The effect of subsidized childcare on the supply of informal care: evidence from public kindergarten provision in the US

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The effect of subsidized childcare on the supply of informal care: Evidence from public kindergarten provision in the US

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Abstract

For informal caregivers in certain demographic groups, the tradeoff between child-care and informal care may be as significant as the tradeoff between informal care and labor supply. We shed light on this tradeoff empirically, by combining detailed time use data with a natural experiment created by differential access to publicly funded kindergarten across households and states. We find a substantial elasticity between informal care supply and kindergarten access, especially for female carers. In fact, for women, kindergarten access appears to largely increase their care supply rather than labor supply.

1 Introduction

Informal caregiving is the principal source of care for older and disabled people in the United States (and in many other countries).[1] Indeed, informal caregiving by family and friends has in the majority of cases (about 70%) become the sole source of long-term care,

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[1]Formal long-term care (LTC) services are expensive (the average annual cost of nursing home care was estimated to be $75,000 (Iglehart 2010)) and less than 15% of the elderly population in the United States is covered by LTC insurance (Brown, Goda and McGarry 2012).

while more than 90% of disabled elderly receive at least some informal care (Spillman and Pezzin 2000, Spillman and Black 2005, Doty 2010, Houser, Gibson and Redfoot 2010, Kaye, Harrington amd LaPlante 2010, Freedman and Spillman 2014, Ankuda and Levine 2016). The supply of informal care for the elderly has therefore traditionally been the subject of considerable academic and policy interest. In recent years, this interest has intensified for a number of reasons: First, the repeal of the long-term care provisions contained in the Affordable Care Act has signaled that informal caregiving will continue to be the principal source of long-term care for the foreseeable future. Second, demand-side factors such as population aging, increasing racial/ethnic diversity, as well as increasing rates of chronic conditions, have the potential to increase the demand for long-term care (Norton 2000, Wolf 2001, Yang et al 2003).

Whether this increased demand can be adequately accommodated by informal caregivers is an important question, given that caregivers face multiple competing demands on their time, which are also evolving as a result of demographic trends. In particular, the trend towards delayed fertility implies that potential caregivers (especially women) are increasingly likely to face simultaneous child care and elder care responsibilities (Spillman and Pezzin 2000). This is an important consideration that is not however routinely taken into account in the traditional framework of time-tradeoffs, which tends to focus on the narrow tradeoff between work and caregiving activities. In reality, many caregivers are allocating time between at last three time-intensive activities - caregiving, work and childcare - and the relevant opportunity cost of time is not necessarily measured by the market wage. Pierret (2006) and Suh (2016) estimate that a sizeable subset of adult women in the United States provide significant amounts of care to both children as well as parents.[2] Among informal caregivers, nearly 1 in 5 are simultaneously providing care to children under the age of 12.[3] This dual responsibility results in greater strain on caregivers' mental health, and also, unsurprisingly, results in a lower supply of informal care, compared to other caregivers (Rubin and White-Means 2009).

In this paper, we utilize time-use data from the American Time Use Survey to examine the tradeoff between the two kinds of care. To provide a clear policy focus for our study,

[2]The size of estimates vary depending on the definition adopted, as well as the data source utilized. Pierret (2006) uses data from the 1997 and 1999 rounds of the National Longitudinal Survey of Young Women (NLSYW), and reports a preferred estimate that 9% of women in the age group 45-56 are dual carers, in the sense of providing significant amounts of care to children as well as parents.

[3]Authors' calculations based on data from the American Time Use Survey (see Section 3 below).

we examine whether the supply of informal care is elastic with respect to publicly subsidized child care/schooling. We focus on full-day kindergarten, whose provision exhibits substantial variation across states, partly due to differences in state-level statutory requirements. We take advantage of these differences in statutory requirements, in conjunction with variation in household eligibility for kindergarten based on the age of the youngest child, to estimate the effect of full-day kindergarten access on caregiving using a difference-in-differences identification strategy.

Within an extended time-tradeoff framework, the relaxation of the childcare constraint may result in an increase in either work hours or caregiving, or both. For some individuals the marginal benefit from caregiving may be greater than the benefit from additional paid work, and these individuals may utilize the freed-up time to provide informal care, while conversely some individuals may reallocate the majority of the extra time to paid work. *A priori* we do not know the relative proportions of these two types of individuals in the population. The aggregate (or average) effect of childcare subsidies on caregiving is therefore an empirical issue, but has not received attention in the existing literature: On the one hand, studies of caregiving typically examine the effect of health care or labor market interventions, while on the other hand, studies looking at the effects of childcare subsidies tend to focus on labor market effects.

Our analysis reveals that access to full-day kindergarten has a large and statistically significant effect on both the probability of being a caregiver, as well as on the amount of care provided. A variety of placebo tests confirm the causal validity of the estimates: We find no significant effects for households whose children are not age-eligible, and no significant effect of kindergarten eligibility of the oldest child. We also find that the increases in caregiving are concentrated in days when schools are in session (weekdays and non-summer months). We also test for and rule out any compositional effects that may be confounding the estimates. The increase in caregiving is more pronounced among women, non-Whites, non-college educated, and those not currently working. Interestingly, we do not find significant effects of full-day kindergarten provision on labor supply, of either men or women.

The significance of these findings is two-fold. First, in the absence of a well-functioning and affordable formal chronic care system, public policies that can support informal caregiving are critical. The academic debate surrounding this issue has emphasized the opportunity costs of caregiving in terms of lost income (i.e. the trade-off between work and caregiving), and this is also reflected in the policy debate which centres around flexible work arrangements, mandated (paid or unpaid) leave, direct compensation of caregivers, etc.[4] Relatively little attention, however, has been paid to the trade-off between child-rearing and caregiving. Our results suggest that this is an important omission, and that targeting the trade-off between child care and caregiving may be an effective complement to policies that target work arrangements.

Second, our findings are also significant in the context of the recent literature on the effect of child-care subsidies on maternal labor supply. The argument that child-care subsidies can crowd-in maternal employment is often cited as a supporting rationale for such subsidies (e.g. Karoly and Bigelow 2005). The empirical evidence in this regard is however mixed. Whereas a number of earlier studies (e.g. Gelbach 2002, Blau and Tekin 2007, Baker, Gruber and Milligan 2008) consistently found maternal labor supply to be elastic with respect to the child care subsidies implicit in publicly funded pre-school programs, studies that have used more recent data tend to find much weaker (and sometimes zero) effects (Fitzpatrick 2010, Havnes and Mogstad 2011, Cascio and Schanzenbach 2013), thereby undermining an important rationale for the provision of such subsidies. Our findings suggest that the margin of adjustment may actually be caregiving rather than labor supply, and that accounting for this channel has the potential to alter the cost-benefit assessment of child-care subsidies. More broadly, our study makes the case for a more extensive approach to policy evaluation that considers policy impacts that manifest in different domains than are being directly targeted by the policy.

The paper proceeds as follows. Section 2 describes the data, Section 3 presents some descriptive evidence on dual-carers and the tradeoff between informal caregiving and child-care; Section 4 presents the main analysis, and Section 5 concludes.

[4]Studies that examine the trade-off between work and caregiving on the part of women (who constitute the majority of caregivers (Feinberg et al 2011) have found mixed results (Mclanahan and Monson 1990, Spitze and Logan 1991, Boaz and Muller 1992, Moen and Fields 1994, Pavalko and Artis 1997, Moen and Dempster- McLain 1995). Studies on sub-populations of caregivers found that co-residence with the care recipient is associated with reduced participation in the labor force (Ettner 1995). Similarly, caregivers engaged with heavy activities are less likely to be in the labor force. On the intensive margin, evidence from the US suggests that caregivers (especially females) tend to reduce their working hours and face a reduced wage (Ettner 1995, Graves 2010, Johnson and Sasso 2000, Van Houtven, Coe and Skira 2013).

2 Data

1. Caregiving and time-use data

To examine the trade-off between caregiving and child care, we utilize time-use data from the American Time Use Survey (ATUS). The ATUS, conducted annually by the Bureau of Labor Statistics, records the time allocated to various activities by non-institutionalised civilians 15 years old and older in the United States. The focus is on collecting a time diary in which survey respondents are asked to report their main activities sequentially for the 24-hour period that began at 4 a.m. on the previous day and ended at 4 a.m. on the day of the interview. Interviewees are randomly selected from households (one respondent per household) participating in their final (eighth) round of the Current Population Survey (CPS) sample.

We use pooled data from the 2013-2018 rounds of the survey. The ATUS includes a special module to assess the time spent on informal caregiving. ATUS respondents are asked if they have provided unpaid care to any relatives/friends in the last three months. In the ATUS elder care is defined as the "provision of care or assistance to an individual because of a medical condition related to ageing" (Denton 2012). The survey framing stresses the importance of ongoing care, and thereby restricts attention to those who are providing care on a long-term basis. We use this information to construct an indicator for whether an individual self-identifies as a long-term caregiver. Approximately 19% of respondents in the sample identify as long-term caregivers.

The survey also elicits information on the age of the care recipient, whether the care recipient is a member of the caregiver's household, and the relation between the two (all reported by the caregiver). Although the definition of elder care adopted by the ATUS is deliberately non-specific with respect to the age of the care recipient, the median care recipient in the data is 79 years old, and fewer than 1% of care recipients are under the age of 50. In the remainder of the paper, we therefore use the terms informal care and elder care interchangeably.

Conditional on respondents having provided care in the last three months, they are then asked how much time they spent doing so during the last 24 hours. The responses are cross-checked with the time diary to verify their accuracy. We use this information to create a second measure of caregiving, as the amount of care (in minutes) that the caregiver provided in the 24-hour recall period (this amount is zero for those who did not provide care on the preceding day). On average, long-term caregivers provided approximately 21 minutes of care in the 24-hour recall period (not all caregivers provided care in the recall period, however: Among those who did, the average care time was approximately 108 minutes).

The time diaries elicited by the ATUS also allow us to measure the amount of time spent by the respondent with children in the household in the 24-hour recall period, including both primary as well as secondary childcare (the former includes activities that are child-focused, such as reading, play, etc, while the latter refers to other activities that were carried out while accompanied by children).

2. Socio-economic variables

In addition to time use, the ATUS elicits information on household composition, including the ages of all children, and socio-economic variables including the respondent's race, marital status, education, employment status in the last seven days and usual weekly working hours, as well as spousal age and education.

3. State-level variation in kindergarten provision

Compulsory education in the United States starts at the age of six (on average). However many school districts offer universal, non-compulsory, free-of-charge public kindergarten. Eligibility criteria to enroll in kindergarten are mostly defined at state level, with the general rule that only children who turned five within a state-specific cut-off date are eligible for enrollment.[5]

While kindergarten provision has spread rapidly in recent years as part of a national effort to increase the rigor of elementary school, raise tests scores and increase learning in higher grades, there remains significant inter-state variation in the extent of kindergarten provision. This variation is a complex phenomenon, reflecting a number of factors, such as variation in funding formulas and priorities, as well as statutory requirements. In this paper, we focus on differences in state-level mandates. Eleven states[6] and District of Columbia

[5]With the exception of a few states that leave it to Local Education Agencies to define the cut-off date, most states set the cut-off between August and September, with only three states (California, Kentucky and Maine) setting in October, one (Michigan) in November and two (Connecticut and Vermont) at the end/beginning of the calendar year.

[6]Alabama, Arkansas, Delaware, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee and West Virginia.

require their school districts to provide publicly funded full-day kindergarten (we refer to these as full-day states),[7] thirty-four states require districts to provide at least half-day kindergarten (we refer to these as half-day states) and five states do not require districts to provide kindergarten at all (we refer to these as no-kindergarten states).[8]

Table 1 shows public kindergarten enrolment rates for 5-year olds, calculated using data from the October 2013 Current Population Survey (CPS), separately for the three groups of states, i.e., full-day, half-day and no-kindergarten.[9] There is a notable difference in full-day enrolment rates between the full-day states and the other two groups, in line with the difference in state-level mandates.[10] On average, full-day states have approximately 19 percentage points higher full-day enrolment rates than the other two groups (a difference of approximately 25%), and this difference is strongly significant at the 1% level. In contrast, there do not appear to be any significant differences in half-day or full-day enrolment between half-day and no-kindergarten states.[11] In our analysis, therefore, we will estimate the effect of full-day kindergarten access by comparing full-day states with all other states (i.e., pooling the half-day and no-kindergarten states in a single comparison group).

3 Descriptive Statistics

We now motivate our study with some descriptive statistics and regressions that shed light on the prevalence of dual-carers, and their time allocation between the two activities of childcare and elder care. For the purpose of this section, we define dual-carers as individuals who have children under the age of 12 in the household, and who also identify as long-term caregivers.[12]

[7]Half-day kindergarten typically lasts two or three hours, while full-day can range from four to seven hours (Kauerz 2005).

[8]Alaska, Idaho, New Jersey, New York and Pennsylvania. (https://nces.ed.gov/programs/statereform/tab5_3.asp, State Education Reforms, 2014)

[9]In order to cleanly focus on the effects of kindergarten eligibility, we exclude from our analysis the three states that offer publicly funded pre-kindergarten (Florida, Georgia, and Oklahoma).

[10]Even in full-time states, the rate of enrolment is not as high as one would expect. This is because approximately 3 out of 10 children who are recorded as 5 years old in the CPS are either still in nursery or are already enrolled in first grade.

[11]As is also evident from Table 1, rates of full-day enrolment are quite high even in non full-day states, reflecting the fact that the lack of a state-level mandate does not prevent individual schools or districts from offering full-day kindergarten if they choose to.

[12]Other definitions are possible, e.g. one could define dual-carers as those who provided both child care as well as elder care in the 24-hour recall period, but the general conclusions drawn below are reasonably robust to the choice of definition.

In the ATUS data, approximately 20% of those identifying as informal caregivers also have children under the age of 12 (the corresponding figures for men and women are 19% and 21%), indicating that dual carers make up a significant proportion of informal care providers. Conditional on having provided care in the 24-hour recall period, dual-carers supplied on average 93 minutes of elder care in the 24-hour recall period, compared to 106 minutes of elder care supplied by non-dual-carers (the difference is statistically significant at the 1% level).[13]

It is possible that dual carers are different from other informal caregivers along other dimensions, implying that the differences in informal care time between the two groups may not necessarily reflect the childcare constraint. We explore this issue further with some simple descriptive regressions to shed light on the extent to which child-rearing responsibilities restrict the supply of informal care. In Table 2, we first regress the caregiving indicator on an indicator for the presence of children under the age of 12 in the household, while controlling for other observable characteristics (Column 1). In order to minimize simultaneity bias, we do not include choice variables such as employment status or family income that may be determined jointly along with caregiving decisions. In Column 2 of Table 2, we examine the intensive margin of caregiving, by regressing elder care time (in minutes) on the same set of variables, but restricting the sample to caregivers who provided care in the 24-hour recall period (note that this restriction considerably reduces the sample size). All regressions in the paper are weighted using survey weights. The regressions also allow for state and year fixed effects, and standard errors are clustered at the state level.

The results in Table 2 indicate that respondents with children under the age of 12 are approximately 5 percentage points less likely to be long-term caregivers - this represents an approximately 25% reduction in the probability of being a caregiver. The presence of children under 12 reduces the amount of care provided by about 13 minutes, an approximately 13% reduction in care time. Given that the regression in Column 2 does not correct for selection into caregiving status, these estimates almost certainly understate the conditional-on-caregiving (i.e. intensive margin) effect of having small children. Overall, these descriptive regressions suggest that childcare responsibilities significantly curtail the provision of informal care.

[13] If we only condition on those who identify as long-term caregivers, the corresponding care time amounts are 14 minutes and 23 minutes respectively (the difference is again significant at the 1% level).

In the remainder of the analysis, we restrict the sample to respondents who have children under the age of 12 in the household, in order to examine the effect of differential access to publicly funded kindergarten on the care supply of this subpopulation. Table 3 presents a number of summary statistics for this analysis sample, separately for the two sets of states in our main analysis (i.e., those that offer full-day kindergarten, and those that do not). It is particularly noteworthy that respondents in full-day states are more likely to be caregivers and provide more care time on average. This is consistent with the fact that many of the full-day states also tend to rank poorly in terms of affordability and access to long term care services (AARP 2020).[14] As we will remark later, this observation has implications for the policy implications of our study. Otherwise there are few notable differences between the two sets of states, with the exception that the full-day sample has a significantly smaller proportion of whites.

4 Analysing the effect of kindergarten subsidies on informal care

Our main analysis compares individuals residing in states that mandate full-day kindergarten to those that do not, i.e. we are effectively estimating the causal effect of the full-day mandate. This reduced-form methodology can also be viewed as providing an Intent To Treat (ITT) estimate of the effect of full-day kindergarten (relative to half-day kindergarten), keeping in mind that not all eligible children in full-day states may avail of full-day kindergarten (and conversely some children in non full-day states may nonetheless be able to access publicly funded full-day kindergarten, as we explained in Section 2).

The identification strategy takes advantage of two sources of variation in kindergarten access: Variation across states in the statutory requirement of full-day kindergarten, and variation across households in eligibility arising from variation in the age of their children. In particular, eligibility at individual level is restricted to a single age-group, namely 5 year olds. As is common in the literature, we restrict attention to kindergarten eligibility of the youngest child, since easing this particular childcare constraint is likely to have the most significant impact on the parent's time (later, we examine how the results differ when looking at eligibility of the oldest child).

[14] In particular, Alabama, Arkansas, Louisiana, Mississippi, Oklahoma, South Carolina, Tennessee and West Virginia score in the bottom quartile of states in terms of long term access according to the AARP scorecard.

The econometric strategy is therefore a form of difference-in-differences (DID): We are comparing differences in outcomes for eligible (5 yr old) and ineligible (non 5-yr old) children in full-day states to the corresponding difference in outcomes in non full-day states. The DID approach can account for both unobserved differences between full-day and non-full-day states, as well as pure age effects (whereby parents with 5-year old children may simply behave differently than parents of children of other ages, independent of kindergarten attendance).

To set the stage, we first estimate a regression in which we estimate the differential effect of residing in a full-day state, for all age groups of children:

$$y_{ist} = \alpha + \beta FD_s + \sum_{j=0}^{12} \gamma_j I[Age_i = j] + \sum_{j=1}^{12} \delta_j I[Age_i = j] \times FD_s + \boldsymbol{\theta} \mathbf{X}_{it} + \eta_{st} + \varepsilon_{ist} \tag{1}$$

where y_{ist} denotes the outcome of interest for person i residing in state s in year t; FD_s is an indicator for whether state s offers full-day kindergarten; $I[Age_i = j]$ is an indicator for whether the youngest child in person i's household is j years old; $\mathbf{X}_{it}$ is a vector of individual controls, including age, sex, race, education, marital status, dummy indicators for diary day and month; η_{st} represents a state-times-year fixed effect, whose inclusion not only controls for fixed differences but also time-varying differences between full-day and other states (e.g. changes in state-level policies over the sample period); and ε_{ist} is a residual error term.

In this specification, the δ_j coefficients are the DID estimates of the effect of the full-day kindergarten mandate on the outcome of a person whose youngest child is j years old; importantly, because the omitted age category is 0, these differential effects are being estimated relative to persons whose youngest child is 0 years old.[15] it is worth nothing that because the specification above (as well as all the subsequent specifications in the paper) controls for state and child-age fixed effects, this is technically a two-way fixed effects specification.[16]

In the panels of Figure 1, we plot the estimated δ_j coefficients along with the associated 95% confidence intervals, for each of three outcomes: (1) Child care time in minutes (Panel A), (2) Elder care time (in minutes) (Panel B), and (3) Indicator for long-term caregiver status (Panel

[15] The omitted category is therefore analogous to the "pre period" in the usual DID regression.

[16] We note however that the analogy to a traditional DID estimation stands given that we have one treatment group and one pure control group, and only one treatment "period" (i.e. when children are aged 5)

C). In Panel A, we observe a large and statistically significant reduction in child care time when the child turns 5 in a full-day state; this reduction is mirrored by large and statistically significant increases in elder care time and the probability of being a caregiver when the child turns 5. We do not find any statistically significant effects of full-day access for any other age groups. These results present a strong visual confirmation of the hypothesis that full-day kindergarten access "crowds-in" informal elder-care.

A disadvantage of the specification in Eqn (1) above is that because the effects are estimated relative to a single age group (0 years), the coefficient estimates are noisy. In the remainder of the analysis, we utilize a more standard DID specification, which estimates the DID effect for the eligible group only, relative to all ineligible groups (which are pooled into a single comparison group):

$$y_{ist} = \alpha + \beta FD_s + \gamma I[Age_i = 5] + \delta I[Age_i = 5] \times FD_s + \boldsymbol{\theta}\mathbf{X}_{it} + \eta_{st} + \varepsilon_{ist} \tag{2}$$

In this specification, the δ coefficient is the DID estimate of full-day kindergarten access for eligible children. In all other respects, the specification is identical to that in Eqn (1).

Table 4 presents the regression estimates for the three outcome variables of child care time, elder care time, and the indicator of caregiving status. As expected, these estimates are more precise, as evidenced by the smaller standard errors and confidence intervals. Full time kindergarten access reduces child care time by 53.8 minutes, increases elder care time by 6.3 minutes, and increases the probability of caregiving by 10 percentage points.

We can express these ITT estimates in terms of per-treated-person effects (or Treatment-On-Treated effects), using the fact that the rate of full-day enrolment is approximately 19 percentage points higher in full-day states (relative to non full-day states). This calculation indicates that the TOT effects are approximately 285 minutes of reduced childcare time, 33 minutes of increased elder care time, and a 53% increase in the probability of being a caregiver, on average across individuals who obtain full-day as opposed to half-day kindergarten. These are all clearly very substantial effects relative to baseline,[17] and testify to the importance of relaxing the childcare constraint. To gain a broader perspective on the magnitude of the estimates, we can ask how much extra caregiving is induced in the aggre-

[17]Keeping in mind the Average Treatment on Treated (ATT) interpretation of DID estimates, we define the baseline as children in full-day states who are not aged 5.

gate as a result of the mandate, taking into account the number of individuals affected by the policy. The ITT estimate implies that in the full-day states, on average parents whose youngest child is 5 years old supply an extra 6.3 minutes of care. Adding up this extra care across all such individuals (and taking into account the survey weights), we estimate that the mandate adds 33.3 million hours of caregiving annually (in total across all full-day states). This amounts to an approximately 2.5% increase in total annual caregiving hours. From the aggregate perspective, therefore, the effect of the mandate on caregiving is modest, but non-trivial.

Validity and robustness checks

We carry out a number of checks of the validity of the identification strategy. First, we estimate the DID effect of eligibility of the oldest child (as opposed to the youngest child). The sample is now restricted to households whose oldest child is under the age of 12. Our expectation is that the estimated effects should be much smaller, since kindergarten access for the oldest child does not alleviate the parent's childcare constraint to the same extent. The results are reported in Appendix Table 1. For the child care variable as well as for both measures of caregiving, we find small and statistically insignificant effects of eligibility, as expected.

As a second placebo check, we test whether there are differential effects on caregiving in the weekend (relative to weekdays), and in the summer months (since the mandate would not be expected to significantly ease the childcare constraint except when the schools are in session). In Appendix Table 2, we report the DID effects separately for summer months (June-August) and non-summer months (Columns 1 and 2), and for weekends and weekdays (Columns 3 and 4).[18] In both cases, the evidence is striking: Significant increases in caregiving time are only found on days when schools are in session, with the coefficient estimates for summer months and for weekends being small and statistically indistinguishable from zero. These results represent yet another striking confirmation of the validity of the identification strategy.

As a third placebo check, we refine the DID specification to explicitly incorporate the policy

[18] We only examine effects on care provided in the 24-hour recall period, since the long-term carer outcome is not related to the day of interview.

variation between half-day and no-kindergarten states:

$$y_{ist} = \alpha + \beta_1 FD_s + \beta_2 HD_s + \gamma I[Age_i = 5] + \delta_1 I[Age_i = 5] \times FD_s \\ + \delta_2 I[Age_i = 5] \times HD_s + \boldsymbol{\theta}\mathbf{X}_{it} + \eta_{st} + \varepsilon_{ist} \tag{3}$$

where HD_s is an indicator for half-day states. In this specification, δ_1 and δ_2 represent the DID coefficients for full-day and half-day states, respectively, compared to no-kindergarten states (the omitted category). The results are reported in Appendix Table 3. For each of the outcomes, the DID effect is small and statistically insignificant for half-day states, which is consistent with the observation that there were no significant differences in either half-day or full-day enrolment rates between half-day and no-kindergarten states.

Our next set of checks addresses the concern that conditioning on the youngest child in the household potentially induces compositional effects, i.e. the sample of households whose youngest child is 5 years old may be systematically different from households whose youngest child is not 5 years old. To examine whether there are any changes in sample composition as a result of the treatment, we estimate the DID specification using observable characteristics as outcome variables. Appendix Table 4 reports the results for each of the observable characteristics, namely, age, Black (dummy variable), Male (dummy variable), years of education, spousal age, and spousal education. Reassuringly, we find little evidence of significant effects of eligibility on any of the observable characteristics, indicating that the estimated effects of eligibility on the supply of care are not an artefact of compositional changes.

Lastly, we examine the robustness of the results to two different sample definitions. First, we estimate the ITT effects while restricting the sample to respondents whose children are up to and under the age of 5. This restriction effectively reduces the control group to only include those with children in the ages 0-4, and addresses a potential concern that kindergarten access may have longer-term effects on caregiving that would in turn make persons with children in older age groups (those above 5 years old) incomparable across the two groups of states. The results are reported in Appendix Table 5: We find that the point estimates remain stable, although the standard errors are larger than before, reflecting the reduction in sample size (in particular, the estimated effect on the probability of caregiving is now only significant at the 10% level). Second, we estimate the ITT effects on the full sample of states, including the three states that offer publicly funded pre-K (one of which, Oklahoma, also has a full-day kindergarten mandate). The results are reported in Appendix Table 6. Since the pre-K states are few in number, their inclusion does not significantly affect the results, which remain similar in terms of point estimates and standard errors to those obtained in the main specification (in Table 3).

Heterogeneity

The effect of the full-day mandate is likely to be more pronounced for some groups of individuals than for others. For instance, women typically have a higher propensity to supply informal care, and may respond more strongly to the relaxation of the childcare constraint. Socio-economic status is also potentially a moderator of treatment effects in our context: On the one hand, one may hypothesize that those belonging to lower wealth groups (or, more precisely, their relatives in need of care) may have access to long-term care via Medicaid, and may therefore not adjust strongly on the caregiving margin. At the same time, from an opportunity cost perspective, one may also hypothesize that those with lower levels of education and those who are not currently working may have lower returns to working, and as such may utilize the freed-up time in caregiving rather than labor market work. To shed light on these sources of heterogeneity, we now disaggregate the results for sub-groups of the population. In particular we investigate whether there are any heterogeneous effects on caregiving by (1) Gender, (2) Race (Blacks vs non-Blacks), (3) Education (college educated versus non-college educated, and (4) Work status (currently working vs unemployed). The results are reported in Table 5. We observe first that the coefficient estimates for women are statistically significant, while the estimates are slightly smaller for men, and not significant. This finding is not clearly conclusive, but our tentative explanation is that the effect on men is likely much more heterogeneous and mediated by many other factors. Columns 3-4 show that on average Blacks respond much less than non-Blacks, which is not inconsistent with the wealth hypothesis. Further, Columns 5-6 indicate that lower educated individuals respond much more strongly to the provision of full-day kindergarten, in line with the opportunity cost hypothesis. A similar result obtains when comparing those currently working and those not currently working (Column 7-8), although we caution that the potential endogeneity of the labor supply variable makes this result difficult to interpret cleanly.

Who receives the extra care?

The increase in the extensive margin of care supply as a result of kindergarten eligibility suggests that some elders who were not previously receiving care may now be receiving care. It is not possible to directly identify these individuals, but we can draw some inferences based on changes in the average characteristics of care recipients. In Table 6 we examine the effect of kindergarten eligibility on age of care-recipient (Column 1), at whether he/she is a household member (Column 2), and whether the he/she is the parent of the caregiver (Column 3). On all of these dimensions, we find little evidence that the new care recipients are systematically different from existing care recipients, although we caution that the sample size in these regressions severely limits our power to draw clear inferences.

Labor supply

Lastly, we examine the effect of full time kindergarten eligibility on labor supply, measured by employment status and by usual hours of work per week. Given the potential importance of the gender dimension in this aspect of the response, we present estimates for the full sample, as well as for men and women separately. The results are reported in Table 7. On both extensive as well as intensive margins of labor supply, we are unable to find any significant impacts of kindergarten access. Perhaps surprisingly, this result also holds in the female sample, where the coefficient estimates are negative (the estimated effects are positive but not statistically significant for males). These results are consistent with some recent studies of the causal effect of subsidized childcare on female labor supply (Fitzpatrick 2010, Havnes and Mogstad 2011). Whereas the potential impact on female labor supply has been frequently touted as a benefit of publicly funded pre-school, and older studies (e.g. Gelbach 2002) found evidence for such impacts, some more recent studies have failed to find significant labor supply responses. Possible explanations for the discrepancy include the hypothesis that female labor supply is less elastic now than before (Fitzpatrick 2010), and that publicly funded childcare is simply replacing privately purchased childcare (Bauernschluster and Schlotter 2015). On the basis of our results, one may even speculate that caregiving responsibilities have increased over time, so that increases in parental time are utilized in caregiving rather than increased work.

5 Conclusions

Understanding how to facilitate and/or increase the supply of informal care has become imperative, with a number of policy options being under active consideration, including flexible work arrangements, mandated (paid or unpaid) leave, and monetary compensation for informal caregivers. Common to these policies is the assumption that the essential trade-off confronted by potential caregivers is that between employment and caregiving; a trade-off that has been extensively studied in the empirical literature (the evidence is surveyed in Lilly et al 2007).

Relatively little attention, however, has been paid to the fact that individuals and households engage in yet another competing activity that is highly time-intensive: child care. An exception is Spillman and Pezzin (2000), who have drawn attention to the trilemma of the "sandwich generation" that is caught between the demands of caregiving, child-rearing and labor-force participation, noting in particular that the trend towards delayed fertility implies that potential caregivers (especially women) are increasingly likely to face simultaneous child care and elder care responsibilities. From a policy perspective, it is important to determine the extent to which individuals are trading off child care and informal care, and to assess whether child care subsidies, especially in the form of public pre-K and kindergarten access, will have traction in terms of inducing a greater supply of caregiving.

The contribution of this paper is to present the first estimates of the elasticity of informal care with respect to child care subsidies. We focus attention on the effect of access to public preschool (kindergarten). Our empirical design exploits the age-eligibility criterion for public kindergarten, in combination with state-level variation in the provision of full-day kindergarten. We find that kindergarten eligibility of the youngest child in states that offer full-day kindergarten sharply increases the probability of caregiving as well as the amount of time spent providing care. These effects are most pronounced among women, non-Whites, non-college educated, and those not currently working. A simple calculation suggests that the full-day kindergarten mandate has added approximately 33.3 million caregiving hours annually in the full-day states, amounting to a 2.4% increase. This is clearly a non-trivial impact, but we caution that the effects of implementing such a mandate in other states may not be quite as large. One particularly relevant difference that we have alluded to earlier is that the full-day states also have on average much worse long term care support systems, which may well account for the sharp increase in caregiving when the childcare constraint is relaxed. In states which provide better access to formal care, the caregiving response could potentially be much lower.

Consistent with some recent studies, we do not find any significant impacts on labor supply of either men or women. In theory, a child care subsidy could result in an increase in work as well as caregiving. In practice, though, both activities require large time commitments and an individual may end up choosing one or the other, unless the subsidy is very large. There is mixed evidence on the impact of child care subsidies in the form of access to public pre-school on female labor force participation, with some studies suggesting that (at least in recent years) there is little to no impact. These results raise the question of how parents spend the extra time that is released from childcare: Our results indicate that participation in informal care activities is one of the activities that are "crowded-in" by the provision of public kindergarten. Our study therefore has potential implications for the cost-benefit assessment of childcare subsidies.

A more general conclusion that emerges from our study is that policy evaluations need to carefully consider all the different margins along which individuals can respond to policy interventions, and to think through the implications of these various adjustments. Some other notable examples of such studies in the literature include Chari et al (2019), who examine the effect of employment policies on maternal child investments, and Kaestner et al (2017) and Pohl (2018) who examine the effect of health care policies on labor supply. In the context of kindergarten funding, attention has largely focused on learning outcomes, and on labor supply, but as we have shown, a broader perspective that takes into account the other kinds of time-intensive activities that individuals engage in reveals previously neglected spillovers that, when accounted for, can change the cost-benefit calculus associated with such policies.

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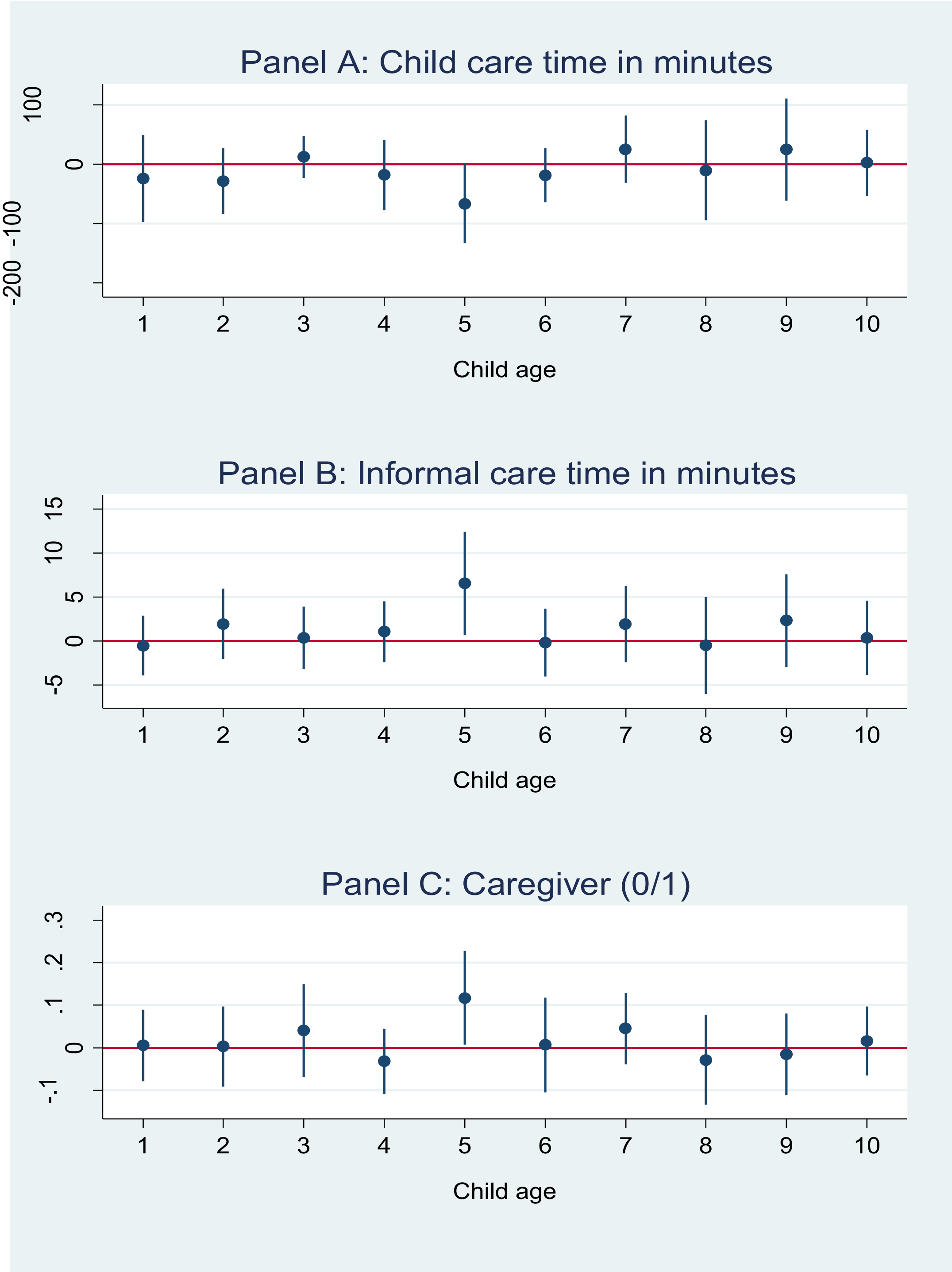

Figure 1. The figure plots the DID coefficients and 95% confidence intervals from an estimation of the effect of full-time kindergarten mandate for each age group relative to children who are 0 years old.

Table 1. Kindergarten enrolment rates by state groups

	1	2	3	4	5	6
	"No kindergarten" states	"Half-day" states	"Full-day" states	"Half-day" - "No kindergarten"	"Full-day" - "Half-day"	"Full-day" - "No kindergarten"
Full-time kindergarten enrolment rate	0.61	0.61	0.80	0.01	0.19***	0.18***
Part-time kindergarten enrolment rate	0.20	0.20	0.03	0.00	-0.17***	-0.17***

Notes: The figures represent the authors' calculations based on data from the October CPS of 2013. Cols 4-6 present pairwise mean differences and the asterisks denote statistical significance based on t-tests.

Table 2. Informal care provision and the presence of small children

	(1) Caregiving (0/1)	(2) Care time (minutes)
Children under 12 (dummy)	-0.051***	-13.421**
	(0.005)	(6.354)
Male	-0.036***	-9.169**
	(0.005)	(4.466)
White	0.025***	-7.090
	(0.005)	(9.325)
Age	0.001***	0.115
	(0.000)	(0.149)
Married	0.026***	-5.305
	(0.005)	(4.545)
Years of education	0.000***	0.006
	(0.000)	(0.023)
Constant	0.095***	111.554***
	(0.010)	(10.284)
Observations	56,283	2,146
Mean of dependent variable	0.193	104

Notes: Regressions are weighted using survey weights. All regressions control for state and year fixed effects, and standard errors (reported in parentheses) are clustered at state level. In Column 2, the sample is restricted to respondents who provided informal care in the 24-hour recall period.

Table 3. Summary statistics

	All states		Full-day states		Non full-day states	
	Mean	Std Error	Mean	Std Error	Mean	Std Error
Child care (minutes)	274.81	2.29	276.99	5.07	274.27	2.57
Elder care (minutes)	2.12	0.17	3.30	0.46	1.83	0.17
Caregiving (0/1)	0.15	0.00	0.17	0.01	0.14	0.00
Working (0/1)	0.71	0.00	0.71	0.01	0.71	0.00
Usual hours worked	28.21	0.19	27.82	0.42	28.31	0.22
Male	0.45	0.00	0.44	0.01	0.46	0.00
White	0.79	0.00	0.71	0.01	0.82	0.00
Age	35.64	0.10	35.76	0.22	35.61	0.11
Married	0.67	0.00	0.65	0.01	0.67	0.00
College educated	0.57	0.00	0.56	0.01	0.57	0.00

Notes: The sample is restricted to respondents with children under the age of 12 in the household. Non full-day states include half-day and no kindergarten states. Summary statistics reflect weighting using survey weights.

Table 4. Effect of full-day kindergarten mandate on child care and caregiving

	(1)	(2)	(3)
	Child care time (minutes)	Elder care time (minutes)	Caregiving (0/1)
Full day x I(Age=5)	-53.848***	6.345**	0.102**
	(15.025)	(2.416)	(0.047)
Observations	16,981	16,981	16,981
Mean of dependent variable	308.1	2.932	0.167

Notes: The table reports difference-in-difference estimates of the effect of the full-day kindergarten mandate. The sample is restricted to respondents with children under the age of 12 in the household. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Effect of full-day kindergarten mandate on caregiving: Heterogeneous effects

	(1) Male	(2) Female	(3) Black	(4) Non-Black	(5) College	(6) No college	(7) Working	(8) Not working
Full day x I(Age=5)	6.766	7.377**	-2.708	9.535***	2.608	7.245*	4.863	13.487**
	(6.823)	(3.414)	(5.443)	(2.815)	(2.197)	(4.135)	(3.867)	(5.954)
Observations	7,204	9,777	1,619	15,362	11,551	5,430	12,648	4,333
Mean of dependent variable	1.625	3.959	2.806	2.969	2.169	3.903	2.249	4.639

Notes: The table reports difference-in-difference estimates of the effect of the full-day kindergarten mandate, for different subgroups of the sample, denoted by the column titles. The sample is restricted to respondents with children under the age of 12 in the household. The dependent variable in all regressions is informal care provided in the recall period (in minutes). All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Table 6. Effect of full-day kindergarten mandate on composition of care recipients

	(1)	(2)	(3)
	Age	Household member (0/1)	Parent
Full day x I(Age=5)	-2.226	0.050	0.126
	(2.300)	(0.057)	(0.103)
Observations	2,432	2,432	2,432
Mean of dependent variable	72.13	0.159	0.445

Notes: The table reports difference-in-difference estimates of the effect of the full-day kindergarten mandate on the characteristics of care recipients. The sample is now restricted to informal caregivers whose youngest child is under the age of 12. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Table 7. Effect of full-day kindergarten mandate on labor supply

	(1)	(2)	(3)	(4)	(5)	(6)
	Working (0/1)			Hours of work		
	All	Men	Women	All	Men	Women
Full day x I(Age=5)	-0.003	0.058	-0.027	-0.454	1.399	-0.475
	(0.033)	(0.039)	(0.054)	(1.633)	(1.825)	(2.357)
Observations	16,981	7,204	9,777	16,981	7,204	9,777
Mean of dependent variable	0.714	0.834	0.620	26.66	34.25	20.70

Notes: The table reports difference-in-difference estimates of the effect of the full-day kindergarten mandate on labor supply. The sample is restricted to respondents with children under the age of 12 in the household. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1. Effect of full-day kindergarten mandate on caregiving: Oldest child

	(1)	(2)	(3)
	Child care time	Elder care time (in minutes)	Caregiving (0/1)
Full day x I(Age=5)	2.452	-1.346	0.018
	(24.884)	(1.266)	(0.040)
Observations	10,427	10,427	10,427
Mean of dependent variable	329	2.644	0.148

Notes: The table reports difference-in-difference estimates of the effect of the full-day kindergarten mandate, based on eligibility of the oldest child. The sample is restricted to households whose eldest child is under the age of 12. The time use variable (elder care time) excludes observations for which the diary day was a Sunday. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2: Effect of full-day mandate during summer months and weekends

Dependent variable: Elder care time (in minutes)	(1) Summer	(2) Non summer	(3) Weekend	(4) Weekdays
Full day x I(Age=5)	-0.113	8.434**	1.778	8.880**
	(2.081)	(3.348)	(2.388)	(3.846)
Observations	4,088	12,893	8,579	8,402

Notes: The table reports estimates of the effect of the full-day kindergarten mandate during summer and non-summer months and during weekends and weekdays. The sample is restricted to respondents with children under the age of 12 in the household. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3. Effect of full-day and half-day kindergarten mandates on child care and caregiving

	(1)	(2)	(3)
	Child care time (minutes)	Elder care time (in minutes)	Caregiving (0/1)
Half day x I(Age=5)	2.233	1.192	-0.011
	(15.352)	(1.179)	(0.021)
Full day x I(Age=5)	-51.163***	7.346***	0.101**
	(18.453)	(2.495)	(0.044)
Observations	16,775	16,775	16,775
Mean of dependent variable	308.9	2.868	0.168

Notes: The table reports difference-in-difference estimates of the effect of full-day and half-day kindergarten mandates. The sample is restricted to respondents with children under the age of 12 in the household. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4. Testing for changes in sample composition

	(1)	(2)	(3)	(4)	(5)	(6)
	Age	Black	Male	Education	Spousal age	Spousal education
Full day x I(Age=5)	-0.458	0.009	-0.046	-0.765	-0.129	-5.651
	(0.639)	(0.026)	(0.048)	(0.572)	(0.464)	(3.886)
Observations	16,981	16,981	16,981	16,981	16,981	16,981
Mean of dependent variable	36.01	0.712	0.440	28.46	304.6	306.2

Notes: The table reports difference-in-difference estimates of the effect of full-day kindergarten mandate on respondent characteristics. The sample is restricted to respondents with children under the age of 12 in the household. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, (excluding the outcome variable of interest), and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 5. Effect of full-day kindergarten mandate on caregiving: Restricted sample

	(1)	(2)	(3)
	Child care time (minutes)	Elder care time (in minutes)	Caregiving (0/1)
Full day x I(Age=5)	-57.647***	5.201**	0.084*
	(17.281)	(2.500)	(0.050)
Observations	9,758	9,758	9,758
Mean of dependent variable	315.4	2.982	0.158

Notes: The table reports difference-in-difference estimates of the effect of full-time kindergarten mandate, restricting the sample to children up to the age of 5. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6. Effect of full-day kindergarten mandate on child care and caregiving (all states)

	(1)	(2)	(3)
	Child care time (minutes)	Elder care time (minutes)	Caregiving (0/1)
Full day x I(Age=5)	-36.744**	5.254**	0.082*
	(17.833)	(2.348)	(0.047)
Observations	18,682	18,682	18,682
Mean of dependent variable	307.7	2.890	0.171

Notes: The table reports difference-in-difference estimates of the effect of the full-day kindergarten mandate. The sample in these regressions includes all states (i.e. including the states that offer publicly funded pre-kindergarten). The sample is restricted to respondents with children under the age of 12 in the household. All regressions are weighted using survey weights, and include state x year fixed effects, and a set of controls for respondent age, sex, race, education, marital status, and dummy indicators for diary day and month, as well as dummies for each of the child age groups. The standard errors (in parentheses) are clustered at state level.*** p<0.01, ** p<0.05, * p<0.1