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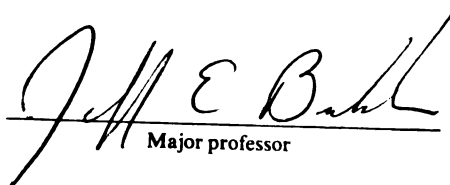
Three Essays on Labor Supply and
Education

presented by

Ali Murat Berker

has been accepted towards fulfillment
of the requirements for

Ph.D. degree in Economics


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THREE ESSAYS ON LABOR SUPPLY AND EDUCATION

By

Ali Murat Berker

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ABSTRACT

THREE ESSAYS ON LABOR SUPPLY AND EDUCATION

By

Ali Murat Berker

This research aims to explore the impacts of state-level public policies on labor market and educational outcomes. In order to address the needs of their residents and increase their economic well-being, states have implemented various policies concerning tuition subsidy and scholarship for college education, and the provision of early childhood education and child care, among others. In this research, I evaluate three specific state programs in two important dimensions. First, I assess whether programs improve labor market and educational outcomes of their residents. Second, I examine to which extent the programs have generated unanticipated and/or undesirable consequences.

In the first chapter, I examine whether the labor market behavior of married mothers is influenced by the potential cost of college education for their child. Since family labor market behavior and the decision to send the child to college are jointly determined, the key issue in estimating the causal effects of the cost college education on the labor market behavior of married mothers is identification. I use within-state and cross-state variation in 4-year public colleges to estimate the cost of college education on

the employment decision of mothers. The results suggest that married mothers of college-age children increase their labor supply to finance their children college education as the cost of college education rises.

In chapter 2, I turn attention to the fact that in the past decade, several states have implemented merit-based scholarship programs, which provide a tuition subsidy for students on the basis of their academic achievement, regardless of their family income. This chapter explores the impacts of the shift in the financial aid delivery system from need-based aid to merit-based aid on access to and choice of college. I exploit an exogenous variation generated by the introduction of the state-funded merit aid across states and over time to identify the causal effect of the financial aid on the college-going behavior of students. The empirical analysis leads to evidence that HOPE-like programs boost the enrollment rate at 4-year public colleges. I also provide evidence to that HOPE-like programs improve educational outcomes in the secondary education.

The last chapter of dissertation focuses on the possible employment effect of early childhood education programs that certainly deserve attention from a public policy perspective. I exploit differences in the availability of prekindergarten programs within a state, across states and over the 1990-2000 time period to identify the effects of child care costs on the employment decision of mothers. The findings suggest that the provision of free prekindergarten programs helps single mothers of eligible children for these programs into employment, while the programs do not affect the employment decisions of married mothers.

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INTRODUCTION

This research aims to explore the impacts of state-level public policies on labor market and educational outcomes. In order to address the needs and increase the economic well-being of their residents, states have implemented various policies concerning tuition subsidy and scholarship for college education, and the provision of early childhood education and child care, among others. In this research, I evaluate three specific state programs in two important dimensions. First, I assess whether the programs improve labor market and educational outcomes of their residents. Second, I examine the extent to which the programs have generated unanticipated and/or undesirable consequences.

Furthermore, states differ in how they implement public policies. The exogenous differences in public policies across states and over time studied here provide me unique quasi-natural experiments to test economics theories and identify causal effects on individual behavior and on labor market and education outcomes without relying on arbitrary assumptions about functional form and exclusion restrictions.

Chapter 1 examines whether the labor market behavior of married mothers is influenced by the potential cost of college education for their child. Since family labor market behavior and the decision to send the child to college are jointly determined, the key issue in estimating the causal effects of the cost of higher education on the labor market behavior of married mothers is identification. I use within-state and cross-state variations in tuitions at 4-year public colleges over the 1990-1993 time period to estimate

the effects of the cost of higher education on the employment decision of married mothers.

The results from within-state and cross-state estimators indicate that among families that have a high “propensity to send a child to college” score, married mothers of college-age children choose to work more as the cost of higher education rises.

Furthermore, the results suggest that mothers from families who have lower “propensity to send a child to college” score either work less or do not change their labor market behavior.

In the specification analysis, I provide evidence that these findings remain robust to different assumptions and specifications, particularly to using alternative definitions of the group of married mothers with a high propensity score, and using state tuition as a binary variable to define states as high-tuition and low-tuition states.

In Chapter 2, I turn attention to the fact that in the past decade, several states have implemented merit-based scholarship programs, which provide a tuition subsidy for students on the basis of their academic achievement and ability regardless of their family income. This paper explores the impacts of the shift in the financial aid delivery system from need-based aid to merit-based aid on access to and choice of college. I exploit an exogenous variation generated by the introduction of the state-funded merit aid across states and over time to identify the effect of financial aid on the college-going behavior of students.

The empirical analysis in Chapter 2 leads to evidence that HOPE-like programs result in a 7.1 –11 percentage point rise in the enrollment among 18-19-year-olds at 4-year public colleges. The results also indicate that these programs do not have significant

positive effects on the enrollment rate among 18-19-year-olds at 2-year public colleges. This evidence suggest that these merit-based scholarship programs fail to expand access to higher education among 18-19-year-olds, or that movement of new college students into the 2-year public colleges is offset by movement of students from 2-year to 4-year institutions. Furthermore, I provide evidence to that HOPE-like programs improve educational outcomes in the secondary education, that is, high school graduation and dropout rates.

The analysis of the dynamics of HOPE-like programs indicates that the introduction of a HOPE-like program can be treated as an exogenous event. Finally, the results suggest that responses to the programs are not significantly different between white and black students except for the enrollment at 4-year private colleges.

In Chapter 3, I exploit differences in the availability of prekindergarten programs within a state, across states and over the 1990-2000 time period to identify the effects of child care costs on the employment decision of mothers. This study contributes to the existing literature in two ways. First, this study is unique in assessing the possible effects of the nationwide provision of early childhood education in helping low-income families to meet child care costs and participate in the labor market. It sheds light on the possible employment effect of other early childhood education programs that certainly deserve attention from a public policy perspective, such as the Head Start program. Second, the econometric approach used here does not require the arbitrary assumptions about functional form and the exclusion restrictions employed in many previous studies.

I find that the provision of free prekindergarten programs helps single mothers of eligible children for these programs to move into employment. The results indicate that

these programs do not affect the employment decision of married mothers. One source of this difference may be that married mothers tend to have more financial resources originating from family income, as well as different tastes for state-funded prekindergarten programs, compared to single mothers. In addition, in two parent families, the husband and perhaps his extended family become available as potential child care providers.

Finally, I find a negative effect of the introduction of the programs on welfare participation. However, these estimated effects are not significant, except the estimated effect of universal prekindergarten programs.

CHAPTER 1

DO MARRIED MOTHERS WORK MORE TO PAY FOR THEIR CHILDREN'S COLLEGE?

Over the past 30 years, the difference in median annual earnings between college graduates and high school graduates has increased from 24 to 56 percent among 25-34-year-old males.¹ During the same period, the percentage of 25-29-year-old individuals who had completed at least some college increased from 44 to 66 percent, while the percentage of those who obtained a college or higher degree rose from 22 to 32 percent.² Therefore, changes in the college-graduate earnings premium and enrollment in postsecondary institutions suggest that as the return to the college education has risen in the labor market, the demand for it has increased.

However, a report by the College Board (1999) documents that student and their families have found increasingly difficult to meet the cost of higher education. For example, since the 1989-90 school year, inflation adjusted tuition at 4-year public colleges and universities has increased 114 percent. At the same time, family incomes have not kept pace with this dramatic rise in the cost of college education. Tuition at 4-year public institutions has grown almost six times faster than average family income over the same period.

¹ National Center for Education Statistics (2000, pp.143).

² National Center for Education Statistics (2000, pp.154-156).

One possible consequence of increasing of tuition that outpaced the growth of average family income is that parents may participate in more labor market activities to contribute the financing of their children's college expenses. Since primary male workers may not be able to make large changes in their labor supply, women may make the largest adjustment. Therefore a relation between parents' labor supply and their children's college-going behavior and college expenses is most likely to show up as changes in female labor supply.

This study is the first to examine whether the labor market behavior of mothers is influenced by the potential cost of college education for their children. I exploit variation in state average tuition at 4-year public universities and colleges to estimate the effects of the cost of higher education on family labor supply.

The outline of this paper is as follows. In the next section, I present a simple static labor supply model to motivate my empirical framework. Section 3 describes the data, while Section 4 describes estimation methods and identification approaches. Results and various specification checks appear in Section 4. The final section reports the main conclusions.

I. Theoretical Framework

Consider a population of families with one child. Families obtain utility from consumption of goods, consumption of leisure, and a college education for the child. A utility function for a family is separable in the utility of the college education for the child. Thus, the single-period utility function for the family takes the following functional form

$$U(C, L_s, E, \varepsilon) = \alpha \log C + \beta \log(1 - L_s) + (\gamma + \varepsilon)E, \quad \alpha > 0, \beta > 0, \gamma > 0, \quad (1)$$

where C denotes the amount of consumption, L_s indicates labor supply and E is a binary variable that equals one if the family decides to send the child to college, and zero otherwise. The variable ε captures the differences in preferences (tastes) for college education as well as differences in parents' investment in their child's ability and academic preparation in pre-college years. In order to simplify the model, I assume that the father works full time and the mother allocates her total time between leisure and labor. In other words, I only allow the mother can change her labor market behavior in order to meet the child's college cost. Furthermore, I assume that families decide only between sending the child to college or not, that college choices have constant quality, and that the cost of higher education (tuition) is exogenous to the family decision making progress. Thus, the budget constraint of the family is

$$C + E \cdot T = W \cdot L_s + A \quad (2)$$

where T is the cost of higher education that the family has to incur to send their child to college, W is the market wage rate facing the mother, and A is the exogenous income of the family, including non-labor income and father's labor income.

Families for whom the marginal utility of education is greater than the cost ($\gamma + \varepsilon > T$) will chose to send their child to college. For these families, the labor supply behavior of the mother is described by a reservation wage and an hours of work function

$$L_s = 0 \quad \text{if} \quad W < (\beta / \alpha)(A - T) \quad (3a)$$

$$L_s = (\alpha W + \beta T - \beta A) / (\alpha + \beta) \quad (3b)$$

The labor supply of mothers from families for whom the marginal utility of education is less than the cost likewise can be described by two equations:

$$L_S = 0 \text{ if } W < (\beta / \alpha)(A) \quad (4a)$$

$$L_S = (\alpha W - \beta A) / (\alpha + \beta) \quad (4b)$$

Thus, we can consider four groups of mothers: those who work and send their child to college, those who do not work and but send their child to college, those who work and do not send their child to college, and those who neither work nor send their child to college. A first thing to notice is that the reservation wage is lower and the level of labor supplied at any wage is higher for mothers from families that plan to send their child to college.

Now, consider the effect of an increase in tuition T on the total labor supply and the participation rate for the population. The labor supply of mothers from families who were not planning to send their child to college will be unaffected, as T does not enter into their reservation wage or labor supply function. For the families that were planning to send their children to college, two types of effects are possible. For some families, the increase in T will be sufficient to discourage them from sending their child to college, as T rises above $(\gamma + \epsilon)$. The mother's labor supply will switch from the regime described by equation 3 to that described by equation 4. If the mother was not working, she will not enter the labor force, for her reservation wage will increase. If the mother was working, her labor supply will decline and she may leave the labor force. For other families, the increase in T is not sufficient to deter them from sending their child to college. Equation 3 still defines the labor supply of mothers from these families, so that the reservation

wage decreases (thus inducing some non-workers to participate) and the labor supply of those mothers already working increases.

Thus, the increase in tuition has an ambiguous effect on mothers' labor supply in the population as a whole. Some give up on the idea of sending their child to college, and thus give up some or all of the work they were doing to help pay tuition. Others keep the goal of sending the child to college, but must enter the labor force or supply more labor to achieve the goal. The analysis of a decline in T proceeds in the same fashion, and likewise leads to an ambiguous result.

II. Data

In this study, I use three different data sets to estimate the effect of the cost of college education on the labor market behavior of married mothers. The first data set is the March Current Population Survey (CPS) for the years 1990 through 1993. It provides extensive individual information on demographic characteristics (such as race, age, marital status, number of children), family income, and labor market experiences.

In order to gauge the impact of the cost of higher education on the labor market behavior of married mothers, I need to identify married mothers with college-age children, and to have information on their children's college-going behavior. Unfortunately, the March CPS does not consistently provide this information for the sample of interest. I can only observe college-age children's schooling behavior and their mother's labor outcomes together for families for which college-age children live with their parents. Therefore, the analysis may be profoundly contaminated by the decision of college-age children to reside with their parents or not. This structure of the March CPS

prohibits me from having clean estimates of the impact of the cost of higher education on married mothers' employment decision.

In order to address this problem, I focus on married mothers whose oldest children in the household are between 14 and 17 years old. Since this age group of children is close to possible college enrollment, their mothers are more likely than mothers of younger children to be making changes in their labor market behavior in order to finance their children's college expenditures; I refer to these mothers as the treatment group (potential college students). The control group is all other married mothers whose oldest children are 6 to 14 years old.

The possible problem with the assignment of married mothers into either the treatment or the control group based on the age of the oldest child in the household is that the age of that child may not be the age of the oldest child in the family: there may be an older child attending college. The extent of the problem is more severe for the comparison group than for the treatment group. For example, if a married woman whose oldest child in the household is between 14 and 17 years old has a 20-year-old child in college, she still belongs in the treatment group, as her labor supply represents that of a woman who needs to finance her children's college expenses. However, it is possible that a married woman whose oldest child in the household is 13 years old may have an 18-year-old child in college. Assigning this married mother into the control group is a misclassification that would bias the estimated effect of the cost of higher education downward.

Because of this problem with the March CPS, I also use the Survey of Income and Program Participation (SIPP) for the years 1990 through 1993. The SIPP is a panel

survey: each household is interviewed every 4 months for over 30 months. The SIPP basically consists of two parts (SIPP: User's Guide, 1991). In the first part, the "core" of the survey collected information on demographic characteristics, labor market activities, social program participation, and income and asset ownership for every interview. The second part, a "topical module", provides detailed information on various aspects of the social and economic well being of individuals, and personal histories, such as assets and liabilities held, school enrollment, and fertility. A different topical module is administered at each interview.

Similar to the March CPS, the SIPP collects information on individuals who live in surveyed households. In addition, individuals who leave the surveyed household are not followed if they do not live with any of the original sample persons. This makes it impossible to use the panel data nature of the SIPP to estimate the cost of higher education on labor market behavior of married mothers. However, the SIPP has a Fertility History Topical Module, in which women are asked whether they have children or not as well as when their first and last children were born. Using the birth date of the first children for married mothers, I can observe more accurately who has college-age children and who does not. The advantage of using the SIPP is that I can identify married mothers whose oldest children are between 18 and 22 years old, and thus can be considered potential college students. I use these mothers as the SIPP treatment group, and again define the control group as all other married mothers with oldest children older than 6 and less than 14.

For the March CPS and the SIPP, I apply the following restrictions to obtain the samples used in the study. For the CPS, I keep women if they are married, have at least

one child in the household older than six and younger than eighteen. With the SIPP, I keep only married women whose first born child is older than six and younger than twenty-two. I then exclude married mothers who are older than 55 years of age to minimize the possible effects of retirement decisions on the labor market behavior of married mothers in the sample. Third, I eliminate married mothers who are below the age of 25, because the schooling decisions of these mothers might affect their labor market behavior. Finally, I merge both the March CPS and SIPP samples with state-year specific information on the state-level unemployment rate and the state average tuition level at four-year public colleges.

In order to measure married mothers' response to the cost of higher education, I focus on two measures of labor market behavior: the first is an indicator for the labor force participation taking value of one if the individual works more than zero hours, and zero otherwise; the second is the actual hours of work for the surveyed week. These two measures of the labor market outcome for the SIPP come from the first month of Wave 2 in which the Fertility History Topical Module is conducted. I use a probit for the labor market participation and a simple linear model for the hours of work. All standard errors are corrected for correlation and heteroscedasticity within-state year cells.

To control for individual characteristics that affect the labor market behavior of married mothers, I include the following demographic variables: age, age squared, a set of dummy variables for education level (high school dropout, high school graduate, some college years and college graduate and beyond), a dummy variable for race (=1 if white), a dummy variable for living in urban areas (=1 if urban), unearned family income and husband's income, number of children aged between 0 and 5-year-old, number of

children between 6 and 15 years old, and number of children between 16 and 17-years-old. All monetary values used in the analysis are expressed in 1999 dollars using the Consumer Price Index.

As I noted before, the education attainment of children depends on various factors that are highly correlated with family income and the education of parents. Having information on a family's propensity to send their children to college aids the empirical analysis in two possible ways: first, I can focus the behavior of married mothers of families who are likely to have a strong desire to send their children college to minimize bias due to the correlation between college attendance and family background. Second, I can use information on the probability that a family sends their children to college to identify the effect of the cost of higher education on the labor market behavior of married mothers by comparing the differences in labor market outcomes between married mothers from families with a high probability and those with a low probability of sending their children to college across the state average tuition levels.

To help identify families with a strong desire or likelihood to send children to college, I use the National Education Longitudinal Study of 1988 (NELS: 88). The base year survey was 1988, when students were in the eight grade. These students were surveyed three times at-two year intervals following the base year survey. The second follow-up surveys were conducted in 1992, when they were in the second term of their senior year. The third follow-up surveys were conducted in 1994, two years after the majority of the sample completed their high school.

For each survey year, the NELS provides extensive information on education, background characteristics, measure of cognitive ability, and family income for students.

In addition, it collected information on their families and school. Using the second and third follow-up, I can observe college education outcomes for the high school graduates of 1992 and estimate a probit model of the probability of college attendance for such sample. Since I plan to use the estimated coefficients from the sample of the NELS to predict the probability of college attendance for each married mother's children in the March CPS and SIPP, I have to use the same set of explanatory variables for each data set. This led me to use a set of dummy variables for the mother's and the father's education level (high school dropout, high school graduate, some college years and college graduate and beyond), a set of dummy variables for family income level of 1991, a dummy variable for whether the student is white or not, and a dummy variable for whether the student lives in city or not. I restrict the sample to students who are high school graduates of 1992 and whose parents' information is available for the base year and the second-follow-up survey. The outcome of interest is a binary variable for whether the student enrolled in a 4-year college. The estimation results are presented in Table 1. The coefficients on the family income level and parents' education level have the expected signs. Educated parents are more likely to send their children to a 4-year college, and the probability of college attendance increases with family income.

III. Empirical Specification and Identification

As shown in the theoretical section, family labor market behavior and the decision to send children to college are jointly determined. Therefore, the key issue in estimating the effect of the cost of higher education on labor market behavior is identification. To elaborate the difficulties in identifying the effect of the cost of higher education on labor

supply, let us assume that we have the data on married mothers' labor market outcomes and their children's college-going behavior. In this hypothetical data set, the estimated equation would be

$$Y = \alpha_0 + \alpha_1 Collegekid + X\phi + \varepsilon, \quad (5)$$

where Y denotes labor market outcomes for married mothers, $Collegekid$ is a binary variable receiving a value of one if a child is enrolled in college, and zero otherwise. α_1 measures to what extent the cost of higher education impacts the employment decision of married mothers.

Here, we have a standard endogeneity problem. It is practically impossible to discern families in which married mothers choose a high level of labor supply because they are willing to send their children to college and to incur their children's college expenses, from families who choose to send their children to college merely because the married mothers enjoy a high level of labor supply, and therefore higher family income. That is, the causality can run in either direction. A good way to determine the direction of this causality would be to use panel data and track down the changes in married mothers' labor market outcomes before, during, and after their children's college attendance. Unfortunately, there is no data set available that provides this information.

An instrumental variables approach to the problem might use state average tuition at 4-year public colleges as an instrument for $Collegekid$, assuming that state average tuition at 4-year public colleges is uncorrelated with the unobserved determinants of the labor market behavior of mothers. However, as noted above, the existing data sets do not provide consistent information on mothers' labor market behavior and their children's

college attendance. Therefore, I cannot implement this instrumental variable estimation strategy.³

Instead, I adopt an alternative approach to using exogenous variation in public college cost across states to identify the causal effect of the cost of higher education on the employment decisions of married mothers. I use average tuition at 4-year public colleges to measure public college cost in a state. During the sample of period, from 1990 to 1993, the mean of state average tuition at 4-year public colleges was \$2679 (2612) and its standard deviation \$945 (831) in the March CPS (SIPP). These summary statistics show that there is significant variation in public college cost to identify the effects of interest. I begin my empirical analysis with the simple difference estimation for the sample of married mothers with potential college students:

$$Y = \beta_0 + \beta_1 Statetuition + X\varphi + \varepsilon, \quad (6)$$

where Y indicates labor market outcomes, $Statetuition$ is state average tuition at 4-year public colleges, and X represents the vector of demographic variables for married mothers. The coefficient of interest, β_1 , captures the impact of public college cost on labor supply of married mothers with potential college students.

In this model, in order to identify the parameter of interest, I use the cross-state variation in tuition at 4-year public colleges. However, since it is possible that state average tuition at 4-year public colleges may be correlated with the unobserved, state-specific determinants of labor market behavior of married mothers and their children's college-going behavior, the simple difference estimation may generate biased results.

³ In the future, I am planning to apply this estimation strategy as a new data set becomes available.

First, there may be state level heterogeneity in preferences over labor supply and the demand for higher education. For example, it may be possible that, in a high-tuition state, married mothers can have strong labor market attachments and high probability of sending their children to college compared to those married mothers living in a low-tuition state. Therefore, there may be a spurious correlation between state average tuition at 4-year public colleges and labor market outcomes in a state. Second, since states can differ in public policies other than differences in state average tuition at 4-year public colleges, the simple difference estimation results may not be reliable.

To deal with these possibilities, I use within-state control groups. This allows me to capture the state level heterogeneity in preferences over labor supply and the demand for higher education, and state differences in public policies that are permanent and common for all married mothers in a state. I can then apply a difference-in-differences framework for the sample of married mothers, estimating the following regression

$$Y = \lambda_0 + \lambda_1 Potcolstudent * Statetuition + \lambda_2 Potcolstudent + \lambda_3 Statetuition + X\varphi + \varepsilon, \quad (7)$$

where *Potcolstudent* is an indicator for whether a married mother has potential college students. The coefficient of interest, λ_1 , compares the responsiveness of labor supply to public college tuition levels of married mothers with potential college students to the responsiveness of labor supply to public college tuition levels of married mothers without potential college students.

Since the sample of married mothers whose oldest child is younger than potential college students serves as a within-state control group to capture state-specific factors that are assumed to be common to all married mothers in that state, the difference-in-differences estimator sweeps out state-specific factors, which may confound the analysis.

However, the estimated effect of the cost of higher education on the labor supply of married mothers may be biased due to the relationship between family income and college-going behavior. A number of studies analyzing the college-going behavior of children from different family backgrounds conclude that differences in access to the credit market, family environment and academic preparation in pre-college years, and derived utility from the consumption of college education, which are strongly correlated with family income and the education of parents, generate diverse probabilities of college attendance. Some families have little or no interest in sending their children to college, or feel (perhaps erroneously) that college education is not a realistic option for their children. For these families one would expect little or no causal effect of a change in the cost of a college education on the labor market behavior of married mothers. Conversely, only mothers from families who have a desire to send their children to college, and who are at least close to having the resources to realize this desire, are likely to change their labor supply behavior in response to changing college costs.

In order to address this concern, I make use of the probit model reported in Table 1. This model used family and parental background variables to explain college attendance behavior of high school graduates. Using the estimated coefficients of this model with the family and parental background information on the March CPS and SIPP samples, I identify those families that are types of families that would want to send a child to college. To be more precise, each family in the March CPS and SIPP is given a “propensity to send a child to college” score, by multiplying the coefficients from the probit model described in Table 1 by the appropriate variables for the March CPS and SIPP household. Households with high score are assumed to be those who plan to send a

child to college, and thus those most likely to consider college costs in making labor supply decisions. I use the top 10% of households with respect to this propensity score as a special sample in which the impact of changes in college cost on labor supply is likely to be larger.

I then re-estimate the simple difference (Eq. (6)) and difference-in-differences (Eq. (7)) estimator for the sample of married mothers of potential college students with a high propensity to send their children to college.

Furthermore, using the predicted probabilities of college attendance, I can examine the difference in the responsiveness of labor supply to public college tuition level between married mothers of potential college students with a high propensity to send their children to college and those married mothers with a low propensity to send their children to college. In this difference-in-difference framework, married mothers of potential college students with a low propensity to send their children to college serve as a within-state control group in a state, controlling for state-specific differences. I estimate the following the difference-in-difference regression for the sample of married mothers of potential college students

$$Y = \kappa_0 + \kappa_1 \text{Collegebound} * \text{Statetuition} + \kappa_2 \text{Collegebound} + \kappa_3 \text{Statetuition} + X\phi + \varepsilon, \quad (8)$$

where *Collegebound* is an indicator for whether the propensity to send a child to college score for the married mother is in the top 10% of households. The coefficient of interest, κ_1 , measures the responsiveness of the labor supply to college costs of married mothers from families with potential college students and a high propensity to send their children to college, relative to married mothers from other families with potential college students.

As discussed earlier, the estimates in Eq. (7) measure the mean difference in labor market outcomes between married mothers with and without potential college students across states with different average tuition levels at 4-year public colleges. These estimators rest on the assumption average differences in labor supply of mothers whose children are of different ages are the same in all states, or at least uncorrelated with state tuition levels. I can relax this assumption by adding an additional difference that uses mothers who are not likely to send their children to college a within-state control group. I apply the difference-in-difference-in-difference estimation for the sample of married mothers with children, using the following regression

$$\begin{aligned}
Y = & \delta_0 + \delta_1 \text{Collegebound} * \text{Potcolstudent} * \text{Statetuition} \\
& + \delta_2 \text{Collegebound} * \text{Potcolstudent} + \delta_3 \text{Collegebound} * \text{Statetuition} \\
& + \delta_4 \text{Potcolstudent} * \text{Statetuition} + \delta_5 \text{Collegebound} * \text{Statetuition} \\
& + \delta_6 \text{Collegebound} + \delta_7 \text{Potcolstudent} + \delta_8 \text{Statetuition} + X\varphi + \varepsilon.
\end{aligned} \tag{9}$$

Finally, it is likely that states may have experienced different changes in their economic conditions. For example, some states may have been affected by recession differently than others. In the specifications presented above, the difference-in-differences and difference-in-difference-in-difference estimator net out state-specific effects if we assume that they are common to all married mothers in a state. Furthermore, I include the state-level unemployment rate in all specification to control the fact that states can differ in economic conditions, which may be correlated with the labor market behavior of married mothers, the state average tuition at 4-year public colleges, and, therefore, their children's college-going behavior. I also allow different effects of state economic condition on married mothers with potential college students, and married

mothers with a high propensity to send their children to college. Specifically, I include the interaction term between *Potcolstudent* and the state-level unemployment rate in Eq. (7); I include the interaction term between *Collegebound* and the state-level unemployment rate in Eq. (8); lastly I add a triple-interaction between *Potcolstudent*, *Collegebound* and the state-level unemployment rate to the previous two interaction terms and their level variables in Eq. (9).

IV. Basic Results

A. The March CPS

Table 2 presents the results on the estimated effect of the cost of higher education on the labor market behavior of married mothers, in the simple difference and difference-in-differences specifications. The point estimate of simple difference indicates that a \$1000 increase in tuition at 4-year public colleges (approximately a one standard deviation increase) is associated with an increase of 1.4 percentage points in labor market participation among married mothers with potential college students, while this difference is associated with 0.017 additional hours of work per week among those mothers. The simple difference estimate for labor market participation is significant at the one-percent level.

The difference-in-difference estimates suggest that married mothers with children approaching the age of college eligibility increase their labor market participation in response to a 1000 increase in tuition at 4-year public colleges, by 1.6 percentage point more than married mothers whose oldest child is below 14. The difference-in-difference

estimate for hours of work per week is 0.82. These two estimated effects are significant at the one-percent level.

In panels A and B Table 3, I restrict the samples to married mothers who have a high propensity to send their children to college. The simple difference estimate for this sample of married mothers suggests that the estimated effect of a \$1000 increase in tuition at 4-year public colleges on labor market participation rises to 3.1 from 1.4 percentage points, and it is significant at the six-percent level. Its effect on hours of work rises to 1.2 from 0.017 hours, and is significant at the three-percent level. As reported in panel B in Table 3, the difference-in-difference estimate for labor market participation is statistically significant at the five-percent level and suggests that a \$1000 increase in tuition at 4-year public colleges makes married mothers of potential college students with a high propensity to send their children to college 3.5 percentage points more likely to work, relative to married mothers of younger children with a high propensity to send their children to college. The estimated effect on hours of work is 2, and it is significant at the one-percent level.

Alternatively, using all married mothers with potential college students, I estimate how the impact of an increase in public college tuition on labor supply differs between married mothers of potential college students with a high propensity to send their children to college and those with a low propensity to send their children to college. The results are presented in panel C in Table 3. The estimated effect on labor market participation is 1.8 percentage points, but this effect is statistically insignificant. The estimated effect on hours of work is statistically significant and implies that married mothers of potential college students with a high propensity to send their children to

college work 1.1 hours more per week due to a \$1000 increase in tuition at 4-year public colleges.

Finally, panel D in Table 3 reports the estimated effects of the cost of higher education on labor market participation and hours of work in the difference-in-difference-in-difference specification. The estimated effect on labor market participation is 1.9 percentage points, but it is not statistically significant. On the other hand, the difference-in-difference-in-difference estimates suggests that married mothers of children approaching college age with high probability of sending their children to college increase their working hours by 1.1 hours to meet a \$ 1000 increase in tuition at 4-year public colleges, and this is significant at the six-percent level.

B. The SIPP

Next, I turn the estimated effect of the cost of higher education on the labor market behavior of married mothers using the SIPP. Columns 3 and 4 in Tables 2 and 3 present the estimated effect from the same specifications I used for the March CPS. All of the estimated effects of the cost of higher education, reported in panel A in Table 2 have positive sign and many are significant, suggesting that a \$1000 increase in tuition at 4-year public colleges leads married mothers of potential college students increase their labor market participation and work more hours. When I focus on married mothers of potential college students with a high propensity to send their children to college, the estimated effect ranges from 5 to 6.6 percentage points. This result is somewhat higher than the one I obtained from the March CPS.

As reported in Table 3, the estimated effect on working hours varies between 0.85 and 2.1 hours for married mothers of potential college students with a high propensity to

send their children to college. Among these reported estimates, the estimate in panel B is statistically significant. Lastly, it should be noted that the range of estimated effect on the hours of work is very similar between the SIIP and the March CPS.

Overall, while the estimated effect of the cost of higher education remains positive, it is concentrated on the labor market participation in the SIPP.

V. Specification Analysis

Since the difference-in-difference framework used in this analysis rests on arbitrary assumptions, it is important to assess to what extent the estimates remain robust to different assumptions and specifications. Specifically, I address various specification issues i) using a within-state estimator where the identification of the model rests on the changes in the labor market behavior of married mothers in a state before and after public college costs increases; ii) examining heterogeneous treatment effects of changes in college cost on the employment decision on married mothers iii) using alternative definitions of the group of married mothers with college-bound children (married mothers with a high propensity score); iv) using state tuition as a binary variable to define states as high-tuition and low-tuition states.

A. Within-State Estimator

The March CPS

So far, I have exploited the cross-state variation in tuition at 4-year public colleges to identify the impact of the cost of higher education on married mothers' employment decisions. Since that tuition at 4-year public colleges may be correlated with the unobserved, state-specific determinants of labor market behavior of married mothers

and their children's college-going behavior, I use a within-state control group resting on the assumption that state-specific effects are common to all married mothers.⁴ An alternative specification to control for these state-specific determinants would be to estimate a within-state estimator by using a model with fixed states effects. In this alternative specification, the source of identification for the impact of the cost of higher education comes from the changes in the labor market behavior of married mothers in a state before and after state average tuition at 4-year public college increases. Therefore, a within-state estimator requires a change in public college cost over time in a state to identify the effects of interest. During the years 1990 to 1993, several states experienced changes in tuition in 4-year public colleges. For example, Massachusetts observed the maximum increase in the public college cost that increased from \$3288 in 1990 to \$4800 in 1993. The average one-year change in the state average tuition at 4-year public colleges in the March CPS (SIPP) sample is \$445 (449), and its standard deviation is \$344(348). Although the variation in college cost over time is less than the variation across state, there is potentially enough variation in college cost in states over time to identify a within-state estimator. Furthermore, the comparison of the estimated coefficient of interest between two results allows me to assess the extent to which a within-state control group can capture state-specific differences, which may confound the analysis.

To obtain a within-state estimator, I include state and year dummies in Eq. (6), Eq. (7), Eq. (8), and Eq. (9). Within this framework, year dummies aim to capture economy-wide trends that may be correlated with married mothers' labor market

⁴ Furthermore, I assume that families do not sort themselves across states based on the cost of higher education for their children.

behavior and their children's college going behavior, reflecting structural shifts over time. State dummies control for state-specific effects, assuming that these state-specific effects do not vary over time in a state.

Tables 4 and 5 depict the similarity of the estimated effect of the cost of higher education on labor supply between cross-state estimators and within-state estimators. In the first column of Table 4, the estimated effect is positive and significant at the five-percent level, indicating that a \$1000 increase in tuition at 4-year public colleges increases the labor force participation of married mothers by 1.5 percentage points. Likewise, the estimated effect for hours of work is 0.78, and this effect is significant at the one-percent level.

In columns 1 and 2 of panel A in Table 5, I report the estimated effect of the cost of higher education, focusing on married mothers with a high propensity score. I find stronger effects of a \$1000 increase in tuition at 4-year public colleges on the employment decisions of married mothers. The estimated effect for labor force participation is 3.6 percentage points, and the estimated effect is significant at the five-percent level. For hours of work, the estimated effect is 2 and significant at the one-percent level.

Panel B in Table 5 report the results for married mothers with potential college students. The results indicate slightly weaker effects of the cost of higher education on the labor supply of married mothers. The estimated effect for labor force participation is 2.1 percentage points, and 1.25 for hours of work. While the former estimator is not significant, the latter estimator is significant at the five-percent level.

Finally, I report the difference-in-difference-indifference estimator in panel C of Table 5. The estimated effect is 1.7 percentage points, but this effect is not significant at the conventional level. On the other hand, the estimated effect is 1, and it is significant at the five-percent level.

In general, these results suggest the estimated effect of the cost of higher education on labor supply of married mother is robust to the choice between identification using cross-state variation in tuition at 4-year public colleges and identification using within state variation in tuition. The results also suggest that the within-state control groups perform well to pick up states-specific differences, indicating that specifications that exploit the cross-state variation in college cost may produce reliable estimates.

The SIPP

Finally, I analyze the extent to which within-state estimates of the impact of cost of higher education are different from cross-state (between-state) estimates for the SIPP. I report within-state estimates of the effect of the cost of higher education married mothers' employment decision using the SIPP in columns 3 and 4 of Tables 4 and 5.

As reported in column 3 of Table 4, the estimated effect is 2 percentage points and significant at the five-percent level. For hours of work, the estimated effect is 0.85, and it is also significant at the five percent level. Similar to the results obtained from the March CPS, the estimated effect is getting larger for mothers of potential college students with a high propensity score. For those mothers, the first panel in Table 5 reveals that the estimated effect is 8.6 percentage points for labor force participation and 2.9 for hours of work. They are significant at the one-percent level.

In the panel B of Table 5, I restrict the sample to married mothers with potential college students. The results suggest that married mothers increase their labor supply in response to a \$1000 increase in tuition at 4-year public colleges. However, these results are not statistically significant at the conventional level. Lastly, I report the estimation results for the specification in Eq. (9) in the last panel of Table 5. The results provide to evidence that a \$1000 increase in tuition at 4-year public colleges makes married mothers increase their labor supply. The estimated effect on labor force participation is 6 percentage points, and it is significant at the ten-percent level. The estimated effect on hours of work is 1.82, but it is not statistically significant. In general, the results for the SIPP contribute in providing evidence to the hypothesis that higher average state tuition at 4-year public colleges increases labor supply of married mothers.

B. Estimates of Heterogeneous Effects of Changes in College Cost

The results presented in previous sections imply positive effects of changes in college cost on mothers' labor supply. Furthermore, as I focus on the sample of married mothers of potential college students who would likely to send their children to college, the estimated effects become stronger. In this section, I take a closer look at the possible heterogeneity in the effect of changing college costs on mothers' labor supply by producing separate estimates for married mothers whose propensity scores for sending their children to college fall in different ranges. To be exact, I turn attention to married mothers with a low propensity score, and to married mothers whose children are likely to be on the college attendance margin. As explained in the theoretical section, since the former group of mothers would be less likely interested in sending their children to college, the expected effect of change in college cost on labor supply is anticipated to be

zero, or negative for those mothers. On the other hand, the estimated effect for the latter group of mothers cannot unambiguously predicted as two possible effect of change in college cost may offset each other. In the first effect, the change in college cost may lead some families not to the send their children to college, and thus they may not change or decrease their labor supply. Conversely, in the second effect, mothers from other families may still continue to send their children to college, and consequently to increase their labor supply in response to the increase in college cost. Thus, the net effects of a change in college cost on labor supply remains to be empirically determined for married mothers whose children are on the college attendance margin.

Now, I turn to the estimation results for married mothers with different propensity score to send a child to college using the difference-in-difference framework presented in Eq. (7). Specifically, I estimate Eq. (7) for each decile of propensity score to send a child to college for the March CPS. Likewise, for the SIPP I report the results for each two deciles of propensity score, considering that the number of observation is relatively small in each decile in the SIPP.

The March CPS

The first panel in Table 6 reports the results for the March CPS. The estimated effects for the first and second deciles are negligible and statistically insignificant. This finding supports to the hypothesis that the increase in college cost does not generate any labor supply effect for married mother who are least likely plan on sending their children to college.

However, somewhat surprisingly, I find that the estimated effect is positive and statistically significant for the third and fourth deciles. The estimated effects for labor

force participation are 4.8 and 3.2 percentage points, respectively. Likewise, the estimated effects for hours of work are 1.54 and 1.3, respectively. These results are not in expected direction, and puzzling.

For the 5th, 6th, 7th, 8th and 9th deciles, the estimated effect is negligible and statistically significant. As explained before, some families may decrease their labor supply in response to the increase in cost of college, while the other families increase their labor supply. The results suggests these two effects in the opposite direction can offset each other in estimating average effect of cost of college for mothers whose children are likely to be on (or close to) the college attendance margin.

Finally, the estimated effect is positive and statistically significant for married mothers from the top 10% households with respect to propensity score.

The SIPP

The second panel in Table 6 reports the same analysis for the SIPP. For the first quintile, the estimated effect is 2.4 percentage points for labor force participation, and 1.4 for hours work. However, they are not significant at the conventional level. For the second quintile, the estimated effects are negligible and statistically significant. The estimated effect for the third quintile is negative, but it is not statistically significant. This evidence suggest that the increase in college may be associated with the decrease in labor supply for married mothers whose children are on the college attendance margin.

However, the inference is weak. For the fourth quintile, the estimated effect is negligible, and statistically insignificant. As is the case for the March CPS, the estimated effect is stronger and statistically significant for the quintile. The estimated effect for labor force participation is 7.8 percentage points, and significant at the one-percent level. Likewise,

the estimated effect for hours of work is 2.54, and significant at the five-percent level. In general, these results substantiate the main hypothesis that the impacts of changes in college cost on labor supply is stronger for married mothers who are most likely to send their children to college.

C. Alternative Definitions of the College-Bound Children

The March CPS

In this section, I examine how the estimated effect of the cost of higher education on the labor market behavior of married mothers changes across the choice of different thresholds of the propensity to send their children to college, which I use to define the group of married mothers with a high propensity to send their children to college. I run the difference-in-differences and difference-in-difference-in-difference specifications using different thresholds of the propensity score, in turn, the 80th and 70th centile of the distribution of the propensity to send their children to college score. The results are presented in columns 1-4 in Table 7.

For the sample of married mothers of potential college students whose propensity score is in the 80th centile, the difference-in-difference estimate from Eq. (7) is 2 percentage points and 1.1 for labor market participation and hours of work. While the former estimate is significant at the twelve-percent level, the later estimate is significant at the four-percent level. However, the difference-in-difference estimate from Eq. (8) and the difference-in-difference-in-difference estimate from Eq. (9) is small and they are not significant.

In columns 3-4 in Table 7, where the 70th centile is used to define college-bound children, the difference-in-difference estimate from Eq. (7) is 1.3 percentage points for

labor market participation, and it is not statistically significant. The estimated effect on hours of work is 0.68 hours per week and it is significant at the eleven-percent level. The difference-in-difference estimate from Eq. (8) suggests that reducing the threshold of defining the group of married mothers with a high propensity to send their children to college to the 70th centile reversed the positive estimated effect of college costs among such married mothers to negative effect, although these estimates are not statistically significant. Furthermore, the negative point estimates remain in the difference-in-difference-in-difference estimation. This result suggests that when the loosened definition of the group of married mothers with a high propensity to send their children to college is used, the positive estimated effect of the cost of higher education is diminished. The possible explanation for this is that families with lower probability of sending their children college choose not to send their children to college and do not change or work less in response to a rise in tuition, otherwise they would have sent their kids to college and have undertaken more labor market activities in low tuition level. In other words, low tuition level as a form of college subsidy may encourages such families to send their children to college, and thus they are willing to increase their labor market efforts.

The SIPP

In columns 5-8 in Table 7, using the SIPP, I check the sensivity of results for different thresholds of the propensity score to define the group of married mothers with a high propensity to send their children to college. When I use the threshold of the 80th centile to identify married mothers with a high propensity score, the difference-in-difference estimators are slightly larger than the baseline results reported in Table 3. The estimated effect for labor force participation is 7.2 percentage points, and significant at

the one-percent level. When I turn to hours of work, the estimated effect is 2.42, and it is also significant at the one-percent level. On the hand, for the 70th centile each of the estimates remains to be positive and significant, but is smaller than the results in Table 3.

As reported in panel B in Table 7, for the threshold of the 80th and 70th centile the difference-in-difference estimates from Eqs.(7) vary between 3.7 and 2.7 percentage points for labor for participation among mothers of potential college students, but are not statistically significant. For hours of work, these estimates continue to be positive, but insignificant.

Finally, I present the difference-in-difference-in-difference estimates for the threshold of the 80th and 70th centile in panel C in Table 7. The estimated effects on labor force participation are 7 and 5.2 percentage points for the 70th and 80th centiles, respectively. These estimates are significant at the conventional level. For hours of work, the estimated effects are 2.1 and 1.3, and the former is statistically significant.

Unlike the results in the March CPS, the estimates using different threshold of the propensity score in the SIPP does not support the hypothesis that the effect of the cost of higher education is weakened when a lower threshold is used for identifying married mothers with high propensity score. Although, the estimated effects diminish as I use the lower threshold (the 70th centile), the positive estimated effects remain positive in all specification, and some of them are statistically significant.

D. Alternative Definitions of High-Tuition and Low-Tuition States

The March CPS

In the analysis, I use the ratio of the of the state average tuition at 4-year public colleges to the actual maximum award of Pell Grant award to assign states into group of

high-tuition or low-tuition states: If the state average tuition at 4-year public colleges exceeds 75% the actual maximum awards of Pell Grant, I define that as a high-tuition state, otherwise it is considered a low tuition state. The results are presented in Table 8. The coefficient of interest in Eq. (7) measures the mean difference in labor market outcomes between married mothers with and without potential college students in high tuition states relative, relative to those in low tuition states. As reported in panel A, the difference-in-difference estimation from Eqs. (7) implies that the estimated effect on the labor market participation is 2.6 percentage points and it is significant at the five-percent level. The estimated effect on the hours of work is 1.31 hours per week and is significant at the two-percent level.

Next, I turn to the sample of all married mothers with a high propensity to send their children to college in Table 9. Among such mothers, a living in high-tuition state makes married mothers with potential college students 9.6 percentage points more likely to participate in the labor market and work 5.93 hours more. These two estimated effects are significant at the one-percent level.

Using the sample of all married mothers with potential college students, I estimate the difference-in-difference specification in Eq. (8). In this specification, the coefficient of interest captures the mean difference in labor outcomes between married mothers of potential college students with a high propensity to send their children to college and those married mothers with a low propensity to send their children to college in high tuition states, relative to those living in low-tuition states. As reported in panel B in Table 9, the resulting estimate for labor market participation is 5.5 percentage points, and it is

significant at the eight-percent level. The resulting estimate for hours of work is 3.8 hours per week, and it is significant at the one-percent level.

Finally, in panel C in Table 9, I report the results using the difference-in-difference-in-difference estimation. The resulting the difference-in-difference-in-difference estimate is 7.4 percentage points for the probability of labor market participation, and it is significant at the five-percent level. The same estimator for hours of work is 4.37 hours per week and it is significant at the one-percent level.

In addition, I estimate the same specifications above using different thresholds of 85% and 100% Pell Grant award. Table 10 presents the estimation results. For the 85% threshold, the estimates are ranging from 2.8 to 10 percentage points for the probability of labor market participation. The estimated effect on the hours of work ranges from 1.45 to 5.32 hours per week. All these estimated effects are statistically significant. Changing threshold from 75 to 85% level results in overall increase in the estimated effect. On the other hand, for the 100% threshold, while the positive estimated effect of living in high-tuition states remains same across different specification. The estimated effects are somewhat getting smaller, and some of these effects become statistically insignificant.

The SIPP

I also use the ratio of the state average tuition at 4-year public colleges to the Pell Grant award to assign states into group of high-tuition or low-tuition states for the estimates using the SIPP. These results are reported in Table 9 for the 75% thresholds, most of estimated effects have positive signs, but only in panel A in both tables are the estimates significant at the five-percent level for both labor market participation and hours of work. The point estimate from the difference-in-difference-in-difference, which

is reported in panel C in Table 6, suggests that living in high-tuition states makes married mothers of potential college students with a high propensity to send their children to college increase their labor force participation and hours of work, 8.6 percentage points and 3.1 hours, respectively. However, the small number of observations of such mothers may cause relatively large standard errors, and thus less precisely estimated results.

Moreover, when I use the 85% thresholds of the Pell-Grant award in Table 10, the estimated effect on labor force participation varies between 15 and 7.4 percentage points for the mothers of potential college students with high propensity to send their children to college. For such married mothers, the range of the estimated effect on hours of work is 0.94 and 5.47 hours of work. For the 100% thresholds, the corresponding estimated effects vary between 10 to 4 percentage points, and between 4.3 and 1.1 hours. These estimation results reported in panel A and B are statistically significant at the one-percent, five-percent or ten-percent level for these two different thresholds of the Pell Grant.

VI. Conclusion

Although there is a great deal of research interest in the dynamics of the college-going behavior of children from different family and academic backgrounds, there is no previous research on the question investigated here. This can be explained by the fact that there is no data set that provides consistent information about parents' labor market behavior and their children's college outcome. In this paper, I combine information from several data sets to examine the effect of the cost of higher education on the labor market behavior of married mothers.

As demonstrated in the theoretical section, changes in the cost of higher education create ambiguous labor supply incentives for mothers of potential college students. I find that among families who have a high probability of sending their children to college, married mothers of college-age children choose to work more for financing their children's college education as the cost of higher education rises. In the difference-in-differences framework presented here, this means that such mothers supply more labor in high-tuition states than in low-tuition states. However, I argue that an increase in the cost of higher education may compel other families, who have low expectations about their children's educational attainment, to not send their children to college. Therefore, these married mothers are expected to either work less or do not change their labor market behavior in response to an increase in college cost. This expected effect is empirically supported by the point estimates showing that the positive effect on labor supply of the cost of higher education weakens for the lower threshold of the propensity score.

There are some limitations of this study, which are mainly due to either the structure of the available data sets or the framework of the difference-in-difference estimation, which uses cross-state variation in the policy variable. First, it should be noted that families' labor market response to the cost of higher education depends on the extent to which they value college education as a consumption good. It is likely that the consumption value of college is also a function of the quality of college chosen, and its prestige. Unfortunately, the available data sets limit my ability to evaluate the effects of the choice of college on the married mothers' employment decision.

Another limitation arises from the design of the difference-in-difference framework used in this study. Here, I assign married mothers in the treatment and control

group based on the definition of their propensity to send their children to college, and living in high-tuition or low-tuition states. However, it is likely that individuals are mistakenly assigned to one of these groups. Unfortunately, unlike in the case of the classical measurement error, I cannot determine a priori the direction of the bias in the estimates of the effect of the cost of higher education. This is because whether the individual belongs to the treatment or control group depends on the outcome of interest, her labor market behavior. As a result, I cannot determine the upper-bound of the estimated effect of the cost of higher education. Future work is needed to obtain the precise range of the estimated effect.

Finally, I obtain the relatively small sample size of married mothers of potential college students with a high propensity to send their children to college. This may generate a less precisely estimated effect of the cost of higher education on parents' labor market behavior. Thus, future research could make significant contributions to understand how the cost of higher education affects the labor supply behavior of parents by using a new data set containing compact and consistent information on parents' labor market behavior and their children's college-going behavior.

Table 1: Probit Estimates of Enrollment in a 4-year College Using the National Educational Longitudinal Study of 1988

Dependent Variable	Enrollment in a 4-year college	
	Coefficient	Std. Error
Father		
High school graduate	0.07	(0.021)
Some college years	0.15	(0.023)
College graduate and beyond	0.27	(0.022)
Mother		
High school graduate	0.082	(0.021)
Some college years	0.15	(0.023)
College graduate and beyond	0.22	(0.024)
Family income		
5,000 to 9,999	0.053	(0.052)
10,000 to 14,999	0.066	(0.051)
15,000 to 19,999	0.065	(0.05)
20,000 to 24,999	0.085	(0.048)
25,000 to 34,999	0.089	(0.046)
35,000 to 49,999	0.12	(0.045)
50,000 to 74,999	0.17	(0.045)
75,000 to 99,999	0.23	(0.047)
100,000 or more	0.33	(0.042)
White	0.03	(0.013)
City	0.042	(0.013)
Number of observations	8162	

Table 2: Estimates of the Effect of College Cost on Married Mothers' Employment**Decisions**

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
A. Simple difference estimation for the sample of married mothers with potential college students				
Tuition at 4-year public colleges	0.014 *** (0.0054)	0.017 (0.22)	0.014 (0.01)	-0.22 (0.41)
Number of observations	13180	13180	3627	3627
B. Difference-in-difference estimation for the sample of all married mothers with children				
PotcolstudentxTuition	0.016 *** (0.006)	0.82 *** (0.23)	0.017 * (0.01)	0.75 * (0.4)
Tuition at 4-year public colleges	-0.001 (0.005)	-0.7 (0.2)	0.004 (0.024)	-0.84 (0.25)
Potential college students	-0.009 (0.029)	1.04 (1.01)	-0.061 (0.07)	-1.86 (2.3)
Number of observations	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 3: Estimates of the Effect of College Cost on Married Mothers' Employment Decisions Using a "Propensity to Send a Child to College" Score

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
A. Simple difference estimation for the sample of married mothers of potential college students with a high propensity score				
Tuition at 4-year public colleges	0.031 * (0.017)	1.2 ** (0.52)	0.05 ** (0.022)	1.04 (0.95)
Number of observations	1066	1066	335	335
B. Difference-in-difference estimation for the sample of all married mothers with a high propensity score				
PotcolstudentxTuition	0.035 ** (0.017)	2 *** (0.57)	0.066 *** (0.025)	2.1 *** (1)
Tuition at 4-year public colleges	-0.001 (0.007)	-0.85 (0.31)	-0.012 (0.013)	-1.1 (0.51)
Potential college students	-0.13 (0.99)	-3.56 (3.31)	-0.001 (13.8)	-5.75 (5.23)
Number of observations	4783	4783	1237	1252
C. Difference-in-difference estimation for the sample of all married mothers with potential college students				
College-boundxTuition	0.018 (0.017)	1.1 ** (0.56)	0.031 (0.027)	0.85 (1.1)
Tuition at 4-year public colleges	0.012 (0.006)	-0.082 (0.23)	0.009 (0.01)	-0.37 (0.46)
College-bound	-0.18 (0.098)	-4.75 (3.31)	0.17 (0.1)	5.72 (5.63)
Number of observations	13180	13180	3627	3627

Table 3 (cont'd)

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
D. Difference-in-difference-in-difference estimation for the sample of all married mothers with children				
College-boundxPotcolstudentxTuition	0.019 (0.018)	1.1 ** (0.053)	0.06 * (0.034)	1.9 (1.3)
College-boundxPotcolstudent	-0.082 (0.098)	-2.07 (3.24)	0.24 (0.095)	4.39 (6.92)
College-boundxTuition	-0.006 (0.008)	-0.019 (0.32)	-0.028 (0.019)	-1.1 (0.72)
PotcolstudentxTuition	0.015 (0.006)	0.71 (0.24)	0.011 (0.012)	0.51 (0.49)
Tuition at 4-year public colleges	-0.002 (0.005)	-0.7 (0.21)	0.002 (0.007)	-0.77 (0.29)
College-bound	-0.057 (0.047)	0.26 (1.79)	-0.053 (0.011)	2.67 (4.92)
Potential college students	-0.006 (0.03)	1.25 (1.03)	-0.084 (0.075)	-2.03 (2.65)
Number of observations	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 4: Within-State Estimates of the Effect of College Cost on Married Mothers' Employment Decisions

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
A. Difference-in-difference estimation for the sample of married mothers				
PotcolstudentxTuition	0.015 ** (0.006)	0.78 *** (0.26)	0.02 ** (0.01)	0.85 ** (0.42)
Tuition at 4-year public colleges	-0.001 (0.014)	0.31 (0.48)	0.003 (0.024)	0.45 (0.96)
Potential college students	-0.017 (0.028)	0.73 (0.98)	-0.071 (0.07)	-2.15 (2.41)
Number of observations	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 5: Within-State Estimates of the Effect of College Cost on Married Mothers' Employment Decisions Using a "Propensity to Send a Child to College" Score

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
A. Difference-in-difference estimation for the sample of all married mothers with a high propensity score				
PotcolstudentxTuition	0.036 ** (0.017)	2 *** (0.6)	0.086 *** (0.027)	2.9 *** (1.14)
Tuition at 4-year public colleges	0.026 (0.044)	1.3 (1.8)	-0.2 (0.076)	-5.4 (3.06)
Potential college students	0.11 (0.99)	-3.47 (3.4)	12.7 (13.8)	-3.86 (6.49)
Number of observations	4783	4783	1237	1252
B. Difference-in-difference estimation for the sample of all married mothers with potential college students				
College-boundxTuition	0.021 (0.017)	1.25 ** (0.56)	0.034 (0.02)	0.095 (1.1)
Tuition at 4-year public colleges	-0.062 (0.022)	-0.81 (1)	0.021 (0.054)	0.97 (2.2)
College-bound	0.21 (0.1)	-6.67 (3.35)	0.11 (0.12)	1.91 (5.63)
Number of observations	13180	13180	3627	3627

Table 5 (cont'd)

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
C. Difference-in-difference-in-difference estimation for the sample of all married mothers with children				
College-boundxPotcolstudentxTuition	0.017 (0.018)	1 ** (0.053)	0.06 * (0.033)	1.82 (1.3)
College-boundxPotcolstudent	-0.74 (0.098)	-2.64 (3.21)	0.24 (0.099)	4.12 (6.89)
College-boundxTuition	0.003 (0.008)	0.16 (0.29)	-0.018 (0.019)	-0.69 (-0.69)
PotcolstudentxTuition	0.015 (0.006)	0.7 (0.23)	0.015 (0.013)	0.63 (0.5)
Tuition at 4-year public colleges	-0.003 (0.014)	0.22 (0.5)	0.003 (0.025)	0.45 (0.99)
College-bound	-0.011 (0.044)	-2.68 (0.63)	-13.09 (0.11)	-1.69 (4.82)
Potential college students	-0.013 (0.028)	0.91 (0.99)	-0.1 (0.076)	-2.55 (2.62)
Number of observations	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 6: Estimates of Heterogeneous Effects of College Cost on Married Mothers'**Employment Decisions**

Propensity score to send a child to college in _percentile	PostcolstudentxTuition	
	LFP	Hours
March CPS		
0-10th (N=4758)	-0.005 (0.018)	0.31 (0.62)
10-20th (N=4867)	0.006 (0.014)	0.58 (0.56)
20-30th (N=4853)	0.048 *** (0.017)	1.54 *** (0.59)
30-40th (N=4617)	0.032 ** (0.016)	1.3 ** (0.56)
40-50th (N=4536)	0.012 (0.017)	0.53 (0.63)
50-60th (N=5273)	0.007 (0.016)	0.57 (0.70)
60-70th (N=4456)	0.01 (0.017)	0.48 (0.61)
70-80th (N=4475)	0.003 (0.016)	-0.064 (0.06)
80-90th (N=5409)	0.008 (0.020)	0.41 (0.74)
> 90th (N=4783)	0.035 ** (0.017)	2 *** (0.57)
SIPP		
0-20th (N=2153)	0.024 (0.026)	1.4 (0.99)
20-40th (N=2199)	-0.003 (0.028)	-0.05 (1.06)
40-60th (N=2032)	-0.017 (0.018)	-1.2 (0.76)
60-80th (N=1881)	0.002 (0.027)	-0.4 (1.10)
> 80th (N=1906)	0.078 *** (0.025)	2.54 ** (0.94)

Table 7: Estimates of the Effect of College Cost on Married Mothers' Employment Decisions Using Different Threshold of the "Propensity to Send a Child to College"

Dependent Variable	March CPS				SIPP			
	Tuition >=85 of PG		Tuition >=100 of PG		Tuition >=85 of PG		Tuition >=100 of PG	
	LFP	Hours	LFP	Hours	LFP	Hours	LFP	Hours
A. Difference-in-difference estimation for the sample of all married mothers with a high propensity score								
PotcolstudentxTuition	0.02 (0.013)	1.1** (0.053)	0.013 (0.01)	0.68 (0.43)	0.072*** (0.023)	2.42*** (0.85)	0.054*** (0.02)	1.7*** (0.67)
Tuition at 4-year public colleges	-0.001 (0.006)	0.72 (0.23)	-0.039 (0.006)	-0.8 (0.24)	-0.014 (0.013)	-1.45 (0.44)	-0.01 (0.01)	-1.1 (0.41)
Potential college students	-0.018 (0.066)	0.072 (2.4)	-0.057 (0.048)	-0.21 (1.83)	-0.098 (0.13)	-1.18 (4.64)	-0.098 (0.12)	-0.74 (3.85)
Number of observations	10192	10192	14667	14667	2464	2464	3653	3653

Table 7 (cont'd)

Dependent Variable	March CPS				SIPP			
	Tuition >=85 of PG		Tuition >=100 of PG		Tuition >=85 of PG		Tuition >=100 of PG	
	LFP	Hours	LFP	Hours	LFP	Hours	LFP	Hours
B. Difference-in-difference estimation for the sample of all married mothers with potential college students								
College-bound*Tuition	0.0025 (0.013)	0.037 (0.51)	-0.006 (0.01)	-0.17 (0.4)	0.037 (0.02)	0.91 (0.95)	0.027 (0.02)	0.56 (0.72)
Tuition at 4-year public colleges	0.013 (0.005)	-0.05 (0.24)	0.015 (0.006)	-0.05 (0.25)	0.006 (0.011)	-0.45 (0.5)	0.005 (0.01)	-0.042 (0.49)
College-bound	-0.13 (0.065)	-5.51 (2.37)	-0.15 (0.48)	-5.3 (2.01)	-0.12 (0.13)	-0.37 (4.7)	-0.23 (0.11)	-4.92 (4.1)
Number of observations	13180	13180	13180	13180	3627	3627	3627	3627

Table 7 (cont'd)

Dependent Variable	March CPS			SIPP		
	Tuition >=85 of PG LFP	Hours	Tuition >=100 of PG LFP	Hours	Tuition >=85 of PG LFP	Tuition >=100 of PG Hours
C. Difference-in-difference estimation for the sample of all married mothers with children						
College-boundxPotcolstudentxTuition	0.004 (0.014)	0.4 (0.55)	0.005 (0.012)	-0.024 (0.47)	0.07*** (0.027)	2.1* (1.09)
College-boundxPotcolstudent	0.4 (0.069)	1.04 (2.6)	-0.046 (0.055)	0.67 (2.2)	0.005 (0.13)	2.97 (5.49)
College-bound*Tuition	-0.002 (0.006)	-0.026 (0.23)	-0.006 (0.006)	-0.15 (0.22)	-0.029 (0.015)	-1.15 (0.55)
PotcolstudentxTuition	0.014 (0.006)	0.68 (0.25)	0.014 (0.007)	0.73 (0.26)	0.003 (0.012)	0.27 (0.52)
Tuition at 4-year public colleges	-0.002 (0.006)	-0.7 (0.22)	0.001 (0.006)	-0.68 (0.22)	0.006 (0.008)	-0.64 (0.32)
College-bound	-0.17 (0.039)	5.62 (1.32)	-0.15 (0.035)	-5.8 (1.33)	-0.13 (0.078)	-3.22 (3.35)
Potential college students	-0.018 (0.032)	0.94 (1.15)	-0.009 (0.034)	0.91 (1.23)	-0.059 (0.076)	-2.12 (2.92)
Number of observations	48095	48095	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Pell Grant (PG). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 8: Estimates of the Effect of College Cost on Married Mothers' Employment Decisions Using Different Definition for High and Low Tuition States

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
A. Difference-in-difference estimation for the sample of all married mothers with children				
PotcolstudentxTuition	0.026 ** (0.013)	1.31 *** (0.51)	0.004 (0.02)	0.33 (0.76)
Tuition at 4-year public colleges	0.008 (0.011)	-0.71 (0.5)	0.013 (0.015)	-0.9 (0.67)
Potential college students	0.01 (0.027)	2.02 (0.94)	-0.026 (0.069)	-0.33 (2.34)
Number of observations	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 9: Estimates of the Effect of College Cost on Married Mothers' Employment Decisions Using a "Propensity to Send a Child to College" Score and Different Definition for High and Low Tuition States

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
A. Difference-in-difference estimation for the sample of all married mothers with a high propensity score				
PotcolstudentxTuition	0.096 *** (0.031)	5.93 *** (1.26)	0.096 ** (0.054)	3.67 ** (2.2)
Tuition at 4-year public colleges	-0.007 (0.017)	-1.76 (0.79)	-0.068 (0.039)	-4.12 (1.54)
Potential college students	-0.12 (0.09)	-2.9 (3.11)	0.18 (0.13)	0.58 (6.1)
Number of observations	4783	4783	1252	1252
B. Difference-in-difference estimation for the sample of all married mothers with a high propensity score				
College-boundxTuition	0.055 * (0.03)	3.38 *** (1.17)	0.012 (0.052)	-0.14 (1.98)
Tuition at 4-year public colleges	0.027 (0.011)	0.31 (0.47)	0.008 (0.019)	-0.83 (0.81)
College-bound	-0.18 (0.093)	-4.66 (3.04)	0.21 (0.084)	7.71 (5.07)
Number of observations	13180	13180	3627	3627

Table 9 (cont'd)

Dependent Variable	March CPS		SIPP	
	LFP	Hours	LFP	Hours
C. Difference-in-difference estimation for the sample of all married mothers with children				
College-boundxPotcolstudentxTuition	0.074 ** (0.035)	4.37 *** (1.27)	0.086 (0.062)	3.1 (2.5)
College-boundxPotcolstudent	-0.091 (0.09)	-3.51 (3.01)	0.29 (0.068)	7.36 (6.4)
College-boundxTuition	-0.017 (0.021)	-1.05 (0.88)	-0.084 (0.042)	-3.38 (1.7)
PotcolstudentxTuition	0.02 (0.013)	0.98 (0.52)	-0.005 (0.022)	-0.002 (0.84)
Tuition at 4-year public colleges	0.009 (0.012)	-0.65 (0.52)	0.02 (0.015)	-0.62 (0.7)
College-bound	-0.051 (0.044)	0.74 (1.72)	-0.08 (0.1)	1.77 (4.87)
Potential college students	0.035 (0.028)	1.66 (0.98)	-0.06 (0.072)	-1.03 (2.47)
Number of observations	48095	48095	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

Table 10: Estimates of the Effect of College Cost on Married Mothers' Employment Decisions Using Different Threshold of the Actual Maximum Award of Pell Grant

Dependent Variable	March CPS						SIPP			
	Tuition >=85 of PG			Tuition >=100 of PG			Tuition >=85 of PG		Tuition >=100 of PG	
	LFP	Hours		LFP	Hours		LFP	Hours	LFP	Hours
A. Difference-in-difference estimation for the sample of all married mothers with a high propensity score										
PotcolstudentxTuition	0.028 ** (0.012)	1.45*** (0.46)		0.037 *** (0.01)	1.95 *** (0.48)		0.023 (0.02)	0.7 (0.79)	0.017 (0.02)	0.55 (0.82)
Tuition at 4-year public colleges	-0.004 (0.01)	-1.18 (0.42)		-0.006 (0.01)	-1.47 (0.41)		0.012 (0.01)	-0.92 (0.57)	0.013 (0.01)	-0.83 (0.57)
Potential college students	0.016 (0.025)	2.36 (0.87)		0.023 (0.02)	2.7 (0.83)		-0.028 (0.07)	2.38 (2.37)	-0.024 (0.07)	-0.24 (2.31)
Number of observations	48095	48095		48095	48095		13102	13102	13102	13102

Table 10 (cont'd)

Dependent Variable	March CPS				SIPP			
	Tuition >=85 of PG		Tuition >=100 of PG		Tuition >=85 of PG		Tuition >=100 of PG	
	LFP	Hours	LFP	Hours	LFP	Hours	LFP	Hours
B. Difference-in-difference estimation for the sample of all married mothers with a high propensity score								
PotcolstudentxTuition	0.1*** (0.027)	5.32*** (1.25)	0.07** (0.03)	4.44*** (1.32)	0.15*** (0.04)	5.47*** (2.01)	0.1** (0.04)	4.43** (2)
Tuition at 4-year public colleges	-0.019 (0.015)	-2.22 (0.7)	-0.02 (0.02)	-2.1 (0.69)	-0.062 (0.03)	-4.78 (1.2)	-0.042 (0.03)	-3.51 (1.21)
Potential college students	-0.078 (0.079)	-0.81 (2.83)	-0.052 (0.08)	0.7 (2.87)	0.18 (0.12)	0.23 (6.14)	0.22 (0.12)	1.81 (5.95)
Number of observations	4783	4783	4783	4783	1252	1252	1252	1252

Table 10 (cont'd)

Dependent Variable	March CPS						SIPP					
	Tuition >=85 of PG			Tuition >=100 of PG			Tuition >=85 of PG			Tuition >=100 of PG		
	LFP	Hours		LFP	Hours		LFP	Hours		LFP	Hours	
C. Difference-in-difference estimation for the sample of all married mothers with potential college students												
College-bound*Tuition	0.056** (0.027)	2.82*** (1.18)		0.018 (0.03)	1.92* (1.25)		0.074 (0.05)	0.94 (1.94)		0.04 (0.05)	1.1 (1.97)	
Tuition at 4-year public colleges	0.02 (0.01)	0.004 (0.45)		0.026 (0.01)	0.27 (0.49)		0.021 (0.02)	-5.62 (0.79)		0.018 (0.02)	-0.63 (0.81)	
College-bound	0.15 (0.082)	-3.25 (2.83)		-0.14 (0.08)	-2.51 (2.82)		0.2 (0.08)	7.36 (4.79)		0.21 (0.08)	7.56 (4.74)	
Number of observations	13180	13180		13180	13180		3627	3627		3627	3627	

Table 10 (cont'd)

Dependent Variable	March CPS				SIPP			
	Tuition >=85 of PG LFP	Hours	Tuition >=100 of PG LFP	Hours	Tuition >=85 of PG LFP	Hours	Tuition >=100 of PG LFP	Hours
D. Difference-in-difference-in difference estimation for the sample of all married mothers with children								
College-boundxPotcolstudentxTuition	0.08*** (0.03)	3.74** (1.21)	0.035 (0.03)	2.36* (1.28)	0.15*** (0.05)	5.36** (2.32)	0.11* (0.06)	4.5* (2.34)
College-boundxPotcolstudent	-0.061 0.081	-1.98 (2.79)	-0.042 (0.08)	-0.85 (2.79)	0.29 (0.07)	6.64 (6.25)	0.3 (0.56)	8.2 (6.1)
College-bound*Tuition	-0.021 (0.019)	-0.97 (0.76)	-0.017 (0.02)	-0.54 (0.73)	-0.093 (0.04)	-4.68 (1.31)	-0.073 (0.04)	-3.5 (1.32)
PotcolstudentxTuition	0.02 (0.012)	1.12 (0.48)	0.033 (0.01)	1.72 (0.48)	0.0082 (0.02)	0.13 (0.88)	0.006 (0.02)	0.048 (0.93)
Tuition at 4-year public colleges	0.012 (0.011)	-1.1 (0.44)	-0.005 (0.01)	-1.43 (0.44)	0.02 (0.01)	-0.5 (0.6)	0.019 (0.02)	-0.51 (0.61)
College-bound	-0.054 (0.044)	0.6 (1.7)	-0.06 (0.04)	0.17 (1.64)	-0.084 (0.1)	2.12 (4.47)	-0.11 (0.1)	0.7 (4.4)
Potential college students	0.008 (0.026)	1.9 (0.91)	0.014 (0.03)	2.17 (0.88)	-0.063 (0.07)	-1.1 (2.46)	-0.062 (0.07)	-1.05 (2.4)
Number of observations	48095	48095	48095	48095	13102	13102	13102	13102

Notes: Labour Force Participation (LFP). Hours of Work (Hours). Pell Grant (PG). Standard errors are reported in parentheses. The 1%, 5% and 10% confidence levels are indicated with ***, **, and * respectively.

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CHAPTER 2

THE IMPACT OF MERIT-BASED AID ON COLLEGE ENROLLMENT: EVIDENCE FROM “HOPE-LIKE” SCHOLARSHIP PROGRAMS

In the past decade, higher education institutions, states and the federal government have increasingly used merit, academic achievement and test scores as selection devices to determine who will receive financial aid. At the institutional level, the growing use of merit aid can be seen as the result of intensified competition among higher education institutions to attract the brightest students and maximize their institutional revenue (McPherson and Schapiro, 1998).⁵ Several states have established state-funded merit programs to financially assist those students who are academically prepared to continue their education at their own state higher education institutions. The amount of the state-funded, merit-based financial aid has increased by 335 percent since 1993, suggesting there is a significant shift in the financial aid delivery system from need-based aid to merit-based aid.⁶

The concern of public policy for state-funded merit aid programs comes from the belief that since the eligibility conditions for state-funded merit-based financial aid are based on students' academic achievement and ability, students from low-income families – families with low level of financial resources in the long-run – may have more trouble qualifying for merit-based aid. To the extent that students from low-income families have

⁵ McPherson and Schapiro (1998) provide a detailed analysis of institutional merit aid.

⁶ Access Denied, A Report of the Advisory Committee on Student Financial Assistance, Washington, D.C. February 2001.

more difficulties succeeding in K-12 education, they will be at a disadvantage in trying to get merit-based scholarships. Therefore, the relevant public policy question is whether the growing use of the merit aid may exacerbate the difference in the college enrollment rate between students from low-income families and those from higher income families.

It is thus important to analyze the impacts of the shift in the financial aid delivery system from need-based aid to merit-based aid on access to college and choice of college. The introduction of state-funded merit aid programs, which result in the dramatic shift in the provision of financial aid, provides a natural experiment framework that can be used to assess the college enrollment response to financial aid. I exploit the variation across states and over time in the introduction of HOPE-like programs to obtain the estimated effect of merit-based financial aid on college enrollment rates.

Furthermore, it is likely that these state-funded merit-based aid programs may change the schooling behavior of youths in the secondary education. I will provide evidence as to whether the introduction of HOPE-like programs change the high school graduation and dropout rates.

The paper proceeds as follows. In Part I, I describe state-funded merit aid, focusing specifically Georgia's HOPE Scholarship Program, which is a leading example of a state-funded merit aid program. Then, I discuss the expected effects of these state programs on the college-going behavior of students. Lastly in Part I, I evaluate the empirical literature on the effects of a state-funded merit aid program. In Parts II and III, I explain the data and the econometric specifications I will be using to estimate the effect of state-funded merit aid. In Part IV, I present the estimation results and check them for robustness. Section V presents conclusions.

I. Background on HOPE-like Programs

Georgia was the first state to institute state-funded merit aid, the HOPE scholarship, which rewards students on the basis of their academic achievement and ability regardless of their family income. Starting in the fall of 1993, Georgia's HOPE scholarship program has provided a tuition subsidy for students to finance their actual tuition and mandatory fees at public colleges or a portion of tuition at private colleges; the scholarship award at a public university and a private college is \$3500 and \$3,000 for the 2000-2001 academic year, respectively.⁷ To be initially eligible for the HOPE program, the student has to be a Georgia high school graduate and is required to have a cumulative grade point average of at least a 3.0. The student has to maintain a cumulative grade point average of 3.0 to renew his HOPE scholarship at the end of each academic term. This scholarship can only be used for a public or private college or university in Georgia. While there is no restriction on the number of hours of enrollment for eligibility at a public institution, students must be enrolled full-time to be eligible for the scholarship at a private institution. Furthermore, Georgia also initiated HOPE Grants to subsidize students who attend non-degree programs at 2-year institutions. Unlike HOPE Scholarship, Georgia residency is the only eligibility condition for a HOPE Grant.

Several states have followed Georgia and established their own "HOPE-like" scholarship programs. I present the characteristics of HOPE-like programs in Table 1. The most distinguishing characteristic of these state-funded merit aid programs is that they reward a student on the basis of his academic achievement and abilities, regardless

⁷ However, the amount of Pell Grant that a student receives is subtracted from the amount of scholarship provided by the HOPE program.

of the family's ability to pay. However, states differ in how to measure students' academic achievement: Georgia uses only high school cumulative grade point average, while New Mexico uses grade point average in students' first college semester as a selection device. Michigan and Missouri rely on students' test scores, and Florida, Kentucky, Louisiana, Nevada, and South Carolina use both high school cumulative grade point average and test scores to determine who will receive the merit award. Furthermore, as reported in Table 1, the standards for academic achievement in high school and test scores vary across states.

States also differ in the amount of award provided in these state-funded merit programs. Florida (Florida Academic Scholars Award), Georgia, Louisiana, and New Mexico provide a full-tuition award for students attending in-state public institutions or eligible institutions defined by the state.⁸ For students attending private state institutions, these states, except New Mexico, provide a fixed award based on some portion of the average tuition and fees at comparable state public institutions. Missouri and South Carolina provide students, respectively, the fixed amount of \$2000 and \$3000 for their each academic year, while Michigan Merit Award amount is one time award of \$2500 for in-state institutions and \$ 1000 for approved out-of-state institutions. The Nevada Millennium Scholarship pays for \$80 and \$40 per enrolled credit hour for 4-year and 2-year colleges, respectively. Finally, the amount Kentucky awards a student is based on his GPA of each high school year and ACT score.

In terms of sources of funding for HOPE-like program, Georgia, Florida, Kentucky, and New Mexico finance their merit-based financial aid programs from their

⁸ New Mexico Lottery Success Scholarship starts rewarding students after their first semester at public state institutions.

own lottery revenues. Michigan and Nevada use money received from the National Tobacco Settlement, and Louisiana, Missouri, and South Carolina use general state revenues.

II. Expected Effects of Merit-based Aid

Financial aid policies can affect substantially both access – whether a student can attend college – and choice, or what the type of higher education institution a student attends. The purpose of merit aid is to acknowledge and reward able students, provide them financial means to reduce the high cost of higher education, and consequently empower them to invest optimally in their higher education (McPherson and Schapiro, 1998). Since students' ability and academic achievement are functions of family income and parental education level, which also determine the college going behavior of students, merit aid would be most likely an educational subsidy to students who would have attended college even in the absence of merit aid. Conversely, students from low-income families are less likely to fulfill the cumulative grade point average or test score requirements associated with state-funded merit aid. Therefore, it is likely that state-funded merit aid programs do little to offset the adverse effects of short-term credit constraints for many students from low-income families in years when they are making their college decision.

The only possible group of students induced to go to college by the state-funded merit aid consists of those who have high academic achievement and would not have gone to college in the absence of this merit aid due to short-term credit constraint. However, as Cameron and Heckman (1999) argue, since family income determines to

what extent a student can be academically ready to attend college, which is ultimately the outcome of the quality of schooling in pre-college years and other long-term family and environmental factors, it is likely that students from low-income families do not have required academic standards to be qualified for the state-funded merit aid.

Therefore, one of the expected effects of these programs is to contribute to the growing differences in the college enrollment rates between students from low-income families and those from high-income families. On the other hand, this difference in college-going behavior may not change as a HOPE-like scholarship is a subsidy to students from high-income families who have necessary means to finance their college education in the absence of the merit-based aid. This means that the change in the college-going behavior due to the introduction of HOPE-like programs is determined by the extent to which these programs induce the change in the college-going behavior of students from low-income families. Therefore, the net effect of state-funded merit aid programs in broadening the access to college remains to be empirically determined.

The second expected main effect of merit aid is likely to be concentrated on the college choice decisions of students who would have attended college anyway in the absence of merit aid. For instance, a student who was planning to attend two-year college may be able to attend four-year college, since the introduction of state-funded merit aid makes four-year college more affordable. Similarly, assuming the quality of education is same across institutions, a student is likely to prefer to attend in-state college instead of out-state college since the state-funded merit aid reduces the relative price of in-state college.

In the literature of the economics of higher education, it has been argued that the change in the enrollment rate at 2-year colleges captures the possible effects of the financial aid program on access to higher education institutions, while the change in the enrollment rate at 4-year college picks up the extent to which the financial aid program affects college choice. Thus, the estimated effect of HOPE-like programs on the enrollment rates at 2-year and 4-year college convey information on the change in access to and choice in higher education.

Moreover, several states have begun to increase the academic requirements for their merit aid programs. In Georgia, the high school graduating class of 2001 and beyond need to have cumulative grade point average of 3.0 in a college preparatory curriculum, not all courses taken in high school. In addition to the growing use of the merit aid, states' efforts to increase the academic requirements to qualify for the state-funded merit aid may increase the difference in the college-going rate between students from low-income families and those from middle and high-income families.

Finally, the state-funded merit aid program may have an impact on students' achievement and their secondary educational outcomes, the high school graduation and dropout rate. One possible positive effect of HOPE-like programs is to increase aspirations of high school students, and consequently encourage them to study more to reach the academic standards required by these merit-based programs. Supporting this positive effect of HOPE-like programs, Cornwell et al. (2000) report that the mean of SAT scores for Georgia freshman rose higher than the mean of SAT score for all US freshmen. Likewise, a study conducted by the American Association of State Colleges and Universities (2000) reports that the introduction of Georgia's HOPE Scholarship has

been boosting the grades, SAT scores, and the graduation rate of high school students in Georgia. However, it is possible that the introduction of HOPE-like programs leads high school students to take less-academically oriented courses to keep their GPA high enough to be eligible for these programs. Moreover, the introduction of HOPE-like programs may also results in grade inflation in high school. In this study, while I provide evidence whether these programs improve the secondary educational outcomes, I cannot address whether the improvement in the high school graduation and dropout rate is due to grade inflation or the change in schooling behavior of students and the increased quality of students.

III. Previous Literature

In previous research, Dynarski (2000) and Cornwell et al. (2000) examined the effect of the introduction of Georgia's Hope Scholarship program, which provides an exogenous shift in the delivery of financial aid, on the college-going behavior of students. Both studies employ the difference-in-differences method, comparing the mean difference in college attendance rates between Georgia and other states that do not have HOPE-like Scholarship programs, before and after the introduction of Georgia's Hope Scholarship.

Using the ratio of first-time freshman to the college eligible population as a dependent variable, Cornwell et al. (2000) find that the estimated effect of Georgia's HOPE program ranges between 8.5 and 7.9 percentage points, depending on which control variable and control states they used in their specification. Considering that before Georgia's HOPE program was initiated, the mean of the ratio of first-time

freshman to the college eligible population is .76, their difference-in-difference estimator indicates that HOPE program increases the college enrollment rate by between 10.4 and 11.6 percent. Furthermore, Cornwell and et al. (2000) decompose this positive effect of Georgia's HOPE program by institution type; while they find that the HOPE program generates an 8.5 percentage point increase in the college enrolment rate at four-year universities, Cornwell and et al. do not obtain a significant effect of Georgia's HOPE on the college enrollment rate at two-year universities. They also analyze how the introduction of Georgia's HOPE program influences race differences in the college enrollment rate: Georgia's HOPE program boosts the college enrollment rate among blacks 24 and 12 percent at four-year public and private universities, respectively, while the estimated effect for whites is 7 and 12 percent at 4-year public and private universities, respectively.

Dynarski (2000) examines the change in the college enrollment rate of 18-19 year-olds after the introduction of Georgia's HOPE Scholarship program, using the October Current Population Survey. She finds that the estimated effect of Georgia's HOPE program varies from 7.0 to 8.0 percentage points. Since the college attendance enrollment rate in Georgia was 29.9 percent before Georgia's HOPE program was initiated, the percentage increase in the college enrollment rate is between 23 and 27 due to the introduction of Georgia's HOPE program. When Dynarski analyzes the effect of Georgia's HOPE program by students' family income, she concludes that students from high-income families are the main beneficiaries of Georgia's HOPE program: the college enrollment rate of those students has increased 11.4 percentage points. Dynarski also

finds a significant race difference in the college enrollment rate due to the introduction of Georgia's HOPE program.

There might be two sources of these differences in the estimated effects of Georgia's HOPE program. First, the two studies differ in defining their dependent variables: Dynarski (2000) uses college attendance of 18-19-year-olds as a dependent variable, while Cornwell et al. (2000) use the ratio of first-time freshman to the college eligible population, where they define the college eligible population as the current high school graduates or high school graduates from the previous three years. The ways of grouping of potential the recipients of Georgia's HOPE program in these two studies may also contaminate the actual effect the program on the college going behavior of students: Dynarski's definition of the dependent variable ignores the heterogeneity of response of the change in the net cost of college between high school graduates considering enrolling for the first time as freshmen, and college freshman considering whether to return for their sophomore years. The first group of students is likely to be more responsive to changes in the net cost of college than the second group of students. Therefore, to the extent 18-19-year-olds individuals include sophomore students, Dynarski's results may underestimate the actual effect of Georgia's HOPE program. Similarly, the aggregation of recent freshmen (who are graduated from high school within first 12 months) and other freshmen (who are graduated from high school more than 12 month ago) may produce negatively biased results in the study of Cornwell et al., since in the early years of the program only recent high school graduates were eligible for the scholarship. Moreover, the construction of dependent variable, the ratio of first-time freshmen to the college eligible population, is also problematic in the study of Cornwell et al.. They implicitly

assume that the introduction of Georgia's HOPE program may only affect the number of first-time freshman, the numerator in the dependent variable. However, it is likely that Georgia's Hope program may also affect the high school graduation rate in Georgia. This brings in an increase in the denominator of the dependent variable, and consequently decreases the ratio of first-time freshmen to the college eligible population. Thus, their way of constructing dependent variable may generate a downward bias in their estimated effects of Georgia's HOPE program.

The second source of difference in results between these two studies may be the difference in the data set that they employ. Dynarski (2000) uses individual level data from the October Current Population Survey. Cornwell et al. (2000) use the aggregate level data, Integrated Postsecondary Education Data System (IPEDS). Furthermore, Dynarski assign states in the South Atlantic and East South Central Census Divisions into the control group, whereas Cornwell et al. use states which are members of the Southern Educational Board (SREB).

IV. Data

In this study, the data come from the October Current Population Survey (CPS) for the period 1989 to 1999. The October CPS provides basic information on demographic characteristics such as age, sex, race, and educational attainment as well as labor market behavior for each individual in a household. Moreover, special attention is given to the schooling behavior of individuals in the October CPS.

In order to obtain the sample used to estimate the effect of HOPE-like scholarship on the college-going behavior, I apply the following procedures: first, I restrict the

sample to 18-19-year-olds. Second, in order to focus on the high-school graduate cohort, I exclude individuals who do not have a high school diploma as well as individuals who completed high school by means of a GED. Then, I merge this individual level data with data on the state-level unemployment rate, and information on the characteristics of any state-funded merit aid programs, using state and year identifiers.

However, the design of the October CPS limits substantially the scope of the analysis of the effect of HOPE-like programs on college enrollment (Dynarski, 2000; Cameron and Heckman). First, the October CPS does not always correctly record the state in which a student attended high school or is attending college. This affects efforts to estimate the impact of HOPE-like programs on college attendance, because as reported in Table 1 the eligibility for every state merit scholarship program requires a high school diploma from one's own state secondary education institutions, and can only be used within that state. When a student attends college and lives with either his family of origin or in a college dormitory, he is counted as a resident of the state in which his parents resides.⁹ For these students, I am able to correctly code states in which they attended high school, except in the few cases in which the family has moved across state lines since the student graduated from high school. If the student is attending college out of state, however, his recorded state of residence will not be the state in which he is attending college. Since I examine the impact of HOPE-like programs on the overall college enrollment rate of a state's high school graduates, not just the in-state enrollment rate, the

⁹ Enumerators will include in a household record an 18-19 year old who is away at college but still maintains living quarters (e.g., a room) in the household. This is intended to include students living in dormitories, which are not considered separate residences or households for CPS purposes. It may also include students away at college who do not live in dormitories, but are expected to return to the household during the summer, depending on how the respondent perceives the question.

estimated effect will not be biased due to the misclassification of states in which students attend college.

Although the majority of 18-19-year-old college students live either with their parents or in college dormitories (almost all 4-year institutions require freshmen to live in dormitories), some students may live on their own, and appear in the CPS because their independent household is chosen for the survey. This would cause the college enrollment rates for the high school graduates of each state to be mismeasured to an extent related to the ratio out-of state high school graduates attending college within the state to in-state high school graduates attending school out of state. If these ratios are relatively fixed over time, and unaffected by the introduction of HOPE-like programs, the inclusion of state dummies in the model captures the differences in mismeasurement, and the estimated impact of HOPE-like programs, which is identified by within state changes in the enrollment rate, will not be biased. It is possible that introduction of a HOPE-like program causes some students who would have attended out of state (and lived independently) to now attend in-state (and live independently) thus removing a negative bias in the state's enrollment rate. This would bias upward the estimated impact of HOPE-like programs. However, in addition to the fact that few 18-19 year old college students live independently, the proportion of students who attend school out of state is also relatively low. I thus conclude that this bias is of little concern.

Another difficulty with the October CPS is that it does not provide family background information for all youth. Specifically, information on a youth's family background is available only if he resides with his parent or he is attending college and living in college dormitories. In other words, the important choice variable for a youth,

whether he lives independently of his parent or not, may influence the probability of having information on his family background. Thus, examining only individuals with family background information may bias the estimated effect of HOPE-like programs due to this sample selection procedure. Furthermore, since it is likely that the introduction of the state-funded merit program will influence a youth's decision on living arrangements, it inflates the bias in the estimation results. One important implication of this attribute of the October CPS is that I will not be able to gauge the impact of the HOPE-like program across the distribution of family income. Nevertheless, since black youths are more likely to come from a disadvantaged family background, I examine the effect of HOPE-like programs on college enrollment rate by race to infer the possible distributional effect of these programs.

In the first part of the empirical analysis, dependent variables aim to capture individuals' post-secondary schooling outcomes. I focus on the following post-secondary schooling measures: an indicator for whether the individual attends a higher education institution; an indicator for whether the individual attends a 4-year higher education institution; an indicator for whether the individual attends a public or private 4-year higher education institution; an indicator for whether the individual attends a public 2-year higher education institution.

The second part of the empirical analysis focuses on the effect of HOPE-like programs on students' behavior in the secondary education. Particularly, the interest of educational outcome is the high school graduation rate for 18-19-year-olds. For 16-19-year-olds, I examine the effect of HOPE-like programs on the high school dropout rate.

In the empirical strategy, I examine the effect of HOPE-like programs implemented by Georgia, Florida, Kentucky, Louisiana, and South Carolina. Since these states are in the Southeastern region, I use states that are members of the Southern Regional Education, as the control group of states.¹⁰ Then, in order to gauge the robustness of results across the choice of the control states in the main specifications, I estimate the effect of HOPE-like programs using different sets of control states. First, I assign states in the South Atlantic and East South Central Census Divisions as the control states like Dynarski (2000).¹¹ Second, I use all states in U.S, except states that have implemented HOPE-like programs.¹² Furthermore, I extend my analysis by including New Mexico and Missouri into group of states with HOPE-like program and using all other states as the control states.

V. Empirical Methodology

I use an exogenous variation generated by the introduction of the state-funded merit aid across states and over times to identify the effect of the financial aid on the college-going behavior of students. I estimate the following equation for the sample of 18-19-year-olds

$$Y_{it} = \beta_0 + \beta_1 \text{Meritstate}_{it} + X_{it}\phi + \theta_t + \gamma_i + \delta_{it} + \varepsilon_{it},$$

¹⁰ Cornwell and et al. (2000) use the same control group of states, but their analysis is confined to Georgia's Hope Program so that Florida, Louisiana, South Carolina were in their control group of states. In this paper, the control states that are members of the Regional Education Board and do not have HOPE-like Scholarship program, are Alabama, Arkansas, and Kentucky, Delaware, Maryland, Mississippi, North Carolina, Oklahoma, Tennessee, Texas, Virginia, West Virginia.

¹¹ These states, except those with the HOPE-like program, are Alabama, Delaware, District of Columbia, Kentucky, Maryland, Mississippi, North Carolina, Tennessee, Virginia, and West Virginia.

¹² Since New Mexico and Missouri have introduced their own program, they are not included in the control group of states.

where Y_{ijt} is a binary variable denoting educational outcomes for the individual i in state j at time t , and zero otherwise; $Meritstate_{jt}$ is an indicator for whether the individual lives in state j with a merit aid program in time t ; X_{ijt} contains individual characteristics such as a dummy variable for white individuals, a dummy variable for male, a dummy variable for each individual's age, a dummy variable for living in an urban area, and state level unemployment rate. The inclusion of state level unemployment rate controls for the fact that labor market conditions in state determine an important component of the college cost – foregone earnings - and consequently influences the college-going behavior. ε is a random error term.

In this difference-in-difference framework θ_j are state-fixed effects that aim to capture state-specific factors that may be correlated with the college going behavior and the introduction of the state-funded merit aid, assuming that they do not change over time. In addition, I include year dummies, γ_t , to control aggregate time effect that change over time but are same across states.

However, it is likely that states that implemented the HOPE-like program are systematically different from other states in other higher education policies and labor market condition and policies which may determine the college-going behavior. This implies that the estimated effect may be confounded, and thus it may be biased. For example, it is possible that unobserved state trending factors that affect college attendance rate might also be correlated with the introduction of state-funded merit aid programs. The estimated effect of interest may fail to capture the causal effect of the introduction of state-funded program on college attendance, but picks up the spurious

relationship between two. To address this concern, I allow for separate specific time trends for each state by including δ_{it} .

Alternatively, in theory, the model can be estimated using a within state control group to difference out both unobserved time-invariant and trending factors in a state. 18-19-year-olds from low-income or high income families are likely candidates to serve as within state control groups to control for state-specific differences that are assumed to be common to all 18-19-year-olds. However, since students from either group can be affected by the introduction of HOPE-like programs, using one of these groups provides information on the effect of these programs by family income (heterogeneous treatment effect). Furthermore, as discussed before, since the CPS does not provide consistent parental background information for all youth, I cannot assign these individuals into the treatment and control group based on family income level. Second, 20-24-year-olds may be used as a within-state control group to capture state-specific differences, assuming that these state-specific differences are the same for 18-19 and 20-24-year-olds. A likely problem is that some of 20-24-year-olds may also be eligible for Hope-like scholarship programs. It is possible that since the eligibility for programs is conditioned on the date of high school graduation, 20-24-year-olds might have postponed their high school graduation, anticipating the introduction of the Hope-like program. In addition, Florida and South Carolina provide scholarship for older cohort of high school graduates for the first year of programs, and all years after, resulting in the postponement of college attendance among 20-24-year-olds. Also note that after the program has existed for several years, all 20-24-year-olds have made their college attendance decisions under the influence of the program. Finally, it is likely that unobservable individual characteristics

that influence college attendance may not be comparable between 18-19 and 20-24-year-olds. Because of these concerns, I rule out the use of 20-24-year-olds as a within-state control group.

Having discussed the framework of natural experiment used in the analysis, I now turn to how to interpret the coefficient of interest, β_1 , that is intended to measure the estimated effect of the introduction of HOPE-like scholarship programs on college attendance. Since I include state-fixed effects, my specification generates a within-state estimator. To be precise, I examine average change in enrollment rates among 18-19-year-olds relative to trend in states which have implemented the HOPE-like program, relative to states that have not implemented the HOPE-like scholarship program, before and after the program was introduced. I use a linear model, correcting standard errors for heteroscedasticity and correlation within state-year cells.¹³

VI. Estimation Results

Table 2 presents estimates of the effect of HOPE-like programs on the college enrollment rate for 18-19-year-olds by institution type and race. The first panel in Table 2 suggests that HOPE-like programs increase the mean of the overall college enrollment rate by 5.3 percentage points. This estimated effect is significant at the ten-percent level. Next, I examine the estimated effect of HOPE-like programs on the enrollment rate at 4-year colleges. HOPE-like programs increase the enrollment rate at 4-years colleges by 9.4 percentage points, and this estimate is statistically significant at the one-percent level. When I break down the estimated effect of HOPE-like programs on the enrollment rate at

¹³ I also use a probit model for all the interest of equations in the paper. The results do not change.

4-year colleges in terms of whether they are private and public institutions, I observe that the introduction of these programs has a substantial and statistically significant effect on the attendance rate of 18-19-year-olds at 4-year public colleges. HOPE-like programs boost the attendance rate of 18-19-year-olds at 4-year public colleges by 9.6 percentage points, and the effect is significant at the one-percent level. However, for the same age group these state merit scholarships have negligible and statistically insignificant effect on the mean of enrollment rate at 4-year private colleges. Lastly, the estimated effect of HOPE-like programs on the average enrollment rate at 2-year colleges is -2.8 percentage points and statistically significant at the ten-percent level.

It is possible that the estimated effect of HOPE like programs may be somewhat biased due to unobserved state-specific effects that change over time. In the second panel of Table 2, I include state-specific time trends to capture changes in the college enrollment rate that stemming from unobservable changes in a state over time. The estimated effect of HOPE-like programs becomes larger and more statistically significant.

Panels III and IV Table 2 present the estimated effects of state merit programs on the college enrollment rate by race. When the sample of analysis is only restricted to white 18-19-years-olds, the estimated effect of HOPE-like programs on the overall college attendance rate is 6.5 percentage points, and it is significant at the five-percent level. The breaking down of this effect indicates the estimated effect of HOPE-like programs is larger for the enrollment rate at 4-year public institutions. The effect is 11 percentage points, and it is significant at the one-percent level. On the other hand, the

estimated effect is negligible and insignificant for the enrollment rate at 4-year private and 2-year public colleges.

When the population of interest is black 18-19-year-olds, the estimation results show that there is a 4.1 percentage points increase in the overall college attendance rate, but the effect is not statistically significant. However, the estimated effect of HOPE-like programs on the enrollment rate at 4-year colleges is larger, implying that the introduction of HOPE-like programs is associated with a 16 percentage points rise in enrollment rate at 4-year colleges among black 18-19-year-olds. It should be noted that unlike whites, HOPE-like programs result in a 7.6 percentage points increase in enrollment rate at 4-year private colleges, and it is significant at the five-percent level. One possible explanation for that is the presence of private historically black institutions in states that have implemented the HOPE-like program. Finally, the point estimates is – 0.11 for the enrollment rate at 2-year public colleges, and it is significant at the ten-percent level.

In Table 3, I examine the sensitivity of estimated effects of HOPE-like programs to the choice of control states. Panel I uses states that have not implemented the HOPE-like program in the South Atlantic and East South Divisions as control states. Panel II uses all states that have not implemented the HOPE-like program as control states. Using the different set of control states, the estimation results suggest that the estimated effects of HOPE-like programs on college going behavior remain same in terms of sign, statistical significance, and somewhat in terms of magnitude. Specifically, the introductions of HOPE-like programs boost the overall college attendance of 18-19-year-olds by 4.4 to 6.4 percentage points. The estimated effect in Panel II is significant at the five-percent

level. Likewise, the estimated effect of these programs on the college enrollment rate at 4-year public colleges is positive, suggesting that the introduction of HOPE-like programs increases the enrollment rate at 4-year colleges by 8.7 to 10 percentage points among 18-19-year-olds, relative to those in states which have not implemented these scholarship programs. Similar to the results reported in Table 2, the positive estimated effect of HOPE-like programs is concentrated on the college attendance at 4-year public colleges. The estimated effect is negligible and statistically insignificant for 4-year private college, while the estimated effect of programs on attendance at 2-year public college remains to be negative and statistically significant. Finally, Panel III includes New Mexico and Missouri in the treatment group of states to estimate the effect of HOPE-like programs. The estimated effect of HOPE-like programs becomes slightly smaller. The point estimates are 4.4 and 8.9 percentage points for the overall college enrollment rate and the enrollment rate at 4-year colleges.

Next, I turn attention to the question whether the introduction of HOPE-like programs alters the schooling behavior of high school students. The results are presented in Table 5. The first panel in Panel I suggest that the introduction of HOPE-like programs is associated with a 5.4 percentage points increase in the high school graduation rate of 18-19-year-olds. This estimated effect is significant at the five-percent level. In term racial difference in response to the introduction of HOPE-like programs, the estimated effect is 4.8 percentage points, and significant at the ten-percent level. For black 18-19-year-olds, the estimated effect is 7.3 percentage points, but it is not statistically significant at the conventional level.

I also examine the effect of HOPE-like programs on the high school dropout rate among 16-18-year-olds. The results are depicted in the second column of Table 5. The estimated effect of HOPE-like programs is –2.3 percentage points, suggesting that the introduction of HOPE-like programs reduces the likelihood of dropping out of high school. This estimated effect is significant at the ten-percent level. When I focus only on white 16-18-year-olds, the estimated effect is negligible and statistically insignificant. On the other hand, HOPE-like programs reduce the high school drop out rate of black 18-19-year-olds by 6.2 percentage points, and it is significant at the five-percent level.

Finally, in Table 6, I examine whether the estimated effects of HOPE-like programs on the high school graduation and drop out rates remains robust to the choice of the control group of states. The results indicate that the estimated effects of HOPE like programs do not change across the choice of different control states.

VII. Dynamic Analysis of the Effects of HOPE-like Programs

The empirical framework used in this study relies on the assumption that the introduction of HOPE-like programs is the only the source of change which may influence the college going behavior of 18-19-year-olds across states and over time. However, this identification may not be valid in two circumstances. First, if there is an underlying trend toward higher college enrollment among 18-19-year-olds in states with the HOPE-like program relative to states without the HOPE-like program, it then can be argued that states may implement their programs in response to this trend. Therefore, the introduction of HOPE-like program may fail to provide the exogenous source of variation in the delivery of financial aid.

Second, it is possible that individuals who anticipate the introduction of the Hope-like program may change their schooling behavior. For example, if the required year of high-school graduation to be eligible for the scholarship overlaps with the year in which states introduced the HOPE-like program (as in Georgia and Louisiana) it is possible that some students will postpone their high school graduation to qualify for the scholarship. Likewise, if the eligibility condition covers certain years of high school graduation prior to the introduction of the HOPE-like program (as in Florida and South Carolina), some students may delay their college attendance, anticipating tuition subsidy provided by HOPE-like programs. Although these anticipatory adjustments in college attendance would be minimal among 18-19-year-olds, it is worthwhile to examine this possibility.

In order to examine the presence of these two dynamic factors, I estimate the model by including an indicator, *Meritlag*, for whether the state enacts the program in the following year. The coefficient on *Meritlag* captures the mean difference in the college enrollment rate between 18-19-year-olds in states with HOPE-like programs and those in state without HOPE-like programs, one year before programs have implemented.

The estimation results are depicted in Table 12. The first panel in this table documents that the estimated effect of HOPE-like programs remains robust to the inclusion of *Meritlag*. The coefficient on *Meritlag* is negative or negligibly positive, and its point estimates are not statistically significant. One way to read this estimation result is that I can rule out the possibility that a state's introduction of HOPE like programs is the outcome of the increasing trend in college attendance among its 18-19-year-olds.

Alternatively, one can argue that the negative coefficient of *Meritlag* may provide evidence for the expected delay in college attendance before HOPE-like programs are

introduced, but it should be noticed that this estimated effect is not statistically significant. In addition, although 18-19-year-olds may change their schooling behavior in one year before the introduction of HOPE-like programs they are less likely to be in the population of interest for years in which Hope-like programs are implemented and all years after. Moreover, it is possible that the negative coefficient of *Meritlag* suggest that HOPE-like programs might be the outcome of efforts to promote college education and retain academically able students in states where college attendance is low.

When I control for state-specific time-trends in the second panel of Table 4, the coefficient on *Meritlag* becomes negligible except for the overall college enrollment rate and the enrollment rate at 2-year public colleges, but all these estimates are not statistically significant. The estimated effect of HOPE-like programs on the enrollment rate at 4-year public colleges is large and statistically significant, reassuring that the estimation results capture the causal effect of HOPE-like program on the college going behavior of 18-19-year-olds individuals.

Furthermore, I provide evidences on the dynamics of HOPE-like programs by adding a dummy variable indicating the HOPE-like program is in its second year, and all years after; a dummy variable indicating the HOPE-like program in its third year, and all years after. These variables are denoted by *Meritlead1* and *Meritlead2*. They measure additional change in corresponding year, compared to pre-program years, over the changes observed in the previous years of the programs. The estimation results are reported in the third and fourth panel of Table 4. As expected, the coefficients on *Meritlead1* and *Meritlead2* are smaller when we control for state-specific time trends. After HOPE-like programs are introduced, the effect of HOPE-like programs are negative

and insignificant for the second year. The effects then become positive and larger in the third year, but they are not significant. Furthermore, *Meritlead1* and *Meritlead2* are not jointly significant at the conventional level, suggesting that there are no additional changes in the second and third year of HOPE-like programs over the changes observed in the first year.

I also examine the endogeneity of the introduction of HOPE-like programs in assessing the effect of these programs on educational outcomes in the secondary education. It is possible that states, which have improved their residents' educational attainment in the secondary education, may introduce to the merit-based financial aid to encourage students to attend college, or help them in financing college education. The results for the endogeneity test of HOPE-like programs are presented in Table 6. For the high school graduation rate, the coefficient on *Meritlag* is positive, suggesting that it is possible that the implementation of HOPE-like program is the outcome of the increasing trend in the high school graduation rate. However, since this coefficient is not significant at the conventional levels, I reject the endogeneity of these programs. For the high school dropout rate, the coefficient on *Meritlag* is -0.019 , but it is not statistically significant. This evidence suggests the decline in the high school dropout rate is the causal effect of the introduction of HOPE-like programs.

Finally, I provide evidence to whether there are additional changes in the high school graduation and dropout rates in years after the programs have implemented. These effects are intended to be measured by the coefficient of *Meritlead1* and *Meritlead2* in the second panel of Table 6. The results indicate that there are no significant effects of the programs on the high school graduation rate in the second and third year of HOPE-

like programs. For the high school dropout rate, the results suggest that the effect of these programs do not have a consistent pattern in the second and third year of the programs. While the estimated effect is positive and significant in the second year, it is negative and statistically insignificant in the third year.

VIII. Conclusion

In this paper, firstly, I have studied the impact of state-funded merit programs, or HOPE-like programs, on the college going behavior of 18-19-year-olds individuals. The primary results show that the impact of HOPE-like programs appears to be concentrated on the enrollment rate at 4-year public institution. These programs result in a 7.1-11.1 percentage point rise in the enrollment rate among 18-19-year-olds at 4-year public institution. Since HOPE-like programs have a negative impact on the enrollment rate at 2-year public institutions, it may be that these programs fail to expand access to higher education among 18-19-year-olds, or that movement of new college students into the 2-year institutions is offset by movement of students from 2-year to 4-year institutions. Particularly, the latter effect may explain a bigger negative estimated effect of HOPE-like programs on the enrollment rate at 2-year public institution. I provide the evidence that the estimated effects of HOPE-like programs are robust to the choice of control states. Furthermore, the analysis of the dynamics of HOPE-like programs indicates the estimated effects of HOPE-like programs reported here pass the endogeneity test of the introduction of HOPE-like programs. Likewise, the effects do not reflect anticipatory changes in schooling behavior among 18-19-year-olds.

Since the design of the October CPS imposed limitations on the scope of the analysis, I cannot decompose the effect of HOPE like program by family income. Considering that black students are more likely coming from disadvantaged families, I examine the difference in the estimated effect of HOPE-like programs on the college enrollment rate between white and black individuals. My findings suggest the differences in the response to the program are not significantly different between white and black students except for the enrollment at 4-year private colleges.

Secondly, I have examined whether the introduction of HOPE-like programs improves educational outcomes in the secondary education. I find that HOPE-like programs increase the likelihood of being high school graduate among 18-19-year-olds by 3.9 to 7.3 percentage points. Furthermore, the effect of HOPE-like programs on the high school dropout rate of 16-18-year-olds is between -1.8 and -6.2 percentage points. When I break down the effect of HOPE-like programs by race, the results suggest that the impact of HOPE-like programs on the high school graduation rate does not change significantly between white and black students. However, HOPE-like programs reduce significantly the likelihood of being high school dropout for black 16-18 year-olds, while these programs fails to alter the high school dropout behavior for whites. Finally, I provide evidence that these estimation results are not contaminated by the endogeneity of the implementation of HOPE-like programs.

The effect of HOPE-like programs on educational outcomes in the secondary education has an important implication for assessing the effect of HOPE-like programs on the college going behavior of students. It is possible that the increase in the college attendance in states that have implemented the HOPE-like program can be explained by

the shift in the choice of college from out of state institutions to in state institutions, which I cannot empirically verify using the October CPS. However, the empirical evidence that HOPE-like programs boost the high school graduation rate and reduce the incidence of high school dropout suggests that these programs increase the college eligible population, which may attend college. Therefore, I can conclude that the positive estimated effect of HOPE-like programs on the in-state college enrolment comes from the increase in the college eligible population due to the introduction of these programs, suggesting that state-funded merit aid programs may have increased access to college for their residents.

The results presented in this study do not close the debate on whether merit-based financial aid increase the difference in the college enrollment rate between students from low-income families and those from middle and high-income families, or to what extent these programs alter access to college and choice of college. In order to make full assessment of the effects of these programs, we need to have consistent information about students' family background. Thus future research is warranted to estimate the effect of the merit-based financial aid on college-going behavior, using a data source that includes family background of students.

Table 1
The Institutional Characteristics of HOPE-like Scholarship Programs

State	Required high school GPA for initial eligibility	Required test scores	Required academic standard for the renewal of scholarship	Required year of high school graduation	The amount of scholarship award	The sources of funding for scholarship
Florida	3.5 weighted GPA using the required courses and their credits	Best composite score of 1270 SAT or 28 ACT	GPA of 3.0 on a 4.0 scale	within 3 years of high school graduation;	Public Institution: 100% of tuition and fees (including lab fees up to \$ 300 per semester) plus \$ 300 per semester for college- related expenses (excluding summer term) prorated by term and hours Private Institution: Fixed award amount based on 100% of the average tuition and fees covered at comparable Florida public institution	Florida Lottery
	3.0 weighted GPA using the required courses and their credits	Best composite score of 970 SAT or 20 ACT	GPA of 2.75 on a 4.0 scale	within 3 years of high school graduation;	Public Institution: 75% of tuition and fees, including lab fees up to \$300 per semester Private Institution: Fixed award amount based on 75% of the average tuition and fees covered at comparable Florida public institution	Florida Lottery

Table 1 (cont'd)

State		Required high school GPA for initial eligibility	Required test scores	Required academic standard for the renewal of scholarship	Required year of high school graduation	The amount of scholarship award	The sources of funding for scholarship
	Florida Gold Seal Vocational Scholars Award	3.0 weighted GPA using the required courses and their credits, and a 3.5 unweighted GPA in minimum of 3 vocational credits in one vocational program	CPT: Reading 83, Sentence Skills 83, Algebra 72 SAT: Verbal 440, Math 440 ACT: English 17, Reading 18, Math 19	GPA of 2.75 on a 4.0 scale	within 3 years of high school graduation;	Public Institution: 75% of tuition and fees, including lab fees up to \$300 per semester	Florida Lottery
						Private Institution: Fixed award amount based on 75% of the average tuition and fees covered at comparable Florida public institution	
Georgia	HOPE Scholarship	For the class of 2000 and beyond: a 3.0 cumulative GPA in the college preparatory core-curriculum subjects, or a 3.2 cumulative GPA in the general or career/technical curriculum	No	3.0 cumulative GPA	A 1993 or later high school graduate	Public Institution: 100% of tuition, HOPE-approved mandatory fees, and a book allowance	Georgia Lottery
	HOPE Grant	No	No	No	No	Private Institution: Fixed award of \$3000 per academic year.	Georgia Lottery
Kentucky	Kentucky Educational Excellence Scholarship	a 2.5 GPA for each academic year of high school study, meeting the Commonwealth merit scholarship curriculum	No; ACT used to determine the amount of annual bonus for each eligible student: the amount of supplemental award increases as ACT score increase	2.5 GPA for the first award period; 3.0 cumulative GPA for subsequent award periods; a reduction 50% of award for the next award if any eligible student maintains a cumulative GPA of less than 3 but at least 2.5.	Enrollment in a Kentucky high school after July 1, 1998	The amount of award varies, depending on a GPA of each academic year of high school study and ACT scores	Kentucky Lottery
Louisiana	TOPS Opportunity Award	2.5 cumulative GPA	Louisiana high school graduate: The state's reported prior year ACT composite average, but never less than 19	2.30 GPA with completion of less than 48 credit hours, 2.50 after the completion of 48 credit hours;	1998 and beyond high school graduate	100% tuition for full-time attendance at an eligible institution	General state revenues

Table 1 (cont'd)

State		Required high school GPA for initial eligibility	Required test scores	Required academic standard for the renewal of scholarship	Required year of high school graduation	The amount of scholarship award	The sources of funding for scholarship
			Non-Louisiana high school graduate: The state's reported prior year ACT composite average plus 3 points, but never less than 22	2.50 GPA if enrolled in a program for a vocational or technical education certificate or diploma or a non academic undergraduate degree.			
	TOPS Performance Award	3.5 cumulative GPA, must be in the top 5 percent of the 1997-1998 high school graduating class	Non-Louisiana high school graduate: The state's reported prior year ACT composite average plus 3 points, but never less than 26 Louisiana high school graduate: The state's reported prior year ACT composite average, but never less than 27	3.0 GPA	1998 and beyond high school graduate	100% tuition for full-time attendance at an eligible institution, and a \$400 annual stipend	General state revenues
	TOPS Honors Award	3.5 cumulative GPA	Non-Louisiana high school graduate: The state's reported prior year ACT composite average plus 3 points, but never less than 30 Louisiana high school graduate: The state's reported prior year ACT composite average, but never less than 19	3.0 GPA	1998 and beyond high school graduate	100% tuition for full-time attendance at an eligible institution, and a \$800 annual stipend	General state revenues
	TOPS-TECH Award	2.5 cumulative GPA	Non-Louisiana high school graduate: The state's reported prior year ACT composite average plus 3 points, but never less than 22	No	1998 and beyond high school graduates	An amount equal to tuition for up to two years of technical training at a public postsecondary	General state revenues

Table 1 (cont'd)

State	Michigan	Required high school GPA for initial eligibility	Required test scores	Required academic standard for the renewal of scholarship	Required year of high school graduation	The amount of scholarship award	The sources of funding for scholarship
Michigan	Michigan Merit Award Scholarship	No	Students must take the MEAP High School Test in mathematics, reading, science, and writing and score at level 1 (Exceed Michigan Standards) or level 2 (Meet Michigan Standards) on these four tests; or Students must meet or exceed Level 2 on at least two MEAP High School tests and satisfy one of the following plans: Plan A: scores at or above the 75 th percentile on the ACT or SAT tests. (For 2000 and 2001 high school graduates ACT composite score of 24 and above, or SAT combined score of 1170 and above) Plan B (for the award for vocational/technical training only): achieve qualifying score on the ACT Work Keys: Applied Mathematics Test: Score of 6 Reading for Information: Score of 6 Locating Information : Score of 4 Teamwork: Score of 5	The Michigan Merit Award is a one time, non-renewable award	2000 and beyond high school graduate	\$2,500 for the student who enrolled in an approved state institutions or \$1,000 for the student who enrolled in non-state institutions.	National Tobacco Settlement
Missouri	Higher Education Academic Scholarship Program	No	Have a composite score on ACT or SAT in the top 3 percent of all Missouri students taking those test.	To be a full-time student and maintain satisfactory academic progress as defined by the postsecondary institution	Yes, has to be full-time, first time student during the academic year following senior year in high school	\$2,000 for each academic year	General State Revenues

Table 1 (cont'd)

State		Required high school GPA for initial eligibility	Required test scores	Required academic standard for the renewal of scholarship	Required year of high school graduation	The amount of scholarship award	The sources of funding for scholarship
Nevada	Millennium Scholarship	3.0 grade point average	Must pass all areas of the Nevada High School Proficiency Exam	2.0 GPA; maintain satisfactory academic progress as defined by the postsecondary institution; must be enrolled 12 semester credit hours at a university or 6 semester credit hours at a community college	2000 and beyond high school graduates	\$80 per enrolled credit hour at a university; \$40 per enrolled credit at a community college	National Tobacco Settlement
New Mexico	Lottery Success Scholarship	Not for high school, but 2.5 GPA during their first college semester	No	Maintain academic progress	1997 and beyond high school graduation	100 of tuition at a public university, depending on funding availability	New Mexico Lottery
South Carolina	LIFE Scholarship	3.0 cumulative GPA	For 1998, and 1999 high school graduates: For 2000 and 2001 high school graduates: 1050 SAT, or 22 ACT For 2002 and beyond high school graduates: 1100 SAT, or 24 ACT No test score requirement for students attending 2-year or technical colleges	3.0 cumulative grade point average	The student must enroll in an eligible population within 2 years and three months of high school graduation	\$3,000 for students who attend 4-year public or private institutions; tuition for 30 credit hours for students who attend 2-year public and private institutions.	General state revenues

Table 2: Estimates of the Effect of HOPE-like Scholarship Programs on College**Enrollment**

Dependent Variable: Enrollment Rates	College	4-year College	4-year Public College	4-year Private College	2-year Public College
Treatment States: FL, GA, KY, LA, SC					
Control States: AL, AR, MD, MS, NC, OK, TN, TX, VA, WV					
Panel I. Difference-in-difference (DD) estimator for 18-19-year-olds					
Meritstate	0.053 *	0.094 ***	0.096 ***	-0.002	-0.028 *
	(0.029)	(0.028)	(0.026)	(0.012)	(0.017)
Number of observations	6430	6430	6430	6430	6430
State-specific time trend included	No	No	No	No	No
Panel II. DD estimator for 18-19-year-olds including state-specific time trend					
Meritstate	0.058 **	0.1 ***	0.11 ***	-0.005	-0.039
	(0.029)	(0.035)	(0.032)	(0.018)	(0.021)
Number of observations	6430	6430	6430	6430	6430
State-specific time trend included	Yes	Yes	Yes	Yes	Yes
Panel III. DD estimator for white 18-19-year-old					
Meritstate	0.065 **	0.082 *	0.11 ***	-0.03	-0.012
	(0.041)	(0.043)	(0.042)	(0.025)	(0.027)
Number of observations	5008	5008	5008	5008	5008
State-specific time trend included	Yes	Yes	Yes	Yes	Yes
Panel IV. DD estimator for black 18-19-year-olds					
Meritstate	0.041	0.16 ***	0.08	0.076 **	-0.11 *
	(0.060)	(0.062)	(0.067)	(0.035)	(0.055)
Number of observations	1238	1238	1238	1238	1238
State-specific time trend included	Yes	Yes	Yes	Yes	Yes

Notes: Treatment states are states with the HOPE-like program. Control states are states without the HOPE-like program. Each difference-in-difference (DD) estimation includes an intercept, a dummy variable for age 18, a dummy variable for urban status, a dummy variable for black, a dummy variable for sex, state unemployment rate, state dummies, and year dummies. Standard errors are reported in parentheses. The 1%, 5%, and 10% confidence levels are indicated with ***, **, and *, respectively.

Table 3: Estimates of the Effect of HOPE-like Scholarship Programs on College Enrollment Using Different Control and Treatment States

Dependent Variable: Enrollment Rates	College	4-year College	4-year Public College	4-year Private College	2-year Public College
Treatment States: FL, GA, KY, LA, SC					
Control States: AL, DC, DE, MD, MS, NC, OK, TN, TX, VA, WV					
Panel I. Difference-in-difference (DD) estimator for 18-19-year-olds					
Meritstate	0.044 (0.030)	0.087 ** (0.040)	0.071 ** (0.034)	0.015 (0.022)	-0.038 (0.023)
Number of observations	6144	6144	6144	6144	6144
State-specific time trend included	Yes	Yes	Yes	Yes	Yes
Treatment States: FL, GA, KY, LA, SC					
Control States: All other states					
Panel II. DD estimator for 18-19-year-olds including state-specific time trend					
Meritstate	0.1 ** (0.029)	0.1 *** (0.028)	0.1 *** (0.028)	0.0006 (0.017)	-0.03 * (0.022)
Number of observations	23724	23724	23724	23724	23724
State-specific time trend included	Yes	Yes	Yes	Yes	Yes
Treatment States: FL, GA, KY, LA, SC, NM, MO					
Control States: All other states					
Panel III. DD estimator for white 18-19-year-old					
Meritstate	0.044 (0.029)	0.089 *** (0.034)	0.084 *** (0.029)	0.05 (0.017)	-0.041 * (0.022)
Number of observations	23724	23724	23724	23724	23724
State-specific time trend included	Yes	Yes	Yes	Yes	Yes

Notes: Treatment states are states with the HOPE-like program. Control states are states without the HOPE-like program. Each difference-in-difference (DD) estimation includes an intercept, a dummy variable for age 18, a dummy variable for urban status, a dummy variable for black, a dummy variable for sex, state unemployment rate, state dummies, and year dummies. Standard errors are reported in parentheses. The 1%, 5%, and 10% confidence levels are indicated with ***, **, and *, respectively.

Table 4: Estimates for the Dynamics Analysis of the Effect of HOPE-like Scholarship Programs on College Outcomes

Dependent Variable: Enrollment Rates	College	4-year College	4-year Public College	4-year Private College	2-year Public College
Panel I					
Meritstate	0.043 (0.030)	0.088 *** (0.030)	0.09 *** (0.027)	-0.002 (0.013)	-0.031 * (0.018)
Meritlag	-0.046 * (0.027)	-0.026 (0.027)	-0.028 (0.025)	0.002 (0.024)	-0.016 (0.021)
Number of observations	6430	6430	6430	6430	6430
State-specific time trend included	No	No	No	No	No
Panel II					
Meritstate	0.036 (0.033)	0.099 ** (0.044)	0.1 *** (0.040)	-0.014 (0.020)	-0.063 *** (0.025)
Meritlag	-0.038 (0.028)	0.001 (0.035)	-0.01 (0.030)	0.006 (0.024)	-0.041 (0.027)
Number of observations	6430	6430	6430	6430	6430
State-specific time trend included	Yes	Yes	Yes	Yes	Yes
Panel III					
Meritstate	0.035 (0.031)	0.9 *** (0.023)	0.08 *** (0.021)	0.01 (0.011)	-0.049 ** (0.018)
Meritlag	-0.045 * (0.026)	-0.025 (0.026)	-0.027 (0.025)	0.002 (0.024)	-0.018 (0.022)
Meritlead1	-0.022 (0.048)	-0.036 (0.060)	-0.002 (0.056)	-0.032 *** (0.013)	0.022 (0.022)
Meritlead2	0.082 (0.052)	0.074 (0.064)	0.045 (0.066)	0.03 (0.023)	0.015 (0.028)
Number of observations	6430	6430	6430	6430	6430
State-specific time trend included	No	No	No	No	No

Table 4 (cont'd)

Dependent Variable: Enrollment Rates	College	4-year College	4-year Public College	4-year Private College	2-year Public College
Panel IV					
Meritstate	0.048 (0.033)	0.11 *** (0.040)	0.1 *** (0.035)	0.001 (0.019)	-0.068 ** (0.024)
Meritlag	-0.036 (0.031)	-0.002 (0.037)	-0.006 (0.031)	0.0034 (0.024)	-0.038 (0.028)
Meritlead1	-0.028 (0.040)	-0.038 (0.060)	-0.004 (0.061)	-0.034 *** (0.012)	0.018 (0.021)
Meritlead2	0.042 (0.043)	-0.045 (0.074)	0.037 (0.076)	0.008 (0.013)	0.012 (0.023)
Number of observations	6430	6430	6430	6430	6430
State-specific time trend included	Yes	Yes	Yes	Yes	Yes

Notes: Treatment states are states with the HOPE-like program. Control states are states without the HOPE-like program. Meritlag ==1 indicates if the state enacts the program in the next year. Merilead1 ==1 indicates if the program is in the first year, all years after. Merilead2 ==1 indicates if the program is in the second year, all years after. Difference-in-difference (DD) estimation includes an intercept, a dummy variable for age 18, a dummy variable for urban status, a dummy variable for black, a dummy variable for sex, state unemployment rate, state dummies, and year dummies. Standard errors are reported in parentheses. The 1%, 5%, and 10% confidence levels are indicated with ***, **, and *, respectively.

Table 5: Estimates of the Effect of HOPE-like Scholarship Programs on the High School Graduation and Dropout Rates

Dependent Variable	High School Graduate	High School Dropout
Treatment States: FL, GA, KY, LA, SC		
Control States: AL, AR, MD, MS, NC, OK, TN, TX, VA, WV		
Panel I		
DD estimators for high school graduate (18-19-year-olds) and dropout (16-18-years-olds)		
Meritstate	0.054 ** (0.034)	-0.023 * (0.034)
Number of observations	9240	17945
State-specific time trend included	Yes	Yes
Panel II		
DD estimors for high school graduate (white 18-19-year-olds) and dropout (white 16-18-years-olds)		
Meritstate	0.048 * (0.028)	-0.009 (0.015)
Number of observations	7128	13313
State-specific time trend included	Yes	Yes
Panel III		
DD estimors for high school graduate (black 18-19-year-olds) and dropout (black 16-18-years-olds)		
Meritstate	0.073 (0.060)	-0.062 ** (0.024)
Number of observations	1848	4080
State-specific time trend included	Yes	Yes

Notes: Treatment states are states with the HOPE-like program. Control states are states without the HOPE-like program. Each difference-in-difference estimation includes an intercept, a dummy variable for age 18, a dummy variable for urban status, a dummy variable for black, a dummy variable for sex, state unemployment rate, state dummies, and year dummies. Standard errors are reported in parentheses. The 1%, 5%, and 10% confidence levels are indicated with ***, **, and *, respectively.

Table 6: Estimates of the Effect of HOPE-like Scholarship Programs on the High School Graduation and Dropout Rates Using Different Control and Treatment States

Dependent Variable	High School Graduate	High School Dropout
Treatment States: FL, GA, KY, LA, SC		
Control States: AL, DC, DE, MD, MS, NC, OK, TN, TX, VA, WV		
Panel I		
Meritstate	0.039 (0.027)	-0.018 (0.013)
Number of observations	8835	17068
State-specific time trend included	Yes	Yes
Treatment States: FL, GA, KY, LA, SC		
Control States: All other states		
Panel II		
Meritstate	0.059 ** (0.026)	-0.023 * (0.013)
Number of observations	32638	62611
State-specific time trend included	Yes	Yes
Treatment States: FL, GA, KY, LA, SC, NM, MO		
Control States: All other states		
Panel III		
Meritstate	0.062 *** (0.025)	-0.027 ** (0.008)
Number of observations	32638	62611
State-specific time trend included	Yes	Yes

Notes: Treatment states are states with the HOPE-like program. Control states are states without the HOPE-like program. Each difference-in-difference estimation includes an intercept, a dummy variable for age 18, a dummy variable for urban status, a dummy variable for black, a dummy variable for sex, state unemployment rate, state dummies, and year dummies. Standard errors are reported in parentheses. The 1%, 5%, and 10% confidence levels are indicated with ***, **, and *, respectively.

Table 7: Estimates for the Dynamics Analysis of the Effect of HOPE-like Scholarship Programs on the High School Graduation and Dropout Rates

Dependent Variable	High School Graduate	High School Dropout
Treatment States: FL, GA, KY, LA, SC		
Control States: AL, AR, MD, MS, NC, OK, TN, TX, VA, WV		
Panel I		
Meritstate	0.063 ** (0.032)	-0.035 *** (0.014)
Merilag	0.015 (0.023)	-0.019 (0.013)
Number of observations	9240	17945
State-specific time trend included	Yes	Yes
Panel II		
Meritstate	0.06 * (0.035)	-0.048 *** (0.013)
Merilag	0.018 (0.024)	-0.019 (0.014)
Merilead1	0.01 (0.042)	0.034 ** (0.015)
Merilead2	0.019 (0.039)	-0.027 (0.022)
Number of observations	9240	17945
State-specific time trend included	Yes	Yes

Notes: Treatment states are states with the HOPE-like program. Control states are states without the HOPE-like program. Merilag ==1 indicates if the state enacts the program in the next year. Merilead1 ==1 indicates if the program is in the first year, all years after. Merilead2 ==1 indicates if the program is in the second year, all years after. Difference-in-difference (DD) estimation includes an intercept, a dummy variable for age 18, a dummy variable for urban status, a dummy variable for black, a dummy variable for sex, state unemployment rate, state dummies, and year dummies. Standard errors are reported in parentheses. The 1%, 5%, and 10% confidence levels are indicated with ***, **, and *, respectively.

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CHAPTER 3

THE EFFECTS OF EARLY CHILDHOOD EDUCATION PROGRAMS ON EMPLOYMENT DECISIONS OF MOTHERS: EVIDENCE FROM STATE PREKINDERGARTEN PROGRAMS

There has been flourishing debate about early childhood education programs for the last fifteen years in the American public policy arena. One area of concern in this debate is related to education reforms aimed at improving quality of entrants in primary education. Another area of concern focuses on equity issues and providing enough resources to children from low-income families so that they can be ready to learn in their future schooling, as are their peers from higher income families.

In addition to the early childhood programs funded at the federal level, states have also allocated considerable resources to provide prekindergarten services. States increased their expenditures on prekindergarten programs from \$700 million to 1.7 billion between the fiscal years of 1991-1992 and 1998-1999. Parallel to this development, the number of children served in these programs jumped from 290,000 to 725,000 (Blank et al., 1999). By comparison, the federal level Head Start program spent 4.3 billion and served over 822,000 children in 1998-1999.

The main objective of state prekindergarten programs is to provide education resources to children in their prekindergarten years to make sure that low-income children start their formal schooling with necessary and sufficient tools to learn (Adams et al., 1994). Furthermore, since the major welfare reform, Welfare-to-Work, was

implemented in 1996, concern about child care arrangements and early childhood services for welfare recipients and/or ex-welfare recipients have created growing interest in prekindergarten programs. In 1996, the Welfare-to-Work Act established time limits on how long welfare recipients can receive benefits before being obligated to move into employment. One obvious obstacle to employment for welfare recipients is the availability of child care, and their capacity to cover its cost.

In this paper, I exploit differences in the availability of prekindergarten programs across states and over the 1990-2000 time period to identify the effects of child care costs on the employment decision of mothers. I aim to contribute to the existing literature in three ways: first, I provide evidence on the impact of state prekindergarten programs on labor market behavior of mothers with eligible children, where the provision of prekindergarten services can be considered as a 100% subsidy for child care.¹⁴ Thus, I am able to estimate the price and income effects of child care subsidies even though I can't differentiate these two effects from each other.¹⁵ Second, this study is unique in assessing the possible effects nationwide of the provision of early childhood education in helping low-income families to meet child care costs and participate in the labor market. It sheds light on the possible employment effects of other early childhood education programs that certainly deserve attention from a public policy perspective, such as the Head Start program. Finally, my econometric approach does not require the arbitrary assumptions about functional form and the exclusion restrictions employed in many previous studies on these issues.

¹⁴ Gelbach (1999) pursued the same line research to analyze the effect of public school enrollment on mothers' employment decisions with five-year-old children.

¹⁵ Previous literature, except Gelbach (1999), estimated only the child care price elasticity of employment.

The structure of this paper is the following: the first part outlines how previous literature has attempted to identify the effect of child care costs on mothers' labor market outcomes. In the second part, I lay out the basic structure of prekindergarten programs. The third part explains how state funded prekindergarten programs affect labor supply behavior in a basic static labor supply model and identifies what predictions can be derived from the model. In the fourth part, I explain the estimation method and the data used in the analysis. In the fifth part, I present the main estimation results and provide robustness checks for the results. I then discuss the implications of the results.

I. Literature Review

In the literature analyzing the effects of child care costs on mothers' employment decisions, there are three different types of studies. The first type of study uses a probit model with a sample selection correction. These studies basically estimate the labor force participation equation including the market wage and the market child care prices as explanatory variables. Since these two variables are only observable for mothers who are already working and paying for child care, their values are imputed for others, after being corrected by a selection correction model. The reduced form of labor force participation is estimated to derive a selection correction term for estimating the wage equation. Deriving this term requires the exclusion of some variables from the wage equation that are included in the labor force participation equation. Second, the bivariate probit model for two binary outcomes, - whether the mother participates in the labor market or not and whether the mother pays for child care or not-, is estimated to obtain two distinct selection correction terms to compute fitted values for market child care prices. To

identify the effects of selection, some variables in the bivariate selection model are excluded from the market child care price equation. Finally, these imputed values of market child care prices and wages are included in the probit employment equation to estimate the effects of child care cost on mothers' employment decisions. Again, in order to identify child care cost in the employment equation, it is essential to find a variable that may be one of the determinants of the market child care price but does not have a partial effect on the labor force participation decision.

We can differentiate the studies within this group by the level at which they identify exogenous sources of variation in the child care cost. First, Connelly (1992), Ribar (1992), and Kimmel (1995, 1998) use variation in child care cost across individuals. Their results show that the estimated elasticity of employment with respect to the price of child care is between -0.20 and -0.92 .¹⁶ Second, Blau and Robbins (1988), and Anderson and Levine (2000) investigate geographic variation in child care cost. Their estimates range from $-.21$ to -0.303 .¹⁷

Blau (2000) argues that these studies may generate different estimated effects of child care cost on mothers' labor outcomes due to different ad-hoc identification assumptions in each study. To be more precise, Blau compellingly explains two important source of divergence in these estimates:

“It is possible that some of this variation is due to two problems discussed here: treating paid child care as if it were the best option for all mothers, and inappropriate exclusion restrictions to identify the child care price equation. Different identification restrictions are used in each study, possibly leading to different degrees of bias.” (Blau 2000, pp.50)

¹⁶ These estimates are for married mothers. Kimmel (1995, 1998) reports that the estimated employment elasticity with respect to the price of child care is -0.35 and -0.22 for single mother.

¹⁷ Their estimates for single mother range from -0.31 and -0.473 , except Blau and Robbins (1988) whose sample of analysis are only restricted to married mothers.

The second type of study that identifies the effect of child care cost on the labor market behavior of mothers applies the natural experiment method. Berger and Black (1992) define single mothers who are on a waiting list as the control group and the child care subsidy recipients as the treatment group, and analyze the labor market outcome differences between these two groups in Kentucky. They find that the impact of the subsidy on labor force participation is 12 percentage points. They argued that their estimates are likely to be biased for reasons that are common in the evaluation of social programs. First, single mothers may be chosen non-randomly from the waiting list to receive the subsidy based on their likelihood of being employed (the creaming effect). Second, there may be heterogeneity in preferences over child care and labor supply between single mothers in the waiting list and those who do not sign up for the subsidy program (the sign up effect). Third, single mothers in the waiting list start to change their employment behavior as they expect to enter the program and receive the subsidy (the waiting list effect). After Berger and Black (1992) controlled for these selection effects, the estimated effect of a subsidy on employment of single mothers is reduced to 8.4 %.

Gelbach (1999) analyzed the effects of public school enrollment of five-year-old children on the employment decision of mothers, recognizing that public school can be considered a comprehensively underwritten child care arrangement. Since the enrollment and the employment decision may be determined simultaneously, he exploited the quarter of birth of children as source of exogenous variation to identify the effects of public kindergarten school enrollment on mothers' labor market behavior. The idea is that mothers with five-year-old children born before the cutoff date to be enrolled in public kindergarten are more likely to participate in the labor market than mothers with the same

age children born after the enrollment cutoff date. Using 1980 Census data, Gelbach (1999) finds that public kindergarten enrollment increases the employment probability of mothers with youngest children aged five by 4-5 percentage points. Furthermore, his estimation results indicate that public school enrollment generates positive effects on hours of work per week, weeks worked per year, and wage income, while it reduces the likelihood of participating in welfare programs. However, these estimated positive effects do not remain for mothers with five-year-old and younger children.

In the third type of study, Ribar (1995) and Robins et al. (1992) estimate a structural model to gauge the effects of child care cost on the mothers' employment probabilities. Robins et al. (1992) find that there is no effect of child care cost on mothers' employment; while Ribar (1995) ascertained that the estimated employment elasticity ranges from -0.07 to -0.09 . Their results are extremely sensitive to their functional form specifications of the utility function.

II. Background Information on State Prekindergarten Programs

States design their prekindergarten programs in various ways. Some states prefer to initiate a separate prekindergarten program to deliver early childhood education services. Using this approach, states can determine how to fund their prekindergarten programs, the structure of the services in terms of funding and operation, and who will be served in these programs. A second alternative that some states opt for is to complement existing federal level programs such as Head Start. Under this approach, states may allocate funds to provide extra facilities, augment the quality of existing Head Start programs, or they may provide funding to match federal level funding without being

actively involved in delivering prekindergarten programs (Blank et al. 1999).

Furthermore, states may decide to initiate their own Head Start program with similar characteristics to the federal Head Start program. As a last alternative, states combine both strategies to serve prekindergarten age children in their state. Table 1 presents the diversity of early childhood education services across states. 36 states have their own prekindergarten programs, 11 states invest in state level Head Start programs and state prekindergarten programs, and 3 states only invest in Head Start programs.

This study exclusively focuses on the first type of approach in which states fund prekindergarten programs. Across states, these programs exhibit distinctive differences in how funding and provision of prekindergarten services take place.

A first difference is the institutional setting in which the programs operate. One is the school-based setting. States that provide prekindergarten programs in public schools focus on delivering childhood education services. In a second institutional setting, that of child care centers, Head Start programs as well as public schools can receive funds from state to provide prekindergarten education services along with social and health services that are not offered in the school based setting.

The second source of differences among state prekindergarten programs is how funds are distributed to institutions for the provision of their prekindergarten services. There are three different methods used by states to finance prekindergarten program initiatives. The first method is funding on a formula basis, which uses state aid per pupil and number of students in different weighing schemes to calculate the amount of grant the school receives. The second method is funding through a noncompetitive funding allocation process. Under this method, states try to assign funds to each school district

based on some measure of need. States have more discretionary power over which districts can receive funding, the eligibility conditions to participate in the program, and the kinds of services can be provided. The last method is based on the competitive grant process, which allows state to set their goals about what they expect from providers in delivering prekindergarten services and choose the provider who seems most likely to meet these goals.

Another source of differences among state prekindergarten programs is hours of child care service they provide. Table 3 depicts state programs by hours of operation, half school-day (half-time) and full school-day (full-time) programs.

Finally, states have developed certain criteria to define their target population for prekindergarten services. The eligibility conditions for these programs can certainly differ across states and over time within states. The most distinctive eligibility condition to restrict prekindergarten resources to specific groups of children is the age eligibility condition. Table 2 shows that fifteen states limit the participants in their programs to four-year-old children; thirteen states allow the participation of both three and four-year-old children; and ten states choose to serve a broad range of age groups. It should be noted that all states with prekindergarten programs give high priority to four-year-old children, who are one year from kindergarten.

As previously noted, the main concern of state prekindergarten programs is to prepare children from low-income families to be successful in school. Furthermore, since prekindergarten programs are basically about development and education of children, states naturally aim to serve children with various risk factors that make them more likely to be unsuccessful in school and to come from low income families. However, low-

income eligibility rules differ across states. Only five states define an income cutoff point specifically for prekindergarten services, while some of other states apply the same eligibility conditions to prekindergarten services as they do for the reduced lunch program or free lunch program.¹⁸ In addition, other groups of children often considered at risk and thus eligible for the programs are children whose primary language is other than English, children of families with low education level, children of teen parents, children who have been abused or neglected, children who live and/or lived in families with a history of substance abuse and children with inadequate housing (Mitchell et al. 1998).

In contrast, Georgia and Oklahoma¹⁹ serve all four-year-old children regardless of their family income. New York planned to provide universal prekindergarten services for all four-year-old children in 2002.

III. The Incentive Effects of Prekindergarten Programs

The effect of state prekindergarten programs on mothers' budget constraints is depicted in Figure 1. Assume that a prekindergarten program delivers fixed hours of child care, $h1$, with given constant quality.²⁰ If a mother chooses to work, she will receive the market wage per hour, w , and she has to incur the child care cost s for every hour she is working, assuming that she does not have unpaid child care. The income eligibility cutoff for the program is assumed to be $w * h2$. In the absence of any state-funded prekindergarten program, the budget constraint is denoted by the segment $abcd$. When

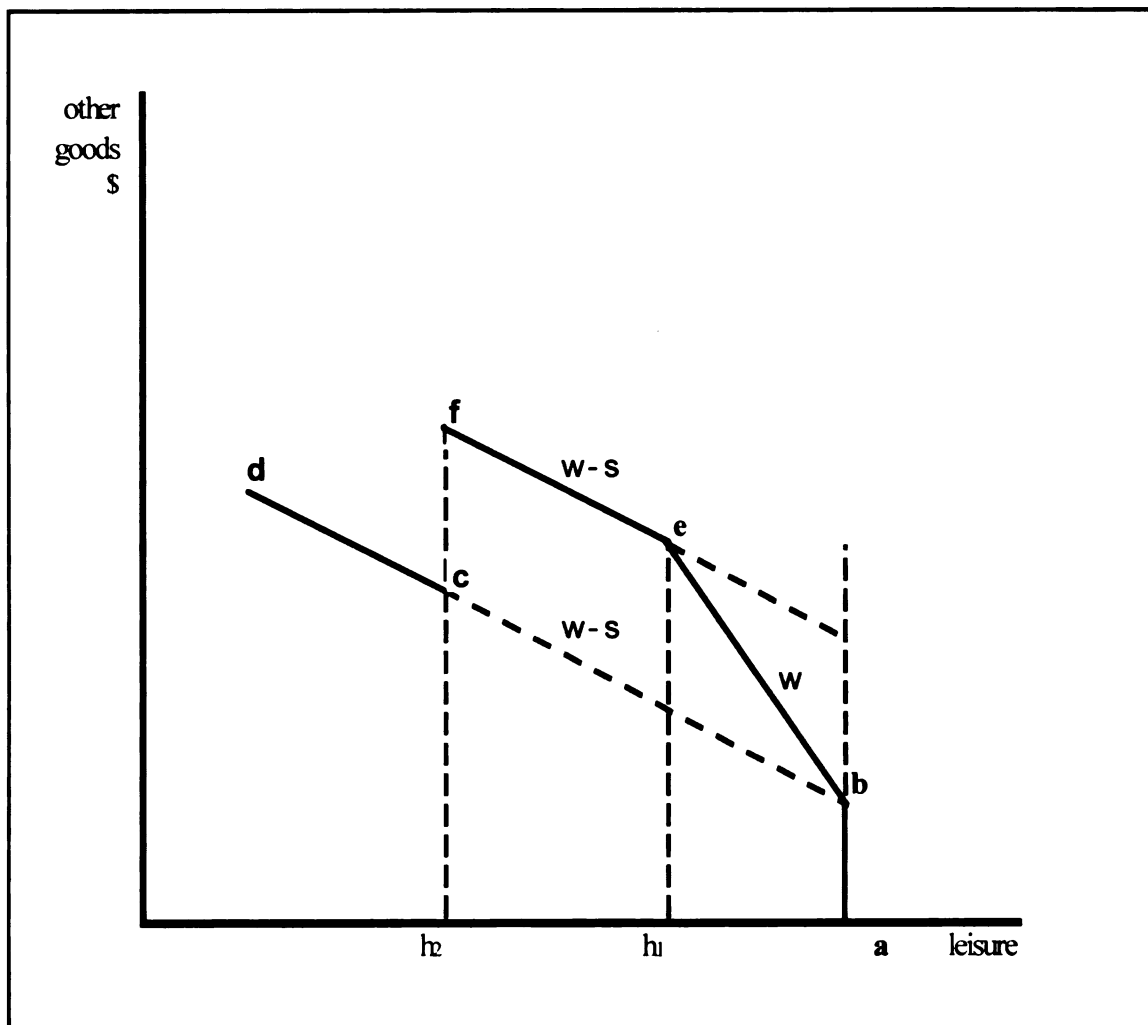
¹⁸ Their income eligibility condition for free and reduced-price lunches in the National School Lunch Program is below 130 and 185 percent of the federal poverty line.

¹⁹ In 1998, Oklahoma eliminated all requirements except the age eligibility condition for its prekindergarten program.

²⁰ I assume that the quality of child care service provided by the prekindergarten programs is constant across states.

the prekindergarten program is introduced, the mother's budget constraint becomes the segment *abefcd*. As the figure illustrates, the introduction of prekindergarten program increases income and utility for every individual choice of hours of work. The net-wage gain due to saving from the child care cost through participating in the prekindergarten program encourages some mothers to enter the labor force. Mothers who are already working remain in the labor market after the prekindergarten program has been implemented. Therefore the effect of prekindergarten programs on labor force participation of mothers is therefore unambiguously positive.

Figure 1: Prekindergarten Program and Labor Supply Decisions of Mother



The effect of the availability of the prekindergarten programs on hours of work of mothers who are already working depends on which segment of the budget constraint they are on before the prekindergarten programs are introduced. Mothers on segment be , working less than h_1 , will experience a rise in net wage. This causes a substitution effect encouraging an increase working hours, but also an income effect that causes hours to decrease. Mothers in the segment ef incur child care cost for each additional hour of child care beyond h_1 so their net wage remains at $w - s$. On the other hand, she receives $s \times h_1$ amount of income subsidy, which will lead her to reduce her working hours because of an income effect. Finally, there is a potentially a negative effect of prekindergarten programs on hours of work for those on the segment cd : Since they work more than h_2 , they are not eligible for the program. Some however, may be able to increase utility by moving into the segment ef after the introduction of the program So, the net effect of state prekindergarten program on aggregate hours of work for mothers is ambiguous, and will be determined empirically in this study.

IV. Data

The sample created to investigate the effects of state prekindergarten programs on the labor market behavior of mothers uses data from the March Current Population Survey (CPS) years 1990 to 2000. The March CPS contains information on labor market outcomes, income, demographic characteristics and state of residence for each household. Thus, I am able to determine the age of each household member, especially children, and thus determine whether the family has a preschool child.

To reduce variations in labor supply that are due to age and cohort effects, I further restrict the sample to women between 20 and 35 years of age who have at least one child. Also I exclude mothers whose oldest child is older than 15. Each mother in the sample is matched to information about the prekindergarten program in her state, based on information from various sources including personal communications with the departments of education in several states.

I examine the effects of state-funded prekindergarten programs on the probability of employment and weekly hours of work for both married mothers (two-parent families) and single mothers (single-parent families). Furthermore, I provide evidence on whether state-funded prekindergarten programs influence welfare program participation among single mothers with 12 or fewer years of education. I use a probit model to estimate the probability of employment and welfare participation. On the other hand, the hours of work is estimated by a simple linear model. Standard errors are adjusted to take into consideration correlation and heteroscedasticity within state-year cells in each model.

V. Empirical Methodology

In this paper, I study the labor market behavior of single and married mothers, but I give particular attention to single mothers for the following reasons. First, the decision to participate in the labor market for single mothers is most likely to be affected by lack of financial resources to meet child care costs. Anderson and Levine (2000) document that single mothers, who command fewer family resources than married mothers, spend a higher fraction of their income on child care arrangements for their children. Second, given the disproportionate number of single mothers who receive welfare, single mothers

are the most relevant group to examine to understand how early childhood education programs may help welfare recipients to have stable employment and ultimately become independent of welfare programs.

As explained previously, states use age-based and family income eligibility criteria to determine which groups of children can be served in the prekindergarten program. First, I use age of preschool children in households to identify mothers whose budget constraints are more likely to be altered by the introduction of the prekindergarten program. Since most states' age-based eligibility condition either requires children to be four years old or strongly favors four-year-olds over younger children in allocating spots, I primarily focus on the labor market behavior of mothers with four-year-old children.²¹ However, some caution is needed when using age eligibility conditions to identify the effects of these early childhood education programs. The impact of prekindergarten programs on mothers' employment decisions may differ between mothers whose youngest child is four years old and mothers who have four year old and younger preschool children. First, they are subject to different budget constraints, since mothers in the latter group have to incur additional child care costs for children who are younger than four. Second, there may be some cross-program effects for mothers whose youngest child is less than four years old. These mothers can have a different work incentive structure to become eligible for other means-tested programs such as Medicaid programs that may prevent us from capturing the causal effects of prekindergarten programs. Third, heterogeneity in fertility decisions across these two groups may bias the estimated effect of the prekindergarten program in the sense that the labor market decisions of mothers in

²¹ For example, Texas and Florida prekindergarten programs allow three and four-years-old to be eligible, but they mainly serve four-year-old children.

the later group may be more likely to be contaminated by the fertility decision, that is, the decision to have another child (Gelbach, 1999). In order to deal with these concerns, I examine the employment decisions of married mothers whose youngest children are aged four years, and drop mothers with four year old and younger preschool children from the sample. Finally, it should be noted that since the CPS contains information on age of children as of the month of the interview year, using age-based eligibility may cause measurement error. If a four-year-old child turns five between the enrollment cutoff date for a prekindergarten program and the month of the interview, he will be coded as a five-year-old child. Using married mothers with five-year-old child in the comparison group is evidently a misclassification that may bias the estimated effect downward. To address this problem, I exclude mothers with a five-year-old child from the sample.²²

As a second eligibility condition, states use family income-based criteria that makes free provisions of prekindergarten services available for children with low-income families.²³ To identify this condition in my empirical specification, I use the predicted income eligibility condition as a device to assign mothers to the treatment group and the control group. Specifically, I identify mothers whose family income is low enough to be eligible for the programs. For each family, I predict the probability that family income is below poverty level, which makes a family to be eligible for a prekindergarten program. Then I refer mothers whose predicted probability is in the 75th centile of the predicted

²² In addition, it is also possible that a three-year-old child may turn four between the enrollment cutoff date for a prekindergarten program and the month of the interview. The effect may be somewhat underestimated due to the misclassification of mothers with three-year-old children as mothers in the treatment group. Unfortunately, I cannot provide a solution to this measurement error.

²³ Although the income eligibility rule for the prekindergarten services differs across states, most states serves children who are eligible for free or reduced price lunch federal program services. In addition to that, one of the characteristics of state prekindergarten programs, which make them different from most of means-tested programs, is that children are not required to leave the program if their family income increases above the income eligibility level.

probability of being eligible as the treatment group. On the other hand, I refer mothers whose predicted probability is in the lower half of the distribution of the predicted probability of being eligible as the control group.

In this framework, I compare the labor force participation and hours of work for mothers with eligible children based on age between states with full-time prekindergarten programs and states without prekindergarten programs. I use the availability of prekindergarten program across states, which is the first source of variation to identify the effects of child care cost on mothers' employment decisions. Furthermore, since most state prekindergarten programs are initiated during the time period in which this study covers, I have another source of variation: that is, the labor market behavior of mothers with eligible children before and after prekindergarten programs within a state if the state has launched prekindergarten programs between 1990-2000. Finally, since the participation in prekindergarten programs depends on the age of children and family income, state prekindergarten programs create different budget sets for families with different levels of income and with different ages of children. This implies that the treatment group is mothers with eligible children and family income who are subject to the entire shift in their budget constraint due to the existence of prekindergarten program. Therefore state-funded prekindergarten programs result in variation in the budget constraint of mothers with children of different ages and family income level within a state, across states, and over time, providing a unique natural experiment to examine the effect of child care cost on the employment decisions of mothers.

I first apply the difference-in-difference-in-difference estimation (DDD) to the sample of mothers whose youngest child is four and mothers whose youngest child is older than five, using the equation below:

$$Labor\ Market\ Outcomes_{ist} = \alpha_0 + \alpha_1 pre - K_{st} + \alpha_2 child4_{ist} + \beta (pre - K * child4)_{ist} + X_{ist}\phi + state\ dummies + year\ dummies + \varepsilon_{ist} \quad (1)$$

where the dependent variable, *labor market outcomes*, is indexed by *i* for individual, *s* for state, *t* for years, and *pre-K* is a binary variable that equals one after state *s* has a prekindergarten program in year *t*. The variable *child4* is equal to one for mother with youngest child aged four, and zero otherwise. The interaction term, *pre-K*child4*, indicates the treatment group composed by mothers with youngest child aged four residing in states with prekindergarten programs. *X* is a vector that includes age and age squared, a set of dummy variables indicating different mother's education levels (high school dropout, high school graduate, some college years education, college graduate and beyond), a dummy variable for each child in the household, other family income, a dummy variable for whether individual lives in urban area or not, a dummy variable for whether individual is white or not.

In this difference-in-difference method, α_1 captures any time-varying factors affecting the labor supply of all women that may have been correlated with the introduction of state prekindergarten programs. α_2 captures the mean difference in labor market outcomes between mothers whose youngest child is four and those whose youngest child is older than five. The coefficient of interest, β , measures the mean differences in labor market outcomes between mother whose youngest child is aged four and mothers whose youngest child is older than five in states with prekindergarten

programs, relative to those in states without prekindergarten programs, before and after the programs have been introduced. This coefficient is intended to estimate the true causal effect of the prekindergarten program on mothers' employment decisions.

Theoretically, using the variation in the introduction of state-funded prekindergarten programs, one can obtain the difference-in-differences estimates of the effect of these programs for the sample of mothers whose youngest child is four-year-old. However, since there have been significant changes in welfare programs at the federal and state level over the 1990-2000 time period –the period studied-, the estimated effect of state-prekindergarten programs on labor supply of those mothers is most likely to be contaminated by changes in welfare programs that may have accompanied changes in state prekindergarten programs. In order to address this problem, I use the DDD estimator from Eqs. (1) in which mothers whose youngest child is older than five serve as a within-state control group to capture any changes in welfare programs as well as changes in child care programs and policies within state and over time. In addition, it captures state-specific differences that are common to all mothers in a state assuming that there are not underlying trends in labor market behaviors and child care arrangements that are to be distinct between the treatment and control groups.

In the second part of the empirical analysis, I examine whether the introduction of state prekindergarten programs assists mothers from low-income families to participate in labor market. As explained before, except Georgia and Oklahoma, most states aim to serve children from low-income families. However, only five states explicitly specify their income eligibility rule. These states are reported in Column 1 of Table 2. To be able to exploit the income eligibility rule for single and married mothers, I create a binary

variable for income eligibility condition. The binary variable indicating eligibility condition, *eligibility*, receives the value one if family income is below poverty level, and zero otherwise. I would then estimate the probability of being eligible for state prekindergarten programs using the following probit model.

$$\Pr(\textit{eligibility} = 1) = Z\varpi + \varepsilon \quad (2)$$

where, for single mothers, Z is a vector includes a set of dummy variables for mother's education, a dummy variable for white, a dummy variable for whether the individual lives in urban area, age, age squared, a set of dummy variables for previous marital status. On the other hand, for married mothers, Z includes a set of dummy variables for mother's education, a dummy variable for white, a dummy variable for whether the individual lives in urban area, age, age squared, a set of dummy variable husband's education, husband's age and age squared.

I use probit estimates from Eq. (2) to obtain predicted probabilities of being eligible for state-funded prekindergarten programs. To be exact, the mother is assigned to the treatment group (most likely to be eligible for the programs) if her predicted probability is in the highest quartile of the distribution of the predicted probability of being eligible. Conversely, on the other hand, the mother is assigned to the control group (least likely to be eligible for the programs) if her predicted probability is in the lower half of the distribution of the predicted probabilities.

In this framework, I now use the eligibility condition for the programs in the following regression

$$\begin{aligned}
Labor\ Market\ Outcomes_{ist} = & \alpha_0 + \alpha_1 pre - K_{st} + \alpha_2 child4_{ist} + \alpha_3 eligibility_{ist} + \\
& \beta_1(pre - K * child4)_{ist} + \beta_2(pre - K * eligibility)_{ist} + \beta_3(child4 * eligibility)_{ist} + \\
& \gamma(pre - K * child4 * eligibility)_{ist} + X_{ist}\phi + state\ dummies \\
& + year\ dummies + \varepsilon_{ist}
\end{aligned} \tag{3}$$

where eligibility is taking value of one for most likely eligible mothers for the programs, and zero otherwise.²⁴

In this regression the coefficient of interest is γ . It measures the extent to which the mean difference in labor market outcomes between eligible and ineligible mothers whose youngest child is four year old, relative to the difference between eligible and eligible mothers whose youngest child is older than five year, varies between states with prekindergarten programs and states without prekindergarten programs, before and after the programs have been implemented.

Finally, I explore the impact of state-funded prekindergarten programs on welfare program participation. Since single mothers with low education level is the most relevant group for welfare program participation, I focus on single mother with 12 or fewer years of education. I use DDD estimator similar to Eq. (1) below.

$$\begin{aligned}
WelPart_{ist} = & \eta_0 + \eta_1 prek_{st} + \eta_2 child4_{ist} + \iota(prek * child4)_{ist} + X_{ist}\kappa \\
& + state\ dummies + year\ dummies + v_{ist}
\end{aligned} \tag{4}$$

where *WelPart* is an indicator for welfare participation.²⁵ The coefficient of interest, ι , is intended to measure the causal effect of state prekindergarten programs on the welfare participation decision of mothers.

²⁴ In Eq. (3), X , a vector of control variables, does not contain other family income, which would be highly correlated with an indicator for the eligibility condition.

²⁵ Different from Eq. (1), X in Eq. (4) does not contain other family income to avoid the endogeneity problem in the welfare participation equation.

In all specifications, I include state dummies to capture state-specific effects. I also pick up aggregate time effects by including year dummies. It is necessary to control for the fact that aggregate time effects may be different across states. In order to address this concern, I use the state level unemployment rate in all specification. Alternatively, since specifications in Eqs (1), (3) and (4) allow me to use a within-state control group, I can control for state-specific aggregate time affects, assuming that they affect a within-state treatment and a within-state control group in a same way.

VI. Estimation Results

A. Single Mothers

I begin the empirical analysis by comparing differences in labor market outcomes between single mothers whose youngest child is four and single mothers whose youngest child is older than five in states which have implemented state-funded prekindergarten programs to the same differences in states which have not implemented state-funded prekindergarten programs. In panels A1 and A2 of Table 4, I present the results from the difference-in-difference-in-difference (DDD) estimation and the regression in Eq. (3). These estimation results suggest that single mothers with youngest child aged four increase their labor force participation. Specifically, the DDD estimator indicates that single mothers with eligible children increase their labor force participation by 7.1 percentage points, and it is significant at the 5% level. Likewise, state-funded prekindergarten programs increases hours of work by 3.6 per week, and this estimate is significant at the 1% level. Furthermore, the results in panel A2 also provides evidence for the positive estimated effect of these programs on single mothers. When I use the

family income to determine eligibility for the program, the estimated effect is 15 percentage points for labor force participation and 7.32 for hours of work per week, and these both estimates are significant at the 5% level.

Next, I examine whether the impact of prekindergarten programs may differ between single mothers with youngest child aged four and single mothers with four-year-old and younger preschool children. I allow for separate impact of the state-funded prekindergarten programs for single mothers with youngest child aged four and single mothers with four-year-old and younger preschool children, and report the results in panels B1 and B2 of Table 4. The estimated effect of prekindergarten programs on single mothers with youngest child aged four are quite similar to those in panels A1-A2. However, as for single mothers with four year old and younger preschool children, the coefficients of interest are imprecisely estimated. The estimated effect of these programs for labor supply of mothers remains positive, but they are smaller and not statistically significant. Therefore, it can be argued that additional child care cost for younger preschool children and different fertility preferences may weaken the estimated positive effect of prekindergarten programs on the employment decisions of mothers with preschool children aged four and younger.

Up to this point, I have used single mothers with youngest child over five as a within-state control group. Alternatively, I can use single mothers whose youngest child is two and under as within-state control group to capture state-fixed effects that are common to all mothers. It should be noted that since such mothers living in states with prekindergarten programs would be eligible for these programs when their children turn four, they may have different human capital investment behavior, and therefore different

labor market behavior than mothers living in states without prekindergarten programs. In order to examine this possible effect of prekindergarten programs, I add single mothers with youngest child two and under to the original sample. I then separately dummy this group of mothers and interact this dummy variable with the variable of interest. For both single mothers with youngest child 4 years old and single mothers with youngest child two and under, panel C1 reports the positive estimated effects of state prekindergarten programs on labor market behavior. The former group of mother increases their labor force participation by 7.3 percentage points and the weekly hours of work by 3.45. For the later group, the estimated effect is 6.9 percentage points and 3.45 additional hours per week. All of these estimates are significant at conventional levels. When I use the income eligibility rule to estimate the effect of these programs, the results in Panel C2 suggest that for single mothers whose youngest child is four-year-olds, the estimated effect 0.17 percentage points for labor force participation and 7.55 for hours of work per week, these effects are significant at the 5 % level. Conversely, single mothers with youngest children two and under reduce their employment probabilities by 8.2 and the weekly hours of work by 1.9 hours. However, I fail to reject the hypothesis that these mothers' labor market behavior is not significantly different from single mothers with youngest child over five.

Another specification issue I consider is the heterogeneity in terms of hours of operation that prevails in state prekindergarten programs. I assess to what extent the impact of state prekindergarten programs on single mothers' employment decision differs between states with full-time programs and states with half-time programs. To do so, I add states with half-time programs to the sample and dummy states with half-time

programs in Panels D1 and D2. While the estimated positive effects of the state prekindergarten programs remains robust for full-time programs, they are weakened in states where programs operate on the basis of a half school day. In addition, for states with half-time programs, the estimated effect becomes insignificant. This suggests that the differences in labor market outcomes between single mothers with eligible children in states with half-time programs and those in states without prekindergarten programs at all is not statistically significant. Thus, one can argue that 2-3 hours of free provision of prekindergarten services does not help single mothers much in overcoming the child care cost barrier which may inhibit their labor supply behavior.

Finally, I aim to detect differences in the estimated effect between means-tested (need-based) and not means-tested (universal) full-time state prekindergarten programs. The results are presented in panels E1 and E2. As shown in E1, each of the estimates of the effect of these programs is positive and statistically significant, except the estimated effect of latter program on labor force participation. However, when I use the predicted income eligibility condition in the estimation, the estimated effects are bigger and statistically significant for the programs with an income eligibility condition, relative to those of universal prekindergarten programs. This result suggests that the shift from need-based to universal provision of free prekindergarten services may reduce available resources to low income families to meet child care cost and have stable employment.

B. Married Mothers

I estimate the effects of state prekindergarten programs on the employment decisions of married mothers applying the difference-in-difference-in-differences estimation. The estimation results are reported in Table 5.

In panel A1 the DDD point estimates shows that there is negative and insignificant effect of prekindergarten programs on the employment probabilities of married mothers. Likewise, as reported in panel A2, the estimation results using income eligibility rule indicates that these programs do not alter mothers' labor force participation decision. For hours of work per week, the estimated effects of prekindergarten programs are also negative, negligible and insignificant.

Next, I examine the estimated effect of prekindergarten program on married mothers with youngest child aged four and married mothers with four-year-old and younger preschool children in panels A1 and A2 in Table 5. Overall, the results indicates the estimated effects on labor force participation and hours of work are small and insignificant for both groups of married mothers.

Alternatively, I allow separate effects of state funded prekindergarten programs for married mothers with youngest child four and married mothers with youngest child two and under in Panels C1 and C2. Each of estimates on the likelihood of participating in the labor force and hours of work suggest that state prekindergarten programs also fail to influence the labor supply of mothers with youngest child two and under.

I also provide the estimation of the impact of half-time state-funded prekindergarten programs in Panels D1 and D2. In this specification, the estimated effect of full-time prekindergarten program is -4.4 percentage points for the employment probabilities and -2.22 hours of work per week, these effects are imprecisely estimated. In addition, the estimation results imply that married mothers do not change their labor market behavior in response to the provision of half-time prekindergarten services.

The only statistically significant results for the estimated effect of the programs on the employment decisions of married mothers are observed in panel E1 of Table 5. The DDD estimate of universal programs is 4.6 percentage points, suggesting that these programs boost the employment probabilities of married mothers whose youngest child is four-year-old. This estimated effect is significant at the 10% level. Likewise, the estimated effect of hours of work per week is 2.36, and significant at the 5% level. On the other hand, the estimated effect of means-tested programs is -3.7 percentage points for labor force participation and -1.51 for hours of work per week. One possible reason for this finding is that it is possible that married mothers may be discouraged from using free prekindergarten services provided by mean-tested prekindergarten programs due to the social stigma attached to means-tested programs. Nevertheless, when I turn to the estimation results using the predicted income eligibility condition, the estimated effects for labor supply of married mothers, reported in panel E2, are negative, but they are not statistically significant.

C. Welfare Program Participation

In addition to the employment decision of mothers, I analyze the effect state prekindergarten programs on the likelihood of being a welfare recipient for the sample of single mothers with 12 or fewer years of education. This analysis allows us to evaluate whether the introduction of state prekindergarten programs helps these mothers to eliminate the availability of child care and its cost as obstacles to have stable employment and break dependency on welfare programs.

The most important issue in estimating the causal effect of the program on welfare program participation is to control for the fact that the introduction of

prekindergarten programs, or changes in these programs, are likely to be related with changes in welfare programs in the period studied here. The estimators presented in Table 6 takes care of this problem by using a within-state control group, assuming that changes in welfare programs are common to all married mothers.

Except the point estimate reported in panel E of table 6, the estimated effect of the full-time programs on welfare participation varies in very tight interval, (-0.054 to -0.057) for low-educated single mothers whose youngest child is aged four, suggesting that the programs reduce the likelihood of being a welfare recipient

However, these estimates are not statistically significant. In addition, the results in panels B1-D1 show that there is no heterogeneity treatment effect these programs. However, the DDD estimator in Panel E1 is -10 percentage points, and statistically significant at the 5% level. This result indicates that the universal prekindergarten programs reduce the probability that low-educated single mothers participate in welfare programs, unlike need-based programs. However, this finding is not consistent with the difference in the previously estimated employment effects of these two programs, which show that need-based programs have larger positive effect on the labor supply of low-income single mothers.

VII. Discussion and Conclusion

Overall, these results suggest that the provision of free prekindergarten programs does not affect the employment probabilities of married mothers whose youngest child is aged four residing in a state with a full-time prekindergarten program, relative to those mothers in states without a full-time prekindergarten program, in the same way that it

helps single mothers to move into employment. Specifically, my estimation results show that while the estimated effect of full-time prekindergarten programs on married mothers' employment probabilities varies between -5.55 and 4.4 percentage points, single mothers increase their labor force participation by 6.2 to 19 percentage points due to the introduction of full-time prekindergarten programs. In addition, I find that these programs enable single mothers to work 3.47 to 10.1 more hours per week. However, there is no significant effect of these programs on hours of work for married mothers, except for the estimated effect of universal prekindergarten programs..

One source of this difference is that married mothers are likely to have more financial resources originating from family income compared to single mothers. In addition, in two-parent families, the husband and perhaps his extended family become available as potential child care providers. These two characteristics of two-parent families may contribute to weaken the estimated effect of state-funded prekindergarten program on the labor supply of married mothers.

It can also be argued that this difference in the labor supply response to prekindergarten programs may be related to underlying tastes and preferences in child care services. Most of prekindergarten services are provided in a public school setting. It is possible that married mothers have different tastes relative to single mothers towards sending their preschool children to public school, suggesting that they are less likely to use these services, and hence to change their labor market behavior when they are introduced.

As noted before, state-funded prekindergarten programs mainly serve children from low-income families. However, it is likely that school and district policies on

providing prekindergarten services may be different within a state. To be more precise, some districts may receive more funding and be more willing to address the need child care services than are others. For example, prekindergarten programs in New Jersey and Connecticut mainly serve children in low-income districts (Blank and et al. 1999). One can argue that single mothers (single-parent families) may more likely sort across school districts to use child care services provided by prekindergarten program, compared to married mothers (two-parent families), reaching the conclusion that the estimated effect is expected to be greater for single mothers.

Moreover the variation in implementation of the prekindergarten program within states may limit the use of natural experiment framework applied in the analysis. Since local programs in a state may use different eligibility requirements, it is almost impossible to capture differences in state-funded prekindergarten program in how they formulate income eligibility condition. Likewise, the use of state-level availability of prekindergarten program may be somewhat limited by the lack of consistent eligibility income condition across states and over time. Therefore, I acknowledge that the estimation results are somewhat biased due to the measurement error in the income eligibility requirement.

In the analysis, I take account of the fact that the effects of prekindergarten programs are heterogeneous. First, the estimation results show that if the prekindergarten programs provide only half-day service, the positive employment effects of these programs for single mothers are diminished. This suggest that the provision of full-day prekindergarten services is an important public policy to alleviate the negative effects of the cost of child care on the employment decisions of single mothers, particularly when

recent changes in welfare programs require recipients to work. Second, I show that the estimated effect on labor supply is heterogeneous between single mothers with youngest child aged four and single mothers with both four-year-olds and younger preschool children. For the latter group the estimated effect is weakened, suggesting that the extension of free provision of prekindergarten services or additional child care services are needed to promote employment among single mothers with younger preschool children. In addition, need-based full-time prekindergarten programs have a greater positive effect on the employment behavior of single mothers than programs without the income eligibility requirement.

In the last part of the empirical analysis, I explore the effect of state prekindergarten programs on welfare participation. The results suggest that the implementation of the programs reduces the likelihood of being a welfare recipient by about 5.5 percentage points. Yet, these estimated effects are not statistically significant. On the other hand, the estimated effect of universal prekindergarten programs is bigger (-0.10) and statistically significant. It is difficult to offer an explanation for this finding.

Finally, I turn to compare my estimation results with findings in previous literature. Kimmel (1995, 1998) and Connelly (1992) simulate their estimation results to determine the impact of 100 percent child care subsidy on the labor force participation probabilities of mothers. Kimmel (1995, 1998) indicates that free child care increases the employment probabilities of single mothers by 16 percent,²⁶ while Connelly (1992) finds that married mothers increase their employment probabilities by 17 percent with the

²⁶ She does not simulate her estimation result for married mothers.

provision of free child care.^{27 28} Therefore, I can argue that the estimated effect of state prekindergarten programs on labor force for single mothers, reported here, is consistent results provided by previous studies, suggesting that child care subsidy increase the likelihood of being employed for single mothers. However, unlike previous studies, the estimated effect of prekindergarten programs for married mothers is negative, and statistically insignificant.

Similar to my study, Gelbach (1999) successfully estimates the total effect of child care subsidy--that is the price and the income effect. Analyzing the effects of free provision of public schooling on the employment decisions of mothers with youngest children aged five, Gelbach finds that public school enrollment increases single mothers' employment probabilities by 4 percentage points and hours of work by 2 hours, while corresponding estimates for married mothers are 4.7 percentage points and 1.7 hours.²⁹ Likewise, I find the positive effects of state prekindergarten programs on the labor supply of single mothers, which is consistent with Gelbach's results. On the other hand, unlike Gelbach, I did not find positive and significant effects for married mothers. However, I report that the estimated effect of universal prekindergarten programs is 4.6 percentage points for labor force participation and 2.36 for hours of work per week among married mothers whose youngest child is aged four, and these results are significant at the conventional level. Considering the fact that public school serve all five-year-old children, the finding for universal prekindergarten programs is consistent with his results.

²⁷ I calculated these two numbers by comparing their simulation results between when they pay the market full care price and when they receive 100 percent child care subsidy.

²⁸ Kimmel (1998) and Connelly (1992) both restrict their attention to the employment probabilities of mothers, and thus they do not simulate their results for hours of work. Furthermore, they study the labor market outcome of mothers with broad range of age of children.

²⁹ His estimation results are robust for married mothers with five and younger children, but not for single mothers with five and younger children.

One extension of this study would be to match information on prekindergarten programs in school districts into the data set, and then use information on variation in the provision of prekindergarten programs across school districts to identify the effects of child care costs on the labor market behavior of mothers. Furthermore, one could analyze the possible employment effects of different school programs such as before-school, after-school and special education programs using variation in these programs across school districts.

Table 1: Summary of the Existence of State Early Childhood Education Programs

	Year	State funding		No state funds for Prek or Head Start
		PreK programs	Head Start	
Alabama				x
Alaska			x	
Arizona	1991	x		
Arkansas	1991	x		
California	1966	x		
Colorado	1988	x		
Connecticut	1997	x	x	
Delaware	1994	x		
District of Columbia		x		
Florida	1987	x		
Georgia	1995	x		
Hawaii	1989/90	x	x	
Idaho				x
Illinois	1985	x		
Indiana				x
Iowa	1989	x		
Kansas	1999	x		x
Kentucky	1990	x		
Louisiana	1985	x		
Maine	1983	x	x	
Maryland	1979	x		
Massachusetts	1986	x	x	
Michigan	1985	x		
Minnesota	1991	x	x	
Mississippi				x
Missouri	2000	x		
Montana				x
Nebraska	1992			
Nevada				x
New Hampshire			x	
New Jersey	1996	x	x	
New Mexico	1991		x	
New York	1966*	x		
North Carolina				
North Dakota				x
Ohio	1990	x	x	
Oklahoma	1998**	x	x	
Oregon	1987	x		
Pennsylvania	1965	x		
Rhode Island	1997	x	x	
South Carolina	1984***	x		
South Dakota				x

Table 1 (cont'd)

	Year	State funding		No state funds for Prek or Head Start
		PreK programs	Head Start	
Tennessee	1998****	x		
Texas	1984	x		
Utah				x
Vermont	1987	x		
Virginia	1995	x		
Washington	1987	x	x	
West Virginia	1983	x		
Wisconsin	1800	x	x	
Wyoming				x

Sources: Mitchell, A., Ripple C. and Nina Chanana (1998). Kintzer, J and Page, S. (1998). Adams, Gina and Jodi S. (1994). Blank, H., Ewen, D., Schulman, K. (1999).

** 2002 Universal Pre-School for 4-year-old children.

*** Program renewed in 1991.

**** Pilot Pre-School Program.

Table 2: Age and Income Eligibility Conditions in State Prekindergarten Programs

	<i>Income eligibility requirement</i>	<i>State funding PreK programs</i>				
		<i>4-year-olds</i>	<i>3- and 4-year-olds (to K entry)</i>	<i>3-, 4- and 5-year-olds (to K entry)</i>	<i>3-years to 3rd grade</i>	<i>Birth to five years (to K entry)</i>
Alabama						
Alaska						
Arizona					x	
Arkansas				x		
California		x				
Colorado		x				
Connecticut			x			
Delaware		x				
District of Columbia		x				
Florida			x			
Georgia		x				
Hawaii	75% SMI		x			
Idaho						
Illinois	130% FPL			x		
Indiana						
Iowa	130% FPL		x			
Kansas		x				
Kentucky			x			
Louisiana		x				
Maine		x				
Maryland		x				
Massachusetts	100% SMI			x		
Michigan		x				
Minnesota				x		
Mississippi						
Missouri			x			
Montana						
Nebraska						
Nevada						
New Hampshire						
New Jersey			x			
New Mexico						x
New York			x			
North Carolina						x
North Dakota						
Ohio	185% FPL		x			
Oklahoma		x				
Oregon				x		
Pennsylvania		x				
Rhode Island					x	
South Carolina		x				
South Dakota						

Table 2 (cont'd)

	<i>Income eligibility requirement</i>	<i>State funding PreK programs</i>				
		<i>4-year-olds</i>	<i>3- and 4- year-olds (to K entry)</i>	<i>3-, 4- and 5- year-olds (to K entry)</i>	<i>3-years to 3rd grade</i>	<i>Birth to five years (to K entry)</i>
Tennessee			x			
Texas			x			
Utah						
Vermont			x			
Virginia		x				
Washington			x			
West Virginia		x				
Wisconsin		x				
Wyoming						

Sources: Mitchell, A., Ripple C. and Nina Chanana (1998). Kintzer, J and Page, S. (1998). Adams, Gina and Jodi S. (1994). Blank, H., Ewen, D., Schulman, K. (1999).

Table 3: Hours of Early Childhood Education Services in State Prekindergarten Programs

	<i>Half School-Day (2 -3.5 hours per day)</i>	<i>Full School-Day (6 -8 hours per day)</i>
Alabama		
Alaska		x
Arizona	x	x
Arkansas	x	x
California	x	
Colorado	x	
Connecticut	x	x
Delaware	x	
District of Columbia	x	x
Florida	x	x
Georgia	x	x
Hawaii	x	x
Idaho		
Illinois	x	
Indiana		
Iowa	x	
Kansas	x	
Kentucky	x	
Louisiana	x	x
Maine	x	x
Maryland	x	x
Massachusetts	x	x
Michigan	x	
Minnesota	x	x
Mississippi		
Missouri	x	
Montana		
Nebraska	x	
Nevada		
New Hampshire		x
New Jersey	x	x
New Mexico	x	
New York	x	
North Carolina	x	
North Dakota		
Ohio	x	x
Oklahoma	x	x
Oregon	x	
Pennsylvania		
Rhode Island	x	x
South Carolina	x	

Table 3 (cont'd)

	<i>Half School-Day</i> <i>(2 -3.5 hours per day)</i>	<i>Full School-Day</i> <i>(6 -8 hours per day)</i>
South Dakota		
Tennessee		
Texas	x	
Utah		
Vermont	x	
Virginia	x	x
Washington	x	x
West Virginia	x	
Wisconsin	x	x
Wyoming		

Sources: Mitchell, A., Ripple C. and Nina Chanana (1998). Kintzer, J and Page, S. (1998). Adams, Gina and Jodi S. (1994). Blank, H., Ewen, D., Schulman, K. (1999).

Table 4: Estimates of the Effects of State Prekindergarten Programs on Labor Force Participation and Hours of Work for Single Mothers

	Labor Force Participation	Hours of Work
Panel A1: DDD estimator for full-time programs		
Full-time pre-kxchild4	0.071 *** (0.029)	3.6 *** (1.34)
Full-time prek-k (==1 if pre-k program is a full-time program)	-0.023 (0.04)	0.11 (1.43)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.028)	-5.1 (0.93)
Number of observation	4748	4748
Panel A2: Estimates for full-time programs using the predicted income eligibility condition (PIEC)		
Full-time pre-kxeligiblexchild4	0.15 ** (0.057)	7.32 ** (3.32)
Eligiblexchild4	-0.017 (0.057)	-2.04 (2.04)
Full-time pre-kxeligible	-0.031 (0.042)	-1.73 (1.51)
Full-time pre-kxchild4	-0.064 (0.061)	1.32 (2.32)
Full-time pre-k	0.021 (0.046)	2.06 (1.84)
Child4	-0.094 (0.039)	-4.16 (1.35)
Eligible	-0.071 (0.051)	-2.8 (2.06)
Number of observation	3581	3581
Panel B1: DDD estimators for mothers with youngest child aged 4 versus mothers with children aged 4 and younger		
Full-time pre-kxchild4	0.076 *** (0.032)	3.52 *** (1.33)
Full-time pre-kxchild<=4	0.032 (0.045)	1.22 (1.55)
Full-time prek-k (==1 if pre-k program is a full-time program)	-0.014 (0.039)	0.13 (1.3)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.028)	-5.07 (0.91)
Child<=4 (==1 if mother has four-year-olds and younger children)	-0.14 (0.059)	-7.78 (1.66)
Number of observation	5404	5404

Table 4 (cont'd)

	Labor Force Participation	Hours of Work
Panel B2: Estimates for mothers with youngest child aged 4 versus mother with children aged 4 and younger using PIEC		
Full-time pre-kxeligiblexchild4	0.17 ** (0.059)	7.41 ** (3.29)
Full-time pre-kxeligiblexchild<=4	0.13 (0.08)	5.48 (3.95)
Eligiblexchild4	-0.03 (0.06)	-1.15 (2.06)
Eligiblexchild<=4	-0.039 (0.073)	0.63 (2.8)
Full-time pre-kxeligible	0.022 (0.043)	-1.91 (-0.83)
Full-time pre-kxchild4	-0.062 (0.061)	-0.92 (2.27)
Full-time pre-kxchild<=4	-0.008 (0.082)	-0.82 (3.4)
Full-time pre-k-k	0.022 (0.043)	1.55 (1.57)
Child4	-0.11 (0.036)	-4.5 (1.32)
Child<=4	-0.028 (0.085)	-6.91 (2.52)
Eligible	-0.074 (0.053)	-4.1 (1.81)
Number of observation	4063	4063
Panel C1: DDD estimators for mothers with youngest child aged 4 versus mothers with children aged 2 and younger		
Full-time pre-kxchild4	0.073 *** (0.033)	3.45 *** (1.32)
Full-time pre-kxchild<=2	0.069 *** (0.037)	2.48 ** (1.09)
Full-time pre-k-k (==1 if pre-k program is a full-time program)	-0.033 (0.037)	-0.19 (1.24)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.027)	-4.84 (0.88)
Child<=2 (==1 if mother has two-year-olds and younger children)	-0.054 (0.055)	-3.91 (1.74)
Number of observation	6719	6719

Table 4 (cont'd)

	Labor Force Participation	Hours of Work
Panel C2: Estimates for mothers with youngest child aged 4 versus mother with children aged 2 and younger using PIEC		
Full-time pre-kxeligiblechild4	0.17 ** (0.063)	7.55 ** (3.48)
Full-time pre-kxeligiblechild<=2	0.082 (0.069)	1.9 (2.81)
Eligiblechild4	-0.024 (0.06)	-2.01 (2.02)
Eligiblechild<=2	-0.006 (0.041)	0.86 (1.88)
Full-time pre-kxeligible	-0.058 (0.045)	-2.5 (1.83)
Full-time prekxchild4	-0.034 (0.056)	0.062 (2.16)
Full-time prekxchild<=2	0.051 (0.045)	2.5 (1.84)
Full-time prek-k	-0.022 (0.039)	0.66 (1.41)
Child4	-0.11 (0.036)	-4.47 (1.21)
Child<=2	-0.12 (0.06)	-5.97 (2.16)
Eligible	-0.061 (0.053)	2.9 (1.85)
Number of observation	5055	5055

Table 4 (cont'd)

	Labor Force Participation	Hours of Work
Panel D1: DDD estimators for full-time versus half-time programs		
Full-time pre-kxchild4	0.062 ** (0.03)	3.47 *** (1.3)
Half-time pre-kxchild4	0.026 (0.028)	1.47 (0.99)
Full-time prek-k (==1 if pre-k program is a full-time program)	-0.022 (0.037)	0.22 (1.25)
Half-time prek-k (==1 if pre-k program is a half-time program)	-0.071 (0.041)	-0.57 (1.44)
Child4 (==1 if youngest child is aged 4)	-0.099 (0.024)	-4.98 (0.82)
Number of observation	9689	9689
Panel D2: Estimates for full-time versus half-time programs using IPEC		
Full-time pre-kxeligiblexchild4	0.14 ** (0.059)	6.56 ** (3.17)
Half-time pre-kxeligiblexchild4	-0.001 (0.067)	2.09 (2.54)
Eligiblexchild4	-0.014 (0.054)	-2.04 (1.94)
Full-time pre-kxeligible	-0.034 (0.039)	-1.93 (1.46)
Half-time pre-kxeligible	-0.01 (0.032)	-1.74 (1.24)
Full-time prekxchild4	0.03 (0.058)	1.92 (1.49)
Half-time prekxchild4	-0.058 (0.049)	0.15 (1.86)
Full-time prek-k	0.03 (0.038)	1.91 (1.49)
Half-time pre-k	-0.058 (0.049)	0.15 (1.86)
Child4	-0.091 (0.036)	-4.34 (1.25)
Eligible	-0.022 (0.041)	-0.87 (1.62)
Number of observation	7256	7256

Table 4 (cont'd)

	Labor Force Participation	Hours of Work
Panel E1: DDD estimators for not means-tested versus means-tested programs		
Not means-tested full-time pre-kxchild4	0.065 (0.04)	4.36 *** (1.76)
Means-tested full-time pre-kxchild4	0.077 *** (0.033)	3.65 *** (1.57)
Not means-tested full-time prek-k (==1 if pre-k program is not means-tested full-time program)	0.021 (0.044)	1.41 (1.5)
Means-tested full-time prek-k (==1 if pre-k program is means-tested full-time program)	-0.017 (0.043)	0.07 (1.63)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.044)	-5.34 (0.95)
Number of observation	4748	4748
Panel E2: Estimates for not means-tested versus means-tested programs using PIEC		
Not means-tested full-time pre-kxeligiblexchild4	0.08 (0.093)	1.89 (4.67)
Means-tested full-time pre-kxeligiblexchild4	0.19 *** (0.054)	10.15 *** (3.75)
Eligiblexchild4	-0.022 (0.058)	2.07 (2.08)
Not means-tested full-time pre-kxeligible	-0.023 (0.052)	0.29 (1.95)
Means-tested full-time pre-kxeligible	-0.053 (0.046)	-3.38 (1.67)
Not means-tested full-time prekxchild4	-0.038 (0.083)	1.95 (2.11)
Means-tested full-time prekxchild4	-0.062 (0.069)	-1.68 (2.66)
Not means-tested full-time pre-k	0.053 (0.058)	1.94 (2.11)
Means-tested full-time pre-k	0.03 (0.05)	2.78 (2.08)
Child4	-0.097 (0.039)	-4.44 (1.4)
Eligible	-0.065 (0.052)	-2.62 (2.07)
Number of observation	3581	3581

Notes: The standard errors are reported in parenthesis. ***, **, * indicate the coefficient of interest is significant at the 1%, 5% and 10% level. For the labor force participation, the reported coefficients are scaled to measure the effects of marginal changes on the probability of employment. All specification includes a set of dummy variable for education levels, age, age squared, a dummy variable for black, a dummy variable for living in city, a dummy variable for each child in the household, state level unemployment rate, state dummies, and year dummies.

Table 5: Estimates of the Effects of State Prekindergarten Programs on Labor Force Participation and Hours of Work for Married Mothers

	Labor Force Participation	Hours of Work
Panel A1: DDD estimator for full-time programs		
Full-time pre-kxchild4	-0.012 (0.021)	-0.37 (0.88)
Full-time prek-k (==1 if pre-k program is a full-time program)	0.001 (0.021)	-0.53 (0.9)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.013)	-4.91 (0.57)
Number of observation	10588	10588
Panel A2: Estimates for full-time programs using the predicted income eligibility condition (PIEC)		
Full-time pre-kxeligiblexchild4	-0.016 (0.053)	-0.49 (2.14)
Eligiblexchild4	0.01 (0.03)	2.05 (1.42)
Full-time pre-kxeligible	0.002 (0.027)	-0.37 (1.16)
Full-time pre-kxchild4	0.004 (0.03)	-0.028 (1.15)
Full-time pre-k	-0.007 (0.028)	-0.81 (1.14)
Child4	-0.12 (0.021)	-5.53 (0.8)
Eligible	-0.053 (0.023)	-1.12 (0.95)
Number of observation	8030	8030
Panel B1: DDD estimators for mothers with youngest child aged 4 versus mothers with children aged 4 and younger		
Full-time pre-kxchild4	-0.018 (0.021)	-0.62 (0.87)
Full-time pre-kxchild<=4	0.001 (0.024)	0.2 (0.87)
Full-time prek-k (==1 if pre-k program is a full-time program)	0.004 (0.019)	-0.27 (0.82)
Child4 (==1 if youngest child is aged 4)	-0.12 (0.014)	-4.81 (0.55)
Child<=4 (==1 if mother has four-year-olds and younger children)	-0.1 (0.035)	-5.68 (1.12)
Number of observation	13976	13976

Table 5 (cont'd)

	Labor Force Participation	Hours of Work
Panel B2: Estimates for mothers with youngest child aged 4 versus mother with children aged 4 and younger using PIEC		
Full-time pre-kxeligiblexchild4	0.012 (0.05)	0.67 (2.09)
Full-time pre-kxeligiblexchild<=4	-0.055 (0.056)	-0.95 (1.86)
Eligiblexchild4	0.012 (0.032)	1.95 (1.39)
Eligiblexchild<=4	0.056 (0.031)	3.4 (1.29)
Full-time pre-kxeligible	0.01 (0.028)	-0.37 (1.17)
Full-time pre-kxchild4	0.002 (0.032)	-0.33 (1.15)
Full-time pre-kxchild<=4	0.029 (0.033)	0.58 (1.2)
Full-time pre-k-k	-0.006 (0.025)	-0.31 (0.98)
Child4	-0.13 (0.021)	-5.45 (0.79)
Child<=4	-0.11 (0.041)	-6.25 (1.31)
Eligible	-0.04 (0.022)	-0.54 (0.62)
Number of observation	10180	10180
Panel C1: DDD estimators for mothers with youngest child aged 4 versus mothers with children aged 2 and younger		
Full-time pre-kxchild4	-0.018 (0.022)	-0.62 (0.87)
Full-time pre-kxchild<=2	-0.003 (0.014)	0.15 (0.53)
Full-time pre-k-k (==1 if pre-k program is a full-time program)	-0.0031 (0.018)	-0.007 (0.76)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.013)	-4.4 (0.55)
Child<=2 (==1 if mother has two-year-olds and younger children)	0.034 (0.029)	0.66 (1.13)
Number of observation	20232	20232

Table 5 (cont'd)

	Labor Force Participation	Hours of Work
Panel C2: Estimates for mothers with youngest child aged 4 versus mother with children aged 2 and younger using PIE		
Full-time pre-kxeligiblexchild4	-0.028 (0.056)	-1.67 (2.11)
Full-time pre-kxeligiblexchild<=2	0.002 (0.026)	-1.2 (1.36)
Eligiblexchild4	-0.006 (0.032)	-1.6 (1.34)
Eligiblexchild<=2	0.012 (0.024)	-0.6 (0.94)
Full-time pre-kxeligible	-0.032 (0.032)	-0.37 (1.15)
Full-time pre-kxchild4	0.018 (0.028)	0.35 (1.14)
Full-time pre-kxchild<=2	-0.007 (0.021)	0.46 (0.77)
Full-time pre-k-k	0.007 (0.023)	0.25 (0.95)
Child4	-0.11 (0.02)	-3.86 (0.77)
Child<=2	0.031 (0.036)	0.69 (1.32)
Eligible	-0.009 (0.021)	-0.71 (0.61)
Number of observation	14715	14715
Panel D1: DDD estimators for full-time vs.half-time programs		
Full-time pre-kxchild4	-0.01 (0.021)	-0.24 (0.87)
Half-time pre-kxchild4	0.006 (0.017)	-0.049 (0.71)
Full-time pre-k-k (==1 if pre-k program is a full-time program)	-0.001 (0.019)	-0.43 (0.82)
Half-time pre-k-k (==1 if pre-k program is a half-time program)	-0.002 (0.022)	-0.044 (1.02)
Child4 (==1 if youngest child is aged 4)	-0.11 (0.012)	-4.78 (0.52)
Number of observation	21222	21222

Table 5 (cont'd)

	Labor Force Participation	Hours of Work
Panel D2: Estimates for full-time versus half-time programs using IPEC		
Full-time pre-kxeligiblexchild4	-0.044 (0.053)	-2.22 (2.03)
Half-time pre-kxeligiblexchild4	-0.01 (0.035)	-0.72 (1.44)
Eligiblexchild4	-0.002 (0.027)	-0.89 (1.13)
Full-time pre-kxeligible	-0.02 (0.025)	-0.018 (0.94)
Half-time pre-kxeligible	0.012 (0.021)	1.24 (0.78)
Full-time prekxchild4	0.019 (0.03)	0.73 (1.24)
Half-time prekxchild4	0.013 (0.023)	0.15 (0.9)
Full-time prek-k	0.005 (0.023)	-0.47 (1.05)
Half-time pre-k	-0.026 (0.025)	-1.71 (1.19)
Child4	-0.12 (0.018)	4.51 (0.7)
Eligible	0.007 (0.019)	-0.51 (0.58)
Number of observation	16118	16118
Panel E1: DDD estimators for not means-tested vs means-tested programs		
Not means-tested full-time pre-kxchild4	0.046 * (0.026)	2.36 ** (1.11)
Means-tested full-time pre-kxchild4	-0.037 * (0.023)	-1.51 ** (0.96)
Not means-tested full-time prek-k (==1 if pre-k program is not means-tested full-time program)	-0.021 (0.03)	-0.85 (1.18)
Means-tested full-time prek-k (==1 if pre-k program is means-tested full-time program)	0.017 (0.022)	0.29 (0.93)
Child4 (==1 if youngest child is aged 4)	-0.12 (0.014)	-5.03 (0.57)
Number of observation	10588	10588

Table 5 (cont'd)

	Labor Force Participation	Hours of Work
Panel E2: Estimates for not means-tested versus means-tested programs using PIEC		
Not means-tested full-time pre-kxeligiblechild4	-0.029 (0.068)	-2.83 (3.07)
Means-tested full-time pre-kxeligiblechild4	-0.074 (0.064)	-2.77 (2.24)
Eligiblechild4	0.02 (0.029)	-0.43 (1.3)
Not means-tested full-time pre-kxeligible	-0.037 (0.032)	-1.71 (1.31)
Means-tested full-time pre-kxeligible	0.004 (0.029)	0.97 (1.09)
Not means-tested full-time prekxchild4	0.062 (0.038)	3.75 (1.8)
Means-tested full-time prekxchild4	-0.005 (0.031)	-0.63 (1.32)
Not means-tested full-time pre-k	-0.012 (0.037)	-0.55 (1.51)
Means-tested full-time pre-k	0.012 (0.029)	-0.35 (1.41)
Child4	-0.12 (0.02)	-4.83 (0.78)
Eligible	0.016 (0.021)	-0.44 (0.63)
Number of observation	8030	8030

Notes: The standard errors are reported in parenthesis. ***, **, * indicate the coefficient of interest is significant at the 1%, 5% and 10% level. For the labor force participation, the reported coefficients are scaled to measure the effects of marginal changes on the probability of employment. All specification includes a set of dummy variable for education levels, age, age squared, a dummy variable for black, a dummy variable for living in city, a dummy variable for each child in the household, state level unemployment rate, state dummies, and year dummies.

Table 6: Estimates of the Effects of State Prekindergarten Programs on Labor Force Participation and Hours of Work for Single Mothers

	Welfare Program Participation
Panel A: DDD estimator for full-time programs	
Full-time pre-kxchild4	-0.055 (0.042)
Full-time prek-k (==1 if pre-k program is a full-time program)	0.064 (0.048)
Child4 (==1 if youngest child is four years old)	0.14 (0.036)
Number of observation	2902
Panel B: DDD estimators for mothers with youngest child aged 4 versus mothers with children aged 4 and younger	
Full-time pre-kxchild4	-0.057 (0.046)
Full-time pre-kxchild<=4	-0.006 (0.056)
Full-time prek-k (==1 if pre-k program is a full-time program)	0.056 (0.048)
Child4 (==1 if youngest child is aged 4)	0.16 (0.036)
Child<=4 (==1 if mother has four-year-olds and younger children)	0.24 (0.081)
Number of observation	3415
Panel C: DDD estimators for full-time versus half-time programs	
Full-time pre-kxchild4	-0.054 (0.044)
Half-time pre-kxchild4	0.015 (0.036)
Full-time prek-k (==1 if pre-k program is a full-time program)	0.057 (0.018)
Half-time prek-k (==1 if pre-k program is a half-time program)	0.018 (0.049)
Child4 (==1 if youngest child is aged 4)	0.13 (0.033)
Number of observation	5918

Table 6 (cont'd)

	Welfare Program Participation
Panel D: DDD estimator for not means-tested versus means-tested programs	
Not means-tested full-time pre-kxchild4	-0.1 ** (0.054)
Means-tested full-time pre-kxchild4	-0.05 (0.049)
Not means-tested full-time prek-k (==1 if pre-k program is not means-tested full-time program)	0.045 (0.056)
Means-tested full-time prek-k (==1 if pre-k program is means-tested full-time program)	0.023 (0.051)
Child4 (==1 if youngest child is aged 4)	0.15 (0.036)
Number of observation	2902

Notes: The standard errors are reported in parenthesis. ***, **, * indicate the coefficient of interest is significant at the 1%, 5% and 10% level. For the labor force participation, the reported coefficients are scaled to measure the effects of marginal changes on the probability of employment. All specification includes a set of dummy variable for education levels, age, age squared, a dummy variable for black, a dummy variable for living in city, a dummy variable for each child in the household, state level unemployment rate, state dummies, and year dummies.

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