From Crayons to Diplomas: The Impact of State Preschool Programs on Mothers in Florida and Washington, DC

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Abstract

Access to affordable child care through programs like universal public preschool offers time to parents (and mothers in particular) who may otherwise stay home to care for their child. Using data from the American Community Survey (ACS), I explore whether mothers use this time to pursue further education. I use Florida and Washington, DC's universal preschool programs as case studies. Regression discontinuity design allows me to take advantage of age eligibility cutoffs in both programs. I find little to no effect on all educational outcomes in Florida. In Washington, DC, I find that mothers whose youngest child was young enough to enroll in public preschool in 2009 had slightly higher educational attainment (around 1.4 years, statistically significant) than mothers whose child was slightly too old to enroll. However, the results across all outcomes fluctuate, potentially due to limitations in the data.

1 Introduction

Child care for infants and toddlers in the US is inaccessible and unaffordable. Over 50% of families live in "child care deserts", where the demand for child care slots exceeds the available supply (Malik et al, 2018). For families that do manage to find a slot, child care costs take an average of 20% of an average married American couple's income, while averaging at around 10% in other OECD countries. Single parents have it worse: child care costs an average of 35% of their income, which is five times higher than the OECD average (OECD, 2023).

Many rich countries outside the US offer significant public support for children with families through public child care, child benefits, and other programs. Comparatively, the US is lacking: its federal child care support includes subsidies through the Child Care Development Fund (CCDF), which reaches only 15% of eligible children (Ullrich, 2019), and Head Start, which reaches around 36% of eligible children (NHSA, 2020). This means that families who cannot find or afford child care must choose between a parent staying home or finding an informal child care arrangement. These responsibilities often fall onto mothers.

To supplement insufficient federal support, many states have implemented public preschool programs. Some of these programs specifically serve children from low-income families, and others are universal meaning that any child that is age-eligible and is a resident of the state can enroll.

My analysis focuses on universal public preschool programs. I explore the impact that the implementation of these programs has on mothers of young children. Many studies have shown that access to affordable child care increases maternal employment, but child care theoretically does more than encourage mothers to work; it offers them time they wouldn't otherwise have. When given this time, mothers may also choose to pursue further education. I use two case studies— Florida and Washington, DC— to investigate this effect. I will explore the impact of access to universal public preschool on mothers' educational attainment and the rate they obtain college degrees.

In Section 1, I've introduced the broader state of child care in the US. Wishing this section, I review existing literature that studies the impact of public preschool programs on mothers and statues my research within it. I also provide overviews of the public preschool programs in Washington DC and Florida, respectively. In Section 2, I explain the data I use in my analysis and provide general summary statistics for DC and Florida during the years of analysis. Section 3 explains regression discontinuity design, the methodology I use for my analysis. Section 4 explores my main results for both DC and Florida, including outcomes for educational attainment and the rate of mothers with college degrees. In Section 5, I discuss the limitations of my data and analysis. I explore the implications of my findings in Section 6 and suggest future research directions in Section 7. I include supplemental figures and tables in my Appendix.

1.1 Literature Review

Child care policies are often discussed as two-generation policies: they aim to provide high-quality care for children and improve the socio-economic status of their parents. Research on child care programs tends to explore one or both of these areas.

A substantial body of research has found the positive impacts of high-quality child care on children. Highly cited studies have measured cognitive and language skills, memory, and socio-emotional outcomes. They found that children exposed to high-quality childcare improved on these outcomes, with a higher impact on children who were enrolled in these programs for longer periods (Belsky, 2007; Li, 2013; NICHD, 2006; Barnett, 2011). Yoshikawa, et al. (2013) reviews many aspects of the literature around preschool education in particular and highlights its strong positive impacts on children's early learning and social outcomes.

Research that examines the socio-economic impacts of access to child care on parents focuses on maternal employment rates and labor supply. Several early influential studies found that early childhood education, public preschool, and kindergarten all have a small positive effect on maternal employment, especially for low-income mothers and mothers with no younger children (Hofferth, 2000; Brooks-Gunn, 1994; Cascio, 2009; Gelbach, 2002). Since then, hundreds of studies have investigated different aspects of this question. In her literature review, Morrissey (2016) finds that studies with more recent US data have seen smaller effects, but generally the effect of child care on maternal employment is heterogeneous due to differences in programs, data sources, and methodological approaches.

My research aims to address two significant gaps in the literature: a lack of emphasis on state preschool programs despite their growing popularity nationwide, and minimal exploration of maternal education outcomes.

1.1.1 State Preschool Programs

Most studies in the field focus on federal policies like the CCDF and Head Start. These programs have existed for decades and their national scale makes it easier to find relevant data with high sample sizes. However, federal programs are only one aspect of child care support in the US. Public preschool programs sponsored by individual states have become popular in recent decades. In the late 1990s, Georgia and Oklahoma implemented the first universal preschool programs in the country. They have been some of the most commonly studied state programs in the field.

An early study that focused on Georgia and Oklahoma was Fitzpatrick (2010): using census data from 2000, she ran a regression discontinuity analysis based on the birthday cutoff for Georgia and Oklahoma's preschool programs to explore their impact on preschool enrollment and maternal employment rates. Her results found an increase in enrollment and no effect on maternal employment. In 2020, Li looked at these same programs using a synthetic control method. She found a small effect on maternal labor force participation and employment rate in Georgia and little to no effect in Oklahoma.

In the past 20 years, 44 states and the District of Columbia have implemented some form of public preschool. They vary in the year they were implemented, eligibility requirements (age of the child, household income, etc), and in what is offered within the program. Sall (2014) used a difference-in-differences design and took advantage of these different timing of the beginning of preschool programs in 10 states to find that pre-K increases the likelihood that married mothers participated in the labor force by 4.3% and that they were employed by 5.5%. There was no effect on single mothers. Sall (2014) did not take into account income eligibility rules, however, which means that his design wrongly assumed that some mothers were eligible when they did not have access. Ilin, et al (2021) expanded on this analysis, using data from all 44 states and the District of Columbia, as well as including income eligibility requirements where relevant. Using 16-month individual CPS panel data, she compared changes in mothers' labor force participation when their child becomes age-eligible to mothers who live in a state where they are ineligible for public preschool. She found that access to any public preschool program increases mothers' labor force participation rates by 2.3%.

Nation-wide studies are useful because they offer large sample sizes and can be informative for any general policy initiative in the US. However, each state's program differs in function and quality. An abundance of state preschool programs means that there is an opportunity to investigate differences between them and how these differences translate to larger or smaller impacts on mothers.

1.1.2 Education Outcomes

Though many studies have investigated the impact of child care programs on parents (mothers in particular), very few have meaningfully examined education outcomes.

A few recent papers have explored education outcomes, but largely on a federal scale. In 2019, Schochet & Johnson looked at the impact of child care subsidies (CCDF) on maternal educational attainment. Using longitudinal data from the Early Childhood Longitudinal Study- Birth Cohort and propensity score matching, they compared subsidy recipients with subsidy-eligible non-recipients. They found that a one-time receipt of child care subsidies increases mothers' educational attainment, and the effects are stronger for less-educated mothers and mothers who receive subsidies when their children are 2 years old compared to preschool age. Schochet & Johnson (2019) builds on two previous studies that surveyed women at either one point in time (Blau and Tekin, 2007) or during only one follow-up (Herbst and Tekin, 2011) to measure their subsidy receipts and education rates. This means that neither of these studies could properly measure any change education caused by child care subsidies.

Schiman (2022) measured maternal education outcomes along with maternal employment, household income, and enrollment in welfare programs using data from the Head Start Impact Study, a randomized control trial in which a nationally representative sample of families was randomly assigned to treatment in 2002-2008. He found that mothers who enrolled in the program when their child was three years old increased full-time employment, decreased part-time employment, and increased course enrollment. In contrast, mothers who only enrolled their child at four years old saw little effect in any measured outcomes.

Both of these studies look at federal programs, but state preschool programs are increasingly becoming more relevant as more states expand their role in providing child care. To my knowledge, no studies have investigated the impact of specific state preschool programs on maternal education outcomes.

1.2 DC's Universal Preschool Program

In 2009, DC implemented a voluntary universal public preschool program through the Pre-K Enhancement and Expansion Act of 2008. The city had offered some income-based programs since 1976; however, since 2009, public preschool has been available to all 3 and 4-year-olds that are residents of the District of Columbia, regardless of income. The program was considered fully implemented by 2011. Residents access programs through a "mixed delivery system", which includes public schools, charter schools, and other publicly funded community-based organizations (CBOs). Figure 1 shows the borders of the District of Columbia, where the program is targeted. Preschool is funded under the same guidelines as K-12 education, meaning that the program is offered 5 days a week for at least 6.5 hours per day and at least 180 days per year. Parents therefore may need to find additional care on weekends, in the summers, and outside of typical school hours, though many institutions that offer preschool programs also offer some sort of after-school and summer care.

Uptake is high, especially when compared to other public preschool programs in the US. DC ranks first among all state programs in access to both 3 and 4-year-olds. In 2014, 99% of 4-year-olds and 69% of 3-year-olds in DC were enrolled in public preschool (National Institute for Early Education Research, 2020). Figure 2 shows the enrollment rates for 3 and 4-year-olds from 2002 to 2014. Very few other programs surpass even half of eligible children enrolled, making this program a particularly useful case study since we can assume most eligible children receive treatment.

A recent study of DC's preschool program's effect on mothers found that it increased maternal labor force participation rates by 10 percent (Malik, 2018). This study looks at another potential way that mothers could use the time afforded to them when they have the option of accessible and affordable childcare: pursuing further education.

1.3 Florida's Voluntary Pre-K Program

In 2005, Florida introduced a relatively similar preschool program. Like in DC, the program is universal and voluntary, meaning any age-eligible child living in the state of Florida could attend. While DC covers 3 and 4-year-olds, Florida's VPK is limited only to 4-year-olds. Florida's VPK program allows parents to enroll their children in either a school-year program (540 instructional hours) or a summer program

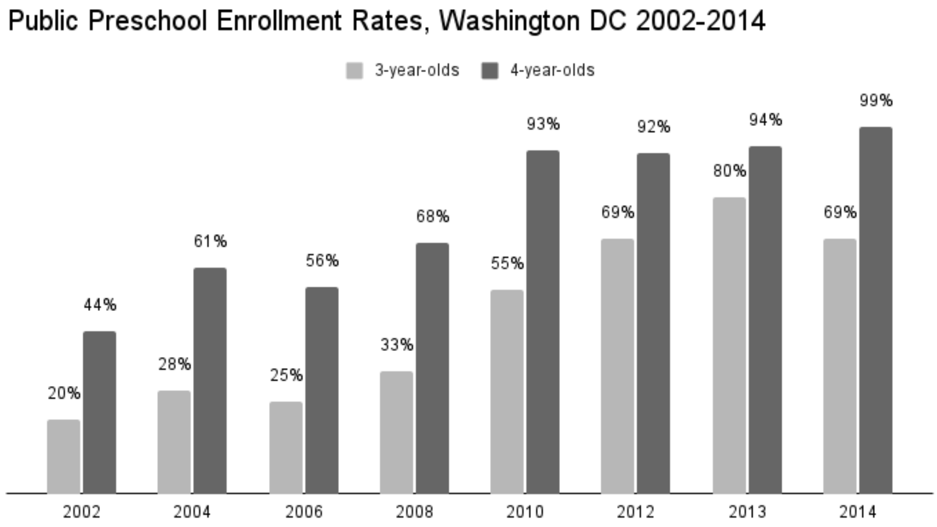

Fig. 1. The take-up rate for public preschool over time in Washington, DC. Enrollment rates for both 3 and 4-year-olds are among the highest in the country. The preschool extension was implemented in 2009. Source: National Institute for Early Education Research

(300 instructional hours). Like DC, this is a mixed-delivery system: preschools are hosted in schools, child care centers, and community centers.

This program was the state's first public preschool, and Florida saw a relatively high take-up rate among eligible families. Figure 3 shows the enrollment rate in public preschool before and after VPK was implemented, covering the years 2002-2010. Around 68% of 4-year-olds were enrolled in 2010. Enrollment increases to 75-80% in later years, but those are not included in my analysis. Though uptake is not as high as in DC, the difference in enrollment before and after program implementation is much sharper since there was no state preschool program before 2005.

2 Data

For my analysis, I use data from the American Community Survey (ACS), obtained from Integrated Public Use Microdata Series (IPUMS). [1] ACS is a cross-sectional, nationally representative survey. Its sample size is larger than other surveys like CPS, which makes it useful for studying specific subgroups. In particular, it gives me a decent sample size for mothers of young children in Washington, DC and Florida. I restrict my sample to women with children who are residents of the

[1] My data and code are accessible on GitHub: https://github.com/allie001/Effect-of-Preschool-Mothers-Education

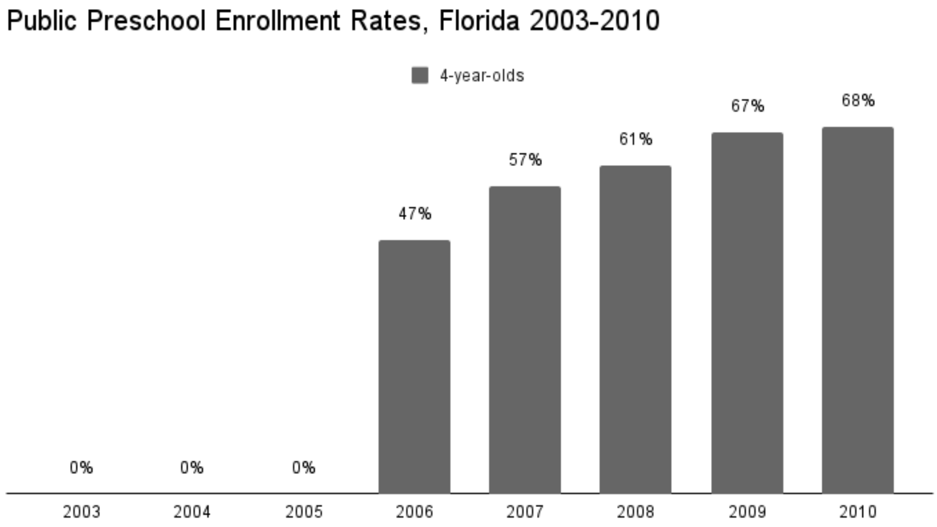

Fig. 2. The take-up rate for public preschool over time in Florida. Enrollment rates for 4-year-olds are among the highest in the country. The preschool program was implemented in 2005. Source: National Institute for Early Education Research

District of Columbia and Florida, since they are geographically eligible for their states' universal preschool programs. My analysis looks at impacts 2, 3, 4, and 5 years after a state's DC program was implemented. So, the data I use covers the years 2009-2014 in DC and 2005-2010 in Florida.

A significant disadvantage of using ACS data for this analysis is that it is not longitudinal. I cannot follow up on the educational outcomes of a particular individual who had access to the program in a previous year. Instead, I use aggregated data to identify population-wide trends. Since individuals can move in and out of states within the years of my analysis, the demographic of the groups I am looking at change over time. This means there is potential for confounding variables to bias my results. To supplement my analysis, I calculate summary statistics for key demographic measures— race, age, marital status, and household income— to track their potential effects on my analysis (see Tables 1-2 and Tables D.1-D.2).

I make transformations and assumptions for the following variables in my analysis:

- **Educational Attainment:** I make some estimates to define educational attainment when ACS data did not give me exact years. For example, I assumed that everyone with a Master's degree finished high school (13 years), spent 4 years in undergraduate school, and then 2 years in graduate school. It may be the case that an individual had done a joint Bachelors/Masters degree in only 5 years total or took a 3-year masters course, but for the sake of simplicity and since I do not have access to this information, I assumed that everyone took the average amount of time to obtain their degrees.

- **Age of Youngest Child:** I use the age of a woman's youngest child to measure whether she had access to DC's preschool program. This means that some women who may have used the program are counted as untreated if their older child is enrolled but they have a toddler who is not old enough to enroll in preschool yet. I use this metric as opposed to any child being eligible because having a younger child might negate the treatment effect of access to public preschool since a mother would still have to care for that younger child regardless of whether their preschooler is in school or not. This is standard practice for research that studies the impact of child care policies on parents (Morrissey, 2016).
- **Race:** I include variables for White, Black/African American, Asian/Pacific Islander, and other or mixed race in my models. Though I could have been more detailed, I chose these categories because they were the most prominent racial groups in my data.
- **Marital Status:** I use a binary variable that indicates only whether an individual is married or not (this does not include people who are separated). This means that some individuals marked as unmarried may be living with a partner, so they would likely receive many of the similar benefits of having a spouse like an additional income or a partner available to share childcare responsibilities.
- **College degree variables:** Using information from ACS's "detailed education" variable, I create a set of binary variables to signal whether an individual has a college degree (this includes associate's degrees, bachelor's degrees, master's degrees, PhDs, and other professional postgraduate degrees). Additional variables specify whether an individuals has a bachelor's degree and a postgraduate degree. ACS only includes information about an individual's highest level of education, so I assume that everyone with a postgraduate degree also has a bachelor's degree. This means that I don't lose any individuals who pursue a postgraduate degree when I calculate the percent of mothers with a bachelor's degree.

2.1 Summary Statistics

My analysis focuses on the 2, 3, 4, and 5-year impacts on educational outcomes after the implementation of universal public preschool in Washington, DC and Florida. Tables 1 and 2 display summary statistics for the mothers in DC and Florida when the programs just started and 2, 3, 4, and 5 years after, when we would expect to see an effect on educational attainment. These statistics cover all mothers with children ages 18 and under. I will use a smaller subset of this population in my analysis, but I include this data to show overall population trends. Appendix A, Tables 1 and 2 show summary statistics using only the data in the analysis.

Over the period of analysis (2009-2014), moms in Washington DC become whiter and richer on average. During these years, more white families moved into city center while many families of color moved to outer parts of the DC metropolitan area (Malik, 2018). Though most measures remain relatively similar across all years, these differences show that the people I use to represent 5-year effects on mothers that had access to public preschool when it just began in 2009 are not the same as those who actually used the program in that year. I attempt to make these groups similar by focusing on a smaller subset (see Appendix D, Tables D.1 and D.2 for those statistics), but relying on aggregate statistics limits my ability to make find unbiased effects, even in the short and medium-term.

Moms in Florida are more stable. They remain at similar levels in all measures likely because the sample size is higher. This means that moms 5 years after the preschool program was implemented are more comparable to moms that lived in Florida when the program started in 2005.

Table 1. *Summary Statistics for Mothers in Washington DC, 2009-2014*

	2009	2011	2012	2013	2014
# Observations	463	482	490	535	508
Married	44.5%	48.0%	50.8%	53.1%	53.2%
Median Household Income	$66,500	$86,000	$78,000	$80,100	$89,600
Average Household Income	$105,903	$137,434	$129,315	$141,815	$136,646
Average Age (years)	38.9	38.2	38.5	38.5	34.0
Average Education (years)	15.2	15.6	15.4	15.8	14.7
Racial Breakdown					
White	33.1%	36.5%	34.7%	38.3%	34.0%
Black/African American	58.3%	54.2%	54.5%	52.9%	51.0%
Asian/Pacific Islander	2.6%	2.9%	3.5%	3.7%	1.6%
Mixed/Other	6.1%	6.0%	7.3%	5.1%	7.0%

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

3 Methodology

Both Florida and DC's preschool programs have an age threshold, meaning there is an opportunity to take advantage of a natural experiment through a regression discontinuity design (RDD). This method has been used by other studies that investigated the effects of public preschool on maternal employment (Russo, 2017; Fitzpatrick, 2010).

My analysis compares mothers whose youngest child was just above and below the age threshold when preschool programs in their respective states were implemented. Mothers whose child was only slightly too old to qualify are theoretically not very different from mothers who did have access, so they serve as a control.

My analysis of both states relies on the same basic model:

$$Y = f(X_t) + \beta_1(X_t >= c_t)_{+}\varepsilon_t$$

Since my analysis looks at the impact of access to public preschool at multiple points in time, t represents a given year. Y is the outcome variable, either average educational attainment or the proportion of moms that have a particular college degree. $f(X_t)$ represents the relationship between the age of a mother's youngest child and her education without any intervention in a given year. β_1 represents the effect of access to Florida or DC's preschool program, which is represented by $(X_t >= c_t)$. This is the age-eligibility cutoff point in a given year (t). $X_t >= c_t$ is either 1 (eligible to enroll) or 0 (ineligible to enroll). c changes according to the year because the children that would have been eligible to enroll in public preschool get older each year. For example, children who were 4 years old when DC implemented

Table 2. *Summary Statistics for Mothers in Florida, 2005-2010*

	2005	2007	2008	2009	2010
# Observations	20,215	20,345	20,149	19,957	20,147
Married	68.8%	68.5%	68.4%	66.8%	64.9%
Median Household Income	$57,400	$63,000	$64,000	$60,000	$57,400
Average Household Income	$74,535	$82,696	$83,297	$78,737	$74,983
Average Age (years)	38.2	38.4	38.6	38.5	38.5
Average Education (years)	14.4	14.5	14.5	14.5	14.5
Racial Breakdown					
White	75.3%	74.5%	75.5%	74.9%	74.1%
Black/African American	14.7%	15.2%	15.3%	15.8%	17.0%
Asian/Pacific Islander	2.9%	3.3%	3.7%	3.7%	3.6%
Mixed/Other	7.1%	7.0%	5.5%	5.7%	5.3%

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

its preschool program in 2009 are 6 in 2011, 7 in 2012, 8 in 2013, and 9 in 2014. Finally, ε_t represents random noise.

I am using a sharp discontinuity design, which means that I assume that all women with a child below the age cutoff are treated and all women with a child above the age cutoff are not treated. In terms of actual state preschool attendance, this is not strictly true (see enrollment rates in Florida and DC in Figures 1 and 2). DC reaches close to 100% enrollment in most years after the 2009 expansion, but it had a different preschool program in place before 2009 (though this program was means-tested and functioned differently than the preschool post-expansion). In contrast, Florida did not have a state preschool program before 2005, but only saw enrollment rates of around 60% in its early years. I do not have access to enrollment data on an individual level that would allow me to implement a perhaps more accurate fuzzy design. However, the age threshold does sharply determine whether or not mothers have access to universal public preschool. So, my results are measuring the impact of access to universal public preschool, not necessarily the impact of enrollment.

My model specifications are informed by previous literature. I use a method proposed by Imbens & Kalyanaraman (2012) to determine an optimal bandwidth for each RD model based on minimizing squared error loss in a local linear regression. I use a linear functional form because this generally fits my data better, and is arguably better practice for regression discontinuity since models with high polynomials often return incorrect and unintuitive results (Gelman & Imbens, 2015).

Finally, I use default settings in the rdrobust package to calculate bin sizes in each model.

In Appendix B and C, I run tests to ensure that my model is not overly sensitive to changes in model specifications. Changes in bandwidth predictably have the largest impact on treatment effect because this drastically changes the data that the model is working with (Appendix B). Adjustments to the polynomial level have small effects on the magnitude of the treatment effect, but do not show any dramatic changes (Figures C.1 and C.2). Bin size (the number of individual data points that each point on the plot represents) has the smallest effect on the model, only causing the regression lines to move when the bins are very large (Figures C.3 and C.4).

A few assumptions must be met for a regression discontinuity design to produce unbiased results. First, there should be no manipulation of the running variable. This means that we assume that individuals cannot adjust where they fall on the running variable to receive or avoid treatment. There is no obvious clustering around the cutoff point, which often implies this kind of manipulation. The only way to manipulate the age of one's youngest child is to intentionally time their birth so that they are young enough to enroll in preschool. This is generally implausible since these programs were implemented within a few years of being proposed. There is a chance, however, that families moved to DC or Florida partially because they offer free universal preschool. Because I am not using longitudinal data, I cannot control for this bias in my analysis beyond tracking overall population trends.

Second, the only discontinuity in the models should be in the outcome variables. If any potential covariate also has a discontinuity at the cutoff point, this may bias the treatment effect. To test this, I ran continuity tests on each relevant demographic variable I measure in my summary statistics: age, household income, marital status, and race (Figures 3 and 4). In Washington, DC, there are some small discontinuities in each variable, but the data has high variance and the discontinuity isn't visually obvious or statistically significant.

In Florida, most discontinuities in these variables are even smaller than in DC. However, household income is an exception: in Florida there is a statistically significant discontinuity, though its magnitude is relatively small. Household income is correlated with education, which means that this could bias models using Florida data. There could be an additional bias if mothers with different income levels respond to the treatment differently.

Generally, these continuity tests increase confidence in my models. For the most part I can assume any discontinuity observed in the outcome variables is due to treatment, not another covariate.

Though the treatment effect in these models is not a simple difference of averages between the pre- and post-cutoff groups, it is still important that these groups are relatively similar to each other. Tables D.1 and D.2 display summary statistics that show how each pre- and post-cutoff group compare for every variable and year measured. In the Washington, DC data, there are multiple years where the groups are very different on many key measures (age, income, race, marital status). This was due to a generally small sample size— there is a trade-off between ensuring that treatment and control groups are similar and including enough data for a robust regression on either side of the cutoff. Florida, on the other hand, has significantly higher sample sizes, and the pre- and post-cutoff groups are essentially identical on all key indicators except for age.

These statistics are less meaningful than the continuity tests, however, since averages do not necessarily reflect the actual differences between individuals close to the cutoff point. For example, age is unbalanced in almost every model. However, it has the smallest discontinuity at the cutoff point compared to other covariates in both Florida and DC.

Finally, because my analysis only involves women whose youngest child is just below and above the age cutoff, the treatment effect I find is not generalizable to a larger population. Instead, it is a local average treatment effect (LATE), which is only applicable to mothers who fall just below or above the cutoff point.

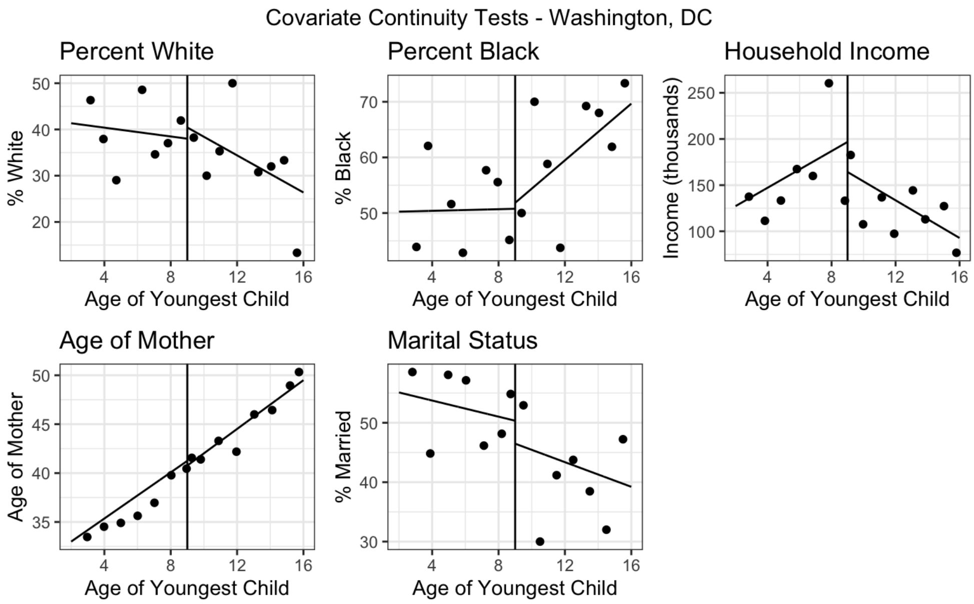

Fig. 3. These plots test whether relevant covariates (the proportion of white and black mothers, household income, age, and marital status) are continuous at the point where a mother's youngest child would have been eligible for Washington DC's public preschool program. If a covariate has a significant discontinuity, this means that an observed discontinuity in the outcome variable may not be solely due to the treatment effect. For Washington DC, I run this test on data from 2013.

4 Results

I investigate the 2, 3, 4, and 5-year effects on educational outcomes, including educational attainment and the percentage of mothers with bachelor's degrees,

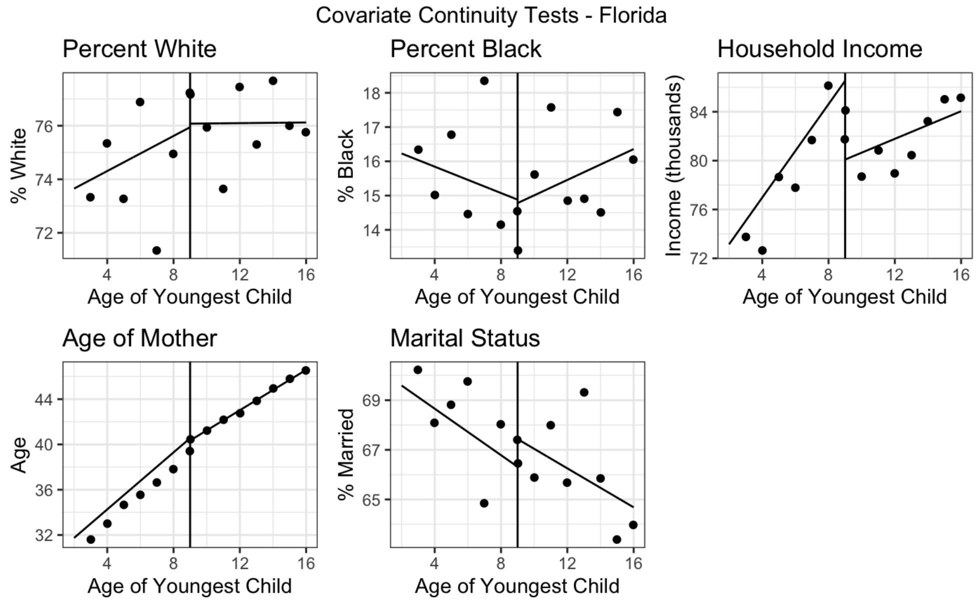

Fig. 4. Similar to Figure 3, these plots test for continuity at the eligibility cutoff point for Florida's public preschool across five covariates: household income, age, marital status, and race (specifically the rate of white and black mothers). For Florida, I run this test on data from 2009.

postgraduate degrees, and any college degree overall. Washington, DC implemented its preschool program in 2009, so I look at the years 2011-2014. Florida implemented its program in 2005, so my analysis involves the years 2007-2010. If a regression discontinuity design finds a negative effect, this would imply that mothers whose children were young enough to be eligible to enroll in their state's public preschool used that time to pursue additional education, while mothers whose children were too old to enroll were not able to. In Appendix B, I show bandiwdth sensitivity tests for all models.

4.1 Educational Attainment

Educational attainment, or the number of years of schooling that an individual has received, offers detail on the effect of Florida and DC's universal preschool programs on mothers' educational outcomes. Using this variable, I include any mothers that pursued further education but have not yet received a degree in my analysis.

4.1.1 Educational Attainment in Washington, DC

Washington, DC's regression results (displayed in Figure 5 and Table 3) show a mixed picture. In 2011 and 2013 (2 and 4 years after enrollment), the model finds that women whose child was just young enough to enroll in preschool had approx-

Table 3. *Regression Results for Educational Attainment in Washington, DC*

	2011	2012	2013	2014
Educational Attainment	-1.43 (0.84)	0.75 (0.72)	-1.44 . (0.84)	0.88 (1.03)

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

imately 1.43 years more education on average compares to women whose children were too old to enroll. These effects are both significant at a p = 0.1 level. The treatment effects in 2012 and 2014 are positive, meaning the women eligible to take advantage of the preschool program had just under one year less education than women above the cutoff. However, these effects are not statistically significant at a p = 0.1 level.

This fluctuation likely occurs because the educational attainment variable naturally favors people who receive more advanced degrees, so it is particularly sensitive to fluctuations in rates of postgraduate degrees. Table D.1 shows the differences between the degree attainment in the treatment and control groups across each year in the analysis. In 2011 and 2013, the years with a larger negative effect, the rate of postgraduate degrees in the pre-cutoff group was 15% higher than in the post-cutoff group. These differences are smaller in 2012 and 2014, where the regression returns a positive result.

The regression discontinuity plots (Figure 5) in 2011 and 2013 show more obvious discontinuities in the data, and the linear regression fits the data well. Individuals on the left side of the cutoff point seem to have higher education than those to the right of the cutoff. In 2012 and 2014, a discontinuity is less visually obvious and both include outliers that fall far from the regression line, which is reflected in smaller and less statistically significant effects.

Bandwidth sensitivity tests (Figure B.1) show fairly stable treatment effects across all years except for 2011, which quickly approaches zero as the bandwidth increases. While the magnitude of treatment effects is relatively robust, 0 falls within the confidence interval for most bandwidths in all years of the analysis. This is expected for non-significant results in 2012 and 2014 but suggests that even significant results in 2011 and 2013 quickly become insignificant if model specifications are adjusted.

Importantly, DC's small sample size means that the treatment and control groups on each side of the cutoff point differ from each other in key demographic measures. The treatment and control groups are relatively similar to each other in 2013 and 2014 but are insufficiently similar in 2011 and 2012 (see Table D.1). Differences in educational attainment likely come at least partly from these difference in demographics.

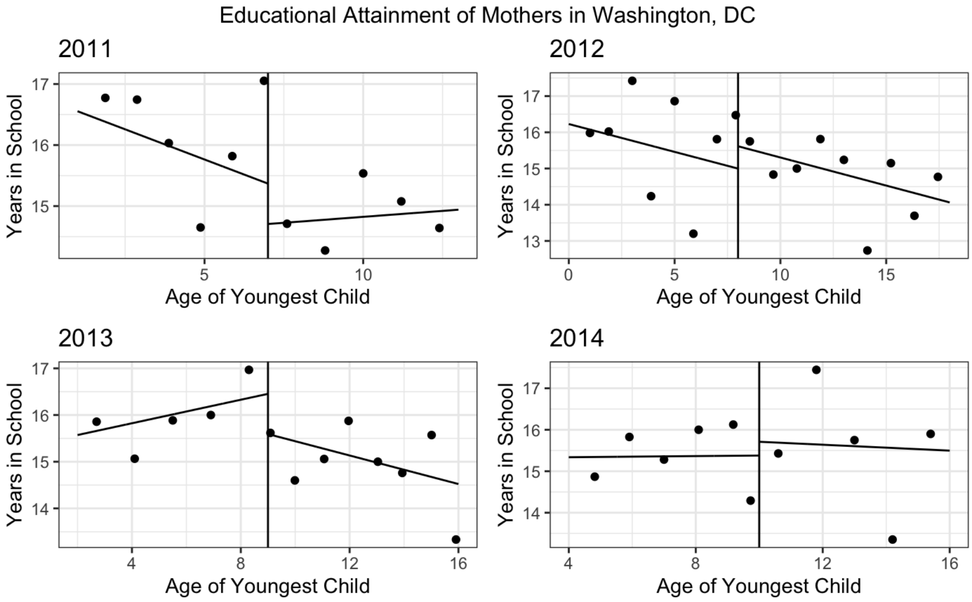

Fig. 5. These regression discontinuity plots show educational attainment for mothers in Washington, DC 2, 3, 4, and 5 years after public preschool was implemented. They separate mothers based on their youngest child's age. For example, in 2014 children 9 years old and younger were eligible to enroll in DC's public preschool when it was implemented in 2009 or later. Mothers above the cutoff point have children who were too old to be eligible, so they did not receive treatment.

4.1.2 Educational Attainment in Florida

An analysis of Florida's data (Table 4) finds similarly mixed, but smaller overall effects. Though the treatment effect fluctuates between positive and negative values, these values are all very close to zero. The largest and only statistically significant effect is in 2010, 5 years after the implementation of the state's preschool program. In that year, mothers whose children were young enough to enroll in preschool had 0.28 more years of education on average compared to mothers whose children were slightly too old to enroll.

Data in the regression discontinuity plots for each year (Figure 6) show little visual discontinuity. 2010 seems to have the most difference before and after the cutoff, but this is largely driven by two outliers to the left of the threshold.

Additionally, bandwidth sensitivity tests (Figure B.2) show that manipulating the models' bandwidths causes treatment effects in all years to approach a treatment effect of 0. In general, these results can be interpreted as having little to no effect. A very small effect after 5 years is difficult to attribute directly to the year of access to public preschool because children enrolled in public school soon after, so some mothers may have pursued further education in those years as well.

Overall, access to universal public preschool maybe have slightly increased educa-

Table 4. *Regression Results for Educational Attainment in Florida*

	2007	2008	2009	2010
Educational Attainment	0.00 (0.09)	-0.07 (0.09)	0.08 (0.11)	-0.28 * (0.13)

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

tional attainment in Washington, DC, and had little to no effect in Florida. Though the preschool programs are similar, these differences may occur because DC offers an extra year of preschool and because the demographics of moms in Florida and DC are very different. It is plausible that preschool might have a higher impact on women with higher income and higher baseline education like in DC.

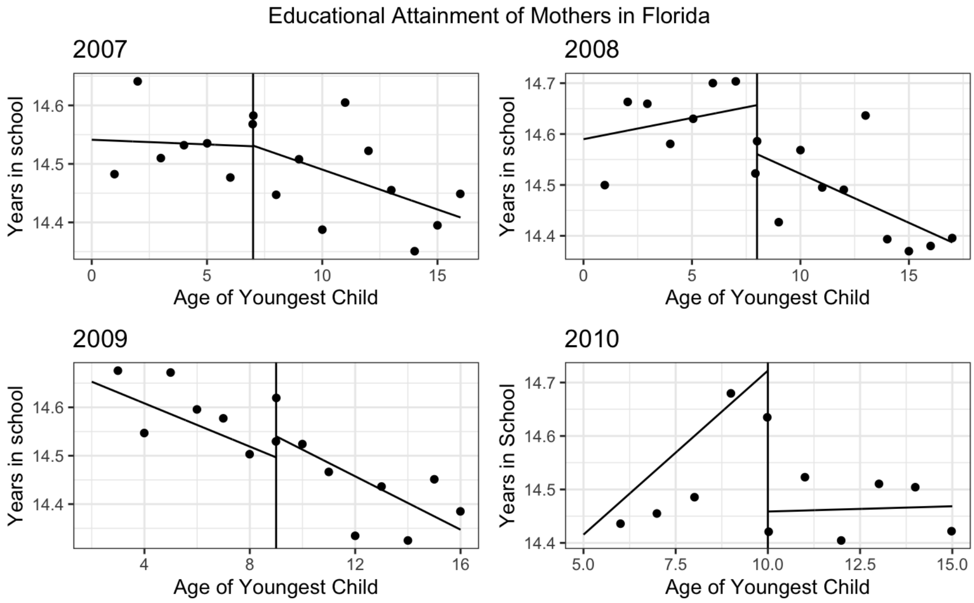

Fig. 6. These regression discontinuity plots show educational attainment for mothers in Florida 2, 3, 4, and 5 years after public preschool was implemented in 2005. They separate mothers based on their youngest child's age. Mothers above the cutoff point have children who were too old to be eligible, so they did not receive treatment.

4.2 College Degrees

I also run an analysis to investigate the effect of state preschool programs on the percentage of mothers with college degrees, including associate's degrees, bachelor's degrees, and postgraduate degrees. The treatment effects in these models are in terms of percentage points, and a negative effect implies that women who had access to public preschool had higher rates of obtaining a college degree. In Appendix A, I include results for rates of bachelor's degrees and postgraduate degrees specifically. In Appendix B, I show bandwidth sensitivity tests for all models.

4.2.1 College Degrees in Washington, DC

The results in Table 5 are relatively in line with regression results for educational attainment. There is a similar fluctuation across all outcomes, which is reasonable considering these variables have very close relationships with each other. I also run models that focus only on mothers with bachelor's degrees and postgraduate degrees (Table A.1 and Figures A.1 and A.2), and their results follow a similar pattern.

Figure 5 shows regression discontinuity plots for the rate of mothers with college degrees. In almost all models, there are no visually obvious discontinuities and a high variance in the data, suggesting overall weak effects. Bandwidth sensitivity tests show that most treatment effects remain at similar magnitudes after the bandwidth is larger than around five years on each side (Figures B.3, B.5, and B.7).

In 2011, 2 years after the beginning of DC's universal preschool program, there is a negative effect of -0.095, which means that the rate of mothers with college degrees was 9.5% higher for mothers whose children could enroll in public preschool than mothers whose children were too old to enroll. This effect is largest for mothers with postgraduate degrees (Figure A.2), which might occur because many postgraduate degrees only take 2 years to obtain (such as master's degrees, MBAs, etc).

However, this effect does not stay consistent. It turns positive in 2012 and goes back to negative in 2013, suggesting the program had an opposite impact from on year to another. This pattern that is difficult to justify with consistent patterns of behavior. None of the effects in 2012 or 2013 are statistically significant, and regression discontinuity plots do not suggest a strong discontinuity, which means their variation likely comes from a random error in the data.

In 2014, the treatment effect flips again: it's positive and statistically significant at a significant level of $p = 0.1$. The model finds that in 2014, the rate of mothers with college degrees was approximately 20% lower for mothers who could enroll their child in preschool compared to mothers whose children were slightly too old. A strong positive effect could emerge after 5 years because the mothers whose child was too old to enroll in public preschool have had time to catch up and surpass while their child was enrolled in school. However, if this was the case the analysis would likely show the pattern as a gradual progression, not an abrupt flip from a relatively strong negative effect to a strong positive effect in one year. The regression plot for all college degrees in 2014 (Figure 5) does not show an obvious strong discontinuity

Table 5. *Regression Results for the Rates of Mothers with College Degrees in Washington, DC*

	2011	2012	2013	2014
College degree rate	-0.10	0.10	-0.13	0.20 .
	(0.10)	(0.10)	(0.10)	(0.12)

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

as the results suggest and includes a particularly high outlier on the right of the cutoff point. However, a bandwidth sensitivity test shows that the treatment effect remains fairly constant regardless of model specifications (Figure B.3).

The fluctuations in these results are most likely caused by shifting demographics in the samples (Table D.1), similar to the educational attainment results. Interestingly, the fluctuations between each year are not consistent the overall migration trends in DC— the samples become less white and less wealthy. Small sample sizes and significant differences between the characteristics of control and treatment groups, particularly in 2011-2013, potentially caused treatment effects to fluctuate.

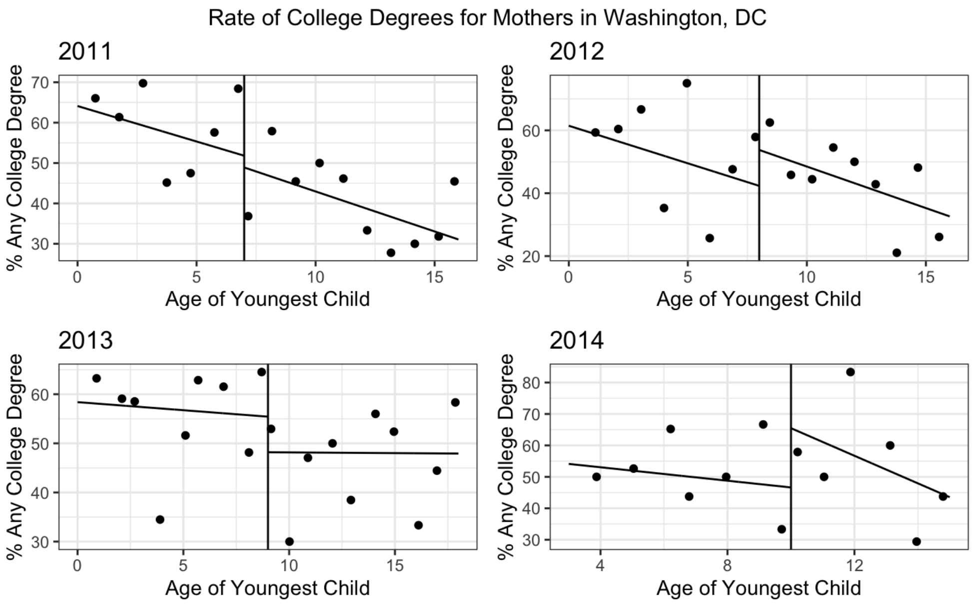

Fig. 7. These regression discontinuity plots show the percentage of mothers with any college degree in Washington, DC 2, 3, 4, and 5 years after public preschool was implemented. Mothers to the left of the cutoff point had access to universal public preschool because their youngest child was young enough to enroll in the program when it was implemented.

Table 6. *Regression Results for the Rates of Mothers with College Degrees in Florida*

	2007	2008	2009	2010
College degree rate	-0.02	-0.01	0.00	-0.04 .
	(0.02)	(0.02)	(0.02)	(0.02)

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

4.2.2 College Degrees in Florida

In Florida, the impact of access to public preschool on the proportion of mothers with college degrees is very small and generally trends slightly negative, similar to Florida's results for educational attainment.

The effect on rates of mothers with college degree is close to zero in 2007-2009. Regression discontinuity plots for those years show data without clear discontinuities (Figure 8). In 2010 it reaches its highest magnitude, where the rate of mothers with college degrees is around 4% higher for mothers who could enroll their child in preschool compared to those that couldn't. The majority of this effect is made up of changes in the rate of bachelor's degrees: treated mothers had a rate around 3% higher than untreated mothers (Table A.2). An increase after 5 years suggests that mothers began their bachelor's degree programs either while their child was in preschool or kindergarten. The regression plot for 2010 (Figure 8), however, has high variance and potential outliers that make a discontinuity harder to spot. Overall, these treatment effects remain stable across different bandwidths (Figure B.4).

The rate of bachelor degrees also has a particularly large effect in 2007 (two years after the implementation of preschool): the rate of mothers with bachelor's degrees this year was approximately 4% higher for women before the cutoff point compared to women after the cutoff point. There is a visual discontinuity in the regression plot (Figure A.3), and the effect is robust across bandwidths (Figure B.6). Since bachelor's degrees typically require four years to complete, it is possible that access to public preschool allowed these women to remain in college programs they were already enrolled in, use an existing associate's degree or other earned credits to pursue bachelor's degree, or have the chance to return to a degree program they had previously left unfinished.

Overall, the impact of public preschool on the rates of mothers with college degrees in Florida and Washington, DC mirrors educational attainment. DC has larger but more mixed effects, and effects in Florida are close to zero and show much less fluctuation.

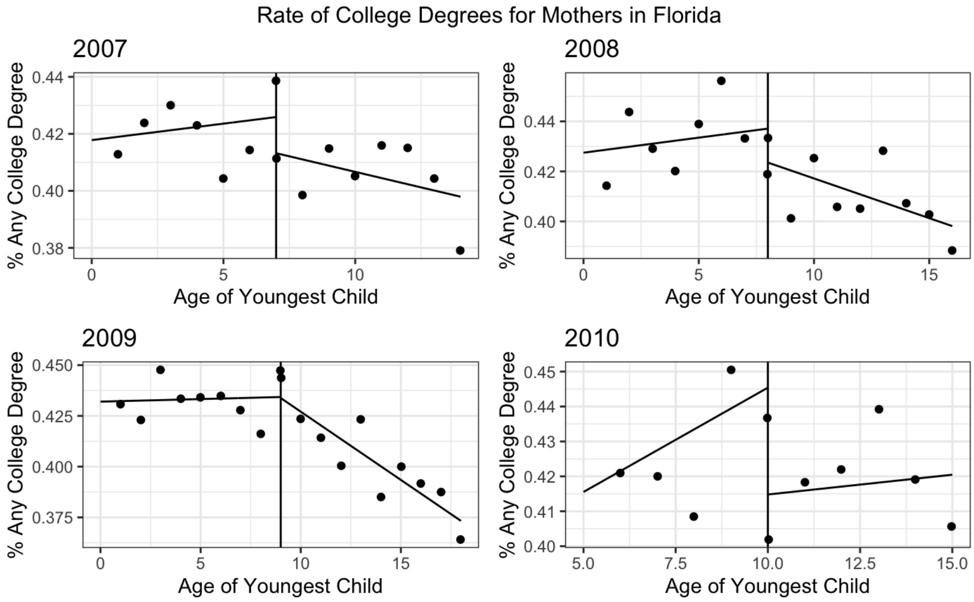

Fig. 8. These regression discontinuity plots show the percentage of mothers with college degrees in Florida 2, 3, 4, and 5 years after public preschool was implemented. Mothers on the left of the cutoff point were able to enroll their youngest child in public preschool, while mothers on the right of the cutoff could not enroll their child because they were not age-eligible.

5 Limitations

There are several limitations in this analysis. Though regression discontinuity using an age cutoff is a popular method for studying the impacts of preschool programs, limitations in my data hinder the validity of my results.

First, I am measuring the effect of access to universal public preschool, not the effect of enrollment itself. Because not all eligible children were enrolled and some children in the control group may have been enrolled in a private preschool or, in Washington, DC's case, a previous state preschool program, the treatment effects are not as precise as they could be with additional enrollment data. There are also many other federal, state, and city programs that some mothers may have access to that impact their access to child care, like subsidies from the CCDF or city-specific preschool programs. Welfare programs in general help reduce financial burdens for low-income families, which makes it more likely that they can afford child care. These have less impact in states like Florida and Washington, DC because they offer free universal preschool (so subsidies are less necessary), but they still play an important role in decisions to work or pursue higher education.

Additionally, cross-sectional data is not ideal for this study. I cannot ensure that individuals I am using in my analysis are the same ones that actually had access to treatment a few years earlier. Washington DC in particular saw big changes in population demographics over this time, which means it's likely that I am including mothers in my sample that did not live in DC when their child was in preschool. Cross-sectional data also means that I cannot measure baseline educational attainment before treatment, which makes it more difficult to isolate a treatment effect. Since I am not following the same individuals over time, I don't know when mothers actually went to school. I try to ensure that the only difference between the treatment and control group is access to treatment, but differences in demographics could easily impact my results.

Further limitations in sample sizes, particularly in Washington, DC, meaning that my bandwidths are unsatisfactory for most years. Any causal effect that my models find is likely biased by confounding variables. Bandwidths range between 5 to 10 years on either side of the cutoff point, which means that the women on either side are not directly comparable. Tables D.1 and D.2 display summary statistics for the treatment and control group each year. Treatment and control groups are particularly unbalanced in Washington, DC in 2011 and 2012. Most covariates are continuous at the cutoff point, except for household income in Florida. The test suggests that treated mothers have a slightly higher household income on average. Household income is correlated with higher baseline educational attainment, which biases my results.

Finally, a large bandwidth also means that the treatment effect of access to universal public preschool is less isolated. Women in 2014 may have gained educational attainment when their child was already in public school, not preschool, but this would still factor into the treatment effect. It's difficult to avoid this since I want to investigate the short and medium-term outcomes of the program but do not have access to longitudinal data. It's also important to note that ACS only collects data on the current age of a child, not their date of birth. This makes the RDD model's threshold less precise. It is possible that some "treated" mothers had children who were too old to enroll in the preschool program by a few months.

6 Conclusion

Overall, my results suggest that access to universal public preschool had little to no effect on educational outcomes for mothers with a child who was just young enough to qualify for Florida's preschool. Washington, DC's results are more difficult to interpret: treatment effects with negative values tend to be stronger and more statistically significant (meaning the preschool program encouraged mothers to pursue education), but effects alternate between positive and negative values in all measured outcomes. These variations are likely due to the limitations of each model and available data rather than a reflection of an actual effect.

Several factors could explain the lack of a strong treatment effect. First, the data itself is limited because it is not longitudinal. Educational outcomes are particularly difficult because they vary significantly between individuals for very different reasons, such as personal value of education or ambition towards a particular career, and aggregate statistics may dilute potential heterogeneous effects. Perhaps there is a stronger effect on a particular subgroup that this study did not focus on. There are also practical issues such as migration. It is possible that many of the people surveyed 5 years after the implementation of Florida or DC's preschool program did not live in the state when the program actually began. This is more likely in DC than Florida, since Florida's summary statistics remained more stable over the years of analysis.

Beyond the models, it is reasonable and expected that people generally choose to work rather than go to school when offered the opportunity to reduce their childcare responsibilities. This might be especially true around the time that the DC program was implemented: the 2008 recession likely meant that many young families were less financially stable and therefore more likely to seek employment rather than invest time and money they may not have in education. Malik (2018) found that DC's preschool program increased maternal labor force participation by 10 percent; such a large increase suggests that employment was the primary choice for most mothers rather than education.

As discussed in the methodology section, it is important to note that due to the regression discontinuity design, these results are not generalizable to the entire population. A local average treatment effect (LATE) only applies to mothers whose child was close to the age cutoff in the year of implementation, not all mothers who have had access to DC or Florida's preschool program or mothers in general. Though this study does not look at whether the treatment effects change across age, race, marital status, socio-economic status, or previous education, other research has found heterogeneous treatment effects of child care policies across these sub-groups (Morrissey, 2016). Therefore, it's likely that as the characteristics of mothers in each state change over time and in the future, the impact of the same program may be very different than what was found in this study. Additionally, there is significant variation in the preschool programs between states, so we can't assume that any type preschool program will have the same effect. Still, this study is a useful starting point to understand the impact of voluntary preschool programs on maternal education. Though the results of this case studies are not externally valid, they offer an understanding of how a particular population was affected. Further research can expand on how these impacts change when applied to different populations and through programs with different characteristics.

6.1 Washington, DC vs. Florida

Larger sample sizes mean that treatment and control groups in Florida are more similar and suggest that its results are more internally valid. Still, the difference in effects between Florida and Washington, DC can be informative.

Preschool in Washington, DC is available for both 3 and 4-year-olds, and the state offers both school-year and summer care (though summer care is not guaranteed to be free). Only 4-year-olds are eligible in Florida, and parents must choose to enroll their child in either summer or school year programs. Effects in Washington, DC are larger than in Florida; this aligns with previous literature that finds that longer exposure to affordable child care generally yields larger impacts. This is true in studies that look at educational outcomes in particular (Schochet Johnson, 2019), including a study that found that two years of Head Start positively impacted maternal education outcomes, but one year of Head Start had no effect (Schimen, 2022). These differences in results support this outcome: one year of public preschool may not impact decisions to pursue further education, but two years reduce costs enough to influence decision-making.

Additionally, there are large differences between mothers in Florida and Washington, DC. Mothers in DC are richer, more educated, less white, and less likely to be married than mothers in Florida. Previous literature has found heterogeneous effects across income levels, marital status, baseline education level, and race (Morrissey, 2016). This suggests that mothers in either state are likely to respond differently on average to access to public preschool programs, which my results support.

Another important difference is spending: Washington, DC spends much more per child compared to Florida. However, this is more likely to affect the quality of care rather than accessibility, which has a less direct link to offering time to pursue further education. Relatively high uptake rates suggest that parents enroll their children in both programs despite their potential difference in quality.

7 Future Research Directions

As child care policies increase in popularity in the US on the federal, state, and local levels, there are opportunities for further research into the effect of access to affordable child care on parents.

This analysis was limited by access to data. A more informative study would include individual enrollment data to better understand who takes advantage of the preschool program. To improve an analysis using rescission discontinuity, larger sample sizes and more precise measures of age eligibility such as a child's birthdate would allow more direct comparisons. Additionally, larger sample sizes would allow for the analysis of specific subgroups that may respond differently to access to public preschool. These groups may be based on income level, race, marital status, baseline education level, or occupation. These effects could be compared to fathers, as well as any differences in individuals in same-sex relationships where there is less influence from gender roles on child care responsibilities (Goldberg et al, 2012).

On a basic level, access to affordable child care gives parents time. Employment and education are not necessarily the only way to use this time, and these outcomes do not represent all the changes that could occur in a parent's life. Time-use data is useful to more fully represent how a parent's day changes when they have access to child care. Several outcomes can supplement research already done on employment and education, as well as introduce new interesting variables such as time spent on leisure, volunteering, and personal care.

Given the range of state preschool programs that have been implemented in the past 20 years, the research could compare differences in impact between these. Perhaps certain characteristics of public preschool programs lead to higher impacts for parents (this has already been found to be true for 2-year preschool programs rather than just 1 year).

Finally, there is a need for qualitative research to explain the effect of public preschool on maternal education outcomes in particular, since many factors that influence these decisions are difficult to measure quantitatively.

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Table A.1. *Regression Results for the Rates of Mothers with Bachelor's and Postgraduate Degrees in Washington, DC*

	2011	2012	2013	2014
Bachelor's degree rate	-0.15 (0.10)	0.05 (0.1)	-0.04 (0.09)	0.16 (0.11)
Postgraduate degree rate	-0.17 . (0.10)	-0.02 (0.09)	-0.17 (0.11)	0.10 (0.10)

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

A Additional College Degree Results

A.1 Washington, DC

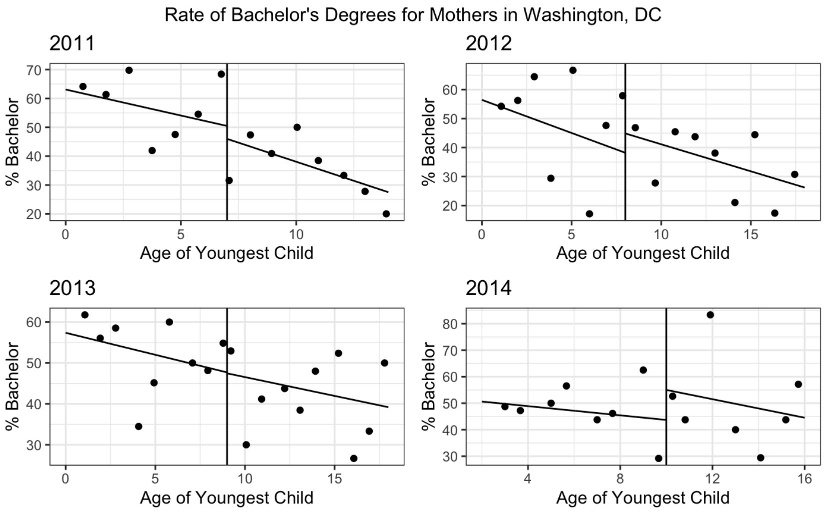

Fig. A.1. These regression discontinuity plots show the percentage of mothers with bachelor's degrees in Washington, DC 2, 3, 4, and 5 years after public preschool was implemented. Mothers on the left of the cutoff point were able to enroll their youngest child in DC's universal public preschool, and mothers on the right did not have access.

A.2 Florida

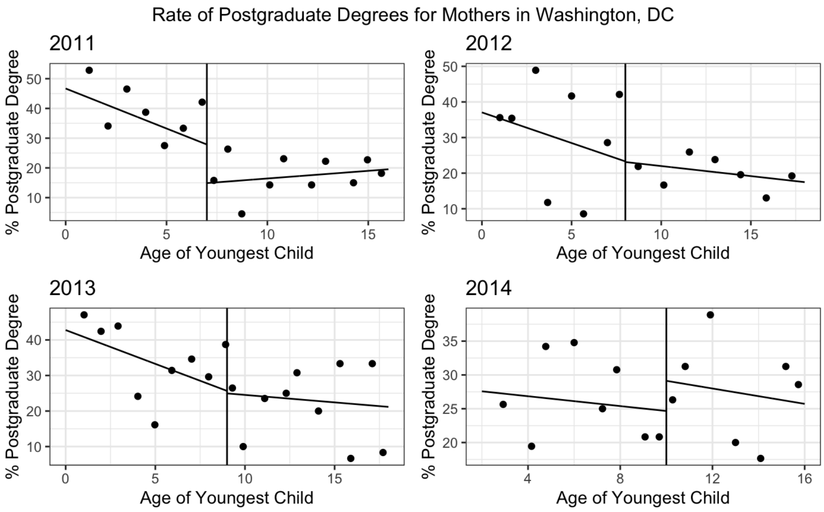

Fig. A.2. These regression discontinuity plots show the percentage of mothers with postgraduate degrees in Washington, DC 2, 3, 4, and 5 years after public preschool was implemented. Mothers on the left of the cutoff point were able to enroll their youngest child in DC's universal public preschool, and mothers on the right did not have access.

Table A.2. *Regression Results for the Rates of Mothers with Bachelor's and Postgraduate Degrees in Florida*

	2007	2008	2009	2010
Bachelor's degree rate	-0.04 * (0.02)	-0.01 (0.02)	0.02 (0.00)	-0.03 (0.00)
Postgraduate degree rate	0.01 (0.01)	-0.02 . (0.01)	0.00 (0.01)	0.00 (0.01)

*Standard error are in parentheses. Significance codes: '***' p = 0.001 '**' p = 0.01 '*' p = 0.05 '.' p = 0.1*

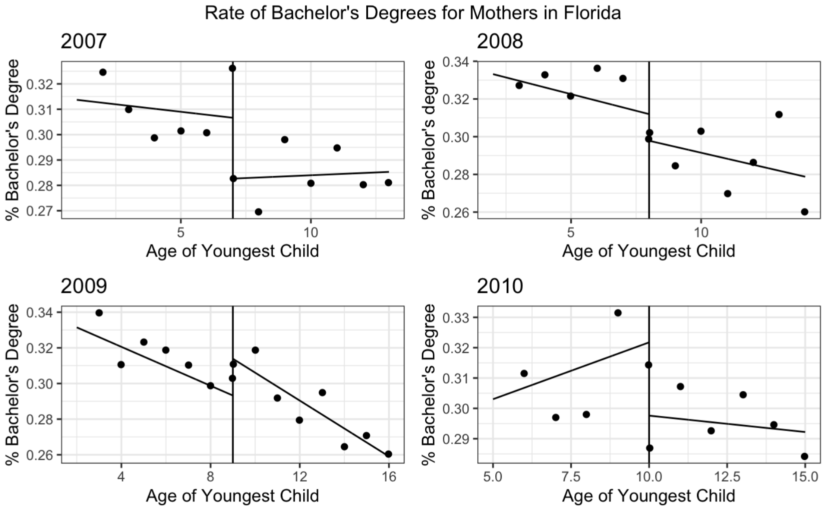

Fig. A.3. These regression discontinuity plots show the percentage of mothers with bachelor's degrees in Washington, DC 2, 3, 4, and 5 years after public preschool was implemented. Mothers on the left of the cutoff point were able to enroll their youngest child in DC's universal public preschool, and mothers on the right did not have access.

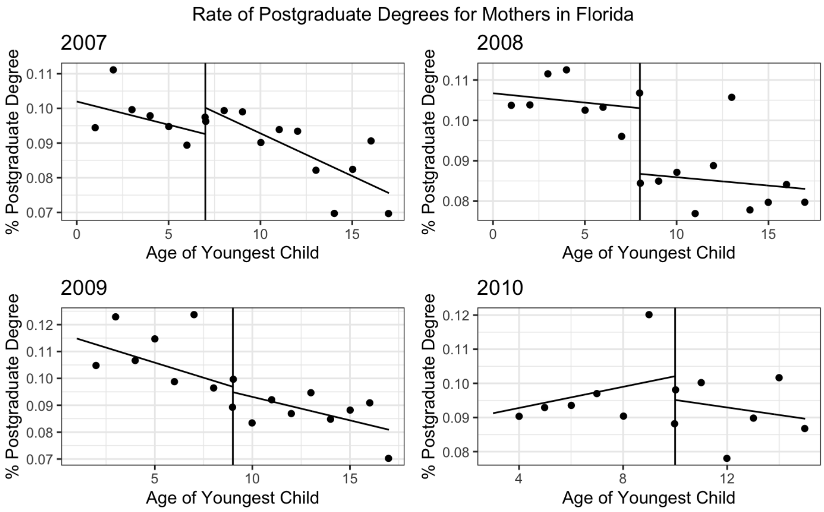

Fig. A.4. These regression discontinuity plots show the percentage of mothers with postgraduate degrees in Washington, DC 2, 3, 4, and 5 years after public preschool was implemented. Mothers on the left of the cutoff point are treated, and mothers on the right are untreated.

B Bandwidth Sensitivity Tests

B.1 Educational Attainment

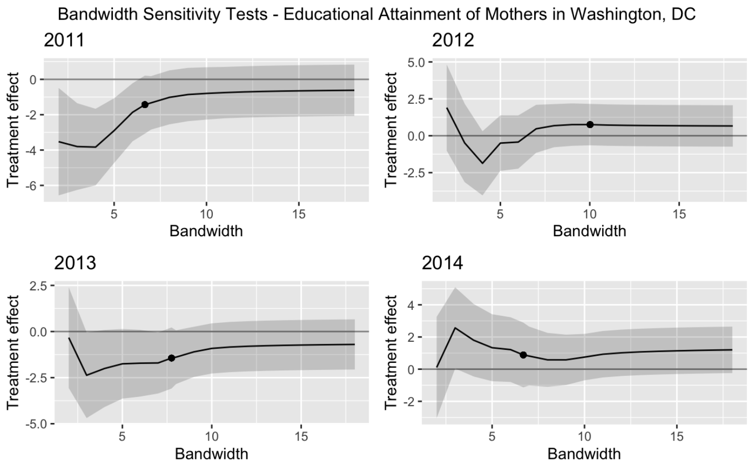

Fig. B.1. Using regression models for educational attainment in Washington, DC, these tests run many regression models with a range of bandwidths, and plot the treatment effects found in each model with 95% confidence intervals. This shows how sensitive a treatment effect is to particular decisions I made in each model. If a treatment effect is driven largely by including or excluding a few outlier points, this test would show that the effect changes significantly between one bandwidth and another. The points on each plot show the chosen bandwidth in the main model used for analysis.

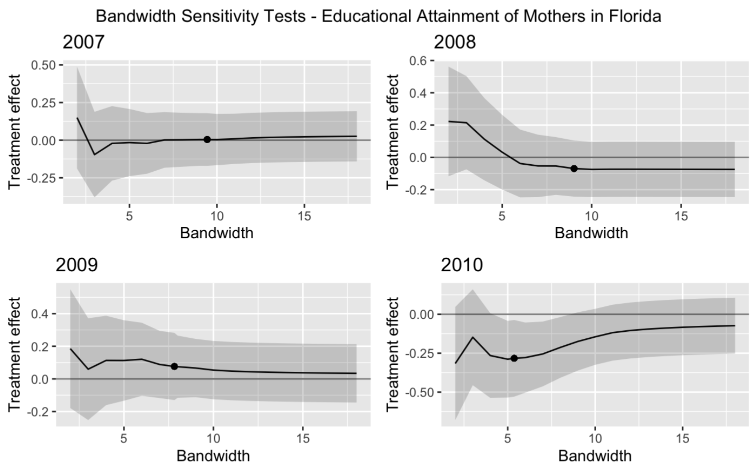

Fig. B.2. These are bandwidth tests of models for educational attainment in Florida. These tests run many regression models with a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth for the model used for analysis.

B.2 College Degrees

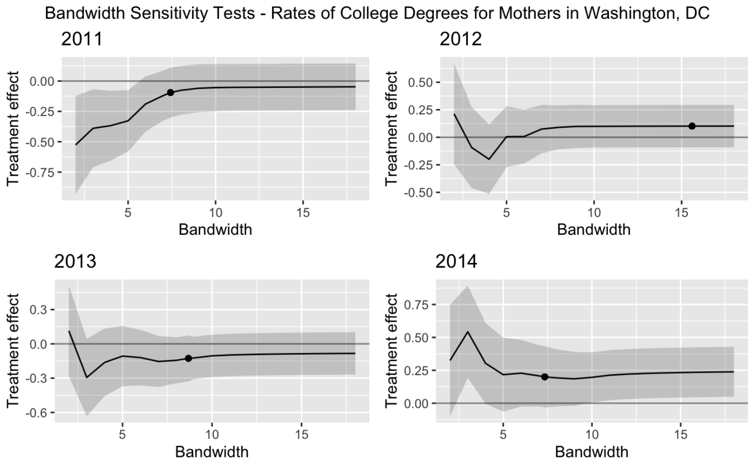

Fig. B.3. These are bandwidth tests for the rate of mothers with college degrees in Washington, DC. These tests run regression models using a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth in the main model used for analysis.

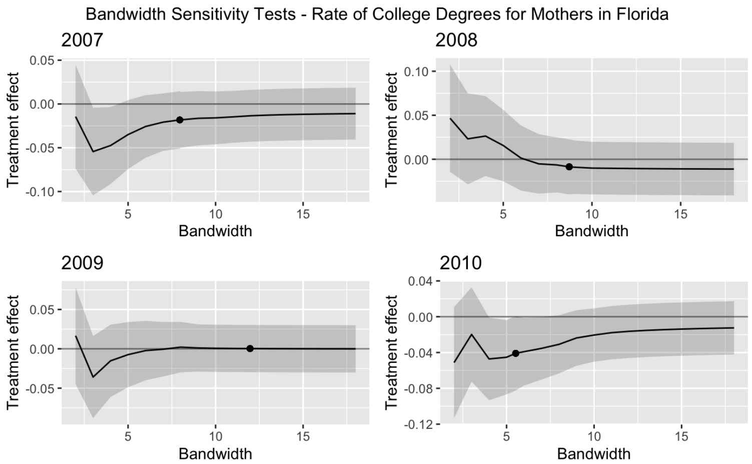

Fig. B.4. These are bandwidth tests for the rate of mothers with college degrees in Florida. These tests run regression models using a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth in the main model used for analysis.

B.3 Bachelor's Degrees

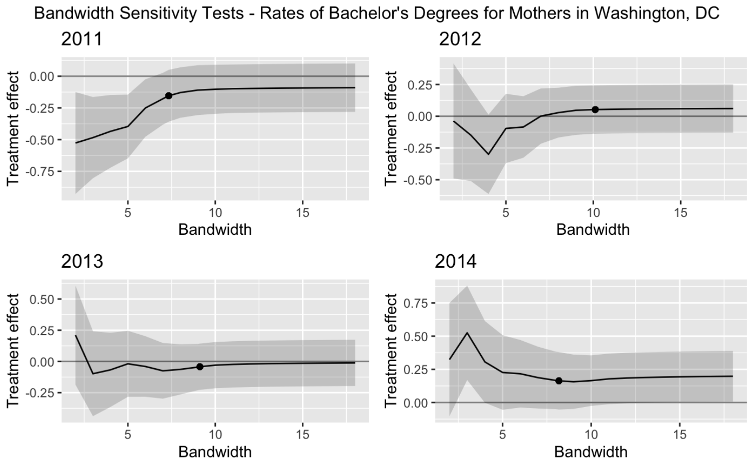

Fig. B.5. These are bandwidth tests for the rate of mothers with bachelor's degrees in Washington, DC. These tests run regression models using a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth in the main model used for analysis.

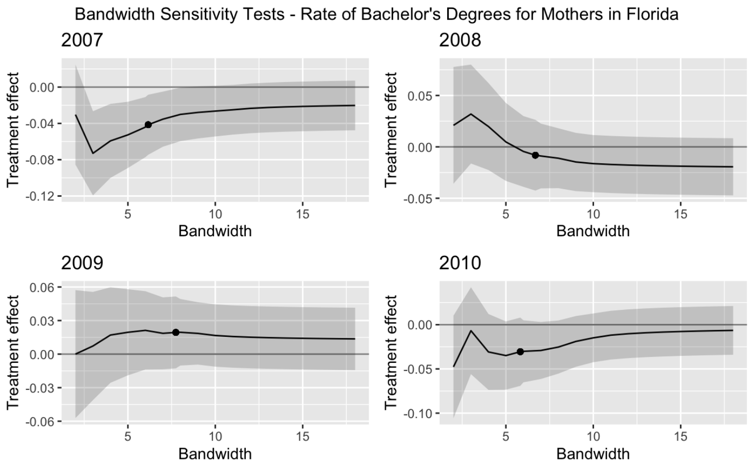

Fig. B.6. These are bandwidth tests for the rate of mothers with bachelor's degrees in Florida. These tests run regression models using a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth in the main model used for analysis.

B.4 Postgraduate Degrees

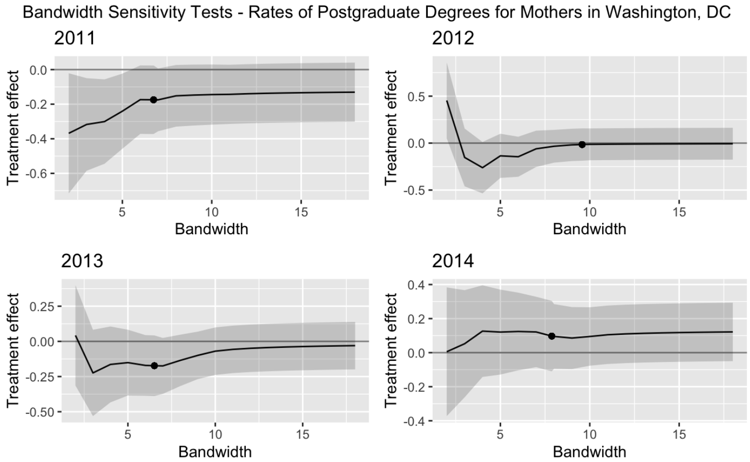

Fig. B.7. These are bandwidth tests for the rate of mothers with postgraduate degrees in Washington, DC. These tests run regression models using a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth in the main model used for analysis.

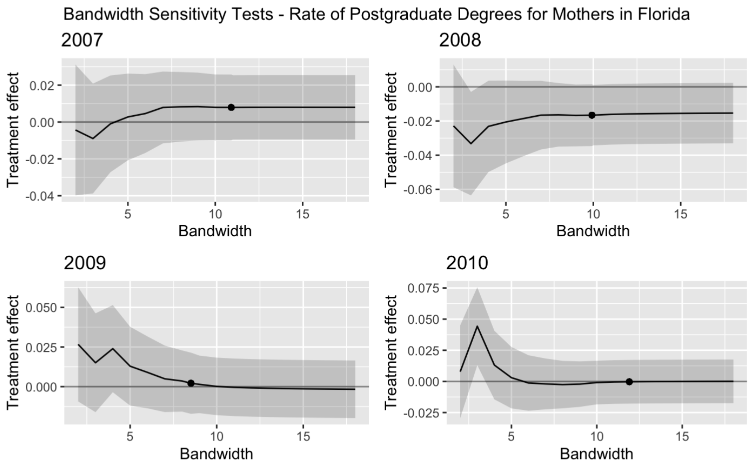

Fig. B.8. These are bandwidth tests for the rate of mothers with postgraduate degrees in Florida. These tests run regression models using a range of bandwidths and plot the treatment effects found in each model with 95% confidence intervals. The points on each plot show the chosen bandwidth in the main model used for analysis.

C Polynomial and Bin Size Tests

C.1 Polynomial Tests

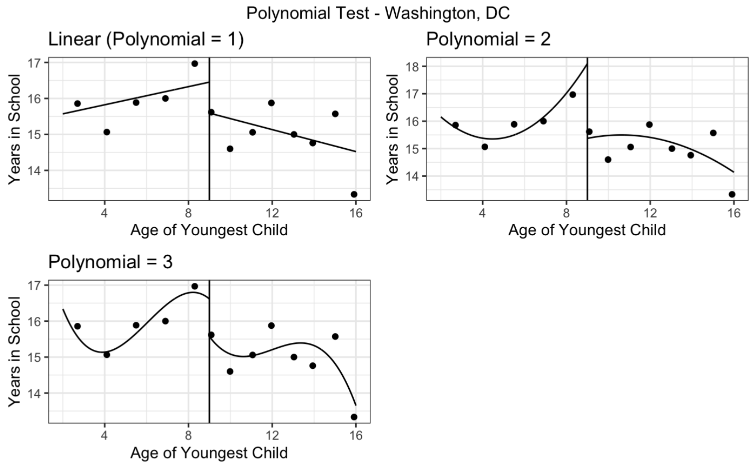

Fig. C.1. Each of these plots run the same regression model testing the impact of access to Washington DC's public preschool on maternal educational attainment in 2013, but the polynomial level varies in each test. This test determines whether my models are sensitive to changes in the polynomial level.

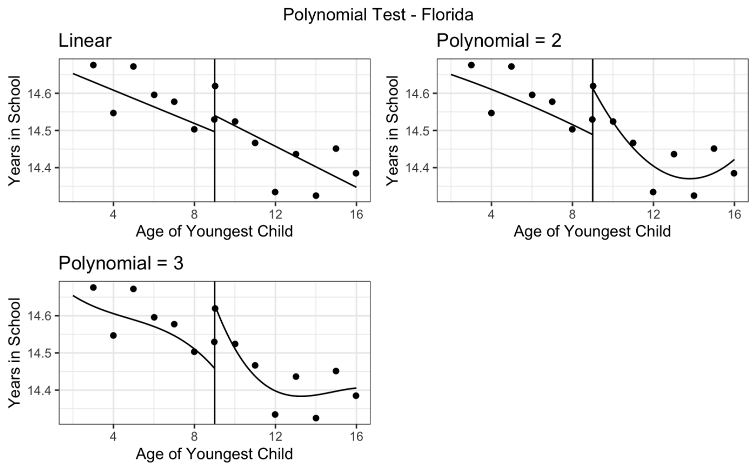

Fig. C.2. Each of these plots run the same regression model testing the impact of access to Florida's public preschool on maternal educational attainment in 2009, but the polynomial level varies in each test. This test determines whether my models are sensitive to changes in the polynomial level.

C.2 Bin Size Tests

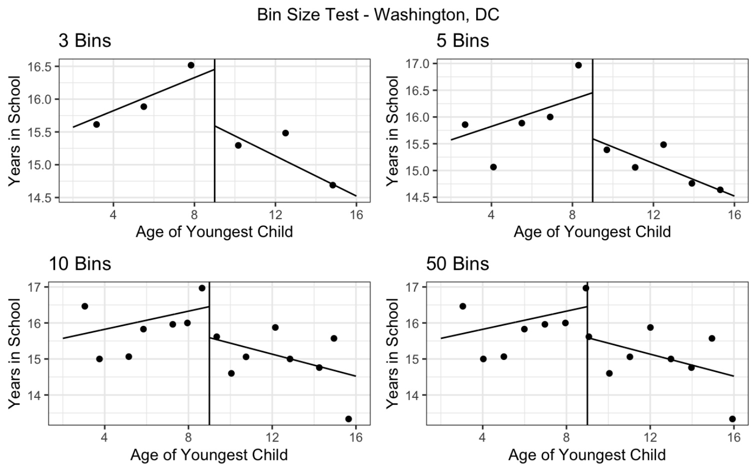

Fig. C.3. Each of these plots run the same regression model testing the impact of access to Washington DC's public preschool on maternal educational attainment in 2013, but the bin size varies in each test. This test determines whether my models are sensitive to changes in the number of bins on either side of the cutoff point.

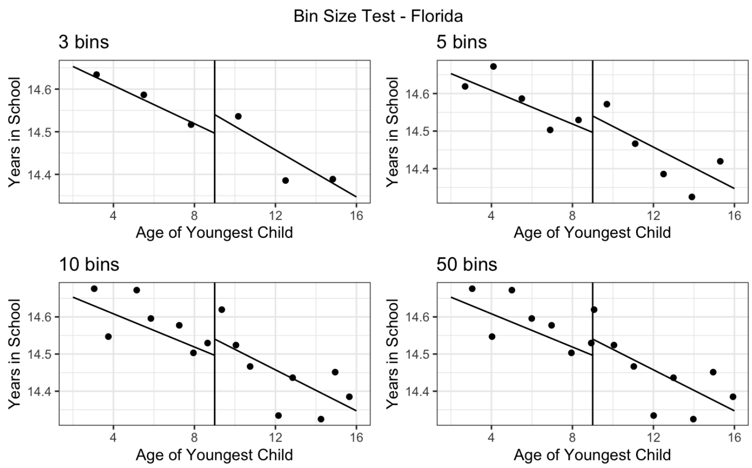

Fig. C.4. Each of these plots run the same regression model testing the impact of access to Florida's public preschool on maternal educational attainment in 2009, but the bin size varies in each test. This test determines whether my models are sensitive to changes in the number of bins on either side of the cutoff point.

D Bandwidth-specific Summary Statistics

Table D.1. *Summary Statistics for Observations Used in Regression Discontinuity Plots, Washington DC*

	2011		2012		2013		2014	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
# Observations	191	159	278	212	189	182	143	114
Average Age (years)	33.8	41.5	33.5	44.9	35.7	44.1	38.3	45.6
Percent married	56.0%	41.5%	57.9%	41.5%	52.9%	45.0%	49.0%	47.9%
Median Household Income	$113,100	$64,300	$88,000	$60,400	$88,000	$68,170	$84,000	$84,350
Average Household Income	$157,155	$129,777	$131,141	$126,921	$158,977	$130,872	$130,917	$134,337
Average Education (years)	16	15.1	15.8	15	15.8	15.4	15.5	15.4
Bachelor's degree	19.9%	23.3%	18.0%	15.1%	19.6%	20.3%	21.7%	20.1%
Postgraduate degree	36.1%	19.5%	31.7%	22.2%	30.7%	25.8%	29.4%	26.4%
Racial Breakdown								
White	47.6%	27.7%	42.4%	24.5%	39.7%	35.2%	38.5%	32.6%
Black/African American	42.9%	65.4%	46.0%	65.6%	51.3%	57.7%	51.7%	59.0%
Asian/Pacific Islander	3.7%	0.6%	4.0%	2.8%	3.0%	3.3%	4.2%	2.8%
Mixed/Other	5.8%	6.2%	7.6%	7.1%	5.8%	3.8%	5.6%	5.6%

Source: American Community Survey, Integrated Public Use Microdata Series

Table D.2. *Summary Statistics for Observations Used in Regression Discontinuity Plots, Florida*

	2007		2008		2009		2010	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
# Observations	9,043	9,531	9,904	9,507	7,611	7,414	4,828	5,639
Average Age (years)	32.7	42.2	33.3	43.3	35.2	43.5	37.8	43.6
Married	70.0%	67.8%	69.1%	67.9%	68.3%	66.1%	66.0%	65.0%
Median Household Income	$60,000	$65,000	$60,000	$66,820	$59,100	$62,450	$57,000	$61,000
Average Household Income	$77,182	$86,892	$79,877	$86,339	$78,391	$82,055	$75,761	$80,775
Average Education (years)	14.5	14.5	14.6	14.5	14.6	14.4	14.5	14.5
Bachelor's degree	21.2%	18.8%	22.0%	19.8%	20.8%	19.7%	21.3%	20.3%
Postgraduate degree	9.8%	9.0%	10.5%	8.5%	10.9%	9.0%	9.8%	9.2%
Racial Breakdown								
White	72.7%	75.7%	74.2%	76.8%	74.5%	76.1%	74.2%	75.9%
Black/African American	15.9%	14.8%	15.7%	14.9%	15.7%	15.6%	16.6%	15.9%
Asian/Pacific Islander	3.7%	3.0%	4.1%	3.4%	4.0%	3.4%	4.0%	3.3%
Mixed/ Other	7.8%	6.5%	6.0%	4.9%	5.9%	5.0%	5.2%	4.8%

Source: American Community Survey, Integrated Public Use Microdata Series

E HCs and LOs

E.1 LOs

E.1.1 #cp193-qualitydelivarables:

My final submission is a complete, polished, professionally formatted research paper. I include all important aspects of a research project, including the relevant sections, and appendixes, and I make my data publicly available on GitHub. For this draft and all previous drafts, I've met all expectations set for them.

E.1.2 #cp193-curation:

I include all relevant aspects of a research paper in this draft. I only discuss the analysis I plan to include in my final draft, even though I've done a lot of other analyses previously. In my paper, I make heavy use of my Appendixes so that I don't include too many figures in the main body. This makes the paper more readable but still gives my readers access to important details. Additionally, all parts of my paper are well-organized. I include a Table of Contents with informative section headings and subheadings, and I separate my supplemental material across multiple Appendixes to make them easier to navigate.

E.1.3 #cp193-navigation:

Throughout the Capstone process, I've maintained consistent communication with my advisor. I scheduled meetings, asked for help and clarification when needed, and followed through on any feedback by the appropriate deadline. I also reached out to my second grader to hear more detailed feedback after my full draft deadline.

E.1.4 #cp193-outcomeanalysis:

I include the appropriate number of HCs and LOs and I provide justifications for all of them, including a specific example for each project LO and HC tag. I chose HCs and LOs that cover the most prominent parts of my project, and I made them central to my process of writing my Capstone.

Project LOs

E.1.5 #ss154-Data

I include a detailed discussion of the data that I use in my analysis, mostly in my data section. I explain the kind of data I am using (cross-sectional) and how this data was collected (US census survey). I explain that I chose to use ACS data because of its large sample size compared to other national surveys that collect similar information, which is useful in a study like mine that focuses on a very specific subset of people. I also discuss the limitations of my data of using cross-sectional data to study educational outcomes that may take a few years to change. Because I am not following the same individuals each year, I can only make claims on an aggregate level. I also note that migration and changing demographics may affect my models.

Additionally, I explain what my dataset covers. I describe the subsample that I use: mothers with children who are residents of DC or Florida during the 5 years after the state's public preschool programs were implemented. Beyond this, I explain the assumptions I made about specific variables that could impact my analysis. For example, on page 9 I explain how I constructed an educational attainment variable from "EDUC" and "EDUCD" in ACS. The original education variables were coded in categories, not in years of schooling. I had to make assumptions to transform some of these categories into years, like assuming that it takes an individual an average amount of time to obtain a college degree. I acknowledge that it's likely that I slightly misrepresented some individuals in my dataset through this process, but I do not have enough information to improve my outcome metric in this case.

Finally, my data and code are accessible on GitHub and linked in this document. Each file is clearly labeled and explained.

E.1.6 #ss154-InterpretingResults

My interpretation of my results is clear and informative. I show an understanding of regression discontinuity design and what it can and cannot tell me about my research question. In my results section, I explain what the treatment effect coefficient means in the context of each outcome variable. I repeat this explanation multiple times because the interpretation is unintuitive: a negative regression result means that there was a positive treatment effect.

To fully and accurately interpret my results, I reference several statistical concepts as well as contextual knowledge about public preschool programs.

For example, on page 16, I interpret the impact of DC's preschool program on mothers' educational attainment. First, I clearly explain what the regression results show the average difference in the years of school between moms whose children were just young enough to enroll compared to moms whose children were slightly too old to enroll, and I note whether each of the treatment effects is statistically significant. Then, I connect the results to the data that the model relies on. I explain that educational attainment is particularly sensitive to differences in rats of postgraduate degrees since an individual with a postgraduate degree is coded to have many more years of schooling than someone without a postgrad degree. This is a potential reason for fluctuations that I identify in the data. I also look at the regression discontinuity plot itself and identify whether there are obvious visual discontinuities that support a strong treatment effect or any outliers that might be biasing the model. Finally, I reference bandwidth sensitivity tests, which test whether the magnitude and significance of the treatment effect are sensitive to changes in the bandwidth. These offer more information on the strength of my results. Overall, I determine that though my regression results suggest some statistically significant results, the quality of my data limits the internal validity of these results.

Later in the section, I compare DC results to Florida, which is much smaller. I use all the statistical tools mentioned above to evaluate Florida but also incorporate the knowledge I gathered in my literature review to hypothesize where these differences could come from: Florida only offers one year of preschool while DC offers two, and Florida's demographics look very different to DC.

E.1.7 #ss154-CausalAssumptions

To answer my research question, I develop a regression discontinuity model. I explain and justify all parts of this model including the regression discontinuity design, using a linear functional form and other specific model specifications, and the assumptions required to use this method.

Regression discontinuity design relies on several assumptions. In my methodology section on page 13, I explain each of these assumptions, why they are important, and whether they apply to my model. The two main assumptions are 1) no manipulation of the running variable and 2) the only discontinuity should be in the outcome variable, not any potential confounding variables.

To address the first assumption, I explain what it may look like in this context. It's difficult to manipulate the age of a child beyond timing their birth. There were perhaps a few years between when the preschool programs in each state were proposed and when they were implemented, but it is unlikely that this sort of "manipulation occurred." If there are a lot of data points around the cutoff point, this sign that this assumption might be violated, but I don't observe this in my models. So I conclude that the first assumption is met.

For the second assumption, I run discontinuity tests on important confounding variables like age, race, income, and marital status. I include these graphs in my methodology section. I also include summary statistics for the treatment and control group in each model in Appendix D. For some years in the Washington DC data, the treatment and control groups are very different from each other. However, all of the confounding variables I tested were continuous at the cutoff point, meaning that my models meet the conditions of this assumption. Still, the difference between the treatment and control groups is important to consider when interpreting my results, and I emphasize this in my results section.

In my methodology section, I explain the assumptions that regression discontinuity design relies on. I describe why they are required for an unbiased result, and I evaluate how they apply in this case. To justify my use of sharp design, I cite take-up rates in DC and Florida, and I cite summary statistics of the treatment and control groups in each model to discuss their similarities and differences. I justify my overall use of the method, but I note important limitations in my data that may bias my results.

E.1.8 #ss154-CausalStudyDeisgn

I use regression discontinuity design, a quasi-experimental method. To do this I take advantage of an arbitrary cutoff point (age eligibility at a particular point in time). This allowed some mothers to have access to the preschool program, while others that were relatively similar did not have access because their child was too old to enroll.

In my introduction, I explain the context around both Florida and DC's preschool programs that make them good candidates for regression discontinuity, specifically that they have relatively high take-up rates compared to other state programs and they both have a strict age-eligibility threshold. I elaborate on this in my methodology section on page 12. First, I explain how I build my model around the age-eligibility threshold. I justify my use of sharp discontinuity design by referencing the take-up rates in each state. I discuss the fact that a fuzzy design might have been ideal, but I don't have the individual enrollment data necessary. So, my results measure the impact of access to public preschool rather than the impact of a mother enrolling her child. Then, I explain every decision I made in my model and test my result's sensitivity to changes in model specifications, including bandwidth, functional form, and bin size. I include plots that show these changes generally have little impact on my results in the appendix.

I also explain how some aspects of the model might bias my results in my limitations section. For example, I don't have exact birthdates for each child, meaning I may be miscategorizing some on either side of the cutoff point. Additionally, a smaller sample size means that I need to have to include moms with children of a wider range of ages. It would be ideal to focus on moms just below and above the cutoff point by a few months, but I sacrifice this potential greater accuracy for a sufficient sample size. I further address how this method could be improved with additional data in the future research section.

E.1.9 #ss154-ExternalValidity

I address external validity in my conclusion section on page 25. First, I explain that a regression discontinuity design only produces a local average treatment effect, which means that the effect only applies to individuals that fall just below and above the cutoff point. So, the treatment effects in this study apply to moms whose youngest child was either just young enough to be eligible for public preschool when it began in DC and Florida or moms whose child was slightly too old. Beyond the methodology, shifting demographics over time limits external validity. Other research has shown that mothers with different demographic characteristics respond to childcare programs differently. Additionally, public preschool programs vary across states. Some offer full-day programs, others only half. Some include the summer, others only run during the school year. Some only accept 4-year-olds, and others allow 3 years old to enroll as well. They may also have different quality standards or eligibility requirements. All of these factors mean that we cannot assume any public preschool program is equivalent treatment.

I identify factors that limit external validity, explain why they have this effect, and I situate my study as one of many that would contribute to an overall understanding of the impact of access to public preschool on maternal educational outcomes. In the future research section, I discuss how studies could expand on my research question to build this understanding.

E.2 HCs

E.2.1 #regression

I construct and interpret regression models that model the relationship between the age of a mother's youngest child and her educational outcomes. The age of a mother's youngest child, my dependent variable, is a proxy for eligibility for public preschool. I justify the relationship between my independent and dependent variables in my introduction and throughout my literature review. In my methodology section on page 12, I construct a regression equation and explain each component. Additionally, I explain my choice to use a linear functional form: it generally fits my data better and research has found that using polynomials in a regression discontinuity design often finds misleading and inaccurate results. Still, I show that my regression is only somewhat sensitive to changes in functional form.

In my results section, I clearly explain what my regression results mean in the context of the outcome variable. For education attainment, the coefficient represents a change in the average number of years of school that an individual has attended. For the rate of college degrees, the coefficient represents a change in the percent of the rate of college degrees. I make sure to include that the differences that the coefficients find between the treatment and control group only apply to data points just below and above the cutoff point, not the population in general.

E.2.2 #significance

I use significant measures in conjunction with regression coefficients, sensitivity tests, and overall data quality to interpret my results holistically. I don't explain the concept of significance in detail because that is not standard practice in research papers, but I do include significance levels in my regression results tables and reference them throughout my analysis. Many of my models find positive or negative treatment effects, but few are statistically significant at a significance level of $p = 0.1$ or lower.

I emphasize this in my results section. For example, the results for educational attainment in Washington, DC fluctuate from one year to another (page 16). The negative results are statistically significant and the positive results are not. I offer explanations for why these treatment effects could occur, but explain that the negative effects are more reliable than positive effects because of their significance level as well as the more obvious visual discontinuity in their plots and stronger bandwidth sensitivity tests (of course still being hesitant to interpret the results too strongly due to the data issues). Here, I acknowledge the importance of statistical significance tests but also don't use them as sole evidence for an effect.

E.2.3 #descriptivestats

I present and discuss relevant descriptive statistics in my paper. Summary statistics about mothers in Washington DC and Florida are particularly important to address because I am using cross-sectional data and not including confounding variables in my models. My analysis assumes that a sample taken 5 years after a preschool program was implemented represents the same group of people as a sample from the year of implementation. So, the treatment effects I find are subject to bias from overall changes in the population over the years of my analysis. I chose to calculate statistics for variables that cover relevant demographic information. I generally use percentages and averages for each variable but also include the median for income since I anticipated that this would include particular strong outliers that would affect the mean.

I include general summary statistics in my data section on pages 12 and 13. I discuss the population shifts of moms in DC and Florida. In DC, they got whiter and richer over time. Meanwhile, the demographics of moms in Florida stayed relatively similar, likely because the sample size is higher. These trends imply that moms surveyed in Florida 5 years after public preschool started are more comparable to those surveyed the year the program began than moms in DC. I use averages to represent aggregate changes in the population, which is what would affect my models. For household income, I also include the median since an average might be affected by particular large outliers in the data. A median offers a more stable statistic, which is useful to understand how much of the fluctuation of the variable is due to larger shifts rather than the introduction of a few outliers.

However, these general statistics aren't enough to understand the kind of data that I use in my models. Because it compares individuals just below and just above the cutoff point, a regression discontinuity design only uses a subset of the data. To focus on the data used in my analysis, I calculate and display summary statistics for just the data within each model's bandwidth (these tables are in Appendix D). This allows me to better compare the treatment and control groups each year, as well as the characteristics of the samples over time. In the methodology section on page 14, I interpret these statistics and write that DC's results are less reliable because the treatment and control groups are significantly different from each other in some years, though this is expected given the larger bandwidths that I use. I also discuss the population trends of this subset over time in my results section on page 17 which differ from the trends in the overall statistics I discuss in my data section.

E.2.4 #composition

I explain complex econometric methods in a clear, concise, and precise way throughout my paper. I focus on this particular in my executive summary, where I explain all important aspects of my paper within one page. When describing study design, I avoid using technical jargon that would overcomplicate my explanation. My sentences are straightforward and structured simply. In my actual paper, I describe all results in a way that connects them to the outcome I am investigating. Rather than using technical regression terms, I make potentially complicated relationships between variables easy to understand. I also focused on doing this in my oral defense, where I conveyed the most important parts of my study in a 10-minute presentation.

E.2.5 #organization

My draft is organized in a way that makes my analysis easy to follow and understand for my reader. The section and subsection titles in my table of contents on page 2 are informative enough for a reader to be able to find any specific information easily. To avoid making the main body of the paper too long, I only include the most relevant figures and tables in it. My appendix is clearly labeled and cited throughout the paper. Finally, using Latex ensures that the formatting of my paper is consistent and navigable.

E.2.6 #professionalism

I formatted my capstone to look like a professional journal article using Latex. I needed to essentially learn how to use Latex from scratch to do this. I follow the necessary guidelines in formatting and ensure that my sections, figures, and appendixes are consistent and easy to understand. I include all important parts of an econometrics research paper including an abstract, introduction, literature review, data and methodology section, results section, limitations, conclusion, future research directions, and an appendix with further results.

I use an appropriate academic tone throughout the paper. I include some technical jargon that I expect my audience (other social science researchers or readers that are generally familiar with econometrics research) to understand, but I still explain these terms when I use them.

For example, in my methodology section on page 13, I write, "I am using a sharp discontinuity design, which means that I assume that all women with a child below the age cutoff are treated and all women with a child above the age cutoff are not treated." It's important to reference the term "sharp discontinuity" because it indicates the kind of analysis I am doing. However, I don't expect all my readers to know what this means, so I offer a short explanation.

E.2.7 #gapanalysis

To provide context for my research question, my literature review is structured as a gap analysis. First, I briefly explain the overall state of research on the impact of childcare programs on parents and children. On page 5, I identify two main gaps in the literature that focused on the impact of these programs on mothers: 1) few studies look at specific state preschool programs, especially given the recent expansion of state preschool programs around the country and 2) most studies focus on employment outcomes and few look at educational outcomes. I cite studies that cover aspects of each gap and explain why they do not sufficiently address the research question that my paper focuses on.

The section reflects my actual research process: as I read the literature on this topic I noted where papers seemed to be lacking and intentionally planned my analysis around these gaps. In my literature review, I show the relevance of my research and how it expands on existing literature in a meaningful way.

E.2.8 #observationalstudy

In the project, I ran an observational study from start to finish. I use econometric methods to find the impact of public preschool on educational attainment, even though I cannot conduct a precise RCT. To do this, I determined a research question, obtained public data, designed a method that allowed me to answer my research question with my data, interpreted my results, and communicated my findings in a professional research paper. In my methodology section on page 12, I explain the underlying principles that my study relies on (the assumptions that make a regression discontinuity design work). I show that my data and case studies work well with the design: the dataset has a relatively high sample size compared to other public data, universal preschool programs mean that I can use a larger population within a state, and DC and Florida programs have high uptake rates, and age-eligibility cutoffs offer an arbitrary point that determines whether an individual receives treatment or not.

E.2.9 #dataviz:

I use many data visualizations in this paper to communicate information about my method and results. These go beyond just my results. For example, on pages 8 and 9 I include plots that show enrollment rates for public preschools in DC and Florida. These are important to understand since my method relies on the assumption that mothers are enrolling their kids in these preschool programs. These plots are easy to understand because of their simple design: I included the enrollment rate above each bar so the reader doesn't have to guess any exact percentages. I don't have an axes label for the y-axis because this information is intuitive and would be repetitive on top of the individual data points above each bar. There aren't too many points, so this choice doesn't make the plot too crowded. The plots have titles, figure captions, and axis labels where necessary, and I explain and interpret the plots in my text.

I also followed the guidelines of data visualization to display my results, which are much more complex figures than the enrollment plots in the earlier section. I needed to ensure that readers who may not have a deep understanding of regression discontinuity design could understand these graphs. To do this, I include informative figures and axis labels, and I write a detailed figure caption that explains how the treatment variable is measured in each model. My x-axis is not standardized around zero because this would've been more confusing for a reader to understand; instead, the x-axis is a variable that a reader is familiar with and can more easily connect to the treatment mechanism.

Additionally, I took extra time to create manual plots that are consistent with the aesthetic of my paper. For the enrollment plots, NIEER already has similar graphs but their color and labeling would not fit with the rest of the paper. Additionally, the bandwidth tests were part of the rddata package and were blue by default. I generated the data using the package and then recreated the output graph in ggplot so that I could edit the colors. This gives my paper a more professional look overall.

Finally, I group many of my plots. For my results, this allows the reader to see changes in the effect over time much more easily. For my bin size, polynomial, and bandwidth tests, these groups make it easy to directly compare the impact of different model specifications.

E.2.10 #variables

I clearly define and explain each variable I use in my models, including my treatment variable (access to public preschool), outcome variable (educational attainment and rate of college degrees), and potential confounding variables. I explain how I use the age of a mother's youngest child as a running variable to determine whether she has access to treatment or not. I cite previous literature and explain more straightforwardly why this makes sense as a measurement of treatment.

In general, I explain the assumptions that I make as I use each variable, and the impact these assumptions have on my analysis. On pages 9 and 10, I show how the way that I defined certain variables might affect my overall analysis. I explain how the assumptions I make do or do not reflect the realities of the overall system that I am studying. For example, when I convert ACS' educational attainment variable into years of schooling, I assume that anyone with a college degree took an average amount of time to receive that degree. This is not necessarily true for every individual—perhaps some did a dual degree program or others took longer than average to receive their degrees (this could be particularly true for mothers who may not be able to attend school full-time). Because I don't have an actual measurement of years of schooling, I acknowledge that my outcome variable could not be completely representative of the actual individuals that I am studying.

E.2.11 #sourcequality:

My paper is built on high-quality sources. In my literature review starting on page 5, I only reference highly cited journal articles. There are countless studies in the field I'm attempting to cover, but I include only the most relevant to provide a well-informed picture of previous literature. I discuss studies that had a large impact on the field and/or are directly related to the question I am asking in my paper. In addition to my literature review, I cite peer-reviewed papers that support choices I make in my methodology, like a method to find an optimal bandwidth and use linear regression. I also use information from well-respected early childhood education research centers to develop context around each case study I look at. These sources provide an important foundation for my study, justifying its relevance and legitimizing the choices I made in my methods.

E.2.12 #responsibility:

Working on this project was a major test of responsibility. Throughout the process, I've worked to stay organized, and communicative, and maintain realistic goals for what I can accomplish. I relied on a lot of planning to do this. I've maintained a notion workspace where I regularly crafted detailed to-do lists that broke down larger goals into smaller tasks. I had a "high priority" section at the top of this workspace, where I set short-term deadlines to motivate myself to keep working on this project consistently. Meeting with my advisor regularly was extremely helpful for this, since I tried to set actionable goals that I would accomplish before each meeting. I knew telling my advisor that I was committing to these smaller goals every two weeks or so would ensure that I felt pressure to do them on time. Finally, for each major draft deadline, I understood what was expected of me, and I submitted high-quality drafts that met these expectations. When I could not meet some expectations, I communicated any changes in the plan ahead of time.

E.2.13 #designthinking:

As I designed my study, I iterated on my methods and analysis based on feedback from my advisor, as well as some trial and error. I've tried many more methods than are included in this paper using a variety of datasets.

I began my process knowing that I generally wanted to do a policy analysis around the effect of childcare policies on mothers. First, I read dozens of studies in this area and noted potential gaps in the research I could explore. For each potential research question, I thought through the characteristics that an ideal dataset would need to have to answer it. As I explored publicly-available data (and unsuccessfully tried to access some restricted data), I couldn't find these ideal datasets. So I adjusted my approach— instead of thinking of a research question and then exploring whether the data can answer it, I looked at the data first and crafted my question based on the data that I actually had. For example, I knew that the majority of papers I'd read focused on employment outcomes, so I looked for other relevant outcome variables that are available in ACS data.

I tried multiple methods, datasets, and policies before I settled on a regression discontinuity design using ACS data. I considered the following alternative options to this capstone (in addition to many smaller ideas):

- 1. Using longitudinal data — was not able to access important restricted variables (the state that an individual lived in)
- 2. Using ATUS (American Time Use Survey) as my outcome variable— the sample sizes were too small for the specific populations I wanted to study.
- 3. Studying a national policy, such as the CCDF (child care development fund)— this policy is extremely complex and varies significantly by states
- 4. Difference-in-difference with ACS data— when I restricted the data only to mothers I wanted to study, the aggregate statistics did not meet the parallel trends assumption

My approach was to try methods early. Even if I wasn't sure whether a method or dataset would be ideal, I created prototypes (basic versions of an analysis). This would expose any particularly impactful limitations and meant I could adjust to these problems sooner than if I had waited until I felt completely confident in a method. An iterative approach meant that I ended up with a stronger methodology than I would've otherwise. Of course, if I had more time, I would keep this iterative process going and come up with an even better one!

E.2.14 #confidence:

I display an appropriate level of confidence in this paper. I position myself as a very knowledgable source on the topic in my introduction and literature review, but I back up all my claims with evidence from other reputable sources. In other sections, I am explicit about what my methodology and data can and cannot answer about my research question. Throughout my results section, I qualify all my findings by mentioning the limitations that could bias my models, and I expand on these in my limitations section. In general, I am not overselling my results. Finally, I theorize about my findings in my discussion section on page 23. I show that theories are supported by previous literature, but I make it clear that my explanations are not necessarily accurate. I use words and phrases like "may", "it is possible that...", and "it is likely that..." to avoid making overly strong claims.

E.2.15 #casestudy:

In this study, I use two case studies: Washington DC, and Florida. I explain their context in the introduction sections, and I justify their use in the methodology section. They are both universal public preschool programs, which are a type of program that is increasing in popularity around the US. The universal aspect also means that I can use the entire population of mothers with preschool-aged children in my analysis, which increases my sample size. These two programs also have relatively high take-up rates. The fact that people use the program makes it more likely that an impact is due to the actual policy. Additionally, public preschool programs have an age-eligibility threshold, which is a variable that's difficult to manipulate beyond timing the birth of a child. This means that treatment is assigned arbitrarily (probably). I look at two case studies because I can make slightly broader claims from these results. These case studies are "typical" examples of a state public preschool program, but emphasize the differences between them in my analysis. In general, childcare policies vary widely between states so research that focuses on differences in impact, not just the impact of one program, is useful in building an understanding of what factors might lead to a higher impact.

In my conclusion section on page 25, I discuss the generalizability of these case studies. I do not consider these externally valid because previous literature and my results suggest that different populations respond differently to these programs, and slightly different characteristics in these programs lead to very different effects. Even though these are not necessarily externally valid, I justify the use of case studies as a useful way to study public preschool programs because 1) that's the only way we have for now, and 2) future programs will look very different in each state and it's important to research many different types of policies in this area to build a holistic understanding of their general impact.

F AI Use

I did not use AI in any part of my Capstone, except for a portion of my title (as suggested by Professor Hadavand).