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# ABSTRACT

## A Matter of Time? Measuring Effects of Public Schooling Expansions on Families*

We leverage pronounced changes in the availability of public schooling for young children—through duration expansions to the kindergarten day—to better understand how an implicit childcare subsidy affects mothers and families. Exploiting full-day kindergarten variation across place and time from 1992 through 2022 and novel data on state-level policy changes, combined with a comparison of children of typical kindergarten age to older children, we measure effects on parental labor supply and family childcare expenses. Results suggest that families are responsive to these shifts. Full-day kindergarten expansions were responsible for as much as 24 percent of the growth in employment of mothers with kindergarten-aged children in this time frame.

**JEL Classification:** H75, I28, J13, J22
**Keywords:** public schooling, kindergarten, maternal labor supply

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# 1 Introduction

Since 1970, mothers have steadily entered the labor market in large numbers, leading to employment rates on par with the employment of men and women with no children in the home (see Figure 1). Mothers have also proven to be strongly attached to the labor force, as evident in the rapid rebound in their employment from the COVID-19 pandemic-induced recession. This "quiet revolution" has contributed to a "grand gender convergence" in the labor market (Goldin, 2006, 2014). Despite the convergence in employment rates, the division of other household responsibilities has not converged. Today, mothers of young children are both more likely to be working and spending more time with their children than they were even a few decades ago, potentially creating a tension as mothers' time constraints become binding. If mothers are not time- or resource-constrained, changes in childcare policy and public schooling hours may have little to no impact on families. However, if mothers' constraints are binding, parental time use and family resources may respond to changes in the length of the school day. We leverage public schooling duration expansions across the U.S. in the form of full-day kindergarten provision to better understand how policy shifts that facilitate greater flexibility through increased hours of childcare coverage affect parents' time allocation and family resources.

Over the last 30 years, the share of kindergartners in full-day schooling in the United States has nearly doubled. In a relatively short period of time, states which previously supported only half-day kindergarten programs significantly changed course and began offering (and in many cases, mandated provision of) full-day kindergarten programs. Once the exception, full-day kindergarten has become the normative kindergarten context, with over 80 percent of kindergartners enrolled in full-day programming. This growth is perhaps one of the most dramatic, but least examined, changes to the delivery of early childhood education in the United States. Not only does public kindergarten facilitate investments in children's human capital, it also frees up time and resources for parents who would otherwise provide for children's care (Cascio, 2009).

Economic theory predicts that expansions in early childhood education increase maternal labor supply, given the implicit childcare subsidy (Cascio, 2009). The degree to which families can respond, however, depends in part on the length of the school day. Compared to full-day programming, half-day kindergarten (or preschool) is less conducive to work and often requires families to pair schooling with other child care and midday transportation. To date, extant literature on publicly provided early childhood education in the United States has primarily evaluated expansions in program availability, without sensitivity to program hours (Cascio, 2009; Cascio and Schanzenbach, 2013; Fitzpatrick, 2010, 2012; Gelbach, 2002), including recent work (Humphries et al., 2024; Ilin et al., 2022). With the exception of Fitzpatrick (2010) who provides separate results at the state level, these studies average across programs without exploring variation in the

length of the school day. These studies primarily evaluated time periods prior to or at the onset of full-day kindergarten provision and were not situated to capture the widespread shift to full-day kindergarten. Recent research has started to explore the role of the length of the school day for select samples of preschool-aged children on maternal labor supply (Dhuey et al., 2021; Wikle and Wilson, 2023). Still with select samples, general questions remain about how more generous programming hours in kindergarten may interact with family decision-making for families. We provide the first large-scale study that can causally identify the role of full-day kindergarten expansions across the United States on parents' time and resources.

This study explores the impact of the substantial full-day kindergarten expansions across states and over time on parents and children. We first show descriptively when, where, and among whom the shift to full-day kindergarten participation is the most pronounced. Considering that access to full-day kindergarten for children may change the way parents allocate time and resources, we then show that kindergarten length of day matters for families' time allocation. Given these descriptive patterns, we next use a triple-difference approach that leverages the substantial variation in full-day kindergarten exposure across states and over time, as well as age eligibility for kindergarten, to isolate the effects of full-day kindergarten expansions on parents'—and in particular, mothers'—labor supply and childcare expenses. Additionally, because states enacted policies to expand full-day kindergarten, we leverage newly collected data to construct a state-by-year policy measure to instrument for the full-day kindergarten share and isolate changes in enrollment due to the policy setting. In supplemental analyses, we use the same empirical approaches to explore impact on children's subsequent academic performance. While this work speaks directly to the broader impact of full-day kindergarten policy, it also pertains to the likely effects of many federal, state, and local education policies regarding preschool provision, time in school, and school schedules on parents and families.

Full-day kindergarten expansions have led to widespread and dramatic changes in the kindergarten landscape. Indeed, between 1992 and 2022, many states moved to expand funding for full-day kindergarten, and the share of public-school kindergartners nationally in full-day programming nearly doubled, from 43 to 83 percent. Perhaps surprisingly, early adoption of full-day kindergarten was not driven by states with other, progressive early education policies. The trend began largely in the South, followed by adoption in the Northeast and Midwest, with full-day kindergarten participation lagging in the West. It is also of note that trends in full-day kindergarten participation are similar across maternal marital status, education level, and child race/ethnicity. There has been a corresponding increase in state-level public policy and public resources devoted to full-day kindergarten. This increased access to longer kindergarten programming has the potential to change the time allocation of mothers and fathers.

Using the American Time Use Survey, we next document, descriptively, how access to full-day kindergarten corresponds with time allocation of mothers and fathers. Full-day kindergarten access is associated

with stark differences in time at work, housework, time spent with children, and time traveling with children. Among mothers with children in full-day kindergarten, patterns of time use are more similar to those of mothers with older school-aged children, while mothers with half-day kindergartners work less throughout the day, spend more time with children (particularly between 8 AM and 3 PM), and spend significantly more time with children in the car between 11 AM and 1 PM. Meanwhile, fathers' time with children does not differ by full-day kindergarten status, and fathers with half-day kindergarten students actually spend more time at work than fathers of full-day kindergarten students. These time use patterns suggest that the expansion of full-day kindergarten has relaxed time and resource constraints of mothers in ways that facilitate greater flexibility to participate in the labor market.

We next provide causal evidence on how full-day kindergarten expansions have affected mothers and their ability to engage or engage more intensively in the labor market. We compare labor supply outcomes of mothers with kindergarten-aged children to similar mothers in their state and metropolitan area with older school-aged children to see if increased access to full-day kindergarten affects the labor supply behavior of mothers with kindergartners. Our identifying assumption is that employment outcomes of parents with kindergarten-aged children would have evolved similarly to employment of parents with older children in the same local labor market if access to full-day kindergarten had not expanded.

We observe increases in the likelihood that mothers worked at all in the last year, which is split between increases in both part- and full-time work, and more hours and weeks worked. A 10 percentage-point increase in full-day kindergarten enrollment increases maternal employment in the last year by 0.43 percentage points. These patterns are consistent with the descriptive time use evidence which shows that during school hours, mothers of full-day kindergartners spend less time with children, provide less midday transportation for them, and instead use school hours to work more. Increases in full-day kindergarten enrollment have no corresponding effect on the labor supply of fathers. Heterogeneity analyses suggest that employment effects were fairly broadly realized and not solely concentrated among disadvantaged mothers, as prior research has seen for other public schooling expansions. This finding aligns with the fact that full-day kindergarten expansions generally pull more advantaged populations into full-day settings, as full-day kindergarten had previously been a targeted intervention aimed at disadvantaged children.

These estimates are robust to including individual and family controls, controlling for changes in preschool availability, restricting to the pre-COVID time period, or restricting to the non-South. Additionally, to highlight the policy impacts of expanding this form of early childhood education, we use state-level full-day kindergarten policy changes to instrument for full-day kindergarten access, and results of this analysis are similar.

Both the increase in maternal employment and the descriptive time use patterns suggest that for many

households, access to publicly provided child care relaxes mothers' time constraints. If full-day kindergarten had not expanded, many mothers would have provided the additional child care. In addition, data on consumer expenditures suggest that for some families, public schooling for young children plays a central role in lowering childcare costs (Cascio, 2017). We find some suggestive evidence that the share of households with childcare expenses fell, but only for families without younger children. The shift to full-day kindergarten provision — and the accompanying reallocation of mothers' time — sheds light on how family childcare needs affect men and women differently, providing background to the additional obstacles women face in balancing labor force participation and parenthood.

Ultimately, changes in investments in children both at school and at home have implications for child development. Full-day kindergarten access is associated with less mother-child time and more maternal employment. These effects of changes in time use, family resources, and time in school could be countervailing. As this is an empirical question, we also explore the net effects on children's subsequent academic achievement. These supplemental analyses are constrained by the fact that we only observe child outcomes in the form of standardized test scores in math and reading/language arts beginning in third grade, three years removed from exposure to kindergarten length of day. In addition, we have comparable test score measures across states and over time for a more limited time period, 2009 to 2019. Using similar identification strategies to explore the effect of full-day kindergarten access on children's later test scores, we find suggestive evidence of durable positive effects on students' third grade reading test scores, with particularly sizable effects for Hispanic students, boys, and those who qualify for free or reduced-price meals. As such, it appears that the trade off between in-school time and maternal time investments associated with full-day kindergarten access does not harm children's academic outcomes in the short run, and may in fact boost performance, particularly in reading and for certain subgroups.

The paper proceeds as follows. Section 2 discusses the rationale for studying this question, reviews previous related research, presents trends in full-day kindergarten access, and documents how parental time use differs by their children's full-day kindergarten status. Section 3 details the analysis of parental labor supply, including the data and empirical strategy. In Section 4, we summarize the results of the labor supply and childcare expenses analysis for parents, and in Section 5, we discuss implications for child outcomes. Section 6 concludes.

## 2 Motivation

Economists have long been interested in how women allocate time and effort to household production and market-based employment (Becker, 1981; Goldin, 2006, 2021). As women, and mothers in particular, have

participated in the labor market at increasing rates, their decisions around balancing family life and work have become more pertinent. Gender differences in the demand for flexible work arrangements have been pointed to as a potential cause that there has not been complete convergence in the gender gap (Goldin, 2014). Because parents usually cannot care for children and generate income at the same time, parents make trade-offs between work and time spent with children, both of which are important inputs into child development (Becker, 1981). Parents use strategies to optimize child development, such as establishing a division of labor between market and non-market work (with mothers most often focusing on home domains), reducing other types of time use, utilizing paid child care, and leveraging access to public school for children (Del Boca et al., 2014). While increases in women's labor force participation could crowd out parental time investments in children, trends in parental time use illustrate that — across the distribution of family socioeconomic status — parents are also spending more time with their children (Prickett and Augustine, 2021; Wray et al., 2021). Over the past three decades, the time parents devote to child care (i.e., focused time with children) has been increasing in the United States (Bianchi et al., 2006; Sayer et al., 2004) as well as in many Western countries (Dotti Sani and Treas, 2016; Wray et al., 2021) as mothers have substituted away from housework and leisure (Sayer, 2016).

Abundant evidence demonstrates that mothers' time use and labor force attachment track closely with their children's ages and schooling trajectories (Goldin et al., 2022; Price and Wasserman, 2024). Existing evidence suggests that public schooling can function as subsidized child care, pointing to the expansion from half to full-day provision as a factor that could affect mothers' labor force participation. In this vein, estimated childcare cost elasticities of maternal employment range from 0 to approximately -1, with the most credible estimates between -0.1 and -0.5 (Anderson and Levine, 1999; Morrissey, 2017). Overall, subsidization of child care for young children contributes to increased maternal employment (Blau and Currie, 2006; Blau and Tekin, 2007; Herbst, 2010; Tekin, 2007).

While overall childcare cost elasticities are relevant, there is also evidence that speaks to the effects of public education for children on women's labor force attachment and generally finds that school enrollment increases maternal labor supply, often concentrated among certain subgroups of women (Barua, 2014; Cascio, 2009; Gelbach, 2002). Using birthdate-based school enrollment rules, Fitzpatrick (2012) finds that the labor supply of single mothers of five-year-olds without additional young children increases as a result of a child's enrollment in public kindergarten, but is unchanged for other mothers. Similarly, evidence from expansions to Head Start in the 1990s and from the randomized Head Start Impact Study shows that access to the federally-funded preschool program for young children from disadvantaged households increases employment and earnings of single mothers (Wikle and Wilson, 2023). Cascio and Schanzenbach (2013) find a positive effect on maternal employment among mothers of four-year-olds, as compared to mothers of five-year-olds,

when universal preschool was introduced in Georgia and Oklahoma, concentrated among mothers with high school diplomas or less schooling. Extant literature therefore supports a broad notion that publicly provided early childhood education impacts mothers and families. While full-day kindergarten was beginning to expand during the sample periods for some of these studies, the study time periods did not correspond with large expansions in the length of the school day. Most of the studies pooled across half- and full-day programs to give average effects. This literature provides an important foundation for the current study by evaluating the role of program access, which continues to be a salient margin to consider in policy conversations. Nonetheless, this literature does not engage with questions about the intensity of the treatment as a key determinant of family-level responses.

Building on this foundation, it is important to consider how program structure may also be relevant to family time and resource constraints, and limited evidence exists on how families adjust to the more specific context of a longer school day for children. Cascio (2009) documents the early rise in full-day kindergarten availability and conceptually ties that to increases in full-time employment for mothers but does not explicitly evaluate the connection between the two.[1] Cannon et al. (2006) use variation in full-day kindergarten exposure within a nationally representative 1998–99 kindergarten cohort in the U.S. to explore associations with maternal employment and children's early academic performance, documenting correlations with short-term outcomes, including mothers' increased likelihood of full-time work during the kindergarten year. Dhuey et al. (2021) exploit the rollout of universal full-day kindergarten in Ontario, Canada (which corresponds to 4-year-old preschool in the U.S.) to find that mothers increase hours worked and decrease work absenteeism. This approach faces the common concern that the timing of policy adoption might be correlated with other factors affecting maternal labor supply, which we address through a triple-difference approach. Additionally, these existing efforts leverage different contexts, e.g., more limited geography and an earlier time period, leaving unresolved the question of how pronounced full-day kindergarten policy changes in the U.S. over three decades have affected families.

Evaluating maternal labor supply responses to Head Start access using a randomized control trial, Wikle and Wilson (2023) find stronger labor supply responses among single mothers whose children are eligible for full-day programming. However, in this study, program access is the only margin of randomization, and the length of the school day is not randomized. Therefore, findings related to full-day programs do not have a causal interpretation. Additionally, the age targets and means-tested nature of Head Start limits generalizability of the findings to other ages and income groups. Similarly, Humphries et al. (2024) investigate the impact of pre-K access on parental employment, documenting positive effects in a current

[1] Some of the early work exploring the maternal labor supply effects of kindergarten included full-day programming in the early-adopting South, but does not explicitly explore differences in program length (Fitzpatrick, 2010; Gelbach, 2002).

context, and while the extended duration of programming likely plays an important role, the authors are not able to formally test that hypothesis. While each of these studies adds valuable information on maternal labor supply patterns when the school day is lengthened, there remains a need for a large-scale well-identified study of the importance of program hours on parents' time and resources.

Parents themselves express that their decision to utilize full-day kindergarten relates to both perceived benefits to their children as well as family resource constraints related to child care, transportation, midday transitions, stress, and work (Boardman, 2005; Elicker and Mathur, 1997; Rothenberg, 1995). Employed parents, in particular, report reduced parenting demands when full-day kindergarten becomes available (Stover and Pelletier, 2018). The full-day kindergarten schedule typically aligns with older siblings' school schedules, allowing parents to streamline transportation and out-of-school child care (i.e., before- and aftercare) for all school-age children. The existing evidence is certainly suggestive that full-day kindergarten expansions to public schooling function as subsidized child care, potentially affecting the labor market attachment of marginal mothers and childcare expenses for families already attached to the labor market.

Investment in early childhood has gained significant traction in recent years as an efficient and equitable means to invest in young children, and particularly children in under-resourced environments (Currie, 2001; Heckman, 2000; Heckman and Masterov, 2007). The developmental literature has coalesced around the notion that children experience declining developmental plasticity and thus early investments—by altering cognitive and social skill development when the brain is most malleable and able to adapt its functioning—are more likely to substantially and permanently affect long-term life chances (Knudsen et al., 2006; Shonkoff and Phillips, 2000).

There is also a growing body of empirical evidence that early childhood programs reap long-term effects for participants, generating substantial private and social returns that far outweigh program costs. Long-term evidence from the Abecedarian Project, Boston pre-kindergarten program, Head Start, the Perry Preschool Project, and the Project STAR class-size reduction intervention suggests that interventions in the preschool and early school years can have substantial effects on educational attainment and labor market success (Chetty et al., 2011; Deming, 2009; Dynarski et al., 2013; Gray-Lobe et al., 2022; Schweinhart et al., 2005). Benefits also include better health, reduced criminal engagement, and improvements in other measures of well-being in adulthood (Bailey et al., 2021; Campbell et al., 2014; Chetty et al., 2011; Schweinhart et al., 2005). While the evidence base on participant effects for children is growing, more research is needed to better understand the effects of such interventions on the parents and families of those exposed, particularly in current contexts and in light of large and growing public investments in the early childhood years more recently.

To more fully understand the relationship between full-day kindergarten policies and household decision-

making, this study seeks to explore the effects of expansions on families, through maternal employment, childcare expenses, and child outcomes. This study complements and extends the existing literature in a few important ways. First, this study is the first large-scale evaluation of parental responses to full-day kindergarten provision, which may differ fundamentally from half-day kindergarten because it provides more time in school. Given the widespread recent shifts to full-day kindergarten in the United States, the move to full-day kindergarten is a valuable context to explore the importance of program structure and hours (moving beyond program availability) for families. The research design relies on a quasi-experimental approach, leveraging differential exposure to kindergarten policy changes by geography, over time, and relative to similar families with older children to generate plausibly causal estimates of program impacts. We also use policy instruments to capture plausibly exogenous changes in access. The study focuses on compositional changes in full-day kindergarten attendees to better understand who is affected by policy changes, particularly when an intervention shifts from a targeted approach to near-universal availability. Finally, we focus on a range of outcomes to better understand the interaction of full-day kindergarten policies and families' decision-making and resource allocation. Time use, maternal employment, childcare expenses, and children's academic progress are important pieces to fully capture how early childhood education policy affects children and families. No work to date has leveraged the large changes in full-day kindergarten policy across the U.S. in the 1990s and 2000s to look at these outcomes of interest.

## 2.1 Full-day Kindergarten: Trends and Policy Context

Full-day kindergarten as a policy lever has been an area of considerable activity over the past three decades. Before exploring how these changes shape family decisions, we first provide context about when, where and for whom kindergarten expansions were most dramatic. Although the federal government releases administrative data on total state-level kindergarten enrollment, these data do not differentiate between full-day and half-day enrollment. There is no administrative data source that provides full- and half-day kindergarten enrollment separately. To document the expansion in full-day kindergarten enrollment we rely on the Current Population Survey (CPS) October School Enrollment Supplement (Flood et al., 2023b). The October Supplement is completed by 100,000 to 150,000 individuals each year in October and includes schooling related questions, including the grade or level of schooling in which household members are currently enrolled. Importantly, full-day and half-day kindergarten are reported as separate levels. As such, we can estimate state-level averages and trends over time in the share of kindergartners enrolled in full-day kindergarten. On average, just over 1,800 kindergarten students are included each year. Because of the small kindergarten sample, we do not estimate full-day enrollment rates for geographic areas smaller than states. We also construct

three-year rolling averages (-1,0,+1) to reduce measurement error from small samples in some states. We exploit the October Supplement data to document descriptively the changes in the public school, full-day kindergarten landscape over time. More details related to the October School Enrollment Supplement of the CPS are included in the Data (Appendix B).

The structure of kindergarten has changed significantly over time. As depicted in Figure 2, full-day kindergarten eclipsed half-day provision as a proportion of public kindergarten enrollment in 1995 and now constitutes greater than 80 percent of kindergarten enrollment in both private and public schools.[2] There has not been a corresponding change in full-day preschool enrollment during this time period. Notably, recent increases in full-day kindergarten access are driven by expansion in public schools (Appendix Figure A2). The majority of students shifted from public half-day kindergarten, although there have also been small shifts from private half-day kindergarten and preschool (Appendix Table A1).[3]

Both levels and trends of full-day kindergarten vary by Census region (Figure 3). Ever since 1990 the full-day kindergarten share in the South has been high, starting at 72 percent, and rising to 86 percent by 2019. The growth in the other Census regions has been more pronounced. Full-day enrollment in the Northeast started at a moderate level in 1990 (42 percent) and by 2019 had slightly surpassed the South at 88 percent. The West and the Midwest both had low full-day kindergarten shares around 20 percent in 1990, and full-day enrollment in the Midwest has grown rapidly to match the South and Northeast at 86 percent in 2019, while full-day enrollment has grown at a slower rate in the West, only reaching 71 percent by 2019. These regional trends mask considerable variation across states, with many Southern states already having complete full-day enrollment at the start of the period, and some states in the West (Idaho and Utah) never reaching 50 percent full-day enrollment shares (Appendix Figure C1).[4]

The increase in full-day enrollment is demographically broad based. As seen in Figure 4, the patterns are fairly similar by race, maternal marital status, and maternal education.[5] Non-Hispanic Black children and children with never married mothers have higher full-day enrollment shares initially, but these trends converge by 2019. These patterns are consistent with the shift from full-day kindergarten as a policy intervention targeted towards low-income households to a universal policy.

To a large extent, the increase in full-day kindergarten enrollment is correlated with state-level policy, which we identified and compiled in a new database covering all state policy changes from 1990 through the present. Alongside the policy variation we document—the number of states requiring districts to pro-

[2]Patterns are similar if we look at the share of 5-year-olds enrolled in public, full-day kindergarten, suggesting this is not driven by increases in kindergarten attendance (Appendix Figure A1).

[3]We find no evidence of the public full-day kindergarten expansion causing substitution away from private full-day kindergarten.

[4]We do not find evidence that the rapid expansion in full-day kindergarten was accompanied by a change in the composition of kindergarten teachers (Appendix Figure A3).

[5]Given the higher initial levels in the South we plot demographic trends separately for the South and Non-South.

vide full-day options and the number of states passing other legislation that encourages full-day kindergarten provision—we see a corresponding rise in state-level, full-time equivalent kindergarten teachers over this time period (Figure C1). There was brief federal attention on full-day kindergarten during the Obama Administration, but all policy activity on full-day kindergarten has taken place at the local and state levels. Several states have made considerable legislative efforts towards full provision of full-day kindergarten, including Arizona, Indiana, Ohio, Oregon, and Minnesota. While only 11 states formally define full-day kindergarten in state statute, 24 states specify a funding formula that funds full-day kindergarten at or above the level of funding for first grade (Parker et al., 2016). Interestingly, as full-day kindergarten participation has increased substantially over time, other aspects of the early childhood experience have remained relatively stable, including preschool and Head Start participation (see Figure 2).

## 2.2 Full-day Kindergarten and Parental Time Use

Given the dramatic changes in the share of households who have access to full-day kindergarten, often as a result of state-level policy, it seems plausible that this would affect the way that families, and particularly primary caregiver parents, spend their time. To descriptively explore how having a child in full-day versus half-day kindergarten relates to mothers' and fathers' time use on school days, we use data from the American Time Use Survey (ATUS) from 2003-2019 (Flood et al., 2023a).[6] We provide estimates of mothers' and fathers' time use if they have a child in full-day kindergarten, half-day kindergarten, or second grade (for reference). To be clear, evidence from these time use data are not causal and are provided only to illuminate patterns observed in families. As attending full-time kindergarten is not mandatory, full-time working parents might opt into it more than those families where one parent is not working. These estimates capture both selection and any treatment effect associated with attending full-day kindergarten.

We see descriptive evidence that a child's kindergarten context corresponds to large differences in parental time use on school days, as seen in Table 1. Mothers whose children attend a full day of kindergarten work more, spend less time in home duties, and spend less time with children than their counterparts with children in half-day kindergarten (see column 7). Mothers of full-day kindergarten children look most like mothers of second graders who have access to full-day school for their older children. For example, there is less difference in market work time and no difference in home duties when comparing mothers of full-day kindergartners and mothers of second graders. Although overall time with children differs for mothers of half- versus full-

[6]The ATUS sample used in this study includes parents with a full-day kindergartner, half-day kindergartner, or second grader (for comparison) based on a linked October School Enrollment Supplement from the CPS. In addition, the sample is restricted to parents who were surveyed on weekdays during the school year. This left a sample of 1,179 mothers and 790 fathers. Demographic characteristics of parents of half-day kindergartners and parents of full-day kindergartners were generally similar. Detailed information about the sample and time use variable construction is found in the Data Appendix (Appendix B).

day kindergartners, most measures of quality time investments thought to be important for development are not sensitive to a child's kindergarten context. Daily one-on-one time, quality time, reading, physical care, academic time, time spent in direct child care are the same for all mothers of kindergartners. Mothers of full-day kindergartners spend more time with children than mothers of second graders overall, and some of the gap can be attributed to known decreases in parental time with children as children age (Wikle and Cullen, 2022).

Table 1 suggests that when a family has a child in half-day kindergarten, mothers rather than fathers are the ones who usually show increased time investments in child and home domains and are less attached to market work. In the absence of publicly provided full-day kindergarten, mothers spend more time with their children and spend more time performing home duties. When mothers have access to public schooling, time with children is lower, work time is higher, and other domains of time use remain stable. Sleep, leisure time, personal care time and time in activities focused on care for children and adults (e.g. feeding) remain similar for mothers of half- and full-day kindergartners. Gender gaps in work time are largest among parents of half-day kindergartners compared to parents of full-day kindergartners, as highlighted in column 9 (with mothers providing less work time than fathers). Gender gaps are also larger among parents of half-day kindergartners for home duties and time with children (with mothers spending more time in these activities than fathers). Families of full-day kindergartners have more egalitarian time use between mothers and fathers. Some of the narrowing gender gaps associate with mothers' differences. Importantly, fathers of full-day kindergartners also contribute to lower gender gaps. As mothers work more, fathers of full-day kindergartners work less and spend more time with children than their counterparts with half-day kindergartners.

Differences in overall time prompt questions about family tempo—whether parent-child interactions and reduced work hours seen for mothers of half-day kindergartners happen during typical school hours. Figure 5 delineates time use throughout the day to emphasize differences in timing. We see that mothers of full-day kindergartners are over 10 percentage points (30 percent) more likely to work during school hours than mothers of half-day kindergartners and only slightly less likely to be working than mothers with second graders. The patterns in maternal employment converge around 5pm, with slightly more mothers of half-day kindergartners working in the evening than mothers with full-day kindergartners. Higher evening work time among mothers of half-day kindergartners (when a partner may be available to care for children) may point to time constraints during the school day faced by these mothers. For fathers, the pattern reverses, with fathers of half-day kindergartners about 10 percentage points more likely to be working during school hours than fathers of full-day kindergartners, perhaps to compensate for mothers' lower levels of labor force participation or because parents that specialize are more likely to select into half-day kindergarten.

The difference in time with children during school hours between mothers of full-day kindergartners

and half-day kindergartners suggests that families of half-day kindergartners do not completely replace the missing school hours with out-of-home child care.[7] For fathers of full- versus half-day kindergartners, we observe virtually no differences in time with children throughout the school day. One of the most stark differences between full- and half-day kindergarten families is in midday travel. Interestingly, mothers of full-day kindergartners do not often travel with children in the middle of the day, whereas mothers of half-day kindergartners are two or even three times more likely to be traveling with children between 11am and 1pm, suggesting differences in transportation needs in families with half-day kindergartners. Fathers do not differ in their midday travel patterns based on kindergarten context.

These connections between full-day kindergarten and parent time use are descriptive, but nonetheless they provide suggestive evidence pointing to family level responses to full-day kindergarten. They combine both differential selection between full-day and half-day kindergarten families and any effect of full-day kindergarten access on time use. However, these patterns intimate that parents' time use, and particularly mothers', is sensitive to a child's kindergarten schedule. The half-day kindergarten schedule might introduce constraints that affect mothers' ability to spend their time in other activities, such as market work. This interruption may be particularly pronounced in current contexts where there exist more full-day early childhood care options prior to the start of formal schooling. We turn attention now to quasi-experimental models to shed light on how full-day kindergarten access affects parental labor supply, and family childcare expenses.

# 3 Empirical Approach

We investigate the impact of greater full-day kindergarten provision using variation in the full-day kindergarten share across states and over time, and by comparing parents of kindergartners to parents of older children. As much of the variation in full-day kindergarten provision is induced by state-level policy changes, we also provide estimates where we exploit variation in when states implemented policies to scale up (immediately or over time) provision of full-day kindergarten.

## 3.1 Data

To explore the impact of full-day kindergarten expansions on outcomes, this study uses data from several sources. First, to understand parental labor supply responses to full-day kindergarten access, we use the 1992-2022 repeated cross-sections of the Annual Social and Economic Supplement (ASEC) of the CPS, a detailed survey of more than 75,000 households conducted each March, for employment outcomes (Flood et

[7] Much of the difference in mother's time with children is driven by time with the focal kindergartner, not siblings.

al., 2023b). We restrict the sample to parents with a child between the ages of five and nine in the home, and examine patterns separately for men and women. Because we do not observe children's exact dates of birth, we use the household roster to identify parents who have a kindergarten-aged child in March (ages five and six) or an older, grade school-aged child (ages seven to nine).[8] By construction, comparison families in our sample do not also have a kindergarten-aged child (although they may have a child younger than five). We use older children as a comparison group because their school and home arrangements are typically more homogeneous compared to children younger than kindergarten. Older children's school context closely matches the school timing of full-day kindergartners, further making them a logical comparison group.

In Table 2, we present summary statistics for several observable characteristics for these two groups of mothers. The groups are similar as measured by race and citizenship, but differ along other dimensions. One notable, but expected, difference is that mothers with kindergarten-aged children are about 2.7 years younger on average. Other differences between groups, in marital status and educational attainment, comport with a life-cycle model of human capital acquisition and family formation. Also consistent with the time use data, mothers with kindergarten-aged children are on average less attached to the labor market. Our identification relies on the comparison of outcomes between parents with kindergarten-aged children, and parents with older children in the same metropolitan area, or local market. As seen in column (3) of Table 2, even when average characteristics of the two groups within the same labor market are statistically different, the levels are generally similar, with non-employment measures no more than four percentage points apart. Although our identification strategy does not rely on comparability of levels, similarities in observable characteristics lend credence to the key identifying assumption that the older group constitutes a good counterfactual for changes in affected families' outcomes. Our empirical approach augments these comparisons with the additional layer of time-varying exposure by place and child age to the treatment. We merge the individual ASEC observations in March to the state-level, public full-day kindergarten enrollment share constructed from the CPS October School Enrollment Supplement in the preceding calendar year, to capture enrollment in the same academic year. Because dramatic policy shifts over our time period expanded public full-day kindergarten availability, we focus on the share of *public* school students in full-day settings to exploit changes in access to full-day kindergarten isolated from endogenous, individual-level decisions about kindergarten programming.

In the ASEC, we observe two types of labor supply outcomes. First, we observe the individuals' reported

[8] Age in this study is measured in March. Some of the 6-year-old children observed were 5-years-old at the beginning of the school year, likely to be in kindergarten and just turned 6-years-old before March whereas some 6-year-old children at the time of the ASEC are in first grade. Likewise, some 5-year-old children at the time of the ASEC are in preschool. Because of imperfect alignment between a child's age and grade in school, some children who we classify as age-eligible for kindergarten are not in kindergarten, making our age targets rough and likely attenuating our estimates. Including untreated children in our "treated" group works against finding a result, and because of the sample construction, we view our results as a lower bound on the full-day kindergarten effect.

employment status and hours worked in the week prior to the survey (the ASEC is conducted in March). Second, we also observe retrospective outcomes relating to the individual's labor supply during the previous calendar year, including whether the individual worked in the last year, whether or not the individual worked full-time in the previous year, weeks worked in the past year, and individual wage income during the previous calendar year.[9] We also observe demographic measures including race, marital status, and educational attainment.

The study also relies on the 1990-2021 CPS October School Enrollment Supplement for full-day kindergarten data, as described above. Our treatment variable is a measure of local full-day kindergarten availability for each family. We use the three-year rolling average proportion of public full-day school kindergarten students in each state for each year constructed from the CPS October Supplement. This measure provides a state-by-year measure of full-day kindergarten access. Combined with within-place variation, we estimate how increased full-day access affects parental labor supply.[10]

## 3.2 Estimation Strategy

As seen in Figure 2, there has been a dramatic increase in full-day kindergarten enrollment over the past 30 years, with the share of kindergartners in full-day kindergarten nearly doubling between 1990 and 2020. Kindergarten teacher-student ratios and the presence of state-level full-day access mandates or other full-day-friendly policies have experienced a similar trend in growth over this same time period. This study employs a quasi-experimental approach, leveraging variation in the provision of public full-day kindergarten across states and over time. These two levels of variation (time and space) correspond to a difference-in-differences model comparing mothers of kindergarten-age children who experience different levels of full-day kindergarten provision. One concern with this approach is that the states and labor markets that expand full-day kindergarten provision are selected in ways that are correlated with potential outcomes, such as maternal employment or children's test scores. For example, people in areas with upward trending female labor force participation might lobby state leaders for broader full-day kindergarten access, which could introduce bias into our estimates.[11] To overcome this potential bias, we appeal to a third level of variation (child age) and exploit within-state and within-local labor market variation in whether or not a given family has access to full-day kindergarten driven simply by children's ages. We explore how state-level increases in

[9] The retrospective employment questions ask respondents to report on the previous calendar year, which overlaps but does not perfectly align with the academic year. Mothers with children entering kindergarten had about four months of the previous calendar year when children were attending kindergarten. As a result, the estimated impacts on "last year" outcomes are attenuated.

[10] Details related to our use of the CPS ASEC and October Supplement are included in the Data Appendix (Appendix B). Time use outcomes using the ATUS are not evaluated using a triple-difference strategy due to small sample sizes.

[11] Alternatively, families with different preferences around labor force participation may endogenously move in response to expansions in full-day kindergarten, a concern we address with a robustness check focusing on non-movers.

full-day kindergarten provision affect labor supply decisions of parents with kindergarten-aged children (ages five and six), relative to parents with slightly older children in first, second, and third grade (ages seven through nine).

Our baseline specification for assessing the impact of full-day kindergarten expansions on parental labor supply is as follows

$$Y_{imst} = \beta_0 + \beta_1 FullDayK_{st-1} * (Have\ Child\ 5\text{-}6) + \gamma_{st} + \delta_s * (Have\ Child\ 5\text{-}6) + \lambda_{mt} + \varepsilon_{imst} \qquad (1)$$

where $Y$ is the employment outcome for an individual $i$ in metropolitan areas (MSA) $m$ in state $s$ in year $t$. *FullDayK* is the treatment variable measured in the prior fall $(t-1)$, which measures the proportion of public kindergarten students in full-day settings in a particular state and year. This is interacted with a binary indicator variable, *Have Child 5-6*, that equals one if the parent has a child that is 5- or 6-years-old. State-by-year fixed effects ($\gamma$) control for time varying trends within the state, while state-by-child age fixed effects allow for level differences between mothers with 5- or 6-year-olds and mothers with older children to vary across states. These fixed effects absorb both the direct effect of having a 5- or 6-year-old and the direct effect of the full-day kindergarten share. MSA-by-year fixed effects ($\lambda$) are included, making this a comparison between parents in the same MSA at the same time. As such, any local trend in labor markets or gender norms that might affect the outcome and the provision of full-day kindergarten is held constant. The sample is limited to parents with a child between the ages of five and nine. As such, the coefficient $\beta_1$ is an intention-to-treat estimate of the effect of the full-day kindergarten share increasing for families with a kindergarten-aged child, relative to families in the same market with an older child. The full-day kindergarten share ranges from 0 to 1. Observations are weighted using the March ASEC sampling weights. Standard errors are heteroskedasticity-robust, clustered at the state level.

This specification relies on a triple-difference approach to isolate effects. The identifying assumption is that parents of kindergarten-aged children would have experienced the same trends in employment as parents of older children in the same labor market if the increase in full-day provision had not occurred. Many of the drivers of full-day kindergarten provision may be correlated with geography, but this strategy holds those characteristics fixed within a given metropolitan area. Threats to this strategy would have to be concurrently timed with kindergarten expansions in affected states, and also be targeted to families with or parents of kindergarten-aged children.

Analyses are conducted separately for mothers and fathers to explore differential responses by parent

gender. The estimates from equation (1) are provided in Table 3.[12] Subgroup estimates of the effect on employment for different groups of mothers are provided in Figure 7.

### 3.3 Alternative Approach – Policy Instruments

To further assuage concerns about the exogenous nature of local penetration of full-day kindergarten in a given area, we augment the triple-difference model by adding an instrumental variables approach to identify the relationship between full-day kindergarten access and maternal labor supply. This approach isolates changes in public full-day kindergarten enrollment driven by supply-side shifts. In our setting, states instituted a variety of policies which expanded funding and seats in full-day kindergarten, giving rise to policy variation over time and across states, as discussed in more detail in Section 2.1. There are two main categories of state policy action related to full-day kindergarten: (1) mandates that require school districts to offer full-day kindergarten, and (2) other full-day-friendly policies that encourage, fund, or otherwise facilitate expansion, but fall short of a requirement on districts. The latter category includes legislation that provides more state funding for full-day kindergarten, alters the funding formula for kindergarten students to incentivize full-day kindergarten provision, or otherwise induces or supports greater full-day kindergarten availability. We refer to these two policy categories collectively as full-day friendly policies.

We use the passage of these full-day friendly policies as an instrument for the public, full-day kindergarten enrollment share to isolate changes in enrollment due to the policy setting (Figure C2). Detailed information about the compilation of policy changes and construction of the policy instrument is found in the Policy Appendix (Appendix C). Essentially, we start from two points. First, we use current and historic records from the National Center for Education Statistics to construct a state by year panel of states that require school districts to offer full-day kindergarten. This mandate requires a full-day option is available, but does not mandate full-day kindergarten attendance. Second, we supplement this with a state-by-state search of state legislative histories. We identify any laws that are passed that encourage, fund, or otherwise facilitate full-day kindergarten expansion, but fall short of a requirement on districts. This state-by-state internet search is supplemented by a scan of databases maintained by the Education Commission of the States and the National Conference of State Legislators, a state-by-state newspaper scan, as well as LexisNexis searches of individual states' legislative histories to capture any policy changes we might have missed.

Importantly, the introduction of full-day friendly policies led to changes in the public full-day kindergarten enrollment share. For each state, we identify the first year a full-day friendly policy was enacted. We then

[12] One concern is that access to full-day kindergarten has persistent, dynamic effects on maternal labor supply. This would introduce bias into our triple-difference specifications comparing mothers of 5- to 6-year-olds to mothers of 7- to 9-year-olds. However, as we see in Table 4, we do not see treatment effects in the difference-in-differences specification for mothers of 7- to 9-year-olds suggesting that bias from persistent employment effects is not an issue.

limit the sample to states where the first policy was enacted between 1995 and 2017, to ensure a balanced panel. As recommended by (Goodman-Bacon, 2021), we create a panel for each state, combined with all of the states that did not implement a full-day friendly policy during the panel (e.g., control states). We then stack the panels for each treated state to estimate average effects, as follows:

$$FullDayK_{spt} = \sum_{\tau=-5}^{5} \beta_\tau Treated_{sp} * (t = \tau) + \gamma_s + \lambda_{pt} + \mu_y + \varepsilon_{spt} \quad (2)$$

where $FullDayK$ is the public full-day kindergarten share rolling average in state $s$ from panel $p$ in event time $t$. This allows us to estimate the effect of treatment (full-day friendly policy enactment) on full-day enrollment relative to states that never implemented a policy. State fixed effects control for fixed differences between states while panel-by-event time fixed effects make this a comparison between the treated state and never treated states in the same panel and time period. We also include year fixed effects to account for secular trends in full-day enrollment across calendar time. To match the individual-level outcome analysis, we weight each observation by the aggregate ASEC sample weights for the mothers in our main analysis sample.

As seen in Figure 6, this leads to an increase in the public full-day kindergarten share directly after the policy is implemented that grows over time.[13] Given this first stage effect, we use the introduction of full-day friendly policy to instrument for the public full-day kindergarten share and to confirm that our baseline estimates are not driven by changes in local demand for full-day kindergarten (which might be correlated with outcomes) and that the patterns persist if we isolate changes in full-day access that are due to policy action. In practice, in the triple-difference specification we are interested in the interaction between the full-day kindergarten share and having a kindergarten-aged child (five or six). As such, we use the interaction of having a full-day friendly policy in place and having a kindergarten-aged child in the following two stage least squares specification:

$$FullDayK_{st-1} * (Have\ Child\ 5\text{-}6) = \alpha_0 + \alpha_1 Full\text{-}day\ Friendly\ Policy_{st-1} * (Have\ Child\ 5\text{-}6) + \theta_{st} + \psi_s * (Have\ Child\ 5\text{-}6) + \sigma_{mt} + \nu_{imst} \quad (3)$$

$$Y_{imst} = \beta_0 + \beta_1 FullDayK_{st-1} * \widehat{(Have\ Child\ 5\text{-}6)} + \gamma_{st} + \delta_s * (Have\ Child\ 5\text{-}6) + \lambda_{mt} + \varepsilon_{imst} \quad (4)$$

[13] The year-by-year estimated effects are imprecise, but the estimated effects in the post period are statistically different from zero.

This allows us to estimate the effect of supply-driven variation in the full-day kindergarten share on parental outcomes.[14] Comparing equation (1) and equation (4) shows that this approach continues to use the same triple-difference framework with the addition of an instrument for the full-day kindergarten share. The instrumental variable approach implicitly assumes policy movements were unexpected and exogenous from families' characteristics and decisions. With this instrument, we isolate variation in full-day kindergarten share that is driven by state-level policy changes rather than local trends or norms that could influence parental labor supply. Passage of a full-day friendly policy should weakly increase full-day kindergarten attendance, which would satisfy monotonicity. The exclusion restriction requires that among parents with kindergarten-aged children, having a full-day friendly policy in place only affects employment outcomes through its effect on the full-day kindergarten share, which we use to proxy for access to full-day kindergarten. In the triple-difference specification, the policy instrument is highly predictive of the full-day kindergarten share at the state level, leading to a 31 percentage point (standard error of 0.053) increase in the full-day share.[15]

# 4 Results – Mothers and Fathers

## 4.1 Parental Labor Supply

As described in the empirical strategy, we present results from the triple-difference models for mothers' and fathers' employment outcomes in Table 3. Each column in each panel presents the results of a separate regression. The treatment variable is the continuous full-day kindergarten participation proportion (0-1) and the outcomes are either binary or continuous measures of hours, weeks, or wages. The effect sizes correspond to a shift from no full-day kindergarten provision to all full-day kindergarten provision. The average annual increase in full-day kindergarten participation over the time frame of the study is two-percentage points.

We find evidence of positive effects on mothers' labor force attachment as displayed in Panel A of Table 3. A shift from no full-day kindergarten (i.e., all kindergarten is provided in half-day format) to all full-day kindergarten corresponds to a 5.5 percentage-point increase in being in being employed at the time of the ASEC.[16] This effect represents a 8.6 percent increase off a base of 64 percent employment for mothers,

[14] In theory there are two endogenous variables ($FullDayK_{st-1}$ and $FullDayK_{st-1} * (Have\ Child\ 5\text{-}6)$) and two instruments ($Full\text{-}day\ Friendly\ Policy_{st-1}$ and $Full\text{-}day\ Friendly\ Policy_{st-1} * (Have\ Child\ 5\text{-}6)$). Since $FullDayK_{st-1}$ varies at the state by year level, it is absorbed by the state-by-year fixed effects and is not directly included.

[15] These first stage estimates are larger than those shown in Figure 6 because the event study cuts off the sample window five years after passage to create a balanced panel, even though full-day friendly policies continue to increase full-day enrollment over time. As such, the average estimated effect of full-day friendly policy is larger than in the initial years. Since the effect of full-day friendly policies on the full-day kindergarten share is increasing over time (Figure 6), we could impose more parametric structure on our instrument and allow the first stage effects to vary as time from policy passage increases. Although this specification imposes stronger functional form assumptions, we find evidence of maternal employment effects that are consistent with our main results.

[16] As implemented, the year-over-year changes are much smaller. The average annual change in the national full-day enroll-

suggesting full-day kindergarten induces increases in labor supply among the mothers of kindergartners.[17] Looking at a more retrospective measure of employment points in the same direction – movement to full-day kindergarten corresponds to a 4.3 percentage-point increase in the likelihood of a mother working last year, representing an increase of 6 percent.

A complete full-day kindergarten expansion increases the full-time employment rate last year by 2.6 percentage points. Since the employment outcome in column (3) can be decomposed into full-time and part-time employment, this implies a 1.7 percentage-point increase in part-time employment, suggesting the increase in employment is split between full-time and part-time workers. However, it is unclear whether mothers entering the labor force moved into full-time work or if some mothers working part-time moved into full-time work and mothers entering the labor force moved into part-time work.[18] An increase in the full-day kindergarten share from 0 to 1 is associated with an additional 2.6 weeks of work during the previous year (8.3 percent).[19] We see that an increase in the full-day kindergarten share from 0 to 1 increases the likelihood that a mother has any wage income by 5.8 percentage points, similar to the effects on employment. The increase in full-day kindergarten share from 0 to 1 is also associated with an imprecisely estimated increase of $231 dollars.[20]

Turning attention to fathers we see that men with kindergarten-aged children do not adjust labor supply along any of the dimensions we examine (Panel B of Table 3), with point estimates close to zero. These patterns stand in contrast to those observed for mothers. For most fathers, labor supply appears to be insensitive to the public provision of child care through early childhood education, whereas for mothers, who are more often primary care providers for children, we see sensitivity.

A central finding in related papers is that maternal labor supply effects are often larger in families where there are no younger children under the age of five in the household (Cascio (2009); Fitzpatrick (2012)). Like previous research, we explore heterogeneity on this margin. Even in families with younger children in the home, having extended child care (in the form of kindergarten) for one child might relax some time and resource constraints, even if it does not completely remove them. Table A2 stratifies mothers by the presence of younger children. We see that all mothers respond in similar ways in their labor force attachment. However, significant effects are concentrated among mothers whose youngest child is kindergarten aged and

ment share, from 1990 to 2021, was 1.4 percentage points.

[17] We also examine labor force participation in the previous week as an outcome and find similar, effects when accounting for those seeking employment (6.3 percentage points).

[18] Because addresses participate in the CPS for multiple months, it is possible to estimate effects for the subset of households who did not move and were employed during the first ASEC wave. For this subset we observe positive, but imprecise effects on hours worked and wage income.

[19] The ASEC also includes a measure of typical hours worked in the last year. Estimates are similar for this measure as for the weekly measure, so we have not included them in the tables for brevity.

[20] In Figure A4, we document the effect of the full-day kindergarten share on the individual wage income distribution and find that the changes are concentrated among women earnings between 0 and $20,000 or above $55,000.

experiences increased access to full day kindergarten. Additionally, mothers without children under age five show increased hours, weeks worked, and wages, suggesting they may have larger intensive margin adjustments compared to mothers with younger children in the household.

In Figure 7, we present results of additional subgroup analyses among mothers, split by women's education level, marital status, and race. These analyses show that maternal employment gains associated with the increase in full-day kindergarten access are not just concentrated among disadvantaged families. Perhaps due to the universal nature of expansions, women who were married and living with their spouse present, more educated women, and white women also benefited from the full-day kindergarten expansions.[21]

In Table 4 we show that the main results are not dependent on the triple-difference specification. When we estimate results separately for mothers of 5- and 6-year-olds and mothers of 7- to 9-year-olds we see that the effects are driven by the treated group, with small, insignificant effects for the untreated group.[22]

We further probe the robustness of these results in Table 5. First, we show that effect on employment during the previous calendar year is insensitive to our measurement of the full-day enrollment share. Our baseline specification uses the state-level share of public full-day kindergarten enrollment, to capture changes in the supply of full-day provision. In column (2) we show that estimates are similar if we use the full-day share of all kindergartners (both public and private) while in column (3) we show that results are similar if we use the share of 5-year-olds in full-day, public kindergarten.[23]

As we saw in Table 2, mothers of kindergarten-aged children are several years younger on average. One concern is that secular time trends in women's preferences for work might lead to the differences observed in Table 3. However, as seen in column (4) of Table 5, we see similar magnitude effects if we compare women in the same market and year that started having children at the same time. The effect is robust to including controls for mother's demographics (e.g., race/ethnicity, marital status, education level, and age fixed effects), excluding states in the South, which was early in adopting full-day kindergarten, or if we exclude observations from 2020–2022 during the COVID-19 pandemic. During this time period, there were some changes in preschool provision and use, including the 1990s Head Start expansions and the first states adopting universal pre-kindergarten programs. We find that the results are similar if we control for the preschool teacher-to-student ratio, the share of 3- and 4-year-olds in preschool, and the share of 3- and 4-year-olds in full-day preschool in the previous school year, all interacted with the indicator for having a

[21] Results are slightly larger, but not significantly different if we focus on outcomes of mothers whose own mother is in the household (Appendix Table A3). The sample of households with a maternal grandmother in the household is much smaller than the overall sample, and we note that estimates are noisy. We do not find evidence that the expansion of full-day kindergarten led to an increase in parental schooling or education (Appendix Table A4.)

[22] Only one of nine outcomes is significant for mothers of 7- to 9-year-olds, a significant reduction in part-time employment.

[23] This accounts for potential, endogenous decisions to move children ahead or behind in school. As we saw in Table A1, there is minimal substitution between preschool and kindergarten.

child that was 5- or 6-years-old.[24] As seen in Table A5, expansions in full-day kindergarten did not induce out of state mobility, and estimates are similar when excluding cross-state movers. In all specifications, effects for fathers remain small and insignificant.

## 4.2 Results Using Policy-Induced Variation

As noted above, one concern is that full-day kindergarten coverage is driven by households wanting to increase maternal employment. We exploit the state-level passage of full-day friendly policies in an instrumental variables framework to isolate changes in full-day enrollment due to supply-side policy changes. When we leverage state-level full-day friendly policy changes to instrument for full-day kindergarten availability, we see broadly similar patterns in Table 6.[25] Most patterns emerging from this alternate specification — to isolate variation plausibly exogenous to the individual — point in the same direction as results found in the main specification, are slightly larger in magnitude, but less precisely estimated. As seen in Panel A of the table, a shift from no full-day kindergarten to all full-day kindergarten increases mothers' contemporaneous employment at the time of the ASEC interview by 6.3 percentage points. Given a base of 64 percent, this translates to an employment increase of 9.8 percent. We see a similar increase in usual weekly work hours of 2.2 additional hours a week. When considering measures that better capture longer-term employment experiences, we see that movement to full-day kindergarten corresponds with increased employment over the past year by 4.6 percentage points, equivalent to an increase of 6.6 percent. The estimate for full-time employment is similar, but imprecisely estimated. We estimate a marginally significant 3.5 week increase in weeks worked. Results show slightly larger increases in the likelihood of reporting any wage income, while the point estimate on wage income is over ten times as large (but insignificant). We see no responses for fathers (see Panel B).

While the continuous variation in the baseline triple-difference specification is not conducive to event study analysis, we can generate event studies using the policy instrument variation. Because the rollout of full-day friendly policy is staggered across time, we build on the stacked event study analysis used in section 3 to show the first stage effects of full-day friendly policies on full-day kindergarten enrollment. A separate event panel dataset is created for each state that implemented a policy between 1995 and 2017 that includes that treated state and all of the other states that never implemented a full-day friendly policies as the counterfactual. These event panels are then stacked to estimate the average effect. Rather than stack the individual-level data, we aggregate the microdata to the local level to mimic the same stacked design. For each state, we calculate the employment rate for mothers with 5- to 6-year-olds and mothers of 7- to

[24] The other outcomes in Table 3 are similar in magnitude and significance when including the preschool controls.
[25] Reduced-form estimates are provided in Appendix Table A6.

9-year-olds who do not have a 5- or 6-year-old (our treatment and counterfactual groups used above).[26] We estimate how being in a treated state after the enactment of the policy affects employment of mothers of 5- and 6-year-olds relative to mothers of 5- and 6-year-olds in never treated states. We then do the same for mothers of 7- to 9-year-olds. We estimate the effects of these two groups separately so that the underlying trends between the groups are visible. Because the state-by-year level cells have smaller samples, we aggregate years into two year bins.

These results are provided in Figure 8. Although the results are imprecisely estimated, we see that after the full-day friendly policy is introduced there is a general increase in employment of mothers with kindergarten-aged children, but the trends for mothers with older children are largely flat. We also do not see significant trends prior to the policy, consistent with the treated states and counterfactual states following similar trends. This analysis provides consistent evidence that increases in the full-day kindergarten share lead to greater employment among affected mothers.

Throughout, we have viewed the full-day kindergarten share as a proxy for access to full-day kindergarten to understand how access to full-day kindergarten affects maternal employment. However, we might also be interested in how full-day kindergarten attendance affects employment. At the individual-level the state-level full-day kindergarten share captures the individual's full-day attendance with measurement error, introducing bias (Pischke, 2007). A benefit of the instrumental variables approach is that it is well-equipped to address measurement error between the individual-level take-up of full-day schooling and the full-day kindergarten share. Using households surveyed in the October Supplement, where we observe both school enrollment and labor supply, we use the policy variation to estimate how maternal employment responds when all school-aged children are enrolled in full-day school.[27] We find that being in a state with a full-day friendly policy and having a kindergarten-aged child is associated with a significant increase in all of the mother's children aged 5-to 9-years-old enrolled in full-day school (Appendix Table A7). Consistent with our baseline analysis, having all of ones children aged 5- to 9-years-old enrolled in full-day school is associated with a 20 percentage-point increase in the probability of being employed and an increase in hours worked last week by 10 hours.[28]

[26] The 7- to 9-year-old sample does not include mothers that also have a 5- or 6-year-old and these two groups are mutually exclusive.

[27] We do not adopt this as our main estimation strategy because the October Supplement is much smaller, and includes fewer labor supply outcomes.

[28] An alternative way to address this issue in our analysis is to aggregate the data from the individual-level to the local level. If we aggregate outcomes to the metropolitan area-by-year-by-child age group (5- to 6-years-old or 7- to 9-years old) level and re-estimate equation (1), we find similar results.

### 4.3 Household Childcare Expenses

For many kindergartners, the expansion to full-day kindergarten displaces child care performed by their mothers. This helps to explain the maternal labor supply response to the expansions. However, some households were already receiving out-of-home care. For example, 68% of children ages three to five receive at least five hours of non-parental care per week in either paid or unpaid settings (Datta et al. (2023)). For these families, the expansion of full-day kindergarten might not affect maternal labor supply, but rather relax the household budget constraint.[29]

Using data from the Consumer Expenditure Survey (CEX) from 1992-2022, we document how an increase in the full-day kindergarten share affects whether families utilize paid child care and how monthly household childcare expenses respond (U. S. Department of Labor, 2023).[30] Our approach to estimating impacts of full-day kindergarten expansions on household childcare expenses is analogous to the approach on parental labor supply, except that the CEX only provides state-level geography which slightly changes the interpretation. The CEX provides mixed evidence of the effects on childcare expenses. When evaluating paid childcare utilization, we see across the board that access to full-day kindergarten decreases the propensity of families to pay for any child care, although results are imprecise in most specifications (Table 7). For example, families with no younger children are nearly 13 percentage points less likely to utilize any kind of paid child care when full-day kindergarten becomes more available (column 4).

Patterns for the amount spent on child care are less clear. When comparing households with a kindergartner to households with older, school-aged children, we estimate an increase of $5.04 in monthly childcare expenses in households with kindergarten-aged children for a 10 percentage-point increase in the full-day kindergarten share. Increases in childcare expenses load on families with younger children, and childcare expenses in families with no younger children are not changed. If we compare households with a kindergartner to households with younger children (where child care is more regularly needed and used), we estimate an insignificant decrease of $2.97 in monthly childcare expenses in households with kindergarten-aged children for a 10 percentage-point increase in the full-day kindergarten share. Since many families do not pay for child care (i.e., a parent provides care or the family uses unpaid care), the averages represent a larger change among those paying for child care. As seen in the lower panel of Table 7, patterns are similar, but imprecisely estimated, when using policy changes to instrument for full-day kindergarten availability. Overall, the CEX provides suggestive evidence that the number of families utilizing paid child care may decline when full-day kindergarten is introduced, and at the same time the evidence suggests that an increase in the full-day

[29] The theoretical effects on childcare expenses are ambiguous. For example, if the increased access to full-day kindergarten induces a mother to work full-time, she might need additional after-school child care or paid care for younger children.

[30] Because of the structure of the CEX, we cannot isolate child-specific expenses, only the total expenditures on child care at the household level. More details related to our use of CEX data are included in the Data Appendix (Appendix B).

kindergarten share may increase childcare expenses for some families while reducing expenses for others.

# 5 Effects on Children

The shift to full-day kindergarten in the United States has created more school-level investment in young children. Additionally, these expansions may produce higher family-level financial investments in children. These increased resources could take two forms – freed up family resources previously spent on child care and additional wage income as mothers work. At the same time, changes in the length of the school day may increase maternal time out of the home at work. These patterns may lead to countervailing effects on child outcomes. An existing literature speaks to the impact of extending the school day on children's educational performance. Because our approach relies on the concurrent timing of expansions in full-day kindergarten provision and outcomes for families who are likely affected, we are constrained in the application of our approach to exploring impact on child outcomes.

Indeed, a primary policy rationale for emphasizing the importance of the kindergarten year and extending the kindergarten school day, of course, centers on children and their short- and long-run development. Past research establishes that extending the kindergarten school day has substantial positive effects on academic achievement during the kindergarten year (Gibbs, 2023). Other work in early childhood contexts also demonstrates the role of a longer day in affecting children's developmental outcomes (Atteberry et al., 2019; Walters, 2015).

In Appendix D, we present analyses akin to our approach of assessing impact on parental employment to evaluate the net effect of increasing full-day kindergarten access on children. As mentioned, there are important limitations to this approach in our context. Our measures of child outcomes are third grade test scores from end-of-year standardized assessments in reading/language arts and mathematics. These test scores cover a more constrained timeframe of 2009 through 2019, and are thus restricted to expansions among those kindergarten cohorts, for which we have measures of share in full-day kindergarten from October 2005 through October 2015. In addition, our comparison cohorts of fifth graders are also likely treated to some extent, based on the full-day kindergarten share experienced by their cohort, so these estimates likely represent a lower bound on the effect of full-day kindergarten exposure on subsequent academic performance.

We do not find consistent evidence of durable effects on math test scores in third grade, but we do find persistent effects on reading/language arts performance. These effects are particularly large for Hispanic children and male students, and for those qualifying for free or reduced price lunch. These results are somewhat consistent with other work on the impact of full-day programming on more proximate measures of cognitive skill development (Atteberry et al. (2019); Gibbs (2023)). Much of the literature examining the

effects of early childhood education on children's cognitive outcomes in the U.S. finds patterns of fade out (Chetty et al. (2011); Currie and Thomas (1995); Deming (2009); Krueger and Whitmore (2001). Because we are only able to look at academic outcomes in third grade, we might be missing gains that accrue earlier but evaporate by later test-score measurements. In addition, researchers hypothesize that long-term benefits to participants in early childhood programs may be realized through non-cognitive or social-emotional skills (Heckman et al., 2013), which we cannot directly measure in our context.

# 6 Discussion

One of the most dramatic changes in the modern labor force has been the increased attachment of mothers. Between 1970 and 2000, the employment rate of mothers with school-aged children increased by approximately 30 percentage points, while full-time employment of mothers nearly doubled. Employment rates for all mothers except those with the youngest children have caught up to the employment rates of men and childless women. Despite this rise in maternal labor force attachment, there are still stark gender differences in the household division of child care and housework. In addition to working more, mothers continue to spend more time in child care. We show — using a pronounced change to public schooling for young children — that policy surrounding the structure of schooling can affect maternal time constraints and their availability for market work. The time mothers spend in child care, work, and other activities is sensitive to the structure of public school provision.

While there is a limited body of work on the impact of full-day kindergarten on students, this work sheds light on a broader question – that of the policy impact of expanding full-day kindergarten on the parents of children served by the longer school day. We find that full-day kindergarten expansions across U.S. states in the 1990s and 2000s had a significant impact on women's engagement with the labor force. For a 10 percentage-point increase in the state-level full-day kindergarten share, mothers of kindergartners are 0.43 percentage points more likely to be employed, relative to similar mothers with older children. These effects are economically significant. Between 1992 and 2022, the full-day kindergarten share increased by 40 percentage points nationally. Our findings suggest that expansions in full-day kindergarten can explain approximately 2 percentage points, or 24 percent, of the 8.5 percentage-point increase in employment among mothers of 5- and 6-year-olds between 1992 and 2022. These gains are even larger than the aggregate gains associated with the initial introduction of kindergarten (Cascio, 2009), suggesting that full-day school programming might relax additional maternal time constraints and possibly that the elasticity of maternal labor supply has changed over time. Our IV estimates using policy induced variation suggest that for every

14 children who shift into full-day kindergarten, one mother enters the labor force.[31] School schedules that remove typical work hour disruptions can substantively reduce the employment gap that remains for mothers with young children.

The extant literature hypothesizes that public provision of early childhood education acts as a childcare subsidy. Building on this literature, we show evidence that mothers time their workday around children's school schedules and use the extended kindergarten school day to increase market work. These results imply that policies and proposals that reduce or increase in-school time, including duration expansions to public pre-kindergarten programs, shifts to four-day school weeks, and year-round schooling, have important implications for the family. Beyond school enrollment, temporal aspects of schooling, like the number and timing of hours worked and the extent that employees have discretion over work hours (Price and Wasserman, 2024), may be important for parents' decision-making, time allocation and schedules, and employment. Childcare considerations often induce mothers to move toward jobs with flexibility, and mothers often prioritize job flexibility over earnings growth as a way to manage frictions between child care and employment (Adams-Prassl, 2023; Cubas et al., 2023; Goldin, 2014; Mas and Pallais, 2017; Wasserman, 2023). Because mothers carry a disproportionate share of childcare responsibilities, their schedules and employment are often most responsive to changes in temporal aspects of school timing in a variety of contexts. For example, mothers reduce work time and labor supply during summer breaks (Cowan et al., 2023; Price and Wasserman, 2024), increase labor supply when school move from a four-day week to a five-day school week (Duchini and Van Effenterre, 2022), and increase labor supply when children are moved off of year-round school schedules that break up the school year (Graves, 2013). Our work suggests that, as with full-day kindergarten expansions, other schooling policy levers, such as universal preschool, after-school programming, and four-day school weeks, are likely to affect labor market participation of mothers with young children. Universal pre-K provision in particular is receiving policy attention as an area for early childhood education expansion. While kindergarten provision is now predominantly full day, full-day preschool is less often available to families. Although the contexts differ, it is reasonable to imagine similar responses by mothers of preschoolers to lengthening the pre-K school day, a potential policy lever to support maternal labor supply.

The timing and hours of a standard school schedule do not provide complete childcare coverage to support parents' full-time work. Although we do not observe schools' provision of aftercare or wraparound childcare services, policies that extend school-day coverage to better align with a typical work schedule and commuting could produce even larger effects. Recent work on a preschool program with extended duration finds evidence consistent with this hypothesis (Humphries et al., 2024).

[31] From Table 6, a 100 percentage-point increase in full-day enrollment increases maternal labor force participation by 7.3 percentage points (100/7.3=13.7).

While prior work documents the effects of *targeted* public school expansions on maternal employment, particularly for disadvantaged mothers (Cascio and Schanzenbach, 2013; Wikle and Wilson, 2023), the effects of full-day kindergarten expansions are more broadly realized. This result is likely driven by the shift over this time period of full-day kindergarten from a largely targeted intervention to a near-universal program. In other words, more advantaged mothers, as measured by education level, marital status, or race, are precisely the mothers moved into program exposure by these policy expansions. Consistent with past research which finds that impacts are often pronounced among mothers whose youngest child is kindergarten-aged (Cascio (2009); Fitzpatrick (2012)), our significant effects are driven by this group. Our findings diverge from two studies situated in a universal preschool context in which no broad maternal labor supply responses were found (Cascio and Schanzenbach, 2013; Fitzpatrick, 2010). The study time frames overlap, and one of the two states evaluated in these two papers was Georgia, which required school districts to offer full-day pre-K. We suspect that the differences between our findings in a kindergarten context and their findings in a pre-K context may in part be due to underlying differences in how families approach the two settings, with lower participation rates in universal pre-K among 4-year-olds as compared to kindergarten enrollment among 5-year-olds. This difference suggests that for many parents, a child's age is a salient factor in decisions around utilizing early childhood education.[32] These patterns serve as the foundation for understanding broad-based effects on families and mothers in particular.

In recent decades, the expansion of full-day kindergarten has been one of the primary ways that public funds have been used to support young children's early development, and we know little about the broader impact of these shifts. Our paper measures the net policy effect of expanding the school day for young children, thereby relaxing parents' time and resource constraints. This evidence contributes to our improved understanding of the broader return on early childhood and early schooling investments and the implications for families and parents.

[32]Differences could also be related to the specific context of Georgia. Georgia is in the South, and the state experienced much smaller changes in full-day kindergarten participation, having a high concentration of full day in the beginning of our timeframe. Because our treatment exposure is measured at the state-by-year level, we do not have enough variation to do state-specific effects.

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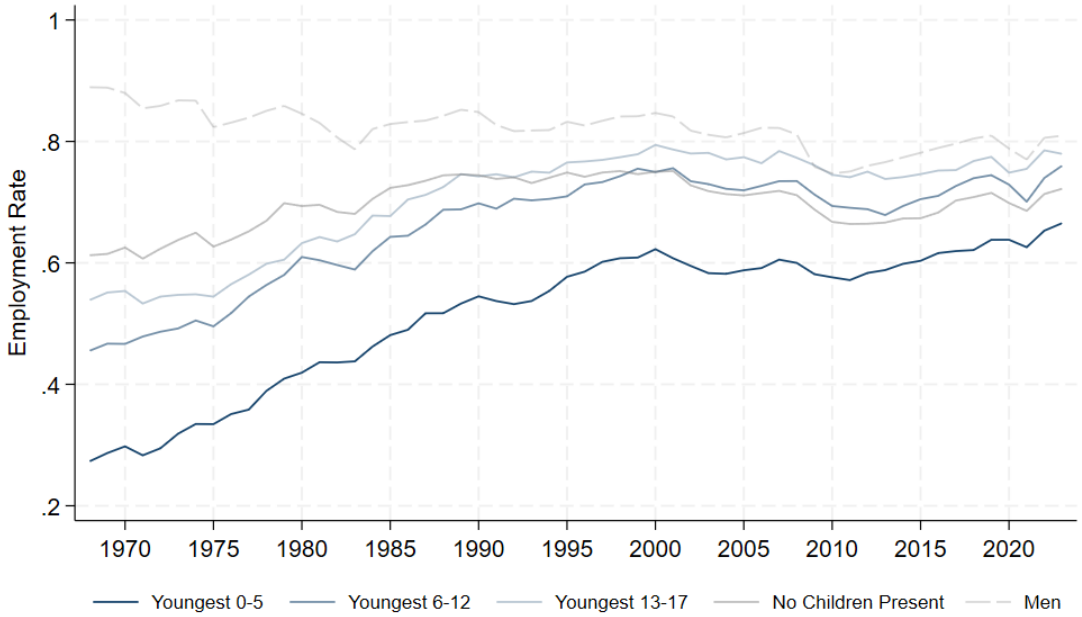
# Tables and Figures

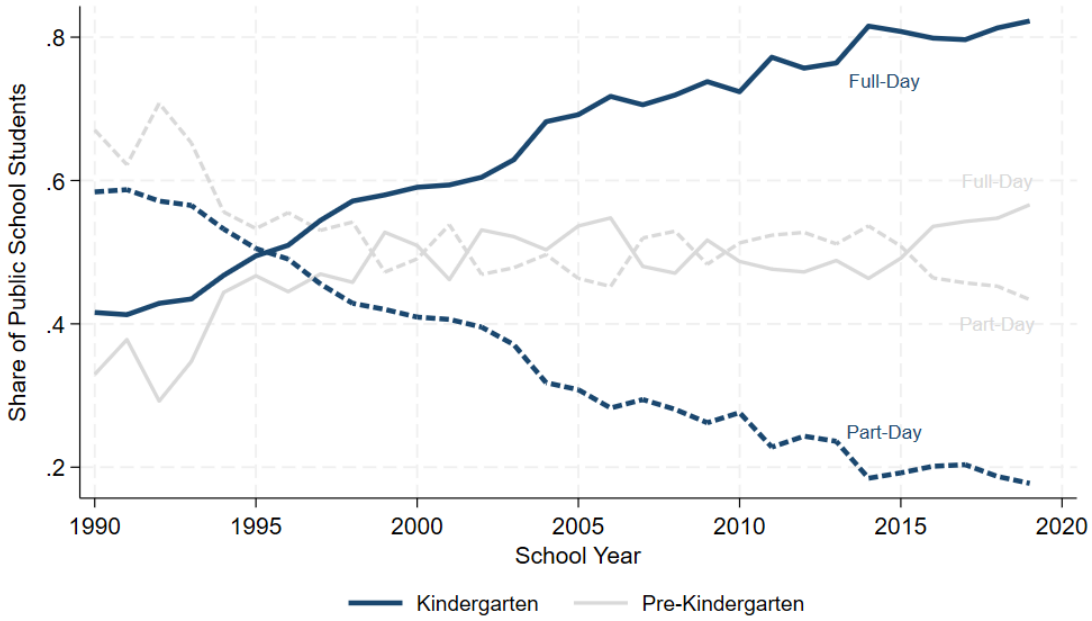
Figure 1: Long-Run Employment Trends of Women




NOTE: Sample restricted to adults 18-54 in the CPS ASEC between 1968 and 2023. Women are split by the age of their youngest child. For reference, the employment rates of men are provided in the dashed gray line. Observations are weighted using the March ASEC sampling weights when collapsing to the group by year level. Individual weights are used to make this nationally representative.

SOURCE: CPS ASEC 1968-2023.

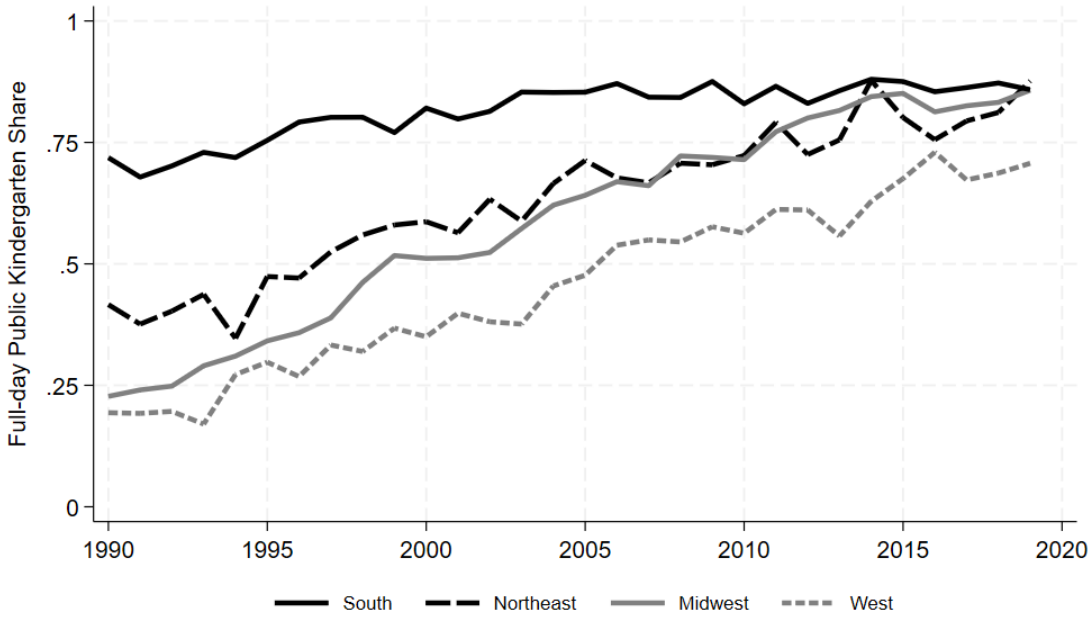
Figure 2: Full- and Half-Day Kindergarten and Pre-Kindergarten Enrollment




NOTE: Sample restricted to children ages 3-7 from the CPS October Supplement between 1990 and 2019 who reported either being in public nursery/pre-kindergarten or in kindergarten. Individual weights are used to make this nationally representative.

SOURCE: CPS October School Enrollment Supplement 1990-2019.

Figure 3: Full-Day Kindergarten Enrollment by Region

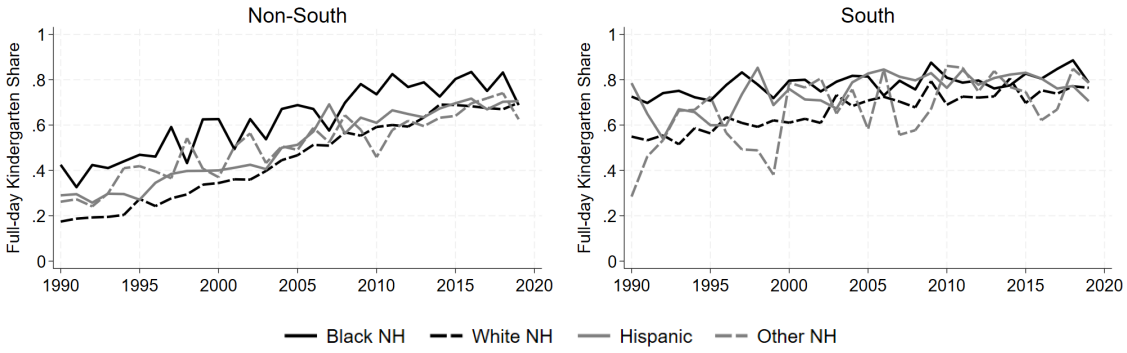



NOTE: Sample restricted to children ages 3-7 from the CPS October Supplement between 1990 and 2019 who reported being in public kindergarten. Region divisions follow typical U.S. Census classification. Individual weights are used to make this nationally representative.

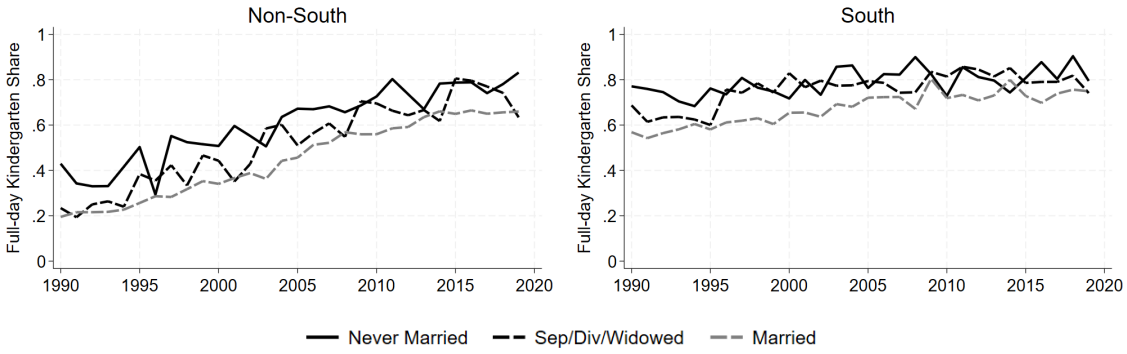
SOURCE: CPS October School Enrollment Supplement 1990-2019.

Figure 4: Full-Day Kindergarten Enrollment by Demographics

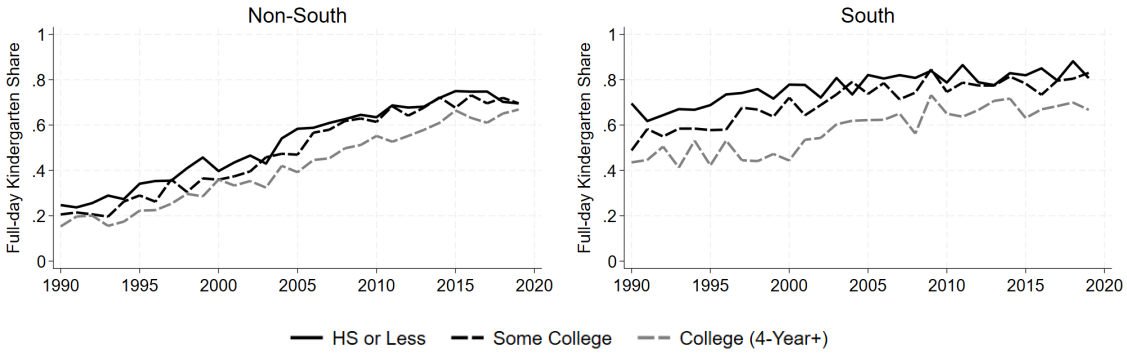
A) Stratified by Mothers' Race/Ethnicity




B) Stratified by Mothers' Marital Status



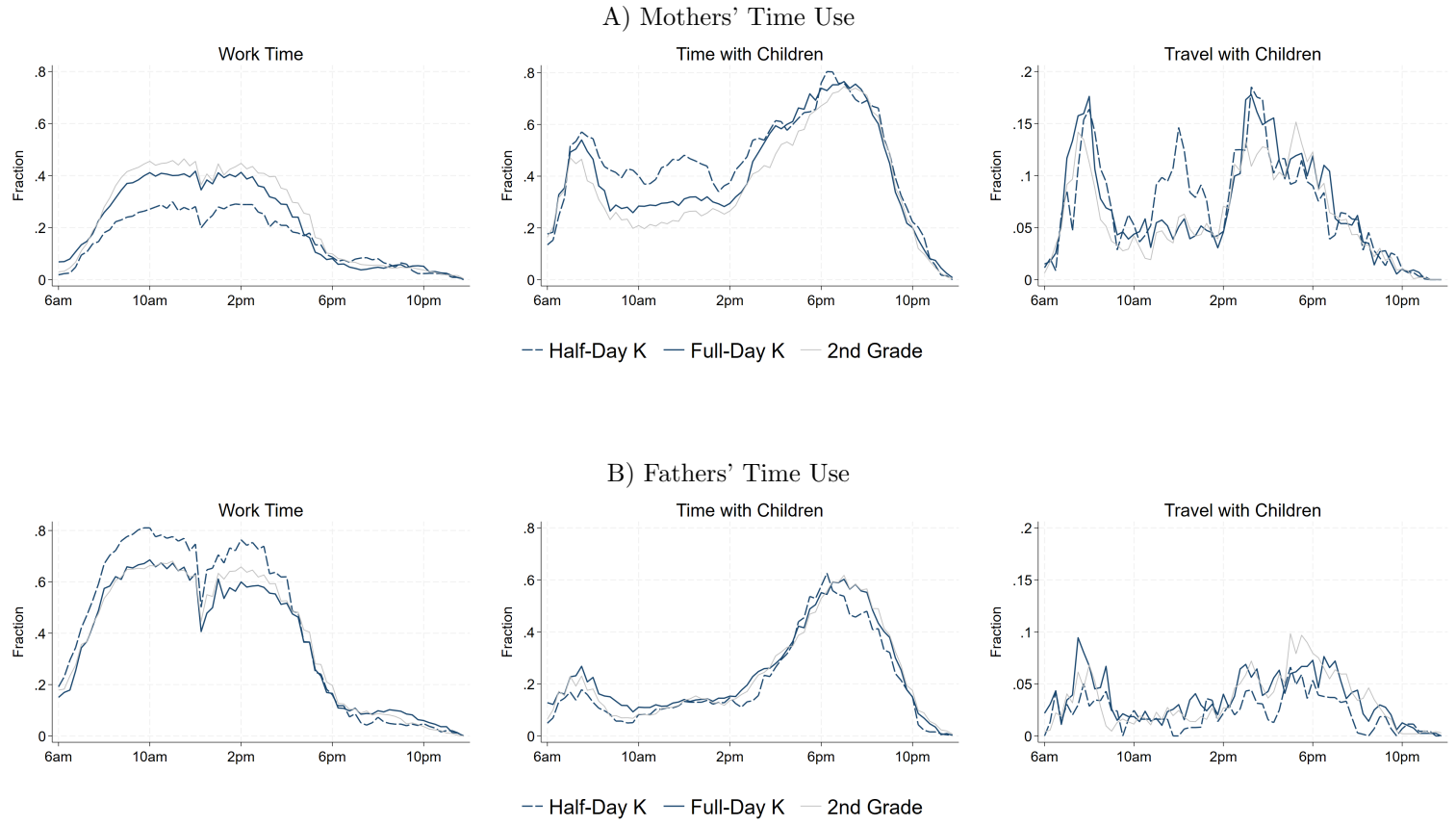

C) Stratified by Mothers' Educational Attainment




NOTE: Sample restricted to children ages 3-7 from the CPS October Supplement between 1990 and 2019 who reported being in public kindergarten. Region divisions follow typical U.S. Census classification. Individual weights are used to make this nationally representative.

SOURCE: CPS October School Enrollment Supplement 1990-2019.

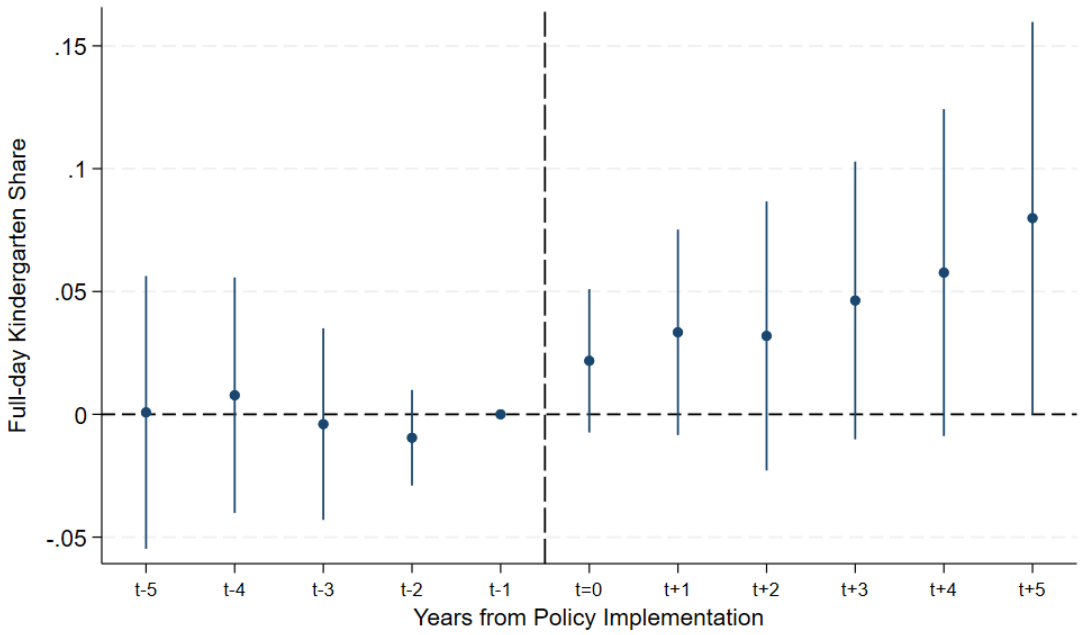
Figure 5: Parent Time Use by Grade and Type of Child's School Enrollment




NOTE: Sample restricted to parents with a child in kindergarten or second grade who linked with the CPS October Supplement just before the ATUS interview. The sample is further restricted to include only parents who reported time use on a school day. Child's grade drawn from the CPS October Supplement. The Y-axis represents the fraction of parents engaged in an activity at a given point in time during the day.

SOURCE: ATUS 2003-2022 linked to most recent CPS October Supplement.

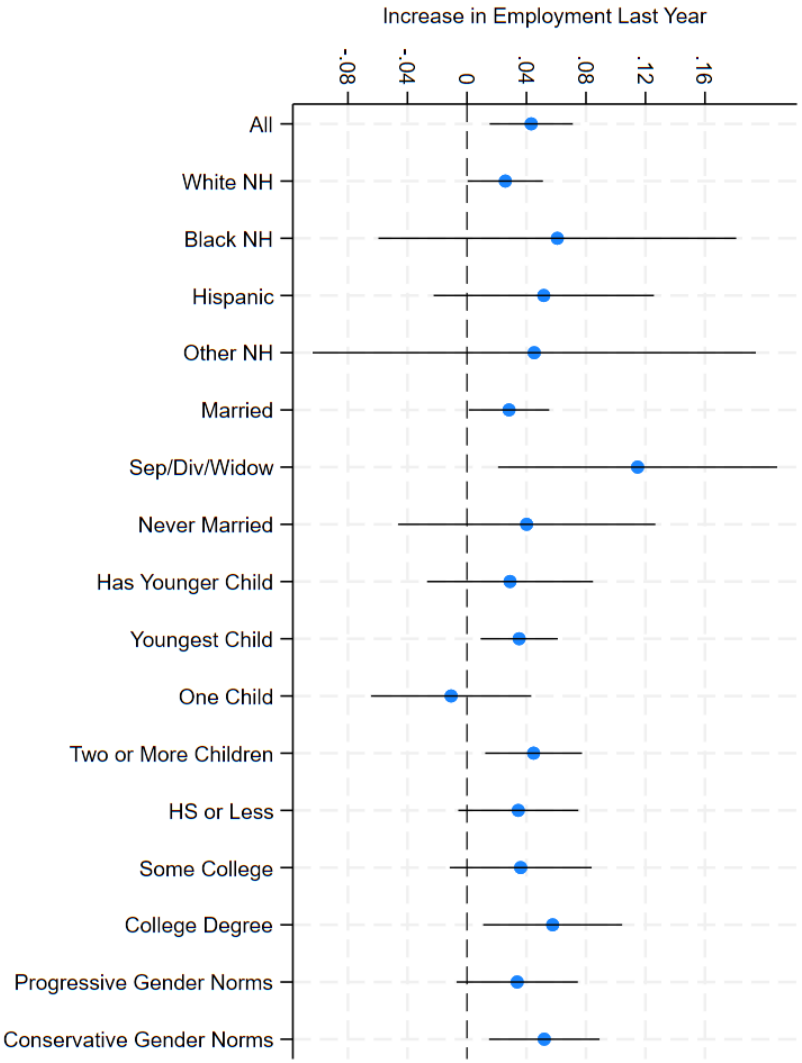
Figure 6: Event Study Impact of Full-Day Friendly Policies on Full-Day Kindergarten Share, 3-Year Rolling Average




NOTE: Level of observation is state-by-year. The outcome is the 3-year rolling average of the full-day kindergarten share in the state, constructed from the CPS October Supplement. For each state that enacts a full-day friendly policy, a separate event study panel is created. The year the policy is implemented is year $t = 0$ for the enacting state and for all other states that never enact a full-day friendly policy. These states provide a counterfactual for the "treated" state that implemented the policy. The event study panel for each of these states is then stacked, so that a state-year observation for a state that did not implement a full-day friendly policy will appear multiple times. The 3-year rolling average of the full-day kindergarten share is then regressed in the balanced panel (+/-5 years) on event time dummies, interacted with treatment dummies, with state, year, and panel-by-year fixed effects. The panel-by-year fixed effects makes this a comparison between the treatment state and the counterfactual states in the same panel over time. Observations are weighted using the sum of March ASEC sampling weights for mothers with 5- to 9-year-olds from our main analysis sample. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state with 95 percent confidence intervals plotted. The treatment effects after policy implementation are jointly significantly different from zero, with a F-statistic of 4.09.

SOURCE: Authors' calculations from the CPS 1991-2021.

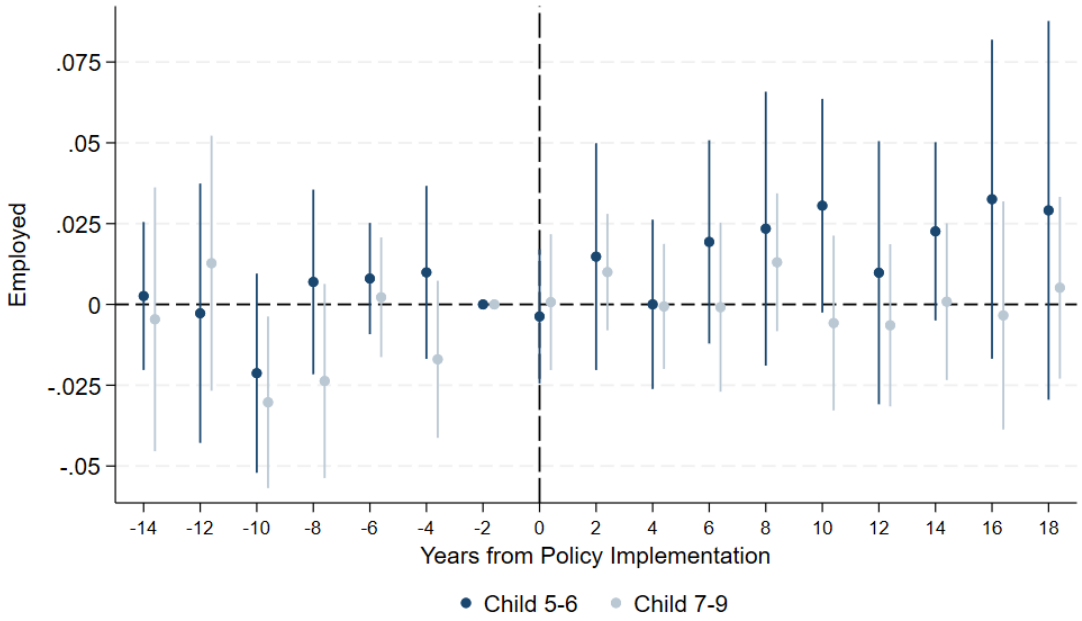
Figure 7: Heterogeneous Impacts of Full-Day Kindergarten Share on Maternal Employment




NOTE: Sample restricted to mothers and fathers with a child between the ages of 5 and 9. The full-day kindergarten share is constructed from the CPS October Supplement and captures the share of kindergartners in public school attending full-day. As such, we use the measure from the previous year, as this corresponds to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the 3-year rolling average of the full-day kindergarten share for each state. State-by-year fixed effects are including, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state with 95 percent confidence intervals plotted.

SOURCE: Authors' calculations from the CPS ASEC 1992-2022.

Figure 8: Event Study Impact of Full-Day Friendly Policies on Maternal Employment




NOTE: Level of observation is state-by-year. The outcome is the average employment last week among mothers who have children between 5 and 6 (treated), or between 7 and 9 (counterfactual), respectively. Mothers who have a 5- or 6-year-old and a child that is between 7 and 9 are included in the treated group so that these groups are mutually exclusive. These averages are constructed from the March CPS. For each state that enacts a full-day friendly policy, a separate event study panel is created. The year the policy is implemented is year $t = 0$ for the enacting state and for all other states that never enact a full-day friendly policy. These states provide a counterfactual for the "treated" state that implemented the policy. The event study panel for each of these states is then stacked, so that a state-year observation for a state that did not implement a full-day friendly policy will appear multiple times. The average employment rates are then regressed on event time dummies, interacted with treatment dummies, where years are grouped into two-year bins, to account for small cell samples at the state-by-year level. State, year, and panel-by-year fixed effects are included. The panel-by-year fixed effects makes this a comparison between the treatment state and the counterfactual states in the same panel over time. Observations are weighted using the sum of March ASEC sampling weights for mothers with 5- to 9-year-olds from our main analysis sample. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state with 95 percent confidence intervals plotted.

SOURCE: Authors' calculations from the CPS 1991-2021.

Table 1: Parent Time Use by Grade and Type of Child's School Enrollment

| | Mothers | | | Fathers | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Half-Day K (1) | Full-Day K (2) | Second Grade (3) | Half-Day K (4) | Full-Day K (5) | Second Grade (6) | (2)-(1) (7) | (5)-(4) (8) | Diff-N-Diff (7)-(8) (9) |
| *Time Use on School Days* | | | | | | | | | |
| Time with Children | 457.74 | 403.45 | 358.33 | 199.11 | 240.24 | 227.16 | -54.29*** | 41.13* | -95.42*** |
| *Activity Measures* | | | | | | | | | |
| Work | 157.87 | 218.37 | 246.30 | 436.22 | 388.82 | 417.52 | 60.50*** | -47.40* | 107.90*** |
| Home Duties | 232.42 | 197.00 | 199.41 | 87.53 | 105.78 | 85.05 | -35.43*** | 18.25 | -53.68*** |
| Care for Children and Adults | 154.30 | 153.88 | 131.11 | 69.78 | 75.28 | 67.98 | -0.42 | 5.50 | -5.91 |
| Personal Care | 44.77 | 40.86 | 45.50 | 41.24 | 35.43 | 33.11 | -3.90 | -5.82 | 1.91 |
| Leisure with Children Present | 133.45 | 121.68 | 116.85 | 85.63 | 111.02 | 106.93 | -11.77 | 25.39** | -37.16** |
| Leisure without Children Present | 102.73 | 106.77 | 98.58 | 154.23 | 145.74 | 141.37 | 4.05 | -8.49 | 12.53 |
| Sleep | 490.00 | 491.44 | 490.68 | 463.90 | 466.39 | 478.07 | 1.44 | 2.48 | -1.05 |
| Other Activities | 127.94 | 113.83 | 115.49 | 106.76 | 115.55 | 113.41 | -14.11 | 8.79 | -22.89 |
| *Quality Parental Time Investment Measures* | | | | | | | | | |
| One On One Time with Children | 110.99 | 108.89 | 100.04 | 28.99 | 43.14 | 44.22 | -2.10 | 14.15 | -16.25 |
| Quality Time with Children | 109.92 | 99.20 | 89.45 | 76.28 | 71.76 | 71.37 | -10.72 | -4.52 | -6.20 |
| Reading with Children | 7.61 | 7.29 | 3.90 | 5.18 | 2.86 | 2.28 | -0.32 | -2.31 | 1.99 |
| Playing with Children | 23.17 | 13.10 | 10.72 | 19.62 | 18.98 | 11.87 | -10.06*** | -0.64 | -9.42* |
| Physical Care of Children | 79.40 | 79.38 | 64.00 | 24.88 | 28.32 | 24.98 | -0.02 | 3.45 | -3.47 |
| Academic Time with Children | 28.63 | 31.78 | 29.85 | 12.94 | 12.45 | 14.53 | 3.15 | -0.49 | 3.64 |
| Direct Childcare | 175.53 | 176.18 | 150.92 | 73.96 | 83.39 | 75.50 | 0.65 | 9.43 | -8.77 |
| Time with Children Under 5 | 279.99 | 208.89 | 157.43 | 116.42 | 106.51 | 80.40 | -71.10*** | -9.91 | -61.19** |
| Traveling with Children | 38.89 | 38.13 | 31.57 | 12.51 | 21.70 | 20.61 | -0.75 | 9.19* | -9.95 |
| Observations | 202 | 448 | 529 | 119 | 298 | 373 | | | |

NOTE: Sample restricted to parents in the ATUS from 2003-2022 with a child in kindergarten or second grade who linked with the CPS October Supplement just before the ATUS interview. The Time with Children category includes any time spent with household children, regardless of the activity. The Care for Children and Adults category includes activities focused on caring for children and adults. The Other Activities category includes educational activities, shopping, civic and volunteer activities, government services, religious activities, telephone calls, pet care, medical care, and travel. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table 2: Summary Statistics for Mothers with 5- to 6-year-olds and Mothers with 7- to 9-year-olds

| | Mothers with 5 to 6-year-old (1) | Mother With 7 to 9-year-old (2) | Within Area Difference (3) |
|---|---|---|---|
| Non-Hispanic White | 0.596 | 0.591 | 0.001 |
| Non-Hispanic Black | 0.135 | 0.138 | -0.002 |
| Non-Hispanic Asian | 0.039 | 0.040 | -0.000 |
| Non-Hispanic Other | 0.036 | 0.034 | 0.002*** |
| Hispanic | 0.195 | 0.198 | 0.000 |
| Age | 34.046 | 36.803 | -2.742*** |
| Married | 0.730 | 0.717 | 0.013*** |
| Never Married | 0.153 | 0.133 | 0.020*** |
| Sep/Div/Widowed | 0.117 | 0.150 | -0.033*** |
| HS or Less | 0.416 | 0.419 | -0.003 |
| Some College | 0.288 | 0.299 | -0.010*** |
| College Degree | 0.295 | 0.282 | 0.013*** |
| Immigrant | 0.203 | 0.211 | -0.006** |
| Citizen | 1.759 | 1.780 | -0.015 |
| In Labor Force | 0.654 | 0.710 | -0.056*** |
| Employed | 0.609 | 0.668 | -0.059*** |
| Hours Worked | 20.810 | 23.407 | -2.564*** |
| Employed Last Year | 0.676 | 0.728 | -0.053*** |
| Employed Full-time Last Year | 0.467 | 0.523 | -0.054*** |
| Employed Part-time Last Year | 0.209 | 0.206 | 0.001 |
| Usual Hours Worked Last Year | 23.720 | 26.055 | -2.303*** |
| Weeks Worked Last Year | 30.023 | 33.193 | -3.146*** |
| Wage Income (1,000s) | 23.286 | 25.880 | -2.498*** |
| Total Family Income (1,000s) | 82.879 | 86.002 | -3.008*** |
| Observations | 157,553 | 168,714 | |

NOTE: Sample restricted to mothers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. Averages for each group reported in column (1) and (2). Column (3) provides the raw difference between mothers with 5-6 year-olds and mothers with 7-9 year-olds (and no 5-6 year-old). Column (4) provides the difference between mothers with 5-6 year-olds and mothers with 7-9 year-olds (and no 5-6 year-old) in the same state, MSA, and year, by including state-by-year and MSA-by-year fixed effects. Observations are weighted using the March ASEC sampling weights. $p< 0.01$ ***, $p< 0.05$ **, $p<0.1$ *.

Table 3: Impact of Full-Day Kindergarten Share on Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds

| | Outcome: Last Week | | Outcome: Last Year | | | | |
|---|---|---|---|---|---|---|---|
| | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | | Sample: Mothers | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.055*** | 1.792*** | 0.043*** | 0.026* | 2.627*** | 0.058*** | 230.5 |
| *Have Child 5-6 | (0.014) | (0.580) | (0.014) | (0.014) | (0.734) | (0.014) | (1358.849) |
| Dependent Mean | 0.64 | 22.15 | 0.70 | 0.50 | 31.65 | 0.67 | 24619.34 |
| Observations | 326,267 | 317,940 | 326,267 | 326,267 | 326,267 | 326,267 | 326,267 |
| | | | | Sample: Fathers | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.002 | -0.358 | -0.004 | -0.001 | -0.004 | -0.004 | -1097.5 |
| *Have Child 5-6 | (0.011) | (0.643) | (0.005) | (0.008) | (0.459) | (0.008) | (1571.502) |
| Dependent Mean | 0.88 | 38.74 | 0.94 | 0.90 | 46.62 | 0.88 | 63155.52 |
| Observations | 266,083 | 260,419 | 266,083 | 266,083 | 266,083 | 266,083 | 266,083 |

NOTE: Sample restricted to mothers and fathers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. Outcomes in column (1) and (2) refer to outcomes in the preceding week (the ASEC is conducted in March). The remaining outcomes refer to the previous calendar year. The full-day kindergarten share is constructed from the CPS October Supplement and captures the share of kindergartners in public school attending full-day. As such, we use the measure from the previous year, corresponding to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the 3-year rolling average of the full-day kindergarten share for each state. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table 4: Impact of Full-Day Kindergarten Share on Labor Supply of Mothers with 5- to 6-year-olds and 7- to 9-year-olds Separately

| | Outcome: Last Week | | Outcome: Last Year | | | | |
|---|---|---|---|---|---|---|---|
| | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | Sample: Mothers with 5-6 Year Old | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.045** | 2.266*** | 0.037* | 0.075*** | 1.354 | 0.054*** | 2719.1* |
| | (0.019) | (0.786) | (0.019) | (0.024) | (0.993) | (0.018) | (1506.993) |
| Dependent Mean | 0.61 | 20.81 | 0.68 | 0.47 | 30.02 | 0.64 | 23282.15 |
| Observations | 157,598 | 153,503 | 157,598 | 157,598 | 157,598 | 157,598 | 157,598 |
| | | | Sample: Mothers of 7-9 Year Old | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.020 | 1.012 | 0.017 | 0.053 | 0.225 | 0.026 | 2517.3 |
| | (0.025) | (1.089) | (0.025) | (0.033) | (1.283) | (0.024) | (2249.291) |
| Dependent Mean | 0.67 | 23.41 | 0.73 | 0.52 | 33.19 | 0.69 | 25879.38 |
| Observations | 168,768 | 164,537 | 168,768 | 168,768 | 168,768 | 168,768 | 168,768 |

NOTE: Sample restricted to mothers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5-6 in the top panel and mothers with a child 7-9, and no 5- or 6-year-old in the bottom panel. Outcomes in column (1) and (2) refer to outcomes in the preceding week (the ASEC is conducted in March). The remaining outcomes refer to the previous calendar year. The full-day kindergarten share is constructed from the CPS October Supplement and captures the share of kindergartners in public school attending full-day. As such, we use the measure from the previous year, corresponding to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the 3-year rolling average of the full-day kindergarten share for each state. This table estimates the effect of the three-year rolling average full day kindergarten share on maternal employment, including state fixed effects, MSA fixed effects, and year fixed effects for the specified group of mothers. These fixed effects absorb level differences in maternal employment across geography as well as aggregate trends over time. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table 5: Robustness: Impact of Full-Day Kindergarten Share on Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds

| | Outcome: Employed Last Year | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Baseline | Public+Private Full-day Share | 5-Year-old Full-day Share | Eldest Age-by-MSA-by-Year Fixed Effects | Parental Controls | Preschool Controls | Restrict to Non-South | Pre-COVID |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Sample: Mothers | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.043*** | 0.046*** | 0.055*** | 0.032* | 0.035*** | 0.039** | 0.048*** | 0.034** |
| *Have Child 5-6 | (0.014) | (0.015) | (0.020) | (0.017) | (0.013) | (0.019) | (0.016) | (0.015) |
| Dependent Mean | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 |
| Observations | 326,267 | 326,267 | 326,267 | 292,802 | 326,266 | 310,968 | 224,389 | 301,855 |
| | | | | Sample: Fathers | | | | |
| Share Full-day Kindergarten$_{t-1}$ | -0.004 | -0.003 | -0.006 | -0.008 | -0.009* | -0.001 | -0.007 | -0.005 |
| *Have Child 5-6 | (0.005) | (0.006) | (0.008) | (0.009) | (0.005) | (0.008) | (0.005) | (0.006) |
| Dependent Mean | 0.94 | 0.94 | 0.94 | 0.95 | 0.94 | 0.94 | 0.94 | 0.94 |
| Observations | 266,083 | 266,083 | 266,083 | 234,099 | 266,079 | 253,726 | 186,544 | 245,661 |

NOTE: Sample restricted to mothers and fathers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. The full-day kindergarten share was constructed from the CPS October Supplement and captures the share of kindergartners in public school attending full-day. As such, we use the measure from the previous year, as this corresponds to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the 3-year rolling average of the full-day kindergarten share for each state. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Column (1) provides our baseline estimates from Table (3). Column (2) uses the Share Full-day Kindergarten for all kindergartners, including those in private school. Column (3) uses the Share of 5-year-olds in public full-day kindergarten. Column (4) replaces the MSA-by-year fixed effects with age of eldest child-by-MSA-by-year fixed effects to compare mothers in the same cohort, labor market, and year. Column (5) controls for parental characteristics including race, education, and marital status bins interacted with the indicator for having a child ages 5 or 6, and age fixed effects. Column (6) includes preschool controls. These controls include the preschool teacher-to-student ratio, the share of 3- and 4-year-olds in preschool, and the share of 3- and 4-year-olds in full-day preschool in the previous school year, all interacted with the indicator for having a child ages 5 or 6. Column (7) only includes the Non-South, to ensure this is not driven only by the early adopting states in the South. Column (8) excludes observations from 2020 and 2021 to avoid concerns about the pandemic. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table 6: IV Estimates: Impact of Full-Day Kindergarten Share on Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds

| | First Stage | Outcome: Last Week | | Outcome: Last Year | | | | |
|---|---|---|---|---|---|---|---|---|
| | Share Full-day*Child 5-6 | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Sample: Mothers | | | | |
| Friendly Policy*Have Child 5-6 | 0.313*** | | | | | | | |
| | (0.053) | | | | | | | |
| Share Full-day Kindergarten$_{t-1}$ | | 0.063* | 2.270* | 0.046* | 0.038 | 3.451* | 0.067* | 3227.1 |
| *Have Child 5-6 | | (0.032) | (1.143) | (0.027) | (0.033) | (2.018) | (0.036) | (1996.952) |
| F-Statistic | 34.9 | | | | | | | |
| Dependent Mean | 0.32 | 0.64 | 22.15 | 0.70 | 0.50 | 31.65 | 0.67 | 24619.34 |
| Observations | 326,267 | 326,267 | 317,940 | 326,267 | 326,267 | 326,267 | 326,267 | 326,267 |
| | | | | Sample: Fathers | | | | |
| Friendly Policy*Have Child 5-6 | 0.317*** | | | | | | | |
| | (0.052) | | | | | | | |
| Share Full-day Kindergarten$_{t-1}$ | | 0.002 | -0.658 | 0.010 | -0.001 | 0.388 | 0.008 | 2248.6 |
| *Have Child 5-6 | | (0.021) | (1.116) | (0.015) | (0.019) | (1.082) | (0.017) | (3134.505) |
| F-Statistic | 37.0 | | | | | | | |
| Dependent Mean | 0.32 | 0.88 | 38.74 | 0.94 | 0.90 | 46.62 | 0.88 | 63155.52 |
| Observations | 266,083 | 266,083 | 260,419 | 266,083 | 266,083 | 266,083 | 266,083 | 266,083 |

NOTE: Sample restricted to mothers and fathers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. The outcome in column (1) is the interaction between the Full-day Share and the indicator for having a child ages 5 or 6. Outcomes in column (2) and (3) refer to outcomes in the preceding week (the ASEC is conducted in March). The remaining outcomes refer to the previous calendar year. The full-day kindergarten share constructed from the CPS October Supplement and captures the share of kindergartners in public school attending full-day. As such, we use the measure from the previous year, as this corresponds to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the 3-year rolling average of the full-day kindergarten share for each state. We use an indicator for having a full-day friendly policy in place, interacted with an indicator for having a child ages 5 to 6 to instrument for *Share Full-day Kindergarten∗ Have Child 5-6*. First stage and reduced form evidence is provided in Appendix Table A6. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

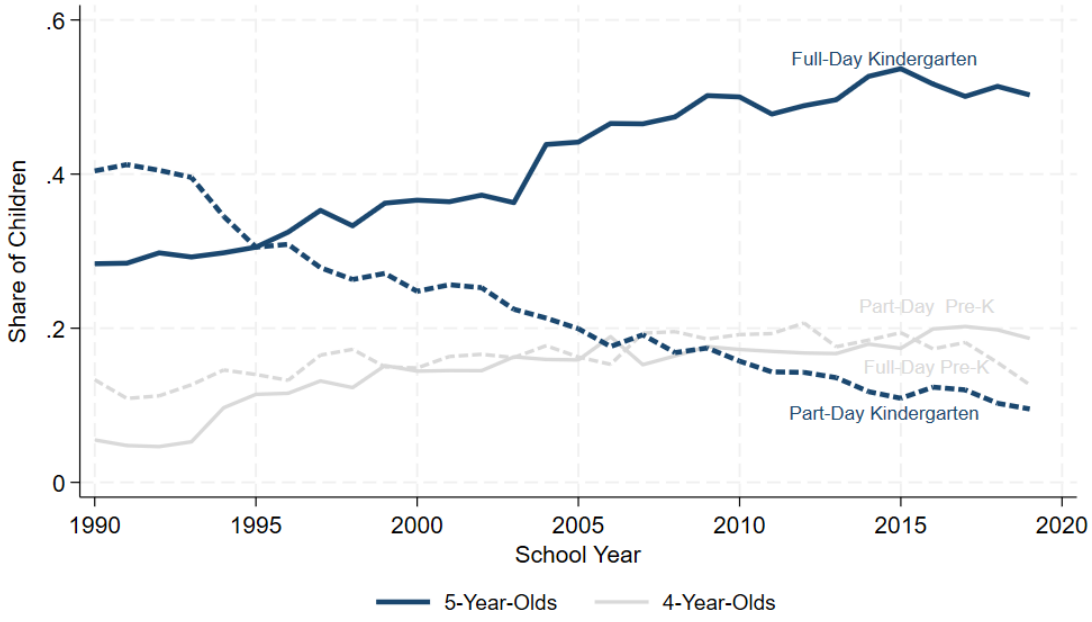
Table 7: Impact of Full-Day Kindergarten Share on Monthly Childcare Expenses in Households with 5- to 6-year-olds

| | | Relative to Families of Children Ages 7-9 | | | | Relative to Families of Children Ages 0-4 | |
|---|---|---|---|---|---|---|---|
| | | All Families | | Focal Child is Youngest Child | | All Families | |
| | First Stage Share Full-day*Child 5-6 | Expense>0 | Expense Amount | Expense>0 | Expense Amount | Expense>0 | Expense Amount |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: Baseline Model | | | | | | | |
| Share Full-day Kindergarten$_{t-1}$ | | -0.034 | 50.443* | -0.128*** | -18.306 | -0.032 | -29.654 |
| *Have Child 5-6 | | (0.035) | (25.794) | (0.035) | (24.210) | (0.042) | (30.088) |
| Dependent Mean | | 0.22 | 103.22 | 0.17 | 59.30 | 0.29 | 162.05 |
| Observations | | 204,181 | 204,181 | 126,196 | 126,196 | 248,681 | 248,681 |
| Panel B: Instrumental Variables Estimates using Full-Day Friendly Kindergarten Policy Instrument | | | | | | | |
| Friendly Policy*Have Child 5-6 | 0.244*** | | | | | | |
| | (0.051) | | | | | | |
| Share Full-day Kindergarten$_{t-1}$ | | -0.089 | 121.262** | -0.141 | -25.951 | -0.124 | -91.756 |
| *Have Child 5-6 | | (0.091) | (48.252) | (0.099) | (52.371) | (0.081) | (62.996) |
| F-Statistic | 23.0 | | | | | | |
| Dependent Mean | 0.26 | 0.22 | 103.22 | 0.17 | 59.30 | 0.29 | 162.05 |
| Observations | 248,681 | 204,181 | 204,181 | 126,196 | 126,196 | 248,681 | 248,681 |

NOTE: Sample restricted to households in the Consumer Expenditure Survey between 1992 and 2022 with a child between the ages of 5 and 9 (columns 1-5) or ages 0 and 6 (columns 6-7). Outcomes in columns (1) - (5) estimated impacts using households with a child between the ages of 7 and 9 as a counterfactual group, and outcomes in columns (6) - (7) estimated impacts using households with a child between the ages of 0 and 4 as a counterfactual group. The sample is restricted to childcare expenses during the school year. Panel A estimates impacts using the baseline model analogous to equation (1). Panel B estimates impacts using the introduction of state-level full-day friendly policy as an instrument. Column (1) estimates the first stage using families with older children as a comparison. First stage estimates using younger children as a comparison remain similar in magnitude and significance. Columns (2) (4) and (6) estimate monthly expenses in dollars ($2020). Columns (3) (5) and (7) use a binary measure of whether a family spent any amount on child care. Columns (4)-(5) restrict the sample to families whose youngest child is the focal child, either kindergarten aged or slightly older for comparison families. The full-day kindergarten share was constructed from the CPS October Supplement and captures the share of kindergartners in public school attending full-day. Year adjustments in the CEX were made for expense reports from January to June to align CEX data with the same academic year used in the CPS. Because the October Supplement samples are small, we use the 3-year rolling average of the full-day kindergarten share for each state. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6, which makes this a comparison between households with 5- and 6-year-olds and households of either older or younger children in the same state and year. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the household level. p< 0.01 ***, p< 0.05 **, p<0.1 *.

# Appendix A: Additional Tables and Figures (Online Appendix)

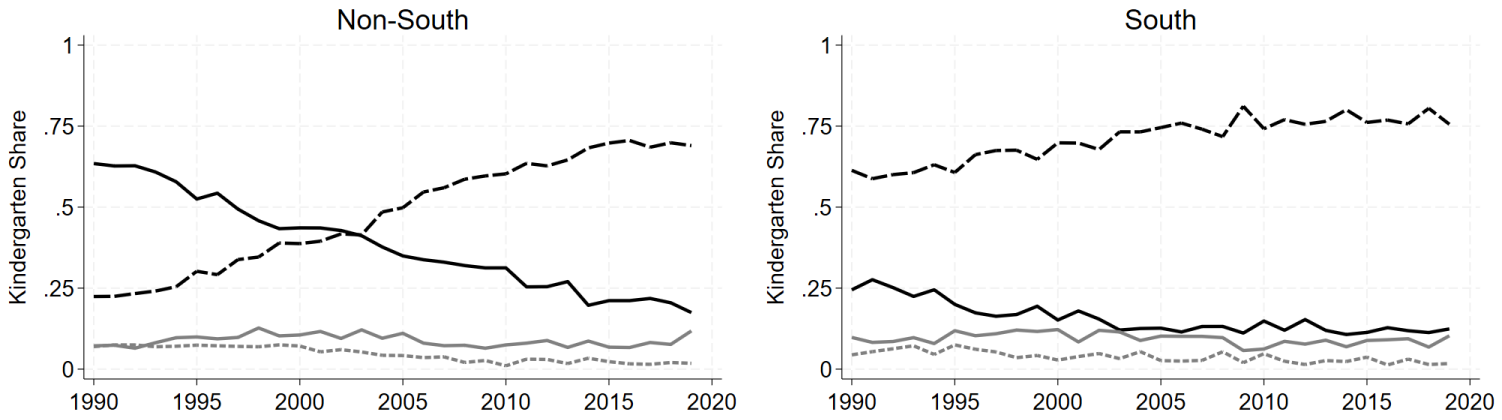
Figure A1: Full-Day and Part-Day Kindergarten and Pre-Kindergarten Enrollment




NOTE: Sample restricted to 4- and 5-year-olds from the CPS October Supplement between 1990 and 2019. Individual weights are used to make this nationally representative. A small fraction of 4-year-olds are enrolled in kindergarten.

SOURCE: CPS October School Enrollment Supplement 1990-2019.

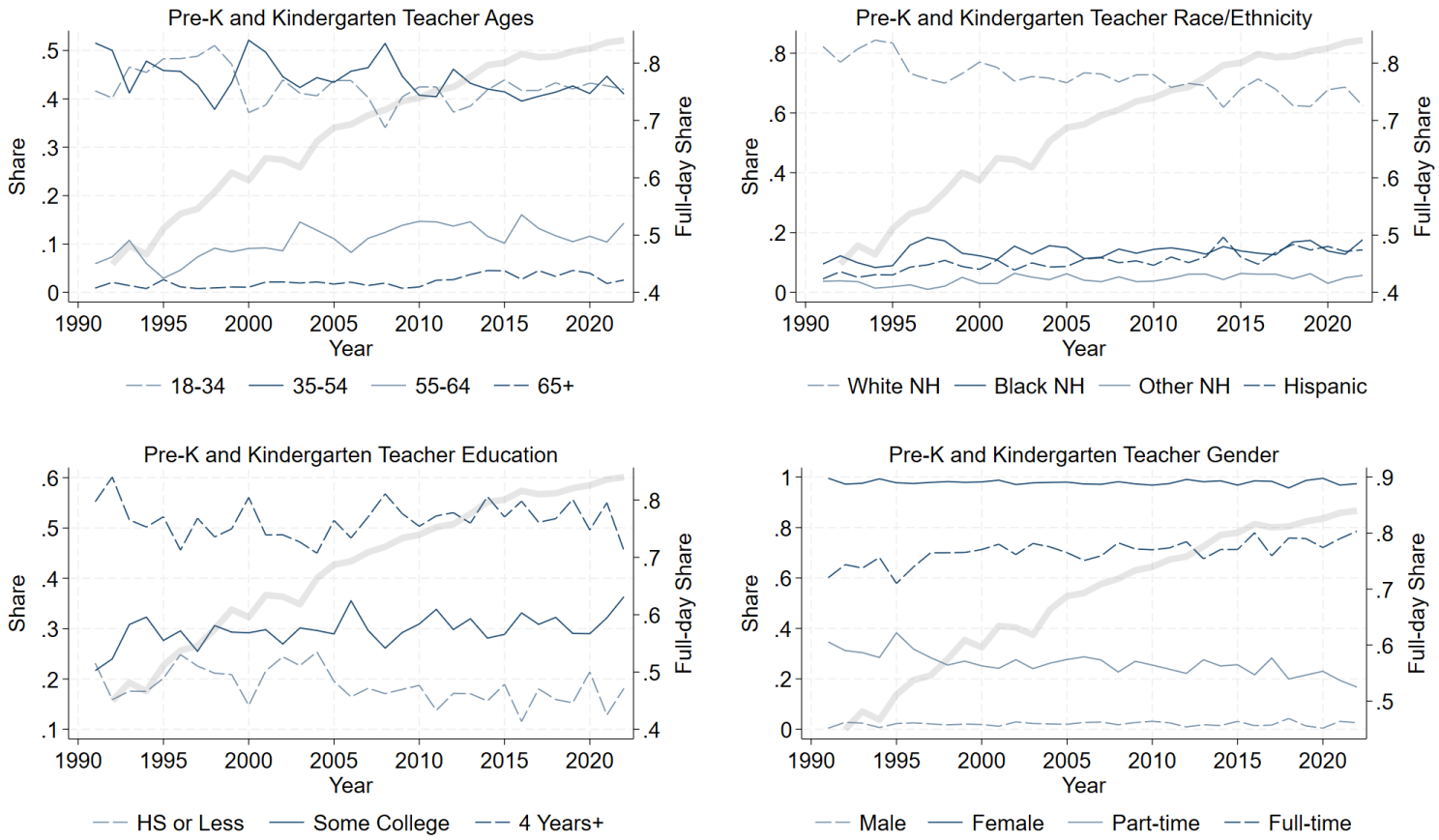
Figure A2: Public vs. Private Kindergarten Enrollment




NOTE: Sample restricted to children ages 3 to 7 from the CPS October Supplement between 1990 and 2019 who reported being in kindergarten. Individual weights are used to make this nationally representative.

SOURCE: CPS October School Enrollment Supplement 1990-2019.

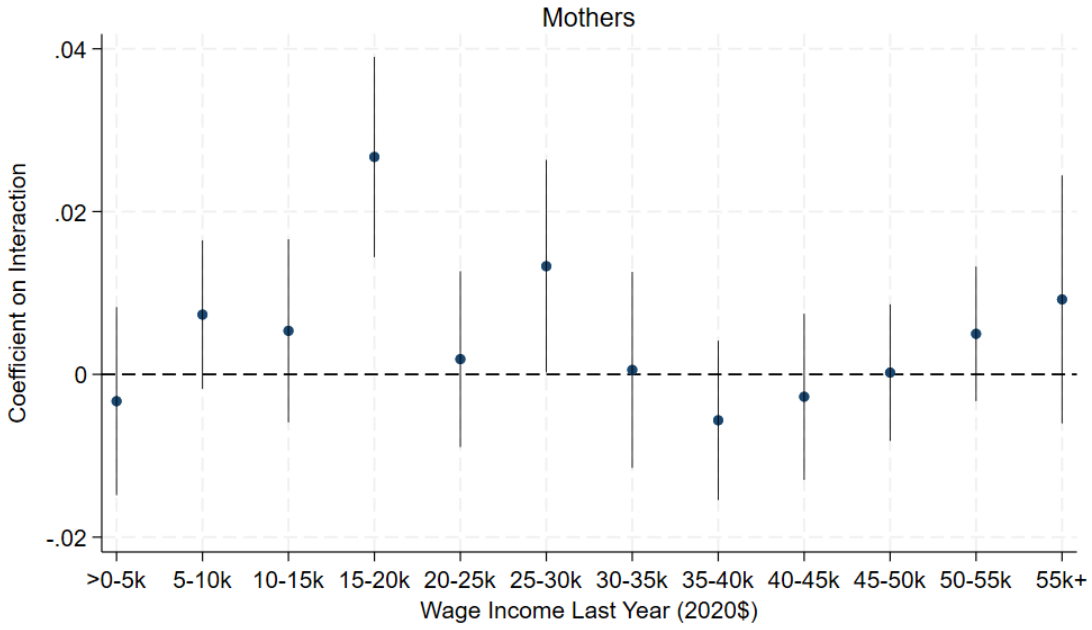
Figure A3: Characteristics of Pre-Kindergarten and Kindergarten Teachers Over Time




NOTE: Sample restricted to individuals reporting occupation code 2300, pre-kindergarten or kindergarten teacher, between 1991 and 2022. The sample is then collapsed to the annual level, using ASEC survey weights to construct mean characteristics of pre-k and kindergarten teachers. For comparison, the light gray line aligns with the right-hand-side axis and shows trends in the share of kindergartners in public school who attend full-day kindergarten (also shown in Figure 2).

SOURCE: Authors' calculations from the CPS 1991-2022.

Figure A4: Effect of Full-Day Kindergarten Share on Wage Income of Mothers with 5- to 6-year-olds Relative to Mothers with 7- to 9-year-olds




NOTE: Sample restricted to mothers and fathers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. Each point represents the coefficient on the interaction between full-day kindergarten share and an indicator for if the mother has a 5- or 6-year-old child, where the outcome is a binary indicator for having wage income within the specified bin. The full-day kindergarten share is constructed from the CPS October Supplement and captures the share of kindergartners in public school that attend full-day. As such, we use the measure from the previous year, as this corresponds to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use each state's 3-year rolling average of the full-day kindergarten share. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state, with 95 percent confidence intervals plotted.

SOURCE: Authors' calculations using ASEC CPS 1992-2021.

Table A1: Schooling Substitution: Impact of Full-Day Kindergarten Share on Other School Enrollment Margins

| | 5-Year-Olds | | | | | | | 6-Year-Olds in t+1 | |
|---|---|---|---|---|---|---|---|---|---|
| | Half-day Public (1) | Full-day Private (2) | Half-day Private (3) | Full-day Pre-k (4) | Half-day Pre-k (5) | First Public (6) | First Private (7) | First Public (8) | First Private (9) |
| Share Full-day Kindergarten$_{t-1}$ | -0.735*** | 0.003 | -0.069*** | -0.041*** | -0.075*** | -0.025 | -0.002 | 0.080*** | -0.022 |
| | (0.037) | (0.014) | (0.014) | (0.015) | (0.016) | (0.020) | (0.004) | (0.027) | (0.014) |
| Dependent Mean | 0.21 | 0.06 | 0.03 | 0.07 | 0.08 | 0.04 | 0.01 | 0.65 | 0.07 |
| Observations | 1,632 | 1,632 | 1,632 | 1,632 | 1,632 | 1,632 | 1,632 | 1,632 | 1,632 |

NOTE: Observation at the state by year level from 1991 to 2022 constructed from CPS October Supplement microdata. For each observation, the share of 5-year-olds in each type of schooling is constructed. For columns (8) and (9) the type of schooling is constructed for 6-year-olds in the following year, to observe persistent changes in school enrollment. The Share Full-day Kindergarten is the share of 5-year-olds in the CPS October Supplement that were enrolled in public full-day kindergarten. State and year fixed effects are included. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A2: Impact of Full-Day Kindergarten Share on Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds, by Presence of Younger Children

| | Outcome: Last Week | | Outcome: Last Year | | | | |
|---|---|---|---|---|---|---|---|
| | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | Sample: Mothers for Whom Focal Child is Youngest | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.045*** | 1.271*** | 0.035*** | 0.015 | 2.060*** | 0.047*** | -893.5 |
| *Have Child 5-6 | (0.013) | (0.474) | (0.013) | (0.016) | (0.671) | (0.013) | (2005.898) |
| Dependent Mean | 0.69 | 24.46 | 0.75 | 0.54 | 34.46 | 0.71 | 27246.55 |
| Observations | 205,069 | 200,174 | 205,069 | 205,069 | 205,069 | 205,069 | 205,069 |
| | | | Sample: Mothers with Younger Children | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.041 | 1.249 | 0.029 | 0.010 | 1.996 | 0.035 | 2654.6 |
| *Have Child 5-6 | (0.028) | (1.308) | (0.028) | (0.029) | (1.405) | (0.027) | (1852.968) |
| Dependent Mean | 0.55 | 18.19 | 0.62 | 0.42 | 26.89 | 0.58 | 20190.96 |
| Observations | 120,208 | 116,717 | 120,208 | 120,208 | 120,208 | 120,208 | 120,208 |

NOTE: Sample restricted to mothers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. Outcomes in column (1) and (2) refer to outcomes in the preceding week (the ASEC is conducted in March). The remaining outcomes refer to the previous calendar year. The top panel restricts the sample to mothers whose youngest child is the focal child in the 5- to 9-year-old sample. The bottom panel restricts the sample to mothers who have a younger child than the focal child in the household. The full-day kindergarten share is constructed from the CPS October Supplement and captures the share of kindergartners in public school that attend full-day. As such, we use the measure from the previous year, as this corresponds to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the state 3-year rolling average of the full-day kindergarten share. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A3: Impact of Full-Day Kindergarten Share on Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds, by Maternal Grandmother in Home

| | Outcome: Last Week | | Outcome: Last Year | | | | |
|---|---|---|---|---|---|---|---|
| | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | Sample: Mothers with Maternal Grandmother in Household | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.083 | 2.872 | -0.039 | -0.083 | 2.649 | 0.006 | 3918.5 |
| *Have Child 5-6 | (0.080) | (3.140) | (0.081) | (0.090) | (3.756) | (0.088) | (4202.455) |
| Dependent Mean | 0.62 | 22.29 | 0.69 | 0.53 | 31.08 | 0.67 | 19848.73 |
| Observations | 16,360 | 15,975 | 16,360 | 16,360 | 16,360 | 16,360 | 16,360 |
| | | | Sample: Mothers with No Maternal Grandmother in Household | | | | |
| Share Full-day Kindergarten$_{t-1}$ | 0.049*** | 1.534*** | 0.041*** | 0.025* | 2.383*** | 0.054*** | -115.0 |
| *Have Child 5-6 | (0.014) | (0.563) | (0.013) | (0.013) | (0.715) | (0.014) | (1423.025) |
| Dependent Mean | 0.64 | 22.13 | 0.70 | 0.49 | 31.68 | 0.66 | 24922.03 |
| Observations | 307,992 | 300,043 | 307,992 | 307,992 | 307,992 | 307,992 | 307,992 |

NOTE: Sample restricted to mothers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. Outcomes in column (1) and (2) refer to outcomes in the preceding week (the ASEC is conducted in March). The remaining outcomes refer to the previous calendar year. The top panel restricts the sample to mothers whose own mother is on the household roster. The bottom panel restricts the sample to mothers whose own mother is not on the household roster. The full-day kindergarten share is constructed from the CPS October Supplement and captures the share of kindergartners in public school that attend full-day. As such, we use the measure from the previous year, as this corresponds to the same school year as the March ASEC observation. Because the October Supplement samples are small, we use the state 3-year rolling average of the full-day kindergarten share. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A4: Impact of Full-Day Kindergarten Share on Schooling of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds

| | In School (1) | Enrolled in College (2) | Full-time College (3) | Part-time College (4) | In School Last Year (5) |
|---|---|---|---|---|---|
| | | | Sample: Mothers | | |
| Share Full-day Kindergarten$_{t-1}$ | -0.002 | 0.003 | -0.001 | 0.004 | 0.006 |
| *Have Child 5-6 | (0.008) | (0.007) | (0.006) | (0.005) | (0.006) |
| Dependent Mean | 0.06 | 0.06 | 0.03 | 0.03 | 0.07 |
| Observations | 184,227 | 184,227 | 184,227 | 184,227 | 184,227 |
| | | | Sample: Fathers | | |
| Share Full-day Kindergarten$_{t-1}$ | -0.002 | 0.002 | 0.005 | -0.003 | -0.011 |
| *Have Child 5-6 | (0.007) | (0.006) | (0.005) | (0.005) | (0.008) |
| Dependent Mean | 0.03 | 0.03 | 0.01 | 0.02 | 0.04 |
| Observations | 146,657 | 146,657 | 146,657 | 146,657 | 146,657 |

NOTE: Sample restricted to mothers and fathers in the October CPS between 1994 and 2021 with a child between the ages of 5 and 9. The full-day kindergarten share constructed from the CPS October Supplement and captures the share of kindergartners in public school that attend full-day. Because the October Supplement samples are small, we use the state 3-year rolling average of the full-day kindergarten share. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the monthly CPS sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A5: Sensitivity to Movers: Impact of Full-Day Kindergarten Share on Moving and Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds

| | | Exclude Cross-State Movers | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Moved States in Past Year | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Sample: Mothers | | | | |
| Share Full-day Kindergarten$_{t-1}$ | -0.006 | 0.054*** | 1.783*** | 0.043*** | 0.030** | 2.579*** | 0.059*** | 302.6 |
| *Have Child 5-6 | (0.005) | (0.015) | (0.599) | (0.014) | (0.014) | (0.739) | (0.014) | (1368.860) |
| Dependent Mean | 0.02 | 0.64 | 22.26 | 0.70 | 0.50 | 31.81 | 0.67 | 24788.81 |
| Observations | 326,267 | 318,907 | 310,756 | 318,907 | 318,907 | 318,907 | 318,907 | 318,907 |
| | | | | Sample: Fathers | | | | |
| Share Full-day Kindergarten$_{t-1}$ | -0.001 | 0.006 | -0.247 | -0.004 | -0.000 | 0.016 | -0.004 | -1144.7 |
| *Have Child 5-6 | (0.004) | (0.011) | (0.653) | (0.006) | (0.008) | (0.448) | (0.008) | (1565.167) |
| Dependent Mean | 0.02 | 0.89 | 38.84 | 0.94 | 0.90 | 46.67 | 0.88 | 63258.12 |
| Observations | 266,083 | 260,332 | 254,795 | 260,332 | 260,332 | 260,332 | 260,332 | 260,332 |

NOTE: In column (1) the sample is restricted to mothers and fathers in the CPS ASEC between 1992 and 2022 with a child between the age of 5 and 9, the same sample used throughout. In columns (2)-(9) individuals that moved from out of state in the past year are excluded. The full-day kindergarten share constructed from the CPS October Supplement and captures the share of kindergartners in public school that attend full-day. Because the October Supplement samples are small, we use the state 3-year rolling average of the full-day kindergarten share. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A6: IV Estimates Reduced Form: Impact of Full-Day Policy on Labor Supply of Parents with 5- to 6-year-olds Relative to Parents with 7- to 9-year-olds

| | Outcome: Last Week | | Outcome: Last Year | | | | |
|---|---|---|---|---|---|---|---|
| | Employed | Hours | Employed | Full-Time | Weeks Worked | Wage Income> 0 | Wage Income (2020) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | | Sample: Mothers | | | |
| Full-day Friendly Policy | 0.022** | 0.814** | 0.016** | 0.013 | 1.187** | 0.023*** | 1033.4* |
| *Have Child 5-6 | (0.009) | (0.326) | (0.006) | (0.009) | (0.478) | (0.008) | (586.434) |
| Dependent Mean | 0.64 | 22.09 | 0.70 | 0.49 | 31.58 | 0.66 | 24345.85 |
| Observations | 335,609 | 327,050 | 335,609 | 335,609 | 335,609 | 335,609 | 335,609 |
| | | | | Sample: Fathers | | | |
| Full-day Friendly Policy | 0.003 | -0.102 | 0.003 | -0.000 | 0.172 | 0.003 | 776.4 |
| *Have Child 5-6 | (0.006) | (0.356) | (0.005) | (0.006) | (0.319) | (0.005) | (952.445) |
| Dependent Mean | 0.88 | 38.75 | 0.94 | 0.90 | 46.64 | 0.88 | 62696.41 |
| Observations | 273,501 | 267,634 | 273,501 | 273,501 | 273,501 | 273,501 | 273,501 |

NOTE: Sample restricted to mothers and fathers in the CPS ASEC between 1992 and 2022 with a child between the ages of 5 and 9. Estimates replicate the reduced form version of Table 3. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the March ASEC sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A7: Individual Enrollment IV Estimates: Impact of Full-Day Attendance on Labor Supply of Mothers

| | First Stage | Reduced Form | | IV Estimates | |
|---|---|---|---|---|---|
| | All Children in Full-day | Employed | Hours Last Week | Employed | Hours Last Week |
| | (1) | (2) | (3) | (4) | (5) |
| | | Sample: Mothers | | | |
| Friendly Policy*Have Child 5-6 | 0.104*** | 0.021*** | 1.047*** | | |
| | (0.027) | (0.007) | (0.220) | | |
| All Children in Full-day | | | | 0.204*** | 10.113*** |
| | | | | (0.055) | (3.745) |
| F-Statistic | 15.3 | | | | |
| Dependent Mean | 0.87 | 0.65 | 22.26 | 0.65 | 22.26 |
| Observations | 188,379 | 188,379 | 188,379 | 188,379 | 188,379 |

NOTE: Sample restricted to mothers in the October CPS between 1994 and 2022 with a child between the ages of 5 and 9. The endogenous treatment measure "All Children In Full-day" is an indicator that equals one if all of the mother's children between the ages of 5 and 9 are enrolled in full-day school. The mother might have children under age 5 that are not enrolled in full-day school. The excluded instrument is an indicator for whether there is a full-day friendly policy passed in the state interacted with an indicator for whether the mother has a kindergarten-aged child, 5- or 6-years-old. Column (1) is the first stage, columns (2)-(3) are the reduced form, and columns (4)-(5) are the individual-level two stage estimates. State-by-year fixed effects are included, absorbing the direct effect of the full-day kindergarten share and controlling for state-specific trends in parental employment. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. MSA-by-year fixed effects are also included, making this a comparison between parents with 5- and 6-year-olds and parents of 7- to 9-year-olds in the same MSA and year. This holds labor market conditions fixed between the treatment and counterfactual groups. Observations are weighted using the CPS monthly sampling weights. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

# Appendix B: Data Appendix

## Full-day Kindergarten Data

We primarily rely on the October School Enrollment Supplement of the Current Population Survey (CPS) to measure trends in full-day kindergarten participation from 1990 to 2023 (Flood et al., 2023b). The CPS is a nationally representative survey in the United States which is administered by the U. S. Census Bureau. Participating households provide information for eight waves that span over sixteen months. Going back before 1990, all households being surveyed during October are offered additional education questions. Families report school enrollment for each child in the household, including participation in full-day and half-day kindergarten. We limit the sample to children between the ages of 3 and 7 who are reported to be in public kindergarten. This sample captures 99.9 percent of all public kindergartners.

We then aggregate these reports to the state level each year to study broad trends in full-day kindergarten. The full-day kindergarten data is constructed as weighted annual counts of total kindergarten students in public schools and full-day kindergarten students in public schools in each state, calculated as a full-day proportion, and averaged as three-year moving averages. While the use of three-year rolling averages smooths out some of the sampling noise in the yearly estimates, variation remains. Because school enrollment reports are made at the child level, we can evaluate trends with sensitivity to geography and family demographics.

Analyses of enrollment trends focus on the period of 1990 through 2019 to avoid complications with reporting and actual enrollment in the October 2020 Supplement and beyond due to the COVID-19 pandemic. We supplement the full-day kindergarten participation data with data from the National Center for Education Statistics' Common Core of Data on full-time equivalent kindergarten teachers and other education resource measures by state for the years of overlap (National Center for Education Statistics, 2023). The Common Core of Data begins in 1992.

## Parent Time Use Data

We draw time use data from the American Time Use Survey (ATUS) (Flood et al., 2023a). The dataset is nationally representative of households in the United States. The Bureau of Labor Statistics administers the survey in connection with the CPS. Households are selected to participate in the ATUS from a random sample of the outgoing rotation of the CPS. The ATUS is a subsample of the CPS offered two to five months after the final wave of the CPS. The ATUS holds an interview with one household member to document their time use over a 24-hour period from 4:00am of the day preceding the interview until 4:00am of the interview day. Time diary data collection using the Day Reconstruction Method has been validated (Kahneman et al.

(2004)) and provides high quality and detailed data on family time use.

The October School Enrollment Supplement of the CPS has detailed information on whether a child is enrolled in full- versus half-day kindergarten. We leverage this information by linking ATUS respondents to their most previous October Supplement. Not all ATUS respondents took the CPS October supplement. Given the October supplement linking and school day requirement, all included respondents were surveyed in the ATUS from December to May. Importantly, the linking facilitates a comparison of families with children in different kindergarten contexts. We used information from the family roster in the October supplement of the CPS to identify parents with a child in either half-day kindergarten or full-day kindergarten. No parents had both a half- and full-day kindergartner. We also identified parents who had a child in second grade and no child in kindergarten, so that we could use these parents who all had older children in a full-day school context as a comparison group.

We measured time use variable from the ATUS. The primary measures of interest were work time, time spent with any children, and travel time with children. We also measured additional activities relevant to the parenting context such as one-on-one time, developmental care time, etc. (see Table 1 for a complete list of time use variables). Time use measures were developed in two ways. First, we measured the total minutes spent through the day to study the duration of time devoted to activities and companions. Second, to explore the timing of parent-child time and other activities through the day, we measured as a binary variable whether a parent participated in an activity of interest or not for each 15-minute intervals through the sample day.

## Parental Employment Data

We obtain parental employment measures from the March Current Population Survey (CPS) (Flood et al., 2023b). The CPS collects responses from approximately 65,000 households each month in a rotating sample. Participants are surveyed for four months, leave the sample for eight months, and then re-enter to be surveyed for four final months. Each March, the monthly CPS is accompanied by the Annual Social and Economic Supplement (ASEC). As part of the ASEC supplement, the household respondent is asked to report on work and income related outcomes, including employment, usual hours worked, weeks worked, and wage income in the previous calendar year. We use these measures (in addition to the monthly employment question) to create our main outcomes of interest. We convert dollar measures to 2020 dollars using the personal consumption expenditures price index.

We identify parents as someone with any of their own children living in the household. We use the relationship to head of household, and parent location variables (created by IPUMS) to identify the ages of

parents' children. The main sample is then restricted to mothers or fathers with a child between the ages of five and nine in the household. Treated families are those with a kindergarten-aged child (ages five and six), and comparison families have children ages 7- to 9-years-old and no kindergarten-aged children. We merge full-day kindergarten enrollment rates constructed from the CPS Education Supplement in October (described above) to March ASEC observations with a one year lag. As such, kindergarten enrollment from October 2015 is applied to parental employment records from March 2016, as these dates are in the same school year. Because of this lagged merge, the analysis sample runs from 1992 to 2022.

## Childcare Expenses Data

Childcare expenditure data at the household level were drawn from the Consumer Expenditure Survey from 1992–2022 (U. S. Department of Labor (2023)), except for two quarters in 1993 which were omitted due to missing state geography information. The dataset is nationally representative of households in the United States. The Bureau of Labor Statistics administers the survey in an ongoing manner, and households rotate into the survey each month through the year. The survey provides state-level geography information for most households in the survey.

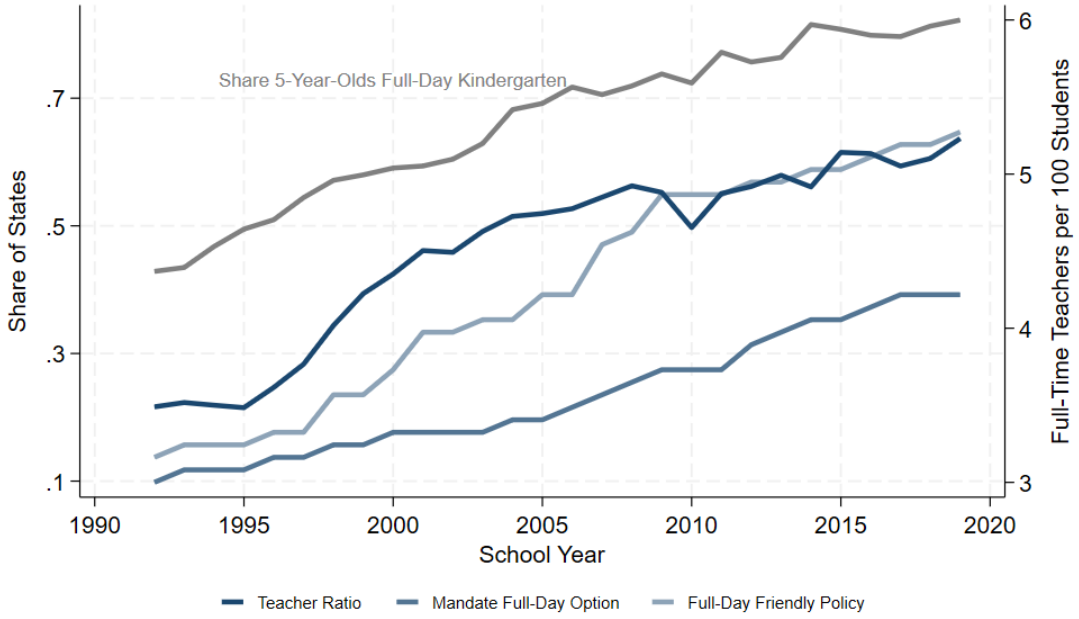
In this project, we used data from the Interview Survey, which collect information about large and/or recurring expenses to the household over the prior 3 months. The focus of the data collection on large and recurring expenses may cause us to underestimate childcare costs in households with smaller or irregular childcare costs. The survey provides information on the month and year that expenses were incurred. All information is collected at the household level, preventing any analysis of child-specific expenses. As part of the data collection, households are asked to report the dollar amount spent on babysitting and/or child care for each of the three prior months. We convert the dollar measure to 2020 dollars using the personal consumption expenditures price index. We treat each month as a separate observation in order to separate school-year and summer months.

Using ages of each household member found in the household roster, we identified the presence of any children under 10-years-old. We classified any household with children ages five and six as kindergarten-aged. We group households with children under 5-years-old and no kindergarten-aged children, and designate them as the younger comparison group. We group households with children ages 7- to 9-years-old and no kindergarten-aged children, and designate them as the older comparison group.

# Appendix C: Policy Appendix

This appendix documents the construction of the state-level policy instruments used in the analysis, including details about the existence and timing of full-day kindergarten policies in each state. Because policies were formulated and implemented at the state level, there was variation in scope and timing across the U.S. and throughout our analytic time frame of interest. Figure C1 illustrates the correspondence between the growth in the share of 5-year-olds in full-day kindergarten settings alongside the growth in the number of full-time equivalent kindergarten teachers per 100 students and the proliferation of state-level full-day kindergarten policies.

Figure C1: Full-Day Kindergarten Enrollment and Policy Environment




NOTE: Sample restricted to 5-year-olds from the CPS October Supplement between 1992 and 2019. Individual weights are used to make this nationally representative. Full-time equivalent kindergarten teachers and total student enrollment by grade obtained from the Common Core of Data.

SOURCE: CPS October School Enrollment Supplement 1992-2019, National Center for Education Statistics' Common Core of Data 1992-2019, and authors' compilation of state-level policy changes.

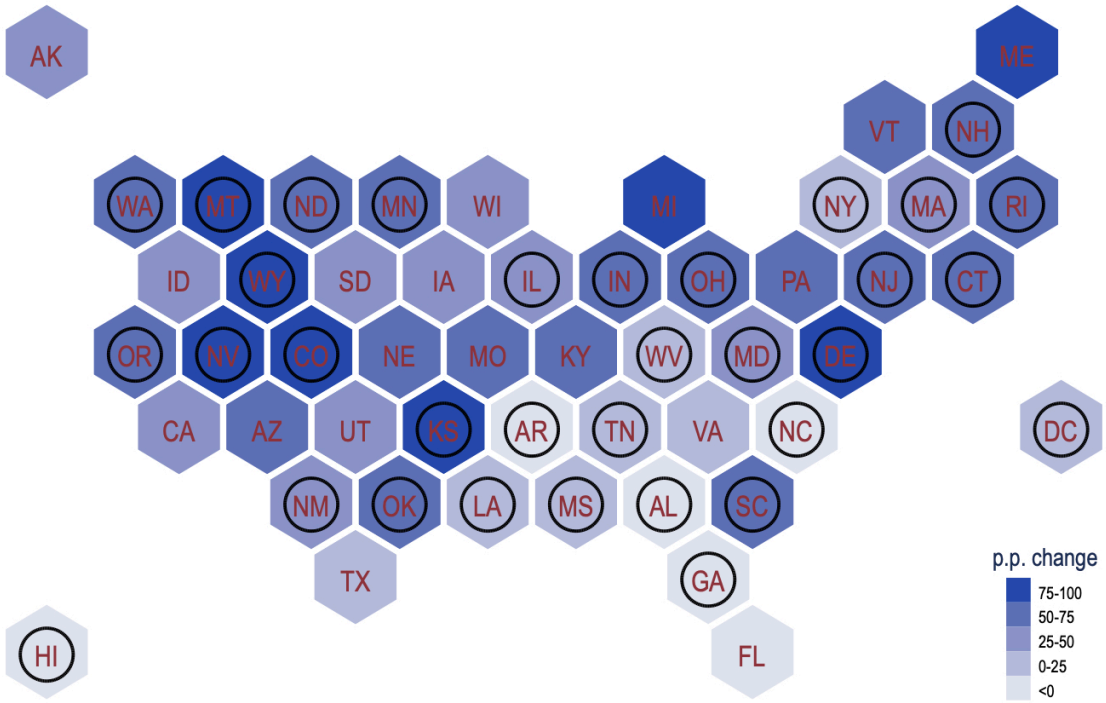
## Nature of Policy Variation

To reflect the variation in scope, we categorize full-day kindergarten policies at the state level into two groups. We first document policy changes in states that enacted statewide full-day kindergarten mandates. These policies require school districts in the state *to offer* a full-day kindergarten option to any child/family who wants that option. We then document all other policies that facilitate the expansion or provision of full-day

kindergarten in the state, which we term full-day kindergarten "friendly" policies. The policy instrument used in the analysis includes any policies that were supportive of full-day kindergarten expansion in the state, including the mandates. In states with multiple policy changes, we use the earliest policy change to assign a state as "treated" by a full-day kindergarten policy. Figure C2 displays the state-level changes in full-day kindergarten participation from 1990 through 2019, and also indicates which states had any full-day kindergarten friendly policy enacted in the same time period.

In some cases, full-day friendly policies allocated state funding if districts want to offer full-day kindergarten (e.g., Indiana, Utah) or established a council or review committee to support full-day kindergarten expansion in the state (e.g., Oregon). In some cases, the date on the enactment of the friendly policy marked the start of a phased rollout of a statewide mandate (e.g., New Mexico, Washington), with the phased rollout targeting provision in the most disadvantaged districts early. One important point to highlight is the mix of states that were early adopters. All eight states that adopted full-day kindergarten mandates prior to 2000 were located in the South.

Figure C2: State-Level Policy Changes and Full-Day Kindergarten Share Over Time




NOTE: The color gradient indicates the within-state change in the full-day kindergarten share between 1990 and 2019. Circles around state labels indicate the states that experienced any state-level full-day kindergarten policy change between 1990 and 2019.

SOURCE: CPS October School Enrollment Supplement 1990-2019 and authors' compilation of state-level policy changes.

## Process of Compiling Policy Variation

To identify legislative action and policy changes, we took three major steps: (1) a state-by-state scan of current code, including Lexis-Nexis database searches, (2) a search of policy tracking databases maintained by the Education Commission of the States and the National Conference of State Legislatures, and finally, (3) a media scan of related articles.

The first step enabled us to capture current code with respect to kindergarten and, in some cases, allowed us to track the dates of major legislative changes prior to the current situation. To better fill in the policy changes over the full time frame of interest, we also used the Education Commission of the States' state policy databases [33] and the National Conference of State Legislatures' database.[34].

Finally, we conducted an extensive scan of newspaper and media articles to ensure we had identified all the state-level policy and legislative changes in the relevant time frame. We first searched the ReadEx newspaper archives, *America's Historical Newspapers*, for articles on "full-day kindergarten" or "all-day kindergarten" in any article post-1990. Since the database focuses on particularly newspapers, we also searched the main newspaper in each state (e.g., *The Indianapolis Star* for Indiana) for the same time frame. Finally, we conducted a Google news search for the same search terms and each state name. Table C1 presents the results of this search process: the dates of state-level policy changes by type of policy, mandates and any full-day friendly policy.

[33] The Education Commission of the States (ECS) State Policy Database, Full-day Kindergarten State Legislation, for the period 1994–2020 (retrieved October 1, 2023) and ECS State Education Policy Tracking for the period 2020–2024 (retrieved March 31, 2024).

[34] The National Conference of State Legislatures Education Legislation Bill Tracking covering the period 2008–2022 (retrieved October 1, 2023).

Table C1: Full-Day Kindergarten Policy and Enrollment Changes by State

| State | Full-Day K Mandate (1) | Other Full-Day K-Friendly Policy (2) | 2019 Full-Day K Share (3) | Change from 1991 to 2019 (4) |
|---|---|---|---|---|
| Alabama | 1990 | – | 0.845 | -0.155 |
| Alaska | – | – | 0.802 | 0.304 |
| Arizona | – | – | 0.840 | 0.756 |
| Arkansas | 1984 | – | 0.875 | -0.058 |
| California | – | – | 0.613 | 0.387 |
| Colorado | – | 2001, 2005, 2007, 2008 | 0.845 | 0.744 |
| Connecticut | – | 2014 | 0.840 | 0.586 |
| Delaware | 2008 | 2003 | 0.872 | 0.715 |
| DC | 2000 | – | 0.878 | 0.004 |
| Florida | – | – | 0.867 | -0.038 |
| Georgia | 1987 | 2009 | 0.874 | -0.040 |
| Hawaii | 2012 | – | 0.820 | -0.165 |
| Idaho | – | 2022 | 0.451 | 0.404 |
| Illinois | 2027 | 1970, 2024 | 0.739 | 0.416 |
| Indiana | 2012 | 2007 | 0.957 | 0.880 |
| Iowa | – | – | 0.769 | 0.347 |
| Kansas | – | 2017, 2022 | 0.866 | 0.733 |
| Kentucky | 2022 | 2021 | 0.868 | 0.635 |
| Louisiana | 1990 | 2004 | 0.916 | -0.072 |
| Maine | – | – | 0.892 | 0.843 |
| Maryland | 2007 | 1998 | 0.827 | 0.583 |
| Massachusetts | – | 2007 | 0.802 | 0.504 |
| Michigan | – | – | 0.912 | 0.792 |
| Minnesota | 2014 | 1998 | 0.912 | 0.776 |
| Mississippi | 2006 | 1985 | 0.886 | -0.110 |
| Missouri | – | – | 0.834 | 0.320 |
| Montana | – | 2007 | 0.897 | 0.797 |
| Nebraska | – | – | 0.785 | 0.662 |
| Nevada | – | 2005 | 0.842 | 0.657 |
| New Hampshire | – | 2019 | 0.879 | 0.573 |
| New Jersey | – | 2000, 2010 | 0.844 | 0.494 |
| New Mexico | 2004 | 2001 | 0.594 | 0.385 |
| New York | – | 2005, 2007, 2018 | 0.864 | 0.248 |
| North Carolina | 1985 | – | 0.905 | -0.091 |
| North Dakota | – | 2008 | 0.925 | 0.672 |
| Ohio | – | 2009 | 0.766 | 0.513 |
| Oklahoma | 2013 | 2001, 2005 | 0.915 | 0.724 |
| Oregon | – | 2009, 2015 | 0.825 | 0.696 |
| Pennsylvania | – | – | 0.755 | 0.504 |
| Rhode Island | 2016 | – | 0.727 | 0.594 |
| South Carolina | 1998 | – | 0.804 | 0.495 |
| South Dakota | – | – | 0.869 | 0.608 |
| Tennessee | 1993 | – | 0.951 | 0.180 |
| Texas | – | – | 0.828 | 0.203 |
| Utah | – | 2022 | 0.304 | 0.265 |
| Vermont | – | – | 0.818 | 0.763 |
| Virginia | 2022 | – | 0.894 | 0.302 |
| Washington | 2017 | 2007, 2011 | 0.808 | 0.660 |
| West Virginia | 1996 | – | 0.745 | 0.252 |
| Wisconsin | – | – | 0.840 | 0.482 |
| Wyoming | 2009 | – | 0.881 | 0.763 |

NOTE: Columns (1) and (2) provide the dates of state-level policy changes, including full-day K mandates and other full-day K-friendly policies. Our policy instruments are constructed based on the earliest date of any policy change in our time period from either column. Column (3) provides the 2019 share of public school kindergartners in full-day K, measured as a 3-year rolling average, and column (4) provides the percentage point change in that value from 1991 to 2019.

SOURCE: Authors' compilation of state-level policy changes and CPS October School Enrollment Supplement 1990–2020.

# Appendix D: Child Outcomes Appendix

While full-day kindergarten expansions in the U.S. have resulted in more in-school instructional time for young children and potentially freed up family resources previously spent on child care, these policy shifts have also led to increased maternal employment and less maternal time with children. These changes could lead to effects on children in opposing directions, but we are limited in our ability to explore the net impact on children's development and learning. In this appendix, we present analysis from approaches similar to those used to estimate impact on parental employment and childcare expenses to explore the impact of greater full-day kindergarten provision on children's subsequent academic outcomes.

## Student Achievement Data

Data on student performance on standardized tests are obtained from the Stanford Education Data Archive (SEDA), a data product of the Educational Opportunity Project at Stanford (Reardon et al., 2023). Each year, states are required to test students in third through eighth grade in both math and reading/language arts. This test information is then sent to the U.S. Department of Education. SEDA uses a restricted version of this data to construct test performance measures that are comparable across states, grade, and time, even though states use different exams and report test results differently. Every two years, the Department of Education's National Center for Education Statistics (NCES) conducts the National Assessment of Educational Progress (NAEP) among fourth and eight graders. This exam is administered to a sample of individuals in each state, making it possible to measure level differences in performance across states. The SEDA data standardizes state level performance measures, relative to the state-level NAEP to construct standardized test scores that are comparable across states and time.

We use the 5.0 version of the SEDA cohort standardized data for the years 2009 through 2019. SEDA also provides data from 2020 and later, but due to the pandemic and methodological differences, we focus on the 2009–2019 data. For our purposes, we rely on student test scores from the third and fifth grade cells. The data correspond to test scores from end-of-year testing, and as such, we merge test scores to state-level full-day kindergarten shares from the October CPS that correspond to the third graders' kindergarten year. We use the SEDA pre-defined score estimates by gender, race/ethnicity, and free and reduced-price lunch receipt, which proxies for low-income status. We conduct analogous analyses when examining student achievement, described in more detail in the next section, with results presented in Tables D1 and D2.

## Student Achievement Analysis

We examine effects on child outcomes along one measurable dimension: children's test scores.[35] Using the state-level, annual standardized performance by grade in the SEDA, we implement an approach analogous to our estimation of labor supply responses in Equation (1). Using the SEDA, we link grade cohorts for each state back to the year that third graders were in kindergarten. We then consider how the full-day kindergarten share of the third grade cohort affects differences in outcomes for third graders, relative to fifth graders. As before, we link test scores (captured in the spring of third grade) to kindergarten enrollment from the corresponding fall. With the age adjustment, we are making a similar comparison of 5- and 6-year-olds to older children (likely 7- and 8-year-olds) as we made when looking at parental employment outcomes in the ASEC. We estimate the effect of the state-level full-day kindergarten share on standardized test scores, as follows:

$$\textit{Std. Score}_{sgt} = \beta_1 \textit{Share Younger Grade in Full-day}_{st} * (\textit{In Younger Grade}_g) + \delta_s * (\textit{In Younger Grade}_g) + \phi_{st} + \varepsilon_{sgt} \quad (1)$$

where *Std. Score* is the standardized test score (either math or reading/language arts) in state $s$ for grade $g$ in year $t$. Scores are standardized to be comparable across states. The coefficient $\beta_1$ represents the effect of a one unit increase in the full-day kindergarten share for the younger of the two grades on test scores for the younger grade. We also control for an indicator for being in the younger grade to capture level differences in performance across grades. State-by-year fixed effects are included to make this a comparison between students in the younger grade and the older grade in the same state and year. State-by-grade fixed effects are included to allow for the average difference between grades to be state-specific. Standard errors are clustered at the state-level. Throughout, we restrict the sample to third and fifth graders. As such, we are examining how the full-day kindergarten share of the third graders is related to test scores for third graders relative to its relation to test scores for fifth graders in the same state at the same time. As in equation (1), we include older children to account for differential trends across place that could affect the outcome. These results are provided in Tables D1 and D2 for the overall average, as well as for student subgroups by race/ethnicity, sex, and poverty status.

[35] Test scores are just one dimension of children's outcomes. It would also be informative to examine children's long-run outcomes, such as high school graduation. However, our identification strategy and variation are not suited to exploring these long-run outcomes for several reasons. First, our strategy uses a triple-difference to compare contemporaneous outcomes for mothers with kindergarten-aged children and mothers with older children to account for local conditions that might be correlated with kindergarten expansions. The same contemporaneous triple-difference approach is not possible when examining a terminal outcome, such as high school graduation. Second, because of cross-state mobility, we can only assign individuals to their kindergarten treatment level with measurement error, unless we are able to observe their location in kindergarten, in a panel. The further in time from kindergarten, the more measurement error will be introduced.

These estimates imply that moving from half-day kindergarten to full-day kindergarten produces no change in later math test scores, but a 0.035 standard deviation increase in third grade reading test scores relative to students two grade levels older. Boosts in students' reading/language arts achievement are concentrated among Hispanic students, boys, and disadvantaged students. We also estimate the analogous policy instrument specification, in which we predict the share with adoption of a full-day friendly policy, in the bottom panel of each table. With this approach, we document a similar pattern of effects but the effect sizes are larger with much larger standard errors. None of the IV results are significant at conventional levels. In sum, findings from both specifications suggest that some groups experience test score gains (particularly in reading/language arts), but perhaps more importantly, that any negative effects on child performance from the decrease in maternal time investment is more than offset in the short run by the gains associated with increased in-school time investments.

We also estimate the stacked panel event study analogous to the maternal labor attachment analysis for test scores in Figure D1. Here, the outcome is the difference in standardized test scores between the third and fifth grade cohorts (to capture the third difference). We see imprecise and mostly flat pre-trends, consistent with the parallel trends assumption, followed by slightly elevated but still imprecisely estimated coefficients in the period following the adoption of a state-level full-day kindergarten policy. These patterns align with the previous results finding no evidence of net losses and, potentially, imprecisely estimated gains.

Table D1: Impact of Full-Day Kindergarten Share on Student Math Test Scores

| | Math | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | Asian | Black | Hispanic | Female | Male | Disadvantaged |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | OLS: Third Grade vs. Fifth Grade | | | | | | |
| Share Younger Grade Full-day Kindergarten | -0.002 | 0.002 | 0.084* | 0.027 | 0.004 | 0.008 | 0.015 |
| *In Younger Grade | (0.027) | (0.066) | (0.047) | (0.041) | (0.031) | (0.029) | (0.035) |
| Dependent Mean | 0.00 | 0.52 | -0.52 | -0.30 | -0.00 | 0.01 | -0.33 |
| Observations | 998 | 978 | 976 | 978 | 986 | 988 | 994 |
| | IV: Third Grade vs. Fifth Grade | | | | | | |
| Share Younger Grade Full-day Kindergarten | 0.070 | 0.213 | 0.107 | -0.047 | -0.035 | 0.173 | 0.014 |
| *In Younger Grade | (0.092) | (0.194) | (0.169) | (0.183) | (0.107) | (0.108) | (0.087) |
| Dependent Mean | 0.00 | 0.52 | -0.52 | -0.30 | -0.00 | 0.01 | -0.33 |
| Observations | 998 | 978 | 976 | 978 | 986 | 988 | 994 |

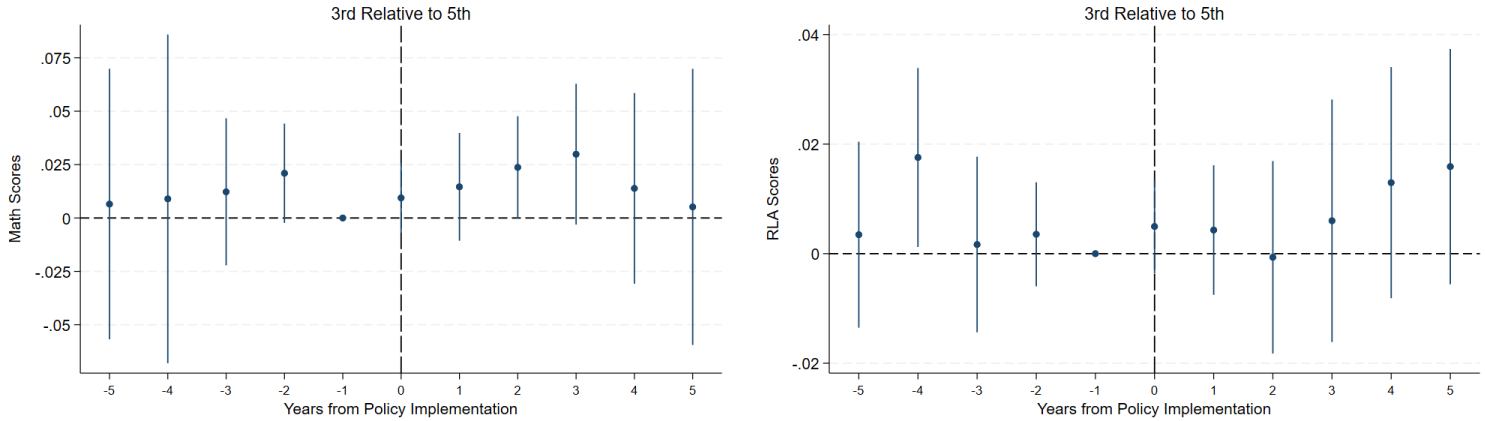
NOTE: Sample restricted to third and fifth grade-level observations. The full-day kindergarten enrollment share (3-year rolling average) from four years prior is used because enrollment is observed in October while test scores are measured in the spring of the corresponding school year. The first stage effect of *full-day friendly policy∗In Younger Grade* is 0.132 with a t-statistic of 3.01. State-by-year fixed effects are included. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table D2: Impact of Full-Day Kindergarten Share on Student Reading and Language Arts Test Scores

| | ReadingLanguage Arts | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | Asian | Black | Hispanic | Female | Male | Disadvantaged |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | OLS: Third Grade vs. Fifth Grade | | | | | | |
| Share Younger Grade Full-day Kindergarten | 0.035** | -0.049 | 0.092 | 0.101** | -0.026 | 0.103*** | 0.057** |
| *In Younger Grade | (0.016) | (0.061) | (0.055) | (0.046) | (0.023) | (0.022) | (0.026) |
| Dependent Mean | 0.02 | 0.36 | -0.43 | -0.30 | 0.14 | -0.09 | -0.32 |
| Observations | 994 | 924 | 948 | 966 | 984 | 984 | 990 |
| | IV: Third Grade vs. Fifth Grade | | | | | | |
| Share Younger Grade Full-day Kindergarten | 0.051 | 0.088 | 0.116 | 0.414 | -0.051 | 0.158 | 0.008 |
| *In Younger Grade | (0.046) | (0.199) | (0.335) | (0.290) | (0.086) | (0.103) | (0.045) |
| Dependent Mean | 0.02 | 0.36 | -0.43 | -0.30 | 0.14 | -0.09 | -0.32 |
| Observations | 994 | 924 | 948 | 966 | 984 | 984 | 990 |

NOTE: Sample restricted to third and fifth grade-level observations. The full-day kindergarten enrollment share (3-year rolling average) from four years prior is used because enrollment is observed in October while test scores are measured in the spring of the corresponding school year. The first stage effect of *full-day friendly policy∗In Younger Grade* is 0.133 with a t-statistic of 3.07. State-by-year fixed effects are included. State fixed effects are also interacted with the indicator for having a child ages 5 or 6. Standard errors are clustered at the state level to account for potential correlation between the error terms within the state. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Figure D1: Event Study Impact of Full-Day Kindergarten Share on Math and Reading Test Scores




NOTE: First, state-level test scores by grade are lagged three years, so that the test scores for the third-grade cohort correspond to policy changes affecting the kindergarten year. For each state that enacts a full-day friendly policy between 2006 and 2016 (to match the years third-grade data are available, 2009-2019), a separate event study panel is created that includes the enacting state and all other states that never enacted a full-day friendly policy. The year the policy is implemented is year $t = 0$ for the enacting state and for all other states that never enact a full-day friendly policy. These states provide a counterfactual for the "treated" state that implemented the policy. The event study panel for each of these states is then stacked, so that a state-year observation for a state that did not implement a full-day friendly policy will appear multiple times. The difference in standardized test scores (either math or reading, language arts) between the third-grade cohort and fifth-grade cohort is then regressed on the sample between 5 years prior and 5 years post policy implementation, including event time dummies, interacted with treatment dummies, with state, year, and panel-by-year fixed effects. The panel-by-year fixed effects makes this a comparison between the treatment state and the counterfactual states in the same panel over time. The outcome represents the difference in test scores for the focal treated cohorts (third grade) relative to an older cohort (fifth grade). Standard errors are clustered at the state level with 95 percent confidence intervals plotted.

SOURCE: Authors' calculations from the SEDA 2009-2019.