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Andrés Hojman
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scl-sph@iadb.org

OPTIONAL: Type Authors name and eMail

Cost-Effective Public Daycare in a Low-Income Economy Benefits Children and Mothers*

Andrés Hojman†
Escuela de Gobierno
Universidad Católica de Chile

Florencia López Boo‡
Inter-American Development Bank

Abstract

This paper evaluates the impacts of a public program that introduced access to part-time childcare centers for children younger than four years of age in poor urban areas in Nicaragua. We explore the effects of this program on several measures of children's and parental outcomes. Our identification strategy exploits the original randomization and the distance to the centers, using Instrumental Variables (IV) and Marginal Treatment Effects (MTE) methods to tackle imperfect compliance with the original treatment assignments. We present a theoretical model to rationalize our IV assumptions. We find a positive impact of 0.35 standard deviations on personal-social skills, and an impact of 14 percentage points on mothers' work participation. We also find suggestive evidence that quality greatly matters for the impacts at the child level, but not at the mother level.

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†andreshojman@uc.cl, Vicuña Mackenna 4860, Santiago, Chile

‡florencial@iadb.org, 1300 New York Avenue, N.W. Washington, D.C. 20577, USA.

1 Introduction

Gaps in cognitive development between children in rich and poor households can be identified at an early age and widen as children grow, as shown by studies in the United States and several countries in the Latin American and Caribbean region (LAC, from here on). Heckman (2008) shows that the difference in the cognitive development scores of 3-year-old children based on the mother's educational level (university education vs. less than secondary education) is close to 1.5 standard deviations, a difference that persists until at least 18 years of age. Verdisco et al. (2016) find a difference of 0.5 standard deviations in language and cognitive development when comparing children between the ages of 24 and 59 months from the richest quintile and the poorest quintile in four LAC countries (Nicaragua, Costa Rica, Peru and Paraguay). Similar results are found by Schady et al. (2015) when comparing the results of the Peabody Picture Vocabulary Test by wealth quartile in rural areas in five LAC countries.

There is substantive evidence of the effectiveness of early childhood education interventions in developed countries. The literature shows that center-based child care can have sustained benefits among poor children, and when the quality of the center-based care is high (Almond et al., 2018; Duncan and Magnuson, 2013; Elango et al., 2015). The strongest evidence comes from small-scale pilots, targeted to disadvantaged populations and with high-quality interventions such as the Perry Preschool Program (Heckman et al., 2010; Schweinhart et al., 1993; Schweinhart, 2005) and the Abecedarian program (Barnett and Masse, 2007; Campbell et al., 2012, 2014).

There is also evidence of benefits from large-scale interventions as Head Start, a nationwide program reaching almost one million poor children in the United States (Carneiro and Ginja, 2014; Kline and Walters, 2016), as well as public government programs in Spain, Germany and Norway (Felfe et al., 2015; Felfe and Lalive, 2018; Drange and Havnes, 2019). Child care has also been shown to have no effect (Carta and Rizzica, 2018) or even negative effects on some children, as shown by recent studies of high quality childcare for 0-2

year-olds in Bologna (Fort et al., 2016), Quebec (Baker et al., 2008, 2015; Kottelenberg and Lehrer, 2017), the US (Herbst and Tekin, 2010), and Denmark (Gupta and Simonsen, 2010), and for universal child care in Norway (Havnes and Mogstad, 2015). Negative effects are more likely when the children attending daycare are not necessarily poor (as in the Quebec, Norway and Bologna studies), so the quality of the alternative care is comparatively high.

Center-based child care programs are usually implemented with the dual objective of promoting child development and increasing female labor market participation, which remains an important policy goal, especially in LAC, where the women's labor market participation rate is still around 30 percentage points lower than men's (Novta and Wong, 2017).[1] For most children in LAC, the alternative to center-based care is home-based care provided by parents or other relatives (including older children) or informal care by neighbors. The quality of the home environment varies by wealth: children in wealthier households are more likely to receive nutritious food, cognitive stimulation, and emotionally supportive care than children in poorer households (Berlinski and Schady, 2015; Elango et al., 2015). Hence, if the center-based care is of relative high quality, moving a poor child from home care to daycare will improve her environment. However, even though access to publicly provided daycare is rapidly increasing in LAC, younger children and children from poorer/less educated households are less likely to participate (Berlinski and Schady, 2015). Moreover, center quality tends to be deficient (Lopez-Boo and Ferro, 2019; Berlinski and Schady, 2015; IADB, 2019).

To the best of our knowledge, this paper presents the first-ever experimental study of the effects of a publicly provided day care program in a developing country.[2] This evidence is relevant for current policy debates in the LAC region, because despite large increases in access, the quality of daycare has not

[1] A revision on the impact of daycare on female labor market participation for developed countries is provided in Baker et al. (2008) and Baker et al. (ress). For LAC, Paes de Barros et al. (2011), Rosero and Oosterbeek (2011) and Berlinski et al. (2011) (although for preschool) found positive effects on female labor market participation due to increased attendance in the Brazil, Ecuador and Argentina, respectively.

[2] The program reached about one third of 0-4 year-old children living in the poorest urban areas of Nicaragua.

been prioritized in the public policy agenda. This contrasts with the evidence (primarily from developed countries) suggesting that quality is a key driver of positive program effects on child development.

Convincing evaluations of the impact of center-based child care on child outcomes in developing countries are scarce. Behrman et al. (2004) and Bernal and Fernández (2013) evaluate the impact of daycare by comparing children with differential exposure in Bolivia and Colombia, respectively. They find positive effects of daycare attendance for some groups, but not others. Rosero and Oosterbeek (2011) use a regression discontinuity strategy to evaluate the impact of daycare provided by local governments or NGOs in Ecuador; they conclude that children who attended daycare had substantially worse outcomes than those who did not. Noboa-Hidalgo and Urzua (2012), also find negative effects on socioemotional development for young children in Chile. None of these evaluations are based on random assignment, however.

Nicaragua's Programa Urbano (PU) was a center-based early childhood development program targeting families living in extreme poverty in the country's poorest urban neighborhoods. The evaluation design randomly assigned centers (Centros Infantiles Comunitarios, or CICOS from now on) at the neighborhood level. This paper identifies the impact of attending the centers on two important outcomes for breaking the intergenerational transmission of poverty: child development and maternal work. Our identification strategy exploits the original randomization and the distance to the centers, using Instrumental Variables (IV) methods under a Marginal Treatment Effects (MTE) framework to tackle imperfect compliance with the original treatment assignments. We present a theoretical model to rationalize our IV assumptions. An advantage of our paper compared to Difference in Difference designs that are common in this literature is that we can adjust the magnitude of our impacts by participation, which is especially important when comparing impacts with previous literature and across subgroups.

Our IV estimates find statistically significant effects of about 0.35 standard deviations on children's social development and an increase of about 14 percentage points on mothers' work participation. We do not reject the hy-

pothesis that these impacts are homogeneous across the unobserved resistance to participate in the program. The impacts are robust to different econometric specifications. Our main effects are stronger among families with lower wealth and among older children, in line with previous findings from this literature. When we adjust our estimations with a measure of quality applied by the CICO supervisors, results strengthen and we also see impacts on fine motor skills, a proxy for cognitive development. These findings imply that a public program, implemented at a very low cost per capita (92 USD), can have a substantial positive impact on a very disadvantaged population, at least in the short term and in two important variables such as maternal labor force participation and children's personal-social development. Findings show that the sole impact on the former would fully pay for the program.

Section 2 presents a theoretical framework. Section 3 discusses the context and the program we evaluate. Section 4 explains how the program assignment was designed for evaluation purposes. Section 5 describes the data. Section 6 discusses the IV and the MTE methodologies. Section 7 shows our main results. Section 8 concludes.

2 Modeling How Distance Changes the Decision to Participate on a Free Child Care Center

Most of the theoretical literature examining the demand for child care focuses on the effect of price on parents' decision to utilize child care. However, in our setting (and in many other countries) public child care centers are free. Thus, the main policy lever of interest is the location of the centers (relative to the families' home). Therefore, we study how families react to exogenous changes in the distance they have to travel to take their child to a center.

We frame this problem from the perspective of mothers, as they are by far the most common primary caregiver for the children in our sample. Let the mother maximize a utility function increasing in consumption, C, the level of human capital of the child, K, and her total time at home, H. H can be used for leisure, for taking care of the child and for non-market work. Possibly,

more than one of those activities can be performed simultaneously. The mother and child both have time constraints. Each of them have the same total time available in the day. The mother can use her time at home, doing market work, $L \in \{0, 1\}$, of commuting to the child care center. D is a binary variable indicating the take-up of child care. The number of hours the child spends at the center (if attending) is γ, which is the same for all children and the same duration as a full-time job day for the mother. The cost in time of commuting to the child care center is δ. The total number of hours of work is also fixed: all mothers who choose to work have to do so for γ hours. The child always has to be taken care of by someone, be it the mother at home, the mother while commuting or the child care center staff. If the mother decides to work, she gets a salary of ω per hour worked. The mother cannot work if she does not use child care. However, she can choose to send the child to the center even if she is not working. She might also have access to an exogenous income, V. Finally, the human capital of the child depends on whether the child attends child care, D and the total time spent with the mother, M, including the time they commute together and the time they spend together at home (which could be less than the time the mother spends at home). Then, the mother faces the following problem:

$$\max_{C, D, L, H} \quad U(C, K, H) \tag{1a}$$

$$\text{s.t.} \quad \tau = H + \delta D + \gamma L, \tag{1b}$$

$$\tau \leq H + \delta D + \gamma D, \tag{1c}$$

$$C = \omega \gamma L + V, \tag{1d}$$

$$K = K(D, M), \tag{1e}$$

$$M = \tau - \gamma D \tag{1f}$$

The skill production function can be expressed as depending solely on child care hours, as any increase in child care hours implies a decrease in time with the mother. Thus, the problem can be restated as depending only on the child

care and the employment decisions. The utility of a mother who uses a positive amount of child care and works is:

$$U\left(\omega\gamma + V, K(\gamma, \tau - \gamma), \tau - \gamma - \delta\right) \tag{2}$$

The utility of a mother who uses a positive amount of child care and does not work is:

$$U\left(V, K(\gamma, \tau - \gamma), \tau - \delta\right) \tag{3}$$

Finally, the utility of a mother who does not use child care is:

$$U\left(V, K(0, \tau), \tau\right) \tag{4}$$

We now discuss the interpretation of our study parameters in this theoretical framework. As mentioned, we define treatment as the (random) provision of free child care centers that are closer to the family's home. All families can go to a CICO in another neighborhood, but treatment families have the advantage of having a center in their neighborhood. We model the treatment assignment as changing δ, the commuting cost.

Based on the theoretical model, we make two points that will be helpful in the discussion of the identification assumptions. First, the model shows that all families should be less likely to use child care if they live farther away from a child care center: δ reduces the utility of the two options with child care, and it does not factor into the utility of the mothers who do not use center child care. This is a theoretical justification for our *monotonicity* assumption.

Second, the model also shows that changes in the distance from the child care center will only affect the child's human capital through changes in participation: δ is not part of the equation for K. It only affects K through changing the probability of participating in child care. This provides a justification for the exclusion restriction.[3]

[3]In addition to reducing the commute time, another way that distance to CICO could affect CICO participation is through peer effects/changes in social norms, i.e., if I see my neighbor taking his kids to the CICO, I think I should take mine, too. In this model, distance does not affect the treatment intensity because maternal time commuting is equivalent to maternal time at home ($M = \tau - \gamma D$). In a model where commuting time was wasted, distance would enter K even

Finally, the model helps us interpret the effect of child care on the child's human capital, which is one of our main outcomes of interest in the paper: each hour of child care takes away one our of mother-child interaction. While this is an extreme case, because usually the crowding out of parental time is likely to be less than one, our estimates should be interpreted considering that the total stimulation at home is not being held constant when participation in the program changes. If the model included the alternative of informal care, our empirical parameters would have to be interpreted as a mixture of the impact of the centers when the counterfactual is parental care and the impacts when the counterfactual is informal care (Heckman and Vytlacil, 2007).

3 Context and Description of Programa Urbano

Nicaragua is a relatively poor country. In 2014, the second year of our evaluation, around 52.9% of the population had an income below US$5 a day. According to that measurement, it was the second poorest country in the Americas.[4]

3.1 Target Population

PU was implemented in the 14 municipalities in Nicaragua with the highest number of extremely poor households.[5] Subsequently, 39 evaluation units (EUs) were selected. The EUs are geographical units, no larger than a neighborhood, defined for evaluation purposes as a potential catchment area for a center. Each EU was required to have a plot of land on which to build a CICO. EUs were then randomly assigned to treatment and control conditions, as described in the next section.

Table 1 shows the characteristics of the PU target beneficiaries in comparison with the overall urban and national population of Nicaragua. The PU population is roughly similar to the national urban population. The main exception is the percentage of children attending early education programs, where

conditional on D, and the exclusion restriction would not hold.

[4] "Sociometro IADB". Based on data from Harmonized Household Surveys from Latin America and the Caribbean.

[5] They were identified based on an unsatisfied basic needs index

Table 1: Family Demographics and Coverage of Early Childhood Services

	Programa Urbano population (a)	Urban Population (1)	National Population
Chronic malnutrition, children 0-5	11%	12.8% (c)]	17.3% (c)
Female labor market participation	54% (a)	51% (c)	61% (c)(2)
Children 0-3 in center care programs	26% (a)	3% (b)	3.3% (b)
Households in extreme poverty	N/A (3)	5.6% (e)	14.6% (e)
Households below poverty line	N/A (3)	26.8% (e)	42.5% (e)
Number of household members	5.2 (a)	4.4 (c)	4.5 (c)
Permanent floor (%)	57%	78% (c)	62.1% (c)
Mother completed secondary educ.	31.9%	50.7% (c)	42.5% (c)

Sources: (a): PU baseline data; (b): Propuesta de Préstamo Programa Urbano de Bienestar para la Niñez IDBDOCS# 2266146; (c): ENDESA 2011-12 (Gobierno de Nicaragua, INIDE); (d): Índice Bienestar Niñez y Adolescencia 2010 (Gobierno de Nicaragua, INIDE); (d): EMNV 2009 (Gobierno de Nicaragua, INIDE)
Table Notes: (1): Refers to all urban census tracts, per administrative data (which overlaps with the PU sample, but is a bit broader); (2): Female labor market participation refers to the % of women of childbearing age who were employed within the last 12 months; (3) PU baseline data did not include comprehensive consumption or income data. The overall poverty rate (i.e., the % of households in poverty) is lower in urban areas than it is in rural areas (26.8% compared to 42.5%). However, of the population living in poverty, a substantial proportion (35%) live in urban areas (64% live in rural areas)

the PU population had a clear advantage because at the time of data collection some of the PU CICOs had already been constructed. The differences in permanent floor and maternal education are probably due to the geographical targeting of the program.[6] We consider this as suggestive evidence of the external validity of our study, in terms of covering a representative population.

3.2 Services Delivered by Programa Urbano

The PU CICOs operated for half a day, five days a week, providing care to children ages 1 to 4. The curriculum was based on the national curriculum for children from birth to age 3, which promotes the areas of personal and social development, communication, and understanding of the child's surroundings. [7] As part of PU, the CICOs adopted updated guidelines for educator/child ratios, requiring one educator: (i) for every 8 children younger than 1 year of

[6]See Section 5 for a discussion on the characteristics of the available data.

[7]In addition to early childhood education, micronutrients were distributed to children ages 6 to 24 months. Given the low proportion of children in this age range in our study, we do not focus on this part of the intervention in this study. Families were also occasionally offered family support services from trained social workers, which were primarily designed to link families to available social services. Parenting workshops were also periodically provided. However, the support visits and the workshops were infrequent and are not well documented.

age; (ii) for every 8 children between 1 and 3 years old and; and (iii) for every 18 children between 3 and 4 years old. Several of the families also received home visits while waiting for the centers to be implemented. The cost of the program is estimated to be $92 dollars per child per year.[8]

4 Evaluation Design

4.1 Program Assignment Process

The 39 EUs were randomly assigned to treatment and control conditions. EUs assigned to the control condition were expected to be phased into the program, receiving CICOs one to two years following the construction of CICOs in intervention EUs. Figure 1 shows that assignment to intervention was very effective in determining the timing of construction of the CICO. CICOs were built during the first 6 months after random assignment in all EUs assigned to treatment. On the other hand, the construction of centers in the control group started much later. By the end of the intervention, only 6 CICOs in EUs assigned to the control condition had been built. However, given that some of the control EUs also received CICOs at the end of the period, we operationalize participation in CICO in two ways: a binary variable (ever participated) and a months of exposure measure. Section 7.5 shows that our results are qualitatively robust to using any of these measurements.

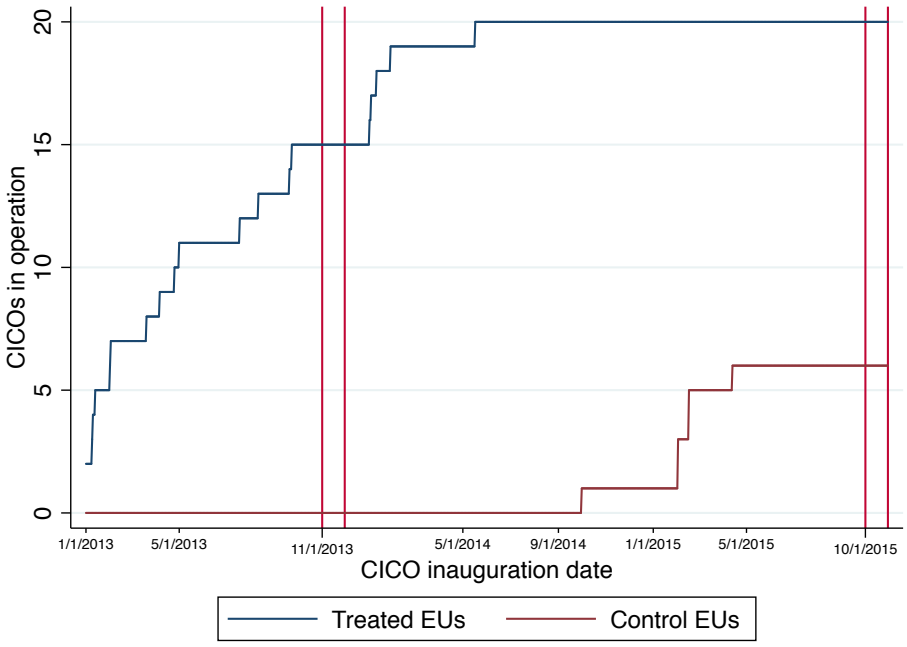
During the time when the centers were being built, the treatment group received some home visits.[9] Some control units also received some home visits, as 6 control EUs were randomly assigned to deliver home visits. However, the visits were not implemented consistently: no household reached even half of the number of visits defined in the program design, and the curriculum was not

[8]Source: Propuesta de Préstamo Programa Urbano de Bienestar para la Niñez IDBDOCS# 2266146. Includes $20 child/year for snack offered at CICOs and micronutrients.

[9]A second round of randomization, with 40 additional units, was later implemented to evaluate the effect of the visits. It intended to randomize EUs between visits and control. We do not use the data from this second round in the evaluation, as the randomization was highly compromised: 5/20 of the new "treatment" units had already been non-randomly assigned to centers. Moreover, an error in the questionnaire implied that the information on home visits was not available for children attending centers.

followed by the program staff (Castro and Laguna, 2016). We discuss the home visits and include them explicitly in our estimates in Section 7.5.

Figure 1: Timing of Construction of the Centers



The lines represent the percentage of EUs that had a center constructed in the date shown in the x-axis, by original randomization assignment. The first pair of vertical lines are showing the range of the timing of the baseline data round. The second pair are showing the range of the timing of the final data round.

5 Data

In 2013, baseline data were collected from 1,726 children from birth to 4 years in the 39 EUs. Information was collected on household and primary caregiver characteristics, and measures of four main areas of child development using the CLAP instrument. As shown in Figure 1, at the moment of the baseline survey some EUs had already begun to participate in the CICO. We still use some of the baseline variables as controls, given that Table 3 suggests they

were not affected by that small exposition to the treatment.[10] At the end of 2015 a second round of data collection was carried out, reaching 1,442 children from the baseline sample. This round of data collection included two child development instruments: the CLAP and the Denver II, the latter of which was administered to a randomly selected subsample of 734 children ages 24 to 72 months. The second round of data collection also included measures of the quality of the child-parent relationship, parental investments (based on the quality of time spent together), and parental knowledge about child development. The follow-up survey also included information regarding the mother's labor market participation and current education status (whether she is studying), and maternal depression. We use these outcomes as the main dependent variables in our analyses. The follow-up survey also includes information used to construct our two measures of CICO participation: a binary indicator describing whether or not the child attended a CICO, and a continuous variable measuring exposure to CICO as the number of months spent in a center. Finally, the households were georeferenced, so we can construct travel time from them to each of the centers.

Out of 1756 children interviewed at baseline, 16% could not be reached at follow-up. The level of attrition cannot be distinguished statistically between the control group (17.4%) and treatment group (15.5%). The p-value of a difference in means test across both groups is 0.28.[11]

We also use observational measures of the structural and process quality of the CICOs, collected in 2015 by trained supervisors. The measures used are loosely based on internationally validated instruments, such as the ITERS and CLASS (Lopez Boo et al., 2016). Scores are based on an average of several observations conducted by supervisors observing different educators in the same CICO over the course of the year. We use this measure of quality to estimate the effect of CICO quality on families' decisions to utilize CICOs, and in the impact of CICOs on child development and caregiving practices/knowledge.

[10] Section 7.5 shows that our main results barely change when we estimate with no control variables.

[11] In Section 7.5 we show that our results are robust to an Inverse Probability Weight attrition correction.

5.1 Characteristics of the Children and the Households

Table 2 presents baseline socio-demographics, child development, and caregiver characteristics from the baseline data but only for children observed both at baseline and followup. We use several indices as outcomes (in bold) that do not have a readily interpretable scale, so we present select index items in order to give a more meaningful depiction of the baseline situation of the sampled children and their families. Mothers in our sample are young, at only 26 years of age on average at baseline, with 7.9 years of education on average. The biological father lives in the same household in 66% of the cases. Some of the items in the indices are also worth noting: although mothers claim to spend 1.9 play hours with the children every day, only 36% of them play with books. Few children seem to have abnormal disruptive behaviors. Finally, about 8% of the parents admitted spanking their children, and a quarter of them admitted hitting them with some type of object.

5.2 CICO Participation and Length of Exposure

Figure 2 shows the individual-level take-up of benefits by the EU-level assignment. As expected, in EUs assigned to CICO, more families participated in CICOs than in control group neighborhoods. The average number of months of attendance, among those who attended at least one month, was 13.1.

5.3 Description of Child Development and Caregiver Outcomes

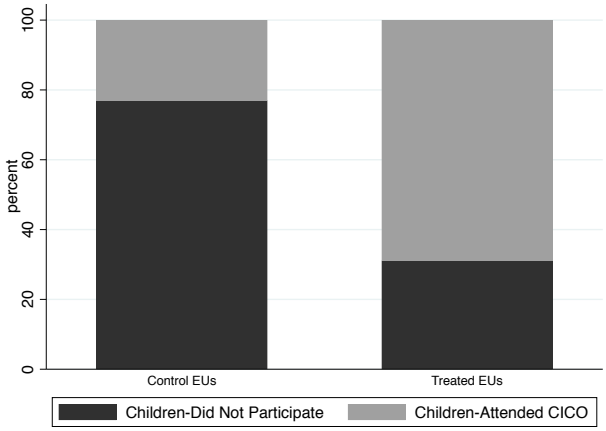
We analyze two development test batteries: the Denver II Test (Frankenburg et al., 1990, 1992) and the CLAP test Martell et al. (1985). The Denver II Test includes four areas: personal-social, language, fine motor, gross motor. The CLAP test also has four areas, closely corresponding to the areas in Denver: social, language, coordination and motor. Both have pros and cons: the Denver is a more rigorous test, which evaluates the child by direct observation, but it is costly to apply, so it was only applied to a random subsample of the surveyed children. The CLAP is a screening instrument that has been used widely in

Table 2: Sample Statistics at Baseline

	N	Mean	SD	Min	Max
Age in Months	1441	23	14	0	48
Sex	1442	.51	.5	0	1
Mother's Age	1442	26	6.7	15	44
Mother Works	1359	.43	.5	0	1
Mother Studies	1359	.035	.18	0	1
Mother's Years of Education	1442	7.9	4.1	0	16
Father's Age	954	31	9.3	18	62
Father Works	953	.94	.24	0	1
Father Studies	953	.0031	.056	0	1
Father's Years of Education	938	7.7	4.1	0	16
Father at Home	1442	.66	.47	0	1
N Child under 6 in Hhld	1442	1.5	.67	0	5
CLAP Social Baseline	1440	-.0014	.98	-4.2	.27
CLAP Language Baseline	1440	.0082	1.2	-5.3	33
CLAP Fine Baseline	1440	.031	.89	-7	.14
CLAP Motor Baseline	1440	-.011	1	-6.4	.16
Parental Investments Baseline	1442	.0066	1	-2.3	2.5
Interactions (Times/week)	1435	2.9	1.5	0	5
Plays with Books	1438	.36	.48	0	1
Family Play Hours/Day	1435	1.9	1.6	0	6
No Disruptive Behavior Baseline	713	.002	.99	-4	1.1
Is Not Excessively Fearful	713	.89	.32	0	1
No Excessive Temper Tantrums	713	.49	.5	0	1
Treats Friends Well	712	.89	.31	0	1
No Sadness (week)	713	.85	.36	0	1
Treats Pets Well	691	.87	.34	0	1
No Harsh Discipline	1442	-.011	1	-5.8	.67
Spanking	1442	.078	.27	0	1
Punching	1442	.081	.27	0	1
Hits with Objects	1442	.25	.43	0	1
Isolation	1442	.0035	.059	0	1
Shaking	1442	.0083	.091	0	1
No Depression Baseline (CES-D)	1442	-.01	1	-2.2	1.1
Knowledge of Child Development Baseline (KIDI)	1442	.0031	1	-5	1.6

Notes: The bolded rows are some of the indices we use as outcomes. The items below each of them are the items that compose it. We do not show the items for KIDI and CES-D because there are proprietary, and because both tests are commonly used. KIDI is a measure of knowledge of basic questions about normal child development patterns. For variables Mother's Age, Father's Age, Mother Education, Father Education, and Family Play Hours/Day we show percentiles 1 and 99 of the distribution rather than Minimum and Maximum because a few people report non-sensical values. The CLAP test scores and the indices are constructed as simple sample-standardized sums of the items.

Figure 2: Participation in CICO: Random Assignment of EUs vs. Actual Family Participation



The left bar includes all children in control EUs. Among them, only the ones in grey attended CICO, around 20%. On the contrary, most of the children in the right bar, depicting children living on treated EUs, attended.

Nicaragua. It encompasses the same four areas of development as the Denver. It is simpler and less precise than the Denver, but it was applied to all surveyed children. The assessment is done by a combination of child observation and parent-reported information.

We also include a measure of disruptive behavior, using caregiver-reported information regarding a range of atypical behavior indicative of externalizing and internalizing behavior problems. This includes things like having more than 10 temper tantrums during the last week, being excessively scared, and not treating animals or peers well.

We measure different forms of parent-child interaction.[12] Parental investment includes parent-child play time, reading and singing time, all of which are widely considered important inputs for the long-term development and well-being of the child in the literature.[13] Second, we present a measure of parental harshness. It is an inverted (to make higher numbers indicate less harsh disci-

[12]Based on the Family Care Indicators (Frongillo et al., 2003)

[13]The index includes: (i) an indicator of whether the child plays with books; (ii) how many hours a day the parents play with the child; and (iii) how many times the parents told stories or sung with the child.

pline) sum of violent disciplinary actions taken by parents, going from shouting (the mildest item) to hitting children in extremely dangerous ways. Finally, we measure parental knowledge about child development. We use the KIDI inventory, a questionnaire about knowledge of child development (MacPhee, 1981). The test has an answer key based on the population timing of normal developmental milestones and on scientific research, and the final score is simply a sum of the the right answers.

We also present three main outcomes pertaining to the child's mother. First, we consider a test of maternal depression, the CES-D. It has several questions taken from standardized maternal depression questionnaires regarding the mother's experience of various symptoms of depression in the last week.The scale is inverted to make higher numbers represent lower levels of depression. The total score is the simple inverted sum of the items. We also consider mother work status and mother studying status, both binary variables. These variables are widely used in the literature, considering the potential of center-based child care to incentivize mothers' to work outside of the home (or study).

6 Methodology

6.1 The Evaluation Problem and Instrumental Variables

Ultimately, our study aims to identify the causal impact of attending CICOs. However, CICO participation was voluntary, so there is an endogeneity problem: there could be unobserved characteristics of the households that are associated both with program participation and our child development and maternal outcomes of interest. This creates a consistency problem if we attempt to identify the effect of CICOs using just the statistical relation between outcomes and program participation.

PU was designed to overcome this problem by randomly assigning treatment status. Through randomization, EUs were assigned to treatment and control status, and this initial (randomized) assignment is a strong determinant of participation in CICOs. But compliance was not perfect at the EU or individual family/ child level. Some control EUs received CICOs by the end of the study,

children in untreated EUs attended CICOs, and many children in treated EUs did not attend CICOs. Thus, we cannot just compare groups of children living in the treated EUs with groups of children living in the control EUs. Instead, we leverage the initial randomization by using an instrumental variables (IV) estimation under a Marginal Treatment Effects framework to obtain the causal effect of participation in CICOs on child development and maternal outcomes. In our main estimates, we combine the randomization and variables indicating travel time to the nearest center as our instruments. In Section 7.5, we show that we obtain similar results if we only use the randomization as an instrument.

6.2 Methodology for Estimating the Effects of Participating in CICO

The MTE framework was developed across a series of papers (Björklund and Moffitt, 1987; Heckman and Vytlacil, 1999, 2005; Heckman et al., 2006), and our analysis draws on their developments. In this section we follow the notation in Heckman et al. (2006). We use a choice model setup to frame our estimations. In practice, we estimate main effects using regular IV and MTE. As our sample is not very large, it is important to assess the degree of precision of our estimations, and the IV estimations assuming treatment effect homogeneity help in this way by providing an easily interpretable, simple parameter. Using IV estimates as our benchmark estimate also has the advantage that we can test for the weakness of our econometric identification using well-known tests. We delve on the heterogeneity in the effects of the program, both in observables and in unobservables using estimations of the MTE. These estimations allow us to present a richer interpretation our IV estimates. At the same time, we are interested in understanding how people self-select into the program, and the MTE approach delivers unique information to shed light about this process.

The MTE estimates are interpreted using a standard selection model. We start by presenting a switching regression and potential outcomes model. We assume additive separability between the unobserved and the observed components of the outcome and linearity of the effects of the covariates on the potential outcomes to make estimation more reliable. Let D be participation

in a CICO, Y be the outcomes, X be a vector of controls and Z be the instruments. Y_1 is the potential outcome in the treated state, and Y_0 in the untreated state. U_j is the unobservable component of the outcome in treated state j.

$$Y = DY_1 + (1 - D)Y_0 \tag{5}$$

$$Y_1 = X\beta_1 + U_1 \tag{6}$$

$$Y_0 = X\beta_0 + U_0 \tag{7}$$

$$D = 1[P(Z) > U_D] \tag{8}$$

We are interested in identifying summary statistics of the treatment effect, $Y_1 - Y_0 = X(\beta_1 - \beta_0) + U_1 - U_0$. The choice equation is given by a threshold-crossing model. People will choose to participate if their unobserved resistance to the treatment is lower than their observed propensity score. As long as the distribution of the unobservables is continuous, this model can be derived without loss of generality from a standard threshold-crossing model, as shown by Heckman and coauthors.

We are interested in understanding the relationship between the unobservables determining participation and the unobservables determining impacts. The MTE represents the value of the treatment effect of the program for a specific value of the unobservable determining participation. As the effect is identified for individuals at the margin of indifference between participating and not participating, it is useful to write the value of the unobservable in terms of the value of the propensity score.

$$\Delta^{MTE}(x, u_D = p) = E[Y_1 - Y_0 | X = x, U_D = P(Z)] \tag{9}$$

$$= x(\beta_1 - \beta_0) + E[U_1 - U_0 | U_D = P(Z)] \tag{10}$$

Then, the potential outcomes can be identified from the observed outcomes. Thanks to index sufficiency, conditioning on Z is equivalent to conditioning on

$P(Z)$. Keeping X implicit, the observed outcomes can be written as:

$$\begin{aligned} E[Y|P] &= E[Y_{1i}|P, D=1]P + E[(Y_{0i}|P](1-P) = E[Y_{1i} && (11) \\ &= X\beta_0 + E[U_0] + X(\beta_1 - \beta_0)P + E[U_1 - U_0|D=1]P && (12) \end{aligned}$$

Thus, the MTEs are identified by calculating the derivatives of the observed outcomes model with respect to the propensity score:

$$\frac{\partial E[Y|P(Z)=p]}{\partial p}\Big|_{P(X,Z)=p} = X(\beta_1 - \beta_0) + E[U_1 - U_0|U_D = p] \quad (13)$$

For IV to work, we need our instruments to satisfy two conditions: the exclusion restriction and the relevance condition. The exclusion restriction requires that the outcome of each individual is affected by the randomization only through the change in the probability of attending CICO. The randomization did not change anything in the neighborhoods besides the positioning of the child care centers, so we assume that the assignment instrument is only related to the outcomes of interest through participation in the centers. The distance instrument could raise suspicion. The program searched for plots to build the CICO all over the neighborhoods, and we have no reports from the program indicating that the location of the land lot within a neighborhood was special in any relevant way. They were not known to be located centrally or in poorer parts of the neighborhoods or closer to any relevant infrastructure. To reinforce this argument, Table 3 shows that the distance to the land lot does not determine any relevant baseline characteristic of the households besides what could be expected if distances were allocated as good as randomly across households. Moreover, Table 9 compares estimations using the randomization as a single instrument. We find that the estimates are very similar across both specifications, with the exception of a slightly stronger impact on Mother Works when including the travel time instruments. The relevance condition is clearly satisfied: our Kleinberger-Paap statistics are close to 90, and easily beat all the usual thresholds for weak instrument tests. The monotonicity condition is discussed in section 2. We do not see a strong argument to believe that there could be defiers for either the randomization or the distance.

6.3 Baseline Balance Tests

Our methodologies assume that our instruments do not determine outcomes in any way other than by changing the probabilities of participation into the program. While that assumption is not directly testable, we can assess whether our instruments are related to substantive variables that should not be changed by the programs. If the instruments are systematically related to important outcomes at baseline, this would pose a threat to our assumption that the instruments will not be directly related to those same outcomes at follow-up. To address this problem, Table 3 replicates the specification that we use for our main estimates, but using baseline variables as the outcomes. If our assumptions are correct, baseline variables should only be determined by our outcomes by chance. The table shows the results of a regression of the baseline variable on our prediction of participation in the program, estimated using all of our instruments: the randomization and the distance indicators. This is a multi-instrument analog of the standard baseline balance tests. Out of 20 variables we test, we see that only one is significantly determined by our instruments.[14] We take these results as suggestive evidence that our instruments are not systematically related to observable or unobservable characteristics that are also related to our outcomes of interest.

6.4 Choice of Controls

From Table 3, we see that only mother's age is strongly "unbalanced" (seemingly affected by the treatment). We include this variable as a covariate in our analyses. To increase our precision, we also include a set of baseline covariates that are relevant in the literature, both as predictors of participation or as predictors of child development. Our main specification therefore includes mother age, mother education, a wealth index, child sex, child age, mother work, father at home and number of children under 6 years living at the household

[14] For this test, we included all the main baseline variables available, with the exception of father's age and father's education, which were very strongly related to mother's age and mother's education, so they seemed redundant. Indeed, father's Age is "unbalanced" (as is mother's age), while father's education is "balanced".

Table 3: Balance Tests for Baseline Variables

Baseline Variable	N	Beta	t	p
Age	1,432	0.25	0.15	0.88
Sex	1,433	-0.00	-0.03	0.98
CLAP Social Baseline	1,431	0.14	1.14	0.25
CLAP Language Baseline	1,431	-0.05	-0.43	0.67
CLAP Fine Motor Baseline	1,431	-0.09	-1.06	0.29
CLAP Motor Baseline	1,431	-0.02	-0.13	0.90
KIDI Baseline	1,433	0.17	1.12	0.26
Investments Baseline	1,433	0.00	0.01	0.99
No Depression Baseline	1,433	0.05	0.37	0.71
No Disruptive Behavior Baseline	708	0.18	1.20	0.23
No Harsh Discipline	1,433	0.07	0.39	0.69
Mother's Age	1,433	1.52	1.92	0.06
Mother Works	1,352	0.04	0.51	0.61
Mother Studies	1,352	-0.00	-0.11	0.91
Education of the Mother	1,433	0.06	0.05	0.96
Father Works	949	0.01	0.25	0.80
Father Studies	949	-0.01	-1.37	0.17
Father at Home	1,433	0.02	0.24	0.81
N Child under 6 in Hhld	1,433	-0.04	-0.40	0.69
Wealth Index	1,433	0.05	0.13	0.90

Each row of the table presents the results of a 2SLS regression of the baseline variable on our indicator of participation in child care centers and controls. Participation is instrumented by the propensity score. The excluded instruments are the randomization and bins of travel time to the center.

as covariates. In our robustness checks we estimate with and without these covariates.[15]

7 Results

7.1 Analysis of Participation

Table 4: Determinants of the Participation in CICO

	Beta	p-value
Randomization 1	0.41	(0.00)
Time to Center Pctile 1-20	0.35	(0.00)
Time to Center Pctile 21-40	0.29	(0.00)
Time to Center Pctile 41-60	0.17	(0.05)
Time to Center Pctile 61-80	0.14	(0.15)
Mother's Age	-0.0017	(0.48)
Education of the Mother	-0.0051	(0.25)
Wealth Index	-0.013	(0.47)
Sex	-0.032	(0.22)
Age	0.0035	(0.02)
Mother Works	0.039	(0.29)
Father at Home	0.022	(0.50)
N Child under 6 in Hhld	0.017	(0.44)
Observations	1351	

The table presents the results of a Probit regression explaining attendance to CICO. The coefficients we present are the marginal effects calculated at the mean of the variables.

Table 4 shows the estimated coefficients when the participation in a CICO is modeled using a Probit specification. We present marginal effects evaluated at the means of all the variables included. We use all of our control variables and our instruments as potential determinants of participation. The travel time instruments are presented using a flexible specification in which we allow the effect of travel time to be different for groups of people living at different distances from the center. We consider that the quantiles specification we use

[15]These variables had no more than 5% missings each. We impute the missings using the same variables in the follow-up round or linear regression, to avoid losing any observations due to our control variables.

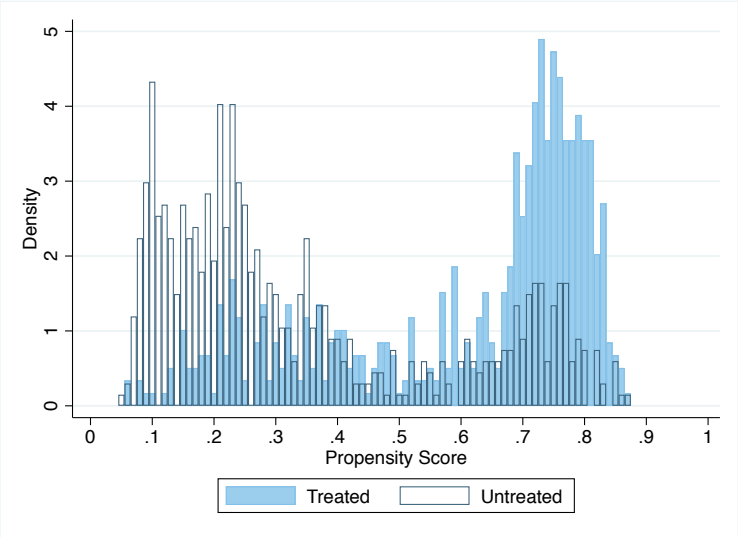
is simple and flexible enough for modeling the effect of travel time. We can see that randomization and distance are both highly effective in determining participation in the centers. Other than the randomization and the travel time indicators, the strongest determinant of participation is the age of the child, with older children being more likely to participate. This is consistent with the literature (Berlinski and Schady, 2015).

7.2 Essential Heterogeneity Analysis

The essential heterogeneity analyses show how the treatment effects are related to the unobserved resistance to taking the treatment. Thus, they complement our analysis of participation, indicating how people self-select into attending child care centers not only based on their observable characteristics, but also based on their expected gains. This is necessary to understand whether a program can eventually have an effect on a population of interest.

We start by showing how the propensity score is distributed across our sample. Figure 3 shows that, when using the randomization and the travel time variables as part of our participation model, we predict propensity scores going roughly from 0.08 to 0.85. Those values correspond to the lower and upper 1% for the individuals in our sample. If we consider the lower 1% for the treatment group and the upper 1% for the control group, the boundaries would be 0.1 and 0.82 instead.

Figure 3: Propensity Score



The propensity score is calculated using the randomization, 4 bins of travel time to the nearest center, and our control variables.

As discussed in Heckman et al. (2006), the IV estimates are not meaningful in some situations. This problem is especially important if participation (treatment take-up) does not monotonically increase with the scalar variable combining the instruments. To avoid this problem, it is possible to estimate a propensity score from the instruments and the controls, and use that as an instrument for our IV estimations. Another problem of IV is that it is usually hard to interpret, as the *compliers* switched by the instrument to participate could be a very particular group. Although the common support of our propensity score does not cover the whole population (so we cannot recover the Average Treatment Effect), it does cover a very wide range of values, which strengthens the *external validity* of our estimates. Thus, we consider the basic IV estimates to be interesting for this case.

To assess whether we should be exploring MTEs for each of our outcome variables, we implement the essential heterogeneity tests in Heckman et al. (2006). The null hypothesis of these tests is that participation in the program, D is not determined by unobservable gains from it $(U_1 - U_0)$. For each of the variables, we present tests using both a normal specification and polynomial

Table 5: Tests of Essential Heterogeneity

Outcome	Normal	Polynomials			
		2	3	4	5
Denver Social	0.58	0.69	0.52	0.68	0.74
Denver Language	0.92	0.08	0.04	0.08	0.04
Denver Motor	0.51	0.81	0.45	0.60	0.69
Denver Fine Motor	0.31	0.52	0.46	0.59	0.67
CLAP Social	0.36	0.18	0.13	0.05	0.04
CLAP Language	0.24	0.40	0.24	0.10	0.12
CLAP Motor	0.40	0.69	0.64	0.29	0.41
CLAP Fine Motor	0.03	0.09	0.18	0.22	0.33
No Disruptive Behavior	0.08	0.24	0.09	0.10	0.17
Not Harsh Discipline	0.14	0.09	0.09	0.13	0.17
Parental Investments	0.68	0.05	0.04	0.07	0.11
KIDI	0.78	0.86	0.89	0.75	0.59
No Depression (CES-D Scale)	0.31	0.62	0.01	0.01	0.01
Mother Works	0.87	0.77	0.58	0.73	0.76
Mother Studies	0.95	0.67	0.82	0.92	0.96

Each value on the table represents the p-value of a test of essential heterogeneity. The null hypothesis for these tests is that there is no essential heterogeneity. The tests are based on the fact that in absence of essential heterogeneity there should be a linear relationship between the outcome and the propensity score. The first column assumes that the errors have a normal biavariate distribution, while the others use polynomial approximation of the control functions.

specifications, going from order 2 to order 5 polynomials. As our sample is not very large, some of the tests are inconclusive, as the p-values we obtain change too much across different specifications. However, we can identify some clear patterns. First, we fail to reject the null hypothesis that there is no essential heterogeneity for the Denver social-individual domain score and the mother works outcome, which are (as we will see) the outcomes for which our impact is concentrated. The same is observed for most of the other outcomes. We do find some evidence that the impacts on the Denver language score, parental investments and maternal depression present patterns of essential heterogeneity, but they are not consistent across all of the different specifications.

We present the estimated MTE charts in the appendix. We are not able to find a consistent pattern of impacts throughout the unobserved resistance

Table 6: Main Results

Outcome	Beta	SE	p	Obs	KP
Denver Social	0.35	0.17	0.05	685	161.1
Denver Language	0.19	0.16	0.23	658	169.9
Denver Motor	0.12	0.14	0.41	654	166.4
Denver Fine Motor	-0.15	0.18	0.39	679	174.1
CLAP Social	0.05	0.14	0.70	1,348	225.3
CLAP Language	0.06	0.13	0.64	1,348	225.3
CLAP Motor	-0.02	0.12	0.87	1,348	225.3
CLAP Fine Motor	0.03	0.10	0.73	1,348	225.3
No Disruptive Behavior	0.11	0.13	0.39	1,351	223.1
Not Harsh Discipline	0.02	0.20	0.93	1,351	223.1
Investments	-0.00	0.18	0.99	1,351	223.1
KIDI	0.08	0.12	0.50	1,351	223.1
No Depression (CES-D Scale)	0.04	0.13	0.79	1,340	217.3
Mother Works	0.14	0.06	0.02	1,207	207.3
Mother Studies	-0.03	0.03	0.29	1,207	207.3

This table presents estimates from our preferred specification. We use a propensity score estimated using (i) the randomization, (ii) 4 indicators of quintiles of travel time to nearest center and (iii) the baseline controls as our instrument. EU-level clustered standard errors used to calculate p-values. Baseline controls: Mother Age, Child Age, Mother Education, Child Sex, Mother Working, Father at Home, Wealth index, Number of Children under 6 in Household. The first column shows the estimated impact from the centers. The second presents the standard error. The third presents the p-value. The fourth is the number of observations available for that outcome in particular. The fifth is the Kleibergen-Paap F-stat.

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to the treatment for any of the variables studied: few of the estimated effects across the distribution of the unobservable are statistically different from zero. Given that our sample size is not very large, this does not necessarily imply that there is no essential heterogeneity. Considering these results, in the main paper we mostly focus on instrumental variables estimates and interpret our findings as the average impacts for the range of individuals for whom we have strong support, while we discuss the MTE results for language in the discussion section.

7.3 Main Results Using Instrumental Variables

Table 6 shows the main results of the paper. We find a significant positive effect on the personal-social dimension of the Denver Test, in the magnitude of one third of a standard deviation. We do not find any effect on the outcomes measured by the CLAP. However, as discussed above, the CLAP instrument is less precise than the Denver.[16] We find no effect on any of the parenting variables (knowledge as measured by the KIDI, discipline or investments and practices). We find a positive but insignificant parameter on children's disruptive behavior, as reported by caregivers. Our measure of disruptive behavior focuses on behaviors that are relatively extreme, so this finding is mostly important for the tail of the distribution. We find no effects on whether or not the mother studies status, but there is an economically important effect on the probability that the mother works. The increase of 14 percentage points is aligned with the developed countries literature (Carta and Rizzica, 2018; Baker et al., 2008), as well as with the literature from LAC: Berlinski et al. (2011) find an increase of about 20 percentage points in labor outcomes of mothers of 4 year-olds attending preschool in Argentina.

Our positive (and large) finding on the social domain of the Denver is consistent with results from children in high-risk populations, such as the Abecedarian Experiment (Campbell and Ramey, 1994) and the Perry Preschool Program (Heckman et al., 2010), which have produced positive effects through improving socioemotional traits. They are also aligned well with those in the literature of developing countries: Bernal and Fernández (2013) and Noboa-Hidalgo and Urzua (2012) find positive effects on social development of children (with magnitudes of 0.30 SD and 0.49 SD, respectively). The lack of impact on motor skills is common in the literature as this is the domain where poorer children do not experience deficits in relation to richer children. However, the lack of impacts on language is inconsistent with other findings in the literature; the cited studies from LAC find positive impacts in cognition (which includes language). The Nicaraguan CICO curriculum emphasized personal and social de-

[16] For instance, CLAP has 2-4 items administered to a child for a given age-range, while Denver has 15.

velopmental domains over cognitive development. Also, the adult/child ratios were relatively low for these age groups, so children may not have interacted effectively with their educators on an individual basis. Both of these facts could explain our lack of impacts on language.

An advantage of using IV is that there are well-known tests to assess whether our instruments are weak: we use the Kleibergen-Paap statistic, which is a cluster-robust statistic (Kleibergen and Paap, 2006). Our KP statistics are extremely high, so we can strongly vouch for our instruments not having a weakness problem, whether we use (i) the Randomization and the travel time bins or (ii) the Randomization on its own.

7.4 Heterogeneity of Impacts Across Observables

Table 7 shows that out of all the subgroups analyzed, the impacts are stronger in low-wealth and in older children. The estimated magnitude of the impacts are usually stable across groups. For example, the impact on Denver Social takes values between 0.27 and 0.44. The impact on Mother Works take values between 0.10 and 0.18.

7.5 Robustness

Table 9 shows that the estimated impacts are robust to different specifications. The significant impacts on Denver social development and mothers' labor market participation have very similar magnitudes and significance when we estimate (i) without controls (No-Controls); (ii) Including the receipt of home visits as a control variable; and (iii) correcting for attrition using Inverse Probability Weights (IPW). The magnitudes mechanically change when we use months of exposure to the treatment (EXP) as our treatment variable, but the impacts are still significant (although in the case of mothers' labor market participation, it becomes significant only at 10%). We also calculate Romano-Wolf p-values, as some of the dimensions we measure are related. In particular, for each instrument we group the social and language subtests, and the motor and fine motor subtests. Our estimated CICO parameters lose some significance with

Table 7: Heterogeneous Impacts

	Low Educ	High Educ	Age 0-2	Age 3-4	Low Wealth	High Wealth	Girl	Boy
Denver Social	0.28	0.42	0.31	0.38	0.41	0.32	0.27	0.44
p	*0.14*	*0.13*	*0.23*	*0.04*	*0.03*	*0.23*	*0.18*	*0.07*
Denver Language	0.17	0.25	0.26	0.19	0.14	0.36	0.24	0.13
p	*0.32*	*0.33*	*0.24*	*0.33*	*0.50*	*0.10*	*0.31*	*0.63*
Denver Motor	0.03	0.28	0.20	0.04	0.05	0.25	0.06	0.18
p	*0.86*	*0.34*	*0.48*	*0.78*	*0.78*	*0.34*	*0.75*	*0.47*
Denver Fine Motor	-0.25	0.08	-0.22	-0.06	-0.13	-0.09	-0.02	-0.28
p	*0.24*	*0.79*	*0.42*	*0.72*	*0.53*	*0.76*	*0.91*	*0.25*
CLAP Social	0.12	-0.11	0.13	-0.02	0.33	-0.36	0.22	-0.12
p	*0.46*	*0.58*	*0.54*	*0.90*	*0.01*	*0.06*	*0.12*	*0.49*
CLAP Language	0.03	0.09	-0.09	0.19	0.05	0.10	0.06	0.08
p	*0.82*	*0.63*	*0.69*	*0.04*	*0.73*	*0.65*	*0.71*	*0.66*
CLAP Motor	-0.12	0.14	-0.15	0.10	-0.02	0.01	0.06	-0.12
p	*0.28*	*0.52*	*0.40*	*0.38*	*0.86*	*0.98*	*0.73*	*0.48*
CLAP Fine Motor	0.01	0.12	0.04	0.03	-0.00	0.11	0.02	0.05
p	*0.96*	*0.49*	*0.77*	*0.81*	*1.00*	*0.54*	*0.87*	*0.71*
No Disruptive Behavior	0.02	0.27	-0.05	0.24	0.12	0.11	-0.03	0.24
p	*0.89*	*0.16*	*0.80*	*0.08*	*0.48*	*0.60*	*0.85*	*0.16*
No Harsh Discipline	0.01	0.05	-0.04	0.08	0.04	-0.01	-0.05	0.07
p	*0.97*	*0.88*	*0.89*	*0.71*	*0.85*	*0.97*	*0.81*	*0.77*
Parental Investments	0.01	-0.03	-0.01	0.00	-0.04	0.05	0.10	-0.09
p	*0.97*	*0.90*	*0.98*	*0.98*	*0.82*	*0.85*	*0.56*	*0.70*
KIDI	-0.00	0.28	0.10	0.05	0.02	0.19	-0.03	0.19
p	*0.99*	*0.19*	*0.45*	*0.75*	*0.90*	*0.34*	*0.87*	*0.27*
No Maternal Depression	0.15	-0.12	0.11	-0.05	0.02	0.08	0.16	-0.10
p	*0.34*	*0.53*	*0.59*	*0.72*	*0.89*	*0.69*	*0.34*	*0.60*
Mother Works	0.14	0.16	0.16	0.11	0.17	0.11	0.18	0.10
p	*0.02*	*0.10*	*0.09*	*0.07*	*0.03*	*0.19*	*0.03*	*0.22*
Mother Studies	-0.03	-0.02	-0.01	-0.04	-0.02	-0.04	-0.04	-0.01
p	*0.23*	*0.61*	*0.89*	*0.07*	*0.37*	*0.37*	*0.28*	*0.82*
Count Positives	11	11	8	11	10	11	10	9

This table presents estimates obtained using our base specification, and conditioning on specific subgroups. Low Educ: mother has 8 years of education or less at baseline. High Educ: 9 or more years. Low age: child has less then 24 months of age at baseline. High age: more than 24 months. Low wealth: under the median of a wealth index at baseline. High wealth: over then median. Girl/Boy: sex of the child. The final row presents the number of outcomes that have a positive treatment effect.

Table 8: Heterogeneous Impacts - Significance

	Low Educ	p	Toddler	p	Higher Wealth	p	Boy	p
Denver Social	-0.02	0.94	-0.03	0.92	-0.21	0.44	0.20	0.45
Denver Language	-0.11	0.66	0.13	0.66	0.24	0.39	-0.13	0.70
Denver Motor	-0.27	0.45	0.15	0.66	0.21	0.53	0.08	0.81
Denver Fine Motor	-0.34	0.32	-0.18	0.52	-0.01	0.98	-0.34	0.24
CLAP Social	0.13	0.53	0.23	0.25	-0.63	0.00	-0.27	0.11
CLAP Language	-0.01	0.95	-0.17	0.48	0.09	0.73	0.06	0.77
CLAP Motor	-0.29	0.16	-0.02	0.93	-0.02	0.94	-0.21	0.32
CLAP Fine Motor	-0.06	0.71	0.02	0.91	0.10	0.62	0.04	0.81
No Disruptive Behavior	-0.21	0.38	-0.29	0.19	-0.07	0.80	0.24	0.25
No Harsh Discipline	-0.01	0.98	-0.10	0.65	-0.10	0.79	0.15	0.50
Parental Investments	0.06	0.81	-0.04	0.88	0.06	0.83	-0.24	0.25
KIDI	-0.22	0.26	0.07	0.70	0.13	0.55	0.19	0.39
No Maternal Depression	0.26	0.27	0.14	0.50	0.11	0.67	-0.33	0.12
Mother Works	0.02	0.80	0.06	0.61	-0.05	0.57	-0.08	0.49
Mother Studies	0.01	0.77	0.02	0.64	-0.02	0.52	0.04	0.43

The regressions in this table include the participation in the CICO and the interaction of it with a covariate as the main endogenous variables of interest. The covariate is also included as an exogenous variable. The instruments are the propensity score and the interaction of the propensity score with the covariate. This table presents the coefficient associated to the interaction and the corresponding p-values. The covariates used are: Low Educ-mother has less than 8 years of education; Toddler-Child has less than 24 months of age; Higher Wealth-the family is over the median in wealth; Boy-The child is male.

Table 9: Robustness to different Specifications

	Base	No Controls	Visits	IPW	Rand	Exp	RW
Denver Social	0.35	0.39	0.47	0.35	0.47	0.02	
p	*0.05*	*0.04*	*0.02*	*0.05*	*0.01*	*0.03*	*0.08*
Denver Language	0.19	0.18	0.20	0.17	0.10	0.01	
p	*0.23*	*0.32*	*0.34*	*0.26*	*0.59*	*0.29*	*0.22*
Denver Motor	0.12	0.15	0.10	0.11	0.11	0.01	
p	*0.41*	*0.27*	*0.58*	*0.45*	*0.48*	*0.37*	*0.56*
Denver Fine Motor	-0.15	-0.13	-0.04	-0.15	-0.16	-0.01	
p	*0.39*	*0.52*	*0.86*	*0.37*	*0.42*	*0.51*	*0.56*
CLAP Social	0.05	0.30	0.04	0.07	0.11	0.00	
p	*0.70*	*0.03*	*0.82*	*0.60*	*0.45*	*0.74*	*0.86*
CLAP Language	0.06	0.30	0.09	0.09	0.12	0.01	
p	*0.64*	*0.07*	*0.57*	*0.50*	*0.43*	*0.47*	*0.86*
CLAP Motor	-0.02	-0.24	0.03	0.01	-0.02	-0.00	
p	*0.87*	*0.08*	*0.81*	*0.96*	*0.85*	*0.79*	*0.92*
CLAP Fine Motor	0.03	-0.29	0.18	0.04	0.08	0.00	
p	*0.73*	*0.01*	*0.17*	*0.72*	*0.48*	*0.51*	*0.92*
No Disruptive Behavior	0.11	0.11	0.12	0.12	0.11	0.01	
p	*0.39*	*0.37*	*0.40*	*0.32*	*0.35*	*0.56*	
No Harsh Discipline	0.02	-0.03	-0.13	0.04	-0.10	0.00	
p	*0.93*	*0.90*	*0.60*	*0.85*	*0.69*	*0.88*	
Investments	-0.00	-0.03	-0.02	-0.01	0.14	0.00	
p	*0.99*	*0.87*	*0.93*	*0.97*	*0.51*	*0.81*	
KIDI	0.08	0.04	0.11	0.06	0.21	0.01	
p	*0.50*	*0.72*	*0.45*	*0.60*	*0.18*	*0.26*	
No Depression	0.04	-0.07	0.17	0.04	0.04	0.01	
p	*0.79*	*0.62*	*0.29*	*0.78*	*0.78*	*0.57*	
Mother Works	0.14	0.16	0.14	0.14	0.11	0.01	
p	*0.02*	*0.02*	*0.07*	*0.02*	*0.10*	*0.08*	
Mother Studies	-0.03	-0.04	-0.03	-0.03	-0.02	-0.00	
p	*0.29*	*0.13*	*0.31*	*0.29*	*0.52*	*0.41*	
Count Positives	11	8	11	12	11	12	

BASE: Original specification; No controls: same specification without controls; Visits: Base specification adding home visits as a control (in case of the Denver tests, which are not available at baseline, the corresponding CLAP test at baseline is included); IPW: base estimation with Inverse Probability Weights to correct for attrition; Exp: Base specification but using months of exposure as treatment variable; Rand: using the randomization as the only instrument. RW: Romano-Wolf p-values. Hypotheses groups are (i) Denver Social and Denver Language; (ii) Denver Motor and Denver Fine Motor; (iii) CLAP Social and CLAP Language; and (iv) CLAP Motor and CLAP Fine Motor. No Depression (maternal) is measured using the CES-D Scale.

this procedure. The most important difference we find across the estimates is for the impact on mothers' labor market participation when using the randomization variable as our only instrument. Although the effect on maternal work is not significant when using the randomization alone, it is sizable in both estimations (0.14 and 0.11). Both of these magnitudes are important enough for these impacts to be considered economically relevant, so we still interpret the impact on mothers' labor market participation as a positive effect of the program.

As discussed before, a second phase of the experiment design aimed to identify the impact of the visits. Our main estimates for the effect of CICO participation might be contaminated, as more children in the treatment group obtained visits than did children in the control group. To make our parameters interpretable as the pure effect of CICOs, we include the indicator "ever attended home visits" as a control. As receiving visits is a relatively low-cost investment for families, we assume they are exogenous: the receipt of the benefit depends on the supply, not on the unobserved characteristics of the family. Table 9 show that our results are robust to the inclusion of home-visits, and the estimated parameters barely change.[17]

7.6 The Effects of Quality on Parental Decisions to Participate in CICOs and Quality-Adjusted Impacts

In this section, we present quality-adjusted estimates, considering the effect of the quality of the child care centers on the outcomes of children attending. For the estimates in this section to be consistent, we need to assume that quality is an exogenous variable: it is not related to unobserved variables that could be determining the outcomes of the children.[18]

[17] Unfortunately, visits are not observed for many of the children that reported attending CICO, because the original survey protocol considered skipping the visits questions if the child had attended a center. Using administrative and baseline data, we fixed the problem for many of the observations, but we still have a relatively serious level of missings: we do not have visits data on 26% of the children attending CICO (while the visits data is only missing for 1% of the children not attending CICO).

[18] We do not observe the quality measure for all EUs, only for those which eventually received a CICO (see Figure 1). This includes 31 of the 39 EUs in our sample, meaning that our estimates in this section include around 500 observations for the Denver test and around 1000 observations for

7.6.1 The Effect of Quality on Parental Decisions to Participate in CICOs

Table 10 shows the determinants of choice, including a measure of *process quality*, reflecting the quality of the classroom interactions. It is believed that this is the most relevant type of quality to generate positive impacts on children. It is also the least easily observable of the aspects of the quality (López-Boo et al., 2019).[19] Surprisingly, process quality determines participation in a significant way, which is remarkable given that few factors do so. This finding is relevant because it strongly suggests that parents are able to observe center quality (something researchers are rarely able to observe), and make decisions accordingly. The results using a more complete quality measure including materials and infrastructure are very similar. As the correlation between process quality and materials quality is very high in our sample (0.88), our results might come from parents observing materials and interpreting them as proxies of quality.

7.6.2 The Effect of Quality on Program Impacts

For this section, we are interested in studying how the impact of CICO participation change according to the quality of the CICO. We maintain the assumption that quality is an exogenous variable from the point of view of the families. We also assume that the program did not have impacts on the quality of the CICO, besides making the quality and the center to be built first.[20] Thus, we use quality as a *moderator* of the impacts of the program, rather than as a *mediator* (Heckman and Pinto, 2015). Table 11 presents the results of this exercise.

For the IV regression in this section, we include the quality of the CICO as an

the other outcomes.

[19] "Structural" quality involves the presence of those resources that facilitate the interactions characteristic of an environment of care; they are also variables that can be more easily controlled and observed.

[20] Although we can test this relationship in the data by studying the independence of the randomization with the quality measurement among the EUs whose quality is observed, the test can only be performed with 31 observations, so it might be underpowered. We do not reject the null of no relationship in the test, and it shows a treatment effect of the randomization on quality of 0.12 standard deviations, which we consider very low.

Table 10: Determinants of the Participation in CICO, Including a Process Quality Measure

	Beta	p-value
Randomization 1	0.41	(0.00)
Time to Center Pctile 1-20	0.27	(0.00)
Time to Center Pctile 21-40	0.21	(0.00)
Time to Center Pctile 41-60	0.049	(0.38)
Time to Center Pctile 61-80	-0.029	(0.68)
Mother's Age	-0.00097	(0.68)
Education of the Mother	-0.0053	(0.30)
Wealth Index	-0.030	(0.08)
Sex	-0.020	(0.49)
Age	0.0045	(0.01)
Mother Works	0.037	(0.37)
Father at Home	0.0092	(0.80)
N Child under 6 in Hhld	0.019	(0.36)
Quality: Process	0.042	(0.02)
Observations	1089	

We present marginal effects evaluated at the means of all the variables included. We use all of our control variables, our instruments and a process quality measure as potential determinants of participation.

Table 11: Impacts Adjusted by Quality

Outcome	CICO	p-value	Quality	p-value
Denver Social	0.60	0.01	0.39	0.05
Denver Language	0.33	0.20	0.33	0.26
Denver Motor	0.28	0.16	0.61	0.00
Denver Fine Motor	0.24	0.45	0.91	0.00
CLAP Social	0.15	0.38	0.04	0.82
CLAP Language	0.13	0.53	0.38	0.08
CLAP Motor	0.02	0.89	-0.06	0.72
CLAP Fine Motor	0.24	0.08	0.20	0.18
No Disruptive Behavior	0.07	0.58	0.03	0.79
Not Harsh Discipline	-0.12	0.69	-0.38	0.26
Investments	0.14	0.52	0.47	0.02
KIDI	0.42	0.05	0.50	0.05
No Depression (CES-D Scale)	0.05	0.77	0.04	0.84
Mother Works	0.11	0.21	0.11	0.20
Mother Studies	0.01	0.86	-0.02	0.44

This table presents estimates for quality-adjusted impacts. We include the quality of the CICO as an independent exogenous variable. The participation in CICO and an interaction of CICO participation with the quality measure are our independent endogenous variables, and the coefficients associated to them are presented in the table, next to their p-values. The instruments are the randomization and the interaction of the randomization and the quality.

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independent exogenous variable. The participation in CICO and an interaction of CICO participation with the quality measure are our independent endogenous variables. The instruments are the randomization and the interaction of the randomization and the quality. The table shows that (i) there are no impacts of quality on the maternal variables, as expected; but (ii) there are significant or close to significant impacts on KIDI and parental investments, implying that higher-quality centers are more active in engaging and training parents; (iii) there are some strong impacts of CICO quality on Denver fine motor, gross motor and social personal development, which is remarkable given the smaller number of observations in this sample. Overall, the findings are suggestive that quality -even within a low resource, low quality environment- is an important determinant of the impacts of child care centers.

8 Conclusions

While there is abundant evidence for the impacts of daycare programs in developed countries, rigorous evaluations of center-based child care programs in developing countries are scarce. Moreover, it leaves room to question the causality of the estimates provided. Given the massive expansion in daycare coverage and the potential continuation of this trend, quantitative evidence of the effects of center-based early childhood development care is much needed. This paper evaluates the impact of a daycare program implemented between 2013-2015 in Nicaragua, the second poorest country in Latin America.

Our findings are mostly consistent with previous evaluations of center-based programs serving highly disadvantaged populations in developed countries, except for two substantial things: firstly, inasmuch as we find important positive impacts on socio-emotional development, we do not see any complementary impact on the more cognitive domain of language; and secondly, we do see a significant impact in maternal labor force participation which was either not measured/analyzed in the previous LAC literature (Behrman et al., 2004; Noboa-Hidalgo and Urzua, 2012; Bernal and Fernández, 2013), or else it was found to be a positive impact that was accompanied by a negative impact on child development (Carta and Rizzica, 2018; Baker et al., 2008; Rosero and Oosterbeek, 2011), proving some sort of trade off between maternal and child welfare not supported by this study. In this sense our study is more aligned with Berlinski et al. (2009).

We do find some heterogeneous effects (larger impacts in low wealth, older children and boys) aligned with the literature, but the sample is not big enough to reject the null hypotheses that they are different. Still, our sample is large enough to find a moderation effect of our quality variable, showing that even small increases within the existing range of quality could pay off in terms of developmental outcomes and possibly extend to the more cognitive outcomes.

The CICO program in Nicaragua was substantially cheaper than the programs that have been evaluated in developed countries, or even in LAC (Araujo et al., 2013). It still managed to have statistically and economically significant

impacts on children and their mothers. This provides encouraging evidence for the potential of developing countries to foster their human capital through early childhood policies. At the running minimum wage level in Nicaragua (close to $2400/year), the impact of 14 percentage points on maternal work would be more than enough to fully pay for the program. The policy implications of our results point to the challenge of expanding daycare coverage, particularly for the most disadvantaged children. There is clear need for continuous quality improving and rigorously evaluating the quality of services provided.

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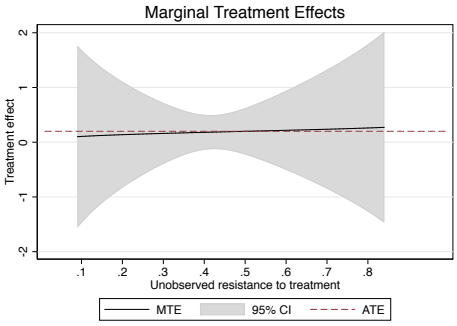
A Marginal Treatment Effect Graphs

Figure 4: Denver Language



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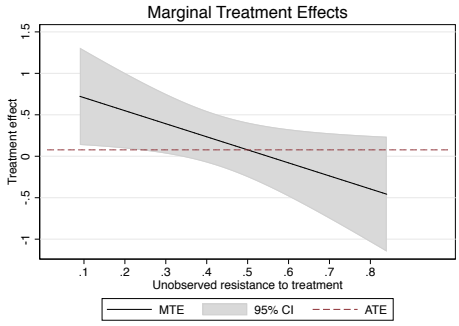

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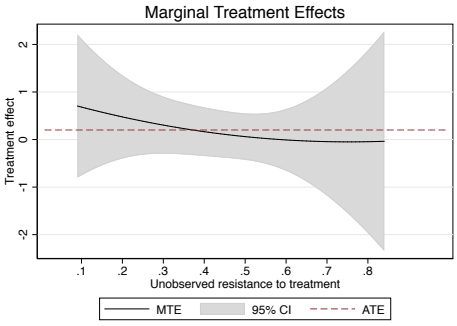

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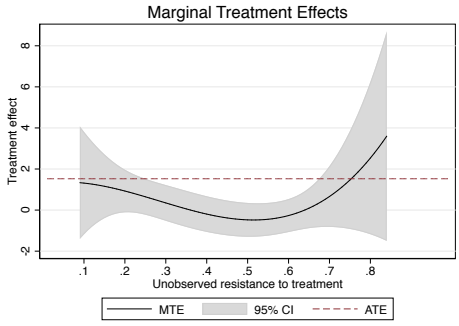

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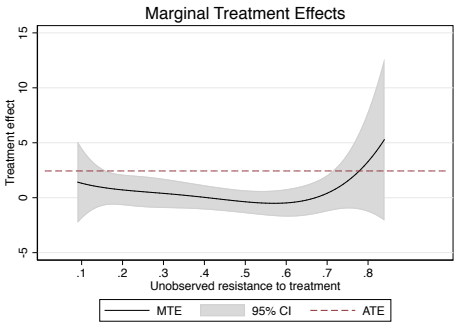

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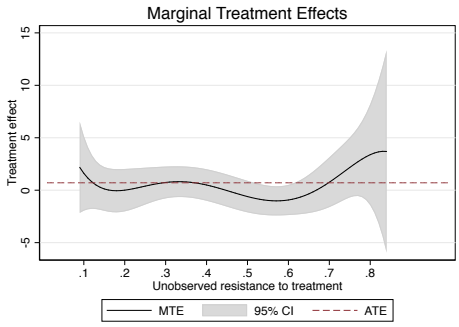

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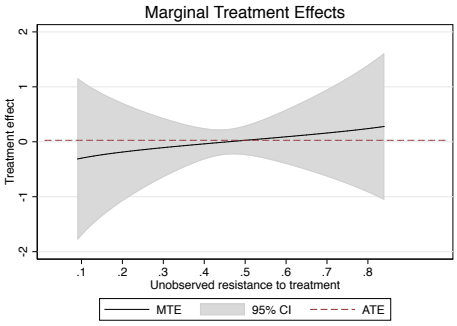
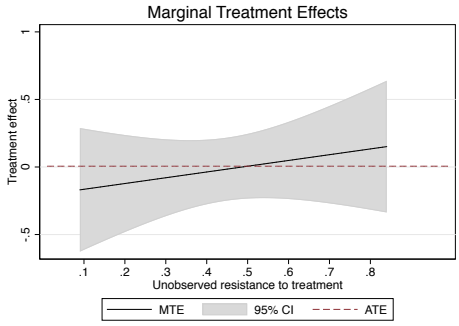
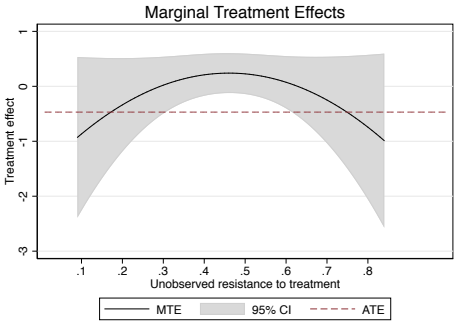
Figure 5: Parental Investment



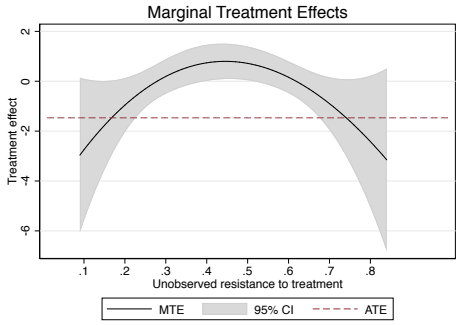
(a) Normal



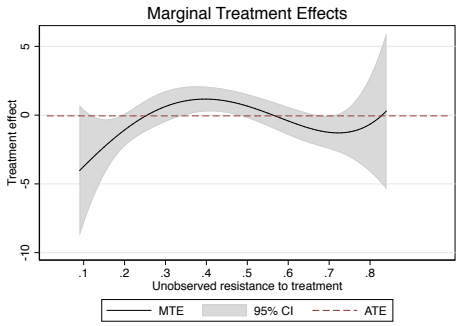
(b) Polynomial Degree 1



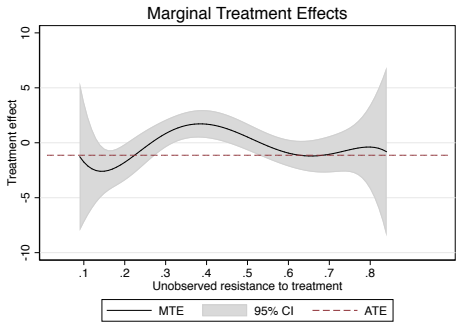
(c) Polynomial Degree 2



(d) Polynomial Degree 3

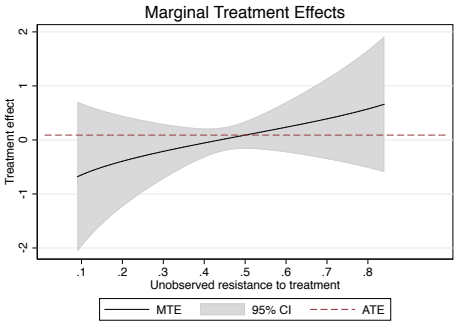


(e) Polynomial Degree 4

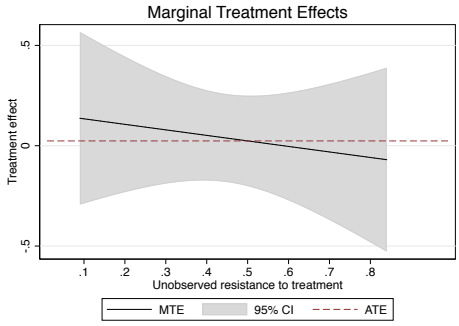


(f) Polynomial Degree 5

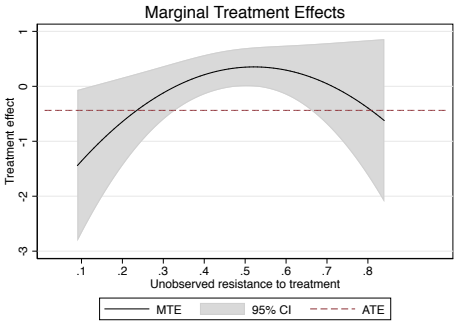
Figure 6: No Maternal Depression



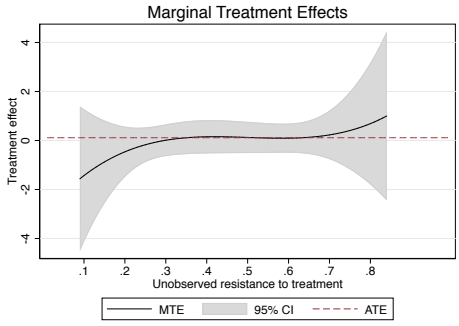
(a) Normal



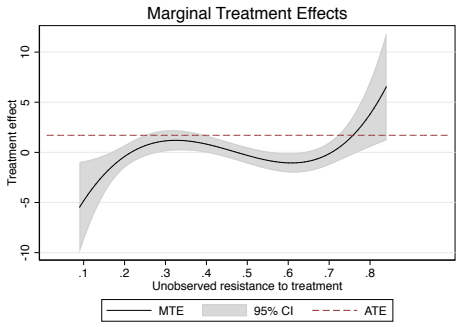
(b) Polynomial Degree 1



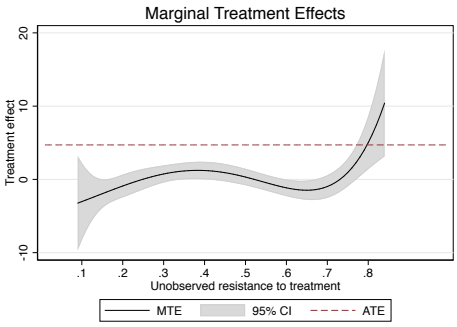
(c) Polynomial Degree 2



(d) Polynomial Degree 3

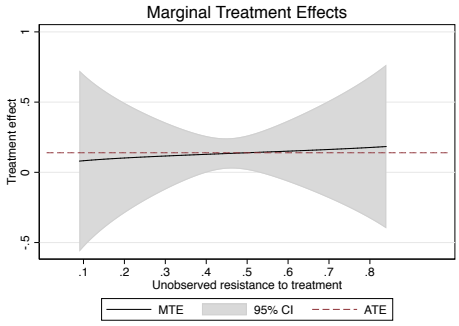


(e) Polynomial Degree 4

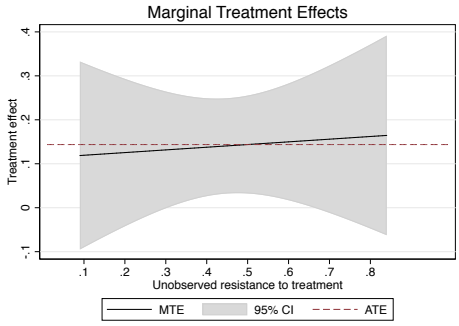


(f) Polynomial Degree 5

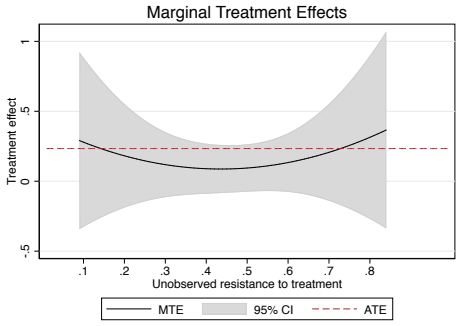
Figure 7: Mother Works



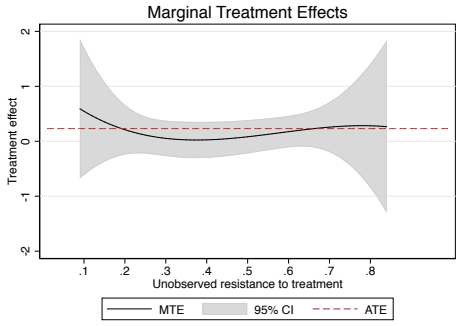
(a) Normal



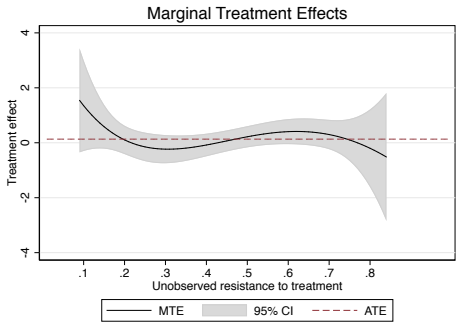
(b) Polynomial Degree 1



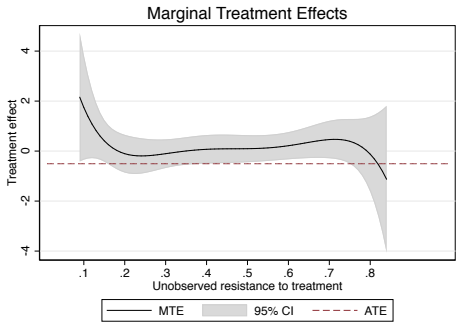
(c) Polynomial Degree 2



(d) Polynomial Degree 3

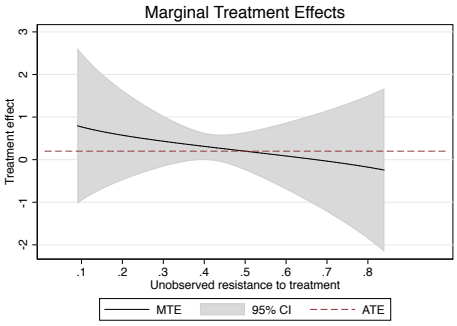


(e) Polynomial Degree 4

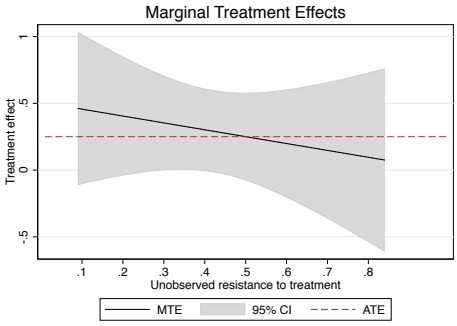


(f) Polynomial Degree 5

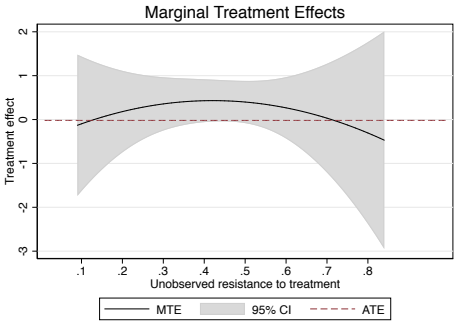
Figure 8: Denver Social



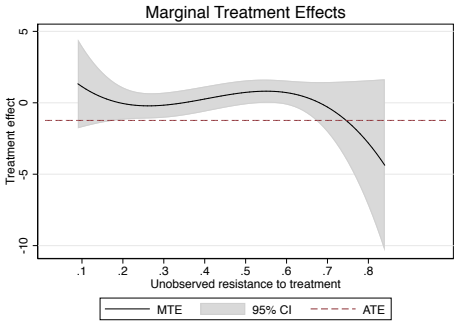
(a) Normal



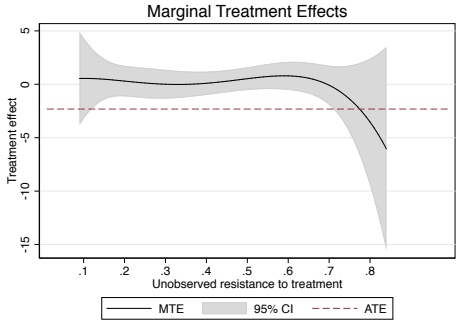
(b) Polynomial Degree 1



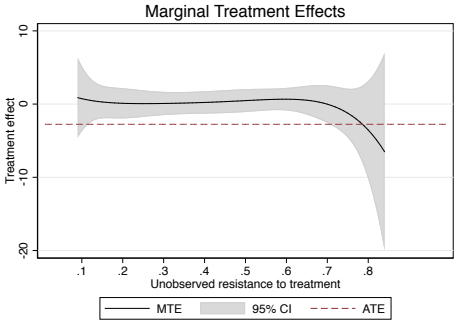
(c) Polynomial Degree 2



(d) Polynomial Degree 3



(e) Polynomial Degree 4



(f) Polynomial Degree 5