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

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Abstract

This paper studies the impact on maternal employment of Arizona's expansion of full-day kindergarten between 2004 and 2010. I hypothesize that the expansion of public kindergarten acts as an implicit child care subsidy by reducing the number of hours of paid child care required outside of school. Relying on monthly data from the Current Population Survey (CPS) between 2000 and 2010, this paper uses difference-in-differences and triple differences models that exploit both between state/over time and within state seasonal variation in exposure to Arizona's kindergarten expansion. I find that full-day kindergarten led to an increase in extensive margin employment among mothers with age-eligible children by 2.5 percentage points. However, this increase is heterogeneous and is driven by single and non-white mothers, amounting to increases of 16.7 and 8.2 percentage points, respectively. On the intensive margin, I find a similar pattern of heterogeneous changes in employment. Specifically, weekly hours worked rose by 4.2 hours for single mothers and 1.9 hours for non-white mothers. This subgroup analysis suggests that the benefits of full-day kindergarten expansion were pronounced among populations with pre-existing child care constraints, contributing to a more equitable distribution of labor market gains.

Keywords: Kindergarten; Maternal Employment; Early Childhood Education; Education Economics

JEL Codes: H04, I26, J02 J08

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 kindergarten provision mandated by each state. For instance, while some states, such as West Virginia and Oklahoma, mandate full-day kindergarten financed by public or state funds[1], others, like New Hampshire and Nebraska, require only half-day kindergarten (Parker et al., 2016). Additionally, some states including Iowa, New Jersey and Wyoming do not mandate any level of publicly funded kindergarten (Parker et al., 2016). 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 200405 and 2009-10 (Figure 1). Libassi notes that Arizona legislators introduced bills mandating the provision of full-day kindergarten in the poorest school districts, based on the percentage of students 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 kindergarten[2]. In March 2010, the new administration abolished funding for full-day kindergarten for the 2010-11 school year (Libassi, 2014). 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.

Existing literature has found positive impacts of full-day kindergarten provision on maternal employment (Cannon et al., 2006; Cascio, 2009; Gibbs, 2014), although many studies find that stronger positive impacts in

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

[2] For school year 2006-07, the then Arizona governor Napolitano changed 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.

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 options, it is likely that this full-day program will 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. Besides Arizona, several other states changed the mandatory length of kindergarten days between 2000 and 2010. As shown 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, the compliance rate for take-up of full-day kindergarten among families of five year old children cannot be assumed. 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 CPS[3] October supplements from 2000 to 2010[4]. 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 May[5] 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 augment 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 difference[6] 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.

Existing studies such as those by Cascio (2009) and Gibbs (2014) suggest that the overall impact of full-day kindergarten on maternal employment is small. However, this impact is more pronounced for specific groups such

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

[4]Post 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.

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

[6]As 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.

as single and low-skilled mothers, where substitution effect outweighs the income effect for maternal employment. Consistent with this literature, a similar hypothesis applies to the Arizona kindergarten expansion, particularly given that certain features of the Arizona experiment make this hypothesis more plausible. 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 compliance[7]. Thus, these estimates likely represent 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, such as single mothers or those with low income or lower skill levels, a child care subsidy in the form of free full-day kindergarten would likely increase the opportunity cost of not working. This means that these mothers are more likely to experience greater positive impacts on employment due to the availability of subsidized child care.

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.

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[7]While 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 Ribarâs and Connellyâs 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 and 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 only slightly higher than hourly child care prices, the mother is not likely to participate in the labor force unless the realized quality of child care increases indirect utility[8].

The utility derived 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 explained 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 becomes available in the form of public school, subgroups of mothers who are either single or low-skilled,

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[8]Such as for example, the utility from child development

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 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.

Research on the impact of publicly funded, full-day kindergarten on maternal employment for age-eligible children includes studies by Cannon et al. (2006), Cascio (2009), Dhuey et al. (2021), and Gibbs (2014). All these studies find some impact of full-day kindergarten on maternal labor supply, though they report varying effects across 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 provides an implicit child care subsidy for age-eligible children. Both theoretical considerations and the existing literature suggest significant substitution effects, particularly among subgroups such as low-skilled or single mothers, who show increased employment responses. In contrast, the substitution effect for other groups, such as married mothers or those with specific skill levels, appears to be much weaker according to current research.

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 instance, the data for the 1999-2000 school year (or 2000) corresponds to CPS months from September 1999 to May 2000. This allows for a more reliable estimate of enrollment changes for identification purposes.

For the maternal labor supply analysis, the sample includes all mothers aged 20 to 55 with a youngest child who is 5 years old. In Arizona, a child must be 5 years old by September 1 of the respective school year to be eligible for kindergarten enrollment. Therefore, only mothers with age-eligible children are included in the analysis. To reliably isolate the treatment effects, I exclude mothers of 5-year-olds who also have younger children, as their employment decisions might be influenced by the presence of these younger children. Additionally, mothers currently enrolled in any form of schooling are excluded. Arizona began offering full-day kindergarten in the 2005 school year, providing five pre-treatment years for baseline comparison.The treatment period spans from the 2005 to 2010 school years, which are treated as a unified treatment era to ensure a sufficient sample size of mothers with a youngest child aged 5.

Starting in the 2011 school year, Arizona abolished state funding for full-day kindergarten. However, many local school districts continued to offer full-day kindergarten using secondary funding sources, such as local property taxes. This led to an era of uncertain and muddled policy environments, making it difficult to isolate the effects of one policy from another within the current difference-in-differences framework. Therefore, I conclude my study with the 2009-10 school year.

3.1   Full-Day Kindergarten Enrollment

One key challenge with using CPS data is that only the CPS-October education supplement differentiates between full-day and half-day kindergarten enrollment. This information is not available for other months in the CPS. For the analysis of maternal employment, I include all mothers whose youngest child is five years old, regardless of compliance with full-day kindergarten enrollment. However, a preliminary analysis of kindergarten enrollment based on CPS-October supplement data is necessary before analyzing maternal employment. This preliminary analysis will ensure that a substantial proportion of five-year-olds in Arizona complied with the intervention by enrolling in full-day kindergarten.

In Figure 2, I use data extracted from the CPS-October education supplement to analyze how full-day vs.

half-day kindergarten enrollment changes over the years, particularly during the intervention years of 2005-2010 for this study. For each year in the observation, I use a moving average of two preceding years and the current CPS year, to smooth out the curve. This is necessary because the number of observations within a single year of data from Arizona is too few, and the resulting volatility induced by a smaller sample size makes it harder to interpret. The vertical dotted line in Figure 2 indicates the beginning year of the intervention. As expected, full-day kindergarten enrollment increases from 45% to close to 90% during the intervention years. Half-day kindergarten enrollment mirrors the trend, showing 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 responded by enrolling their children in full-day kindergarten. Figure 3 graphs these trends for full-day kindergarten enrollments in Arizona as compared to the control states during 2000-2010. Before the intervention, full-day kindergarten enrollment in Arizona was around 20-25%, comparable to the national average across the 37 control states. Post-intervention, full-day kindergarten enrollment in Arizona gradually increased, peaking around 2009 when 33% of children (age 4 or age 5) attended full-day kindergarten. After 2005, the increase in full-day kindergarten attendance in Arizona is remarkably higher than in the control states, but in the years before the treatment, trends in Arizona and the control states overlap and exhibit similar patterns.

To ensure that the observed enrollment trends in Arizona are indeed driven by the intervention and not by any national or other region-specific trends in education, I run a difference-in-differences (DD) specification for full-day kindergarten enrollment amongst five-year-olds. This analysis uses data from the October CPS Education supplement for the study period 2000-2010. Specifically, I estimate a regression of the following form:

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

Where $Y_{i,s,t}$ is the key outcome variable, a binary indicator which represents whether the five-year-old i in state s in year t is enrolled in full-day kindergarten or not, AZ is a binary indicator that equals 1 if individual i is located

in the state of Arizona, and 0 otherwise. FDK is a binary indicator that equals 1 if the observation year t falls within the treatment period (2005 to 2010), and 0 otherwise. The key coefficient of interest lies on the interaction between between AZ_s and FDK_t, which captures the differential impact of the full-day kindergarten policy on enrollment in Arizona compared to other states during the treatment period. The model also controls for a matrix of maternal and family characteristics $X'_{i,s,t}$, including age, race, education levels, marital status, family size, and income. State fixed effects (θ_s) account for time-invariant characteristics of each state, while year fixed effects (ζ_t) control for macroeconomic and policy shocks that affect all states in a given year. 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 show that the intervention led to an increase in full-day kindergarten enrollment in Arizona by 8.4 percentage points during 2005-2010. The estimates however exhibit a great degree of heterogeneity across different sub-populations. Large increases in 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). In contrast, the effects on full-day kindergarten enrollment are either negligible or negative for children of low-skilled, married, non-White, or low-income mothers.

Children of high-skilled, high-income mothers have traditionally been more likely to attend nursery schools and child care (Davis and Bauman, 2013; Baum, 2002). Previous literature also links the availability of full-day kindergarten with increased maternal employment, particularly among single mothers (Cascio, 2009; Dhuey et al., 2020). According to Davis and Bauman (2013), 25% of children enrolled in full-day kindergarten across the U.S. came from households with incomes above $75,000. These trends help explain why high-skilled, high-income, and single mothers show significant increases in full-day kindergarten enrollment. For these groups, the expected substitution effects from increased maternal employment are likely strong enough to drive enrollment in full-day kindergarten. Additionally, high-income households, which are more likely to use child care in the absence of kindergarten (Anderson and Levine, 1999), may shift their children from child care to full-day kindergarten. Differences in full-day

kindergarten consumption between White and non-White mothers can also be attributed to income disparities among these demographics. Overall, while there is substantial evidence of increased full-day kindergarten enrollment during the treatment years, the impact varies significantly across different sub-populations.

3.3   Measures of Maternal Labor Supply

There are two key outcome measures of maternal labor supply. The first measure is employment status, derived from the CPS dataset. This is a binary indicator variable coded as 1 if a mother is either employed during the week she was interviewed or is on granted leave. All other individuals, including those out of work, not in the labor force, retired, or unable to work, are coded as 0.

Figure 4 presents the trends in mean female employment from 2000 to 2010 across Arizona and other states. 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 includes all females aged 20-55, regardless of maternal characteristics. This broader sample size in Arizona allows for more stable plotting. The graph shows that female employment trends in Arizona closely mirror the national average, remaining consistent throughout the study period, although it remains systematically below the national average. The lower part of the graph focuses on mothers aged 20-55 with a youngest child aged 5. The trends indicate that, on average, female employment among this sample of mothers follows the national trend. However, due to the small number of mothers with a youngest child aged 5 in each year of the CPS sample, the trend for Arizona is more volatile[9]. As expected, employment among mothers with five-year-old children is 5-10% lower than the overall female employment rate, but the trends remain similar. Despite some volatility in the lower part of Figure 4, there is no obvious violation of parallel trends.

As noted earlier, the intervention only adds 2.5 hours to the existing half-day kindergarten program. Thus, if full-day kindergarten expansion does affect female employment on the extensive margin, the magnitude of the estimate is likely to be small. To explore the intensive margin of labor supply, I examine the weekly number of hours worked by mothers with age-eligible children. This measure, derived from CPS responses about usual weekly hours worked, is a continuous variable. Figure 5 compares the mean number of hours worked by females in Arizona with those in other states.The upper part of the graph includes all females aged 20-55, while the lower part focuses

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[9]To mitigate this issue, a three-year moving average is used for plotting purposes.

on mothers with a youngest child aged 5. As before, the smaller sample size for mothers with a five-year-old makes the trend for Arizona more volatile in this subset, but the trend for all females in Arizona mirrors the national average. For mothers with five-year-olds, the trends are more stable in the three years immediately preceding the intervention. Similar to trends observed on the extensive margin of labor supply, women in Arizona consistently show lower labor supply on the intensive margin compared to the national average. Notably, 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.

In addition to the main variables, a matrix of control variables is constructed from the CPS data. This matrix includes information on race, marital status, education, income, and the total number of children for all individuals in the sample. Yearly state-level unemployment data is obtained from the Bureau of Labor Statistics.

4 Empirical Strategy

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

The primary outcomes of interest are maternal employment on both the extensive and intensive margins of labor force participation. I run a slightly modified form of the specification (1) in the following forms:

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

$Y_{i,s,t}$ 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 that equals 1 if individual i is located in the state of Arizona, and 0 otherwise. FDK is a binary indicator denoting if the observation year falls 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. The model controls for a matrix of maternal and family characteristics X', which includes variables such as age, race, education levels, marital status, family size, and income. Z' accounts for time-variant state-level controls, specifically unemployment rates. State fixed effects (θ) adjust for time-invariant characteristics unique to

each state, and year fixed effects (ζ) account for year-specific macroeconomic and policy shocks affecting all states. To address seasonal variations in employment, the model includes month fixed effects (σ). To mitigate potential threats to identification from unobserved variables that may vary across states over time, η captures state-specific linear time trends.

One key difference between specification 1 and 2 is that the latter utilizes the the entire sample, not just the October-CPS. For this specification, I am combining CPS months from September to May, aligning with each school year. Although this method of matching CPS monthly data to the school year is somewhat novel, it is not unprecedented 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 earlier, the analysis incorporates several key elements to ensure robust results. Each specification includes state-specific linear time trends to control for any changes in labor supply that vary over time within states. Additionally, a matrix of maternal socio-demographic characteristics is included to account for individual-level factors that could influence employment outcomes. To address seasonal fluctuations in employment, month fixed effects are added, capturing variations that occur throughout the year. The sample is carefully defined to include only mothers aged 20 to 55 with a youngest child aged 5, excluding those with younger children to focus specifically on the impact of full-day kindergarten on this group. To further enhance the accuracy of the results, standard errors are clustered within state-specific cells, which helps to account for correlations in employment trends within states.

The identification strategy used in this study has several limitations. A significant challenge is the inability to trace each age-eligible child and their mother to estimate Treatment-on-the-Treated (ToT) effects for maternal employment directly. Despite this, the evidence of program uptake presented earlier provides strong support for the effectiveness of the intervention, thereby mitigating some concerns about treatment compliance. While data limitations restrict the analysis to lower-bound Intention-to-Treat (ITT) estimates, the robust evidence of program uptake suggests that the actual ToT effects are unlikely to differ substantially from these estimates.

There are two key threats to identification in this study. The first threat relates to the assumption of parallel trends, which posits that in the absence of the treatment, employment trends in Arizona would have been similar

to those in other states. A potential violation of this assumption could undermine the validity of the findings. The second threat arises from the possibility 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 section 2 provides graphical evidence to support the assumption of parallel trends, and the analysis incorporates a range of controls —including demographic characteristics, state-level unemployment rates, two-way fixed effects, and state-specific linear time trends —these controls may not fully eliminate all potential threats to identification. Thus, while the strategy aims to isolate the impact of the kindergarten expansion, some residual threats to identification could still be present.

Two strategies are frequently used to alleviate identification concerns. The first potential method is to use a triple-difference estimator. While a difference-in-differences approach requires the assumption that the treatment and control groups would have followed parallel trends in the absence of the intervention, a triple-difference estimator only requires that the parallel trends assumption holds within the subgroups used for the third difference Olden and Møen (2022). Moreover, a triple difference estimator can isolate treatment effects more precisely by accounting for unobserved confounding factors that may vary over time and across groups. Furthermore, by comparing changes over time within subgroups, a triple difference estimator can help mitigate bias from time-varying unobservables that might differentially affect the groups being studied.

A simple triple-difference design with mothers of 4 year or 6 year old children as a comparison group would have been ideal, if the time period of the intervention was a little brief. However, the structure of the data and the duration of the Arizona kindergarten experiment do not allow for this approach. For example, a 6-year-old in 2006 would have been treated in 2005, and a 10-year-old in 2010 would have been 5 in 2005 and therefore treated. Similarly, a 3- or 4-year-old in 2005 would likely be treated under the intervention in 2006 or 2007. Larger age gaps between control and treatment groups are also not ideal, as they could introduce additional variability unrelated to the intervention. Thus, while a simple triple-difference design using mothers of 4-year-old or 6-year-old children as a comparison group would have been ideal, it is not feasible in this context.

However, there is one other way a triple-difference strategy can be implemented under the current settings. Since the school year in the U.S. roughly lasts from September to May, children typically do not attend kindergarten during the summer months of June, July, and August. I exploit this timing of the school year in my triple-difference

setting. The third difference[10] is provided by using the summer months (June-August) as the control period, and the school-year months (Sept-May) as the treated period.

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

$$Y_{i,s,t} = \beta_1(FDK_t X Schyr_m) + \beta_2(AZ_s X Schyr_m) + \beta_3(AZ_s X FDK_t) +$$

$$\gamma(AZ_s X FDK_t X Schyr_m) + X'_{i,s,t} + Z'_{s,t} + \theta_s + \zeta_t + \sigma_m + \eta_{s,t} + \varepsilon_{i,s,t} \tag{3}$$

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 the 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 in recent years. These 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), assigning a weight matrix to predictor variables from all donor units. Consequently, a single placebo control group is generated, which is a combination of all control groups, with predictor units aggregated according to assigned weights. TThe resulting counterfactual control group is very closely matched to the treated group during the pre-treatment years. This combination of control groups presents a counterfactual of the treatment group in the absence of treatment (Abadie et al., 2010). Abadie et al. (2010) highlight the methodological advantages of synthetic control groups, including (i) the 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 "synth" developed by Abadie et al. (2011).

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

There are limitations with the application of synthetic controls and "synth" package in Stata software. A key limitation is that the synthetic control method requires a balanced panel dataset, which rules out using microdata without aggregation. When data is aggregated, each treated unit has only one synthetic control for comparison. Estimates obtained from a single counterfactual can be spurious. However, these concerns can be mitigated to some extent by running placebo tests, as suggested by Abadie et al. (2010). Placebo tests involve assigning placebo treatments to control states iteratively and generating counterfactuals for these placebo states, with Arizona included in the donor pool for each iteration. According to Abadie et al. (2010), if the gap in the outcome variable(s) between Arizona and its synthetic control is larger and more persistent than the gaps for other placebo-treated states, this provides strong evidence of the treatment effect's significance.

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

The difference-in-differences results for employment and weekly hours are presented in Table 3 and Table 4. Each table includes mean pre-treatment employment statistics for 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. Notably, the employment increase is more pronounced among specific subgroups. For high-skilled mothers, there is a 5.8 percentage point increase in maternal employment, for single mothers there is a noteworthy 16.7 percentage point increase, and low-income mothers experience an increase in employment by 4.2 percentage points. Interestingly, 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 show considerable heterogeneity across different groups. For low-skilled mothers, the substitution effect from the full-day kindergarten program might not be strong enough to significantly impact their employment. This is supported by the slightly negative response in full-day kindergarten enrollment for children of low-skilled mothers, as noted in column 4 of Table 2, which could partly explain the lack of a notable employment effect. Conversely, high-skilled mothers experience a statistically significant 5.8 percentage point increase in employment, despite already high pre-treatment employment levels of 72.5%. This substantial effect, though only about one-quarter of the 24 percentage point increase in full-day kindergarten enrollment for their children (Table 2), indicates a strong substitution effect among this group.

For married mothers, there is a 3.3 percentage point decrease in employment. This decline may be attributed to the income effect, which could outweigh the substitution effect when other household income sources are available. Single mothers show the largest employment response, with a 16.7 percentage point increase, consistent with expectations from previous research (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 for employment. A large employment effect among single mothers despite a high pre-treatment employment level of 77.6%, suggests that the substitution effect significantly dominates over the income effect.

For mothers of non-white origin, there is a notable positive employment effect of 8.2 percentage points, with pre-treatment employment levels around 62%. In contrast, for mothers of white origin, no significant employment effect is observed, despite their higher pre-treatment employment levels of about 7%. This is surprising given that there was no observed effect on full-day kindergarten enrollments for children of non-white mothers, while there was a substantial 17.9 percentage point increase in enrollment for children of white mothers (Table 2). One potential explanation for the positive employment effect among non-white mothers could be the higher proportion of single mothers within this group —around 38% —compared to 22.5% for white mothers. Single mothers are more likely to experience a significant substitution effect, which could explain the observed increase in employment among non-white mothers. For mothers of white origin, the already high employment levels and likelihood of higher income effects can explain the absence of any employment effect.

Similarly, the employment effect among low-income mothers, a statistically significant 4.2 percentage points,

contrasts with the modest and statistically insignificant 2.6 percentage point increase observed among high-income mothers. This discrepancy may be attributed to the fact that 40% of low-income mothers are single compared to only 10% of high-income mothers. The larger proportion of single mothers among low-income households likely amplifies the substitution effect, leading to a more pronounced increase in employment.

The difference-in-differences estimates for weekly hours worked, which represent the intensive margin of employment, are presented in Table 4. Overall, I find a small but statistically insignificant impact on number of hours worked, the point estimate suggests an increase of 0.83 hours per week over a baseline of 23.32 mean number of hours worked per week. Among the subgroups, notable effects are observed for single mothers and non-white mothers. For single mothers, the increase in weekly hours worked is 4.2 hours, while for non-white mothers, the increase is 1.9 hours per week. These subgroup results are consistent with expectations, as the intensive margin of labor supply is often less responsive compared to the extensive margin. The relatively small and non-significant overall impact on the number of hours worked can be attributed to the nature of labor supply decisions. While many individuals can choose whether to participate in the labor force, fewer have substantial control over the exact number of hours they work once employed. This limitation in control over working hours may explain why changes in the intensive margin are less pronounced compared to the more substantial shifts observed in employment rates.

Additionally, the observed increases in weekly hours for specific subgroups align with the broader understanding that those who experience larger increases in employmentâsuch as single mothersâmight also exhibit more significant changes in their working hours. The higher impact for single mothers may reflect greater responsiveness to changes in childcare availability, enabling them to adjust their work hours more flexibly.

Overall, these findings underscore the complexity of labor supply responses, where changes in employment participation might not always translate into equally substantial changes in working hours, particularly in cases where individuals have limited flexibility in adjusting their work schedules.

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 term represent the difference in treatment effects between the school-year months (September to May) and the summer months (June to August). These

triple-difference estimates thus rely on the assumption that full-day kindergarten expansion has a more pronounced effect during the school year compared to the summer.

The overall maternal employment estimate presented in column 2 of Table 5 indicates a 5.1 percentage point increase in employment. This result suggests that the increase in employment observed 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. For single mothers, the substitution effect is so pronounced that the brief summer break does not incentivize them to leave their employment. The strong influence of the substitution effect persists despite the short duration of the summer months, maintaining their employment levels throughout the year. 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 months and school year months, but not so for married mothers. For married mothers, a strong income effect supported by reduced child care costs likely outweighs the substitution effect during the school year, which explains the slight negative estimate in the difference-in-differences setup. Given that married mothers might have access to other sources of household income, a significant proportion of those already working, might not feel as compelled to remain in the labor force during the summer months. Instead, they could opt to exit the labor market due to the increased child care burden, which becomes more prominent when the kindergarten program is not available, explaining a positive employment effect in the triple differences specification.

For non-white mothers, the point estimate suggests a significant 5.6 percentage point increase in employment, aligning with the hypothesis of a strong substitution effect. Surprisingly, a 4.5 percentage point increase in employment is also observed among mothers of white origin. Given that white mothers are generally better off socioeconomically and income-wise, it is possible that some of them might choose to exit the labor force during the summer months due to the increased burden of child care.

For low-income mothers, the 4.2 percentage point increase in employment aligns with the hypothesis of a strong substitution effect, where reduced child care costs lead to increased labor force participation. The positive triple-difference estimates for high-income mothers are more surprising. Similar to the observations for married and white mothers, it is possible that high-income mothers might also opt out of the labor force during the summer months due to the increased burden of child care, possibly owing to their generally better financial position.

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. For other groups, triple-difference estimates are positive and statistically significant, whereas 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 Arizona[11] 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

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

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 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 â from half-day to full-day kindergarten â 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.

The findings of this study offer valuable insights for policymakers considering the implementation or expansion of full-day kindergarten programs. The positive impact of full-day kindergarten on maternal employment, underscores the potential of such programs to support labor force participation and family well-being. Policymakers should consider the heterogeneous effects observed across different sub-populations when designing and evaluating early

childhood education policies. Heterogeneous employment responses for specific subgroups underscore the varying degrees of responsiveness to expanded childcare access. This suggests that the benefits of full-day kindergarten expansion were most pronounced among populations with pre-existing childcare constraints, contributing to a more equitable distribution of labor market gains. Tailoring full-day kindergarten programs to address the specific needs of these groups can maximize their effectiveness and ensure that the benefits are widely distributed. Additionally, the observed positive effect of a modest increase in full-day kindergarten availability suggests that even incremental improvements in early childhood education can have meaningful impacts on employment outcomes.

Future research could further explore the long-term effects of full-day kindergarten on maternal employment and other socio-economic outcomes. Investigating how variations in program implementation, such as quality and accessibility, affect different demographic groups could provide deeper insights into the mechanisms driving these outcomes. Additionally, examining the broader implications of full-day kindergarten on child development and family dynamics would enhance our understanding of the overall impact of such policies. Comparative studies across different states or countries could also help identify best practices and inform more effective policy designs. Overall, continued research is essential to refine early childhood education policies and maximize their benefits for working families.

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Figures and Tables

Figure 1: Timeline of Kindergarten Expansion in Arizona
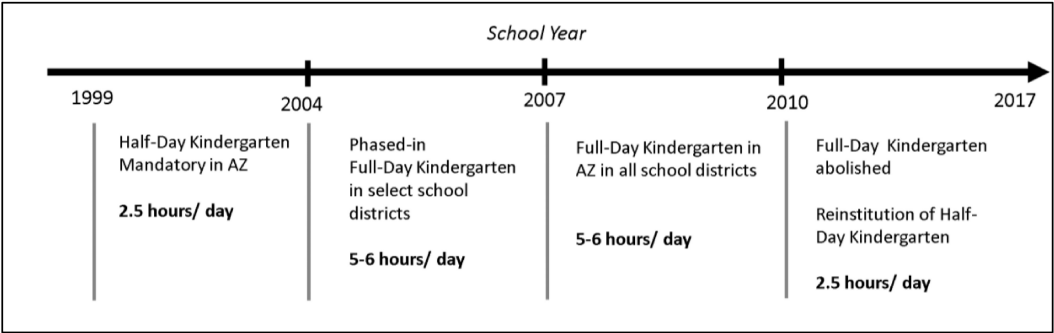
Source: Libassi (2014)

Figure 2: Enrollment Impacts of Kindergarten Expansion in Arizona

Full-Day vs Half-Day Kindergarten Enrollment in AZ: 2000-2010
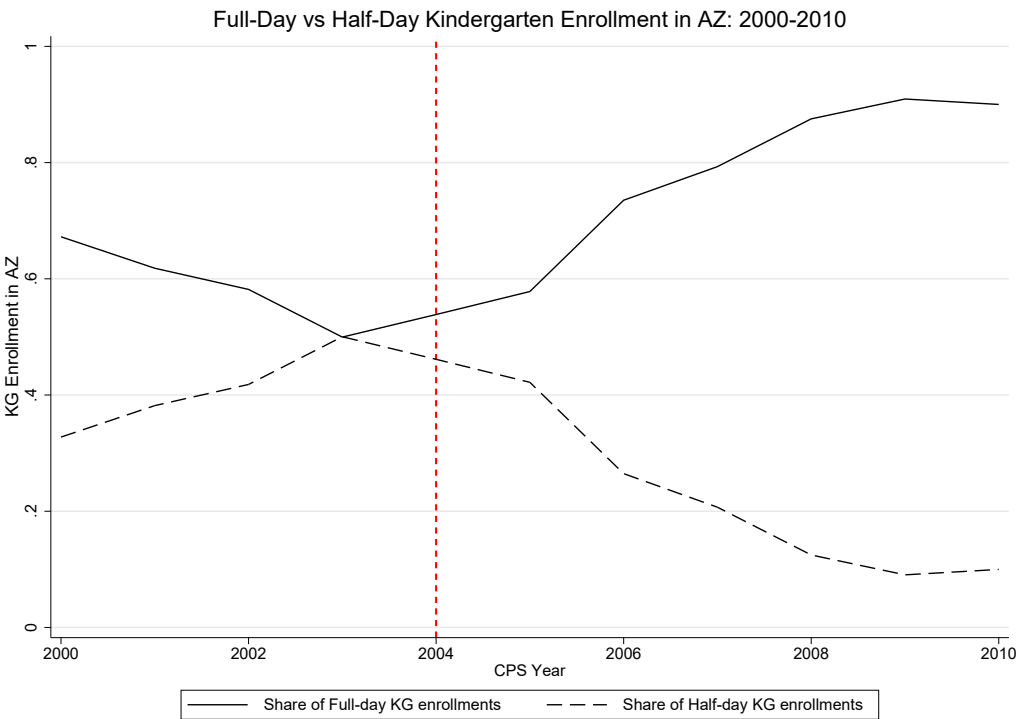
Source: Flood et al. (2021)

Figure 3: Full-day Kindergarten Enrollment

FT Kindergarten Enrollment: AZ vs other States
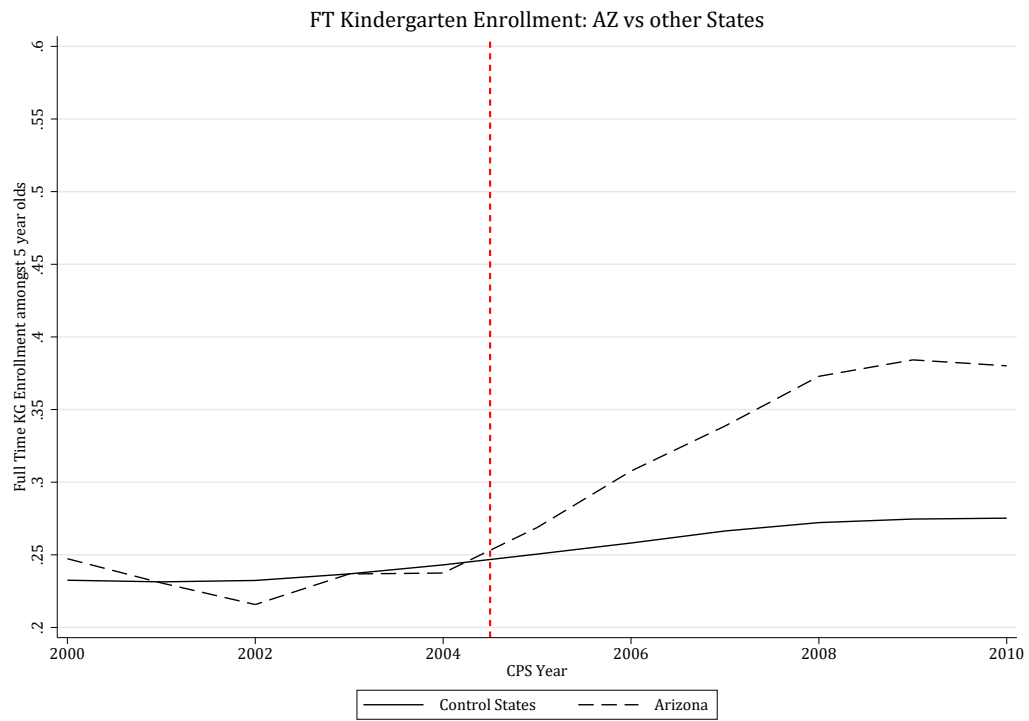
Source: Flood et al. (2021)

Figure 4: Employment among Females: Arizona vs Other States

Employment among females (20-55): Arizona vs other States

Maternal Employment amongst Females 20-55 with 5 year old kids: Arizona vs other States

Source: Flood et al. (2021)
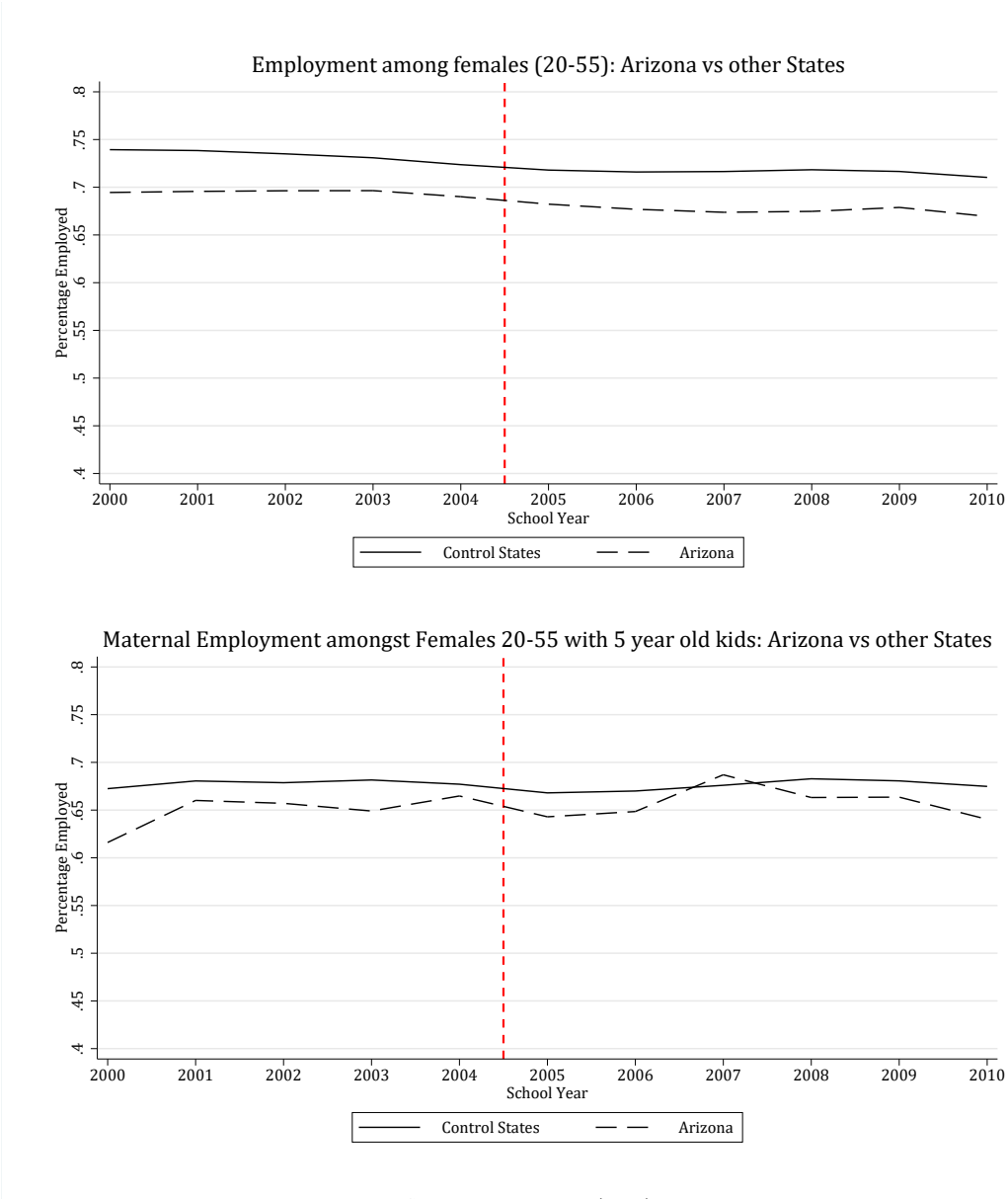
Figure 5: Weekly Hours Worked among Females: Arizona vs Other States

Hours Worked Per Week 20-55: AZ vs other States

Hours Worked Per Week 20-55: Females with 5 year old kids: AZ vs other States
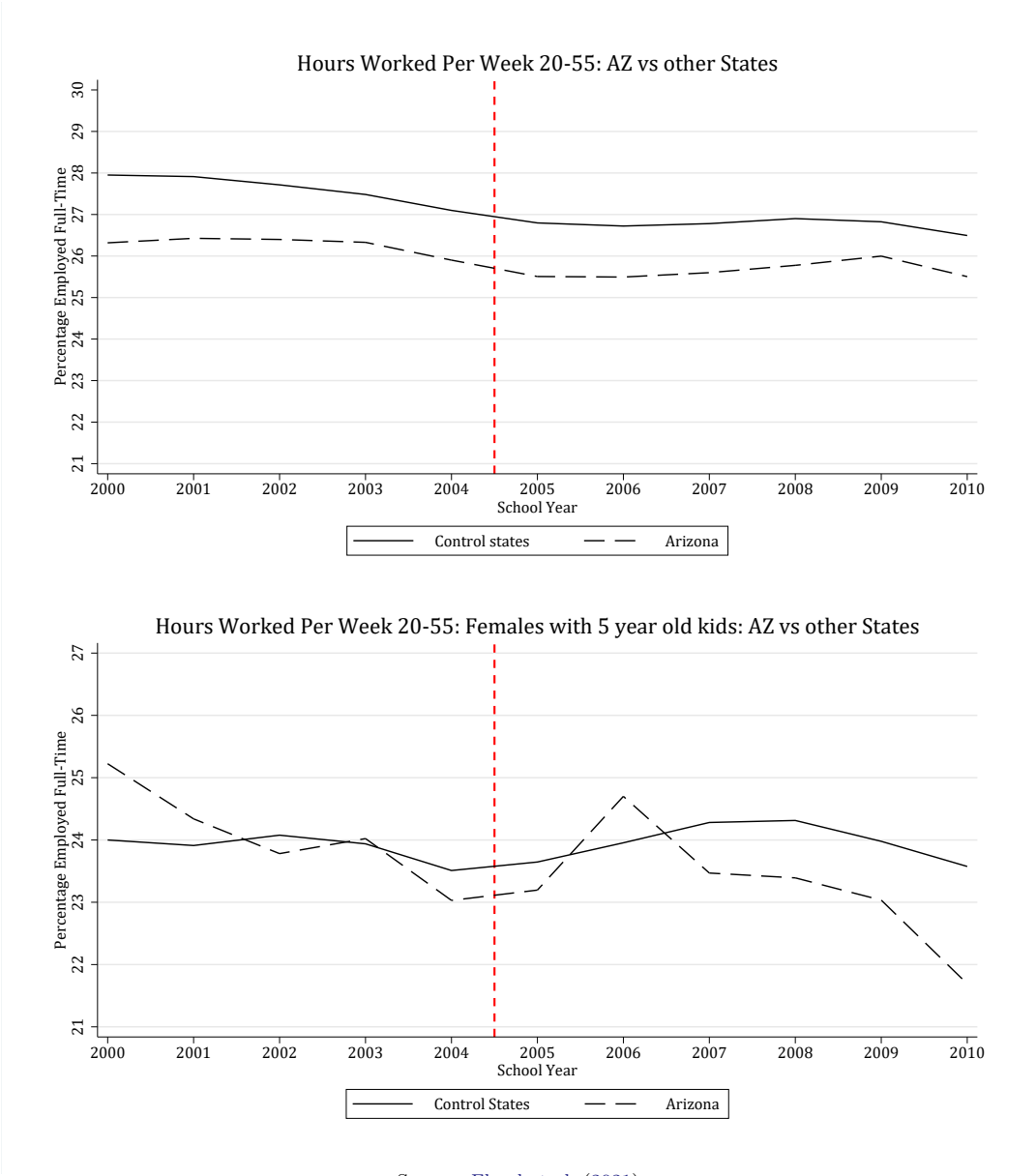
Source: Flood et al. (2021)

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

	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non-White	(8) White	(9) LowInc	(10) HighInc
					Dep Var: Full Day Kindergarten Enrollment=1					
AZ × FDK intervention	0.140***	0.084***	-0.070**	0.240***	-0.002	0.383***	-0.029	0.179***	-0.022	0.268***
	(0.019)	(0.020)	(0.034)	(0.022)	(0.024)	(0.039)	(0.048)	(0.027)	(0.034)	(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

	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
					Dep Var: Mother is employed == 1					
AZ × FDK intervention	0.044***	0.025**	0.001	0.058***	-0.033**	0.167***	0.082***	-0.019	0.042**	0.026
	(0.013)	(0.011)	(0.030)	(0.016)	(0.015)	(0.028)	(0.019)	(0.017)	(0.020)	(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

	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
					Dep Var: Weekly Hours Worked					
AZ × FDK intervention	1.729**	0.832	1.733	0.615	-0.627	4.274***	1.953**	0.159	1.315	1.553
	(0.647)	(0.509)	(1.245)	(0.723)	(0.652)	(1.283)	(0.771)	(0.829)	(0.854)	(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

	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
					Dep Var: Mother is employed == 1					
AZ × School-Year Months	-0.045***	-0.028***	-0.028*	-0.036***	-0.030***	-0.049***	-0.021	-0.028**	-0.043***	0.009
	(0.009)	(0.007)	(0.014)	(0.013)	(0.007)	(0.015)	(0.014)	(0.011)	(0.010)	(0.010)
FDK × School-Year Months	0.004	0.007	-0.013	0.016	0.007	0.008	0.000	0.011	0.014	-0.002
	(0.012)	(0.011)	(0.022)	(0.016)	(0.013)	(0.016)	(0.027)	(0.012)	(0.015)	(0.013)
FDK × AZ	-0.019	0.003	0.017	-0.003	-0.052**	0.173***	0.045	-0.032	0.062**	-0.068***
	(0.013)	(0.015)	(0.024)	(0.018)	(0.022)	(0.029)	(0.028)	(0.023)	(0.025)	(0.025)
FDK × AZ × School-Year Months	0.088***	0.051***	0.031	0.082***	0.058***	0.021	0.056***	0.045***	0.033**	0.056***
	(0.012)	(0.009)	(0.024)	(0.016)	(0.012)	(0.018)	(0.019)	(0.013)	(0.013)	(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.)

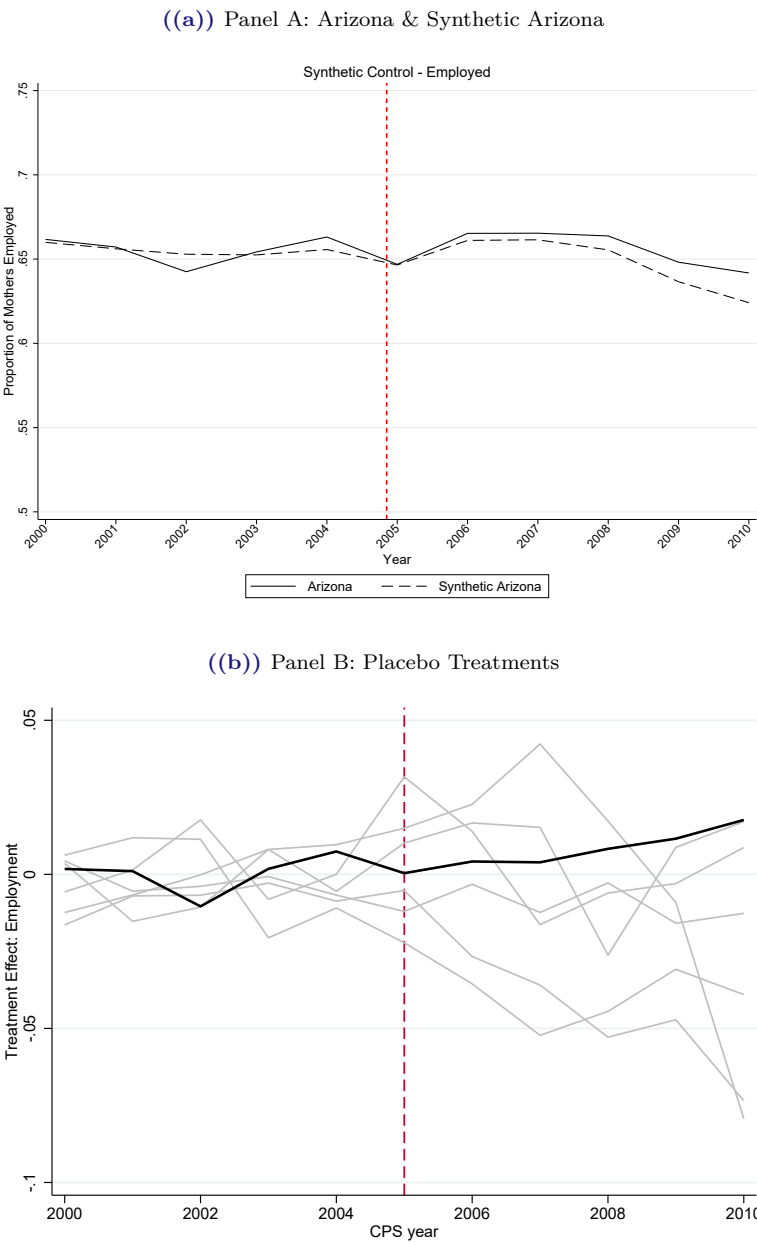**Table 6:** DDD Estimates for Mothers of 5 year olds: Weekly Hours

	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
					Dep Var: Weekly Hours Worked by Mother					
AZ × School-Year Months	-0.314	-0.049	-0.431	0.020	0.259	-1.884***	-2.038***	1.195**	-0.312	0.529
	(0.359)	(0.282)	(0.627)	(0.465)	(0.318)	(0.610)	(0.658)	(0.448)	(0.372)	(0.470)
FDK × School-Year Months	0.243	0.449	-0.918	1.297*	0.511	0.313	-0.211	0.818	0.482	0.388
	(0.564)	(0.519)	(0.864)	(0.644)	(0.565)	(0.778)	(1.109)	(0.591)	(0.633)	(0.592)
FDK × AZ	-0.380	0.003	1.766**	-1.035	-0.667	2.373*	-0.837	-0.056	2.292**	-2.752**
	(0.466)	(0.534)	(0.793)	(0.689)	(0.839)	(1.366)	(1.028)	(0.877)	(0.883)	(1.347)
FDK × AZ × School-Year Months	3.398***	2.110***	1.822*	2.816***	1.246**	3.945***	4.152***	1.190*	1.015*	3.001***
	(0.571)	(0.459)	(1.006)	(0.682)	(0.521)	(0.815)	(0.876)	(0.618)	(0.567)	(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

((a)) Panel A: Arizona & Synthetic Arizona

((b)) Panel B: Placebo Treatments

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