Daycare and Women's Employment:
effects in the short-run, in the long-run, and at home

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Abstract

Enabling women to stay in the labor force and increasing their earnings in the long-run are often raised as arguments for expanding access to childcare. In this paper, I document the impact of a free daycare program in Rio de Janeiro on women's employment and household income both during the eligibility period and four years later, when all applicant children are preschool or school-aged. Relying on the randomized selection of children to daycare spots, I find that access to daycare increases households income and mother's employment while children are young, but this effect largely dissipates once children age into preschool or school, with little evidence of a long-term benefit to women's careers. Longer-lasting gains from access to daycare, however, can be found in the ownership of durable goods, with positive implications for the environment in which children with access to daycare grow up.

1 Introduction

First, we consider the effect of access to daycare on household earnings *during* the daycare period. Consistent with other literature, we find a sizeable impact at the extensive margin, with households whose children were selected into the daycare program reporting income 20% higher than those whose children were not, just 8-10 months after program inception. Next, we find that these effects largely dissipate four years later, once all children who were involved in the randomization have aged out into preschool or school – which are universally available and mandatory in this context. By then, there is no significant difference in the employment rate or earnings for mothers between those whose children had been selected to daycare four years prior and those who were not, indicating a lack of path dependency in employment. That is, a program that primarily targets poor households whose adults have limited education (mothers in our sample are evenly divided between having up to elementary school education and having between elementary and high school education) is unlikely to have long term earnings effects for mothers, as they face jobs that offer little returns to experience.

The short-lived effect on earnings, however, does have a longer-lasting effect on living conditions. In particular, households whose children were selected for the daycare program have better living conditions (as measured by the ownership of durable goods) even four years after the random selection, with obvious implications for household well-being and possibly child development.

2 Literature: Childcare, Women's Employment, and Income

In the literature, there are at least two existing papers using data from Rio de Janeiro to measure the short-run effects of daycare on women's earnings (i.e., while children are still receiving daycare services). In the first one, using the same first-round data as the current paper, Paes de Barros et al. (2011) finds a positive impact from access to a public daycare program on mother's labor force participation and employment just eight months after its expansion. Further, they find the program increased household earnings by an average of R$92 (around U$46) per month, a significant but relatively small impact when compared to the cost of the program – R$250 per child per month. An older study conducted prior to the expansion of the public daycare program but based on a random sample of households in low-income areas of Rio de Janeiro shows that the elasticity of earnings with respect to the use of public daycare (in this case including philanthropically-provided services; in all, around 6% of the sample) varies from negligible to a twenty percent increase in earnings, depending on the specification and sub-sample; this relationship is largest for part-time workers. Notably, the effect of education on earnings is strong and large (at least 4 percentage points per year of education) under all specifications (Deutsch, 1998).

Research conducted on childcare in other settings have found mixed results for women's employment, varying across countries and by childcare type. Considering the childcare options for residents of poor areas in Guatemala City, for example, Hallman et al. (2005) finds that a decrease in childcare costs can lead to an increase in female labor force participation in the extensive margin, but does not encourage entry into the labor market. In contrast, using the gradual roll-out of public daycare in Chile for identification, Medrano (2009) observes that, once controls are included for individual and family characteristics, it had no impact on female labor force participation. This, the author speculates, could be because the program mainly attracted women who were already working and paying for childcare, or because of income effects. Manley and Vasquez (2013) corroborate these results using an updated dataset and a different approach for analysis of the same program in Chile. Interestingly, Berlinski et al. (2011) show that in Argentina there is an increase in both labor force participation and hours worked for mothers whose youngest child enrolls in public preschool, but no effect from older children. The positive effect from youngest children is still present at the transition point between preschool and primary school, but is short-lived, as there is no difference in mothers' employment rates by the time the children are six years of age.

Considering a public childcare subsidy program in Mexico, Ángeles et al. (2011) show lower childcare

costs raised female labor force participation but did not increase women's or household earnings, though the authors note that this may be due to poor economic conditions at the time of data collection, the short window between intervention and data collection, or possible income under-reporting from beneficiaries. Toroyan (2003) have similar results in the United Kingdom, where 18 months after gaining access to daycare, beneficiary mothers were more likely to be employed than those who were not randomly selected for daycare, but "no more likely to have a weekly household income of above 200". With the exception of Berlinski et al. (2011) mentioned above, research on the effect of access to childcare on women has been largely limited to the short-run, while children are still age-eligible for the service; long-term research on childcare has tended to focus on educational and employment outcomes for the children themselves.

The current paper adds to this literature by exploring the existence of a medium-run effect for a program in Rio de Janeiro, since engagement in jobs that offer returns to experience may allow any positive effects of access to daycare on women's employment to persist. Consider, for example, a scenario under which there is no childcare (formal or informal) available, so that a mother is out of the labor force for the first four years of her child's life, until he starts preschool. Assume that her monthly income prior to her child's birth was X and that for every additional year of experience her earnings increase by Δ. Given that she was not employed and therefore did not acquire professional experience while she cared for her child, she can expect an income of X when she resumes employment, $X(1+\Delta)$ the following year, and $X(1+\Delta)^n$ each n year after returning. In contrast, if she has access to childcare and responds by remaining in her job, her monthly income will be $X(1+\Delta)$ after her child's first year. After four years, when her child enters preschool, it will have reached $X(1+\Delta)^4$, in contrast to X if she is unable to accrue those four years of experience while her child is young. This difference will persist and in fact widen over time, as her income N years after her child's birth will be $X(1+\Delta)^N$ if she worked during those first four years and $X(1+\Delta)^{(N-4)}$ if she did not. Of course, if she is employed in a sector that offers no returns to experience, $\Delta = 0$ and her income is constant at X in every period she is employed.

3 Setting and Model

3.1 Political Background

In 2006-2010, access to daycare and preschool in Brazil transitioned from being an under-funded "right" provided through social assistance programs to part of the educational system. Access to care for children 0-6 had been guaranteed by the Constitution since 1996, but only in the mid-2000s was there a significant political push to fund these programs, expanding the number of daycare centers and preschool spots and bringing them into the educational system. Until then, public daycare and preschool programs were delivered through piecemeal programs run by the federal, state, or municipal government; through this shift, public daycare and preschool were moved under municipal responsibility with federal funding. In 2009, preschool

enrollment became mandatory for 4- and 5-year olds, and the federal and municipal governments set goals for increasing enrollment for younger cohorts.[1] In the southeast region, for example, 22% of children 0-3 were in daycare in 2010, and the federal goal was to bring that share up to 50%.

With the devolution of responsibility for daycare and preschool provision, municipal governments were given substantial flexibility in the transition process, including on decisions for addressing excess demand during this period. In the municipality of Rio de Janeiro, the increased demand for daycare for children aged 0-3 was managed through a lottery for all eligible applicants.[2]

3.2 Data, Randomization, and Take-up

In the second half of 2007, the government of Rio de Janeiro announced the expansion of daycare centers and spots available, and opened a lottery for selecting children aged 0-3 into the program. Eligibility was defined as meeting at least one of various criteria indicative of vulnerability, such having the person primarily responsible for the child working or looking for work; the child having a chronic health issue; or a household income below twice the minimum wage. The municipal government received applications for just over 24,000 eligible children and anticipated an availability of 10,000 daycare spots.

Households could only apply to one daycare center per child, and lottery assignment was done at the daycare and class level, with a class generally corresponding to an age group. Within a daycare center, then, if the number of applications for a given class was less than or equal to the number of planned spots for that class, all applicant children were admitted (a lottery was carried out for record-keeping). On the other hand, if the number of applications was greater than the number of spots for a given class in a center, eligible children were selected for admission at random. Children who were selected could then enroll, with services starting in early 2008, while those not selected were placed on a waitlist.

Data for the current analysis comes from three sources: (1) a one-page application form submitted in 2007 for all 24,136 eligible children; (2) a survey conducted in October 2008 with 3,768 households with children that applied to the program the previous year, evenly distributed between those selected and waitlisted by the randomization; (3) a follow-up survey in 2012 with 1,400 households, a subset of those in the 2008 survey. The 2008 survey was carried out when the first year of daycare services under the expanded municipal system was well underway (the school year in Brazil follows the calendar year), and reflects short-term outcomes after 8-10 months of daycare services. By 2012, in contrast, all children who had applied for the daycare program in 2007 were age-eligible for pre-school or school, which, by then, were universally available and mandatory starting at age 4. As such, results for 2012 largely reflect outcomes *after* daycare, once all children (selected and not selected) are required to attend pre-school or school.

[1] A 2009 revision to the Constitution made preschool mandatory for 4- and 5-year-olds, and a law on guidelines for education (Lei de Diretrizes e Bases), passed in 2013, included this standard.

[2] Preschool demand was managed separately, as attendance soon became constitutionally required and meeting demand essential.

Table 1: Age Distribution, Daycare History, and Assignment

Daycare Class	All eligible applicants	Applicants subject to random selection[a]	Selected (sample)[b]	Control (sample)[b]
Infant I	13.15%	14.59%	11.38%	11.88%
Infant II	29.55%	24.44%	21.13%	21.33%
Toddler I	37.06%	46.23%	56.11%	55.72%
Toddler II	20.23%	14.74%	11.38%	11.07%
Total %	100%	59.37%	50.61%	49.39%
Total N	24,136	14,329	1,907	1,861

[a] i.e., Applicants to daycare classes that had at least one available spot but for which there were more applicants than spots.
[b] 2008 dataset, sample from all applicants subject to lottery (Column 2)

Table 1 presents the age distribution for eligible children who applied for the public daycare program (24,136 children; first column) and for those subject to the lottery and in the 2008 sample. The greatest demand for daycare spots was for children aged 12-23 and 24-35 months (Infant II and Toddler I; first column). Comparing the first and second column, one can see that 12-23-month-olds and 36-47-month olds (Infant II and Toddler II classes) were *less* likely to be subject to the lottery while 24-35-month olds (Toddler I) were most likely to participate in the random selection. This is important considering that the 2008 survey sample was drawn at random from classes that were subjected to the lottery in a two-step process. First, classrooms that used the lottery were selected. Then, children were randomly chosen for the household interview, with an equal number of selected and control children (5, 10, or 20) from each of the classroom strata. Because of this process, even though Toddler I classrooms had the greatest demand among all applicants (first column), that cohort is nonetheless substantially over-represented in the sample (third and fourth columns). From this point forward, then, we use weights in the descriptive statistics and analysis, so that the estimates in this paper are representative of all eligible households with children who applied for the public daycare program.[3]

Table 2 shows the balance between children selected and not selected (control) by the daycare random selection. In general, households have 4–5 members and 87% of applicant households met eligibility for the daycare program based on their household income alone. Notably, 90% of children reported living in the community in which the daycare was located, indicating that the vast majority of applicants went to the center closest to them rather than seeking out centers that may have been perceived as "better" or "higher quality". This is consistent with the fact that 2008 was the first year of the expanded municipal daycare program, so the majority of daycare centers were new (or significantly changed) and did not yet have an

[3]Without weights, results would be valid for only households in the random sample, which is a subset drawn according to a child's age, but also the daycare they happened to try to enroll in and initial gaps between supply and demand for spots by daycare class.

Table 2: Balance

Application data (2007)	Selected	Control
Sex (% male)	53.53	51.36
HH size (mean)	4.45	4.36
HH income (mean, R$)	490.66	496.12
Poor HH (%)	87.92	86.15
Parent/guardian working (%)	69.74	70.15
Lives in the community (%)	90.20	90.54
Survey data (2008)		
Attended daycare in previous year (%)	10.27***	7.49***

Note: HHs defined as poor if total income was below 2x the minimum wage. "Parent/ guardian" is defined as "the person who makes decisions regarding the child's education and health and not necessarily the person financially responsible for the child" (2012 survey wording). *** Difference between selected and control groups significant at $p < 0.01$ level.

established reputation. Moreover, fewer than 9% of children in the lottery had attended any daycare center in 2007, so expectations and familiarity with programs may also have been less clear (Table 1). All of the characteristics reported in the 2007 application form do not differ between the randomly selected and control groups, indicating that the lottery was successful in producing a balance between the two groups. In contrast, reported daycare attendance in the previous year is higher for the treated group compared to the control; this may reflect a real imbalance in 2007 attendance that should be controlled for, or confusion in what the "previous year" referred to in the 2008 questionnaire.[4]

It is clear that daycare spots were quite desirable, as 92% of children who were selected by the lottery enrolled in the center to which they applied (Table 3). At the same time, since this was a period in which the municipal daycare system was expanding dramatically and deliberately, the number of available daycare spots by mid-2008 was substantially underestimated at the time of application and randomization. In the follow-up survey, we find that about half of the children who were initially not selected by the randomization were nonetheless enrolled in a public daycare by October 2008 – 42% in the same center that had originally placed them in the waitlist.

To contextualize our applicants and sample, we can use the Brazilian National Household Survey Sample (PNAD - *Pesquisa Nacional por Amostra de Domicílios*), an annual survey which, until 2016, was designed to produce representative information for most major metropolitan areas in Brazil, including Rio de Janeiro.

[4]In the 2008 survey, respondents were asked whether the child "attended any daycare the previous year".

Table 3: Enrollment in Daycare, 2008

Assignment	Center to which Applied	Any Public Daycare	Other Daycare
Selected	91.95%	94.23%	0%
Control	42.14%	49.77%	7.30%

Note: Among those using other daycare services (control group only), these were evenly split between using free and paid services.

Table 4: National Household Survey Sample (PNAD) Information - Metropolitan Rio de Janeiro, 2008

	All HHs	HHs with Children 0-4	Poor HHs with Children 0-4
HH size (mean)	2.98	4.29	4.10
HH Income (mean, R$)	2286.26	1914.73	534.79
Poor HH (%)	28.87	33.71	100
Mother of child 0-4 working (%)	-	46.61	25.02

Author's calculations using the 2008 PNAD (IBGE, 2008). HHs defined as poor if total income was below 2x the minimum wage.

In 2008, the mean household income in metropolitan Rio de Janeiro was R$2286, over four times the mean for households that applied for the daycare program (Table 4). Focusing only on households with children aged 0-4, total household income is slightly lower, and about one-third of them are classified as poor (i.e., household incomes up to 2 times the minimum wage). It is clear, then, that households that applied to the public daycare program are not representative of metropolitan Rio de Janeiro. Rather, consistent with the program's intended target, households that applied to public daycare are relatively poorer than the general population. At the same time, however, they are not a small niche group; following the eligibility rules, at least one-third of households with children 0-4 in metropolitan Rio de Janeiro would have met eligibility through the income criteria (column 2).

In terms of mother's employment, in Rio de Janeiro as a whole, 47% of mothers with children aged 0-4 were working. Among poor households, this rate is just about half as high, with 25% of poor mothers with young children working. Note that this points to a possible inconsistency with the employment of the child's guardian reported in the application form (Table 2), where 70% of applicant children have an employed guardian. This issue is explored in more detail in subsection 4.1.

3.3 Model

In this paper, we measure the impact of access to daycare on employment and income – and, later, durable goods – using the following model:

$$y_{it} = \beta\, S_i + \gamma\, X_i + \alpha_c + u_i. \tag{1}$$

y_{it} refers to the income or employment indicators collected in $t = \{2008, 2012\}$ for each child i, while S_i denotes the lottery outcome for child i. X_i are stable or baseline child or household characteristics, such as child sex and household declared income in 2007. α_c are daycare center fixed effects, which capture any unique characteristics of a center such hours of service (though all public daycare services under this system are available for at least 8 hours a day during the week) and neighborhood. And, as the randomization was carried out by class/age in each center, errors u_i are correspondingly clustered at the daycare center-class level.

As shown in Table 3, compliance with the lottery assignment S_i was low for those who were initially put on the waitlist (S_i=0), with almost half of them nonetheless receiving the intended treatment (T_i; enrollment in the public daycare program) within the first year. Information was not gathered on when each of the non-compliers in the control group moved out of the waitlist and enrolled in the center; it may have been at any point from the beginning of services – making the exposure of the child to daycare services the same as that of a selected child – to soon before the survey in October 2008. Nonetheless, this relatively high take-up rate among children initially placed in the waitlist dilutes the results and calls for interpreting the intent-to-treat (ITT) results obtained from the model above as a conservative estimate of the effect of access to daycare on those selected into daycare. Further, we can also consider enrollment in any public daycare center as the treatment (T_i) and the initial lottery assignment an instrument (S_i), so that

$$S_i = \delta T_i + e_i \tag{2}$$

is the first stage for obtaining local average treatment effects (LATE). Given that around half of those assigned to the control group (S_i=0) nonetheless enrolled in a public daycare center (T_i=1) but the vast majority of those in assigned to treatment (S_i=1) complied with enrollment (T_i=1), estimated LATE effects will be about twice as large as the corresponding ITT estimates. Since this is a repetitive process, we present LATE estimates for household income in our results in the main text but leave the remaining in the appendix.

4 Analysis and Empirical Results

4.1 Income and Employment in 2008: During Daycare Services

By October 2008, 8–10 months after daycare services started under the expanded public program, selected households report incomes that are 15 percent higher than those in the control group (Table 5). This result provides evidence that not only does access to daycare increase household income but also that either households that were initially placed in the waitlist but who ultimately enrolled did not, for the most part, get enrolled at the beginning of 2008, at around the same time as the selected group, or that the positive effect of access to daycare on household income is even larger than 15 percent in the short run. In fact, the

Table 5: Results: Household Income in 2008

	(1) Poor HH	(2) Poor HH	(3) HH income (ln)	(4) HH income (ln)	(5) HH income (ln)
Selected	–0.03*	–0.02	0.19***	0.17**	0.16**
	(0.01)	(0.02)	(0.05)	(0.05)	(0.04)
HH income (2007, ln)				–0.41***	–0.47***
				(0.06)	(0.06)
HH income (2007, $\ln^2$)				0.07***	0.08***
				(0.01)	(0.00)
Poor HH (2007)		0.22***		0.10	0.07
		(0.03)		(0.06)	(0.08)
HH size (2007)		0.00		-0.01	-0.00
		(0.01)		(0.03)	(0.02)
Parent/ guardian working (2007)		–0.04		0.11	0.11
		(0.02)		(0.06)	(0.09)
Previously in daycare (2007)		–0.10**		0.19*	0.17
		(0.02)		(0.07)	(0.11)
N	3,768	3,669	3,768	3,669	3,669
Control mean	0.82	0.82	6.03	6.03	6.03
Clustered errors (center x class)		X		X	X
Fixed effects (center)		X			X

estimated LATE, which reflects the effect of access to daycare for those households who *complied* with the lottery by enrolling, is 32 percentage points (Table 6).

We do not see a significant effect of the program on the likelihood that a household is poor in 2008 (Table 5). This appears to be because the vast majority of applicant households are poor, and though selection into the daycare program leads to an increase in income of 15% on average, this effect is concentrated at the bottom of the income distribution. Appendix Table A2 shows quantile regression results – selection into daycare has a large and significant effect at lower quantiles, but this effect decreases and weakens for higher quantiles, which is where the difference between poor and non-poor households would be.

4.1.1 Employment rates in 2007 and 2008

To analyze the effect of the daycare program on mothers' employment in 2008 (short-run), we must address two issues with the data: First, a discrepancy between employment rates reported in 2007 and in 2008, which we deal with using the PNAD data. Second, the fact that, in 2008, employment data was collected for most but not all mothers of children in the study. We discuss and resolve each of these issues in sequence below.

In both the application form filled out in 2007 and the 2008 survey data, respondents were asked about the employment status of the person responsible for the child ("*responsável*"; hereafter, "guardian"). This identity is generally clearly understood in Brazil, and is best defined as "the person who is responsible for deciding on education and health matters relating to the child and lives with them, but not necessarily the

Table 6: Results: LATE on Household Income in 2008, using random selection as IV for enrollment

	(1) Poor HH	(2) Poor HH	(3) HH income (ln)	(4) HH income (ln)	(5) HH income (ln)
Child in public daycare	–0.06*	–0.04	0.41***	0.36***	0.34**
	(0.03)	(0.04)	(0.10)	(0.06)	(0.09)
HH income (2007, ln)				–0.42***	–0.47**
				(0.06)	(0.10)
HH income (2007, $\ln^2$)				0.07***	0.07***
				(0.01)	(0.01)
Poor HH (2007)		0.22***		0.07	0.03
		(0.03)		(0.05)	(0.09)
HH size		0.00		-0.01	-0.00
		(0.01)		(0.02)	(0.02)
Parent/ guardian worked (2007)		–0.04		0.11***	0.13
		(0.02)		(0.04)	(0.05)
Previously attended daycare (2007)		–0.11**		0.20***	0.16*
		(0.02)		(0.07)	(0.06)
N	3,738	3,612	3,738	3,612	3,612
Control mean	0.82	0.82	6.03	6.03	6.03
Clustered errors (center x class)		X		X	X
Fixed effects (center)		X			X

person financially responsible for the child" (2012 survey instrument). In our study, the mother is listed as the person responsible for the child in 82% of observations in 2008; in the 2007 application form the identity of the guardian was not collected.

In 2007, 70-73% of application forms noted that the child's guardian was employed. In 2008, only about 42% of guardians are noted as employed. Here, we can once again rely on the PNAD to assess and reconcile these reported employment rates. As noted in Table 4, in metropolitan Rio de Janeiro, 47% of women with children aged 0-4 are employed. Among poor households, 25% of these mothers are employed; for households with earnings up to R$1000 (the 90th percentile in our daycare applicant sample), 28% are employed. The information given in the 2007 application forms, then, is unlikely to be a reflection of mothers' actual employment rates, and is probably heavily driven up according to how applicant households assumed their answers would affect eligibility. That said, the reported employment rate of the guardian is balanced between treatment and control arms in 2007 (Table 2), as selection was randomized among all eligible households based on reported data – including all those that incorrectly reported employment.

The employment data for children's guardians collected in 2008 appears to be more reliable, however. Not only is the response bias likely lower because these surveys were conducted by enumerators through home visits toward the end of the program year, but the surveys also included further questions on employment, so that respondents who noted that they were employed were asked what time they usually left for work,

started work, left work, etc., reducing the likelihood that one would falsely report employment status; that was not the case in the application form, where respondents were not asked any follow-up questions related to the employment of the child's guardian.[5] Moreover, the employment rate reported in 2008 for guardians in the control group – 42% for all guardians; 37% for guardians in poor households; and 37% for mothers in poor households – are consistent with the data for metropolitan Rio de Janeiro as a whole.[6].

4.1.2 Information on Guardians in 2008

By 2008, households in the treatment group are 5 percentage points more likely to report that the child's guardian is employed than households not originally selected for the program (Table 7). In 84% of households, mothers are listed as the guardian. However, taking the naive approach of restricting the analysis to cases in which the mother is listed as the guardian decreases the coefficient and its statistical significance (columns 4–6).

At the same time, however, households in the treatment group were slightly more likely to report the mother as the person responsible for the child when compared to the control group, though the difference is not very large: 85.9% versus 83.5%.[7] As employment data was only collected in 2008 for the person responsible for the child, this means that we are more likely to have information on the mother's employment in 2008 for treatment households compared to control (Table 8). This presents a case in which information on *mother's* employment – the outcome variable of interest here – may be missing not at random (MNAR). In particular, missingness may be dependent on the value of the outcome itself: On one hand, a mother who is not in the labor force may be more likely to be listed as a guardian because she spends more time with the child, but, on the other, an employed mother may be more likely to be listed as a guardian because she is more likely to be "financially responsible" for the child (even though being designated as "responsável" does not strictly depend on the ability to financially support the child).[8]

To deal with this missingness of data on the employment status of some mothers, we use multiple imputation modeling to impute the unobserved employment status of mothers who are not listed as the child's guardian. This is done over 20 iterations, with a new regression estimation run each time. Table 9 presents the combined result of these multiple regressions using imputed values for mother's employment.

[5] It is possible that some households surveyed in 2008 were concerned about losing their eligibility while others were hopeful they could still get a spot in the daycare program, and that through observation households surmised that *unemployment* might improve their eligibility. In that case, we might expect the bias to be there for both the selected group, who may be concerned about maintaining their eligibility, and the waitlisted group, half of whom want to maintain their eligibility and the other half who want to get a daycare spot.

[6] Besides the incentive to over-report employment in the application form in 2007 if one believed that a guardian's employment would help eligibility, some of the difference may also be explained by possible changes in *who* is designated the child's guardian in each of the two years, as there was no requirement that it be the same person in both the application form and the follow-up survey. Note also that the employment rate for men aged 20-40 with children 0-4 in the household (not necessarily their own children) and with household incomes up to R$1000 was 88% in 2008 (IBGE 2008). It is possible that many households thought of the child's father as the guardian in the application form, though that does not explain the switch to mostly listing mothers as the guardian in 2008.

[7] The next most common group of persons responsible for the focal child are grandparents, listed at twice the rate of fathers.

[8] Fewer than 6% of guardians are in school and not working.

Table 7: Results: Employment Status of Guardian or Mother, 2008

	(1) Guardian	(2) Guardian	(3) Guardian	(4)	(5)	(6)
				if Guardian = Mother		
Selected	0.07***	0.05**	0.05*	0.03*	0.05	0.04
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
HH size		–0.01	–0.03		–0.02*	–0.03*
		(0.01)	(0.01)		(0.01)	(0.01)
Previously in daycare (2007)		0.16*	0.12		0.17*	0.15*
		(0.05)	(0.06)		(0.06)	(0.05)
Sex of child			0.01			0.00
			(0.04)			(0.04)
HH has other kids age ≤ 4			–0.02			–0.04
			(0.03)			(0.03)
N	3,760	3,701	3,379	3,150	3,100	2,843
Control mean	0.39	0.39	0.39	0.39	0.39	0.39
Control for age of guardian			X			X
and for HH income (2007)			X			X
Clustered errors (center x grade)		X	X		X	X
Fixed effects (center)		X	X		X	X

Note: "employed" = is currently employed, on the day of the survey

Table 8: Results: Likelihood that Mother is Listed as Guardian, 2008

	(1)	(2)
Selected	0.02*	0.02*
	(0.01)	(0.01)
N	3,756	3,545
Control mean	0.84	0.84
Controls		X
Clustered errors (center x grade)		X
Fixed effects (center)		X

Controls: child sex, HH size (2007), whether guardian worked

Table 9: Results: Mother's Employment, 2008 (using multiple imputation)

	(1)	(2)	(3)
Selected	0.06***	0.05**	0.04**
	(0.02)	(0.02)	(0.02)
HH size (2007)		–0.02***	–0.03***
		(0.01)	(0.01)
Child attended daycare in 2007		0.16***	0.14***
		(0.04)	(0.04)
Sex of child			0.00
			(0.02)
HH has other kids age ≤ 4			–0.04*
			(0.02)
N	3,243	3,192	2,935
Control mean	0.40	0.40	0.40
Control for age of guardian			X
and for HH income (2007)			X
Clustered errors (center x grade)		X	X
Fixed effects (center)		X	X

We find that the daycare program increased the likelihood that a child's mother is employed 8–10 months after the start of daycare services by 4 percentage points. This would imply that mothers who are employed are less likely to be listed as the child's guardian than those who are not working, consistent with the idea that a mother may be more likely to be listed as a guardian if she spends more time with the child.

4.2 Income and Employment in 2012: After Daycare Services

The results presented above pertain to 2008, when the children in our sample were age-eligible for the daycare program and were enrolled in public daycare, on the waitlist for it, or, for a very small minority, neither. This is where most of the literature on the effect of access to childcare on women's employment has focused. In contrast, in this subsection we consider the effect of *having had* access to daycare in 2008 on outcomes in 2012, when all original applicant children are between ages 4 and 8 and eligible for preschool or school, by then universally available and mandatory. We find that, again, access to daycare does not appear to change household poverty status (60% of households in this sample remain poor; Table 10, columns 1 and 2). By 2012, the effect of access to daycare on household income is also substantially smaller and only significant at the 10% level (columns 3–5). To further assess whether there is a long-lasting effect on household income, given this weak significance, we can also look at monthly household food expenditures. We find no significant effect of access to daycare on household food expenditures four years later (columns 6 and 7).[9]

With the 2012 data, allowing for different effects for the older two age groups (those who applied for the

[9]The results presented in Tables 5–9 use data from all 3,500+ households surveyed in 2008. Appendix Tables [FIX] present the respective results using data from only those 1,400 households that were also surveyed in 2012. The results are generally the same, indicating that the lack of significant long-lasting effects from access to daycare, as measured in 2012, is not simply a result of the smaller sample size in that follow-up.

Table 10: Results: Household Income and Expenditures in 2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Poor HH		HH income (ln)			HH expenditures (ln)	
Selected	–0.06*	–0.06	0.09*	0.08*	0.09	0.06	0.05
	(0.03)	(0.03)	(0.05)	(0.03)	(0.05)	(0.05)	(0.03)
Older child					–0.08		–0.11
					(0.13)		(0.08)
Selected x Older child					0.02		0.02
					(0.08)		(0.07)
HH income (2007, ln)				–0.34**	–0.34**	–0.19	–0.17
				(0.09)	(0.08)	(0.09)	(0.09)
HH income (2007, $\ln^2$)				0.06**	0.06**	0.03*	0.03*
				(0.02)	(0.02)	(0.01)	(0.01)
Poor HH (2007)		0.04		0.27*	0.27*	0.02	0.04
		(0.04)		(0.10)	(0.10)	(0.07)	(0.07)
HH size (2007)		0.00		–0.05*	–0.05*	0.03	0.01
		(0.01)		(0.02)	(0.02)	(0.02)	(0.01)
Daycare in 2007		–0.14**		0.04	0.04	–0.05	–0.07
		(0.04)		(0.07)	(0.06)	(0.09)	(0.11)
N	1,225	1,193	1,225	1,193	1,193	1,330	1,319
Control mean	0.60	0.60*	6.88	6.88	6.88	5.97	5.97
Clustered errors (center x class)		X		X	X		X
Fixed effects (center)		X		X	X		X

toddler classrooms in 2007; "Older child") versus those in the younger two age groups (those who applied for the infant classrooms in 2007), accounts for the fact that, in 2012, the former group was age-eligible for school while the younger cohorts were age-eligible for preschool. At this point, both levels were largely universally available and mandatory, but pre-school, like daycare, is a full-day program (8 hours per day) while public school is administered as a half-day program (5 hours per day). As such, the interaction term in the regression between selection and the child of interest being an older child allows for possibly opposing forces coming from a greater ability to work with an older child (who may not have to be cared for at home) and a lesser ability to work with an older child (who is out of school for more hours) or even a greater need for income to cover meals and additional childcare costs for an older child (who is out of school for more hours). Nonetheless, we do not find any difference in effect for younger and older cohorts in terms of household income or expenditures (Table 10, columns 5 and 7).

It is possible that the disappearance of an effect on income and the lack of effect on expenditures from previous daycare selection can be attributed to the fact that, especially for the youngest children, those who were initially put on the waitlist continued to apply and enrolled in subsequent years. That is, the difference between treated and control households may be only one (out of at most four) years of daycare service. Nonetheless, these results show that an additional year of daycare service – and consequent additional household income and mother's employment – dissipates within four years.

Table 11: Results: Household Composition, 2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Household Size			Number of Young Kids (<4)			Number of Adults (≥18)		
Selected	0.07	0.18	0.12	–0.05	–0.04	–0.04	0.02	0.09	0.08
	(0.10)	(0.11)	(0.16)	(0.04)	(0.05)	(0.04)	(0.05)	(0.08)	(0.09)
Older child		0.38	0.41*		–0.01	–0.02		0.18	0.16
		(0.18)	(0.15)		(0.07)	(0.06)		(0.10)	(0.09)
Selected x Older child		–0.18	–0.27		–0.01	–0.00		–0.05	–0.05
		(0.13)	(0.17)		(0.07)	(0.05)		(0.13)	(0.12)
HH size (2007)			0.45***			–0.00			0.08*
			(0.06)			(0.01)			(0.03)
Number of kids under 4 (2008)			0.42**			0.02			0.06
			(0.09)			(0.03)			(0.04)
N	1,404	1,403	1,366	1,404	1,403	1,366	1,404	1,403	1,366
Control mean	4.56	4.56	4.56	0.37	0.37	0.37	2.09	2.09	2.09
Control for HH income and poverty (2007)			X			X			X
Clustered errors (center x grade)		X	X		X	X		X	X
Fixed effects (center)		X	X		X	X		X	X

We also note that there is no statistically significant difference in household size or number of children under the age of 4 by treatment arm (Table A3). That is, households do not generally respond to access to daycare by having more children or changing household composition (such as having grandparents move in or out). As such, the lack of significant difference in income and expenditures in 2012 do not appear to be because of changes in childbearing decisions or household composition in response to daycare access.[10]

For 2012, we also have information on the employment status of each household member. We find that, four years after the lottery, selection into the program had no long-lasting effect on women's employment or monthly earnings (Table 12). Although there was a significant difference in mothers' employment in 2008 between the treatment and control group, this difference dissipates and there is no observable difference in earnings once children are in school. That is, there does not seem to be a path dependency whereby an additional period of access to daycare (as determined by the lottery) sets women into a different earnings path, as would be the case if daycare provided more work experience (as evidenced by the 2008 results) *and* their employment options rewarded experience. This is consistent with the types of jobs available to women with a high school degree or less in Brazil (only 3% of mothers in our sample have at least some college education), which tend to have few/no barriers to entry, offer little protection, and tend not to reward experience given ample supply. According to the write-in descriptions of their jobs in our survey, 15% of mothers who were employed in 2012 reported that they worked in areas such as housekeeping, cleaning, and

[10]There are also no significant changes in household composition in 2008 between the treatment and control groups.

Table 12: Results: Mothers' Employment and Income, 2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Mother employed			Mother's earnings (ln, R$)		
treat	0.01	0.01	–0.01	0.04	0.02	–0.02
	(0.03)	(0.02)	(0.03)	(0.05)	(0.04)	(0.07)
Older child			–0.05			–0.15
			(0.05)			(0.10)
Selected x Older child			0.05			0.12
			(0.04)			(0.10)
Daycare in 2007		–0.04	–0.05		0.08*	0.05
		(0.05)	(0.03)		(0.03)	(0.02)
N	1,296	1,269	1,172	851	830	764
Control mean	0.68	0.68	0.68	677.12	677.12	677.12
Control mean, younger kids			71.31			701.96
Control mean, older kids			66.50			660.68
Control for HH size (2007)		X	X		X	X
and for child characteristics			X			X
Clustered errors (center x grade)		X	X		X	X
Fixed effects (center)		X	X		X	X

Note: "employed"=employed during the week prior to the survey; "earnings"=monthly earnings from work

childminding.[11] Another 5% reported working as manicurists and hairdressers, an area that also tends not to reward experience, at least at lower skill levels.[12]

It is perhaps not surprising, then, given the educational background of mothers of children eligible for the daycare program – which set up centers in poor communities and stipulated household earnings below two times the minimum wage as one of the eligibility criteria – and the kinds of jobs available to them, that there was no observed effect on women's earnings four years after their children's random selection (or not) into the public daycare program. We also do not observe any difference between those whose children were in the older cohort (and age-eligible for school in 2012) and those in the younger cohort (and age-eligible for preschool in 2012; Table 12, columns 3 and 6). In the next subsection we consider whether daycare, even if its effect on household income and women's employment is short-lived, nonetheless has other welfare benefits to the household in this medium-run (i.e., when children are still in school).

4.3 Living Conditions: Ownership of Durable Goods

Increases in income are only as meaningful as their contribution to individual well-being – the extent to which this flow of money adds to some stock of goods, living conditions, or welfare. In this setting, we have observed a large but temporary increase income for households selected into the daycare program,

[11] A law implemented in 2015 aligned the rights of domestic workers with those working for businesses, requiring formal employment and setting guarantees such as a minimum wage, maximum number of weekly hours, and overtime pay. Until then (and at the time of this survey), employers of domestic workers were not required to formalize their employment or pay at least the minimum wage.

[12] It is harder to ascertain the nature of some other write-in responses, such as "sales of ..." and "work in a store that sells...", but these are most likely largely jobs with limited growth as well given the education level of mothers in our sample.

Table 13: Ownership of Durable Goods

	2008	2012
Water filter	68.07%	59.67%
Stove	99.59%	99.85%
Refrigerator	96.54%	98.12%
Washing machine	45.62%	59.16%
Color TV	97.50%	98.71%
Computer	18.13%	44.30%
with internet	–	32.99%
Landline	48.14%	45.42%
Mobile phone	74.20%	93.07%
Any phone	85.24%	95.70%
Car or motorcycle	–	13.80%

as differences in income and employment status dissipate once children are in preschool or school. The persistence of the effect of this brief bump in income on living conditions will depend on how it is spent. If some of the increased income is allocated toward durable goods, such as kitchen appliances, computers, and telephones, we would expect access to daycare to have a positive effect on the ownership of these goods well beyond the period of increased income. That is, even once no difference in income or mother's employment can be observed, children who were selected into daycare may live in different environments from those who were not initially selected.

Table 13 presents summary statistics on the ownership of certain durable goods in both 2008 and 2012. While almost all households in our study in Rio de Janeiro have a stove and a color television, a washing machine and a computer are less common. Mobile phone ownership jumped from 74% to 93% in that 4-year-interval, reflecting the dramatic expansion of cellphone use in metropolitan Brazil at the time.

To measure the effect of access to daycare on household ownership of durable goods, we employ two alternative measurements: First, we consider a simple index under which a household is assigned 0 if they do not have a given asset and 1 if they do. Adding across these, each household is assigned a non-negative integer, where higher numbers indicate a greater number of goods. Alternatively, we use an index based on the first principal component score (normalized to be centered at 0 and have a standard deviaton of 1) as an indicator of household goods ownership. While the first approach may be simpler, the latter has the

Table 14: Results: Durable Goods Ownership, 2008

	(1) index (0-4)	(2) index (0-4)	(3) index (0-4)	(4) principal components score	(5)	(6)
Selected	0.10**	0.09	0.08	0.08**	0.08	0.06
	(0.04)	(0.05)	(0.08)	(0.04)	(0.04)	(0.07)
Older child			–0.08			–0.07
			(0.14)			(0.13)
Selected x Older child			0.01			0.03
			(0.11)			(0.10)
N	3,768	3,658	3,658	3,745	3,637	3,637
Control mean	2.13	2.13	2.13	–0.05	–0.05	–0.05
Control mean, younger kids			2.11			–0.06
Control mean, older kids			2.14			–0.03
Controls		X	X		X	X
Clustered errors (center x grade)		X	X		X	X
Fixed effects (center)		X	X		X	X

Note: Controls: HH size (2007), HH income (2007, ln and $\ln^2$), and child previously in daycare (2007).

advantage of using the data to assign weights to each asset according to the their contribution to household wealth (as measured by the index). As shown in Filmer and Pritchett (2001), an index of household durable goods and conditions is a valid proxy for household wealth, and arguably a more stable reflection of household welfare than income alone.[13] In constructing both indices, we use only assets that have substantial ownership variation – in this case, at least 10% and less than 90% households own the particular good – in 2008 or 2012 (i.e., water filter, washing machine, computer, mobile phone/ any phone, and, for 2012, car or motorcycle), but results with all assets are included in the appendix.

In 2008, 8–10 months after the start of the daycare program, there is no meaningful difference in durable goods ownership between households with children selected into daycare and those who were not – regardless of whether durable goods ownership is measured through an index or the first principal components score (Table 14). Although access to daycare resulted in higher levels of mothers' employment and of household income within that period, it did not immediately translate into greater ownership of durable goods. This does not mean that households did not convert the increased incomes into expenditures; only that they did not substantially increase their expenditures on durables within the short-run.

The results are entirely different by 2012, once children are in preschool or school. Here, we find that households that *had been* selected for daycare are likely, four years later, to have more durable goods. This is the case regardless of whether durable goods ownership is measured through an index or the first principal

[13]Filmer and Pritchett (2001) demonstrates that an index based on the first principal components score of household assets and conditions is a valid proxy for household wealth; the robustness checks include the application of the approach using only indicators of durable goods ownership, as done in the present paper. Here, we focus on durable goods since neighborhood conditions (e.g., piped water and electricity) are much more dependent on public services and collective action than on individual household expenditure decisions, particularly over a span of a few years in urban Rio de Janeiro.

Table 15: Results: Durable Goods Ownership (Index), 2012

	(1) index 1	(2) index 1	(3) index 1	(4) index 2	(5) index 2	(6) index 2
Selected	0.23***	0.17	0.29**	0.17***	0.17	0.27**
	(0.07)	(0.09)	(0.06)	(0.06)	(0.09)	(0.07)
Older child			0.19			0.10
			(0.12)			(0.13)
Selected x Older child			–0.20			–0.16
			(0.09)			(0.10)
N	1,404	1,361	1,361	1,404	1,361	1,361
Control mean	2.64	2.64	2.64	2.53	2.53	2.53
Control mean, younger kids			2.59			2.49
Control mean, older kids			2.69			2.56
Controls		X	X		X	X
Clustered errors (center x grade)		X	X		X	X
Fixed effects (center)		X	X		X	X

Note: Controls: HH size (2007), HH income (2007, ln and $\ln^2$), and child previously in daycare (2007). Both indices range from 0 to 5.

components score (Tables 15 and 16). This implies that the temporary increase in income experienced by households that were selected for the daycare program – which largely dissipated by 2012 – was at least in part allocated toward durable goods. As a result, households with access to daycare in 2008 have higher levels of durable goods ownership – 0.3 standard deviations higher – than those who were not initially selected for the daycare program.

Ownership of durable goods is not just a good proxy for household wealth (Filmer and Pritchett 2001), but also reflects the living conditions of the household. One way to interpret this result, then, is that children who are selected by the daycare lottery are more likely to grow up in slightly wealthier households, with items such as washing machines, computers, mobile phones, and cars or motorcycles than children initially put on the waitlist. Having a computer at home may be beneficial to child development, while having a washing machine or a car may have a time-saving function to parents. In this sense, selection into daycare improved the child's and family's wellbeing in the long run, even though the effect on household *income* was not observed past the short-run.

4.3.1 Exploratory Analysis: Durable Goods and Access to Credit

As we find that households that were selected into the public daycare program in Rio de Janeiro had an increase in household income of 16% just 8–10 months after the start of the program, compared to those who were waitlisted, and that, four years later, the former group also had more durable goods than the latter, we next explore whether access to credit played a role in the acquisition of durable goods.

If a credit-constrained household experiences a sizeable but temporary increase in income, we might

Table 16: Results: Durable Goods Ownership (Principal Components), 2012

	(1)	(2)	(3)	(4)	(5)	(6)
	principal components 1			principal components 2		
Selected	0.19***	0.15	0.26**	0.20***	0.15	0.24**
	(0.06)	(0.07)	(0.05)	(0.06)	(0.08)	(0.06)
Older child			0.19			0.11
			(0.10)			(0.11)
Selected x Older child			–0.18*			–0.16
			(0.07)			(0.09)
N	1,404	1,361	1,361	1,402	1,360	1,360
Control mean	–0.13	–0.13	–0.13	–0.13	–0.13	–0.13
Control mean, younger kids			–0.18			–0.17
Control mean, older kids			–0.08			–0.10
Controls		X	X		X	X
Clustered errors (center x grade)		X	X		X	X
Fixed effects (center)		X	X		X	X

Note: Controls: HH size (2007), HH income (2007, ln and $\ln^2$), and child previously in daycare (2007). Both principal components scores are standardized to have a mean of 0 and a variance of 1.

expect the household to allocate some of that additional income toward expenditures. In particular, for poor households that do not have access to credit but are able to save, a rule-of-thumb is that they will save 30 percent of any income they receive above their mean income (Deaton 1992).[14] The remainder will be allocated toward expenditures, potentially including that on durable goods. For credit-unconstrained households, however, a temporary increase in income may not have a large effect on the ownership of durable goods, as they could have borrowed to purchase these goods. In Brazil especially, households with a credit card can easily purchase durable goods on credit in interest-free installments.[15]

It is theoretically possible, then, that access to daycare – and the consequent temporary increase in mother's employment and household income – may increase the ownership of durable goods among credit *constrained* households, while having a smaller or negligible impact on households that are *not* credit constrained. We propose a switching regression model to explore this formally. First, under this scenario there are two regimes:

$$y_{1i} = \beta_1 S_i + \gamma_1 X_i + u_{1i} \tag{3}$$

$$y_{2i} = \beta_2 S_i + \gamma_2 X_i + u_{2i}\,, \tag{4}$$

[14]The model proposed in Deaton (1992) for less developed countries also assumes that income is auto-correlated over time but stationery and that the household is impatient (i.e., has a discount rate higher than the interest rate, a realistic assumption, particularly for poor households).

[15]In Brazil, purchases can be often by split into installments at the point of sale. These are usually offered interest-free by a variety of vendors, including grocery and appliance stores, and effectively apply the bill to the credit card in installments. This is distinct from the balances on which credit card companies charge interest.

and

$$y = \begin{cases} y_1 \text{ if } I_i = 1 \\ y_2 \text{ if } I_i = 0\,. \end{cases} \tag{5}$$

I_i is an indicator of whether child i's household has access to credit ($I_i = 1$) or not ($I_i = 0$). S_i is again the lottery outcome for child i and X_i are stable or baseline child or household characteristics. y_{1i} and y_{2i} are household i's ownership of durable goods in 2012 under each of the two regimes. And u_{1i} and u_{2i} are assumed to be normally distributed as $N(0, \sigma_1^2)$ and $N(0, \sigma_2^2)$, respectively. With this switching regression model, we are interested in whether $0 \leq \beta_1 < \beta_2$. That is, whether the effect of access to daycare on the ownership of durable goods in 2012 is greater for households that are credit constrained than for those that are unconstrained.

If household constraint status were known (and not dependent on childcare access), this analysis could be carried out by splitting the sample into constrained and unconstrained households and running the regressions for each regime separately. In practice, though credit constraint status is not directly observed, we do have information on whether the household has a credit card ($W_i = 1$) or not ($W_i = 0$). This measurement can serve as an imperfect indicator of the household regime with the following probabilities:

$$p_{11} = Prob\,(\,W_i = 1 \,|\, I_i = 1\,); \quad p_{10} = 1 - p_{11}$$
$$p_{01} = Prob\,(\,W_i = 1 \,|\, I_i = 0\,); \quad p_{00} = 1 - p_{01},$$

with the requirement that $p_{11} > p_{01}$. That is, that W_i contains some useful information about household credit status, so that households that are not credit constrained are more likely to have a credit card than constrained households.

We then denote $\lambda = Prob\,(I = 1)$, or the likelihood that a household is unconstrained, and can write out the likelihood function for this model with imperfect separation (following Maddala 1986) as:

$$\mathcal{L}(\beta_1, \gamma_1, \beta_2, \gamma_2, \sigma_1^2, \sigma_2^2) = \Pi[f_1\,\lambda\,p_{11} + f_2\,(1-\lambda)\,p_{01}]^{W_i}\,[f_1\,\lambda\,p_{10} + f_2\,(1-\lambda)\,p_{00}]^{1-W_i}, \tag{6}$$

where f_1 and f_2 are the normal distribution functions of u_{1i} and u_{2i}, respectively. This likelihood function is then used to carry out a maximum likelihood estimation, which simultaneously estimates the coefficients for both regimes and the probabilities that households fall into one regime or another given their credit card ownership status.

The results for this switching regression are presented in Table 17. The top panel shows the results for credit unconstrained households (regime 1), while the bottom panel shows that for credit constrained households (regime 2), with the division *imperfectly* proxied by credit card ownership. The estimate

Table 17: Results: Switching Regression for Durable Goods Ownership, 2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	index 1		index 2		pc score 1		pc score 2	
regime 1								
Selected	0.17**	0.29**	0.15**	0.13	0.13	0.24*	0.12	0.10
	(0.07)	(0.12)	(0.08)	(0.15)	(0.08)	(0.14)	(0.08)	(0.17)
Older child		0.12		-0.03		0.12		-0.01
		(0.11)		(0.13)		(0.13)		(0.12)
Selected x Older child		-0.18		0.04		-0.17		0.03
		(0.15)		(0.17)		(0.17)		(0.19)
regime 2								
Selected	0.13	0.20	0.14**	0.26**	0.13**	0.22**	0.13**	0.25***
	(0.08)	(0.13)	(0.07)	(0.11)	(0.07)	(0.11)	(0.06)	(0.10)
Older child		0.14		0.14		0.12		0.11
		(0.14)		(0.12)		(0.12)		(0.11)
Selected x Older child		-0.11		-0.18		-0.13		-0.18
		(0.17)		(0.15)		(0.14)		(0.12)
λ	0.47	0.46	0.38	0.36	0.32	0.31	0.26	0.26
p01	0.55	0.55	0.57	0.57	0.57	0.57	0.59	0.59
p11	0.86	0.86	0.90	0.91	0.97	0.97	1	1
N	1,381	1,381	1,381	1,381	1,381	1,381	1,379	1,379

Note: All regressions include controls for HH income (2007, ln and $\ln^2$). Center-level fixed effects are not included. Errors are clustered at the center x grade level.

for λ indicates that about one-quarter to half of households (depending on the specification) are credit unconstrained, while the estimate for $p11$ implies that 86–100% of households that are not credit constrained have a credit card.

At the same time, note that while the coefficients for credit constrained households tend to generally be larger and more statistically significant than those for unconstrained households – as one would expect if the impact of daycare on the ownership of durable goods is greater for the former group – we cannot reject the hypothesis that the coefficients for the two groups are the same. That is, taking column 4 as an example, although the coefficient for selection into the daycare program is 0.13 for unconstrained households and 0.26 for the unconstrained households, we cannot reject the hypothesis that the two coefficients are the same at conventional levels of significance. This result indicates that these regimes can be collapsed into one, and a single regression better describes the data than a switching regression according to credit constraint status. As such, we find that while households that were selected into the daycare program are likely to be slightly wealthier and have more durable goods *four years after* the program started, this is not driven by differences in access to credit nor does it disproportionately benefit credit constrained households. This also implies that the effect of access to daycare on durable goods ownership – and consequently living standards – does not replace and cannot be replaced by improved access to credit, an arguably cheaper intervention.

5 Conclusion

In this work, we found three general results: (1) Consistent with several other papers in the literature, access to public daycare in Brazil increased household income and mothers' employment rates while children were in daycare; (2) This effect dissipated by the time children were in preschool or school; (3) Nonetheless, previous access to daycare led to increased household wealth and children's living conditions even once they were in preschool or school. Altogether, our results show that measuring the effect of daycare on short-run employment or household income offers a incomplete picture of a program's effect on the household's livelihood, particularly once children have aged out of the program. Improvements in a child's living conditions may also be a factor in the higher cognitive development and educational achievement for children who previously attended daycare found in other research (e.g. Loeb *et al.* 2007; National Institute of Child Health and Human Development Early Child Care Research Network 2000).

Through exploratory analysis, we also find that the increase in durable goods ownership is not concentrated according to a household's credit constraint status. That is, the increase in income (perhaps complemented with greater investment in early childhood development) does not affect durable goods ownership in the same way as improving access to credit would. The improvement in living conditions occurs regardless of credit constraint status, so that the external relevance of these results is not limited to contexts or target households with little or no access to credit.

The null result on women's employment in the medium run also speaks to calls for increasing access to childcare in order to address the child-bearing penalty in women's earnings trajectories observed in higher income countries (e.g., Wilde *et al.* 2010). Here, we find that, for a target group with relatively low levels of education and facing a labor market that does not generally reward work experience, access to childcare raises current employment and household income, but there does not appear to be a long-lasting effect on women's earnings and employment. This, of course, does not contradict the existence of a child-bearing penalty; rather, it highlights that while highly educated women may experience long-term income losses if they take breaks in their careers after child-bearing, this is less of a concern for less educated women. Instead, the benefits of improved access to childcare for poorer women rests in higher incomes *during* the daycare period and improved living conditions beyond that period. For longer-term impacts on employment and earnings for women facing a labor market that does not reward experience, it may be worthwhile exploring the design of a daycare program that also encourages further education for mothers, such as prioritizing those who are in school or want to return to school or providing daycare services in days and locations compatible with adult education.

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Appendix

Table A1: Results: Household Income in 2008 - using only households in 2014 sample

	(1) Poor HH	(2) Poor HH	(3) HH income (ln)	(4) HH income (ln)	(5) HH income (ln)
Selected	–0.03	–0.04	0.16**	0.17	0.22*
	(0.02)	(0.03)	(0.07)	(0.07)	(0.07)
HH income (2007, ln)				–0.24	–0.21
				(0.11)	(0.09)
HH income (2007, $\ln^2$)				0.05*	0.04*
				(0.02)	(0.02)
Poor HH (2007)		0.25***		–0.08	–0.20*
		(0.03)		(0.11)	(0.07)
HH size		0.01		–0.02	–0.02
		(0.01)		(0.02)	(0.03)
Parent/ guardian working (2007)		–0.01		–0.03	–0.03
		(0.01)		(0.12)	(0.12)
Previously in daycare (2007)		0.05		–0.02	–0.00
		(0.03)		(0.11)	(0.05)
N	1,404	1,367	1,404	1,369	1,367
Control mean	0.84	0.84	6.04	6.04	6.04
Clustered errors (center x class)		X		X	X
Fixed effects (center)		X			X

Note: The regressions carried out for this table are the same as those for Table 5, except that here the data was restricted to only households that participated in the 2014 follow-up survey.

Table A2: Results, Quantile Regression: HH Income Quantile, 2008

	(1) 10th	(2) 25th	(3) 50th	(4) 75th	(5) 85th	(6) 90th
Selected	0.147**	0.0584***	0.0552**	0.0517*	0.0334	0.0380
	(0.0699)	(0.0206)	(0.0226)	(0.0283)	(0.0228)	(0.0301)
N	3,642	3,642	3,642	3,642	3,642	3,642

*** p<0.01, ** p<0.05, * p<0.1

Controls from 2007: HH income (ln and $\ln^2$), poor HH, HH size, parent/ guardian worked, previously attended daycare

Table A3: Results: Household Composition, 2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Household Size			Number of Young Kids (<4)			Number of Adults (≥18)		
Selected	0.07	0.18	0.12	–0.05	–0.04	–0.04	0.02	0.09	0.08
	(0.10)	(0.11)	(0.16)	(0.04)	(0.05)	(0.04)	(0.05)	(0.08)	(0.09)
Older child		0.38	0.41*		–0.01	–0.02		0.18	0.16
		(0.18)	(0.15)		(0.07)	(0.06)		(0.10)	(0.09)
Selected x Older child		–0.18	–0.27		–0.01	–0.00		–0.05	–0.05
		(0.13)	(0.17)		(0.07)	(0.05)		(0.13)	(0.12)
HH size (2007)			0.45***			–0.00			0.08*
			(0.06)			(0.01)			(0.03)
Number of kids			0.42**			0.02			0.06
under 4 (2008)			(0.09)			(0.03)			(0.04)
N	1,404	1,403	1,366	1,404	1,403	1,366	1,404	1,403	1,366
Control mean	4.56	4.56	4.56	0.37	0.37	0.37	2.09	2.09	2.09
Control for HH income and poverty (2007)			X			X			X
Clustered errors (center x grade)		X	X		X	X		X	X
Fixed effects (center)		X	X		X	X		X	X

Table A4: Bounds: Employment Status of Mother, 2008

	(1)	(2)	(3)	(4)	(5)
	if mother not guardian=working			if mother not guardian =not working	
Selected	0.03	0.04**	0.03	0.07***	0.06***
	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)
HH size			0.01		-0.00
			(0.00)		(0.01)
Child attended			0.07		0.09**
daycare in 2007			(0.05)		(0.02)
Sex of child			-0.00		-0.03
			(0.04)		(0.03)
N	1,401	1,400	1,321	1,401	1,321
Control mean	0.47	0.47	0.47	0.29	0.29
Clustered errors (center x grade)		X	X		X
Fixed effects (center)		X	X		X