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A Meta-impact Analysis

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The Welfare Impact of Social Housing Programs in Latin American: A Meta Impact Analysis

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ABSTRACT

Most Latin American countries have ABC (acronyms from the Spanish words of voucher, mortgage and savings) designed social housing program. Little is known of the impacts of these programs. In this paper we estimate the welfare affects of twelve of these programs. We estimate whether the programs crowd out or in pure private housing solutions, the impacts: on homeownership, the quality of the housing solution (walls and floors), overcrowding, and the access to basic services (potable water, sewerage, electricity and garbage collection), household labour supply, income, credit access, and education outcomes. We use the single difference propensity score method with a nearest neighbourhood matching technique. The estimated pooled effect sizes, using meta-analysis with random weighting, reveal no crowding out; increased ownership, improved physical quality of the house, increased access to potable water and sewerage but no increase in access to electricity and garbage collection. There are no discernable effects on education outcome of household's children, household's labour supply, access to credit, income and poverty status.

INTRODUCTION

All Latin American countries have acute housing shortage problems (see Ruprah 2010). As part of the policy response most Latin American countries have one or more public housing programs to complement pure private housing solutions. Public expenditure on housing by Latin American countries is about 1.2% of GDP. Another rationale, although not often invoked in poverty reduction policies in Latin America (see Solimano 2006), is social welfare policy that focuses on accumulation of assets rather than housing consumption per se.

However, there is little to no empirical literature on the welfare outcomes of these programs. The objective of this paper is to fill this lacuna. The paper presents the findings of the welfare effects of public housing programs in Latin American countries, where the welfare effects are estimated through propensity score matching method.

The welfare effects of public housing programs can be divided into private and external benefits. Private effects are those that accrue to the beneficiaries of the program. External benefits are those that accrue to society at large. We mainly consider private effects: ownership, quality of the housing solution, overcrowding, access to basic services (potable water, sewerage, electricity and garbage collection), labour supply, credit access, income and education outcomes. Neighbourhood effects and those to society at large are excluded given data limitations. However, we do evaluate if these programs crowd out or complement pure private housing solutions, i.e. they reduce housing shortage.

The countries included in the study are Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, Nicaragua, Panama, and Peru. These countries were chosen because they have the kind of housing programs studied and because data was available from household surveys that allowed identifying beneficiaries of the programs. The housing program(s) selected within each country are those that are based on the ABC (acronyms from the Spanish words of voucher, mortgage and savings) design that are directed at increasing housing owner occupancy rates. We do not study the other main type of public program that is directed at improving neighbourhoods–sites rather than increasing housing ownership.

The method used is a non-parametric impact calculation using single difference propensity score nearest neighbourhood matching techniques (see Guo and Fraser 2007). This method attempts to overcome the problem that non randomised studies are subject to confounding when households who receive the program benefit differ systematically from non beneficiaries. In this case factors or characteristics that influence treatment selection are also associated with the outcome of interest. Propensity score is defined as the a household's probability of receiving the program's benefit conditional on the observed covariates, such

that households with the same propensity score have the same distribution of measured covariates independent of whether the household received the benefit or not. Thus beneficiaries and the comparison group differ only in terms of their treatment status hence differences in outcome can be attributed to the treatment: receiving a program house. With the objective of synthesising the evidence from the individual impact evaluations we present our results using meta-analysis techniques that allow a transparent, objective, and replicable framework for a statistical synthesis of the individual program evaluations unlike a traditional narrative review (see Borenstein *et al* 2009). Further, presenting the results of 12 programs and 19 outcomes individually would become unmanageable.

Thus, we add to the literature by extending it to Latin American countries, use a methodology that attempts to isolate the program's impact, we consider multiple outcomes, and summarise out finding using a statistical based summary measure for each outcome.

The rest of the paper is divided into the following sections. First, we describe the ABC housing programs. We then briefly present a narrative review the existing literature. In the following section we outline the research methodology and describe the data used. We then present the meta-evaluation findings. We end with a discussion of our findings.

THE TREATMENT: A DESCRIPTION OF THE PROGRAMS

The treatment is the program's house. In Table 1 are shown the characteristics of nine selected programs in seven countries.

All programs require prior savings. The required saving requirements are either specified in absolute monetary value terms or as a percent of the monetary value of the housing solution, or as a percent of a voucher. The voucher is defined either as a given monetary sum or as a percentage of the value of the house but rarely is linked to household income. Mortgages are required for practically all the programs, although some programs have an option for low income households with zero mortgages. Typically, vouchers are provided by the government, mortgages are granted either by a private lender backed by some public guarantee system or financed by second tier public bank, and construction is carried out by private construction companies.

In the application-eligibility determination phase often, in the first instance, third parties like non government organisations or commercial banks are involved. Note these programs are not typically where a household gets a voucher and goes to the market to look for a new house to buy but where applications are done collectively for a given proposed housing park, and where financing is used in the construction phase with a buy contract by the beneficiary. The programs differ regarding their targeted population particularly regarding their upper

income cut off for eligibility. In general the upper ceiling is greater than the countries' poverty lines (see Ruprah 2010).

Table 1: A Description of Housing Programs

Country	Name of Program	Savings	Voucher	Mortgage M= (Price) - (V+S)	Solution
Chile	Fondo Solidario de Vivienda	400- 1,200 USD	11,000 -13,000 USD	Not required	At least 38 m2 with option to growth. <$25,500 USD
	Sistema de Subsidio Habitacional (DS 40)	2,000 USD	3,900-8,000 USD	Required (private financial institutions)	<140 m2. <$39,220 USD
Colombia	Programa de Proteccion al Patrimonio	120-190 USD	2,000-7,700 USD	Not required Required (Cajas de Compensasión	upgrading and extentions 7,000-31,700 USD (VIP and
	Subsidio Familiar de Vivienda	20% - 30% of housing unit price	83%-15% of the housing uni	familiar or other financial institution)	VIS)
Costa Rica		Not required if less than 1MW. More	100% - 7% of housing unit	Required (private or public	At least 42 m2 $8,400- 28,550
	Bono Ordinario (BFV)	than 1 MW 3-4% of housing unit price	value	financial institutions)	USD
	Bono ABC (BFV)	3%-10% of housing unit price	97%-21% of housing unit price	Required (private or public financial institutions)	At least 42 m2. <30,130 USD
Peru	Fondo Mi Vivienda	10% of the housing unit value	>5,400 USD or 15% of the housing unit value	Required (private or public financial institutions)	<15,000 unconditional bond, 30,000-60,000 USD good payer conditional bond
Panama	PARVIS	Not required	1,500 USD	Not required	Materials for housing improvement
	PROVISOL	Not required	2,000 USD	Not required	<21,243 USD
	PARVIS mejorado	Not required	around 7,500 (average value of actions)	Not required	Construction in owned plot of a housing unit of 36m2
Nicaragua	Programa Multifase de Vivienda	15% of the subsidy value or not required	up to 50% of the housing solution value or 1,300 USD (later 1,850 USD) for housing acquisition and 600 USD for improvements	Required (private financial institution) (not implemented)	Improvements (implemented). Housing units less than 60m2 and <$10,000 (not implemented)
Ecuador	Sistema de Incentivos de Vivienda	up to 10% of housing value not required for rural or marginal household with income less than 220 USD	<$5,000 USD	Required for housing acquisition (Mortgage genarator pending)	<25,000 USD

Note: Amounts in dollars are estimated for Costa Rica, Chile, Colombia and Peru using current exchange rates in 2008. In Ecuador and Peru the amounts wrre orginally expressed in US$.

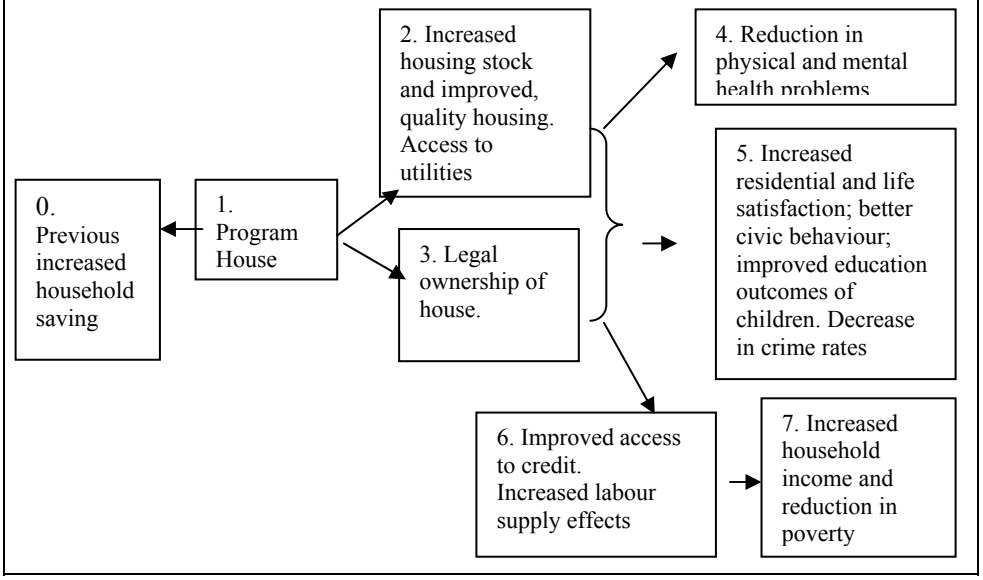
Thus, the individual housing programs, the treatments, evaluated in this paper have a number of common characteristics; they are similar but are not identical. Further, the programs are embedded in different socio-economic contexts that bear on their uptake, mix of participants, and program implementation. As Pauson et al 2005 note: "... *rarely, if ever, is a program equally effective in all circumstances because of the influence of context*", thus, a priori, we expect different effect sizes of the programs.

III. Expected Results: A Literature Review

Housing fulfils multiple roles. At its most basic it is a structure that provides shelter. As a physical space it can be defined by its technical features, i.e., the physical attributes i.e. the quality of the walls and ceilings and the building's connections to networks (piped potable water, electricity, and sewerage) and natural features (geographical location and surrounding environment). As a physical asset of the household that can be sold or used as a collateral. From a social aspect housing represents social capital with ramification of neighbourhoods and citizen behaviour.

These different aspects of housing separately or together have a number of purported benefits. A convenient way to interrelate the purported benefits of the program and the literature is a schematic causality chain (see Chart1).

Chart 1: A Schematic Casualty Chain of the Purported Benefits of ABC housing Programs



The programs are designed to increase household savings (link 0). The programs are expected to increase the housing stock and improve the quality of the house (link 1). The quality of the building materials and improved access to utilities (sewerage, electricity, potable water, and garbage collection) are expected to improve health outcomes particularly of the household's infants and children (link 4). Improved quality of the house and homeownership is expected to increase residential satisfaction, like satisfaction and positive civic behaviour (link 5). By providing legal ownership of the house (link 6) the programs are expected to increase households' access, as they can use the house as collateral, to both formal and informal credit and increase household's labour supply (link 6). This in turn is expected to increase household's income hence reduce poverty (link 7). An improved quality house plus homeownership is expected to increase both residential satisfaction, social and political participation and improved education outcome of household's children.

There is a large literature on housing. There are a number of surveys of housing and expected benefits covering both the theoretical basis and the empirical evidence. Amongst the most comprehensive; in that they cover the links listed in Chart 1, are by McCarthy Van Zandt and Rohe (2001) , Dietz and Haurin (2003), Lerman and Mckernan (2008). There are also reviews of the literature on a

particular link, for example the link between overcrowding and health and education outcome (Deputy Prime Ministers Office 2004), between home ownership and labour market effects (see Havet and Perot 2010), between the quality of the house and health outcome (Thomson et al 2001).

The reading of these reviews reveals the following characteristics of the literature. First, program theory is often ambiguous regarding the direction of the expected outcomes. For example, Coulson and Fisher (2008) compare the comparative static results of four models that relate individual's housing tenure and the individual's labour market outcomes (wages and unemployment). As shown the effects range from positive to zero to negative.

Table X: Individual Tenure and Individual Labour Market Outcomes: Model Predictions

	Comparative static Effects:	
Model	Unemployment	Wage
Oswald	+	0
Search	+/-	Same as unemployment
Search with firm entry and bargaining	+	-
Search with firm entry and wage posting	-	-

Source: modified from Table 1 in Coulson and Fisher (2008)

Second, empirical studies generally do not isolate the casual effect of the treatment. As Dietz and Haurin (2003) note that" … *early studies of homeownership's consequences may be seriously flawed when viewed from contemporary perspective*…", particularly of "*studies of the consequences of homeownership because research requires the isolation of a single variable in what is often a complex behavioural relationship*", and conclude that "… *a new generation of studies is needed to confirm established and intuitively sensible results.*".

Third, the literature is generally and of housing program evaluations in particular mainly USA and UK centric. This poses problems in terms of reference for our evaluation. The program "treatment" evaluated is typically tenant-based rental assistance rather than for vouchers for homeownership. An experimental designed evaluation, see Wood et al. 2008 Improvement in quality of the housing often invoke treatments like "… installation *of heating, insulation, double glazing, and general refurbishmen*t." (See Thomson, Petticrew and

Morrison, 2001). In Latin America the concern is much more basic, for example dirt floors replaced by concrete floors.

There are a number of exceptions to the above threefold characterisation of the existing literature. A study that used natural experimental data in Mexico to evaluate the program *Piso Firme* (which replaces dirt floors with cement floors) found that the program improved children's health (incidence of parasitic infestations, diarrhea, and the prevalence of anemia) and improved residential and life satisfaction of adults (Cattaneo et al 2008). Another example, drawing on data of underdeveloped countries, is a meta-impact evaluation of the effect of potable water and sanitation on diarrhoea by Waddington and Snilstveit (2009). An evaluation of a housing program for displaced households due to violence in Colombia used a double difference propensity score matching method. The study by Rodriguez (2008) found that although the program improved the physical quality of housing it had no effect on education, health and income of the beneficiaries.

In addition, there are a number of individual program evaluations that were part of the research agenda leading to this paper.[1] Pecha (2010a) evaluates two Colombian programs and in a separate paper two of Panama's programs (2010b). Marcano (2010) studies Ecuador's housing program. Medellin (2010) evaluates a housing program in Costa Rica. Ruprah and Marcano (2007) evaluate three Chilean housing programs.

RESEARCH STRATEGY: METHODOLOGY AND DATA

The method used is a single difference propensity score approach for isolating the effect of the housing programs. In this paper we use existing data bases, i.e., administrative data from the public entities that manage the programs and household surveys normally carried out by the country's national statistical institutes.

Method

For determining the effect attributable to the program we use the propensity-score matching method. This involves three steps. First, the construction of the propensity score. Second, an assessment of the degree that matching with the propensity score has resulted in a matched sample, i.e., in which the distribution

[1] The individual housing program evaluations, in addition to impact estimations also evaluate transparency, incidence and targeting efficiency of the programs and adequacy of the size of the program, and the adequacy of the size of the monetary value of the voucher and cost-benefit analysis of the programs. The individual evaluations were part of a housing program evaluation of the Office of Evaluation and Oversight of the Inter-American Development Bank.

of variables are similar between the treated and untreated households. The third step is to calculate the impact of the program.

This method balances the distribution of observed covariates between the treatment group (beneficiaries of the program) and a comparison group (non-beneficiaries) based on similarity of their predicted probabilities of having a given treatment, i.e. their propensity score. Specifically, two groups are constructed: households that are beneficiaries of the program (denoted $D_i=1$ for household i) and non beneficiary households ($D_i=0$). Beneficiaries are matched to non beneficiaries on the basis of their propensity score, P (x_i): P (x_i) = Prob ($D_i=1 \mid x_i$) Where x_i is the vector of control variables and P (x_i) is obtained from the predicted values from a standard logit participation equation using the vector of covariates x_i. Using the estimated propensity scores matched pairs are constructed based on how close the scores are across the two samples. Specifically we use nearest neighbour with replacement method.

Second, any estimated impact depends critically on the success of having created a relevant comparison group. Therefore, a number of procedures are used to test the adequacy of the comparison group. We use the following. For individual covariates the tests consist of a simple inspection of the average values of the covariates between treated and comparison group before and after matching (the balancing table) plus a "t" test with the null hypothesis that the means of the covariates for the treated and comparison group are individually equal. In addition we use the Hotelling's T-square test that tests the equality of the vectors of means between the two groups. For the entire distribution of propensity score the tests consist of: (a) a chart of the distribution of propensity score after matching compared to the chart of propensity score before matching; (b) the formal Kolmogorov-Smirov test, which has the null that the treated and comparison group propensity score distributions are equal.

The third step is to calculate the impact of the program on the outcome of interest. This is calculated as the difference in the average value of the outcome between the treated and comparison groups. Specifically, the mean impact of program on the outcome of interest, ΔO, is calculated from:

$$\Delta O_k = \frac{1}{N}\sum_{i \in N}\left(y_{ik} - \frac{1}{J}\sum_{j \in J_i} y_{jk} \right)$$

Where ΔO_k is the difference in the outcome category k of the household attributable to program, y_{ik} is the k outcome category of the i^{th} non-homeowner matched to the j^{th} homeowner, N is the total number of beneficiaries, J is the total number of matched non- beneficiaries.

Our intent is to compute a summary effect size and its confidence interval. Although the individual program evaluations are functionally equivalent, we previously noted that due to differing contexts we expect there to be different effect size, therefore we use the random effects model to obtain a summary effect size. The random effects model weighs each evaluation with the inverse of the variance, with the goal of minimising two sources of variance within program study error in estimating the effects and the variation in true effects across the individual evaluations. The weight given to each program's evaluation is:[2]

$$W_i^* = \frac{1}{V_{Y_i}^*},$$

Where $V_{Y_i}^*$ is the within-evaluation variance for program i plus the between – evaluations variance, T^2, i.e. $V_{Y_i}^* = V_{Y_i} + T^2$

The weighted mean: $M^* = \frac{\sum_{i=1}^{k} W_i^* Y_i}{\sum_{i=1}^{k} W_i^*}$, is the sum of the products (of the effect size multiplied by weight) divided by the sum of the weights.

Data and variables

The data used in this evaluation are from the country's household survey. The surveys allow the identification of the beneficiaries of the housing programs. The following Table 2 summarises key features of the data used by country and the sample size of the treated and comparison groups after the matching process.

There are a number of limitations of the data that that should be kept in mind in the reading of the findings of this paper. First, although the household survey indentifies the beneficiaries they do not ask if the respondent's current house is the one that was financed by the program. The size of this problem may not be important as no self declared beneficiary reports that he or she is currently renting, further, most programs prohibit sale before full payment of the mortgage. Second, only for programs in Chile, and Colombia and Costa Rica is there information of the year in which the program house was received, although for all programs the reference period is greater than one year. If the expected outcomes are time dependent then with households for whom only a few years have passed from the date of treatment we will be underestimating the impacts. Third, for some countries the sample size may be a problem as it reduces the precision of the impact estimates. Thus each limitation could undermine our conclusions.

[2] See page 73 of Borenstein et al (2009).

Table 2: Key Characteristics of the Household Surveys					
Country	Size (# of households)	Beneficiaries (# of households)	Used in the Impact Calculations (# of households)		
			Matched sample	Treated	Control
Chile: FSV	268,873	127	418	126	292
Chile SSH	268,873	68	134	67	67
Chile RURAL	102,979	1,179	3492	1145	2347
Colombia (2003)	22949	613	1162	581	581
Colombia (2008)	13611	85	165	83	82
Costa Rica (2007)	12,361	613	1162	581	581
Dominican Republic	37,817	865	1,270	635	635
Ecuador (2005)	12361	613		41	43
Nicaragua (2005) \	6,898	101	190	95	95
Panamá	6363	127	441	120	321
Peru (2007)	22,204	68	5031	30	5001

Further, another limitation is on the feasible set potential outcomes. The household surveys' information limits the set of outcomes that can be evaluated. The surveys have the information to estimate the following outcomes: (i) the physical quality adequacy of the program house (measured by quality of the walls and floors); (ii) overcrowding i.e. persons per room; (iii) the connection to utilities networks i.e. access to potable water, sewerage, electricity and garbage collection; (iv) legal status of tenure; (v) access to credit (formal and informal); (vi) education indicators (school assistance and schooling lag); (vii) labour market indicators (occupation ratio, and average hours worked); and (viii) income (household income, and poverty status of the household). Thus, the effects, identified in Chart 1 above, on health outcomes, increased residential and life satisfaction and social and political participation, i.e. the good citizen effect, and crime rates, cannot, in this study, be evaluated. [3] Nonetheless, from program theory point of view links 2, 3, 6 and 7 can be estimated.

The participation equations have a high degree of commonality in terms of the covariates used. Typically the individual participation equation covariates

[3] For an empirical investigation of the relation between life satisfaction and homeownership see Ruprah (2010), and for the relation between homeownership and civic behaviour see Pecha and Ruprah (2010). Both studies use data for eighteen Latin American countries from the opinion survey Latin Barometer

include household characteristics (gender, age, education, marital status, and occupation of household head, family size and composition) and location (if program house is in the metropolitan area, urban or rural and in which state).

However, the data set also limits the covariates used in the estimated participation equations of the individual evaluations. For example the variables capturing neighbourhood characteristics of where the program house is located are unknown. This excludes measures of residential segregation which may break the casual links depicted in Chart 1.

WELFARE EFFECTS: OVERALL FINDINGS

In this section we present a summary of the welfare impact calculations. We do so in two stages first we present the post matching tests. Second we present the impact calculations.

Matching Tests

We report two statistical tests to determine the adequacy of the comparison group[4]. For the vector of covariates used in the participation equation we use the Hotelling "t" test with the null hypothesis that the means of the covariates for the treated and comparison group are jointly equal. For the entire distribution of propensity score tests consist of a Kolmogorov-Smirov, K-S, test, which has the null that the treated and comparison group propensity score distributions are equal. The estimated statistics for each program evaluation is given in Table 3.

Using the significance level of 0.05 the null hypotheses of the means of individual covariates of the treated and comparison groups are jointly equal and that density distributions of the propensity scores of the treated and comparison group are equal—cannot be rejected. Thus for all programs the matched groups pass both the Hotelling and the Kolmogorov-Smirov tests

[4] The individual program participation equations and the corresponding balancing tables (with "t" tests, reduction in bias , etc) are available from the author.

Table 3: Balancing Tests

Country	Hotelling Test		K-S Test: Smaller group	D	P-Value	Corrected
Chile: FSV	F(25,314) =	0.3690	0:	0.106	0.149	
	Prob > F(25,314) =	0.9979	1:	-0.029	0.863	
			Combined K-S:	0.106	0.296	0.254
Chile: SSH	F(26,97) =	0.3154	0:	0.016	0.984	
	Prob > F(26,97) =	0.9993	1:	-0.032	0.938	
			Combined K-S:	0.032	1.000	1.000
Chile RURAL	F(32,2325) =	0.2325	0:	0.005	0.97	
	Prob > F(32,2325) =	1.0000	1:	-0.004	0.979	
			Combined K-S:	0.005	1.000	1.000
Colombia (200	F(25,66) =	1.6317	0:	0.022	0.978	
	Prob > F(25,66) =	0.5888	1:	-0.022	0.978	
			Combined K-S:	0.022	1.000	1.000
Colombia (200	F(26,139) =	0.6548	0:	0.012	0.988	
	Prob > F(26,139) =	0.8961	1:	-0.024	0.953	
			Combined K-S:	0.024	1.000	1.000
Costa Rica	F(13,1148) =	0.5513	0:	0.005	0.987	
	Prob > F(13,1148) =	0.8927	1:	-0.011	0.929	
			Combined K-S:	0.011	1.000	1.000
Ecuador	F(15,66) =	0.3340	0:	0.024	0.976	
	Prob > F(15,66) =	0.9893	1:	-0.024	0.976	
			Combined K-S:	0.024	1.000	1.000
Dominican Re	F(14,1612) =	0.8524	0:	0.003	0.997	
	Prob > F(14,1612) =	0.6117	1:	-0.008	0.976	
			Combined K-S:	0.008	1.000	1.000
Nicaragua	F(11,530) =	1.1200	0:	0.011	0.967	
	Prob > F(11,530) =	0.3428	1:	-0.004	0.996	
			Combined K-S:	0.011	1.000	1.000
Panama (2003)	F(23,332) =	0.6376	0:	0.006	0.994	
	Prob > F(23,332) =	0.9017	1:	-0.011	0.978	
			Combined K-S:	0.011	1.000	1.000
Panama (2008)	F(27,358) =	0.4282	0:	0.005	0.995	
	Prob > F(27,358) =	0.9951	1:	-0.01	0.979	
			Combined K-S:	0.01	1.000	1.000
Peru	F(14,51) =	0.3099	0:	0.03	0.97	
	Prob > F(14,51) =	0.9902	1:	-0.03	0.97	
			Combined K-S:	0.03	1.000	1.000

Meta-Impact Estimations

Presenting each of the 12 programs and the set of 19 outcomes individually would be unmanageable. Further, we are interested in integrating the findings of the individual studies to estimate the overall effect. Thus we use meta-impact techniques. Further, an oft made criticism of Meta –evaluations is that they suffer from a large methodological diversity of the individual studies reviewed; a problem we do not have as the method is the same for each of the individual evaluations.

An implicit rationale for housing programs is that in their absence people would consume inadequate amounts of housing. Thus housing programs must satisfy the necessary condition for effectiveness that they increase the number of households housed in their own units and do not crowd out pure private housing solutions. The pooled size effect, see Table 4, of the outcome "quantitative shortage (i.e., the number of households doubling (or more) up or/and living in slums) is negative and statistically significant. Thus there is no crowding out, instead the programs add to the housing stock. Further, they improve the physical quality of the house in terms of the quality of the walls and floors and reduce overcrowding (measured by more than three persons per room).

Table 4: Quantitative Shortage, Walls, Floors, Overcrowding		
Outcome	Pooled Effect Size	Confidence Interval (95%)
Quantitative Shortage	-0.05	-0.08, -0.03
Walls	0.03	0.00, 0.05
Floors	0.26	0.07, 0.38
Overcrowding	-0.02	-0.01, -0.03

A second set of outcomes are measures of access to utilities potable water, sewerage, electricity, and garbage collection. The pooled effect sizes, see Table 5, show that the programs increase access to potable water and sewerage but do not increase access to electricity and garbage collection. For the latter two access measures the pooled effect sizes although are positive are statistically insignificant.

Table 5: Access to Utilities		
Outcome	Pooled Effect Size	Confidence Interval (95%)
Potable Water	0.02	0.00, 0.04
Sewerage	0.06	0.00, 0.12
Electricity	0.01	-0.00, 0.03
Garbage Collection	-0.00	-0.03, 0.03

The physical quality of the house, access to utilities particularly electricity, and the reduction in overcrowding have all been invoked as channels of causality in improving household's children's educational outcomes. Table 6 shows that the housing programs have no effect on children's education as measured by school attendance and schooling lag. The pooled effect sizes are statistically insignificant.

Table 6: :Education Indicators of Children 10 to 16 years)		
Outcome	Pooled Effect Size	Confidence Interval (95%)
School Attendance: Male	0.01	-0.01, 0.03
Female	0.02	-0.04, 0.08
Schooling Lag: Male	0.03	-0.02, 0.08
Female	-0.07	-0.15, 0.01

The third set of outcomes is related to home equity and access to credit. Home equity is measured by home ownership and reduction in irregular tenure). The increase in housing equity is, in turn, expected to increase access to credit. The pooled effect sizes, see Table 7, shows that the programs increase home ownership and increase regular tenure. However, the increased home equity does not result in an increase in access to formal and informal credit. A possible explanation for this result is that commercial lenders typically have conservative requirements for clear title and collateral (that differs from the definition in

household surveys) and if program beneficiaries mortgage payments may already represent 25-30% of the household's income, excluding further credit.

Table 7: Ownership and Access to Credit		
Outcome	Pooled Effect Size	Confidence Interval (95%)
Ownership	0.28	0.19, 0.36
Regular Tenure	0.13	0.05, 0.21
Formal Credit	-0.00	-0.03, 0.03
Informal Credit	0.22	-0.02, 0.05

Table 8 shows that the programs have no effect on labour supply either measured by the occupation ratio or by the average hours worked by household members (aged between 17 and 64 years) neither for mares nor females.

Table 8: : Labour Supply Effects (Adults aged 17 to 64 years)		
Outcome	Pooled Effect Size	Confidence Interval (95%)
Occupation Ratio: Males	0.01	-0.01, 0.04
Females	0.02	-0.01, 0.04
Hours Worked: Males	0.13	-0.75, 1.01
Females	-1.30	-3.26, 0.65

The final set of outcomes evaluated contains the outcomes: household income, moderate poverty and extreme poverty status. Table 9 shows that the housing programs have no effect on household income hence no effect on moderate and extreme poverty.

Table 9: Income, and Poverty		
Outcome	Pooled Effect Size	Confidence Interval (95%)
Household Income	-21.48	- 50.06, 7.10
Moderate Poverty	-0.01	-0.03,0.10
Extreme Poverty	0.02	-0.02, 0.06

Sensitivity to hidden bias

The method used in the above calculations of the impact of the housing programs was dictated by the absence of baseline data. Panel data would have allowed using double difference method. Most of the programs use an eligibility index, administrative data of applicants who were given the treatment and those that were rejected based on eligibility cut off criterion could have allowed us to use regression discontinuity method. Such data was not available. The type of data available, cross- sectional data, restricted the method to single difference propensity score matching method. This method to estimate impacts is risky. *A priori* is the weakest method in terms of the confidence that selection bias has been eliminated. By definition matching is done only on observables (the vector

x of variables used in the logit regression) and assumes that there are no systematic differences in unobservable between treated households and those in the comparison group. As we have previously noted the x vector of variables used in the estimations may be missing key variables.

Although this assumption, that unconfoundedness holds, cannot be tested it can be assessed, Rosembaum Bounds (Rosembaum, 2002, and Becker and Galindo, 2007) approach determines how strongly an unmeasured variable (i.e. not included in the vector x used in the participation equation) must influence the selection to undermine the impact conclusions.

The procedure determines for increasing levels of odds of different assignment increasing bounds and their significance of over estimation and under estimation of impacts. Overestimation occurs if there is positive (unobserved) selection, i.e. households most likely to participate in the program also have a high probability of a given outcome, even in the absence of participation and given that they have the same x vector as the households in the comparison group. Underestimation occurs if there is negative (unobserved) selection bias.

Table 10: Rosenbaum Tests

	Chile FSV		Chile SSH		Chile Rural		Colombia 2003		Ecuador		Costa Rica	
Gamma	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**
1	0.00	0.00	0.00	0.00	0.00	0.00	0.220632	0.220632	0.02	0.02	3.30E-16	3.30E-16
1.1	0.00	0.00	0.00	0.00	0.00	0.00	0.184967	0.257936	0.02	0.01	3.40E-14	0.00
1.2	0.00	0.00	0.00	0.00	0.00	0.00	0.155877	0.294986	0.03	0.01	1.50E-12	0.00
1.3	0.00	0.00	0.00	0.00	0.00	0.00	0.131617	0.3308	0.04	0.00	3.60E-11	0.00
1.4	0.00	0.00	0.00	0.00	0.00	0.00	0.111334	0.365202	0.06	0.00	5.60E-10	0.00
1.5	0.00	0.00	0.00	0.00	0.00	0.00	0.094334	0.398096	0.07	0.00	5.90E-09	0.00
1.6	0.00	0.00	0.00	0.00	0.00	0.00	0.080054	0.429444	0.09	0.00	4.60E-08	0.00
1.7	0.00	0.00	0.00	0.00	0.00	0.00	0.068033	0.459246	0.11	0.00	2.80E-07	0.00
1.8	0.00	0.00	0.00	0.00	0.00	0.00	5.79E-02	0.487527	0.13	0.00	1.30E-06	0.00
1.9	0.00	0.00	0.00	0.00	0.00	0.00	4.93E-02	0.514331	0.15	0.00	5.40E-06	0.00
2	0.00	0.00	0.00	0.00	0.00	0.00	4.21E-02	0.53971	0.17	0.00	1.80E-05	0.00
2.1	0.00	0.00	0.00	0.00	0.00	0.00	3.59E-02	0.563724	0.19	0.00	5.60E-05	0.00
2.2	0.00	0.00	0.00	0.00	0.00	0.00	3.07E-02	0.586437	0.21	0.00	1.48E-04	0.00
2.3	0.00	0.00	0.00	0.00	0.00	0.00	2.63E-02	0.607912	0.23	0.00	3.58E-04	0.00
2.4	0.00	0.00	0.00	0.00	0.00	0.00	2.25E-02	0.628213	0.26	0.00	7.90E-04	0.00
2.5	0.00	0.00	0.00	0.00	0.00	0.00	1.93E-02	0.647405	0.28	0.00	1.61E-03	0.00
2.6	0.00	0.00	0.00	0.00	0.00	0.00	1.65E-02	0.664279	0.30	0.00	3.06E-03	0.00
2.7	0.00	0.00	0.00	0.00	0.00	0.00	1.42E-02	0.64919	0.32	0.00	5.45E-03	0.00
2.8	0.00	0.00	0.00	0.00	0.00	0.00	1.22E-02	0.634467	0.35	0.00	9.19E-03	0.00
2.9	0.00	0.00	0.00	0.00	0.00	0.00	1.05E-02	0.620108	0.37	0.00	1.47E-02	0.00
3	0.00	0.00	0.00	0.00	0.00	0.00	9.00E-03	0.606109	0.39	0.00	2.26E-02	0.00

	DR		Nicaragua		Panama 2003		Panama 2008		Peru	
Gamma	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**	**p_mh+**	**p_mh-**
1	0.012269	0.012269	0.181928	0.18193	0.00	0.00	0.00	0.00	0.18	0.18
1.1	0.050847	0.002084	0.146957	0.22017	0.00	0.00	0.00	0.00	0.20	0.15
1.2	0.140578	0.000303	0.119136	0.25879	0.01	0.00	0.00	0.00	0.23	0.13
1.3	0.286339	0.000039	0.096705	0.29683	0.01	0.00	0.00	0.00	0.26	0.11
1.4	0.464603	4.70E-06	0.078594	0.33391	0.02	0.00	0.00	0.00	0.28	0.10
1.5	0.421769	5.20E-07	0.063949	0.36979	0.03	0.00	0.00	0.00	0.31	0.08
1.6	0.270601	5.50E-08	0.052089	0.40429	0.05	0.00	0.00	0.00	0.33	0.07
1.7	0.158722	5.70E-09	0.042472	0.43732	0.06	0.00	0.00	0.00	0.35	0.06
1.8	8.59E-02	5.80E-10	3.47E-02	0.46881	0.08	0.00	0.00	0.00	0.37	0.05
1.9	4.33E-02	5.70E-11	2.83E-02	0.49877	0.10	0.00	0.00	0.00	0.39	0.05
2	2.05E-02	5.60E-12	2.31E-02	0.5272	0.13	0.00	0.00	0.00	0.41	0.04
2.1	9.19E-03	5.50E-13	1.89E-02	0.55413	0.16	0.00	0.00	0.00	0.43	0.04
2.2	3.93E-03	5.40E-14	1.55E-02	0.5796	0.18	0.00	0.00	0.00	0.45	0.03
2.3	1.62E-03	5.30E-15	1.27E-02	0.60367	0.22	0.00	0.00	0.00	0.47	0.03
2.4	6.40E-04	5.60E-16	1.04E-02	0.62639	0.25	0.00	0.00	0.00	0.49	0.02
2.5	2.46E-04	0.00	8.54E-03	0.61388	0.28	0.00	0.00	0.00	0.50	0.02
2.6	9.20E-05	0.00	7.01E-03	0.59019	0.31	0.00	0.00	0.00	0.52	0.02
2.7	3.40E-05	0.00	5.76E-03	0.56711	0.35	0.00	0.00	0.00	0.53	0.02
2.8	1.20E-05	0.00	4.73E-03	0.54467	0.38	0.00	0.00	0.00	0.55	0.01
2.9	4.30E-06	0.00	3.89E-03	0.52289	0.41	0.00	0.00	0.00	0.56	0.01
3	1.50E-06	0.00	3.20E-03	0.50179	0.44	0.00	0.00	0.00	0.57	0.01

Table 10 shows the results of applying the procedure. The table shows the value of gamma (the odds ratio) of increasing values, by intervals of 0.1, and the corresponding significance level for overestimation (p_mh+) and overestimation (p_mh-). For gamma equal to zero the study is free from bias. If for a given value of gamma greater than unity when either p_mh+) or (p_mh-_) < 0.05 then the confidence interval for the impact would include zero, hence at that level of gamma an unobserved variable would use the treatment assignment to differ between the treatment and comparison group and cast doubt on the conclusions obtained by the matching used

The Rosembaum test results of the upper bound and lower bound for Chile FSV, Chile SSH, Chile rural, Costa Rica and Panama (2009) programs shows that they are insensitive, at the five percent significance level, to bias that would up to triple the odds. However, the impact estimations become insignificant for Ecuador at odds of 1.4, for DR at odds at 1.1, for Panama (2003) at odds of 1.6. Only two programs, Nicaragua and Colombia (2003) are very sensitive to bias from unobserved variables.

DISCUSSION

Most Latin American countries have one or more public housing programs. However, there is little to no empirical literature on the welfare outcomes of these programs. The objective of this paper is to fill this lacuna.

In this paper we estimate the welfare effects of housing programs in Latin America. We estimate the impacts on homeownership, the quality of the housing solution, overcrowding, access to basic services (potable water, sewerage, electricity and garbage collection), labour supply, income, credit access, education outcomes. The method used is a non-parametric impact calculation using single difference propensity score nearest neighbourhood matching techniques. The individual program size effects were aggregated into pooled size effects using a random weighting scheme.

We find that the housing programs do not crowd out private market. They result in an increase in home ownership and a reduction in irregular tenancy. They result in an improvement in the physical quality of housing in terms of the quality of the floors and walls, and in an increased access to potable water and sewerage. However, they have no discernable impact on access to electricity, and garbage collection. There is no increased labour supply in terms of occupation ratio and hours worked, or household income increasing, or poverty reduction effects. There are no education effects as measured by school attendance and education lags, effects on household's children.

The findings that housing programs do have an impact on first round outcomes like homeownership, overcrowding, the quality of housing and access to some utilities, but does not have any further impact on second round outcomes could possibly be due to residential segregation. Social housing is built typically in housing parks located in the periphery of cities that lead to the segregation of low-income families, which would undermine the expected benefits of housing programs that aim for increased owner-occupancy rates.

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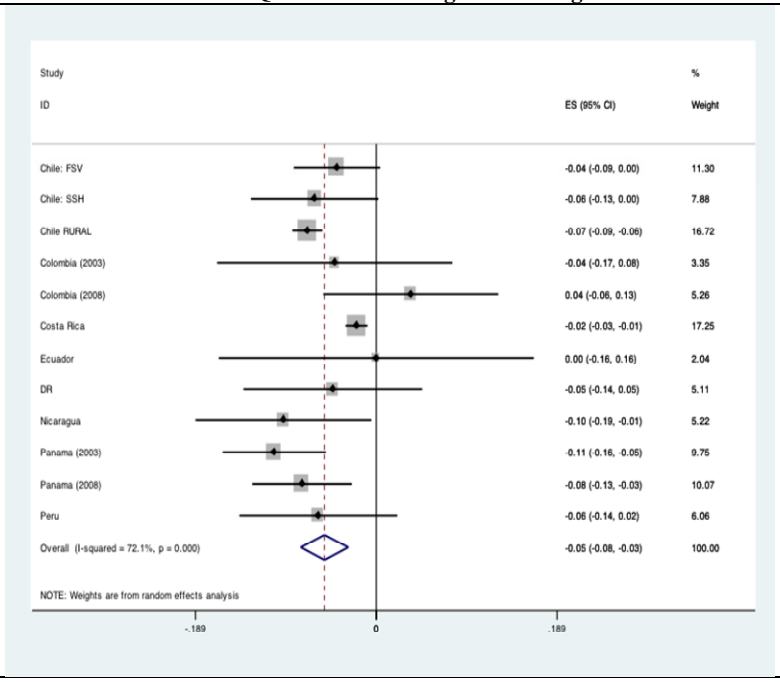
ANNEX

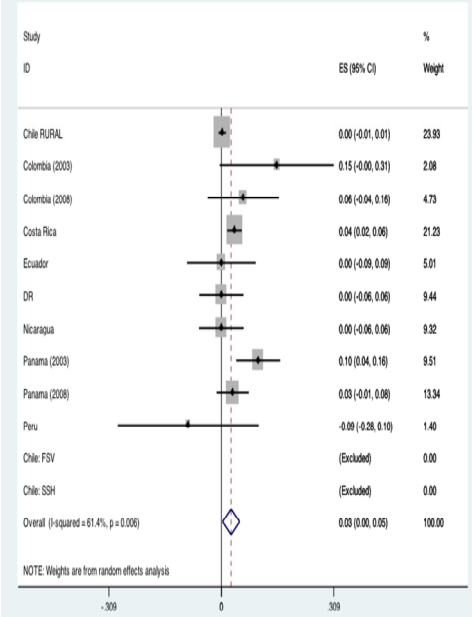
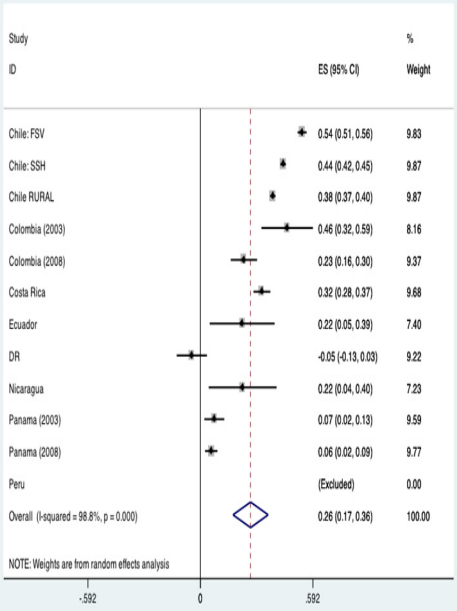
The meta-impact estimations are presented in the forest charts below. In the charts the horizontal line is the scale measuring the treatment effect, the solid vertical line in the middle is where the treatment and comparison groups have the same mean level; there is no difference between the two, hence a zero impact. Towards the left of the solid line is a negative impact and towards the right is a positive effect. The pooled results are shown with a diamond shape where the widest bit in the middle of the diamond is located at the meta-point estimate (drawn also as a dotted vertical line) and the horizontal width of the diamond is the confidence interval, at 95% level, which are also given numerically in the penultimate column in the charts for the individual programs. If the confidence interval crosses the line of no effect this implies that there is no statistically significant difference. The ultimate column is the weight, in our case using the random weighing mechanism, given to each program evaluation..

Do public housing programs crowd out private solutions.?

Chart A1: Quantative Shortage: Crowding out?

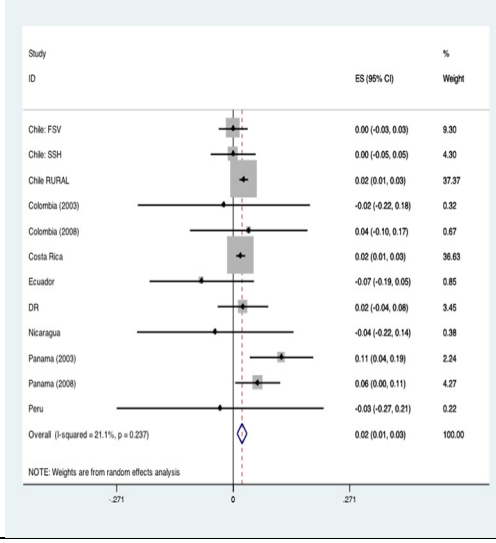


Do the housing programs improve the physical quality of housing?

Chart A2: Walls	**Chart A3: Floors**
	

Do the programs reduce overcrowding?

Chart A4: Households with less than three people per room



Do programs increase access to utilities?

Chart A5: Access to Potable Water

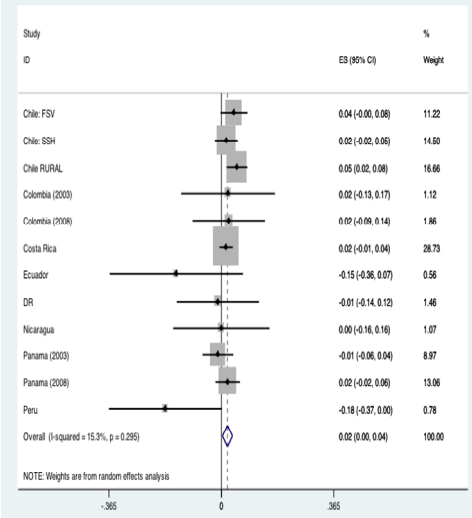


Chart A6: Access to Sewerage

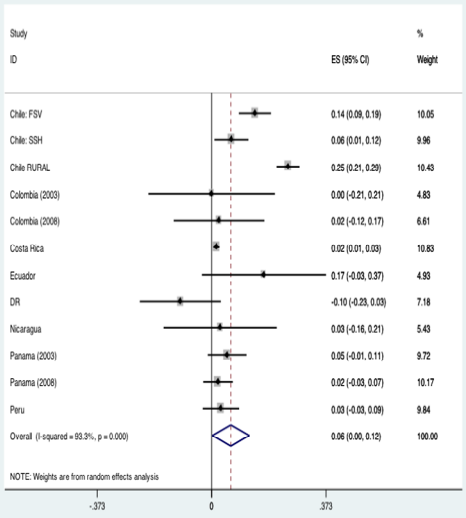


Chart A7: Access to Electricity

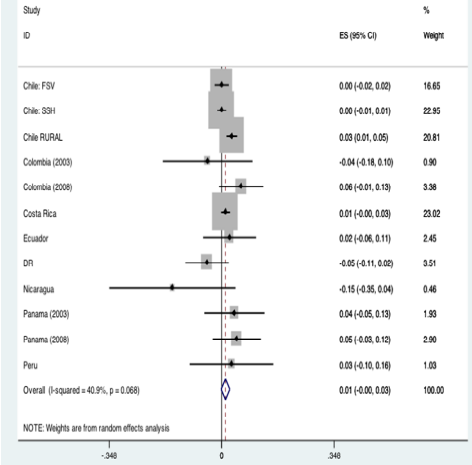
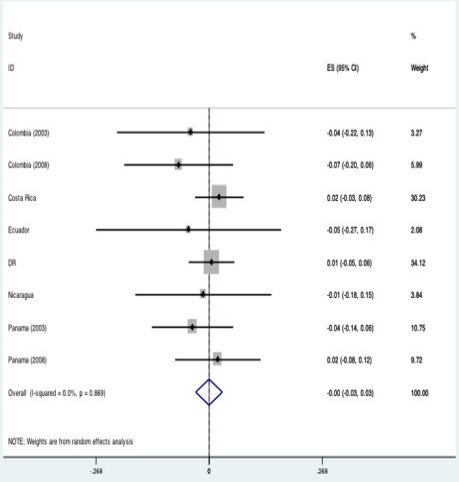
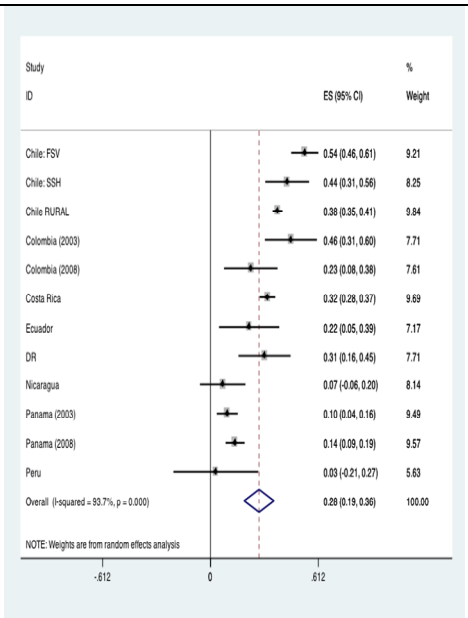
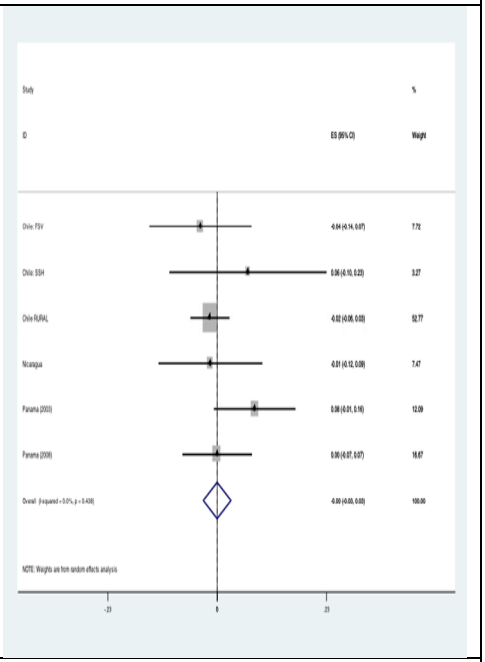


Chart A8: Access to Garbage Collection



Did he housing programs increase home ownership and access to formal and informal credit?

Chart A9: Home Ownership	**Chart A10: Access to Formal Credit**
	

Was there a labour supply effect?

Chart A11: Occupation Ratio (females 17 to 64 year age)	**Chart A12 : Occupation Ratio (males 17 to 64 year age)**

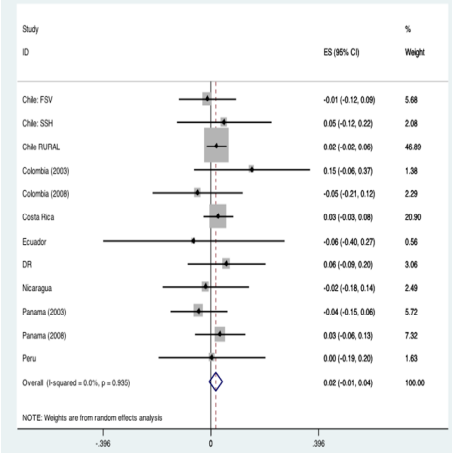
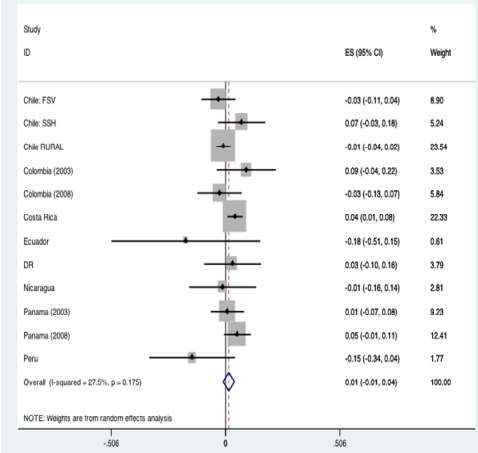
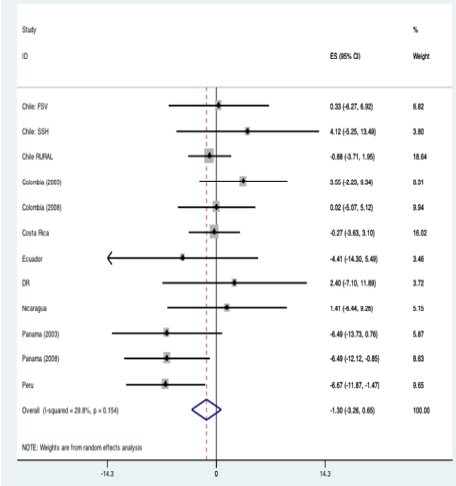
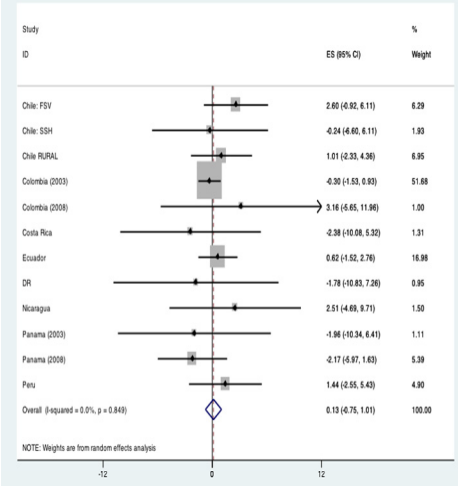

	

Chart Hours Worked Females 17 to 64 age	**Chart : Hours Worked Males 17 to 64 age**
	

Was there an education effect on children?

Chart A13School Attendance (6-14 male)	**Chart A14: School Attendance (6-14 female)**

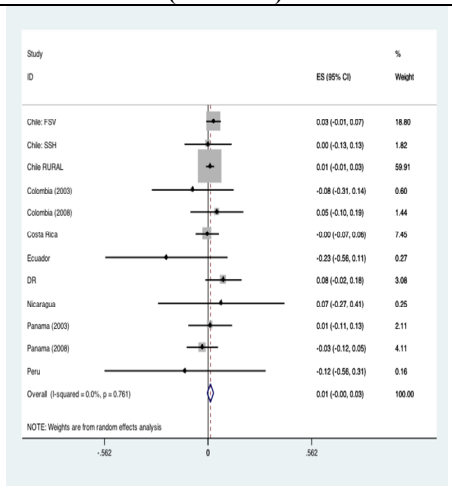
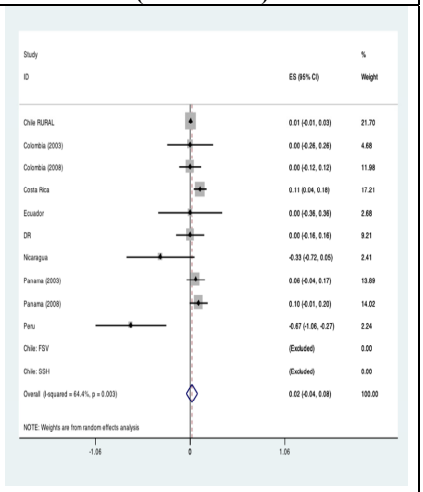
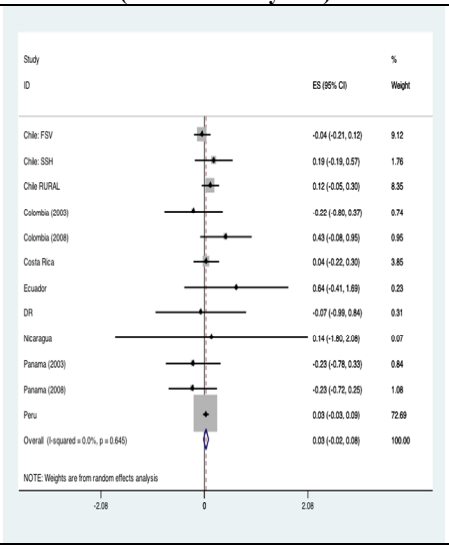
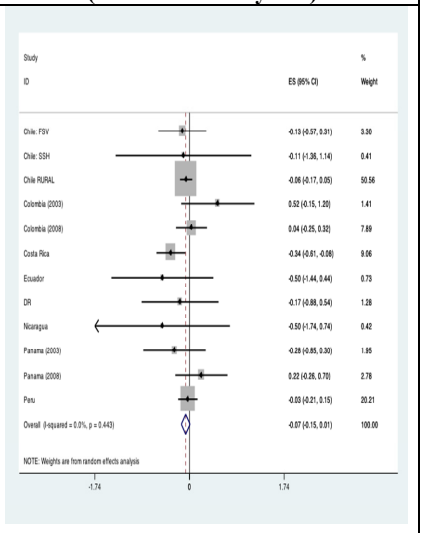

	

Chart A15 Education Lag (male 10 to 14 years)	**Chart A16: Education Lag (female 10 to 14 years)**
	

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