INSTITUTE FOR
THE EVALUATION
OF PUBLIC POLICIES

FONDAZIONE
BRUNO KESSLER

FBK-IRVAPP Working Paper Series

Working Paper No. 2025-01

# Children's Educational Enrollment and Maternal Labor Supply

Clemente Pignatti
Alessandro Tondini

January 2025

FBK-IRVAPP Working Paper No. 2025-01

# Children's Educational Enrollment and Maternal Labor Supply

**Clemente Pignatti**
University of Milan
clemente.pignatti@unimi.it

**Alessandro Tondini**
FBK-IRVAPP
atondini@irvapp.it

INSTITUTE FOR THE EVALUATION OF PUBLIC POLICIES
FONDAZIONE BRUNO KESSLER

Institute for the Evaluation of Public Policies
Fondazione Bruno Kessler
Via Santa Croce, 77
38122 Trento, Italy
https://irvapp.fbk.eu

# Children's Educational Enrollment and Maternal Labor Supply*

Clemente Pignatti, Alessandro Tondini

**Abstract**

We investigate the impact of a reform in South Africa anticipating children's entry into primary school on children's school enrollment and mothers' labour supply. We use Census data and exploit month-of-birth discontinuities and the before/after variation introduced by the reform. We report a net increase of 7pp. in school attendance at age 5. However, contrary to an established finding in the literature, we find no impact on mother's employment and the type of jobs held. We reconcile our finding with those of previous studies by noting that South Africa is characterized by relatively high initial rates of school attendance and relatively low rates of maternal employment. In districts where these contextual factors are more similar to previous studies, we find that higher enrollment does lead to higher maternal employment.

**JEL Classification:** I28, J13, J16
**Keywords:** Childcare, Maternal labor supply, South Africa

* We thank Michele Pellizzari for helpful feedback. All remaining errors are ours.

# 1 Introduction

Around the world, women have lower employment rates than men as well as lower earnings upon employment. This is unfair from a normative perspective, but also inefficient. An important source of these gender inequalities is related to the impact of childbirth on women's labor market outcomes, or the so-called child penalty (Kleven et al., 2019). The relative size of the child penalty varies across countries, depending on the level of economic development and rate of urbanization, among others. In South Africa, our country context, the birth of a child is associated with a reduction in female employment by 30 per cent. This can explain one third of the country's gender gap in employment (Kleven et al., 2023).

Against this backdrop, the attention of researchers has focused on what policy options are available in order to keep women attached to the labor market after the birth of a child. In most countries, this critically depends on the availability, affordability and quality of childcare services. A vast body of evidence from both developed and developing and emerging economies has studied the effects of increasing children's access to education on maternal labor supply. In virtually all cases, the results of these studies show a positive effect on either women's employment probability or the quality of the jobs found (see Halim et al. (2023) for evidence from a recent meta-analysis specific to emerging and developing countries). These positive results have motivated governments worldwide to expand childcare coverage as a means to stimulate female employment.

This paper explores the effect on children's school attendance and maternal employment of a reform in South Africa that lowered school admission age. Specifically, the reform allowed children born in the first half of the year to anticipate their enrollment in primary education (or in a reception year, known as grade R). Instead, school admission age was unchanged for children born in the second half of the year. Additionally, the school compulsory age (i.e., when children are obliged to be enrolled) was not affected. The paper uses data from the 2011 Census and the 2007 Community Survey, and exploits discontinuities in the school admission age generated by the reform for each cohort at June 30, together with the before and after variation introduced by the policy, to causally identify the effect on children's enrollment and mothers' employment.

We find that, at the margin, the reform had a positive impact on overall school attendance (+7 percentage points, pp.). This is a sizeable effect, given that a high share of pupils was already attending education (above 70 per cent in the control group) and this mechanically capped the potential effect on overall school enrollment. The effect comes primarily from an increase in primary school attendance (+9 pp.), with only limited reallocation from pre-school (-2 pp.) to primary school. The positive effects on school attendance dissipate over one school year. This is expected, as school enrollment approaches 100 per cent as children grow and school attendance becomes mandatory.

At the same time, and contrary to an established finding in the literature, we find that the increase in school enrollment does not lead to higher employment for mothers of affected children. Specifically, we are able to rule out both contemporaneous or lasting effects of comparable magnitude of what has been found in the literature. Additionally, we do not find any effect on mothers' job search behavior or the self-reported reasons for not working; including the share of women not working because of household responsibilities. We also do not see results on the type of jobs held (i.e., whether individuals work formally or informally, and whether they are in wage employment or self-employment) or the quality of the jobs found (e.g., as measured, among others, by earnings and hours worked).

In the last part of the paper, we try to reconcile our findings with those of previous studies. To start with, we note that our study context is characterized by relatively high initial levels of school enrollment and relatively low initial levels of female employment, compared to values reported in other studies. We then find evidence that these differences in contextual factors might explain the difference in the results on maternal employment. Specifically, we find a positive (negative) association between our estimates on maternal employment and the district-level baseline values of maternal employment (children's school attendance). Additionally, we find positive effects on maternal employment in a sub-sample of South African districts with higher initial employment and lower initial school attendance, that is more comparable to the samples used in previous studies.

We contribute to a large literature exploring the relationship between childcare and mothers' labour supply. Previous studies have found a positive effect of childcare availability or eligibility on mothers' labour supply, in spite of a strong heterogeneity in terms of context and policy setting, the type of childcare, and the age of the child affected. In developed countries, several studies have identified a positive effect of childcare on mother's labor supply: for example, see Gelbach (2002), Cascio (2009) and Fitzpatrick (2010) in the United States[1]; Finseraas et al. (2017), Bauernschuster and Schlotter (2015) and Bousselin (2022) in European countries; as well as Del Boca (2015) and Morrissey (2017) for reviews of the

[1]Among the studies mentioned, Fitzpatrick (2010) is the only one who does not find a positive effect on labour supply of mothers, when providing pre-kindergarten care for very young children.

literature. In emerging and developing countries, a recent review of the literature draws very similar conclusions across a wide set of studies (Halim et al., 2023): among "22 studies which plausibly identify the causal impact of institutional childcare on maternal labor market outcomes in lower-and-middle income countries. *All but one study finds positive impacts on the extensive or intensive margin of maternal labor market outcomes*".[2]

We contribute to this literature in two important ways. First, we provide an important benchmark for the vast literature on the effects of childcare on maternal employment, by showing that the positive effect might be contingent on contextual factors, such as initial maternal employment levels and children's' school attendance rates. In this way, we show that the positive relationship between children's school enrollment and maternal labor supply does not hold under all circumstances, but it is rather reliant on the specific characteristics of the context analyzed. When these characteristics differ, other types of interventions can be more useful, depending on the existing constraints to female employment.[3]

Second, we examine the long-term effects of an expansion in childcare availability. In particular, our empirical strategy allow us to recover effects on both child school attendance and maternal labor supply spanning 7 years after the differential exposure to primary school access for the child. To the best of our knowledge, only one study in the literature in developing countries is able to estimate effects on mothers' employment beyond 1 or 2 years (Attanasio et al. (2022), see literature summary in Table B3 in the Appendix). Therefore, we are able not only to rule out contemporaneous effects in our setting, but also potentially lasting effects that might occur, for example, through more extensive or better job search.

The paper is organized as follows. Section 2 presents the institutional background and the reform at the center of the analysis; Section 3 presents the data used and some descriptive evidence; Section 4 presents the identification strategy and the empirical specification; Section 5 introduces the main results of the paper; Section 6 discusses the main mechanisms behind these results; while Section 7 concludes.

[2]We summarize these studies (with the addition of others that fulfill the criteria) in Appendix Table B3.

[3]Our results would suggest that, at this level of pre-school enrollment and maternal employment, other policies reducing different kind of frictions might be more effective or necessary to increase female employment (Tondini (2022), Banerjee and Sequeira (2023) to increase job search; Abel et al. (2020) to increase employment through reference letters, Abel et al. (2019) to increase employment through plan making.)

## 2 Institutional background

In South Africa, the school year runs from January to December, following the calendar year. Primary and secondary education is organised in 13 grades (grade R, and grades 1 to 12), where grade R (or grade 0, originally) indicates a "reception" grade marking the transition from pre-school to school, or, in general, the preparation for primary school. Until recently, grade R was not mandatory and was delivered by most primary and some pre-schools, but not all.[4] Over the period of our study, although not mandatory, there was a massive expansion of Grade R coverage of South African students (Van der Berg et al., 2013).[5]

Grade R and Grade 1 roughly cover half of the day over 5 days. Specifically, Grade R covers 23 hours of instruction time in a given week, while Grade 1 is up to 25 hours.[6] Instead, pre-schools (also known as ECD centres) tend to cover a larger portion of the day, although this is much more heterogeneous.[7] Moreover, contrary to primary schools, most pre-schools (94%) charge some type of fees to pupils, although "most (62%) of them also allow at least some children to attend the ECD programme without having to pay a fee."[8]

**Compulsory Age** The *South African School Act of 1996*[9] is the founding act of the post-Apartheid South African school system, and specifies age boundaries for *compulsory* education, which is when a parent must send the child to school. The law implies that schooling is compulsory from the calendar year in which a learner turns 7.[10] Given that in South Africa the school year coincides with the calendar year, everybody who is 6 and turns 7 before December 31st must be in school in that year.

[4]In 2022, the government has set for grade R to be part of compulsory education for all students (Basic Education Laws Amendment Bill (B2-2022)), https://www.parliament.gov.za/bill/2300398

[5]As a result, the share of 5 years old children attending some form of education increased from 39% in 2002 to 78% in 2009.

[6]*National Policy pertaining to the Programme and Promotion Requirements of the National Curriculum Statement Grades R-12* https://www.education.gov.za/Portals/0/Documents/Policies/PolicyProgPromReqNCS.pdf

[7]According to the 2021 census of Early Childhood Development centers in South Africa, 64% of pre-schools closed after 4 pm (and the remaining still covered a portion of the afternoon), with a large majority also opening before 8am.

[8]https://datadrive2030.co.za/wp-content/uploads/2022/09/ecdc-2021-report.pdf

[9]Original: https://www.gov.za/sites/default/files/gcis˙document/201409/act84of1996.pdf

[10] *"..from the first school day of the year in which such learner reaches the age of seven years until the last schoolday of the year in which such learner reaches the age of fifteen years or the ninth grade, whichever occurs first".*

**Admission Age** The 1996 law did not directly specify an *admission* age, i.e., the age when children have to be admitted to school, even before compulsory education kicks in.[11] This has first been specified in 1998, with an implementation starting in 2000[12], where the law specifies that a learner may be admitted to grade R only if she/he turns 6 by that calendar year, and must be admitted to grade 1 if he/she turns 7 by the end of that calendar year.[13] Therefore, since the year 2000 at least, compulsory age was equal to admission age for Grade 1 (the year in which a kid turns 7). For grade R, attendance is not compulsory but admission age is in the year in which a kid turns 6.

In 2002, the government decided to change the admission age of pupils to schools.[14] The new law[15] stated that children should be admitted to Grade R at age 4 in January of a given school year if they turn 5 by June 30, or the following year if they are born after June 30. Similarly, they should be admitted to Grade 1 at age 5 in January of a given school year if they turn 6 by June 30, or the following year if they are born after June 30. This means that the reform lowered the admission age for some kids, but not for others born in the same year. This change occurred *without* a change in compulsory education, which means that students were allowed to enroll and had to be admitted, but were not *obliged* to do so. There

[11] "The Minister may by notice in the Government Gazette.. determine age requirements for the admission of learners to a school or different grades at a school."

[12] "Age requirements for admission into an ordinary public school" https://archive.gazettes.africa/archive/za/1998/za-government-gazette-dated-1998-10-19-no-19377.pdf.
It is not clear what the norm was between 1996 and 2000, but it is likely that is was the same as what stated afterwards.

[13] The exact phrasing of the different articles of the law is: " *The statistical age norm per grade is the grade number plus 6. Example: Example: Grade 1 + 6= age 7 Grade 9+6 = age 15 Grade 12+6=age 18* "

"*A learner must be admitted to grade 1 if he or she turns seven in the course of that calendar year. A learner who is younger than this age may not be admitted to grade 1.*"

"*A learner may be admitted to grade. R only if he or she turns six in the course of that calendar year. Attendance of grade R is not compulsory.*"

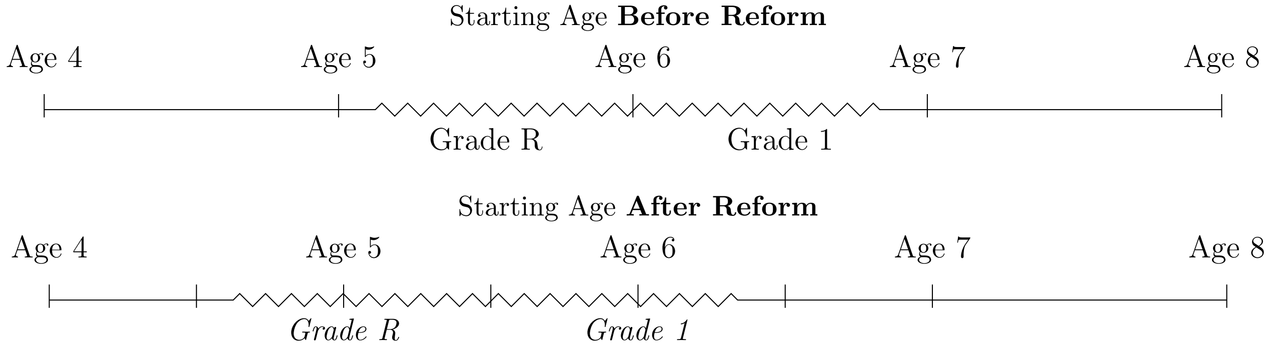
"*This notice is called the Age Requirements for Admission to an Ordinary Public School, and it comes into effect on 1 January 2000*"

[14] It is unclear exactly what brought about the reform. One reason might have been that the admission age set by the government gazzette in 1998 (with application) in 2000, where challenged by pupils in some independent schools, who would have had to postpone entry into Grade 1 (Court case here: https://www.saflii.org/za/cases/ZACC/2001/25.html). It seems that for this reason the government decided to more clearly change the original 1996 law itself, rather than just publishing a decree. The new 2002 law applies both to public and independent schools, with the same threshold. In doing so, it also decreased by 6 months the age admission threshold

[15] The *Education Laws Amendment Act 50 of 2002* (coming into operation on January 1st 2004, available at https://www.gov.za/sites/default/files/gcis˙document/201409/a50-020.pdf) specifies the following provisions for admission age at school:

*"The admission age of a learner to a public school to (i) grade R is age four turning five by 30 June in the year of admission; (ii) grade 1 is age five turning six by 30 June in the year of admission"*

**Figure 1:** Expected effect of the reform on starting age




**Notes:** The figure reports the expected effect on children's starting age for both Grade R and Grade 1, before and after the implementation of the *Education Laws Amendment Act of 2002* (which came into operation in 2004).

are, to our knowledge, no provision regarding pre-school.

**Expected Effects on Age Starting School** Figure 1 outlines how the reform impacts the age of pupils starting Grade R and Grade 1. Before the reform, kids starting Grade R in January of a given school year were always 5 years old, and those starting Grade 1 were always 6 years old. After the reform, kids can start Grade R at age 4, if they are born before June 30, and Grade 1 at age 5, if they are born before June 30.

The reform is printed in the official gazette in 2002, and implemented as of January 1st 2004. For this reason, children born in 1998 are not impacted by the reform: when the reform kicks in, they are already too old to start school with the new age requirements. The first impacted cohort is that of children born in the first half of 1999; they can start Grade R according to the new rules together with those born in the second half of 1998.

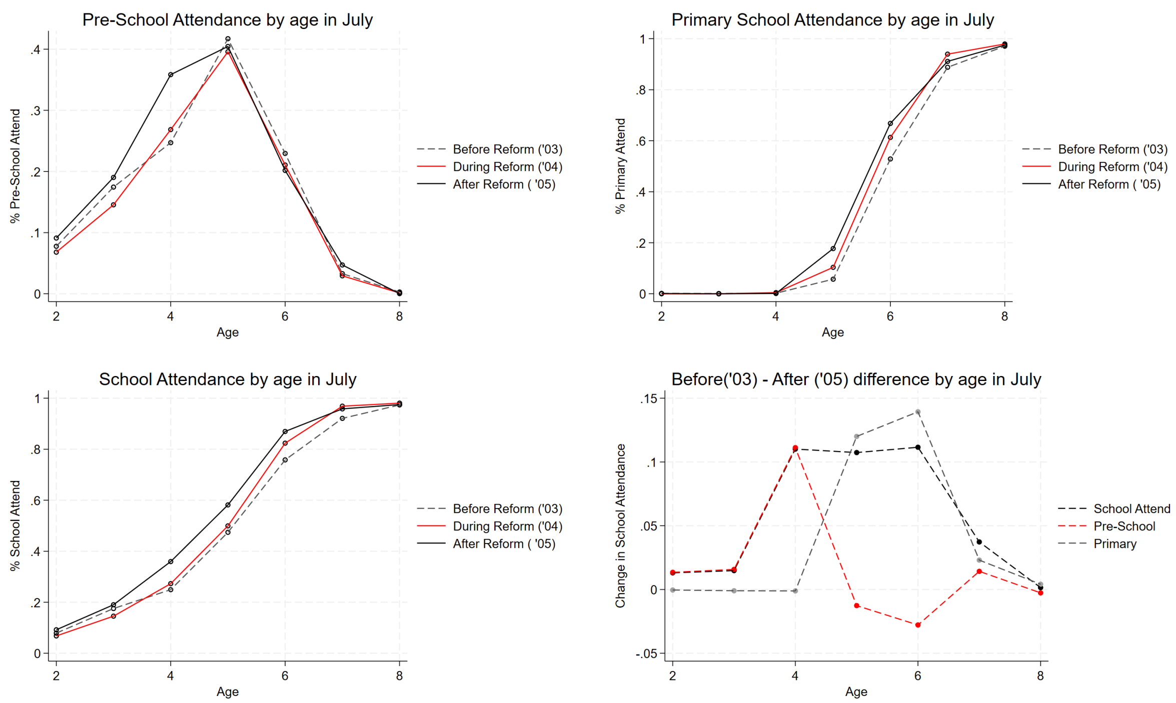
**Descriptive Evidence on the Impact of the Reform on School Enrollment** The effect of the reform on school enrollment is clearly visible from a variety of surveys. In Figure 2, we plot some descriptive statistics calculated on the General Household Survey (GHS). The GHS is a nationally representative survey with a yearly rotating panel, starting as early as 2002, hence before the implementation of the reform. While the survey does not contain information on the exact date of birth of the respondent, sampling always takes place in the month of July. Therefore, even if only a discrete variable indicating age is available in the data, we know that if a respondent is aged 5 in July it has to be because he/she turned 5 *before* June 30. This allows us to use age vales that match those specified by the reform.

In Figure 2, we plot pre-school attendance (panel A), primary school attendance (panel B), overall (pre + primary) school attendance (panel C), and the before-after difference in these shares (panel D). We plot these shares by age (from 2 to 8, indicated on the x-axis) and in the years before (2003), during (2004), and after (2005) the reform. Despite a general increase in school attendance over time, the graphs clearly show a greater increase in the age values treated by the reform: age 5 (turning 5 before June 30) and age 6 (turning 6 before June 30). The overall increase in these age values is around 7-8 pp. relative to unaffected age-values. This effect on increased attendance comes entirely from increased attendance of primary school at both age 5 and 6.

However, alongside the increase at age 5 and 6, which are the values specified by the reform, we also observe an increase at age 4 of similar magnitude. By decomposing between primary and pre-school, we see that this is due to an increase in pre-school at age 4, while it is mostly accounted for by primary school at ages 5 and 6. We interpret the increase at age 4 as an indirect consequence of the reform: on the one hand, because attendance of pre-school at the treated age values (ages 5 and 6) decreases slightly, this potentially "frees-up" some slots in pre-school; on the other hand, with primary school starting at age 5 for some, pre-schools might begin accepting kids at younger ages. If this interpretation is correct, this is a cohort effect, rather than a direct effect of the reform. In line with this interpretation, we should not see this effect in the main results of the paper, where we compare individuals within the same cohort, which differ only based on the month of birth.

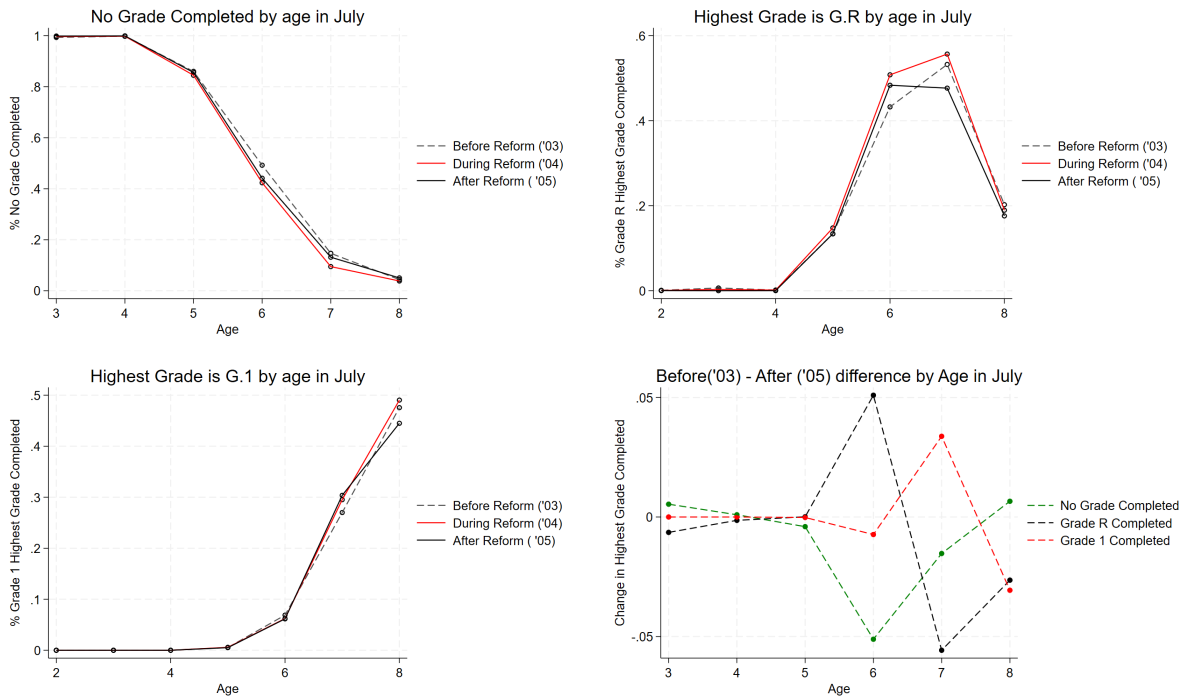
The effect of the reform is also clearly visible when looking at information on the highest grade completed. Specifically, in Figure 3 we plot the share of individuals who have completed at most a given degree at a given age (reported on the x-axis), before (2003), during (2004) and after (2005) the reform. We present this information in levels separately for no grade (panel A), Grade R (panel B) and Grade 1 (panel C) as well as in difference (before vs after) for all the three grades (panel D). The data shows that the reform increased the share of kids of age 6 in July reporting to have completed grade R, while decreasing the proportion of those who report that they have not yet completed any grade. Similarly, the reform increases the share of students of age 7 reporting to have completed Grade 1.

**Figure 2:** School enrollment by age, GHS 2003–2005




**Notes:** The figure plots the rates of school attendance for pre-school (Panel A), primary school (Panel B) and overall school (i.e. pre-school and primary school, Panel C) as well as the difference in these three rates before and after the reform at the centre of this study (Panel D). In each panel, the information is reported separately by age of the child (which is plotted on the x-axis). Information comes from different waves of the GHS, which takes place in July of each year.

**Figure 3:** Grade completed by age, GHS 2003–2005




**Notes:** The figure plots the share of children with no grade completed (Panel A), whose highest completed grade is Grade R (Panel B) and whose highest completed grade is Grade 1 (Panel C) as well as the difference in these three rates before and after the reform at the centre of this study (Panel D). In each panel, the information is reported separately by age of the child (which is plotted on the x-axis). Information comes from different waves of the GHS, which takes place in July of each year.

# 3 Data

**Census 2011** We run the bulk of the analysis on the 2011 Census data. The 2011 Census data is a 10% publicly-available dataset extracted from the population census, which in South Africa occurs every 10 years. It offers a very large sample size, at the cost of relatively scarce information on income (only categorical) and employment (only status in employment, and occupation/industry of employment). The Census contains key information on exact birth dates, which is unavailable in most other South African datasets. This information is available for every respondent. Additionally, all women between the age of 15 to 50 are asked the exact date of birth of their youngest child. This information will be key to define our treatment and control groups, and construct groups of children and mothers that are observationally similar but differently exposed to the reform.

**Community Survey 2007** The Community Survey (CS) is a survey that also takes place every ten years. The survey is done to supplement information from the Census in-between waves, and it is very comparable in terms of the structure of the questionnaire. It is also a very large survey, with roughly a 1/50 sampling rate of the population. A key advantage with respect to the 2011 Census is that all children, irrespective of their age, are asked about school attendance, while this question is limited to children 5 years or older in Census data. This allows us to check whether children younger than the affected age values (5 and 6 by June 30) are affected by the reform. Contrary to the Census, this data does not have information on exact birth dates of the respondent, but only information on the respondent's age. As in the Census, however, women between 15 and 50 are asked information on the exact date of birth of their youngest child.

**Sample definition** A main advantage of our Census (and CS) data is the ability to define mothers' treatment status without having to link mothers and children, because in both data sources mothers are asked about the exact date of birth of their youngest child. This takes away the concern of selection due to migration, co-residence choices or household composition endogenous to the policy (Ardington et al. (2009); Hamoudi and Thomas (2014)). For example, if a mother is more or less likely to be away from the household because of the child goes to school, the data allows us to observe and account for that. In the paper, we refer to this as the *overall* sample of mothers, meaning the full sample of mothers regardless of whether their youngest child is observed in the household. In similar fashion, we can refer to an *overall* sample of children, meaning the full sample of children (not only the youngest), regardless of whether the mother is observed within the household.[16] On these two samples, we can estimate treatment effects that are unbiased by any sample selection: with the *overall* sample of children, we can estimate the impact of the reform on school attendance for all children (in 2011 only); with the *overall* sample of mothers we can estimate the impact of the reform on mothers' employment (in both 2007 and 2011). The only drawback of using these samples refers to the fact that the two estimates are not directly comparable in terms of interpretation, given that the first refers to the full population of all children, while the second one refers only to a mother's youngest child.

We thus also create a subsample matching the mother with her youngest child within the household. We refer to this as the *matched* sample. This allows us to obtain a sample on which we observe the labour market outcomes of the mothers and the education outcomes of the youngest child, in particular school attendance, at the same time. In order to do so, in the 2011 Census, we conduct a match using the date of birth of the youngest child (available for each woman aged 15 to 50) and the date of birth of each respondent.[17]

In this way, we end up with three working samples: a *matched* subsample of co-residing mothers and children on which we can estimate the effect of the reform on both child's school enrollment and mother's employment; a complete *overall* sample of mothers and a complete *overall* sample of children (in 2011 only) on which we can estimate the effect of the reform on employment and school attendance, respectively. In each case, we restrict the sample only to Black and Coloured individuals. We will present results on both children's' school enrollment and maternal labor supply using the *matched* sample in the main sections of the paper, in order to ensure comparability across outcomes. However, we will also present robustness checks on employment results and school attendance on the *overall* samples to show that results are consistent and not driven by selection.

[16]This *overall* sample of children is only relevant in the 2011 Census, because in 2007 we lack exact birth dates of survey respondents.

[17]The matching process takes place differently when using data from the 2007 CS. Specifically, given that data on the exact birth date of every person is not available, we can only match (within the household) on discrete age values. In other words, a mother's youngest child should be 5, and there is a child of corresponding age within the household, we assign that child to the mother. In cases of conflict (more than one child of the same age in the household, more than one mother with the same age of the youngest child in the household), we do not match mother and child.

# 4 Methods

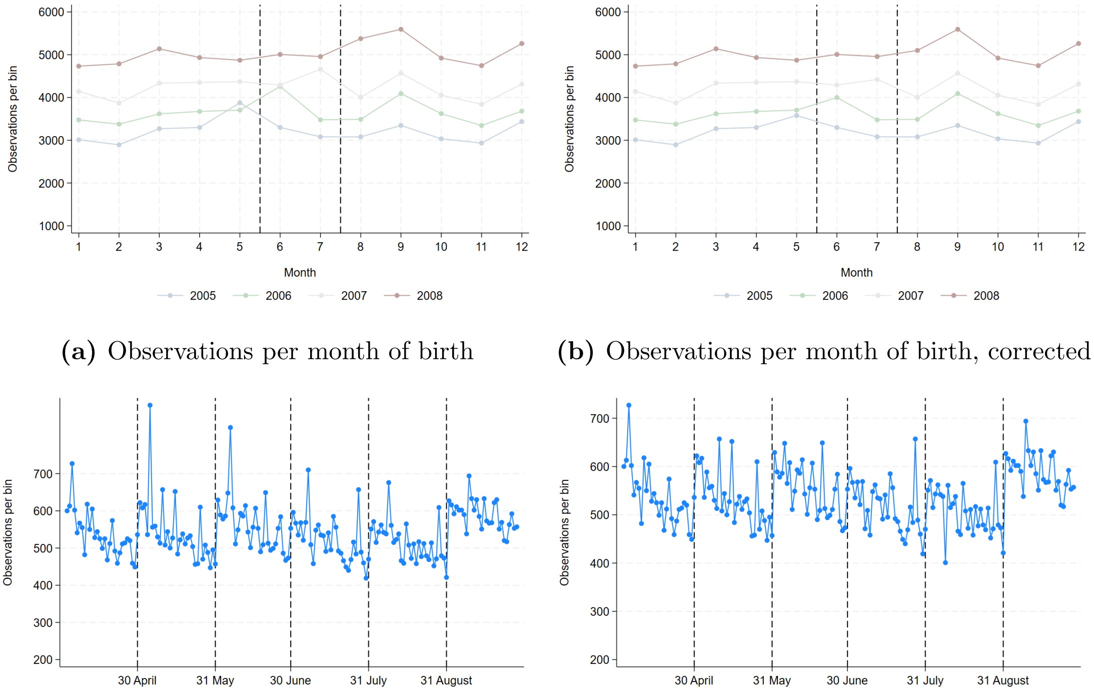
The aim of the paper is to estimate the causal effects of exposure to the policy on children's school enrollment and maternal labor supply. This requires identifying comparable treated and control children (mothers), who differ only based on the age at which they (their children) can enroll in education. Given that the reform generated discontinuities in the school admission age for each cohort at June 30, the introduction of this cutoff can be used as an exogenous source of identification. If the allocation of children's birth around this cutoff is as good as random, comparing outcomes of treated and control individuals on the two sides of the cutoff will allow to estimate the causal effect of the policy.

In this setting, causal estimates could be obtained with a regression discontinuity design (RDD). This would imply comparing individuals on each side of the cutoff, with the exact date of birth as the running variable. Given that information on the exact date of birth is available in our data, the running variable can be constructed in terms of days before or after 30 June. An alternative empirical approach would be to simply conduct a comparison of means for individuals on the two sides of the cutoff. As the bandwidth size shrinks to zero, the two approaches would yield equivalent estimates (i.e., for a small enough bandwidth, the RDD approximates a local experiment with random allocation on each side of the threshold) (Cattaneo and Titiunik (2022)). Instead, with larger bandwidths further away from the threshold, estimates might differ depending on how the running variable is correlated with the outcome of interest.

Regardless of the estimation strategy employed, the continuity assumption needs to hold for comparison around the threshold to provide a causal estimate. This requires assuming that all other observable and unobservable factors (i.e., different than treatment status) are continuously related to the assignment variable, such that any change in the outcome of interest at the cutoff can only be ascribed to changes in treatment status.

While this assumption cannot be directly tested, supportive evidence can be provided by verifying whether (i) the number of observations evolves smoothly around the cutoff, and (ii) individuals around the cutoff have the same distribution of observable characteristics. In particular, we would expect the number of births as well as the characteristics of both children and their parents to evolve smoothly at the threshold. Importantly, a violation of the continuity assumption would occur (in the *matched* sample) if mothers and children are more (or less) likely to co-reside *because* of the policy. However, as in our data we are

**Figure 4:** Density of observations by month and day of birth of youngest child




**(a)** Observations per month of birth

**(b)** Observations per month of birth, corrected

**(c)** Observations per day of birth

**(d)** Observations per day of birth, corrected

**Notes:** The figure reports the number of observations in the *overall* 2011 Census database that report being born in a given month of the year (Panels A and B) or in a given day between April and September (Panels C and D). Panels A and C report the information from the raw database, while in Panels B and D we exclude observations for which the birth date corresponds to 5.5.2005, 6.6.2006, 7.72007 and 8.8.2008. In each panel, the analysis is conducted for cohorts of individuals born between 2005 and 2008. These include cohorts that are affected by the reform (2005 and 2006) and cohorts that are instead not affected by the reform (2007 and 2008). Trends for these cohorts are reported separately in Panels A and B, and jointly in Panels C and D.

able to define mothers' treatment status without matching with the child, we can avoid this risk in the *overall* sample and test for it directly when checking the balance of observables.

We start by exploring the number of observations by month and day of birth of the youngest child as shown in panels A and C of Figure 4. We focus on four selected cohorts, two affected by the threshold (2005 and 2006) and two not affected (2007 and 2008) because children are too young. These different cohorts are presented separately in Panel A, and jointly in Panel C. The graphs shows two patterns: i) first, the number of observations per month spikes in a specific month each year, which is different across years (Panel A), and ii) the number of observations spikes in a specific day of the month, which is different across months (Panel C).[18] Both patterns are easy to explain: Census data does not have missing

[18]The fact that the number of observation in each cohort increases with the year of birth of the child is instead simply a mechanical consequence of the structure of the data: younger kids are more likely to be the youngest children of a mother.

values on the exact date of birth of the youngest child, and missing information is imputed for some values. The way in which this is conducted is easy to spot: missing values are imputed with the same number of day, month, and year, for example 5.5.2005 or 6.6.2006, which explains both the varying spikes in months (across years) and in days (across months).

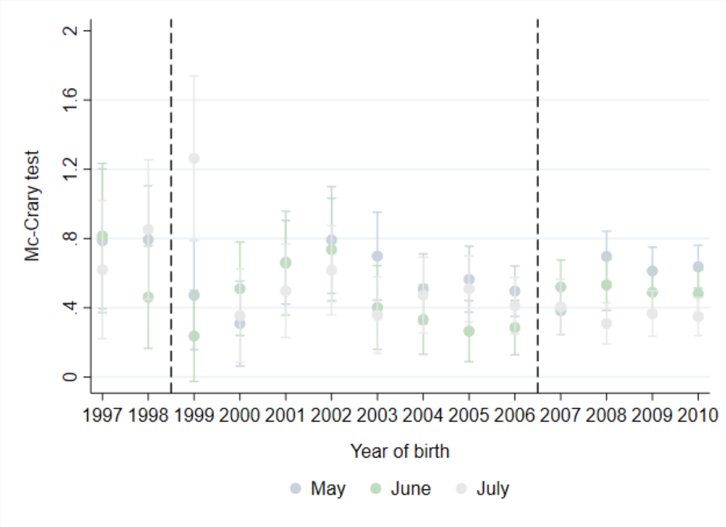
In panels B and D, we exclude these observations. When applying this correction, the number of observations per month of birth is relatively stable across months (with the exceptions of September), but still shows a clear jump in the density at the beginning of each month. Specifically, the number of observations gradually declines from the beginning to the end of the month, and then spikes at the beginning of the following month. This pattern occurs in a consistent way across months and is not specific to the threshold set by the reform (June 30th) nor to the birth years affected by the reform (i.e., 2005 and 2006, compared to 2007 and 2008). This is easily confirmed by a standard McCrary analysis, which we show in Figure 5. The test shows that the density of observations breaks at the beginning of each month of birth for all cohorts and months of birth.

Misreporting of date of birth could occur in two ways: both within month, if people misreport the day of birth and only report the month correctly, or across months, if people tend to round to the beginning of the closest month. In our interpretation, the shape of the density in Figure 4 suggests this second mechanism is most likely at play. The number of observations spikes at the beginning of each month, then declines gradually, with the lowest number at the end of the month. If misreporting occurred within month (people only remember the month of birth and not the day), this is not the pattern that we would expect. Instead, the shape of the distribution suggests that people born towards the end of the month are more likely to report being born in the following month.

This pattern is consistent with two possible explanations. First, the demography literature has found that some individuals deliberately lie about their birth date. Across different countries, this has been related, among others, to the willingness of delaying the age of school enrollment or postponing military service (Anelli et al., 2023). Second, respondents might simply not know the exact birth date of their children and round age values (or birth dates) to a given number, or to the closest month.[19] While we are not able to distinguish between these two hypotheses, we can rule out that the misreporting of the date of birth is specific to the reform we aim to study. The evidence clearly shows that the pattern is consistent across different months of the year, not only the threshold specified by the reform,

[19]For example, Ranchhod (2006) shows this with respect to age values of pensioners in South Africa.

**Figure 5:** Mc Crary test for the months of May, June and July; separately by cohort




**Notes:** The figure reports the point estimates and 95% confidence intervals of a series of Mc Crary tests. These are conducted separately for each cohort of individuals born between 1997 and 2010, and for three different break points within each cohort. These break points correspond to (i) 30 May, (ii) 30 June and (iii) 30 July. The 30 June break point is the relevant one for the analysis, while the other two break points are used for reference purpose. In each case, a bandwidth of 30 days before and after the cutoff is used. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. 1997 and 1998), (ii) those who were affected by the reform (i.e. from 1999 to 2006), and (iii) those who are too young to be affected by the reform (i.e. from 2007 to 2010). The analysis is conducted using the 2011 Census data.

and is present for all years of birth: for children that were never affected by the reform (pre-1999), cohorts that were affected (1999-2006), cohorts not yet affected (2007-2010).

In Figure A1 in the Appendix, we show the implications of this for the choice of our preferred estimator: we simulate data with sorting unrelated to the outcome of interest and spikes in the density similar to the ones we observe in our sample. Logically, we show that when sorting is random we recover the correct effect with both an RDD and a simple comparison of means on each side of the threshold. Furthermore, we also show that, if sorting is not random and related to the outcome of interest, then the comparison of means approach over the same bandwidth is much less sensitive to bias than the RDD, regardless of whether the sorting happens within or across months of birth. The conclusion from this simple exercise is quite straightforward: the RDD, estimating causal effects by fitting a relationship between the forcing variable and the outcome, is more subject to the bias due to sorting; the comparison of means approach, by disregarding entirely information on the slope of the function between the forcing variable and the outcome and only focusing on the mean difference, is unaffected.

The validity of our identification strategy is further confirmed by looking at the distribution of observables. Specifically, Panels A to G of Appendix Figure A2 plot the mean values of selected individual or household characteristics computed for bins of one day for a bandwidth of 30 days around the 30 June cutoff. The results show that these individual characteristics evolve rather smoothly around the cutoff. Additionally, Panel H shows that the probability that mothers and children are matched in the same household does not change discontinuously around the threshold, validating the use of our *matched* sample. We also compute the predicted employment probability based on a set of observable mothers' characteristics and see that the mean on each side of the threshold, with a 30 day bandwidth, is virtually identical on the two sides of the threshold (Appendix Figure A3).

With this evidence is mind, we prefer to focus on the difference of means on a relatively small bandwidth (one month) as our preferred specification. The equation takes the following form:

$$Y_i = \alpha + \beta_{DM} June_i + \gamma X_i' + \epsilon_i \tag{1}$$

Where $Y_i$ is the outcome of interest for mother $i$. In most of the analysis, outcomes of interest will correspond to binary variables for child's school attendance and maternal employment. $June_i$ is the main regressor of interest, and it corresponds to a dummy equal to one if the birth date of the child took place in June. We restrict the sample to individuals born in either June or July. In this way, our coefficient of interest, $\beta_{DM}$ indicates, conditional of control variables, the average difference in the outcome of interest. Our analysis is run separately for each cohort of birth, such that we obtain a coefficient $\beta_{DM}$ for as many years of birth as we include in our analysis. We also include a rich set of individual-level characteristics $X_i$.[20] Standard errors are clustered at the household level, to account for the correlation in children's enrollment and labour supply of mothers living in the same household. Weights account for the fact that the database is a 10% subset of the Census.

We also present complementary evidence following a standard RDD. Indeed, while the RDD is more subject to bias due to sorting as explained above, this needs not to be an issue in our setting. As we can exploit both the variation across months of birth but also cohorts, if the sorting is unrelated to the reform, we can still recover the causal effect by netting out the potential bias of sorting by comparing coefficients across years of birth. For this reason, we also run a standard RDD analysis, with a specification that will take the

[20]These correspond to controls for age of the mother and its square, mother's education, number of children in the household and its square, dummies for the province of residence, a dummy for whether the mother lives in a province different from its province of birth, household size and its square, race and urban area of residence.

following form:

$$Y_i = \theta + DayBirth_i + June_i \times DayBirth_i + \beta_{RDD} June_i + \lambda X_i' + \eta_i \tag{2}$$

Where $DayBirth_i$ is the running variable, constructed in terms of the number of days between the child's birth date and the June 30 of each year. A polynomial of order 1 is used in the analysis. We set the bandwidth equal to 30 days, in order to have the same sample restriction criterion as in our difference of means approach results. All other terms have the same interpretation as above.

We look at treatment effects separately for each cohort of children. We construct cohorts based on the age individuals have on 30 October of the year in which they are interviewed (i.e., 2011 when using the Census data and 2007 when using the CS).[21] This is done in order to harmonize interpretation of the effects between the two data sources, as the data collection were conducted in different times of the year (October for the 2011 Census and February-March for the 2007 CS).

Given the functioning of the policy as explained above, the cohort of affected individuals include children aged between 5 and 12 years old in 2011, and children aged 5 to 8 years old in 2007. For those aged 5, the estimated results will be of contemporaneous nature (i.e., these are individuals who have been given the possibility to enroll earlier in the same school year in which we observe them in the data). For other cohorts of affected individuals, the estimates capture the treatment effects $x$ years after treatment, where $x = (Age - 5)$. This means that we are able to observe treatment effects up until 7 years after treatment in the 2011 Census, and up until 3 years after treatment in the 2007 CS.

[21]This means that, for instance, treatment effects for individuals aged 5 should be read as those for individuals who are 5 years old on October 30th of the year of the interview.

# 5 Results

*5.A Effects on children's school enrollment*

**Effect on overall enrollment** The effects of the reform on children's school enrollment are plotted in Figure 6. Specifically, the figure presents the estimates from Equation (1), run separately for children of different ages (defined on the x-axis) and using data from either the 2011 Census (Panel A) or the 2007 CS (Panel C). The figure also plots the school enrollment rate for children born in June and July of different years, again separately using data from the 2011 Census (Panel B) or the 2007 CS (Panel D).

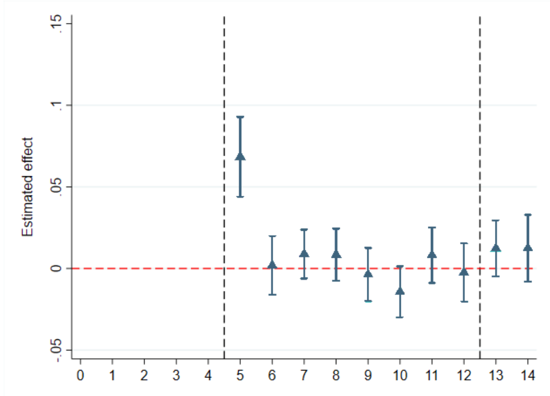
The results confirm the descriptive evidence shown before. Looking at the results obtained with the 2011 Census (Panel A), we estimate that the effect of the reform on school enrollment is around 7 percentage points at age 5, implying an overall increase in the proportion of kids who go to school by that age. The effect does not last further: treatment effects are precisely estimated around zero for children between 6 and 12. The absence of long-lasting effects on school enrollment is explained by the fact that, eventually, all kids enroll in education. The reform simply changed the timing at which this happens, allowing children born in June to enroll one year earlier compared to those born in July. This pattern is also evident when looking at the school enrollment rates (Panel B): the school enrollment rate increases with the child's age and reaches close to 100 per cent for kids aged 7 as the mandatory age kicks in, with no detectable differences between those born in June or July.

Data from the 2007 CS corroborates these findings. Specifically, treatment effects at age 5 are of the same magnitude of those obtained in the 2011 Census, although less precisely estimated (Panel C). The data also shows the absence on any lasting effect on school enrollment, with the difference that the coefficient at age 6 is also significant but smaller in size.[22] The main advantage of using data from the 2007 CS is that it allows also to explore whether the reform affected individuals before the age of 5, as information about school enrollment is asked also to younger children. We find that the positive difference in school enrollment between those born in June and July materializes exactly at age 5, with no notable differences in school enrollment for younger children.[23] This is also evident when

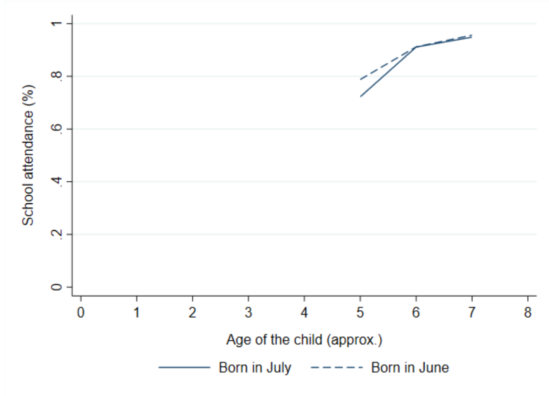
[22]This small difference could be due to the differential timing of the CS (in February, at the start of the school year) and the Census (in October, at the end of the school year). Alternatively, this difference could be explained by the overall increase in enrollment over time, as the attendance rates at age 6 are higher overall in 2011 than 2007.

[23]The descriptive evidence in Section 2 showed an increase of school attendance also at age 4 that does

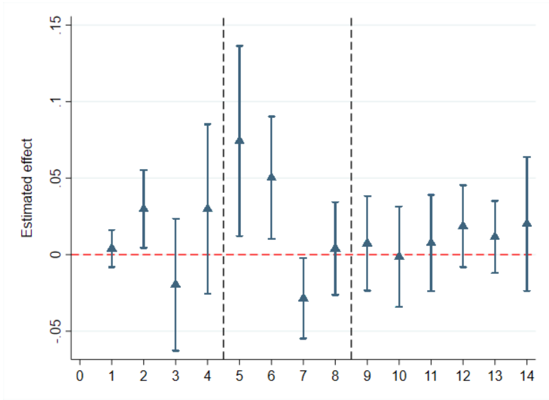
**Figure 6:** Effects on youngest child's school enrollment



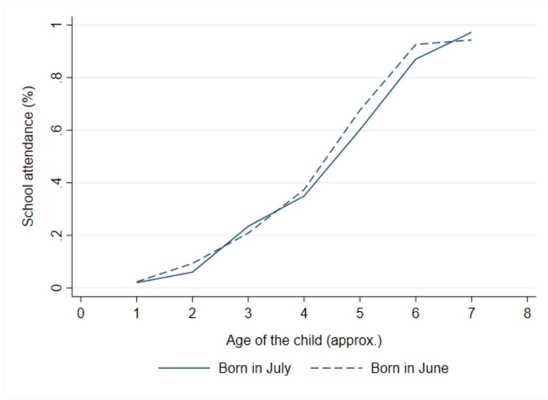

**(a)** 2011, estimated effects




**(b)** 2011, school attendance rates




**(c)** 2007, estimated effects




**(d)** 2007, school attendance rates

**Notes:** The figure presents the results on the effects of the reform on children's school attendance. Panel A presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1) using 2011 Census data, while Panel C reports the same parameters but using information from the 2007 CS. Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above in the 2011 analysis and aged 9 and above in the 2007 analysis), (ii) those who were affected by the reform (i.e. aged 5 to 12 in the 2011 analysis and aged 5 to 8 in the 2007 analysis), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5 in the 2007 analysis). Panels B and D report instead the school attendance rates by age of the child, separately using data from the 2011 Census and the 2007 CS, respectively. These rates are presented separately for individuals born in June (dashed line) and July (continuous line). All results are obtained using the *matched* sample (see main text for details).

looking at children's school enrollment rates using data from the 2007 CS (Panel D): the figure shows that school enrollment increases with child's age and this increase is parallel for children born in June and July from age 1 to age 4; the reform creates a temporary gap in school enrollment rates at age 5, which disappears again as school enrollment reaches almost full coverage among older children.

In Appendix Figure A4, we show a nearly identical estimate on the *overall* sample of children, i.e., focusing on all children and not on the sample of youngest child matched with the mother.[24] On this sample, the increase in the probability of school attendance at age 5 is of identical magnitude (7 pp.), and centered around 0 in all other age values.

**Primary vs pre-school** We decompose the increase in overall school enrollment between pre- and primary education (Appendix Figure A5).[25] We see that the increase in overall school attendance is entirely an increase in primary school attendance, only partly at the expense of pre-school. This is fully consistent with the descriptive evidence presented in Section 2. Specifically, pre-school attendance decreases by 2pp. at age 5, and by 3pp. at age 6, while primary school attendance increases by 9pp. at age 5 and 3pp. at age 6. Therefore, at age 5, more than 3/4 of the total increase in primary school is a net increase in overall attendance, and only 1/4 is a reallocation from pre-school to primary. At age 6, the increase in primary school comes instead entirely from a decrease in pre-school attendance.

This result can be seen in light of the functioning of the education system in South Africa. Pre-school is available to most South African children at a fee, even though a relatively low one, and usually has longer opening hours (up to 4pm, see discussion in Section 2). Primary school, instead, is accessible to everyone for free but covers a shorter portion of the day. The main effect of the reform was thus to allow pupils, who would not have gone to school otherwise, presumably because of lack of access, availability, or affordability of pre-school, to access primary school in advance. A secondary and smaller effect is to reallocate some children from pre-school to primary school at both ages 5 and 6.

---

not appear when comparing children born in June versus those born in July. This is perfectly consistent with the thresholds put in place by the reform that binds only for those who turn 5 and 6 after June 30th. We interpret the increase in overall enrollment at age 4 as an indirect spillover of the reform, as pre-schools adjusted to the reform and the lower attendance of kids aged 5 and 6 by admitting more kids aged 4.

[24]As specified above, this sample is not available in 2007 as exact birth dates are not in the data, and we can obtain exact birth dates of children *only* by matching them with their mothers.

[25]We conduct this part of the analysis only with 2011 Census data, as we have strong reservations about the validity of this question in the 2007 CS: the question on the type of education that individuals attend report missing values for around 30% of children aged 5 in the CS.

**Table 1:** School enrollment rates for 5 years old children, by their mothers' personal characteristics

| | Born in June | | Born in July | | Difference | | t-test |
|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | pp | % | |
| Overall | 0.788 | 0.409 | 0.722 | 0.448 | 0.066 | 9.09% | 5.13 |
| Marital status: married | 0.787 | 0.409 | 0.742 | 0.438 | 0.046 | 6.15% | 2.54 |
| Marital status: unmarried | 0.788 | 0.409 | 0.703 | 0.457 | 0.086 | 12.19% | 4.70 |
| Education: Grade 11 or less | 0.741 | 0.438 | 0.669 | 0.471 | 0.072 | 10.83% | 4.05 |
| Education: More than Grade 11 | 0.850 | 0.357 | 0.798 | 0.402 | 0.052 | 6.51% | 2.98 |
| Number of children: One | 0.789 | 0.408 | 0.729 | 0.445 | 0.060 | 8.20% | 2.78 |
| Number of children: More than one | 0.787 | 0.409 | 0.719 | 0.450 | 0.069 | 9.55% | 4.31 |
| Internal migrant: Yes | 0.773 | 0.419 | 0.685 | 0.465 | 0.088 | 12.88% | 2.91 |
| Internal migrant: No | 0.793 | 0.405 | 0.730 | 0.444 | 0.063 | 8.59% | 4.42 |
| Area of residence: Urban | 0.763 | 0.426 | 0.716 | 0.451 | 0.047 | 6.52% | 2.77 |
| Area of residence: Rural | 0.823 | 0.382 | 0.732 | 0.443 | 0.091 | 12.49% | 4.65 |
| Grand parent: In HH | 0.802 | 0.399 | 0.703 | 0.457 | 0.099 | 14.11% | 3.83 |
| Grand parent: Not in HH | 0.783 | 0.412 | 0.728 | 0.445 | 0.055 | 7.56% | 3.74 |
| Spouse: Employed | 0.797 | 0.403 | 0.754 | 0.431 | 0.042 | 5.62% | 1.77 |
| Spouse: Not employed | 0.735 | 0.442 | 0.730 | 0.445 | 0.005 | 0.72% | 0.14 |

**Notes:** The table reports the mean and standard deviation (SD) of the school attendance rate for children born in June and July of 2006 (i.e. five years old at the time of the 2011 Census). These statistics are reported for the overall sample, as well as separately for groups of children defined by selected mothers' individual characteristics as well as household characteristics. The table also reports the mean difference in values between those born in June and those born in July (in both percentage points and per cent), as well as the results of a t-test for equality of means.

**Heterogeneous results** We check whether this interpretation of the results is confirmed in the data by looking at heterogeneous effects for different groups of children. Specifically, using data from the 2011 Census, we divide the population of children based on selected mothers' individual characteristics (i.e., marital status, educational attainments, migrant status and spouse's employment status) and other household characteristics (i.e., number of children, area of residence, presence of grandparents in the household). We compute the difference between school enrollment rates of children born in June and July in each of these groups, presented both in pp. and in per cent, and present a t-test of equality of means.

The results of this exercise are shown in Table 1, and confirm the interpretation of the results outlined above. Specifically, we find stronger effects on school enrollment among children whose mothers (i) are unmarried, (ii) have lower educational attainments, and (iii) live in rural areas. Results are also similar for children whose mother has migrated, defined as residing in a province different from the province of birth. The effect on school enrollment is larger when the grandparents live in the household. Finally, the positive effect on school enrollment appears when the spouse is employed, while it is almost equal to zero when the spouse is not employed. This suggests that the incentives to enroll the child in education might be lower, when the partner is available to take care of the child.

We run the same analysis for children aged 6 in Appendix Table B1 and confirm that almost no group experiences an increase in overall enrollment at age 6 (in 2011).

### *5.B Effects on maternal employment*

**Effect on employment probability** The effects of the reform on maternal employment are plotted in Figure 7, which is organized as Figure 6 above. Starting from the results for 2011, we observe no simultaneous (i.e., at child's age 5) nor lasting (i.e., from 6 to 12) change in the employment rates of mothers affected by the reform (Panel A). Those whose youngest child is born in June are just as likely to be employed, both at age 5 and when the child is older, than those whose youngest child is born in July. The evolution of maternal labor supply with respect to the age of the child (Panel B) shows an increasing trend. This is in line with the fact that mothers return to the labor market over time, as the child grows older. However, there is no differential impact for mothers whose children are born in June (dashed line) or July (continuous line).

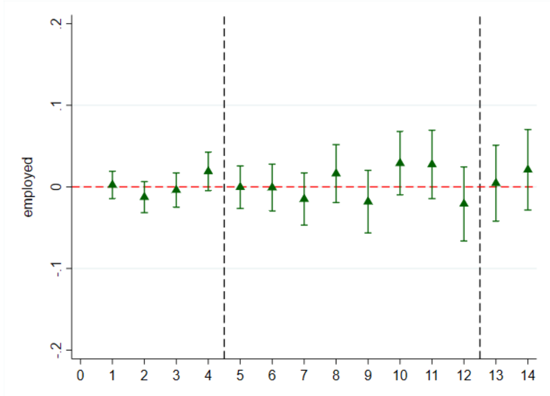
The results obtained using data from the 2007 CS (Panels C and D) are less precisely estimated, but qualitatively similar to those discussed for 2011. Specifically, we do not find any contemporaneous effect (i.e., at child's age equal to 5), nor any effect for mothers of directly affected kids in previous years (i.e., from age 6 to 8 in the 2007 CS).

Importantly, the absence of treatment effects is not due to lack of statistical power. With the 2011 Census data, we are able to rule out even small increases in the employment rate of affected mothers: the minimum detectable effect would be 2pp in the *overall* sample and 2.5pp in the *matched* sample.

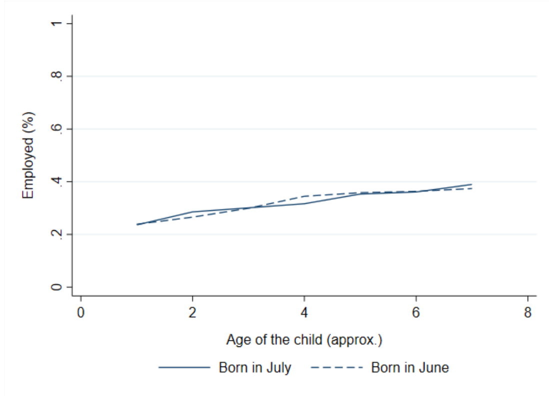
In Appendix Figure A6, we plot the coefficients of the same estimation on the *overall* sample of mothers (irrespective of whether the child is observed in the household), and find qualitatively identical results.

We also look at heterogeneous treatment effects on maternal labor supply, by dividing the 2011 sample along the same groups introduced above. Table 2 presents the results of this exercise, and it is structured in the same way as Table 1. It shows that the zero employment effect is equally distributed across different groups in the population. Specifically, none of the t-tests of equality of means is statistically significant at conventional levels.

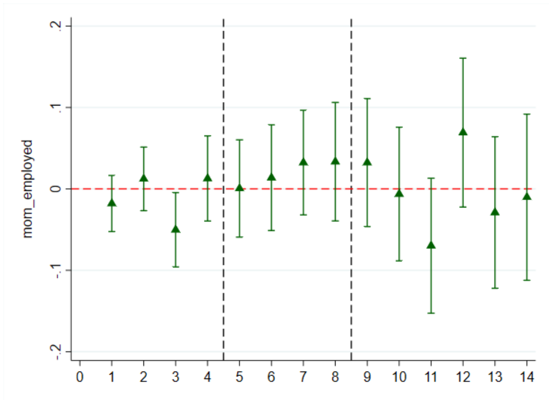
**Figure 7:** Effects on maternal employment, matched sample



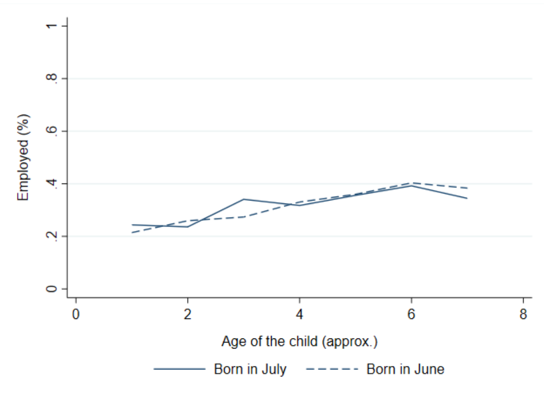

**(a)** 2011, estimated effects




**(b)** 2011, mothers' employment rates




**(c)** 2007, estimated effects




**(d)** 2007, mothers' employment rates

**Notes:** The figure presents the results on the effects of the reform on maternal employment. Panel A presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1) using 2011 Census data, while Panel C reports the same parameters but using information from the 2007 CS. Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above in the 2011 analysis and aged 9 and above in the 2007 analysis), (ii) those who were affected by the reform (i.e. aged 5 to 12 in the 2011 analysis and aged 5 to 8 in the 2007 analysis), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5 in both 2007 and 2011). Panels B and D report instead the maternal employment rate by age of the child, separately using data from the 2011 Census and the 2007 CS, respectively. These rates are presented separately for individuals born in June (dashed line) and July (continuous line). All results are obtained using the *matched* sample (see main text for details).

**Table 2:** Maternal employment rates for 5 years old children, by mothers' personal characteristics

| | Born in June | | Born in July | | Difference | | t-test |
|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | pp | % | |
| Overall | 0.358 | 0.480 | 0.353 | 0.478 | 0.005 | 1.47% | 0.37 |
| Marital status: married | 0.379 | 0.485 | 0.370 | 0.483 | 0.009 | 2.32% | 0.42 |
| Marital status: unmarried | 0.339 | 0.474 | 0.336 | 0.473 | 0.003 | 0.88% | 0.15 |
| Education: Grade 11 or less | 0.272 | 0.445 | 0.275 | 0.447 | -0.003 | -1.20% | -0.19 |
| Education: More than Grade 11 | 0.473 | 0.499 | 0.463 | 0.499 | 0.010 | 2.05% | 0.42 |
| Number of children: One | 0.351 | 0.478 | 0.333 | 0.472 | 0.018 | 5.47% | 0.76 |
| Number of children: More than one | 0.362 | 0.481 | 0.364 | 0.481 | -0.002 | -0.43% | -0.09 |
| Internal migrant: Yes | 0.456 | 0.499 | 0.446 | 0.498 | 0.010 | 2.32% | 0.30 |
| Internal migrant: No | 0.336 | 0.472 | 0.332 | 0.471 | 0.004 | 1.09% | 0.23 |
| Area of residence: Urban | 0.447 | 0.497 | 0.436 | 0.496 | 0.011 | 2.51% | 0.58 |
| Area of residence: Rural | 0.234 | 0.424 | 0.227 | 0.419 | 0.008 | 3.42% | 0.39 |
| Grand parent: In HH | 0.299 | 0.458 | 0.272 | 0.445 | 0.027 | 9.94% | 1.00 |
| Grand parent: Not in HH | 0.378 | 0.485 | 0.379 | 0.485 | -0.001 | -0.28% | -0.06 |
| Spouse: Employed | 0.488 | 0.500 | 0.467 | 0.499 | 0.021 | 4.42% | 0.72 |
| Spouse: Not employed | 0.255 | 0.437 | 0.252 | 0.435 | 0.003 | 1.19% | 0.09 |

**Notes:** The table reports the mean and standard deviation (SD) of the employment rates of mothers whose children were born in June and July of 2006 (i.e. five years old at the time of the 2011 Census). These statistics are reported for the overall sample, as well as separately for groups of children defined by selected mothers' individual characteristics as well as household characteristics. The table also reports the mean difference in values between those born in June and those born in July (in both percentage points and per cent), as well as the results of a t-test for equality of means.

Additionally, the differences in employment rates between mothers of children born in June and July are always small in magnitude: out of the 14 different groups that compose our sample, 10 present differences in employment rates below or equal to one percentage points, and two other groups report differences between one and two percentage points.

Appendix Table B2 reproduces the results in Table 2, but for mothers whose kids are aged 6. It confirms that there is no employment effect even one year after mothers were able to send their kids to school earlier. This result rules out that the absence of a contemporaneous employment effect is driven by the fact that finding a job takes time. Rather, it suggests that mothers are not using the additional time that has become available to them thanks to their children's school enrollment, in order to find a new job. In line with this hypothesis, Appendix Figure A7 shows that there is no treatment effect on the probability of looking for a job (i.e., being unemployed). This is true both in our 2011 sample (Panel A) as well as in our 2007 sample (Panel B). Using data from the 2011 Census, we also investigate the effect on the reasons for not working (Appendix Figure A8). We find that there is no effect on any of the self-reported reasons for not working, including individuals not working because they are taking care of the household or because of the lack of jobs.

**Effect on employment types and job quality** While the evidence discussed so far has shown the absence of an effect on employment along the extensive margin, it is still possible that the reform affected maternal labour market outcomes by changing the type of jobs or the quality of the jobs held. Specifically, one possibility is that higher enrollment of kids allows mothers to find better jobs. Similar results have been found, among others, by Ryu (2020) in Brazil, Dang et al. (2022) in Vietnam and Ajayi et al. (2022) in Burkina Faso.[26]

We explore this hypothesis by first looking at the types of jobs held (Figure 8). Specifically, we use the 2011 Census data and divide the sample of individuals by employment status (i.e., wage employment or self-employment, Panels A and B) and by formal nature of the employment relationship (i.e., formal employment and informal or employment by private households, Panels C and D). We do not find evidence of an effect of the reform on maternal employment across any of these outcomes.

We further look at treatment effects on measures of job quality (Figure 9), by calculating an index of job quality across four dimensions (hourly earnings, monthly earnings, tenure, and permanent contract; from Panels A to D). We calculate the median for each sector×occupation cell from the Quarterly Labour Force Survey (QLFS)[27], and merge this into our 2011 Cenus data, which does not contain information on these four job characteristics, but has very precise occupation and industry of employment information.[28] We then divide the sample into quartiles based on these estimated measures of job quality. Overall, we can reject the hypothesis that the reform led to an increase in the quality of the jobs held by women, as it did not change the probability to be employed in higher quartiles for any of these job quality dimensions that we construct.

### *5.C Alternative estimation*

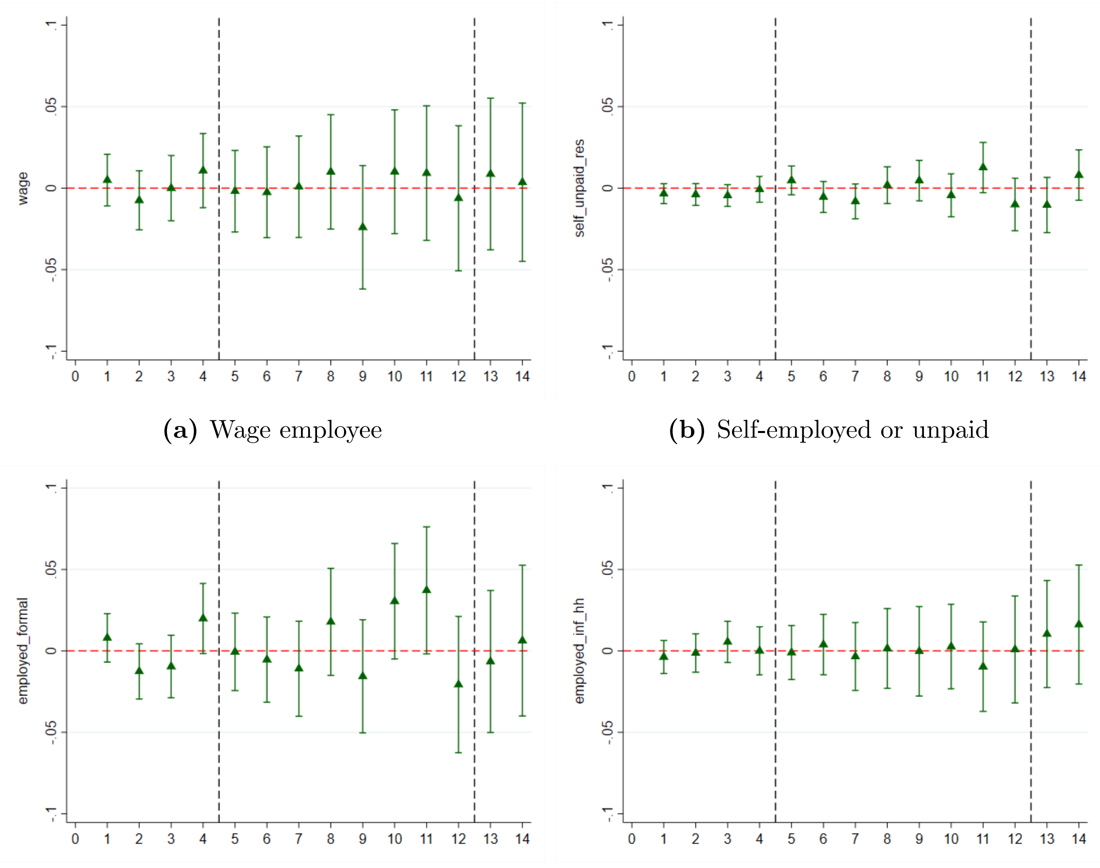
We replicate our findings using a standard RDD. As shown in Section 4, the distribution of observations is not constant around the cutoff, which led us to prefer a difference of

[26]Both Ryu (2020) in Brazil and Dang et al. (2022) find zero effects on overall employment, but positive effects of formal and wage employment with increases in enrollment of similar magnitude to ours. Ajayi et al. (2022) finds that access to free childcare increases income from salaried employment.

[27]The QLFS is a nationally representative, quarterly rotating panel, starting in 2008, with a very large sample size. It contains detailed information on labour market outcomes, including indicators of job quality.

[28]In order to choose at what level to aggregate our job quality indexes, we perform a Lasso estimation to check what level of aggregation of the 4-digit sector×occupation cells has the best out-of-sample prediction power.

**Figure 8:** Effects on maternal employment, matched sample: by status in employment and nature of the job

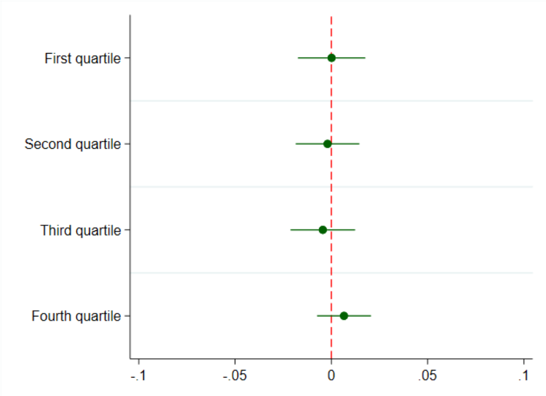



**(a)** Wage employee **(b)** Self-employed or unpaid

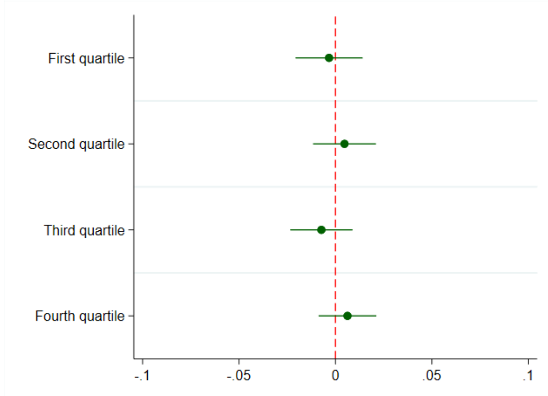
**(c)** Formal employment **(d)** Informal or household employment

**Notes:** The figure presents the results on the effects of the reform on status and nature of maternal employment. In the upper part of the figure, the outcomes of interest correspond to dummy variables for whether the individual belongs to the mutually exclusive categories of wage employee (Panel A) or self-employed or unpaid worker (Panel B). In the lower part of the figure, the outcomes of interest correspond to the mutually exclusive categories of formal employment (Panel C) and informal or household employment (Panel D). In each panel, the analysis is conducted using data from the 2011 Census and the information reported corresponds to point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1). Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). All results are obtained using the *matched* sample (see main text for details).

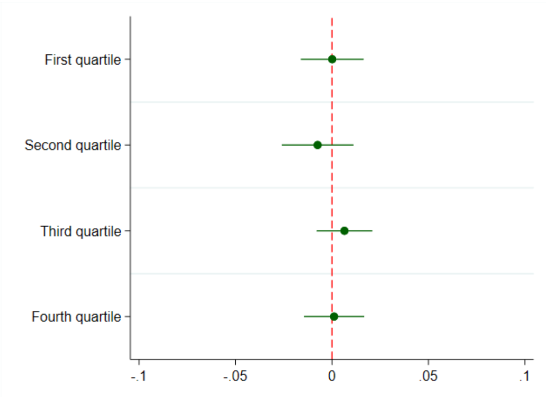
**Figure 9:** Effects on maternal employment, matched sample: job quality measures



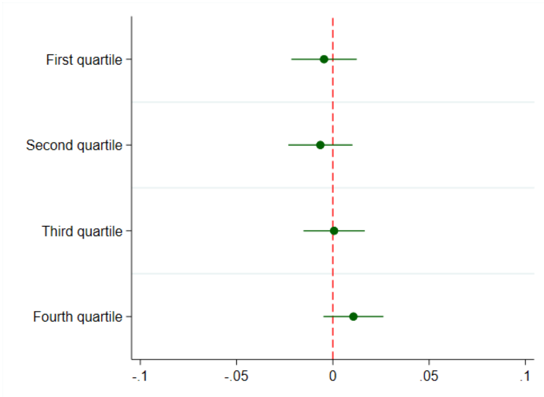

**(a)** Hourly earnings




**(b)** Monthly earnings




**(c)** Tenure




**(d)** Permanent contract

**Notes:** The figure presents the results on the effects of the reform on the quality of maternal employment. Specifically, in each panel the outcomes of interest correspond to dummy variables equal to one if the individual is estimated to be in one of four quartiles with respect to different measures of job quality. These correspond to hourly earnings (Panel A), monthly earnings (Panel B), tenure in the job (Panel C) and permanent nature of the employment contract (Panel D). The sample in the analysis comes from the 2011 Census. However, the Census does not report information on these job quality variables. For this reason, we calculate the median of these variables in each sectorxoccupation cell in the QLFS, and merge this information in the Census (see text for details). In each panel, the information reported corresponds to point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1). Standard errors are always clustered at the household level and sampling weights are included. Results are obtained by estimating treatment effects only for one cohorts of mothers with children are of age 5 and for the *matched* sample (see text for details).

means approach as the main specification. However, if the bunching in the distribution of observations is not systematically related to the presence of the rules defining policy eligibility, then the results from the RDD should be similar to those discussed above.

We select a bandwidth of 30 days on each size of the cutoff in order to have the same sample as in the main results. However, we also produce results from a restricted sample that excludes individuals born in the last five days of June and the first five days of July. This donut approach is common in the RDD literature, and it is implemented with the purpose of eliminating observations who might have been more likely to manipulate their value of the running variable. We control for the running variable linearly. This is constructed in terms of difference in the number of days from the cutoff of each year, with 0 corresponding to individuals born on 1 July. The set of covariates included in the analysis is also the same as for the baseline specification.

Appendix Figure A9 presents the RDD results on children's school enrollment; while Appendix Figure A10 plots the RDD results on maternal employment. Starting from the results on school enrollment, they are very similar to those presented above. Specifically, (i) the magnitude of the treatment effect at child's age of 5 is once again around 7pp, and (ii) treatment effects are close to zero for all older cohorts of children. However, the point estimates are less precisely estimated. This is less of an issue when we present the results using all observations in the month (Panel A), compared to when we apply a 5-day donut (Panel B). A similar conclusion arises when looking at the results on maternal employment. We produce them both for the matched sample (Panels A and B, with all observations and with the 5-day donut restriction, respectively) as well as for the overall sample (Panels C and D, with all observations and with the 5-day hole restriction, respectively). Point estimates are a little less stable from one cohort to another, and confidence intervals increase in magnitude. However, these results also indicate the absence of an effect on maternal employment, as in the results obtained from our baseline specification.

# 6 Mechanisms

The previous section has shown that, while the reform that decreased the school admission age increased children's school enrollment, this did not have any effect on maternal labor supply. That is, mothers did not use the extra time that was becoming available to them to enter the labor force. Additionally, the results have shown that there were no effects on the types of the jobs held, nor on the quality of these jobs. This means that free school availability did not allow women who were already in employment to find better jobs.

These results stand in contrast with those of previous studies. Specifically, existing studies have found a positive relationship between school/childcare availability, or eligibility, and maternal labor supply and/or the quality of the jobs held by mothers. These findings are rather consistent across studies, despite the substantial heterogeneity in country contexts and institutional settings. This relationship has been found to hold also in emerging and developing countries: in the review by Halim et al. (2023), just one study out of 22 in low- and middle-income countries did not find any significant impact of childcare availability on maternal labor supply, on either the intensive, extensive or job quality margin.[29]

The purpose of this section is to shed light on this puzzle. Specifically, we aim to explore the mechanisms behind the (lack of) effects on maternal employment of higher school enrollment. By doing so, the discussion also aims to relate our findings to those of previous studies and provide further evidence to benchmark when one could expect higher school enrollment to lead to higher maternal employment in developing countries.

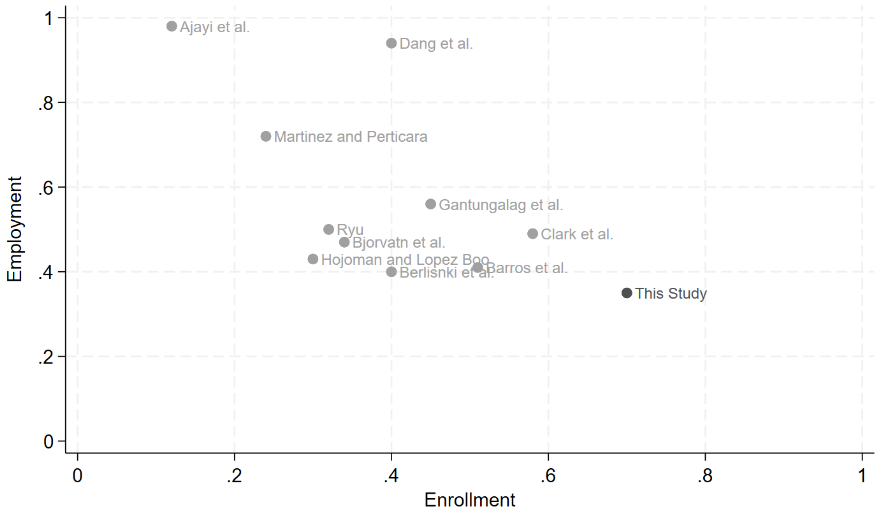
The starting point is to analyze differences in study contexts that could motivate the differences in results. We summarize the causally-identified studies exploring the effect of childcare on mothers' labor supply in middle- and low-income countries in Appendix Table B3.[30] For each study, the table reports (i) the age at which children are treated (i.e., at what age school enrollment is offered or increases), (ii) the baseline children's school enrollment rate, (iii) the estimated effect on school enrollment, (iv) the baseline female employment rate, and (v) the estimated effect on maternal employment. The table also reports the maximum length over which the effect on maternal employment is measured.

Relative to the studies that report the necessary information to conduct this bench-

[29]Note, however, that there was no previous study on the relationship between childcare availability, or eligibility, and maternal labor supply in South Africa.

[30]The list of studies examined is taken from the recent review done by Halim et al. (2023), with the addition of other studies that fulfill the criteria.

**Figure 10:** Relationship between employment and enrollment in the literature on mothers' labor supply and childcare




**Notes:** The figure reports the mean rates of maternal employment and children's school enrollment at baseline in selected previous studies, as well in the context of the present analysis.

marking exercise, on average our study (where roughly 70% of children age 5 already attend school) has higher baseline enrollment levels than other settings. On the contrary, the baseline employment rate of mothers is much higher in other settings, whereas, in the period we focus on, only 35% of Black and Coloured mothers were employed in South Africa. The relationship between baseline employment of the control group and baseline enrollment is plotted in Figure 10. Our study context (darker dot in the figure) stands out for its relatively high initial enrollment and relatively low maternal employment in the control group.[31]

Motivated by this observation, we want to explore whether differences in the initial rates of children's school enrollment and maternal labor supply between our context and previous studies can explain the difference in results on maternal employment.

To explore this hypothesis, we conduct two separate exercises.

To start with, we re-run our regression, using maternal employment as the outcome of interest, separately in each district (52 districts) or province (9 provinces). We run the analysis at the finer district level and also at the more aggregated province level, as the district-level estimates might be too noisy to show any kind of statistical relationship with baseline employment or school attendance. We then plot the coefficient estimate that we

[31]Instead, we also see from Table B3 that the literature has already explored comparable studies both in terms of size of the increase in enrollment with respect to the control group, and age of the child affected.

obtain from each regression, against the district's (or province's) (i) maternal employment rate, and (ii) children's school attendance rate.[32]

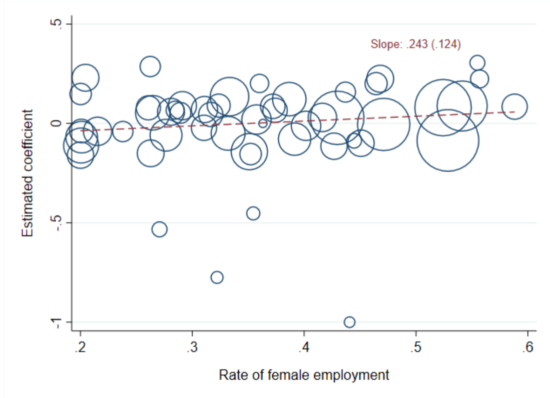
Figure 11 shows the results of this exercise. Each panel plots the estimate of $\beta_{DM}$ (on the y-axis), obtained from running Equation (1) separately in each district or province, against the baseline maternal employment rate (Panels A and B present the results at the district and province level, respectively) or the baseline children's school attendance rate (Panels C and D, again at the district and province level, respectively), on the x-axis. The panels also presents the linear relationship between the two variables, where we weight each observation with the inverse of the square of the standard error. The size of the dot is instead proportional to the number of observations in each district or province.

The evidence suggests that there is a positive relationship between our estimated effects on maternal employment and the baseline maternal employment rate: districts or provinces with higher baseline rates of maternal employment also show higher estimated effects of the reform on maternal labor supply. This relationship is statistically significant at the 10% in the district-level analysis (Panel A), while being only close to significance in the province-level analysis (Panel B). At the same time, the findings report a negative relationship between our estimated effects and the rate of school enrollment at baseline: districts or provinces with a lower initial rate of school enrollment, reported on average higher estimated effects on maternal labor supply. However, this time the relationship is not statistically significant, either at the district (Panel C) or province (Panel D) level.

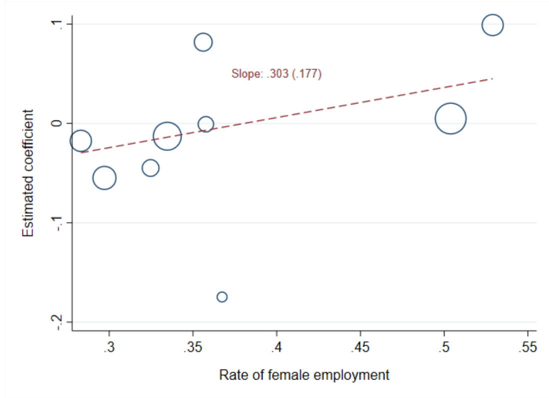
This suggests that the difference in our results compared to those of previous studies might relate to the lower than average initial maternal employment rate and (partially) the higher than average initial children's school enrollment rate. To further test this hypothesis, we re-run our main analysis by restricting the sample to those districts in the top 25 per cent of maternal employment rate and the bottom 25 per cent of the children's school enrollment rate. The underlying idea is to make our sample more similar to the ones prevailing in the literature used: Appendix Figure A11 replicates Figure 10 above, but each dot in red now corresponds to districts that simultaneously meet the two conditions above (the blue dots are instead using values from previous studies, as in Figure 10). In this restricted sample, the maternal employment rate is similar to the one of previous studies. Instead, the children's school attendance rate is still generally higher than in previous studies.

[32]Maternal employment rates are computed for all mothers aged 15 to 50, with at least one child (irrespective of whether the mother and the child were matched in the database). Children school enrollment rates are computed on the sub-sample of children born between January and May of 2006.

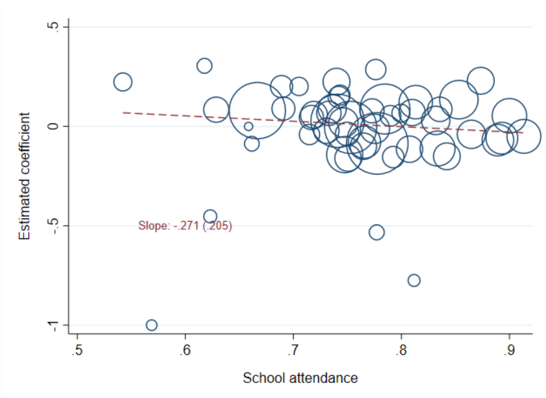
**Figure 11:** Employment estimates, by district's and province's maternal employment and children's school attendance



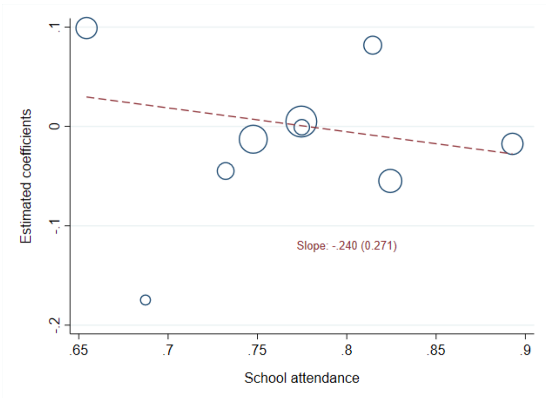

**(a)** Results by maternal employment rate, by district




**(b)** Results by maternal employment rate, by province




**(c)** Results by rate of children school enrollment, by district

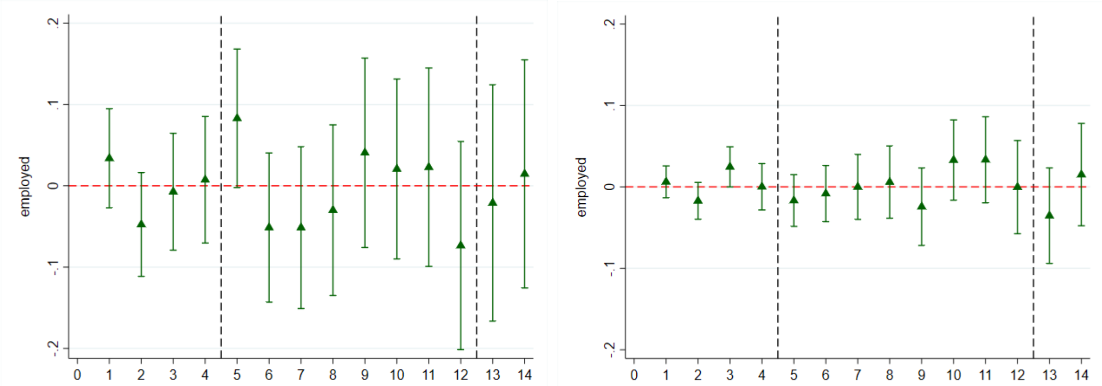



**(d)** Results by rate of children school enrollment, by province

**Notes:** The figure plots the results of the analysis on maternal employment, run separately at the level of each district (Panels A and C) and at the level of each province (Panels B and D). Specifically, the dots in each panel correspond, on the y-axis, to different estimates of $\beta_{DM}$ from regressions run in each different district or province, where the dependent variable always corresponds to a dummy equal to one if the mother is in employment. These estimates are plotted against the district's (or province's) rate of maternal employment (Panels A and B) or the rate of children school enrollment (Panels C and D), which are reported on the x-axis. The size of the dot corresponds to the number of observations in each district or province. In each panel, we plot also the linear relationship between the estimated treatment effects and the variable on the x-axis. In these regressions, whose estimates and standard errors are also reported in the figure, we weight each observation by the inverse of the square of the standard error. All results are obtained using data from the 2011 Census. Results are obtained by estimating treatment effects only for one cohorts of children of age 5 (and their mothers) and for the *matched* sample (see text for details).

The results of running the main estimation in high-employment-low-enrollment districts in Panel A of Figure 12, which replicates Panel A of Figure 7. Confidence intervals are larger in magnitude, owing to the smaller sample size, but they indicate a positive effect on maternal employment for mothers whose children are aged 5. As explained in Section 4, this is the cohort of children who have been given the possibility to enroll in education in the same year in which we observe them in the data. This means that, in this sample, we observe positive simultaneous treatment effects on maternal employment in the same year

**Figure 12:** Effects on maternal employment in selected districts




**(a)** High maternal employment and low children school enrollment

**(b)** Low maternal employment and high children school enrollment

**Notes:** The figure reports the results of the analysis on maternal employment by splitting the sample based on the baseline rates of maternal employment and children's school attendance. Specifically, observations used in Panel A correspond to individuals living in districts in the top 25 per cent in terms of the rate of maternal employment and in the bottom 25 per cent in terms of the rate of children's school enrollment, while observations used in Panel B live in the other districts. In each panel, the analysis is conducted using data from the 2011 Census and the information reported corresponds to point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1). Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). All results are obtained using the *matched* sample (see main text for details).

in which children can enroll in education.

When we instead focus on the sample of districts with low maternal employment (i.e., bottom 75 per cent of the districts in terms of maternal employment) and high school enrollment (i.e. top 75 per cent of the districts in terms of school enrollment), we observe a precisely estimated zero effect in terms of maternal employment (Panel B of Figure 12).

# 7 Conclusions

This paper has presented novel evidence from South Africa on the effects of lowering school starting age on children's school enrollment and maternal labor supply. We contribute to a large literature on the topic, where most previous studies in both developed and emerging and developing countries have found a positive effect of expanding school or childcare availability on maternal labor supply. Contrary to previous studies, however, we do not find any evidence of a positive effect on maternal labor supply, either on the extensive margin or on the intensive and job quality margins. We are also able to follow individuals for up to seven years after exposure to the policy, and can rule out long-lasting effects on either children school enrollment or maternal labor supply. We reconcile our findings with those of previous studies, by noting that our study context is characterized by relatively high initial rates of children's school enrollment and relatively low initial rates of maternal employment.

Overall, the results suggest that in our country context constraints to female employment might be of a structural nature, and unlikely to be overcome by only increasing the provision of childcare. In labor markets with higher employment opportunities, we recover the same positive relationship between enrollment and maternal employment found in the literature. This suggests that the well-established positive relationship between children's school enrollment and maternal labor supply that has been found in the literature is not bound to hold under any conditions. Rather, our argument is that this is likely to depend on context-specific circumstances.

It is well-known that the South African labor market is characterized by important spatial inequalities, which complicate both job search and commute to and from work. Specifically, commuting times are high and vary substantially by race.[33] As a result, commuting costs are also high: in 2013, it is estimated that around 13% of labor income is spent for getting to and from work. These facts reflect, to a large extent, the way in which cities were shaped in South Africa following segregation policies of the Apartheid era. This means that work opportunities are located far from home, especially for Black and Coloured individuals. At the same time, the very low self-employment rate limits the number of informal work opportunities available to Black and Coloured women close by.

While the reasons behind high commuting times in South Africa might be country

[33] Kerr (2017) estimates that in 2003, commuting times in South Africa were equal to 104 minutes in 2013. Commuting times were equal to 117 minutes for Black, 98 minutes for Coloured (the two groups analysed in this paper), and 74 minutes White individuals.

specific, large spatial inequalities exist in most developing and emerging countries. Given that primary education only covers only part of the day in South Africa (a feature that is common to many other countries), these spatial inequalities might have constrained any positive response of maternal labor supply in our context. This suggests that, in labor markets where maternal labor supply is constrained by structural factors, simply increasing access to education does not necessarily lead to an increase in employment levels or the quality of the jobs found by mothers.

# References

M. Abel, R. Burger, E. Carranza, and P. Piraino. Bridging the intention-behavior gap? the effect of plan-making prompts on job search and employment. *American Economic Journal: Applied Economics*, 11(2):284–301, 2019.

M. Abel, R. Burger, and P. Piraino. The value of reference letters: Experimental evidence from south africa. *American Economic Journal: Applied Economics*, 12(3):40–71, 2020.

K. F. Ajayi, A. Dao, and E. Koussoubé. The effects of childcare on women and children: Evidence from a randomized evaluation in burkina faso. Technical report, Center for Global Development, 2022.

M. Anelli, T. Colussi, and A. Ichino. Rule Breaking, Honesty, and Migration. *Journal of Law and Economics*, 66(2):409–432, 2023. doi: 10.1086/723112. URL https://ideas.repec.org/a/ucp/jlawec/doi10.1086-723112.html.

C. Ardington, A. Case, and V. Hosegood. Labor supply responses to large social transfers: Longitudinal evidence from south africa. *American economic journal: Applied economics*, 1(1):22–48, 2009.

O. Attanasio and M. Vera-Hernandez. Medium-and long run effects of nutrition and child care: evaluation of a community nursery programme in rural colombia. 2004.

O. Attanasio, R. P. De Barros, P. Carneiro, D. K. Evans, L. Lima, P. Olinto, and N. Schady. Public childcare, labor market outcomes of caregivers, and child development: Experimental evidence from brazil. Technical report, National Bureau of Economic Research, 2022.

A. Banerjee and S. Sequeira. Learning by searching: Spatial mismatches and imperfect information in southern labor markets. *Journal of Development Economics*, 164:103111, 2023.

R. P. d. Barros, P. Olinto, T. Lunde, and M. Caralho. The impact of access to free childcare on women's labor market outcomes: evidence from a randomized trial in low-income neighborhoods of rio de janeiro. 2013.

S. Bauernschuster and M. Schlotter. Public child care and mothers' labor supply—evidence from two quasi-experiments. *Journal of Public Economics*, 123:1–16, 2015.

S. Berlinski and S. Galiani. The effect of a large expansion of pre-primary school facilities on preschool attendance and maternal employment. *Labour Economics*, 14(3):665–680, 2007.

S. Berlinski, S. Galiani, and P. J. Mc Ewan. Preschool and maternal labor market outcomes: Evidence from a regression discontinuity design. *Economic Development and Cultural Change*, 59(2):313–344, 2011.

M. Berthelon, D. I. Kruger, and M. A. Oyarzun. The effects of longer school days on mothers' labor force participation. 2015.

K. Bjorvatn, D. Ferris, S. Gulesci, A. Nasgowitz, V. Somville, and L. Vandewalle. Childcare, labor supply, and business development: Experimental evidence from uganda. 2022.

A. Bousselin. Access to universal childcare and its effect on maternal employment. *Review of Economics of the Household*, 20(2):497–532, 2022.

G. Calderón. The effects of child care provision in mexico. Technical report, Working Papers, 2014.

E. U. Cascio. Maternal labor supply and the introduction of kindergartens into american public schools. *Journal of Human resources*, 44(1):140–170, 2009.

M. D. Cattaneo and R. Titiunik. Regression discontinuity designs. *Annual Review of Economics*, 14(1):821–851, 2022.

S. Clark, C. W. Kabiru, S. Laszlo, and S. Muthuri. The impact of childcare on poor urban women's economic empowerment in africa. *Demography*, 56(4):1247–1272, 2019.

H.-A. H. Dang, M. Hiraga, and C. V. Nguyen. Childcare and maternal employment: evidence from vietnam. *World Development*, 159:106022, 2022.

E. De la Cruz Toledo. Universal preschool and mothers' employment. *A Working Paper of the Columbia Population Research Center*, 2015.

D. Del Boca. Child care arrangements and labor supply. 2015.

F. Du and X.-y. Dong. Women's employment and child care choices in urban china during the economic transition. *Economic Development and Cultural Change*, 62(1):131–155, 2013.

F. Du, X.-y. Dong, and Y. Zhang. Grandparent-provided childcare and labor force participation of mothers with preschool children in urban china. *China Population and Development Studies*, 2(4):347–368, 2019.

H. Finseraas, I. Hardoy, and P. Schøne. School enrolment and mothers' labor supply: evidence from a regression discontinuity approach. *Review of Economics of the Household*, 15(2):621–638, 2017.

M. D. Fitzpatrick. Preschoolers enrolled and mothers at work? the effects of universal prekindergarten. *Journal of Labor Economics*, 28(1):51–85, 2010.

A. Gantungalag, B. Soyolmaa, B. Altantsetseg, and S. Dulbadrakh. The impacts of access to free childcare on women's labor market outcomes and children's development and health. 2019.

J. B. Gelbach. Public schooling for young children and maternal labor supply. *American Economic Review*, 92(1):307–322, 2002.

D. Halim, H. C. Johnson, and E. Perova. Preschool availability and women's employment: evidence from indonesia. *Economic Development and Cultural Change*, 71(1):39–61, 2022.

D. Halim, E. Perova, and S. Reynolds. Childcare and mothers' labor market outcomes in lower-and middle-income countries. *The World Bank Research Observer*, 38(1):73–114, 2023.

A. Hamoudi and D. Thomas. Endogenous coresidence and program incidence: South africa's old age pension. *Journal of development economics*, 109:30–37, 2014.

A. Hojman and F. López Bóo. Cost-effective public daycare in a low-income economy benefits children and mothers. 2019.

A. Kerr. Tax(i)ing the poor? commuting costs in south african cities. *South African Journal of Economics*, 85(3):321–340, 2017. doi: https://doi.org/10.1111/saje.12161. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/saje.12161.

M. R. Kilburn and A. Datar. The availability of child care centers in china and its impact on child care and maternal work decisions. Technical report, 2002.

H. Kleven, C. Landais, J. Posch, A. Steinhauer, and J. Zweimuller. Child penalties across countries: Evidence and explanations. *AEA Papers and Proceedings*, 109:122–26, May

2019. doi: 10.1257/pandp.20191078. URL https://www.aeaweb.org/articles?id=10.1257/pandp.20191078.

H. Kleven, C. Landais, and G. Leite-Mariante. The child penalty atlas. Working Paper 31649, National Bureau of Economic Research, August 2023. URL http://www.nber.org/papers/w31649.

C. Martínez and M. Perticará. Childcare effects on maternal employment: Evidence from chile. *Journal of Development Economics*, 126:127–137, 2017.

S. Martinez, S. Naudeau, and V. A. Pereira. Preschool and child development under extreme poverty: evidence from a randomized experiment in rural mozambique. *World Bank Policy Research Working Paper*, (8290), 2017.

P. Medrano. Public day care and female labor force participation: evidence from chile. *Documento de Trabajo*, 306, 2009.

T. W. Morrissey. Child care and parent labor force participation: a review of the research literature. *Review of Economics of the Household*, 15(1):1–24, 2017.

R. M. Padilla and H. F. Cabrera. The effect of children's time in school on mothers' labor supply: Evidence from mexico's full-time schools program. 2018.

V. Ranchhod. The effect of the south african old age pension on labour supply of the elderly. *South African Journal of Economics*, 74(4):725–744, 2006.

J. Rosero and H. Oosterbeek. Trade-offs between different early childhood interventions: Evidence from ecuador. 2011.

H. Ryu. The effect of compulsory preschool education on maternal labour supply. *The Journal of Development Studies*, 56(7):1384–1407, 2020.

V. Sanfelice. *Essays on Public Policies Using City Neighborhoods Variation*. PhD thesis, University of Rochester, 2019.

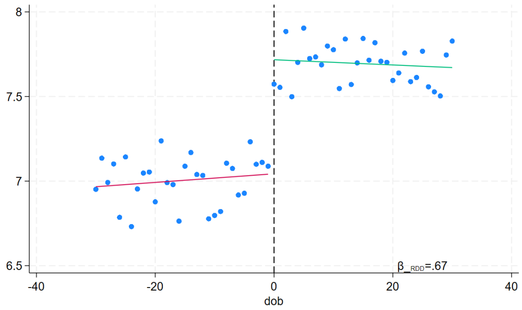
A. Tondini. The lasting labor-market effects of cash transfers: Evidence from south africa's child support grant. *The World Bank Economic Review*, 36(4):934–954, 2022.

S. Van der Berg, E. Girdwood, D. Shepherd, C. Van Wyk, J. Kruger, J. Viljoen, O. Ezeobi, and P. Ntaka. The impact of the introduction of grade r on learning outcomes. *University of Stellenbosch, Stellenbosch*, 2013.

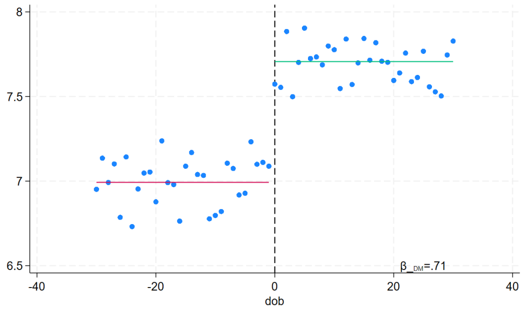
# Appendices

## A Appendix: Additional figures

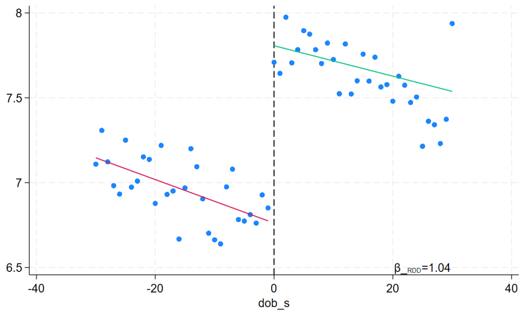
**Figure A1:** Methods' comparison



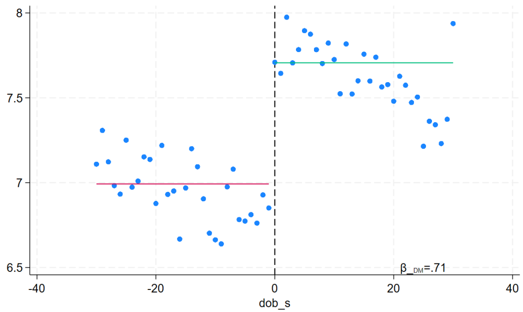

**(a)** RDD - No sorting



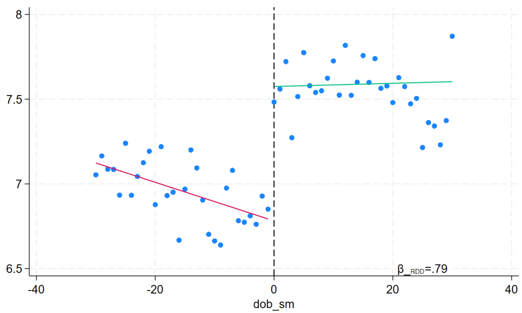

**(b)** Difference of means - no sorting



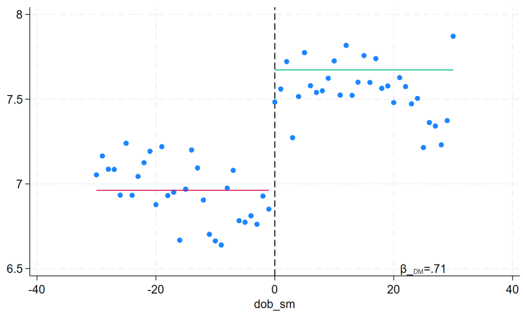

**(c)** RDD - Non-random sorting to beginning of the same month




**(d)** Difference of means - Non-random sorting to beginning of the same month



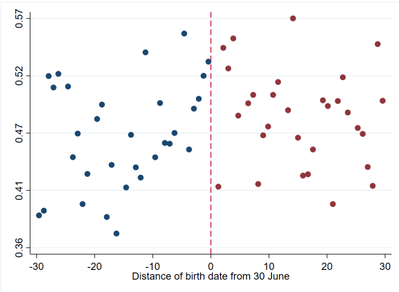

**(e)** RDD - Non-random sorting to beginning of next month



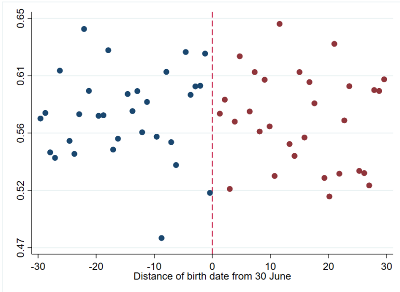

**(f)** Difference of means - Non-random sorting to beginning of next month

**Notes:** This figure presents the results of a simulation exercise comparing the two different estimation methods presented in Section 4. Different types of sorting around the thresholds are simulated: Panels (a) and (b), no sorting; panels (c) and (d), non random sorting where people misreport their birth dates to the beginning of the same month; panels (e) and (f) non random sorting where people misreport their birth dates to the beginning of the next month. The panels to the left show the results of an RD estimate with a 30-day bandwidth and a linear fit; the panel to the right show the results of a difference-of-means estimate with a 30-day bandwidth. The panels also report the estimated coefficient from each model.

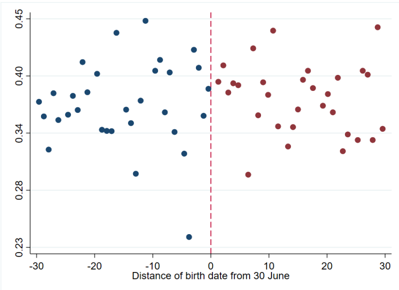
**Figure A2:** Smoothness of covariates



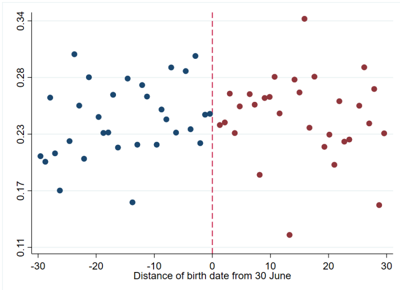

**(a)** Married



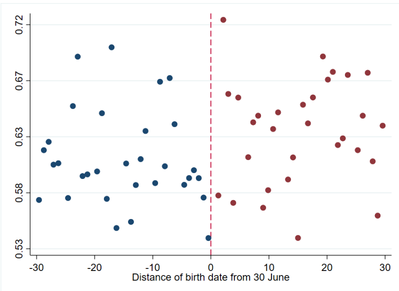

**(b)** Grade 11 or less



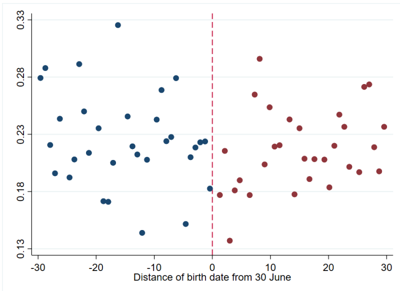

**(c)** One child



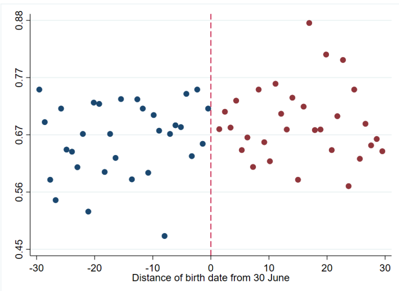

**(d)** Internal migrant



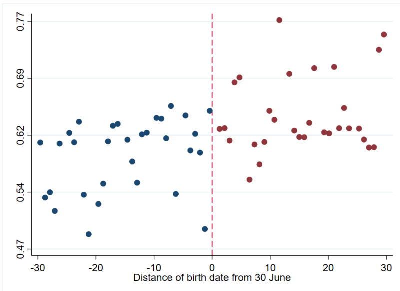

**(e)** Urban




**(f)** Grand parent in HH



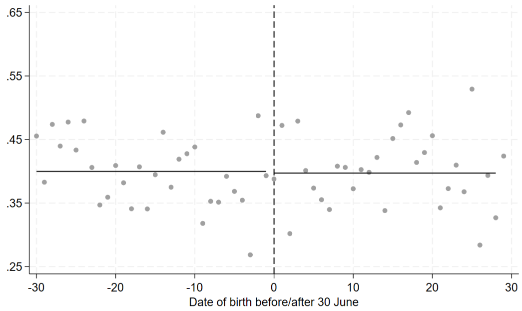

**(g)** Spouse is employed



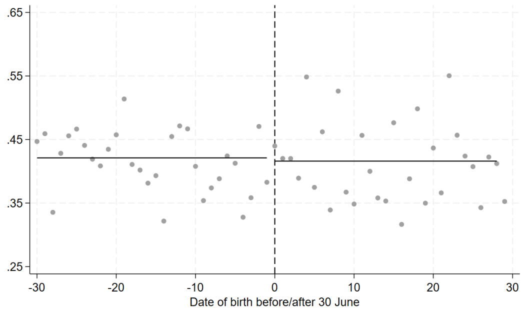

**(h)** Matched mother and child

**Notes:** Panels from A to G show the evolution of selected mother's individual characteristics as well as household characteristics around the 30 June cutoff. These are plotted only for mothers whose youngest child is aged 5, and using the *overall* sample (see text for details). Panel H plots instead the evolution of the probability that mothers and children in the same household are matched, using the information on the exact date of birth (see Section 3 for details). In each panel, the information reported corresponds to the mean of the variable of interest computed at the daily level. The bandwidth always corresponds to 30 days before and after the cutoff.

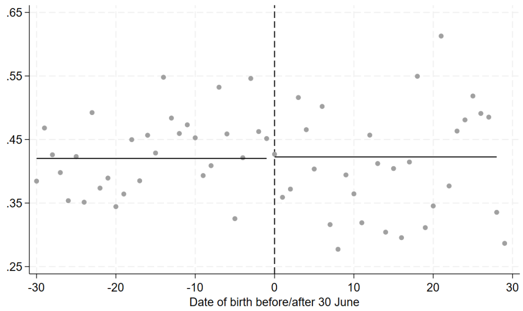
**Figure A3:** Balance of mothers' predicted employment probability based on covariates




**(a)** Age 4



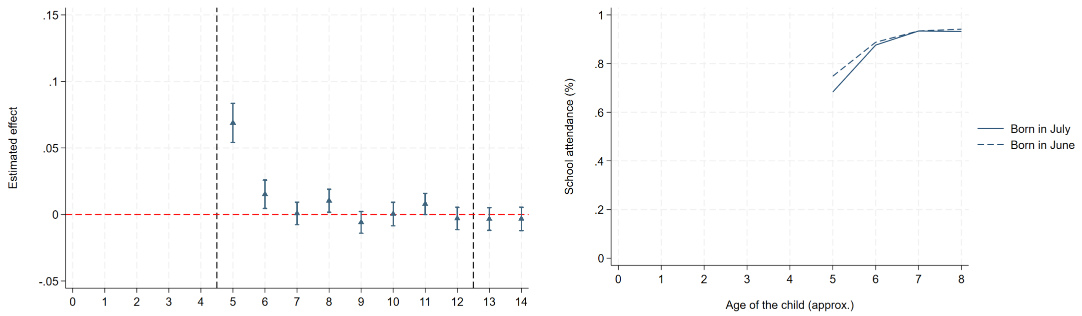

**(b)** Age 5




**(c)** Age 6

**Notes:** The graph shows the predicted probability to be employed based on observables characteristics for mothers whose youngest child is 4,5 or 6. The list of covariates includes: married low educational attainments, one child in the household, migrated, mother has zero income, grandfather is in the household, grandmother is in the household, the spouse is employed, the mother has been matched with the child within the household.

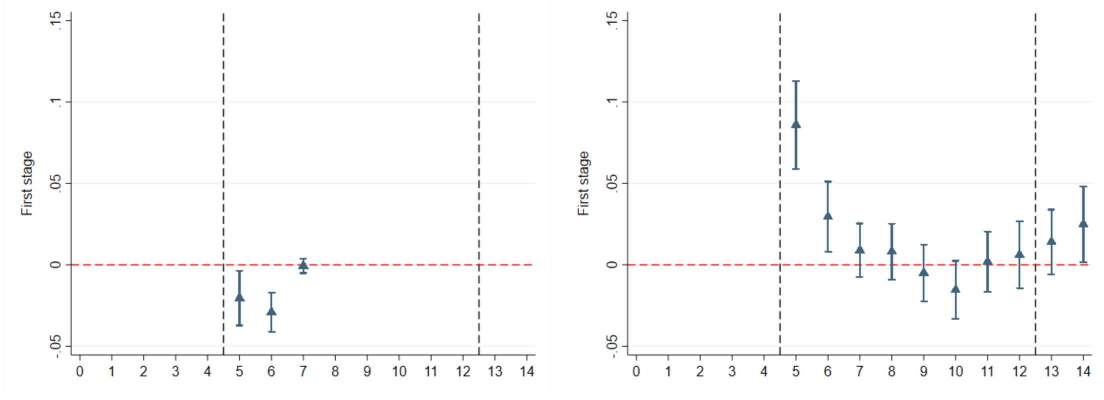
**Figure A4:** Effects on child's school enrollment on overall children sample




**(a)** 2011, estimated effects **(b)** 2011, school attendance rates

**Notes:** Panel A presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1) using 2011 Census data. Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). Panels B reports instead the school attendance rates by age of the child, separately for individuals born in June (dashed line) and July (continuous line). Compared to the results presented in the main text, these are obtained using the *overall* sample (see main text for details).

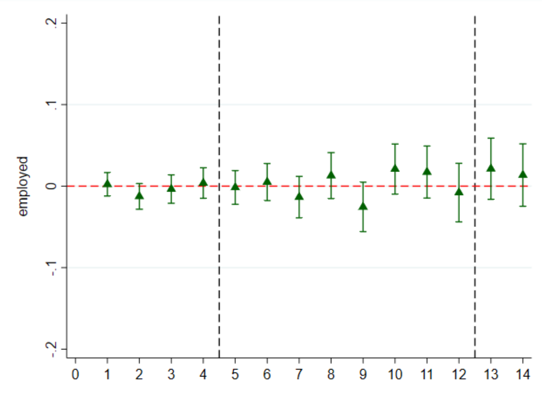
**Figure A5:** Effects on pre-school and primary school attendance



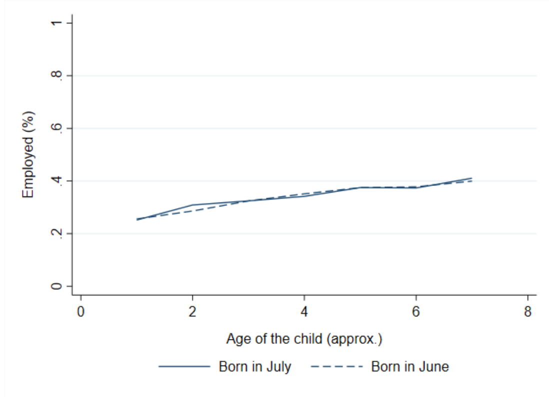

**(a)** Effects on pre-school attendance **(b)** Effects on primary school attendance

**Notes:** The figure presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1) using 2011 Census data. Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). All results are obtained using the *matched* sample (see main text for details). Compared to the results presented in the main text, these are presented separately for pre-school (Panel A) and primary school (Panel B) attendance.

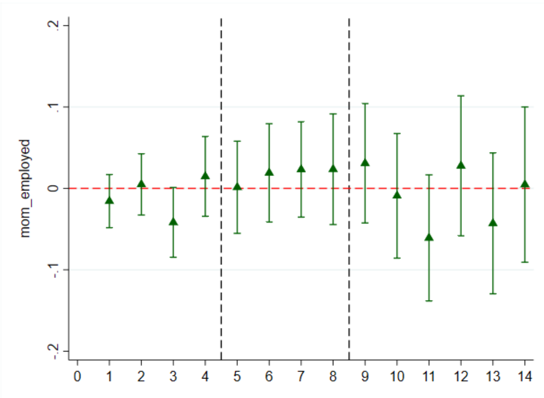
**Figure A6:** Effects on maternal employment, overall sample



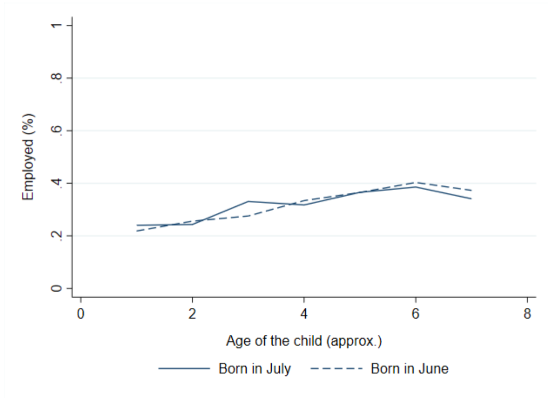

**(a)** 2011, estimated effects




**(b)** 2011, mothers' employment rates



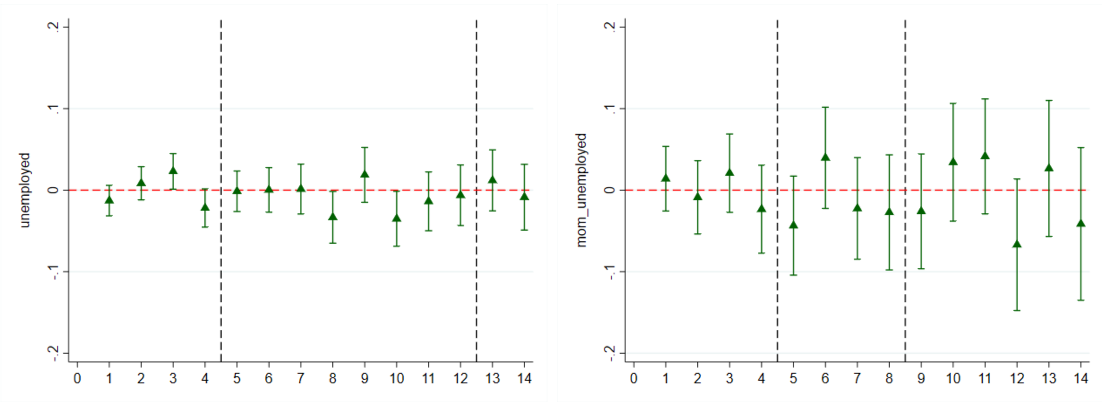

**(c)** 2007, estimated effects




**(d)** 2007, mothers' employment rates

**Notes:** Panel A presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1) using 2011 Census data, while Panel C reports the same parameters but using information from the 2007 CS. Standard errors are always clustered at the household level and sampling weights are included. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above in the 2011 analysis and aged 9 and above in the 2007 analysis), (ii) those who were affected by the reform (i.e. aged 5 to 12 in the 2011 analysis and aged 5 to 8 in the 2007 analysis), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5 in both 2007 and 2011). Panels B and D report instead the maternal employment rate by age of the child, separately using data from the 2011 Census and the 2007 CS, respectively. These rates are presented separately for individuals born in June (dashed line) and July (continuous line). Compared to the results presented in the main text, these are obtained using the *overall* sample (see main text for details).

**Figure A7:** Effects on maternal unemployment, matched sample

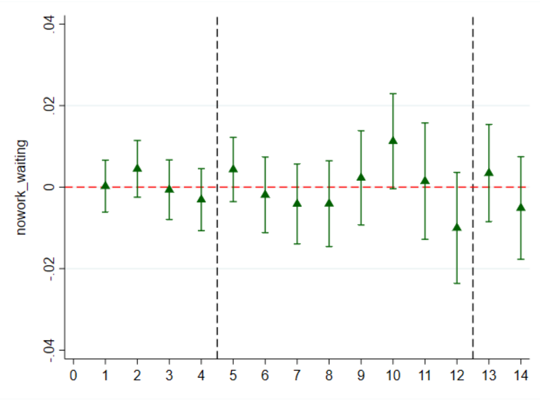



**(a)** 2011 Census

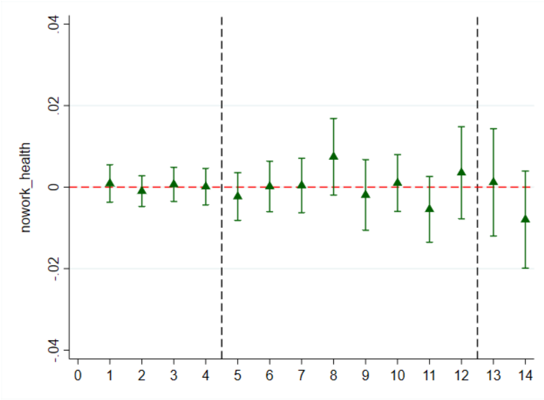
**(b)** 2007 CS

**Notes:** The figure presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1). Standard errors are always clustered at the household level and sampling weights are included. The outcome of interest correspond to maternal unemployment status, and the analysis is conducted separately using the 2011 Census data (Panel A) or the 2007 CS (Panel B). The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above in the 2011 analysis and aged 9 and above in the 2007 analysis), (ii) those who were affected by the reform (i.e. aged 5 to 12 in the 2011 analysis and aged 5 to 8 in the 2007 analysis), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5 in both 2007 and 2011). In all cases, results are obtained using the *matched* sample (see text for details).

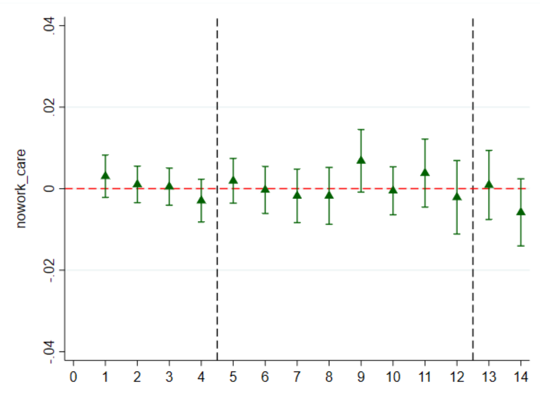
**Figure A8:** Effects on reasons for not working, 2011 matched sample



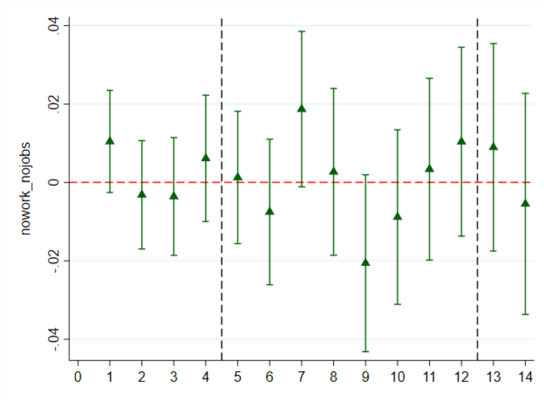

**(a)** Waiting for season or to be recalled



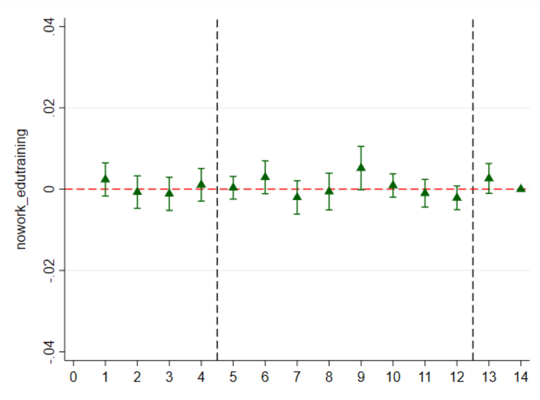

**(b)** Health, pregnant or unable



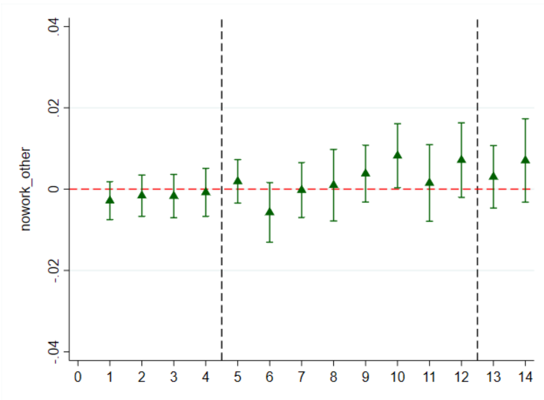

**(c)** Housewife/homemaker




**(d)** No jobs available



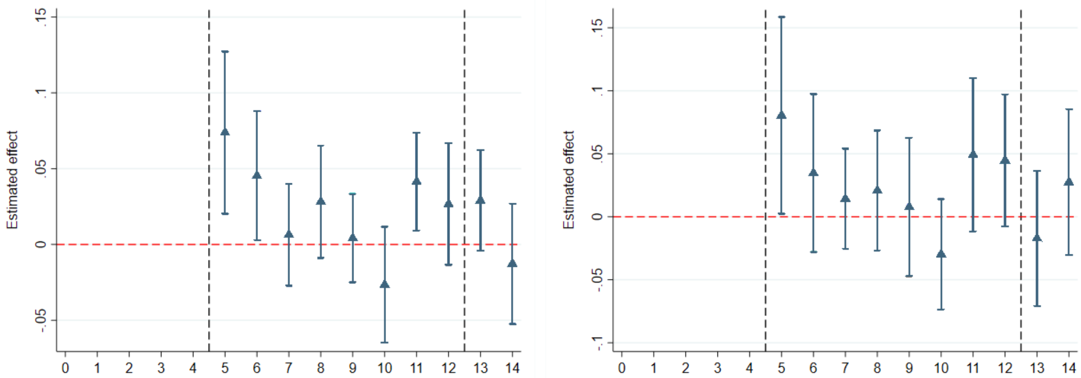

**(e)** Education or training




**(f)** Transport and other reasons

**Notes:** The figure presents point estimates and 95% confidence intervals for $\beta_{DM}$ in Equation (1) using 2011 Census data. Standard errors are always clustered at the household level and sampling weights are included. The outcome of interest correspond to different reasons reported for not working, as available in the 2011 Census. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). In all cases, results are obtained using the *matched* sample (see text for details).

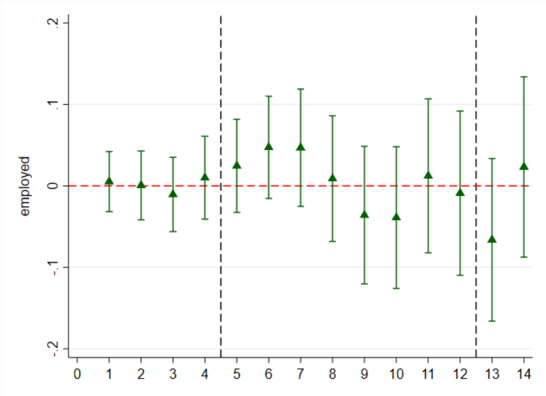
**Figure A9:** RDD results on school enrollment, 2011 Census (overall sample)



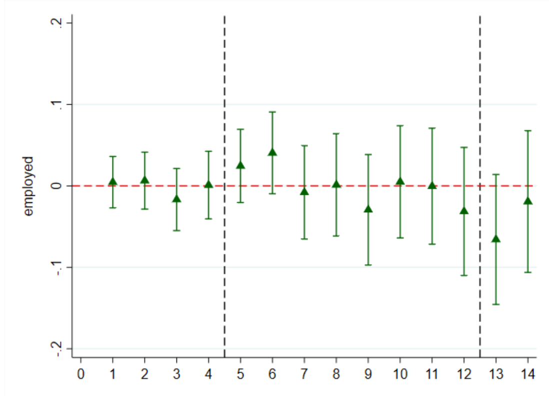

**(a)** All observations  **(b)** 5 days donut

**Notes:** The figure presents point estimates and 95% confidence intervals for $\beta_{RDD}$ in Equation (2) using 2011 Census data. The outcome of interest corresponds to children's school enrollment. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). Panel A presents RDD results using all observations in the sample, while Panel B excludes observations whose reported birth date lies in the five days before and after the cutoff. The bandwidth used in the analysis always corresponds to 30 days before and after the 30 June. In all cases, results are obtained using the *overall* sample (see text for details).

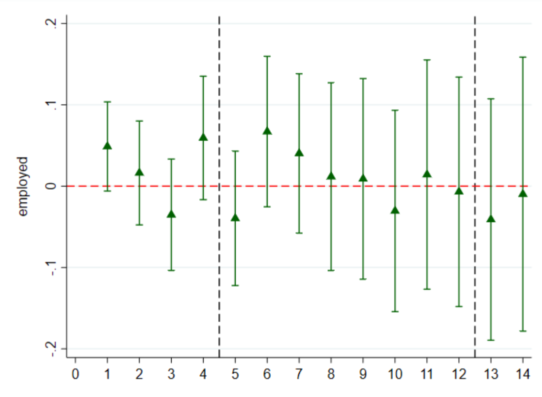
**Figure A10:** RDD results on maternal employment, 2011 Census



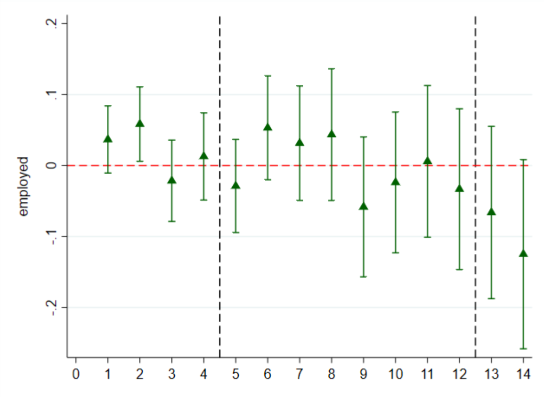

**(a)** Matched, all observations




**(b)** Overall, all observations



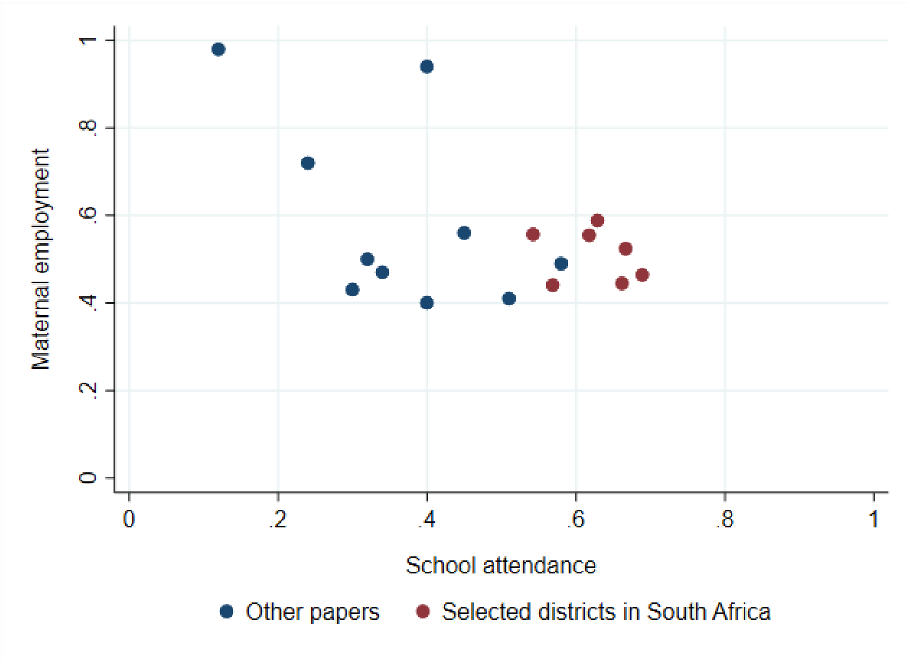

**(c)** Matched, 5 days donut




**(d)** Overall, 5 days donut

**Notes:** The figure presents point estimates and 95% confidence intervals for $\beta_{RDD}$ in Equation (2) using 2011 Census data. The outcome of interest corresponds to maternal employment. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (i.e. aged 13 and above), (ii) those who were affected by the reform (i.e. aged 5 to 12), and (iii) those who are too young to be affected by the reform (i.e. below the age of 5). Panel A presents RDD results using all observations in the *matched* sample; Panel B presents results using all observations in the *overall* sample, Panel C excludes from the *matched* sample observations whose reported birth date lies in the five days before and after the cutoff in the; Panel D makes the same restriction but for the *overall* sample.

**Figure A11:** Relationship between maternal employment and children enrollment, selected South African districts and values from previous studies




**Notes:** The figure reports the mean rates of maternal employment and children's school enrollment at baseline in selected previous studies (blue dots in the figure), as well in the South African districts in the sample that are, at the same time, in the top 25 per cent in terms of maternal employment and in the bottom 25 per cent in terms of children's school enrollment. These districts are presented in red in the figure.

## B Appendix: Additional tables

**Table B1:** School enrollment rates for 6 years old children, by their mothers' personal characteristics

| | Born in June | | Born in July | | Difference | | t-test |
|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | pp | % | |
| Overall | 0.912 | 0.284 | 0.911 | 0.285 | 0.001 | 0.07% | 0.07 |
| Marital status: married | 0.913 | 0.283 | 0.915 | 0.279 | -0.002 | -0.25% | -0.17 |
| Marital status: unmarried | 0.911 | 0.285 | 0.908 | 0.290 | 0.003 | 0.37% | 0.25 |
| Education: Grade 11 or less | 0.884 | 0.320 | 0.890 | 0.314 | -0.005 | -0.59% | -0.38 |
| Education: More than Grade 11 | 0.951 | 0.216 | 0.942 | 0.234 | 0.009 | 0.97% | 0.81 |
| Number of children: One | 0.923 | 0.267 | 0.882 | 0.322 | 0.040 | 4.57% | 2.40 |
| Number of children: More than one | 0.907 | 0.291 | 0.926 | 0.262 | -0.020 | -2.11% | -1.75 |
| Internal migrant: Yes | 0.931 | 0.254 | 0.909 | 0.288 | 0.022 | 2.39% | 1.08 |
| Internal migrant: No | 0.907 | 0.290 | 0.910 | 0.286 | -0.003 | -0.33% | -0.28 |
| Area of residence: Urban | 0.916 | 0.278 | 0.899 | 0.302 | 0.017 | 1.85% | 1.36 |
| Area of residence: Rural | 0.906 | 0.292 | 0.929 | 0.257 | -0.022 | -2.42% | -1.54 |
| Grand parent: In HH | 0.918 | 0.275 | 0.924 | 0.266 | -0.006 | -0.61% | -0.31 |
| Grand parent: Not in HH | 0.910 | 0.287 | 0.907 | 0.290 | 0.003 | 0.29% | 0.24 |
| Spouse: Employed | 0.917 | 0.275 | 0.913 | 0.281 | 0.004 | 0.44% | 0.21 |
| Spouse: Not employed | 0.892 | 0.311 | 0.899 | 0.302 | -0.007 | -0.76% | -0.26 |

**Notes:** The table reports the mean and standard deviation (SD) of the school attendance rate for children born in June and July of 2005 (i.e. six years old at the time of the 2011 Census). These statistics are reported for the overall sample, as well as separately for groups of children defined by selected mothers' individual characteristics as well as household characteristics. The table also reports the mean difference in values between those born in June and those born in July (in both percentage points and per cent), as well as the results of a t-test for equality of means.

**Table B2:** Maternal employment rates for 6 years old children, by mothers' personal characteristics

| | Born in June | | Born in July | | Difference | | t-test |
|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | pp | % | |
| Overall | 0.364 | 0.481 | 0.361 | 0.480 | 0.002 | 0.66% | 0.15 |
| Marital status: married | 0.394 | 0.489 | 0.374 | 0.484 | 0.020 | 5.30% | 0.88 |
| Marital status: unmarried | 0.333 | 0.472 | 0.349 | 0.477 | -0.016 | -4.56% | -0.73 |
| Education: Grade 11 or less | 0.271 | 0.445 | 0.270 | 0.444 | 0.001 | 0.37% | 0.05 |
| Education: More than Grade 11 | 0.496 | 0.500 | 0.491 | 0.500 | 0.005 | 1.05% | 0.20 |
| Number of children: One | 0.351 | 0.478 | 0.339 | 0.474 | 0.012 | 3.61% | 0.46 |
| Number of children: More than one | 0.369 | 0.483 | 0.373 | 0.484 | -0.004 | -0.96% | -0.18 |
| Internal migrant: Yes | 0.464 | 0.499 | 0.448 | 0.498 | 0.016 | 3.50% | 0.42 |
| Internal migrant: No | 0.338 | 0.473 | 0.339 | 0.474 | -0.002 | -0.50% | -0.10 |
| Area of residence: Urban | 0.459 | 0.499 | 0.441 | 0.497 | 0.018 | 3.99% | 0.84 |
| Area of residence: Rural | 0.229 | 0.420 | 0.245 | 0.430 | -0.016 | -6.44% | -0.71 |
| Grand parent: In HH | 0.307 | 0.462 | 0.323 | 0.468 | -0.016 | -4.83% | -0.50 |
| Grand parent: Not in HH | 0.381 | 0.486 | 0.373 | 0.484 | 0.008 | 2.13% | 0.44 |
| Spouse: Employed | 0.5154 | 0.5002 | 0.4785 | 0.5001 | 0.037 | 7.72% | 1.14 |
| Spouse: Not employed | 0.2161 | 0.4123 | 0.2345 | 0.4245 | -0.018 | -7.82% | -0.49 |

**Notes:** The table reports the mean and standard deviation (SD) of the employment rates of mothers whose children were born in June and July of 2005 (i.e. six years old at the time of the 2011 Census). These statistics are reported for the overall sample, as well as separately for groups of children defined by selected mothers' individual characteristics as well as household characteristics. The table also reports the mean difference in values between those born in June and those born in July (in both percentage points and per cent), as well as the results of a t-test for equality of means.

**Table B3:** Summary of the Literature on Childcare and Mother's Employment

| Paper | Country&Year | Policy | Age | Enrollment | $\beta$Enrolled | Emp. | $\beta$Emp. | Long Term |
|---|---|---|---|---|---|---|---|---|
| **RCTs** | | | | | | | | |
| Ajayi et al. (2022) | Burkina Faso, 2019 | Free childcare | 0-6 | 0.12 | +24pp. | ≈0.98 | + 0pp. | 1.2y |
| | | | | | | | + wage emp. | |
| Attanasio et al. (2022) | Brazil, 2008 | Free childcare | 0-3 | | See Barros et al. (2013) | | | 4-7y. |
| | | | | +0.64 y. wrt to 1.9y. in control group | | | | |
| | | | | | | 0pp. mothers t+4, t+7; | | |
| | | | | | | ++ other in hh. | | |
| Barros et al. (2013) | Brazil, 2008 | Free childcare | 0-3 | 0.51 | +43pp. | 0.41 | +4.2pp | <1y. |
| Bjorvatn et al. (2022) | Uganda, 2018 | Childcare subsidy | 3-5 | 0.82 | +15pp. | 0.47 | +1pp. | <1y. |
| | | | Full day: | (+0.34) | (+48pp.) | +14pp. for single mothers | | |
| Clark et al. (2019) | Kenya, 2015 | Childcare vouchers | 1-3 | 0.58 | +25pp | 0.49 | +8.5pp | 1y. |
| Gantumgalag et al. (2019) | Mongolia, 2017 | Free childcare | 2 | 0.45 | +45pp. | 0.56 | +4.6pp | <1y. |
| Hojman and López Bóo (2019) | Nicaragua, 2013 | Free childcare | 0-4 | ≈ 0.30 | +50pp. | ≈0.43pp. | ≈ +7pp.† | ≈ 2y. |
| Martinez et al. (2017) | Mozambique, 2008 | Free childcare | 3-5 | ≈0.06 | ≈ +30pp. | (0.24)‡ | (≈ +3.7pp)‡ | 2y. |
| Martínez and Perticará (2017) | Chile, 2012 | 3h after school | 6-13 | 0.24 | +32pp | 0.72 | +3.4pp | <1y. |
| **RDDs** | | | | | | | | |
| Berlinski et al. (2011) | Argentina, 1995 | School entry age | 4 | 0.40 | +30pp | 0.40 | +0-6 pp. | <1y. |
| Dang et al. (2022) | Vietnam, 2010 | School entry age | 1-5 | 0.40 | +6.6pp. | ≈ 0.94 | ≈ +0pp. | 2y. |
| | | | | | | | + wage emp. | |
| Rosero and Oosterbeek (2011) | Ecuador, 2006 | Free childcare | 0-6 | Not comparable | | ≈ 0.3 | ≈+30-35pp. | ≈ 2y. |
| Ryu (2020) | Brazil, 2011 | School entry age | 4 | 0.32 | +8.1pp. | ≈ 0.5 | ≈ +0pp | <1y. |
| | | | | | | | + formal emp. | |

† Estimate from a 14pp. IV estimate and roughly a 50pp. first stage.

‡ Refers to all caregivers in the household and not only mothers.

Other Literature on Childcare and Mother's Employment - Continued

| Paper | Country&Year | Policy | Age | Enrollment | $\beta$Enrolled | Emp. | $\beta$Emp. | Long Term |
|---|---|---|---|---|---|---|---|---|
| **District/Province/Region- level DiD** | | | | | | | | |
| Berlinski and Galiani (2007) | Argentina, 1994 | Free childcare | 3-5 | Not Comparable | | Not Comparable | | |
| Berthelon et al. (2015) | Chile, 2004 | Longer school day | 6-13 | Not Comparable | | Not Comparable | | |
| Calderón (2014) | Mexico, 2000-2010 | Free childcare | 1-3 | Not Comparable | | Not Comparable | | |
| De la Cruz Toledo (2015) | Mexico, 2004 | Free childcare | 3-5 | Not Comparable | | Not Comparable | | |
| Halim et al. (2022) | Indonesia, 1993 | Free childcare | 3- 6 | Not Comparable | | Not Comparable | | |
| Kilburn and Datar (2002) | China, 1991 | Free childcare | 0-5 | Not Comparable | | Not Comparable | | |
| Medrano (2009) | Chile, 2003 | Free Childcare | 0-4 | Not Comparable | | Not Comparable | | |
| Padilla and Cabrera (2018) | Mexico, 2007 | Longer school day | 6-12 | Not Comparable | | Not Comparable | | |
| **Other** | | | | | | | | |
| Attanasio and Vera-Hernandez (2004) | Colombia, 2002 | Subsidized Care | 0-6 | Not Comparable | | Not Comparable | | |
| Du and Dong (2013) | China, 1991 | Free childcare | 0-6 | Not Comparable | | Not Comparable | | |
| Du et al. (2019) | China, 1991 | Free childcare | 0-6 | Not Comparable | | Not Comparable | | |
| Sanfelice (2019) | Brazil, 2010 | Free Childcare | 1-4 | Not Comparable | | Not Comparable | | |

*Note*: Aggregate-level estimates of enrollment and employment are not comparable to the one of our study or the individual-level studies presented in the first portion of the tables; for this reason they are not recovered and presented.