The effect of children's school starting age on women's labor supply - evidence from Brazil's Curriculum reform

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Abstract

This paper exploits the gradual implementation of a Curriculum change in Brazil which increased mandatory primary schooling years from 8 to 9, leading most children to enter school at the age of 6 instead of 7. It provides evidence that increasing the supply of childcare and implicitly subsidizing these services may help alleviate child penalties in Brazil, namely by increasing women's labor supply probabilities and their chances of having formalized employment.

1 Motivation

While gender inequalities in the labor market have significantly decreased during the past decades, large differences still exist in most countries. In Brazil, the human capital gap has decreased starkly in recent years. In fact, as indicated by Beltrão and Alves (2013), the second half of the 20th century was marked by the reversal of the country's gender gap in education, leading to significant improvements in female labor market outcomes.

Labor force participation, for instance, increased by 12 percentage points between 1960 and 1992 (Soares and Izaki 2002) and by 8.5 percentage points between 1992 and 2012 (Codazzi et al, 2018). Roughly, 50% of the increase in women's participation rates can be explained by human capital improvements (Soares and Izaki 2002). This effect was even stronger for married women, whose participation rates increased by 10 p.p in only 10 years (Soares and Izaki 2002), consistently with the U-shaped model's prediction that an increase in education would affect married women's labor force participation through an income effect (Goldin, 1990).

In recent years, the country also experienced a modest reduction in the wage gap, originating mainly from the human capital gap closure (Codazzi et al, 2018). Nevertheless, unexplained differences in labor market outcomes are still large and, albeit at a lower level than in past decades, the gap closure trend seems to have stagnated (Madalozzo, 2010). While these differences may be in part attributed to earlier patterns, such as gendered selection into lower-paying careers, evidence indicates that even when controlling for industry and personal characteristics, wage differentials persist in Brazil (Madalozzo, 2010). Not unlike other countries, the remainder of Brazil's wage gaps appears to stem importantly from gender norms and the unequal allocation of household chores (Codazzi et al, 2018).

Indeed, in spite of the strong increase in their participation rates, married women in Brazil still fall behind their unmarried counterparts by 4 p.p and have even more considerable gaps when compared to men (Soares and Izaki 2002). Various intrahousehold factors have been shown to contribute to these gaps in women's labor supply decision, ranging from gender norms leading them to work less if their probability of earning more than the spouse is higher (Codazzi et al, 2018) to the disproportional burden with unpaid household activities limiting available time. For this reason, placing bigger attention on research questions regarding intrahousehold dynamics, particularly for married women and mothers, seems to be policy relevant.

In particular, child penalties have been shown to account for a large share of remaining labor market inequalities in high and middle income countries, as the burden of childcare falls disproportionately on women. Indeed, the birth of a woman's first child is starkly associated with a permanent negative shock in labour outcomes such as earnings, hours worked and probability of being a manager (Kleven et al, 2019). For this reason, much of the economic literature relating to female labor outcomes has since focused on understanding how to relax childcare constraints, namely through parental leave policies and childcare supply.

In this analysis, I focus on the the impact of formal childcare provision in Brazil on women's labour outcomes, as increasing the supply of such services, especially if at subsidized rates, could be an important tool to decrease child penalties for women. This is especially relevant in Brazil's context, as the supply of childcare is often limited (Ryu, 2019, from data from the PNAD). Furthermore, Brazilian families rely starkly on grandparents for informal childcare, indicating that the ongoing retirement reforms would increase the scope for the public provision of these services.

2 Evidence Review

The impact of education reforms on the number of schooling years provided for young children goes beyond human capital formation for the affected individuals. By increasing schooling at primary levels, be it at the intensive or the extensive margin, policy makers are both expanding the supply of childcare and implicitly subsidizing it. Various studies have thus exploited these changes to look at their impact on women's labor supply.

Most researchers have looked at these relationships in developed countries (Cascio, E., 2009, Fitzpatrick, M., 2010 and Gelbach, J., 2002), finding evidence that childcare provision is linked to women's labor supply, but its exact effect depends on a number of initial characteristics. Gelbach (2002) finds a strong increase in the enrollment of children in labor supply of single mothers, comparable in magnitude to those of married mothers. However, the effect disappears completely if single mothers have a child younger than 5, whereas, for married women, the positive supply effect still holds. Similarly, Cascio (2009) examines the results of state funding targeted towards public kindergartens, finding that single mothers respond strongly to the enrollment of their 5-year old child, as long as they have no children younger than 5.

Conversely, Fitzpatrick (2010) finds almost no effects of subsidized pre-school supply on women's labor supply, even if the policy was successful in increasing children's enrollment, except for women living in less populous areas. While such results indicate that the cost-benefit of these policies needs to be further analyzed, they also suggest that women for which alternative childcare is less readily available or relatively more costly respond well to these expansions. Constraints as those mentioned by Fitzpatrick, which might be binding in low and middle income countries, indicate the necessity of gathering more evidence of this relationship in the developing world.

However, estimates on these correlations for developing countries are scarce. Notably, Berlinski and Galiani (2007) have shown that this association also holds in Argentina: the expansion of the primary school system is linked to higher school enrollment in young children and to an increase in maternal labor supply in similar magnitude to what was found in other countries. Similar work has also been conducted in Brazil: Ryu (2019) exploits a lower-limit to primary school enrollment, through which only children born before a certain day are able to start school at the age of 4, to estimate the effect of public childcare provision on women's outcomes through an RD design. The author finds that, for mothers without younger children, provision of childcare has the effect of increasing labor supply, especially in formalized employment, and decreasing time spent doing household chores. This lower-bound to enrollment, which was established in 2018, is a part of a larger reform in Brazil's primary education that began in 2006, described in the next section.

To contribute to the estimates of the relationship between childcare availability and mother's labor supply, I exploit a previous step of the school system reform, which established an increase in compulsory primary schooling years from 8 to 9. Relative to other policy changes used as identification strategies, such as the lower-bound in age to admissions before its federal adoption, this has the advantage of offering an easily observable variable, which is available for all schools during the (2004-2007) period: the adoption of the 9-year curriculum. Because this transition took place in a staggered manner and was made official at the federal level in 2006, I have data on share and length of exposure to these changes. I estimate these relationships through a triple differences model (DDD) in a panel data of individuals from 6 Metropolitan Regions.

3 Context

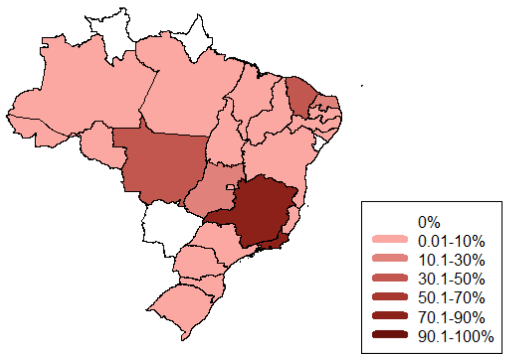
3.1 Brazil's Curriculum Reform

While the primary school system in Brazil initially consisted of a 8-year curriculum starting at the age of 7, from 1996 onwards local governments and academic institutions were granted the possibility to include the final year of pre-school in their primary school curriculum. Initially, most schools did not change the school-entering age. However, in 2004, the state of Minas Gerais determined that the basic education period would now expand to 9 years and include children from the age of

6. Following Minas Gerais' positive experience, the Federal Government established the 9-year primary school curriculum in 2006, prompting all schools to adhere to it by 2010.

3.1.1 Reform by State

I exploit this staggered change from a 8-year compulsory primary school system in Brazil to a 9-year one to conduct my analysis. Because the change happened at different times and with different intensities in various states, I can consider both differences in the percentage of schools that have adopted the 9-year curriculum and in the years of exposure to such treatment.



Year at which reached 15% of schools under new curriculum	Metropolitan Region
2004	Rio de Janeiro, Minas Gerais
2005	
2006	Salvador, Porto Alegre
2007	São Paulo, Recife

Figure 1: Reform Adoption by State in 2004(left), first year of 15% Adoption (right)

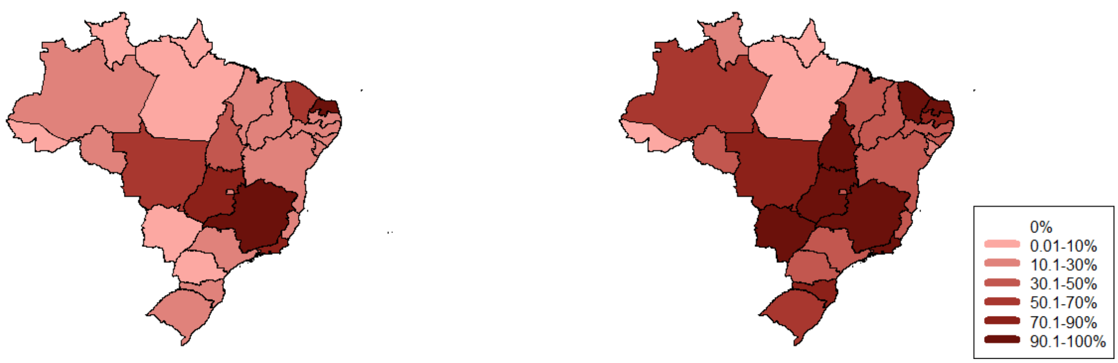


Figure 2: Reform Adoption by State in 2006 (left) and 2007 (right)

Figure 1 shows that the initial compliance levels in 2004 were low in most States, with the exception of Minas Gerais and Rio de Janeiro. Figure 1 also indicates the year in which the Metropolitan Regions of the PME panel data achieved a share of school adherence to the new system that was higher than 15%, which I arbitrarily chose as the threshold for it to be considered exposed to the change (reaching 15% would consititute a first year of treatment). Figure 2 shows the variation in share and exposure to the policy change, showing that in 2006 a majority of Brazilian States still had very low adoption. Of the Metropolitan Regions considered in the study, São Paulo and Recife only passed the 15% mark in 2007 and Salvador and Porto Alegre, although already exposed since 2006, had a significant jump in share of compliant schools in 2007.

3.2 Brazil's Gender Inequalities in Labor Outcomes

3.2.1 Oaxaca-Blinder Decomposition

As mentioned in the introduction, gender gaps in labour outcomes in Brazil are considerably high, in spite of a better school performance of women in the country. It would then follow that the unexplained part of the income difference should be high enough to compensate for the differences in educational attainment and still have women have lower salaries. To get a descriptive idea of what the differences in earnings look like within the PME, and of how I could expect the responses of fathers and mothers to differ with the increase in childcare provision, I plotted a Oaxaca-Blinder decomposition (Figure 3), which describes the magnitude of the unexplained part of income differentials, by comparing how much men and women's incomes differ at the same levels of education.

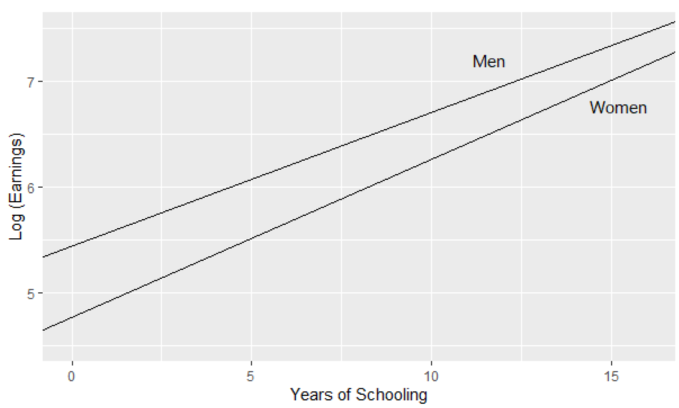


Figure 3: Unexplained differences in earnings

Indeed, unexplained differences tend to be high, as men with the same amount of schooling have much higher wages than women, on average. While the returns to education seem roughly comparable, women appear to only have a chance of catching up to their male counterparts at very high levels of education. This more or less echoes estimates found by Madalozzo (2010), which suggest that women only fare better than men at very high levels of education. While merely descriptive, these estimates provide some base for the idea that I should expect fathers of young children to be a fair robustness test for my estimation, as they do not seem to be subject to the same residual constraints that may be hampering women's labour outcomes.

4 Data

4.1 School Census (CE)

The School Census (CE) provides census data on all schools in Brazil, including information on their public/private status, on whether they followed the 8 or the 9-year primary school curriculum and on which institutions provide pre-school and day-care services. I used the data from 2003 to 2007 to track the level of adoption in each state during the period. However, because the panel data from PME used in the analysis only covered the years of 2006 and 2007, the School Census data was used to construct a variable indicating the percentage of schools that followed the 9-year curriculum in each Metropolitan Region in 2006 and 2007 and the percentage that offered pre-school and daycare services in 2006. Data from adoption in previous years was used to calculate the period of exposure to the policy, with an arbitrary level of 15% of schools offering 9 years of primary schooling being considered the first year of exposure. While the CE offered data disaggregated at the Municipality level, all variables constructed from this sample were aggregated at Metropolitan Region level to match the disaggregation level of the PME.

4.2 Monthly Labour Survey (PME)

The Monthly Labour Survey (PME) is a discontinued sample survey which provides monthly employment data on individuals in six Metropolitan Regions in Brazil (Recife, Salvador, Rio de Janeiro, Minas Gerais, São Paulo and Porto Alegre). The PME follows individuals for an initial period of 4 months, then stops observations during 8 months and provides survey data on the individuals for 4 additional months. I used the 2006 and 2007 PME to construct a panel data with all individuals that had at least one observation in 2006 and one in 2007.

From the PME, three different databases were created: one that included all women with children between 0 an 10 years old (unrestricted), a second one including only information about women with children between the ages of 4 and 6 in 2006 (restricted). A third database, including only men with children between the ages of 4 and 6 in 2006, was created to be used as a robustness check. I give preference to the results found in the restricted samples because by using individuals with children of roughly the same ages, although only those that were 5 in 2006 would be treated, we remove confounders that may be common to women of similar-aged children, as suggested by Cascio (2009).

4.3 Summary Statistics

4.3.1 Balance of Women's Characteristics by group

From the table below, it is possible to see that women in the unrestricted sample (those with children between 0 and 10 years of age) appear to be significantly different in the groups that have a 5-year old in 2006 and those who do not, both in basic individual characteristics and in variables more specific to their employment status. This suggests that estimates obtained for the entire sample of women are most likely biased. The entire balance table for the unrestricted sample can be found in the appendix.

On the other hand, restricting our sample to only women with 4 to 6 year olds in 2006 appears to provide for a surprisingly good level of comparability. Individuals in comparison groups (mothers of young children who have a child that is 5 in 2006 and those who do not) do not appear to have, in general, characteristics that are significantly different from each other in 2006. Except for the number of individuals in the household, characteristics seem to be balanced at the baseline, suggesting the two groups can be compared for my estimations. This falls in line with Cascio (2009), which suggests that individuals with children of around the same age would be subject to roughly similar limitations and possibly also to similar age-targeted government benefits.

	Mothers with children between 4 and 6 in 2006					**Mothers with children between 0 and 10 (Unrestricted Sample)**
	One child is 5 in 2006		**No child is 5 in 2006**		**Differences in Means 2006**	**Differences in Means 2006**
	2006	*2007*	*2006*	*2007*		
Characteristics						
Age	**31.42364**	**31.39836**	**32.29197**	**32.37591**	**.0252769**	**-.9292785**
	(7.336985)	*(7.632095)*	*(7.342213)*	*(7.51339)*	*(.2152599)*	(.1499339)
White=1	**4.469418**	**4.722601**	**4.566504**	**4.408427**	**-.0013868**	**.0213018****
	(1.540465)	*(1.980431)*	*(1.552513)*	*(1.834047)*	*(.0143792)*	(.0100642)
Number of individuals in house	**0.467917**	**0.469304**	**0.475669**	**0.464831**	**-.2531828 *****	**-.4594873*****
	(0.499063)	*(0.49917)*	*(0.498844)*	*(0.499509)*	*(.0505057)*	(.0373227)
Finished High School=1	**0.090624**	**0.102086**	**0.110698**	**0.094548**	**-.011462**	**.0159509*****
	(0.287138)	*(0.302844)*	*(0.292646)*	*(0.31383)*	*(.009295)*	(.0066726)
Can read=1	**0.978987**	**0.974079**	**0.979319**	**0.979429**	**.0049077**	**.0100926*****
	(0.143455)	*(0.158935)*	*(0.141966)*	*(0.142344)*	*(.0043402)*	(.0025294)
Labour Characteristics						
Employed=1	**0.53823**	**0.530192**	**0.555147**	**0.540207**	**.0080376**	**.0027003**
	(0.49863)	*(0.499202)*	*(0.498464)*	*(0.497051)*	*(.0144083)*	(.0101277)
Non-paid labour=1	**0.006354**	**0.011396**	**0.015721**	**0.011773**	**-.0050418**	**-.0060079****
	(0.079492)	*(0.106193)*	*(0.1079)*	*(0.124446)*	*(.0038675)*	(.002822)
Earnings	**686.9**	**710.9**	**635.4**	**688.2**	**-51.58797**	**-2076184**
	(885.6)	*(786.6)*	*(711.3)*	*(766.6)*	*(35.52577)*	(2333027)
Formal Labor=1	**1.412545**	**1.39607**	**1.428975**	**1.420133**	**.0164294**	**-.0321483****
	(.4925894)	*(.4893324)*	*(.4951626)*	*(.4937855)*	*(.0228916)*	(.016586)
Minimum N	829	967	1,063	1,202		

5 Methodology

5.1 Main Model

Since I have data on percentage of schools that have adopted the new curriculum and on how long each area has been exposed to the curriculum change at Metropolitan Region level, I estimate a DDD model exploiting the differences in reach and in time exposure between those that received treatment and those that did not (respectively, women with and without children that were 5 years old in 2006). As mentioned before, I do so in both the unrestricted and the restricted panel data sample of women who had 4-6 year olds in 2006.
The main model I estimate is the following:

$$\begin{aligned} P(Outcome_{irt} = 1) = \alpha_{irt} + \gamma_t + \theta_r + \alpha_2 X_{irt} + \beta_1 Share_{rt} + \beta_2 Exposure_{rt} + \beta_3 Treat_{irt} \\ + \beta_4 Treat_{irt} * Share_{rt} + \beta_5 Treat_{irt} * Exposure_{rt} + \beta_6 Share_{rt} * Exposure_{rt} \\ \beta_7 Share_{rt} * Exposure_{rt} * Treat_{irt} + \epsilon_i \end{aligned} \tag{1}$$

I include year, individual and metropolitan region fixed-effects as well as a vector X of individual characteristics (if the person is white, if the person can read and their age). ”Share” represents the percentage of schools that have adopted the 9-year curriculum in the metropolitan region, ”Exposure” represents the number of years since the metropolitan region has reached 15% of schools having adopted the primary school reform. ”Treat” represents whether or not the woman has a 5 year old child in 2006, which will be affected in 2007, within the sample of women that has children between 4-6 in 2006. I report the estimates for the triple-difference coefficient in the Results section.

If the policy change had the effect of increasing labour outcomes in women with affected children, I would expect the triple-differences interaction coefficient (beta 7) to have a significant impact on probability of the outcome variables. I use three different binary variables as ”Outcome”: Employment, Formal Employment if Employed, and Unpaid work. Additionally, I also run the same regression using earnings as dependent variable.

$$\begin{aligned} Earnings_{irt} = \alpha_{irt} + \gamma_t + \theta_r + \alpha_2 X_{irt} + \beta_1 Share_{rt} + \beta_2 Exposure_{rt} + \beta_3 Treat_{irt} \\ + \beta_4 Treat_{irt} * Share_{rt} + \beta_5 Treat_{irt} * Exposure_{rt} + \beta_6 Share_{rt} * Exposure_{rt} \\ \beta_7 Share_{rt} * Exposure_{rt} * Treat_{irt} + \epsilon_i \end{aligned} \tag{2}$$

5.2 Robustness Test

To check if these effects are driven by some other factors within a family or community, I estimate the same model as in equations 1 and 2 using a sample that includes only men with children between

4-5 years of age in 2006. I assume that this sub-sample would be subject to roughly the same labor market trends and welfare benefits targeted at children. However, as men are often not burdened with the implicit costs associated with childcare, I would not expect the policy change to have any significant impacts on outcomes.

6 Results

6.1 Main Model

I estimate the same model in both the restricted and unrestricted sample. I would expect the estimates from the unrestricted sample to be smaller in magnitude and less significant. However, taking into account the results in section 4.3, the unrestricted estimates are to be taken with a grain of salt, due to the high likelihood that they are biased.

The first column in table 2 show the DDD coefficient in the unrestricted sample. As expected, increasing the exposure time and the share of adherent schools for mothers with 5-year old in 2006 does not have a significant effect on the probability of employment or monthly earnings of the entire sample, as those that have younger children are not likely to benefit as much from the policy change. Similarly, while maintaining a high level of significance, point estimates for the effect of the treatment on the probability of formal employment are much lower (15 pp lower). Perhaps more surprisingly, the treatment seems to have a positive significant effect on the probability of doing unpaid chores. Since the effect is small and the pattern does not repeat itself in the restricted estimates, I do not think this is enough to argue that there could be a true increase of unpaid work, especially since this variable was initially unbalanced in the unrestricted sample.

The estimates in column 2, on the other hand, are the ones I would give more importance to, both based on common sense and on the initial t-tests of covariates. Column 2 indicates that women with 5 year olds in 2006 living in Metropolitan Regions that have been exposed to the curriculum change for longer and at a higher share have a higher probability of being employed and of participating in the formal labour market. Indeed, the treatment seems to be linked to 10.7% higher probability of being employed in a given week and a 32.4 % higher probability of having formal work contracts. These results are similar in magnitude to the estimates found in Gelbach (2002) and Cascio (2009). However, because I did have information on marital status in the PME panel data, it is not possible to see how these outcomes differ for married and single mothers. On the other hand, we see no significant effects on the probability of working unpaid hours and on monthly earnings.

Dependent Variables	Women - Entire Sample (children 0 to 10)	Women - Reduced Sample (children 4 to 6 in 2006)	Placebo Model (Men)
	DDD Coefficient	**DDD Coefficient**	**DDD Coefficient**
	(Share* Exposure) Comparison Group	(Share* Exposure) Comparison Group	(Share* Exposure) Comparison Group
Employment previous week = 1	**.0037504**	**0.1072128 *****	**.0125558**
	(.0421337)	*(.0496374)*	*(.0477911)*
Formal Employment = 1	**.1714911 *****	**.3249392 *****	**.0135151**
	(.0680615)	*(.0762952)*	*(.0731261)*
Non-paid work previous week = 1	**.0287216 *****	**.0762952**	**-.009415**
	(.0119719)	*(.0160329)*	*(.0343931)*
Monthly Earnings	**6048675**	**-66.34875**	**101.1235**
	(9221522)	*(117.184)*	*(157.1475)*
Controls			
Individual Characteristics (X)	X	X	X
Year Fixed Effects	X	X	X
Individual Fixed Effects	X	X	X
Metropolitan Region Fixed Effects	X	X	X
N Observations (smallest)	11,376	3,611	1,525

6.2 Robustness Test

As expected, the results in column 3 indicate that the triple-difference coefficient does not affect the outcomes of fathers of children between 4-5 years old in 2006, suggesting that the treatment effect captured for their female counterparts could indeed be attributed to the alleviation of the higher child-caring burden placed on women when childcare becomes more readily available and affordable.

7 Conclusion

I find evidence corroborating that the change to a longer primary school curriculum in Brazil relaxed childcare constraints felt by women, as treatment is associated with higher probabilities of employment and of formalized contracts. I cannot, however, disentangle how much of this effect originated from the increase of childcare supply and how much was due to implicit subsidies, as I do not have sufficient data to do so.

Generally, the estimates I find for the probability of employment are very consistent with evidence from the United States and Argentina, both in magnitude and in direction of the effect. Indeed, increasing exposure by one year and the share of compliant schools from 0 to 100% seems to be linked with a 10.7% higher probability of being employed in a given week. I do not, however, find any association between treatment and weekly wages.

Comparing my results to Ryu's (2009) findings in Brazil, I also find a similar increase in the probability of formalized contracts that guarantee employment benefits ("Carteira Assinada"). Indeed, treatment is linked to a 32.4 % higher probability of having formal work contracts for mothers of 6 year old children in 2007. These results are very policy relevant, as Brazil faces a growth of informal employment and precarious work contracts. Contrary to findings in Ryu (2019), I was not able to find an association with a decrease in the probability of performing domestic chores, possibly due to the low granularity of the variable found in the PME ("performed unpaid activities last week").

Overall, estimates found seem to provide good confirmation that women in Brazil face constraints in their labor choices due to the disproportionate responsibilities towards childcare and household chores, as my estimates are similar to the existing evidence. Furthermore, my results are supported by the fact that the same regressions responded as expected to changes in samples, returning non-significant and smaller estimates for groups that were not believed to have been affected in the same way by the changes: namely, mothers with children younger than 5 in 2006 and fathers. Additionally, the initial balance of covariates in my restricted sample indicates that the two groups are roughly comparable.

Nevertheless, my analysis is not granular enough to separate the effect felt by single and married mothers and by those that have pre-existing childcare, calling for further studies to more carefully analyze intra-houshold patterns that may affect women's labor supply, such as marriage status, reliance on grandparents and how changes in retirement policy and government cash transfers to the elderly may affect informal childcare arrangements.

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9 Appendix: Covariate Balance - Unrestricted Sample

Mothers with children between 0 and 10					
	One child is 5 in 2006		**No child is 5 in 2006**		**Differences in Means 2006**
	2006	*2007*	*2006*	*2007*	
Characteristics					
Age	**30.66459**	**31.48704**	**29.73531**	**30.51246**	**-.9292785**
	(7.487285)	*(7.318994)*	*(7.411878)*	*(7.372987)*	*(.1499339)*
White=1	**0.4483619**	**0.4582373**	**0.4696638**	**0.46993**	**.0213018****
	(0.497404)	*(0.4983246)*	*(0.4991026)*	*(0.499116)*	*(.0100642)*
Number of individuals in house	**5.107644**	**4.978687**	**4.648157**	**4.583135**	**-.4594873*****
	(2.121163)	*(1.999886)*	*(1.75815)*	*(1.707574)*	*(.0373227)*
Finished High School=1	**0.0917431**	**0.0925684**	**0.107694**	**0.10834**	**.0159509*****
	(0.288716)	*(0.289874)*	*(0.3100108)*	*(0.310824)*	*(.0066726)*
Can read=1	**0.9762871**	**0.9781106**	**0.9863797**	**0.987577**	**.0100926*****
	(0.1521771)	*(0.1463435)*	*(0.1159142)*	*(0.110768)*	*(.0025294)*
Labour Characteristics					
Employed=1	**0.4904058**	**0.5068175**	**0.4931061**	**0.492061**	**.0027003**
	(0.4999866)	*(0.5000261)*	*(0.4999764)*	*(0.499958)*	*(.0101277)*
Non-paid labour=1	**0.0149701**	**0.0134907**	**0.0089622**	**0.00917**	**-.0060079****
	(0.1214693)	*(0.1153959)*	*(0.0942521)*	*(0.095328)*	*(.002822)*
Earnings	**6684031**	**4549603**	**4607847**	**4012565**	**-2076184**
	(81500000)	*(67300000)*	*(67700000)*	*(63200000)*	*(2333027)*
Formal Labor=1	**1.42236**	**1.415049**	**1.390212**	**1.385776**	**-.0321483****
	(0.4941546)	*(0.4929298)*	*(0.4878615)*	*(0.486834)*	*(.016586)*
Minimum N	1,127	1,236	3,821	4,373	