

Maternal Employment Responses to Publicly Funded Full-Day Kindergarten in Arizona

Umair Ali *

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Abstract

The paper investigates Arizona's full-day kindergarten experiment conducted between 2004 and 2010, during which the state increased public funding to expand kindergarten from half-day to full-day programs. This expansion implicitly acted as a subsidy for child care, by reducing the number of after-school care hours that were previously required with half-day kindergarten. Using monthly data from the Current Population Survey (CPS) for mothers of age-eligible children across U.S. states from 2000 to 2010, this paper employs a difference-in-differences approach to estimate the impact of full-day kindergarten on maternal labor supply, considering heterogeneity across race, marital status, age, and education levels. Employment increases among different subgroups of mothers such as those who are either single, or are high skilled or belong to non-White races. Significant positive employment response on the intensive margin is only found for mothers who are either single or belong to non-White races. Relying on the fact that schools are usually not open during summer months of June through August, triple differences estimates are run as a robustness test.

Keywords: Full-day Kindergarten; Female Labor Force Responses; Extensive Margin; Intensive Margin; Female Employment; Half-day Kindergarten; Early Childhood Education; Education Economics; Employment Economics

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*Umair Ali: Arizona State University, umair.ali@asu.edu. I am grateful to Professor Chris M. Herbst at School of Public Affairs, Arizona State University for his comments and mentorship. I am grateful to my dissertation committee & colleagues at Arizona State University whose useful insights helped build this project in the current shape.

1 Introduction

Provision of publicly funded kindergarten varies across different states of the U.S. One significant source of this variation lies in differences in levels of provision of kindergarten across states. [Parker et al. \(2016\)](#) note that while some states – for example West Virginia and Oklahoma – have mandated the provision of full-day kindergarten financed by public/state funds¹, others (for example New Hampshire and Nebraska) have chosen to mandate only half-day kindergarten. [Parker et al. \(2016\)](#) also note how others like Iowa, New Jersey and Wyoming have chosen to not mandate any level of publicly funded kindergarten. Across states, policies also vary in the provision of public funds for kindergarten for constituent school districts which voluntarily choose to offer kindergarten ([Loewenberg, 2017](#)).

The state of Arizona provides an interesting and unique case in this context. Arizona has traditionally only mandated half-day kindergarten across public school districts. However, this policy has gone through considerable variation during the last 20 years ([Libassi, 2014](#)). The state of Arizona experimented with full-day kindergarten during the school years 2004-05 and 2009-10 (Figure 1). Libassi notes that Arizona legislators introduced bills mandating the provision of full-day kindergarten in the poorest school districts. Selection of the poorest school districts for funding full-day kindergarten was based on the percentage of total student population eligible for free or reduced meals. For school year 2004-05, only those school districts were funded for full-day kindergarten in which at least 90 percent student population was receiving either free or reduced-price lunches. In school year 2005-06, full-day kindergarten was funded across school districts in which at least 80 percent student population was eligible for either free or reduced-price lunches. From school year 2006-07, all school districts were funded for full-day kindergarten². As Libassi further notes, funding for full-day kindergarten was abolished in March 2010 (for the 2010-11 school year) under the new administration. Later, [Loewenberg \(2017\)](#) notes that the governor of Arizona once again signed legislation to partially fund some of the poorest school districts in Arizona for full-day kindergarten from the 2018-19 school year.

¹As of 1998, Alabama, Arkansas, DC, Florida, Georgia, Louisiana, Mississippi, North Carolina, North Dakota, South Dakota, Texas, Vermont offered full-day kindergarten ([Chmelynski, 1998](#)).

²For school year 2006-07, the then Arizona governor Napolitano decided to change the way how full-day kindergarten was being funded. From the school year 2006-07, the legislation increased the per-pupil funding weight of kindergarten – to amount to 62 percent of a regular elementary school pupil funding. Although, this funding was not at par with the elementary school grades, this allowed all school districts willing to offer full-day kindergarten an opportunity to offer six hours of full-day kindergarten.

Existing literature has found positive impacts of availability of full-day kindergarten on maternal employment (Cannon et al., 2006; Cascio, 2009; Gibbs, 2014), although many studies find that stronger positive impacts in employment are concentrated among certain subgroups of mothers, such as mothers who are single or have lower levels of skills (Cascio, 2009; Dhuey et al., 2021). For the case of Arizona, public provision of kindergarten is free of any costs for parents. Given that the free full-day kindergarten has the potential to substitute other paid child care settings, it is likely that full-day kindergarten could provide the required substitution effect for an increase in maternal employment. The paper is an attempt to find empirical evidence that supports this connection between full-day kindergarten availability and maternal employment.

By using a difference-in-differences specification, my estimation strategy exploits the variation in the timing of availability of full-day kindergarten in Arizona as compared to 39 other states which did not make any substantial changes to the availability of kindergarten. Other than Arizona, a number of states indeed legislate to change the mandatory length of kindergarten day during 2000-2010. As show in Table 1, 13 states (excluding Arizona) changed the availability or length of kindergarten day during this time period. These 13 states are excluded from the analysis. While Arizona did offer full-day kindergarten to age-eligible children during the treatment years, it cannot be assumed that full-day kindergarten treatment was actually taken up by families of five year old children. To estimate treatment compliance, I start by examining the treatment impact on enrollment in full-day kindergarten in a difference-in-differences setting based on data from the annual CPS³ October supplements from 2000 to 2010⁴. This analysis is followed up by an analysis of maternal employment in a similar difference-in-differences setting, based on data extracted from CPS monthly samples from September to May⁵ for each year from 2000-2010. Specifically, the paper targets mothers of age-eligible children within the State of Arizona, analyzing maternal employment outcomes, both on the extensive and the intensive margin. I follow up the baseline difference-in-differences specifications with two robustness tests. First, I use a triple-difference (DDD) estimator to examine maternal outcomes, where the third difference⁶ lies across maternal outcomes during regular school-year months (Sept-May) as compared to maternal outcomes in summer months (June-August) when schools are not in session. Second, I use a basic synthetic control approach to examine if the overall employment estimates hold.

³Current Population Survey (Flood et al., 2021).

⁴Post 2010, the policy environment with respect to kindergarten underwent rapid changes, making the time period unsuitable to be examine under a difference-in-differences setting.

⁵A period of September to May resonates closely with the school year.

⁶As is the case with the baseline difference-in-differences specifications, the first difference is between post and pre-treatment employment levels within Arizona, and the second difference lies between the post and pre-treatment employment levels in control states.

Existing literature suggests small positive impacts for mothers of age-eligible children, specifically for groups (e.g. single, low-skilled mothers) where substitution effect is larger than the income effect for maternal employment. This hypothesis is not only rooted in existing literature but is also suggested by the research approach taken in this paper. There are three concerns that indicate that my estimates are indeed lower-bound estimates. First, the intervention occurred at the intensive margin of kindergarten availability, it only expanded the length of kindergarten day. The state-funded half-day kindergarten â both before and after the intervention â already consisted of 2.5 hours per day (Libassi, 2014). The full-day kindergarten intervention added another 2.5-3.5 hours of time to this half-day kindergarten during the time of full implementation of the intervention (2007 to 2010). Second, as earlier described, only specific qualifying school districts were eligible for full-day funding during the phase-in (2004/05 school year - 2006/07 school year) of the intervention. Third, my analysis includes all mothers of age-eligible children for full-day kindergarten, regardless of treatment compliance⁷. Hence it is reasonable to assume that the estimates from this study are lower-bound, intent-to-treat (ITT) effects. In this context, it is likely that indirect income effect for certain subgroups such as married or high-income mothers, remains higher than the substitution effect realized from the implicit subsidy of child care costs provided through full-day kindergarten.

For other groups however, such as mothers who are either single or have low-income or are low-skilled, one would expect that a child care subsidy in the form of free full-day kindergarten would increase the opportunity cost of the foregone income in case of not working. This means that one can expect stronger positive impacts on employment among these subgroups of mothers.

I contribute to the existing literature in two ways. First, I add Arizona based evidence to the growing literature on the labor supply impacts of full-day kindergarten. Second, I exploit the structure of the CPS monthly data, so that it closely resonates with the school year, an approach which might be helpful for future research under similar settings.

⁷While I present overall descriptive evidence of increased take-up of full-day kindergarten during the intervention, one cannot ensure perfect compliance, because there are mothers of children in the sample who were either outside the school-districts eligible to participate during 2004/05-2006/07, or who simply chose not to send their children to full-day kindergarten. I do not have sufficient data at this point of time to claim that everybody complied with the availability of the full-day kindergarten even when it was mandatory to be offered by school-districts.

2 Theoretical Considerations

The theory of maternal labor supply decisions in relation to the availability and affordability of child care was first presented by Ribar (1992) and Connelly (1992) for the case of married mothers. Kimmel (1998) builds upon Ribar's and Connelly's work and presents a unified maternal labor supply model for single and married mothers. Maternal labor supply or labor force participation is a function of the standard utility maximization, where a mother maximizes her utility as a function of child care quality, market goods, and leisure (Kimmel, 1998). In the above model time allocation for the mother is constrained within 24 hours, the utility from child care depends on child care prices (at a given acceptable quality), while the utility of market goods depends upon the hourly wage rate that a mother can acquire in the market (Kimmel, 1998). The respective utility that an individual derives from leisure and market goods is a function of hourly wage rates. Wage rates decide the accessibility of market goods. Wage rates also help an individual compare the respective utility obtained from work and leisure. Under the standard assumption that the labor force participation is not time constrained other than the total hours available in a day (Kimmel, 1998), it can be deduced that the relative employment elasticity to income and child care costs are significant factors in maternal labor supply decisions.

For a mother to participate in the labor force, the utility from labor force participation and the utility from child care quality must be higher than the foregone utility of leisure and material goods in order to pay for child care. For instance, if the hourly wage is lower than (or even for some cases not substantially higher than) child care prices per hour, the mother is not likely to participate in the labor force unless the realized quality of child care increases indirect utility⁸.

The utility driven from the consumption of full-day kindergarten is expected to be heterogeneous across various subgroups of mothers. Heterogeneous utility gains from kindergarten consumption across married and single mothers are driven by the respective strength of substitution and income effects. Ribar (1992) has argued that total family income directly impacts the utility maximization function for married mothers. For the case of married mothers, the substitution effect of full-day kindergarten subsidy is not as strong as it is for single mothers (Blau and Hagy, 1998; Tekin, 2007). In the absence of informal child care or child care subsidy, when publicly funded child care

⁸Such as for example, the utility from child development

becomes available in the form of public school, subgroups of mothers who are either single or low-skilled, are likely to substitute other child care arrangements with this source of care (Blau and Hagy, 1998; Tekin, 2007). Holding non-labor income constant (Ribar, 1992), it is also plausible that single mothers have more motivation to enter the labor force as compared to married mothers, who are more likely to have access to overall higher levels of household income.

Previous literature has shown that early childhood education and care (ECEC) costs are a major factor affecting maternal labor supply decisions (Baum, 2002; Blau and Tekin, 2007; Herbst, 2010; Kimmel, 1998; Morrissey, 2017). Publicly available early childhood education (preschool, kindergarten etc.) is a highly subsidized, low-cost alternative to child care.

Provision of publicly funded, full-day kindergarten in connection with employment amongst mothers of age-eligible children has been studied by Cannon et al. (2006), Cascio (2009), Dhuey et al. (2021), and Gibbs (2014). All of the above studies find maternal labor supply impacts of full-day kindergarten, although they find heterogeneous employment outcomes for different subgroups of mothers. Cannon et al. (2006) find that mothers of children attending kindergarten are more likely to participate in the labor force. Cascio (2009) finds positive impacts on employment participation amongst single mothers of five-year-olds. Dhuey et al. (2021) do not find any significant impact on overall maternal employment. For single mothers however, they find strong evidence of a positive impact on maternal employment, both on the extensive as well as the intensive margin of work. Gibbs (2014) finds small but significant positive impacts on maternal employment outcomes.

Full-day kindergarten includes an implicit child care subsidy for age-eligible children. The theoretical linkages as well as the literature cited above suggest strong substitution effects from the availability of full-day kindergarten, as suggested by increased employment responses among only subgroups of mothers such as those who are low-skilled or are single by marital status. For other maternal groups (married, or skill-specific distinction across mothers), the substitution effect from full-day kindergarten appears to be much weaker in the present literature.

3 Data Description

The primary source of data for this paper comes from the Current Population Survey (CPS) spanning school years 1999-00 to 2009-10 (Flood et al., 2021). I use CPS samples for the months September to May of each calendar year

between 1999 and 2010. There is a compelling reason to do so. A typical school year runs from September to May. For this reason, the months from each CPS year are carefully matched to synchronize with the school year. For example, school year 1999-2000 (or school year 2000) is matched with CPS months Sept 1999 to May 2000. This allows for a more reliable estimate of enrollment changes for identification purposes.

For maternal labor supply analysis, the sample includes all mothers aged 20 to 55 with a youngest child aged 5. To be eligible for kindergarten enrollment in Arizona, a child must be 5 years of age. A child is deemed to be age 5 only if the child reached the age of 5 before September 1 of the respective school year. Hence it is logical that only mothers with age-eligible children are included in the analysis sample. In order to reliably isolate the treatment effects among mothers with age-eligible children, I exclude mothers of children aged 5 who have other children younger than 5 in the household, as their employment decisions might be affected by the younger child in the household. I also exclude mothers who are currently enrolled in any kind of schooling. Since Arizona started offering full-day kindergarten in school year 2005, this provides me with five pre-treatment years before the start of the intervention, as a baseline for comparison. School year 2005 to school year 2010 are coded as the primary treatment years. I treat school years 2005-2010 as a single unified treatment era, to allow enough sample size of mothers with the youngest child aged 5, based on yearly observations from Arizona.

From school year 2011, Arizona abolished full-day kindergarten funding. However, even without state funding, many local school districts still continued to offer full-day kindergarten based on secondary funding sources, such as local property taxes. This brought about an era of uncertain and muddled policy environment, during which it is not really possible to isolate the effect of one policy from another, at least under the current difference-in-differences framework. I therefore end my study period post school year 2009-10.

3.1 Full-Day Kindergarten Enrollment

One key challenge that comes with the use of CPS data is that only CPS-October education supplement differentiates across full-day kindergarten enrollment and half-day kindergarten enrollment. This information is not present for other months of CPS. For the analysis of maternal employment, I include all mothers who have their youngest child aged five, regardless of compliance with full-day kindergarten enrollment. To ensure that a sizeable proportion of five-year-olds in the state of Arizona complied with the intervention by enrolling in full-day kindergarten, a

preliminary analysis of kindergarten enrollment based on the data from CPS-October supplement is needed.

In Figure 2, I use data extracted from the CPS-October education supplement to analyse how full-day vs half-day kindergarten enrollment changes over the years, and particularly so over 2005-2010, the intervention years for this study. For each year on the observation, I use a moving average of two preceding years and the current CPS year, to smooth out the curve as the number of observations within a single year of data from Arizona are too few, and the resulting volatility induced by smaller sample size makes it a bit harder to interpret. The vertical dotted line in Figure 2 indicates the beginning year of the intervention. As one would expect, full-day kindergarten enrollment increases from 45% to close to 90% during the intervention years. Half-day kindergarten enrollment mirrors the trend, with a sharp decline over the same years. It is important to note that the full-day kindergarten enrollment rose as children who were earlier attending half-day kindergarten, moved to full-day kindergarten.

3.2 Treatment Compliance

While the State of Arizona offered full-day Kindergarten it is not yet clear if families and parents do indeed respond by enrolling their children into full-day Kindergarten. There is a need to verify that the full-day kindergarten enrollment trends in Arizona are indeed driven by the intervention within Arizona and are not driven by any national or other region-specific trends in education.

Figure 3 graphs trends for full-day kindergarten enrollments in Arizona as compared to the control states during 2000-2010. Prior to the intervention, full-day kindergarten enrollment in Arizona stood around 20-25%, comparable to the national average across the 37 control states. Post-intervention, full-day kindergarten enrollment in Arizona gradually increases, peaking out around 2009 when 33% children (age 4 or age 5) attended full-day kindergarten. Post 2005, the increase in full-day kindergarten attendance across Arizona is remarkably higher than the control states, but in the years prior to the treatment, trends across Arizona and the control states overlap and exhibit similar patterns.

To do so and make a stronger case for identification, I run a difference-in-differences (DD) specification for full-day kindergarten enrollment amongst five-year-olds, across the sample drawn from October CPS Education supplement during the study period 2000-2010. Specifically, I run a regression of the following form:

$$Y_{i,s,t} = \gamma(AZ_s X FDK_t) + X'_{i,s,t} + \theta_s + \zeta_t + \eta_{s,t} + \varepsilon_{i,s,t} \quad (1)$$

Where $Y_{i,s,t}$ is the key outcome variable, a binary indicator indicating if the five-year-old i in state s in year t is enrolled in full-day kindergarten or not, AZ is a binary indicator if individual i is located in the state of Arizona or not, FDK is a binary indicator indicating if the observation year for each observation lies within the time period of the treatment years (2005 to 2010). Key coefficient of interest lies on the interaction between the AZ and FDK indicators. I control for a matrix of maternal and family characteristics X' , including age, race, education levels, marital status and family size and income. State fixed effects (θ) control for time-invariant state fixed effects, while year fixed effects (ζ) control for year specific macroeconomic and relevant policy shocks that systematically affect all states in the country. To account for potential unobserved confounding factors that might correlate with the plausibly exogenous policy introduction of full-day Kindergarten, $\eta_{s,t}$ is a vector containing state-specific linear time trends.

3.2.1 Estimates for Treatment Compliance

The difference-in-differences estimates from equation 1 are presented in Table 2. Controlling for maternal and family demographics in column (2), the estimates indicate that the intervention led to an increase in full-day kindergarten enrollment in Arizona by 8.4 percentage points over 2005-2010. The estimates however exhibit a great degree of heterogeneity across different sub-populations. Large increases in full-day kindergarten enrollments are found among children of high skilled mothers (24 percentage points), single mothers (38.3 percentage points), White mothers (17.9 percentage points), and high income mothers (17.9 percentage points). For children of low-skilled, married, non-white, or low-income mothers, effects on full-day kindergarten enrollments are either absent or negative.

Children of high-skilled, high-income mothers have been traditionally more likely to attend nursery schools (Davis and Bauman, 2013) and child care (Baum, 2002). Previous literature also links full-day kindergarten availability with maternal employment of single mothers (Cascio, 2009; Dhuey et al., 2020). Davis and Bauman (2013) report that 25% of children enrolled in full-day kindergarten across the U.S. belonged to households with incomes higher than \$75000. Given these trends, many of these results are not surprising. For high-skilled and

high-income mothers or single mothers, it is likely that the expected substitution effects from any subsequent increases in maternal employment are strong enough for them to enroll their children in full-day kindergarten. Since high-income households are also more likely to consume child care in absence of kindergarten (Anderson and Levine, 1999), it might be the case they move their children from child care to full-day kindergarten. Full-day kindergarten consumption across White and non-White mothers can also be attributed to the income differences across these demographics. To summarize, there is enough evidence of a substantive increase in full-day kindergarten enrollment during the treatment years, although the estimates are heterogeneous across different sub-populations.

3.3 Measures of Maternal Labor Supply

There are two key outcome measures of maternal labor supply. First is the measure for employment which is derived from the employment status variable in the CPS dataset. This is a binary indicator variable coded as 1 when a mother is either at work during the week she was interviewed or is on granted leave. Everyone else including those out of work, out of labor force, retired or unable to work are coded as 0.

Mean female employment comparison trends over the study period (2000-2010) across Arizona and other states are presented in Figure 4. The vertical red lines in both parts of the graph separate pre-treatment years prior to 2004 and intervention years during 2005-10. The upper part of the graph consists of all females aged 20-55, regardless of maternal characteristics. This increases the sample size within Arizona to allow for more stable plotting. This graph shows that female employment trends in Arizona are identical to the national average, and female employment in Arizona has remained consistent throughout the time, although it has remained systematically below the national average. The sample for the lower part of the graph consists of mothers aged 20-55 with youngest child aged 5. The trends signal that on average, female employment among the sample of mothers with five year old children follows the national trend. However, since the number of mothers with the youngest child aged 5 is so small within a given year (in the CPS sample), the trend for Arizona is too volatile in the lower part of the graph⁹. As expected employment among mothers with five-year-old children remains 5-10% lower than female employment in general, but the trends remain similar. Even with some volatility in the lower part of Figure 4, there is no obvious violation of parallel trends.

⁹To address this issue to some extent, I use a moving average of three years for plotting purposes.

As explained above, the intervention only adds 2.5 hours to the half-day kindergarten. Theoretically, it is likely that if the full-day kindergarten expansion does affect female employment on the extensive margin, the magnitude of this effect would be small. I estimate a measure of intensive margin of labor supply by using a measure of the weekly number of hours worked by mothers with age-eligible children. The measure of weekly hours worked is a continuous variable which relies on the responses to the question in CPS which asks individuals about usual hours worked per week. Mean number of hours worked by females in Arizona are compared to females across all other states in Figure 5. The upper half of the graph is based on all females aged 20-55 regardless of maternal status, while the lower half consists of mothers with a youngest child aged 5. Once again, smaller sample confined to mothers with young children age 5 mean that the trend for Arizona in the lower part of the graph is too volatile, but for all mothers, the trend for Arizona mirrors the national average. For mothers with five-year-old children, the trends are more stable in three years immediately prior to the intervention. Similar to the trends on the extensive margin of labor supply, women in Arizona consistently trend below the national average on the intensive margin of labor supply as well. To be noted, it is only during the peak of full-day kindergarten availability (2007 or so) that maternal labor supply on the intensive margin peaks above the national average.

Besides these, a matrix of control variables is also constructed from the CPS data. CPS details race, marital status, education, income as well as total number of children for all individuals in the sample. Yearly state level unemployment data has been retrieved from the Bureau of Labor Statistics.

4 Empirical Strategy

Based on the timing of the full-day kindergarten Arizona, I use a difference-in-differences approach to estimate changes in maternal labor supply during the treatment. The control group for these estimates are 36 states (Table 1), which did not change the length of state-funded kindergarten during 2000 and 2010.

The key outcome of interest for this study is maternal employment on the extensive and intensive margin of labor force participation. I run a slightly modified form of the specification (1) in the following forms:

$$Y_{i,s,t} = \gamma(AZ_s X FDK_t) + X'_{i,s,t} + Z'_{s,t} + \theta_s + \zeta_t + \sigma_m + \eta_{s,t} + \varepsilon_{i,s,t} \quad (2)$$

$Y_{i,s,t}$ is the key outcome variable, a binary indicator indicating main employment outcomes which are employment and weekly hours for mothers. AZ is a binary indicator if individual i is located in the state of Arizona or not, FDK is a binary indicator indicating if the observation year for each observation lies within the time period of the treatment years (2005 to 2010). The key coefficient of interest lies on the interaction between the AZ and FDK indicators. I control for a matrix of maternal and family characteristics X' , including age, race, education levels, marital status as well as family size and income. Z' is a vector of time-variant state level control for unemployment. State fixed effects (θ) control for time-invariant state fixed effects, while year fixed effects (ζ) control for year specific macroeconomic and relevant policy shocks that systematically affect all states in the country. To counter seasonal trends in employment, σ is a vector of month fixed effects. Threat to identification exist from unobserved variables which can vary across states over time â to counter this, η is a vector of state-specific linear time trends.

One important difference from specification 1 is that the specifications in 2 relies on the entire sample, not just the October-CPS. Here I am combining CPS months from September to May, corresponding to each school year. Although this convention of matching CPS monthly data to school-year is a bit novel, it is not the first instance in labor economics. Similar approach has been adopted in time-use studies for school-age students by [Morisi \(2008\)](#) and [Porterfield and Winkler \(2007\)](#). This strategy allows for a larger sample size to generate more precise estimates, and also rules out sampling bias from the estimates, specially so when I limit my analysis to any subgroup of mothers.

As discussed above, each specification contains state-specific linear time trends as well a control matrix of maternal socio-demographic characteristics. To account for seasonal variation in employment characteristics, I also include month fixed effects across all specifications. The sample consists of mothers aged 20 to 55, with a youngest kid aged 5. All mothers who have kids younger than 5 are excluded from this analysis. I cluster standard errors within state-specific cells, to account for state-specific trends in employment.

There are several limitations to this identification strategy. One of the most difficult one is the issue that I cannot trace each age-eligible child and his/ her mother to estimate Treatment-on-the-Treated (ToT) effects for maternal employment. However, given that I show robust evidence of program uptake in the above subsection, a major source of this threat to identification related to the compliance of treatment is removed. Given data limitations, I am only able to generate lower-bound ITT estimates, but I have good reason to believe that ToT effects would not have been much different.

There are two identification threats in a difference-in-differences context. While the first assumption is of parallel trends, which requires that trends of treatment and control groups in pre-treatment outcomes should remain same. Another identification threat emerges from the fact that the Arizona economy might have been very different prior to the intervention, and full-day kindergarten might have been pushed by unobserved political and economic factors. Although I present some graphic evidence of these trends in section 2, and my difference-in-differences strategy relies on a bunch of demographic controls, control for time-variant state-level unemployment, two-way fixed effects as well as state-specific linear time trends to isolate the impact of the kindergarten expansion, these threats might nevertheless still exist to a limited extent.

Two strategies are frequently used to alleviate identification concerns. The first potential method is to use a triple-difference design. A triple-difference design has an advantage that it only requires that parallel trends across the two differences persist over time. If the time period of the intervention was a little brief a simple triple-difference design with mothers of 4 year or 6 year old children as a comparison group would have been ideal. But the structure of the data and the length of the Arizona kindergarten experiment does not allow me this luxury. For example, a 6 year old in 2006 would still have been treated in 2005, a 10 year old in 2010 would have been 5 in 2005, and still treated. On the other hand a 3 or 4 year old in 2005 would be likely to be treated under the intervention during 2007 or 2006. Larger age gaps between control and treatment groups are also not ideal.

However, there is one other way a triple-difference strategy can be taken under the current settings. Since the school year in the U.S. roughly lasts from September to May, schools are usually not in session during the summer months of June, July and August, and hence children are not likely to be attending kindergarten during the summer months. I exploit this timing of the school year in my triple-difference setting. The third difference¹⁰ is provided by using the summer months (June-August) as a control period, and the school-year months (Sept-May) as a treated period.

I modify equation 2 in the following form to run triple-difference regressions for labor supply outcomes:

$$Y_{i,s,t} = \beta_1(FDK_t X Schyr_m) + \beta_2(AZ_s X Schyr_m) + \beta_3(AZ_s X FDK_t) + \gamma(AZ_s X FDK_t X Schyr_m) + X'_{i,s,t} + Z'_{s,t} + \theta_s + \zeta_t + \sigma_m + \eta_{s,t} + \varepsilon_{i,s,t} \quad (3)$$

¹⁰Already used in equation 2, the first difference exists between pre and post-treatment Arizona, and the second difference lies across Arizona and untreated states.

AZ is a binary indicator if individual i is located in the state of Arizona or not, FDK is a binary indicator indicating if the observation year for each observation lies within the time period of the treatment years (2005 to 2010), and $Schyr_m$ is a binary indicator indicating if the observations occur during the school year ($=1$) or the summer months ($=0$). The key coefficient of interest is γ which lies on the triple interaction between the AZ , FDK and $Schyr$ indicators. The remaining structure of equation 3 is exactly identical to equation 2.

A second potential method to ensure robustness of estimates is to rely on matching or synthetic controls. First conceptualized by [Abadie and Gardeazabal \(2003\)](#) and further developed by [Abadie et al. \(2010\)](#), synthetic control methods have gained popularity over the recent past. Synthetic control methods help formulate a synthetic counterfactual of the treated unit, by closely matching outcome predictors in treated units to outcome predictors from a pool of donor units (control units in this case) â assigning a weight matrix to predictor variables from all donor units. Thus, a single placebo control group is generated which is a combination of all control groups, with predictor units aggregated according to assigned weights. The resulting counterfactual control group is very closely matched to the treated group during the pre-treatment years â and the combination of control groups present a counterfactual of treatment group in the absence of a treatment ([Abadie et al., 2010](#)). [Abadie et al. \(2010\)](#) further present methodological advantages of synthetic control groups in terms of (i) transparency of assigned weights to quantify exact contributions from each control group, and (ii) the explicit avoidance of extrapolation.

In the current study, synthetic control method is applied to Arizona as suggested in [Abadie et al. \(2010\)](#) and, [Abadie and Gardeazabal \(2003\)](#), leading to the generation of a counterfactual Arizona in the absence of the treatment. The procedure to generate a synthetic control group is further aided by a companion Stata software package âsynthâ developed by [Abadie et al. \(2011\)](#).

There are limitations with the application of synthetic controls and synth package in Stata software. A key limitation is that synthetic control is extracted from a balanced panel dataset â which rules out using microdata without aggregation. When data is aggregated, for each treated unit there is only one counterfactual or synthetic control available for comparison. On argumentative basis, estimates obtained from a single counterfactual can be spurious. The arising concerns from this limitation can however be reduced to some degree, by running placebo tests as suggested in [Abadie et al. \(2010\)](#). Using placebo tests, the significance of the estimate is tested by running iterative placebo tests â by generating placebo treatment units across other states. It is done by iteratively assigning

each control state a placebo treatment one by one in an iterative manner and by generating counterfactuals for the placebo states, while putting Arizona in a donor pool in each iteration. As [Abadie et al. \(2010\)](#) suggest, if the gap for the outcome variable(s) between Arizona and counterfactual Arizona is persistent or bigger than the gap for other placebo treated states, this should suffice as sufficient proof.

Indeed both synthetic controls and matching have similar issues, trading one form of bias for another ([Kellogg et al., 2021](#)). While synthetic controls minimize extrapolation bias ([Abadie et al., 2010](#)), they do it at the expense of interpolation bias ([Kellogg et al., 2021](#)), but propensity score matching has the exact opposite limitation. The selection of control states under the synthetic control method is relatively formal, and synthetic control has an added advantage that re-weighting on pre-treatment outcomes and control variables corrects for potential issues with common trends assumption under a difference-in-difference setting ([Bonander et al., 2021](#)).

5 Results

Difference-in-differences results for employment and weekly hours are presented in [Table 3](#) and [Table 4](#). Mean pre-treatment employment statistics for each sample are included in each table to form a point of reference.

Column 1 in [Table 3](#) presents the baseline OLS specification, while column 2 adds demographic and state level controls. For estimates on the extensive margin of employment during the intervention, column 2 in [Table 3](#) indicates a 2.5 percentage point increase in employment for the overall sample. For high-skilled mothers, there is a 5.8 percentage point increase in maternal employment, for single mothers there is a huge 16.7 percentage point increase, and low-income mothers experience an increase in employment by 4.2 percentage points. Surprisingly non-White mothers also exhibit a substantial 8.2 percentage point increase in employment. For separate subgroups that include low-skilled mothers, White mothers, and high-income mothers, there are no changes in employment, and for married mothers there is a 3.3 percentage point reduction in employment.

As expected, the results are heterogeneous across different groups. It could be the case for low-skilled mothers, the substitution effect is not as strong to motivate them for employment. Also, the full-day enrollment response in children of low-skilled mothers was slightly negative in column 4 of [Table 2](#), which might explain lack of employment effect to some extent. For high-skilled mothers however, there is a statistically significant, 5.8 percentage point increase in employment. Given that mean pre-treatment employment levels for high-skilled mothers are so high to

begin with (72.5%), this is a substantive effect, although it is only around one fourth of the 24 percentage point increase in full-day enrollment response among children of high-skilled mothers in Table 2. This signals that for high-skilled mother the substitution effect is quite substantial. For married mothers there is a slightly negative employment effect, a 3.3 percentage point decrease. For married mothers, with access to other sources of household income, the income effect can be greater than the substitution effect, which explains this decline. For single mothers however, I find the largest employment response, an increase of 16.7 percentage points. Again, this was expected as indicated by previous research on the topic (Cascio, 2009). In comparison, there was a 38.3 percentage point increase in full-day enrollment response among children of single mothers in Table 2, almost twice the effect size of employment. Mean treatment employment level among single mothers was already at 77.6%, which signals that substitution effect dominates income effect for the case of single mothers. For mothers of non-White origin, there is a positive employment effect of 8.2 percentage points, while the mean pre-treatment employment levels were around 62%. For mothers of White origin, there is no employment effect, while the mean pre-treatment employment levels were around 70%. Given that there was no effect on full-day kindergarten enrollments across children of non-White origin, and there was a large, positive effect (17.9 percentage points) on full-day kindergarten enrollments across children of White origin in Table 2, these estimates are a bit surprising. For mothers of White origin, the already high employment levels and likelihood of higher income effects can explain the absence of any employment effect. The positive treatment effect for employment among non-White mothers is more puzzling. In the analysis sample, around 38 percent of non-White mothers are single, while only 22.5 percent mothers of White origin are single. Given that single mothers are more likely to experience a higher substitution effect, this could be driving the positive employment effect among non-White mothers. This argument is more accurate for the case of low-income mothers. While the employment effect for low-income mothers is a statistically significant increase of 4.2 percentage points (from a mean pre-treatment employment level of about 60%), the employment effect for high-income mothers is more modest, a statistically insignificant increase of 2.6 percentage points. But then again, in our sample only 10 percent of high income mothers are single, but among low-income mothers, 40 percent are single.

Difference-in-differences estimates for weekly number of hours worked, i.e. the intensive margin employment estimates are presented in Table 4. Overall, I find a small but statistically insignificant impact on number of hours worked, the point estimate showing an increase of 0.83 hours per week over a baseline of 23.32 mean number of

hours worked per week. Only subgroups such as single mothers or those mothers who are non-White, show an increase in number of hours worked per week. Specifically, for single mothers, there is a 4.2 units (hours) increase in the number of weekly hours, and for non-White mothers there is a 1.9 units (hours) increase in weekly hours. These estimates are also expected. In a majority of cases, labor force participants can presumably choose whether to work or not, but only a few have control over the number of hours that they work. This explains why the intensive margin estimates are more modest as compared to the employment estimates.

5.1 Triple-difference estimates

Triple-difference estimates for employment and weekly number of hours are presented in Table 5 and Table 6 respectively. In Table 5, the coefficients of interest on the triple interaction capture the difference across the treatment effect for sample population during the school-year months (Sept-May) and the treatment effect for sample population during the summer months (June-July). The triple-difference estimate would thus depend on the sensitivity levels of the treatment effects with respect to the summer months, when schools are closed.

The overall maternal employment estimate in column 2 of Table 5 shows a 5.1 percentage point increase in employment. This goes on to show that for the overall sample, the estimate in column 2 of Table 3 is almost exclusively driven by a strong employment effect in the months when school is in session. This also lends strong credibility to the fact that increases in employment among mothers of 5- year-olds are indeed linked with the offering of full-day kindergarten in the state.

For most sub-populations, the triple-difference employment estimates in Table 5 are positive and statistically significant for the following subgroups of mothers: (i) high-skilled mothers, (ii) married mothers, (iii) non-White mothers, (iv) White mothers, (v) low-income mothers and (vi) high-income mothers, the effect size ranging from 3.3 to 8.2 percentage points. The positive triple-difference estimates for high-skilled, non-white and low-income mothers are in line with the double differences estimates in Table 3, and somewhat expected, giving my identification assumption that these employment estimates are indeed driven by FDK enrollment, which is only possible during the months when school-year is in session.

The estimates that examine heterogeneous effects across single mothers and married mothers were the most surprising. It appears that married mothers had a strong income effect during the school year, and it dominated

the substitution effect. During the summer months, the substitution effect goes away, but because the summer months in this setting is just a very short window, married mothers do not realize the existing strong income effect due to reduced child care expenses has also gone away. Even if they realize it, they might not be able to get back into employment just for 3 months of private child care costs. For single mothers, it appears that the substitution effect is so dominant that a short window of summer months does not motivate them to leave employment. I run the difference-in-difference regression (similar to equation 2) for single as well as married mothers separately over summer and school-year months to test this, and found this to be correct. The point estimates for single mothers are exactly identical for summer and school year months, but not so for married mothers. For high-income mothers, the positive triple-difference estimates are also somewhat unexpected. For the case of high-income mothers, the point estimate in Table 3 was positive though borderline insignificant, which can explain why the treatment effects could have been stronger in school-year months, and could have been weaker in the summer months, which would make the triple-difference estimate positive and statistically significant.

Triple-difference estimates on the intensive margin are presented in Table 6. Analogous to the estimates on the extensive margin, estimates in column (1) and column (2) for weekly hours are statistically significant and almost double in magnitude as compared to the baseline specifications in Table 4. Column 2 of Table 6 shows a 2.1 hours increase in weekly hours worked. Across all sub-populations of analysis, the triple-difference intensive margin estimates are positive and statistically significant, ranging from a 1 hours increase to a 4.15 hours increase in weekly hours. The highest increase in weekly hours are seen for the following, separate subgroups of mothers: (i) single mothers, (ii) non-White mothers and, (iii) high-income mothers. The estimates for the subgroup of single mothers and the subgroup of non-White mothers are inline with the baseline estimates in Table 4. triple-difference estimates stronger for cases when the double differences estimates were statistically insignificant in Table 4. Once again, this lends further credibility to the identification assumption that increases in maternal employment are indeed driven by the full-day intervention.

5.2 Synthetic Control Method

Using synthetic control method, I construct a synthetic Arizona¹¹ for the overall employment outcome for mothers of 5-year-olds. The employment trend across Arizona and synthetic Arizona is plotted in panel A of Figure 6. The trends overlap each other during the pre-treatment but post treatment there is a persistent 1 to 2 percentage point difference across Arizona and synthetic Arizona. This estimate is quite close to the 2.5 percentage point increase in employment (column 2, Table 3).

To provide further robustness to the synthetic control, I run placebo tests by assigning treatment to the six neighboring states of Arizona which include California, Nevada, Utah, Colorado, New Mexico and Texas. The treatment effects are plotted in panel B of Figure 6. The black line presents the state of Arizona and the six gray lines present placebo treatment states. While many placebo treatments show some increase post the treatment year, it is only for the case of Arizona that the treatment effect remains positive and consistent over time from 2005-2010.

6 Conclusion

My estimates confirm that full-day kindergarten enrollment increased as a response to the intervention, signalling treatment compliance, although the estimates for compliance are heterogeneous across different sub-populations. The first finding is that for the overall sample as well as sub-populations of single or high-skilled mothers, increases in full-day kindergarten enrollment map onto increases in employment, although the magnitudes of treatment effects for employment are considerably smaller. This finding is consistent with the observation that Gibbs (2014) makes in her study on full-day kindergarten impacts in Indiana.

Difference-in-differences estimates for the intervention period are not surprising. The most relevant work in this regard includes that of Cascio (2009) and Gibbs (2014). On the extensive margin, I find positive employment estimates for the following subgroups: (i) single mothers, (ii) high-skill mothers, (iii) non-White mothers, and (iv) low-income mothers. For single mothers, Cascio (2009) also finds the highest treatment effects, although she does not study skill, income or ethnicity-based heterogeneity. Indeed Cascio (2009) finds negative but statistically insignificant negative treatment effects for married mothers, while the negative employment estimate for married

¹¹The constituent control states and corresponding weights are available with me.

mothers in my study is statistically significant. On the intensive margin of employment, I find positive treatment effects for weekly hours for the subgroup of single mothers and the subgroup of non-white mothers, [Cascio \(2009\)](#) finds the same for single mothers. Similarly, [Dhuey et al. \(2020\)](#) finds positive treatment effects for single mothers, both on the extensive and intensive margin on employment. Although my estimates resonate more closely with what [Cascio \(2009\)](#) finds, the results are not much different from [Gibbs \(2014\)](#).

In sub-populations for which positive treatment effects are found, the extensive margin estimates across sub-population range from an increase of 4.2 to 16.7 percentage points, which are quite a big effect in the context of (i) existing higher levels of employment amongst these subgroups and (ii) these are lower-bound intent-to-treat estimates. Also, the increase in availability of full-time kindergarten occurs only on the intensive margin $\hat{\alpha}$ from half-day to full-day kindergarten $\hat{\alpha}$ which adds a mere two or so hours of free kindergarten each day. However, this is not surprising as these groups are more likely to experience dominant substitution effect from the availability of kindergarten. A slightly unexpected estimate is the employment estimate for the overall sample, which is a smaller, yet statistically significant positive effect of 2.5 percentage points. Although this impact is not totally surprising, this effect is not that small in the context of a small increase in the availability of full-day kindergarten.

The triple differences framework in this paper is an innovative use of the school-year cycle, and helped me test my identification assumption. For the majority of the employment estimates on the extensive as well as the intensive margin, the triple estimates lend credibility to the argument that maternal employment does respond to the availability of full-day kindergarten. Finally, synthetic control method also confirms the initial findings from the baseline double differences estimations.

This paper was an attempt to identify if there are any maternal employment effects of full-day kindergarten availability in Arizona during 2005-2010. I present robust evidence that there are indeed positive treatment effects both on the extensive as well as intensive margins of employment, although these are driven by certain sub-populations, such as mothers who are either single, or those who are non-White or those who are low-skilled. Most of these findings are consistent with the existing literature. I add to the literature by providing Arizona based evidence to the full-day kindergarten literature on employment effects. In addition, I pay attention to heterogeneity across skill, income, and ethnicity groups, which is relatively new in the context of this body of literature. The triple differences strategy is novel and innovative and strengthens the credibility of estimates from my paper as well

as previous literature. I provide further evidence of robustness through a limited application of the synthetic control method.

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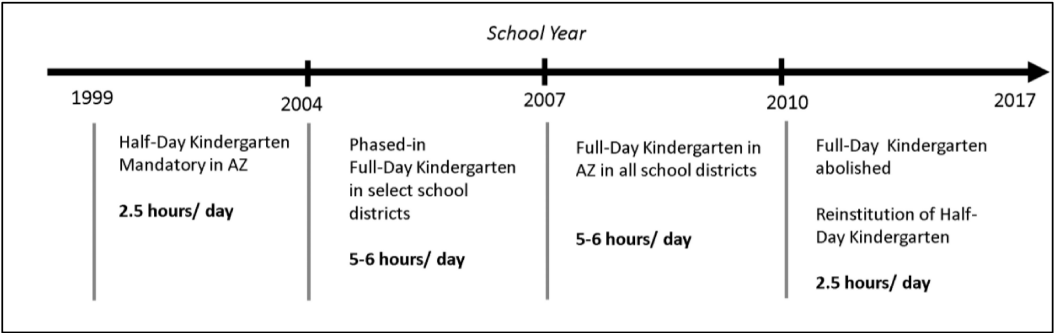
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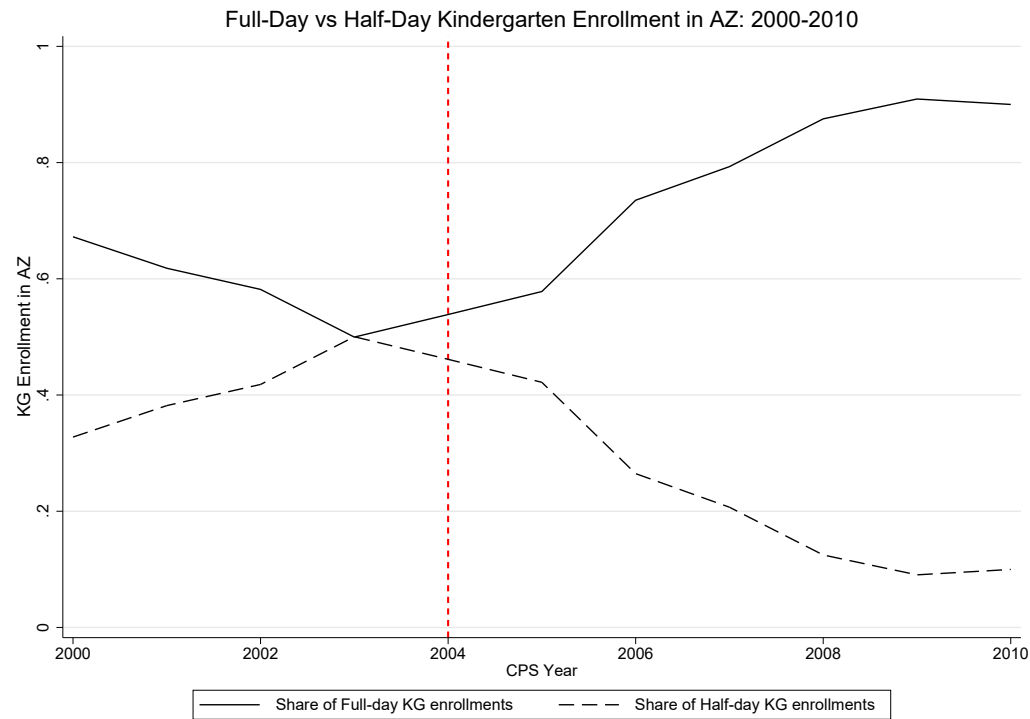
Figures and Tables

Figure 1: Timeline of Kindergarten Expansion in Arizona

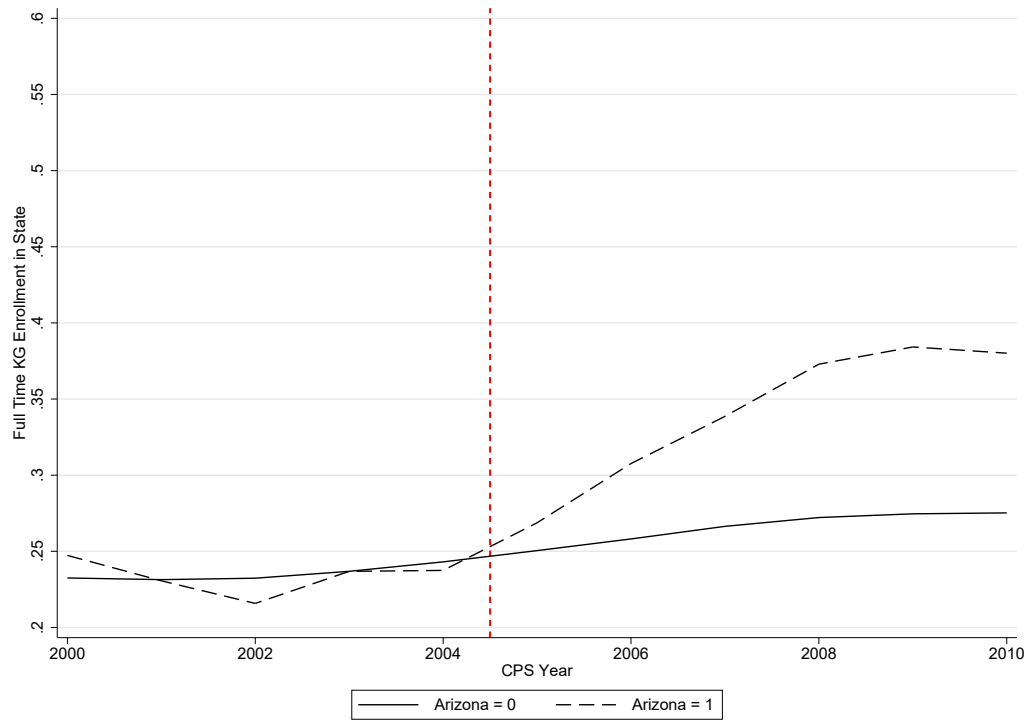


Source: Libassi (2014)

Figure 2: Enrollment Impacts of Kindergarten Expansion in Arizona

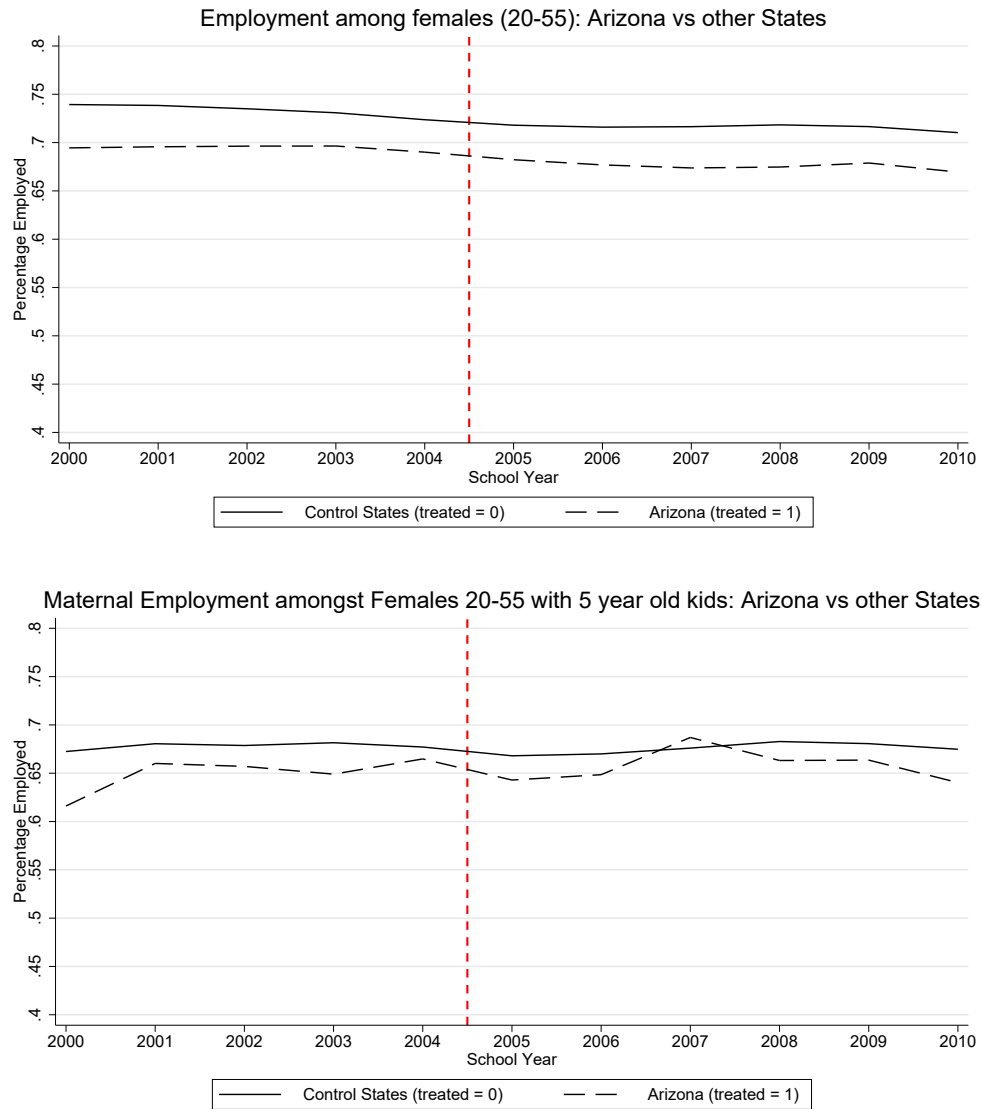


Source: Flood et al. (2021)

Figure 3: Full-day Kindergarten Enrollment

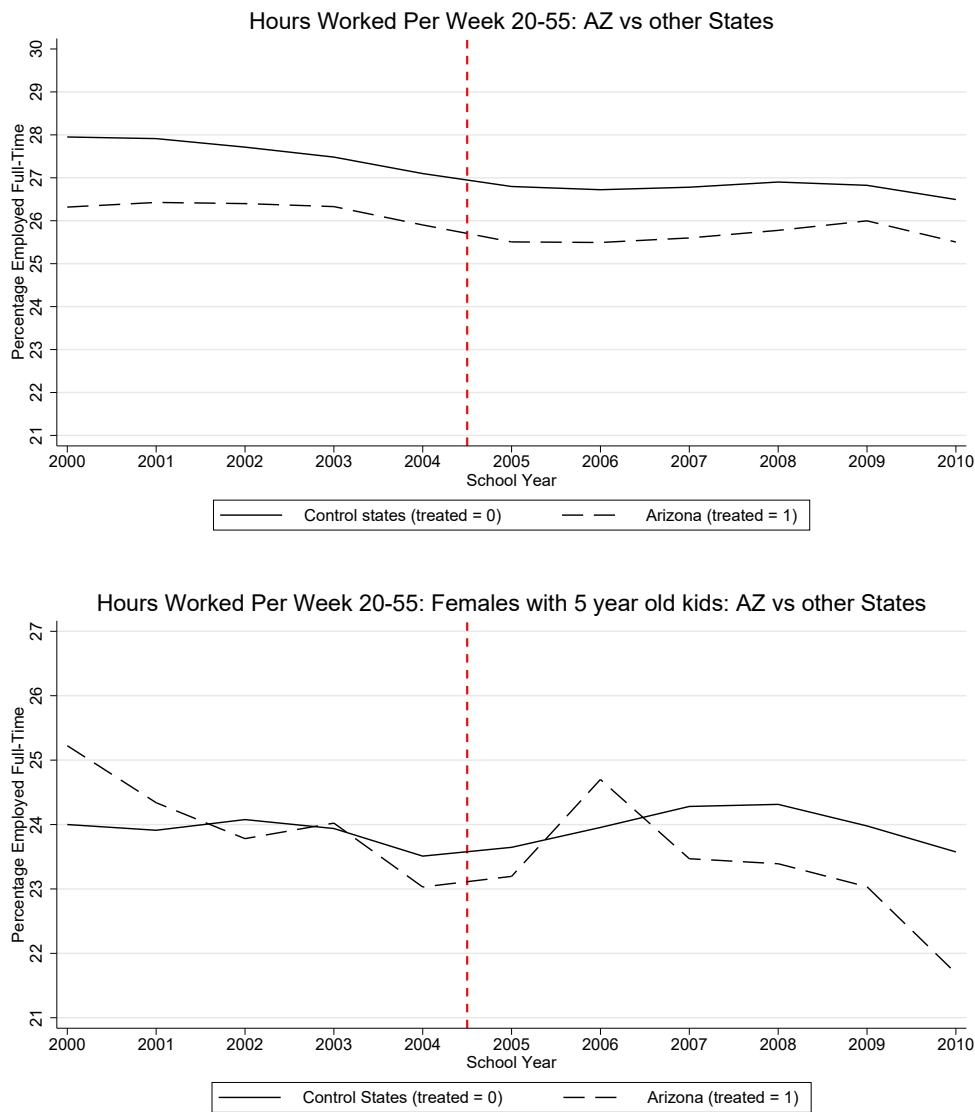
Source: Flood et al. (2021)

Figure 4: Employment among Females: Arizona vs Other States



Source: Flood et al. (2021)

Figure 5: Weekly Hours Worked among Females: Arizona vs Other States



Source: Flood et al. (2021)

Table 1: State Mandated Levels of Kindergarten Offered

State	Level of Kindergarten	
	2000	2010
Alabama	Full-day	Full-day
Alaska	–	–
Arizona	Half-day	Full-day
Arkansas	Full-day	Full-day
California	Half-day	Half-day
Colorado	–	–
Connecticut	Half-day	Half-day
Delaware	Half-day	Half-day
District of Columbia	–	Half-day
Florida	Full-day	Half-day
Georgia	Full-day	Full-day
Hawaii	Full-day	Half-day
Idaho	–	–
Illinois	Half-day	Half-day
Indiana	Half-day	Half-day
Iowa	Half-day	Half-day
Kansas	–	Half-day
Kentucky	Half-day	Half-day
Louisiana	Full-day	Full-day
Maine	–	Half-day
Maryland	Half-day	Full-day
Massachusetts	Half-day	Half-day
Michigan	–	–
Minnesota	Half-day	Half-day
Mississippi	Full-day	Full-day
Missouri	Half-day	Half-day
Montana	Half-day	Half-day
Nebraska	Half-day	Half-day
Nevada	Half-day	Half-day
New Hampshire	–	–
New Jersey	–	–
New Mexico	Half-day	Half-day
New York	–	–
North Carolina	Full-day	Full-day
North Dakota	Full-day	–
Ohio	Half-day	Half-day
Oklahoma	Half-day	Half-day
Oregon	Half-day	Half-day
Pennsylvania	Half-day	–
Rhode Island	Half-day	Half-day
South Carolina	Full-day	Full-day
South Dakota	Full-day	Half-day
Tennessee	Half-day	Half-day
Texas	Full-day	Half-day
Utah	Half-day	Half-day
Vermont	Full-day	Half-day
Virginia	Full-day	Half-day
Washington	–	Half-day
West Virginia	Full-day	Full-day
Wisconsin	Half-day	Half-day
Wyoming	Half-day	Half-day

Source.— [National Center for Education Statistics \(2022\)](#)

Table 2: DD Estimates for Full Day Kindergarten Enrollment in 5 year olds

	Dep Var: Full Day Kindergarten Enrollment=1									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	OLS+	LS	HS	Married	Single	Non-White	White	LowInc	HighInc
AZ \times FDK intervention	0.140*** (0.019)	0.084*** (0.020)	-0.070** (0.034)	0.240*** (0.022)	-0.002 (0.024)	0.383*** (0.039)	-0.029 (0.048)	0.179*** (0.027)	-0.022 (0.034)	0.268*** (0.031)
Mean Pretreatment Enrollment	0.205	0.205	0.247	0.158	0.212	0.187	0.214	0.192	0.234	0.151
Observations	13994	13994	5586	8408	10472	3522	5084	8910	8252	5742
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey October Supplement (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with difference-in-differences regressions of full day kindergarten enrollment on a binary indicator interacting an indicator for the state of Arizona with an indicator for the years in which full day kindergarten was available in Arizona. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. All regressions include state and year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 3: DD Employment Estimates for Mothers of 5 year olds

	Dep Var: Mother is employed == 1									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ × FDK intervention	0.044*** (0.013)	0.025** (0.011)	0.001 (0.030)	0.058*** (0.016)	-0.033** (0.015)	0.167*** (0.028)	0.082*** (0.019)	-0.019 (0.017)	0.042** (0.020)	0.026 (0.017)
Mean Pretreatment Employment	0.659	0.659	0.587	0.725	0.617	0.776	0.619	0.696	0.604	0.744
Observations	66698	66698	27303	39395	48078	18620	22599	44099	38867	27831
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey ([Flood et al., 2021](#))

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with difference-in-differences regressions of employment on a binary indicator interacting an indicator for the state of Arizona with an indicator for the years in which full day kindergarten was available in Arizona. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 4: DD Estimates for Mothers of 5 year olds: Weekly Hours

	Dep Var: Weekly Hours Worked									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ \times FDK intervention	1.729** (0.647)	0.832 (0.509)	1.733 (1.245)	0.615 (0.723)	-0.627 (0.652)	4.274*** (1.283)	1.953** (0.771)	0.159 (0.829)	1.315 (0.854)	1.553 (0.990)
Mean Pretreatment Hours	23.32	23.32	20.53	25.96	20.79	30.25	23.21	23.43	21.05	26.87
Observations	63012	63012	25938	37074	45427	17585	21685	41327	36705	26307
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with difference-in-differences regressions of weekly hours on a binary indicator interacting an indicator for the state of Arizona with an indicator for the years in which full day kindergarten was available in Arizona. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 5: DDD Employment Estimates for Mothers of 5 year olds

	Dep Var: Mother is employed == 1									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ × School-Year Months	-0.045*** (0.009)	-0.028*** (0.007)	-0.028* (0.014)	-0.036*** (0.013)	-0.030*** (0.007)	-0.049*** (0.015)	-0.021 (0.014)	-0.028** (0.011)	-0.043*** (0.010)	0.009 (0.010)
FDK × School-Year Months	0.004 (0.012)	0.007 (0.011)	-0.013 (0.022)	0.016 (0.016)	0.007 (0.013)	0.008 (0.016)	0.000 (0.027)	0.011 (0.012)	0.014 (0.015)	-0.002 (0.013)
FDK × AZ	-0.019 (0.013)	0.003 (0.015)	0.017 (0.024)	-0.003 (0.018)	-0.052** (0.022)	0.173*** (0.029)	0.045 (0.028)	-0.032 (0.023)	0.062** (0.025)	-0.068*** (0.025)
FDK × AZ × School-Year Months	0.088*** (0.012)	0.051*** (0.009)	0.031 (0.024)	0.082*** (0.016)	0.058*** (0.012)	0.021 (0.018)	0.056*** (0.019)	0.045*** (0.013)	0.033*** (0.013)	0.056*** (0.017)
Mean Pretreatment Employment	0.659	0.659	0.587	0.725	0.617	0.776	0.619	0.696	0.604	0.744
Observations	81170	81170	32610	48560	58487	22683	27670	53500	47255	33915
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with triple differences regressions of employment on a indicator which is a three-way interaction between: (i) an indicator for the years in which full day kindergarten was in operation, (ii) an indicator for the state of Arizona, and (iii) an indicator for the months of regular school year Sept to May, conditional on demographic controls, and a combination of fixed effects. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 6: DDD Estimates for Mothers of 5 year olds: Weekly Hours

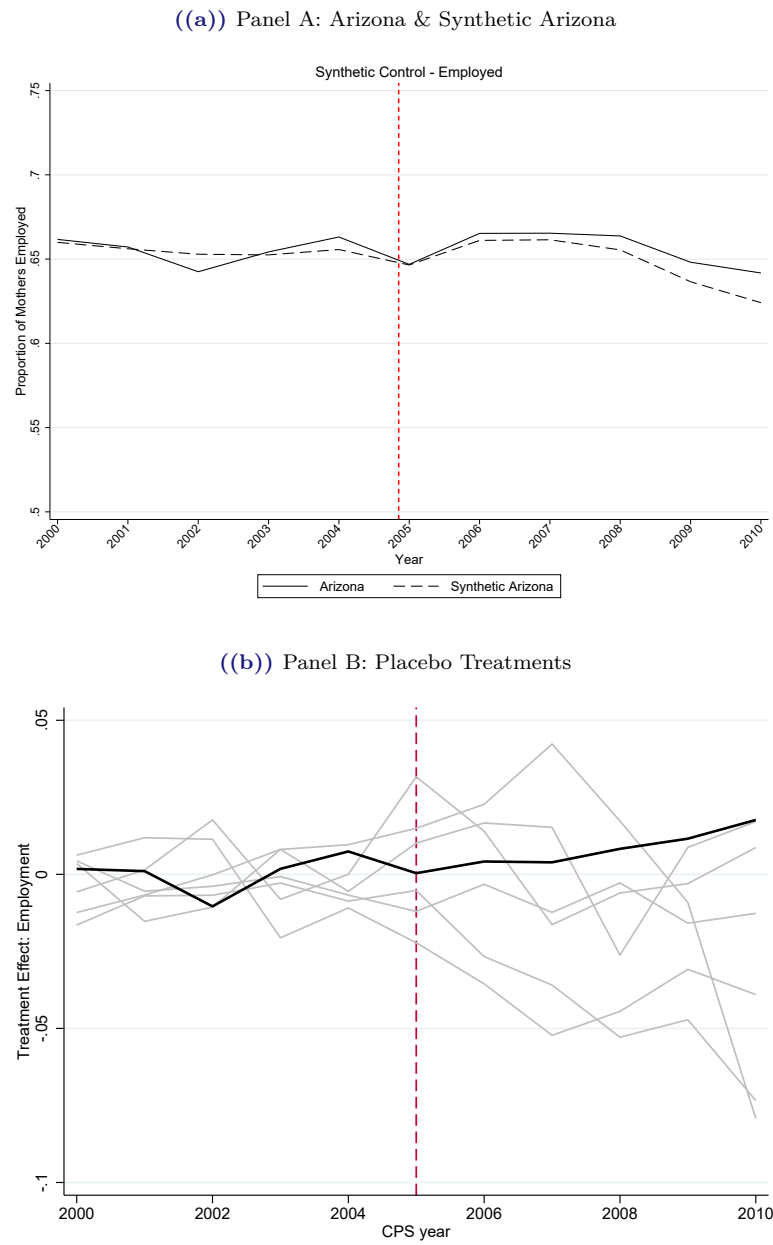
	Dep Var: Weekly Hours Worked by Mother									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ \times School-Year Months	-0.314 (0.359)	-0.049 (0.282)	-0.431 (0.627)	0.020 (0.465)	0.259 (0.318)	-1.884*** (0.610)	-2.038*** (0.658)	1.195** (0.448)	-0.312 (0.372)	0.529 (0.470)
FDK \times School-Year Months	0.243 (0.564)	0.449 (0.519)	-0.918 (0.864)	1.297* (0.644)	0.511 (0.565)	0.313 (0.778)	-0.211 (1.109)	0.818 (0.591)	0.482 (0.633)	0.388 (0.592)
FDK \times AZ	-0.380 (0.466)	0.003 (0.534)	1.766** (0.793)	-1.035 (0.689)	-0.667 (0.839)	2.373* (1.366)	-0.837 (1.028)	-0.056 (0.877)	2.292** (0.883)	-2.752** (1.347)
FDK \times AZ \times School-Year Months	3.398*** (0.571)	2.110*** (0.459)	1.822* (1.006)	2.816*** (0.682)	1.246** (0.521)	3.945*** (0.815)	4.152*** (0.876)	1.190* (0.618)	1.015* (0.567)	3.001*** (0.790)
Mean Pretreatment Hours	23.32	23.32	20.53	25.96	20.79	30.25	23.21	23.43	21.05	26.87
Observations	76671	76671	30931	45740	55239	21432	26506	50165	44633	32038
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with triple differences regressions of weekly hours worked on a indicator which is a three-way interaction between: (i) an indicator for the years in which full day kindergarten was in operation, (ii) an indicator for the state of Arizona, and (iii) an indicator for the months of regular school year Sept to May, conditional on demographic controls, and a combination of fixed effects. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Figure 6: Employment of Mothers with youngest child aged 5: Synthetic Control Method



Source: Current Population Survey (Flood et al., 2021)