0.061	0.031	0.0335
	(0.031)	(0.036)	(0.036)	(0.034)	(0.042)	(0.043)	(0.043)	(0.036)	(0.036)
Inverse Mills Ratio	-0.047	-0.057	-0.068	-0.086	-0.130	-0.133	-0.139	-0.066	-0.074
	(0.119)	(0.122)	(0.121)	(0.102)	(0.119)	(0.119)	(0.119)	(0.122)	(0.121)
Number of observations	6602	6602	6602	6602	5172	5051	5051	6602	6602
Chi2	178.65	186.5	186.8	195.34	141.33	133.92	133.75	186.62	186.78

Notes: Standard errors in parenthesis. *** Significant at 1%. ** Significant at 5%. * Significant at 10%.
All wage regressions include controls for experience, industry, occupation, and region. All regressions apply Heckman 2-step procedure. The Inverse Mills Ratio is not significant in most cases.

Appendices

APPENDIX 1

Treated and Control Groups: Group comparability

In order to conduct the before and after analysis, we needed to find evidence that the two groups (treatment and control) were comparable over time in the absence of the treatment (recall the key assumption of common trend for the difference-in-difference estimations). When comparing the two groups, ideally we wanted to rule out other influences (other than changes in the minimum wage) such as industry developments (or others that may be omitted variables), overall trends in outcomes, or changes in the reporting of the survey questionnaire (measurement errors) as explanations of the outcomes of interest (changes in wages and hours worked). In our study we achieved this by including variables that control for industry development (1-digit ISIC), occupation (1-digit ISCO), and region. It is also important to recall that we analyzed the impacts of a change in the minimum wage that took place in January 2012 using panel data from December 2011 as the before period, and panel data from December 2012 as the after period. We want to have the panel data close enough to the date of the change in the minimum wages to capture the impact of the minimum wage on the labor outcomes of interest.

To address the comparability of the treatment and control groups, we grouped individuals in the national employment survey (ENEMDU) as follows:

(1) Wage workers (treatment): workers subject to the minimum wage legislation, which in the survey data included private-sector wage workers, domestic workers, "jornaleros", and "tercerizados".

(2) Control workers: workers not subject to the minimum wage legislation. In the survey data they included government employees, self-employed, and "patrones" (business owners).

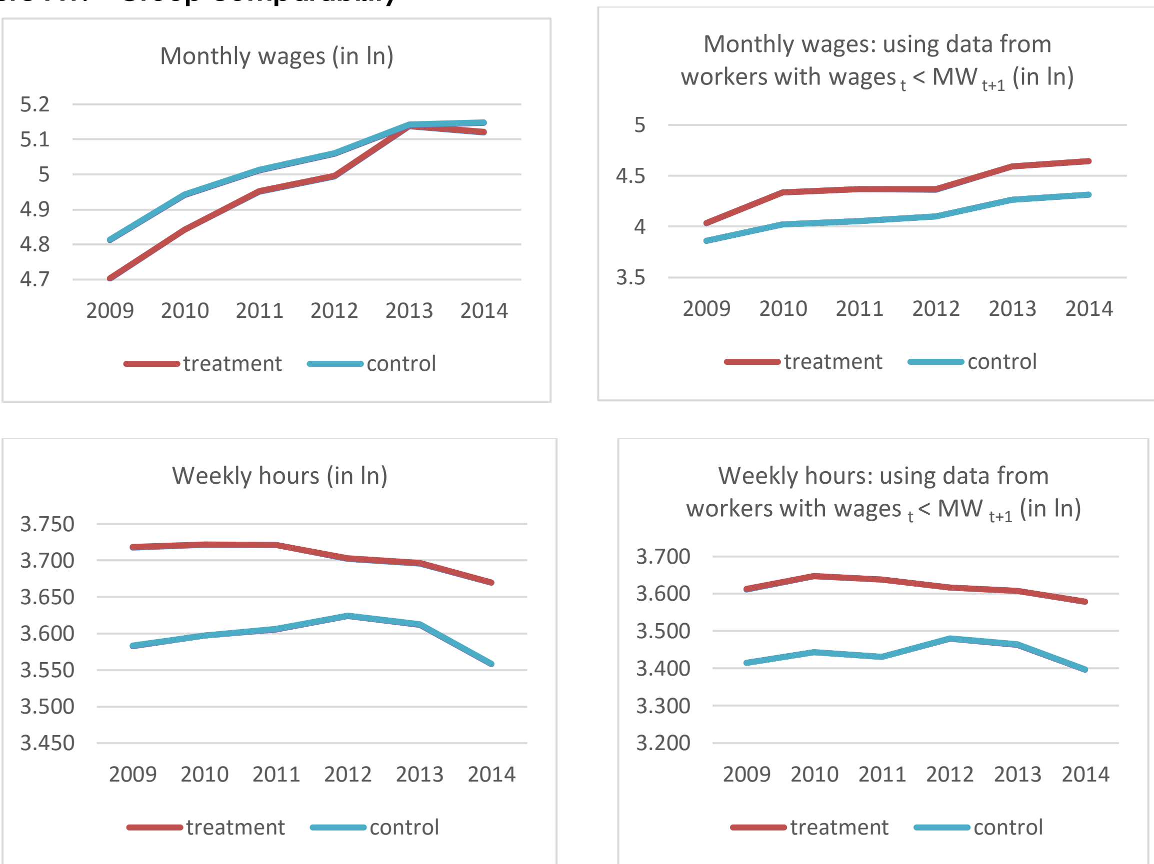
To address the comparability of the treatment and the control groups we used a couple of procedures. Firstly, we visually compared trends in outcomes (wages and income, hours worked[23]) between the treatment and the control groups using data from December 2009 to December 2014; that is, we included a few periods before and after the two periods under study (December 2011 and December 2012). By doing so, we wanted to visually examine whether data from the two groups moved in parallel for each of the indicators (income and hours worked). We also checked these outcomes of interest for the low-earnings groups (i.e. only for individuals whose wages or income were below the next-period minimum wage, both for the treatment and the control groups). The results show that there were parallel movements in income, in particular for the period up to 2012 when we used the whole survey data, and in particular throughout 2014 when using data for the low-earnings workers only. For hours worked, the parallel movements between the two groups hold as well, although for the period 2011 and 2012, the two lines apparently tend to approach to each other –in both cases, when using the whole survey data and when using only hours worked by low earners.

Secondly, we were able to perform a test for differences in means of these variables between treatment and control groups. Although it is common to focus on analyzing mean differences between the treatment group and the control group, such an indicator may be influenced by the presence of

[23] As mentioned before, hours worked throughout this study comprised of weekly hours in the main job. Most individuals had only one job, and there may have been just a few who occasionally had another job. We did not take into account earnings or hours from this second job. The employment survey reports up to two jobs.

extreme values and thus give a completely different picture, despite the groups not being much different. Therefore, other summary statistics may be of interest such as the median or the 75th percentile. However, from the graph with upper and lower bounds (not shown in Figure A1), it is clear that the values for the treatment and control groups do not intersect, therefore there are significant differences in the mean values of the two groups.

Figure A1.- Group comparability



Source: Own construction using data from the national employment survey (ENEMDU). This survey is administered by the National Institute of Statistics and Census.

We could check if the groups that differ in the mean value of the variable of interest responded to other factors (e.g. control by systematic variation as mentioned in Meyer (1995) and discussed in Rosenbaum (1987): "If the groups do respond similarly, it would support the assumption of no omitted interactions and the converse if they do not"(Meyer, 1995, p. 157). In any event, as discussed before, the lines of the two groups are parallel, suggesting that the responses of the two groups were similar. In our regressions we introduced controls for variables that may explain differences in their behavior (e.g. industry and occupation effects).

It is important to mention that we used a set of workers as the control group: government employees, self-employed, and business owners –none of whom were covered by the minimum wage legislation. As mentioned by Meyer (1995), adding up comparison groups reduce the importance or biases or random variation in a single comparison group (Meyer 1995, p. 157). This author suggests that the more comparison groups a study has, the better. We had three groups as comparison pooled into one (self-employed, owners, and government employees).

Another explanation in systematic differences between the two groups might be due to a nonrandom exit from the sample (in the employment survey and/or the construction of the panel data at the individual level from the household level survey), or because of the way we constructed the sample for the regression (taking changes in two periods so that there might have been individuals who dropped out). We accounted for this potential source of attrition bias by applying the Heckman two step procedure.

APPENDIX 2

Matching algorithm[24]

The construction of the panel at the individual level entails the pairing of observations which are the same individual throughout different periods (December 2011 and December 2012). The ENEMDU survey, administered by the National Institute for Statistics and Census (INEC), has an identification system which allowed us to identify each household inside a physical house and then to identify each member inside the household. The way INEC constructs the unique individual identifier, however, is far from problem-free and required us to develop our own matching procedure to eliminate the inconsistencies with the survey identification system.

There were two broad categories of inconsistencies. The first of which was different households with the same identifier: Because the survey identifier is partly based on physical houses, if a family had decided to move out, the replacing family would adopt their unique identifier. In these cases, most characteristics within members of the household would have been inconsistent between years as in fact all the observations in the household were from different people.

The second category of inconsistencies was the same individuals with different identifiers: Because the survey identifier in also based on a sequential number of people surveyed within a household, if for any reason the people were not surveyed in the same order as in previous years, then the survey identifier at the household level would be the same, but the individuals inside the household would have different identifiers. We encountered this situation when a new family member was included or any family member stopped living in the household. In conclusion, every time the household structure changed the sequence of surveys was changed and although we might have had the same individuals, they had different identifiers.

In both scenarios the INEC identifier for any two panel observations was the same but the characteristics of each observation tended to change enormously: observations aging irrational amounts of years or even rejuvenating, and people doing doctoral studies in a 1-year time frame; and on a more serious note, but equally unlikely (in theory, from one year to the following), people drastically changing ethnicity or gender.

[24] This appendix rests heavily on a note prepared by José Salcedo about the panel construction for a study on the domestic workers' labor market impacts of the social security enrollment mandate, a research study by the author within the GDN sponsorship. See Wong (2013).

For these reasons, we developed an algorithm, or more precisely a procedure, that tries to eliminate, or at least reduce, these inconsistencies in the panel while preserving the accuracy of the information on the dataset.

The procedure starts by assuming that the comparison set – for the pairing – must be between households identified as the same under the survey households' identifier and pooling both periods. It then proceeds to group members according to the variables which we believed had the least unexpected variability between years: gender, race, age and schooling years.

Notice that the first two variables should not change on a regular basis while the last two will experiment some progression between periods. This idea lays out the foundation for the matching procedure and pairs observations in the panel data when they belong to the same household, and when the four variables behave as expected between years: gender and race do not change and age and schooling years increase according to the reasonable progression limits, within 0 to 2 years.

It is important to note that although the variables gender, age and schooling can be objectively verified, the answer pertaining to race is based on self-opinion and could be influenced in the way it is framed. We argue this to be true as careful selection of households show entire families' race swaps. To overcome this difficulty, we created broader categories that bundle together similar races, for example, black and afro-Ecuadorian.

We feel compelled to disclose the main argument against the implemented procedure. Although it solves important issues with the survey identifier, it creates problems of its own; most importantly, the observations deemed as panels are sort-dependent (when there are duplicates). We controlled for different ways of pairing up the members in the household and we determined that the cases prone to a different match were negligible.[25]

From this procedure we finished with around 36% (of total observations) individuals in our dataset that were in the panel and all analyses are based on them. See Table A1.

Table A1. Statistics by panel and non-panel of interest (individual-level data)

	2011		2012	
Individuals in population of interest (15-70 year old in the second year)	panel	nopanel	panel	nopanel
Women	581,499	1,708,759	582,959	1,769,180
	35.8%	39.3%	35.9%	39.9%
Youth (15-24)	249,804	735,180	218,716	707,613
	15.4%	16.9%	13.5%	16.0%
Average real income (US$/month)	266.2	265.8	271.4	274.9
Average work hours (per week)	42.0	41.4	41.8	40.8
Average age (years)	39.8	39.7	40.7	40.3
Average years of education	11.7	11.3	12.0	11.4
Total	1,624,422	4,351,678	1,623,781	4,432,402

Source: Own calculations using the household survey data for employment in Ecuador (ENEMDU).

[25] There are about 500 pairs of duplicate observations between 2011 and 2012, but most of them belonged to individuals outside the range of age of interest (15 to 70 years).

Finally, to account for the sorting and elimination of duplicate observations, we created another panel dataset in which instead of eliminating duplicated observations we kept them and weighed the observations (for the outcome variables of interest), whereby the weight is simply 1/n where n is the number of duplicates. In this way, we attempted to incorporate the uncertainty about the matching procedure within the model randomness by a weighting scheme.