Public Pre-Primary and Maternal Employment in Algeria:

Evidence from a Natural Experiment

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Abstract

Globally, employment rates of women remain substantially below those of men. Since women disproportionately care for children, policies that offer care alternatives or lower the cost of care, should, theoretically, increase women's employment. This paper tests whether public pre-primary education can increase women's employment, using a natural experiment in Algeria. Education reforms in Algeria substantially expanded public pre-primary, targeting children aged five. The paper uses data from 2006 (early in the expansion), 2012, and 2018 (after pre-primary had substantially expanded). The analyses use a discontinuity in whether children are eligible for pre-primary, based on their birthdates, to identify the effect of pre-primary on women's employment. Increased pre-primary education did not increase and may have actually decreased women's employment. One potential explanation for these findings is the half-day schedule of pre-primary may be difficult to reconcile with employment. Negative effects are concentrated among women living in nuclear families, who lack alternative caregivers.

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Declaration of interest statement

None.

Data availability

This study uses the 2006, 2012, and 2018 Multiple Indicator Cluster Survey data. The 2006 data were received in correspondence with the Ministry of Health Population and Hospital Reform, 2012 and 2018 data are publicly available from https://mics.unicef.org/surveys. Stata .do files for replication will be made available on the corresponding author's website.

1. Introduction

Globally, female employment rates are substantially below male employment rates. As of 2022, only 44% of women were employed compared to 68% of men (ILO 2022). Women's employment rates are particularly low (below 20%) in the Middle East and North Africa (MENA) region (ILO 2022). Children and particularly the (opportunity) costs of working while raising children are one potential driver of low rates of female employment (Attanasio, Low, and Sánchez-Marcos 2008; Gathmann and Sass 2018). Since women disproportionately care for children, policies that offer care alternatives or lower the cost of care, should, theoretically, increase women's employment. Expanding access to early childhood care and education (ECCE) is one such policy. This paper tests the impact of expanding pre-primary,[1] a form of ECCE, on women's employment in Algeria.

Expanding ECCE access, primarily through greater public provision or lower costs, can increase women's employment, but its impacts are not guaranteed. Moreover, most of the evidence comes from high-income countries. Child care costs fully explain the lower participation of mothers of preschoolers in the U.S. (Connelly 1992). Estimates from the U.S. indicate that child care subsidies increase the employment of (low-income) mothers between 12 and 33 percentage points (Berger and Black 1992; Blau and Tekin 2007; Crawford 2006; Davis et al. 2018). Public provision of pre-primary in high-income countries has more mixed results. Some studies have found no or little effect from expanding pre-primary, or only impacts on specific sub-groups, e.g. single mothers (Cascio 2009; Fitzpatrick 2010, 2012; Havnes and Mogstad 2011). Other studies do find significant effects of pre-primary on women's employment (Baker, Gruber, and Milligan 2008; Gelbach 2002).

In low and middle-income countries (LMICs), the evidence on the impact of ECCE to date is overwhelmingly positive. A review of 22 studies with plausibly causal identification strategies, examining the impact of institutional childcare on women's labor market outcomes, found positive effects in 21 of the 22 studies (Halim, Perova, and Reynolds 2022). However, none of these studies were in settings with low rates of women's employment.[2] The mean baseline/control group rate of maternal labor force participation across studies was 54%. The lowest baseline/control group rate of maternal labor force participation across the studies was 28% (Halim, Perova, and Reynolds 2022).

Can pre-primary increase women's employment in a context where their employment rates are very low? The paper answers this question in Algeria, where the rapid expansion of public pre-primary education facilitates analyses. The analyses use the cut-off age for pre-primary enrollment (age as of December 31 of that school year) to identify the effect of pre-primary on women's employment using a regression discontinuity design (RDD), comparing 2006, 2012, and 2018.

The results demonstrate that the expansion of pre-primary did not increase women's employment. If anything, it actually *decreased* their employment. To the best of the authors' knowledge, this paper is the first to show that pre-primary schooling may actually *reduce* women's employment. This finding is an important contribution and caution for assuming provision of pre-primary has unidirectional and positive effects on women's employment outcomes.

One potential explanation for these findings is the half-day schedule of expanded pre-primary may be difficult to reconcile with employment. The one study of 22 in the review of institutional childcare on women's labor market outcomes (Halim, Perova, and Reynolds 2022) that did not find a positive effect noted that care was only part-day and did not align well with work schedules (Medrano 2009). Studies elsewhere show full-day kindergarten or afterschool care can increase women's employment (Berthelon, Oyarzún, and Kruger 2015; Cannon, Jacknowitz, and Painter 2006; Dhuey, Lamontagne, and Zhang 2019; Martínez A. and Perticará 2017).

These results may also be due to the challenging and low female employment rate context in Algeria. The same study of the 22-paper review that did not find positive effects of childcare was also in the lowest-participation context of all the studies (28%) (Halim, Perova, and Reynolds 2022; Medrano 2009). When reconciling employment and care responsibilities is already especially difficult, or in contexts with weak female labor demand, pre-primary may not be sufficient to change women's employment outcomes. Careful policy design around hours of care may also be particularly important when attempting to increase women's employment in such lower-employment contexts.[3]

The paper is organized as follows. Section 2 provides background on pre-primary and the labor market in Algeria. The paper then describes the survey data in Section 3. Section 4 describes the identification strategy of RDD. Section 5 presents the results and a number of mechanisms, specifications, and robustness checks. Section 6 concludes with a discussion of implications for policy and increasing women's employment globally.

2. Background

Algeria's labor market

A rich literature examines the determinants of women's employment in MENA countries. The determinants can generally be organized into "needs," the economic and care requirements of the household, "values" including gender norms, and "opportunities," whether jobs are accessible and suitable (Spierings, Smits, and Verloo 2010). Empirically, women's participation and employment depends on household composition, especially the presence of young children (Keo, Krafft, and Fedi 2022). However, little is known about what programs or policies might increase women's employment in the MENA region. In particular, there is limited empirical evidence on the effects of child care costs, an important component of women's opportunity costs, for women in the MENA region.

Algeria is one of the many MENA countries with low female employment rates, estimated at 13.8% as of 2019 (Algeria National Statistics Office 2019). This compares to an employment rate of 60.7% for men. Algeria had the fourth-largest (of 146 countries) increase in years of schooling from 1980-2010 (Campante and Chor 2012). Recent cohorts have achieved gender parity in education (Assaad et al. 2020). Despite increases in women's education, their employment has only increased very slightly over time, from 11.4% in 2009 to 13.8% in 2019 (Algeria National Statistics Office 2019). Participation is higher for unmarried than married women (Assaad et al. 2020). This suggests that domestic responsibilities, such as child care, that come with marriage and family formation, are a key constraint on women's employment.

Education system in Algeria

Algeria's official education system begins with pre-primary education. Children then proceed to primary education (starting at age six). There are two types of pre-primary education in Algeria (Mahdjoub 2017). Preparatory education is one year in duration and starts at age five. Preschool education is officially three years and starts at age three but may be two years starting at age four. Both preparatory and preschool education are forms of pre-primary, and this paper refers to them collectively as pre-primary. When the paper describes features related to one type of pre-primary specifically it names that type as, distinctly, preparatory or preschool. Pre-primary education is voluntary (Mahdjoub 2017). Public preparatory classes are physically and administratively attached to primary schools, overseen by the Ministry of Education (Secretary General of the Republic of Algeria 2008). Preschool is implemented by a variety of actors, including other ministries, such as the Ministry of Religious Affairs, and employers (Mahdjoub 2017).

When children can enroll in school is determined by their birthdate. Children can enroll in preparatory starting the calendar year in which they will turn five. For example, a child born between January 1 and December 31, 2016, would enroll in preparatory for the 2021-2022 school year (Chalal 2018; H. 2021). The analyses exploit the discontinuity this policy creates for the RDD identification strategy. Children born between January 1 and March 31 can, in some cases, petition to be accepted a year early (Chalal 2018).

Primary and pre-primary school days are short and may be difficult to reconcile with employment. For instance, starting in 2011, preparatory students and those in the first two years of primary had only 21 hours of school per week. They finished school earlier, at 2:30pm, as well as having a lunch break between 11:15-1:00pm and no class on Tuesday afternoons (Nawel 2011; UNESCO-IBE 2012).

Pre-primary expansion in Algeria

After its independence, in 1962, Algeria did away with public pre-primary education to focus on achieving universal compulsory (primary and lower secondary school) education (Bouzoubaa and Benghabrit-Remaoun 2004). With its 2003 education reform, Algeria planned to expand pre-primary once more starting in the 2004-2005 school year. In January of 2008, the National Education Guideline Law No. 08-04 further emphasized pre-primary education (Mahdjoub 2017).

Algeria massively expanded pre-primary in a period of less than five years (Figure S1.1, in the supplementary online appendix, available with this article at *The World Bank Economic Review* website). The expansion focused on children aged five (the preparatory year) (UNICEF

Algeria 2014). Starting in 1993, gross enrollment in pre-primary (for five-year-olds) was only 1%. As recently as 2005, the gross enrollment rate in pre-primary was only 6%, but starting in 2006, enrollments rapidly increased, to 29% in 2006, 36% by 2008, and 80% by 2009 (then 84% in 2010 and 79% in 2011).[4] Of primary school entrants at the beginning of the 2015 school year, 70% had attended pre-primary (Mahdjoub 2017). Among those who attended pre-primary, 71% had attended a preparatory class, 25% a Ministry of Religious Affairs preschool, and the rest other private or public programs (Mahdjoub 2017). This paper uses data from 2006 (early in the expansion), 2012, and 2018 (after pre-primary had substantially expanded).

3. Data

The identification strategy is a RDD, based on the cut-off age for pre-primary enrollment (age as of December 31 of that school year). This section discusses the three rounds of survey data, overall sample, and analysis sample. The analysis sample focuses on children whose age is within six months above or below the cutoff for those who would be age four or age five on December 31 of that school year. This section also describes the outcome (women's employment) and the measure of pre-primary. The next section provides the exact RDD estimation methods.

Surveys

This paper uses data from the Algeria Multiple Indicator Cluster Surveys (MICS), which were conducted in 2006,[5] 2012/13 (referred to as 2012), and 2018/19 (referred to as 2018) by UNICEF in collaboration with the Algerian government (Ministry of Health Population and Hospital Reform 2015; Ministry of Health Population and Hospital Reform and National Office of Statistics 2008; Ministry of Health Population and Hospital Reform and United Nations Children's Fund (UNICEF) 2020).[6] The MICS is a nationally representative survey, representative of both households and individuals (including subgroups such as women or women with young children).[7] The MICS contains four questionnaires: a household questionnaire, a household member questionnaire, a questionnaire for women aged 15-49, and a questionnaire for children under the age of 5 (addressed to the mother or primary caretaker of the child).

For the survey conducted in 2006, 29,008 households, 43,642 women, and 15,000 children under the age of five were included (Ministry of Health Population and Hospital Reform and National Office of Statistics 2008). Fielding took place from March 2006 to June 2006. For the survey conducted in 2012, 27,198 households, 38,548 women, and 14,701 children under the age of five were included (Ministry of Health Population and Hospital Reform 2015). Fielding took place from October 2012 to March 2013. For the survey conducted in 2018, 29,919 households, 35,111 women, and 14,873 children under the age of five were included (Ministry of Health Population and Hospital Reform and United Nations Children's Fund (UNICEF) 2020). Fielding took place from December 2018 to April 2019. Thus, while some of the surveys span two calendar years, none span multiple school years, facilitating the identification strategy.

Because the identification strategy (described in detail below) focuses on mothers with children near the age cutoffs for pre-primary, the analysis samples are somewhat smaller: 2,499 observations around the age four cutoff and 2,667 around the age five cutoff in 2006; 2,666 around the age four cutoff and 2,678 around the age five cutoff in 2012; and 2,976 around the age four cutoff and 2,996 around the age five cutoff in 2018.

Outcome measures

The paper investigates the impact of pre-primary on women's employment. The survey specifically asks for each household member (aged 15+) their situation in the 30 days preceding the interview, classified as: (1) employed (2) studying/training (3) searching for work (unemployed) (4) retired or (5) other out of labor force. The specific outcome the analyses examine is employment (as a percentage of the population).

Pre-primary measures

There is information on pre-primary from both the under-five (for ages three and four) questionnaire and the household member questionnaires (for ages five and six).[8] Note that these questionnaires and questions were based at the age at the time of the survey. This age at time of survey distinction in the questionnaires does not affect the discontinuity used for identification of age on December 31 of that school year.[9] For ages three and four, their parent or guardian is asked whether the child attends a preschool education program such as a public or private center or kindergarten.[10] A "yes" response to this question is considered attending pre-primary for ages 3-4. For ages five and six, a question on whether or not the child is attending school in the current school year is used (2005-2006 for 2006; 2012-2013 for 2012; 2018-2019 for 2018). If the child is not attending school, he or she is not in pre-primary. If she or he is attending school, if he or she is in the preparatory or preschool level, she or he is classified as attending pre-primary. The pre-primary measure thus includes a mix of preparatory school and preschool types (the data do not allow us to distinguish the type of pre-primary). Some five-year-olds and most six-year-olds are already in primary school, and analyses count them as missing (since they are in primary, they are ineligible for pre-primary) for the pre-primary measure. Moreover, some six-year-olds at the time of the survey might have been five in terms of eligibility, and therefore eligible and are included in the pre-primary measure. Essentially, analyses measure pre-primary for children under age six as of December 31 in the relevant school year, who would have been eligible for pre-primary.

Covariates

Analyses test for differences in covariates as a check on RDD assumptions and also include controls in a sensitivity analysis for the RDD estimation. In terms of controls, analyses include data on the location of the household, both in terms of urban vs. rural residence and region (seven regions are used). Since household composition will affect childcare options and labor force decisions, analyses also control for whether the household is extended compared to nuclear (nuclear includes only the head, spouse, and their children). For women, analyses include information on their education level, age group, and marital status.

4. Methods

Fuzzy regression discontinuity design

As with past studies (e.g. Berlinski, Galiani, and McEwan 2011; Dang, Masako, and Nguyen 2019; Fitzpatrick 2010), this paper uses a fuzzy RDD (FRDD) strategy and exploits policies that set a cutoff date for pre-primary eligibility. The essential idea of RDD is that an individual's 'treatment' status (pre-primary) is determined by some 'assignment variable' (Imbens and Lemieux 2008; Lee and Lemieux 2010). In this case, children are eligible for a particular grade of pre-primary only if they are of an age for that pre-primary grade as of December 31 of that school year[11] (the cutoff) (Chalal 2018; H. 2021). Therefore, weeks from the age cutoff at December 31 of that round's school year is the assignment variable. The data have the full date of birth (day, month, year) for all children to facilitate this identification strategy. If the birth date is December 31 or earlier, this assignment variable is zero or positive, and the child can enter that grade of pre-primary sooner. If the birth date is later, this value is negative, and the child has to wait until the next school year to enter that grade of pre-primary.

Since children can enter pre-primary at various ages, analyses divide the sample into discontinuity groups, based on the age on December 31, for the December 31 date included in that round's school year. Specifically, analyses use six months older or younger than would turn age four on December 31 of that round's school year, and likewise for age five.[12] Analyses thus have two groups, what are referred to as the age four group (3.5-4.5 years old on December 31) and the age five group (4.5-5.5 years old on December 31). Children thus fall into one (and only one) discontinuity group and are near equally distributed on each side of the discontinuity.[13] Children in the age five group whose age was above the cutoff would be eligible to enter the preparatory grade of pre-primary, which substantially expanded over time. Children below the cutoff may, however, still access other types of pre-primary. Children in the age four group whose age was above the cutoff would be able to access preschool grades for age four. This level did not, however, experience much of an expansion, and thus the age four group acts in some ways as a placebo relative to age five.

Polynomial and bandwidth selection

Visually inspecting the relationships between the treatment and assignment variable around the cutoff, as well as the outcome and assignment variable is critically important. Analyses initially undertake a visual inspection and "bin" the data, estimating bin means locally throughout the distribution on each side of the cutoff, the standard non-parametric approach.[14] This approach allows for visual inspection of the data for relationships, confirm whether there is a discontinuity, see potential functional forms, and check for any violations of key assumptions.

Analyses use the typical FRDD estimator of the treatment effect, namely the ratio of the jump in the outcome (employment) to the jump in the probability of treatment (pre-primary) (Imbens and Lemieux 2008). In the first stage, it is critically important for there to be a discontinuity, that is, for being above the cutoff to effectively predict pre-primary.[15] Only if this assumption holds can analyses possibly identify the effect of pre-primary on women's employment.[16]

The main results are based on local non-parametric polynomial kernel estimators estimated on either side of the cutoff. Analyses estimate both conventional, bias-corrected, and bias-corrected robust (robust for short) treatment effects.[17] The bias-correction method uses local linear regression to address leading bias when using kernel regressions (Calonico, Cattaneo, and Titiunik 2014a). This approach is more robust to large bandwidth choices, improving power. The robust standard error accounts for the additional variability generated by the bias (Calonico, Cattaneo, and Titiunik 2014a). Analyses use a rectangular kernel, as recommended (Imbens and Lemieux 2008; Lee and Lemieux 2010). The conventional treatment effect estimates with this local non-parametric approach thus are the same as parametric estimates with an equivalent specification using two-stage least squares (2SLS).[18] Analyses additionally present those 2SLS estimates, with their robust standard errors.

Given the well-known problems with higher order polynomials (Gelman and Imbens 2019) and lacking any reason to expect a higher-order polynomial is applicable in the case at hand, analyses follow the recommendations of Gelman and Imbens (2019) and estimate at most a quadratic, as well as linear and differences in means estimates. Analyses empirically test which model fits best, based on the reduced form model using being above the cutoff rather than treatment itself (Lee and Lemieux 2010).[19] The preferred model is the one with the smallest Akaike information criterion (AIC), the recommended approach to choosing the polynomial's order (Lee and Lemieux 2010). The paper presents sensitivity analyses with the alternative polynomials as well. Analyses prioritize the functional form for the outcome and use the same functional form (polynomial) for the treatment equation as well (Imbens and Lemieux 2008; Lee and Lemieux 2010).

In addition to questions of polynomial selection and estimation, the bandwidth from the cutoff within which results are estimated is an important decision for RDD. Analyses take as their starting point a bandwidth that includes observations within six months above and below the December 31 cutoff for each group (group above/below age 4 cutoff, group above/below age 5 cutoff). The paper tests the sensitivity of the results to smaller bandwidths (e.g. within 3 months above/below the cutoff). In general, bandwidth selection is a tradeoff between precision and bias, where additional curvature in the underlying relationship will mean more bias, and a smaller optimal bandwidth (Lee and Lemieux 2010). As recommended, analyses use the same bandwidth for estimates of both the treatment and outcome (Imbens and Lemieux 2008).

If covariates are balanced and the assignment variable is as-good-as-random, covariates are irrelevant to estimation (Imbens and Lemieux 2008; Lee and Lemieux 2010). Their inclusion can, however, increase precision, so sensitivity analyses include estimations with covariates.

Assumptions and threats to identification

There are a number of assumptions that need to hold for FRDD to identify a causal parameter. If there is a change in outcome (employment), at the treatment threshold, it can be attributed to the treatment (pre-primary) so long as one can expect a smooth (continuous) relationship between the outcome and assignment variable in the absence of treatment. In this case, aside from the ability to enroll children in pre-primary, the effect of a child's birth date moving from a few weeks before December 31 to a few weeks after should be no different than an equivalent shift at any other date.

The FRDD also assumes monotonicity (Imbens and Lemieux 2008; Lee and Lemieux 2010). In this case, that means that crossing the cutoff does not cause some children to take up pre-primary and others to reject pre-primary. This is at least conceptually plausible; it cannot be directly tested, but analyses confirm below there is the expected jump in pre-primary at the cutoff.

One concern with RDD identification strategies is potential for precise control of the assignment variable (Imbens and Lemieux 2008; Lee and Lemieux 2010). Conceptually, intentional manipulation is less likely to be the case with a child's birthdate occurring several years in the past relative to pre-primary than with some other assignment variables (e.g., income is much more manipulable). Parents have some control over when children are born, but much less so the exact date, precluding precise control. Age/birthdate is a very common assignment variable in the literature for this reason (Lee and Lemieux 2010).

Nonetheless, analyses check the distribution of the assignment variable as a credibility check (Imbens and Lemieux 2008; Lee and Lemieux 2010). Analyses also check for balance of other covariates above and below the cutoff, as if the birthdate is as-good-as-random, other covariates should be orthogonal to whether a child is above or below the cutoff (Imbens and Lemieux 2008; Lee and Lemieux 2010).[20] This test is similar to excludability of the cutoff; a child being a few weeks older cannot be related to labor market outcomes except through their pre-primary enrollment.

Two other issues that can threaten identification are anticipatory behavior and general equilibrium effects. If women anticipate that they will soon receive child care, when their children reach the pre-primary threshold, they may change their behaviors, for instance, begin to work or seek work. The jump in the outcome may thus be somewhat attenuated. In terms of general equilibrium effects, if women increase their labor supply in response to pre-primary, this could shift wages and opportunities for women with children below the threshold. For both issues, these effects are arguably second order concerns (anticipatory effects of child care are going to be much smaller than actual child care effects; general equilibrium effects will be small given that a fraction of women have children of this age, some fraction of whom might change their labor market behavior). Second, both these issues will attenuate estimates, making the results conservative.

If these assumptions hold, then the FRDD estimates the treatment effect based on compliers (children who attend pre-primary because their birthdate is older than the cutoff, but who would not have otherwise attended) (Imbens and Lemieux 2008; Lee and Lemieux 2010). The resulting estimate is a local average treatment effect (LATE) (Lee and Lemieux 2010). The results thus do not necessarily generalize to what would happen if there were different cutoffs than currently implemented.

5. Results

This section begins the presentation of results with an investigation of pre-primary enrollment, investigating the potential discontinuity in pre-primary enrollment based on birthdates. Analyses then test two key assumptions of FRDD, checking the distribution of birthdates and balance of covariates around the cutoff. The FRDD results for women's employment are then presented both visually and for a number of specifications and sensitivity analyses. The final results sub-section explores potential mechanisms.

Pre-primary results

Figure 1 presents pre-primary attendance by weeks from the cutoff, age group (+/- six months of being age four or five on December 31), and year. The points are the bin means (six bins on each side of the cutoff, thus roughly months[21]). The bins are used for descriptive visualization, but the full underlying distribution is retained for subsequent analyses. The figure also presents confidence intervals around the bins and a linear fit. Notably, in 2006 (early in the expansion), for age four, there is no discontinuity. There is a visible discontinuity at age five in 2006. In 2012 and 2018, there is evidence of a small/slight discontinuity for age 4. The discontinuity for age 5 in 2012 is modest, but larger in 2018. There is also a clear increase in pre-primary attendance with age (weeks older than Dec. 31), not just at the cutoff. The relationship is roughly linear. In the appendix, Figure S1.2 and Figure S1.3 present intercept (mean only) and quadratic fits; visually, the intercept (mean only) fit is fairly similar or slightly worse than the linear specification. The quadratic fit is fine in some cases, but in others is very ill fitting and does not show a consistent functional form for the quadratic across years and age groups.

Checks of FRDD assumptions

A critical assumption of the FRDD approach is the absence of assignment variable manipulation. Figure 2 presents a histogram of the distribution of children's age in weeks from the cutoff, by age group and year, as a check of this assumption. The distribution appears random relative to the cutoff; there is not, for example, a much higher share of births just a few weeks older than the cutoff. No other point shows unusual or systematic density. Manipulation test procedures based on local density also generally support lack of manipulation.[22] Across the two age groups and three time periods (six tests), age four cutoffs are never significant. The age five cutoff is significant in 2006 using both the robust and conventional estimators, in 2012 with the conventional but not robust estimator, and in 2018 with the robust but not conventional estimator. Although in some cases tests are significant, differences in density are small and also have variable directionality across the samples, corroborating lack of manipulation.

If the assumptions of RDD hold, there will not be any differences in covariates around the cutoff. Table S2.1, in the appendix, tests that assumption, presenting the means of various mother characteristics[23] above and below the threshold for each age group cutoff, their difference, and the t-test. Note that one would expect some significant differences by chance with this many tests. There are few significant differences and not consistent ones; for example in 2012 for the age four cutoff there is a four-percentage point difference (28.4% below; 32.4% above) in middle education, but in other years this difference is insignificant or the sign reversed. F-tests for overall balance are significant, except for age 4 in 2006, but with small F-statistics (1.099-2.456). The results suggest the cutoff is as-good-as-random or nearly so, supporting the identification strategy.

Employment of mothers: FRDD estimates

The presentation of the FRDD results starts with the "reduced form" model (using being above or below the cutoff as the key covariate) to identify the correct functional form, based on the AIC. Analyses test simple intercept (mean only), linear, and quadratic functional forms for the employment outcome. The results for age four are in Table 1 and for age five are in Table 2. First, in terms of fit, the AIC (smallest is best) indicates that an intercept-only functional form (polynomial order zero) tends to be the best fit, although in 2012 quadratic forms fit better. In order to be consistent, analyses use the intercept-only functional form as the main model but present sensitivity analyses with the other models.

The reduced form models allow us to assess the statistical significance of the results as well. In the preferred, intercept-only model, being above the cutoff does not have a statistically significant relationship with employment for the age four group but does for age five. The contrast between age four and age five also bolsters the results, since the expansion targeted age five. In 2006, having a child above the cutoff caused significantly *higher* employment, by around three percentage points (relative to a base employment rate of nine percentage points). In 2012, employment *decreased* a significant 3 percentage points if the child was above the cutoff (qualified for pre-primary). In 2018, employment was also a significant 3 percentage points *lower*. The expansion of pre-primary in Algeria thus appears to have worsened, rather than improved, women's employment outcomes for women with eligible children.

Next are the visualizations of these employment outcomes around the discontinuity. Figure 3 presents the intercept (mean only) FRDD fits for the outcome of employment with binned data (the points are the bin means (six bins on each side of the cutoff, thus roughly months)) above and below the cutoff. The bins are used for descriptive visualization, but the full underlying distribution is retained for subsequent analyses. In the appendix, Figure S1.4 presents a linear fit while Figure S1.5 presents a quadratic fit. The intercept-only functional form appears, visually, to be an appropriate fit. The linear and quadratic fits lack a clear pattern across ages and years in terms of the slopes, slope on each side of the cutoff, and concavity or convexity (for the quadratic form).

The results of the FRDD estimator for this specification are presented in Table 3 (for age four) and Table 4 (for age five). The tables present the first stage estimates (the percentage point jump in pre-primary at the discontinuity), and the treatment effects (the percentage point change in employment for a one percentage point increase in pre-primary, based on the discontinuity). Conventional and bias corrected estimates are presented, as well as robust standard errors for the bias-corrected estimates. There are no significant treatment effect estimates (although the first stage under the conventional estimates is strong) for age four except a negative effect with the bias-corrected estimate in 2006 (but this is not significant with the robust standard error).

For five-year-olds, in 2006, pre-primary has a significant and positive effect on employment with the conventional estimates (does not remain significant in the bias-corrected or robust models). In 2012, pre-primary has a negative effect on employment, which becomes larger and significant in the bias corrected models (remains significant with the robust standard errors).

In 2018, pre-primary has a significant negative effect on employment in the conventional estimators, which increases in the bias corrected estimators, remaining significant (but not with the robust standard errors). In sum, although there are a number of estimates and the significance of results is sensitive to the estimator and the standard error used, the balance of evidence indicates pre-primary's expansion in 2012 and 2018 worsened women's employment outcomes.

Sensitivity analyses

There are a series of further sensitivity analyses for the estimates of the impact of pre-primary on women's employment in the appendix, which are summarized here. In Table S2.2 (age four) and Table S2.3 (age five), analyses halve the bandwidth used, from six months to roughly three months (13 weeks). Results are substantively similar to the main results, although, as expected, standard errors increase and therefore statistical significance does not always persist. In Table S2.4 (age four) and Table S2.5 (age five), analyses use a linear functional form (polynomial degree one). In Table S2.6 (age four) and Table S2.7 (age five), analyses use a quadratic functional form (polynomial degree two). Results for treatment effects are similar, but most of the significant results become insignificant as standard errors are large. In Table S2.8 and Table S2.9 analyses include covariates in the FRDD estimations. As expected, since covariates are nearly random relative to the cutoff, these results are quite similar to the main results. In Table S2.10, analyses include in the age five sample children who are in primary school as not in pre-primary rather than excluding them, and the results are generally similar, although some lose significance.

An additional set of estimates use 2SLS directly to calculate the FRDD treatment effect. Table S2.11 (age four) and Table S2.12 (age five) present the first stages and Table S2.13 (age four) and Table S2.14 (age five) present the second stage results. The intercept only and linear models are presented (these are the same point estimates as conventional FRDD estimates, albeit different SEs), along with an intercept (for employment) and linear (for pre-primary enrollment) combination that is not estimable with rdrobust. The effect of pre-primary on age four is insignificant in all the models. The age five results are, unsurprisingly, similar to the main results, with some differences in statistical significance.

Potential mechanisms

One reason that the expansion of pre-primary, and particularly public preparatory (age five) may have been ineffective in increasing women's employment is that the hours of care were difficult to reconcile with employment. For instance, public preparatory provided only 21 hours of care per week starting in 2011 (Nawel 2011; UNESCO-IBE 2012). Figure 4 presents the distribution of hours of pre-primary for the age four group (ages 3.5-4.5) by women's employment.[24] Women who were employed tended to have children with longer hours of pre-primary at age four, a median of 20 hours per week, compared to a median of 12 for women who were not employed. Moreover, a third (33%) of women who were employed had more than 21 hours of care. The following year, if their children attended public preparatory, these women might find it difficult to reconcile employment and preparatory's shorter hours, leading to the drop in employment in 2012 and 2018, after the expansion of the preparatory grade (age five). The preparatory expansion in 2012 and 2018 may have induced mothers who would have previously kept their children in other care arrangements if they worked to send their five-year-olds to preparatory instead. Table S2.15, in the appendix, demonstrates for the age five group that pre-primary enrollment not only increased over time but became less selected.

This interpretation is corroborated by undertaking heterogeneity analysis, examining the results for nuclear households (the woman, spouse, and children only) versus extended households in Table S2.16 and Table S2.17, in the appendix. Women in nuclear households will lack alternative caregivers for after the short pre-primary day, whereas women in extended households are likely to have other caregivers available. The first stage (discontinuity in pre-primary around the cutoff) is significant and similar in magnitude for nuclear and extended households in each year and for each age group. The FRDD estimates of the impact of pre-primary on employment show that the negative effects at age five for 2012 and 2018 are concentrated among nuclear households only. Short days of pre-primary without other caregiving assistance are difficult for women to reconcile with employment.

In a similar vein, Table S2.18 and Table S2.19, in the appendix, explore FRDD estimates for being in wage employment (versus non-wage or non-employment) and non-wage employment (versus wage or non-employment). Women in MENA tend to leave wage work at or in anticipation of marriage, given difficulties reconciling such work with caregiving, but at marriage they maintain or increase their levels of non-wage work (e.g., self-employment), which is easier to reconcile with caregiving (Assaad, Krafft, and Selwaness 2022; Selwaness and Krafft 2021). The results demonstrate that decreases in employment at age five in 2012 and 2018 are primarily for wage employment, with much smaller and usually insignificant effects for non-wage work. This result is consistent with challenges in reconciling wage work, which typically has a more rigid schedule than non-wage work, with preparatory schedules.

6. Discussion and conclusions

Pre-primary education is frequently cited as an important intervention to raise women's employment in LMICs generally and MENA in particular. Conceptually, increasing access to pre-primary would address a key constraint on women's employment, namely women's domestic responsibilities and opportunity cost of time. Empirically, evidence from other countries shows reducing costs or increasing access to child care generally has the potential to increase women's employment (Baker, Gruber, and Milligan 2008; Berger and Black 1992; Berlinski and Galiani 2007; Blau and Tekin 2007; Clark et al. 2019; Crawford 2006; Dang, Masako, and Nguyen 2019; Davis et al. 2018; Martinez, Naudeau, and Pereira 2012). The literature to date from LMICs has likewise almost universally demonstrated positive effects of child care access on women's labor market outcomes (Halim, Perova, and Reynolds 2022). This previous literature has, however, come from contexts with comparatively high rates of women's participation (Halim, Perova, and Reynolds 2022).

This paper investigated whether pre-primary could increase employment in Algeria, a context with very low employment rates (14%) for women. The paper specifically tested the effect of a substantial pre-primary expansion in Algeria on women's employment. Pre-primary enrollment rates for children aged five were 5% in 2005 but rose to 79% by 2011. The analyses identified the effect of pre-primary using FRDD, based on the December 31 age cutoff to be eligible for pre-primary. The research compared results from 2006 (early in the expansion), 2012, and 2018, and for ages four and five. Analyses demonstrated that covariates were close to as-good-as-random and there was no manipulation around the cutoff, to ensure the validity of the identification strategy. The analyses demonstrated a clear jump in pre-primary enrollment at the cutoff in recent years and particularly for age five.

What the paper found was that the pre-primary expansion did not increase women's employment. If anything, it appears to have actually *decreased* employment for women with children aged five in 2012 and 2018. Age five was the target of the expansion and the significant effects at age five (but not age four) support the identification strategy and findings. The significance of results is somewhat sensitive to the specification, particularly functional form (we used the functional form most often recommended based on the AIC).

Why would pre-primary have a *negative* effect on women's employment in Algeria? The part-day (and lunch break) structure of public preparatory may make it harder for women to work. The length of pre-primary care for employed women with children in the age four group is often longer than public preparatory (age five) hours. Moreover, negative effects are concentrated among women living in nuclear families, who lack alternative caregivers. Other types of care than public preparatory may be better able to meet women's need for care during work hours.

The fact that Algeria's pre-primary expansion appears to have not increased, and likely instead decreased employment underlines the importance of policy design. In order for pre-primary to increase women's employment, it may need to provide work-day length care. The importance of all-day school or afterschool to raising women's employment rates has been demonstrated in other contexts (Berthelon, Oyarzún, and Kruger 2015; Cannon, Jacknowitz, and Painter 2006; Dhuey, Lamontagne, and Zhang 2019; Martínez A. and Perticará 2017). Ours is the first work to show that part-day care can reduce women's employment, but is congruent with other evidence that part-day care may not be effective in raising women's employment (Medrano 2009).

Careful policy design may be particularly important in contexts where women's baseline employment is low. A study of Israeli Arab mothers, whose baseline labor force participation was only 17%, found that the implementation of full-day pre-primary for ages 3-4 led to a 7 percentage point increase in women's participation (Schlosser 2011). Ensuring that pre-primary expansions align with women's work schedules will be a critical area for future investigation; if Algeria switched to all-day pre-primary, would this then increase women's employment?

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Figures

Figure 1. Pre-primary attendance (percentage) by weeks from cutoff, age group and year

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018
Notes: "Age [X] cutoff" refers to the group of children within (+/-) six months of being age [X] on December 31 of that school year (the cutoff). Points are bin means for six bins on each side of the cutoff (roughly months), which were the most common integrated mean squared error optimal bins. Shading represents 95% confidence intervals. The line is a linear fit.

Figure 2. Histogram (proportion of observations) of age in weeks from cutoff, by age group and year

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018
Notes: Showing 95% confidence intervals for linear polynomial, robust standard errors. This figure is a test for assignment variable manipulation.

Figure 3. Employment (percentage) by weeks from cutoff, age group and year

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018
Notes: "Age [X] cutoff" refers to the group of children within (+/-) six months of being age [X] on December 31 of that school year (the cutoff). Points are bin means for six bins on each side of the cutoff (roughly months). Shading represents 95% confidence intervals. The line is an intercept (mean only) fit.

Figure 4. Distribution of pre-primary hours per week by mother's employment, age four

Source: Authors' calculations based on Algeria MICS 2006, 2012
Notes: Pooling 2006 and 2012 data. Not available in 2018. Kernel density (Epanechnikov), bandwidth 3. Using children in the age four group, within (+/-) six months of being age 4 on December 31 of that school year (the cutoff). Question was only asked of those ages 3-4 at the time of the survey. Dashed vertical line marks 21 hours of public preparatory.

Tables

Table 1. Reduced-form FRDD intercept, linear, and quadratic models of employment (percentage points) by year, age four

	2006			2012			2018		
Above cutoff	-0.501	-3.471	-3.375	0.161	-0.105	-1.161	0.015	-0.502	3.057
	(1.216)	(2.384)	(3.621)	(1.051)	(2.190)	(3.336)	(1.166)	(2.379)	(3.562)
Age in weeks from cutoff (age 4)		0.129	0.556		0.026	1.285**		0.041	-0.145
		(0.118)	(0.504)		(0.103)	(0.433)		(0.115)	(0.489)
Above cutoff # Age in weeks from cutoff (age 4)		-0.028	-0.917		-0.032	-2.365***		-0.044	-0.509
		(0.159)	(0.650)		(0.142)	(0.578)		(0.151)	(0.624)
Age in weeks from cutoff (age 4) # Age in weeks from cutoff (age 4)			0.016			0.048**			-0.007
			(0.019)			(0.016)			(0.018)
Above cutoff # Age in weeks from cutoff (age 4) # Age in weeks from cutoff (age 4)			0.001			-0.007			0.032
			(0.024)			(0.021)			(0.023)
Constant	10.521***	12.148***	14.018***	7.914***	8.259***	14.026***	11.351***	11.908***	11.068***
	(0.885)	(1.735)	(2.756)	(0.751)	(1.570)	(2.482)	(0.848)	(1.763)	(2.773)
N (obs.)	2499	2499	2499	2666	2666	2666	2976	2976	2976
R-sq.	0.000	0.001	0.002	0.000	0.000	0.007	0.000	0.000	0.001
Adj. R-sq.	-0.000	-0.000	-0.000	-0.000	-0.001	0.005	-0.000	-0.001	-0.001
AIC	24151	24153	24155	25167	25171	25158	29027	29031	29032

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018

Notes: *p<0.05; **p<0.01; ***p<0.001. OLS models. Standard errors in parentheses. Outcomes is employment (as a percentage).

Table 2. Reduced-form FRDD intercept, linear, and quadratic models of employment (percentage points) by year, age five

	2006			2012			2018		
Above cutoff	2.686*	1.865	4.271	-2.639*	-5.118*	-6.128	-3.328**	-4.221	-4.270
	(1.160)	(2.272)	(3.413)	(1.141)	(2.309)	(3.458)	(1.142)	(2.273)	(3.431)
Age in weeks from cutoff (age 5)		0.006	-0.970*		0.284**	-0.168		0.167	-0.054
		(0.114)	(0.466)		(0.105)	(0.445)		(0.110)	(0.462)
Above cutoff # Age in weeks from cutoff (age 5)		0.051	1.491*		-0.386*	0.739		-0.260	0.210
		(0.153)	(0.612)		(0.150)	(0.606)		(0.149)	(0.606)
Age in weeks from cutoff (age 5) # Age in weeks from cutoff (age 5)			-0.038*			-0.017			-0.008
			(0.018)			(0.017)			(0.017)
Above cutoff # Age in weeks from cutoff (age 5) # Age in weeks from cutoff (age 5)			0.020			-0.008			-0.001
			(0.023)			(0.022)			(0.022)
Constant	8.630***	8.703***	4.388	10.930***	14.760***	12.794***	12.684***	14.841***	13.846***
	(0.824)	(1.647)	(2.590)	(0.794)	(1.624)	(2.482)	(0.814)	(1.636)	(2.603)
N (obs.)	2667	2667	2667	2678	2678	2678	2996	2996	2996
R-sq.	0.002	0.002	0.004	0.002	0.005	0.007	0.003	0.004	0.004
Adj. R-sq.	0.002	0.001	0.003	0.002	0.004	0.005	0.002	0.003	0.002
AIC	25706	25710	25707	25731	25727	25727	29128	29129	29132

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018

Notes: *p<0.05; **p<0.01; ***p<0.001. OLS models. Standard errors in parentheses. Outcomes is employment (as a percentage).

Table 3. FRDD intercept models of employment (percentage points) by year, age four

	2006	2012	2018
Conventional	-0.081	0.016	0.001
	(0.236)	(0.133)	(0.099)
Bias-corrected	-0.655**	-0.001	-0.031
	(0.236)	(0.133)	(0.099)
Robust	-0.655	-0.001	-0.031
	(0.482)	(0.298)	(0.211)
First conventional	6.158***	10.227***	15.863***
	(1.454)	(1.903)	(1.772)
First bias-corrected	-0.758	4.295*	12.222***
	(1.454)	(1.903)	(1.772)
First robust	-0.758	4.295	12.222**
	(2.885)	(3.939)	(3.737)
N (Obs.)	2499	2666	2976
N right	1321	1391	1564
N left	1178	1275	1412

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018
Notes: *p<0.05; **p<0.01; ***p<0.001. Models based on rdrobust. Standard errors in parentheses. Outcome is employment (as a percentage). "First" denotes the first-stage discontinuity in pre-primary. N right denotes the number of observations to the right of the cutoff; N left denotes the number of observations to the left of the cutoff, and N (Obs.) is the total number of observations (sum of N left and N right).

Table 4. FRDD intercept models of employment (percentage points) by year, age five

	2006	2012	2018
Conventional	0.180*	-0.113	-0.090*
	(0.089)	(0.069)	(0.044)
Bias-corrected	0.116	-0.293***	-0.157***
	(0.089)	(0.069)	(0.044)
Robust	0.116	-0.293*	-0.157
	(0.167)	(0.148)	(0.091)
First conventional	14.899***	23.407***	36.906***
	(1.971)	(2.454)	(2.213)
First bias-corrected	15.645***	7.880**	19.392***
	(1.971)	(2.454)	(2.213)
First robust	15.645***	7.880	19.392***
	(3.785)	(5.150)	(4.567)
N (Obs.)	2667	2678	2996
N right	1294	1413	1553
N left	1373	1265	1443

Source: Authors' calculations based on Algeria MICS 2006, 2012, 2018
Notes: *p<0.05; **p<0.01; ***p<0.001. Models based on rdrobust. Standard errors in parentheses. Outcome is employment (as a percentage). "First" denotes the first-stage discontinuity in pre-primary. N right denotes the number of observations to the right of the cutoff; N left denotes the number of observations to the left of the cutoff, and N (Obs.) is the total number of observations (sum of N left and N right).

[1] Pre-primary is defined, per the International Standard Classification of Education (ISCED), as programs targeting ages 3-5 with educational objectives (UNESCO 2021).

[2] Schlosser (2011) examines the impact of Israel's expansion of preschool on Arab mothers (who have low rates of participation) within Israel and finds an increase in participation.

[3] For example, Schlosser (2011) shows that a full-day preschool expansion increased the participation of Arab mothers in Israel.

[4] The 2011 data is the most recent reported as of January 2023.

[5] There were rounds of the MICS in 1995 and 2000 but they only asked about education in the household roster for ages 6+ (Ministry of Health and Population Republic of Algeria - National Institute of Public Health 2001; Ministry of Health and Population Republic of Algeria 1996).

[6] The 2006 data were received in correspondence with the Ministry of Health Population and Hospital Reform, 2012 and 2018 data are publicly available from https://mics.unicef.org/surveys.

[7] Data are representative after the application of sampling weights, which are used throughout. Sampling weights account for non-response. The response rate was 98% in 2006, 98% in 2012, and 96% in 2018 on the household level (outcomes such as employment are from the household roster). The sampling frame for each MICS was the most recent population census.

[8] While in 2006 and 2012 education questions in the household module were asked of ages 5-24, in 2018 they were asked of ages 3-24, and a skip included in asking about preschool in the under-five module based on the education status in the household module, so effectively both modules are used for ages 3-4 in 2018.

[9] For example, a child who was born December 1, 2007, who was interviewed on October 31, 2012, would be age five (by 30 days) on December 31, 2012, but age four at the time of

interview and asked the age four questions. A child who was born December 1, 2007, who was interviewed on January 15, 2013, would be age five (by 30 days) on December 31, 2012, but age five at the time of interview and asked the age five questions.

[10] The exact question is slightly different over time. The 2006 question focuses on out of home or preschool programs and includes Koranic programs in the list of examples (2012 does not, but 2018 does), whereas the 2012 question mentions educational learning programs and nurseries (2006 does not, but 2018 does), and 2018 also explicitly mentions the preparatory year (Ministry of Health Population and Hospital Reform 2015; Ministry of Health Population and Hospital Reform and National Office of Statistics 2008; Ministry of Health Population and Hospital Reform and United Nations Children's Fund (UNICEF) 2020).

[11] This date is therefore age December 31 of 2006 for the 2006 round, December 31 of 2012 for the 2012/13 round, and December 31 of 2018 for the 2018/19 round.

[12] This is the same as in Berlinski, Galiani, and McEwan (2011), except in their case the cutoff was June 30 rather than December 31.

[13] In the rare cases where a mother had more than one child in the same discontinuity group, the information for the youngest child was used.

[14] Analyses use the Stata package rdplot (Calonico, Cattaneo, and Titiunik 2014b).

[15] Denote treatment (pre-primary) as T_i, the running variable (weeks of age relative to December 31 cutoff) as X_i, c as the cutoff, and D_i as an indicator variable for being above the cutoff ($D_i=1[X_i \geq c]$). The effect of the assignment rule on treatment (attending pre-primary) is then: $T_i = \alpha + f(X_i - c) + \beta D_i + \varepsilon_i$, where $f()$ is some polynomial functional form and ε_i an independent error term.

[16] Analyses estimate the effect of pre-primary on women's employment (Y_i) based on: $Y_i = \gamma + g(X_i - c) + \tau T_i + \nu_i$, where $g()$ is some polynomial functional form and ν_i an independent error term.

[17] Analyses use the Stata package rdrobust (Calonico, Cattaneo, and Titiunik 2014b).

[18] The instrument for treatment (pre-primary) is the indicator for being above the cutoff (Imbens and Lemieux 2008). If the model assumptions hold, being above the cutoff will predict treatment (pre-primary) but be otherwise unrelated to outcomes (employment).

[19] Analyses specifically estimate $Y_i = \gamma + g(X_i - c) + \tau D_i + \nu_i$. Here, τ is the intent-to-treat estimator (Lee and Lemieux 2010).

[20] Additionally, to the best of the authors' knowledge, there are not any other programs or services for young children in Algeria that use this same cutoff.

[21] As well as the intuitive appeal of months, the integrated mean squared error optimal bins (calculated with the rdplot package (Calonico, Cattaneo, and Titiunik 2014b)) tend to be around six on each side: six on the left and nine on the right for age 5 in 2018, six on the left and four on the right for age 5 in 2012, three on the left and six on the right for age 5 in 2006, 11 on the left and seven on the right for age 4 in 2018, five on the left and right for age 4 in 2012, five on the left and eight on the right for age 4 in 2006. It is preferable to consistently present the same number of bins for comparability in the visual.

[22] Tests are implemented using rddensity version 2.3 (Cattaneo, Jansson, and Ma 2018).

[23] Table S2.1 also allows an assessment of sample characteristics generally. For instance, fewer than 1% of mothers are single; parenthood is almost exclusively reserved for within marriage in Algeria.

[24] The question was only asked for those children ages 3-4 at the time of the survey. The question was not asked in 2018; figure pools 2006 and 2012 data.