The Impact of Lowering the Payroll Tax on Informality in Colombia

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Abstract

In 2012, the Colombian Government reduced payroll employer contributions from 29.5% to 16%. Two years later, the informality rate had diminished by about 4.0 p.p. This paper attempts to estimate how much of this reduction was due to the tax reform, isolating the impact of other macroeconomic variables. A natural approach to performing this task is to apply a Differences in Differences methodology using a household survey panel. Since the Colombian survey does not have a panel structure, we simulated one using the Matching Difference in Differences methodology (Heckman, Ichimura and Todd, 1997). According to the results, the tax reform is associated with a 4.8 p.p. decrease in the informality of the workers affected by the reform in the 13 main metropolitan areas, approximately half the reduction of the relevant informality rate during that

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period, affecting mostly salaried men and workers in general with low levels of education.

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I. Introduction

In 2012, the Colombian government reformed the tax law (Ley 1607, 2012) by reducing payroll contributions from 29.5% to 16% of the monthly wage and substituting them with a profit tax. The reform only affected the payments made by the employers/firms of two or more workers that earn wages between one and ten times the minimum wage, and did not change the amount of taxes or contributions payable by the workers. NGOs, the government, unipersonal businesses, and self-employment were also excluded from the reform. From a fiscal perspective, the source of the contributions was substituted with a profit tax (CREE[2]) under the hypothesis that it is preferable to tax capital than to tax labor.

In December 2014, two years after the law was passed, labor-informality rates in the 13 main Colombian metropolitan areas (henceforth 13-areas) had dropped

[2] *Contribución sobre la Renta para la Equidad y el Empleo*

from 56% to 52%.[3] Including smaller cities and rural areas, the reduction went from 68% to 64%. These results hold using different measurements of informality. The period after the reform was also characterized by high, yet diminishing growth rates, changes in the tax rates, and increasing real minimum wages. What we are mainly interested in is knowing how much of the reduction in the informality rate was due to the tax reform.

Two empirical facts provide support for the hypothesis that part of the recent reduction of informality in Colombia was due to the tax reform and not only to growth. The first is that the relationship between growth and informality weakened after the reform. The coefficient of correlation between the output gap and informality was -0.9 between 2001 and 2012, and -0.7 when calculated for the 2001–2015 period,[4] signaling that something other than growth had influenced informality in recent years. The second is depicted by the change in the informality rates of the groups included in the reform after its implementation. The standardized informality rate of workers that earn between one and ten minimum wages decreased significantly after the reform, when

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[3] *All the data regarding informality in this section are based on the Great Integrated Household Survey (Gran Encuesta Integrada de Hogares, GEIH) of the National Department of Statistics, DANE and they use the legal measurement of informality. Details are provided in Section III.*

[4] *Output gap from Fedesarrollo, both correlations are significant at 1%. This estimation makes use of the firm definition of informality (see section III) since the series are longer. The correlation between the firm definition and the labor market legal definition across time is 0.93.*

compared to the informality rate of the workers outside this bracket, as shown in Figure 1.

[insert Figure 1]

This paper attempts to formally estimate how much of the reduction in the informality rate was due to the tax reform, isolating the impact of other macroeconomic and regulatory variables. A natural approach to perform this task is to apply a Difference in Difference (DID) methodology using a household survey panel. In fact, the change in the difference of the informality rate of the workers affected by the reform, and of those who were not, provides an estimate of the impact of the reform, netting, among other things, the change in those macroeconomic conditions that affected the workers included in the reform and those who were not, in a similar fashion. The Differences in Differences technique has been widely used in the labor market. One of the most well-known papers is Card and Krueger's (1994), which analyses the impact of the increase in the minimum wage in New Jersey on employment in fast food restaurants. On informality, Bergolo and Cruces (2011) also applied a Difference in Differences technique to estimate the impact of an increase in the coverage of health services for dependent children of private sector salaried workers on informality rates. Also Slonimczyk (2011), in a very similar setting to the one presented in this paper, found that a 17% reduction in payroll taxes in Russia in 2001 reduced the informality rate by between 2.5% and 4%.

Since the Colombian Household Survey does not have a panel structure, we estimated the impact of the reform by using a Matching Difference in Differences (MDID) method with repeated cross sections, as suggested by Heckman et al. (1997). This method simulates an experiment by matching the treated and not treated population before and after the reform. The mix of Differences in Differences and matching techniques has not been widely used in the literature. One notable exception is the evaluation of training programs as in Blundell; Costa-Dias; Meghir and Van Reenen (2004); and Bergemann, Fitzenberger and Speckesse (2004). Encina (2013) too used this method to analyze the impact of the pension reform on the labor participation outcomes in Chile; Villa, Fernandes and Bosch (2015) applied the MDID to estimate the impact of behavioral interventions on the self-employed informality rate, and Betcherman and Pages (2007) used a synthetic panel to find that a one percentage point decrease (increase) in the labor cost ratio (formal to informal) results in a 2.2 percentage point rise (fall) in the fraction of jobs that are registered.

According to our MDID estimations, the 2012 Colombian tax reform can be associated with a 4.8 p.p. reduction in the 13-areas informality rate of the workers affected by the reform. This is equivalent to a reduction in the overall informality rate of around 2.1 p.p., provided that the treated population was 43% of the 13 areas working population in 2014. The reduction in informality rates was greater for men than it was for women, for urban than for rural workers; and for those with lower levels of education. Our estimated results compare relatively well with previous literature, but the magnitude of the effects is rather on the lower side. Anton (2014) estimated that the 2012 tax reform in Colombia increased formal employment by between 3.4 and 3.7%, and decreased informal employment by between 2.9 and 3.4%. Similarly, recent studies sponsored by the IADB (Steiner and Forero (2016), Kugler and Kugler (2015); and Bernal, Eslava and Meléndez, 2016) found that the reform increased the absolute number of formal jobs by between 200.000 and 800.000 employments (an increase of the number of formal jobs of between 3.1% and 3.4% with respect to December 2012), and increased wages from 1.9% to 4.4%. Previous work on payroll taxes in Colombia has found similar results. Kugler and Kugler (2009) found that an increase of 10% in payroll taxes leads to an increase in informal employment of between 4% and 5%, and Mondragon et al. (2010)

found, that an increase of 10% in payroll contributions was correlated to an increased probability of informality ranging between 5 and 8 p.p.

Our estimated results were robust to different specifications. The data also indicates that before the reform, the outcome of affected and non-affected workers evolved similarly, signaling that the divergence in series might be associated with the reform. However, three issues might affect the interpretation of our results. First, recent formalization policies applied to the self-employed might be biasing our results, as self-employment is an important share of the control group and was not affected by the tax reform. To control for this possible bias, we estimated the MDID over a sample of only salaried workers (both in the control and in the treatment groups). The results showed even higher effects of the tax reform on informality of salaried workers, though those effects were less robust than those observed in the full sample, in terms of some of their statistical characteristics. Second, some workers that earned less than the minimum wage before the tax reform may have marginally increased their income coinciding with the tax reform, which would imply that they shifted from the control group to the treated group, explaining some of our estimated impact. We argue that this possible effect goes in exactly the same direction as the spirit of the reform, which sought a reduction of informality and increased wages paid to workers through a reduction in the tax wedge, making it unnecessary to isolate the impacts. And third, the decrease in the payroll taxes after the 2012 tax reform was accompanied by an increase in the minimum wage, that cannot be easily isolated from the payroll tax; therefore, as the increase in the minimum wage may have reduced the positive impact of the reduction in the payroll tax on the informality rate, our results may be underestimating their total effect.

In sum, discounting the caveats made, the reduction in payroll taxes seems to be responsible for some of the recent reduction in Colombian informality rates. This result is important not only for Colombia but for other countries facing high hiring costs and debating whether payroll taxes should be either waived entirely or exchanged for other taxes to reduce informality. To formally present our estimation of the impact of the reform, Sections II to V of this paper explain the methodology, present the data, show the results and discuss the limitations of the estimation. Section VI concludes the article.

II. The DID and MDID methods

One of the most adequate methodologies for evaluating the impact of the tax reform on informality, while isolating the impact of growth and other macroeconomic variables over time, is the Differences in Differences (DID) method. This method, applied to the informality framework, involves dividing the population into two groups: one affected by the reform, the *treated* group, and the other unaffected by the reform, the *control* group. The change in the probability of informality within the control group is then compared with the change observed in the probability of informality within the treated group. By taking the difference between these changes –or the Difference in Differences- one isolates structural differences between the groups and factors that affect both groups simultaneously, such as macroeconomic conditions, assuming that the impact of unobservable variables on informality is evenly spread between the two groups. The DID equation, over repeated cross sections can be written in two ways. One of them is the traditional Ordinary Least Squares (OLS) notation.

$$INF_{it} = \beta_0 + \beta_1 Y_t + \beta_2 T_{it} + \beta_3 (T_{it} \times Y_{it}) + \beta_4 X_{it} + u_{it} \quad (1)$$

where i refers to the individual, $t=0$ refers to the value of variables before the reform and $t=1$ to the value of variables after the reform, INF_{it} is a binary variable that takes value 1 if person i at time t is an informal worker and zero if he or she is a formal worker; Y_{it} (Year) is a dummy variable that takes the value of zero in the baseline period and a value of one in the period after the reform; T_{it} (Treated) is a dummy that takes the value of one if the individual is from the treatment group and zero if not; and X_{it} refers to the observable characteristics of each individual i at time t, as the probability of an individual being informal changes according to his observable characteristics. The second way to write an equation for changes in informality of a repeated cross section uses Heckman et al.'s (1997) notation:

$$DID = \{E(INF_{it=1} | D_{it=1} = 1, T_{it=1} = 1, X_{it=1}) - E(INF_{it=1} | D_{it=1} = 0, T_{it=1} = 0, X_{it=1})\} - \{E(INF_{it=0} | D_{it=0} = 0, T_{it=0} = 1, X_{it=0}) - E(INF_{it=0} | D_{it=0} = 0, T_{it=0} = 0, X_{it=0})\} \quad (2)$$

where DID, is the Difference in Differences estimation; $D_{it=0}$ is the treatment indicator in the DID setting, and $E\ (INF_{it=1} | D_{it}, T_i\ , X_{it})$ is the average outcome by group. Villa (2016) clearly illustrates the equivalence between the two notations.

Ideally, the DID framework should be applied over a panel data. However, if a panel structure is not available, it can also be applied over repeated cross sections, but the estimations suffer from multiple limitations since the model assumes common time effects across groups,[5] and no changes to the composition of each group, which are difficult assumptions to prove in a

[5] *In fact, the model can control for non-observable individual specific effects and non-observable macroeconomic effects because they cancel one another out, but not for non-observable temporary individual specific effects.*

repeated cross section (Blundell & Costa Dias, 2009). To reduce these limitations, Heckman et al. (1997) devised the so-called method of 'Matching Differences in Differences' (MDID). As in the DID approach, MDID compares the differences in outcomes between the treated and control groups over time. However, by creating counter-factuals of the control and the treated groups, in the MDID approach, the difference among the two groups before and after the reform provides information about the impact of the reform, isolating other effects that may have affected both the treated and the control groups.[6]

There are multiple ways to find a counterfactual for the individuals in each of the four groups: the control and the treated groups before and after the reform. The method used to perform the match in this paper is the kernel propensity score matching, following Heckman et al. (1997). This method does not take single individuals, but averages of individuals weighted by their propensity score of being treated. As suggested by Rubin and Rosembaum (1983), matching on the propensity score is equivalent to matching on co-variables, without losing degrees of liberty in the estimation. The kernel method has the advantage of

[6] *One of the advantages of this matching over the standard panel is that we can control the change in the observable characteristics of the individuals that might affect their probability to be informal over time, such as getting married, being older, more educated etc. They also suffer much less from typical panel data problems such as attrition and non-response (Verbeek, 2008).*

reducing variance and making use of most of the available information. The Kernel propensity score MDID can be explained as follows:

$$DID = \{E(INF_{it=1} | D_{it=1} = 1, T_{it=1} = 1) -$$
$$E(INF_{it=1} | D_{it=1} = 0, T_{it=1} = 0) \times W^{c}_{it=1}\} -$$
$$\{E(INF_{it=0} | D_{it=0} = 0, T_{it=0} = 1) -$$
$$E(INF_{it=0} | D_{it=0} = 0, T_{it=0} = 0) \times W^{c}_{i,t=0}\} \times W^{t}_{it=0} \quad (3)$$

where, W_{it} are the kernel weights that estimate the distance between the propensity score of each observation in the treated group after the reform and the propensity score of each observation in the three other groups (the control groups before and after the reform and with the treated group before the reform) giving the highest weight to those with p-scores closest to the treated individual. The treated group after the reform is assigned a value of 1. Note that the expected values of informality are no longer controlled by observed characteristics since the weights already include this information. Therefore, the second stage of the MDID only estimates an OLS using weights and without co-variables. Depending on the researcher's preferences, the procedure can make use of all the p-score information available, or trim it to an area where there is available information for both the treated and the control groups (common support).

In sum, by mixing the methodologies, Matching and Differences in Differences, we can control for differences in the composition of the treated group before and after the treatment. This is not possible in a single DID approach, and therefore is more suitable for repeated cross-sections.

III. Data description:

Survey: The data set used in this paper is from the *Gran Encuesta Integrada de Hogares* (GEIH, 2008-2015), provided by the Colombian National Department of Statistics (Departamento Nacional de Estadística de Colombia, DANE). This survey collects information for an average of 20,669 households a month, which makes it representative on a monthly basis at national level and for the 13 main metropolitan areas. It includes information about household members' incomes and labor status. Most of the exercises that follow, use the 13 main metropolitan areas sample -13 areas - which is more representative and more commonly used by the Colombian authorities.[7] However, we also checked the results for the whole sample. This Survey does not interview the same individuals across time.

Period of analysis: The implementation of the Law involved several milestones. Most of the discussions were held between October and November 2012, the

[7] *The GEIH total aggregate covers 23 cities with their rural areas, gathering information on more than 62 thousand individuals per month, of which more than 23 thousand are in the 13 metropolitan areas aggregate. These areas represent 51% of total working population.*

Law was passed in December 2012, the waiver over part of the payroll taxes became effective in May 2013, and the reform was fully implemented on January 1st 2014[8]. We defined our period of analysis from 2012 (January to December), before the implementation of the reform (t=0), to 2014 (January to December), after the implementation of the reform (t=1).

Dependent variable: Throughout this analysis, we mostly applied the legal definition of informality in which informal workers include those who do not make contributions to either health or pension schemes. However, we checked the robustness of the exercises by also applying the so called "firm definition" of informality, in which informal workers include those employed in firms with no more than five employees, unpaid family helpers or housekeepers, self-employed except for independent professionals and technicians, and business owners of firms with no more than five workers. Results are similar. The results were more conclusive when estimated using the legal definition.

Treatment: The treatment group in our exercise included all workers that were directly impacted by the reduction in payroll taxes. According to the law, this

[8] *Out of the reduction of 13.5 pp in payroll taxes, 5 pp, corresponding to the contributions to the National Learning Service (Servicio Nacional de Aprendizaje, SENA) and the National Institute of Family Welfare (Instituto Nacional de Bienestar Familiar, ICBF) became effective in May 2013, while the waiver of the additional 8.5 pp over the health service contributions became effective on January 1st, 2014.*

includes workers that earn between one and ten minimum wages excluding NGOs, the government, unipersonal businesses, and the self-employed. After all these exclusions, the reform only covered 43% of the 13-areas' working population in the follow up period. Our control group includes all other workers. The 13-areas sample provides 219,058 observations in the treated group and 126,671 observations in the control group. Given that the control group is rather diverse, and contains groups that do not share the same logic as the reform's target group -as the government or the self-employed- we performed another exercise in which we restricted our universe to the private salaried workers. Figure 2 presents the standardized series of the treated and control groups for the 13 areas sample, the sample that includes rural areas, and the sample that restricts the sample to salaried workers. All cases, show a significant reduction of the informality rate of the treated group over the control group, after the reform was implemented.

[insert Figure 2]

Co-variables: According to the MDID setting, all the co-variables chosen should affect both the treatment and the outcome variable, without predicting it perfectly but, at the same time, exogenous to both. Hence, we included the control variables that according to Fernandez and Villar (2016) have a greater impact on informality in the regression. We did not include the income related variables or the type of occupation since they did not satisfy the requirement of being exogenous to the treatment, or the anticipation of it. The list of co-variables used is the following:

Gender: We separated the impact of being women when they are registered as spouse from the impact of being women when they are heads of household, daughters, etc., since the two groups have different preferences for formality.

Age: We included dummy variables in the regression for workers younger than 25 and older than 50, leaving workers between 25 and 50 years old as the base group.

Education: We included a dummy in the regression for workers with primary education or less, another for workers with tertiary education, and a third for workers who had completed high school, leaving workers with middle and high school studies as the base group.

City: The equation includes dummy variables for workers who live in the three biggest cities and those who live in border cities where informality often goes hand-in-hand with smuggling.

Rural/urban: When we used the whole country sample that included rural and urban areas, we included the probability of being in the rural areas and in the main 13 metropolitan areas as a co-variable. The base group being the less populated urban areas.

Weights As suggested by Dugoff et al. (2014), it is a good idea to include the expansion weight of each-individual in the survey as a control variable in the MDID estimation as it can account for variables that may capture relevant factors -such as where individuals live, their demographic characteristics, and perhaps their availability to respond to surveys- that might intercede in the estimation of informality, but are burdensome to include in the estimations.

Months: We included all months, minus one in the estimations to simulate a month to month matching.

Tables 1a, 1b and 1c present, for 2012, the mean difference between the weighted treated and non-treated groups in the 13-areas, full survey, and 13-areas salaried aggregates. The data shows significant differences in observable characteristics between the treated and control groups that make the informality rates impossible to compare directly, justifying the use of a DID approach when panel data is available or, alternatively, an MDID approach when it is not.

[insert Tables 1a, 1b and 1c]

IV. Applying the MDID approach to the Colombian case:

Using the data and the framework explained in previous sections, we performed three main estimations: (1) MDID estimation for the 13-areas weighted sample; (2) MDID estimation for whole weighted samples, and (3) MDID estimation for the salaried workers in the 13-areas weighted sample. In performing these estimations, we applied the kernel (Epanechnikov) propensity score, using weights and no common support to better approximate the national results and to respect the data generation process, and month/city clusters to reduce the problem of autocorrelation, as suggested by Bertrand, Duflo, and Mullainathan (2004).[9]

[9] *According to the authors, the standard errors in Difference-in-Difference estimations (DD) are underestimated and inconsistent due to severe serial correlation problems caused by three factors. DD estimations with long time series, outcome variables serially correlated, and treatment variables changing very little within groups. However, we argue that this serial correlation problem is less severe in our data, because we use only two periods: before and after the payroll reform.*

Table 2 presents the results of these estimations.[10] According to Table 2, in the 13 Metropolitan areas (Est.1), the control group showed an informality rate of 71.6% before the tax reform (2012), that decreased to 71.3% after the reform (2014). Meanwhile, the treated group reduced its informality rate from 28.5% to 23.5%. The difference between the control and the treated group in the baseline was -43.1 p.p. and in the follow up -47.8 p.p., meaning that the Difference in Differences estimator is -4.8 p.p. This indicates that the reform can be associated with a reduction of the total informality rate of 2.1 p.p., considering that the weighted participation of the control group in the population is 43%.

Table 2 also shows the exercises over the whole sample (Est.2) and the salaried 13-areas sample (Est.3). The estimated impact on the treated group using the whole national sample is lower than the 13-areas estimate (-4.0 vs. -4.8 p.p.), a result that can be explained by a lower impact of the reform on the rural population. The higher impact obtained in the 13-areas salaried sample vs the whole 13-areas sample (-5.1 vs. 4.8 p.p.) might be related to the positive results of the monitoring and control policies applied to self-employment in recent years. However, care should be taken with these two estimations (Est. 2 and

[10] *The Stata code that we used to apply the MDID with was designed by Villa (2016). Annex 1 presents the estimation of the p-score for being treated, used for the matching procedure in the MDID and the 13-areas aggregate. We also estimated all the exercises using the firm definition of informality, with similar results. These are available upon request.*

Est.3) since the validity of the assumptions required by the model proved to be weaker in these cases, as will be shown in the next section.

[insert Table 2]

As shown in Table 3, we also performed the MDID exercise per socio-economic group, using the 13 areas sample specifications to allow comparisons. According to the results, women and workers with tertiary education tend to be less affected by the reform. Following Fernandez and Villar (2016), this can be explained by the fact that these two groups tend to show higher preferences for informality, explained by the amenities of being informal as flexibility and independence, and therefore their decision to be informal is proportionally less driven by monetary factors. We also reduced the men’s sample to workers earning less than two minimum wages. The higher observed impact when reducing the sample (-6.8 vs -5.0 in the full male’s sample) can be explained by the fact that the reform removed a constraint that was bigger for minimum wage earners compared to workers receiving higher levels of income, where wages are more flexible. This result is coherent with the higher impact we found for workers with lower levels of education.

[insert Table 3]

In sum, according to the results, the reform seemed to have a relatively strong impact on the target group. The next section, presents the robustness and limitations of these estimations and results.

V. Robustness of Results

The results obtained in the previous section proved to be relatively robust to the sample used in the estimation. Table 4 shows that these results are also robust to changes in the basic specifications of the MDID, such as the use of weights, clusters and common support. As expected, with respect to the 13 areas sample, the weights have an impact on coefficients, the use of clusters affects the standard errors, and common support reduces the number of observations. However, all these changes are minimal. The relatively bigger impact that we found in the equation without controls -which is the same as a DID equation estimated with OLS- corroborates the importance of using MDID instead of DID in this specific setting. We now turn to analyze the robustness of these results in terms of the validity of the assumptions behind the MDID model.[11] For this purpose, we will refer to the 13-areas sample, the full sample that includes rural and small urban areas, and the 13-areas salaried workers sample.

[insert Table 4].

[11] *See Blundell et al. (2009) and Lechner (2011) on MDID assumptions.*

Parallel trends. Perhaps the most critical assumption of the MDID approach is parallel trends. This feature ensures that, in the post treatment period, the impact is caused by the reform and not by other factors or trends linked to the fact of belonging to either the treated group or the control group. According to this assumption, unobservable variables such as growth, should affect the outcome variable (informality) of the treated and control groups in a parallel (but not equal) fashion. In other words, if parallel trends hold, in the absence of the treatment (the tax reform) both populations would have experienced the same time trends, conditional on co-variables. Figure 2 in the introduction of this paper, shows that the treated and control groups broadly behaved similarly before the reform was implemented and diverged after.

A simple OLS regression over the 2009-2015 period, simulating a reform for each of these years, allows us to identify the changes in time able to generate significant divergence in the informality rate of the treated and the control population, as in Autor (2003). We simulated reforms comparable to the 2012 reforms, that involve three years: one pre-reform, one for implementation, and another post-reform. More specifically, we simulated reforms implemented in 2010, 2011, 2012, 2013 and 2014, with a dummy variable one year ahead.[12] Formally, Equation [1] can be rewritten as follows:

$$INF_{it} = \beta_0 + \beta_1 Y_t + \beta_2 T_{it} + \beta_3 X_{it} + \sum_{k=2010}^{k=2014} \beta_k \left(T_{it} \times D_{k+1}\right) + u_{it} \quad (4)$$

where D_{k+1} is a dummy variable that takes the value of one in the period after the reform. Unfortunately, in applying this exercise, we had to control by observable characteristics (or use DID) instead of using the matching (MDID), running with all the limitations explained in Section II, because it is not clear how the weights of the matching can be estimated and included in Equation 4. According to the results shown in Table 5, in the 13 areas sample, the 2012-2014 reform -that we have considered so far- had a significant impact on the informality rate. The 2013-2015 reform also being significant means that it took some time for the reform to reach full impact, after it was implemented. The 2009-2011, 2010-2012 and 2011-2013 reforms were not significant, confirming that the parallel trend assumption holds on the series. This is also the case for the salaried 13-areas sample. When rural and small cities are included, the coefficients of earlier reforms are significant but the impact is rather small.

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[12] *As we are using dummies, coefficients should be understood as relative to the missing dummies (2009 and 2010), or relative to reforms implemented in 2008 and 2009.*

[insert Table 5]

However, probably the most accurate test to prove parallel trends in an MDID approach is the Placebo test. For this, the MDID method is applied to any other year with similar external characteristics, faking the existence of a tax reform or a similar shock, with the expectation that the results will not be affected. We performed this exercise using 2012/2010 and 2012/2009 as alternative periods, simulating a reform that took one year and another that took 2 years to be fully implemented. In contrast with the years for which we performed our base exercise (2014/2012), this alternative should reflect the impact of an inexistent tax reform. According to Table 6, in the 13-areas sample, we obtained no significant differences between the treatment and control groups in the results on informality. However, results are less clear in the other two samples, confirming the results of the previous exercise.

[Insert Table 6]

Exogeneity of the treatment. (Ashenfelter's dip). A common criticism of the Difference in Differences models with matching, and particularly with MDID with cross-sections, is the fact that they have a treated/untreated variable endogenous to the policy implemented. This identification problem has been largely analyzed in the literature (Abbring and Van den Berg (2003), Blundell et al. (2009) and Lechner (2011)) and is one of the downsides of using Matching Difference in Differences that does not control for unobserved individual-specific shocks that may influence the participation decision. A similar problem in another context might be easier to understand: A benefit program is implemented in two neighboring towns and individuals migrate to the town where the program is implemented to obtain the benefits.

There are two clues that indicate that some workers entered the treated group to obtain the benefits of the reform. The first is that the percentage of formal workers in the treated group did indeed increase from 41% to 43%, during the period of analysis. This increase can be explained by a reduction of workers that earned less than a minimum wage in the control group. The second is that when we observe the histograms of wages, presented in Figures 3a, 3b and 3c, we can see that after the reform, there is an increase in the frequency of workers earning a minimum wage, and a decrease in the preceding bracket.

[insert Figures 3a, 3b and 3c]

Unfortunately, we do not have panel data to observe the number of formal workers that transit from control to treatment. However, the direction of the bias that they create goes in the same direction as the spirit of the law. In the case of the lower bound, the impact on the lower threshold was not only positive but matched the purpose of the reform exactly: to reduce the labor cost and to make it more affordable to earn the minimum wage. It is, in a way, a channel through which the reform reduced informality. This is a desirable result, as “quasi-formal workers” that worked in the informal sector earning less than a minimum wage moved to the formal bracket and are likely contributing to health and pension. This problem is very different from cases in which, for example, the individual does not accept a job to qualify to get an employment benefit or to what can happen in the upper limit of the Colombian reform (more the 10 minimum wages): workers reporting an income of less than 10 minimum wages to obtain access to the benefits. In the case of Colombia, only 0.8% of the workers earn more than 10 times the minimum wage, so the movements in this segment caused by the reform are not significant.

Quality of the matching. The robustness of the results also depends on the matching creating a counter-factual, or in other words how similar the treated and control groups are after the matching. The composition of these two groups after the matching (Table 7) contrasts with the wide-ranging differences observed before the matching (Tables 1a, 1b and 1c), and shows the effectiveness of the p-score kernel matching. In most cases, the standardized difference of means is lower than 5%, complying with Rosenbaum and Rubin's (1985) rule of thumb.[13.] However, it should be considered that since we are working with p-score matching instead of one-to-one matching, the average bias of the co-variables is what matters the most. In all three samples, the average bias is less than 1%, broadly fulfilling Rosenbaum and Rubin's criteria.

[insert Table 7]

Common support. A key assumption of the MDID procedure is the overlap of the region of common support between the treatment and the control group. It rules out the perfect predictability of the treatment, given that workers with the same characteristics (X_{it}), might have a positive probability of being both participants and non-participants

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[13] *With the exception of the tertiary education variable, and diploma in the whole sample, that are on the limit.*

(Heckman, LaLonde, and Smith, 1999). In other words, we require that: [0 < $P(D_{it}) = 1|X_{it}) < 1$]. To prove common support, a visual analysis is suggested by Caliendo et al. (2005). According to Blundel et al. (2009), in the MDID model, the p-score distribution after the reform should be compared with the three other control groups (treatment before the reform and control before and after the reform). Figures 3a, 3b and 3c show that the p-score regions of the treated and the non-treated groups overlap in the 3 samples and concerns the perfect predictability of the treatment given the observable characteristics are ruled out.[14]

[insert Figures 3a, 3b and 3c]

Corroborant to the previous result is the fact that when the MDID exercises were applied with and without common support, the number of trimmed observations was minimal (0.01%) and the differences in the outcome were only reflected in minimal changes in the standard errors and no differences were observed in the coefficients.

Identification of the unobservable change that might have impacted the reform: Our discussion in previous sections attempts to find the impact of

[14] *The last two distributions are almost equal because the distribution does not change much across years.*

an unobservable change that affected the treated but not the control group of workers. So far, we have interpreted this change as the reduction in payroll taxes, but it can be related to other regulatory or macroeconomic changes affecting the treated group but not the control group. The general increase in income taxes that accompanied the reduction in payroll taxes should have affected the treated and the control group similarly; however, even if the effect was greater on the treated group, we claim that the effect of this increase in income taxes would be to induce informality rather than reduce it. Therefore, our results suggest that the reduction in payroll taxes help to reduce informality even if they are replaced by higher income taxes.[15] In the case of the increase of the minimum wage, it is more difficult to isolate the impact, as it mostly affected the workers targeted by the reform. During 2012 and 2014, the minimum wage increased annually by 1.8% in real terms (in comparison with 1.1% between 2007 and 2011). This increase in the minimum wage should have induced an increase in informality. The impact of the minimum wage is rather difficult to isolate from the reform, but according to Forero and Steiner (2016), the general

[15] *Some argue that the creation of the CREE might have offset the impact of the reform, since one tax was replaced by other. The impact of the reform that we found in the previous section goes against this claim.*

impact of the tax reform on informality would have been one percentage point greater if the minimum wage had not increased.

In sum, we found that after the 2012 reform, there was a greater reduction of the informality rate of the workers treated by the reform, when compared to the informality rate of non-treated workers. This difference in behavior contrasts with what happened in previous years, which suggests that there was causality. This can be partially caused by some workers that earned less than the minimum wage, and who marginally increased their income to obtain the advantages of the reform; this was the exact purpose of the reform, meaning that this mechanism reinforces the idea that the reform reduced informality. It is especially remarkable that this happened despite the coincidence with elements that should have opposite effects on informality, such as the large increase in the minimum wage. We also found that the results that expand the sample over the whole national population (including smaller cities and rural areas) and those that restrict it to the salaried population should be taken with extreme care, as their robustness proved to be weaker, at least with respect to the parallel trend assumption.

VI. *Conclusions*

This paper attempts to isolate the impact of the 2012 reform on informality from the impact of other macroeconomic and regulatory changes. Given that the Colombian Household survey does not have a panel structure, we used a Matching and Difference in Differences (MDID) methodology to estimate the impact. After the tax reform was implemented in Colombia, the informality rate diminished by 4 p.p. We argue that approximately half of this reduction is related to the waiver in the payroll contributions adopted by the government. We also found that some of this reduction might be explained by workers that before the reform earned less than a minimum wage and subsequently became fully paid workers to obtain the benefits of the reform. Results were more robust using the 13 main metropolitan areas aggregate than when expanding the survey to include the smaller cities and rural areas, or when limiting the sample to the salaried workers, although the impact was higher in this case. The reform also showed a higher impact on men and workers with low levels of education.

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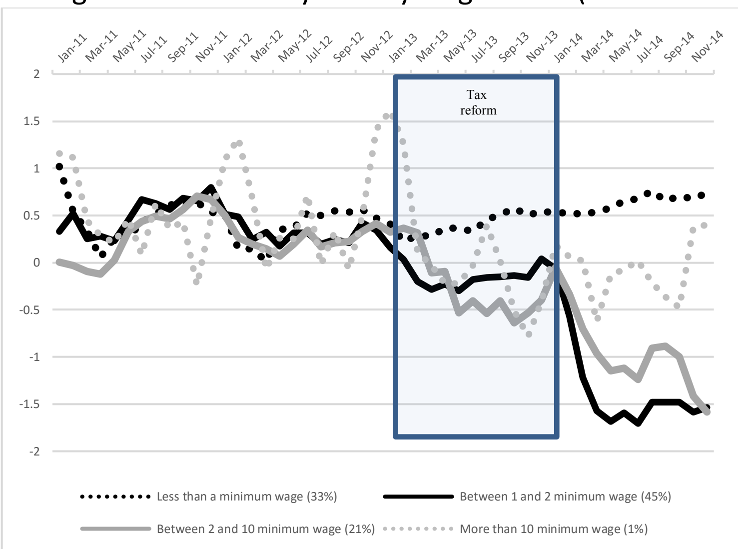
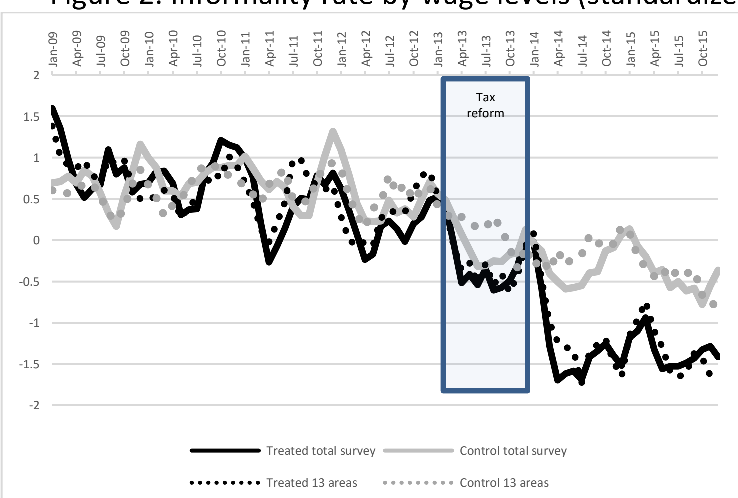
Tables and Figures

Figure 1: Informality rate by wage levels (standardized series)



Source: GEIH. 13 metropolitan areas, own calculations
Note: In parenthesis, the shares of each group among the occupied.

Figure 2: Informality rate by wage levels (standardized series)



Source: GEIH. 13 metropolitan areas, own calculations

Table 1a. Wald test on mean differences between treated and control groups. Weighted 13 areas survey

Variables	Mean control	Std. error (control)	Mean treated	Std. error (Treated)	Diff	Adjusted Wald test (F-statistic)	Prob.>F
Informality (legal)	0.76	0.002	0.27	0.003	0.49	22596	0.000
Women. Second earner	0.20	0.002	0.13	0.002	0.07	684	0.000
Women. Head or other	0.31	0.002	0.26	0.003	0.05	217	0.000
Less than 25	0.16	0.002	0.17	0.002	-0.01	16	0.000
More than 50	0.26	0.002	0.12	0.002	0.14	2645	0.000
Elementary or less	0.26	0.002	0.13	0.002	0.13	2114	0.000
Tertiary or more	0.29	0.002	0.41	0.003	-0.11	1008	0.000
Diploma	0.53	0.002	0.73	0.003	-0.20	3200	0.000
Big city	0.63	0.002	0.75	0.002	-0.12	2107	0.000
Border city	0.06	0.001	0.03	0.000	0.03	1746	0.000
Self- employed	0.63	0.002	0.00	0.000	0.63	81265	0.000
Salaried	0.18	0.002	0.89	0.002	-0.71	81111	0.000
Less than 1 min. wage	0.54	0.002	0.01	0.001	0.53	50934	0.000
More than 10 min. wage	0.05	0.001	0.00	0.000	0.05	2880	0.000
Between 1 and 10 min. wage	0.41	0.002	0.99	0.001	-0.58	62270	0.000
Observations	112110		61164				

Source: GEIH, own calculations

Table 1b. Wald test on mean differences between treated and control groups. Weighted full survey

Variables	Mean control	Std. error (control)	Mean treated	Std. error (Treated)	Diff	Adjusted Wald test (F-statistic)	Prob.>F
Informality (legal)	0.83	0.001	0.33	0.002	0.50	34055	0.000
Women. Second earner	0.19	0.001	0.12	0.002	0.07	1284	0.000
Women. Head or other	0.26	0.001	0.22	0.002	0.04	278	0.000
Less than 25	0.20	0.001	0.17	0.002	0.02	102	0.000
More than 50	0.25	0.001	0.13	0.002	0.12	2901	0.000
Elementary or less	0.40	0.002	0.20	0.002	0.20	5126	0.000
Tertiary or more	0.20	0.001	0.34	0.002	-0.14	2857	0.000
Diploma	0.40	0.002	0.64	0.003	-0.24	6612	0.000
Big city	0.26	0.001	0.48	0.003	-0.22	5770	0.000
Border city	0.03	0.000	0.02	0.000	0.01	379	0.000
13 met. Areas	0.41	0.002	0.65	0.003	-0.24	6100	0.000
Rural	0.27	0.002	0.13	0.002	0.14	3105	0.000
Self- employed	0.62	0.002	0.00	0.000	0.62	141081	0.000
Salaried	0.14	0.001	0.83	0.002	-0.69	86242	0.000
Less than 1 min. wage	0.68	0.002	0.02	0.001	0.66	160624	0.000
More than 10 min. wage	0.03	0.001	0.00	0.000	0.03	3768	0.000
Between 1 and 10 min. wage	0.29	0.001	0.98	0.000	-0.69	183785	0.000
Observations	260026		100169				

Source: GEIH, own calculations

Table1c Wald test on mean differences between treated and control groups. Weighted salaried workers

Variables	Mean control	Std. error (control)	Mean treated	Std. error (Treated)	Diff	Adjusted Wald test (F-statistic)	Prob.>F
Informality (legal)	0.61	0.005	0.21	0.003	0.40	4605	0.00
Women. Second earner	0.17	0.004	0.13	0.002	0.04	69	0.00
Women. Head or other	0.34	0.005	0.27	0.003	0.07	162	0.00
Less than 25	0.32	0.005	0.19	0.002	0.13	567	0.00
More than 50	0.13	0.004	0.09	0.002	0.03	75	0.00
Elementary or less	0.15	0.004	0.12	0.002	0.03	66	0.00
Tertiary or more	0.39	0.005	0.41	0.003	-0.02	14	0.00
Diploma	0.65	0.005	0.74	0.003	-0.09	262	0.00
Big city	0.68	0.004	0.75	0.002	-0.07	292	0.00
Border city	0.05	0.001	0.03	0.001	0.02	197	0.00
Less than 1 min. wage	0.72	0.005	0.01	0.001	0.71	18947	0.00
More than 10 min. wage	0.05	0.003	0.00	0.000	0.05	333	0.00
Between 1 and 10 min. wage	0.23	0.005	0.99	0.001	-0.76	25251	0.00
Observations	19673		53799				

Source: GEIH, own calculations

Table 2. MDID matching results
(baseline=2012, follow up=2014)

	(1)	(2)	(3)
Dependent variable	Informality (legal)	Informality (legal)	Informality (legal)
Sample	13-areas	Full sample	Salaried 13 areas
Mean control t (0)	0.716	0.735	0.573
Mean treated t (0)	0.285	0.315	0.220
Mean control t (1)	0.713	0.722	0.593
Mean treated t (1)	0.235	0.263	0.189
Diff-in-Diff (p.p.)	**-4.78*** **	**-3.97*** **	**-5.14*** **
Standard errors	(0.00595)	(0.00421)	(0.0143)
R-squared	0.21	0.195	0.152
% treated 2014 population	43.0%	32.4%	78.2%
Impact on relevant informality rate	-2.1	-1.6	-3.8
Observations	345,729	716,914	149,709
Control 2012	112110	260026	19673
Control 2014	106948	249071	16655
Treated 2012	61164	100169	53799
Treated 2014	65507	107648	59582

Source: GEIH, own calculations
Note: All the estimations used kernel matching (Epanechnikov), weights, no common support and month/city clusters.

Table 3. MDID matching results for some socio-economic groups. 13 areas. Workers 25-50 years old
(baseline=2012, follow up=2014)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	informality (legal)	informality (legal)	informality (legal)	informality (legal)	informality (legal)	Informality (legal
Socio-economic group	Males	Females	Males, primary school or less	Males, middle and high school	Males, tertiary education or more	Males less than 2 minimum wages
Diff-in-diff (p.p.)	**-5.01*** **	**-3.10*** **	**-9.78*** **	**-7.51*** **	**-3.16*** **	**-6.8*** **
Standard errors	(0.00751)	(0.00663)	(0.0157)	(0.0108)	(0.00929)	(0.010)
R-squared	0.196	0.214	0.258	0.265	0.123	0.32
Observations	109,480	102545	20,126	54,149	35,205	81136

Source: GEIH, own calculations
Note: All the estimations were applied to workers between 25 and 50 years old in the 13 areas and using kernel matching (Epanechnikov), weights, no common support and month/city clusters.

Table 4. MDID matching results. Robustness to different specifications
(baseline=2012, follow up=2014)

	(1)	(2)	(3)	(4)	(5)
	Original	No weights	No clusters	Common support	No controls
Dependent variable	Informality (legal)	Informality (legal)	Informality (legal)	Informality (legal)	Informality (legal)
Diff-in-Diff (p.p.)	**-4.78*** **	**-4.76*** **	**-4.78*** **	**-4.8*** **	**-2.42*** **
Standard errors	(0.00595)	(0.00589)	(0.00303)	(0.006)	(0.006)
R-squared	0.21	0.202	0.209	0.21	0.25
Observations	345,729	345,729	345,729	345,711	345,729

Source: GEIH, own calculations
Note: All the estimations were applied over the 13 areas sample.
Original: Estimation 1, Table2. Calculated using kernel matching (Epanechnikov), weights, no common support and month/city clusters. The other estimations vary in their specifications according to the title of the estimation.

Table 5. OLS 2009-2015

		(1)			(2)			(3)		
		Informality (legal)			Informality (legal)			Informality (legal)		
		13 areas-sample			Full sample			Salaried-13 areas sample		
		Coef.		SE	Coef.		SE	Coef.		SE
Constant		4.352	***	[0.713]	1.619	***	[0.416]	5.756	**	[1.824]
Year		-0.002	***	[0.000]	0.000		[0.000]	-0.002	**	[0.001]
Treated		-0.425	***	[0.002]	-0.394	***	[0.002]	-0.348	***	[0.003]
Impact of the reform (p.p.)	2013-2015	-2.608	***	[0.346]	-3.979	***	[0.295]	-1.083		[0.568]
Impact of the reform (p.p.)	2012-2014	-2.377	***	[0.337]	-4.004	***	[0.293]	-1.385	**	[0.495]
Impact of the reform (p.p.)	2011-2013	-0.275		[0.327]	-1.330	***	[0.291]	0.201		[0.429]
Impact of the reform (p.p.)	2010-2012	0.318		[0.320]	-0.761	**	[0.289]	0.2		[0.373]
Impact of the reform (p.p.)	2009-2011	0.165		[0.311]	-0.565	*	[0.288]	-0.301		[0.326]
Observations		1193947			2469176			509855		
F		21178			38271			640		
R2		0.36			0.40			0.26		

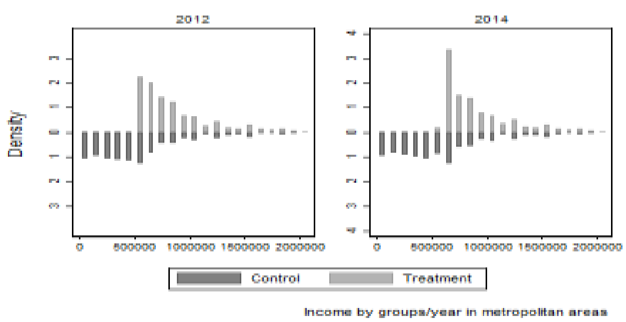
Source: GEIH, own calculations
Note: These results were controlled by the co-variables detailed in Section III. Similar results were obtained when controls were not included.

Table 6. Placebo Test.

	Period	Diff-in-Diff (p.p.)	Std. errors	Observations	R-squared
13-areas sample	2012-2014	-4.78 ***	(0.006)	345729	0.21
	2012-2010	0.033	(0.005)	339128	0.18
	2012-2009	-0.27	(0.005)	333848	0.18
Full sample	2012-2014	-3.97 ***	(0.004)	716914	0.19
	2012-2010	-0.86 **	(0.004)	704921	0.17
	2012-2009	-1.48 ***	(0.004)	692787	0.17
Salaried 13-areas sample	2012-2014	-5.14 ***	(0.014)	149709	0.15
	2012-2010	4.5 ***	(0.014)	142075	0.15
	2012-2009	1.9 *	(0.013)	139698	0.14

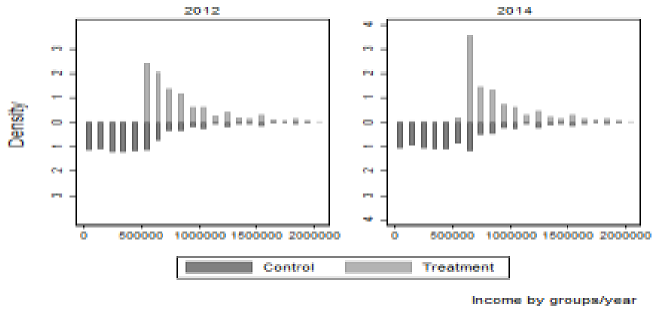
Source: GEIH, own calculations
Note: These results were controlled by the co-variables detailed in section III, similar results were obtained when controls were not included.

Figure 4a. Histogram of wages before (2012) and after the reform (2014). 13 areas, unweighted



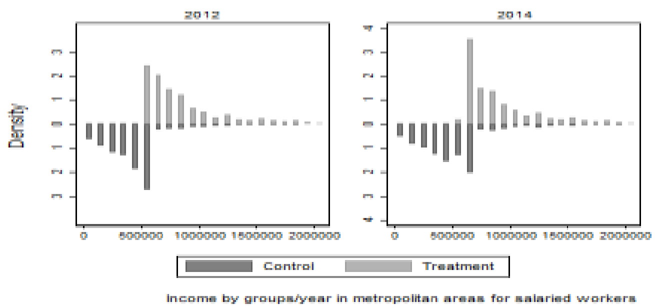
Source: GEIH and own estimations.

Figure 4b. Histogram of wages before (2012) and after the reform (2014). 13 areas, unweighted



Source: GEIH and own estimations.

Figure 4c. Histogram of wages before (2012) and after the reform (2014). 13 areas, unweighted



Source: GEIH and own estimations

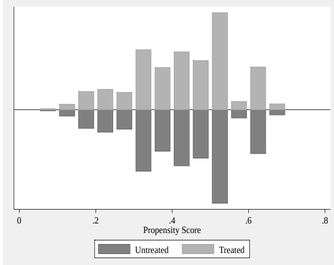
Table 7. Quality of the matching

Sample	13-areas			Sample including rural and small cities			13-areas, salaried		
Dependent variable: Treated	Mean in treated	Mean in control	STD diff	Mean in treated	Mean in control	STD diff	Mean in treated	Mean in control	STD diff
Women (spouse)	0.12	0.12	1.0%	0.12	0.1	4.0%	0.12	0.12	0.0%
Women (other)	0.27	0.27	-0.5%	0.26	0.25	3.4%	0.29	0.3	-3.9%
Less than 25	0.17	0.19	-2.9%	0.17	0.16	2.3%	0.19	0.2	-2.4%
More than 50	0.12	0.12	0.9%	0.13	0.13	-1.3%	0.1	0.1	1.3%
Primary (-)	0.12	0.12	-1.6%	0.14	0.15	-3.5%	0.11	0.12	-3.4%
Tertiary (+)	0.42	0.39	5.3%	0.41	0.38	7.0%	0.43	0.41	5.0%
Diploma	0.75	0.74	2.3%	0.73	0.71	5.3%	0.77	0.76	2.4%
Big city	0.37	0.36	1.3%	0.22	0.21	3.9%	0.37	0.36	1.6%
Border city	0.08	0.08	0.0%	0.07	0.07	0.2%	0.08	0.09	-3.9%
January	0.08	0.08	0.8%	0.08	0.08	1.4%	0.08	0.08	2.0%
February	0.08	0.08	-0.5%	0.08	0.08	-1.0%	0.08	0.08	-0.8%
March	0.08	0.08	-0.2%	0.08	0.08	-0.7%	0.08	0.08	-1.1%
April	0.08	0.08	-0.4%	0.08	0.08	-1.0%	0.08	0.08	-0.8%
May	0.09	0.09	-0.1%	0.08	0.09	-1.0%	0.09	0.08	0.3%
June	0.08	0.08	-0.9%	0.08	0.08	0.4%	0.08	0.08	-0.7%
July	0.08	0.08	0.5%	0.08	0.08	0.2%	0.08	0.08	0.5%
August	0.08	0.09	-0.5%	0.08	0.09	0.0%	0.08	0.08	0.9%
October	0.08	0.09	-0.3%	0.09	0.09	-0.5%	0.08	0.09	-1.3%
November	0.09	0.09	0.1%	0.08	0.08	0.6%	0.09	0.08	0.7%
December	0.08	0.08	1.2%	0.08	0.08	1.2%	0.09	0.09	-0.4%
13 Metropolitan areas	1	1	.	0.61	0.59	3.3%	1	1	.
Rural	0	0	.	0.05	0.06	-2.5%	0	0	.
STD average			0.28%			**1.0%**			**-0.2%**

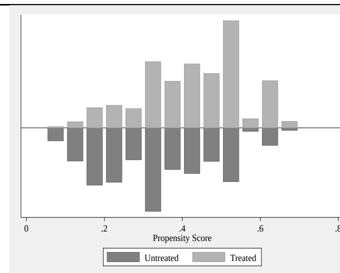
Source: GEIH own estimations.

Figure 3a. P-score distribution. 13-areas.

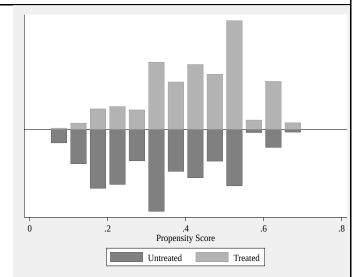
Treated at the follow-up and treated at the baseline



Treated at the follow-up and control at the baseline



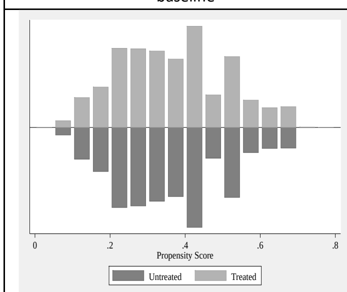
Treated at the follow-up and control at the follow-up



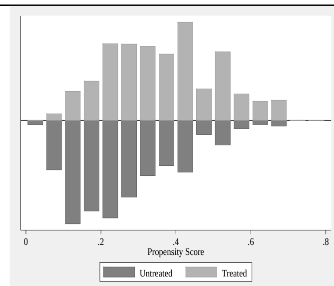
Source: GEIH, own calculations

Figure 3b. P-score distribution. Total sample

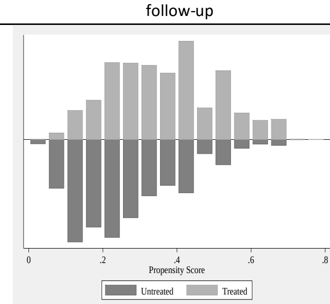
Treated at the follow-up and treated at the baseline



Treated at the follow-up and control at the baseline



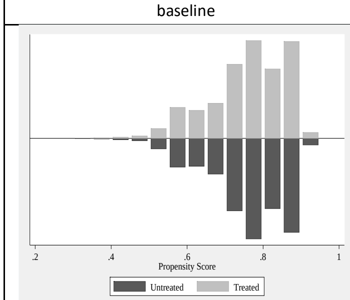
Treated at the follow-up and control at the follow-up



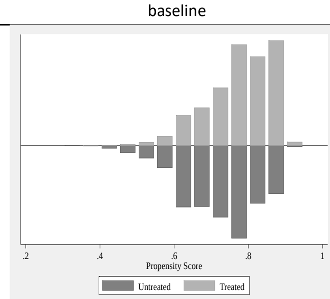
Source: GEIH and own calculations.

Figure 3c. P-score distribution. 13 areas-salaried

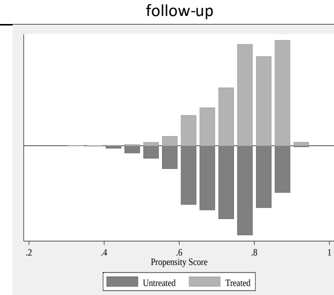
Treated at the follow-up and treated at the baseline



Treated at the follow-up and control at the baseline



Treated at the follow-up and control at the follow-up



Source: GEIH, own calculations

Annex 1. Estimation of the p-score at the baseline (salaried and full sample estimation)

Dependent variable: Treated			
			Total
	Coef.		SE
Constant (B0)	-1.151	***	0.033
Women (spouse)	-0.946	***	0.019
Women (other)	-0.625	***	0.015
Less than 25	-0.397	***	0.018
More than 50	-0.703		0.019
Primary (-)	-0.220	***	0.022
Tertiary (+)	0.095	***	0.017
Diploma	0.607	***	0.020
Big city	0.442	***	0.017
Border city	-0.304	***	0.018
13 Metropolitan areas	0.069	**	0.033
Rural	0.013		0.032
Weights	0.046		0.032
January	0.052		0.032
February	0.078	**	0.033
March	-0.039		0.032
April	0.059	*	0.032
May	0.068	**	0.032
June	0.078	**	0.032
July	-0.003		0.032
August	0.069	**	0.032
October	0.504	***	0.017
November	-0.179	***	0.024
December	0.000	***	0.000
N	360195		
Wald.chi2(23)	14108.86		
Prob>chi2	0.0000		
Pseudo R2	0.1044		

Source: GEIH and own estimations.