Three Essays on Labor Economics

A Dissertation
submitted to the Faculty of the
Graduate School of Arts and Sciences
of Georgetown University
in partial fulfillment of the requirements for the
degree of
Doctor of Philosophy
in Economics

By

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Washington, DC
May 18, 2016

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Abstract

Chapter 1 investigates the labor market implications of single fatherhood. Samples from the Panel Study of Income Dynamics reveal that men experiencing single fatherhood after marital separation experience dramatic decreases in both labor hours and income. I identify the effects of marital separation for men with and without children and show that the decrease in income and hours for single fathers goes beyond those experienced by other separated men, likely due to the additional constraints on time and scheduling faced by single fathers.

Chapter 2 estimates the effects of recent half day state Pre-Kindergarten programs on school attendance and mothers' labor supply. I show that state Pre-K programs have succeeded in increasing school attendance for some 4-5 year olds, and in this way have succeeded in their stated goal. However, I show that the introduction of these programs is also associated with decreased labor hours for mothers of eligible children.I show that the decrease in labor hours can be explained by the fixed costs of childcare which cause may cause some mothers to decrease work from full-time hours to avoid having to pay for and arrange supplementary childcare after Pre-K has ended.

Chapter 3 analyses the effectiveness of the National Science & Mathematics Access to Retain Talent Grant (SMART) in its goal of increasing the science and math preparation of college graduates. Although estimates are limited by a small sample size and imperfect observations of SMART eligibility, I do not find evidence that program eligibility increases the number of declared science, technology, engineering, and mathematics (STEM) majors.

Index words: Labor Supply, Marital Separation, Parenting, Family, Education

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Chapter 1

Parental Duties, Labor Market Behavior, and Single Fatherhood in America

1.1 Introduction

The delay of marriage coupled with increased legal protection for children with unmarried parents has greatly increased the proportion of children born out of wedlock (Stevenson and Wolfers 2007). For married couples, divorce increased rapidly in the 1970s due to many factors including social change and the adoption of unilateral and no-fault divorce laws (Wolfers 2006). Although mothers typically assumed custody of children after divorce, states began to move away from explicit preference of mothers in the 1960s with most states explicitly stating preference for joint custody by the 1980s (Nunley 2011). As a result, more children with separated parents began to live with their father at least some of the time.

Single fatherhood has become increasingly prevalent totaling 3 million fathers as of 2010 (compared to 10.5 million single mothers) (see Figure (A.1). Moreover, this group shows important differences from other men. Single fathers work fewer hours, earn lower wages, and perform much more housework when compared to married fathers and unmarried non-fathers of similar age and education (see Tables (A.2)-(A.4)).

This paper seeks to determine the effects of sole parenting on men's labor market outcomes. This is important for two reasons: the career interruptions experienced by single fathers are concerning because of their potential for lasting effects on men's career opportunities and earnings. Moreover, a large body of research has established the importance of family income on child outcomes. If sole parenting puts a considerable strain on men's earnings this could imply decreased educational and career opportunities for their children (Carneiro 2003). Single fathers may also provide evidence of the effects of changes to parenting status for all families, especially since transitions into single father status are unlikely to be anticipated from a young age when key education and career decisions are made.

I first create a profile of single fathers in the United States using recent cross sections from the American Communities Survey (ACS). This shows how single fathers differ from other men in their labor market preparation, behavior, and outcomes. Compared to married fathers, single fathers are most often selected from the economically disadvantaged as evidenced by their lower educational attainment. Moreover, single fathers work fewer hours and earn less per hour than married fathers in all age and education groups. I also use samples from the Panel Study of Income Dynamics (PSID). Even controlling for individual differences, sole parenting is associated with significant changes for men entering into single fatherhood through martial separation. These "separated fathers" experience a sizable decrease in income and wages and an increase in weekly hours of housework.

To quantify the observed differences between separated fathers and other men I estimate the effects of single fatherhood by comparing outcomes of single fathers to outcomes of the same men in periods before and after single fatherhood, as well as to other men with similar age, education, etc. The effect of single fatherhood is thus defined as the additional effect of marital separation on men living with children after separation. The observed changes exceed what would be predicted by either marital separation or increased housework. Single fathers are also more likely to change industry or career and are less likely to work full time, which suggest that men may select into more flexible (but less lucrative) careers during years of sole parenting. Although marital separation is likely a traumatic experience, their lost labor income is not counteracted by increases in other family income and does not appear to be due to decreases in either physical or mental health status.

1.2 Related Literature

Household division of labor and its implications for labor market outcomes was first analyzed theoretically in Becker (1973). Becker showed that, due to gains from specialization, small differences in comparative advantage between market and home productivity can lead to large differences in the behavior of married men and women. This model suggests that marriage (and children) should increase specialization causing men's labor hours to increase while women's fall (with the opposite effect on home production). If human capital accumulates on the job, this could lead to increased market productivity and thus increased wage rates for men. Empirical studies support this specialization story as marriage appears to have zero or negative effect on female wages (Korenman and Neumark 1992) and a positive effect on male wages (Korenman and Neumark 1991). Children also seem to increase this specialization as the advent of children is associated with decreases in female wages (Korenman and Neumark 1992) and hours (Bianchi 2000, Angrist 1998), and increases in male wages and hours (Lundberg and Rose 2002). As one would expect, marital separation has the opposite effect of marriage on specialization. Empirical work confirms that females work more after divorce while males work less (Mueller 2005).

Becker (1985) offers an alternate explanation for the changes in hours and wages of married men and women. Becker suggests that productivity (and thus wages) may be hurt by housework due to its exhausting nature. If married women perform more housework due to comparative advantage, the fatiguing nature of housework may suppress their market wage (and thus hours). Although most empirical work on housework suggests no effect on male wages, Bryan (2011) shows that male wages are negatively affected by housework in the UK (see Maani and Cruickshank (2010) for a survey of the housework literature). Other research has suggested that flexible hours, which may be necessary for primary caretakers, may also have a negative effect on earnings (Flabbi 2012).

The effects of single parenting have been well-detailed from the perspective of single mothers. Because single-motherhood is common among the poor, much of the work on single mothers focuses on topics such as incidence of poverty, welfare burden, and response to relevant policy reforms (for instance see Meyer and Sullivan 2008, Card and Blank 2008, Mammen 2008, and many others).

Although the effect of children have been well studied for other groups single fathers provide unique evidence on the effects of changes to parenting status. Although natural experiments (such as Angrist 1998) may provide evidence of the effect of changes in number of children, men and women choose education and careers with expectations regarding their future roles as both parent and provider. Similarly, many children are born out of wedlock and mothers most often assume custody after marital separation so single motherhood may be anticipated as a possible outcome for many mothers. Because single fatherhood is comparatively less common, men may not anticipate their future role as single father. For this reason, the changes experienced by single fathers may be more likely due to changes in parenting status rather than the result of years of decisions leading to an anticipated parenting status.

The closest work to what follows is Brown (2000) which profiles the American single father population using CPS data from the 1980s and 1990s. This study, however, shows only general patterns without pursuing any causal link between single fatherhood and outcomes. Another similar work is Lin and Chen (2006). This paper shows that a group they define as "custodial fathers" work more hours than married fathers. This study is over 10 years old and does not explicitly define any causal effects.[1] Lerman and Sorensen (2000) shows that the earnings of non-custodial fathers increase with contact and participation in the lives of their children.[2] Unlike these papers I will explicitly seek causal effects using recent longitudinal data, and will analyze single fatherhood due to spousal death.

1.3 Cross Section Findings

To provide a cohesive profile of the current economic status of single fathers, I present evidence combining the 1950-2000 Census with the American Community Survey 2011 1% Sample.[3] 'Married father' will be defined as married male living with at least one own-child less than 18 years old. 'Single father' will be defined as an unmarried male living with at least one own-child.[4] Note that neither includes non-custodial fathers, i.e. men who have fathered children living elsewhere. I will also distinguish between 'never married' fathers and 'separated fathers'. Never married fathers are men living with own children before their first marriage - these men may later marry, but at

[1] 'Custodial father' is a term specific to their paper, meaning that a child lives in the home but at least one of the child's parents lives elsewhere.

[2] The focal group of this paper is men with children living outside their household.

[3] Data assembled using Integrated Public Use Microdata Series (IPUMS), Ruggles et. al (2010) Detailed information on household members no longer appear in decennial censuses beginning in 2010, so use of equivalent ACS data is necessary.

[4] Residence in Census data is determined by where child lives majority of the time. Residence in ACS samples is determined by where child is staying at time of interview. Child residency may be distinct from legal child custody. Since the focus of this paper is on parenting and domestic responsibilities this abstraction seems inconsequential. Also, "own child" in household may be step or adopted but possible distinctions between biological, step, or adopted children will be ignored.

time of single fatherhood their marital status is ‘single, never married’. Separated fathers consist of divorced, separated, or widowed men living with own children, i.e. ‘previously married fathers’ or ‘separated fathers’. Although both groups are charged with sole parenting the two groups differ in many ways.

First, note that as of 2010 there were about 3 million single fathers (never married and separated combined), compared to 10.5 million single mothers, (see Figure A.1). This is equal to about 3% of the male population aged 18-65 and 4% of male heads-of-household (for females about 10% and 15%). This reflects a sizable increase in single fathers considering that they totaled less than 1% of adult males in years 1950-1970. Of custodial parents (i.e. those living with own children less than 18), single fathers represent 11% of all fathers (30% for mothers). This number has shown an especially sharp increase for men starting in 1970. The increase for both genders since 1960 corresponds to the rapid increase in both out-of-wedlock births and divorce rates throughout this period.

In 2010 about half of single fathers report being separated or divorced, 3% are widowers, and the rest are ‘never married’ (see Table A.1). These totals, however, reflect only current marital status - fathers once divorced or separated, for instance, cannot be identified after remarriage.[5] The inability to identify men who previously reported single fatherhood partly motivates the use of panel data in later sections.

Single fathers differ significantly from married fathers in their workforce preparation and participation. Single fathers have lower educational attainment than their married counterparts (see Table A.2). Around 25.5% of never married fathers and 14% of previously married fathers have no high school diploma, compared to 11.7% for married fathers. Only 7.4% of never married and 20.2% of previously married

[5] About 1% of fathers report status as ‘married, spouse absent’; I group these men with married fathers because they are still married. This group may include couples who interact very little, but their circumstances are likely varied and cannot be determined.

fathers report holding a 4-year college degree, compared to 36.0% among married fathers. Never married (and previously married) single fathers report higher rates for both unemployed and not-in-labor force: 14.1% (9.4%) and 11.9% (11.9%) vs. 5.0% and 5.5% for married fathers. Among those who work, never married fathers report roughly 335 hours fewer work hours per year then married fathers; previously married report 150 fewer hours than married fathers.[6]

With lower education and less time at work it is not surprising that single fathers report lower earnings on average.[7] Approximately 23.7% of never married and 11.8% of previously married single father report income below the poverty level, compared to 7.1% of those headed by a married father (see Table A.2). Single fathers' annual incomes are lower on average as well. Married fathers report an average labor income of approximately $62,511 while single fathers earn only $28,844 if never married or $46,959 if previously married (see Table A.3).[8] Single fathers report lower wage rates as well (See Table A.4). The average never married father earns about $16.46 per hour and previously married fathers earn $24.08 per hour, both falling short of the $29.19 per hour earned by married fathers.

Table A.5 shows the coefficients of regressing hours, labor income, and wage rate on single fatherhood (both never married and previously married) for the population of all ACS males living with children under 18. Covariates control for indicators of each year of age, three education groups (no diploma, high school diploma, or at least 4-year college degree), indicators for each state, race (indicator for 'white'), and

[6]Hours per week (and thus year) must be roughly approximated because ACS only reports hour data in intervals. Numbers assigned are the middle of each reported interval. Although exact hour differences cannot be determined, single fathers report lower-hour intervals with higher frequency than married fathers.

[7]As measured by wage income.

[8]Married fathers also earn more across all age and education groups when comparing total incomes instead of just labor. These figures are available upon request.

number of young children (age 5 years or less). Compared to other fathers, never married fathers earn about \$8560 less per year (about 26%). Separated fatherhood is associated with a loss of about \$9590 (20%) annually. Some of this stems from fewer work hours: never married fathers work almost 296 fewer hours per year and separated fathers work about 246 hours fewer. The remaining difference stems from lower wage rates: never married fathers earn about \$2.11 less per hour or 13.5% less and separated fathers earn about \$2.21 less per hour (9.0%).

Current ACS data shows clear differences between single and married fathers. Most importantly single fathers report less education, fewer work hours, lower earnings, and a lower wage rate. This information obviously does not establish causality between single fatherhood and lower economic outcomes because the two populations are different in many ways, both observable and unobservable. The sections below continue with regression analysis using panel data to better address these issues.

1.4 Panel Data Description

For further analysis I will use the Panel Study of Income Dynamics (PSID). Surveys were conducted annually 1968-1997 and biannually 1999-2009. I make use of all years, but restrict the sample to male heads of household aged 18-65. After these exclusions I retain a sample of 143,416 observations over 16,281 unique individuals (see Table A.6).

I continue to distinguish between married, never married, and separated single fathers.[9] Single fathers without missing wage information total 3,888 observations over 2,999 individuals (31,581 observations including those for years before or after

[9]Note that the PSID categorizes couples living together for multiple years as married. For this reason the 'Married' population will also include cohabiting couples that are not legally married.

single fatherhood). Information on housework hours is not available for 1982, so all observations from this year will be excluded from specifications including housework.[10]

The outcomes on which I focus will be annual work hours, weekly hours of housework, the log of hourly wage rate, and log of annual labor income which should capture any major labor market changes associated with single fatherhood. I will also consider fulltime status, to examine how many single fathers experience large decreases in labor hours.[11] All dollars are converted to 2009 values using Bureau of Labor Statistics Consumer Price Index (CPI) data.[12] I top-code income data to 99th percentile values for each year to limit the effect of extreme outliers. To maximize sample size, I define wage rate as annual wage income divided by labor hours instead of relying reported wage rates, which have limited availability. Defining wage rate in this way introduces measurement error which can be problematic in specific contexts, e.g. when estimating the wage elasticity of labor supply (see Borjas 1980). Since men's wage rate and hours will not appear together in any estimates, this measurement error should not introduce bias.

Table A.7 shows comparisons similar to those of the previous section using a 2009 cross section of the PSID data (only one year is used to abstract from possible year effects). Labor force participation is very high for all men, so censoring is not likely to bias results (since hours and income are only available for those with nonzero labor hours). Married fathers are also much more likely to work full time. Single fathers report lower income, lower wage rate, and fewer work hours as seen in the ACS data. Single fathers also report lower education levels.

[10] To allow for maximum sample size, I report estimates for individuals from all PSID subsamples (Core SRC, Immigrant, Core SEO, and Latino). Similar results hold throughout using only the nationally representative core subsample and are available upon request.

[11] I define fulltime status as an indicator variable for 1680 or more hours of work per year.

[12] ftp://ftp.bls.gov/pub/special.requests/cpi/cpiai.txt

Utilizing all years sampled in the PSID, Table A.8 shows regression coefficients for single fatherhood on labor outcomes controlling for age and year (with indicators for each year and year of age), education groups (no diploma, high school diploma, or at least 4-year college degree), state, and race (indicator for white). As in the ACS data, both never married and separated fatherhood is associated with lower likelihood of fulltime work, fewer work hours, a lower wage rate, and lower annual labor income. The differences are larger in magnitude in the PSID than in the 2011 ACS data, possibly due to the inclusion of observations across many additional years. Single fathers perform more hours of housework per week; although some additional time spent on household duties is unsurprising, it is worth noting that separated fathers perform almost 4 additional hours per week. This is sizable considering that married fathers perform only 10 hours of housework per week on average. Moreover, these additional hours may be an underestimate because the PSID housework question, as it is phrased, does not explicitly include child care time (Keith and Malone 2005).

Next, I will consider year-by-year changes in the focal variables before and after single fatherhood begins. Figure A.2 shows regression coefficients associated with years before and after separated fatherhood begins, conditioned on individual fixed effects.[13] Indicators show the different outcomes associated with 4-5 years before single fatherhood, 2-3 years before, 1 year before or just beginning single father, 1-2 years after, ..., 9-10 years after, and 10 or more years after separated fatherhood.[14] Figure A.3 shows similar year-to-year transitions for the never-married father population.

[13]Similar patterns are apparent looking only at averages by year. Looking instead at regression coefficients allows us to abstract from potential age and year effects

[14]More than 5 years before separation is excluded to avoid collinearity. Indicators are also added to control for remarriage and children leaving the household after the beginning of single fatherhood (e.g. maturing or leaving to live with another relative).

Adding year-to-year effects reveals dynamic changes in all four focal variables as the men enter separated fatherhood (year zero). Many fathers appear to leave fulltime status as sole parenting begins, but most return within 6 years. Wage rate falls by 10% initially and does not appear to recover. Labor hours fall by 200 as sole parenting begins and recovers quickly - no statistical difference remains after 4 years. The combined decrease in wage rate and hours cause a lasting effect on labor income (not pictured); income falls by 20% as separated fatherhood begins, and a difference of 15-20% persists in all remaining observed years. Housework rises by around 2.5 hours a week when separated fatherhood begins and slowly falls - a difference around 1.5 hours per week is present 6-10 years after separated fatherhood begins.

The results for never-married fathers are strikingly different. Housework hours increase slightly as men enter into single fatherhood, but few significant changes are present in their labor market variables. Although the lack of significant changes is surprising, especially compared to the large changes seen in separated fathers, this closely resembles patterns seen among teen mothers as detailed by Kearney & Levine (2012). They find that teen mothers are selected from the very poor, but that teen mothers experience similar economic outcomes as peers choosing to delay childbirth. Teen motherhood is only appealing to those with the lowest opportunity costs, therefore it does not significantly interrupt the careers of its affected population. In this way, never married fatherhood may have little apparent effect precisely because it is most common among those with little opportunity for higher education or career advancement.

Some of the changes experienced by separated fathers may be explained by marital separation in itself. For comparison, Figure A.4 compares the year-to-year effects of marital separation on two groups: separated fathers and men experiencing marital separation but not separated fatherhood. The effects on wage rate are almost indistinguishable; separated fathers' wage rates seem to drop more quickly at first but there are no significant differences in any years. Labor hours fall less sharply in later years for the separated fathers. In fact, labor hours seem to almost completely recover for separated fathers. The coefficients related to housework show some difference as well - separated fathers show a larger increase around marital separation but the two trends become similar after 4 years. All in all, the four trends around marital separation are quite similar across the two groups. Moreover, what differences appear suggest the single fathers fair better after marital separation - their hours and income seem to recover more quickly, despite its initial decreases.

Exploiting the longitudinal dimension of the PSID sample reveals some important patterns. First, examining year-to-year differences in labor market outcomes reveals that very few significant labor market changes occur during periods of never married fatherhood. However, separated fathers undergo significant changes in wage, income, hours, and housework even after controlling for individual fixed effects. Examining year-to-year effects shows lasting changes to income and hours and wages, as well as a sizable increase in housework at the beginning of single fatherhood. The next section will explicitly seek to identify effect of single parenting on separated fathers.[15]

1.5 The Effects of Single Fatherhood

Now I will explicitly seek to identify the effects of single fatherhood on fulltime status, annual labor hours, log of labor income, log of wage rate, and weekly hours of housework. My approach is to estimate a series of equations where I capture single fatherhood as the effect of having children in the household after marital separation.

[15] Because never married fathers experience few labor market changes, analysis below will focus on outcomes for separated fathers. Similar estimates were performed for the never married father population with few significant results.

First assume that outcomes are determined by equations:

$$Y_{it} = \alpha_0 + \beta_0 X_{it} + \beta_1 D_{it}^{SepFather} + \beta_2 D_{it}^{Post-SF-R} + \beta_3 D_{it}^{Post-SF-M} + u_{it} \tag{1.1}$$

$$u_{it} = \gamma_i + \tau_t + \epsilon_{it} \tag{1.2}$$

Here Y_{it} is the dependent variable for individual 'i' in year 't', $D_{it}^{SepFather}$ indicates separated father status, $D_{it}^{Post-SF-R}$ and $D_{it}^{Post-SF-M}$ indicate years after single father status has ended (either due to remarriage or children no longer in household respectively), and X_{it} is a matrix of relevant covariates. Relevant covariates include a 'Kids' indicator for one or more children in the household, and indicators for each year of age, as well as log of wage in the hours equation. I assume that the error term is composed of γ_i which reflects unobserved individual characteristics, τ_t the time varying component common to all individuals, and random error ϵ_{it}. Unobserved individual characteristics include such things as ambition, skill, and affinity for market work and housework which might influence the dependent variables; these are assumed to be time-invariant. Although ability and ambition may change every year for each individual, this would be impossible to estimate as the number of covariates would exceed the number of observations. There may be annual effects as well due to data differences, business cycles, or technological changes influencing work or housework; these are assumed to be the same for all individuals.

In equation 1.1 the effect of single fatherhood is identified as a difference-in-difference between separated and married fathers. This estimate is credible if one assumes that married fathers are the relevant comparison group for separated fathers. One limitation to this estimates is that previous research has shown that men's labor hours and wage rates fall after marital separation (regardless of single father status). For this reason, a more conservative estimate might be the additional effect of marital separation for separated fathers. Adding indicator variables for separated and never married marital status, the effect of single fatherhood is determined by difference-in-difference-in-difference estimation of the form:

$$Y_{it} = \alpha_0 + \beta_0 X_{it} + \beta_1 D_{it}^{SepFather} + \beta_2 D_{it}^{Separated} + \beta_3 D_{it}^{NeverMarried} + \beta_4 D_{it}^{Post-SF-R} + \beta_5 D_{it}^{Post-SF-M} + u_{it} \tag{1.3}$$

Here $D_{it}^{SepFather}$ reflects separated fatherhood, $D_{it}^{Separated}$ indicates divorced, separated, and widowed, $D_{it}^{NeverMarried}$ indicates no married years observed in or before year t, and X_{it} is a matrix of relevant covariates. Marital separation may be associated with changes in the dependent variables regardless of single parent status, but this should be captured by the 'Separated' indicator. Although men experiencing single fatherhood may be characterized by lower economic status, this is captured by the individual fixed effects.

The previous section showed evidence supporting the idea that marital separation may have both anticipatory and lasting effects on the variables of interest. To consider the possible influence of these year-by-year effects, I will also present estimation including covariates controlling for dynamic effects of marital separation. These effects may differ by individual, but this cannot be estimated (the number of coefficients estimated would exceed observations). Instead one might assume that the effects of marital separation are distinct for two groups - separated fathers and other separated men. This possibility was investigated in Figure A.2. The trends in changes associated with marital separation suggest that the effects are quite similar for the two groups, except for the larger effect of marital separation beginning 4 years after separation on hours and income for separated men who do not experience sole parenting. Combining the effects of marital separation for these two groups might, therefore, overstate the effect of marital separation on separated fathers. This is not problematic; because separated fatherhood is defined as the additional effect of sole parenting for separated men, overstating the effect of marital separation would only lead to underestimation of the effect of single fatherhood.[16]

Controlling for dynamic effects before and after marital separation with year indicators leads to:

$$Y_{it} = \alpha_0 + \beta_0 X_{it} + \beta_1 D_{it}^{SepFather} + \beta_2 D_{it}^{Separated} + \beta_3 D_{it}^{NeverMarried} + \\ \beta_4 D_{it}^{Post-SF-R} + \beta_5 D_{it}^{Post-SF-NC} + \sum_{j=1}^{9} \lambda_j D_{SepYearGroup_j} + u_{it} \tag{1.4}$$

Here all variables are defined exactly as in equations (1.1) and (3.2) which the exception of year-groups. As in the previous section, years around marital separation are grouped as follows: 4-5 years before single fatherhood, 2-3 years before, 1 year before or just beginning single father, 1-2 years after, ..., 9-10 years after, and 10 or more years after separated fatherhood (more than 5 years before separation is the excluded group).

Estimation of equations 1.1 - 1.4 yield the effect of separated fatherhood conditioned on observable differences such as age and marital status, as well as unobservable time invariant individual differences. The identifying assumption is that single fatherhood does not occur concurrently with important unobserved changes at the individual level. This is problematic if, for instance, men are more likely to become single fathers during periods of low individual wages (perhaps due to lower opportunity costs of child care during these years). To address this possibility I will also show results for the effect of separated fatherhood on widower-fathers alone. Widowers are an interesting subpopulation for many reasons. First, death is used in many studies

[16] Including different dynamic effects for the two groups is also feasible, with identification stemming from differences in years spent in separated father state. The results are very similar, but interpretation is not as straight forward.

as an exogenous shock because it is less likely to be correlated with omitted characteristics than divorce (e.g. Corak 2001). Although spousal death may be related to socio-economic status, the longitudinal structure of the PSID allows me to limit this bias through use of individual fixed effects (as in Fronstin et al 2001 and many others). Using widowers also allows me to abstract from custody decisions because widowers almost always retain custody after spousal death. The yearly change in number of household children for widowers (-0.1 children) is not significantly different from married fathers (+0.09 children).[17] Sole parenting for widowers allows us to abstract from the choice elements of both marital separation and child custody, allowing more plausible identification of the causal effects of single fatherhood on labor outcomes.

Although spousal death is less likely to be correlated with unobserved characteristics, this approach has some disadvantages. First, spousal death may still be correlated with unobservable individual income variation (although this is less likely to be the case than with divorce). As before, this will be limited by the DIDID approach which will condition all estimates on widower status. Moreover, since widowers tend to be older and economically disadvantaged, the effect on widowers may be different from other men. This would be particularly concerning if estimates using the widower subpopulation differ substantially from estimates using all separated fathers. Also, focusing on the widower subpopulation necessitates small sample sizes and thus less precise estimates.

Lastly, spousal death likely causes a variety of changes beyond fatherhood and the labor market. Apparent labor market changes for widowers may be the result of many things including changes in physical and emotional health, household size, etc. Because single fatherhood is defined as the additional effect of spousal death for men

[17] Some difference here is expected; married men are likely to father new children while new widowers are not.

with kids, this will be partly ameliorated by conditioning on spousal death. Estimates for the effect of single fatherhood will therefore only be biased if spousal death is more traumatic for men with children. While differences in the effect of spousal death for men with and without children is possible, I will show evidence that suggests the health status, other income, family size, etc. do not appear to explain the changes observed, at least as measured in the PSID surveys.

1.6 Results

Difference-in-Difference estimates reveal that separated fathers experience significant decreases in wages and hours at the beginning of single father status, as well a significant increases in housework hours (see Table A.9). The effects of single fatherhood are significant at the 0.1% confidence level for fulltime status, log of income, log of labor hours, log of wage rate, and housework hours, as identified by the 2540 single fathers with at least one observation outside single father status (2582 for housework). On average separated fathers work 268 hours less per year, are 15.8% less likely to work full time, earn 14.5% less per hour and 33.1% less per year. This is accompanied by an increase in housework of 3.5 hours per week - a sizable difference considering the average married father performs only 10 hours of housework per week.

Difference-in-Difference-in-Difference estimates show the effect of single fatherhood that cannot be explained by marital separation (see Table A.10). The effects here are quite similar to those found above. Single fathers experience a 24.9% decrease in annual income during their single fatherhood years. This is largely due to reduction in labor hours, as they work 197 fewer hours in each single father year. Single father years are also typified by a large increase in housework, as they perform an additional 2.3 hours of chores per week. Interestingly, all of these effects appear to persist after remarriage. This suggests that less specialization may occur in single fathers' second marriages (perhaps due to increased ability to perform household chores, or decreased confidence in the permanence of marriage). Table A.10 also shows estimation adding in year-to-year changes around year of marital separation, and the results are similar.

I next estimate the effects of separated fatherhood on widowers only. The results in Table A.11 show results without dynamic separation effects. These effects are identified by the 313 men with observations both during and outside widower-fatherhood (326 for housework). The effect of widowed fatherhood is similar to that seen in previous specifications with all separated fathers with increases in housework and large decreases in work and income. Widower fathers are 4.7% less likely to work fulltime, which is significant at the 5% confidence level. Widower fathers also report 87 fewer hours per year, significant at the 5% confidence level. Widowed fathers experience a decrease in income similar to that of other separated fathers, significant at the 10% confidence level. Repeating these estimates including dynamic separation effects for widowers does not change the results. Although custodial fathers after divorce may represent a select group of divorced men, this selection cannot explain the appearance of similar results among widower-fathers.

Using a similar approach, Table A.12 shows that separated fathers are more likely to experience a variety of career changes as single fatherhood begins. Although career flexibility is difficult to measure, results above show that these men are much less likely to work full time hours. Moreover, single fathers are 4.7% more likely to have to miss work due to a sick child, significant at the .1% confidence level. Separated fathers are also about 10% more likely to change occupation or industry during their years of sole parenting. This increase in job turnover continues after remarriage which suggests persistence in career volatility. Widower fathers are also significantly more likely to change industry or report missed work.

One concern is that selection out of separated father status may be non-random due to remarriage. If men with highest earning potential and least emotional distress remarry more easily, we will observe more years of single fatherhood for those with the lowest earning. Table A.13 repeats estimation using only the first year of single fatherhood. Assuming widowers remain single for two years, this estimate should provide evidence of initial effects without any effects contaminated by remarriage. Estimates for all five focal variables are at least as large as in estimates using all observed years, which suggests that nonrandom remarriage cannot explain the apparent effects of single fatherhood.[18]

It may seem reasonable to attribute the apparent labor market changes to the increase in housework at the onset of single fatherhood. To control for this possibility I add housework hours as an additional explanatory variable in the estimates of Table A.13. The coefficient for housework here must be viewed critically because housework hours is likely to be endogenous. Although housework does appear to have a significant negative effect on both income and labor hours, the coefficients for separated fatherhood are largely unchanged.[19]

If single father households receive significantly more non-labor income, their decreased hours may be a rational response to the additional cash received. To establish whether loss of labor income was counteracted by increases in other income, Table A.14 show results for Total Family Income, Total Family Income Per Person,

[18]Separated fathers may also have higher attrition than other men. Focusing on the first year of single fatherhood will also limit any bias due to men leaving the sample after single fatherhood begins.

[19]Although the PSID may underestimate housework hours by not explicitly including childcare time (Keith 2005), separate analysis using cross sections from the American Time Use Survey (ATUS) shows that although single fathers perform more housework then other men, these differences do not exceed what is found using the PSID.

and Lump Sum Payments.[20] Results indicate that widower fathers are more likely to receive lump sum payments, and widower fathers receive on average an additional $2785. Moreover, widower father households are typified by higher family income but lower income per person. This is most likely due to some fathers moving in with relatives, therefore increasing both income and size. Since family income per person falls, it does not appear that decreases in labor income are counteracted by additional money from lump sum payments or income from other family members. Similar results hold throughout using the sample of all separated fathers. Moreover, including inheritances or total family income (excluding labor income) as additional explanatory variables does not significantly influence coefficients for the effect of single fatherhood on labor outcomes.[21]

We may also be concerned that single fatherhood is associated with declining physical and/or emotional health which may adversely influence labor outcomes. Results in Table A.15 show no statistically significant differences in physical or emotional health associated with separated fatherhood. Unsurprisingly, separated men are more likely to have been diagnosed with emotional and psychiatric problems (3.2%) likely due to the very damaging nature of marital separation (or possibly due to the difficulty in maintaining one's marriage during periods of mental/emotional problems).

[20] PSID defines 'family units' broadly, including all persons who are pooling income or otherwise economically integrated and living within a household. Lump sum payment data includes an indicator for whether lump sums were received in the previous year, and the total value of all such payments for all family members from sources such as insurance payouts and inheritances. Information on whether any lump sum income was received is available in all years except 1985. Cash values (which are adjusted to 2009 dollars) are available in all years except 1969-1972 as well as 1979.

[21] Estimates of men's wage elasticity to other income received are typically very small as well, so it would be surprising if differences in non-labor income were large enough to explain the decreased labor income for separated fathers (Bargain, 2014)

Note also that separated fathers are not more likely to smoke or drink, which are health risks and may serve as coping mechanisms in periods of high stress.[22]

The PSID also features questions about overall mental distress in years 2001, 2003, 2007, and 2009. To determine levels of sadness, for instance, individuals were asked "In the past 30 days, about how often did you feel so sad nothing could cheer you up?" Similar questions followed to assess degrees of nervousness, restlessness, hopelessness, worthlessness, and to what extent "everything was an effort."[23] Estimates in Table A.16 show that separated fathers are slightly more likely to indicate elevated levels in all of the six measure of mental distress. Effects are identified by the 900 men with observations in the appropriate years. Most strikingly, separated fatherhood is associated with a 5.7% increase in frequency of elevated 'nervousness' which is significant at the 0.1% level; effects on the other indicators are insignificant at the 5% level. Results weakly support the claim that marital separation is associated with small increases in mental distress for separated fathers. That said, increases in mental distress are very small and could also be attributed to the additional stress of sole parenting rather than the marital separation itself.

[22] Overall health status was assessed in all survey years from 1984-2009. Participants were asked "Would you say your health in general is excellent, very good, good, fair, or poor?" I convert this into a binary measure with '1' indicating 'excellent', 'very good' or 'good' and '0' indicating 'fair' or 'poor'. Individuals were also asked about emotional/psychiatric problems biannually from 1999. Participants were asked "Has a doctor ever told you that you have or had any of the following– any emotional, nervous, or psychiatric problems?" with accepted answers either 'yes' or 'no'. Questions about smoking and drinking habits were asked biannually from 1999-2009. Participants were asked "Do you smoke cigarettes?" and "Do you ever drink any alcoholic beverages?" and accepted answers included either 'yes' or 'no' for both questions. Similar effects are also found using the sample of all separated fathers.

[23] Answers were selected from 'all of the time', 'most of the time', 'some of the time', 'a little of the time', or 'none of the time.' I convert these to a binary response categorizing 'all of the time' or 'most of the time' as '1', and other responses as '0'.

I will conclude with Tables A.17-A.21, showing how the effects of single fatherhood change by subgroup. For simplicity I show results only for log of wage income throughout.[24] Single fatherhood appears to have a smaller for men after 1994. This could be due to decreased welfare benefits following "Personal Responsibility and Work Opportunity Reconciliation Act of 1996" or to changes in household's typical division of labor through time. Single fatherhood appears to have strongest effects on those beginning single fatherhood before the age of 30; this may suggest that the additional responsibilities of single fatherhood are more of a career obstacle to the young. The effects of single fatherhood are also strongest for men with 4+ years of college - possibly because the careers of more educated men are more significantly interrupted by sole parenting. The effects of single fatherhood are also stronger for those with 1 or more small children, which require more direct childcare time and thus may be more disruptive to men's careers. Looking at division of labor before beginning single fatherhood, the largest income losses are experienced by those previously married to women performing high levels of housework and/or few labor hours. This may suggest that sole parenting is more of a shock to men less accustomed to housework before separation, although this could be due to differences in education / socio-economic status.

1.7 Conclusions

Single fathers are less common than single mothers but represent a large and growing population. Analyzing the large cross sections available in the American Communi-

[24]Single fathers are grouped by characteristics. Men with no single father years observed are included in all groups for comparison. Subsets for age, starting year, education, and presence of small children are based on conditions during first year of single fatherhood. Wives' labor and housework hours are based on conditions 2 years before first year of single fatherhood.

ties Survey reveals a variety of differences between single fathers and their married counterparts: single fathers are generally less educated, less attached to the work force, and have lower labor income, hours, and wages. Similar patterns are found in the PSID data which, despite smaller sample size, allows for longitudinal analysis. Longitudinal analysis with the PSID data suggests that the effects of single fatherhood differ by type. Never married fatherhood is more common among the poor, but seems to have little effect on wages, hours, or earnings after controlling for individual effects. During their years of sole parenting, separated fathers show substantially reduced hours, income, and (in some specifications) wages. Moreover, the labor market changes go beyond what can be explained by marital separation or housework increases alone, and similar results hold using the widower subpopulation. Because many men change occupation & industry and switch to part time work status as single fatherhood begins, this suggests that sole parenting may cause men to switch to more flexible but less lucrative careers.

This provides preliminary evidence for patterns which have not yet been reported in the literature. Further study of this group is of importance due to the observed decreases in their income which are quite large and appear to persist beyond initial years of sole parenting. Poor economic outcomes for these men are particularly concerning because they are charged with raising children alone, and these income decreases are likely to have real effects on the development and education of their children. Moreover, although much work has studied the effect of parenting on women (including plausible natural experiments), the career outcomes of mothers are influenced by a lifetime of career and education decisions. Single fathers, however, are unlikely to anticipate their future role as parent & provider, so their outcomes provide unique evidence of the effect of unanticipated changes to parent status.

More detailed modeling of work-housework decision process would be beneficial, particularly considering the effects of housework in more complex models of promotion with human capital. Because of gender differences in typical household responsibilities, a model allowing for lingering effects of domestic responsibilities on wages may help to explain phenomena such as persistence of the gender wage gap.

Chapter 2

Reexamining State Pre-K Programs: Enrollment and Mothers' Labor Supply

2.1 Introduction

Increased financial support for public preschool (Pre-K) has been championed by many, including President Obama who stated during his 2013 State of the Union Address, "I propose working with states to make high-quality preschool available to every single child in America."[1] Although it is not a stated objective of the programs, some also argue that Pre-K allows poor mothers to return to work. For example, The Center for American Progress argues "these programs provide important benefits to working parents, especially working mothers ... often left to choose between the lesser of two evils: low-quality care or forgoing needed pay to stay at home and care for a child themselves."[2,3] This paper provides evidence that although recent half day UPK programs are associated with small increases in school attendance, they appear to have decreased the labor supply of mothers with eligible children.

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[1] `https://www.whitehouse.gov/the-press-office/2013/02/12/remarks-president-state-union-address`

[2] www.americanprogress.org/issues/education/report/2013/05/08/62519/the-importance-of-preschool-and-child-care-for-working-mothers

[3] Many shared similar sentiments in regards to recent Pre-K expansion efforts in NYC. For instance, Congresswoman Yvette Clarke stated, "These programs will assist parents who want to work but had been unable to afford the high cost of child care" `http://www1.nyc.gov/office-of-the-mayor/news/174-15/pre-k-all-22-000-families-apply-pre-k-first-day#/0`

The NIEER defines preschool (Pre-K) as programs for pre-kindergarten age children with primary focus of child (not parent) education, with group learning experience for children two or more days per week, and not primarily designed to serve children with disabilities (NIEER, 2003). Public Pre-K exists in all but ten states,[4] but eligibility and hours differ greatly from state to state. This paper will examine the effect of changes in Pre-K availability on child school attendance and mothers' labor force outcomes, with focus on recent Pre-K expansion in Iowa, Florida, and Vermont. Each of these states has increased Pre-K enrollment by more than 50% of state 4-year-olds since 2002 and offers primarily half-day schooling (about 3 hours per day). Although previous research has focused on full day Pre-K programs, these programs are only available in 2 of the 8 states enrolling at least 50% of state 4-year-olds[5] and only 2 of the 8 states that have increased enrollment by at least 25% of state 4-year-olds since 2002.[6] Some states have expanded by gradually adding classrooms at the town and district level, but seven have grown through statewide UPK initiatives guaranteeing funding for all Pre-K aged children.[7] These UPK initiatives cause a sudden surge in school enrollment by eligible children with little change in the school attendance of other children or work behavior of their parents. Variation caused by the introduction of UPK in Florida, Iowa, and Vermont will be used to investigate the effects of Pre-K enrollment in this paper.

Unsurprisingly, I find that UPK initiatives appear associated with increases in school attendance by eligible children. However, there appears to be no effect on school attendance for children of unmarried mothers. This result may be explained by their

[4]HI, ID, IN, MS, MT, ND, NH, SD, UT, and WY

[5]Full day Pre-K being 6 hours or more. Pre-k in Georgia is uniformly full-day while Oklahoma offers a mixture of full-day and half-day by district.

[6]AR and LA

[7]FL, GA, IA, NY, OK, VT, and WV

eligibility for pre-existing income-based programs (like Head Start and targeted state Pre-K programs). I also find that mothers' work hours decrease with the adoption of UPK. Married mothers work about one hour less per week on average after states introduce UPK initiatives. This reduction in hours appears mostly due to decreases in full and near-full time employment. No similar changes are witnessed for mothers with children that are not of Pre-K age, and effects are much stronger during the school year (not summer months) which suggests that observed changes in mother's work hours cannot be attributed to regional or state-level economics changes.

Decreases in mothers' labor supply with Pre-K entitlement can be explained by a static labor supply model, introducing fixed costs for both labor force entry and the purchase of a uniform childcare commodity. To use childcare services mothers must find suitable childcare, evaluate its quality, and then arrange daily transportation. After receiving half day Pre-K, high wage mothers find it optimal to arrange half-day childcare to supplement half-day Pre-K and continue working full time. Women with lower wages, however, may reduce their work to occur only during Pre-K hours to avoid arranging and paying for supplementary childcare. Accordingly some mothers may decrease work hours after introduction of universal half day pre-k. Examination of data from the Survey of Income and Program Participation (SIPP) Childcare Supplement confirms that women are more likely to care for their own children after Pre-K enlargement, and are less likely to use other forms of childcare. This suggests that the observed reduction in labor hours may be due to incentive for some women to care for children themselves rather than find supplementary childcare after the introduction of universal half day pre-k.

2.2 Related Literature

There is substantial research on the effects of early childhood education. Two of the most studied interventions include the Carolina Abecedarian Project and the Perry Preschool Project, based on evidence using randomized controlled trials. The Perry Preschool program, taking place in Michigan in the 1960s, has been linked to a myriad of long term benefits including increased educational attainment as well as earnings, and decreases in welfare receipt and criminal activity. Heckman (2011) suggests that the long run rate of return for preschool investment may be as high as 10%. Conversely, the 1972 Abecedarian participants have higher educational attainment but show no changes in either income or criminal activity (Campbell, 2012). More recent programs allow for analysis of larger-scale interventions. For instance, Currie's survey (2001) finds Head Start participants better prepared for later schooling, leading to less special education and grade repetition. Deming (2009) finds that Head Start also increases the adult educational outcomes. Research on UPK, more specifically, has shown that it may increase kindergarten readiness, especially for children of disadvantaged backgrounds (e.g. Gormley et al 2005, Wong et al 2008). Fitzpatrick (2008), however, suggests that these effects do not persist; Georgia's UPK program improved 4th grade test scores only for disadvantaged youths in rural areas, while other populations saw no long term improvements after gaining public Pre-K eligibility.

Other work investigates the effects of school expansion for young children on mothers' labor supply. Gelbach (2002) show that female labor supply increased after the introduction of state kindergarten programs, with mothers increasing labor supply by at least 6%. Cascio (2009) finds that single mothers are 6.9% more likely to work after the introduction of public Kindergarten. Subsidized Pre-k in Quebec was found to significantly increase mothers' labor supply by around 8% (Lefebvre and Merrigan

2005, Baker et. al 2009). Similarly, Bauernschustera and Schlotterd (2015) show that German public childcare is associated with higher mothers' labor supply. Other papers have examined such topics as the political economy of state Pre-K expansion (Kahn and Barron 2015) and childcare market implications of state Pre-K (Bassok et al. 2014).

The two papers most related to what follows are Fitzpatrick (2010) and Sall (2014), both examining specifically the effects of public Pre-K on mothers' labor supply. Most importantly, the expansions studied in Fitzpatrick and Sall have included mostly states / counties offering full day pre-k. The labor force effects of Pre-K expansion may differ greatly for half-day programs due to childcare fixed costs, as explained in the next section. Both papers also examine the results from Pre-K expansion at an earlier time period and the effects of more recent Pre-K expansion efforts may differ for many reasons. For instance, changes in alternative childcare options, women's labor force behavior, and other cultural changes may cause the effects of modern Pre-K expansions to differ from what was found in the earlier reforms analyzed by Fitzpatrick and Sall. This paper also investigates the effect of Pre-K programs using alternate estimation technique.

Fitzpatrick (2010) uses regression discontinuity (RD) methods to show that although UPK eligibility increases preschool enrollment by about 14% in Georgia and Oklahoma, there is little effect on female labor supply. Fitzpatrick (2010) relies on RD methods for identification which, despite intuitive appeal, may provide biased results. Children are eligible for Pre-K only if they are 4-years old before their states' cutoff date, typically a day in August or September. Although some children eligible for Pre-K each year are born 1 week (or less) earlier than others which are not yet eligible, sufficient sample size requires comparisons across a much wider window. Fitzpatrick (2010) expands the treatment group to those born up to 100 days before eligible date, and compares them to those born up to 100 days too late - effectively using a sort of birth quartile instrument. Recent work (Buckles and Hungerman, 2013) shows that quarter-of-birth are likely related to mothers' labor outcomes. Children born in winter months are more likely to have mothers that are teenagers, unmarried, and have not received a high school diploma. Although these differences may be mitigated via differencing by state, these birth quartile effects may vary between states.

Sall (2014) showed state Pre-K programs associated with large increases in mothers' labor supply using difference-in-difference by county. Sall measures Pre-K expansion via percentage of schools offering Pre-K (rather than actual Pre-K enrollment or eligibility). Although number of schools offering Pre-K within a county may be correlated with attendance this introduces measurement error. Endogeneity of Pre-K may also be troublesome in Sall (2014) because county Pre-K funding is likely influenced by local economic conditions (with more prosperous towns likely to introduce Pre-K classrooms). I address this by focusing on the effect of statewide UPK reforms that are explicitly educational in focus.

2.3 Static Labor Supply With Pre-K and Fixed Costs

Consider the following model demonstrating two possible effects of free Pre-K given labor supply with childcare with fixed costs. Start with a standard static labor supply model (e.g. Pencavel 1986). I add fixed costs to labor force entry, for which empirical support abounds starting with Cogan (1981); these fixed costs are due to necessary non-marginal costs of work including job search, transit, negotiation, training, etc. Unlike Cogan's work, I also add a fixed cost of childcare due to obstacles such as child-care search and transit. The existence of childcare fixed costs is consistent with higher estimated labor fixed costs for women with children. I will also assume decreasing marginal cost of childcare, because childcare cost surveys suggest that hourly costs of childcare decrease in weekly hours.[8]

Assume mothers have 16 awake hours to allocate every day to leisure (L), or childcare (E), or work (H) at wage W_i.[9] To allow nonzero work hours, mothers must purchase D hours of childcare from the market for $f(D)$ and/or partake in free Pre-K (K) if available. Mothers' then maximize utility

$$U(C, L) = \frac{C^{1-\gamma} L^{1-\delta}}{(1-\gamma)(1-\delta)} \tag{2.1}$$

s.t.

$$C + f(D) < R + W * H - a_1 * I(H > 0) \tag{2.2}$$

$$E + H + L = 16 \tag{2.3}$$

$$E + D + K = 8 \tag{2.4}$$

where C is consumption, a_1 is fixed cost of work due to transportation etc., δ and γ determine elasticity of consumption versus leisure, and (2.2)-(2.4) are income, hours, and childcare constraint respectively.

Maximization yields first order conditions: $W = f'(D)$, and $W = C/L = MU_L/MU_C$. Although transfers with high cash value may cause changes in work hours via income effects, I will assume no changes in the marginal utility of consumption versus leisure with state Pre-K eligibility. This is due to both the low

[8]For example see New York State OCFS (2014) pages 14-24.

[9]I will assume mothers do not perform childcare during leisure hours. If mothers could perform childcare during leisure hours utility would be maximized only if mothers performed leisure during all childcare hours. This could be prevented by designating "active" and "passive" childcare, the first of which could not be performed during leisure. Alternately, childcare could be reduced in quality if done during leisure hours. Because the intent of this model is only to show possible incentives to decrease work given free half-day childcare I will avoid these additional complications by assuming all hours are single purpose.

average value of state Pre-K entitlement, as well as the lack of income effects found in full day Pre-K programs which are far more generous. Note, however, that $W_i * H - a_1 > f(D)$ is a necessary condition for work. To make clear distinctions between not working, part time employment, and full time employment assume that after realizing individual wages mothers can choose to work either 0, 4, or 8 hours daily i.e. $H \in \{0, 4, 8\}$. Also, for simplicity, assume f(D) is typified by a fixed cost (a_2) and decreasing hourly costs in 2 tiers: D_H per hour for the first 4 hours, and D_L for each additional hour (see Figure B.2). Then, women will work 8 hours (and not 4) only if $W_i * 8 - f(8) > W_i * 4 - f(4)$ without Pre-K entitlement and $W_i * 8 - f(4) > W_i * 4$ with Pre-K entitlement. This functional form is chosen for algebraic convenience, and only a fixed cost of childcare is required for some women to reduce hour with Pre-K entitlement and decreasing marginal cost of childcare increases the range of wage rates affected.

The possible effect of 4 hours free daily Pre-K can then be summarized by wages in 4 intervals (see Figure (B.3) and Figure (B.4)). Women earning a wage high enough (W_H) will rationally choose to work full time with or without state pre-k.[10] Similarly, women with a relatively low wage (W_L) will choose zero work hours even with Pre-K entitlement.[11] Women with wages in between, however, may exhibit one of two distinct changes. Women with moderately low wages (W_{ML}) will not enter the workforce in absence of free Pre-K but will work part time during Pre-K hours.[12] Similarly, women with moderately high wages (W_{MH}) will choose to work with or without Pre-K entitlement, but will rationally switch to part time employment facing Pre-K to avoid the fixed costs of obtaining and arranging childcare after Pre-K hours.[13]

[10] W_H are wages such that $W_i * 4 - a_1 > f(4)$.
[11] W_L are wages such that $W_i < a_1/4$.
[12] W_{ML} are wages that satisfy $a_1/4 < W_i < (f(8) - a_1)/8$.
[13] W_{MH} are wages that satisfy $(f(8) - a_1)/8 < W_i < (f(4) - a_1)/4$.

The addition of half day Pre-K may have differing effects across 4 intervals. Sufficiently high or low wage women will be unaffected, while women of moderate wages may either decrease or increase hours. Some women may enter the workforce during Pre-K hours, while others may reduce hours to avoid arranging additional childcare after Pre-K hours.

2.4 Data and Institutional Context

Analysis of child school attendance will primarily focus on data from the American Communities Survey (ACS). The ACS represents an annual cross sections of the entire United States with 1% sampled from every state and has both format and questions similar to the decennial census.[14] Focus will be on children aged 9 or younger and their mothers for the period of 2002-2013.[15] Total sample includes 3,105,400 children aged 0-9 years and 629,882 aged 4-5 years.[16] For summary statistics on children school attendance see Tables (B.1). Note that school attendance among 4-5 year olds is 4.7% higher in UPK states. School attendance is also 3.3% higher among 3 year olds. No differences are found for children past Pre-K age.

Although some descriptive evidence below will use observations from the ACS, for primary analysis of mothers' labor force behavior I will use longitudinal data from

[14] ACS began in 2000 with a 1-in-750 sample, growing to 1-in-100 since 2005.

[15] Child-parent links were made using IPUMS constructed variables POPLOC and MOMLOC which identify probable mother and fathers. Similar estimates result restricting the sample to heads-of-household, which allows inclusion of only child-parent relationships explicitly stated in each survey.

[16] Children eligible for Pre-K in the Census data can be either 4 or 5 years old, even though Pre-K is explicitly schooling for 4-year olds in most states. This is the result of ambiguity regarding age at start of school year because although age can be known precisely for all children observed, month of observation is not available in public release ACS data. That is, many children appearing as 5-years old may have been 4 at start of school year. Because ACS samples are performed evenly across all months, roughly half of all children age 4 each fall will be observed in ACS at each ages 4 and 5.

the Survey of Income and Program Participation (SIPP) Core Modules. Although it is smaller in sample size than the ACS data, it allows differentiation between quarters of the year. This allows comparison between fall months - when state Pre-K programs are offered, and the summer months when it is not. I merge all core waves of the 2001, 2004, and 2008 panels yielding observation on 286,732 mothers with a Pre-K aged child from 2002-2013.[17] I will also show results on childcare use from SIPP Topical Modules. SIPP provides a topical module on childcare utilization once per panel. Merging the relevant modules from 2001, 2004, and 2008 panels yields observations on 74,307 mothers of Pre-K aged children. For summary statistics on mothers' labor supply see Table (B.2). Note that there appears to be no clear pattern between UPK status and mothers' labor supply.

The SIPP childcare module includes a total of 13 different childcare options. To simplify comparison I combine childcare sources as follows. I define 'informal care' as having a child cared for by a sibling, grandparent, other relative, or other non-relative at least one day per week. Similarly, let 'formal care' include children cared for by childcare centers, head start, family day care, or preschool. Let parent care be child cared for at least one day per week by a parent that works or attends school, and StayAtHome be a child with a strictly stay-at-home designated caretaker. SIPP summary statistics on childcare utilization can be found in Table (B.3).[18] Note that

[17]For Core data, SIPP households are interviewed every four months regarding their activity in each of the preceding four months. Because this paper is not concerned with month-to-month transitions, I retain only the last month for each wave. This should alleviate possible seam bias and measurement error due to decreasing recall over longer time periods, and still show labor supply changes across both years and quarters.

[18]The composition of school here will differ slightly from that in the ACS. ACS school attendance includes preschool and nursery school, but only for children 3 or older. To prevent 'school' attendance by children under 3, I will group preschool with other non-school formal care arrangements.

school attendance appears higher and both formal and informal care use appears lower in UPK states.

First I will consider the apparent correlation between state Pre-K enrollment and school attendance as well as mothers' labor supply. Data on state Pre-K enrollment comes from the National Institute for Early Education Research (NIEER) "State of Preschool" annual yearbooks.[19] The NIEER yearbooks are the result of surveys from prekindergarten administrators in each state. Yearbooks include data on the percent of all children aged 4 (at start of school year) attending "state prekindergarten programs" in each state in each year. For the purpose of measurement, this includes all Pre-K programs that are funded by the state with education as a primary goal (not childcare) and does NOT include federal Head Start programs or those explicitly for children with special needs.[20]

Comparing state Pre-K enrollment to school attendance and mothers' labor supply data from the ACS reveals very different patterns for two-parent and single mother families. Table (B.4) shows correlations by mothers' marital status both with and without conditioning on state and year fixed effects. Although state Pre-K enrollment and school attendance are positively correlated for children of married and unmarried households; conditioning on both year and state fixed effects suggests that increasing state Pre-K enrollment by 50% of state 4-year-olds induces an additional 3.2% of children with married mothers to attend school, with no effect on those with unmarried parents. Figures (B.5) and (B.6) show that this difference is only present for Pre-K aged children with married mothers. Likewise, there appears to be no significant unconditional correlation between state Pre-K enrollment and mothers' labor hours. Adding state and year fixed effects, however, suggests that increases in state

[19] Available `http://nieer.org/yearbook`

[20] Pre-K enrollment is missing in year 2004 so these are taken as the average of enrollment in 2003 and 2005.

Pre-K enrollment are associated with decreases in mothers' labor hours; in fact, transitioning from state Pre-K enrollment of 0 to 50% of state 4-year-olds is associated with a decrease in mothers' work hours of approximately 0.8 hours per week. Figures (B.7) and (B.8) show this result by hourly margin - with married mothers decreasing work hours at every margin as state Pre-K enrollment increases, and unmarried mothers not significantly affected. Table (B.5) shows that this pattern is also only present for mothers with children aged 4-5 years.

Changes in observed state Pre-K enrollment can be caused by many things. One source of variation in state Pre-K enrollment is states' passage of UPK legislation, which is typically followed by large increases in both funding and eligibility. Table (B.1) shows that UPK states are typified by much higher (about 30%) enrollment and that 15.2% of 4-5 year olds sampled live in a UPK state. In order to show the effect of changes in Pre-K enrollment resulting from state UPK reform, analysis will focus on the three states which experienced major expansionary Pre-K reforms during the period of observation: Florida, Iowa, and Vermont. Enrollment trends in the three UPK states are compared to the four states with previous UPK reforms and all other states in Figure (B.9). These three states are a natural choice for several reasons. Most importantly, these states made dramatic increases in their state Pre-K enrollment that can be linked to legal reform in an observed year that was distinctly educational (not economic) in focus. Second, these three states experienced the largest increases in state Pre-K enrollment during the sample period. Lastly, state Pre-K in these states is currently available in almost all districts and is offered regardless of family income.

Florida's Pre-K program was started in 2002 through a petition sponsored by 'Parents for Readiness Edu. for our Kids' to amend the state constitution guaranteeing universal Pre-K availability. After receiving 514,667 signatures, a ballot was put to public vote requiring that "every four-year-old child in Florida shall be offered a high quality pre-kindergarten learning opportunity by the state no later than the 2005 school year."[21] Florida voters amended the state constitution in 2002 to establish universal voluntary Pre-K which began in fall of 2005.[22] By allowing parents to enroll children in any Pre-K program meeting state standards (including those offered by schools, childcare centers, home care centers, etc.) it was able to expand very rapidly in a short period of time, with NIEER data showing that Florida state Pre-K enrollment increased to almost 50% of state 4-year olds in its first year.

Iowa began Pre-K in 1989 with its Shared Vision program, targeted at low income families. Universal Pre-K began with the creation of Statewide Voluntary Preschool Program in 2007 after legislators passed HF 877 with primarily support from state Democrats.[23] The law was educational in focus, stating clearly "the purpose of the preschool program is to provide an opportunity for all young children in the state to enter school ready to learn."[24] Since introducing SVPP Iowa has increased its coverage from 18% to 90% of school districts, increasing the total percent of state 4-year olds enrolled in state Pre-K to almost 60%.

Vermont's Pre-K is currently offered through Act 62 which is primarily educational in focus but also cites high childcare usage statewide as a reason for increasing public Pre-K.[25] Before 2011, school districts were limited in how many Pre-K students they could count on their annual school census. Because state funding for each town's Pre-K program is based on their school census numbers, this essentially put a cap on how many Pre-K students the state would support each year. State Pre-K enrollment expanded sharply in the 2010 school year as this cap. Note that Vermont formally

[21] http://dos.elections.myflorida.com/initiatives/fulltext/pdf/34708-1.pdf

[22] http://election.dos.state.fl.us/initiatives/initdetail.asp?account=34708&seqnum=1

[23] https://votesmart.org/bill/4035/12560#12560

[24] https://coolice.legis.iowa.gov/cool-ice/default.asp?category=billinfo&service=iowacode&ga=83&input=256C

[25] http://www.leg.state.vt.us/docs/legdoc.cfm?URL=/docs/2008/acts/ACT062.HTM

considers itself a 'Universal' Pre-K state beginning in 2016, but has enrollment and eligibility similar to other 'Universal' states after this cap was removed so I will count this as the beginning of UPK in Vermont. Vermont's current Pre-K program enrolls almost 75% of state 4-year olds and is available to all students in 91% of school districts.

The resulting programs in these three states are similar in many ways. All three offer half day pre-kindergarten, with 3-4 hours per day offered 4-5 days per week. Vermont and Iowa Pre-K initiatives are explicitly academic-year. Florida residents can choose between academic year and summer programs, with over 90% enrolling in the academic year program.[26] Spending ranges from $2242 per child per year in Florida to $3863 per child per year in Vermont, with Iowa spending $2596 per year per child. This is small in comparison to the average family income of $70,207 for parents of Pre-K aged children in these states, and $85,071 in two-parent households which appear most affected by Pre-K eligibility.

Figures (B.10) - (B.13) show annual state averages of child school attendance and mothers' weekly labor hours for 4-5 year olds in Iowa and Florida from 2002-2013, by mothers' marital status. Here Vermont is excluded because its small sample size (only about 100 observations per year) causes yearly averages to move substantially from year to year. Fiture (B.10) shows that school attendance of 4-5 year olds with married parents were near national averages in both Florida and Iowa before their UPK reforms in 1995 and 1997. After UPK reform, school attendance increased to about 4% above national average. Figure (B.11) shows no such changes for children with unmarried mothers. Similarly, Figure (B.12) shows that the national average weekly labor supply for married mothers with 4-5 year olds increased by about 1 hour per week. However, Florida mothers show no increase over this period, and

[26] `http://www.oppaga.state.fl.us/reports/pdf/0823rpt.pdf`

Iowa mothers appear to decrease labor supply sharply following 2007 UPK reform. As with school attendance, Figure (B.13) shows that UPK reform does not appear associated with significant changes for unmarried mothers.[27] These graphs motivate the empirical strategy that follows in the next section, in which the effect of state UPK reform will be estimated via difference-in-difference.

2.5 Empirical Specification

I define binary variable 'UPK' at the state level to be '1' in all years after UPK legislation has passed, and '0' in all years before.[28] This is then used as the independent variable of interest, providing estimates of the effect of Pre-K eligibility on both school attendance and mothers' labor supply. Importantly, all three states offer 'universal' pre-k, without income requirements for eligibility so mothers have no incentive to either reduce actual or claimed work hours.

The empirical strategy of this paper is to compare the school enrollment and mothers' labor supply of children that live in UPK states to those who do not. With school enrollment (for example), this is accomplished most simply via a linear difference-in-difference that can be represented by:

$$YC_{j,t} = \alpha_0 + \gamma_t + \delta_j + \beta_1 UPK_{j,t} + u_{j,t} \tag{2.5}$$

where $YC_{j,t}$ is average school enrollment in state 'j' in year 't', $UPK_{j,t}$ is the variable of interest - denoting the existence of Universal Pre-K in a given state and year, γ_t

[27] Although it is not a focus of this paper, this graph also shows that unmarried mothers appear much more strongly affected by the 2009 recession. This average recession effect will be differenced out in analysis below with the inclusion of Year fixed effects. The possibility of bias due to state differences in recession effects will be examined through the apparent effects of UPK on untreated age groups - namely children aged 2-3 and 6-7 years.

[28] Other states with universal Pre-K programs include Georgia (beginning in 1993), New York and Oklahoma (since 1998), and West Virginia (since 2002). These states will be included as "Universal Pre-K States", but their status will not drive identification due to lack of observations before the start of UPK in that state.

gives year-fixed effect, and δ_j gives state-fixed effect. Identification of β_1 comes by comparing changes in state Pre-K eligibility to changes in state averages of school enrollment ($YC_{j,t}$). If states experienced changes in average mothers' age, education, marital status, or family composition then the estimates of β_1 could be biased by any correlation between these changes and child school attendance. For this reason, I will estimate:

$$YC_{i,j,t} = I(\alpha_0 + \gamma_t + \delta_j + \beta_0 X_{i,j,t} + \beta_1 UPK_{j,t} + u_{i,j,t} > 0) \tag{2.6}$$

where $X_{i,t}$ contains other covariates including mother's age, mother's education level,[29] mother's marital status, and indicators for whether given child is either youngest or eldest of those observed. Note that instead of state averages I will use $YC_{i,j,t}$ as dependent variable but that since $UPK_{j,t}$ varies by state identification comes through variation across time in state-covariate cell averages. I estimate (2.6) via Linear Probability Model, with standard errors clustered by state due to possible correlation of errors within each state (Bertrand et. all 2004).[30]

The effect of UPK legislation on mothers' labor hours can be represented similarly via:

$$YM_{i,j,t} = \begin{cases} YM_{i,j,t}* & \text{if } YM_{i,j,t}* \geq 0 \\ 0, & \text{otherwise} \end{cases} \tag{2.7}$$

$$YM_{i,j,t}* = \alpha_0 + \gamma_t + \delta_j + \beta_0 X_{i,j,t} + \beta_1 UPK_{j,t} + u_{i,j,t}$$

where $YM_{i,j,t}*$ is the mothers' weekly work hours observed. Two complications arise here that were not present before. First, the presence of multiple children per households makes it difficult to estimate effects separately for each child. Therefore I define

[29] no college, some college, and 4 or more years of college

[30] Throughout, most results shown will be LPM. Marginal effects via a logit model give nearly identical results in all cases, so bias due to LPM in unlikely.

the treatment group as mothers with a child aged 4-5 and will compare their outcomes to those of women with no child aged 4-5. To make treatment and control groups otherwise as similar as possible, I will consider control groups having either at least one child either 2-3 or at least one child 6-7 years old. Second, mothers' work hours are only observed when non-negative. Note again that because $UPK_{j,t}$ varies only by state, identification comes through comparison through time across state-covariate cell averages. Since these averages are nonzero, censoring should not be problematic.[31] I will also show LPM estimates with dependent variable being binary indicators for mothers working more than 0, 10, 20, 30, and 40 hours. Although these do not estimate average marginal effect on hours, they do not require distributional assumptions and allow differentiation between changes in labor force participation, full time employment, etc.

I will also show results using triple differences (DIDID) an alternate way to address the presence of multiple children per household.

$$YM_{i,j,t} = \begin{cases} YM_{i,j,t}* & \text{if } YM_{i,j,t}* \geq 0 \\ 0 & \text{otherwise} \end{cases} \tag{2.8}$$

$$YM_{i,j,t}* = \alpha_0 + \gamma_t + \delta_j + \beta_0 X_{i,j,t} + \Gamma(UPK * AgeGroup)_{jt} + u_{i,j,t}$$

where $(UPK * AgeGroup)$ contains the interactions of UPK and indicators for having a child aged 2-3, 4-5, and/or 6-7.

A similar specification to those above will be used to estimate the effect of UPK reform on the utilization of each of the five' types of childcare ('q'):

$$YCC^q_{i,j,t} = I(\alpha_0 + \gamma_t + \delta_j + \beta_0 X_{i,j,t} + \beta_1 UPK_{j,t} + u_{i,j,t} > 0) \tag{2.9}$$

As before I will estimate separately for families with and without child aged 4-5.

[31] Similar results also hold using Tobit, further confirming that censoring is not likely to be problematic.

Equations above assume that $Cov(u_{i,j,t}, UPK_{j,t}|X_{i,j,t}, \gamma_t, \delta_j) = 0$ i.e. that states' UPK status is not correlated with unobserved factors influencing school attendance or labor supply. This assumption would be violated if state UPK passage was correlated with current or anticipated state economic conditions, or changes in mothers' labor supply. The is addressed in part by comparison to results for children who are not treated - no significant changes for children aged 2-3 or 6-7 would cast doubt on correlation of Pre-K enrollment levels on state specific economic factors. Lastly, I will compare outcomes separately across fall and summer: since states offer their major Pre-K program only during the school year, mothers' work outcomes should not be affected in summer months.

2.6 ACS: Child School Attendance

Results in Table (B.6) show that UPK legislation has a substantial effect on State Pre-K enrollment of eligible children in a linear regression using all 50 states from year 2002-2013. Estimates with or without state and year fixed effects suggest that state adoption of UPK causes an increase in state Pre-K enrollment by almost 40%. Changes in actual school attendance, however, are much smaller. Table (B.7) shows that the adoption of UPK legislation is associated with an increase in school attendance by 3.6% of state 4-5 year olds. Observations from all 50 states are included and there are 29,749 observations for the 3 states with UPK reform; 7,941 observations before and 21,808 observations after UPK introduction. Increases in school attendance are fairly similar across education groups, with little difference in coefficient estimated for children of mothers with no, some, or 4 year of college.[32] Very little change is seen

[32] Although mothers' education group is endogenous over the lifespan, the short time period between UPK introduction and implementation makes sample selection bias here unlikely. Moreover, regressing UPK legislation on mothers' educational attainment suggests that mothers do not significantly increase or decrease college attendance after these reforms.

for children with unmarried parents. This is likely because single parent families are typified by significantly lower income level so many of these families will be eligible for targeted state Pre-K programs and/or Head Start.[33]

It is also worth noting that the estimated effect of Pre-K enlargement on school enrollment is small in magnitude. Some of this can be explained by the ambiguity in age vs Pre-K eligibility in ACS data. Children can attend Pre-K in the year which they are 4-years old by the fall start date; however, because month of survey is not available in public ACS data, half of the Pre-K eligible children are observed each at ages 4 and 5. This means about half of the children aged both 4 and 5 are either too young or too old to be eligible, respectively. The effect of UPK on school attendance of eligible children can therefore be approximated by doubling the coefficient - state adoption of UPK increases school attendance by at most 7% of state four-year olds. This means that passage of UPK reform is associated with an increase in state Pre-K enrollment by 38% of Pre-K eligible children, but only a 7% increase in school attendance; therefore it appears that most children enrolled in state Pre-K after UPK reform would have attended school of some sort during their Pre-K year in absence of any state program.

Similar regressions coefficients show the estimated effect of Pre-K enlargement separately by child age, from 3-9. Figure (B.14) shows that children with unmarried mothers are not significantly influenced to attend school in at any age.[34] Interestingly, Figure (B.15) shows the effects by year for married mothers. Note first that children eligible for state Pre-K are significantly more likely to attend school at ages 4 and 5.

[33]Average family income for unmarried mothers with Pre-K aged child in these three states is $25,399. This is very close to the 2014 Federal Poverty Level for a family of 4 ($24,250) and only 126% the Federal Poverty level for a family of 3 ($20,090) `https://www.healthcare.gov/glossary/federal-poverty-level-FPL/`

[34]Although a 1% increase in 6 year old attendance is statistically significant, this is quite small in magnitude.

Moreover, there is essentially no effect of UPK on school at ages 6-9 years old, which suggests that these reforms are not correlated with other school reforms or significant changes in schooling behavior. Interestingly, UPK laws do appear to decrease school attendance among children aged 3 with married mothers - possibly due to crowding out as mentioned in Bassok (2014).

2.7 SIPP: Mothers' Labor Supply and Childcare

UPK legislation appears to decrease the labor hours of married women with eligible children. Table (B.8) shows estimates of equation (2.7). Again the results differ significantly by mother's marital status. For married mothers, increasing state Pre-K to full enrollment is associated with a 2.8% decrease in full time employment and 3.4% decrease in working 30 or more hours per week, significant at the 1% and 5% confidence levels respectively. Again, these estimates include observations from all 50 states with a total of 2,843 observations in the 3 states passing UPK legislation; these observations are split fairly evenly with 1,377 observations before and 1,466 after UPK adoption. Unmarried mothers with a child aged 4-5 years show no decreases at any margin. Moreover, no similar changes are found for mothers with children aged 2-3 and/or 6-7. This suggests that the decrease in labor hours of mothers with children aged 4-5 are unlikely to be explained by local economics changes which would have to influence only mothers with Pre-K eligible children. Results regressing in one equation with interaction of UPK and age group are less clear; Table B.9 shows decreases in full time employment for all age groups and no other changes.

Table (B.10) reports comparison of mothers' outcomes during fall versus summer. Mothers' hours appear to be suppressed at all margins during fall months when Pre-K programs are offered. Conversely very few changes are apparent in summer months

- with only a 1.6% decrease in fulltime status, significant at the 10% level. Again, because mothers' labor supply is suppressed only during the school year and only for mothers of children aged 4-5 it appears unlikely that the observed decreases in mothers' labor supply can be attributed to changes in local economic conditions. Table (B.11) shows that the decrease in school-year hours for mothers of 4-5 year olds are observed for all education groups. This matches results above suggesting that school attendance was also affected equally regardless of mothers' educational attainment.

Tables (B.12) and (B.13) show the marginal effects of UPK legislation on hours via estimating Equations (2.7) and (2.8) with LPM. Regressing separately by child age group shows mothers with a 4-5 year old decrease their labor hours by 3.5 hours per week, and only in the fall when Pre-K programs are offered. No decreases appear during the summer or age groups, although there does appear to be an increase in summer work hours for mothers of 2-3 year olds. This result does not appear in the more conservative estimate regressing the effect of UPK in one equation using interaction variables as in Equation (2.8). This estimate suggests that with mothers of 4-5 year olds working 1.3 hours less per week, again only during fall months and not summer when Pre-K is not offered.

The topical modules offered in each SIPP panel ask each "designated parent" (i.e. primary caretaker) about parents' childcare choices. Table (B.14) shows that Pre-K expansion is associated with increased school attendance as well as a decrease in use of informal care (such as friends and relatives) in for children aged 4-5. Sample includes 421 observations for the 3 states with UPK reform; 208 before and 213 after. Interestingly there is a decrease in stay-at-home parents. This provides some support that Pre-K expansion may increase labor force participation among primary caretakers on the extensive margin. This decrease in strictly stay-at-home parents, however, is accompanied by a large increase in childcare by working parents at least one day per week (an increase over 10% if state Pre-K expands by 50% of state 4-year-olds). For 2-3 year olds, in contrast, there are no changes in parent-care which suggests that results are not driven by state trends unrelated to Pre-K expansion. Interestingly, there is a decrease in formal care for younger children - possibly due to crowding out in states such as Florida (as observed in Bassok 2014). Childcare by parents is still unaffected.

The pattern of work decreases reasonably matches the predictions of previous sections. First, work decreases are most robust for those working full time (or close to full time). Moreover, SIPP data confirms that mothers are substituting paid care for parent-care time, as the model predicts should be common among women with wages satisfying W_{MH}. It is surprising, however, that there is little evidence of labor force increases on the intensive margin. This could be for several reasons. First, state Pre-K expansion does not appear to have large effects on overall school attendance. With only small changes in total school attendance, the population possibly induced to work must be relatively small. In contrast, possible work disincentives due to the need for supplementary childcare will be relevant to all women with Pre-K aged children. Second, it may be more difficult for women to find jobs during only the free school-day hours. Also, there may be additional long run fixed costs to labor entry - if many low wage women were not previously planning to enter the workforce during their child's Pre-K year, it may be difficult to enter the workforce and/or find a good job match.

2.8 Conclusions

Previous work has shown increases in early childhood education can have dramatic short and long term effects on child outcomes, especially when programs are targeted at low income groups. Although universal Pre-K may increase kindergarten readiness, this paper casts doubt on claims that non-targeted Pre-K expansion is an important tool for helping mothers return to work. First, state Pre-K programs have a small effect on school attendance of Pre-K aged children. Moreover, state Pre-K programs appear to have essentially no effect on the attendance of children with unmarried parents - likely due to availability of other programs (like Head Start and targeted state programs).

Non-targeted universal state Pre-K programs also appear to suppress the labor hours of mothers with eligible children. UPK adoption is associated with about 1 fewer work hour per week for all mothers with Pre-K aged children (4-5 years). No similar changes are found for mother with only older (aged 6-7 years) or younger (aged 2-3 years) children. Looking at the distribution of work hours, changes appear to come primarily from mothers reducing hours around full-time and nearly full time margins. These changes are unlikely to be due to income effects because similar results were not found in more generous full day programs. These changes are consistent with a static labor supply model with fixed costs to childcare and labor force entry. With introduction of universal pre-k, some women will rationally reduce work hours and care for their own children after Pre-K hours end to avoid finding, arranging, and paying for additional childcare.

Due to the small effect on child school attendance and apparent negative effect on mothers' labor hours, it does not appear that the adoption of universal half-day Pre-K is an important policy for supporting mothers' labor supply as some claim. Moreover, previous work suggests that full day programs may not lead to the unintended labor force reduction I find with half day programs. Although full day programs are necessarily more expensive per student, a targeted full day program could use eligibility requirements to ensure fewer students receive state funding that would have attended absent any state programs.

Chapter 3

Do STEM incentives work? Evidence from SMART grants and the NLSY79 Children and Young Adults

3.1 Introduction

Many of the majors associated with higher wages and high job security are in science, technology, engineering and math (STEM) fields.[1] In addition to possible individuals benefits for specializing in STEM fields, some have alleged that science preparation is important for the country as a whole. This sentiment is suggested by a 2011 report from the Georgetown Center on Education and the Workforce, "STEM occupations are critical to our continued economic competitiveness because of their direct ties to innovation, economic growth, and productivity."[2] To capitalize on the potential benefits of majoring in STEM fields, some programs have introduced financial incentives to increase college graduates' preparation in the math and sciences.[3]

One recent STEM incentive program was the National Science & Mathematics Access to Retain Talent Grant (SMART) offered nationwide from 2005 to 2011. Through SMART all students eligible for Pell grants and declaring an approved STEM major were offered up to an additional $4000 in federal financial aid during each of

[1] e.g. see Arcidiacono (2004)

[2] `https://cew.georgetown.edu/wp-content/uploads/2014/11/stem-execsum.pdf`

[3] National programs include the America COMPETES Act and the American Competitiveness Initiative. State initiative include the New York STEM Incentive Program and Pennsylvania's NETS Program.

their junior and senior years of college.[4] Although previous papers have found varying levels of success in the SMART programs' goal of increasing the number of registered STEM majors at specific schools, I estimate the effects of the program on the college and career decisions of a national sample of students using the National Longitudinal Survey of Youth 1979 - Children and Young Adults data (79CYA).

Although estimates are limited by a small sample size and imperfect observations of SMART eligibility, I do not find evidence that funds from the SMART program influenced either educational attainment or selection of science major of eligible students. Interestingly, SMART eligible students declaring a STEM major appear less likely to choose careers in science related fields.

3.2 Related Literature

Students select college majors for various reasons. If students cared only about compensation, one would expect individuals with relatively high ability to sort into majors (and careers) associated with higher earnings. Arcidiacono (2004) finds that post-graduation earnings differ greatly by college major and that these differences exceed what can be explained by differences in ability alone. This suggests that compensation cannot solely explain college major selection. This conclusion is supported by student surveys. Arcidiacono, Hotz, and Kang (2012) use student surveys to show that college major choice depends importantly on both perceptions of one's own ability as well as expectations regarding future income. Similarly, Malgwi et al (2005) finds that students report major selection based on factors including individual interest, personal perception of one's aptitude, as well financial concerns such as expected compensation and opportunity for career advancement. Although students appear to compare

[4] Average award received in each year exceeds $3000 so most eligible students received maximum or near maximum award.

college majors across many dimensions, evidence suggests their choices can be influenced by financial incentives. Because of differences in instructional cost and potential earnings by major, some colleges and universities have offered differential pricing by field. Strange (2013) finds that students respond to differential pricing schemes by registering less often for some more expensive majors. Students' understanding of differential pricing and how it influences cost of attendance may play an important role, however, as Harwell (2013) finds that few students changed course selection after the adoption of differential pricing at UIUC, possibly explained by the fact that many students appeared unaware of tuition policy changes and their implications.

Merit scholarships may also influence the decisions of college students. Several papers investigate the response of Georgia students to the Helping Outstanding Pupils Educationally (HOPE) scholarship, offering tuition and fees (worth approximately $4500 per year) to qualifying Georgia state high school graduates attending a Georgia public college or university and maintaining at least a 3.0 grade average. The HOPE scholarship succeeded in improving freshman grades, increasing the GPAs of eligible freshman by about 0.13 points. There is evidence, however, that some students increased their measured GPAs in ways that may be undesirable to policy makers. Cornwell, Lee, and Mustard (2006) shows that students decreased their course loads by an average of 1 credit per year and decrease their enrollment in math and science courses (in which it may be harder to obtain a 3.0 average) by 1.2 credit hours. Cornwell et. al (2005) finds the introduction of HOPE was associated with an increase in summer school courses and course withdrawals.

Looking at national data, Sjoquist and Winter (2015) finds that students attending college while eligible for state merit scholarships appear less likely to select STEM majors. This could be due to strategic behavior as seen in Georgia, but the authors suggest changes may also be explained by income effects. Since state merit scholarship recipients can graduate with less student debt, students may feel less pressure to graduate with math or science majors which are correlated with higher earnings after graduation. Evidence that students alter their career path in response to moderate changes in debt at graduation, however, is surprising. Graduating with, for instance, and additional $10000 should make very little difference on an individual's wealth over their lifetime. Rothstein and Rouse (2011) find that students appear to adjust their career path in response to lower student debt looking at changes after one university's adoption of a "no loans" policy replacing all student loans with grants. This provides further evidence that graduating college students may be debt averse and/or liquidity constrained.

Two papers have looked at specifically at the effect of SMART grants on college major selection. Both Evans (2013) and Denning and Turley (2013) use college administrative data and a regression discontinuity framework to show that the apparent effects of SMART differ greatly by location. Evans exploits both income and GPA thresholds and finds that students in Ohio public colleges do not increase their enrollment or persistence in science major with SMART eligibility. Conversely, Denning and Turley (2013) finds that students in Texas public universities are 3.2% more likely to choose a STEM major with SMART eligibility, while students at Brigham Young University increase their STEM enrollment by 10.1%. It is surprising that the program's success at influencing student major selection varied so much across institutions. This may be due differences between universities in students' awareness of the SMART program, financial need, and/or interest in STEM fields.

3.3 Data and Institutional Context

The SMART program ran from 2005 to 2011 and explicitly intended to increase the science and math preparation of students with low income families. SMART granted up to $4000 of additional federal student aid per year to eligible students. Eligible students must receive the Federal Pell Grant, have U.S. citizenship, be enrolled full-time, have minimum 3.0 GPA, have junior or senior status by credits earned, and be registered for an approved STEM major. Between 2006 and 2011 the program roughly doubled in size, growing from 63,090 to 139,794 recipients and $205,784,340 to $432,652,081 in total awards.[5]. Although distribution of individual awards is not available, in each of the 5 program years the average grant was over $3000 which suggests most students received maximum or near maximum award totals.

To evaluate the long run effects of the SMART program on eligible individuals I use national longitudinal data rather than college administrative data. This permits analysis of educational attainment and adult occupational decisions in addition to college major, and also allows consideration of effects nationwide rather than in a particular university. I make use of data from the NLSY79 Children and Young Adults (79CYA), a longitudinal survey focusing on the outcomes of children of the women selected for the National Longitudinal Survey of Youth 1979. The full panel features 11,509 respondents who are interviewed biennially. Of these I retain 4,294 individuals who have reached the age of 24 years and have responded to the Young Adults survey in either 2010 or 2012 (and thus have observable college and career outcomes). Analysis will focus on the 783 respondents observed with school attendance in either their 3rd or 4th year of college (i.e. reaching their junior or senior year). These individuals were born between 1970 and 1988, with average birth year of

[5] Office of Postsecondary Education (2012) and (2006)

1984. About 63% (491) were late-college aged (19-22) during the years of the smart program. Of these, 181 had family income low enough to be eligible for a SMART grant during their junior or senior year.

The 79CYA includes detailed information on both schooling and occupation choice. Focal outcomes will include college major and field of occupation. I categorize fields as STEM or non-STEM according to guidelines established by the SMART program including Computer Science, Engineering, Life Science, Mathematics, Physical Science, and Technology fields.[6] Respondents are also asked about their occupational choices "What kind of work [do/did] you do?" which is used to identify those working in science and engineering fields.[7,8]

One weakness of the 79CYA data is that although Pell grant and thus SMART eligibility is based on Estimated Family Contribution (EFC) calculations using detailed family income and asset information, actual EFC cannot be determined from the 79CYA data. Instead EFC will be estimated using information on total family income and number of dependent children as reported by mothers via linked NLSY79 data.[9] Estimated family EFC is combined with Pell Grant eligibility cutoffs obtained from annual press releases from the U.S. Department of Education to assign binary eligible / ineligible status to each student attending college. For instance, in 2011 families with one dependent child and gross income below $55,000 are deemed eligible with maximum family income increasing in number of dependent children. The inability to properly identify SMART eligible students is problematic for several reasons. First

[6] `http://www.ifap.ed.gov/dpcletters/attachments/GEN0606A.pdf`

[7] Census 2000 Occupational Categories between 1000 and 1760 as well as those between 3000 and 3650 are considered STEM occupations

[8] Full Census 2000 Occupational Categories are available `https://www.census.gov/people/eeotabulation/documentation/occcategories.pdf`

[9] EFC estimates used will be those found in the annual EFC Quick Reference featured in Forbe's e.g. `http://www.forbes.com/2009/06/25/college-aid-arne-duncan-fafsa-personal-finance-fafsa.html`

if differences between low-income status and actual SMART eligibility are random, this will bias the estimated effect of SMART eligibility towards zero. More troubling, this method may incorrectly assign treatment and non-treatment in a way correlated with outcomes. For instance, if students most likely to study and work in STEM fields are also more likely to be incorrectly classified as eligible (i.e. EFC under-estimated) this would positively bias results. Unfortunately, without observation of actual EFC totals it is impossible to know the likelihood or severity of such bias.

Students with estimated EFC below Pell cutoff are deemed low-income and thus likely meet income requirements for the SMART grant. There may be, however, some difference at the individual level in the treatment as measured (i.e. low income STEM major) and the actual SMART treatment (Pell eligible STEM major). The actual effect of estimated SMART eligibility will be identified by comparing outcomes for low income students in college during the SMART program to low income students in other years. This is important for two reasons. First, this will alleviate bias due to time invariant differences between low income status and actual eligibility. Second, low-income students are much more likely to receive many types of need-based aid; by differencing across years, SMART is identified as the additional effect of low income status during program years. Lastly, students may receive different award totals through the SMART grant. Because individual awards cannot be observed, measured treatment will be the average award received by low income students. As mentioned before, average SMART receipt is roughly \$3000 each year. Since the majority of SMART recipients are awarded maximum or near-maximum values I will assume that individual differences in the value of grant received are not an important determinant of student behavior. Defining treatment with binary eligible / not-eligible status is problematic for the following reason. Students receiving relatively small SMART grants are counted as "treated" just the same as students receiving the full amount. Because those receiving very small grants are unlikely to change behavior due to the additional funding, this will bias the estimated effect of SMART eligibility to zero, even if eligibility were perfectly measured.

Table (C.1) provides summary statistics of the focal outcomes. About 17% of respondents claim a STEM major in their last two years of college and 28% claim a designated major at some point during their college studies. The 79CYA only asks for field of study once per respondent per school year and thus does not allow for 2nd (or 3rd) majors; although this information might be beneficial if observed, declared major is still highly predictive of later science careers - in fact science majors appear more likely to choose science fields by about 27 percentage points. Due to this data limitation, the effects of SMART will only be estimated over the observed major declared (i.e. their primary major). If students switch primary and secondary majors from STEM and non-STEM fields independently, this will bias towards zero the estimated effect of SMART on total major changes because only half of these change-of-majors will be observed for those with a double major. Results could be significantly biased, however if students change ordering of their STEM and non-STEM majors in response to SMART eligibility.[10] Although this possibility cannot be tested, it is hard to think of a reason for students to respond in this way. Moreover, national surveys suggest that students choosing a 2nd major are not in the majority; Pitt and Tepper (2012) suggest that 80% of college students declare only one major. I also observe and categorize occupational choices. A total of 15% of respondents claim occupations in STEM categories during their most recent survey, and 23% claim working in a science field at some point. Figure (C.1) shows the portion of juniors and seniors below the low income threshold and/or declaring a STEM major around SMART program

[10]e.g. changing from primary engineering and secondary sociology to primary sociology and secondary engineering would result in an observed decrease in STEM majors with no change in actual fields of study

years. About 5% fewer students appear to have low income status in 2008 and 2010 (falling from 49 to 44%). There also appears to be a slight increase in STEM majors, increasing from 14.6 to 16.3% between 2006 and 2010.

The 79CYA data also features information on types of college funding received. Students were asked whether they were recipients of Work Study, Scholarship, Assistantship, Grant, Waiver, Fellowship, or Loans. Loan recipients were asked the total amount that year.[11] Table (C.2) provides summary statistics on the aid received by 3rd and 4th year college students. Loans, scholarships, and assistanceships are most common being received by 55%, 38%, and 40% of respondents respectively. In addition 13% report receiving work study, 5% receive a waiver, and 2% report receiving a grant. Because SMART eligibility cannot be perfectly observed, it is important to note that the income cutoffs used are indeed predictive of aid receipt. In Table (C.3) I show the correlation between the income cutoffs used and type of funding received conditioned on year and family size as well as demographic control variables.[12] I control for family income by including the interaction of income and family size as well as this term squared and cubed. Even conditioning on family income, size, and demographic controls the cutoff used (based on the interaction of family income and family size) appears predictive of aid receipt. Those below the income cutoff are 12% more likely to receive a grant, and 4% more likely to receive a waiver.

3.4 Empirical Specification

I am primarily interested in the effects of SMART eligibility on binary outcomes: STEM major selection as well as reporting a STEM occupation as an adult. For STEM

[11] Because data is biennial, total loans across all school years cannot be determined. Rather than estimating total loans with interpolation I focus on loans reported in each survey year.

[12] Additional controls include race, gender, grade, region, urban status, age, mother's age, mother's marital status, and mother's educational attainment

major I will consider both being declared as STEM during junior and/or senior years as well as ever observed as a STEM major. I will also consider STEM occupation ever and most recently observed (i.e. 2012). There will be, therefore, four binary outcomes: ever STEM major, STEM major 3rd or 4th year, ever STEM occupation, STEM occupation last observed. The treated individuals will be those with income below threshold during the years of the SMART program, whose outcomes will be compared to low income students in other years. Baseline regression will be run over the sample of 79CYA respondents who have graduated during the years observed and will focus on treatment defined specifically as being a low income student during their junior and/or senior year of college during the SMART program. Effects will be estimated via linear difference-in-difference:

$$Y_i = I(\alpha_0 + \beta_1 X_i + \beta_2 LowIncome_i + \beta_3 LowIncome * SMART + u_i) > 0 \qquad (3.1)$$

where X includes the interaction of income and family size as well as this term squared and cubed, and additional demographic controls.[13]. For outcomes on binary STEM status, I estimate (3.1) via Linear Probability Model, with standard errors reported robust to heteroscedasticity.[14] I also estimate total years of school via ordinary least squares.

In addition I will consider the effect of SMART eligibility on occupation outcomes for science majors. Because SMART receipt requires being declared a STEM major, the treatment is defined as the additional effect of being a STEM major for low income students during the SMART program years. This results in an alternate specification:

[13] Additional controls include race, gender, grade, region, urban status, age, mother's age, mother's marital status, and mother's educational attainment

[14] Marginal effects via a logit model are similar throughout and available upon request

$$Y_i = I(\alpha_0 + \beta_1 X_i + \beta_2 LowIncome_i + \beta_3 STEM_i + \beta_4 LowIncome * SMART + \beta_5 LowIncome * SMART * STEM + u_i) > 0 \tag{3.2}$$

where X_i is defined as before. I will also estimate total education attainment measured in years of schooling. Here SMART eligible STEM majors are compared to STEM majors not eligible for SMART grants. This equation will therefore be biased if students indeed change their choice of major in response to SMART eligibility. Results from Equation (3.1) will be used, in part, to infer whether such behavior appears likely.

Both specifications will assume that dates for SMART eligibility (as determined by college cohort) are not correlated with unobserved factors influencing the selection into STEM majors. Although eligible students will have lower family income, the effect of SMART eligibility is defined by differences in the effect of income by year. Moreover, estimates will be conditioned on the interaction of family income and family size, defining the effect of low income explicitly through discontinuity caused by the cutoff used. The identifying assumption will be therefore that SMART eligibility is uncorrelated with outcomes conditional on family income, being above or below the income cutoff (in non-treatment years), as well as birth year, race, gender, etc. i.e. $Cov(u_i, LowInc * SMART_i | X_i, LowIcome_i) = 0$.

3.5 Results

If the SMART program succeeded in increasing preparation in the math and sciences it should have a positive effect on educational attainment, the likelihood of graduating with a science major, and/or the likelihood working in a science field. Table (C.4) shows that there is little evidence that students were influenced to change their educational attainment; upperclassman students with income below eligibility cutoff have on average 0.229 additional years of schooling which is statistically insignificant at a 10% confidence level. Moreover the SMART program had no significant positive effect on either selection of college major or of post-graduation occupation as identified by the difference between outcomes of low income students before, during, and after the SMART program. Coefficients for being a STEM major as an upperclassman and for ever being observed as a STEM major are both negative and estimated with low precision. The coefficients for being last observed working in science and ever being observed working in science are 0.002 and -0.095 again measured with low precision. Table (C.5) repeats this analysis over subpopulations by mothers' educational attainment and race. None of these subpopulations show evidence of increase or decrease in STEM majors among junior or senior year students.

Although SMART may not have influenced choice of college major, SMART eligible students choosing a STEM major show changes in career selection after graduation. Table (C.6) shows the results of triple difference estimates as in Equation (3.2). Here the effect of SMART on STEM majors is estimated by comparing career outcomes of STEM majors during and outside program years; while this allows for an additional test of SMARTS effect on STEM career choices, it relies on a smaller subset of the data. Effects here are identified through the 64 respondents observed with STEM major and low income status during the SMART program. As before it does not appear that STEM majors complete any additional years of schooling due to their additional funding eligibility. However, low income students choosing a STEM major during the SMART program are 22% less likely to report working in a science related field in 2012 and 35% less likely to ever be observed working in a science field. Although there may be cohort differences in propensity to work in STEM fields no similar changes are found for non-STEM majors. The results in Table (C.7) show results separated by mothers' educational attainment, which is used as a proxy for families' socioeconomic status. The observed decrease in both initial selection and persistence in STEM occupations is found among children with mothers of both education levels.

Three changes might explain the observed difference of STEM career selection of science majors after the SMART program. It is possible that the SMART program caused an increase in the selection of science majors by those unlikely to choose science careers and a proportionate decrease by students likely to choose science careers after graduation would explain the pattern observed. This explanation seems less likely because it is hard to think of a reason for students to respond in this way and because changes in science major selection are not observed in different subgroups defined by demographic features (like mothers' education and race). Alternately, the additional funding from SMART program may have increased the quantity of science majors pursuing graduate degrees and thus careers in non-science fields (such as law and business). This explanation also appears unlikely because there was no observed increase in years of educational attainment among science majors. Lastly, the decrease in selection of science occupations by SMART recipients may be explained by income effects as in Sjoquist and Winter (2015). Additional college funding from the SMART program may relieve financial pressure on students to pursue careers in the math and sciences after graduation.

Table (C.8) show the estimated effects of SMART eligibility on need-based aid types received. Looking at the sample of all college upperclassmen, SMART eligibility does not appear associated with any changes in college aid received. Results in Table (C.3) showed that the low-income status used was most predictive of grant receipt, and here the results are nearly significant at the 10% confidence level. Table

(C.9) repeats the estimates of SMART eligibility versus grant receipt separately by mothers' education. Among children whose mother received no college education, grant receipt increased by 34 percentage points significant at the 5% confidence level. If the additional frequency of grant receipt resulted in a larger amount of total college financial aid during the SMART program for science majors, this provides some evidence that the SMART program is associated with decreased financial constraint for graduating science majors.

3.6 Conclusions

Previous work has shown that student major choice can be influenced by college pricing. In addition, changes in income may influence student preference for majors associated with high earnings vs. majors appealing for other reasons. Desire for a workforce better prepared in the math and sciences has motivated several STEM-incentive programs including the 2005-2011 SMART program. This STEM scholarship provides a test of the ability for financial incentives to change students' decisions over college majors and careers post-graduation.

Earlier papers by Evans (2013) and Denning and Turley (2013) found conflicting results regarding the ability for SMART to influence college major choices using administrative data from different university systems. This is possibly explained by differences in the student population; some student populations may be more aware or more easily influenced in their course decisions. Using instead samples from the National Longitudinal Survey of Youth 1979 Child and Young Adults, I estimate the effect of the program on a national sample. In addition I observe occupational outcomes, which may provide a better measure of the program's success in increasing preparation for math and science careers.

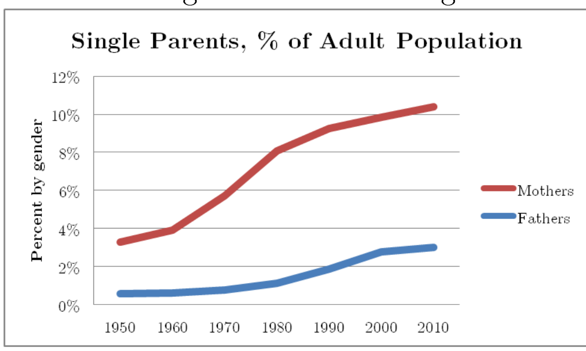
Although estimation is likely imprecise due to small sample sizes and mismeasurement of treatment, I find no evidence that the SMART program increased student registration for the science majors. This includes estimates of both last declared college major and ever declaring a science major. However, I find evidence that SMART eligible STEM majors are less likely to be currently observed in a science occupation by 22 percentage points. This is surprising because no changes are observed in years of schooling or selection into science majors for subgroups by mothers' race or education. Although the total amount of aid received cannot be observed, STEM majors with less educated mothers are more likely to report grant receipt by 34 percentage points. No similar changes in career choice or aid receipt are found for non-STEM majors which suggests these changes are not be explained by trends unrelated to the SMART program.

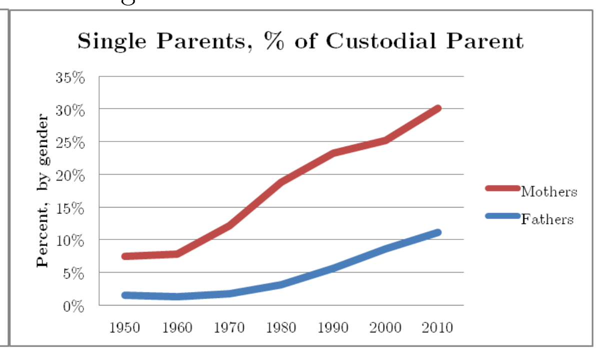
Although results are suggestive, they are far from conclusive. Several empirical concerns encourage further study. First, the sample includes only 181 low-income students with junior or senior status during the program years. Second, eligibility is estimated using family income and size rather than actual EFC which determines grant receipt. Only primary major is observed, hiding any effect due to changes in secondary majors. Together these issues limit precision and may introduce bias to the estimates. Future work would benefit from more complete data which can address these concerns and provide better estimates of the effect of SMART on student college and career decisions.

Appendix A

Single Fathers Appendix

Figure A.1: Increasing Prevalence of Single Parent Households





'Single Parent' including those never married.

Table A.1: Marital Status, US Fathers, 2011

	Est Pop 2011	%
Married	23,415,486	87.26
Spouse Absent	315,212	1.17
Separated	332,220	1.24
Divorced	1,199,241	4.47
Widowed	103,359	0.39
Never Married	1,468,763	5.47
Total	26,834,281	100

Table A.2: Education and Work Habits, Single vs. Married Fathers

	Single Father (NM)		Separated Father		Married Fathers	
	Mean	Std. Error	Mean	Std. Error	Mean	Std. Error
No Diploma %	25.5%	(.005)	14.0%	(.003)	11.7%	(.001)
Highschool Only %	67.2%	(.005)	65.8%	(.005)	52.3%	(.002)
4-Yr College %	7.4%	(.003)	20.2%	(.005)	36.0%	(.001)
Annual Labor Hours	1820	(9.5)	1986	(8.9)	2155	(1.9)
% Unemployed	14.1%	(.004)	9.4%	(.003)	5.0%	(.001)
% Not in Lab Force	11.9%	(.004)	11.9%	(.003)	5.5%	(.001)
% Below Poverty	23.7%	(.005)	11.8%	(.003)	7.1%	(.001)

'Single Father (NM)' indicates never-married fathers. 'Separated Father' includes all other single fathers.
Standard errors using BRR weight in parentheses.

Table A.3: Wage Income, Single vs. Married Fathers

	Single Father (NM)		Separated Father		Married Father	
	Mean	Std. Error	Mean	Std. Error	Mean	Std. Error
All	$28,844	(327)	$46,959	(558)	$62,511	(187)
18-30 years	$23,246	(353)	$28,232	(930)	$34,783	(253)
31-40 years	$31,767	(552)	$42,866	(938)	$58,125	(272)
41-50 years	$36,965	(1386)	$52,308	(986)	$72,246	(307)
51-65 years	$41,161	(3220)	$52,626	(1645)	$72,182	(544)
No Diploma	$20,190	(496)	$25,247	(815)	$25,701	(178)
Highschool	$28,681	(391)	$38,794	(474)	$44,817	(148)
4-Yr College	$54,524	(2181)	$83,091	(1695)	$98,011	(396)

'Single Father (NM)' indicates never-married fathers. 'Separated Father' includes all other single fathers.
Standard errors using BRR weight in parentheses.

Table A.4: Wage Rates, Single vs. Married Fathers

	Single Father (NM)		Separated Father		Married Father	
	Mean	Std. Error	Mean	Std. Error	Mean	Std. Error
All	$16.46	(.33)	$24.08	(.39)	$29.19	(.11)
18-30 years	$13.57	(.24)	$15.81	(.53)	$17.26	(.19)
31-40 years	$18.37	(.71)	$21.15	(.4)	$27.23	(.18)
41-50 years	$19.70	(.85)	$25.92	(.56)	$33.04	(.21)
51-65 years	$21.91	(1.62)	$30.38	(2.01)	$34.57	(.43)
No Diploma	$12.67	(.49)	$14.95	(.51)	$14.41	(.17)
Highschool	$16.56	(.42)	$20.44	(.37)	$22.00	(.15)
4-Yr College	$26.30	(1.1)	$39.86	(1.32)	$43.55	(.2)

'Single Father (NM)' indicates never-married fathers. 'Separated Father' includes all other single fathers.
Standard errors using BRR weight in parentheses.

Table A.5: Labor Outcomes vs. Never Married and Separated Fatherhood, ACS 2011

	Dependent Variable				
	Wage Rate	Labor Income	Labor Hours	Log(Rate)	Log(Income)
Single Father (NM)	-2.113***	-8560.125***	-296.468***	-0.135***	-0.263***
	(0.336)	(351.096)	(11.558)	(0.008)	(0.013)
Separated Father	-2.214***	-9590.665***	-246.190***	-0.090***	-0.195***
	(0.402)	(485.026)	(10.338)	(0.008)	(0.011)
N	234668	253125	253125	218553	218553

Standard errors using BRR weight in parentheses.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.6: PSID Observation Totals, All Years

	N-Individuals	N-Observations
Full Sample	16,281	143,416
All Fathers	11,802	82,829
Married Father	11,311	77,752
Separated Father	2,193	3,552
Never Married Fathers	806	1,348

Table A.7: PSID Averages, Year 2009

Averages, 2009	Single Father (NM)	Separated Father	Married Father
No Work	11.8%	11.3%	6.0%
Fulltime	50.5%	61.9%	80.0%
Income	$ 22,812	$ 39,221	$ 56,363
Wage Rate	$ 14.59	$ 20.44	$ 26.33
Labor Hours	1525	1668	2011
HouseWork	12.0	14.4	8.6
Age	32.0	39.2	38.8
Years of Education	12.2	13.0	13.4
# Children in Household	1.57	1.91	1.96
Observations	82	138	2194

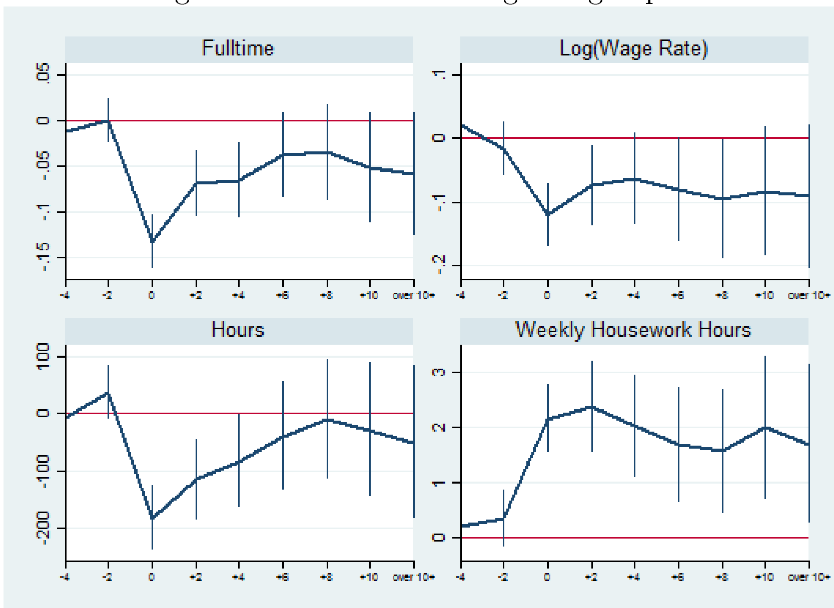
Table A.8: Labor Outcomes vs. Never Married and Separated Fatherhood, All Fathers, PSID

	Dependent Variable				
	Fulltime	Log(Hours)	Housework	Log(Rate)	Log(Income)
Single Father (NM)	-0.225***	-0.246***	2.302***	-0.248***	-0.494***
	(0.013)	(0.022)	(0.293)	(0.024)	(0.032)
Separated Father	-0.240***	-0.247***	3.738***	-0.245***	-0.507***
	(0.008)	(0.012)	(0.174)	(0.016)	(0.018)
Constant	0.494***	7.107***	9.031***	1.794***	8.916***
	(0.054)	(0.098)	(1.015)	(0.119)	(0.152)
N	88132	83819	86583	82344	86341

Estimates include all fathers and are conditional on age and year, education groups (no diploma, high school diploma, or at least 4-year college degree), state, and race (indicator for white). Heteroscedasticity robust errors in parentheses.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Figure A.2: Changes Before and After Beginning Separated Fatherhood



Coefficients for year pairs before and after entering into separated fatherhood. Regression also includes year and age effects as well as indicators for remarriage and children leaving the household. Trend lines show coefficients, vertical lines show 95% confidence intervals.

Figure A.3: Changes Before and After Beginning Single Fatherhood (NM)

Coefficients for year pairs before and after entering into separated fatherhood. Regression also includes year and age effects as well as indicators for remarriage and children leaving the household. Trend lines show coefficients, vertical lines show 95% confidence intervals. Full results available upon request.

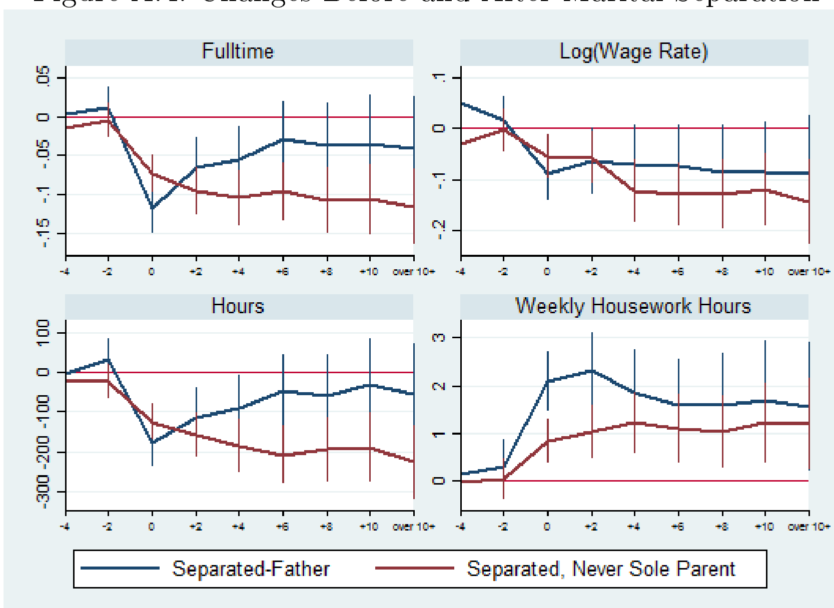
Table A.9: Labor Market Outcomes, FE, Never Married and Separated Fathers, PSID

	Dependent Variable				
	Fulltime	Hours	Housework	Log(Rate)	Log(Income)
Separated Father	-0.158***	-267.674***	3.466***	-0.144***	-0.331***
	(0.010)	(16.396)	(0.221)	(0.018)	(0.021)
Constant	0.494***	1635.703***	13.651***	2.085***	9.273***
	(0.032)	(54.462)	(2.262)	(0.056)	(0.071)
N Observations	145912	145912	156835	143122	149313
N Individuals	16320	16320	16478	16275	16330

Estimates include all men with at least one year of separated or never married fatherhood and are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Figure A.4: Changes Before and After Marital Separation



Coefficients for year pairs before and after marital separation. Regression also includes year and age effects as well as indicators for remarriage. Trend lines show coefficients, vertical lines show 95% confidence intervals. Full results available upon request

Table A.10: Labor Market Outcomes, DIDID, All Men, PSID

	Dependent Variable				
	Fulltime	Hours	Housework	Log(Rate)	Log(Income)
Separated Father	-0.106***	-196.960***	2.319***	-0.124***	-0.249***
	(0.011)	(19.980)	(0.251)	(0.021)	(0.025)
Post-SF Rem	-0.045***	-63.621**	0.957***	-0.060**	-0.098***
	(0.011)	(22.430)	(0.280)	(0.022)	(0.025)
Post-SF Mat	0.005	6.864	0.162	-0.036	-0.036
	(0.012)	(22.599)	(0.256)	(0.021)	(0.026)
	With Separation Trend				
Separated Father	-0.100***	-188.924***	2.289***	-0.116***	-0.235***
	(0.011)	(19.995)	(0.253)	(0.021)	(0.025)
Post-SF Rem	-0.035***	-37.849	0.688**	-0.040	-0.056**
	(0.012)	(23.873)	(0.298)	(0.024)	(0.028)
Post-SF Mat	0.000	5.582	0.044	-0.013	-0.012
	(0.012)	(23.611)	(0.267)	(0.022)	(0.027)
N Observations	145912	145912	156835	143122	149313
N Individuals	16320	16320	16478	16275	16330

Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.11: Labor Market Outcomes, DIDID, Widowers, PSID

	Dependent Variable				
	Fulltime	Hours	Housework	Log(Rate)	Log(Income)
Widowed Father	-0.047**	-86.668**	2.089***	-0.110	-0.149*
	(0.023)	(40.946)	(0.802)	(0.072)	(0.082)
Post-SF Rem	-0.030***	-27.515	0.774**	-0.075***	-0.096***
	(0.010)	(17.165)	(0.299)	(0.026)	(0.030)
Post-SF Mat	0.026**	40.341	0.168	0.000	0.046
	(0.013)	(22.532)	(0.356)	(0.033)	(0.038)
N Observations	132351	132351	141591	130008	135681
N Individuals	15982	15982	16107	15935	15983

Estimates include widowed, married, and never married men and are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.12: Career Changes, DIDID, All Men, PSID

	Dependent Variable		
	Missed Work	Changed Occ	Changed Ind
Separated Father	0.047***	0.109***	0.114***
	(0.010)	(0.012)	(0.013)
Post-SF Rem	-0.026**	0.067***	0.066***
	(0.011)	(0.014)	(0.016)
Post-SF Mat	0.031***	0.017	0.014
	(0.010)	(0.016)	(0.016)
N Observations	137063	139122	138244
N Individuals	15505	15540	15512

Estimates are conditional on age and year, as well as individual fixed effects, and year-group indicators to control for dynamic effects around marital separation. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.13: Labor Market Outcomes Additional Specifications, DIDID, Widowers, PSID

	Dependent Variable				
	Fulltime	Hours	Housework	Log(Rate)	Log(Income)
			First Year Only		
Widowed Father	-0.082**	-133.459**	2.577**	-0.116	-0.142
	(0.034)	(58.680)	(1.067)	(0.123)	(0.130)
N Observations	131143	131143	139945	128865	134470
N Individuals	15974	15974	16071	15926	15974
			With Housework		
Widowed Father	-0.050**	-101.876**		-0.111	-0.131
	(0.025)	(43.075)		(0.077)	(0.086)
Housework	-0.003***	-5.976***		0.000	-0.004***
	(0.000)	(0.270)		(0.000)	(0.000)
N Observations	124713	124713		122404	128003
N Individuals	15501	15501		15453	15509

Estimates include widowed, married, and never married men and are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.14: Other Income, DIDID, Widowers, PSID

	Dependent Variable			
	Received Lump	Lump $	Tot Fam Inc	Fam Inc Per Person
Widowed Father	0.032	3008.028	3418.233	-3625.340**
	(0.02)	(4232.504)	(2252.221)	(1566.703)
Post-SF Rem	-0.013*	-865.546	-1716.291	-392.879
	(0.007)	(574.737)	(2043.080)	(750.286)
Post-SF Mat	-0.008	1085.051	-2077.796	364.819
	(0.009)	(1703.628)	(2545.330)	(1380.570)
N Observations	145596	148458	147730	147237
N Individuals	16567	16579	16448	16389

'Lump sums' reflect additional payments received, including insurance and inheritance money. Total family income is total of income for all members of the family unit. Family income per person is total family income divided by number of individuals in family unit. Estimates include all married and widowed men and are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.15: Physical and Mental Health, DIDID, Widowers, PSID

	Dependent Variable			
	Healthy	Emotional	Smokes	Drinks
Separated Father	-0.002	0.022	-0.011	-0.033
	(0.009)	(0.015)	(0.019)	(0.024)
Post-SF Rem	-0.005	0.047***	0.027	-0.032
	(0.011)	(0.015)	(0.021)	(0.028)
Post-SF Mat	0.015	0.008	-0.000	0.003
	(0.011)	(0.015)	(0.020)	(0.028)
N Observations	105335	30546	30568	30558
N Individuals	14102	7592	7593	7593

Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.16: Mental Distress, DIDID,All Men, PSID

	Dependent Variable					
	Very Sad	Nervous	Restless	Hopeless	Worthless	Effort
Separated Father	0.018	0.057***	0.034	0.019	0.013	0.044*
	(0.016)	(0.017)	(0.021)	(0.014)	(0.012)	(0.023)
Post-SF Rem	-0.014	0.005	-0.032	0.001	0.017	-0.002
	(0.017)	(0.015)	(0.025)	(0.015)	(0.013)	(0.028)
Post-SF Mat	-0.014	-0.005	0.008	-0.022	-0.003	0.026
	(0.017)	(0.019)	(0.022)	(0.013)	(0.013)	(0.025)
N Observations	20781	20784	20780	20781	20780	20769
N Individuals	7269	7268	7268	7268	7269	7267

Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.17: Effect of Single Fatherhood on Income, DIDID By Starting Year , All Men, PSID

	Log(Wage Income)		
	Pre 1980	1980-1994	Post 1994
Separated Father	-0.288***	-0.273***	-0.150***
	(0.052)	(0.034)	(0.041)
Post-SF Rem	-0.076	-0.099***	-0.157***
	(0.053)	(0.033)	(0.054)
Post-SF Mat	-0.054	-0.028	-0.024
	(0.052)	(0.034)	(0.053)
N Observations	128999	137629	130483
N Individuals	14249	14998	14519

Year defined by first year of single fatherhood. Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.18: Effect of Single Fatherhood on Income, DIDID By Initial Age, All Men, PSID

	Log(Wage Income)		
	Before 30 y/0	30-45 y/o	After 45 y/o
Separated Father	-0.350***	-0.208***	-0.199***
	(0.045)	(0.032)	(0.051)
Post-SF Rem	-0.148***	-0.100***	-0.049
	(0.048)	(0.031)	(0.078)
Post-SF Mat	-0.107**	-0.007	-0.031
	(0.051)	(0.032)	(0.067)
N Observations	129289	138067	129755
N Individuals	14465	15061	14240

Initial age as age during first year of single fatherhood. Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.19: Effect of Single Fatherhood on Income, DIDID By Edu, All Men, PSID

	Log(Wage Income)		
	No HS Diploma	HS Diploma	4 Yrs College+
Separated Father	-0.138**	-0.194***	-0.337***
	(0.063)	(0.032)	(0.055)
Post-SF Rem	-0.008	-0.075**	-0.141**
	(0.062)	(0.029)	(0.071)
Post-SF Mat	-0.091	-0.006	-0.053
	(0.062)	(0.032)	(0.057)
N Observations	25826	73781	49706
N Individuals	4382	9575	7743

Educational attainment during first year of single fatherhood. Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.20: Effect of Single Fatherhood on Income, DIDID - Small Children, All Men, PSID

	Log(Wage Income)	
	No Small Children	1+ Small Children
Separated Father	-0.181***	-0.265***
	(0.027)	(0.030)
Post-SF Rem	-0.096***	-0.099***
	(0.028)	(0.033)
Post-SF Mat	-0.019	-0.078**
	(0.028)	(0.034)
N Observations	143744	141067
N Individuals	15684	15731

Small children defined as children under the age of 6 during first year of single fatherhood. Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table A.21: Effect of Single Fatherhood on Income, DIDID - Division of Labor, All Men, PSID

	Log(Wage Income)			
Wife, Pre-Sep:	Low Labor Hrs	High Labor Hrs	High Housework	Low Housework
Separated Father	-0.325***	-0.126***	-0.286***	-0.191***
	(0.033)	(0.030)	(0.031)	(0.031)
Post-SF Rem	-0.126***	-0.075**	-0.148***	-0.042
	(0.030)	(0.036)	(0.028)	(0.035)
Post-SF Mat	-0.071**	-0.011	-0.034	-0.045
	(0.032)	(0.036)	(0.031)	(0.034)
N Observations	139377	136604	139296	138311
N Individuals	15357	15208	15349	15351

High labor hours indicates a wife working 20 or more hours per week 2 years before first year of single fatherhood. High housework indicates a wife's share of total housework hours above the 50th percentile 2 years before beginning single fatherhood. Estimates are conditional on age and year, as well as individual fixed effects. Errors in parentheses clustered on the individual.

****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Appendix B

Pre-K Appendix

Figure B.1: Wages

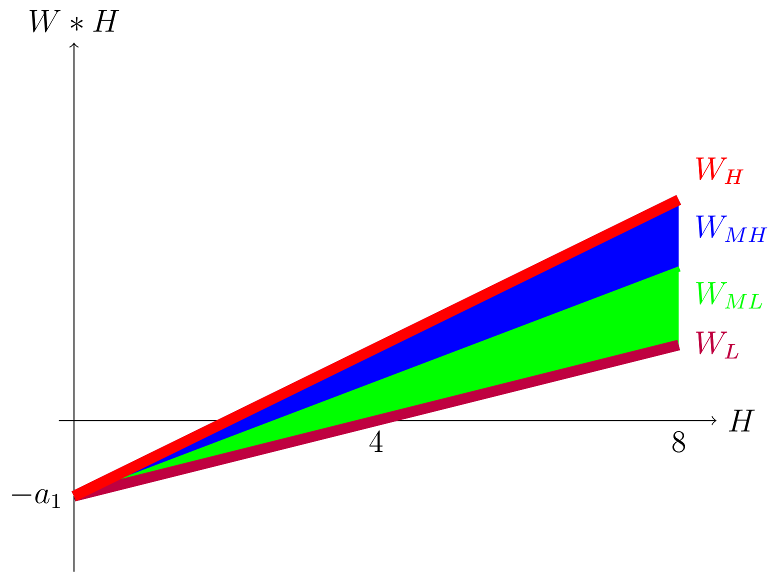


Table B.1: ACS, Summary Stats (Child)

Variable	All Mean	All SE	UPK Mean	UPK SE	NO UPK Mean	NO UPK SE
UPK	0.152	(0.000)	1	.	0	.
School Att (Age 3)	0.357	(0.001)	0.385	(0.003)	0.352	(0.001)
School Att (Age 4-5)	0.739	(0.001)	0.787	(0.002)	0.730	(0.001)
School Att (Age 6-7)	0.975	(0.000)	0.976	(0.001)	0.975	(0.000)
N	891,688		91,811		499,877	

Estimated population means using national-level individual weights with standard errors in parentheses.

Figure B.2: Childcare Costs

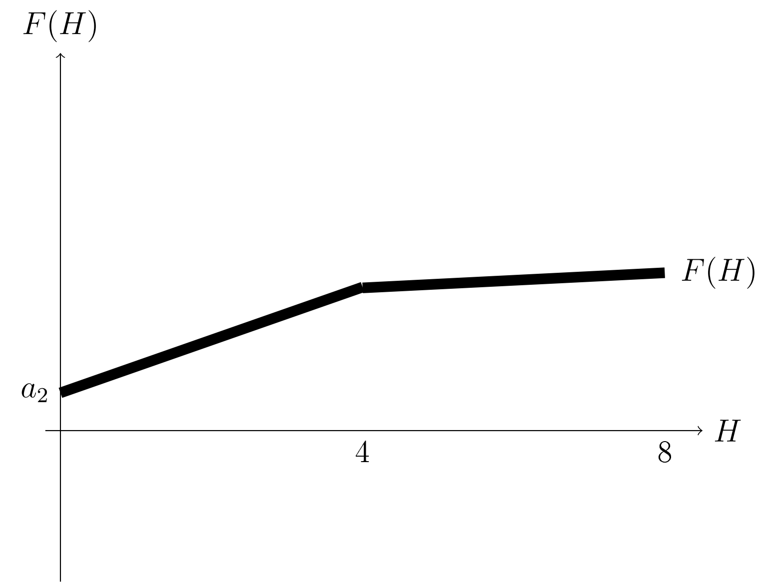


Figure B.3: Wage Minus Childcare - No Pre-K

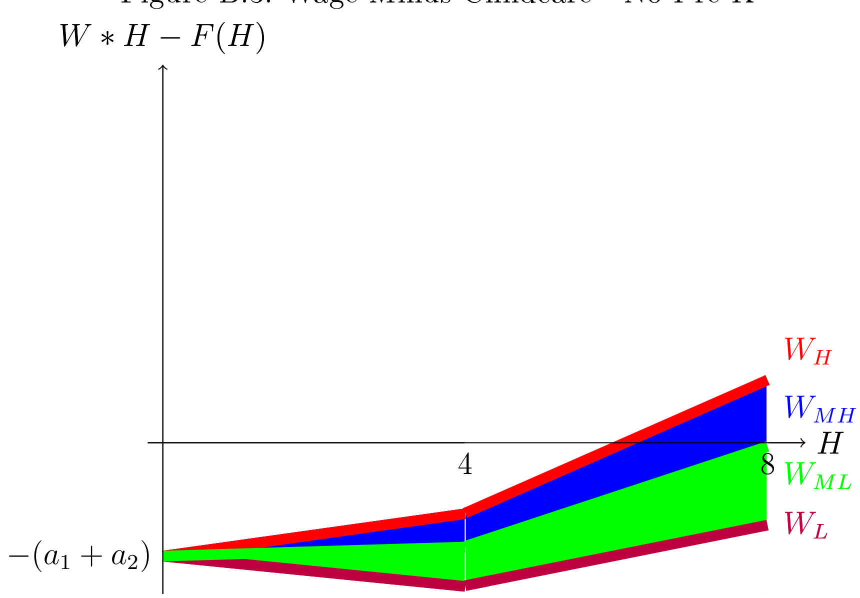


Figure B.4: Wage Minus Childcare - With Pre-K

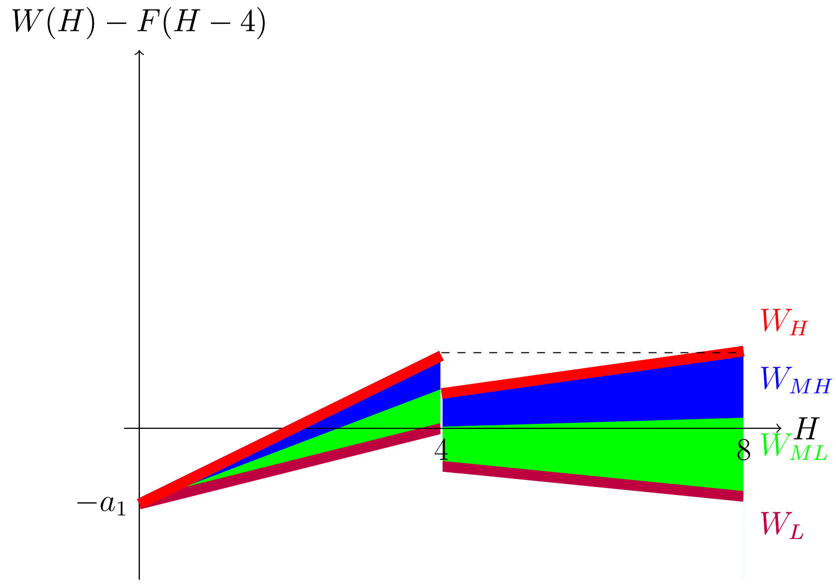


Table B.2: SIPP, Summary Stats, Mothers' Labor Supply, With 4-5 y/o

	All		UPK		NO UPK	
Variable	Mean	SE	Mean	SE	Mean	SE
Hours	19.957	(0.070)	19.698	(0.192)	19.996	(0.075)
Worked	0.569	(0.002)	0.555	(0.005)	0.571	(0.002)
10+ Hrs/Week	0.544	(0.002)	0.534	(0.005)	0.545	(0.002)
20+ Hrs/Week	0.486	(0.002)	0.488	(0.005)	0.486	(0.002)
30+ Hrs/Week	0.416	(0.002)	0.425	(0.005)	0.414	(0.002)
40+ Hrs/Week	0.058	(0.002)	0.042	(0.002)	0.060	(0.001)
N	74,307		9,527		64,780	

Estimated population means using national-level individual weights with standard errors in parentheses.

Table B.3: SIPP, Summary Stats, Childcare Used, Child Age 4-5 Years

	All		UPK		NO UPK	
Variable	Mean	SE	Mean	SE	Mean	SE
School	0.255	(0.004)	0.306	(0.013)	0.248	(0.005)
Informal Care	0.296	(0.004)	0.266	(0.012)	0.300	(0.005)
Formal Care	0.224	(0.004)	0.201	(0.011)	0.227	(0.004)
Parent Care	0.156	(0.004)	0.137	(0.009)	0.158	(0.004)
StayAtHome	0.436	(0.005)	0.462	(0.014)	0.432	(0.005)
Parent+StayAtHome	0.170	(0.004)	0.151	(0.010)	0.172	(0.004)
N	10,328		1,266		9,062	

Estimated population means using national-level individual weights with standard errors in parentheses.

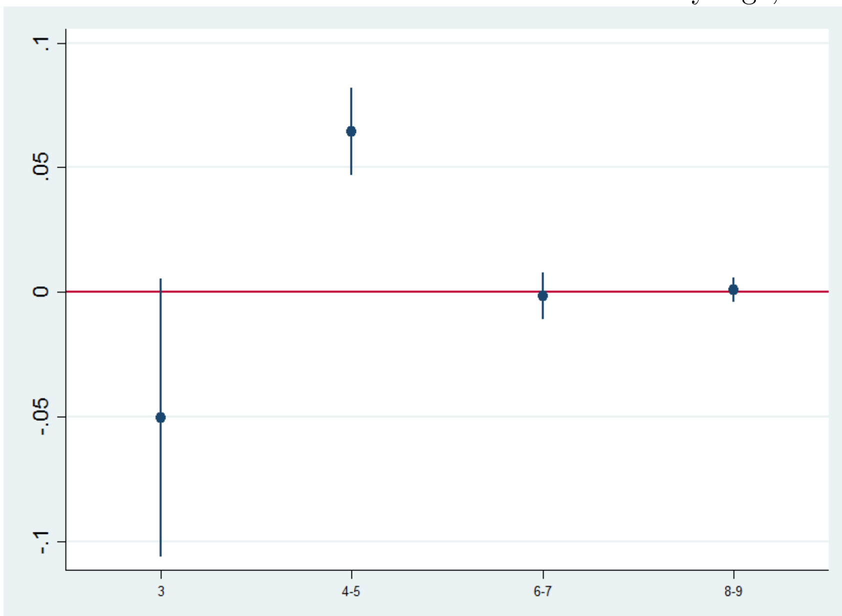
Table B.4: ACS, Pre-K Enrollment vs. School and Mothers' Hours, Age 4-5

	Child School Attendance			
PreK	0.124**	-0.000	0.119*	0.064***
	(0.040)	(0.018)	(0.046)	(0.009)
N	110225	110225	480531	480531
Year, State FE	NO	YES	NO	YES
Mother Married	NO	NO	YES	YES

	Mothers' Labor Hours			
PreK	0.169	0.161	0.670	-1.665***
	(1.110)	(2.108)	(1.428)	(0.460)
N	104261	104261	455906	455906
Year, State FE	NO	YES	NO	YES
Mother Married	NO	NO	YES	YES

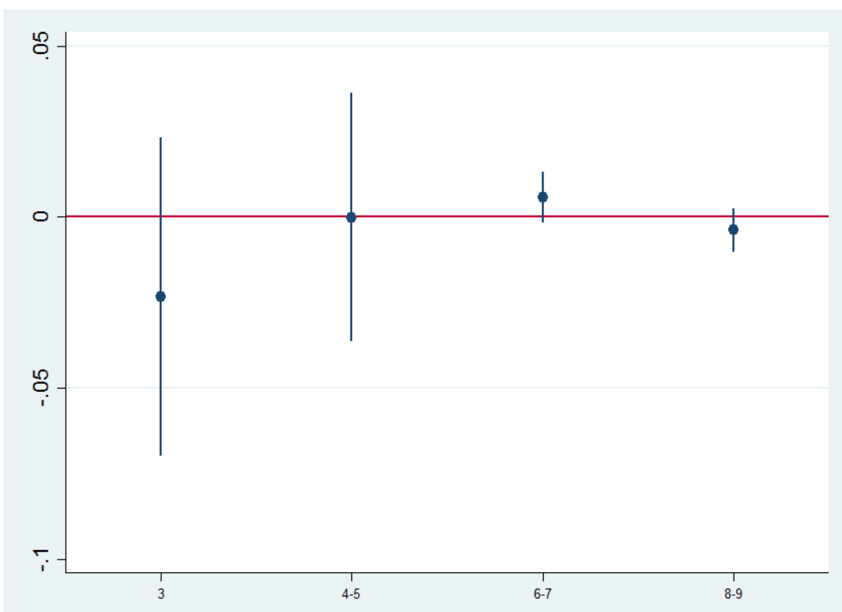
*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Figure B.5: State Pre-K Enrollment vs. School Attendance By Age, Mother Married



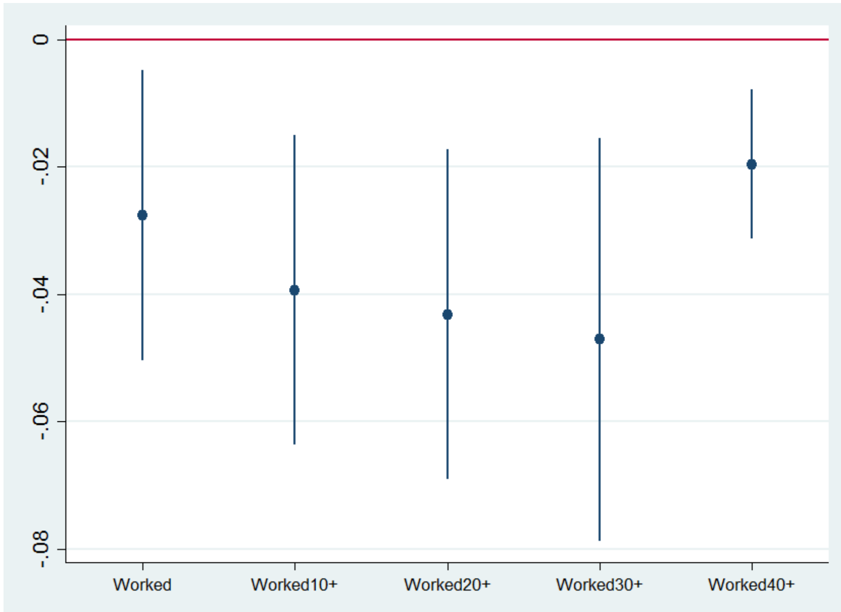
State enrollment levels via NIEER annual The State of Preschool yearbooks. Covariates include state and year fixed effects.

Figure B.6: State Pre-K Enrollment vs. School Attendance By Age, Mother Unmarried



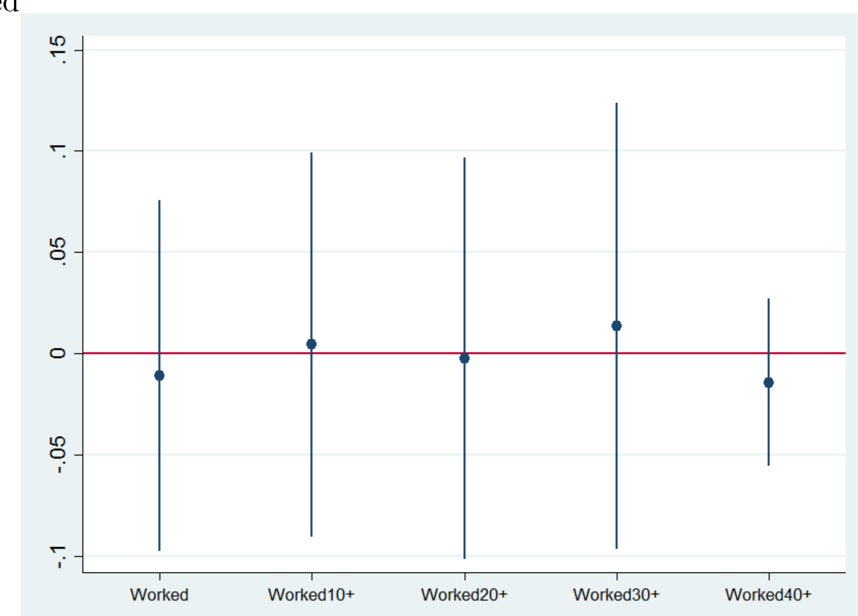
State enrollment levels via NIEER annual The State of Preschool yearbooks. Covariates include state and year fixed effects.

Figure B.7: State Pre-K Enrollment vs. Mothers' Hours, With 4-5 Year Old, Mother Married



State enrollment levels via NIEER annual The State of Preschool yearbooks. Covariates include state and year fixed effects.

Figure B.8: State Pre-K Enrollment vs. Mothers' Hours, With 4-5 Year Old, Mother Unmarried



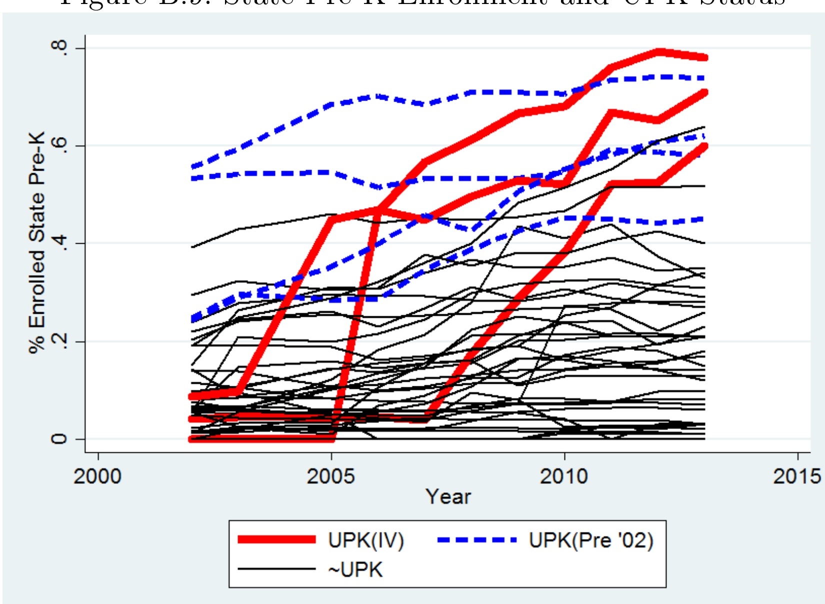
State enrollment levels via NIEER annual The State of Preschool yearbooks. Covariates include state and year fixed effects.

Table B.5: ACS, UPK*I{Child Ages} on Mothers' Hours, Married

		Weekly Work Hours			
	Worked	10+ Hrs	20+ Hrs	30+ Hrs	40+ Hrs
PreK*I{Has 4-5 Year Old}	0.000	0.000	-0.009	-0.020***	-0.013***
	(0.007)	(0.008)	(0.006)	(0.007)	(0.004)
PreK*I{Has 2-3 Year Old}	-0.003	0.003	0.003	-0.002	-0.007
	(0.008)	(0.010)	(0.011)	(0.012)	(0.006)
PreK*I{Has 6-7 Year Old}	0.004	0.002	-0.004	-0.008	-0.004
	(0.011)	(0.012)	(0.012)	(0.012)	(0.003)
N	1041465	1041465	1041465	1041465	1041465

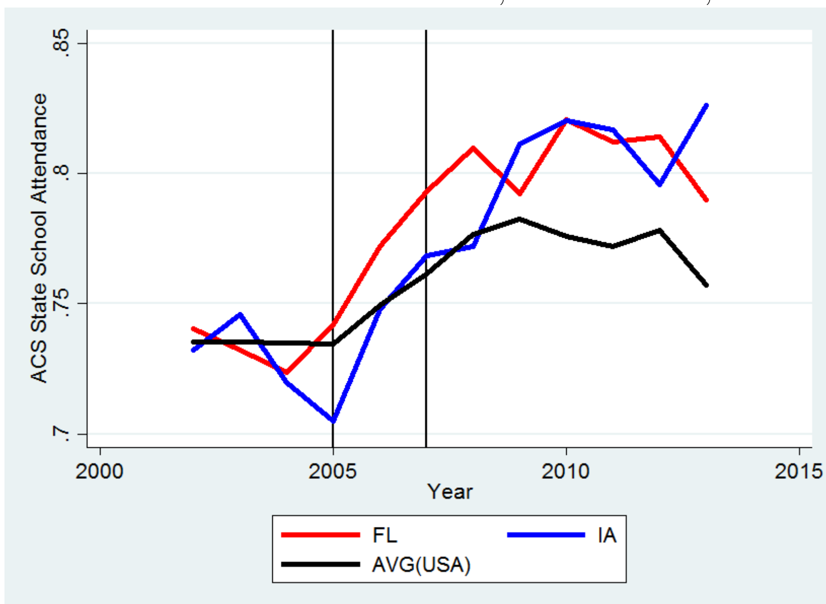
State enrollment levels via NIEER annual The State of Preschool yearbooks. Covariates include state and year fixed effects. Standard errors clustered by state and using national-level individual weight in parentheses.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Figure B.9: State Pre-K Enrollment and UPK Status



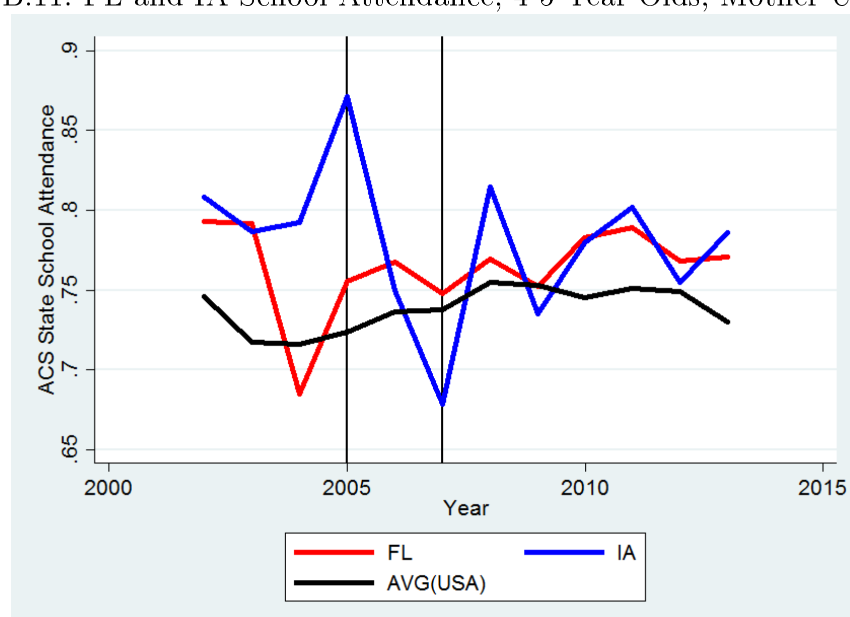
State enrollment levels via NIEER annual The State of Preschool yearbooks

Figure B.10: FL and IA School Attendance, 4-5 Year Olds, Mother Married



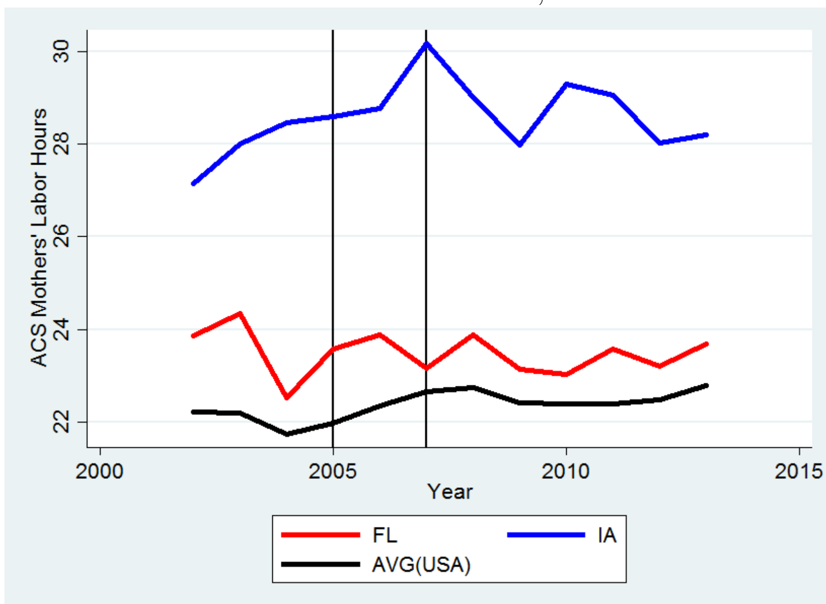
Average of ACS School Attendance, Age 4-5 years, Mother married

Figure B.11: FL and IA School Attendance, 4-5 Year Olds, Mother Unmarried



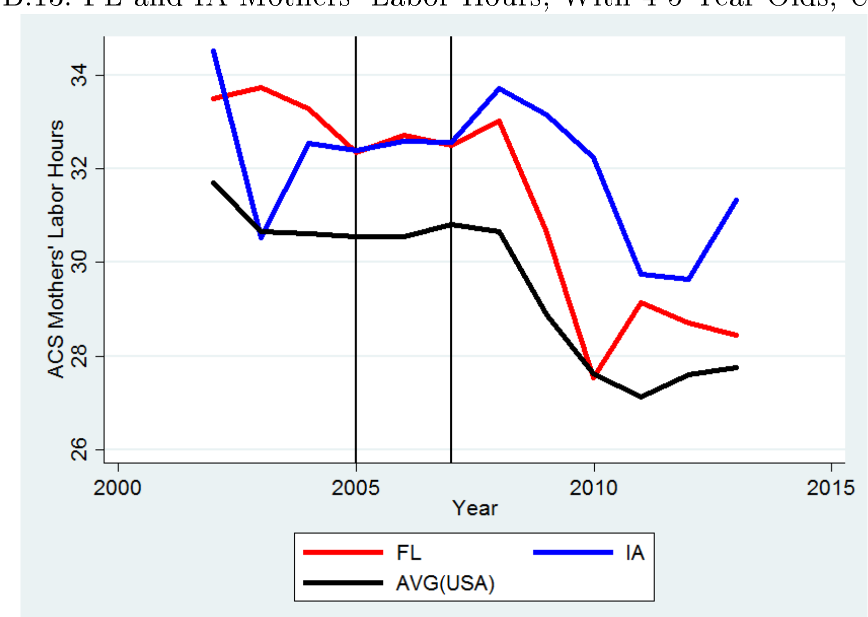
Average of ACS School Attendance, Age 4-5 years, Mother married

Figure B.12: FL and IA Mothers' Labor Hours, With 4-5 Year Olds, Married



Average of ACS Weekly Labor Hours, With child age 4-5 years, married

Figure B.13: FL and IA Mothers' Labor Hours, With 4-5 Year Olds, Unmarried



Average of ACS Weekly Labor Hours, With child age 4-5 years, unmarried

Table B.6: UPK on State Pre-K Enrollment of 4-Year-Olds

	State Pre-K Enrollment	
UPK	0.399***	0.377***
	(0.054)	(0.105)
N	600	600
STATE FE	N	Y
YEAR FE	N	Y

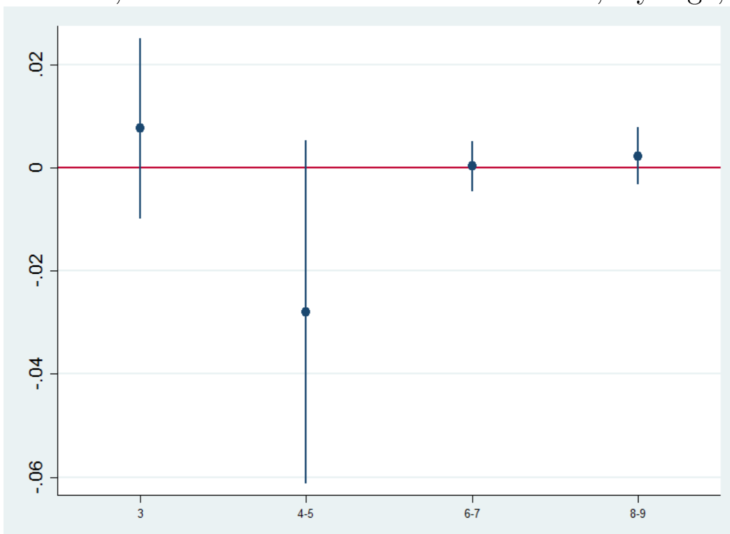
*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*

Table B.7: ACS, UPK on Schooling, Aged 4-5

	Child School Attendance By Mothers' Marital, Edu				
	Unmarried All	Married All	Married No College	Married Some College	Married 4-Year Degree
UPK	-0.028*	0.036***	0.037***	0.033***	0.037***
	(0.017)	(0.004)	(0.005)	(0.010)	(0.005)
N	110589	481099	157057	118960	205082

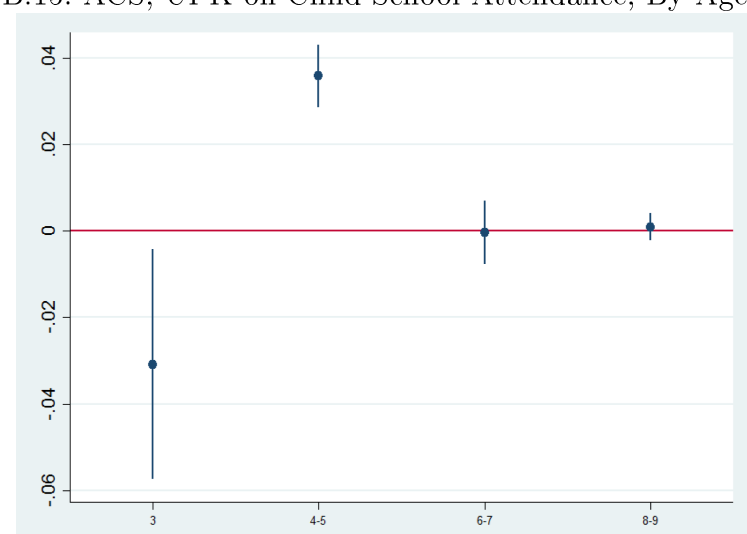
*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for oldest/youngest child status

Figure B.14: ACS, UPK on Child School Attendance, By Age, Unmarried



Estimated coefficients, with vertical bars showing 95% confidence interval. Additional covariates include year, mother's age, education, state, and indicators for oldest/youngest child status

Figure B.15: ACS, UPK on Child School Attendance, By Age, Married



Estimated coefficients, with vertical bars showing 95% confidence interval. Additional covariates include year, mother's age, education, state, and indicators for oldest/youngest child status

Table B.8: SIPP, UPK on Mothers' Hours, By Age & Mothers' Marital Status

	Weekly Work Hours				
	Worked	10+ Hrs	20+ Hrs	30+ Hrs	40+ Hrs
	Married, With 4-5 Year Old				
UPK	-0.006	-0.009	-0.010	-0.030**	-0.027***
	(0.011)	(0.012)	(0.014)	(0.012)	(0.005)
N	50623	50623	50623	50623	50623
	Unmarried, With 4-5 Year Old				
UPK	-0.003	0.008	0.008	0.022	-0.007
	(0.014)	(0.016)	(0.015)	(0.019)	(0.008)
N	35818	35818	35818	35818	35818
	Married, No 4-5 Year Old (Has 2-3 Year Old)				
UPK	0.004	0.015	0.016	0.029	-0.006
	(0.015)	(0.016)	(0.016)	(0.022)	(0.007)
N	35818	35818	35818	35818	35818
	Married, No 4-5 Year Old (Has 6-7 Year Old)				
UPK	-0.017	-0.024	-0.033	-0.038	-0.021
	(0.037)	(0.031)	(0.041)	(0.038)	(0.017)
N	35953	35953	35953	35953	35953

Standard errors clustered by state and using national-level individual weight in parentheses.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Table B.9: SIPP, UPK*I{Child Ages} on Mothers' Hours, Married

	Weekly Work Hours				
	Worked	10+ Hrs	20+ Hrs	30+ Hrs	40+ Hrs
UPK*I{Has 4-5 Year Old}	-0.005	-0.009	-0.009	-0.012	-0.010***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.004)
UPK*I{Has 6-7 Year Old}	-0.007	-0.011	-0.015	-0.018	-0.012***
	(0.013)	(0.013)	(0.014)	(0.014)	(0.003)
UPK*I{Has 2-3 Year Old}	-0.013	-0.011	-0.014	-0.011	-0.010**
	(0.010)	(0.012)	(0.012)	(0.013)	(0.004)
N	113502	113502	113502	113502	113502

*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Table B.10: SIPP, UPK on Mothers' Hours, Married, With 4-5 Year Old, By Season

	Weekly Work Hours				
	Worked	10+ Hrs	20+ Hrs	30+ Hrs	40+ Hrs
			Summer (June- Aug)		
UPK	0.017	0.023	0.034	0.008	-0.016*
	(0.015)	(0.015)	(0.022)	(0.022)	(0.008)
N	12662	12662	12662	12662	12662
			NOT Summer (Sept-May)		
UPK	-0.014	-0.020	-0.025*	-0.042***	-0.030***
	(0.012)	(0.012)	(0.014)	(0.011)	(0.005)
N	37961	37961	37961	37961	37961
			Fall (Sept-Nov)		
UPK	-0.084***	-0.083***	-0.101***	-0.127***	-0.011*
	(0.020)	(0.021)	(0.030)	(0.022)	(0.006)
N	12529	12529	12529	12529	12529

*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Table B.11: SIPP, UPK on Mothers' Hours, With 4-5 Year Old, By Edu, School Year

	Weekly Work Hours				
	Worked	10+ Hrs	20+ Hrs	30+ Hrs	40+ Hrs
UPK*I{No College}	-0.029	-0.039	-0.053**	-0.069**	-0.026***
	(0.029)	(0.028)	(0.025)	(0.031)	(0.009)
UPK*I{Some College}	-0.044*	-0.049**	-0.050*	-0.068**	-0.015**
	(0.023)	(0.021)	(0.029)	(0.029)	(0.007)
UPK*I{4 Years+ College}	0.030	0.024	0.023	0.005	-0.047***
	(0.020)	(0.019)	(0.022)	(0.022)	(0.008)
N	37961	37961	37961	37961	37961

*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Table B.12: SIPP, OLS UPK on Mothers' Hours, By Child Ages, Married

	Weekly Work Hours		
	Has 4-5 y/o	NO 4-5 y/o	
		2-3 y/o	6-7 y/o
	Full Year		
UPK	-0.626	0.499	-1.330
	(0.467)	(0.606)	(1.534)
N	50623	35818	35953
	Fall (Sept-Nov)		
UPK	-3.533***	1.665	-1.508
	(0.920)	(1.115)	(1.051)
N	12529	8806	8891
	Summer (Jun-Aug)		
UPK	0.731	2.088**	-1.589
	(0.678)	(0.892)	(1.547)
N	12662	8974	9014

*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Table B.13: SIPP, OLS UPK on Mothers' Hours, By Child Ages, Married

	Weekly Work Hours		
	Full Year	Fall	Summer
UPK*I{Has 4-5 Year Old}	-0.461	-1.293**	-0.636
	(0.365)	(0.601)	(0.537)
UPK*I{Has 6-7 Year Old}	-0.587	-0.901	-0.412
	(0.504)	(0.745)	(0.557)
UPK*I{Has 2-3 Year Old}	-0.555	-0.299	-1.128
	(0.460)	(0.720)	(1.048)
N	113502	28019	28443

*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Table B.14: SIPP, UPK on Childcare Use

	Percent of Families Using CC Method				
	School	Informal	Formal	Parent	Home
	With 4-5 Year Old				
UPK	0.044***	-0.007	-0.071***	0.165***	-0.112***
	(0.010)	(0.017)	(0.011)	(0.038)	(0.019)
N	7158	7158	7158	7158	7158
	NO 4-5 Year Old, With 2-3 Year Old				
UPK	0.005	0.047***	-0.053***	0.032	0.022
	(0.011)	(0.013)	(0.015)	(0.030)	(0.029)
N	5095	5095	5095	5095	5095

*Standard errors clustered by state and using national-level individual weight in parentheses. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively.*
Covariates include year, mother's age, education, state, and indicators for child ages.

Appendix C

SMART Appendix

Table C.1: 79CYA Summary Statistics, 2012

Variable	Obs	Mean	Std. Dev.	Min	Max
SMART-Aged	783	0.627	0.484	0	1
SMART-Aged, Low Income	783	0.231	0.422	0	1
SMART-Aged, Low Income, College	783	0.199	0.400	0	1
Working in Stem Field (Last)	783	0.153	0.360	0	1
Working in STEM Field (Ever)	783	0.225	0.418	0	1
STEM Major (Jr or Sr Year)	783	0.174	0.379	0	1
STEM Major (Ever)	783	0.276	0.447	0	1
Year of Birth	783	1984.185	3.161	1972	1988
Years of Education	783	16.204	1.478	12	20
4 Yrs College +	783	0.736	0.441	0	1
Some College	783	0.230	0.421	0	1
No College	783	0.034	0.183	0	1

STEM major as defined by SMART program. STEM Field as observed by occupational choice in 2012.

Table C.2: 79CYA Summary Statistics - Aid Received, 2012

Variable	Obs	Mean	Std. Dev.	Min	Max
Work Study	783	0.133	0.340	0	1
Scholarship	783	0.374	0.484	0	1
Assistanceship	782	0.403	0.491	0	1
Grant	783	0.022	0.146	0	1
Waiver	783	0.046	0.210	0	1
Fellowship	783	0.011	0.107	0	1
Loan	783	0.548	0.498	0	1
Loan Amount ($)	764	3584.875	6303.605	0	81022.930
Loan Amount (If Positive)	376	7284.162	7336.914	363.629	81022.930

Aid received as college junior or senior.

Figure C.1: STEM and Low-Income by Year (Upperclassmen)

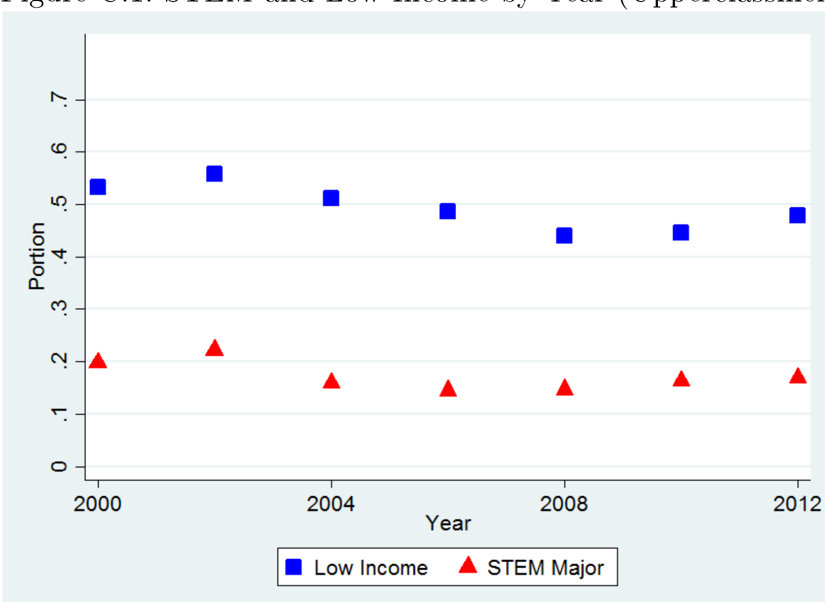


Table C.3: Income Cutoff vs. Funding Received

	WorkStudy	Scholarship	Assistantship	Grant
LowInc	0.029	-0.029	-0.009	0.124**
	(0.035)	(0.050)	(0.013)	(0.051)
N	1390	1390	1385	1384

	Waiver	Fellowship	HaveLoans	LoanAmount
LowInc	0.039**	-0.001	-0.034	215.997
	(0.020)	(0.014)	(0.050)	(677.899)
N	1390	1389	1394	1290

LowInc defined by family-size X income cutoffs. Binary dependant variables for having received aid of each type (except loan amount which is current owed adjusted to USD-2009). College juniors and seniors only.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table C.4: SMART vs. STEM for College Graduates

	Years Edu	Major JrSr	Major Ever	OCC Last	OCC Ever
LowInc X JrSr	0.249	0.169	0.186	0.110	0.165
	(0.422)	(0.109)	(0.117)	(0.106)	(0.118)
LowInc X JrSr X SMART	0.229	-0.036	-0.094	0.002	-0.095
	(0.261)	(0.068)	(0.079)	(0.063)	(0.073)
N	726	726	726	726	726

LowInc defined by family-size X income cutoffs. Education measured in total years as of last survey. Others binary dependent variables based on STEM status.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table C.5: SMART vs. STEM Major (JrSr), by Mothers' Race and Edu

LowInc X JrSr	0.315	0.141	0.107	0.079	0.211
	(0.255)	(0.128)	(0.408)	(0.361)	(0.171)
LowInc X JrSr X SMART	-0.028	-0.035	0.167	0.268	-0.119
	(0.120)	(0.093)	(0.290)	(0.224)	(0.109)
N	283	443	119	166	441
Mother's Edu	No College	Some College	Any	Any	Any
Mother's Race	Any	Any	Hispanic	Black	Other

*LowInc defined by family-size X income cutoffs. SMART defined by program years. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table C.6: SMART x STEM vs Occupation

	Yrs Edu	STEM OCC Last	STEM OCC Ever
STEM-Maj	0.300*	0.298***	0.440***
	(0.165)	(0.050)	(0.051)
LowInc X JrSr	0.064	0.027	0.077
	(0.257)	(0.066)	(0.075)
LowInc X JrSr X SMART	0.127	0.009	-0.052
	(0.246)	(0.059)	(0.067)
LowInc X JrSr X SMART X STEM-Maj	0.447	-0.220**	-0.352***
	(0.375)	(0.096)	(0.101)
N	725	725	725

*LowInc defined by family-size X income cutoffs. SMART defined by program years. STEM major based on that declared as upperclassman. ***,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table C.7: SMART x STEM vs Occupation

	STEM OCC (Last)	
STEM-Maj	0.228***	0.366***
	(0.075)	(0.069)
LowInc X JrSr	0.068	0.095
	(0.097)	(0.099)
LowInc X JrSr X SMART	0.047	-0.027
	(0.087)	(0.086)
LowInc X JrSr X SMART X STEM-Maj	-0.286**	-0.252*
	(0.138)	(0.131)
	STEM OCC (Ever)	
STEM-Maj	0.376***	0.498***
	(0.077)	(0.065)
LowInc X JrSr	0.158	0.095
	(0.120)	(0.113)
LowInc X JrSr X SMART	-0.076	-0.043
	(0.100)	(0.098)
LowInc X JrSr X SMART X STEM-Maj	-0.304*	-0.439***
	(0.167)	(0.124)
N	283	442
Mothers' Education	No College	Some College

LowInc defined by family-size X income cutoffs. SMART defined by program years. STEM major based on that declared as upperclassman.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table C.8: SMART x STEM vs Funding

	WorkStudy	Scholarship	Assistantship	Grant
STEM-Major	0.012	0.032	0.035	0.022
	(0.041)	(0.055)	(0.052)	(0.019)
LowInc X JrSr	0.125**	-0.057	0.123	-0.030
	(0.062)	(0.084)	(0.084)	(0.023)
LowInc X JrSr X SMART	-0.116*	0.037	0.016	0.023
	(0.064)	(0.081)	(0.082)	(0.023)
LowInc X JrSr X SMART X STEM	-0.071	0.079	0.024	0.150
	(0.100)	(0.129)	(0.133)	(0.091)
N	725	725	725	725

	Waiver	Fellowship	HaveLoans	LoanAmount
STEM-Major	-0.035	0.000	0.040	2051.801**
	(0.023)	(0.012)	(0.054)	(1014.644)
LowInc X JrSr	0.082*	-0.029	-0.117	-663.503
	(0.045)	(0.029)	(0.084)	(982.389)
LowInc X JrSr X SMART	-0.094**	-0.000	0.035	-380.379
	(0.045)	(0.026)	(0.083)	(989.165)
LowInc X JrSr X SMART X STEM	-0.018	-0.003	0.019	-554.609
	(0.033)	(0.017)	(0.124)	(2186.646)
N	725	725	725	707

LowInc defined by family-size X income cutoffs. SMART defined by program years. STEM major based on that declared as upperclassman.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

Table C.9: SMART x STEM vs Grant (By Mother's Educational Attainment)

	Received Grant	
STEM-Maj	0.008	0.032
	(0.021)	(0.025)
LowInc X JrSr	-0.103**	0.018
	(0.044)	(0.038)
LowInc X JrSr X SMART	0.003	0.034
	(0.031)	(0.037)
LowInc X JrSr X SMART X STEM-Maj	0.336**	0.038
	(0.161)	(0.094)
N	283	442
Mothers' Education	No College	Some College

LowInc defined by family-size X income cutoffs. SMART defined by program years. STEM major based on that declared as upperclassman.
****,**, and * indicate statistical significance at 1%, 5%, and 10% confidence levels respectively*

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