

No. 53

Mayo 2018

ISSN 2215 – 7816 (En línea)

Documentos de Trabajo

Escuela de Gobierno Alberto Lleras Camargo

**Does the form of delivering incentives in
conditional cash transfers matter over a
decade later?**

Andrés Ham y Hope C. Michelson

Serie Documentos de Trabajo 2018

Edición No. 53

ISSN 2215 – 7816

Edición digital

Mayo de 2018

© 2018 Universidad de los Andes - Escuela de Gobierno Alberto Lleras Camargo

Carrera 1 No. 19 -27, Bloque AU

Bogotá, D.C., Colombia

Teléfonos: 3394949, ext. 2073

escueladegobierno@uniandes.edu.co

<http://egob.uniandes.edu.co>

Director Escuela de Gobierno Alberto Lleras Camargo

Eduardo Pizano de Narváez

Autores

Andrés Ham y Hope C. Michelson

Jefe de Mercadeo y Comunicaciones, Escuela de Gobierno Alberto Lleras Camargo

Camilo Andrés Torres Gutiérrez

Gestora Editorial, Escuela de Gobierno Alberto Lleras Camargo

Angélica María Cantor Ortiz

Gestor de Comunicaciones, Escuela de Gobierno Alberto Lleras Camargo

Camilo Andrés Ayala Monje

El contenido de la presente publicación se encuentra protegido por las normas internacionales y nacionales vigentes sobre propiedad intelectual, por tanto su utilización, reproducción, comunicación pública, transformación, distribución, alquiler, préstamo público e importación, total o parcial, en todo o en parte, en formato impreso, digital o en cualquier formato conocido o por conocer, se encuentran prohibidos, y solo serán lícitos en la medida en que cuente con la autorización previa y expresa por escrito del autor o titular. Las limitaciones y excepciones al Derecho de Autor solo serán aplicables en la medida en se den dentro de los denominados Usos Honrados (Fair Use); estén previa y expresamente establecidas; no causen un grave e injustificado perjuicio a los intereses legítimos del autor o titular; y no atenten contra la normal explotación de la obra.

Does the form of delivering incentives in conditional cash transfers matter over a decade later?*

Andrés Ham[†]

Hope C. Michelson[‡]

Abstract

We study whether Honduran children exposed to a conditional cash transfer program from 2000-2005 experience lasting effects on human capital and labor market outcomes in early adulthood. The government randomly assigned three forms of delivering program benefits across targeted municipalities: demand (vouchers), supply (clinic and school subsidies), and a combination of both. This program provides an opportunity to explore if and how differential exposure to incentives produces longer term effects. Using municipal-level panel data, these effects are estimated using difference-in-differences. We find that the form of delivering cash transfers influences the degree to which these programs make progress towards their objective of reducing future poverty. Compared to municipalities receiving support from the Honduran Poverty Reduction Strategy, our study indicates that exposure to demand-side incentives individually has no lasting impact. However, joint exposure to both demand- and supply-side incentives does lead to measurable improvements in schooling and labor market participation.

Keywords: conditional cash transfers, long-term effects, demand- and supply-side incentives, human capital, labor markets.

JEL Classification: O12, I25, I28, I38, J20.

Resumen

Este trabajo investiga si niños que recibieron transferencias monetarias condicionadas durante la primaria en Honduras muestran un mejor desempeño educativo y laboral en su juventud. El Gobierno asignó tres formas de entregar las transferencias: incentivos a la demanda (*vouchers*), incentivos a la oferta (subsídios a centros de salud y escuelas) y ambas juntas. Este programa permite explorar la efectividad de distintos mecanismos para la entrega de subsidios una década después del comienzo del programa. Utilizando datos longitudinales a nivel municipal, estimamos los efectos del programa por el método de diferencias en diferencias. Encontramos que la forma de entregar las transferencias condicionadas afecta la efectividad de estos programas para lograr su objetivo de reducir la pobreza intergeneracional. En comparación con municipios que recibieron programas de la Estrategia para la Reducción de la Pobreza, nuestro trabajo indica que recibir incentivos adicionales a la demanda no tiene impacto duradero. Sin embargo, recibir ambos incentivos juntos –demanda y oferta– genera mejorías significativas en los resultados educativos y laborales más de una década después del inicio del programa.

Palabras clave: transferencias monetarias condicionadas, efectos de largo plazo, incentivos a la demanda y oferta, capital humano, mercados laborales.

Códigos JEL: O12, I25, I28, I38, J20.

*We are grateful for comments from Leonardo Bonilla, Sandra García, Patrick McEwan, and Soomin Lee, as well as seminar participants at Universidad de los Andes. We also thank two anonymous referees that provided insightful comments and suggestions, which have greatly improved this study. All remaining errors and omissions in this work are entirely our own and do not necessarily reflect the views of Universidad de los Andes or the University of Illinois. A revised draft of this article was published at the *Journal of Development Economics*. Please cite as: Ham, A. and Michelson, H.C. (2018). Does the form of delivering incentives in conditional cash transfers matter over a decade later?, *Journal of Development Economics*, Volume 134, September 2018, Pages 96-108, ISSN 0304-3878, DOI: <https://doi.org/10.1016/j.jdeveco.2018.05.007>.

[†]School of Government, Universidad de los Andes. a.ham@uniandes.edu.co

[‡]Department of Agricultural and Consumer Economics, University of Illinois. hopecm@illinois.edu

Content

1	Introduction	3
2	The PRAF conditional cash transfer program	7
3	Data and Identification	13
3.1	Data	13
3.2	Identification Strategy	14
4	The long-term impact of exposure to PRAF incentives	18
5	Threats to identification and caveats	26
6	Conclusion	28
A	Supplementary Material	35

1 Introduction

Conditional cash transfers (CCTs) have been shown to produce positive short- and medium-term gains during their first ten years of implementation. Households and individuals experience higher school enrollment rates, better health outcomes, increased household consumption, and lower poverty rates.¹ Evidence of CCT success in developing countries has supported a wave of additional implementation (Stampini and Tornarolli, 2012). These programs include both short- and long-run objectives in their design. Short-term objectives include alleviation of household poverty through cash transfers as well as health and nutrition improvements among children. Longer-term objectives are related to reducing the intergenerational persistence of poverty by fostering human capital accumulation that leads to greater labor market participation and higher earnings.

Quantifying and evaluating longer-term effects of CCT programs beyond their first decade is important for at least two reasons: first, to determine whether such programs do in fact impact intergenerational transmission of poverty; and second, to identify program components and incentives that are most effective in facilitating upward social mobility for beneficiaries. Consequently, research has begun to focus on these longer-term impacts. Molina-Millán et al. (2016) review estimates of the long-term impacts for programs in Colombia, Nicaragua and Mexico;² Parker and Vogl (2018) also provide evidence for Mexico; Araujo et al. (2017) study a similar program in Ecuador. These studies show general agreement that CCTs generate higher levels of human capital. Their impact on other domains, including cognitive skills, soft skills, fertility, labor market outcomes, and poverty, remains unclear, and estimated effects differ from program to program.

How transfers are delivered helps determine their success. Previous research has analyzed if conditionalities should be required (Baird et al., 2011), who should receive the transfers (Akresh et al., 2016), and timing of payments (Barrera-Osorio et al., 2011). CCTs are often provided either as a demand-side voucher to poor households, intended to motivate investments in education, health, and nutrition; or as supply-side subsidies to educational and health centers, which aim to enable

¹For broad reviews of CCT impacts over their first decade, see Rawlings and Rubio (2005), Barrientos and DeJong (2006), Fiszbein and Schady (2009), Ganimian and Murnane (2016), and García and Saavedra (2017).

²Their review includes studies for *Progres-Oportunidades* in Mexico (Behrman et al., 2011; Parker et al., 2012); *Red de Protección Social* in Nicaragua (Barham et al., 2017; Molina-Millán and Macours, 2017); as well as *Familias en Acción* (Báez and Camacho, 2011; García et al., 2012) and *Subsidios* (Barrera-Osorio et al., 2017) in Colombia.

these providers to facilitate access to their services and improve quality. Although rare, some studies demonstrate the importance of supply-side CCT support in the short-run. Behrman et al. (2011) study the variation in Progresa educational outcomes as a function of pre-program school availability and quality (using student-teacher ratios) and find that outcomes differ conditional on baseline schooling quality. Maluccio et al. (2010) find that Nicaragua's Red de Protección Social was more effective on student grade progression in areas with the poorest conditions at the program's start (proxied by grade availability and distance to schools), and which experienced improved school supply conditions due to the program. However, many of the longest-running programs did not randomly assign different incentive delivery schemes, so there is almost no evidence on whether demand- or supply-side incentives in CCTs are more effective in the long run.

This paper contributes to the literature on long-term effects of CCT programs by studying the Honduran Family Allowances Program (*Programa de Asignación Familiar* or PRAF in Spanish). Designed to implement and test multiple initiatives relative to an untreated group, this program began in 2000 and continued through 2005 in 70 of the poorest municipalities in Honduras. The interventions consisted of demand-side incentives for health and education (vouchers), supply-side incentives for clinics and schools to improve the quality of their services (cash subsidies), and a combination of demand- and supply-side support. This design allows us to isolate the effects of each incentive as well as their impact when delivered together, offering a window into whether the form of delivering CCTs influences long-term effects on schooling and labor market outcomes.

Over the arc of its history, however, PRAF faced administrative and political difficulties. Supply-side support was delayed by two years because legal barriers withheld cash subsidies for clinics and schools (Moore, 2008). This issue was resolved and supply-side incentives were paid out from 2002-2005. In addition, all targeted municipalities received development assistance from the newly-elected government's flagship Poverty Reduction Strategy (PRS) midway through PRAF (from 2002 onward), contaminating the intended randomization (IDB, 2006). The PRS included a variety of demand- and supply-side interventions that did not differentially benefit either treated or untreated PRAF municipalities (Government of Honduras, 2001). Due to these difficulties, previous work on PRAF has focused on its immediate effects during the first two years, which only captures the impact

of demand-side incentives. These studies have found positive impacts of the CCT on school enrollment, dropout rates, child labor, antenatal care coverage, and nutritional check-ups (Glewwe and Olinto, 2004; Morris et al., 2004; Galiani and McEwan, 2013).

Despite PRS contamination, we argue that exploiting differences in PRAF roll-out over time provides an opportunity to study an unanswered question in the CCT literature: does the form of delivering incentives influence long-term outcomes? While we cannot identify the absolute impact of PRAF because the original untreated group was contaminated, we are able to estimate the differential effects of additional CCT exposure. Moreover, we can also explore whether individual or combined incentive delivery leads to observable differences in the program's long-term effects.

We construct municipal-level panel data from Honduran population censuses in 2001 and 2013 using publicly-available information from the National Statistics Institute. Since PRAF did not contemplate an exit strategy upon program completion, untreated communities were never phased in to receive the incentives granted by the program (IDB, 2012). Program expense reports also indicate there was essentially no intervention in treated supply-side only municipalities. Therefore, we pool untreated and supply-side only communities into a combined comparison group, the technique employed by the short-term PRAF evaluation literature. These municipalities received three years of PRS assistance but no direct support from the CCT program. We employ a difference-in-differences strategy that controls for time-varying demographic variables, household characteristics, assets, adult outcomes, leakage from subsequent CCT programs, municipality and time fixed effects.

Our empirical strategy allows us to identify the differential effects of additional exposure to the following PRAF incentives: two years of demand-side incentives only (Group 1, hereafter G1) and two years of demand- plus three years of supply-side incentives (Group 2, hereafter G2). Our interest lies in estimating PRAF's long-term impact on young adults (aged 18-24) who were exposed to the program as primary school-age children (aged 6-12) in treated communities.

Results show that relative to the comparison group, additional exposure to demand-side incentives (G1) individually has no lasting impact. However, combined exposure to both demand and supply support (G2) leads to significant gains in human capital but no effect on labor outcomes. We estimate that years of schooling increase by 8.5% and that the share of young adults with secondary studies

rises by 38.2%. These effects are higher than the estimates in Araujo et al. (2017), but lie below most other estimates of long-run impacts of CCTs (Molina-Millán et al., 2016; Parker and Vogl, 2018), and are likely smaller due to the relatively small transfer amounts paid in PRAF (about 4% of pre-program monthly consumption) in comparison with other CCT programs.

In addition to testing whether coefficients on G1 and G2 communities are significantly different from comparison municipalities, we test whether they are equal to each other. Our results suggest that joint provision of demand- and supply-side incentives outperforms exposure to individual demand-side support more than a decade after PRAF's initial implementation.

We also find suggestive but inconclusive evidence of gender differences in demand plus supply-side municipalities. Young women complete 7.2% more years of schooling, 36.7% more girls attend secondary school, labor force participation increases by 24.5% and employment rates rise by 24.8%. Young men accumulate more schooling (9.3%) and high school studies (39.1%), but show no significant change in their labor market situation. Further analysis of these gender differences indicates that estimated effects for young women and men are not statistically different from each other. A potential explanation for the slightly larger coefficients on young women may be attributable to the context in PRAF municipalities: rural areas where agriculture is the main economic activity. Previous research has shown that the returns to educational investment in "brawn-based" economies differ by gender (Pitt et al., 2012). In such scenarios, girls often benefit more due to their specialization in skill-intensive activities that value education, while boys work in physically-intensive jobs that do not reward higher schooling. However, further research is needed to identify whether this channel drives our results and previous findings that girls benefit from CCT programs more than boys (Araujo et al., 2017; Baird et al., 2016; Parker and Vogl, 2018).

Findings are robust to alternative econometric specifications, identification strategies, and adjustments for multiple hypothesis testing. We also rule out several potential confounders. Despite using aggregate data, our empirical strategy has sufficient statistical power to detect small effects on the selected outcomes. Differential migration of individuals in treated and comparison municipalities may bias our estimates, but we show that there is no evidence that young adults in communities that received transfers were more likely to migrate than those in comparison municipalities. Our analysis,

however, has some limitations. Given data restrictions, we cannot isolate beneficiaries. Our estimates therefore constitute lower bounds of the true long-term impact of PRAF on individuals exposed to its incentives as children, which does not affect our conclusions about the effectiveness of separate and joint provision of conditional cash transfer incentives.

Our results contribute to an important question in the development literature: does the type of incentive delivered by social programs matter in the long run? Previous work analyzing demand-side cash transfer programs tends to find long-term human capital gains but no impact on labor outcomes (Molina-Millán et al., 2016; Araujo et al., 2017), while those that study interventions where demand- and supply-side incentives are coupled together do estimate positive gains (Barham et al., 2017). We provide an explanation for this mixed evidence within the same program: the form in which transfers are delivered matters. These findings emphasize the importance of considering the supply-side in CCT design and evaluation since separate provision of demand and supply incentives seems to be less effective than joint provision. One interpretation of our results is that individuals in municipalities that received combined demand- and supply-side incentives had higher returns due to improved health and education quality, implying some level of complementarity between both types of incentives that enhances the extent to which the poor benefit from CCTs.

The remainder of this paper is organized as follows. Section 2 describes the Family Allowances program. Section 3 introduces the data and presents our identification strategy. Section 4 reports estimates of the long-term impact of exposure to PRAF on human capital and labor market outcomes, and whether effects differ by incentive type. We also use this section to test the robustness of our results. Threats to identification and caveats are discussed in Section 5. Section 6 concludes.

2 The PRAF conditional cash transfer program

The Honduran Family Allowances Program (PRAF, for its acronym in Spanish) was created in 1990. Its goal was to compensate extremely poor Hondurans for the negative impact of the country's structural adjustment policies,³ providing educational and health benefits through subsidies without enforcing

³Poverty in Honduras is measured as income deprivation, which yields two classifications of poverty: extreme and moderate. The former includes households whose per capita income is insufficient to afford a basic food basket, while the latter identifies families who are able to purchase food but cannot cover additional expenses (e.g. housing, education, health, transport, etc.). Individuals are classified as extremely poor if their household per capita income falls below the basic poverty line and moderately poor if their family's per capita income lies below the moderate threshold. See Sobrado and Clavijo (2008) for a thorough explanation of official poverty measurement in Honduras.

co-responsibilities (Moore, 2008). While initially devised as a temporary safety net, the government later changed the program status to run indefinitely. PRAF changed in many ways over time, but consistently aimed to improve the well-being of the poor.⁴

Between 2000 and 2005, the Inter-American Development Bank (IDB) provided a \$50 million loan to fund a phase II expansion of PRAF and offer technical support to the Honduran government to evaluate the program's performance (1026/SF-HO). Phase II was designed as a program distinct from the original PRAF, with separate beginning and end points, and prioritizing a different set of goals. While the government created PRAF phase I to compensate poor households for the adverse impact of structural adjustment policies, phase II was inspired by Mexico's *Progresa* program (IDB, 2006) and aimed to alleviate current poverty by providing transfers to poor households and to increase human capital accumulation for children in these families to break the cycle of poverty. Phase II was thus conceived as a CCT, with demand- and supply-side incentives to promote health, nutrition, and education, a focus on rural areas with the weakest infrastructure, and an experimental evaluation design with randomly assigned transfers at the municipality level (IFPRI, 1999 2000). For simplicity, we refer to phase II of this program throughout the paper as PRAF.

PRAF provided demand-side incentives for nutrition and health, as well as education. The nutrition and health component gave eligible households (those with pregnant or nursing mothers and infants aged 0-3) a voucher of approximately \$48 per person per year for at most two children per household (\$96 maximum). Educational support for eligible families (extremely poor households, whose children aged 6-12 had not yet completed fourth grade) took the form of a voucher worth \$38 per year per child for up to three children per household (\$114 maximum). Families had to fulfill certain conditions in order to receive the transfers. For the health voucher, mothers were required to attend five pre-natal medical check-ups, as well as one post-natal check-up to monitor the newborn's development. Infants were also required to visit their local health center for periodic nutritional check-ups. To receive educational vouchers, children aged 6-12 had to be enrolled in school, miss no more than 20 days of instruction, and not be held back in the same grade more than once. However, there is evidence that school and health center attendance conditionalities were weakly enforced in practice (Galiani and

⁴See Moore (2008) for a comprehensive description of PRAF's history.

McEwan, 2013; Benedetti et al., 2016).

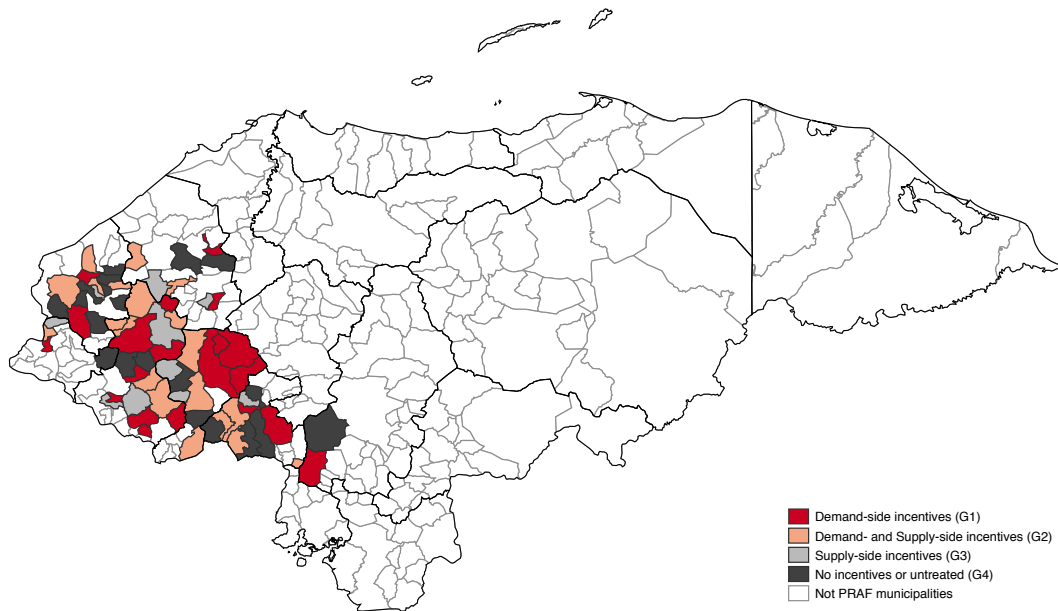
Supply-side incentives were a fundamental part of PRAF's design. The nutrition and health component included an annual health quality incentive of \$5,000-6,000, conditional on local medical centers providing better quality services according to standards outlined by the Honduran Secretary of Health and active participation in the process by clinic managers. In addition, the Comprehensive Attention to Children in the Community program (*Atención Integral a la Niñez en la Comunidad* or AIN-C in Spanish) was created to teach mothers how to monitor child health.⁵ The educational component provided legally authorized parent-teacher organizations with about \$4,000 annually to fund learning development initiatives in schools. These organizations received funds through NGO intermediaries in order to benefit local schools by improving their facilities, purchasing equipment or educational materials, and/or providing training for teachers. The Continuing Training Program (*Programa de Formación Continua* or PFC in Spanish) was also created to provide technical education for teachers. Instruction lasted two years and took place on weekends, with successful completion guaranteeing teachers an improved two-year contract in their schools.

To evaluate the effect of each type of incentive, the IDB design set up three treatment groups: a) demand-side group (G1); b) demand- and supply-side group (G2); and c) supply-side group (G3). Each arm was to be compared to G4: a group that received neither component (individually or combined). Honduras is divided into 298 municipalities; the program targeted municipalities with mean height-for-age z-scores (HAZ) below -2.304 in the 1997 Height Census of First-Graders (IFPRI, 2000).⁶ The 70 municipalities that met this criterion were located in western Honduras, the poorest region in the country. Figure 1 presents the location of the 70 selected municipalities. Municipalities were further classified into five quintiles or blocks of average HAZ scores comprising 14 communities each, from poorest (block 1) to "richest" or least poor (block 5). Treatment and comparison groups were selected by stratified randomization in a public event held in 1999. Ten municipalities belonging to each of the five blocks were randomly chosen to receive PRAF benefits, leaving 20 communities in G1, 20 in G2, 10 in G3, and 20 in the untreated G4 group.

⁵Mothers were trained by NGOs to measure height and weight, identify when children were sick and refer them to health clinics, treat dehydration, improve living conditions, and train local mothers in the same skills (Moore, 2008).

⁶The government excluded three municipalities due to practical considerations related to remoteness and cost of implementation. See IFPRI (1999 2000) and Galiani and McEwan (2013) for details.

Figure 1. Geographic location of PRAF municipalities



Source: Own elaboration from PRAF randomization data.

Administrative and political issues affected PRAF implementation in practice. On the one hand, supply-side incentives in education were not implemented during the first two years (2000-2001) due to legal barriers that restricted NGOs and parent-teacher organizations from receiving and managing public funds (Moore, 2008). Delivery of supply-side support in health was also limited, with only 17% of funds disbursed in the first two years (Galiani and McEwan, 2013). Therefore, most short-term studies of PRAF combine G1 and G2 into a single treated group and include G3 as part of the comparison group to evaluate the effect of PRAF's demand-side incentives. Using this definition, Morris et al. (2004) report a large impact of the CCT on antenatal care and health check-ups using primary survey data, but find mixed evidence on immunization rates. Using the same survey, Glewwe and Olinto (2004) find that children in households who received demand-side incentives had higher school enrollment, attendance, and lower dropout rates relative to individuals in comparison communities. Galiani and McEwan (2013) used secondary census data and found large enrollment increases and declines in child labor inside and outside the home. Moreover, they estimated heterogeneous effects by blocks, finding evidence that PRAF mainly benefitted the poorest municipalities (blocks 1 and 2). Ham (2014) confirms that vulnerable children had greater access to

education than more-advantaged groups due to the transfers.⁷

On the other hand, PRAF implementation was affected by the newly-elected administration's flagship development program: the Poverty Reduction Strategy (PRS). Many communities across Honduras benefited from new social programs under the PRS, including PRAF municipalities. The PRS assigned \$1.8 billion to a variety of demand- and supply-side interventions in six broad areas.⁸ Approximately \$224 million was allocated to increase access to better quality health services, provide a basic packet of health services, promote children's health and nutrition, create and maintain healthy schools, and improve health infrastructure. Nearly \$600 million was assigned for educational investments: expanding school coverage, providing scholarships and free enrollment, training teachers, and improving school infrastructure. Program documents show no indication that PRS programs differentially benefited either treated or untreated PRAF municipalities (Government of Honduras, 2001; IDB, 2006). Implementation of the PRS does complicate identifying the absolute impact of PRAF treatments because the original untreated group (no intervention) was contaminated. We consider the possible implications of PRS spillovers in our empirical analysis.

Table 1 summarizes how PRAF implementation occurred in practice. Starting in 2002, untreated G4 communities received support from the PRS. At the same time, the outstanding legal issues with NGOs and parent-teacher organizations were resolved, so supply-side incentives were paid out (IDB, 2006). The program also experienced a 50% budget cut in its final year, meaning that final payments were cut in half. By the end of the program in 2005, G4 municipalities had received about three years of PRS support. The remaining (treated) groups received an *additional*: two years of demand-side support (G1), two years of demand and three years of supply-side support (G2), and three years of supply-side support (G3). Table 1 also shows that PRAF's actual costs differed with respect to its budget. There was essentially no supply-side intervention in G3 municipalities, since only 12.4% of allocated funds were disbursed. Moreover, expenditures in G1 (demand-only) and G2 (demand- and supply-side) municipalities differed from the initial plan. Funds for G1 communities were mostly executed (94.5%), while expenditures in G2 municipalities increased (123.7%). This difference is mainly due to changes

⁷Other studies have assessed the effects of PRAF's demand-side incentives on adult fertility (Stecklov et al., 2007), adult labor supply (Alzúa et al., 2013), and voting behavior (Linos, 2013).

⁸The six components of the PRS were: accelerating equitable and sustainable economic growth, reducing poverty in rural areas, reducing urban poverty, investing in human capital, strengthening social protection for specific groups, and guaranteeing the sustainability of the strategy (Government of Honduras, 2001).

in the education component. Disbursed funds for educational incentives in G1 and G2 municipalities were roughly similar in total, about \$10 million. However, while all funds were spent on demand-side incentives (vouchers) in G1 communities, the distribution was 80% for demand and 20% on supply (cash subsidies) in G2 municipalities. Therefore, both G1 and G2 spent similar amounts of money but distributed differently, providing an opportunity to test if such allocation choices had a lasting impact.

Table 1. PRAF implementation from 2000-2005

Group	Incentives		Health and nutrition		Education	
	2000-2001	2002-2005	Budget (US\$)	Actual cost (US\$)	Budget (US\$)	Actual cost (US\$)
G1	Demand	PRS + Demand	7,617,000	8,863,000	12,615,000	10,247,000
G2	Demand	PRS + Demand and Supply	6,791,000	6,971,000	6,810,000	9,855,000
G3	No intervention	PRS + Supply	1,331,000	41,000	1,200,000	272,000
G4	No intervention	PRS	—	n.a.	—	n.a.

Source: Own elaboration from the IDB final program report (IDB, 2006).

Notes: PRS-Poverty Reduction Strategy. n.a.-Not available. These calculations include basic health and education transfers for G1 and G2, and exclude administrative costs, expenditure on materials, consultancies, and unforeseen expenses for all groups.

PRAF did not have a well-defined exit strategy, so G4 municipalities were never phased in to receive the incentives granted by the CCT (IDB, 2012). At the end of 2005, PRAF was terminated. The IDB continued to cooperate with the Honduran government under a different initiative: the Integral Social Protection Program (*Programa Integral de Protección Social* or PIPS in Spanish), which targeted villages across Honduras instead of municipalities. The PIPS program included some overlap with PRAF areas; 28% of beneficiary households in 14 PRAF G1 and G2 municipalities were selected for PIPS.⁹ Households in G3 and G4 municipalities did not receive PIPS. In 2011, the *Bono 10 Mil* CCT replaced PIPS. This program was implemented nationwide, with 16% of the villages in *Bono 10 Mil* located within PRAF municipalities (Benedetti et al., 2016).

The remainder of this paper explores whether PRAF achieved its objective of promoting human capital accumulation and greater labor market participation to reduce persistent poverty. We exploit the original experimental assignment, combining G3 and G4 communities as our comparison group, the same group employed by the studies of PRAF’s short-term impacts. While using the original randomization provides differential effects of program exposure instead of an absolute “no program” comparison, we show that outcomes in the combined G3 and G4 comparison municipalities are

⁹These municipalities are: Masaguara, San Francisco de Opalaca, San Isidro, and Yamaranguila in the department of Intibucá; Cabañas and Yarula in the department of La Paz; Belén, Erandique, Gualcinse, Lepaera, San Francisco, Santa Cruz, and Tomalá in the department of Lempira; and Protección in the department of Santa Bárbara (IDB, 2012).

similar to non-PRAF municipalities, who also benefited from Poverty Reduction Strategy support. Moreover, a unique feature of program roll-out is that it allows testing if and how additional exposure to demand-side incentives or a combination of demand- and supply-side incentives impacts long-term human capital and labor market outcomes. Little evidence exists on these long-term effects for CCT programs, and this represents the main contribution of our study.

3 Data and Identification

3.1 Data

We construct longitudinal data using publicly-available information on the National Statistics Institute's website (www.ine.gob.hn). The “*Baseine en Línea*” provides open access to municipal-level statistics from the 2001 and 2013 population censuses in Honduras.¹⁰ We assemble the data from the 70 PRAF communities into a panel with two time periods covering a span of 12 years.

For each census year, we calculate municipal-level means for selected educational and labor market outcomes.¹¹ These include years of schooling, the fraction of individuals with at least some secondary schooling, labor force participation, employment rates, and the share of workers employed in non-farm jobs. We also calculate demographic, household, dwelling, and municipality-level variables to include as controls, which are described in the next sub-section.

We complement the census statistics with PRAF randomization data made available by Galiani and McEwan (2013).¹² Since these authors had a cross-section of microdata to evaluate the short-term effects of PRAF, they were able to identify eligible beneficiaries (children aged 6-12 who had not completed fourth grade). Our use of aggregate data requires that we pool eligible and ineligible beneficiaries together, likely attenuating our estimates. Reported findings can thus be interpreted as lower bounds of the long-term effects of exposure to PRAF during primary school age.

The data show that PRAF communities are among the poorest in Honduras (see Table A.1 in the Supplementary Material), consistent with previous studies. PRAF municipalities have low levels

¹⁰The *Baseine en Línea* also contains information for the 1988 population census. However, we do not use this data due to large differences in methodology and missing information on several relevant outcomes and controls.

¹¹We restrict the number of outcomes to those commonly evaluated in the long-run CCT literature (Molina-Millán et al., 2016). Unfortunately, the National Statistics Institute does not ask questions about income or consumption in the census, so we cannot measure how exposure to PRAF affects these and other outcomes of potential interest.

¹²This data and the replication files for their study are available at <http://www.patrickmcewan.net/>.

of human capital (on average less than primary schooling), contain the majority of the indigenous *lenca* ethnic group population, and are predominantly rural. Most households have limited access to basic services (electricity, water, and sewage), few assets, and large family sizes. Over the period of study, average conditions in these municipalities have improved. How much of this improvement can be attributed to PRAF is one of the questions we attempt to answer in this paper.

While using aggregate-level data for this purpose is less ideal than microdata, we argue that the data are still informative. An alternative analytical approach is to use repeated cross-sections of household survey data as in Rackstraw (2014) or Demographic and Health Surveys as in Li (2016). However, these survey instruments are not designed to be representative at the municipal-level, only by aggregate dominions.¹³ In many survey rounds, PRAF municipalities are unobserved, so drawing inferences from such data will likely introduce problems related to representativeness, insufficient statistical power, and missing information.¹⁴

One potential limitation of relying on the aggregated data stems from problems of low statistical power. As we discuss in Section 5, our empirical strategy provides estimates that lie within the range of calculated minimum detectable effects, suggesting that power is not a problem for the analysis. Our research does leave the door open to future work using microdata to complement the reported findings, perhaps exploring potential heterogeneity in PRAF's long-run impact by individual, household, or municipality-level characteristics.

3.2 Identification Strategy

Our identification strategy exploits the fact that children in some municipalities experienced differential exposure to PRAF incentives to test whether some forms of support have more lasting effects than others. Given the evidence in Table 1 that treatment in G3 communities was not implemented, we pool G3 and G4 communities into a single comparison group, which received three years of support from the Poverty Reduction Strategy but virtually no direct support from PRAF. This empirical decision provides an opportunity to compare our results with the short-run findings in Galiani and McEwan

¹³The National Statistics Institute uses five (5) dominions in Honduras: Tegucigalpa, San Pedro Sula, intermediate cities, small cities, and rural areas. These regions are more aggregated than departments (18) and municipalities (298).

¹⁴Census microdata could similarly improve our analysis. Unfortunately, requests at the National Statistics Institute to access individual-level census records have been unsuccessful despite multiple efforts.

(2013). The differential exposure of treated municipalities relative to this pooled comparison group is an *additional*: two years of demand-side incentives for G1, and two years of demand-side and three years of supply-side support for G2. We can thus identify whether receiving additional demand incentives individually or combining demand and supply supports leads to different long-term effects, which remains an unanswered question in the CCT literature.

Children shown to have experienced higher school enrollment and lower rates of child labor due to PRAF in 2001 (when they were 6-12) are young adults in 2013 (aged 18-24). Our identification strategy compares municipal-level means for individuals aged 18-24 in 2013 relative to the same group's baseline means in 2001. We select this age group for our main analysis because these individuals have the highest probability of exposure to the full length of the PRAF CCT program.

However, using the age group that benefited from PRAF during primary school excludes most recipients of the nutrition and health vouchers. We believe this is a justifiable decision for two reasons. On the one hand, the possible contamination of human capital and labor market outcomes from more recent social programs is higher for infants targeted by these incentives (aged 0-3 in 2001). Furthermore, their outcomes are less likely to have been determined completely (Araujo et al., 2017; Molina-Millán et al., 2016). Finally, Honduran census data provides limited information on nutrition and health variables to credibly assess longer-term impact on these outcomes. Long-term effects on these beneficiaries and outcomes thus remains an area for future research.

Given that we have panel data at the municipal-level, we employ a difference-in-differences strategy that controls for time-invariant and time-varying community factors, and uses within-municipality variation over time to identify the long-run effects of exposure to PRAF incentives:

$$y_{it} = \alpha + \beta_1 G1_{it} + \beta_2 G2_{it} + \gamma X_{it} + \lambda_i + \delta_t + \mu_{bt} + u_{it} \quad (1)$$

where y_{it} is the outcome for municipality i at time t . The differential long-term effects of PRAF relative to the comparison group (G3 and G4) are the coefficients on an interaction between a binary indicator for the group the district was assigned to and a dummy variable for 2013. Specifically, β_1 captures the differential effect of exposure to demand-side incentives only (G1), while β_2 estimates the differential impact of exposure to combined demand- and supply-side incentives (G2).

Given that over a decade has passed between censuses we include time-varying controls at the municipality-level in X_{it} , chosen similarly to those employed by Galiani and McEwan (2013): the share of female population, average age in the municipality and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group population, share of urban population, an index of access to basic services (simple average of households with electricity, water, and sewage), an asset index (simple average of car, refrigerator, computer, and television ownership), average household size and its square, the share of internal migrants (who lived in another municipality five years prior), the share of households with at least one member who has emigrated from Honduras, and the municipality's population density. We also control for relative schooling and labor supply for adult females relative to males (aged 18-65 in 2001, hence 30-77 in 2013). Previous research has highlighted that human capital and labor market analyses should account for persistence across generations (Lindahl et al., 2015), especially in rural contexts where gender gaps are significant (Pitt et al., 2012). Moreover, prior studies for PRAF find no indication that adult schooling and labor market outcomes were affected in the short-term (Alzúa et al., 2013; Galiani and McEwan, 2013). Equation (1) also includes municipality fixed effects (λ_i) and a secular time trend (δ_t).

To account for overlap with more recent CCT programs we include two additional controls. Because PIPS targeted children aged 6-18 from 2006-2009, we include a binary variable that identifies municipalities whose villages received this program using information from IDB (2012). A second program, *Bono 10 Mil*, began in 2011, when PRAF beneficiaries were aged 16-22. This program also targeted 6-18 year olds, so contamination is less troublesome compared to PIPS for our cohort of interest. Regardless, we include block-specific time trends (μ_{bt} , where $i \in b = \{1, \dots, 5\}$) to control for potential leakage from *Bono 10 Mil* and also to account for the stratified randomization design in PRAF (Bruhn and McKenzie, 2009).¹⁵ This strategy helps control for spillovers due to subsequent CCT programs and other time-varying attributes at the block-level that may confound our estimates. Unfortunately, we cannot include municipality-specific time trends because this leaves no remaining within-municipality variation to identify the coefficients of interest.

¹⁵While we could potentially control for this contamination using a binary variable that identifies which communities received the *Bono 10 Mil* CCT, the results of the randomization are not publicly available.

Table 2. Descriptive statistics and baseline balance

	PRAF municipalities	Groups			p-value
		G3 and G4 Comparison	G1 Demand	G2 Demand and Supply	
<i>Outcomes for children 6-12</i>					
Enrollment	0.726	0.687	0.754	0.757	0.021
Works outside home	0.074	0.094	0.062	0.056	0.026
Works in home	0.088	0.105	0.081	0.069	0.012
<i>Outcomes for young adults 18-24</i>					
Years of schooling	3.777	3.699	3.914	3.758	0.656
At least some secondary studies	0.074	0.076	0.076	0.068	0.861
Labor force participation	0.523	0.533	0.503	0.527	0.209
Works outside home	0.518	0.529	0.498	0.522	0.186
Works in non-farm job	0.200	0.209	0.195	0.193	0.806
<i>Outcomes for young women 18-24</i>					
Years of schooling	3.920	3.811	4.102	3.902	0.450
At least some secondary studies	0.085	0.090	0.089	0.076	0.693
Labor force participation	0.131	0.143	0.118	0.127	0.466
Works outside home	0.130	0.141	0.117	0.125	0.471
Works in non-farm job	0.775	0.791	0.738	0.788	0.616
<i>Outcomes for young men 18-24</i>					
Years of schooling	3.653	3.601	3.751	3.633	0.838
At least some secondary studies	0.064	0.064	0.065	0.062	0.981
Labor force participation	0.891	0.901	0.870	0.898	0.363
Works outside home	0.884	0.895	0.861	0.890	0.311
Works in non-farm job	0.125	0.128	0.127	0.117	0.881
<i>Controls</i>					
Female	0.491	0.491	0.490	0.492	0.800
Age	21.508	21.734	21.278	21.398	0.396
Share of individuals born in municipality	0.881	0.895	0.889	0.853	0.437
Lenca	0.285	0.277	0.336	0.245	0.350
Share of urban population	0.065	0.079	0.056	0.055	0.801
Access to basic services (electricity, water, sewer)	0.368	0.362	0.381	0.364	0.752
Household assets (car, computer, refrigerator, TV)	0.049	0.053	0.047	0.045	0.758
Household size	5.414	5.325	5.528	5.434	0.093
Share of internal migrants	0.036	0.034	0.041	0.035	0.492
Share of international migrants	0.013	0.010	0.016	0.014	0.419
Population density	66.20	64.36	60.59	74.56	0.260
Relative schooling: adult females/males	0.870	0.861	0.874	0.878	0.935
Relative labor supply: adult females/males	0.151	0.155	0.129	0.165	0.418
Number of municipalities	70	30	20	20	

Source: Own calculations on municipal-level data from the 2001 Honduran population census.

Notes: The p-values in the final column are obtained by regressing each variable on two treatment group dummy variables and block fixed-effects with clustered standard errors by municipality, and test the hypothesis that coefficients for the G1 and G2 treatment groups are jointly zero.

Our empirical strategy assumes that young adult outcomes and controls at baseline (in 2001) are not significantly different between treatment and comparison municipalities. We believe this is a valid assumption given previous evidence using the same census data and our own estimates of baseline balance in Table 2. Consistent with Galiani and McEwan (2013), we find significant differences on school enrollment and labor outcomes in 2001 for children aged 6-12 in the aggregate data. However,

outcomes for young adults and the selected controls indicate few statistically relevant differences between treated municipalities relative to the G3 and G4 comparison group.

All regressions are estimated by OLS with clustered standard errors by municipality, which are subjected to a number of robustness checks. Given that we evaluate multiple outcomes using the same identification strategy, we make adjustments for multiple hypothesis testing to ensure that our findings are not driven by chance, following the procedures in Kling et al. (2007) and Anderson (2008). We also consider several threats to our identification assumptions in Section 5.

4 The long-term impact of exposure to PRAF incentives

Difference-in-difference estimates for the long-term effects of exposure to PRAF on young adults' human capital and labor market outcomes are shown in Table 3. We present results for the full sample in Panel A, and separately for young women and men in Panels B and C, respectively.¹⁶

We find evidence of greater human capital accumulation in municipalities exposed to additional incentives on the demand-side (G1) and for demand plus supply (G2), but differences with respect to the comparison group are only significant for the latter group. Years of schooling for young adults exposed to both types of incentives increased by 0.315 (s.e. 0.111), or 8.5%. Young adults in G2 municipalities with at least some secondary studies grew by 2.9 percentage points (s.e. 0.014), a gain of 38.2% relative to the comparison group mean of 7.6%. However, these educational gains are not accompanied by greater labor force participation or employment rates. Estimated effects on labor market outcomes in municipalities exposed only to demand-side incentives are close to zero and precisely estimated. While the coefficients on labor market outcomes for young adults in demand and supply communities are positive, most are statistically insignificant. The evidence does however, indicate that 19.6% more young adults were employed in non-agricultural jobs.

¹⁶We estimate the same specification using the original randomized treatment groups in Supplementary Table A.2. The main results discussed throughout this section remain unchanged. However, the analysis confirms that treatment in G3 communities was not effectively implemented since few estimated effects are statistically significant for this group.

Table 3. The long-run effects of exposure to PRAF incentives

	Years of schooling	At least some secondary studies	Labor force participation	Works outside home	Works in non-farm job
<i>A. Young adults</i>					
G1: +2 years of demand	0.102 (0.119)	0.016 (0.014)	-0.002 (0.017)	0.002 (0.018)	-0.004 (0.021)
G2: +2 years of demand and 3 years of supply	0.315 (0.111)***	0.029 (0.014)**	0.013 (0.017)	0.015 (0.017)	0.041 (0.019)**
p-value G1=G2	0.069	0.371	0.512	0.554	0.025
Adjusted R^2	0.980	0.942	0.576	0.577	0.702
Mean Comparison Group (G3+G4) in 2001	3.699	0.076	0.533	0.529	0.209
Observations	140	140	140	140	140
<i>B. Young women</i>					
G1: +2 years of demand	-0.026 (0.116)	0.009 (0.015)	0.010 (0.010)	0.009 (0.010)	0.075 (0.060)
G2: +2 years of demand and 3 years of supply	0.273 (0.119)**	0.033 (0.015)**	0.035 (0.012)***	0.035 (0.012)***	0.036 (0.044)
p-value G1=G2	0.012	0.142	0.010	0.010	0.491
Adjusted R^2	0.980	0.943	0.866	0.860	0.216
Mean Comparison Group (G3+G4) in 2001	3.811	0.090	0.143	0.141	0.791
Observations	140	140	140	140	140
<i>C. Young men</i>					
G1: +2 years of demand	0.210 (0.137)	0.023 (0.014)	-0.007 (0.026)	0.000 (0.026)	-0.015 (0.019)
G2: +2 years of demand and 3 years of supply	0.335 (0.129)**	0.025 (0.015)*	0.010 (0.024)	0.014 (0.025)	0.022 (0.016)
p-value G1=G2	0.377	0.896	0.614	0.686	0.037
Adjusted R^2	0.970	0.930	0.426	0.432	0.642
Mean Comparison Group (G3+G4) in 2001	3.601	0.064	0.901	0.895	0.128
Observations	140	140	140	140	140

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report Difference-in-Differences (DID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy (see Section 3 for details). All regressions control for time-varying municipal-level variables: the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Panels B and C reveal gender differences in PRAF's long-term impact across treatment arms. Both young women and men in communities that received only demand-side incentives from PRAF (G1) show few significant gains relative to the comparison group, with many coefficients not significant and close to zero. Girls' schooling does increase by 0.273 (s.e. 0.119) in G2 municipalities that received combined demand- and supply-side incentives, a gain of 7.2%. Relative to the comparison group, the fraction of young women who enrolled in secondary school grew by 36.7%. Increases in labor market

outcomes for girls are statistically and economically significant with respect to the comparison group. Young women's labor force participation grew by 24.5% and their employment rate increased 24.8%. However, there is no indication that significantly more young women were occupied in non-farm jobs. Young men exposed to PRAF as boys display a statistically significant increase in human capital in demand- and supply-side municipalities, with an increase in years of schooling of 9.3% and 39.1% more young men attending at least secondary school. However, young men's labor market outcomes are mostly unaffected in the long-run.

Because we estimate 15 regressions and test two hypotheses per equation ($\beta_1 = 0$ and $\beta_2 = 0$), we analyze whether these findings are robust to multiple hypothesis adjustments. On the one hand, we follow Kling et al. (2007) and reduce the number of comparisons to six outcomes by constructing a composite index of educational and labor market domains for young adults, young women, and young men. These indices are simple averages of our outcomes, which are standardized with respect to the comparison group mean and standard deviation at baseline.¹⁷ Results in Table A.3 confirm the patterns in Table 3. Additional exposure to PRAF leads to a statistically significant increase in the educational index for G2 but not G1 municipalities, and effects on the labor market domain are only significant for young women. On the other hand, we employ the methodology proposed by Anderson (2008) that accounts for the false discovery rate to each hypothesis in order to rule out chance findings.¹⁸ Table A.4 shows the resulting q-values for the estimated coefficients in Table 3. The conclusions support that exposure to PRAF had significant long-term effects on outcomes in G2 but not G1 communities relative to the comparison group. Moreover, the gains for young women in terms of labor market outcomes seem to outweigh the impact for young men.

Reported results are stable when using alternative specifications and identification strategies. Given that municipalities differ in terms of population size, we estimate regressions weighted by the share of young adults in the total population for each municipality-year cell (Supplementary Table A.5). While the weighted results provide similar conclusions, alleviating concerns of misspecification or

¹⁷The education domain is the simple average of years of schooling and the share of individuals with at least some secondary schooling. The labor market domain is the simple average of labor force participation, employment rate, and the share of individuals employed in non-farm jobs.

¹⁸Anderson's paper and code, which implements the method by Benjamini and Hochberg (1995) that provides adjusted q-values may be found at: <https://are.berkeley.edu/~mlanderson/ARE.Website/Research.html>.

endogenous sampling (Solon et al., 2015), weighted estimates are noisier.¹⁹ We also consider two triple difference approaches comparing results for young adults with respect to two groups: i) prime-age adults aged 25-29 (who would have been 13-17 in 2001 and thus ineligible for PRAF), and ii) adults aged 30-77 (18-65 in 2001 and unaffected by PRAF). The results in Supplementary Tables A.6 and A.7 are largely similar to the reported findings in Table 3, although triple difference estimates reduce statistical power because they include multiple interaction terms. This imprecision is higher when comparing young adults to prime-age adults aged 25-29, likely due to the latter group's inclusion in subsequent CCT programs and our inability to disentangle eligible and ineligible PRAF beneficiaries in our data, attenuating our estimates. Overall, the analysis suggests that our empirical results are robust. Combined exposure to demand- and supply-side CCT incentives has a larger impact than individual exposure to demand-side incentives relative to the comparison group that benefitted from PRS assistance but no direct support from PRAF.²⁰

Besides testing if the coefficients on G1 and G2 communities are significantly different from comparison municipalities, we question whether long-term impact differs between incentive types. The p-values for these hypothesis tests are shown in the first column of Table 4. For the full sample, we reject equality between G1 and G2 for years of education (p-value=0.069), but not secondary schooling. We find statistically relevant differences in non-farm employment (p-value=0.025), but no differences in impact for labor force participation or employment rates. The evidence is less ambiguous when observing the results by gender. Most outcomes are significantly different for young women in G2 relative to G1 communities, with the exceptions of secondary schooling (marginally) and non-farm employment. For young men, schooling and labor market differences are insignificant between G1 and G2, although impact on non-farm employment does differ between incentive types (p-value=0.037), despite the coefficient being indistinguishable from zero relative to comparison municipalities. Findings are similar but more imprecise in the aggregate domain analysis (Table A.3), weighted estimates (Table

¹⁹In unreported results, we also test for evidence of heteroscedasticity with respect to the within-municipality population size. The t-ratios for a Breusch-Pagan test are mostly below 2, which suggests that weighted estimation is not required to correct for this purpose (Solon et al., 2015). Weighted and unweighted coefficients are highly similar and we opt to use the former due to their higher level of precision.

²⁰As an additional exercise, we employ the synthetic control method (Abadie et al., 2010) to approximate if there is evidence of contamination in comparison communities since G3 communities were assigned to receive supply-side incentives, although treatment was not delivered (see Table 1). We assume that comparison municipalities are “treated” since they were targeted by PRAF and construct a “synthetic comparison” group using a convex combination of non-PRAF municipalities ($N=228$). Table A.8 in the Supplementary Material presents the results using aggregate outcome domains. We find no evidence that G3 and G4 comparison municipalities experienced significant outcome differences relative to non-PRAF municipalities who also benefitted from PRS assistance. Implementation of the synthetic control method was carried out using the `synth_runner` package developed by Quistorff and Galiani (2017). See https://github.com/bquistorff/synth_runner for full details on the procedure and the notes in Table A.8 for specifics.

A.5), and triple difference strategies (Tables A.6 and A.7).

Table 4. Hypothesis tests: Equal long-term effects on G1 and G2 municipalities

	Cluster-robust standard errors		Block-bootstrapped errors	Wild-bootstrapped standard errors
	p-value	q-value	p-value	p-value
<i>A. Young adults</i>				
Years of schooling	0.069	0.172	0.178	0.126
At least some secondary studies	0.371	0.628	0.527	0.751
Labor force participation	0.512	0.693	0.605	0.138
Works outside home	0.554	0.693	0.643	0.146
Works in non-farm job	0.025	0.095	0.097	0.171
<i>B. Young women</i>				
Years of schooling	0.012	0.060	0.063	0.095
At least some secondary studies	0.142	0.305	0.293	0.565
Labor force participation	0.010	0.060	0.069	0.114
Works outside home	0.010	0.060	0.069	0.110
Works in non-farm job	0.491	0.693	0.608	0.560
<i>C. Young men</i>				
Years of schooling	0.069	0.172	0.509	0.196
At least some secondary studies	0.371	0.628	0.928	0.996
Labor force participation	0.512	0.693	0.688	0.158
Works outside home	0.554	0.693	0.750	0.177
Works in non-farm job	0.025	0.095	0.132	0.253

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: p-values are obtained from the Difference-in-Difference (DID) regressions with clustered standard errors by municipality in Table 3, and correspond to the hypothesis that coefficients for G1 are equal to G2. *q-values* are obtained using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008). Block-bootstrapped p-values are estimated using the procedure in Bertrand et al. (2004) with 5,000 replications. Wild bootstrapped p-values are obtained following the suggested method in Cameron et al. (2008) with 5,000 replications and implemented with the *boottest* Stata command (Roodman, 2015).

Given that we estimate 15 hypothesis tests of equality between G1 and G2 (5 outcomes for the full sample, women, and men), we present adjusted q-values that account for the false discovery rate in column 2 of Table 4. The differences between demand-only (G1) and demand plus supply (G2) municipalities remain significant at conventional levels after applying multiple hypothesis corrections, providing credible evidence that both incentives have greater long-run impact delivered together than provided separately. We conduct further scrutiny of this result for robustness. Since PRAF was implemented in 70 municipalities, which is often considered a suitable number of clusters on which to conduct inference (Angrist and Pischke, 2009; Bertrand et al., 2004; Cameron and Miller, 2015), we also present p-values obtained from block-bootstrap and wild-bootstrap methods in columns 3 and 4 (Cameron et al., 2008). Most of our calculated p-values are larger using these methods, with some but not all the significant differences in columns 1 and 2 becoming marginally insignificant at the 10% level. These results highly suggest that combined exposure to demand- and supply-side incentives in CCTs seems to be more effective in fostering human capital accumulation and labor market participation

compared to receiving only demand-side support.

Our estimates suggest both that the combination of demand- and supply-side incentives leads to greater effects and that young women seem to benefit more than men from exposure to PRAF. This raises the question of whether the estimated gender differences are statistically significant. Table 5 shows p-values for the hypothesis that PRAF’s long-run effects for young women and men are equal.²¹ Results are mixed depending on the procedure used to calculate the standard errors, but the findings generally indicate that we cannot reject equal effects for young women and men within each incentive type. Exceptions include the effect on years of schooling in G1 communities where boys accumulated more education than girls and on labor force participation and employment in G2 communities when using wild-bootstrapped standard errors. However, given the mixed evidence, we choose to err on the side of caution and conclude that gender differences from exposure to PRAF incentives do not seem to be statistically significant for most outcomes in the long-run.

Table 5. Hypothesis tests: Equal long-run effects of exposure to PRAF incentives by gender

	Years of schooling	At least some secondary studies	Labor force participation	Works outside home	Works in non-farm job
<i>A. H₀: Young women in G1=Young men in G1</i>					
p-value Cluster-robust standard errors	0.041	0.223	0.602	0.856	0.304
q-value Cluster-robust standard errors	0.207	0.508	0.753	0.856	0.508
p-value Block-bootstrapped standard errors	0.032	0.230	0.597	0.854	0.312
p-value Wild-bootstrapped standard errors	0.050	0.199	0.012	0.019	0.338
<i>B. H₀: Young women in G2=Young men in G2</i>					
p-value Cluster-robust standard errors	0.469	0.426	0.225	0.309	0.453
q-value Cluster-robust standard errors	0.470	0.470	0.470	0.470	0.470
p-value Block-bootstrapped standard errors	0.464	0.436	0.235	0.317	0.487
p-value Wild-bootstrapped standard errors	0.341	0.629	0.066	0.080	0.489

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of hypothesis tests are drawn from a separate regression. The regressions are estimated by triple differences in a fully interacted specification of Equation (1). The table shows the resulting p-values and q-values for the hypothesis that coefficients on the interaction term ($G1_{it} \times \text{Young Women}$ and $G2_{it} \times \text{Young Women}$) are different from zero. We include the controls in Table 3 as main effects and interacted with a gender dummy variable that is equal to one for young women. We test the hypothesis assuming: i) cluster-robust standard errors by municipality, ii) adjusting the p-values using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008), iii) block-bootstrapped standard errors using the procedure in Bertrand et al. (2004) with 5,000 replications, and iv) wild bootstrapped standard errors following the suggested method in Cameron et al. (2008) with 5,000 replications and implemented with the *boottest* Stata command (Roodman, 2015).

We highlight three main findings from the empirical analysis of the long-term effects of PRAF. First, relative to comparison communities that received three years of Poverty Reduction Strategy benefits but no support from PRAF, we find no gains from additional exposure to two years of demand-side

²¹We estimate whether effects on young women and men are statistically different using a triple difference approach, comparing young women and men in treated and comparison municipalities over time. Estimated regressions include all main effects, double interactions, and the controls in Table 3 (by themselves and interacted with a dummy variable equal to one for young women). We test the statistical significance of the triple interaction terms ($G1_{it} \times \text{Women}$ and $G2_{it} \times \text{Women}$) to determine whether gender differences from exposure to PRAF are significant.

incentives individually. Exposure to two additional years of demand-side incentives plus three years of supply-side support does lead to significant gains in human capital and labor market outcomes. These effects are higher than the estimates in Araujo et al. (2017) but lie below most other estimates of long-run impacts of CCTs (Molina-Millán et al., 2016; Parker and Vogl, 2018). The relatively small impact estimates may be due to the small transfer size from PRAF (about 4% of monthly pre-program consumption).²² Results for demand-only municipalities resemble those from programs that provided the same incentives in Mexico, Ecuador, and Colombia (Araujo et al., 2017; Behrman et al., 2011; Parker et al., 2012). Findings for demand- and supply-side communities are consistent with estimates for Nicaragua's Red de Protección Social, a CCT program that delivered both demand and supply support, and which to date has shown the most positive estimates of long-term impacts (Barham et al., 2017; Molina-Millán and Macours, 2017).

Second, a rather unique feature of this study is that we test whether differential exposure to demand or supply incentives yields differences in longer-term outcomes. Our evidence is highly suggestive that combining demand and supply-side incentives outperforms separate provision of demand-side support. These conclusions are robust to specification choice, variance corrections, and are unlikely to be chance findings given their stability when adjusting for multiple hypothesis testing. Could we have foreseen this result given short-term estimates of PRAF impact? The results in Galiani and McEwan (2013) show no indication that short-run effects on school enrollment and child labor were statistically different between G1 and G2 communities.²³ To further explore this result, we estimate short-term effect regressions on the same outcomes using municipal-level data in Table 6. Separate estimates are shown for children aged 6-12: full sample, girls, and boys. The results reveal an interesting pattern: the reduction for household work seems to be larger in G2 municipalities compared to G1. We can reject equal coefficients between incentive types for the full sample and girls, and marginally accept equality for boys. Galiani and McEwan (2013) did not estimate gender-specific effects for G1 and G2 communities because the full sample results indicated no differences in short-term impact across the treatment arms. However, our results suggest that PRAF led more children to substitute household labor

²²In comparison, the size of transfers in neighboring Mexico (Progresa) and Nicaragua (Red de Protección Social) comprised almost 18 and 20% of baseline expenditures, respectively (Caldés et al., 2006).

²³In Table 2 of Galiani and McEwan (2013), the authors test whether coefficients on G1 and G2 communities are equal using census microdata. Their analysis yields p-values above 0.200 in all their specifications, leading them to conclude there was no differential impact between G1 and G2 communities in the first two years of PRAF implementation.

for schooling in G2 municipalities, which may be one potential explanation contributing to the larger longer-term effects that we find in the same communities over a decade later.

Table 6. The short-run effect of exposure to PRAF incentives on children

	Enrolled in school	Works outside home	Works only in home
<i>A. Children aged 6-12</i>			
G1	0.053 (0.025)**	-0.018 (0.020)	-0.015 (0.014)
G2	0.068 (0.016)***	-0.039 (0.011)***	-0.041 (0.010)***
p-value G1=G2	0.470	0.214	0.069
Adjusted R^2	0.681	0.311	0.450
Mean Comparison Group (G3+G4) in 2001	0.687	0.094	0.105
Observations	70	70	70
<i>B. Girls aged 6-12</i>			
G1	0.051 (0.024)**	-0.009 (0.011)	-0.018 (0.020)
G2	0.062 (0.016)***	-0.018 (0.009)*	-0.052 (0.014)***
p-value G1=G2	0.607	0.357	0.070
Adjusted R^2	0.671	0.150	0.462
Mean Comparison Group (G3+G4) in 2001	0.700	0.036	0.154
Observations	70	70	70
<i>C. Boys aged 6-12</i>			
G1	0.055 (0.026)**	-0.028 (0.029)	-0.011 (0.012)
G2	0.074 (0.017)***	-0.061 (0.016)***	-0.030 (0.008)***
p-value G1=G2	0.388	0.194	0.130
Adjusted R^2	0.675	0.350	0.285
Mean Comparison Group (G3+G4) in 2001	0.675	0.149	0.058
Observations	70	70	70

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report cross-section estimates of the short-term effects to PRAF relative to G3 and G4 municipalities that received no intervention. All regressions control for municipal-level variables in 2001: the share of eligible children for PRAF (individuals aged 6-12 who had not completed fourth grade in 2001), share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as block fixed-effects.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Finally, we find suggestive but inconclusive evidence that PRAF effects differ by gender. While the results from hypothesis tests cannot reject that estimated coefficients are equal, this may be due to two reasons. On the one hand, given our data limitations and the fact that we cannot separate eligible from ineligible individuals, our estimates can only provide lower bounds for PRAF's long-term impacts.

On the other hand, the Honduran context may be playing a role. Previous research has shown that returns to educational investment systematically differ by gender in rural areas of developing countries or “brawn-based” economies (Pitt et al., 2012). This prior evidence indicates that schooling has greater labor market returns for women because they specialize in skill-intensive activities, while men tend to work in physically-intensive jobs. PRAF municipalities fit this scenario, since while 75.7% of the adult labor force was employed in agricultural activities during 2013, the percentage of men and women employed in farm work is 77.2% and 13.1%, respectively. Our slightly larger estimates for young women may be partly explained by this channel, although further research that can overcome our empirical limitations is needed to better understand the driving mechanisms of potential gender differences in CCT program impact.

5 Threats to identification and caveats

We discuss several threats to identification that could bias our reported estimates of the long-run effects of exposure to PRAF and our conclusions about the effectiveness of its demand and supply-side incentives. This section discusses some of these factors and other empirical concerns.

First, it is possible that we fail to find effects on some outcomes or groups due to low statistical power, given that we have observations for 70 municipalities at two points in time. To explore this possibility, we conduct two analyses. First, we replicate the findings in Galiani and McEwan (2013) in Table A.9. If we had insufficient statistical power, we would not find significant short-run effects. Our estimates replicate these authors’ findings in significance and magnitude. Second, we calculate ex-post minimum detectable effects for each outcome using baseline means and standard deviations (full sample, young women, and men), following the procedure in Duflo et al. (2007). Table A.10 shows these values. Since more than a decade passes between censuses, adjusting for covariates becomes essential. We scale the minimum detectable effects, multiplying them by one minus the R^2 of a regression that includes all our controls as well as block indicators on baseline outcomes. This procedure reduces variation in outcomes, providing minimum detectable effects that lie within range or below our estimated coefficients for the majority of our selected outcomes.

Second, selective migration may push treated individuals to move toward large urban centers to

seek out better opportunities (Molina-Millán and Macours, 2017). Due to the aggregate nature of our data, we cannot track people who moved away from the 70 experimental municipalities. To test for differential migration patterns, we pool all available household surveys from 2001-2013 for the exposed cohort of young adults (born between 1989 and 1995, hence aged 6-12 in 2001 and 18-24 in 2013). Then, we estimate a difference-in-difference regression to test whether children in G1 and G2 municipalities were more likely to leave their municipality of birth.²⁴ Despite pooling 22 surveys, the sample size is small because surveys are not representative by municipality ($N=17,622$). Supplementary Figure A.1 suggests no evidence of differential migration by young adults in G1 and G2 communities relative to the same age cohort in comparison municipalities.

While the above analysis rules out differential migration within Honduras, it does not account for varying levels of emigration from Honduras to the United States. To approximate whether international migration may be confounding our estimates, we estimate a version of Equation (1) where the dependent variable is the log population of young adults, women, and men. This strategy captures whether the denominator used to calculate our outcomes changes differentially across PRAF treatment groups with regards to comparison communities. Results are shown in Table A.11. The findings suggest that there were no considerably significant population changes in G1 and G2 communities relative to G3 and G4 comparison municipalities, lending further support to the claim that migration, internal or external, does not seem to be largely affecting PRAF municipalities.

Like many other randomized CCT programs, PRAF was not designed with long-term evaluation in mind. As shown in the previous section, the comparison group shows no differences with respect to non-PRAF municipalities also exposed to the Poverty Reduction Strategy. This evidence suggests that at least in Honduras, the estimated long-term effects of PRAF and the differences between individual and combined provision of demand and supply incentives would remain regardless of circumstances in the comparison group. As mentioned by Molina-Millán et al. (2016), the design of poverty alleviation programs should, in the future, incorporate short-, medium-, and long-run evaluation in order to provide

²⁴The estimated equation is $M_{imt} = \alpha + \beta_t^g(PRAF_g \times \delta_t) + \gamma X_{imt} + \lambda_m + \mu_b \times t + \delta_t + u_{imt}$. M_{imt} is a binary variable that identifies if individual i in municipality m at time t lives in a different municipality than that of their birth. Thus, we assume that children who were born in PRAF municipalities were living there when exposed to the program in 2001. We report estimates of β_t^g in Figure A.1, which capture the annual differences in migration rates between individuals in the three treated groups relative to the comparison group. The regressions control for birth-year effects, municipality fixed effects, linear time trends by stratification block, and survey wave effects. Standard errors are clustered by municipality and regressions are weighted using the official survey weights for each wave.

clear evidence of CCT impact throughout their many stages.

Some empirical issues may be solved by applying our strategy on microdata but not all of them. Even with micro-level data, anonymity is ensured by constitutional right or Habeas Data²⁵, so an individual-level panel cannot be constructed from these records. While using repeated cross-sections of census data would be an improvement, census data would not allow us to control for time-invariant attributes like individual ability that may be related to human capital and labor market outcomes. Furthermore, because the randomization unit was the municipality, statistical power will not increase by using more observations within a cluster; rather, increasing statistical power would require actually increasing the number of clusters. Identifying eligible individuals to isolate the direct effect on beneficiaries (instead of broader exposure) would be problematic because the census does not ask retrospective questions that would allow us to reconstruct eligibility conditions.

The arguments in this section suggest that our estimates are capturing the long-run effects of exposure to PRAF's educational incentives over a decade later, and that these estimates likely constitute lower bounds for its true long-term impact. Given program roll-out and data limitations, we address an unanswered question in the CCT literature: does the manner in which incentives are provided matter in the long run? Our findings are intended to provide a starting point to discuss the overall effectiveness of conditional cash transfers, but also to identify what aspects of these policies are most useful to increase social mobility and reduce the transmission of poverty. We expect that our findings will be complemented by future work that overcomes some of our empirical challenges by using individual-level data to examine and evaluate this and other CCT programs.

6 Conclusion

This paper estimates the long-term effects of exposure to a conditional cash transfer program in Honduras over a decade later. While political objectives ultimately contaminated the comparison group, we take advantage of program roll-out to test an unanswered question in the CCT literature: does individual exposure to demand-side incentives (vouchers) or combined demand- and supply-side incentives (vouchers plus clinic and school subsidies) influence the degree to which these programs

²⁵Habeas Data was established by Decree No. 10-2013, published by the Honduran Senate on Wednesday March 27, 2013 in the official newspaper La Gaceta.

impact human capital and labor market outcomes in the long-term? Using municipal-level panel data from population censuses, we estimate difference-in-difference regressions that account for time-invariant and time-varying factors. We find no lasting impact attributable to demand-side incentives alone, but do find that joint provision of both types of support results in greater human capital gains and labor market participation. Our empirical results are robust to alternative specifications, variance corrections, and multiple hypothesis adjustments.

Studies that analyze demand-driven CCT programs tend to find long-term human capital gains but no impact on labor outcomes (Molina-Millán et al., 2016; Araujo et al., 2017), while those that study interventions where demand and supply incentives are combined estimate positive gains (Molina-Millán and Macours, 2017). We provide a plausible explanation for this mixed evidence: that the form (demand or supply incentives) of conditional cash transfer delivery matters. These findings emphasize the importance of considering the supply-side in CCT design and evaluation since they highly suggest that separate provision of demand incentives is less effective than combining demand and supply provision. One interpretation of our results is that individuals in municipalities exposed to both incentives had better outcomes due to improved health and education quality, prompted by investments in infrastructure, supplies, and training programs. We agree with studies that support the hypothesis that supply-side factors can constrain or enhance the extent to which the poor benefit from CCTs (Coady and Parker, 2004; Behrman et al., 2011; Maluccio et al., 2010), and also contribute to the literature that analyzes the effectiveness of different program components (Baird et al., 2011; Barrera-Osorio et al., 2011; Akresh et al., 2016). Future work, however, is necessary to explain in detail why combining incentives yields special impact, perhaps further exploring the suggestive evidence that CCT impacts on men and women might differ.

While our evidence for Honduras is highly suggestive that combining incentives in conditional cash transfers is a more effective means to reduce persistent poverty, further research is required to understand CCT effects more completely. First, given that the ultimate objective of these programs is to reduce the intergenerational transmission of poverty, evidence on living standards of beneficiaries' children is essential. Second, long-term evaluation designs are necessary to provide more credible long-term assessments, especially with regards to heterogeneous impact. Currently, most studies require

making simplifying (and sometimes restrictive) assumptions to examine the lasting consequences of cash transfers due to design, political, and administrative issues. Finally, further evidence on cash transfer programs in other contexts is required to determine what components and incentive schemes in these programs are working and how less effective parts may be enhanced. Our findings suggest that the form of delivering incentives matters, but future studies may address other relevant and unanswered questions to inform academics and policymakers and to maximize the anti-poverty benefits of aid programs.

References

- Abadie, A., Diamond, A., and Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, 105(490):493–505.
- Akresh, R., De Walque, D., and Kazianga, H. (2016). Evidence from a randomized evaluation of the household welfare impacts of conditional and unconditional cash transfers given to mothers or fathers. Policy Research Working Paper Series 7730, The World Bank.
- Alzúa, M. L., Cruces, G., and Ripani, L. (2013). Welfare programs and labor supply in developing countries: Experimental evidence from Latin America. *Journal of Population Economics*, 26(4):1255–1284.
- Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, 103(484):1481–1495.
- Angrist, J. D. and Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.
- Araujo, M. C., Bosch, M., and Schady, N. (2017). Can Cash Transfers Help Households Escape an Inter-Generational Poverty Trap? In *The Economics of Poverty Traps*. University of Chicago Press.
- Báez, J. E. and Camacho, A. (2011). Assessing the Long-term Effects of Conditional Cash Transfers on Human Capital: Evidence from Colombia. IZA Discussion Papers 5751, Institute for the Study of Labor (IZA).
- Baird, S., McIntosh, C., and Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *The Quarterly Journal of Economics*, 126(4):1709–1753.
- Baird, S. J., McIntosh, C., and Ozler, B. (2016). When the money runs out: Do cash transfers have sustained effects on human capital accumulation? Policy Research Working Paper Series 7901, The World Bank.
- Barham, T., Macours, K., and Maluccio, J. A. (2017). Are Conditional Cash Transfers Fulfilling Their Promise? Schooling, Learning, and Earnings After 10 Years. CEPR Discussion Paper Series DP11937, Centre for Economic Policy Research.
- Barrera-Osorio, F., Bertrand, M., Linden, L. L., and Perez-Calle, F. (2011). Improving the design of conditional transfer programs: Evidence from a randomized education experiment in Colombia. *American Economic Journal: Applied Economics*, 3(2):167–195.
- Barrera-Osorio, F., Linden, L. L., and Saavedra, J. (2017). Medium- and Long-Term Educational Consequences of Alternative Conditional Cash Transfer Designs: Experimental Evidence from Colombia. Working Paper 23275, National Bureau of Economic Research.
- Barrientos, A. and DeJong, J. (2006). Reducing child poverty with cash transfers: A sure thing? *Development Policy Review*, 24(5):537–552.

- Behrman, J. R., Parker, S. W., and Todd, P. E. (2011). Do conditional cash transfers for schooling generate lasting benefits? A five-year followup of PROGRESA/Oportunidades. *Journal of Human Resources*, 46(1):93–122.
- Benedetti, F., Ibararán, P., and McEwan, P. J. (2016). Do education and health conditions matter in a large cash transfer? Evidence from a Honduran experiment. *Economic Development and Cultural Change*, 64(4):759–793.
- Benjamini, Y. and Hochberg, Y. (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society. Series B (Methodological)*, pages 289–300.
- Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1):249–275.
- Bruhn, M. and McKenzie, D. (2009). In pursuit of balance: Randomization in practice in development field experiments. *American Economic Journal: Applied Economics*, 1(4):200–232.
- Caldés, N., Coady, D., and Maluccio, J. A. (2006). The cost of poverty alleviation transfer programs: A comparative analysis of three programs in Latin America. *World Development*, 34(5):818–837.
- Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, 90(3):414–427.
- Cameron, A. C. and Miller, D. L. (2015). A practitioners guide to cluster-robust inference. *Journal of Human Resources*, 50(2):317–372.
- Coady, D. P. and Parker, S. W. (2004). Cost-effectiveness analysis of demand-and supply-side education interventions: The case of PROGRESA in Mexico. *Review of Development Economics*, 8(3):440–451.
- Duflo, E., Glennerster, R., and Kremer, M. (2007). Using randomization in development economics research: A toolkit. *Handbook of Development Economics*, 4:3895–3962.
- Fiszbein, A. and Schady, N. R. (2009). *Conditional cash transfers: Reducing present and future poverty*. World Bank Publications.
- Galiani, S. and McEwan, P. J. (2013). The heterogeneous impact of conditional cash transfers. *Journal of Public Economics*, 103:85–96.
- Ganimian, A. J. and Murnane, R. J. (2016). Improving education in developing countries: Lessons from rigorous impact evaluations. *Review of Educational Research*, 86(3):719–755.
- García, A., Romero, O., Attanasio, O., and Pellerano, L. (2012). Impactos de Largo Plazo del Programa Familias en Acción en Municipios de menos de 100 mil habitantes en los Aspectos Claves del Desarrollo del Capital Humano. *Informe final. Unión temporal Econometría y SEI*.
- García, S. and Saavedra, J. E. (2017). Educational Impacts and Cost-Effectiveness of Conditional Cash Transfer Programs in Developing Countries: A Meta-Analysis. *Review of Educational Research*, 87(5):921–965.

- Glewwe, P. and Olinto, P. (2004). Evaluating the impact of conditional cash transfers on schooling: An experimental analysis of Honduras' PRAF program. *Final Report for USAID*.
- Government of Honduras (2001). Honduras - Poverty Reduction Strategy Paper. Available at: <https://goo.gl/D2pAUQ>.
- Ham, A. (2014). The impact of conditional cash transfers on educational inequality of opportunity. *Latin American Research Review*, 49(3):153–175.
- IDB (2006). Informe de Terminación de Proyecto: Programa de Asignación Familiar, Fase II. Available at: <http://idbdocs.iadb.org/wsdocs/getdocument.aspx?docnum=865397>. Inter-American Development Bank, Washington DC.
- IDB (2012). Informe de Terminación de Proyecto: Programa Integral de Protección Social. Available at: <http://idbdocs.iadb.org/wsdocs/getdocument.aspx?docnum=36914783>. Inter-American Development Bank, Washington DC.
- IFPRI (1999). First Report: PRAF-II Project. Technical report, International Food Policy Research Institute.
- IFPRI (2000). Third Report: PRAF-II Monitoring and Evaluation System. Technical report, International Food Policy Research Institute.
- Kling, J. R., Liebman, J. B., and Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, 75(1):83–119.
- Li, J. (2016). An Assessment of the Medium Term Impacts of a Honduran Conditional Cash Transfer Experiment on Education and Fertility. Available at: <http://repository.wellesley.edu/thesiscollection/352>. Honors Thesis Collection, 352.
- Lindahl, M., Palme, M., Massih, S. S., and Sjögren, A. (2015). Long-Term Intergenerational Persistence of Human Capital An Empirical Analysis of Four Generations. *Journal of Human Resources*, 50(1):1–33.
- Linos, E. (2013). Do conditional cash transfer programs shift votes? Evidence from the Honduran PRAF. *Electoral Studies*, 32(4):864–874.
- Maluccio, J. A., Murphy, A., and Regalia, F. (2010). Does supply matter? Initial schooling conditions and the effectiveness of conditional cash transfers for grade progression in Nicaragua. *Journal of Development Effectiveness*, 2(1):87–116.
- Molina-Millán, T., Barham, T., Macours, K., Maluccio, J. A., and Stampini, M. (2016). Long-Term Impacts of Conditional Cash Transfers in Latin America: Review of the Evidence. Technical report, Inter-American Development Bank.
- Molina-Millán, T. and Macours, K. (2017). Attrition in randomized control trials: Using tracking information to correct bias. Technical report, Universidade Nova de Lisboa, Faculdade de Economia, NOVAFRICA.

- Moore, C. (2008). Assessing Honduras? CCT Programme PRAF, Programa de Asignación Familiar: Expected and Unexpected Realities. Country Study 15, International Policy Centre for Inclusive Growth.
- Morris, S., Flores, R., Olinto, P., and Medina, J. M. (2004). Monetary incentives in primary health care and effects on use and coverage of preventive health care interventions in rural Honduras: cluster randomised trial. *The Lancet*, 364(9450):2030–2037.
- Parker, S., Rubalcava, L., and Teruel, G. (2012). Do Conditional Cash Transfer Programs Improve Work and Earnings among its Youth Beneficiaries? Evidence after a Decade of a Mexican Cash Transfer Program. Unpublished manuscript.
- Parker, S. and Vogl, T. (2018). Do Conditional Cash Transfers Improve Economic Outcomes in the Next Generation? Evidence from Mexico. Working Paper 24303, National Bureau of Economic Research.
- Pitt, M., Rosenzweig, M., and Hassan, M. (2012). Human capital investment and the gender division of labor in a brawn-based economy. *American Economic Review*, 102(7):3531–60.
- Quistorff, B. and Galiani, S. (2017). The synth_runner package: Utilities to automate synthetic control estimation using synth. Available at: https://github.com/bquistorff/synth_runner. Version 1.6.0.
- Rackstraw, E. (2014). A Decade Later: An Evaluation of the Longer-Term Impacts of a Honduran Conditional Cash Transfer. Available at: <http://repository.wellesley.edu/thesiscollection/215>. Honors Thesis Collection, 215.
- Rawlings, L. B. and Rubio, G. M. (2005). Evaluating the impact of conditional cash transfer programs. *The World Bank Research Observer*, 20(1):29–55.
- Roodman, D. (2015). BOOTTEST: Stata module to provide fast execution of the wild bootstrap with null imposed. Statistical Software Components, Boston College Department of Economics.
- Sobrado, C. and Clavijo, I. (2008). Honduras: Informe sobre revisión de la medición de la Pobreza en Honduras. Report for the Poverty and Gender Unit, The World Bank.
- Solon, G., Haider, S. J., and Wooldridge, J. M. (2015). What are we weighting for? *Journal of Human Resources*, 50(2):301–316.
- Stampini, M. and Tornarolli, L. (2012). The Growth of Conditional Cash Transfers in Latin America and the Caribbean: Did They Go Too Far? IZA Policy Papers 49, Institute for the Study of Labor (IZA).
- Stecklov, G., Winters, P., Todd, J., and Regalia, F. (2007). Unintended effects of poverty programmes on childbearing in less developed countries: Experimental evidence from Latin America. *Population Studies*, 61(2):125–140.

A Supplementary Material

Table A.1. Descriptive statistics

	All municipalities in Honduras				PRAF municipalities			
	2001	(SD)	2013	(SD)	2001	(SD)	2013	(SD)
<i>Outcomes for children 6-12</i>								
Enrollment	0.761	(0.097)	0.891	(0.050)	0.726	(0.102)	0.883	(0.054)
Works outside home	0.059	(0.050)	0.024	(0.016)	0.074	(0.054)	0.028	(0.018)
Works in home	0.084	(0.044)	0.050	(0.026)	0.088	(0.047)	0.052	(0.024)
<i>Outcomes for young adults 18-24</i>								
Years of schooling	4.657	(1.146)	6.670	(1.046)	3.777	(0.818)	5.981	(0.869)
At least some secondary studies	0.153	(0.111)	0.338	(0.132)	0.074	(0.055)	0.240	(0.089)
Labor force participation	0.493	(0.064)	0.456	(0.058)	0.523	(0.053)	0.488	(0.055)
Works outside home	0.484	(0.065)	0.444	(0.063)	0.518	(0.054)	0.481	(0.055)
Works in non-farm job	0.326	(0.213)	0.354	(0.218)	0.200	(0.100)	0.248	(0.127)
<i>Outcomes for young women 18-24</i>								
Years of schooling	4.953	(1.223)	7.020	(1.080)	3.920	(0.856)	6.204	(0.850)
At least some secondary studies	0.178	(0.123)	0.376	(0.141)	0.085	(0.063)	0.264	(0.095)
Labor force participation	0.152	(0.092)	0.138	(0.077)	0.131	(0.078)	0.124	(0.075)
Works outside home	0.148	(0.091)	0.132	(0.074)	0.130	(0.078)	0.121	(0.075)
Works in non-farm job	0.846	(0.143)	0.866	(0.114)	0.775	(0.178)	0.838	(0.116)
<i>Outcomes for young men 18-24</i>								
Years of schooling	4.365	(1.095)	6.323	(1.043)	3.653	(0.859)	5.764	(0.933)
At least some secondary studies	0.129	(0.100)	0.300	(0.126)	0.064	(0.050)	0.216	(0.087)
Labor force participation	0.826	(0.101)	0.770	(0.105)	0.891	(0.067)	0.842	(0.073)
Works outside home	0.812	(0.107)	0.751	(0.115)	0.884	(0.071)	0.832	(0.075)
Works in non-farm job	0.246	(0.211)	0.279	(0.217)	0.125	(0.081)	0.169	(0.112)
<i>Controls</i>								
Female	0.495	(0.013)	0.502	(0.014)	0.491	(0.012)	0.497	(0.012)
Age	22.519	(1.655)	25.318	(1.907)	21.508	(1.376)	24.130	(1.699)
Share of individuals born in municipality	0.824	(0.123)	0.804	(0.116)	0.881	(0.091)	0.865	(0.073)
Lenca	0.096	(0.184)	0.130	(0.292)	0.285	(0.237)	0.398	(0.407)
Share of urban population	0.173	(0.239)	0.251	(0.256)	0.065	(0.143)	0.108	(0.160)
Access to basic services (electricity, water, sewer)	0.443	(0.169)	0.588	(0.147)	0.368	(0.101)	0.534	(0.127)
Household assets (car, computer, refrigerator, TV)	0.118	(0.099)	0.272	(0.126)	0.049	(0.045)	0.180	(0.091)
Household size	5.236	(0.442)	4.653	(0.394)	5.414	(0.373)	4.824	(0.408)
Share of internal migrants	0.052	(0.037)	0.047	(0.029)	0.036	(0.019)	0.042	(0.028)
Share of international migrants	0.024	(0.027)	0.017	(0.014)	0.013	(0.017)	0.013	(0.009)
Population density	71.90	(70.22)	95.75	(100.66)	66.20	(29.15)	85.87	(36.34)
Relative schooling: adult females/adult males	0.962	(0.146)	1.036	(0.117)	0.870	(0.188)	0.959	(0.151)
Relative labor supply: adult females/adult males	0.187	(0.110)	0.189	(0.118)	0.151	(0.108)	0.141	(0.095)

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Table A.2. The long-run effects of exposure to PRAF incentives (all treatment groups)

	Years of schooling	At least some secondary studies	Labor force participation	Works outside home	Works in non-farm job
<i>A. Young adults</i>					
G1: +2 years of demand	0.090 (0.130)	0.022 (0.014)	0.007 (0.019)	0.010 (0.020)	-0.014 (0.025)
G2: +2 years of demand and 3 years of supply	0.301 (0.124)**	0.036 (0.015)**	0.023 (0.018)	0.024 (0.019)	0.031 (0.024)
G3: + 3 years of supply	-0.033 (0.155)	0.017 (0.021)	0.023 (0.016)	0.020 (0.016)	-0.024 (0.019)
p-value G1=G2	0.072	0.344	0.485	0.529	0.029
p-value G2=G3	0.029	0.339	0.993	0.837	0.004
Adjusted R^2	0.979	0.942	0.580	0.580	0.705
Mean Comparison Group (G4) in 2001	3.613	0.071	0.539	0.535	0.209
Observations	140	140	140	140	140
<i>B. Young women</i>					
G1: +2 years of demand	-0.033 (0.126)	0.017 (0.015)	0.006 (0.013)	0.004 (0.013)	0.057 (0.068)
G2: +2 years of demand and 3 years of supply	0.265 (0.129)**	0.042 (0.016)**	0.032 (0.014)**	0.030 (0.015)**	0.016 (0.051)
G3: + 3 years of supply	-0.020 (0.158)	0.022 (0.021)	-0.008 (0.014)	-0.011 (0.014)	-0.047 (0.041)
p-value G1=G2	0.013	0.123	0.012	0.012	0.467
p-value G2=G3	0.079	0.339	0.002	0.002	0.168
Adjusted R^2	0.979	0.944	0.866	0.860	0.216
Mean Comparison Group (G4) in 2001	3.697	0.085	0.151	0.149	0.769
Observations	140	140	140	140	140
<i>C. Young men</i>					
G1: +2 years of demand	0.189 (0.153)	0.028 (0.014)*	0.015 (0.027)	0.021 (0.028)	-0.021 (0.024)
G2: +2 years of demand and 3 years of supply	0.312 (0.152)**	0.030 (0.015)*	0.035 (0.025)	0.038 (0.026)	0.016 (0.021)
G3: + 3 years of supply	-0.054 (0.176)	0.012 (0.023)	0.057 (0.022)***	0.055 (0.022)**	-0.016 (0.017)
p-value G1=G2	0.388	0.869	0.563	0.636	0.040
p-value G2=G3	0.029	0.400	0.415	0.550	0.043
Adjusted R^2	0.970	0.930	0.454	0.456	0.643
Mean Comparison Group (G4) in 2001	3.541	0.060	0.906	0.900	0.125
Observations	140	140	140	140	140

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report Difference-in-Differences (DID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy (see Section 3 for details). All regressions control for time-varying municipal-level variables: the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.3. The long-run effects of exposure to PRAF incentives on composite outcome domains

	Young adults		Young women		Young men	
	Education	Labor	Education	Labor	Education	Labor
G1: +2 years of demand	0.211 (0.174)	-0.012 (0.234)	0.053 (0.156)	0.264 (0.197)	0.376 (0.200)*	-0.098 (0.288)
G2: +2 years of demand and 3 years of supply	0.468 (0.175)***	0.316 (0.227)	0.407 (0.160)**	0.375 (0.173)**	0.475 (0.196)**	0.230 (0.278)
p-value G1=G2	0.141	0.266	0.034	0.563	0.604	0.410
Adjusted R^2	0.972	0.611	0.971	0.649	0.964	0.396
Observations	140	140	140	140	140	140

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression and should be interpreted as standard deviations relative to the comparison group mean in 2001. Cluster-robust standard errors by municipality in parentheses. We report Difference-in-Differences (DID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy (see Section 3 for details). All regressions control for time-varying municipal-level variables: the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.4. Multiple hypothesis adjustments: The long-run effects of exposure to PRAF incentives (Table 3)

	G1		G2	
	p-value	q-value	p-value	q-value
<i>A. Young adults</i>				
Years of schooling	0.391	0.812	0.006	0.030
At least some secondary studies	0.247	0.812	0.046	0.086
Labor force participation	0.923	0.989	0.437	0.505
Works outside home	0.914	0.989	0.372	0.505
Works in non-farm job	0.838	0.989	0.035	0.076
<i>B. Young women</i>				
Years of schooling	0.827	0.989	0.024	0.074
At least some secondary studies	0.549	0.916	0.034	0.076
Labor force participation	0.332	0.812	0.003	0.030
Works outside home	0.394	0.812	0.004	0.030
Works in non-farm job	0.220	0.812	0.419	0.505
<i>C. Young men</i>				
Years of schooling	0.129	0.812	0.012	0.044
At least some secondary studies	0.108	0.812	0.091	0.153
Labor force participation	0.779	0.989	0.676	0.677
Works outside home	0.988	0.989	0.564	0.605
Works in non-farm job	0.433	0.812	0.178	0.267

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: p-values are obtained from Difference-in-Difference (DID) regressions of the long-term effects of exposure to PRAF on restricted assignment groups (Table 3), and correspond to the hypothesis that estimated coefficients are significantly different from zero. *q-values* are obtained for each hypothesis using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008).

Table A.5. The long-run effects of exposure to PRAF incentives (weighted estimates)

	Years of schooling	At least some secondary studies	Labor force participation	Works outside home	Works in non-farm job
<i>A. Young adults</i>					
G1: +2 years of demand	0.109 (0.185)	0.016 (0.022)	-0.002 (0.027)	0.002 (0.028)	-0.005 (0.032)
G2: +2 years of demand and 3 years of supply	0.320 (0.174)*	0.030 (0.023)	0.013 (0.027)	0.015 (0.027)	0.041 (0.030)
p-value G1=G2	0.239	0.548	0.669	0.698	0.147
Adjusted R^2	0.967	0.913	0.513	0.514	0.862
Mean Comparison Group (G3+G4) in 2001	3.699	0.076	0.533	0.529	0.209
Observations	140	140	140	140	140
<i>B. Young women</i>					
G1: +2 years of demand	-0.020 (0.182)	0.008 (0.023)	0.009 (0.015)	0.008 (0.016)	0.078 (0.097)
G2: +2 years of demand and 3 years of supply	0.281 (0.186)	0.033 (0.024)	0.035 (0.018)*	0.035 (0.018)*	0.036 (0.071)
p-value G1=G2	0.098	0.329	0.088	0.089	0.636
Adjusted R^2	0.966	0.915	0.901	0.895	0.334
Mean Comparison Group (G3+G4) in 2001	3.811	0.090	0.143	0.141	0.791
Observations	140	140	140	140	140
<i>C. Young men</i>					
G1: +2 years of demand	0.218 (0.214)	0.023 (0.022)	-0.007 (0.040)	0.001 (0.041)	-0.015 (0.030)
G2: +2 years of demand and 3 years of supply	0.336 (0.201)*	0.026 (0.023)	0.010 (0.039)	0.015 (0.040)	0.022 (0.026)
p-value G1=G2	0.588	0.917	0.749	0.797	0.178
Adjusted R^2	0.955	0.896	0.384	0.407	0.847
Mean Comparison Group (G3+G4) in 2001	3.601	0.064	0.901	0.895	0.128
Observations	140	140	140	140	140

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report Difference-in-Differences (DID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy (see Section 3 for details). All regressions control for time-varying municipal-level variables: the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends. We weight the estimates using the share of young adults, women, and men relative to the total population in each municipality and year.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.6. The long-run effects of exposure to PRAF relative to prime-age individuals (aged 25-29)

	Years of schooling	At least some secondary studies	Labor force participation	Works outside home	Works in non-farm job
<i>A. Young adults</i>					
G1: +2 years of demand	-0.039 (0.095)	0.003 (0.011)	-0.017 (0.015)	-0.014 (0.015)	0.008 (0.015)
G2: +2 years of demand and 3 years of supply	0.183 (0.131)	0.014 (0.011)	-0.018 (0.015)	-0.017 (0.015)	0.029 (0.020)
p-value G1=G2	0.065	0.331	0.939	0.827	0.241
Adjusted R^2	0.967	0.919	0.508	0.504	0.653
Mean Comparison Group (G3+G4) in 2001	3.285	0.066	0.538	0.535	0.246
Observations	280	280	280	280	280
<i>B. Young women</i>					
G1: +2 years of demand	-0.225 (0.114)*	-0.013 (0.013)	0.002 (0.012)	0.001 (0.012)	-0.002 (0.058)
G2: +2 years of demand and 3 years of supply	0.091 (0.141)	0.017 (0.013)	0.030 (0.014)**	0.030 (0.014)**	-0.023 (0.033)
p-value G1=G2	0.011	0.017	0.036	0.035	0.707
Adjusted R^2	0.964	0.921	0.828	0.824	0.167
Mean Comparison Group (G3+G4) in 2001	3.289	0.070	0.144	0.143	0.833
Observations	280	280	280	280	280
<i>C. Young men</i>					
G1: +2 years of demand	0.148 (0.134)	0.020 (0.012)	-0.008 (0.013)	-0.001 (0.014)	-0.000 (0.016)
G2: +2 years of demand and 3 years of supply	0.259 (0.175)	0.009 (0.013)	0.001 (0.013)	0.004 (0.014)	0.001 (0.018)
p-value G1=G2	0.495	0.320	0.521	0.730	0.967
Adjusted R^2	0.946	0.879	0.473	0.477	0.555
Mean Comparison Group (G3+G4) in 2001	3.281	0.062	0.930	0.924	0.162
Observations	280	280	280	280	280

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report triple difference (DID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy: comparing outcomes for eligible young adults (aged 18-24) relative to ineligible prime-age adults (aged 25-29) between treatment and comparison municipalities over time. All regressions control for time-varying municipal-level variables and their interactions with a dummy variable for eligible PRAF young adults (aged 18-24): the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.7. The long-run effects of exposure to PRAF relative to adults (aged 30-77)

	Years of schooling	At least some secondary studies	Labor force participation	Works outside home	Works in non-farm job
<i>A. Young adults</i>					
G1: +2 years of demand	0.028 (0.119)	0.013 (0.013)	-0.010 (0.012)	-0.007 (0.012)	-0.004 (0.015)
G2: +2 years of demand and 3 years of supply	0.172 (0.117)	0.024 (0.013)*	0.006 (0.013)	0.009 (0.013)	0.018 (0.014)
p-value G1=G2	0.245	0.386	0.187	0.174	0.128
Adjusted R^2	0.981	0.930	0.617	0.617	0.660
Mean Comparison Group (G3+G4) in 2001	2.137	0.040	0.531	0.528	0.234
Observations	280	280	280	280	280
<i>B. Young women</i>					
G1: +2 years of demand	-0.139 (0.129)	0.002 (0.013)	0.003 (0.010)	0.002 (0.010)	0.024 (0.063)
G2: +2 years of demand and 3 years of supply	0.112 (0.139)	0.025 (0.014)*	0.030 (0.011)***	0.030 (0.011)***	-0.007 (0.030)
p-value G1=G2	0.075	0.094	0.008	0.006	0.574
Adjusted R^2	0.983	0.935	0.886	0.885	0.181
Mean Comparison Group (G3+G4) in 2001	1.970	0.039	0.141	0.141	0.805
Observations	280	280	280	280	280
<i>C. Young men</i>					
G1: +2 years of demand	0.179 (0.142)	0.024 (0.015)	-0.019 (0.013)	-0.013 (0.013)	-0.008 (0.014)
G2: +2 years of demand and 3 years of supply	0.221 (0.135)	0.023 (0.015)	-0.002 (0.012)	0.004 (0.013)	0.002 (0.013)
p-value G1=G2	0.789	0.953	0.192	0.221	0.468
Adjusted R^2	0.971	0.908	0.452	0.462	0.545
Mean Comparison Group (G3+G4) in 2001	2.303	0.041	0.917	0.911	0.151
Observations	280	280	280	280	280

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report triple difference (DIDID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy: comparing outcomes for eligible young adults (aged 18-24) relative to ineligible adults (aged 30-77) between treatment and comparison municipalities over time. All regressions control for time-varying municipal-level variables and their interactions with a dummy variable for eligible PRAF young adults (aged 18-24): the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.8. Synthetic control estimates of contamination in comparison municipalities

	Young adults		Young women		Young men	
	Education	Labor	Education	Labor	Education	Labor
Comparison vs. Synthetic comparison	-0.087	0.366	-0.141	-0.190	-0.069	0.215
p-value of chance finding	0.777	0.678	0.893	0.334	0.542	0.845
Number of comparison municipalities	30	30	30	30	30	30
Number of donor municipalities	228	228	228	228	228	228

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: The estimates are obtained by synthetic control method (Abadie et al., 2010). We define G3 and G4 comparison municipalities ($N = 30$) as the treatment group and select synthetic controls from the pool of non-experimental municipalities ($N = 228$). We do not include treated units in G1 and G2 for this analysis, $N = 40$. Coefficients should be interpreted as standard deviations relative to synthetic comparison municipalities in 2001. The method selects donors controlling for variables measured in 2001: the share of female population, average age and its square, share of individuals born in the municipality, fraction of *lenca* ethnic group, fraction of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), household size and its square, share of internal migrants, share of international migrants, population density; as well as the relative schooling and labor force participation of adult females relative to males (aged 30-77). Implementation is carried out using the *synth_runner* Stata package. A full description of the package may be found in Quistorff and Galiani (2017).

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.9. Replication of results in Galiani and McEwan’s (GM) 2013 study with municipal-level data

	Enrolled in school		Works outside home		Works only in home	
	GM (2013)	Replication	GM (2013)	Replication	GM (2013)	Replication
<i>Panel A</i>						
PRAF	0.080	0.071	-0.030	-0.038	-0.040	-0.033
	(0.023)***	(0.019)***	(0.011)***	(0.015)**	(0.013)***	(0.010)***
Adjusted R^2	0.160	0.689	0.090	0.195	0.064	0.451
<i>Panel B</i>						
PRAF \times Blocks 1-2	0.149	0.083	-0.068	-0.057	-0.061	-0.031
	(0.034)***	(0.030)***	(0.016)***	(0.028)**	(0.018)***	(0.016)*
PRAF \times Blocks 3-5	0.035	0.062	-0.006	-0.024	-0.026	-0.034
	(0.025)	(0.023)***	(0.013)	(0.018)	(0.017)	(0.013)**
p-value	0.009	0.564	0.004	0.331	0.065	0.903
Adjusted R^2	0.163	0.685	0.092	0.199	0.163	0.440
<i>Panel C</i>						
PRAF \times Block 1	0.178	0.106	-0.079	-0.068	-0.063	-0.016
	(0.045)***	(0.033)***	(0.022)***	(0.036)*	(0.027)**	(0.026)
PRAF \times Block 2	0.104	0.055	-0.050	-0.046	-0.058	-0.050
	(0.042)**	(0.056)	(0.020)**	(0.043)	(0.019)***	(0.016)***
PRAF \times Block 3	0.047	0.046	-0.011	-0.022	-0.039	-0.020
	(0.045)	(0.036)	(0.016)	(0.033)	(0.036)	(0.025)
PRAF \times Block 4	0.016	0.074	0.001	-0.008	-0.011	-0.041
	(0.041)	(0.035)**	(0.029)	(0.032)	(0.026)	(0.023)*
PRAF \times Block 5	0.044	0.063	-0.009	-0.042	-0.031	-0.046
	(0.046)	(0.033)*	(0.021)	(0.027)	(0.028)	(0.021)**
p-value	0.074	0.841	0.060	0.816	0.545	0.629
Adjusted R^2	0.163	0.673	0.093	0.157	0.065	0.429
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	120,411	70	98,783	70	98,783	70

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each column marked “Replication” is drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report cross-section estimates of the short-term effects to PRAF relative to G3 and G4 municipalities that received no intervention. Following Galiani and McEwan (2013), we control for: the share of eligible children for PRAF (individuals aged 6-12 who had not completed fourth grade in 2001), share of female population, average age and its square, share of individuals born in the municipality, fraction of *lenca* ethnic group, fraction of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), household size and its square, share of internal migrants, share of international migrants, population density, the adult literacy rate, adult schooling and its square; as well as block fixed-effects. Panel A shows the coefficient on a binary variable that identifies whether the municipality was assigned to receive PRAF, Panel B shows the coefficient on an interaction between block-groups and PRAF, and Panel C shows the interaction between each block and whether the municipality was assigned to receive PRAF.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Table A.10. Minimum detectable effects (MDE) given sample size

	Standard deviation at baseline		MDE	
	G3 and G4	G1 and G2	Unadjusted	Adjusted
<i>A. Young adults</i>				
Years of schooling	0.795	0.841	0.5457	0.2111
At least some secondary studies	0.055	0.056	0.0375	0.0115
Labor force participation	0.050	0.054	0.0343	0.0234
Works outside home	0.051	0.056	0.0349	0.0243
Works in non-farm job	0.108	0.094	0.0740	0.0131
<i>B. Young women</i>				
Years of schooling	0.879	0.840	0.6034	0.2416
At least some secondary studies	0.065	0.062	0.0446	0.0149
Labor force participation	0.083	0.074	0.0567	0.0111
Works outside home	0.083	0.074	0.0567	0.0112
Works in non-farm job	0.130	0.207	0.0892	0.0673
<i>C. Young men</i>				
Years of schooling	0.790	0.915	0.5421	0.1964
At least some secondary studies	0.048	0.052	0.0330	0.0116
Labor force participation	0.054	0.075	0.0369	0.0315
Works outside home	0.057	0.079	0.0388	0.0324
Works in non-farm job	0.090	0.074	0.0619	0.0156

Source: Own calculations from municipal-level panel data from population censuses.

Notes: Minimum detectable effects are shown in units of each dependent variable and show the smallest effect size we can detect given our sample size and baseline standard deviations for each outcome. Unadjusted values are calculated using the formula in Duflo, Glennerster, and Kremer (2007) while adjusted values multiply this value times $(1 - R^2)$ of an OLS regression on baseline outcomes that includes all available covariates in 2001 and block fixed effects with cluster-robust standard errors by municipality .

Table A.11. Differential changes in young adult population, PRAF municipalities

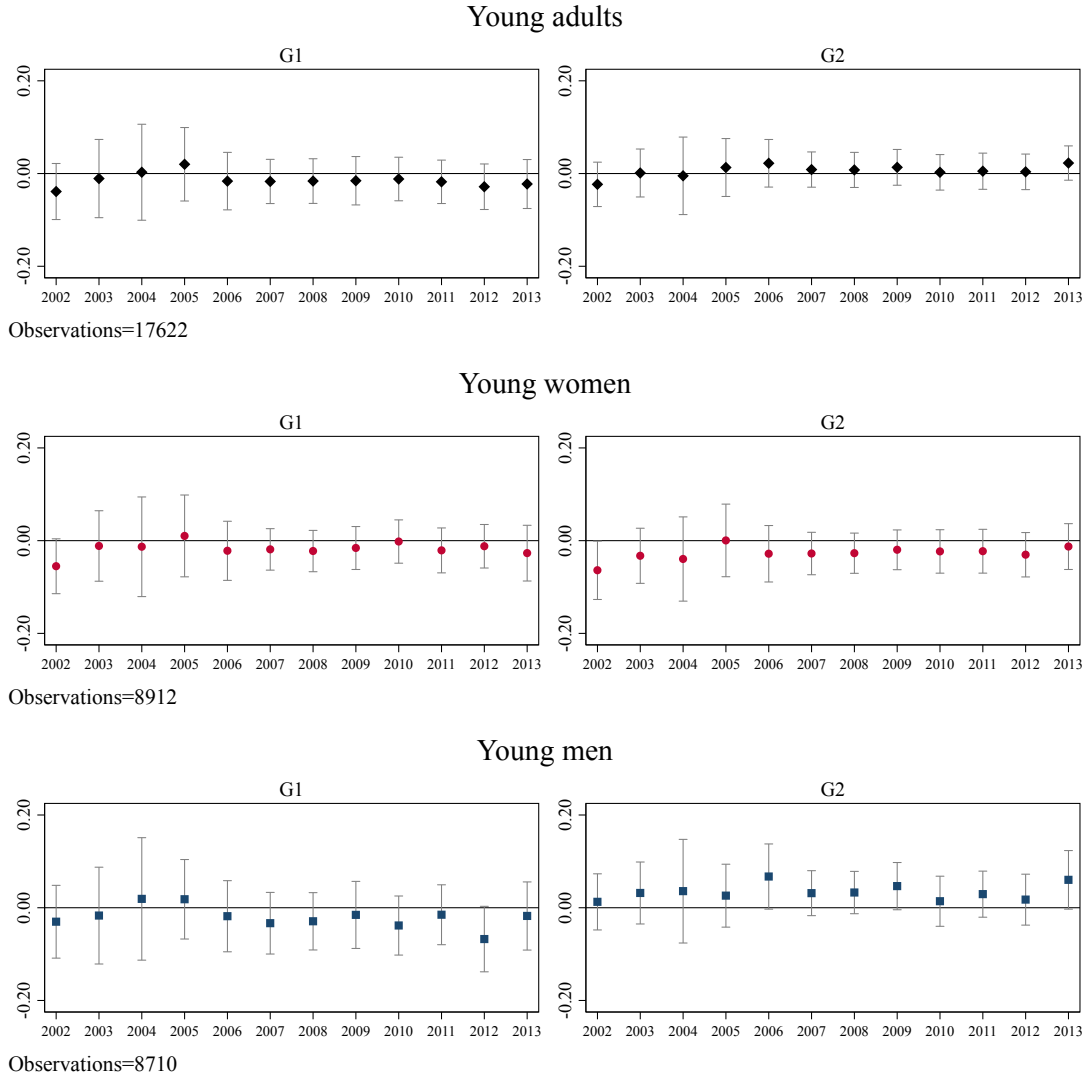
	Dependent variable: Log population size		
	Young adults	Young women	Young Men
G1: +2 years of demand	0.073 (0.046)	0.086 (0.052)	0.061 (0.044)
G2: +2 years of demand and 3 years of supply	-0.047 (0.039)	-0.021 (0.042)	-0.071 (0.041)*
p-value G1=G2 (Cluster-robust standard errors)	0.031	0.086	0.014
p-value G1=G2 (Wild bootstrapped standard errors)	0.529	0.547	0.505
Adjusted R^2	0.876	0.861	0.876
Observations	140	140	140

Source: Own calculations on municipal-level panel data from Honduran population censuses.

Notes: Each set of group coefficients are drawn from a separate regression. Cluster-robust standard errors by municipality in parentheses. We report Difference-in-Differences (DID) estimates of the long-term effects of exposure to PRAF relative to support from the Poverty Reduction Strategy (see Section 3 for details). All regressions control for time-varying municipal-level variables: the share of female population, average age and its square, share of individuals born in the municipality, fraction of the *lenca* ethnic group, share of urban population, access to basic services (electricity, water, and sewage), a household asset index (car, refrigerator, computer, and television ownership), average household size and its square, share of internal migrants, share of international migrants, population density, the relative schooling and labor force participation of adult females relative to males (aged 30-77) to account for intergenerational persistence in human capital and labor market outcomes, as well as overlap with the PIPS CCT program. All regressions include municipality fixed effects, a secular time trend, and block-specific time trends.

*** Significant at 1 percent, ** 5 percent, and * 10 percent.

Figure A.1. Differential migration rates for cohort exposed to PRAF, 2002-2013



Source: Own calculations on repeated cross-sections of household surveys from 2001-2013.

Notes: Estimates are obtained from a difference-in-difference regression: $M_{imt} = \alpha + \beta_t^g (PRAF_g \times \delta_t) + \gamma X_{imt} + \lambda_m + \mu_b \times t + \delta_t + u_{imt}$. M_{imt} is a binary variable that identifies if individual i in municipality m at time t lives in a different community than that of their birth. Thus, we assume that children who were born in PRAF municipalities were living there when exposed to the program. We report estimates of β_t^g for G1 and G2 treatment arms and their 95% confidence interval for 2002-2013, which capture the yearly difference in migration rates between individuals in treatment municipalities relative to the (G3+G4) comparison group in 2001. The regressions control for birth-year effects, municipality fixed effects, linear time trends by stratification block, and survey wave effects. The number of observations used to obtain the estimates is presented below each graph. Standard errors are clustered by municipality and all regressions are weighted using the official survey weights for each wave.



NUEVA

Maestría en Gestión Pública



Título otorgado

Magíster en Gestión Pública

SNIES

106656 Registro calificado: resolución No. 572 del 22 de enero de 2018, por 7 años

Duración

Un año y medio (36 créditos académicos, distribuidos en 3 semestres)

Modalidad

Presencial en Bogotá

Dirigido a

Profesionales con más de dos años de experiencia laboral vinculados al sector público, privado, organizaciones no gubernamentales y sin ánimo de lucro

La Maestría se ofrecerá en sesiones presenciales y virtuales, que permitirá cursarla desde cualquier lugar del país

Maestría en Políticas Públicas



Título otorgado

Magíster en Políticas Públicas

SNIES

90798 Registro calificado: resolución No. 2056 del 17 de febrero de 2015, por 7 años

Duración

2 años (42 créditos académicos, distribuidos en cuatro semestres)

Modalidad

Presencial en Bogotá

Dirigido a

Profesionales sobresalientes que demuestren interés por los asuntos públicos

Maestría en Salud Pública



Título otorgado

Magíster en Salud Pública

SNIES

91281 Registro calificado: resolución No. 20781 del 9 de octubre de 2017, por 7 años

Duración

2 años (44 créditos académicos, distribuidos en cuatro semestres)

Modalidad

Presencial en Bogotá

Dirigido a

Profesionales de diversas disciplinas con interés en aportar a la discusión, el análisis, el diseño, la implementación y la evaluación de las políticas en salud pública

Más información



Escuela de Gobierno Alberto Lleras Camargo
Universidad de los Andes
Carrera 1 No. 19 - 27 - Bloque AUJ tercer piso
teléfono: 3394949 ext. 2073 - Bogotá, Colombia



egob.uniandes.edu.co



fb.com/EGOBUniandes



[@EGOBUniandes](https://twitter.com/EGOBUniandes)

Documentos de trabajo EGOB es una publicación periódica de la Escuela de Gobierno Alberto Lleras Camargo de la Universidad de los Andes, que tiene como objetivo la difusión de investigaciones en curso relacionadas con asuntos públicos de diversa índole. Los trabajos que se incluyen en la serie se caracterizan por su interdisciplinariedad y la rigurosidad de su análisis, y pretenden fortalecer el diálogo entre la comunidad académica y los sectores encargados del diseño, la aplicación y la formulación de políticas públicas.

egob.uniandes.edu.co

[fb.com/EGOBuniandes](https://www.facebook.com/EGOBuniandes) [@EGOBUniandes](https://twitter.com/EGOBUniandes)