

“Preschool for All”? Enrollment and Maternal Labor Supply Implications of a Bilingual Preschool Policy *

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Abstract

Previous research support the effectiveness of preschool in various contexts, yet there is limited evidence whether universal-type preschool policies induce changes in enrollment. While certain states have enacted universal preschool policies, some have also considered bilingual preschool mandates, either as a supplementary or stand-alone policy, requiring schools to open up bilingual classrooms for children from non-English speaking families. The question of whether bilingual preschool policies can induce enrollment and close achievement gaps between English learners and English speakers is particular important today for urban cities and states with large immigrant populations. In this study, I exploit exogenous variation from the first bilingual prekindergarten mandate in Illinois to estimate the causal effects on preschool enrollment and maternal labor supply of recently immigrated and Hispanic families. Utilizing a difference-in-differences strategy, estimates suggest significant effects on preschool enrollment between 18-20 percent and no effects of increasing maternal labor supply in Illinois. Estimates are robust to various specifications, control groups, and timeframes. I use the analysis to further discuss whether universal preschool policies are designed sufficiently for access and inclusion of various student types, and contribute to our understanding on the effectiveness of using child care subsidies to increase welfare of low-income families.

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I Introduction and Motivation

This paper studies the effects of a bilingual preschool mandate on preschool enrollment and maternal labor supply of non-White Hispanic and immigrant families. Using nationally-representative data sources, I provide evidence on the implications of state-mandated public bilingual classrooms for English language learners (ELLs). The question of whether such a mandate could increase enrollment into preschool for children of non-English speaking families and induce their mothers to enter the workforce is of immense importance for policy due to the substantial achievement gap between English speakers and English language learners, the growing evidence of academic and social benefits of preschool, and the potential for mothers to increase the welfare of low-income families by increasing the amount time they are able to be in the labor force. Beyond the immediate policy relevance of this question, this paper also contributes to the understanding of racial heterogeneity in maternal labor supply effects and their choices between consumption and leisure in a traditional two-good model.

A recent push for expansion of universal preschool and prekindergarten across the United States has been driven by child advocates' belief that voluntary preschool for all children would grant more children the opportunity to derive positive academic and social outcomes often associated with early childhood education. Research has documented moderate-to-large impacts of preschool/pre-K on short- and medium-term cognitive outcomes, such as language, literacy, numeracy and mathematics skills (Weiland & Yoshikawa, 2013; Gormley, Gayer, Phillips, & Dawson, 2005; Hustedt, Barnett, Jung, & Thomas, 2007; Wong, Cook, Barnett, & Jung, 2008) and mixed to small impacts on socioemotional skills such as attentiveness (Gormley, Phillips, Newmark, Perper, & Adelstein, 2011; Magnuson, Ruhm, & Waldfogel, 2007). Politically, federal and local governments have seen increased bipartisan involvement in pushing for early childhood education. Both the Obama administration, who had proposed instruments to establish high-quality early learning centers, and the Trump ad-

ministration, which increased block grant budgets for early childhood, have made compelling steps towards broader early childhood offerings.

Yet some have questioned the sufficiency of universal preschool or pre-K policies, as universal access to preschool does not necessarily equate to access for students of all types. In particular, questions surround access, equity, and quality for families with limited English proficiency. Between 2005 and 2009, a precipitous and unexplained drop occurred in Latino children's preschool enrollment in the United States (Fuller & Kim, 2011). While there is little evidence of why the decrease occurred, some potential explanations include a declining political environment for immigrants, particularly along the Mexican border, and dissatisfaction in the quality of preschool programs.

Since the turn of the century, the issue of increasing or stagnant achievement gaps between ELL and non-ELL students has arisen through both mainstream and academic forums, from newspaper and advocate organization articles (Ang, 2014), academic publications (e.g., Rardon & Galindo, 2009), to influential lawsuits (Gandara, Rumberger, Maxwell-Jolly, & Callahan, 2003). These add growing evidence of substantial achievement gaps and inequitable distribution of resources between English-speaking and ELL students. For example, in California, the class-action lawsuit *Williams vs. State of California* in 2000 alleged that the state allocated funding based on both wealth and language status. The lawsuit resulted in a major settlement requiring the state to allocate nearly a billion dollars in additional funding for facility repairs and resources to schools in the two lowest deciles of achievement (California Department of Education, 2004). Further, the think tank California Common Sense noted that the descriptive achievement gap in the reading levels of ELL and non-ELL students grew 23% between 2003 and 2013, which they partially attribute to the quickly growing population of immigrants in the state (Ang, 2014). As a result of such debates, some have advocated for expansion of requiring districts to open up bilingual preschool classrooms. According to the National Institute for Early Education Research (2018), only five states were deemed as having the necessary supports for early childhood bilingual students to succeed

and a majority of states do not even collect data on children's home language.

In this study, I begin the first examination into the enrollment and maternal labor supply effects of the first statewide bilingual preschool requirement in Illinois. Initially passed by the Illinois state legislature to be effective as of January 1, 2009, the original law extended the designation of "children of limited English-speaking ability" in public schools to include 3- and 4-year olds (Zehr, 2010). This law change then prompted the Illinois state board of education to adopt regulations requiring all publicly-funded preschool programs to (a) identify children with limited English proficiency and (b) to provide transitional bilingual classrooms for those students, beginning in September 2010. The requirement covered all 585 preschool programs in publicly funded schools and districts with enrollment of twenty or more ELLs who speak the same language in preschool. The mandate itself was subsequent to a universal preschool policy that began in 2007.

I examine the policy's effects on two types of outcomes: (1) enrollment by 3- and 4-year olds for the policy's target population, and (2) maternal labor supply of those same students in Illinois. My identification strategy exploits the variation created by the policy. Specifically, I utilize a difference-in-differences empirical strategy in two manners: (1) by time and by state using bordering contiguous states as the control, and (2) by time and treatment population within the state, where I use target populations as the treated subjects and all others in the state as control. I find evidence of higher enrollment (between 18-20%) for target students and none-to-small effects on maternal labor supply.

This research provides both acholarly and policy implications. First, it is the first causal analysis of the effects of a statewide bilingual preschool mandate. Second, it adds to the literature to give a more nuanced understanding of how universal- or supplementary-type preschool policies can affect enrollment and maternal labor supply while simultaneously providing another context in which we can test a simple labor supply model and the tradeoffs between consumption and leisure. Third, in a context where universal (though not well-funded) preschool policy was in place, there were still multiple populations that chose not to

send their children to preschool. This gives policy-makers a sense of how they can improve upon current designs of universal preschool policies.

II Relevant Literature

Effects of Preschool and Pre-K in General

Since the 1960s, much experimental research has been completed on the effects of preschool/pre-K on various outcomes. The original and most well known project to date has been the High/Scope Perry Preschool Project, in which 3- and 4-year old African-American subjects were randomly divided into treatment group receiving high-quality preschool and control group receiving no preschool program between 1962 to 1967. Educational researchers have studied the short-, medium-, and long-term effects of the project and found that African Americans treatment participants born into poverty and labeled as “high-risk” of failing in school were more likely to be committed to school, have better academic outcomes in fourth grade, have better relationships with friends, have better marriage outcomes for girls, graduate high school, have higher earnings, to hold a job, and committed fewer crimes relative to their peers in the control group (Weikart, Bond, & McNeil, 1978; Schweinhart et al., 1993; Schweinhart et al., 2005). These studies, along with most studies of its kind, suffered greatly from limited samples sizes and attrition. However, Barnett's (1995) literature review of thirty-six such studies conclude that large-scale public early childhood care and education programs can induce positive long-term academic and social outcomes for disadvantaged children.

Recent quasi-experimental research supports the general findings of the experiments. Studies generally rely on the variation from discontinuities to identify causal estimates. For example, Weiland and Yoshikawa (2013) exploited age discontinuities to estimate the short-term effects of a quality pre-K program in Boston on cognitive and socioemotional outcomes. They study differences between students just above and below the birthdate

cutoff and as a result were close in age but only those above the cutoff had experienced the program. Their analysis finds mostly moderate-to-large impacts on mathematics, literacy, language outcomes, and mixed-to-small effects on socioemotional outcomes, such as attention shifting, emotional recognition, and impulse control. In their subgroup analysis, the most statistically significant and robust results were for Hispanic students and free-or-reduced-priced lunch students. In a study using variation created by funding discontinuities, Ludwig and Miller (2005) identified substantial and lasting effects of the Head Start program in future mortality rates and educational attainment.

Similar to the aforementioned literature, various other studies have also found moderate-to-large impacts of pre-K on short- and medium-term cognitive outcomes, such as language, literacy, numeracy and mathematics skills (Gormley, Gayer, Phillips, & Dawson, 2005; Hustedt, Barnett, Jung, & Thomas, 2007; Wong, Cook, Barnett, & Jung, 2008) and mixed to small impacts on socioemotional skills (Gormley, Phillips, Newmark, Perper, & Adelstein, 2011; Magnuson, Ruhm, Waldfogel, 2007). A growing body of literature has provided evidence that Hispanic students benefit more than most other racial and ethnic groups (Currie and Thomas, 1995, 1999; Garces, Thomas, & Currie, 2002; Gormley, 2002; Loeb et al., 2005). These examples represent only a small subset of the early childhood literature finding wide-ranging causal effects from such programs in various contexts and for specific populations.

Effects of Bilingual Education

Despite the substantial amount of research done on the positive effects of pre-K, it is not without question that the effects generalize to an immigrant or bilingual population. Recent causal literature on bilingual education has been limited in quantity and rigor. Rossell and Baker (1996), a highly cited literature review on bilingual education, reviewed over 300 evaluations, only 72 of which were considered quantitatively rigorous at the time¹. Among

¹The authors described rigorous studies as those that included a treatment and control group, and where participants were either randomly assigned or pre-treatment differences were at a minimum controlled for when not randomly assigned.

those considered rigorous, they find limited effectiveness of transitional bilingual education. A follow-up meta-analysis of the evaluations used in Rossell and Baker's literature review, though, analyzed eleven highly quantitative projects and noted only small effects in language outcomes, primarily stemming from all-English approaches to bilingual education (Greene, 1997). Further, Joseph Robinson (2011) suggested that in cases where OLS methods, a dominant method in many of those supposedly rigorous studies, are used to evaluate the effects of bilingual education, belief in such estimates might be naive. He develops a regression discontinuity method combined with an instrumental variable and shows that reclassification of a bilingual student to full-English classes could negatively affect English assessment outcomes, despite positive OLS estimates. While the history of bilingual education has been as contentious and enduring as preschooling, the limited causal evidence has been driven by the ethical and legal implications of assigning ELL students to non-ELL programs. Thus, no study has yet successfully determined the true causal effects of ELL programs relative to non-ELL programs for ELL students.

Effects of Universal Preschool and Pre-K Policies

The novelty of universal-type policies in preschool also means limited causal evidence on how these policies affect outcomes. Maria Fitzpatrick has multiple studies on this topic. Her 2008(a) study uses a difference-in-differences framework to find that a universal pre-K policy in Georgia increases fourth grade reading and math scores for disadvantaged children residing in rural and small town contexts. Since her estimates are intent-to-treat estimates, she notes that much of the results are driven by higher enrollment gains in rural areas relative to urban areas. Her second study in 2008(b) examined two universal pre-K programs in Georgia and Oklahoma using restricted-access Census data. Her identification strategy focused on regression discontinuities in birthdate cutoffs and she finds that universal pre-K availability increases preschool enrollment by about 14-17%, with mixed-to-no effects on maternal labor supply. Gormley et al. (2005) also used birthdate discontinuities in

Oklahoma to find improvements of enrollment in specific cognitive assessment scores, such as spelling and word identification. In another study, William Gormley (2008) specifically examined the effects of Oklahoma's universal pre-K program on Hispanic students in Tulsa, Oklahoma. Using a regression discontinuity design, he found Hispanic students benefited greatly academically, experiencing test score gains in reading, writing, and mathematics. These studies give policymakers limited external validity, since there has only been few contexts studied.

Effects of Pre-K on Maternal Labor Choice

In order to ponder maternal decisions, a traditional two-good framework modeling labor supply choices as a tradeoff between consumption and leisure may be applied. In this model, mothers derive utility from either consumption purchased with labor income or leisure itself (Browning, Chiappori, & Weiss, 2011). Leisure here can implicitly include taking care of children, whereas labor income would mean that child-care be purchased. Child-care availability at a partial- or fully-subsidized rate, such as the public education context we are examining, should provide incentive for mothers to both enroll their child in pre-K and to either enter work or increase the number of hours they work. The number of hours worked, however, may be limited to the time a mother's child is in care. On the other hand, the child-care subsidy may also be perceived as an income subsidy for mothers, which in such a case would exert downward pressure on the amount of hours worked in the labor market. Previous works have shown that theoretical predictions from such models on working women typically find that the effects of child care subsidies are ambiguous (e.g., Gelbach, 2002).

In limited contexts, there is evidence that preschool enrollment have a positive effect on maternal labor supply. Jonah Gelbach (2002) uses a quarter of birth instrument to estimate that free public schooling for both single and married mothers whose youngest child is five years of age increased labor supply significantly. For single mothers with an additional younger child under five, though, no significant effects were found. Studies have

also consistently found that married mothers have significant negative labor-supply response to child care prices (e.g., Robins & Blau, 1988; Connelly, 1992; Kimmel & Kniesner, 1998). Among single mother, elasticities found have typically been mixed (Kimmel & Kniesner, 1998).

Contribution to Literature

This study contributes to this literature in two significant ways. The first contribution is that it is the first to study a statewide bilingual preschool mandate. This is important for a few reasons. First, given that households with young non-English speakers should be greatly different from those who do speak the language and the substantial educational achievement gap between ELLs and non-ELLs, it is paramount to understand the nuanced differences between presenting opportunities to parents for preschool versus presenting opportunities to parents for bilingual preschool. Additionally, despite Illinois already having a “universal” preschool policy in place, such mandates may promote the notion that such generalized policies aren’t inclusive enough to give sufficient opportunities to a universe of citizens. We must then figure out whether there is a genuine and substantial demand for bilingual preschool beyond universal preschool. The second contribution is that this study presents additional evidence of the effects of increased preschool availability in a context not studied before. Much of the previous evidence on universal Pre-K policies used policies from Georgia and Oklahoma, whereas studying Illinois - given the presence of Chicago - includes a significant urban presence. Third, it also presents additional evidence on maternal labor market effects for Hispanic mothers resulting from a form of free child care.

III The Context: Bilingual Preschool in Illinois

In 1985, Illinois legislation established the availability of preschool for eligible children between ages 3 to 5 considered at-risk of academic failure in the future. The legislation

greatly increased funding for preschool programming and professional development. Over the decades that followed, funds were used to expand child care, Head Start programs, and community-based preschool organizations. In 2006, the Preschool for All program was formally introduced with the goal of giving every 3- and 4-year olds (and 5-year olds whose date of birth prevented them from kindergarten eligibility) in the state an opportunity to enroll in high-quality public preschool. Thus, beginning in Fall 2007, Illinois officially enacted a universal preschool policy mandating that public preschool be offered to all parents with three- and four-year olds in the state. One of the primary goals of this policy was to close persistent achievement gaps between those who could and those who could not afford preschool for their children. Soon after, policymakers and districts arrive at the realization that the policy may still perpetuate the vast achievement gaps that have persisted between English language learners (ELLs) and English speakers for many years². Anecdotes of how immigrants and limited English speaking parents either found that programs were not well-equipped to teach their children or did not send their child to preschool at all perpetuated, especially as bilingual preschool were rarely offered in public schools (Severns, 2012). The importance of closing this gap in Illinois is due to the quickly growing immigrant population; there was an increase of 200,000 foreign-born in Illinois's population during the 2000s, with many immigrant families moving to the suburbs where school districts were not accustomed to serving an immigrant population (Hall & Lubotsky, 2011; Severns, 2012).

Therefore, in 2009 the state legislature established first statewide bilingual preschool requirement in Illinois. The law, to be effective as of January 1, 2009, extended the designation of “children of limited English-speaking ability” in public schools to include all 3- and 4-year olds. This law change then prompted the Illinois state board of education to adopt regulations requiring all publicly-funded preschool programs to identify children with limited

²The language achievement gap is vast in Illinois. 97% of fourth graders with limited English proficiency (LEP) in 2008-09 scored in the two lowest levels of the statewide standardized assessment, compared to 58% of White students. For the senior class of 2008-09, 63% of students with limited English proficiency and 28% of migrant students graduated from high school, compared to 93% White/Asians, 77% of Blacks/Hispanics, and even 78% of special education students (Illinois Board of Education, 2009).

English proficiency and to provide transitional bilingual classrooms for those students, beginning in September 2010 in time for the start of the 2010-11 school year. The requirement covered all 585 preschool programs in publicly funded schools and districts with enrollment of twenty or more ELLs who speak the same language in preschool (Zehr, 2010).

There were three major requirements of the mandate. The first major requirement, under 23 Illinois Administrative Code Part 228 Traditional Bilingual Education, is for public districts to identify children who are ELLs. This process involve the administration of a home language survey to the parents of children new to the district and conducting an English language proficiency screening process for children whose home language or language background is not English. Those who do not demonstrate English language proficiency during the screening process would be considered ELLs. The second major requirement of the mandate is for all 585 state-funded preschool programs to provide bilingual classrooms for those students classified as ELL students if a minimum of twenty such students enrolled speaking the same language. The third major requirement is that by the beginning of the school year in 2014, any teacher in bilingual preschool classrooms must be certified in bilingual instruction or English as a second language, in addition to being a certified in early childhood education.

Given the implied costs and scope of the mandate, the implementation of the mandate in September 2010 was not without its hurdles. As with many others matters of schooling, mandated increases in resource requirements often compel districts to make difficult decisions on allocation of limited resources. One limitation of the mandate is that it arrived without additional funding, forcing districts to comply using “dwindling reserves and funding delays,” much of it a result of the Great Recession (Malone, 2010). Another limitation is that districts must scramble to hire qualified bilingual aides, who were used as an assistant in a majority of classrooms due to the lacking supply of fully certified bilingual teachers in early childhood education. Additionally, the supply of teachers with both certifications is highly limited as the requirements to obtain both are difficult to reach. An analysis of the internal validity

implications of these issues is discussed later.

Descriptively, there was an increase in the raw numbers of ELL students enrolling into preschool in Illinois at the time of implementation. Figure 1, based on numbers from the Illinois Department of Education, shows more than a doubling in both enrollments - from 7,260 students during the 2009-10 school year to 17,564 during the 2010-11 school year - and the proportion of preschool ELL students relative to the total ELL students in the state at the time of the mandate. While we can hypothesize that the policy may have increased enrollment, we cannot determine a causal interpretation without experimental or quasi-experimental methods. For example, districts may not have tested for English skills or identified language needs since it was not a requirement at the time. Therefore, the primary goal of this paper is to develop an estimate of enrollment increase with causal interpretation due to the mandate itself.

IV Data and Descriptive Statistics

To estimate the initial effects of bilingual PK in Illinois, I use publically available data from the 2008-2013 American Community Survey (ACS)³. The ACS is an annual survey given by the US Census Bureau and provides a cross-sectional representative snapshot of US inhabitants each year. The survey solicits information on subjects such as household demographics and relationships, income and public benefits, education, disabilities, and employment information.

The mandate is specifically targeted at families with young English language learners in Illinois, of which well over 80% are from Spanish-speaking families. Given this, I generate a single analytical sample by restricting the dataset to three targeted groups of interest: non-white Hispanics, recent immigrant families who immigrated within 5 years of their child becoming eligible for preschool, and children of families where both parents are of limited

³ACS datasets are publically available and can be obtained from IPUMS for research purposes. Here, I used the 1% nationally-representative samples from 2008-2013 (See Ruggles et al., 2015).

English speaking ability⁴. The latter two categories captures families who generally have limited English proficiency, and I include in the sample only *non-white* Hispanic families rather than white Hispanic families (which makes up 57% of Hispanics in Illinois) primarily due to large differences in demographics. White Hispanic mothers with preschool age children, according to the ACS data, speaks English either “well” or “very well” at a 88% rate and have mean household incomes 340% of the poverty level status. Only 47% of non-white Hispanic mothers speaks English well, with household incomes only 183% of the poverty level on average. It is clear that most white Hispanic families, at least demographically, are not a part of the main target population for this policy in Illinois. For the most part, white Hispanic families in Illinois should have been able to take advantage of the initial universal preschool policy⁵.

I also limit my main analysis to three years before and after the implementation of the policy (2008-10 before, 2011-2013 after). As a check of robustness, I test estimates when timeframes are varied. In analyses of enrollment, I use three- and four-year old children⁶ from the three treated groups, as these are the primary interest groups of the mandate. The outcome variables of interest are enrollment in public preschool for three- and four-year olds, and probability of being in the labor force, usual hours worked last week, and wage and salary income for mothers.

Table 1 shows descriptive statistics for preschool age children and their mothers during the three years before and after the policy change. Observably, there is little different about children and mothers before and after the policy change. Admittedly, there could be unobservable differences between parents, especially if migration patterns become unique around the timeframe of the mandate. Demographically, though, there are no substantial differences in children or mothers’ age, mothers’ education, or ethnic/racial makeup. Mothers, on aver-

⁴In the sample, a large majority of non-English speaking families are from non-White Hispanic families. Because of that, around 95% of the samples are composed of non-English speaking Hispanic households.

⁵This can be tested by examining how non-White and White Hispanics compare when it comes to the effects of the initial 2007 universal preschool policy. It will be a part of future drafts.

⁶While officially, five-year olds are also eligible to be in a preschool program, most are eligible for kindergarten based on birth date cutoffs. For the most part, five-year olds in preschool are those who started when they were four. Therefore, the effects for five-year olds may not be seen in the estimates and may attenuate estimates so I leave them out of the analyses.

age, are slightly older and more educated, after the mandate, but these differences are not significant. In terms of outcome variables, there are little descriptive differences in maternal labor supply outcomes and several percentage point increase in preschool attendance for children of preschool age.

V Empirical Strategy

I exploit the sudden variation in preschool enrollment induced by the bilingual mandate to use a difference-in-differences (DD) strategy in order to credibly estimate the effects of the mandate on preschool enrollment and maternal labor supply of the population of interest. The primary strength of a difference-in-differences approach is that it differences out biases in comparisons between treatment and control that may be the result of both permanent differences between groups and trends over time. In this manner, the result can give us credible estimates of treatment effects. To study the said outcomes, I first estimate the following regression:

$$Y_{ist} = \beta_0 + \beta_1 * Treat_{is} + \beta_2 * Post_{it} + \beta_3 * (Treat_{is} * Post_{it}) + X'_{ist} + \varepsilon_{ist},$$

where Y_{ist} is the outcome of interest for child or mother i living in state s during time t . $Treat_{is}$ is a dummy referring to the treatment or control state, $Post_{it}$ is a dummy referring to the before and after of policy implementation, and X'_{ist} is a vector of covariates. The coefficient of interest will be β_3 , which interacts the treatment dummy with the time dummy and gives us an average treatment effect of the bilingual preschool policy. This specification restricts the sample to only the three group of interest and uses neighboring contiguous⁷ states as a control group. I use neighboring states as a control group as they should typically be a representative counterfactual due to proximity, similarity in culture and values, and face similar natural, economic, and social issues.

⁷The counterfactual states used in this specification are Indiana, Iowa, Kentucky, Missouri, and Wisconsin.

To test for robustness and provide more comfort of estimates, I also estimate the specification using non-treatment group individuals (e.g., Caucasians and African-Americans who speak English) within the state of Illinois, such that:

$$Y_{igt} = \beta_0 + \beta_1 * Treat_{ig} + \beta_2 * Post_{gt} + \beta_3 * (Treat_{ig} * Post_{gt}) + X'_{igt} + \varepsilon_{ist},$$

where g denotes the target group as 1 and the counterfactual group as 0. The use of counterparts within the state as the counterfactual is slightly less intuitive, as White families and target families are different in observables and unobservables. However, they should hypothetically experience similar laws and events and any variation caused by these similarities would be differenced out. The two specifications presented have different interpretations: The first compares the treatment group to their similar peers in neighboring states; the second compares the treatment group to the to their counterparts and neighbors within the state. However, the combination of studying across-state peers and within-state counterparts provides for both a robustness test and additional believability of results. As a second test of robustness, I also utilize propensity score matching to select states that are similar along education and economic covariates, and I find that estimates are highly robust regardless of method used⁸.

VI Results

Effects on Preschool Enrollment

In order to support the reasoning behind the bilingual policy, effects of the mandate on preschool enrollment for its targeted populations should be substantial. Without its intended primary effects of increasing enrollment, the mandate would have no alternative mechanism of improving academic achievement for English language learners. The estimation results of the bilingual mandate on enrollment are reported in Table 2. Three specifications are

⁸The results for the propensity-matched states are not shown in this paper, but are available upon request.

reported here: (1) a basic DD without covariates, (2) a DD specification with background and socioeconomic status as covariates, and (3) the same specification with state fixed effects. Additionally, the results are delineated by control strategy - either using across- or within-state control groups.

The effects of the bilingual preschool mandate on enrollment are estimated to be an increase of approximately seven to eight percentage points. Given that pre-mandate enrollment levels for these groups were between 38 to 41 percent, the estimations approximately equates to an 18 to 20 percent increase in preschool enrollment for the groups of interest. My estimates are robust to additional controls and control strategy. In the across-state comparison of targeted populations and its similar counterfactual group, standard errors are calculated by aggregating year-group cell means.

A significant threat to internal validity is the presence of a previous universal preschool policy enacted for the Fall 2007 school year. The bilingual preschool policy of interest went into effect three years later during the Fall of 2010. Given that the data runs from 2008-2013, it is possible that such a policy may affect estimates as one would expect. For example, if the additional preschool availability from 2007 induces a Hispanic parent to send their first child to preschool and finds a good experience, they are more likely to also send a second child to preschool when the second child is of age three years later. This enrollment effect would be derived from the first policy rather than the latter. However, the consistent estimates by varying timeframe thresholds should also provide confidence that the estimates are not significantly biased by the presence of the first policy. One way to test both the robustness and whether there are larger coefficients around time of the mandate is to restrict samples by year. Table 3 shows the results of such an analysis. Here, I find that while estimates closer to the mandate⁹ are higher, it is not substantially higher. The consistent estimates between 7.6 to 9.2 percentage points increase from the furthest to the closest margin around

⁹The estimates using ACS data is limited to the 2010-2011 timeframe, but the 2010 data includes a portion of surveys completed during both the prior and after mandate implementation in September 2010, which may cause attenuation of estimates. As a secondary test, I also ran DD estimates by restricting data to just 2009 and 2010 ACS data. Once again, the estimate is similarly in the 8% range.

the mandate gives us an indication that lasting effects of the initial universal preschool policy should be minimal.

While I have confidence that the enrollment increase is credible according the Illinois context three years after universal preschool, it does not preclude the possibility that effects of the bilingual mandate would have been even larger in alternative contexts where universal preschool is not available. This notion limits external validity of this study.

Overall, estimates show a substantial increase in preschool enrollment. However, as Figure 1 had shown, there was an even larger increase in pre-K enrollment of English language learners, from 7,260 to 17,564 (an increase of 142%), between the schools years starting 2009 and 2010. My results does not substantiate such a strong increase in enrollment, requiring that other major factors be at work, such as large increases in immigrant or Hispanic populations in Illinois or vast amounts of students who would not have been assessed to be ELL students prior to the mandate were suddenly being assessed and categorized as such. In the latter case, since my estimates are independent of ELL classification, they would not capture a similar measurement as that of the Illinois Board of Education and so would not be an apples-to-apples comparison.

Effects on Maternal Labor Supply

While the effects of the mandate on enrollment are a direct effect, effects on maternal labor supply tend to be more indirect in the sense that any effects could only be realized if a mother sends their child to preschool and subsequently choose to find or increase labor. An estimation of an effect on maternal labor supply therefore must follow a couple of general assumptions about the average household. First, economists assume that it is the mother, rather than the father or someone else in the family, who assumes primary responsibility as caretaker of their children. This is believable in the Illinois context, where well over 70 percent of households with preschool age children have a woman listed as the household head. Second, we assume that mothers often choose not to work or limit their work to take

care of their child during the early years. Under these assumptions, we may be able to detect effects on maternal labor supply. Since the target population of this policy, demographically, is of lower socioeconomic class than the general population, I would expect that there is a better chance that a public subsidization of their child's education in preschool should lead to an increase in the choice to work over leisure.

The DD estimates for the effects of the mandate on maternal labor supply are shown in Table 4. Three dependent variables are examined here: the probability of being in the labor force, the amount of usual hours worked per week, and wage and income salary. The estimates show that there are little, if any, effects on maternal labor supply. While the estimates show significance for an increase of the mothers' probability of being in the labor force by two percentage points under the first control strategy, it is not robust to neither the second control strategy nor varying time frames (the latter of which is shown in Table 5). Coefficients for usual hours worked per week range from 1.7 to 2.6, though insignificant. Wage income for mothers are also insignificant, with positive coefficients in the hundreds using the first control strategy and negative coefficients in the thousands from the second control strategy.

VII Discussion of Results and Internal Validity

Underlying Assumptions

The two primary assumptions of the DD method are parallel trends and stability of group composition. The first assumption means that, under a counterfactual world in which the treatment condition did not occur, the trend of the treatment group will parallel that of the control group. One indicator of parallel trends is for the treatment and control group to display similar trends in the dependent variable prior to the implementation of the policy.

Figures 2 and 3 shows the annual changes in enrollment percentage rates for the first and second control strategy, respectively. The validity of the DD method is strengthened by the

visually similar enrollment trends prior to the first year of implementation. In the first control strategy, the changes in enrollment rate for the years prior to the mandate's implementation had the same signs for two of three years and were very similar in magnitude for all three year. In the second control strategy, the same can be said of all three years.

The second assumption is that the groups prior to and after the mandate remain unchanged. While I am unable to test the strict compliance of this assumption, I am able to examine the observable characteristics of the treatment and control groups prior to and after implementation. If we find that the treatment group is different under observable dimensions before and after the policy implementation, then internal validity will be limited. As stated prior, Table 1 shows the descriptive statistics of the treatment groups. Here, we find little variation in baseline covariates, and so I would be confident that there were no major changes, at least observably, in demographic makeup of the treatment group before and after the policy change.

Discussion of Enrollment Effects

Due to the strong evidence of effectiveness of early childhood education, one hypothetical method of closing achievement gaps between student groups is to increase availability to preschool. However, universal preschool in itself, while celebrated, often does not go far enough to grant access to its most disadvantaged populations.

In this study, I estimate that the 2010 Illinois bilingual preschool mandate increased preschool enrollment for children of non-White Hispanic, recently immigrated, and limited English-speaking families by 18-20 percent. Given the specific higher need population, it is not surprising to find that it is slightly higher than Fitzpatrick's (2008b) estimates of 14-17 percent for general universal preschool policies in Georgia and Oklahoma. However, two primary reasons may have limited the effectiveness of Illinois's bilingual mandate. First, a universal preschool policy preceded the bilingual mandate in Illinois three years earlier. While the robustness of estimates by various timeframes lend credence to my estimates, it

does not preclude the fact that many of the target families had already been induced to send their child to preschool based on the previous policy. This could have led to higher proportions of enrollment for target groups relative to the proportion that would have occurred under a universal preschool-less world, which would in turn lead to smaller effect estimates in this study.

The second major reason was that there were probably various implementation-based biases, many of which were the subject of media attention in Illinois. There were three primary implementation-related reasons that lead me to believe enrollment estimates could have been biased.

One issue is that the requirement for all districts to determine the language skills of potential preschool students suffers from inconsistency. In particular, districts and programs are given the autonomy to choose a process by which to judge English skills. While the department of education provided a limited amount of guidance, including rules about using multiple measures and minimal ‘English skills’ on which to measure (Illinois Board of Education, 2011), the relatively high level autonomy could restrict the consistent accuracy of language assessments and could assign some limited-English children into full English classrooms and vice-versa. An inconsistent process combined with lax accountability could skew towards Type II error, whereby ELL students are incorrectly classified and assigned to English classrooms. Parents of bilingual children may then unenroll them, thereby negating effects.

A second issue is that the perceived quality of bilingual pre-K in Illinois may hinder enrollments. As previously stated, due to the limited supply of fully-qualified teachers many districts made the choice at time of implementation to add a bilingual aide to classrooms to assist teachers instead. Since this was a publicized issue, it could be seen by parents as poor implementation of bilingual classrooms and as a result forgo enrolling their children.

A third issue is the lack of funding for implementation of the mandate. Districts, especially large urban districts, may be constrained by both limited budgets and high amounts

of bilingual enrollees. Of particular concern is the potential for displacement of English-speaking students. For example, if the mandate led to larger class sizes and lower-quality, middle-class white parents may elect to remove their child from public preschool. This would lead to an overestimation of effects using the second control strategy, as the gap in enrollment between the treatment group and control groups becomes larger than it would be under a scenario where additional resources are devoted to the implementation.

Overall, though, effects of the bilingual mandate on preschool enrollments for non-White Hispanic and other limited English speaking families in the Illinois context provide for strong support for similar policies around the country. The finding that the bilingual mandate have strong effects on enrollment of targeted groups even after the recent introduction of universal preschool implies two important piece of information: (1) universal preschool in itself may not have the fully intended effects on disadvantaged populations and (2) even after the introduction of universal preschool, there can still be improvements made upon it to induce parents to enroll their child into preschool.

Discussion of Maternal Labor Supply Effects

The second finding of this study is that there is little-to-no effects in maternal labor supply. While this is consistent with the findings of Fitzpatrick (2010) in her analysis of general universal preschool policies, my hypothesis would be that more effects should be found in a low-SES population. There are multiple reasons why estimates do not suggest effects for maternal labor supply. One significant concern of estimating the effects of the mandate on maternal labor supply is the timing of the effects. It is possible that, as of the time of the ACS survey, mothers either have not chosen to work yet, they have not yet been able to find a job, or they have not been able to increase their hours worked because of a lack of opportunity. In such cases, the effects of maternal labor supply would not be seen in the data. A second concern is that the literature often does not find effects for mothers who have multiple young children. In my sample restricted to non-White Hispanic, recently

immigrated, and non-English families, 37% of households have multiple children five and under. Given that a substantial amount of mothers may choose to provide childcare rather than work, the effects may again be limited without limiting estimates to households with a single young child. Third, recent literature has pointed to a decline in female labor supply elasticities over time (e.g., Heim, 2007). This, combined with the fact that a majority of mothers in the baseline sample (51%) were already working may mean that mothers who wish to work are already working regardless of whether they send their child to school.

Other Implications: Potential for Displacement and Sorting

In order to develop a more nuanced understanding of the bilingual mandate, we must dive deeper into the specific implication at the school- and student-levels. Many studies have studied the potential for public preschool crowding out existing supply of private provision (e.g., Bassok et al., 2014). Given the specific target of low-SES populations from the bilingual mandate, I would not expect that to be the case. However, one key observation suggests two important areas for further research. Figure 3 - a year-to-year comparison of changes in preschool enrollment between the groups of interest compared to all other demographic groups in Illinois - documents an unusual observation. Prior to implementation, both groups experience similar patterns in annual enrollment levels. Post implementation, a new pattern emerges: The two groups experience opposite trend signs from 2010 to 2013.

This pattern forces us to question whether there is any displacement or sorting effects. In terms of displacement, an explanation of the pattern based on economic supply is possible: Allocation of district resources may be tipped towards either English-limited or English-proficient students on a year-to-year basis, an especially important notion during a time when public advocates and parents may be forcing administrators to find a balance in serving groups equally. Media attention in Illinois lends credence to this possibility: Funding for Illinois preschools peaked in 2009 with \$327 million and dropped considerably in years after. In 2013, the budget was down to \$241 million and funding was used to “target the neediest”

(Neufield, 2014). This would be consistent with a theory of displacement, whereby perceived inequitable allocation of resources pushes one group out. Under scarce and limited resources, improving quality for one group over another can become a zero-sum game.

Alternatively, the chart may also be partially explained by sorting. On the demand side, year-to-year variance in perception of changes in preschool quality could drive swings in enrollment. For example, if White parents were afraid of decreasing quality during the first year of implementation, they may be more inclined to enroll their children in private school, keep them at home, or move them to a low-ELL district. Hispanic parents, though, may send their children given the supposedly expanded resources for bilingual students. During the second year, White parents may have noticed that quality for their group did not decline, and so decide to enroll their children into public preschool. Hispanic and Immigrant parents, on the other hand, may have noticed that resources for bilingual students were insufficient and preschool quality suffered and unenroll their children as a result, or move them to a high-ELL district.

While these hypotheses are intriguing, as knowing the answer can help us design better policy going forward, further analyses of these factors are required to fully determine the unexpected side effects and costs of such a policy.

VIII Conclusions

Given the perceived limitations of universal preschool policies in granting equal access to pre-K for all disadvantaged populations, many states and cities have started turning towards discussions over universal bilingual preschool policies. Large achievement gaps between English language learners and English-proficient students point toward the need for educational reform in closing the gaps. However, are bilingual preschool mandates, which might attract more limited-English families to enroll their children, be a part of the solution?

This study adds two primary contributions to the literature. First, it is the first causal

analysis of the effects of a statewide bilingual preschool mandate. Second, it adds to the literature to give a more nuanced understanding of how universal- or supplementary-type preschool policies can affect enrollment and maternal labor supply. My estimates find significant effects on student enrollment for target populations in Illinois, and little-to-no effects on maternal labor supply. These findings are consistent with the universal preschool findings of Fitzpatrick (2010). They also support the need for similar policies to be assessed based on enrollment and future student outcomes rather than its potential for closing labor market gaps, which seemingly require alternative policies to become a factor in family welfare. More importantly, while there were 18-20 percent increases in enrollment for targeted groups, I conclude that there is still a long way to go in fulfilling Illinois's wish to have 'Preschool for all.' The evidence shows that it is not sufficient to just develop a one-size-fits-all solution to giving all children an opportunity into preschool, but that it is important to develop nuanced opportunities for the many children with specific needs. These may include more than just bilingual students; it can also mean limitations in the system in serving students with special education needs. Finally, for states, cities, and districts, this evidence may add to the notion that there is in fact demand for bilingual preschool classrooms, even in a context where the proportion of non-English speakers is average relative to the rest of the states.

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Pre-K English Language Learner Students Total and Proportion of all ELL Students in IL

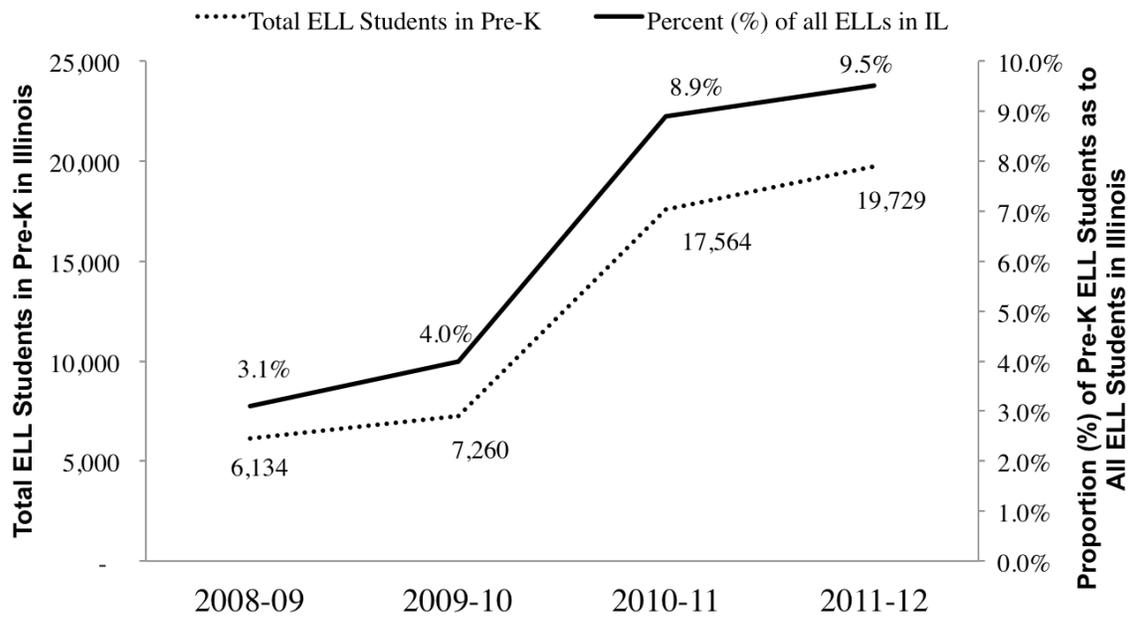


Figure 1: Percentage and Number of English Language Learners in Illinois

This figure shows the amount students identified as ELLs in preschool in Illinois publicly-funded programs from 2008-2011 school years. Percentages are shown as number of ELL preschool students divided by total number of preschool students. Source: Calculated from numbers from Illinois Department of Education.

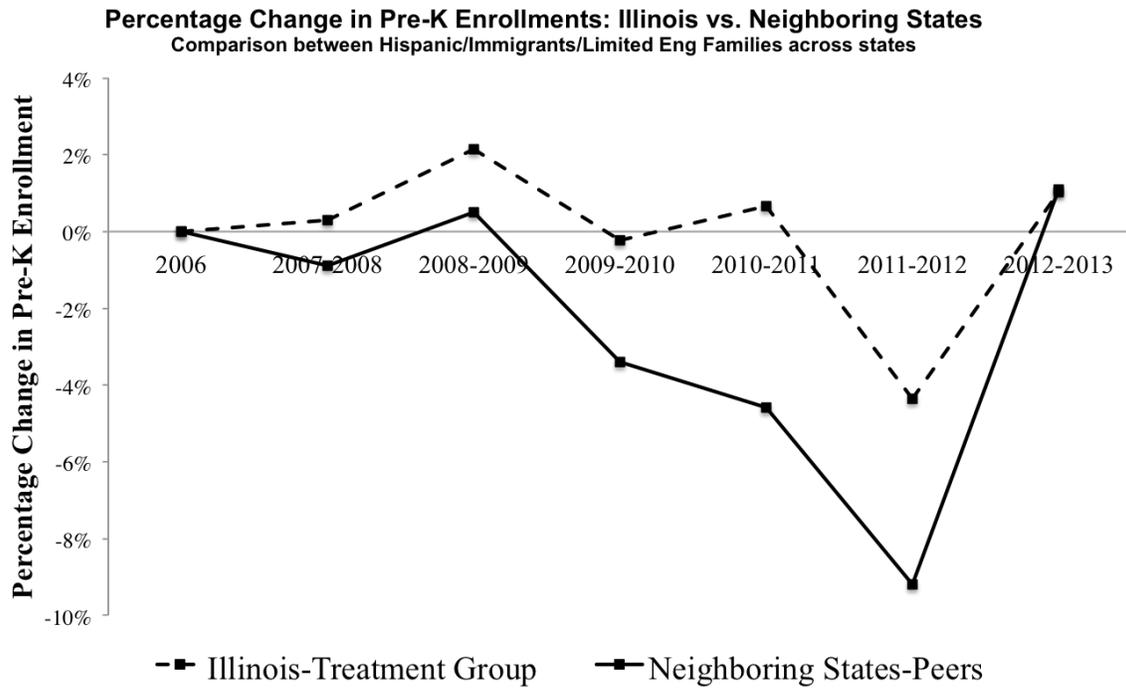


Figure 2: Annual Percentage Changes in Preschool Enrollments of target groups: Illinois vs. Neighboring States

This figure shows the percentage change in preschool student enrollments over time from non-white Hispanic, immigrant, and limited English speaking families, and a comparison between the mandate-treated target groups and control target groups from the neighboring states. Source: Author calculations from ACS data.

Annual Percentage Change in Pre-K Enrollments in Illinois Comparison Between Hispanic/Immigrants and Non-Hispanic/Immigrants

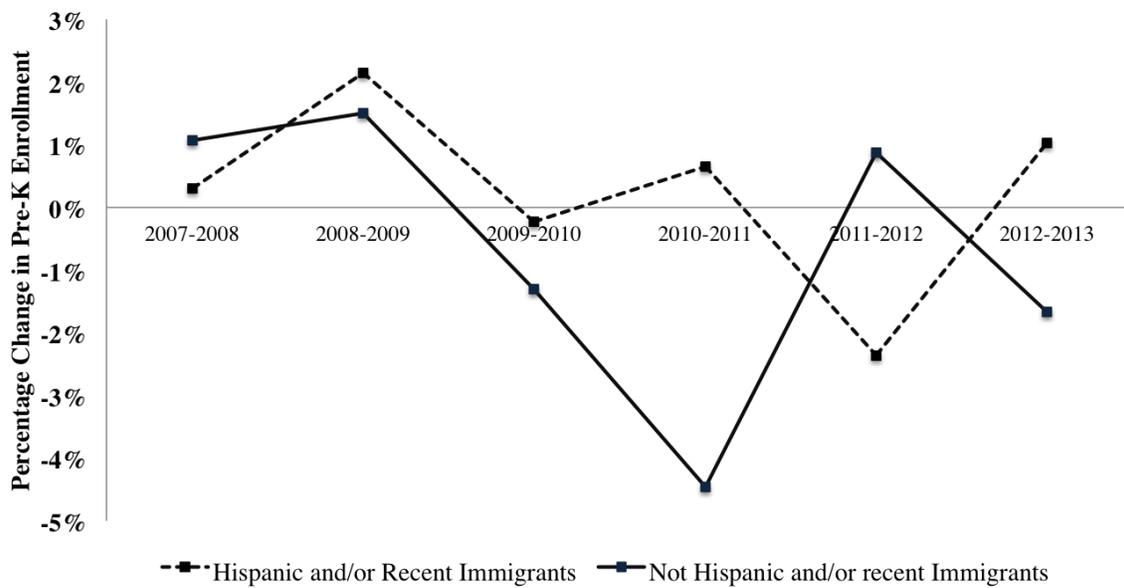


Figure 3: Annual Percentage Changes in Preschool Enrollments: Mandate-Treated Target Group vs. Control Group within Illinois

This figure shows the percentage change in preschool student enrollments over time from the mandate-treated target group (non-white Hispanic, immigrant, and limited English speaking families) and control group (all other races and English-speaking families) within Illinois. Source: Author calculations from ACS data.

Table 1: Descriptive Statistics of “Treated” Children and Mothers of Preschool Age in Illinois, Pre- & Post-Mandate

<u>Variable</u>	<u>Children of PreSchool Age</u>		<u>Mothers, PS Age Children</u>	
	<u>2008-2010</u>	<u>2011-2013</u>	<u>2008-2010</u>	<u>2011-2013</u>
Female	0.48	0.49	1.00	1.00
Age	3.50	3.52	29.90	30.50
<i>Education</i>				
Less than HS	-	-	0.31	0.29
HS Grad	-	-	0.32	0.31
Some College	-	-	0.22	0.25
College +	-	-	0.15	0.15
<i>Hispanic</i>				
Hispanic Origin	0.94	0.95	0.95	0.95
<i>Race</i>				
White	0.03	0.03	0.03	0.03
Black	0.03	0.02	0.03	0.02
Asian	0.13	0.13	0.13	0.13
Native American	0.01	0.02	0.01	0.01
Other/Multi Race	0.81	0.82	0.81	0.81
<i>SES</i>				
Family in Poverty Status	-	-	0.49	0.49
<i>Outcome Variables</i>				
Total Personal Income Earned (Annual)	-	-	12,891	13,108
Usual Hours/Week	-	-	19.7	19.3
In Labor Force	-	-	0.54	0.58
Attends Preschool	0.37	0.43	-	-

Notes: Descriptive Statistics calculated from American Community Survey Data (2007-2010). The first two columns represent preschool age children in Illinois from subgroups of interest, which are non-White Hispanics, recent immigrants, and where parents do not speak English well. The third and fourth columns represent only those included in the analytic sample, which is composed of non-White Hispanics, recent immigrants, and children/mothers of children in households where parents do not speak English well.

Table 2: DD Estimates of Bilingual Preschool Mandate on Enrollment

A. Compare Target Groups Across States			
	Specification		
Dep. Var.: Enrollment	(1)	(2)	(3)
Post x Treat State	0.068*** (0.014)	0.076*** (0.013)	0.076*** (0.013)
<i>Covariates</i>			
Demographic		Yes	Yes
Socioeconomic Status		Yes	Yes
State Fixed Effects			Yes
N (students)	2,281	2,281	2,281

B. Compare Target and Control Groups w/n Illinois			
	Specification		
Dep. Var.: Enrollment	(1)	(2)	(3)
Post x Treat State	0.075** (0.003)	0.076*** (0.003)	0.080*** (0.001)
<i>Covariates</i>			
Demographic		Yes	Yes
Socioeconomic Status			Yes
N (students)	3,100	3,100	3,100

Notes: Estimates are based on difference-in-differences specifications as detailed in paper text. Dependent variable is a binary enrollment variable from American Community Survey data in which parents state whether their child of each age are enrolled in preschool or preschool. Data are restricts to the years between 2008 to 2013. Robust standard errors are clustered at the state level in the first control strategy and are shown in parenthesis.

Table 3: DD Estimates of Bilingual Preschool on Enrollment using different timeframes

A. Compare Target Groups Across States			
	Timeframe Restriction		
Dep. Var.: Enrollment	2008-2013	2009-2012	2010-2011
Post x Treat State	0.076*** (0.013)	0.088*** (0.014)	0.092*** (0.027)
<i>Covariates</i>			
Demographic	Yes	Yes	Yes
Socioeconomic Status	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
N (students)	2,281	1,500	727
B. Compare Target and Control Groups w/n Illinois			
	Timeframe Restriction		
Dep. Var.: Enrollment	2008-2013	2009-2012	2010-2011
Post x Treat State	0.080*** (0.001)	0.082*** (0.001)	0.084*** (0.001)
<i>Covariates</i>			
Demographic	Yes	Yes	Yes
Socioeconomic Status	Yes	Yes	Yes
N (students)	3,100	2,171	1,123

Notes: Estimates are based on difference-in-differences specifications as detailed in paper text. Dependent variable is a binary enrollment variable from American Community Survey data in which parents state whether their child of each age are enrolled in preschool. Robust standard errors are clustered at the state level in the first control strategy and are shown in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: DD Estimates of Bilingual Preschool on Mothers' Labor Supply

A. Compare Target Groups Across States			
Dependent Variable	Specification		
	(1)	(2)	(3)
Prob of being in Labor Force	0.0197* (0.012)	0.023** (0.012)	0.024** (0.012)
Usual Hours Worked per week	1.7 (2.2)	2.6 (3.8)	2.5 (3.8)
Wage and Salary Income	129.6 (778.52)	442.5 (677.20)	452.18 (677.19)
<i>Covariates</i>			
Demographic		Yes	Yes
Socioeconomic Status		Yes	Yes
Year x State			Yes
N (mothers)	2,281	2,281	2,281

B. Compare Target and Control Groups w/n Illinois			
Dependent Variable	Specification		
	(1)	(2)	(3)
Prob of being in Labor Force	-0.011 (0.033)	-0.016 (0.033)	-0.016 (0.033)
Usual Hours Worked	-0.85 (1.13)	-0.67 (1.11)	-0.63 (1.11)
Wage and Salary Income	-438.8 (837.60)	-403.81 (857.30)	-401.24 (845.56)
<i>Covariates</i>			
Demographic		Yes	Yes
Socioeconomic Status		Yes	Yes
Year x State			Yes
N (students)	3,100	3,100	3,100

Notes: Estimates are based on difference-in-differences specifications as detailed in paper text. Dependent variables are from American Community Survey data. Data are restricted to the years between 2008 to 2013. Robust standard errors are clustered at the state level in the first control strategy and are shown in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: DD Estimates of Bilingual Pre-K on Mothers' Labor Supply using various timeframes

A. Compare Target Groups Across States			
Dependent Variable	Timeframe Restriction		
	2008-2013	2009-2012	2010-2011
Probability of being in Labor Force	0.024** (0.012)	0.022 (0.014)	0.017 (0.020)
Usual Hours Worked	2.5 (3.8)	5.1 (4.5)	3.8 (6.4)
Wage and Salary Income	452.18 (677.19)	162.71 (818.17)	-233.05 (1134.9)
<i>Covariates</i>			
Demographic	Yes	Yes	Yes
Socioeconomic Status	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
N (mothers)	2,281	1,500	727
B.: Compare Target and Control Groups w/n Illinois			
Dependent Variable	Timeframe Restriction		
	2008-2013	2009-2012	2010-2011
Probability of being in Labor Force	-0.016 (0.033)	-0.007 (0.04)	0.02 (0.06)
Usual Hours Worked	-1.1 (1.11)	-0.95 (1.30)	-0.86 (1.88)
Wage and Salary Income	-401.24 (845.56)	-516.57 (795.11)	-433.09 (696.50)
<i>Covariates</i>			
Demographic	Yes	Yes	Yes
Socioeconomic Status	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
N (mothers)	3,100	2,171	1,123

Notes: Estimates are based on difference-in-differences specifications as detailed in paper text. Dependent variables are from American Community Survey data. Robust standard errors are clustered at the state level in the first control strategy and are shown in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1