Conditional cash transfers and labor informality: the effect of the *AUH* in Argentina's slums *

Student: Diego Lestani
Advisor: Hernán Ruffo

June 2017

Abstract

Since its implementation in 2009, the *Asignación Universal por Hijo (AUH)* has become a symbol of social protection in Argentina. Eligibility for this program is linked, among other things, to the age of children in a household. In order for a parent to receive this benefit, their children must be under the age of eighteen. In this study, the age of the youngest child is used to simulate a "natural experiment", as it provides an exogenous source of variation in the amount of money that the head of a household receives. From this rule, three identification strategies are designed based on this variable to infer the causal effect of this program on labor informality of the head of household. The estimations show that household heads who receive this benefit have a significant (though very low) probability of holding informal employment.

*Final Project for the Master's in Econometrics Program at the Universidad Torcuato Di Tella. I thank TECHO - Argentina for providing me with the data used in this study and my advisor for his ideas and assistance throughout the research process.

1. Introduction

Over the past twenty years, conditional cash transfers have been the predominant form of social protection in Latin America. Such programs transfer money to poor families provided that they meet certain conditions, generally linked to investment in the human capital of the families' children. Conditional cash transfers (hereinafter CCTs) may deter labor market participation [1], Many experts have concerns that these programs create a vicious cycle, in effect subsidizing the informal sector. They argue that such programs constitute incentives for workers to seek informal, low productivity employment (thus avoiding compulsory social security contributions), therefore contributing to the low productivity of labor and capital and the creation of "bad" jobs (Levy, 2010). In contrast, a report prepared by the ECLAC and ILO (2014) argues that there is no clear evidence that these incentives to informality are produced by non-contributory social programs, and it is therefore incorrect to blame them for contributing to informality, particularly CCTs. They cite the fact that the rate of informality in Latin America fell from 54.6% of workers in 1990 – prior to the implementation of CCTs – to 49.1% in 2009.

The varied results of impact evaluations on labor informality among beneficiaries of *Bolsa de Familia* in **Brasil** do not allow for definitive conclusions to be made. De Braw et al. (2013) show that results depended on whether or not the beneficiary lived in an urban area. In urban areas, the number of hours worked in the formal sector decreased an average of eight hours a week per household member, while the number of hours worked in the informal sector grew in the same proportion. This pattern was not observed in rural areas. Nevertheless, Barbosa y Caorseuil (2013) argue that there is no evidence to suggest that the program increases employment in the informal sector among beneficiaries. With regard to another of Brazil's social protection programs, *Beneficio Programa Continuo (BPC)*, (a pension for the elderly and the disabled), Medeiros, Britto and Vera Soares (2008) did not find evidence that it led to a loss in social security contributions.

In **Uruguay**, Vigorito (2014) posits that child benefits provided through Plan Equidad have potentially had a negative impact on labor formalization among beneficiaries, especially women. The author points out that this is mainly due to issues with the program's design. The Uruguayan Social Security Institute carries out bimonthly evaluations of beneficiaries' formal income in order to determine if they remain eligible for the program's non-contributory component. Households that are above the income threshold are suspended from the program for six months, which is a strong incentive to not declare a rise in income or to keep informal

[1] For a further discussion of the relationship between conditional cash transfers and labor markets, see *"The employment situacion in Latin America and the Caribbean. Conditional transfer programmes and the labour market" (ECLAC/ILO, 2014)*

or partially formal employment.

In the case of **Argentina**, Hatrick (2015) found a decline in both household income and the number of weekly hours worked among beneficiaries of the *Ciudadanía Porteña (CP)* program implemented by the government of the City of Buenos Aires. Regarding the issue of labor informality, Garganta and Gasparini (2015) evaluated the effect of the *Asignación Universal por Hijo* (AUH) on labor informality – similar to the objectives of this study – but with a different approach. These authors used data from the Argentine statistical bureau's quarterly national household survey (*Encuesta Permanente de Hogares – EPH*), which does not collect data on the effective reception of the AUH. Therefore, they identified intention-to-treat effect, using eligibility criteria in order to analyze the *potentially eligible population*.

The approach I propose utilizes a novel source of data, provided by an NGO dedicated to poverty alleviation through the construction of emergency housing by young volunteers and slum dwellers. These data include indicators of the reception of the AUH and the amount of money received by beneficiaries, valuable tools for evaluating this public policy. This thesis is structured as follows: section 2 presents a brief description of the data used in this study; section 3 presents three identification strategies and the estimation of the causal effects; section 4 provides detailed explanation of the causal interpretation of the estimates in section 3; and finally, section 5 presents the main conclusions.

2. Data

The data utilized in this study were collected as part of a series of household surveys carried out in slums by the Center for Social Research at *TECHO-Argentina*. *TECHO* is an NGO devoted to the construction of emergency housing that aims to promote social integration among slum dwellers. Prior to the construction of emergency housing, volunteers assess the socioeconomic and housing conditions of families residing in slums in order to establish priorities and to select beneficiaries. The organization maintains a database with information on 5,875 households (including indicators such as the quality of construction materials) and 26,764 individuals, with information on age, sex, education, employment status, income, and so on. Surveys were carried out between 2009 and 2014 in slums found in the provinces of Buenos Aires, Córdoba, Corrientes, Misiones, Neuquén, Río Negro, Santa Fe, and Salta. From this dataset, a new database was constructed with information on 5,290 heads of household aged 18 to 65 years old, with individual information and aggregated metrics corresponding to their households. Throughout this study, the term income (either from AUH or wages) will refer to real income (nominal income adjusted for inflation using the informal private sector Salary Index calculated by the INDEC, base Period=April 2009). It is crucial that the reader keep in mind the target population of this study, given that this group is not representative of the whole Argentinian population, but rather is comprised of potential beneficiaries of emergency housing programs living in slums. The characteristics of the target population's housing serve to illustrate this. For example, average home size is approximately 30 square meters, one-third of surveyed families' homes are at risk of flooding, and over half are vulnerable to wind and humidity. Twenty percent of these homes can be characterized as unstable buildings, and the most common building materials are wood (56%), brick (30%), and sheet metal (7%), among others. Additionally, 30% of these homes either have no flooring or carpeting directly on top of the soil, while 70% have irreparable damage to floors, walls, and ceilings. Table 1 presents descriptive statistics of the database used in this study. The variables included are the following: real income from AUH (IncAUH); real income from wages (IncLab); a binary variable that indicates if the head of household is foreign-born (Foreign); their age in years (Age); their education level (coded from 1 to 11[2]); if they are female (Female); a pregnancy indicator (Pregnant); the number of pregnant women in the household (NumPreg); dimensions of the home (HDim); the number of chronically ill people in the household (NumChronIll); and indicators on occupations (construction worker, day

[2]Categories of the variable Education are 1: none; 2: preschool; 3:primary in progress; 4: primary incomplete; 5: primary complete; 6: secondary in progress; 7: secondary incomplete; 8: secondary complete; 9: post-secondary/university in progress; 10: post-secondary/university incomplete; 11: post-secondary/university complete.

laborer, commerce employee, etc.).

Table 1: Descriptive statistics

Variable	**Mean**	**SD**	**Min**	**Max**	**p25**	**p50**	**p75**
IncAUH	137.81	187.83	0	991.08	0	0	230.74
IncLab	1092.93	794.15	0	8790.04	574.53	1014.24	1459.30
Foreign	0.21	0.41	0	1	0	0	0
Age	31.94	10.01	18	65	24	30	38
Education	4.05	2.35	1	11	1	4	5
Female	0.56	0.49	0	1	0	1	1
Pregnant	0.06	0.23	0	1	0	0	0
ConstWorker	0.13	0.33	0	1	0	0	0
Maid	0.06	0.24	0	1	0	0	0
SelfEmployed	0.04	0.20	0	1	0	0	0
DayLaborer	0.12	0.32	0	1	0	0	0
CommEmployee	0.03	0.18	0	1	0	0	0
Salesman	0.03	0.16	0	1	0	0	0
NumPreg	0.11	0.32	0	2	0	0	0
HDim	28.94	22.61	1	350	15	24	36
NumChronIll	0.16	0.47	0	5	0	0	0

3. Identification and Estimation Strategy

Assuming there is a relationship between AUH income and head of household's labor formality, I propose the following model for the microdata:

$$F = \gamma_0 + X_1\gamma_1 + \rho I + \epsilon \tag{1}$$

Where F is an indicator variable of formality, X_1 is a row vector of covariates, γ_1 is a vector of coefficients, I is AUH income [3] received by the head of household and ρ represents the approximate percentage of variation in the probability of formality per 100 pesos of AUH income. Equation (1) can be augmented with an equation that describes how AUH income relates the other covariates X_1, and other additional covariates, Z

$$I = \delta_0 + X_1\delta_1 + Z\delta_2 + \omega \tag{2}$$

OLS applied to (1) can lead to biased estimations of ρ OLS applied to (1) can lead to biased estimations of (rho) even if the response function is linear. For example, omitted-variable bias can occur if individuals with disabled children (a non-observable variable in this case) receive a higher AUH income but have less available time for work, and are therefore more likely to hold informal employment. It could also could be the case that people with fewer "social skills" (such as the ability to establish relationships with others, low self-esteem, and few interpersonal skills) receive a higher amount of AUH income but have access to lower quality jobs, in this case, in the informal sector. In terms of equations (1) and (2), these non-observable factors are common to both errors ϵ and ω), and therefore these errors are correlated, which implies that I and (epsilon) are correlated, thus violating the exogeneity assumption of OLS. An infeasible solution to this problem would be to carry out an experiment in which AUH income is randomly assigned. Random assignment would eliminate the correlation between AUH income and the non-observable factors previously mentioned. Even in the absence of a real experiment, a "natural experiment" could generate instrumental variables that effectively do the same. Instrumental variables are variables related to the outcome of interest (F) solely through the treatment of interest (I). In this case, **youngest child age** (YCA hereinafter) of a head of household interacts with the eligibility criteria for the AUH (that children be under 18) to generate variability in the amount of received AUH income. The higher the age of the youngest child, the lower the number of potentially eligible children under 18 in the household, and therefore the lower the potential amount of AUH income. This induces a negative relationship between AUH

[3] Measured in units of 100 Argentine Pesos.

income and YCA (Figure 1)

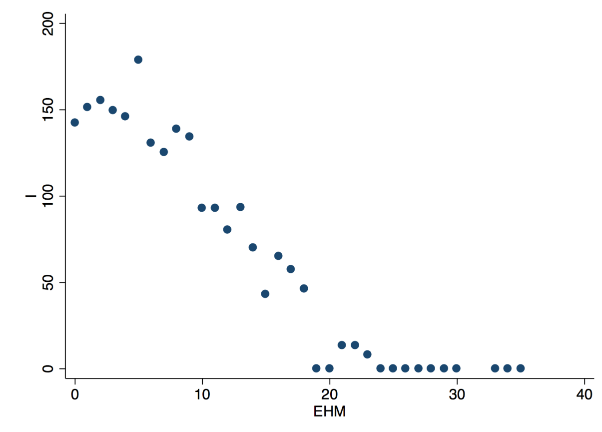

Fig. 1. AUH Income and YCA

If YCA is correlated with labor formality only because it is correlated with AUH income, which is a plausible assumption, it is an instrument for the AUH income in the formality equation. Based on this instrumental variable, I propose three identification strategies for the causal effect of interest:

3.1. Binary Instrument

One method of transforming this idea into an estimation strategy is to compare AUH income and employment formality of heads of household whose youngest child are under 18 with the income and employment formality of heads of household whose children are 18 and over. This leads to the simplest possible IV estimator: the Wald method of fitting straight lines (Durbin 1954). The calculations of Wald estimators based on this comparison are summarized in Table 2. There are no significant differences in employment formality for heads of household whose youngest child is under 18 and those whose children are 18 and over. However, heads of household whose youngest child is under 18 receive a higher amount of AUH income (reflected in their greater average income range, I). The ratio of these differences generates a Wald estimator close to zero. This simple instrumental variable estimator reflects the omitted-variable bias of the OLS estimator reported in the last row of Table 2, which either way is very close to zero.

The result of this first simple estimation strategy does not shed much light on the link between AUH income and labor formality due to the fact that the estimations lack statistical significance.

Table 2: Formality and YCA

	(1) $YCA < 18$	**(2)** $YCA \geq 18$	**(1) - (2)** (standar error)
Formalidad (F)	0.1041	0.0987	0.0054
			(0.0246)
Rango Ing AUH (I)	1.6960	0.1447	1.5512
			(0.7169)
Estimador de Wald			0.0035
			(0.825)
Estimador OLS			-0.0172
			(0.0019)

3.2. Multiple-value instrument

An alternative estimation strategy based on the same idea is 2SLS. There are 35 potential IV (or Wald) estimators that could be computed using as binary variables the values (0 to 35) of the instrument , (AYC - Z hereinafter). A 2SLS estimator that uses all the available information regarding YCA is first calculated by regressing AUH income (the endogenous regressor) on each covariate included in the equation, X_1, and on each potential instrument excluded from the equation, Z – ein this case, 35 dummies, each corresponding to a value of YCA (except Z=0). The second stage in the 2SLS procedure is to estimate:

$$F = \gamma_0 + X_1\gamma_1 + \rho\hat{I} + \upsilon \tag{3}$$

Where $\hat{I}$ is the estimated value in the first stage regression and $\upsilon = \epsilon + \rho(I - \hat{I})$. EIn this case 2SLS can be interpreted as an IV estimator with instruments X_1 and $\hat{I}$ (Theil, 1971),or as an efficient linear combination of alternative IV estimators using unique dichotomous variables. Columns (1)-(2) in Table 3 present OLS and 2SLS estimates without covariates. Excluded instruments used to calculate 2SLS estimators in column 2 are the 35 dummies. In other words, the estimators are regressions of formality (F) on the estimated values of AUH income in a "first stage" regression on a constant and 35 dummies. Columns (3)-(4) present the estimates based on equations where the set of covariates X_1 includes the variables included in Table 1 (except income variables). The excluded instrument used to construct 2SLS estimators in column (4) are, again, the 35 dummies with the values that the instrument takes. OLS and 2SLS estimations are similar and differ little in each specification of the model.

Using F – a dichotomous variable – as the dependent variable leads to linear probability models when OLS is applied, which is reflected by the low values of R^2 in Table 3. In order to get a continuous proxy variable for the dependent variable, a Probit model is estimated with

Table 3: Estimation of Equation (1)

Variable	**OLS (1)**	**2LS (2)**	**OLS (3)**	**2SLS (4)**
I	-0.0172***	-0.0118	-0.0154***	-0.0181*
	(0.0019)	(0.0118)	(0.0019)	(0.0105)
R^2	0.0139	0.0125	0.1180	0.1177
$P(F)$	0.0000	-	0.0000	-
$P(\chi^2)$	-	0.3149	-	0.0000
First Stage				
R^2	-	0.0299	-	0.1448
$P(F)$	-	0.0000	-	0.0000
Exogenous Covariates			✓	✓
Instruments		✓		✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$,
Robust est. errors in parenthesis
N=5290

the same covariates (giving it good predictive power) [4] and the estimates of the dependent variable in that model (called $ProbF$) are used to carry out the same estimation procedure with OLS and 2SLS. Results of the estimation of Equation (1) using $ProbF$ instead of F as the dependent variable are presented in Table 4.

Table 4: Estimation of Equation (1)

Variable	**OLS (1)**	**2LS (2)**	**OLS (3)**	**2SLS (4)**
I	-0.0165***	-0.0149***	-0.0146***	-0.0160***
	(0.0006)	(0.0039)	(0.0003)	(0.0016)
R^2	0.0975	0.0966	0.8709	0.8702
$P(F)$	0.0000	-	0.0000	-
$P(\chi^2)$	-	0.0002	-	0.0000
First Stage				
R^2	-	0.0299	-	0.1448
$P(F)$	-	0.0000	-	0.0000
Exogenous Covariates			✓	✓
Instruments		✓		✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$,
Robust est. errors in parenthesis
N=5290

It can be observed that using $ProbF$ as a proxy for the probability of holding formal employment generates almost identical results as the dichotomous variable F. Therefore, this variable will be employed as the dependent variable throughout the rest of this study, given that linear models are dealt with.

This second identification and estimation strategy provides significant results, due to the simple fact that it uses more information on the instrument. Tables 3 and 4 reveal that each additional 100 pesos of AUH income that a head of household receives reduces the probability of formal employment between approximately 1.5% and 1.8%. Considering that average AUH income is 281 pesos (for individuals who receive the AUH), the average effect of AUH on the probability of holding formal employment is approximately -4.5%. It is very

[4] Area under ROC curve=0.80

important to keep in mind that this is a local effect in the sense that it is a weighted average effect for those individuals whose treatment status is affected by the instrument. Section 4 explains this causal interpretation in greater detail.

3.3. Instrumental variables and regression discontinuity design

Finally, the fact that one of the requirements to receive the AUH is that every potential child beneficiary be under age 18 can be useful in thinking about a regression discontinuity design (RDD), which will be explained in further detail in section 4.3. The idea is to verify if there is discontinuity in the outcome variable (formality) as a function of the instrument which is generated by discontinuity in the probability of treatment. Figure 2 shows the conditional expectation of the outcome variable as a function of the age of the youngest child.

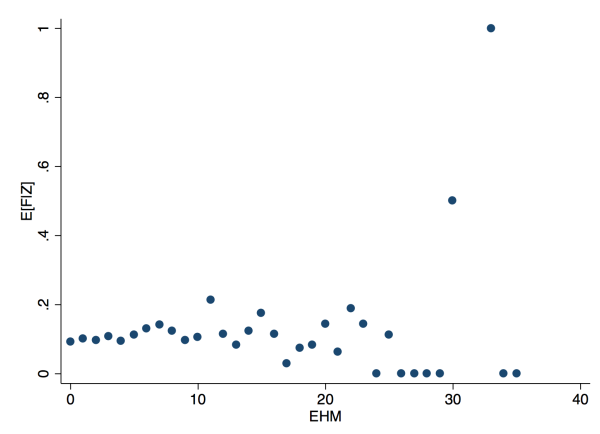

Fig. 2. Formality and Youngest Child Age

Again, in this section, z_i is AYC. D_i denotes treatment status, which in this case (different from previous strategies) is a dichotomous status separating those who receive AUH from those who do not ($D_i = 1\{I > 0\}$).

The RDD can be expressed formally using a simple model. Suppose that potential outcomes can be described by a constant effects model

$$E[F_{0i}|z_i] = f(z_i) = \alpha + \beta_1 z_i + \beta_2 z_i^2 + ... + \beta_p z_i^p \tag{4}$$

$$F_{1i} = F_{0i} + \rho \tag{5}$$

Esto lleva a

$$F_i = f(z_i) + \rho D_i + \eta \tag{6}$$

where ρ is the causal effect of interest.

It is assumed that the conditional expectation of the outcome variable is continuous and can be described by a *p-order* polynomial. Figures 3 to 5 show first- to third-order polynomials to describe this conditional expectation.

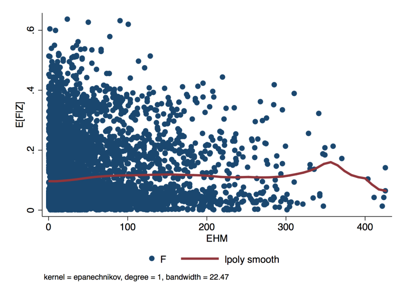

Fig. 3. $E[F|Z]$. Order 1 Polynomial.

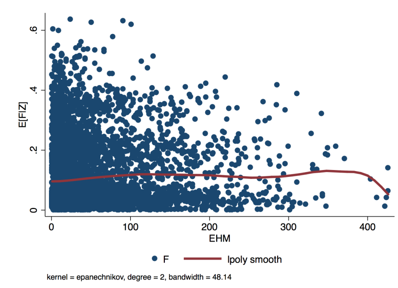

Fig. 4. $E[F|Z]$. Order 2 Polynomial

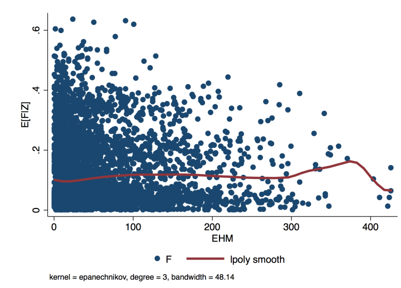

Fig. 5. $E[F|Z]$. Order 3 Polynomial.

In this case, there is discontinuity in the probability of receiving treatment at the threshold value of a covariate. Specifically, the probability of receiving AUH falls sharply when AYC is greater than 18 ($z_i > 18$) (Figura 6).

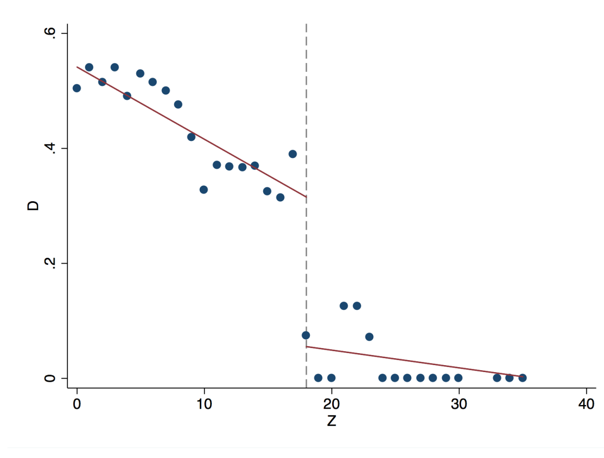

Fig. 6. Probability of receiving AUH and Youngest Child Age

In the literature, this is called Fuzzy Regression Discontinuity Design (Fuzzy RD hereinafter), which is a research design in which the discontinuity in the probability of treatment turns into an instrumental variable for treatment status. In the case of AUH, there is a jump in the probability of treatment at 18, such that

$$P(D_i = 1|z_i) = \begin{cases} g_1(z_i) \text{ si } z_i < 18 \\ g_0(z_i) \text{ si } z_i \geq 18 \end{cases} \tag{7}$$

Functions $g_0(z_i)$ and $g_1(z_i)$ pucan be anything as long as they differ at 18 (and the more they differ, the better). It is assumed that $g_1(18) > g_0(18)$ so that $z_i \geq 18$ makes treatment less probable. The relationship between the probability of treatment and z_i can be described as

$$E[D_i = 1|z_i] = P(D_i = 1|z_i) = g_0(z_i) + [g_1(z_i) - g_0(z_i)]T_i, \tag{8}$$

where $T_i = 1\{z_i < 18\}$.

The binary variable T_i indicates the point where $E[D_i|z_i]$ is discontinuous. Fuzzy RD naturally leads to a simple 2SLS estimation strategy. Assuming that $g_0(z_i)$ and $g_1(z_i)$ can be described by p-order polynomials, we arrive at

$$\begin{aligned} E[D_i|z_i] &= \gamma_{00} + \gamma_{01}z_i + \gamma_{02}z_i^2 + ... + \gamma_{0p}z_i^p + [\pi + \gamma_1^* z_i + \gamma_2^* z_i^2 + ... + \gamma_p^* z_i^p]T_i \\ &= \gamma_{00} + \gamma_{01}z_i + \gamma_{02}z_i^2 + ... + \gamma_{0p}z_i^p + \pi T_i + \gamma_1^* z_i T_i + \gamma_2^* z_i^2 T_i + ... + \gamma_p^* z_i^p T_i \end{aligned} \tag{9}$$

where the γ_* are the coefficients of the polynomial interactions with T_i. From this, it can be observed that T_i, and interaction terms $\{z_iT_i, z_i^2T_i, ..., z_i^pT_i\}$ can be used as instruments for D_i in Equation 6. The simplest Fuzzy RD estimator uses only T_i as an instrument without the interaction terms. The first stage in this case is

$$D_i = \gamma_0 + \gamma_1 z_i + \gamma_2 z_i^2 + ... + \gamma_p z_i^p + \pi T_i + \xi_{1i} \tag{10}$$

where π is the effect of T_i in the first stage. The reduced form of Fuzzy RD is obtained by substituting equation 10 in equation 6

$$F_i = \mu + \kappa_1 z_i + \kappa_2 z_i^2 + ... + \kappa_p z_i^p + \rho\pi T_i + \xi_{2i} \tag{11}$$

where $\mu = \alpha + \rho\gamma_0$ y $\kappa_j = \beta_j + \rho\gamma_j$ para $j = 1, ..., p$. La Identification in Fuzzy RD depends on the ability to distinguish the relationship between F and the discontinuous function, $T_i = 1\{z_i < 18\}$, from the effect of polynomial controls included in the first and second stage.

Table 5 shows the results of 2SLS estimation of D in equation 6 with first- to third-degree polynomial controls.

It can be observed that this identification strategy indicates that the probability that a

Table 5: Estimation of Equation (6). 2SLS.

Variable	**p=1**	**p=2**	**p=3**
$\hat{D}$	-0.0495*	-0.0795***	-0.0836**
	(0.0254)	(0.0281)	(0.0353)
R^2	0.8545	0.8425	0.8412
$P(\chi^2)$	0.0000	0.0000	0.0000
First Stage			
R^2	0.1351	0.1358	0.1365
$P(F)$	0.0000	0.0000	0.0000
Exogenous Covariates	✓	✓	✓
Instruments	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$,
Robust st. error in parenthesis
N=5290

head of household who receives the AUH will hold formal employment is reduced by 5 to 8 percentage points. One way to check the robustness of this result is to do a non-parametric Fuzzy RD estimation. The non-parametric version consists of the IV estimation in a small neighborhood around the discontinuity without the polynomial controls and checking that the estimation result is not altered. The reduced form of the conditional expectation of F_i close to 18 is

$$E[F_i|18 \leq z_i < 18 + \Delta] - E[F_i|18 - \Delta < z_i < 18] \simeq \rho\pi \tag{12}$$

Similarly, the first stage is

$$E[D_i|18 \leq z_i < 18 + \Delta] - E[D_i|18 - \Delta < z_i < 18] \simeq \pi \tag{13}$$

Hence,

$$lim_{\Delta \to 0} \frac{E[F_i|18 < z_i < 18 + \Delta] - E[F_i|18 - \Delta < z_i < 18]}{E[D_i|18 < z_i < 18 + \Delta] - E[D_i|18 - \Delta < z_i < 18]} = \rho \tag{14}$$

The sample analog of Equation 14 is a Wald estimator using T_i as an instrument for D_i in a Δ-neighborhood of 18. Like the other binary instruments, the result is a local average treatment effect. In particular, the Wald estimator for Fuzzy RD captures the causal effect in "compliers", defined as those individuals whose treatment status changes as we change the value of z_i dfrom just under 18 to just above 18. This interpretation for Fuzzy RD was introduced by Hahn, Todd and van der Klaaw (2001). However, there is another sense in which this version of LATE[5] is local: the estimates are valid for heads of household whose YCA is close to 18. Table 6 summarizes the results of the estimates in four neighborhoods (in ascending order) around the age of 18.

Table 6: Estimation of Equation (6). 2SLS.

Variable	$\Delta = 1$	$\Delta = 2$	$\Delta = 3$	$\Delta = 4$
$\hat{D}$	-0.0616*	-0.0559**	-0.0596**	-0.0652***
	(0.0369)	(0.0251	(0.0240)	(0.0200)
R^2	0.8374	0.8253	0.8322	0.8391
$P(\chi^2)$	0.0000	0.0000	0.0000	0.0000
N	87	136	192	265
Exogenous Covariates	✓	✓	✓	✓
Instruments	✓	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$,
Robust st. errors in parenthesis

Effectively, the **local** non-parametric estimation, which predicts an effect of approximately -6% – makes the results of the parametric version (which uses the full sample and that had an effect ranging from -5% to -8%) more robust. Both strategies do not differ too much from the estimated effect by the estimation strategy in the previous subsection (-4.5%).

[5]LATE: Local Average Treatment Effect, exlpained in detail in section 4

4. Interpretation of the estimations as causal effects

This study aims to estimate the existence of a causal effect between the reception of AUH and head of household employment formality. Angrist and Imbens (1995) show how 2SLS can be used to estimate the average causal effects in a version of Rubin's causal model that allows for variable treatment intensity, multiple instruments, and covariates. In particular, they show that 2SLS applied to a causal model with variable treatment intensity identifies a weighted average per-unit treatment effect along the length of a causal response function

Equation 1 in Section 3 is a structural relationship derived from assumptions about human behavior, but it is not necessarily a causal relationship in Rubin's (1974) sense. Angrist and Imbens (1995) consider that estimations of ρ in Equation 1 have a causal interpretation when the probability limit equals a weighted average of $E(F_j - F_{j-1})$ for every j in some subpopulation of interest.

4.1. The case of a binary instrument (Wald Estimator)

Let $I_Z \in \{0, 1, ..., J\}$ represent the range of real AUH income in a household conditional to the age of the youngest child, Z. The initial assumption is that Z is coded to take only two values, 0 and 1 (indicating if the youngest child is less than 18). I_0 would be the range of AUH income of a head of household whose youngest child is 18 or over and I_1 would be the range of AUH income of a head of household whose youngest child is less than 18. For each head of household included in the sample, the set (Z, I, F), is observed where Z is the indicator variable mentioned above, I is the range of real AUH income and F is the indicator variable of formal employment. The main identification assumption (besides the existence of F_j) is that Z es independiente de todos los resultados potenciales is independent from every potential outcome and possible treatment intensities. In practical terms, this assumption can hold only after conditioning on covariates. Formally, the assumption can be stated as follows:

Assumption 1 (independence): : *the random variables I_0, I_1,F_0 ,F_1,...,F_J Fj are mutually independent from Z.*

This assumption rests on the fact that the age of the youngest child being under 18 has no effect on head of household's employment formality beyond the effect of the AUH. In this sense, YCA provides a natural experiment that can be used to estimate the effect of AUH income on labor formality. Assumption 1 can also be viewed as a nonparametric version of the assumption that, Z, is not correlated with ϵ and ω in equations (1) y (2) of Section 3.

It is important to note that outside of a context of linear regression, the independence assumption alone is not sufficient to identify a significant average treatment effect. This point is easier to see in an example where I is binary (for example, indicating those who receive AUH versus those who do not). A comparison of the results of each instrument is:

$$E[F|Z=1] - E[F|Z=0] = E[F_0 + (F_1 - F_0)I_1|Z=1] - E[F_0 + (F_1 - F_0)I_0|Z=0] \quad (15)$$

Per assumption 1, this simplifies to

$$E[F_0 + (F_1 - F_0)I_1] - E[F_0 + (F_1 - F_0)I_0] = E[(F_1 - F_0)(I_1 - I_0)] \quad (16)$$

Without imposing additional restrictions , $I_1 - I_0$ can be equal to 1, 0 or -1. A value equal to 1 indicates that individuals are induced to receive benefits by the instrument (that their child is under 18); a value equal to 0 indicates individuals whose potential AUH income status (receives AUH/does not receive AUH) is not modified; and a value equal to -1 indicates individuals who are induced to not receive benefits. Therefore:

$$\begin{aligned} E[(F_1 - F_0)(I_1 - I_0)] = E[F_1 - F_0|I_1 - I_0 = 1]Pr[I_1 - I_0 = 1] + \\ E[F_1 - F_0|I_1 - I_0 = -1]Pr[I_1 - I_0 = -1] \end{aligned} \quad (17)$$

Clearly, heads of household whose potential treatment status does not change do not contribute anything to the comparisons of average outcomes according to the instrument status. However, the group that contributes includes “switchers in” and “switchers out”. It is clear that it is theoretically possible to have a situation where the treatment effect $I_1 - I_0$ is positive for everyone, but the size of the group of switchers in and switchers out is such that the differences in outcomes is zero or even negative. For example, suppose that the treatment effect (the effect of receiving AUH) equals α for heads of household induced to receive benefits and 2α for those induced not to receive them. If $Pr[I_1 - I_0 = 1] = 2/3$ and $Pr[I_1 - I_0 = -1] = 1/3$, then the average difference of F conditional in Z is zero, despite $I_1 - I_0 > 0$ for every head of household. The most common way to solve this problem is by simply assuming a constant unit-treatment effect, $I_j - I_{j-1} = \alpha$, for every j and every individual. Instead of restricting treatment effect heterogeneity, this study proposes a non-parametric restriction in the process that determines I as a function of Z. This restriction is that $I_1 - I_0 \geq 0$ o $I_1 - I_0 \leq 0$ for every head of household. Assuming, for example, that $I_1 - I_0 \geq 0$ Equation 17 turns into:

$$E[(F_1 - F_0)(I_1 - I_0)] = E[F_1 - F_0|I_1 - I_0 = 1]Pr[I_1 - I_0 = 1] \quad (18)$$

The conditional expectation, $E[F_1-F_0|I_1-I_0 = 1]$ is what Angrist and Imbens (1994) call local average treatment effect or LATE. LATE is the average causal effect of the treatment for those whose treatment status is affected by the instrument (those for whom $I_1 = 1$ y $I_0 = 0$).). Formally, the monotonicity condition (given independence) to identify LATE is:

Assumption 2 (Monotonicity: *With probability 1,* $I_1 - I_0 \geq 0$ *o* $I_1 - I_0 \leq 0$.

In the case of AUH, monotonicity means that heads of household whose youngest child is under 18 receive at least the same AUH income that they would receive if their youngest child were over 18. Assumption 2 is not verifiable because it involves non-observable variables (only one is observed, I_1 or I_0). SNevertheless, for multivalued treatments ($J > 1$), cas in this case, Assumption 2 has the testable implication that the cumulative distribution function (CDF) of I given $Z = 1$ and the CDF of I given $Z = 0$ 0 should not cross, because if $I_1 \geq I_0$ with probability 1, then $Pr(I_1 \geq j) \geq Pr(I_0 \geq j)$ for every j. This implies that $Pr(I \geq j|Z = 1) \geq Pr(I \geq j|Z = 0)$ o $F_I(j|Z = 0) \geq F_I(j|Z = 1)$, then F_I is the CDF of$I$. Figure 7 shows that in the case of a binary instrument, the implications of Assumption 2 are correctly verified.

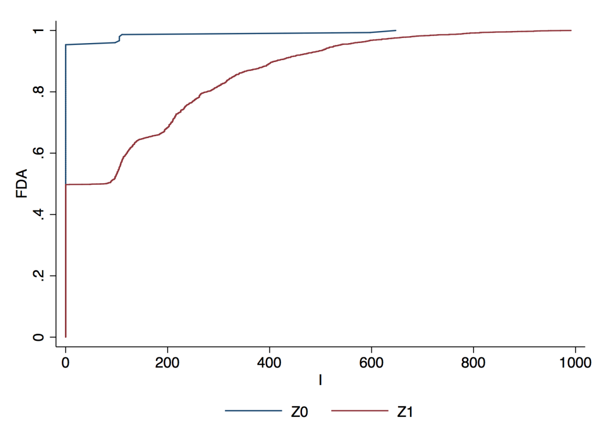

Fig. 7. Cumulative Distribution Function of I

The main theoretical contribution of Angrist and Imbens, applied to this case, is the following:

Teorema 1. Suppose that assumptions 1 and 2 hold and that $Pr(I_1 \geq j > I_0)$ 0 for at least one j. Then,

$$\frac{E[F|Z=1]-E[F|Z=0]}{E[I|Z=1]-E[I|Z=0]}=\sum_{j=1}^{J}\omega_j E[F_j-F_{j-1}|I_1\geq j>I_0]\equiv\beta \tag{19}$$

where

$$\omega_j=\frac{Pr(I_1\geq j>I_0)}{\sum_{i=1}^{J}Pr(I_1\geq i>I_0)} \tag{20}$$

This implies that $0\leq\omega_j\leq 1$ and $\sum_{j=1}^{J}\omega_j=1$

Proof of this theorem can be found in the appendix of Angrist and Imbens (1995). The requisite is $Pr(I_1\geq j>I_0)>0$ means that the instrument must affect the level of treatment, I.

The parameter β is the **Causal Response Function (CRF)**. This parameter captures a weighted average of causal responses to a change of a unit in treatment, for those whose treatment status is affected by the instrument. The weight given to the average of F_j-F_{j-1} is proportional to the number of people that, because of the instrument, change their treatment from less than j units to j or more units. This proportion is $Pr(I_1\geq j>I_0)$. In the case of the AUH, this is the proportion of heads of household that, due to the date of birth of their youngest child alone, are induced to receive a higher amount of AUH income. It should be noted that this group is not representative of the whole population considered in this study, and that the members of this group cannot be identified from the data because it involves counterfactual treatment status. The CRF for the case of the binary instrument is the Wald estimator in Table 2, which is not significantly different from zero.

4.2. The case of an instrument with multiple values or multiple binary instruments (2SLS)

Let $I_Z\in\{0,1,...,J\}$ be the range of real AUH income for a head of household conditional to YCA, Z. As with F_j, it is assumed that it exists for every value of Z for every person, though only one I_Z is observed.

$Z\in\{0,1,...,K-1\}$ indicates that the age of the youngest child I_k) would be the range of AUH income for a household head whose youngest child is k years old, while I_i would be the range of AUH income for a head of household whose youngest child is i years old. For each head of household, the set (Z,I,F), where Z is the age of the youngest child, I is the range of real AUH income and F the formal employment indicator.

The main identification assumption (besides the existence of F_j) is that Z is independent from every potential outcome and every potential treatment intensity. Formally, the following assumption is held:

Assumption 1 (Independence): *The random variables I_0, I_1,....I_K,F_0 ,F_1,...,F_J are mutually independent from Z.*

This assumption requires that YCA (whatever it is) does not affect the head of household's employment formality more than its effect through AUH income. In this sense, YCA provides a natural experiment that can be used to estimate the effect of AUH income on labor formality. Assumption 1 can also be viewed as a non-parametric version of the assumption that the instruments, z_i, are not correlated with ϵ and ω in equations (1) and (2).

Assumption 2 (Monotonicity: *With probability 1, $I_j - I_{j-1} \geq 0$ or $I_j - I_{j-1} \leq 0$.*

In this case, monotonicity means that heads of household whose youngest child k years old would have the same AUH income range as they would have if their child were another age.

This assumption is not verifiable because it involves non-observable variables (just one I_k, $k = 0, 1...K - 1$ is observed). In this case, K mutually-orthogonal binary instruments are combined to form a simple 2SLS estimator, such that using K orthogonal indicators can be thought of as a way to exploit a simple instrument Z that takes K values. Moreover, a saturated model for the first stage consistently estimates the conditional expectation of the endogenous regressors given the instrument. This leads to the most efficient 2SLS estimator in homoscedastic regression models with constant treatment effects (Newey 1990).

Angrist and Imbens (1995) show that the 2SLS estimator constructed by using as instruments a constant plus K linearly independent binary variables, $d_K = I(Z = k)$, chas a probability limit equal to the weighted average of K linearly independent CRFs, $\beta_{k,k-1}$, where

$$\beta_{k,k-1} \equiv \frac{E[F|Z = k] - E[F|Z = k - 1]}{E[I|Z = k] - E[I|Z = k - 1]}$$

Given that $\beta_{k,k-1}$ is a weighted average of points along the causal response function, the 2SLS estimator also converges to a weighted average of points in the causal response function. The authors' theoretical result in this case is:

Theorem 1 1. *Suppose that $E[I|Z]$ and a constant are used to construct instrumental variables of β_Z in the equation*

$$F = \gamma + \beta_Z I + \epsilon$$

The resulting estimator has a probability limit

$$\beta_Z \equiv \frac{E\{F.(E[I|Z] - E[I])\}}{E\{E[I|Z].(E[I|Z] - E[I])\}} = \sum_{k=1}^{K} \mu_k \beta_{k,k-1}$$

Proof of this theorem can be found in the appendix of the previously cited study. The theorem follows standard formulas that interpret 2SLS using mutually orthogonal instruments as a weighted average of each of the estimators of the instrumental variables obtained taking the instruments one by one. Moreover, when the instruments are mutually exclusive binary variables, 2SLS can be written as a combination of linearly independent Wald estimators (Angrist 1991).

This theorem provides a useful interpretation for the conventional 2SLS estimators. In the same way that a simple Wald estimator converges to a weighted average effect along the causal response function, the 2SLS estimators provide a way to combine a set of different weighted average effects into a new single average. The weights used to construct the 2SLS estimators from the Wald estimators are proportional to $(E[I|Z = k] - E[I|Z = k - 1])$. Hence, the better the Wald estimator – in the sense of being an instrument with a higher impact on the regressor – the greater the weight that it receives in the 2SLS linear combination. Figure 8 shows the results of the estimation of ρ (cwith 95% confidence intervals) in Equation 1 taking the 35 instrumental variables one by one. For the purpose of comparison, it also shows the 2SLS estimator that is a weighted average of each of these Wald estimators.

4.3. Causality and Regression Discontinuity Design

The identification strategy in this research design relies on the fact that individuals cannot manipulate with precision the treatment assignment variable in order to be eligible. Lee (2008) shows formally that it is not necessary to assume that the RD design isolates the treatment variation as if it were as good as randomized, rather that random variation is a consequence of the inability of agents to precisely control the assignment variable near the threshold (in this case, youngest child age equal to 18). This is a crucial feature of the design, and it is the reason why it is usually convincing. Intuitively, when individuals have an imprecise control over the assignment variable, even if some of them are likely to have children near the threshold age, every individual will have the same probability of having a child that is just below (receive treatment) or just above (denied treatment) the threshold – similar to flipping a coin. This result clearly differentiates RD designs from IV approaches.

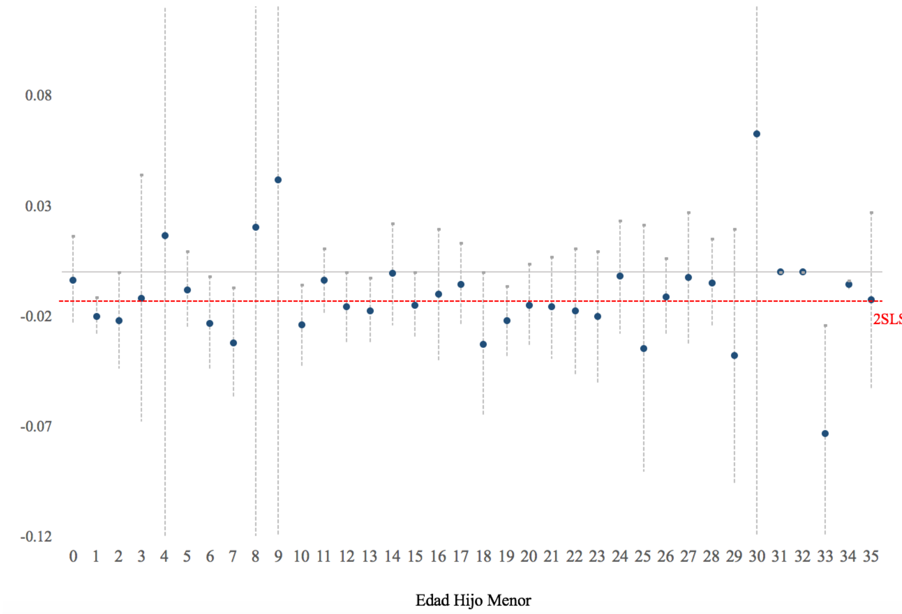

Fig. 8. Estimations of $\hat{\rho}$. Instrumental Variables

When using IV for causal inference, it must be assumed that the instrument is exogenously generated as if generated by a coin toss. In contrast, the variations that RD isolates are random as a consequence of the assumption that individuals have an imprecise control over the assignment variable. A way to check this assumption is to take a look at the histogram of this variable and see if there is any jump near the threshold value. Figure 9 shows that there is no evidence of such jumps in the probability distribution of the assignment variable (YCA), which is evidence that heads of household cannot manipulate it in order to become eligible.

Since the RDD seems to be valid, covariates were added to reduce the sample variability of the causal effect estimations. While it is true that it is not recommended to include these covariates in the non-parametric version (since they should be balanced on each side of the neighborhood close to the threshold as a consequence of the mentioned "randomization"), in this study they have been included. The rationale for this decision is that the number of observations in these neighborhoods is not large enough, and since it is a "fuzzy" and not "sharp" RDD, there is "imperfect compliance". TAll these facts do not help to generate randomness (in the sense of flipping a coin) close to the threshold value generated by a "sharp" design. For these reasons, it is not completely certain that every covariate is balanced on each side of the threshold and therefore they were added in order to control possible unbalances in them.

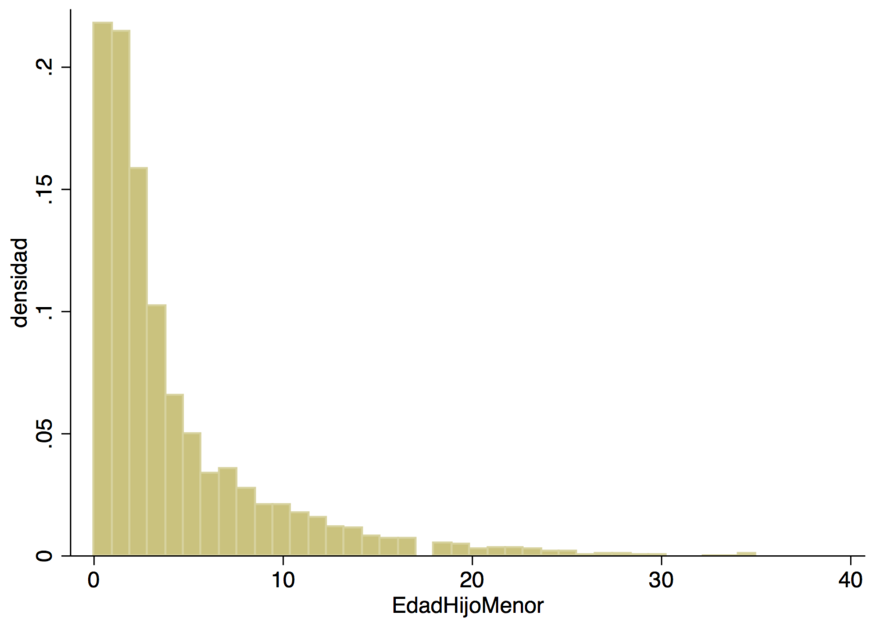

Fig. 9. Histogram of the assignment variable

5. Conclusion

This study presents OLS and IV estimates of the effect of the *AUH* on employment formality among heads of households in Argentina's slums. Three identification strategies are proposed. Using the age of the youngest child in the household is appropriate for identifying the causal effect given that it provides a source of exogenous variation. However, the manner in which it is used is very important. For example, if it used as a tool to separate the population into two groups (those whose youngest child is under 18 and those who are not), the estimated effect is null. On the other hand, if the multiple values that the instrument has are utilized, there is a significant decrease in the probability of formal employment (approximately -4.5%). This effect is obtained with the estimation of 2SLS, an estimator that can be interpreted as a weighted average of local effects. Exploiting the discontinuity between treatment probability and the chosen instrument, I estimate the causal effect with Regression Discontinuity Design in its two versions (parametric and non-parametric). The estimates are robust to different specifications in both versions and indicate a reduction in the probability formal employment from 5 to 8 percentage points. These estimates are not dissimilar to those obtained with Instrumental Variables. These results, though local average treatment effects, are similar to those obtained by Garganta and Gasparini (2015), who found that the probability of transitioning from an informal job is 5% lower for the poorest 40% of the population and approximately 10% lower for the poorest 25%. Both groups intersect with the population of interest of this study, and the result of the estimates are similar in spite of using different data and estimation strategies. The contribution of this study is to validate and complement Garganta and Gasparini's results, by analyzing a potentially eligible population (intention-to-treat) with specific data collected by an NGO regarding the reception of the plan and the amount of money received by heads of households, thus generating more certainty about the effect of the AUH on labor informality in Argentina.

Appendix A. Details on utilized estimations

Table 7: Estimation of Equation (1). OLS

Variable	**F**	**F**	**ProbF**	**ProbF**
IncAUH (I)	-0.0172***	-0.0154***	-0.0165***	-0.0146***
	(0.0019)	(0.0019)	(0.0006)	(0.0003)
Foreign		-0.0161		-0.0146***
		(0.0110)		(0.0014)
Age		0.0132***		0.0129***
		(0.0025)		(0.0003)
Age2		-0.0002***		-0.0002***
		(0.0000)		(0.0000)
Education		0.0038**		0.0035***
		(0.0018)		(0.0002)
Female		-0.2026***		-0.1971***
		(0.0138)		(0.0024)
Pregnant		0.0520**		0.0514
		(0.0214)		(0.0029)
ConstWorker		-0.1050***		-0.0984***
		(0.0192)		(0.0022)
Maid		0.1079***		0.1087***
		(0.0206)		(0.0022)
SelfEmployed		-0.0548**		-0.0504***
		(0.0246)		(0.0025)
DayLaborer		-0.1949***		-0.1889***
		(0.0138)		(0.0037)
CommEmployee		0.1168***		0.1253***
		(0.0347)		(0.0055)
Salesman		-0.1325***		-0.1271***
		(0.0134)		(0.0076)
NumPreg		-0.0476***		-0.0479***
		(0.0183)		(0.0018)
HDim		0.0004**		0.0005***
		(0.0002)		(0.0000)
NumChronIll		-0.0078		-0.0090***
		(0.0080)		(0.0012)
Constant	0.1323***	0.0343	0.1312***	0.0305***
	(0.0059)	(0.0415)	(0.021)	(0.0058)
N	5290	5290	5290	5290
R^2	0.0139	0.1180	0.0975	0.8709
Prob > F	0.0000	0.0000	0.0000	0.0000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Robust est. error in parenthesis

Table 8: Estimation of Equation (1). 2SLS (First Stage).

Variable	**(1)**	**(2)**	**Variable**	**(1)**	**(2)**
Foreign		-1.4689***	YCA11	-0.5883***	-1.2239***
Age		0.2053***	YCA12	-0.6613***	-1.3073***
Age2		-0.0023***	YCA13	-0.5952**	-1.2997***
Education		-0.0120	YCA14	-0.7470***	-1.2510***
Female		0.5080***	YCA15	-1.037***	-1.6166***
Pregnant		-0.0406	YCA16	-0.8548***	-1.3592***
ConstWorker		0.1878*	YCA17	-0.9064***	-1.6683***
Maid		-0.2688**	YCA18	-1.2304***	-1.8530***
SelfEmployed		0.0686	YCA19	-1.7119***	-2.1076***
DayLaborer		0.2435**	YCA20	-1.7119***	-2.2343***
CommEmployee		-0.3687***	YCA21	-1.4619***	-1.7513***
Salesman		-0.0930	YCA22	-1.5244***	-1.8194***
NumPreg		0.3609***	YCA23	-1.5690***	-1.7721***
HDim		-0.0002	YCA24	-1.7119***	-1.9403***
NumChronIll		0.0370	YCA25	-1.7119***	-2.2972***
YCA00	-	-	YCA26	-1.7119***	-2.5251***
YCA01	0.1037	0.1045	YCA27	-1.7119***	-1.6783***
YCA02	0.1216	-0.0184	YCA28	-1.7119***	-2.3392***
YCA03	0.1002	-0.1341	YCA29	-1.7119***	-2.0440***
YCA04	-0.0107	-0.2694**	YCA30	-1.7119***	-1.7876***
YCA05	0.3677**	0.2042	YCA33	-1.7119***	-1.0018***
YCA06	-0.1202	-0.6557***	YCA34	-1.7119***	-2.8670***
YCA07	-0.2119	-0.7267***	YCA35	-1.7119***	-2.1056***
YCA08	-0.0429	-0.4888***	Constant	1.7119**	-2.0687***
YCA09	-0.1214	-0.6356***			
YCA10	-0.6350***	-1.1300***			
N	5290	5290			
R^2	0.0299	0.1448			
$P(F)$	0.0000	0.0000			

Table 9: Estimation of Equation (1). 2SLS (Second Stage

Variable	**F**	**F**	**ProbF**	**ProbF**
$\hat{A}UHIncome(\hat{I})$	-0.0118	-0.0181*	-0.0149***	-0.0160***
	(0.0118)	(0.0105)	(0.0039)	(0.0016)
Foreign		-0.1999		-0.0167***
		(0.0187)		(0.0026)
Age		0.0137***		0.0132***
		(0.0034)		(0.0005)
Age2		-0.0002***		-0.0002***
		(0.0000)		(0.0000)
Education		0.0038**		0.0034***
		(0.0018)		(0.0002)
Female		-0.2015***		-0.1964***
		(0.0146)		(0.0025)
Pregnant		0.0519**		0.0514***
		(0.0214)		(0.0030)
ConstWorker		-0.1045***		-0.0981***
		(0.0193)		(0.0022)
Maid		0.1069***		0.1081***
		(0.0209)		(0.0023)
SelfEmployed		-0.0546**		-0.0503***
		(0.0245)		(0.0024)
DayLaborer		-0.1943***		-0.1885***
		(0.0140)		(0.0037)
CommEmployee		0.1158***		0.1245***
		(0.0348)		(0.0055)
Salesman		-0.1329***		-0.1273***
		(0.0136)		(0.0077)
NumPreg		-0.0467**		-0.0474***
		(0.0187)		(0.0019)
HDim		0.0004**		0.0005***
		(0.0002)		(0.0000)
NumChronIll		-0.0077		-0.0090***
		(0.0080)		(0.0013)
Constant	0.1235***	0.0289	0.1286***	0.0277***
	(0.0200)	(0.0467)	(0.067)	(0.0065)
N	5290	5290	5290	5290
R^2	0.0125	0.1177	0.0966	0.8702
$P(\chi^2)$	0.3149	0.0000	0.0002	0.0000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Robust est. errors in parenthesis

Table 10: Estimation of $\hat{\rho}$ in Equation (1). Instrumental Variables.

YCA (Z)	$\hat{\rho}$	**Std. Error**	R^2	**P**(χ^2)
00	-0.0038	0.0101	0.8332	0.000
01	-0.0204***	0.0043	0.8601	0.000
02	-0.0221**	0.0109	0.8533	0.000
03	-0.0119	0.0281	0.8685	0.000
04	0.0162	0.0958	0.5688	0.000
05	-0.0081	0.0088	0.8575	0.000
06	-0.0233**	0.0106	0.8471	0.000
07	-0.0321***	0.0124	0.7733	0.000
08	0.0202	0.1150	0.4840	0.000
09	0.0415	0.1313	.	0.000
10	-0.0242***	0.0092	0.8417	0.000
11	-0.0037	0.0073	0.8329	0.000
12	-0.0158**	0.0079	0.8704	0.000
13	-0.0180**	0.0075	0.8673	0.000
14	-0.0011	0.0116	0.8124	0.000
15	-0.0152**	0.0074	0.8708	0.000
16	-0.0105	0.0149	0.8655	0.000
17	-0.0059	0.0096	0.8464	0.000
18	-0.0329	0.0162	0.7645	0.000
19	-0.0222***	0.0080	0.8528	0.000
20	-0.0152*	0.0093	0.8708	0.000
21	-0.0163	0.0115	0.8700	0.000
22	-0.0177	0.0141	0.8678	0.000
23	-0.0206	0.0150	0.8596	0.000
24	-0.0022	0.0128	0.8216	0.000
25	-0.0346	0.0280	0.7435	0.000
26	-0.0117	0.0089	0.8681	0.000
27	-0.0030	0.0149	0.8279	0.000
28	-0.0051	0.0100	0.8422	0.000
29	-0.0381	0.0287	0.6947	0.000
30	0.0626	0.1121	.	0.000
33	-0.0736***	0.0246	.	0.000
34	-0.0061***	0.0012	0.8478	0.000
35	-0.0126	0.0199	0.8695	0.000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 11: Estimation of Equation (6). Fuzzy RDD (parametric). First Stage.

Variable	$p = 1$	$p = 2$	$p = 3$
Foreign	-0.3655***	-0.3651***	-0.3651***
Age	0.0148***	0.0131***	0.0131***
Age2	-0.0002***	-0.0001**	-0.0001**
Education	-0.0059**	-0.0060**	-0.0060**
Female	0.1269***	0.1267***	0.1267***
Pregnant	-0.0045	-0.0061	-0.0047**
ConstWorker	0.0635***	0.0634***	0.0641***
Maid	-0.0545**	-0.0564**	-0.0565**
SelfEmployed	0.0404	0.0390	0.0407*
DayLaborer	0.0866***	0.0861***	0.0863***
CommEmployee	-0.0678*	-0.0688*	-0.0706*
Salesman	0.0075	0.0089	0.0075
NumPreg	0.1106***	0.1096***	0.1050***
HDim	0.0000	0.0000	0.0000
NumChronIll	-0.0123	-0.0131	-0.0132
T	0.4487***	1.3801***	7.0360**
YCA	-0.0001	0.0064*	0.0638**
YCA.T	-0.0010***	-0.0067**	-0.0623**
YCA^2		-0.0000**	0.0002**
$\text{YCA}^2.T$		-0.0000	-0.0002*
YCA^3			0.0000**
$\text{YCA}^3.T$			0.0000
Constant	-0.1770	-1.096**	-6.7733**
N	5290	5290	5290
R^2	0.1351	0.1358	0.1367
$P(\chi^2)$	0.0000	0.0000	0.0000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 12: Estimation of Equation (6). Fuzzy RD (parametric). Second Stage.

Variable	$p = 1$	$p = 2$	$p = 3$
$\hat{D}$	-0.0495*	-0.0795***	-0.0809**
Foreign	-0.0116	-0.0224**	-0.0230*
Age	0.0108***	0.0110***	0.0110***
Age2	-0.0001***	-0.0001***	-0.0001***
Education	0.0033***	0.0032***	0.0032***
Female	-0.1982***	-0.1945***	-0.1943***
Pregnant	0.0519***	0.0515***	0.0515***
ConstWorker	-0.0979***	-0.0961***	-0.0960***
Maid	0.1098***	0.1080***	0.1079***
SelfEmployed	-0.0495***	-0.0485***	-0.0485***
DayLaborer	-0.1882***	-0.1857***	-0.1856***
CommEmployee	0.1272***	0.1251***	0.1250***
Salesman	-0.1251***	-0.1251***	-0.1251***
NumPreg	-0.0481***	-0.0448***	-0.0446***
HDim	0.0005***	0.0005***	0.0005***
NumChronIll	-0.0101***	-0.0106***	-0.0106***
YCA	0.0000	0.0001**	0.0001
YCA^2		0.0000***	0.0000
YCA^3			0.0000
Constant	0.0692***	0.0801***	0.0805***
N	5290	5290	5290
R^2	0.8545	0.8425	0.8412
$P(\chi^2)$	0.0000	0.0000	0.0000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 13: Estimation of Equation (6). Fuzzy RD (non-parametric). Second Stage.

Variable	$\Delta = 1$	$\Delta = 2$	$\Delta = 3$	$\Delta = 4$
$\hat{D}$	-0.0616*	-0.0559**	-0.0596**	-0.0652***
Foreign	-0.0197	-0.0136	-0.0153**	-0.0166***
Age	0.0080	0.0172**	0.0091	0.0092**
Age2	-0.0001	-0.0002***	-0.0001**	-0.0001***
Education	-0.0050*	-0.0021	-0.0012	0.0007
Female	-0.1536***	-0.1691***	-0.1713***	-0.1797***
Pregnant	-	0.0313	0.0020	0.0121
ConstWorker	-0.0691***	-0.0768***	-0.0792***	-0.0831***
Maid	0.1050***	0.0992***	0.0960***	0.0976***
SelfEmployed	-0.0242	-0.0331**	-0.0295***	-0.0323***
DayLaborer	-0.1725***	-0.1720***	-0.1727***	-0.1787***
CommEmployee	0.1384**	0.0925**	0.1118***	0.1001***
Salesman	-0.1206***	-0.1137***	-0.1081***	-0.1019***
NumPreg	-0.0224	-0.0352***	-0.0305***	-0.0322***
HDim	0.0006***	0.0006***	0.0006***	0.0006***
NumChronIll	0.0108	0.0026	-0.0014	-0.0034
Constant	0.1387	-0.0805	0.1267	0.1248
N	87	136	192	265
R^2	0.8374	0.8253	0.8322	0.8381
$P(\chi^2)$	0.0000	0.0000	0.0000	0.0000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

References

[1] Angrist, J., Krueger, A. (1991), “Does Compulsory School Attendance Affect Schooling and Earnings?,” *The Quarterly Journal of Economics*, 106 (4), 979-1014.

[2] Angrist, J., Imbens, G. (1994), “Identification and Estimation of Local Average Treatment Effects,” *Econometrica*, 62 (2), 467-475.

[3] Angrist, J., Imbens, G., and Rubin, D. (1995), “Identification of Causal Effects Using Instrumental Variables,” *Journal of the American Statistical Association*.

[4] Angrist, J., Imbens, G. (1995), ”Two-stage least squares estimation of average causal effects in models with variable treatment intensity,” *Journal of the American Statistical Association*, 90 (430).

[5] Angrist, J., Lavy, V. (1999), “Using Maimonides’ Rule to Estimate the Effect of Class Size on Scholastic Achievement,” *Quarterly Journal of Economics*, 114 (2), 533–575.

[6] Barbosa, A. and Corseuil, C (2013), “Bolsa Família, occupational choice and informality in Brazil”, , *International Policy Centre for Inclusive Growth Working Paper No. 118.*

[7] CEPAL-OIT (2014). *The employment situation in Latin America and the Caribbean. Conditional transfer programmes and the labour market*, Santiago de Chile.

[8] De Brauw, A., et al (2014), “Bolsa Família and household labour supply,” One Pager, No. 239, [online] http://www.ipc-undp.org/ pub/IPCOnePager239.pdf.

[9] Durbin, J. (1954), “Errors in Variables,” *Review of the International Statistical Institute*, 22, 23-32.

[10] Garganta, S., Gasparini, L. (2015), “The impact of a social program on labor informality: The case of AUH in Argentina,” *Journal of Development Economics*, 115, 99-110.

[11] Hahn, J., Todd,P. and Van der Klaauw,W (1999), “Evaluating the effect of an anti discrimination law using a regression-discontinuity design,” *NBER Working Paper No. 7131.*

[12] Hahn, J., Todd, P., and Van der Klaauw, W. (2001), “Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design,” *Econometrica*, 69 (1), 201–209.

[13] Hatrick, A. (2015), "Los efectos de un programa de transferencias de ingresos en la ciudad de Buenos Aires," *CAF Documento de Trabajo Nro. 2015/03*

[14] Imbens, G. and Kalyanaraman, K. (2012). "Optimal Bandwidth Choice for the Regression Discontinuity Estimator," *Review of Economic Studies*, 79 (3), 933–960.

[15] Imbens, G. and Lemieux, T. (2008), "Regression discontinuity designs: A guide to practice," *Journal of Econometrics*, 142 (2), 655-674.

[16] Lee, D., Card, D. (2007), "Regression discontinuity inference with specification error," *Journal of Econometrics*, 142.

[17] Lee, D., Lemieux, T. (2008), "Regression discontinuity designs in economics," *Working Paper*, 14723, NBER.

[18] Levy, S. (2010), *Buenas intenciones, malos resultados: política social, informalidad y crecimiento económico en México*, México: Océano.

[19] Medeiros, M., Britto, T. y Veras Soares, F. (2008), "Targeted cash transfer programmes in Brazil: BPC and the Bolsa Familia," *International Policy Centre for Inclusive Growth Working Paper,No. 46.*

[20] Newey, W. (1990), "Efficient Instrumental Variables Estimation of Nonlinear Model," *Econometrica*, 58, 809-838.

[21] Rubin, D. (1974), "Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies," *Journal of Educational Psychology*, 66, 688-701.

[22] Theil, H. (1971), *Principles of Econometrics*. New York: John Wiley.

[23] Van der Klaauw, W. (2002), "Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach," *International Economic Review*, 43 (4), 1249–1287.

[24] Vigorito, A. (2014), "Principales resultados de la evaluación de impacto de las Asignaciones Familiares-Plan de Equidad," *CEPAL.*

[25] Wald, A. (1940), "The Fitting of Straight Lines if Both Variables are Subject to Error," *Annals of Mathematical Statistics*, 11, 284-300.