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Children's Educational Enrollment and Maternal Labor Supply*

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Abstract

We examine the impact of a nation-wide reform in South Africa anticipating admission into primary school on children's school enrollment and mothers' labor supply. Using Census data and exploiting month-of-birth discontinuities and the before/after variation introduced by the reform, we find a 7pp. increase in school attendance at age 5 but no effect on maternal employment and the type of job held. We conduct a meta-analysis to reconcile our findings with those of previous studies in developing and emerging economies. Results show that the high baseline school enrollment rate in our study context can explain these effects. Heterogeneity analysis across South African districts further corroborates this conclusion.

JEL Classification: I28, J13, J16

Keywords: Childcare; Maternal labor supply; South Africa

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1 Introduction

In virtually all countries around the world, women have lower employment rates than men as well as lower earnings upon employment. 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). Gender and racial inequalities often overlap in South Africa, and this leaves Black and Coloured mothers, the population at the center of this study, in a particularly disadvantaged situation in the labor market.

Against this backdrop, the attention of researchers and policy makers has focused on the options available to keep women in the labor market after the birth of a child. In most countries, this critically depends on the availability, affordability and quality of childcare services.[1] 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. These studies generally report positive effects on either women's employment probability or the quality of the jobs found (see Halim et al. (2023) for evidence from a recent literature review on emerging and developing countries). However, estimates differ significantly across studies, especially in developing countries, potentially owing to differences in the type of treatment offered, the target population or other contextual factors.

This paper explores the effect on children's school attendance and maternal employment of a national reform in South Africa that lowered school admission age. 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 – when enrollment is mandatory – 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 school

[1] Childcare availability can have many positive effects on mothers beyond their labor force participation effects (i.e., mental and physical health as well as intra-household bargaining). However, the focus of this contribution is on its potential role to maintain attachment to the labor market.

enrollment and mothers' employment. One key advantage of our data is that we are able to define mothers' treatment status without having to link mothers and children, thus taking away concerns of selection due to migration, co-residence choices or household composition.

We find that the reform had a positive impact on overall school attendance (+7 percentage points, pp.). This result, which is robust to the use of different data sources, the definition of different samples as well as alternative econometric specifications, is large in magnitude: considering that a high share of pupils was already attending some type of education (above 70% in the control group) and this mechanically capped the absolute effect on overall school enrollment, an increase of 7pp. implies almost one out of four children – among those who had not yet enrolled – attended education earlier than anticipated because of the reform. The effect on school enrollment is stronger for children whose mothers (i) are unmarried, (ii) have lower educational attainments, and (iii) live in rural areas, potentially indicating that the expansion of childcare availability benefited more relatively disadvantaged groups who could not afford to send their children to pre-school, which was generally at a fee. We also find that the effect on overall school attendance 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 effect on overall school attendance converges to zero over one school year, as school attendance becomes mandatory for everyone.

At the same time, 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 contemporaneous effects of comparable magnitude to those found in the literature. The zero effect on maternal employment is precisely estimated for different groups of mothers, defined by their marital status, educational attainments and number of children; among others. 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. Consistently, we find no long-term change in the employment probability of affected mothers also several years after exposure to the policy. 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, by average hourly and monthly earnings, tenure and hours worked in a given occupation and sector).

In the last part of the paper, we benchmark our findings to those of comparable previous studies on the expansion of childcare or school availability in developing countries.

This allows us to explore what policy characteristics or contextual factors are behind differences in results across different contexts, and better place the results of our study. We extract a set of comparable parameters for all the causally-identified previous studies with settings comparable to ours (N=14), which include baseline characteristics (e.g. enrollment, employment), as well as policy and study features. Results from this meta-analysis show that, among the characteristics we collect, a high baseline enrollment rate is consistently linked with a lower overall impact on childcare/school enrollment. If we predict, from the sample of studies at our disposal, what would be the effect on school enrollment in South Africa given baseline conditions, we obtain an estimate in line with the one identified in this study. At the same time, impacts on school enrollment and maternal employment are linked in the literature by a proportion of roughly five to one, which is again consistent with our null result on maternal employment within the context of the South African reform.

Our overall conclusion is that the literature indicates that the magnitude of effects on children's school enrollment and maternal employment are tightly linked, and that these depend crucially on baseline conditions, in particular the school enrollment rate. Motivated by this observation, we explore the heterogeneity of effects across South African districts (N=52), with varying initial conditions but holding the national policy shock constant. The results further corroborate the conclusion that effects are negatively related to the baseline enrollment rate: in districts where baseline enrollment was lower to begin with, the impacts on children's enrollment and mother's employment are significantly higher.

This paper contributes to a large literature exploring the relationship between childcare and mothers' labor supply. Previous studies have found a positive effect of childcare availability or eligibility on mothers' labor 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 mothers' labor supply.[2] In emerging and developing countries, a recent review of the literature draws very similar conclusions across a wide set of studies[3], although with important heterogeneity on whether the effects of higher childcare materialize as higher overall employment or better

[2]For example, see Gelbach (2002), Cascio (2009) and Fitzpatrick (2010) in the United States; 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 literature. Among the studies mentioned, Fitzpatrick (2010) is the only one who does not find a positive effect on labor supply of mothers, when providing pre-kindergarten care for very young children.

[3]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*" (Halim et al., 2023).

quality jobs (Halim et al., 2023). We summarize the studies most directly comparable to ours (with the addition of others that fulfill the criteria) in Appendix Table B1.

We directly contribute to this literature by providing the first, micro-identified evidence from a nation-wide reform in Sub-Saharan Africa. While other studies on the effects of childcare on maternal labor supply exist from the region, they come from randomized controlled trials that are limited in terms of eligible individuals as well as the geographical coverage of the intervention (Martínez and Perticará, 2017; Clark et al., 2019; Ajayi et al., 2022; Donald et al., 2023; Bjorvatn et al., 2025). Compared to these studies, we are able to examine the effects *at scale* of a positive shock in school eligibility to all eligible individuals across the country. As a result, the context presented is ideal to study the possible effects of expanding public childcare at scale and through public funding, which might differ from more targeted interventions provided by private institutions, non-governmental organizations or local communities. Moreover, our setting is characterized by a *change* in the age-based threshold that determines school eligibility as a result of the reform that we study, which is not the case for previous studies exploiting similar age-based discontinuities outside of Sub-Saharan Africa (Berlinski et al., 2011; Ryu, 2020; Dang et al., 2022; Pritadrajati, 2026). In short, we argue that exploiting the before/after change, together with the age threshold discontinuities, allows us to approximate the internal validity of a more targeted RCT, but within the context of a national policy at scale, a combination not yet available in the literature.

The second contribution of the study is to provide further evidence on how contextual factors relate to the impact of expanding childcare or school access on children's enrollment and maternal employment. This is particularly important, given that the body of evidence on the effects of childcare availability on maternal employment in emerging and developing economies has not identified clear drivers that explain these differences in (the magnitude of) results. Our contribution is to conduct a meta-analysis of all comparable studies, from which we extract the finding that initial levels, in particular high (low) initial enrollment, are a strong predictor of smaller (larger) effects on school enrollment and maternal employment. This conclusion is confirmed by heterogeneous effects across South African districts – which are equally impacted by the reform, but with different baseline conditions – and provides an important policy implication on the potential targeting of interventions at scale aimed at increasing school enrollment/maternal employment through higher childcare availability in emerging and developing economies.

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 relation to the literature, the results of the meta-analysis and presents heterogeneity across South African districts; Section 7 concludes.

2 Institutional background

Context 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), with the share of 5 year-old children attending some form of education increasing from 39% in 2002 to 78% in 2009.

Grade R and Grade 1 roughly cover half of the day over 5 days. Specifically, Grade R covers 23 hours of instruction time per week, while Grade 1 is up to 25 hours.[5] Instead, pre-schools (also known as ECD centres) tend to cover a larger portion of the day, although this is much more heterogeneous.[6] 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" (DBE, 2021).

Compulsory Age The *South African School Act of 1996* 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 sets that schooling is

[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))

[5]National Policy pertaining to the Programme and Promotion Requirements of the National Curriculum Statement Grades R-12.

[6]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.

compulsory from the calendar year in which a learner turns 7.[7] Given that in South Africa the school year coincides with the calendar year, everybody who is 6 on January 1st and turns 7 before December 31st must be in school in that year.

Admission Age The 1996 law did not specify an *admission* age, i.e., the age when children have to be admitted to school, even before compulsory education kicks in. This is set directly by the Ministry and has first been specified in 1998, with an implementation starting in 2000[8]; the provision specifies that a learner should be admitted to grade R if she/he turns 6 by that calendar year, and should be admitted to grade 1 if he/she turns 7 by the end of that calendar year.[9] 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 was not compulsory but admission age was in the calendar year in which a kid turns 6.

In 2002, the government decided to change the admission age of pupils to schools.[10] The new law[11] 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

[7] *"..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".*

[8] "Age requirements for admission into an ordinary public school" It is not clear what the norm was between 1996 and 2000, but it is likely that it was the same as that stated in 1998.

[9] The exact phrasing of the different articles of the law is: " *The statistical age norm per grade is the grade number plus 6. 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.*"

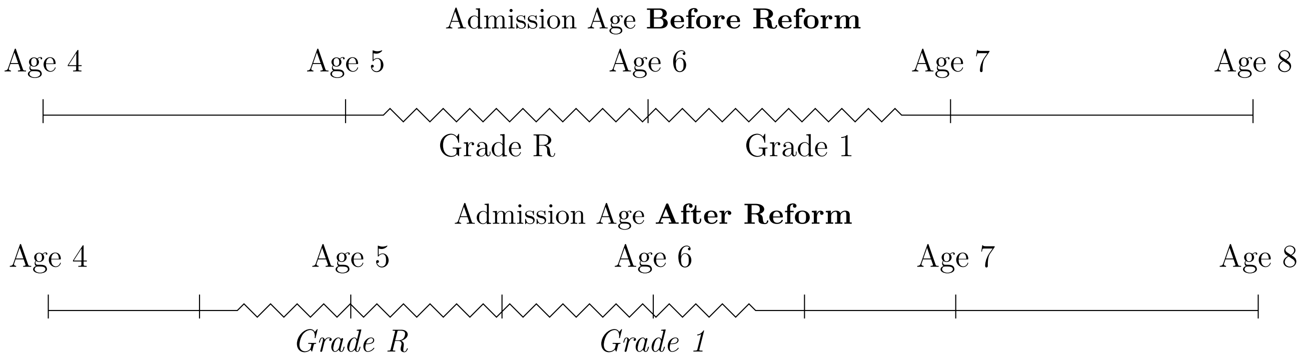
"*This notice is called the Age Requirements for Admission to an Ordinary Public School, and it comes into effect on 1 January 2000*"

[10] It is unclear exactly what brought about the reform. One reason might have been that the admission age set by the government gazette in 1998 (with application) in 2000 was challenged by pupils in some independent schools, who would have had to postpone entry into Grade 1 (Court case). 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 admission-age threshold.

[11] The *Education Laws Amendment Act 50 of 2002* (coming into operation on January 1st 2004, available here) 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 admission age



Notes: The figure reports the effect on children's admission 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).

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 are, to our knowledge, no provisions 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 was 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.

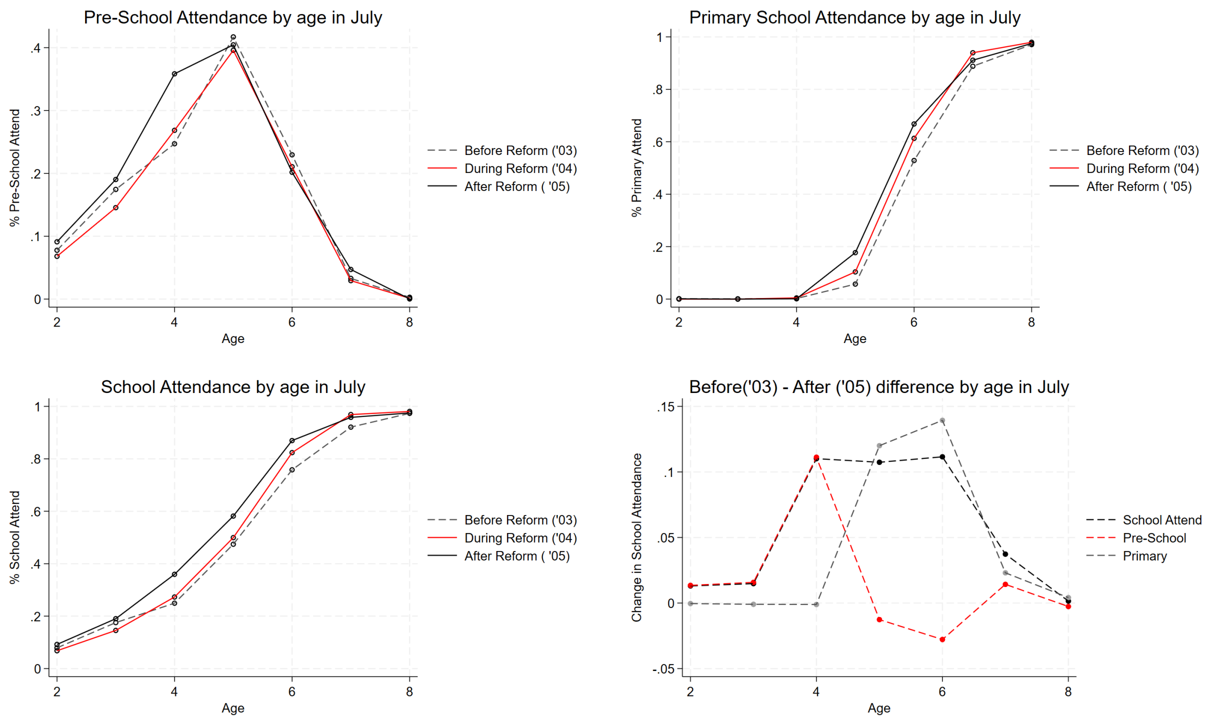
Descriptive Evidence on the Impact of the Reform on School Enrollment The effect of the reform on school enrollment is clearly visible from a variety of surveys. In Figure 2, we plot some descriptive statistics calculated on the General Household Survey (GHS). The GHS is a nationally representative survey 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 values 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.

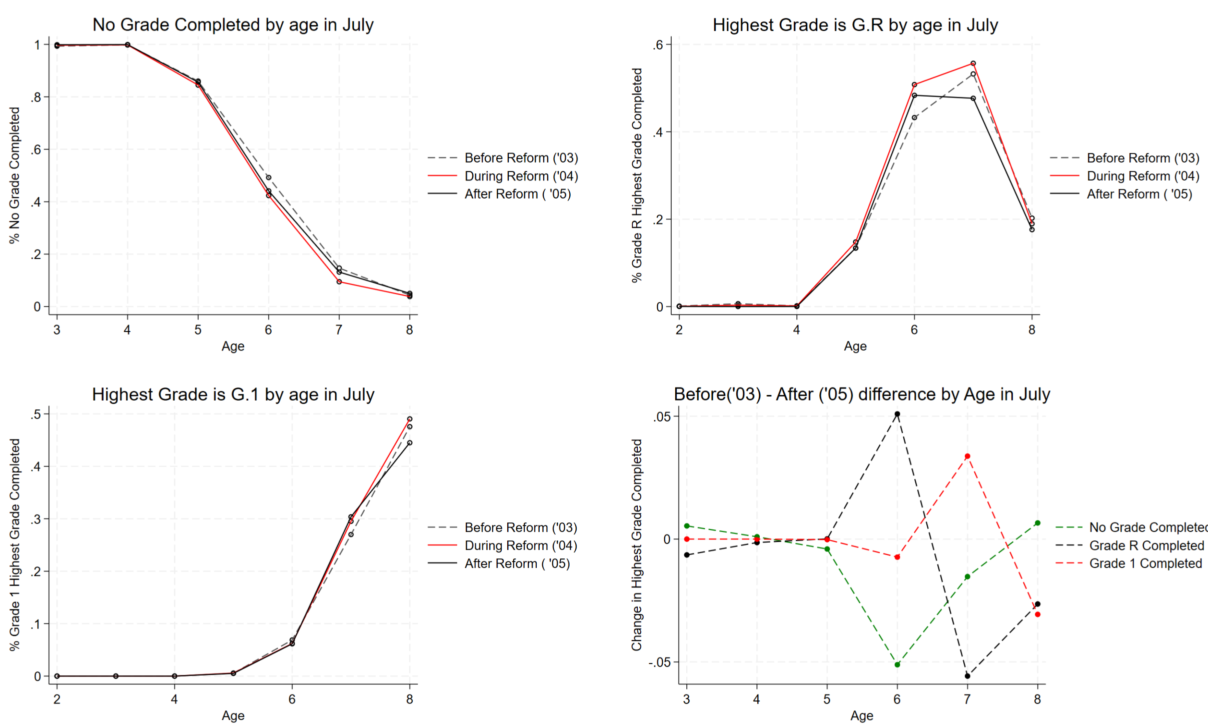
The effect of the reform is also clearly visible when looking at information on the highest grade completed. Specifically, in Figure 3 we plot the share of individuals who have completed at most a given grade at a given age (reported on the x-axis), before (2003), during (2004) and after (2005) the reform. 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 show 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 (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 *General Household Survey*, 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 *General Household Survey*, 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 with basic information on income (only categorical) and employment (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 aged 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 of the 2007 CS 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 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. 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, because in 2007 we lack exact birth dates of survey respondents); 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.[12]

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.[13] We will present results on both children's school enrollment and maternal labor supply using the *matched* sample in the main body 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.

[12]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, if 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.

[13]Black and Coloured individuals represent around 92% of the sample in the 2011 Census; we restrict the analysis to these population groups to reduce heterogeneity in the labor market conditions faced and in other characteristics, given the drastically different outcomes in terms of children's schooling and labour market conditions of White South African mothers.

4 Methods

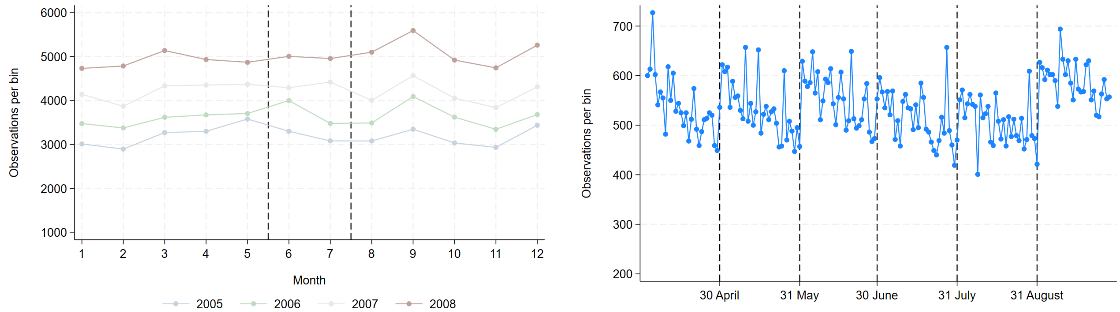
Source of exogenous variation The aim of the paper is to estimate the causal effects of exposure to the policy on children's school enrollment and maternal labor supply. This requires identifying comparable treated and control children (mothers), who differ only based on the age at which they (their 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 variation. 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 us 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 the number of days before or after June 30th. An alternative empirical approach would be to compare the means of individuals on the two sides of the cutoff. As the bandwidth size shrinks to zero, the two approaches would yield equivalent estimates: 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.

Validity of the continuity assumption Regardless of the estimation strategy, 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 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

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



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

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 (Panel A) or in a given day between April and September (Panel B). We exclude observations for which the date of birth is likely imputed (see footnote). 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 too young to be affected by the reform (2007 and 2008). Trends for these cohorts are reported separately in Panel A, and jointly in Panel B.

are more (or less) likely to co-reside *because* of the policy. However, as in our data we are 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 B of Figure 4.[14] 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 at the time of the 2011 Census. These different cohorts are presented separately in Panel A (which reports information at the monthly level for the entire year), and jointly in Panel B (which restricts the focus from April to September, but reports information at the daily level). The graphs show two patterns: the number of observations per month of birth is relatively stable across months (with the exception of September)[15], but 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.

An uneven distribution of dates of births is common to other studies in the literature

[14]Here and in the rest of the analysis, we are excluding date of birth values that have been imputed or misreported in the Census data (which does not have missing values); the pattern is easy to spot in the raw data, as missing values are imputed with the same number of day, month and year, for example 5.5.2005 or 6.6.2006. These observations account for only around 1.1% of the total number of observations in the sample.

[15]The fact that the number of observations in each cohort increases with the year of birth of the child (evident from Panel A) 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.

that have exploited similar day-of-birth discontinuities to study the impact of childcare on mothers' employment (Berlinski et al., 2011; Dang et al., 2022).[16] However, differently from these settings, we can also take advantage of the change in cutoff over time, which applies to certain cohorts but not for others. If we find that the discontinuous jump in the number of observations at the end of each month is common to cohorts both affected and unaffected by the reform, we can then conclude that this pattern is not related to the policy change.

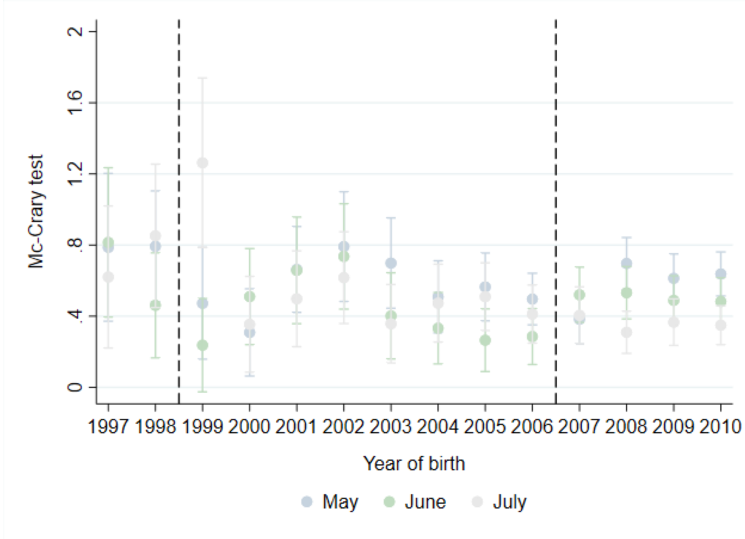
Misreporting of date of birth Misreporting of date of birth could occur in two ways: both within month, if people misreport the day of birth and only report the month correctly, or across months, if people tend to round to the beginning of the closest following month. In our interpretation, the shape 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.

While we are not able to test formally the two hypotheses, we can rule out that the misreporting of the date of birth is specific to the reform we aim to study. This is confirmed by a standard McCrary analysis, which we show in Figure 5. The evidence clearly shows that the pattern is consistent across different months of the year, not only the threshold specified by the reform (June 30th), 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), and cohorts not yet affected (2007-2010).

Preferred estimator 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 comparison of means on each side of the threshold. However, we also show that,

[16]Other studies have found that some individuals misreport birth dates, either deliberately, for example to delay the age of school enrollment or postpone military service (Anelli et al., 2023), or because they do not know the exact birth dates and round age values (or birth dates) to a given number, or to the closest month. For example, Ranchhod (2006) shows evidence of rounding with respect to age values of pensioners in South Africa.

Figure 5: McCrary 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 breakpoints within each cohort. These breakpoints correspond to (i) 1 June, (ii) 1 July and (iii) 1 August. The July 1st break point is the relevant one for the analysis, while the other two breakpoints are used for reference purposes. In each case, a bandwidth of 30 days on each side of the threshold is used. The vertical dashed lines delimit the cohorts of individuals who (i) were not affected by the reform (1997 and 1998), (ii) those who were affected by the reform (from 1999 to 2006), and (iii) those who are too young to be affected by the reform (from 2007 to 2010). The analysis is conducted using 2011 Census data.

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 exercise is clear: 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.

Distribution of observable characteristics around cutoff The validity of the setting 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 for a bandwidth of 30 days around the June 30th cutoff, together with estimates obtained by fitting both an RDD and a comparison of means regression. The results show that these individual characteristics evolve smoothly around the cutoff.[17] As a joint test,

[17] The only exception is represented by the dummy equal to one if the observation refers to a mother who has been matched in the household with her child. The coefficient of the comparison of means approach in this case is statistically significant; however, the RDD coefficient on the same plot is not statistically significant. In light of this, we interpret this imbalance as potentially spurious and as a result of testing multiple hypotheses on several variables, rather than as an effect of the reform. Importantly, we do not need

we also compute the *predicted outcomes* on each side of the threshold, with a 30 day bandwidth, based on these observable characteristics: for both school enrollment of the child (Figure A3), and maternal employment (Figure A4), the predicted values evolve smoothly at the threshold and have virtually identical predicted means on each side.

Specification With this evidence in mind, we prioritize the difference of means on a bandwidth of one month as our preferred specification.[18] The specification 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, β_{DM} indicates, conditional on control variables, the average difference in the outcome of interest. We also include a rich set of individual-level characteristics X_i.[19] 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. Population weights account for the fact that the data is a 10% subset of the overall 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 takes the

balance on this variable for correct identification of the effects of the reform, as we can exploit the *overall* samples where treatment status is defined irrespective of potentially endogenous co-residence decisions.

[18]The decision to select a bandwidth of one month is informed by the fact that the mean squared error optimal bandwidth selector for a non-parametric RDD delivers an optimal bandwidth of 30.2 days. We approximate this with the full months of June and July, also to take into consideration the unequal distribution of birth dates within a month, as discussed above.

[19]These correspond to controls for age of the mother and its square, mother's years of 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, population group and urban area of residence.

following form:

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

Where $DayBirth_i$ is the running variable, inserted linearly as the number of days between the child's birth date and the June 30 of each year. We set the bandwidth equal to 30 days, in order to have the exact same sample as in the preferred specification. All other terms have the same interpretation as above.

Interpretation of coefficients We construct cohorts based on the age individuals have on October 30th of the year in which they are interviewed and estimate treatment effects separately for each cohort of children. This is done in order to harmonize interpretation of the effects between the two data sources, as the data collection was conducted at 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 cohorts 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: these are individuals who have 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 3 years after treatment in the 2007 CS, and until 7 years after treatment in the 2011 Census.

Importantly, in our view, an accurate characterization of the decisions of sending the children to school and joining the labor market is that of *joint* decisions; in most cases, mothers would jointly decide whether to send the kid to school, when childcare is available but not mandatory, and enter the labor market. This is important for the interpretation of the coefficients; if take-up of childcare and joining the labor force are *sequential* decisions, one could read the coefficients on enrollment and employment as a first stage and intention-to-treat respectively. If, instead, kid's enrollment and mother's employment are joint decisions, the two outcomes are determined *simultaneously*.[20] For this reason, we do not reweigh our estimates on mothers' employment by the effect on school enrollment, as that would imply that mothers decide on the employment only after having decided on the child's enrollment, which seems implausible. This would (potentially) lead us to incorrectly estimate large

[20] See Berlinski et al. (2024) for a recent example of a modelization of the two decisions as joint.

effects on the treated, simply because the two decisions are joint and hence strongly related.

5 Results

5.A Effects on children's school enrollment

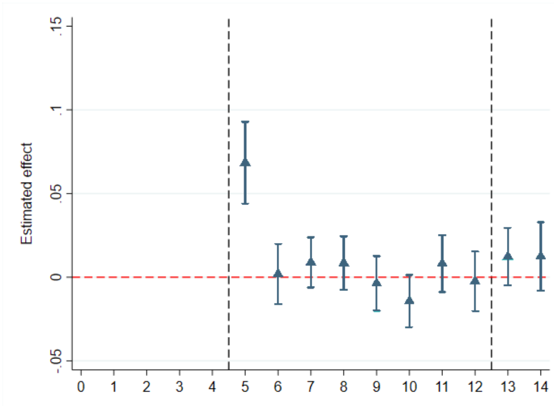
Effect on overall enrollment The effects of the reform on children's school enrollment are plotted in Figure 6. Specifically, the figure presents the estimates from Equation (1), run separately for children of different ages (defined on the x-axis) and using data from either the 2011 Census (Panel A) 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).

Looking at the results obtained with the 2011 Census (Panel A), we estimate that the effect of the reform on school enrollment is around 7pp. at age 5, implying an overall increase in the proportion of kids who go to school by that age. This effect is quite large in magnitude, especially if one considers that the baseline school enrollment rate was already high, at 70 per cent. This means that, among the 30 per cent of children who were not already in school at age 5, almost one out of four enrolled earlier because of the policy reform. However, 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 must 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.

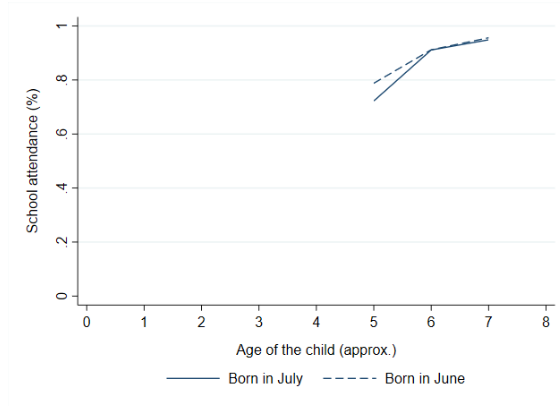
Data from the 2007 CS corroborates these findings. Specifically, treatment effects at age 5 are of the same magnitude as those obtained in the 2011 Census, although less precisely estimated (Panel C). The data also shows the absence of any lasting effect on school enrollment, with the difference that the coefficient at age 6 is also significant but smaller in size.[21] The main advantage of using data from the 2007 CS is that information

[21] 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

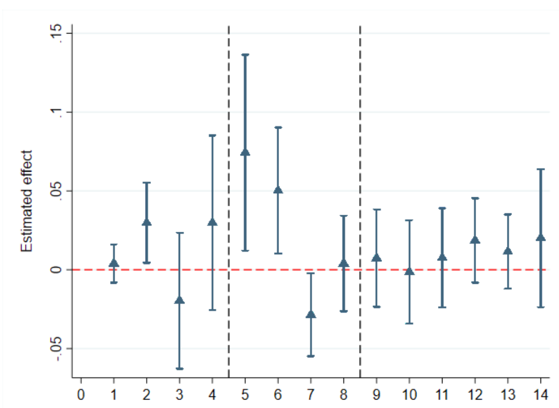
Figure 6: Effects on youngest child's school enrollment



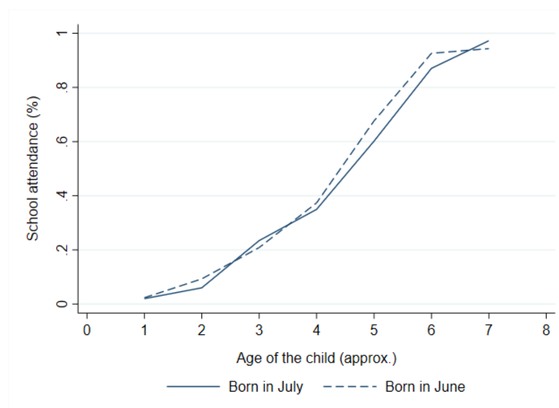
(a) 2011, estimated effects



(b) 2011, school attendance rates



(c) 2007, estimated effects



(d) 2007, school attendance rates

Notes: The figure presents the effects of the reform on children's school attendance. Panel A presents point estimates and 95% confidence intervals for β_{DM} of Equation (1) using 2011 Census data, while Panel C reports the same parameters but using information from the 2007 Community Survey. Standard errors are 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).

about school enrollment is also asked for children younger than 5 years old. 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.[22] This is also evident when 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.[23]

Appendix Table B2 presents the results on overall school attendance for the *matched* sample in 2011 and 2007 in table format. In Appendix Figure A5, 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 A6).[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

be explained by the overall increase in enrollment over time, as the attendance rates at age 6 are higher overall in 2011 than in 2007.

[22]The descriptive evidence in Section 2 showed an increase of school attendance also at age 4 that does not appear when comparing children born in June versus those born in July. This is perfectly consistent with the threshold 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.

[23]We note that coefficient estimates in Panel C are statistically significant also at ages 2 and 7, in addition to ages 5 and 6 as discussed in the text. We interpret these results as spurious and due to the smaller sample and lower precision of the 2007 estimates.

[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 reports missing values for around 30% of children aged 5 in the CS.

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 second-order effect is to reallocate some children from pre-school to primary school at both ages 5 and 6.

Heterogeneous impacts 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 create groups based on selected mothers' individual characteristics (marital status, educational attainments, migrant status and spouse's employment status) and other household characteristics (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, (iv) have migrated internally, defined as not living in the province of birth. The effect on school enrollment is larger when the grandparents live in the household. For married mothers, 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 B3 and confirm that almost no group experiences an increase in overall enrollment at age 6 (in 2011).

Table 1: School enrollment rates for 5 years old children, by households' and mothers' characteristics

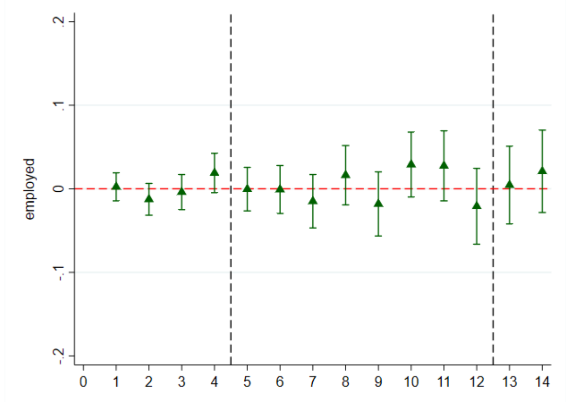
	Born in June		Born in July		Difference		t-test
	mean	sd	mean	sd	pp	%	
Overall	0.788	0.409	0.722	0.448	0.066	9.09%	5.13
Marital status: married	0.787	0.409	0.742	0.438	0.046	6.15%	2.54
Marital status: unmarried	0.788	0.409	0.703	0.457	0.086	12.19%	4.70
Education: Grade 11 or less	0.741	0.438	0.669	0.471	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.

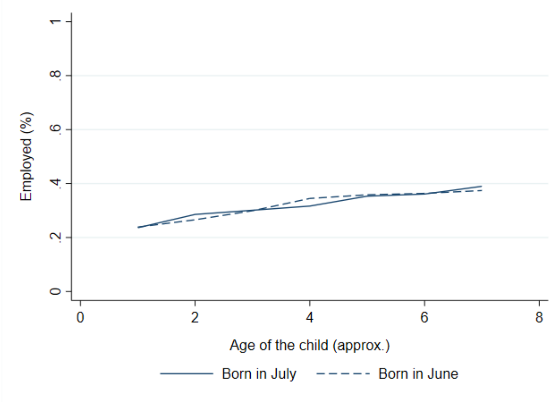
5.B Effects on maternal employment

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

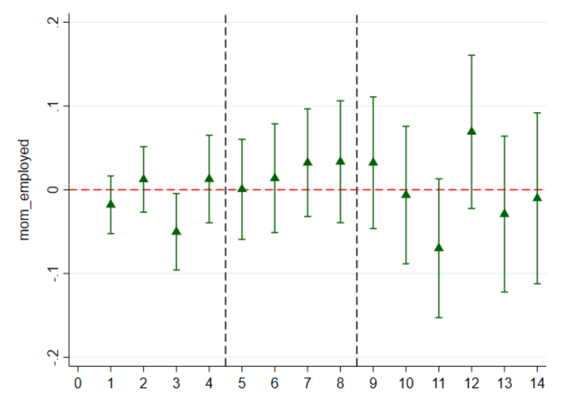
Figure 7: Effects on maternal employment, matched sample



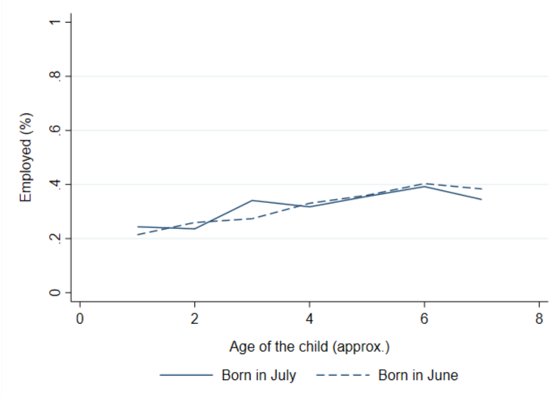
(a) 2011, estimated effects



(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 β_{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).

older. However, this increasing trend is similar 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).

In Appendix Table B4, we present the results on maternal employment for the

Table 2: Maternal employment rates for 5 years old children, by households' and mothers' 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.

matched sample in 2011 and 2007 in table format. In Appendix Figure A7, 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. 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. Instead, we find zero estimates at age 5 on both the 2011 Census and the 2007 CS.

Heterogeneous Impacts 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. 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 pp., and two other groups report differences between one and two pp. Appendix Table B5 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.

Effect on Job Search This result suggests that the absence of a contemporaneous employment effect is not driven by the fact that finding a job takes time. Rather, it suggests that affected mothers are not using the additional school enrollment of the children in order to find a new job. In line with this hypothesis, Figure A8 shows that there is no treatment effect on the probability of being unemployed and looking for a job. 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 (Figure A9). We do not find that mothers impacted by the reform are more likely to report different reasons for not working, including reasons such as taking care of the household or lack of jobs.

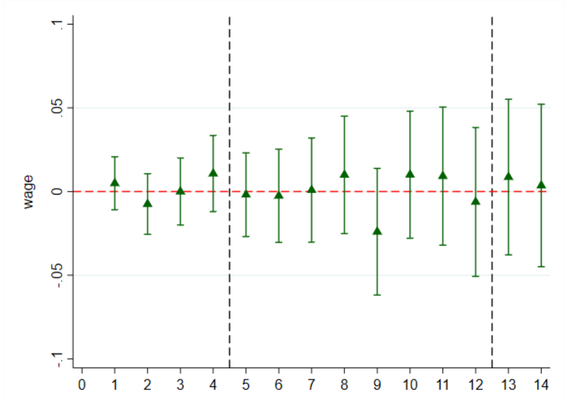
Effect on type of employment and job quality While the evidence discussed so far has shown the absence of an effect on employment along the extensive margin, it is still possible that the reform affected maternal labour market outcomes by changing the type of jobs or the quality of the jobs held. Specifically, one possibility is that higher enrollment of kids allows mothers to find better jobs. Focusing on studies from developing and emerging economies, 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.

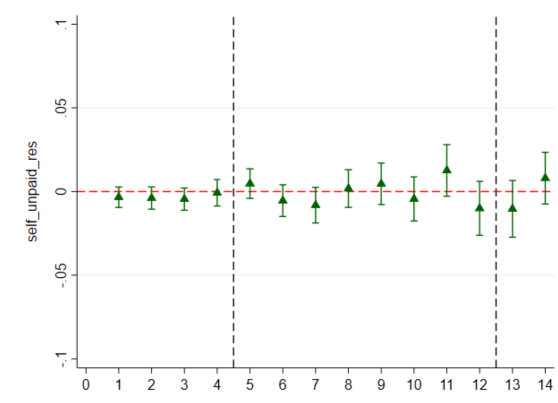
We further look at treatment effects on measures of job quality (Figure 9), by cal-

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

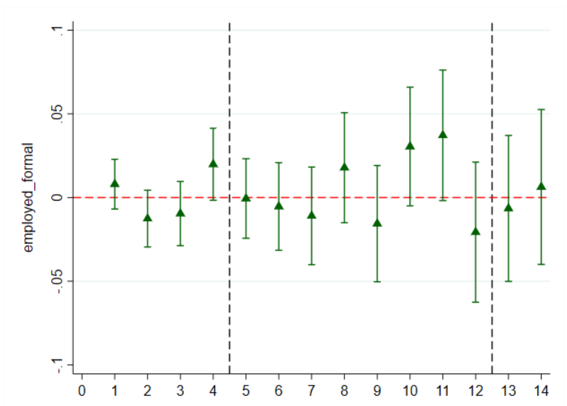
Figure 8: Effects on maternal employment, matched sample: by status in employment and nature of the job



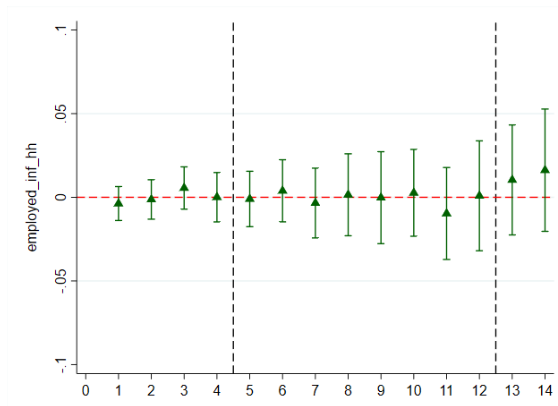
(a) Wage employee



(b) Self-employed or unpaid



(c) Formal employment



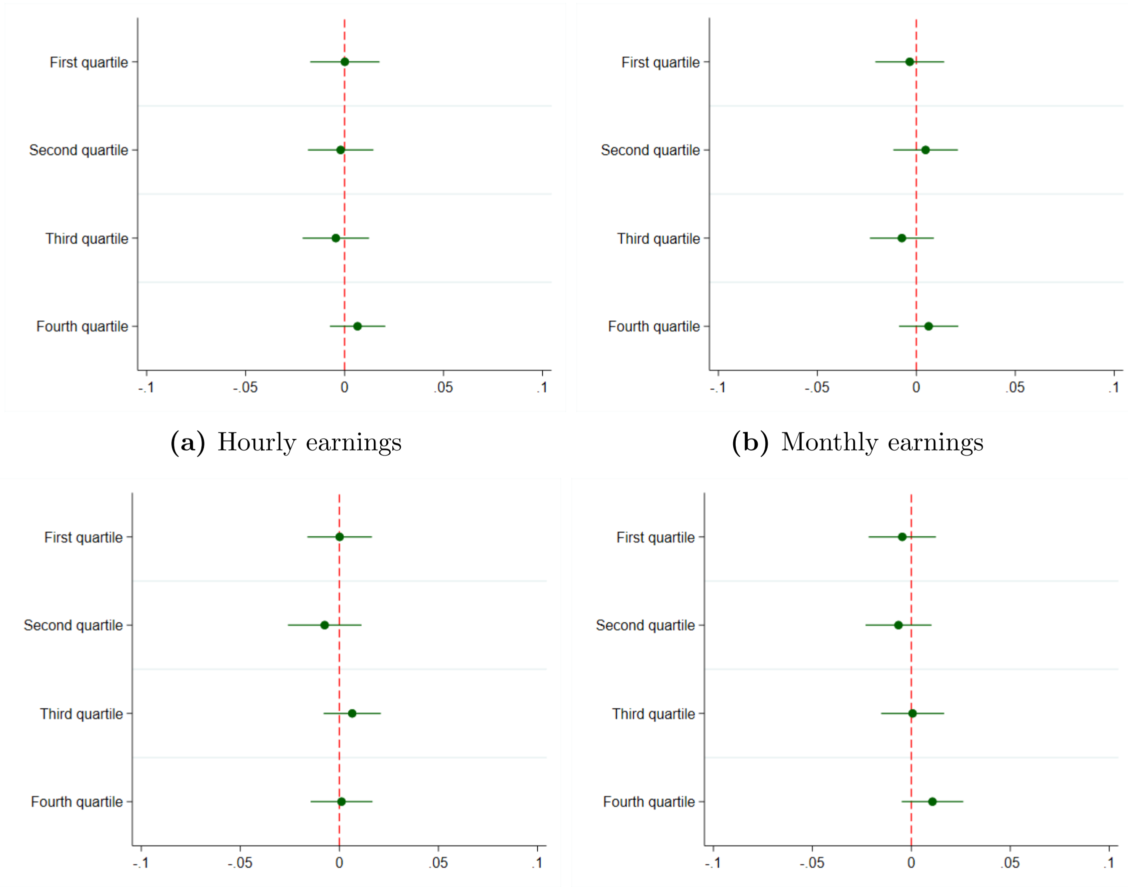
(d) Informal or household employment

Notes: The figure presents the 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 dummy variables for whether the individual belongs 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 β_{DM} in Equation (1). Standard errors are 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).

culating 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 Census data, which does not contain information on these four job characteristics, but has very precise occupation and industry of employment information. We then divide the sample into quartiles based on these estimated measures of job quality. Overall,

[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.

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 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 2011 Census does not report information on these job quality variables. For this reason, we calculate the median of these variables in each sector×occupation cell in the *Quarterly Labour Force Survey*, 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 β_{DM} of Equation (1). Standard errors are clustered at the household level and sampling weights are included. Results are obtained by estimating treatment effects only for mothers with children aged 5 in the *matched* sample (see text for details).

we do not observe a change in the probability of being employed in higher quartiles for any of these job quality dimensions.

5.C Alternative estimation

As shown in Section 4, the distribution of observations is not constant around the cutoff, which led us to prefer a difference of 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 present results with a local linear RD design both with the same bandwidth and sample as the main specification (30 days on each side of the cutoff), and with a "donut" approach that excludes individuals born in the last five days of June and the first five days of July, with the purpose of eliminating observations who might have been more likely to misreport their value of the running variable.

Appendix Figure A10 presents the RDD results on children's school enrollment; while Appendix Figure A11 plots the corresponding results on maternal employment. Both figures replicate the organization of Figures 6 and 7, presenting separately results for different cohorts of individuals. Appendix Figure A12 shows in graphical form the relationship between the running variable (i.e. distance from the cutoff, on the x-axis) and the outcome of interest (on the y-axis).

Overall, both results on school enrollment and maternal employment are qualitatively very similar to the ones of the preferred specification, although point estimates are less stable and more imprecise from one cohort to another. Nonetheless, results indicate an increase in school enrollment of roughly 7pp. at age 5, and the absence of a clear effect on maternal employment.

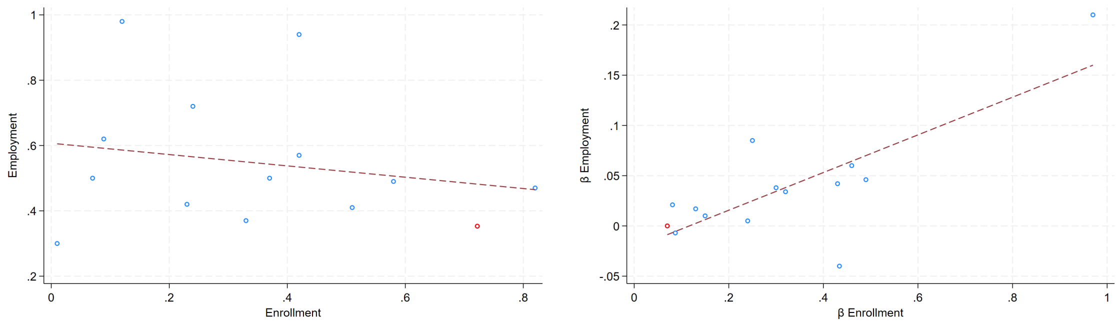
6 Relation to the literature and further evidence

6.1 Relation to the literature

The previous section has shown that, while the decrease in school admission age increased children's school enrollment, this did not have any effect on maternal labor supply. Additionally, the results have shown that there were no effects on the types of jobs held, nor on the quality of these jobs. This means that earlier school admission of children did not lead Black and Coloured South African mothers to find more or better jobs.

We want to interpret our results within the context of the literature studying the effects of the expansion of childcare availability on maternal labor supply in developing and emerging countries. Specifically, this section explores how our results on children's school enrollment and maternal employment compare to those of previous studies, and what are the underlying factors explaining differences in results across studies. By doing so, the discussion also aims to highlight under which conditions one could expect higher school enrollment to lead to higher maternal employment. While several studies exist in low- and middle-income countries examining the effects of childcare availability on maternal employment, the results

Figure 10: Evidence from the literature



(a) Relationship between baseline maternal employment and baseline children's school enrollment

(b) Relationship between treatment effects on children's school attendance and maternal labor supply

Notes: Panel A reports the mean rates of maternal employment and children's school enrollment at baseline, while Panel B plots the estimated treatment effects on children's enrollment and maternal employment. These parameters have been extracted from previous studies in the literature, as included in Appendix Table B1.

differ.

To do so, we first select a subset of studies exploring the effect of childcare on mothers' labor supply in middle- and low-income countries for which we can obtain parameters comparable to the estimates of our paper, i.e., papers that analyze the effects of an exogenous shock to childcare or school-entry age on enrollment and maternal employment, at the individual level. *De facto*, this implies focusing on papers with an RCT or RDD methodology.[28] We summarize these studies in Appendix Table B1.[29] For each study, the table reports (i) the policy analyzed and whether it covers the full day or only a portion of it; (ii) the age at which children are treated (i.e., at what age school enrollment is offered or increases), (iii) the baseline children's school enrollment rate, (iv) the estimated effect on school enrollment, (v) the baseline maternal employment rate, and (vi) the estimated effect on maternal employment. The table also reports up to how many years after treatment the effect on maternal employment is measured.

Using this sample of studies, panel A of Figure 10 documents the relationship between baseline maternal employment and baseline children's school enrollment in the literature. The figure shows a mild, negative, non-statistically significant relation between

[28]The remaining papers in the literature are, for the vast majority, papers that employ a difference-in-differences framework at different aggregation levels, such as district or municipality, that do not estimate parameters directly comparable to ours.

[29]The list of studies examined was initially taken from the review done by Halim et al. (2023); this was then complemented with the addition of other studies that fulfill the criteria, found directly by us or not available when the review was done.

these two variables (coefficient=-0.173, standard error=0.237). Relative to the studies that report the necessary information to conduct this benchmarking exercise, our study context (red dot in the figure) stands out for its relatively high baseline enrollment, as roughly 72% of children aged 5 already attend school in the control group, and relatively low maternal employment, given that the employment rate of Black and Coloured mothers in the control group was only 35%. However, neither of these values are outliers in the overall relationship between employment and enrollment, as our study stands close to the fitted line.

In a similar fashion, panel B of figure 10 presents the relationship between treatment effects on children's school enrollment (β Enrollment) and maternal labor supply (β Employment) for the same sample of studies. The relationship is positive and statistically significant (coefficient=0.187, standard error=0.045). As mentioned in Section 4, in our view, an accurate characterization is that mothers would jointly decide whether to send the kid to school, when childcare is available but not mandatory, and enter the labor market, thus explaining the positive relationship. The estimated relationship indicates that, in the literature, treatment effects on children's school enrollment are on average five times larger than treatment effects on maternal employment. In other words, for every five additional children sent to childcare or school, one additional mother works. We note again that our study, represented by the red dot, lies very close to the fitted line. This suggests that the relationship between the treatment effects on enrollment and maternal employment is in line with other studies: in our context, the lack of effect on maternal employment matches the small effect on children's school enrollment.[30]

6.2 Meta-analysis

Motivated by these observations, we conduct a meta-analysis to explore what drives differences in treatment effects across studies, presented in Table 3. The dependent variables are the estimated treatment effects on school enrollment (β Enrollment, panel A) and maternal employment (β Employment, panel B). The explanatory variables are the baseline rates of maternal employment and children's school enrollment, a continuous variable for the minimum age of children targeted by the intervention, a binary variable capturing whether the

[30] Additionally, while all the 14 studies entering the analysis report a positive and statistically significant effect on children's school enrollment, only eight studies report a statistically significant positive effect on maternal employment *at the extensive margin*. Most studies, however, find a positive effect on the quality margin, in terms of either wages, formal employment or hours, as highlighted by Halim et al. (2023). Our study, alongside Bjorvatn et al. (2025), stands out for its lack of effects at both the extensive and quality margins.

program covered part or the full day, and one for whether enrollment was free or had a cost. We also add other study characteristics such as the methodology of the study, through a binary variable on whether the study follows an RCT or RDD methodology, which *de facto* also captures whether the study examined a nation-wide policy (RDD) or a local intervention (RCT). Lastly, we include a binary variable for whether treatment effects were estimated within the year after treatment or later.

The table shows that treatment effects on children's school enrollment are negatively associated with both the baseline level of maternal employment and, more strongly, with the baseline level of children's school enrollment. This latter variable is statistically significant across all specifications and increases in magnitude in richer specifications. As a back-of-the-envelope calculation, we use the results of the most complete specification – while excluding our study – to predict what we should expect in our own context, given the baseline employment and enrollment rate. We obtain an estimate of 7.7 percentage points, which is very close to the estimated treatment effect (6.8 pp., see Appendix Table B2). This means we obtain a result on school enrollment very much in line with the one that would be predicted by previous studies given our contextual factors. We also note that other explanatory variables included in the regression do not play a meaningful role in explaining differences in treatment effects. The only possible exception is represented by the variable measuring the time covered by education, with interventions offering full-time enrollment being associated with larger effects on school attendance.

The results of the meta-analysis for the treatment effects on maternal employment similarly show a negative association between the outcome of interest and baseline rates of maternal employment and children's enrollment. The main difference is that coefficient estimates are now smaller in magnitude and generally not statistically significant. This is consistent with the relationship between treatment effects on school enrollment and employment (Panel B, Figure 10), which shows β Enrollment is on average five times larger than β Employment. With this in mind, the estimated treatment effect on school enrollment in our study (6.8 pp) is again consistent with zero employment effects on mothers. We also note that all other explanatory variables included in the meta-analytical regression are not significantly associated with the treatment effects on mothers' employment.

Our conclusions from the evidence gathered from the literature are the following: (i) the zero effect on mothers' employment at the extensive margin is coherent with what the literature would suggest given the simultaneous, relatively small effect on children's school

Table 3: Meta-Analysis of selected literature

Panel A: Treatment effects on children's school enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline female employment	-0.392		-0.532*	-0.641**	-0.637**	-0.622**	-0.402	-0.454
	(0.315)		(0.267)	(0.257)	(0.234)	(0.211)	(0.222)	(0.248)
Baseline children's enrollment		-0.476*	-0.568**	-0.628**	-0.524**	-0.665**	-0.694***	-0.681**
		(0.245)	(0.224)	(0.212)	(0.201)	(0.199)	(0.176)	(0.186)
National policy (0/1)				-0.167	-0.102	-0.093	0.018	0.010
				(0.102)	(0.100)	(0.090)	(0.101)	(0.106)
Age of eligible group					-0.043	-0.015	-0.012	-0.009
					(0.024)	(0.027)	(0.024)	(0.026)
Full time school (0/1)						0.183	0.225*	0.228*
						(0.105)	(0.095)	(0.100)
Free school (0/1)							-0.255	-0.234
							(0.140)	(0.151)
Short-term evaluation (0/1)								-0.051
								(0.085)
Observations	14	14	14	14	14	14	14	14
R-squared	0.115	0.240	0.442	0.560	0.672	0.762	0.839	0.848

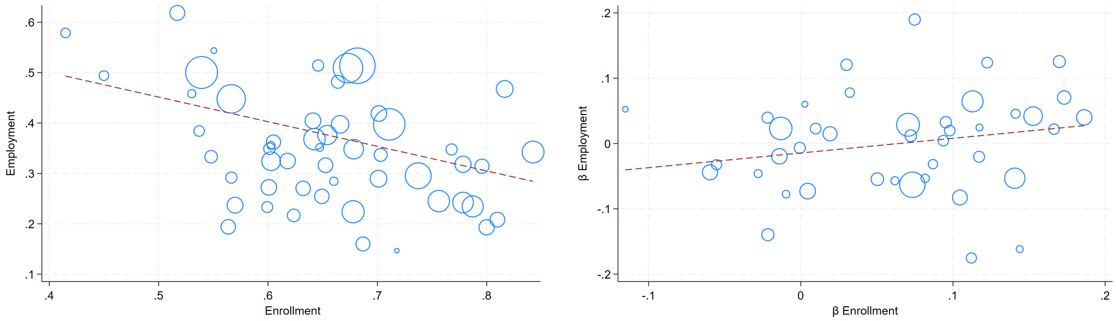
Panel B: Treatment effects on maternal employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline female employment	-0.136*		-0.156**	-0.158*	-0.158*	-0.156*	-0.118	-0.139
	(0.072)		(0.070)	(0.076)	(0.074)	(0.077)	(0.094)	(0.106)
Baseline children's enrollment		-0.058	-0.085	-0.086	-0.062	-0.078	-0.083	-0.078
		(0.066)	(0.059)	(0.063)	(0.063)	(0.072)	(0.075)	(0.079)
National policy (0/1)				-0.003	0.012	0.013	0.032	0.029
				(0.030)	(0.031)	(0.033)	(0.043)	(0.045)
Age of eligible group					-0.010	-0.007	-0.006	-0.005
					(0.008)	(0.010)	(0.010)	(0.011)
Full time school (0/1)						0.021	0.029	0.030
						(0.038)	(0.040)	(0.043)
Free school (0/1)							-0.043	-0.035
							(0.059)	(0.064)
Short-term evaluation (0/1)								-0.020
								(0.036)
Observations	14	14	14	14	14	14	14	14
R-squared	0.229	0.059	0.352	0.352	0.454	0.475	0.512	0.537

Notes: The table reports the coefficient estimates and standard errors for a series of meta-analytical regressions. The dependent variables are the estimated treatment effects on children's school enrollment (Panel A) and maternal employment (Panel B). The studies and parameters included in the analysis are those presented in Appendix Table B1. *<10%, **<5%, ***<1%.

enrollment; (ii) the literature suggests that this is the result of high baseline enrollment levels, which caps the potential impact of the reform on both enrollment and employment.

Figure 11: Evidence from South African districts



(a) Relationship between baseline maternal employment and baseline children's school enrollment

(b) Relationship between treatment effects on children's school attendance and maternal labor supply

Notes: Panel A reports the mean rates of maternal employment and children's school enrollment at baseline, while Panel B plots the estimated treatment effects on children's school enrollment and maternal employment for each district in the sample. The size of the circle corresponds to the population size in the respective district. For graphical reasons, Panel B excludes districts with estimated effects on either employment or enrollment above 0.2 in absolute value. The rest of the analysis always considers the full sample of districts. Appendix Figure A13 presents the full version of the graph in Panel B.

6.3 Evidence from South African Districts

To further corroborate these conclusions, we turn our attention to heterogeneity within the context of our study. Specifically, we re-estimate the relationship between (the effects on) enrollment and employment, *at the district level.*[31] The purpose is to test whether the conclusions gathered from the cross-country evidence replicate in South Africa *across different initial conditions, but holding the policy shock constant.* Similarly to the *across-country* relation estimated from the literature, panel A of Figure 11 shows that, *across districts*, we observe a negative relationship between baseline children's school enrollment and baseline maternal employment. While the baseline enrollment (employment) rates are higher (lower) in South Africa compared to values observed in the literature, the negative relationship holds and is larger in magnitude (coefficient=-0.489, standard error=0.164). Panel B of Figure 11 shows that, across South African districts, there is a positive relationship between the treatment effects on children's school enrollment and the treatment effects on maternal employment. In this case, the relationship between the two treatment effects is very much in line with the one estimated in the literature (coefficient=0.225, standard error=0.104).

This descriptive evidence confirms that the cross-country associations found in the

[31] South Africa has 52 districts. One district is excluded from the estimation because it does not have sufficient observations to run the specification.

Table 4: District-Level analysis

	Panel A: β Enrollment			
	(1)	(2)	(3)	(4)
Baseline female employment	-0.111		-0.224	-0.392*
	(0.127)		(0.136)	(0.200)
Baseline children's enrollment		-0.222	-0.340*	-0.568***
		(0.159)	(0.172)	(0.205)
Observations	51	51	51	51
R-squared	0.015	0.038	0.089	0.367
District-level controls	No	No	No	Yes
	Panel B: β Employment			
	(1)	(2)	(3)	(4)
Baseline female employment	0.063		-0.036	-0.129
	(0.096)		(0.101)	(0.172)
Baseline children's enrollment		-0.284**	-0.303**	-0.237
		(0.116)	(0.129)	(0.173)
Observations	51	51	51	51
R-squared	0.009	0.109	0.111	0.209
District-level controls	No	No	No	Yes

Notes: The table reports the coefficient estimates and standard errors for a set of regressions run at the district level. The dependent variables are the estimated treatment effects on children's school enrollment (Panel A) and maternal employment (Panel B). The regressions are variance weighted (each district is weighted by the inverse of the variance), in order to account for the uncertainty of each estimate at the district level. The table reports the results only for the baseline levels of maternal employment and children's school enrollment. District-level controls include the average age of affected mothers in the district, the average number of years of education attained, the share of Coloured individuals, as well as the average number of places available in ECD in 2011. *<10%, **<5%, ***<1%.

literature resonates in the sample of South African districts: maternal employment and children's school enrollment are negatively related, and the effects of the reform on employment and enrollment are positively related. For this reason, we replicate the meta-analysis presented above using district-level estimates – rather than the study-level estimates – as the unit of observation. Specifically, panel A of Table 4 presents the results of a set of regressions where the dependent variable is the estimated treatment effect on school enrollment at the district level (β Enrollment), while panel B presents the results when the dependent variable is the estimated treatment effect on maternal employment at the district level (β Employment). These district-level regressions are variance-weighted, in order to take into account the statistical uncertainty regarding how the estimate in each district was estimated.[32] In

[32]The coefficient for each district is weighted by the inverse of the squared standard errors. In this way, larger districts with more precise estimates get more weight in the cross-district regressions than smaller districts with fewer observations.

column (4), we include a set of district-level controls to test the robustness of the associations.

Overall, the findings are strongly consistent with the results of the meta-analysis. Specifically, treatment effects on children's school enrollment are negatively associated with both the baseline rate of maternal employment and, more strongly, with the baseline rate of children's school enrollment. Similarly, the treatment effects on maternal employment are negatively associated with the baseline rates of maternal employment and children's school enrollment. As in the literature, the associations are weaker when the outcome of interest is the treatment effect on maternal employment, rather than school enrollment. Results do not substantially differ when we also include district-level controls.

7 Conclusions

This paper has presented evidence from South Africa on the effects of lowering school admission age on children's school enrollment and maternal labor supply. We add to a large literature on the topic by examining a national reform increasing school eligibility at early ages in a Sub-Saharan context. Relative to the existing literature, which focused on smaller scale or local interventions that are generally implemented by private or non-governmental organizations, we are able to provide estimates from a government policy that is already *at scale*. Moreover, relative to other studies that have looked at national policies increasing access to childcare or education outside of Sub-Saharan Africa, we can exploit variation from a reform in age eligibility to obtain precise estimates of the impacts on both children's enrollment and maternal employment. As such, we argue that our study approximates the internal validity of a narrow, local intervention but within the context of a large, public reform. The results of this paper have shown that the reform significantly increased school enrollment (+7pp.), but this did not translate into higher or better employment for mothers.

In order to better place these results within the existing literature, we conduct a thorough literature review and meta-analysis of comparable studies. These results show that what we find is in line with similar findings from the literature, considering the contextual factors of South Africa. In particular, the meta-analysis shows that, in developing or emerging economies, high baseline enrollment is strongly related to smaller effects of interventions expanding childcare or school availability at young ages. We corroborate this conclusion by exploiting heterogeneity across South African districts, which were all similarly affected by the national reform but with different baseline conditions. These results confirm that districts with lower baseline enrollment experienced higher effects on both children's enrollment and employment as a result of the reform.

Overall, we believe there are two main conclusions and policy implications to gather from this paper. First, an important conclusion is that, if greater childcare or school eligibility aims, among others, at increasing maternal employment, targeting based on areas or groups with lower baseline school enrollment is likely to be an effective dimension. Across several studies as well as across districts in our study, we find that the baseline school enrollment rate is the best predictor of larger effects on school enrollment and maternal employment. This might reflect the fact that, when school enrollment is already high, mothers who have not yet decided to send their kids to school and enter the labor market are probably those with the lower probability of doing so, even when these opportunities are offered. This, in turn, could also explain why our findings differ from most of the literature. Specifically, we study a nation-wide policy, rather than a local intervention targeting selected areas where school enrollment might have been lower to begin with (thus motivating the intervention).

Second, this study supports the view that female employment in South Africa might be constrained by other structural constraints – such as important spatial inequalities, which complicate both job search and commute to and from work due to high commuting times and costs (Kerr, 2017) – which are unlikely to be overcome by (only) increasing the provision of childcare. This might suggest that it is important to complement childcare provision with other policies to increase maternal employment. These can include interventions that relieve credit constraints to ease job search, such as cash transfers (Tondini, 2022; Ardington et al., 2009) or transport subsidies (Banerjee and Sequeira, 2023); other interventions such as promoting job search through plan-making prompts (Abel et al., 2019), or reducing information frictions through reference letters (Abel et al., 2020).

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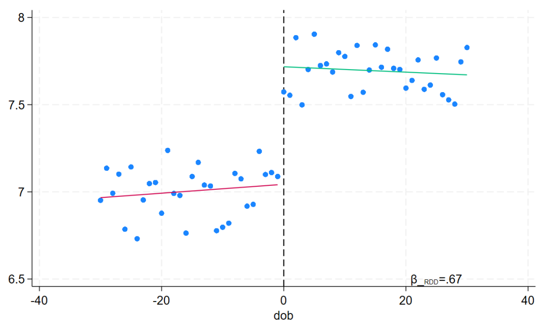
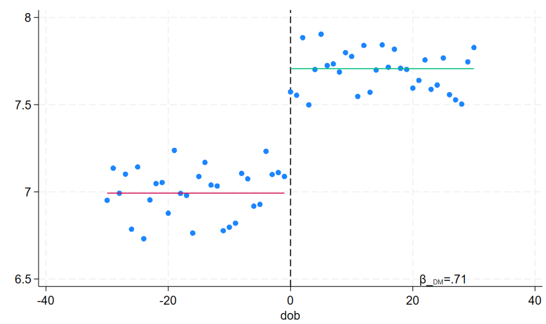
Appendices

A Appendix: Additional figures

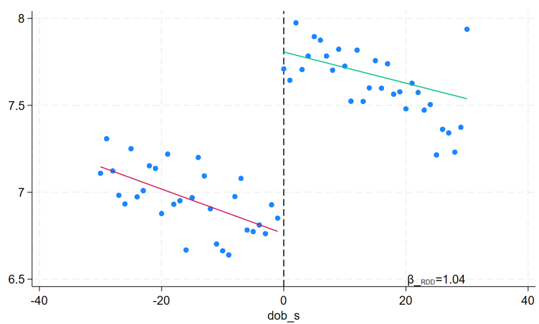
Figure A1: Methods' comparison



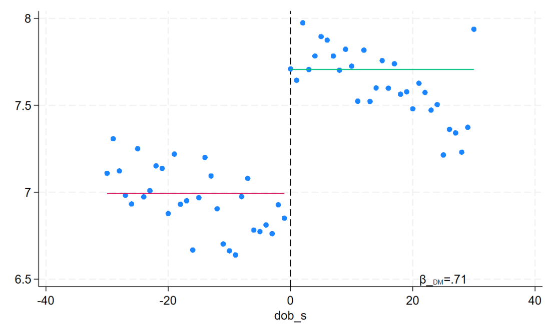
(a) RDD - No sorting



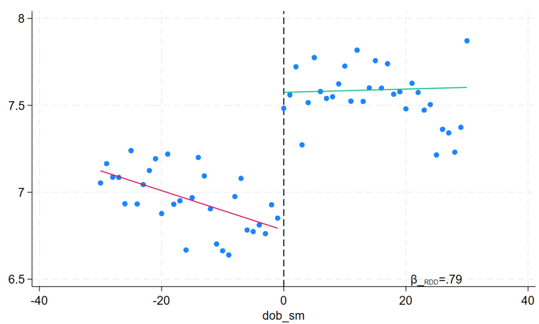
(b) Difference of means - no sorting



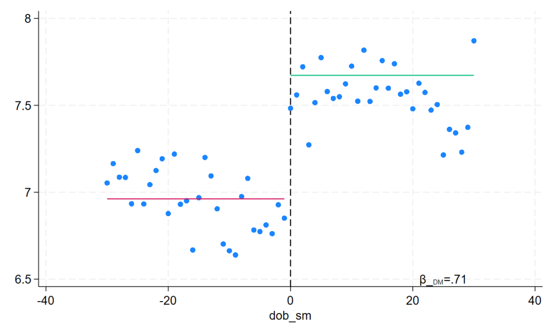
(c) RDD - Non-random sorting to beginning of the same month



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



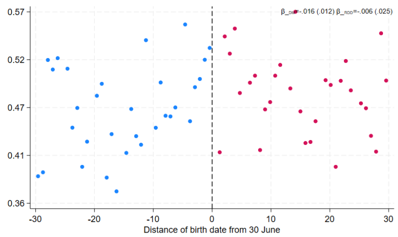
(e) RDD - Non-random sorting to beginning of next month



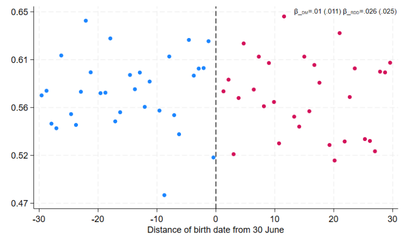
(f) Difference of means - Non-random sorting to beginning of next month

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 RDD estimate with a 30-day bandwidth and a linear fit; the panel to the right show the results of a comparison-of-means estimate with a 30-day bandwidth. The panels also report the estimated coefficient from each model.

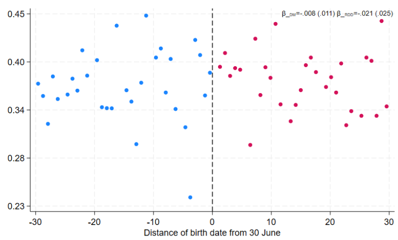
Figure A2: Smoothness of covariates around the June 30th cutoff



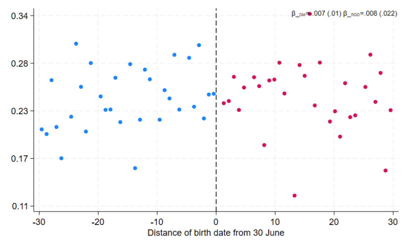
(a) Married



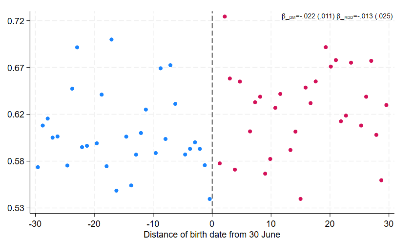
(b) Grade 11 or less



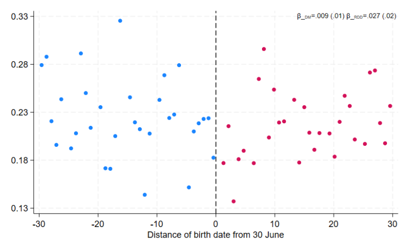
(c) One child



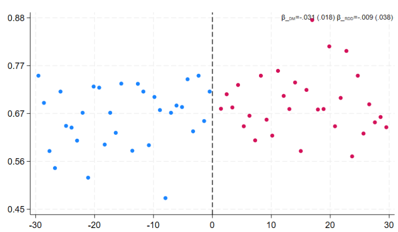
(d) Internal migrant



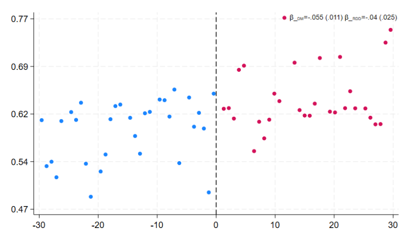
(e) Urban



(f) Grand parent in HH



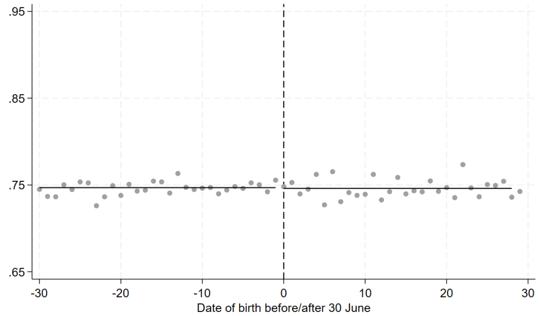
(g) Spouse is employed



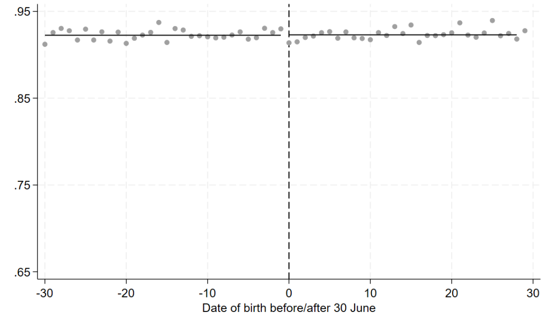
(h) Matched mother and child

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. The figures also report the coefficient estimates and standard errors obtained when running a regression where the outcome variable is the relevant individual or household characteristic, and using either the comparison of means approach (β_{dm}) or the RDD (β_{rdd}).

Figure A3: Balance of children's school enrollment probability based on covariates



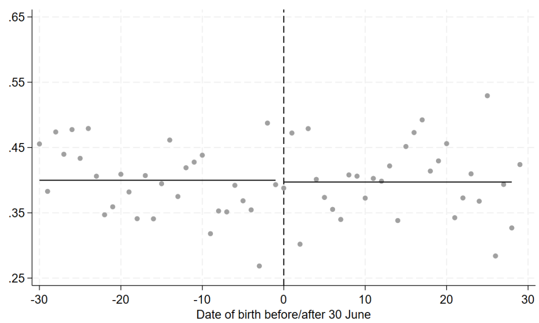
(a) Age 5



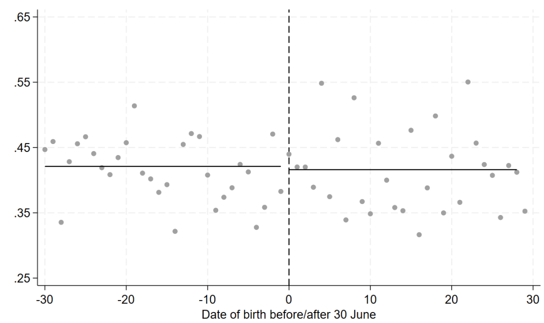
(b) Age 6

Notes: The graph shows the predicted probability to be enrolled into school based on observable households' and mothers' characteristics for children aged 5 or 6. The list of covariates includes: mother is married, mother has low educational attainments, one child in the household, mother has 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.

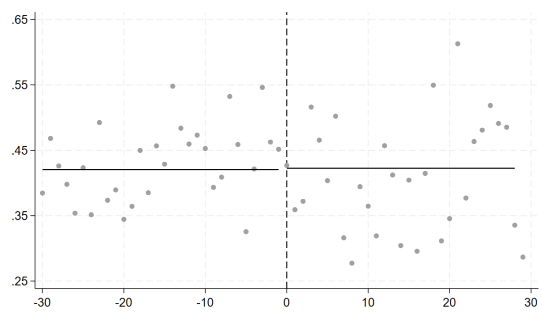
Figure A4: Balance of mothers' predicted employment probability based on covariates



(a) Age 4



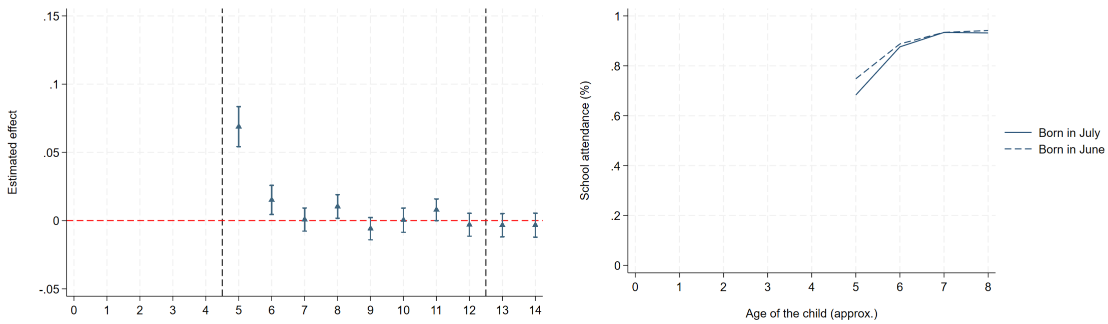
(b) Age 5



(c) Age 6

Notes: The graph shows the predicted probability to be employed based on observable households' and mothers' characteristics for mothers whose youngest child is 4, 5 or 6. The list of covariates includes: mother is married, mother has low educational attainments, one child in the household, mother has 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.

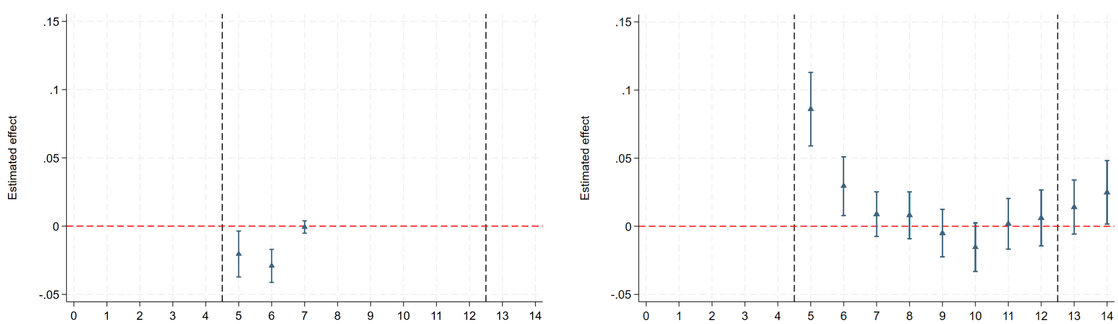
Figure A5: Effects on child's school enrollment on overall children sample



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

Notes: Panel A presents point estimates and 95% confidence intervals for β_{DM} of Equation (1) using 2011 Census data. Standard errors are 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 of children in 2011 (see main text for details).

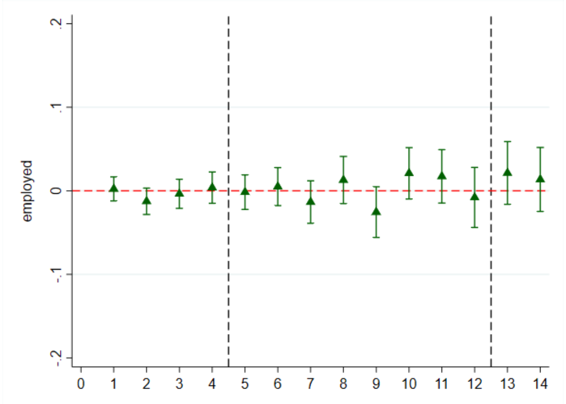
Figure A6: Effects on pre-school and primary school attendance



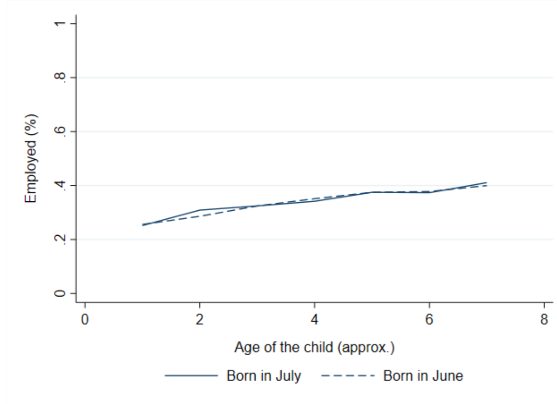
(a) Effects on pre-school attendance   **(b)** Effects on primary school attendance

Notes: The figure presents point estimates and 95% confidence intervals for β_{DM} of Equation (1) using 2011 Census data. Standard errors are 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.

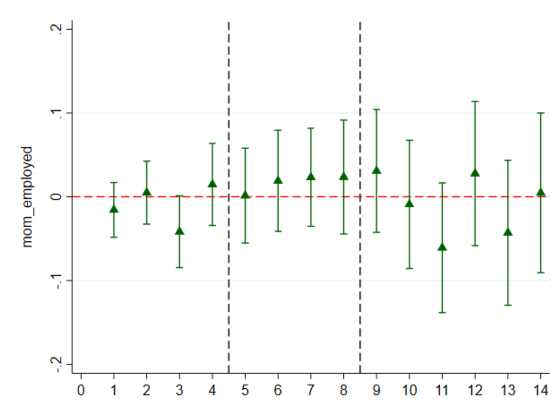
Figure A7: Effects on maternal employment, overall sample



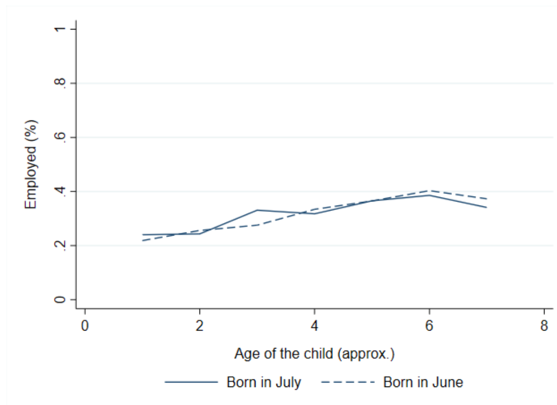
(a) 2011, estimated effects



(b) 2011, mothers' employment rates



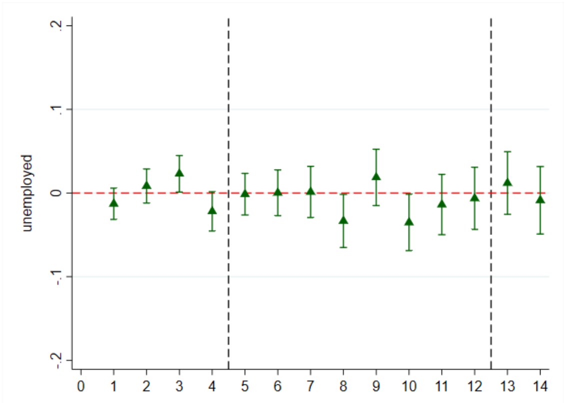
(c) 2007, estimated effects



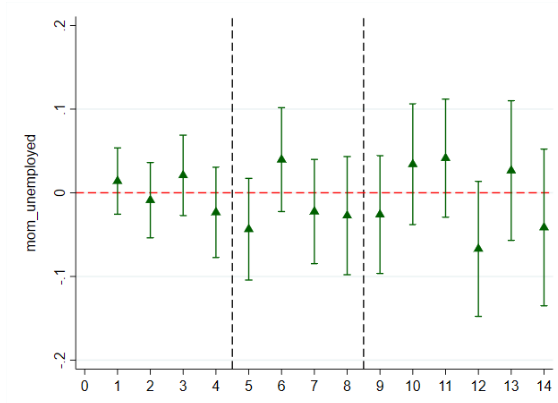
(d) 2007, mothers' employment rates

Notes: Panel A presents point estimates and 95% confidence intervals for β_{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 A8: Effects on maternal unemployment, matched sample



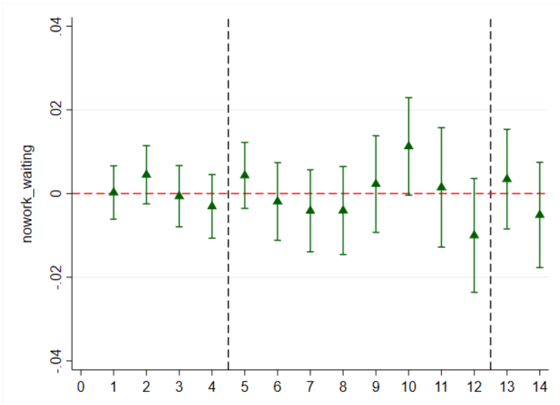
(a) 2011 Census



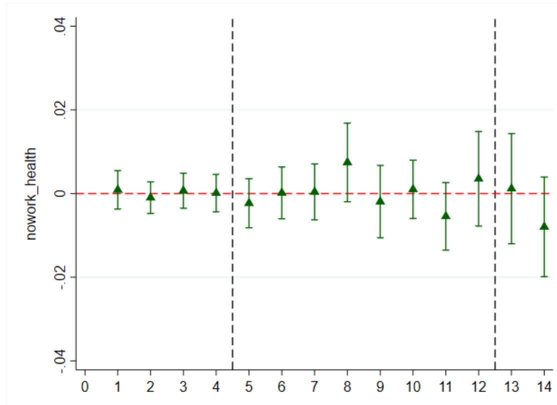
(b) 2007 CS

Notes: The figure presents point estimates and 95% confidence intervals for β_{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).

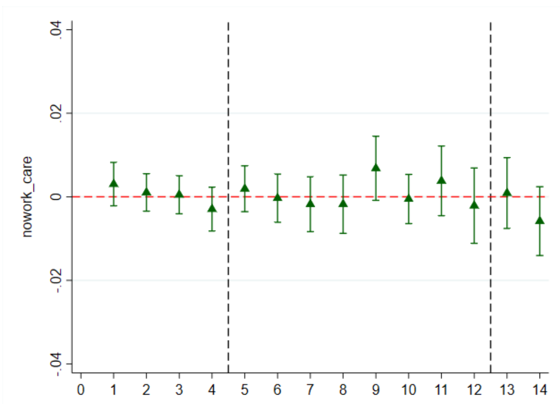
Figure A9: Effects on reasons for not working, 2011 matched sample



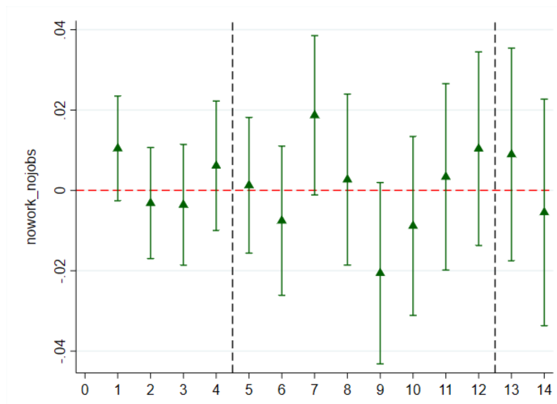
(a) Waiting for season or to be recalled



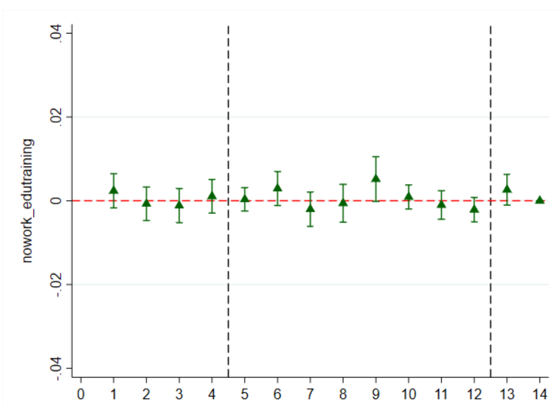
(b) Health, pregnant or unable



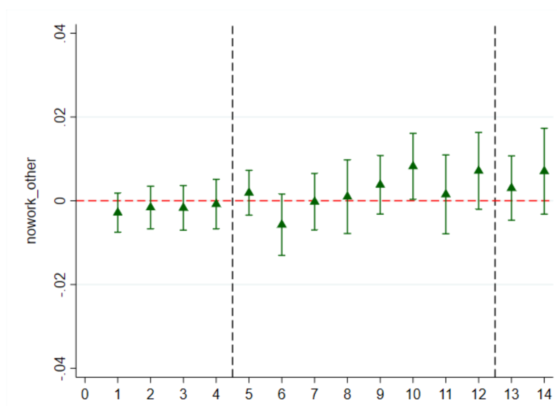
(c) Housewife/homemaker



(d) No jobs available



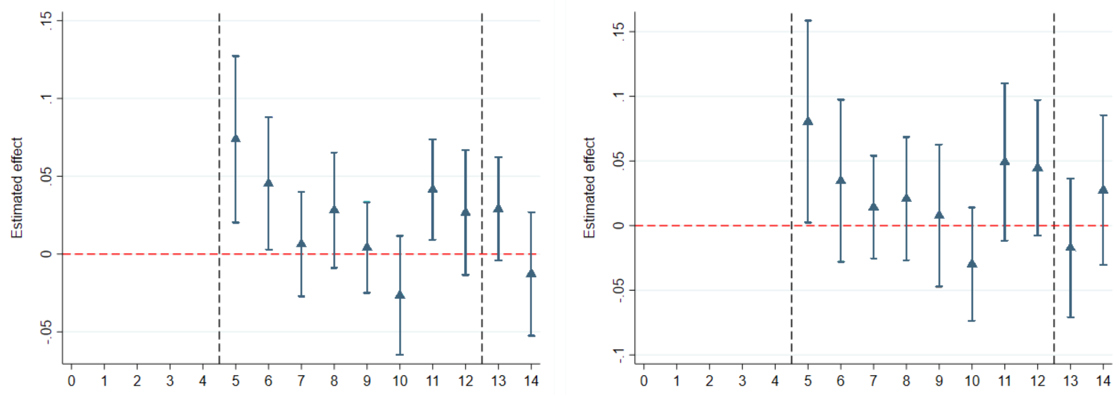
(e) Education or training



(f) Transport and other reasons

Notes: The figure presents point estimates and 95% confidence intervals for β_{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 A10: RDD results on school enrollment, 2011 Census (overall sample)

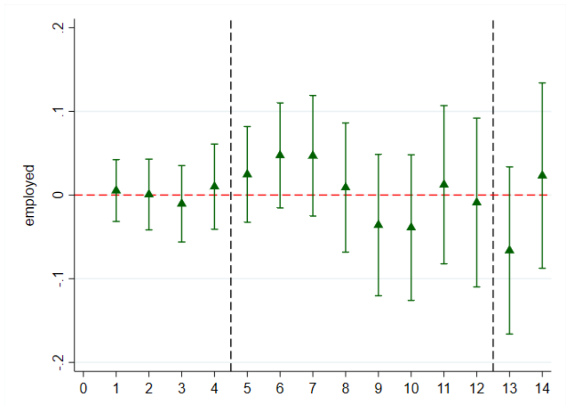


(a) All observations

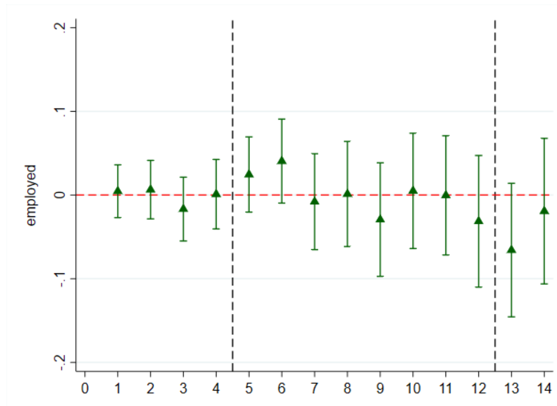
(b) 5 days donut

Notes: The figure presents point estimates and 95% confidence intervals for β_{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).

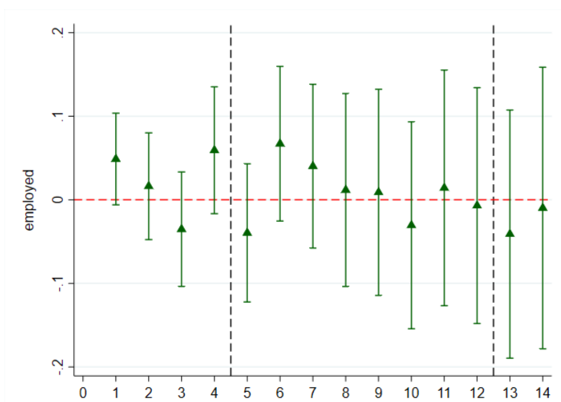
Figure A11: RDD results on maternal employment, 2011 Census



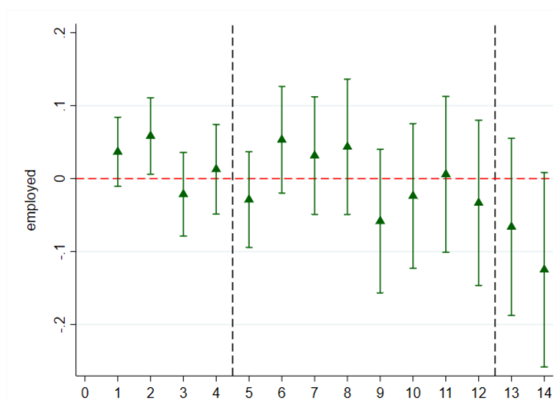
(a) Matched, all observations



(b) Overall, all observations



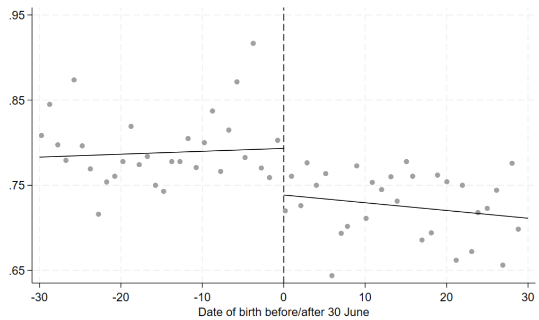
(c) Matched, 5 days donut



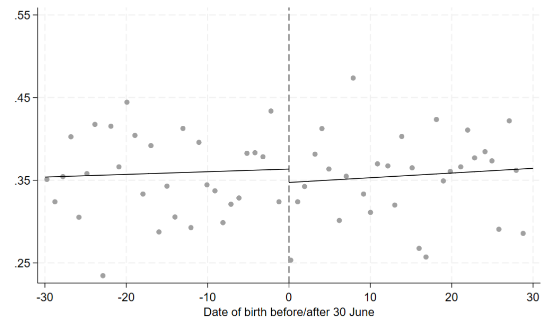
(d) Overall, 5 days donut

Notes: The figure presents point estimates and 95% confidence intervals for β_{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.

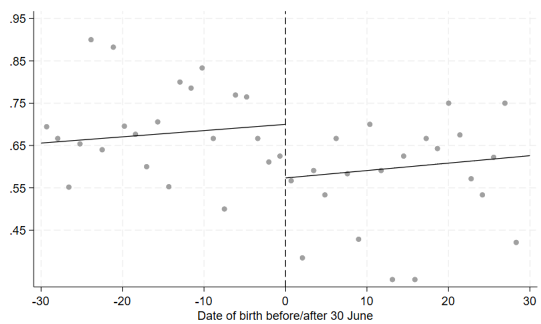
Figure A12: RDD results



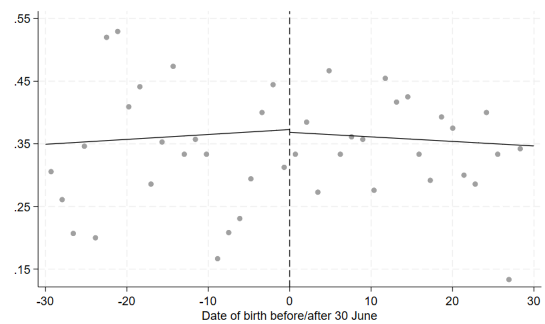
(a) Children's school enrollment, 2011 Census



(b) Maternal employment, 2011 Census



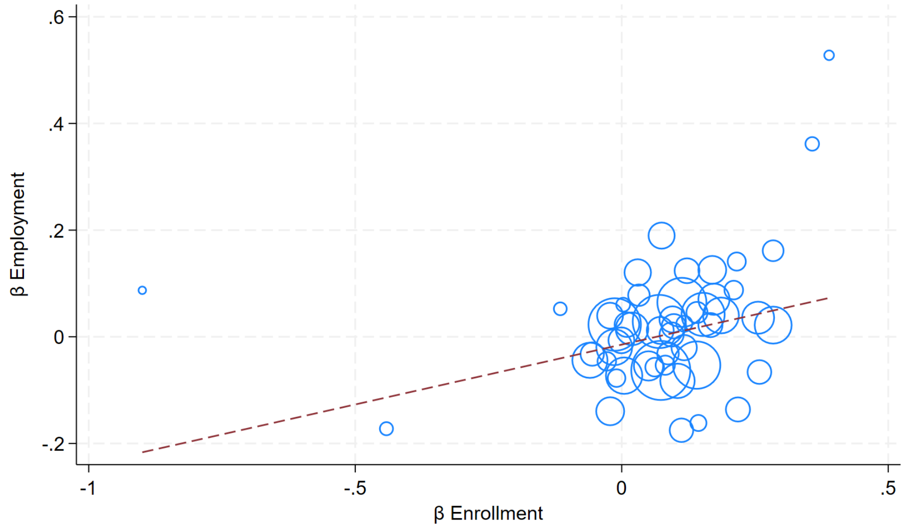
(c) Children's school enrollment, CS 2007



(d) Maternal employment, CS 2007

Notes: The figure reports the RDD results when the outcome of interest is children's school enrollment (Panels A and C) or maternal employment (Panels B and D). The analysis is conducted using data from the 2011 Census (Panels A and B) and the 2007 Community Survey (Panels C and D). For both school attendance and maternal employment, the analysis is conducted when children are of age 5 and a bandwidth of 30 days on each side of the cutoff is selected. The analysis on maternal employment is conducted only on the matched sample. The regression does not include any controls.

Figure A13: Relationship between treatment effects on children's school attendance and maternal labor supply across South African districts, full sample of districts



Notes: The figure plots the estimated treatment effects on children's school enrollment and maternal employment for each district in the sample. The size of the circle corresponds to the population size in the respective district. Compared to the figure presented in the main text (Figure 11), this one does not restrict the sample to districts with estimated effects on either employment or enrollment below 0.2 in absolute value.

B Appendix: Additional tables

Table B1: Summary of the Literature on Childcare and Mother's Employment

Paper	Country&Year	Policy	Age	Enrollment	βEnrolled	Emp.	βEmp.	Long Term
RCTs								
Ajayi et al. (2022)	Burkina Faso, 2019	Free childcare 8am to 2pm $\Longrightarrow$ Full day	0-6	0.12	+24pp.*	≈0.98	+ 0pp. + wage emp.	1.2y
Attanasio et al. (2022)	Brazil, 2008	Free childcare Full day	0-3	See Barros et al. (2013) +0.64 y. wrt to 1.9y. in control group		0pp. mothers t+4,+7; ++ other in hh.		4-7y.
Barros et al. (2013)	Brazil, 2008	Free childcare Full day	0-3	0.51	+43pp.*	0.41	+4.2pp*	<1y.
Bjorvatn et al. (2025)	Uganda, 2018	Childcare subsidy Full day	3-5	0.82 Full day: 0.34	+15pp.* (+48pp.)	0.47	+1pp. ++ single mothers	<1y.
Clark et al. (2019)	Kenya, 2015	Childcare voucher Full day	1-3	0.58	+25pp*	0.49	+8.5pp*	1y.
Donald et al. (2023)	Congo, 2019	Fee-based childcare Half day (6h)	2-6	≈ 0	+73pp.*	N/A	$++^{*\dagger}$	≈ 1.6 y.
Gantungalag et al. (2019)	Mongolia, 2017	Free childcare Full day	2	0.42	+49pp.*	0.57	+4.6pp*	<1y.
Hojman and Boo (2022)	Nicaragua, 2013	Free childcare Half day	0-4	0.23	+46pp.*	≈0.42pp.	+6pp.*	≈ 2y.
Martinez et al. (2017)	Mozambique, 2008	Free childcare Half day	3-5	0.08	+33pp.*	$(0.24)^{\ddagger}$	$(\approx +3.7\text{pp}^{*})^{\ddagger}$	2y.
Martínez and Perticará (2017)	Chile, 2012	Afterschool care Full day	6-13	0.24	+32pp*	0.72	+3.4pp*	≈1y.
Nandi et al. (2020)	India, 2015	Free childcare Full day	1-6	0.089	+43pp.*	0.62	-0.4pp +paid work	≈ 2y

* indicates statistical significance at conventional levels.

† It is not possible to estimate or reconstruct employment levels and the effect on the employment rate from the paper, but both the effect on both the intensive and quality margins are positive.

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

Summary of the Literature on Childcare and Mother's Employment (continued)

Paper	Country&Year	Policy	Age	Enrollment	βEnrolled	Emp.	βEmp.	Long Term
RDDs								
Berlinski et al. (2011)	Argentina, 1995	Entry age Half day	4	0.33	+30pp.*	0.37	+3.8 pp.	<1y.
Dang et al. (2022)	Vietnam, 2010	Entry age Full day	1-5	≈0.42	+8.7pp.*	≈ 0.94	≈ -0.7pp. + wage emp.	2y.
Pritadrajati (2026)	Indonesia,	Entry age Half day	4	≈ 0.07	+13pp.*	≈ 0.5	+1.7pp.*	<1y.
Rosero and Oosterbeek (2011)	Ecuador, 2006	Free childcare Full day	0-6	≈ 0.01	≈ +97pp.*	≈ 0.3	≈+21pp.*	≈ 2y.
Ryu (2020)	Brazil, 2011	Entry age Half day	4	0.37	+8.1pp.*	≈ 0.5	≈ +2.1pp + formal emp.	<1y.

* indicates statistical significance at conventional levels.

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

Table B2: Treatment effects on overall school attendance, 2007 and 2011, matched sample

	Panel A: 2011, overall attendance, matched sample													
Age	1	2	3	4	5	6	7	8	9	10	11	12	13	14
$\hat{\beta_{DM}}$					0.068***	0.002	0.009	0.008	-0.004	-0.014*	0.008	-0.003	0.012	0.012
					(0.012)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)
N					4726	3849	3133	2598	2255	2183	2028	1683	1597	1458
	Panel B: 2007, overall attendance, matched sample													
Age	1	2	3	4	5	6	7	8	9	10	11	12	13	14
$\hat{\beta_{DM}}$	0.004	0.030**	-0.020	0.030	0.074**	0.050**	-0.029**	0.004	0.007	-0.001	0.008	0.019	0.011	0.020
	(0.006)	(0.013)	(0.022)	(0.028)	(0.032)	(0.020)	(0.013)	(0.015)	(0.016)	(0.017)	(0.016)	(0.014)	(0.012)	(0.022)
N	2374	1882	1582	1316	986	933	897	738	643	613	600	479	456	389

Notes: The table reports coefficient estimates and standard errors for the parameter β_{DM} in Equation (1). The results are reported for overall school attendance, using only the *matched* sample (see the main text for details). Results are presented separately using the 2011 Census (panel A) and the 2007 Community Survey (panel B). *<10%, **<5%, ***<1%.

Table B3: School enrollment rates for 6 years old children, by households' and mothers' 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 B4: Treatment effects on maternal employment, 2007 and 2011, matched sample

	Panel A: 2011, maternal employment, matched sample													
Age	1	2	3	4	5	6	7	8	9	10	11	12	13	14
$\hat{\beta}$	0.002	-0.013	-0.004	0.019	-0.000	-0.001	-0.015	0.016	-0.018	0.029	0.027	-0.021	0.004	0.021
	(0.009)	(0.010)	(0.011)	(0.012)	(0.013)	(0.015)	(0.016)	(0.018)	(0.020)	(0.020)	(0.021)	(0.023)	(0.024)	(0.025)
N	8845	7574	6483	5461	4726	3849	3133	2598	2255	2183	2028	1683	1597	1458
	Panel B: 2007, maternal employment, matched sample													
Age	1	2	3	4	5	6	7	8	9	10	11	12	13	14
$\hat{\beta}$	-0.018	0.012	-0.050**	0.013	0.001	0.014	0.032	0.033	0.032	-0.006	-0.070*	0.069	-0.029	-0.010
	(0.018)	(0.020)	(0.023)	(0.027)	(0.030)	(0.033)	(0.033)	(0.037)	(0.040)	(0.042)	(0.042)	(0.047)	(0.047)	(0.052)
N	2341	1876	1565	1309	981	927	895	735	638	608	591	474	453	386

Notes: The table reports coefficient estimates and standard errors for the parameter β_{DM} in Equation (1). The results are reported for maternal employment, using only the *matched* sample (see the main text for details). Results are presented separately using the 2011 Census (panel A) and the 2007 Community Survey (panel B). *<10%, **<5%, ***<1%.

Table B5: Maternal employment rates for 6 years old children, by households' and mothers' 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.