UNIVERSITY OF CALIFORNIA,
IRVINE

Essays on the Economics of Education, Labor, and Health

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Jessica Nicole Monnet

Dissertation Committee:
Professor Damon Clark, Chair
Professor Marianne Bitler
Professor Mireille Jacobson
Professor David Neumark

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DEDICATION

To my better half, Kyle.

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Earning a Ph.D. requires hard work and sacrifice, and over the last five years, I have done both. That said, no one completes a Ph.D. program without help from others along the way. I would, therefore, like to acknowledge those who have taken the time to help me. Let me begin by thanking the chair of my committee, Professor Damon Clark, as well as the other members of my committee, Professor Marianne Bitler, Professor Mireille Jacobson, and Professor David Neumark. You have been incredibly generous with your time and advice, and I am a better economist (and researcher) because of it.

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CURRICULUM VITAE

Jessica Nicole Monnet

Education

- Ph.D. Candidate, Department of Economics, University of California - Irvine, 2018 (*expected*)
- M.S., Economics, Department of Economics, University of Oregon, 2011
- B.A., Economics, Department of Economics, University of Oklahoma, 2009

Research

- **Working Papers**

  "The Effect of Preschool Participation on Intellectual and Behavioral Disorder Diagnoses: Evidence from Surveys on Children's Health," Revise and Resubmit at Economics of Education Review, 2017

  "The Effect of Reduced Coverage for Inpatient Psychiatric Care on Emergency Department Utilization" 2017

  "Bringing Back the Blue Collar: The Effect of Reduced Cost for Career Technical Education in Kentucky," 2017

  "Program Evaluation of Big Brothers Big Sisters of Orange County and the Inland Empire," 2016

- **Academic & Research Experience**

  Big Brothers Big Sisters of Orange County and the Inland Empire, Program Evaluation, supervised by David Neumark of the UCI Economic Self-Sufficiency Policy Research Institute (Spring 2016 - Summer 2016)

  Graduate Student Researcher on Project III of the INID PO1 Grant under the direction of Marianne Bitler at University of California - Davis (Spring 2015 – Winter 2016)

  Research Assistant for Marianne Bitler, University of California - Davis (Spring 2014 - Winter 2015, Summer 2016)

  Research Assistant for Damon Clark, University of California - Irvine (Summer 2014 - Winter 2015)

Conference & Seminar Presentations

- **Conferences**

  APPAM Regional Student Conference, April 2017, Sex, Drugs, and School: Impacts on Social Equity

  Society for Research on Educational Effectiveness (SREE), March 2016, Early Childhood Education Promoting the Early Academic Success of At-Risk Students

- **Seminars**

  University of California at Irvine, Department of Economics, Public & Labor Seminar, Fall 2017

  University of California at Irvine, Department of Economics, Public & Labor Seminar, Spring 2017

  University of California at Irvine, Department of Economics, Applied Microeconomics Seminar, Winter 2016

Awards & Fellowships

- **Awards**

  Charles A. Lave Prize for Creative Modeling in Social Sciences (2016) for *The Effect of a Preschool Education on Cognitive and Non-Cognitive Skill: Evidence from Surveys on Children's Health*

- **Fellowships**

  University of California - Irvine, Associate Dean Fellowship (Winter 2018)

  University of California - Irvine, Department of Economics, Summer Fellowship (Summer 2015, Summer 2016, Summer 2017)

  Economic Self-Sufficiency Policy Research Institute (ESSPRI) Fellowship, UCI (Spring 2016)

ABSTRACT OF THE DISSERTATION

Essays on the Economics of Education, Labor, and Health

By

Jessica Nicole Monnet

Doctor of Philosophy in Economics

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Professor Damon Clark, Chair

In the first chapter of this dissertation, I examine the effect of preschool participation on the probability of ever being diagnosed with certain intellectual and behavioral disorders in childhood. To study this relationship, I use two-sample two-stage least squares where I instrument for preschool participation using an indicator for the availability of universal preschool. I find that, for children from low-education households, preschool participation reduces the probability of ever having been diagnosed with behavioral or conduct problems, and requiring the use of special therapy. For children from high-education households, however, I observe that preschool participation increases the probability of ever having been diagnosed with behavioral or conduct problems, and requiring the use of special therapy.

In the second chapter, I study the effect of participation in career technical education on high school, post-secondary, and labor market outcomes. To study this relationship, I exploit a policy change in Kentucky that reduced the cost of participation in career technical education for high school students. I find that participation in career technical education increases the probability of completing high school, earning a KOSSA certificate, and attending college. I find positive but imprecisely estimated effects for the probability of being employed, and annual earnings. I do not observe a statistically significant effect for the probability of earning a program certificate.

In the final chapter, I study the effect of reduced insurance coverage for inpatient psychiatric care on emergency department utilization. I examine this relationship by exploiting Medicaid's Institutions for Mental Disease exclusion, which states that for beneficiaries between the age of 21 and 64, inclusive, Medicaid will not reimburse for medically necessary inpatient psychiatric care administered at an institution with more than sixteen beds. Using a "fuzzy" regression discontinuity framework, I find that at age 21 reduced coverage for inpatient psychiatric care increases mental health emergency department visits by 11 percent for Medicaid-eligible men. I argue that we can interpret this increase in mental health emergency department visits as the unintended cost of reduced coverage for inpatient psychiatric care. For Medicaid-eligible women, I tend to find statistically insignificant effects.

Chapter 1

The Effect of High Quality Preschool Participation

1.1 Introduction

It is the belief of many researchers and policymakers that a high-quality preschool education can improve a child's long-term outcomes. Often, however, the only options available to low-income families are Head Start programs, many of which are oversubscribed. During the 1990s, Georgia and Oklahoma introduced Universal Preschool (UPK) programs with the intent of addressing this inequity by increasing access to a preschool education. These programs provide free and voluntary access to a preschool education for all four-year-old children statewide (i.e., all children who turn four on or before September 1), while satisfying most of the quality benchmarks set by the National Institute for Early Education Research (NIEER) (Barnett et al., 2015, 2013, 2012).

Prior research routinely observes that UPK programs increase preschool participation (Fitzpatrick, 2010; Cascio and Schanzenbach, 2013) and that children exposed to these programs have higher test scores in kindergarten and the fourth grade (Gormley and Gayer, 2005; Fitzpatrick, 2008). While test scores can inform us about a child's cognitive skill, they tend to provide little insight into a child's non-cognitive skill (Heckman and Rubinstein, 2001). For that reason, other papers have used scores from teacher administered tests and evaluations to measure the effect of UPK on non-cognitive skill (Gormley and Gayer, 2005; Gormley et al., 2011; Magnuson et al., 2007). The literature has also looked at the effect of preschool participation on long-run outcomes such as criminal activity, high school completion, years of schooling, income, and life expectancy (Smith, 2015; Reynolds et al., 2001; Rossin-Slater and Wüst, 2015).

My contribution to this literature is twofold. First, I exploit the introduction of UPK in Oklahoma and Georgia to study the effect of preschool participation on the probability of ever being diagnosed with certain intellectual and behavioral disorders in childhood. In this paper, the phrase intellectual and behavioral disorders is used to describe the following conditions: learning disabilities, ADD/ADHD, behavioral or conduct problems, and requiring the use of special therapy.[1] These outcomes can be thought of as proxies for cognitive and non-cognitive skill. For example, a learning disability diagnosis is arguably representative of a child's ability to reason, remember, and problem solve (i.e., cognitive skill). Similarly, a behavioral or conduct problems diagnosis can be representative of a child's softer skills such as their interactions with peers, and temperament (i.e., non-cognitive skill). In other words, the estimates presented in this paper provide valuable complementary evidence to the existing literature. Second, these diagnosis outcomes are for children between the ages of four and seventeen, allowing me to estimate the short- and potentially medium-term outcomes of high-quality preschool participation.

To examine the relationship between preschool participation and the probability of diagnosis, I use a two-sample two-stage least squares (TS2SLS) framework where I instrument for

[1] Special therapy includes several therapy types, one of which is speech therapy.

preschool participation using an indicator for UPK availability. More specifically, I use data from the October Supplement of the CPS to predict the probability of preschool participation at the age of four. I then use that predicted probability, and data from the National Survey of Children's Health (NSCH), to estimate the effect of preschool participation on the outcomes of interest. Leveraging this strategy, I observe that preschool participation does not have a statistically significant effect on the probability of ever being diagnosed with the intellectual and behavioral disorders considered in this paper. Heckman et al. (2006) provide a potential explanation for this observation. Their work suggests that the ability and engagement of parents plays an important role in the human capital accumulation of children. In other words, children from low-socioeconomic status (SES) households might benefit more from programs like UPK.[2]

I, therefore, stratify my estimates by the highest level of educational attainment in the household, which serves as a proxy for the SES of the household.[3] For children from low-education households, I observe that participating in UPK reduces the probability of ever being diagnosed with behavioral or conduct problems, and requiring the use of special therapy as a child by 7.3 and 13.5 percentage points, respectively. For children from high-education households, I observe that participating in UPK increases the probability of ever being diagnosed with behavioral or conduct problems, and requiring the use of special therapy as a child by 5.6 and 10.8 percentage points, respectively.

My analysis points to the following conclusion. Preschool participation disproportionately benefits children from low-education households by reducing their probability of diagnosis (i.e., an improvement in non-cognitive skill). The obvious explanation is that, for children from low-education households, UPK provides an environment that is of higher quality than

[2]This is consistent with findings from Havnes and Mogstad (2015), who find that universal child care in Norway disproportionately benefits children from low-income households.

[3]In this paper, households are defined as low-education if the highest level of education is a high school degree or less, and households are defined as high-education if the highest level of education is more than a high school degree. An alternative proxy for the SES of the household would be household income, but this could be affected by the introduction of UPK (via increased parental employment).

their counterfactual. Conversely, for children from high-education households, the observed relationship between UPK and the probability of diagnosis suggests that their counterfactual environment is of higher quality than the treatment environment. This finding is consistent with Vandell et al. (2010), who find that high-quality early childhood care is associated with an increase in cognitive skill, and a decrease in externalizing behavior at age 15.

The rest of the paper proceeds as follows. In section 2, I briefly discuss UPK in Georgia and Oklahoma, as well as the related literature. In section 3, I present my empirical strategy. In section 4, I discuss the data, why I use TS2SLS, the observations excluded from the analysis, and how I define the instrumental variable. In section 5, I present the first-stage and two-sample two-stage least squares estimates. And finally, in section 6, I provide some concluding remarks.

1.2 Background & Related Literature

In 1995, Georgia became the first state to make preschool universally available to all four-year-old children. Oklahoma soon followed, and in 1998 it became the second state to introduce universal preschool. These UPK programs share two important characteristics. First, both programs are voluntary and all children who turn four years old on or before September 1 of that year are eligible to participate. Second, both programs satisfy a majority of the NIEER quality benchmarks. Some of the NIEER quality benchmarks satisfied by both programs include: teachers have specialized training in early childhood education, small class sizes (i.e., 20 students or less), and the program provides at least one meal a day. While both states provide a high-quality and universal preschool education, there is some variation between the two programs. For example, in Oklahoma eligible children have the option to attend either a half- or full-day preschool program. In contrast, Georgia's UPK program does not offer a half-day option. In addition, the two programs differ in how they are administered and funded. For example, Oklahoma's UPK program is administered by the school districts and funded through a combination of state, local, and Title 1 funds. Georgia's program, however, is conducted in public and private childcare facilities, and funded entirely by the state lottery.

There are a number of papers that have evaluated these UPK programs. These papers include Fitzpatrick (2010) and Cascio and Schanzenbach (2013), who find evidence suggesting that UPK increases the probability of preschool participation. Specifically, Fitzpatrick (2010) finds that UPK increases participation in public preschool by 10 percentage points in Oklahoma and 16 percentage points in Georgia while reducing participation in private preschool by 3 and 6 percentage points in Oklahoma and Georgia, respectively. Similarly, Cascio and Schanzenbach (2013) find evidence supporting this relationship while also finding that the effect of UPK on preschool participation varies with the level of maternal education. They find that children from households with low levels of maternal education see a 19 percentage point increase in preschool participation while children from households with higher levels maternal education experience a 12 percentage point increase in preschool participation.

In addition, Gormley and Gayer (2005) and Fitzpatrick (2008) find evidence that these programs have a positive effect on test scores. Specifically, Gormley and Gayer (2005) find evidence of a positive relationship between participation in high-quality preschool in Tulsa, Oklahoma and scores from teacher administered tests.[4] The authors observe that this relationship is particularly strong among Hispanic and black children. Fitzpatrick (2008) also finds that the introduction of UPK in Georgia increases the probability of being on-grade for fourth graders. Consistent with findings from Gormley and Gayer (2005), she observes that this relationship is strongest among black students who are not eligible for free or reduced price school lunch.

[4]Gormley and Gayer (2005) study the effect of preschool participation on scores from teacher administered tests measuring social/emotional, cognitive/knowledge, motor, and language skills. They find that preschool participation increases scores for all of the above categories except social/emotional skills, where they do not observe an effect.

To the best of my knowledge, prior research has not studied the effect of preschool participation on the probability of being diagnosed with intellectual and behavioral disorders using parental-reported diagnoses. That said, Magnuson et al. (2007) use the Early Childhood Longitudinal Survey to evaluate the effect of preschool participation on classroom behavior.[5] They find that participation in preschool may reduce a child's self-control while increasing their externalizing behavior. In contrast, Gormley et al. (2011) use teacher ratings of student behavior to study the effect of preschool participation in Tulsa, Oklahoma. They find that participation in high-quality preschool can improve the social-emotional development of participants.

Prior work has also examined the long-run effects of preschool participation. For example, Reynolds et al. (2001) find that participation in the preschool component of the Chicago Child-Parent Center (CPC) Program reduced the rate of juvenile arrests, violent arrests, and school dropout, and increased the rate of high school completion, and years of schooling. They also found that participation in the CPC program (both the preschool and school-age interventions) was associated reduced grade retention, and special education services. Smith (2015) looks at the effect of UPK in Oklahoma on the probability of criminal activity as a young adult. Consistent with the findings from Reynolds et al. (2001), he finds evidence indicating that UPK reduces the probability that at-risk children are charged with a crime at the age of 18 or 19. Rossin-Slater and Wüst (2015) look at the long-run effects of early childcare and nurse home visiting in Denmark. Their results suggest that early childcare has positive effects on long-term outcomes such as years of schooling, income, and life expectancy.

The underlying premise of this paper is that if preschool participation increases human capital accumulation prior to the start of compulsory schooling, and if diagnosis is a function of a child's cognitive and non-cognitive skill as well as other genetic inputs, then this increased human capital will be represented by a reduced probability of ever being diagnosed with

[5] The behavioral outcomes used in Magnuson et al. (2007) come from teacher reports of student's self-control and externalizing behavior in the classroom.

certain intellectual and behavioral disorders in childhood (i.e., between the ages of four and seventeen).[6]

The idea that diagnosis is correlated with a child's achievement is supported by Ysseldyke et al. (1982), who observe that low-achieving and learning disabled students performed similarly on a battery of psychoeducational tests. The authors explained that their results could be interpreted as implying that more low-achieving students should be classified as learning disabled and, therefore, a number of children are going undiagnosed. Alternatively, their results could imply that low-achieving students are being incorrectly identified as learning disabled. Under either interpretation, we should expect to see that increasing human capital accumulation reduces the probability of being diagnosed with one of the conditions considered in this paper. The caveat for this hypothesis is that, conditional on the child's underlying level of human capital, negative peer effects could increase their probability of diagnosis.

Moreover, previous research has shown that certain behavioral disorders are negatively correlated with future outcomes such as high school graduation, college attendance, and future earnings. Stabile and Allin (2012) provide a review of this literature. In that review, they discuss work by McLeod and Kaiser (2004) who find that children who were diagnosed with behavioral problems at six to eight years of age have a reduced probability of graduating high school or attending college. These findings are consistent with those reported by Heckman et al. (2013), who find that improvements in non-cognitive skill explain a significant portion of the long-term effects of the Perry Preschool program.

The above discussion is intended to illustrate two points. First, the findings from Ysseldyke

[6] Children are identified and diagnosed with intellectual and behavioral disorders through a multi-step process that is initiated when a teacher recommends one of their students for further evaluation. A parent or healthcare professional can also initiate this process. Sax and Kautz (2003), however, observe that on average teachers are the first to identify the symptoms of ADD/ADHD in children, so I am assuming that this is true for the above outcomes as well. After consent is obtained from the parents, the child is then evaluated to determine whether or not an official diagnosis is appropriate. If the child is diagnosed, and they qualify for special education services, an individual education plan (IEP) is created and then implemented.

et al. (1982) suggest that an increase in human capital could reduce the probability of a learning disability diagnosis. Second, the findings from McLeod and Kaiser (2004) and Heckman et al. (2013) suggest that non-cognitive skill plays an important role in long-run outcomes. For these reasons, I focus on diagnoses that are arguably functions of ones cognitive and non-cognitive skill, as well as their genetic inputs (i.e., learning disability, ADD/ADHD, behavioral or conduct problems, and requiring the use of special therapy).

1.3 Empirical Strategy

Ordinary least squares (OLS) estimates of the effect of preschool participation on the probability of ever being diagnosed with certain intellectual and behavioral disorders as a child (hence, ignoring the endogeneity) would likely be biased because unobserved characteristics (e.g., parental ability) could potentially influence both preschool participation and diagnosis. Like the previous literature, I address this concern by exploiting the introduction of UPK in Oklahoma and Georgia (Cascio and Schanzenbach, 2013; Fitzpatrick, 2008, 2010). Specifically, I instrument for preschool participation using an indicator for UPK availability.

For UPK to be a valid instrument, it needs to affect the endogenous variable of interest (i.e., preschool participation) and not be correlated with the error term in the regression of diagnosis on preschool participation (i.e., it is excludable from the structural equation). The first condition is satisfied given the presence of a sufficiently strong first-stage, but the validity of the second condition cannot be directly proven. That is, we worry that the excludability condition will be violated if UPK affects the outcomes of interest either directly or through channels other than preschool participation. One specific concern is that this condition would be violated if Oklahoma or Georgia introduced a policy that affects child health around the same time that their UPK programs were introduced. In practice, this is not a concern because neither Oklahoma nor Georgia introduced a program that coincided with the introduction of UPK and affected the same cohorts of children.

The TS2SLS technique proceeds by estimating the first stage using data from the CPS October Supplement and then estimating the second stage using data from the NSCH. This technique was initially introduced in Angrist and Krueger (1992), and has since been used in several papers. Inoue and Solon (2010) show that in a two-sample setting instrumental variables (IV) and two-stage least squares (2SLS) are no longer numerically equivalent.[7] They point out that, in general, TS2SLS is preferred to two-sample instrumental variables (TSIV) because the former corrects for differences in the distribution of the instrument between the two samples. This correction results in the TS2SLS estimator being asymptotically more efficient than the TSIV estimator.

The TS2SLS first- and second-stage equations can be written as follows:

$$ECE_i = \gamma_0 + \gamma_1 UPK_i + \gamma_2 EDU_i^{\leq HS} + \gamma_3 UPK_i \times EDU_i^{\leq HS} + \gamma_4 X_i + \delta_s + \delta_{yob} + u_i \quad (1.1)$$

$$Y_i = \beta_0 + \beta_1 \widehat{ECE_i} + \beta_2 EDU_i^{\leq HS} + \beta_3 \widehat{ECE_i \times EDU_i^{\leq HS}} + \beta_4 X_i + \phi_s + \phi_{yob} + \epsilon_i \quad (1.2)$$

ECE_i is an indicator for preschool participation measured as enrolled in preschool, enrolled in public preschool, or enrolled in private preschool for individual i; UPK_i is a dummy variable that takes a value of one if UPK was available for individual i; $EDU_i^{\leq HS}$ is a dummy

[7] In this two sample setting $\hat{\beta}_{TSIV} = (\frac{Z_2' X_2}{n_2})^{-1}(\frac{Z_1' Y_1}{n_1})$, $\hat{\beta}_{TS2SLS} = (\frac{Z_2' X_2}{n_2})^{-1} C (\frac{Z_1' Y_1}{n_1})$, and $C = (\frac{Z_2' Z_2}{n_2})(\frac{Z_1' Z_1}{n_1})^{-1}$, where C is the correction for distributional differences across the two samples. Simply instrumenting for the endogenous variable of interest (i.e., not plugging in the fitted values from the first-stage regression) does not provide this correction.

variable that takes a value of one if the highest level of household education is a high school degree or less for individual i; X_i is a vector of individual-level covariates including race (i.e., black non-Hispanic and Hispanic), gender, and household size; and Y_i is an indicator for the diagnoses considered in this paper and is set to one if individual i has ever received that specific diagnosis between the age of four and seventeen, otherwise it is set to zero.[8]

State fixed effects, δ_s, are included to account for time-invariant differences in preschool enrollment and diagnosis probabilities across states. Year of birth fixed effects, δ_{yob}, are included to account for changes in enrollment, diagnosis, or grade retention probabilities that are associated with specific birth cohorts. Equation (1) is estimated and the predicted probabilities of preschool participation (i.e., the combination of public and private preschool participation), $\widehat{ECE_i}$, and the interaction of preschool participation and highest level of household education, $\widehat{ECE_i \times EDU_i^{\leq HS}}$, are plugged into equation (2) to estimate the effect of preschool participation on the diagnosis outcomes and grade retention. I then correct the standard errors to account for the variability introduced by using the predicted probabilities of ECE and $ECE \times EDU^{\leq HS}$ in equation (2), where these probabilities are calculated using estimates of the true parameters.[9]

1.4 Data

The data used in this paper comes from the National Survey of Children's Health and the October Supplement of the Current Population Survey. The NSCH is administered to a representative sample of approximately 2,000 households from each state, and a selected child from each household is the primary subject of the survey.[10] The National Center of Health Statistics at the Centers for Disease Control first conducted the NSCH telephone

[8]Household size is bottom-coded at two individuals and is top-coded at seven individuals for both the NSCH and the CPS October Supplement.

[9]Murphy and Topel (1985) explain how this should be done in a two-sample setting.

[10]The NSCH data is made available by the Child and Adolescent Health Measurement Initiative (CAHMI).

survey in 2003. Every four years, a new representative sample of households is identified and surveyed. Data from all three waves (i.e., 2003, 2007, and 2011) are used in this paper, resulting in approximately 290,000 individual-level observations.[11]

The CPS is a monthly household survey that is conducted by the United States Census Bureau for the Bureau of Labor Statistics. The October Supplement was first administered in 1968, and is designed to gather information on enrollment and educational attainment at the elementary, secondary, and postsecondary level. I use information on preschool participation among four year olds from 1990 through 2011, resulting in approximately 43,000 individual-level observations.

UPK programs in Oklahoma and Georgia are available to all children who are four years old, on or before September 1. For that reason, all observations where age is less than four are dropped from the analysis (i.e., approximately 20 percent of the NSCH observations). Observations that are missing information on age, gender, race, and highest level of household education are also dropped (i.e., approximately 5 percent of the remaining NSCH and CPS observations). After these exclusions, the final sample consists of 259,397 individual-level observations.[12]

I am unaware of a dataset that contains information on both preschool participation and diagnosis outcomes for children. However, in combination, the NSCH and the CPS October Supplement contain the information necessary to study the relationship between preschool participation and the probability of diagnosis. In other words, both datasets have the independent regressors and instrumental variable in common but the endogenous regressor (i.e.,

[11] In the NSCH, respondents are asked "Has a doctor, health professional, teacher, or school official ever identified or diagnosed the selected child with a learning disability?" With respect to the other outcomes, respondents are asked "Has a doctor or health professional ever diagnosed the selected child with [AD-D/ADHD, behavioral or conduct problems]" and "Does the selected child need or get special therapy, such as physical, occupational, or speech therapy?" Bussing et al. (2003) and Hoagwood et al. (2000) find that parental reports of children's health and health services tend to be consistent with physician reports.

[12] Based on the above conditions, I created a flag indicating whether or not an observation would be dropped. I then regressed that flag on the indicator for UPK availability in order to verify that the two are not correlated.

preschool participation) is unique to the CPS October Supplement while the outcomes of interest (i.e., ever diagnosed with or identified as having a learning disability, ADD/ADHD, behavioral or conduct problems, or requiring the use of special therapy) are unique to the NSCH.[13] For that reason, I use two-sample two-stage least squares to study the effect of preschool participation on the probability of ever being diagnosed with certain intellectual and behavioral disorders in childhood (i.e., between the ages of four and seventeen). When using a two-sample two-stage technique, you need variables that are common to both datasets. I, therefore, use the selected child's year of birth in place of year fixed effects, which accounts for variation in the outcomes that are specific to year-of-birth cohorts. Moreover, the need for variables that are identically measured in both datasets is why this analysis does not include state-specific time trends.

Regarding the instrument used in this paper (i.e., an indicator for the availability of UPK), I create it using the information available in both samples, and under the assumption that each child turned their reported age during the year associated with that observation. For example, if a child's reported age is four, and the year of the survey is 2003, then the child's year of birth is assumed to be 1999.[14] This indicator will, therefore, be measured with some error as neither the exact month that the NSCH occurred nor is the child's month of birth known. Using the months that the survey could have occurred, along with the potential months of birth for the selected child, a back-of-the-envelope calculation suggests that approximately one percent of the October CPS sample, and two percent of the NSCH sample, will be misclassified with respect to treatment status. To be specific, the only

[13]The NSCH provides some measures of preschool participation; however, they are not consistently coded across years. For example, in 2007 and 2011 respondents were asked "Does the child receive care for at least 10 hours per week from someone not related to him/her? This could be a daycare center, preschool, Head Start program, nanny, au pair, or any other non-relative." In 2003, however, the above non-relative care categories were broken out into separate questions.

[14]That is to say that the treatment group consists of Oklahoma and Georgia while the comparison group consists of all other states, including the District of Columbia. In section 5.3 I briefly discuss the results of a robustness check where the states that have UPK programs but are not statewide and/or fail to meet most of the quality benchmarks set by the National Institute for Early Education Research are excluded from the comparison group.

year of birth cohorts affected by this misclassification are the 1990 and 1991 groups. As a robustness check, I re-run the analysis dropping these year-of-birth groups from my sample. The estimates are briefly discussed in Section 5.3 and presented in Tables A.4 and A.5. These estimates are comparable to the main results, and suggest the same relationship between preschool participation and the probability of ever being diagnosed with certain intellectual and behavioral disorders in childhood.

Table 2.1 presents the summary statistics for the NSCH and the CPS October Supplement and demonstrates, with a few exceptions, the similarities between the two datasets. Specifically, the NSCH and the CPS October Supplement differ in how they collect information on the highest level of household education. The NSCH asks respondents for the highest level of household education, whereas the CPS October Supplement asks for the highest level of education for each individual in the household.[15] To have a common measure across the two datasets, and for all available years, I take the maximum level of education for each household in the October CPS, and I report that value. Even with this approach, however, the highest level of education still varies across the two datasets. As a robustness check, I use the same strategy as Cascio and Schanzenbach (2013), and I stratify my analysis by the highest level of maternal education. These results are discussed in Section 5.3 and presented in Tables A.6 and A.7 in the Appendix.

Lastly, Table A.1 in the Appendix presents counts for children served under the Individuals with Disabilities Education Act (IDEA) Part B in Oklahoma and Georgia during the 2005, 2007, and 2011 academic years. This table is meant to give the reader a sense of how many children in the treated states were served under IDEA Part B during the years represented in the NSCH data. This table does not include an exhaustive list of diagnoses. For example, I excluded child counts for hearing impairments, and autism from the table.

[15] After the 2003 wave, the NSCH specifically asks respondents for the mother and father's highest level of education.

1.5 Results

1.5.1 First-Stage

The empirical strategy used in this paper relies on variation in the location and timing of the introduction of UPK to identify the relationship between preschool participation and diagnosis. We can observe the variation in location, but I conduct several event studies to verify that the introduction of UPK is uncorrelated with trends in preschool participation before 1995 and 1998 in Georgia and Oklahoma, respectively. This verification is done by estimating the following regression:

$$ECE_{t,s} = \alpha_0 + \phi_s + \sum_{y=-5}^{-2} \beta_y D_s 1(t - T_s = y) + \sum_{y=0}^{5} \gamma_y D_s 1(t - T_s = y) + \epsilon_{t,s} \tag{1.3}$$

ϕ_s represents state fixed effects; D_s is equal to 1 if a state ever introduced UPK; t is the year associated with each observation; T_s is the year UPK was introduced; and y is the number of years before and after the introduction of UPK in state, s. I then plot the estimated coefficients from this regression (i.e., β_y and γ_y) against y where the year prior to adoption has been omitted (i.e., $y = -1$).[16]

Figure 1.1 is a plot of the event study estimates for enrolled in preschool (i.e., public and private preschool enrollment combined) and illustrates that, in Oklahoma and Georgia, there is an increase in the probability of preschool participation following the introduction of UPK. Figure 1.2 illustrates that public preschool participation increases following the introduction of UPK in Oklahoma and Georgia, while Figure 1.3 illustrates that the inverse is true for

[16] In order to have a balanced panel, I only include five years before and after adoption for the treated states (i.e., Oklahoma and Georgia).

private preschool participation.[17]

I repeat this exercise for the diagnosis outcomes to verify that the introduction of UPK is not related to pre-trends in the probability of diagnosis. Equation (3) is used in these event studies, but t now represents the year of birth associated with each observation, and T_s represents the year of birth associated with the introduction of UPK in Oklahoma and Georgia. Figures B1 through B5 (in the Online Appendix) present the estimates from these event studies, and suggest that there is little evidence of pre-trends in the probability of diagnosis. That is to say that these Figures indicate that the probability of diagnosis responds to the introduction of UPK.

Table 2.2 presents the first stage estimated effects of UPK on preschool participation (i.e., the combination of public and private preschool participation), which reaffirms the patterns observed in Figures 1 through 3. In order to use the two-sample technique leveraged in this paper, both samples must represent the same population. To satisfy this requirement, I use the sampling weights provided by the CPS October Supplement and the NSCH. I conduct the analysis both with and without the sampling weights, which allows me to verify that the estimates are qualitatively the same.[18]

Panel A of Table 2.2 presents the first-stage estimated effects of UPK on preschool participation. As expected, UPK availability increases the probability of preschool participation for children from low-education households by 19.4 percentage points. Relative to the baseline, this represents a 64.5 percent increase in the probability of participating in preschool. Among children from high-education households, we observe a 15.6 percentage point increase in the probability of preschool participation. Relative to the baseline, this represents a 34.7 percent increase in the probability of participating in preschool. Though not identical, these findings are notably consistent with those of Cascio and Schanzenbach (2013), which I discussed

[17]Event studies stratified by the highest level of household education can be found in the Appendix.
[18]The unweighted estimates are available upon request.

earlier.[19]

Panel B presents the first-stage estimated effects of UPK on public preschool participation. Consistent with the relationship illustrated in Figure 1.2, the response of preschool participation to the introduction of UPK is driven by an increase in public preschool participation. Specifically, for children from low-education households, we observe that UPK availability increases the probability of public preschool participation by 19.5 percentage points. Among children from high-education households, we observe a 21.5 percentage point increase in the probability of public preschool participation. These results are consistent with those reported by Cascio and Schanzenbach (2013).

Lastly, panel C presents the first-stage estimated effects of UPK on private preschool participation. Among children from low-education households, we observe that the effect of UPK availability on the probability of private preschool participation is not statistically different from zero. Consistent with the current literature, the first-stage results show that UPK crowds out private preschool participation among children from high-education households. Specifically, among children from high-education households, we observe that UPK availability reduces the probability of private preschool participation by 5.9 percentage points. It is important to note that, this shift from high-quality private to high-quality public preschool could potentially result in reduced human capital for children from high-education households.

The smaller overall effect of UPK on preschool participation for children from high-education households, presented in Panel A, is the result of UPK shifting enrollment from private preschool to public preschool. This observation is consistent with Cascio and Schanzenbach (2013) who find that UPK reduces the probability of private preschool enrollment by 8

[19]The variation between my first-stage estimates and those presented in Cascio and Schanzenbach (2013) is due to the following. First, we use different ranges of time. Second, I proxy for household SES using the highest level of household education, and they proxy for household SES using the highest level of maternal education. As a robustness check, I stratify the analysis by the highest level of maternal education. These estimates are reported in Tables A.6 and A.7 and briefly discussed in section 5.3.

percentage points for children from high-education households. When I stratify the sample by age groups (i.e., between the ages of four and thirteen, and thirteen and seventeen), the relationships I observe are consistent with those discussed above.

1.5.2 Two-Sample Two-Stage Least Squares

Table 1.3 presents the weighted estimates for the effect of preschool participation on the probability of ever being diagnosed with certain intellectual and behavioral disorders as a child (i.e., between the ages of four and seventeen), and ever repeating a grade after kindergarten. As with the first-stage estimates, the unweighted and weighted estimates are qualitatively the same.[20]

For the full sample of children from low-education households (column 2), the estimates suggest that preschool participation reduces the probability of ever being diagnosed with behavioral or conduct problems as a child and the probability of ever requiring the use of special therapy by 7.3 and 13.5 percentage points, respectively. The estimates also suggest a negative relationship between preschool participation and grade retention, but this estimate is not statistically different from zero. The suggested effect size, however, is comparable to what Fitzpatrick (2008) reports. Conversely, for the full sample of children from high-education households (column 5), I find that preschool participation increases the probability of ever being diagnosed with behavioral or conduct problems as a child and the probability of ever requiring the use of special therapy by 5.6 and 10.8 percentage points, respectively. Relative to the baseline values presented in Table 2.1, these estimates are large and, given the size of the standard errors, we cannot rule out smaller effect sizes.

The estimates for the probability of ever being diagnosed with ADD/ADHD, and a learning disability (reported in columns 2 and 5 of Table 1.3) suggest the same relationship between

[20] The unweighted estimates are available upon request.

preschool participation and probability diagnosis as those discussed above. However, these estimates are not precisely estimated, and therefore, we cannot conclude that they are statistically different from zero.

All of the estimates discussed thus far are for children between the ages of four and seventeen, which provides no insight as to when these observed effects are occurring. To identify which age group is driving these results, I separate the sample into two groups: children between the ages of *4 and 13*, and children between the ages of *14 and 17*. Ideally, the cutoff for these two groups would occur at the beginning of adolescence (i.e., 13 years of age); however, doing so would result in no pre-treatment periods in Georgia for the younger group of children. I, therefore, include an additional year (i.e., 13 year olds) in the younger group in order to have pre-treatment periods for both Oklahoma and Georgia.[21]

The results presented in Table 1.3 suggest that, for children from low-education households and between the ages of *4 and 13* (column 3), preschool participation reduces the probability of ever being diagnosed with behavioral or conduct problems as a child by 11.7 percentage points. Conversely, for the same group of children from high-education households (column 6), preschool participation increases the probability of ever being diagnosed with ADD/ADHD as a child by 18.9 percentage points.

Table 1.3 column 4 presents the estimates for children from low-education households between the ages of *14 and 17* and suggests that preschool participation reduces the probability of ever being diagnosed with behavioral or conduct problems, and requiring the use of special therapy as a child by 12.9 and 17.2 percentage points, respectively. In contrast, for children from high-education households (column 7), I find that preschool participation increases the probability of ever being diagnosed with behavioral or conduct problems as a child by 19.5

[21] The number of observations for these age-specific groups will not sum to the corresponding observation count listed in column 1 of Table 2 because the year of birth for these groups overlaps in the CPS data. For example, the year of birth associated with the *4 and 13* group includes 1990 through 2007, while the year of birth associated with the *14 and 17* group includes 1986 through 1997.

percentage points. These medium-term effects are consistent with Vandell et al. (2010), who find that high-quality early childhood care is associated with an increase in cognitive skill, and a decrease in externalizing behavior at age 15.

The results presented in Table 1.3 (columns 3, 4, 6, and 7) suggest two things. First, children from low-education households between the ages of *4 and 13* are driving the human capital response for the probability of ever being diagnosed with behavioral or conduct problems as a child.[22] Second, the estimated effect for children from high-education households is driven by children between the ages of *14 and 17*. It is important to keep in mind that relative to the baseline values presented in Table 2.1 these point estimates are large and, given the size of the standard errors, we cannot rule out smaller effect sizes.

1.5.3 Robustness Check

This paper focuses on the UPK programs introduced in Georgia and Oklahoma because these programs are high-quality and universally available to all eligible children in those states. However, other states (i.e., Alabama, West Virginia, Florida, the District of Columbia and New York) introduced similar programs during the period considered in this paper. Unlike Georgia and Oklahoma, these programs are not statewide, or they fail to meet a majority of the quality benchmarks set by the National Institute for Early Education Research.

To check the robustness of my results to an alternative comparison group, I repeat the above analysis with a control group that excludes Alabama, West Virginia, Florida, the District of Columbia and New York. These estimates are presented in Tables A.2 and A.3 of the Appendix and they are comparable to the main results. In other words, both sets of estimates

[22] Recall that the outcomes of interest are defined as ever having been diagnosed with one of these outcomes as a child. For that reason, the point estimates for the *4 and 13* age group, if precisely estimated, should provide a lower bound for the effect of preschool participation on the outcomes of interest. In other words, if we observe a negative effect that is statistically different from zero for the *4 and 13* age group, then the estimated effect for the *14 and 17* age group should remain reasonably unchanged or approach zero.

are suggestive of the same relationship between preschool participation and the probability of diagnosis.

In addition to the above, I conduct two other robustness checks. Recall that, because of my year-of-birth assumptions, individuals belonging to year-of-birth groups 1990 and 1991 potentially have a misclassified treatment status. Therefore, I exclude these groups, and I determine that (with a couple of exceptions) my results are not sensitive to this exclusion. In the final robustness check, I stratify my analysis by the highest level of maternal education, and I find that these estimates are comparable to my main results. The results from these robustness checks are presented in Tables A.4, A.5, A.6, and A.7 of the Appendix.

1.6 Conclusion

This paper studies the effect of participating in high-quality preschool on the probability of being diagnosed with certain intellectual and behavioral disorders as a child (i.e., between the ages of four and seventeen). As I mentioned earlier, we can think of these diagnosis outcomes as proxies for cognitive and non-cognitive skill. By looking at the effect of preschool participation on parent-reported diagnosis outcomes, this paper provides valuable complementary evidence to the existing literature.

That said, my analysis yields two main findings. First, for children from low-education households I observe that preschool participation reduces the probability of ever being diagnosed with behavioral or conduct problems, and requiring the use of special therapy as a child. This finding is consistent with Votruba-Drzal et al. (2004), and Vandell et al. (2010). Votruba-Drzal et al. (2004) observe that high-quality care is associated with a reduction in behavioral problems for low-income children. Similarly, Vandell et al. (2010) find that high-quality care is associated with a decrease in externalizing behavior at age 15. Second, for children from high-education households I observe that preschool participation increases the probability of ever being diagnosed with behavioral or conduct problems, and requiring the use of special therapy as a child. This finding is consistent with Cascio and Schanzenbach (2013), and Cascio and Schanzenbach (2014), who discuss how shifting from private to high-quality public preschool could result in small, or even negative, effects to human capital for children from high-education households.

An obvious explanation for this pattern of effects is that UPK provides an environment that is of higher quality than the counterfactual for low-education households, but of lower quality than the counterfactual for high-education households. The quality of these programs can potentially be attributed to the following characteristics: the number of NIEER benchmarks met by the program (i.e., overall program quality), the positive and negative spillovers produced by one's peers (i.e., peer effects), or some combination of the two.

Recent work by Cascio (2017) studies the potential mechanisms driving the short-term cognitive effects of UPK. Consistent with the previous literature, she finds that UPK programs have a positive effect on the reading scores of low-SES four-year-old children. However, targeted preschool programs (i.e., programs targeted towards children from low-education households) do not provide the same effects. Further, Cascio (2017) determines that this difference is not driven by variation in program standards or population characteristics. Instead, the potential mechanism driving this result is the high-quality characteristics of UPK programs that are not captured by traditional measures of quality. That said, and as the author points out, this finding is specific to short-term cognitive outcomes and cannot be generalized to non-cognitive and long-term outcomes.

With that in mind, the analysis presented in this paper points to the following conclusion. High-quality preschool disproportionally benefits children from low-education households by reducing their probability of ever being diagnosed with behavioral or conduct problems, and requiring the use of special therapy as a child. To the extent that an increase in human capital is driving this negative relationship, this reduced probability of diagnosis can have important implications with respect to the academic attainment, criminal activity, and employment outcomes of children from low-education households (Schweinhart, 2003; Reynolds et al., 2002; Heckman et al., 2013).

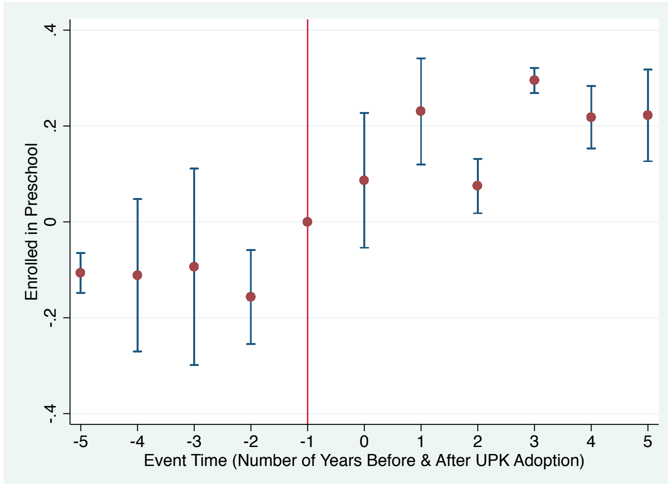
1.7 Tables & Figures

Table 1.1: Summary Statistics - Overall & Baseline

	Overall		Baseline (NSCH - OK & GA)	
	NSCH	Oct. CPS	HS or Less	More than H.S.
Year of Birth	1996.5	1996.5	–	–
	(6.347)	(6.347)	–	–
High School or Less	.280	.353	–	–
	(0.085)	(0.127)	–	–
More than High School	0.720	0.647	–	–
	(0.085)	(0.127)	–	–
Black Non-Hispanic	0.134	0.139	–	–
	(0.148)	(0.155)	–	–
White Non-Hispanic	0.666	0.674	–	–
	(0.191)	(0.201)	–	–
Hispanic	0.099	0.115	–	–
	(0.112)	(0.137)	–	–
Household Size	3.336	3.289	–	–
	(0.187)	(0.292)	–	–
Preschool	–	0.559	0.301	0.449
	–	(0.136)	(0.119)	(0.119)
Public Preschool	–	0.281	0.242	0.118
	–	(0.129)	(0.105)	(0.101)
Private Preschool	–	0.278	0.059	0.332
	–	0.114	(0.063)	(0.087)
Learning Disability	0.107	–	0.185	0.115
	(0.044)	–	(0.082)	(0.054)
ADD/ADHD	0.095	–	0.104	0.127
	(0.043)	–	(0.061)	(0.053)
Behavioral or Conduct Problems	0.051	–	0.082	0.061
	(0.029)	–	(0.059)	(0.032)
Special Therapy	0.077	–	0.054	0.036
	(0.043)	–	(0.074)	(0.024)
Grade Retention	0.097	–	0.246	0.103
	(0.064)	–	(0.070)	(0.035)
Number of Observations	216,755	42,642	683	1,668

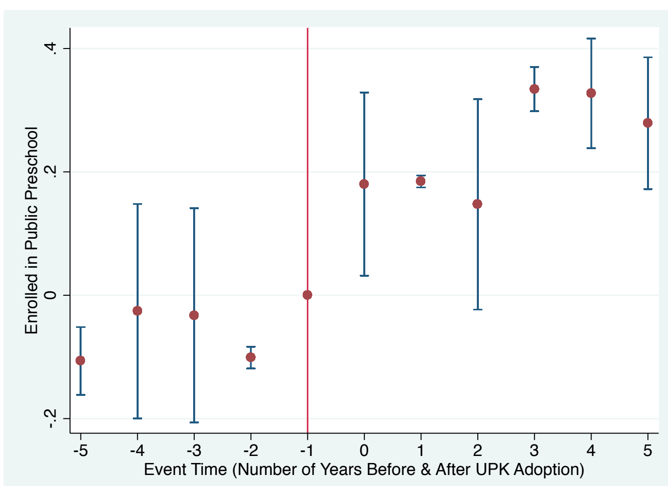
Notes: Standard deviations are reported in parenthesis. *High School or Less*, and *More than High School* are indicators for the highest level of education in the household. The baseline values represent the weighted averages for the treated states (i.e., Oklahoma and Georgia) before the introduction of their UPK programs (i.e., year of birth less than 1991 or 1994 for GA and OK, respectively). Sampling weights were used for both the NSCH and CPS data so that the summary statistics are representative of the population. Data sources: Centers for Disease Control and Prevention, National Center for Health Statistics, State and Local Area Integrated Telephone Survey, National Survey of Children's Health, 2003, 2007, and 2011. The October Supplement of the Current Population Survey.

Figure 1.1: The Effect of UPK on Enrollment in Preschool



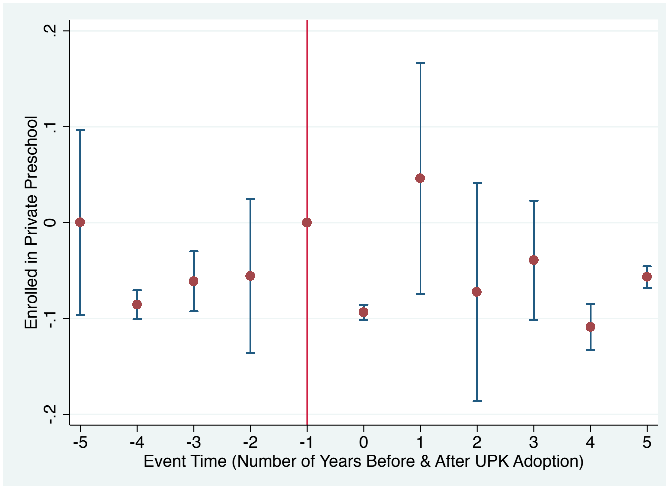
Notes: The dependent variable is preschool participation (i.e., ECE) and the coefficients are weighted estimates of β_y and γ_y from equation (3). Event time represents the number of years before and after the adoption of UPK in Oklahoma and Georgia (e.g., event time equal to zero is 1995 in Georgia and 1998 in Oklahoma). The year prior to the adoption of UPK is omitted (i.e., event time equal to -1). Data Source: The October Supplement of the Current Population Survey.

Figure 1.2: The Effect of UPK on Enrollment in Public Preschool



Notes: The dependent variable is public preschool participation (i.e., ECE) and the coefficients are weighted estimates of β_y and γ_y from equation (3). Event time represents the number of years before and after the adoption of UPK in Oklahoma and Georgia (e.g., event time equal to zero is 1995 in Georgia and 1998 in Oklahoma). The year prior to the adoption of UPK is omitted (i.e., event time equal to -1). Data Source: The October Supplement of the Current Population Survey.

Figure 1.3: The Effect of UPK on Enrollment in Private Preschool



Notes: The dependent variable is private preschool participation (i.e., ECE) and the coefficients are weighted estimates of β_y and γ_y from equation (3). Event time represents the number of years before and after the adoption of UPK in Oklahoma and Georgia (e.g., event time equal to zero is 1995 in Georgia and 1998 in Oklahoma). The year prior to the adoption of UPK is omitted (i.e., event time equal to -1). Data Source: The October Supplement of the Current Population Survey.

Table 1.2: First Stage Estimated Effects of UPK on Preschool Enrollment

	Pooled	H.S. or Less			More than H.S.		
	Full Sample (N = 42,462) (1)	*Full Sample (N = 42,462) (2)*	*Age 4-13 (N = 33,585) (3)*	*Age 14-17 (N = 23,883) (4)*	*Full Sample (N = 42,462) (5)*	*Age 4-13 (N = 33,585) (6)*	*Age 14-17 (N = 23,883) (7)*
A: Enrolled in Preshool							
Universal Preschool	0.172***	0.194***	0.147***	0.232***	0.156***	0.117***	0.149***
	(0.012)	(0.016)	(0.029)	(0.015)	(0.010)	(0.017)	(0.016)
B: Enrolled in Public Preschool							
Universal Preschool	0.206***	0.195***	0.176***	0.204***	0.215***	0.208***	0.212***
	(0.024)	(0.016)	(0.034)	(0.015)	(0.028)	(0.044)	(0.012)
C: Enrolled in Private Preschool							
Universal Preschool	-0.034	0.000	-0.029*	0.028	-0.059*	-0.091***	-0.063***
	(0.029)	(0.015)	(0.017)	(0.020)	(0.034)	(0.036)	(0.023)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains first-stage estimates by highest level of household education using both the full sample and the sample when it has been stratified by age. Each cell contains the results from a single regression predicting the effect of UPK availability on preschool participation. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included. Standard errors are clustered on state and are reported in parenthesis. The above specification uses the sampling weights provided by the CPS. Data Source: The October Supplement of the Current Population Survey.

Table 1.3: Effect of Preschool Participation on Diagnosis Probabilities & Grade Retention

	Pooled	H.S. or Less			More than H.S.		
	Full Sample (1)	*Full Sample* (2)	*Age 4-13* (3)	*Age 14-17* (4)	*Full Sample* (5)	*Age 4-13* (6)	*Age 14-17* (7)
Learning Disability	-0.019	-0.054	-0.053	-0.043	0.032	0.082	0.015
N = 258,767	(0.043)	(0.040)	(0.044)	(0.081)	(0.051)	(0.075)	(0.038)
ADD/ADHD	-0.006	-0.036	0.026	-0.048	0.032	0.189**	-0.054
N = 258,685	(0.026)	(0.044)	(0.060)	(0.053)	(0.035)	(0.082)	(0.043)
Behavioral Problems	-0.016	-0.073***	-0.117***	-0.129***	0.056*	-0.068	0.195***
N = 258,966	(0.013)	(0.023)	(0.043)	(0.044)	(0.029)	(0.074)	(0.053)
Special Therapy	-0.022	-0.135*	-0.200	-0.172***	0.108***	0.114	-0.009
N = 259,087	(0.052)	(0.074)	(0.131)	(0.040)	(0.038)	(0.090)	(0.046)
Grade Retention	-0.007	-0.043	-0.019	-0.089	0.047	0.016	0.030
N = 226,535	(0.030)	(0.040)	(0.111)	(0.060)	(0.060)	(0.150)	(0.074)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. When the full sample is used, each entry contains the results from a single regression predicting the effect of preschool participation on ever being diagnosed with the conditions listed when the child was between the ages of 4 and 17, or 6 and 17 for ever repeated a grade. When the sample is stratified by age group, each cell represents the results from a single regression predicting the effect of preschool participation on the probability of being diagnosed when the child was between the ages of 4 and 13, or 14 and 17. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included. Standard errors are clustered on state and are reported in parenthesis. The above specification uses sampling weights provided by the CPS and the NSCH. The first stage used is *Enrolled in Preschool* (i.e., the combination of public and private preschool enrollment). Data sources: Centers for Disease Control and Prevention, National Center for Health Statistics, State and Local Area Integrated Telephone Survey, National Survey of Children's Health, 2003, 2007, and 2011. The October Supplement of the Current Population Survey.

Chapter 2

The Effect of Career Technical Education Participation in High School

2.1 Introduction

Currently, in the United States there is a debate regarding the role secondary career technical education programs should play in our educational system. Proponents argue that these programs increase student motivation and, therefore, increase the probability of high school completion, college attendance, and employment (Tulenko, 2016). In contrast, opponents are concerned that these programs simply serve as a dumping ground for underperforming, and troubled students (Tulenko, 2016). While previous literature has studied the effect of career technical education participation on human capital accumulation, there is limited causal evidence regarding this relationship. At the start of the 2013 academic year, Kentucky introduced a policy that reduced the per-credit-hour cost of career technical education courses for high school students.[1] I exploit this policy change to study the effect of participation in career technical education on human capital accumulation using several high school, post-secondary, and labor market outcomes.

When studying the effect of participation in career technical education, the primary challenge is that participation in these programs is nonrandom. The previous literature attempts to address this problem using a variety of empirical strategies, and finds suggestive evidence that career technical education is associated with an increase in human capital accumulation. Specifically, the literature has observed that participation in career technical education is associated with an increased probability of graduating from high school, earning program certifications, being employed, and higher earnings (Cellini, 2006; Bishop and Mane, 2004; Arum and Shavit, 1995; Hanushek et al., 2011; Dougherty, 2016; Kemple and Willner, 2008). That said, the literature has produced conflicting results with respect to the effect of career technical education participation on the probability of acquiring post-secondary education (Cellini, 2006; Neumark and Rothstein, 2006).

Given the above, my contributions to this literature are twofold. First, in order to study the effect of participation in career technical education on the accumulation of human capital, I exploit the arguably random variation in participation that resulted from the 2013 policy change in Kentucky. In other words, my main contribution to this literature is that my identification strategy should allow for causal inference. Second, to the best of my knowledge, I am the first to use the linked secondary transcript and workforce data from the Kentucky Center for Education and Workforce Statistics (KCEWS) to evaluate the effect of participation in career technical education during high school.[2] To the best of my knowledge, the

[1] High school students in Kentucky can access career technical education programs through enrollment in dual credit courses, which allow students to earn college credit while in high school. Career technical education courses are classified as elective coursework, and as such, they are not considered to be part of a high school student's core curriculum (i.e., english, math, science, and social studies). I included a sample course load in the Appendix.

[2] That said, Jepsen et al. (2014) use data from the Kentucky Community and Technical College System (KCTCS) giving them access to labor market and post-secondary transcript data for students in Kentucky's community college system.

only other papers to use similar data are Jepsen et al. (2014) and Dougherty (2016).

The career technical education policy change in Kentucky had two objectives. First, policymakers wanted to increase course standards to ensure that course credits would be reflected on students' post-secondary transcripts. The inclusion of these credits on post-secondary transcripts reduces the time required to attain career technical education credentials and, therefore, reduces the fixed cost associated with acquiring those credentials. Second, policymakers wanted to reduce the per-credit-hour cost in order to increase the program's accessibility among high school students.

A common concern among policymakers and researchers is that the cost of acquiring additional education is prohibitive for some students. From a policy perspective, this represents a potential inefficiency that could be addressed by reducing the cost of acquiring additional education. While there are several costs associated with career technical education, this paper focuses on the per-credit-hour cost. Following the 2013 policy change, the reduced cost of participation in career technical education varied by school district and freshmen-year cohort.[3] I exploit this variation by instrumenting for participation using the average level reduction in per-credit-hour cost.[4] I find that reducing the per-credit-hour cost by 140 dollars increases participation by nearly two courses. In addition, I find that participation in career technical education increases the probability of completing high school, earning a KOSSA certificate, and attending some college. I also observe a positive but imprecisely estimated effect for the relationship between participation and the probability of being employed, and annual earnings. I do not observe a statistically significant effect for the probability of earning a program certificate.

[3]Students can access dual credit career technical education courses at one of the following institutions: (i) a Kentucky Community and Technical College, (ii) an Area Technical Center (i.e., a technical high school) taught by a secondary instructor, or (ii) an Area Technical Center taught by a post-secondary instructor. Subsidy amounts vary across dual credit career technical education institution type, and the availability of institution type varies across school districts. Exposure to these subsidies also varies by freshmen-year cohort (e.g., the 2010 cohort was only exposed to these subsidies during their senior year of high school).

[4]In Section 4 I discuss the validity of using the reduction in per-credit-hour cost as an instrument for participation in career technical education.

Given the well-documented gender differences in high school graduation, college attendance, and labor market outcomes (Heckman and LaFontaine, 2010; Goldin et al., 2006; Altonji and Blank, 1999), it is reasonable to expect the 2013 policy change to have varying effects for male and female students. Specifically, I suspect that the policy will have a positive effect on the outcomes of interest for both male and female students, but the effect size might be larger for one group of students relative to the other. For that reason, in addition to the pooled estimates, I also present separate estimates for male and female students. I find that the positive effects for career technical education participation (on both the extensive and intensive margin), high school graduation, and KOSSA certification are equivalent for male and female students. However, the increases in college attendance, attending a 2-year institution, and attending a 4-year institution are larger for female students. The workforce data is aggregated by district and freshmen-year cohort, so for those outcomes, I am unable to present separate estimates for men and women.[5]

2.2 Review of the Literature

When examining the effect of participation in career technical education our primary concern is addressing the problem of endogenous selection into program participation. As the previous literature demonstrates, this is a non-trivial task. Arum and Shavit (1995) and Bishop and Mane (2004) attempt to address the selection problem by controlling for ability (via proxy), as well as family-, community-, and individual-level characteristics. While this strategy is straightforward to implement, it is unlikely to account for all of the unobserved characteristics driving selection into career technical education. Nevertheless, they find that certain career technical education concentrations (i.e., course sequences that focus on specific fields such as Information Technology, Finance, Agriculture, Manufacturing, etc.) are associated with an increased probability of employment and improved earnings.

[5] At the time of this analysis, KCEWS could not legally provide individual-level workforce data.

Neumark and Rothstein (2006) improve upon the above strategy by including school fixed effects (FE) as well as individual-, and school-level characteristics. Using this strategy, they find that participation in career technical education is associated with a reduced probability of acquiring some post-secondary education.[6] The strength of their approach is that it accounts for time-invariant school-level characteristics that may influence career technical education participation. However, their empirical strategy cannot account for unobserved individual-level characteristics that may vary within schools.

Hanushek et al. (2011) attempt to address the selection problem by using a difference-in-differences (DD) strategy, coupled with propensity score matching, and controling for individual measures of ability, family background, and country-specific characteristics. The shortcoming of this strategy, however, is that while the DD approach nets out the time-invariant characteristics, the time-varying characteristics will remain. Moreover, propensity score matching relies on observed characteristics in order to construct the propensity scores. Therefore, the concern with propensity score matching is that the quality of the match depends on the observed characteristics used to construct the score. That said they, observe that individuals who complete vocational education are more likely to be employed when they are young. However, they find that this employment advantage erodes with age. Moreover, when the authors consider only countries without vocational programs per the OECD's official definition (this includes the United States), they find no evidence that the probability of employment varies by education type (i.e., vocational versus general education).

Cellini (2006) includes family fixed effects to account for unobserved time-invariant characteristics within a family. This strategy, however, does not account for unobserved individual-level characteristics that vary within a family. Nevertheless, using this strategy the author finds that participation in career technical education is associated with an increased probability of graduating from high school and attending a two-year college, but a reduced probability

[6]This is consistent with Oosterbeek and Webbink (2007) who observe zero returns to an additional year of vocational education in Denmark.

of attending a four-year college.

Arguably, the most convincing strategies for addressing the problem of endogenous selection into career technical education can be found in Dougherty (2016), and Kemple and Willner (2008). In Massachusetts, oversubscribed technical schools use a ranking system to admit students. Dougherty (2016) exploits this admissions scheme to evaluate the effect of career technical education in Massachusetts. Specifically, the author uses ordinary least squares with fixed effects, and a regression discontinuity framework to study the effect of participation in career technical education on the probability of on-time graduation from high school, and earning an industry recognized certification. In other words, the author uses one approach that provides more externally valid estimates, and another that provides more internally valid estimates. He finds thats participation in career technical education is associated with an increased probability of graduating from high school, and earning an industry recognized certification.

Lastly, Kemple and Willner (2008) use data from a fifteen year study of Career Academies, where applicants were randomly assigned to either the Academy group or non-Academy group. The study consisted of nine urban high schools, and participants were followed for eight years after high school completion. Using this data, the authors find that participation in the Academy group results in increased earnings of 11 percent per year, where this effect is largest for young men. With respect to the probability of acquiring some post-secondary education, the authors do not find an effect for the Academy group.

Given the above, I contribute to this literature by exploiting the arguably exogenous variation in career technical education participation, induced by Kentucky's policy change, to estimate the effect of participation in career technical education on the accumulation of human capital. That is, I instrument for participation in career technical education using the average level reduction in the per-credit-hour cost of career technical education courses. In section 2, I explain why the instrumental variable assumptions hold in this setting allowing for a causal interpretation of my results.

2.3 The Evolution of Career Technical Education Policy in Kentucky

In Kentucky, career technical education programs are designed to provide students with the academic and industry-relevant skills to successfully transition from school to work or to pursue post-secondary education. Before the 2013 academic year, many of the career technical education courses available to high school students did not count towards post-secondary credits, and the cost of participation varied by institution (Zinth, 2013a). As a result, during the 2013 academic year, Kentucky introduced a policy that accomplished the following. First, policymakers increased career technical education course standards so that they aligned with those of post-secondary courses. In addition, they wanted to ensure that the skills being taught in career technical education programs complemented the needs of Kentucky businesses. Second, policymakers structured the per-credit-hour cost of career technical education based on where and by whom the course is taught. In doing so, they reduced the per-credit-hour cost of career technical education, and thus, increased the accessibility of the program for many high school students.

The new cost structure took the following form: (i) tuition for career technical education courses taught at a Kentucky Community and Technical College System (KCTCS) campus are 100% subsidized (provided the course is supported by Support Education Excellence in Kentucky (SEEK) funding); (ii) tuition for career technical education courses taught by a college faculty member at an Area Technical Center (ATC) are 50% subsidized (provided the college absorbs the instructional costs); (iii) tuition for career technical education courses taught by a secondary instructor at an ATC are 100% subsidized (provided the high school covers the instructional costs).[7]

The following example provides a comparison of the old and new cost structures for career technical education courses in Kentucky. Before the 2013 policy change, the per-credit-hour cost of a career technical education course was approximately \$135. In other words, for a three-credit-hour course, a student's financial responsibility was approximately \$455 plus the \$50 administrative fee. After the policy change, a student's financial responsibility for the same three-credit-hour course could range from \$50 (i.e., the administrative fee) to \$455 plus the \$50 administrative fee.

2.4 Empirical Strategy

The empirical strategy used in this paper begins by using ordinary least squares to estimate the effect of participation in career technical education on the outcomes of interest. Specifically, I estimate the following equation, which describes the relationship between the outcomes of interest and participation in career technical education:

$$Y_{ijk} = \beta_0 + \beta_1 CTE_{ijk} + X_i\beta_2 + COHORT_j + DISTRICT_k + \epsilon_{ijk} \tag{2.1}$$

Y is a vector of high school, post-secondary and labor market outcomes; CTE is either an indicator for career technical education participation, or the number of career technical education courses individual i completed while in high school; X is a vector of individual-level characteristics including race, gender, ever enrolled in special education, and age; $COHORT$ and $DISTRICT$ are dummy variables for freshmen-year cohort and district; and β_1 is the

[7] Approximately 15 percent of dual credit career technical education courses are covered by SEEK funds (Zinth, 2013b).

effect of a one course increase on the outcomes of interest.[8] Controlling for district and freshmen year cohort will account for time-invariant differences in participation, and the outcomes of interest, that are district- and cohort-specific.

This strategy, however, ignores the endogenous selection into career technical education, and therefore, will produce estimates for β_1 that are likely biased. That is to say that, this strategy is unable to account for the unobserved characteristics (e.g., student ability, motivation, etc.) that are likely correlated with both participation in career technical education as well as the high school, post-secondary, and labor market outcomes considered in this paper. For example, students who plan to transition to the labor market immediately after high school may have a higher probability of career technical education participation, but a lower probability of high school graduation and college attendance (i.e., estimates for the effect of participation in career technical education on high school graduation would be biased downward). For that reason, I define a second equation which describes the relationship between the change in the per-credit-hour cost of career technical education courses and participation while in high school. Specifically, I define the following first-stage equation:

$$CTE_{ijk} = \gamma_0 + \gamma_1 COST_{ijk} + X_i\gamma_2 + COHORT_j + DISTRICT_k + v_{ijk} \tag{2.2}$$

$COST$ measures the average level reduction in the per-credit-hour cost of career technical education courses; and the other variables have the same definitions as equation (1). Using equations (1) and (2), I estimate the effect of participation on the outcomes of interest using two-stage least squares, where I instrument for participation using the average level change in per-credit-hour cost.

[8] When I evaluate the effect of participation in career technical education on labor market outcomes the data are aggregated by district, freshmen-year cohort, and academic year. Kentucky provided me with individual level secondary and post-secondary transcript data; however, they could not legally provide me with individual level workforce data.

Recall that the level reduction in per-credit-hour cost varies by school district and freshmen-year cohort. The district level variation occurs because the availability of career technical education institution types varies across districts (this is illustrated in Figures 2.1 and 2.2).[9] Having identified the institution types available in each district, I calculate the average level reduction in the per-credit-hour cost for each student.[10] As Figure 2.3 illustrates, the average reduction in cost decreases (i.e., becomes more negative) with each freshmen year cohort.

2.4.1 Identifying Assumptions

Under the following conditions, the average level reduction in per-credit-hour cost is a valid instrument for participation in career technical education. First, the instrument affects the endogenous variable of interest. Second, in the regression of the outcomes of interest on the endogenous variable of interest, the instrument is not correlated with the error term (i.e., the instrument is excludable from the structural equation). We know that the first condition is satisfied given the presence of a sufficiently strong first stage. The second condition, however, can not be directly proven in this setting.

That being said, the second condition will be violated if the instrument affects the outcomes of interest either directly or through other channels. For example, if students are exposed to other programs that have confounding effects on the outcomes of interest, or if the instrument is endogenous, then this condition is violated. I am unable to identify any programs that could have confounding effects on the outcomes of interest (e.g., an apprenticeship program introduced during the 2013 academic year). The exogeneity of the instrument, however, requires further discussion.

Recall that the level reduction in the per-credit-hour cost of career technical education

[9] Recall that the career technical education subsidies vary by career technical education institution types.

[10] Recall that, in Kentucky, career technical education institution types vary across school districts. To calculate the average level reduction in per-credit-hour cost, I calculate the average per-credit-hour cost before and after the policy change. The average level reduction is the difference between those two values.

courses is determined by school district and freshmen-year cohort. The instrument would, therefore, be endogenous if individuals could select into a freshmen year cohort, or school district, in order to take advantage of Kentucky's policy change. While freshmen year cohort assignment is exogenous, some may argue that school district is not. For example, school district would be endogenous if parents moved school districts in response to the reduced per-credit-hour cost. Recall that the maximum reduction in per-credit-hour cost is approximately 130 dollars. This behavior, therefore, seems unlikely since the cost of moving (i.e., buying and selling a home, the time associated with moving, the potential change in commute length, etc.) would likely exceed the benefit of moving (i.e., the reduced per-credit-hour cost).

2.5 KCEWS Linked Transcript Data

I use linked transcript (i.e., secondary and post-secondary) and workforce data for academic years 2009 through 2015 from the Kentucky Department of Education and Workforce Statistics (KCEWS).[11] [12] A shortcoming of this dataset is that for an individual to appear in the post-secondary transcript data they must have attended post-secondary school in Kentucky. Similarly, in order to appear in the workforce data, an individual must participate in Kentucky's labor market.

To match Kentucky's definition of high school completion, I created a measure for high school graduation that captures students who complete high school within four years. Individuals who took (and passed) the general educational development (GED) test are not defined as having completed high school. Students who enrolled in either a two- or four-year institution immediately after high school are identified as college attendees (i.e., the enrollment decision

[11]KCEWS provided me with separate transcript, credential, and certificate files. Before merging the files, I dropped duplicate transcript entries, transcript entries for grade levels less than nine, observations where age is coded incorrectly, and observations with multiple high school graduations. Additionally, I drop individuals who die or move out of state before completing high school.

[12]For my analysis, I collapse the data to a pooled cross-section of Kentucky secondary students.

was made during the final year of high school). I also create an alternative measure of college attendance, one that identifies students who enrolled in a two- or four-year institution in the years following high school. While the alternative measure of college attendance is not included in the main analysis, the results for this outcome are included in Table B.1 of the Appendix.

Kentucky offers career technical education courses in the following concentrations: agriculture, business & marketing, engineering & technology, family & consumer sciences, health sciences, information technology, transportation, manufacturing technology, media arts, and construction technology. For example, students interested in construction technology can take courses in air conditioning technology, building and apartment management, carpentry, electrical technology, heavy equipment sciences, masonry, and plumbing. A sample course load for the air conditioning technology program is included in Figure B.1 of the Appendix.

The certifications considered in this paper include the Kentucky Occupational Skills Standard Assessment (KOSSA), and career technical education program (or industry) certificates. KOSSA certificates provide a measure of skill attainment for career technical education careers that do not offer an industry certificate, or for which the offered certificate is not industry recognized. Students are eligible to take the KOSSA if they have completed at least two courses in a career technical education preparatory program, and are currently enrolled in the third course of the program. Program certificates, like KOSSA certificates, provide a measure of skill attainment that is industry recognized. For example, every program offered within the construction technology concentration has a nationally recognized certification. That is, students that complete the high school course load for air conditioning technology can take the National Center for Construction Education Research (NCCER) Level 1 certification. The NCCER Level 1 certificate enables students to work as a heating and air-conditioning systems (HVAC) assistant after high school.

2.6 Results

2.6.1 The Effect of Reduced Cost on Participation

In this section, I discuss the first-stage estimated effects of the policy change on the extensive and intensive margin of participation in career technical education during high school. Table 2.2, columns 1a and 1b, present estimates from the model specification that includes dummy variables for freshmen-year cohort and district. Columns 2a and 2b present estimates from the preferred model specification, which includes dummy variables for freshmen year cohort, and district, as well as controls for race, gender, ever enrolled in special education and age. Columns 3a, 3b, 4a, and 4b present separate estimates for men and women.

Column 2a of Table 2.2 presents the estimated effect of a reduction in the per-credit-hour cost of participation in career technical education during high school. This estimate indicates that reducing the per-credit-hour cost by 140 dollars increases the probability of participation by 7 percentage points. Relative to the baseline value, this effect represents an 8 percent increase in participation. This result implies that before the policy change the cost of career technical education was detering some students from participating in the program.

The estimate for effect of a reduction in the per-credit-hour cost of career technical education courses on the number of career technical education courses completed during high school is presented in column 2b. This estimate suggests that reducing the per-credit-hour cost by 140 dollars increases participation by approximately 2 courses, which represents an increase of 35 percent. For context, Kentucky secondary students take approximately eight courses per academic year, and on average three career technical education courses are needed to complete a certification.

Using the preferred model specification, columns 3a and 4a present the estimated effects of a reduction in the per-credit-hour cost of career technical education courses on participation for the gender-stratified sample. These estimates indicate that reducing the per-credit-hour cost by 140 dollars increases the probability of participation by 4 percentage points for female students and 8 percentage points for male students. Similarly, columns 3b and 4b indicate that reducing the per-credit-hour cost by 140 dollars increases participation by approximately two courses for both male and female students. These estimates imply that, on both the extensive and intensive margin, the effect of reducing the cost of participation is approximately equivalent for male and female high school students.

2.6.2 The Effect of Participation on High School Outcomes

In this section, I present the estimated effects of participation in career technical education on the high school outcomes of interest. Table 2.3 presents the 2SLS estimates of the effect of participation in career technical education (measured by the total number of career technical education courses completed in high school) on the probability of completing high school, earning a KOSSA certification, and earning a program certification. Columns 1a, 1b, and 1c present the pooled 2SLS estimates using the preferred model specification. The remaining columns present separate 2SLS estimates for men and women.

Column 1a of Table 2.3 presents the estimated effect of participation in career technical education on the probability of graduating from high school, and suggests that completing an additional career technical education course increases the probability of completing high school by 24 percentage points. Relative to the baseline value, this estimate is large, but given the standard errors, we cannot rule out smaller effect sizes. That said, the magnitude of this effect is consistent at the upper end with Dougherty (2016), who finds that participation in career technical education increases the probability of graduating from high school by 7 to 23 percentage points for high-income students, and 10 to 32 percentage points for low-income students. Columns 2a and 3a present the estimated effect of participation in career technical education on the probability of high school completion for female and male students. These estimates indicate that completing an additional career technical education course increases the probability of completing high school by 26 and 22 percentage points for female and male students, respectively.

The estimated effect of participation in career technical education on the probability of earning a KOSSA certificate is reported in column 1b. This estimate suggests that completing an additional career technical education course increases the probability of earning a KOSSA certificate by 4 percentage points. Relative to the baseline value, this estimate is large, and given the standard errors, smaller effect sizes cannot be ruled out. Nevertheless, this estimate is consistent with Dougherty (2016) who finds that participation in career technical education increases the probability of earning an industry-recognized certificate by 5 to 11 percentage points for high-income students, and 4 to 13 percentage points for low-income students. Columns 2b and 3b present the estimated effect of participation in career technical education on the probability of earning KOSSA certificate for female and male students. These estimates indicate that completing an additional career technical education course increases the probability of earning a KOSSA certificate by 4 percentage points for both male and female students.

Column 1c of Table 2.3 presents the estimated effect of participation in career technical education on the probability of earning a program certificate. This result suggests that completing an additional career technical education course has a small and statistically insignificant effect on the probability of earning a program certificate. An interpretation of this result is that while the policy change increases the number career technical education courses completed during high school, either students are opting for the KOSSA certification over the program certification, or they are not taking the courses needed to earn a program certificate.

Comparing the 2SLS estimates (discussed above) to the ordinary least squares estimates presented in Table 2.6 confirms the assertion I made in Section 4. That is, using ordinary least squares to estimate the effect of participation in career technical education on the outcomes of interest ignores the endogenous selection into participation, and therefore, produces biased estimates. A comparison of the estimates presented in column 1a of Table 2.3 and column 1 of Table 2.6 illustrates this point. The ordinary least squares estimate is smaller than the 2SLS estimate, suggesting that the former is biased downward. In fact, this appears to be true for nearly all of the outcomes of interest.

2.6.3 The Effect of Participation on Post-Secondary Outcomes

In this section, I discuss the results for the estimated effects of participation in career technical education (measured by the total number of career technical education courses completed during high school) on the probability of attending college, attending a two-year institution, and attending a four-year institution. Columns 1a, 1b, and 1c of Table 2.4 present the 2SLS estimates from the preferred model specification. Separate 2SLS estimates for men and women are reported in the remaining columns.

Column 1a presents the estimated effect of participation in career technical education on the probability of attending some college, and suggests that completing an additional career technical education course increases the probability of attending some college by 16 percentage points. Relative to the baseline value this estimate represents a 27 percent increase. Columns 2a and 3a present the estimated effects of participation in career technical education on the probability of attending some college for the gender-stratified sample. These estimates indicate that completing an additional career technical education course increases the probability of attending some college by 19 and 13 percentage points for female and male students, respectively.

The estimate for the effect of participation in career technical education on the probability of attending a two-year institution is presented in column 1b. This estimate suggests that completing an additional career technical education course increases the probability of attending a two-year institution by 7 percentage points, or an increase of 24 percent. The magnitude of this estimate is comparable with Cellini (2006), who finds that participation in career technical education is associated with an increased probability of attending a two-year institution by 8 percentage points. The estimates for the gender-stratified sample are presented in columns 2b and 3b. These estimates suggest that completing an additional career technical education course increases the probability of attending a two-year institution by 9 and 6 percentage points for female and male students, respectively.

Finally, column 1c reports the estimated effect of participation in career technical education on the probability of attending a four-year institution. This estimate indicates that a completing an additional career technical education course increases the probability of attending a four-year institution by 9 percentage points. Relative to the baseline value this estimate represents a 30 percent increase. Columns 2c and 3c present the estimated effects of participation in career technical education on the probability of attending a four-year institution for the gender-stratified sample. These estimates suggest that completing an additional career technical education course increases the probability of attending a four-year institution by 11 and 8 percentage points for female and male students, respectively.

2.6.4 The Effect of Participation on Labor Market Outcomes

In this section, I present the 2SLS estimates for the effect of participation in career technical education on the probability of being employed, and log annual earnings. Columns 1a and 1b of Table 2.5 present estimates based on the model specification that includes dummy variables for freshmen-year cohort, and district. Columns 2a and 2b present estimates using the preferred model specification.

Columns 1a and 2a present the estimated effects of participation in career technical education on the probability of being employed. The estimates for both model specifications are large and suggestive of a positive relationship, however, neither are precisely estimated. The estimates for the effect of participation in career technical education on log annual earnings are presented in columns 1b and 2b. Similarly, both estimates are suggestive of a positive relationship, but neither are precisely estimated.

I suspect that these estimates are imprecisely estimated for the following reason. Recall that I observe just two treated cohorts for two or more years after high school. My imprecise estimates are likely due to the limited workforce data that are currently available for these cohorts. Alternatively, it is possible that participation in career technical education simply does not have a meaningful effect on labor market outcomes. However, this explanation seems unlikely given the existing literature regarding the positive relationship between educational attainment and labor market outcomes (Card, 1999; Marcotte et al., 2005; Hoekstra, 2009; Jepsen et al., 2014; Zimmerman, 2014; Bishop and Mane, 2004; Arum and Shavit, 1995; Hanushek et al., 2011). That said, this analysis should be revisited once additional workforce data becomes available for these freshmen-year cohorts.

2.6.5 Robustness Checks

Table B.1 presents the 2SLS estimates for the effect of participation in career technical education on the probability of attending college, attending a two-year institution, and attending a four-year institution when the decision to attend was made in the years following high school. We known from the estimates discussed in the previous section that participation increases the probability of attending college immediately after high school, but how does participation in career technical education affect college attendance in the years following high school? Columns 1a, 1b, and 1c (of Table B.1) present the 2SLS estimates using the preferred model specification. The remaining columns present the 2SLS estimates or the gender-stratified sample using the preferred model specification.

Column 1a presents the estimate for the effect of participation in career technical education on the probability of attending college in the years following high school, and suggests that completing an additional career technical education course increases the probability of attending college by 14 percentage points. Based on the estimates presented in columns 1b and 1c, it appears as though this effect is driven by the decision to enroll in a four-year institution in the years following high school. These estimates are consistent with those presented in Table 2.4.

2.7 Conclusion

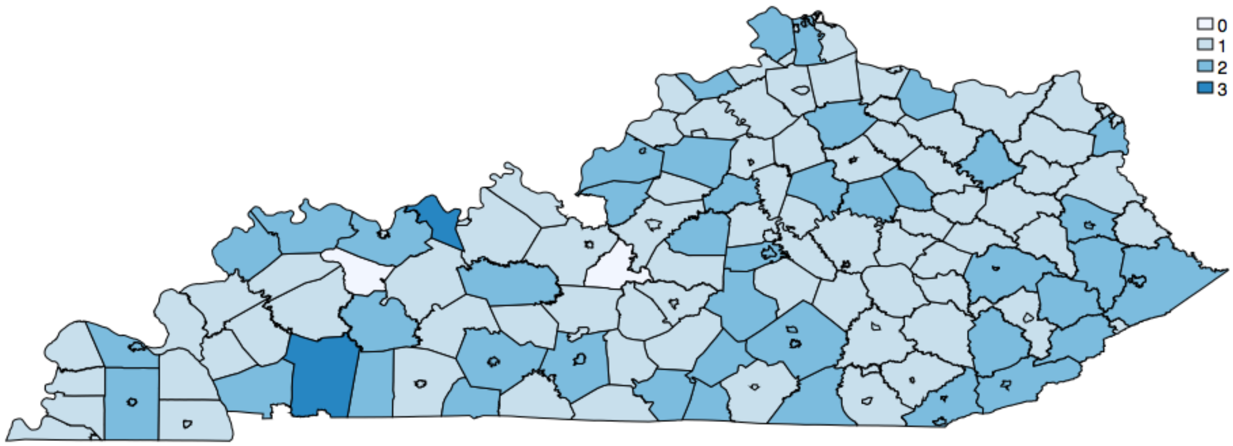
During the 2013 academic year, Kentucky adopted a policy change that increased course standards and lowered the per-credit-hour cost of career technical education courses. Policymakers hoped that these changes would increase the probability that students graduate from high school, pursue post-secondary education, and successfully transition into the labor market. Exploiting the variation in the per-credit-hour cost that resulted from this policy change, I study the effect of participation in career technical education on human capital accumulation using a variety of high school, post-secondary, and labor market outcomes.

My analysis yields three main findings. First, reducing the per-credit-hour cost of career technical education courses increases participation on both the intensive and extensive margin. This finding implies that before the policy change the cost of participation was prohibitive for some students. Second, I find that participation in career technical education (measured by the total number of career technical education courses completed during high school) increases the probability of graduating from high school, earning a KOSSA certificate, attending college, attending a two-year institution, and attending a four-year institution. I do not, however, observe a statistically significant relationship between participation and the probability of earning a program certificate. Lastly, my estimates suggest that there is a positive relationship between participation in career technical education and the probability of being employed, and annual earnings. However, these estimates are imprecisely estimated.

The implications of these findings are twofold. First, reducing the per-credit-hour cost of career technical education courses increases participation among high school students. Further, this paper shows that participation in career technical education has a positive effect on the accumulation of human capital (i.e., an increased probability of graduating from high school, earning a KOSSA certificate, and attending some college). That said, these results are specific to Kentucky, and therefore, may only be generalizable to like states. Second, my results suggest a positive, but imprecisely estimated, relationship between participation in career technical education and the probability of being employed, and annual earnings. For that reason, I suggest that this analysis be revisited once additional workforce data becomes available for the relevant freshmen-year cohorts.

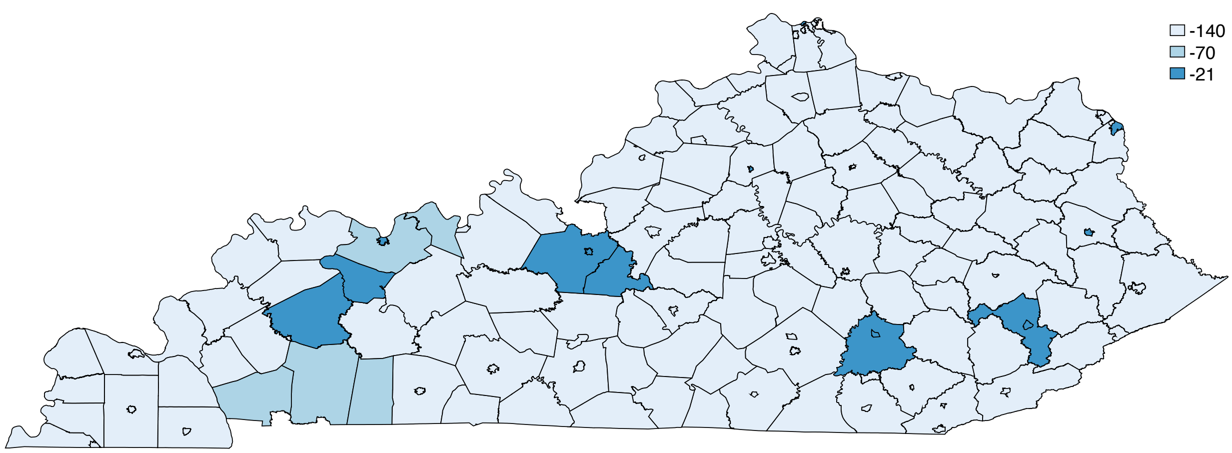
2.8 Tables & Figures

Figure 2.1: Number of Career Technical Education Facilities by School District



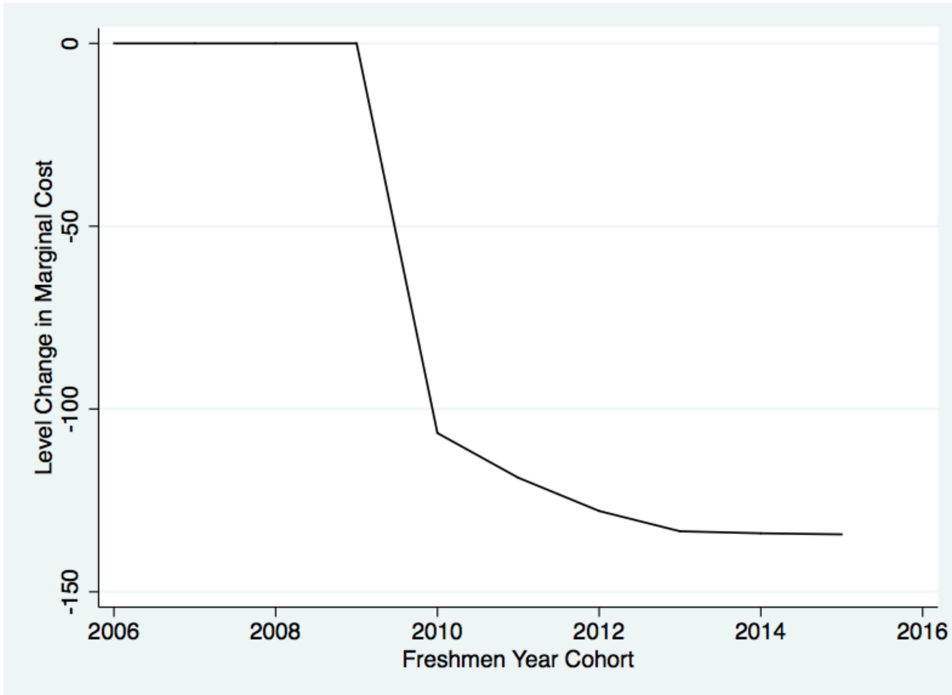
Notes: The above is a heatmap for the number of career technical education facilities by school district (i.e., the number of locations where a high school student can participate in career technical education courses). While some school districts do not have a career technical education facility located within their district, I assume that all districts have access to career technical education courses at one of the KCTCS locations.

Figure 2.2: Level Change in Cost Per Credit Hour by School District



Notes: The above is a heatmap for the level change in per-credit-hour cost by school district. That is, the above represents by how much the per-credit-hour cost for career technical education courses changed in each school district following the policy change.

Figure 2.3: The Average Change in Per-Credit-Hour Cost by Freshmen Year Cohort



Notes: The above plots the average level change in the per-credit-hour cost by freshmen year cohort. An average level change of -150 implies that the per-credit-hour cost is entirely subsidized (i.e., the student only covers the administrative fee).

Table 2.1: Summary Statistics - Overall, Baseline & Career Technical Education

	Overall	Baseline
Year of Birth	1994	1992
	(1.801)	(1.003)
Age	17.423	17.457
	(0.658)	(0.649)
Female	0.488	0.489
	(0.499)	(0.499)
Black Non-Hispanic	0.111	0.113
	(0.314)	(0.317)
White Non-Hispanic	0.821	0.825
	(0.383)	(0.379)
Hispanic	0.031	0.026
	(0.172)	(0.161)
Career Technical Education Participation	0.888	0.878
	(0.315)	(0.326)
Total career technical education Courses	5.829	5.103
	(4.493)	(4.122)
High School Graduate	0.865	0.870
	(0.341)	(0.336)
Attend Some College	0.462	0.581
	(0.498)	(0.493)
Two-Year College	0.219	0.283
	(0.413)	(0.450)
Four-Year College	0.246	0.301
	(0.431)	(0.458)
KOSSA Certificate	0.125	0.087
	(0.331)	(0.283)
Program Certificate	0.012	0.016
	(0.110)	(0.126)
Number of Observations	287,047	141,548

Notes: Standard deviations are reported in parenthesis. The baseline values represent the averages before the policy change (i.e., academic year less than 2013). The summary statistics exclude those who withdrew from the sample (i.e., moved out of state, or the Department of Education lost track of the individual after a move). Data sources: Kentucky Center for Education and Workforce Statistics.

Table 2.2: Estimated Effects of Variation in Career Technical Education Cost on Participation in Career Technical Education

	CTE Participation				CTE Courses			
	Pooled		Female	Male	Pooled		Female	Male
	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
Level Change in Cost Per Hr.	0.0005***	0.0005***	0.0003***	0.0006***	0.015***	0.013***	0.012***	0.014***
	(0.0001)	(0.0001)	(0.001)	(0.0001)	(0.002)	(0.002)	(0.002)	(0.002)
District FE:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Freshmen Yr. Cohort FE:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind. Characteristics:	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F-Stat:	22.33	18.76	16.68	18.06	40.52	34.00	33.79	32.98
N:	287,047	286,724	140,005	146,719	287,047	286,724	140,005	146,719

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains first-stage estimates for the effect of the average change in per-credit-hour cost on an indicator for participation in career technical education courses, and the total number of career technical education courses taken while in high school. Each column contains the results from a single regression predicting the effect of reduced cost on participation in career technical education. The following controls are used: white non-Hispanic, black non-Hispanic, age, ever enrolled in special education, and gender. Dummy variables for district, and freshmen-year cohort are also included. Standard errors are clustered on district and are reported in parenthesis. Data Source: The Kentucky Center for Education and Workforce Statistics.

Table 2.3: 2SLS Estimated Effects of Participation in Career Technical Education on High School Outcomes

	High School Graduation			KOSSA			Prog. Certificate		
	Pooled	Female	Male	Pooled	Female	Male	Pooled	Female	Male
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
Total CTE Courses	0.238***	0.258***	0.224***	0.043***	0.042***	0.044***	-0.001	0.000	-0.002
	(0.026)	(0.036)	(0.021)	(0.007)	(0.010)	(0.006)	(0.002)	(0.002)	(0.002)
N:	286,724	140,005	146,719	286,724	140,005	146,719	286,724	140,005	146,719

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains the 2SLS estimates for the effect of participation in career technical education on the probability of high school graduation, KOSSA certification, and program specific certification. The following controls are used: white non-Hispanic, black non-Hispanic, age, ever enrolled in special education, and gender. Dummy variables for district, and freshmen-year cohort are also included. Standard errors are clustered on district and are reported in parenthesis. Data Source: The Kentucky Center for Education and Workforce Statistics.

Table 2.4: 2SLS Estimated Effects of Participation in Career Technical Education on Post-Secondary Outcomes

	Attend Some College			Two-Year College			Four-Year College		
	Pooled	Female	Male	Pooled	Female	Male	Pooled	Female	Male
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
Total CTE Courses	0.157***	0.193***	0.131***	0.069***	0.088***	0.055***	0.088***	0.105***	0.076***
	(0.016)	(0.024)	(0.013)	(0.009)	(0.013)	(0.007)	(0.012)	(0.017)	(0.009)
N:	286,724	140,005	146,719	286,724	140,005	146,719	286,724	140,005	146,719

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains the 2SLS estimates for the effect of participation in career technical education on the probability of college attendance, attending a two-year college, and attending a four-year college. The following controls are used: white non-Hispanic, black non-Hispanic, age, ever enrolled in special education, and gender. Dummy variables for district, and freshmen-year cohort are also included. Standard errors are clustered on district and are reported in parenthesis. Data Source: The Kentucky Center for Education and Workforce Statistics.

Table 2.5: 2SLS Estimated Effects of Career Technical Education Participation on Labor Market Outcomes

	Employed		Log Annual Earnings	
	(1a)	(2a)	(1b)	(2b)
Total CTE Courses	0.050	0.133	0.157	0.287
	(0.039)	(0.229)	(0.207)	(0.763)
District FE:	Yes	Yes	Yes	Yes
Freshmen Yr. Cohort FE:	Yes	Yes	Yes	Yes
Ind. Characteristics:	No	Yes	No	Yes
N:	3,924	3,924	3,924	3,924

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains the 2SLS estimates for the effect of participation in career technical education on the probability of being employed, and annual earnings. The following controls are used: white non-Hispanic, black non-Hispanic, age, ever enrolled in special education, and gender. Dummy variables for district, and freshmen-year cohort are also included. Robust standard errors are reported in parenthesis. Estimates are weighted by cell count. Data Source: The Kentucky Center for Education and Workforce Statistics.

Table 2.6: OLS Estimated Effects of Career Technical Education Participation on High School & Post-Secondary Outcomes

	H.S. Grad.	KOSSA	Prog. Cert.	Attend Some Coll.	2-Yr. Coll.	4-Yr. Coll.
	(1)	(2)	(3)	(4)	(5)	(6)
Total CTE Courses	0.015***	0.015***	0.001***	0.002**	0.008***	-0.006***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Indicator for CTE Participation	0.152***	0.104***	0.008***	0.054***	0.088***	-0.034***
	(0.022)	(0.010)	(0.002)	(0.013)	(0.008)	(0.008)
N:	286,724	286,724	286,724	286,724	286,724	286,724

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains the ordinary least squares estimates for the effect of participation in career technical education on high school graduation, career technical education certifications, and the probability of college attendance using the preferred model specification. Participation in career technical education is measured as the total number of career technical education courses completed during high school (row 1), and an indicator for participation in career technical education during high school (row 2). The following controls are used: white non-Hispanic, black non-Hispanic, age, ever enrolled in special education, and gender. Dummy variables for district, and freshmen-year cohort are also included. Standard errors are clustered on district and are reported in parenthesis. Data Source: The Kentucky Center for Education and Workforce Statistics.

Chapter 3

The Effect of Reduced Coverage for Inpatient Psychiatric Care

3.1 Introduction

In the United States, approximately one in five adults has a mental illness. According to mental health advocates and researchers, each year many of these Americans do not receive medically necessary treatment for their mental illness (Fuller et al., 2016; Bose et al., 2016). Research suggests that the problem with delaying, or not receiving, medically necessary treatment is twofold. First, psychiatric treatments are effective at addressing the symptoms associated with mental illness (Bartak et al., 2011; Creed et al., 1997). Second, delaying treatment could result in a worsening of symptoms, and over time the illness may become less responsive to treatment (Haas et al., 1998; Loebel et al., 1992; Norman and Malla, 2001).

In 2015 the Substance Abuse and Mental Health Services Administration conducted their National Survey on Drug Use and Health. The results of that survey suggest that in 2015 approximately 43 million adults in the United States had a mental illness, and approximately

10 million had a serious mental illness (Bose et al., 2016).[1] In addition, 57 percent of those with a mental illness and 35 percent of those with a serious mental illness did not receive treatment that year (Bose et al., 2016). It has been observed that when traditional treatment options are not available (e.g., inpatient psychiatric care), individuals turn to the emergency department for care (Torrey et al., 2012; Fuller et al., 2016; Szabo, 2014).[2] However, if the emergency department is not equipped to stabilize their psychiatric symptoms, they could be boarded in either a private room or hallway until an inpatient psychiatric bed becomes available (Fuller et al., 2016; Torrey et al., 2012). Many contend that this practice negatively affects the care given to emergency department patients by increasing wait times and reducing the availability of emergency department beds (Nicks and Manthey, 2012; Richards et al., 2014). Despite the above observations, there is no causal evidence regarding the relationship between untreated mental illness and emergency department utilization.

I contribute to this literature by providing valuable causal evidence for the effect of reduced insurance coverage for inpatient psychiatric care on emergency department utilization. Specifically, I use a regression discontinuity framework to estimate the relationship between insurance coverage for inpatient psychiatric care and emergency department visits.[3] I exploit the variation in coverage for inpatient psychiatric care that results from Medicaid's Institutions for Mental Disease (IMD) exclusion to study this relationship. Medicaid's IMD

[1] The definition of serious mental illness for children is as follows: children (i.e., birth to the age of 18) with a diagnosed mental illness listed in the DSM-III-R (or the equivalent diagnosis in ICD-9-CM), where the diagnosis interferes with their ability to function in daily activities. The same definition applies to adults, but the age range is 18 years or older (SAMHSA, 1993).

[2] For example, if an individual experiences a psychiatric emergency medical condition (EMC), they could be sent to the emergency department for care. The Emergency Medical Treatment and Labor Act (EMTALA) of 1986 mandates that hospitals participating in Medicare examine any person who comes to the emergency department with an EMC. Per the EMTALA, an EMC is defined as "a medical condition manifesting itself by acute symptoms of sufficient severity (including severe pain, psychiatric disturbances and/or symptoms of substance abuse) such that absence of immediate medical attention could reasonably be expected to result in (i) placing the health of the individual... in serious jeopardy; (ii) serious impairment to bodily functions; or (iii) serious dysfunction of any bodily organ or part" (42 CFR 489.24(b)).

[3] Previous research has demonstrated that insurance coverage increases health care utilization (Brook et al., 1984; Finkelstein et al., 2012; Anderson et al., 2012; Card et al., 2008, 2009). Therefore, it is not unreasonable to assume that a reduction in coverage for inpatient psychiatric care will lead to a reduction in the utilization of inpatient psychiatric care. This reduced utilization of inpatient psychiatric care could translate to an increase in emergency department visits.

exclusion states that, for beneficiaries between the age of 21 and 64, inclusive, Medicaid will not reimburse for medically necessary inpatient psychiatric care administered at an institution with more than sixteen beds. In practice, this means that Medicaid beneficiaries experience a reduction in coverage for inpatient psychiatric care at age 21.[4]

To estimate the relationship between coverage for inpatient psychiatric care and emergency department visits, I use the near universe of emergency department and inpatient discharge records from Arizona and Kentucky.[5] I report separate estimates for men and women because of the well-documented gender differences with respect to the age of onset for certain mental illnesses (Kessler et al., 2007; Astbury, 2001). I also stratify my estimates by expected payer type, and I do this for the following reason. At age 21, Medicaid beneficiaries experience a reduction in coverage for inpatient psychiatric care. I, therefore, expect to find a discontinuity in mental health visits for the population of Medicaid-eligible individuals (i.e., the expected payer is either Medicaid or self-pay). My results are consistent with the above. For Medicaid-eligible men, I find robust negative effects of reduced coverage for inpatient psychiatric care on mental health emergency department visits (excluding alcohol-related visits). These effects are large, on the order of 11 percent. I do not observe statistically significant effects for Medicaid-eligible women.

My results provide robust evidence in support of the claim that there is a relationship between untreated mental illness and emergency department utilization. At a minimum, this reduction in coverage for inpatient psychiatric care imposes an externals cost on emergency departments and their patients. Work by Nicks and Manthey (2012) quantifies this cost and

[4]Similar identification strategies have been used by Anderson et al. (2012), Card et al. (2008), and Card et al. (2009) to estimate the effects of insurance coverage on other outcomes. For example, Anderson et al. (2012) study the effect of insurance coverage on emergency department visits by exploiting a change in insurance coverage for young adults. They find that being uninsured results in a decrease in emergency department visits. Similarly, Card et al. (2008, 2009) find that Medicare eligibility increases the use of medical care and reduces the probability of death within seven days of admission by 20 percent.

[5]I selected these states because several years of emergency department and inpatient data are available, and the Medicaid eligibility cutoff for children is 19 and not 21 years old. The eligibility cutoff occurring at age 19 (and not 21) is important because I want to identify the effect of reduced insurance coverage for inpatient psychiatric care at age 21.

suggests that psychiatric boarding in the emergency department is associated with the loss of two bed turnovers per psychiatric patient. For the emergency department, this reduction in bed turnovers translates to a loss of 2,264 dollars per psychiatric patient. Additionally, research by Richards et al. (2014) suggests that the practice of boarding patients in hallways is associated with increased patient morbidity and mortality, where this effect is for both psychiatric and non-psychiatric patients. Many argue that the effects of untreated mental illness could be much larger, affecting incarceration, homelessness, and an individual's ability to work on both the intensive and extensive margin (Torrey et al., 2012; Fuller et al., 2016; Slade and Salkever, 2001; Wu et al., 2005).[6]

To address the confounding effect of the minimum legal drinking age, I define my outcome variable to exclude all alcohol-related visits. Specifically, I exclude the ICD-9-CM codes used in Carpenter and Dobkin (2017), and I show that my results for Medicaid-eligible men are robust to this exclusion.[7] I also conduct a placebo test to check that there is no discontinuity in this outcome for private insurance beneficiaries (i.e., individuals that do not experience a change in insurance coverage at age 21). Consistent with the hypothesis that my outcome variable is not confounded by alcohol use at age 21, I find no discontinuity in mental health emergency department visits for this group.

3.2 Medicaid's IMD Exclusion

Medicaid is a means-tested health insurance program that is jointly funded by the federal government and states. Medicaid eligible individuals include low-income families, pregnant women and children (defined as less than 19 in some states and 21 in others) from low-income households, and individuals receiving supplemental security income benefits. Eligibility rules

[6] In future work I will study the effect of coverage for inpatient psychiatric care on these potential external and internal costs.

[7] Carpenter and Dobkin (2017) study the effect of the minimum legal drinking age on emergency department visits. They find that at age 21 emergency department visits increase by 71 per 10,000 person-years.

like these have been exploited by Card et al. (2008), Card et al. (2009), and Anderson et al. (2012).

Under federal law, certain Medicaid benefits are mandatory (e.g., family planning services, physician services, etc.). But some benefits are optional, and it is up to the States to decide what will be covered (e.g., dental services, eyeglasses, etc.). Moreover, some services are not covered when provided to certain subgroups of Medicaid beneficiaries. The IMD exclusion is an example of the latter.

To see what this means in practice, let us consider coverage for inpatient psychiatric care by comparing two health insurance plans in each state (i.e., Medicaid versus a private insurance plan in Arizona and Kentucky).[8] Specifically, the following section (and Figure 3.1) compares the inpatient psychiatric benefits of Medicaid to Anthem Blue Cross and Blue Shield of Kentucky Silver HMO 3500 and Aetna Leap Everyday HMO in Arizona.[9]

Medicaid provides screening, diagnostic, and treatment services to all Medicaid beneficiaries less than 21 years old for at most a small monthly premium. In Arizona, Medicaid beneficiaries are not responsible for copayments if the child is less than 19 years old, but they are responsible for a small monthly premium. Medicaid beneficiaries in Kentucky, however, are not responsible for copayments or monthly premiums. Once a Medicaid beneficiary turns 21, regardless of their state of residence, their inpatient psychiatric care is only covered by Medicaid if it is administered in an institution with 16 or fewer beds.[10] Since there are relatively few institutions with sixteen or fewer beds, at age 21, Medicaid beneficiaries expe-

[8]Inpatient psychiatric care services are 24-hour service provided in a licensed hospital. These services include clinical interventions for mental health, substance abuse, or both.

[9]To identify a private health insurance plan for this comparison, I went to the Anthem Blue Cross Blue Shield of Kentucky (BCBSKY) website and entered the information for a household of one adult (i.e., 30 years old), with an annual household income equal to the median for Kentucky, and visits the doctor a few times a year. Given the above information, Anthem BCBSKY recommended the Silver HMO 3500 plan. After identifying an insurance plan available to Kentucky residents, I located a comparable plan for Arizona residents.

[10]There are relatively few institutions with 16 or fewer beds available. For example, in 2015, approximately 6 percent of Arizona's inpatient psychiatric beds and 3 percent of Kentucky's inpatient psychiatric beds were located in an institution with 16 or fewer beds.

rience a reduction in coverage for inpatient psychiatric care. This rule is referred to as the IMD exclusion, and it has been in effect since Medicaid was created in 1965.[11]

In contrast, in 2017, the BCBSKY Silver HMO 3500 plan covers half the cost of inpatient mental health services. That is, once the beneficiary has met their 3500 dollar deductible, they are responsible for paying 500 dollars plus 50 percent coinsurance of inpatient hospital facility, residential, and physician fees resulting from mental health inpatient care. Beneficiaries of Arizona's Aetna Leap Everyday plan are responsible for 0 percent coinsurance after meeting their 5000 dollar deductible. Unlike Medicaid, these benefits are not conditional on the number of beds in the inpatient facility, or the beneficiaries age. Since these private plans are not associated with a reduction in coverage, at the age 21 threshold there will be a reduction in coverage that is equal to the fraction of Medicaid beneficiaries that are just less than 21.

3.3 Empirical Strategy

To understand the empirical strategy used in this paper, let us begin by assuming that I can measure insurance coverage for inpatient psychiatric care and emergency department utilization. In that case, I would estimate the relationship between coverage and emergency department utilization using the standard "fuzzy" RD framework. I would begin with an equation for the relationship between Medicaid's IMD exclusion and insurance coverage for

[11]In 1967, the IMD exclusion became binding for all states when the Early and Periodic Screening, Diagnostic, and Treatment (EPSDT) component of Medicaid was introduced. There are a few exceptions to the IMD exclusion, however: (i) if the Medicaid beneficiary is less than 21 years old, (ii) the inpatient psychiatric institution has 16 or fewer beds, or (iii) the Medicaid beneficiary was already receiving inpatient psychiatric care in an IMD when they turned 21. Also, as of 2016, the Centers for Medicare and Medicaid Services allows the in lieu of rule to be applied to IMDs. With respect IMDs, the in lieu of rule states that, for Medicaid beneficiaries between the age of 21 and 64 with an MCO or PIHP plan, Medicaid will reimburse for inpatient psychiatric care administered at an IMD in place of a more expensive setting.

inpatient psychiatric care (i.e., the first-stage):

$$COV_m = \gamma_0 + \gamma_1 AGE21_m + f(AGE_m) + \upsilon_m \tag{3.1}$$

The dependent variable COV_m, is an indicator for insurance coverage for inpatient psychiatric care for age in months group m; AGE is age in months relative to the threshold (i.e., AGE = 0 if the patient is 21 and 0 months at time of visit); $f(AGE)$ captures the relationship between the dependent variable and age in months; and AGE21 is an indicator taking a value of 1 if AGE is ≥ 0. The parameter of interest, γ_1, captures the effect of the IMD exclusion on coverage for inpatient psychiatric care.

Next, I would define an equation that describes the relationship between insurance coverage and emergency department utilization (i.e., the second-stage):

$$ED_m = \beta_0 + \beta_1 COV_m + f(AGE_m) + \epsilon_m \tag{3.2}$$

The dependent variable, ED_m, is the person-year rate for emergency department visits for age in months group m. The parameter of interest, β_1, captures the effect of insurance coverage on the rate of emergency department visits. However, since coverage for inpatient psychiatric care is not randomly assigned, using least squares to estimate this relationship would likely result in a biased estimate for β_1. For that reason, I would use the first two equations to estimate β_1 via two-stage least squares; where $AGE21$ instruments for coverage, COV.

In practice, however, there are two key differences between the data used here and the data required to implement the above. First, I do not have a clean measure of the treatment (i.e., insurance coverage for inpatient psychiatric care). Using data from the CPS, I know the first-stage relationship will be approximately 0.30 (see my back-of-the-envelope first-stage illustrated in Figures 3.12 and 3.13), but I do not have the data to implement two-stage least squares or two-sample two-stage least squares.[12] Instead, I report the reduced form estimates in Section 5, and I discuss the implied treatment effects in Section 6. Second, I do not have the right population estimates for the denominator on the outcome measure (i.e., the rate of emergency department visits). Instead, I use the logged count of emergency department visits. Card et al. (2008) do something similar, and as they point out, if the population trends smoothly then the estimated discontinuity can be interpreted as the percent change in the rate of emergency department visits.[13] That is, using the logged count of emergency department visits for 24 months on either side of the threshold, I estimate the following reduced-form model:

$$ED_m = \delta_0 + \delta_1 AGE21_m + f(AGE_m) + \zeta_m \tag{3.3}$$

When using an RD framework, one concern is that there could be another discontinuity source at the threshold you are exploiting. A potential issue with the above estimation strategy is that 21 is the minimum legal drinking age in the United States, and therefore, could have confounding effects on the outcomes of interest. Recall that Carpenter and Dobkin

[12]To implement two-stage least squares, I would need a clean measure of the treatment, age in months, emergency department visits and the diagnosis codes for those visits, gender, and expected payer type. To implement two-sample two-stage least squares, I would need a clean measure of the treatment, age in months, gender, and expected payer type in one dataset, and age in months, emergency department visits and the diagnosis codes for those visits, gender, and expected payer type in another dataset. What I have is the near universe of emergency department visits for Arizona and Kentucky between 2005 and 2011. That data includes age in months, emergency department visits and the diagnosis codes for those visits, gender, and expected payer type.

[13]Table C.1 in the Appendix presents estimates that are comparable to those from Carpenter and Dobkin (2017).

(2017) find an increase in the rate of emergency department visits and alcohol-related injuries at age 21. To address this potential threat to identification, two of my measures for mental health emergency department visits exclude the ICD-9-CM codes used in Carpenter and Dobkin (2017) (i.e., two measures are based on diagnosis codes that are not associated with alcohol-related visits).

3.3.1 The Identifying Assumption

For this empirical strategy, the key identifying assumption is that the distribution of potential outcomes is smooth at the threshold. That is to say that, individuals must have imprecise control over the running variable (i.e., age in months). If true, then we can assume that the treatment (i.e., turning 21) is as good as randomly assigned near the threshold, and we can attribute any discontinuity at the threshold to the treatment. Since the potential outcomes are functions of observed and unobserved characteristics, we cannot test this assumption directly. However, we can evaluate the distributions of observed characteristics. If these distributions are smooth at the threshold, then that suggests that the treatment is as good as randomly assigned near the threshold.

I test the above assumption by studying the distribution of non-deferrable admissions (i.e., an observable characteristic that is unaffected by the treatment). If the distribution is smooth at the threshold, then that suggests two things. First, nothing else is changing at age 21 that affects emergency department access. Second, individuals have imprecise control over the running variable. Per Card et al. (2009), we can define any diagnosis with an equal probability of showing up in the emergency department during a weekday or weekend as non-deferrable (i.e., the fraction of weekend admissions for non-deferrable diagnoses should be equal to 2/7).[14] In other words, these are conditions for which, all else being equal,

[14] The following is a list of the top ten non-deferrable diagnoses for individuals 19 to 23, inclusive: (i) other disorders of the urethra and urinary tract, (ii) pregnancy - hemorrhage, (iii) symptoms involving the

we would not expect the likelihood of showing up in the emergency department to change discretely by age. Figure 3.2 plots this distribution, and suggests that individuals have imprecise control over the running variable.

3.4 Data

This paper uses emergency department and inpatient discharge records from the Healthcare Cost and Utilization Project (HCUP). The emergency department and inpatient discharge records represent the near universe of discharges for Arizona, and Kentucky from 2005 through 2011.[15] Some emergency department visits result in an inpatient admission (i.e., these records are not defined as emergency department discharges), so my sample includes the inpatient discharges that originated in the emergency department. The empirical strategy employed in this paper relies on knowing the age in months for each emergency department patient. This information is available for emergency department visits in Arizona between 2005 and 2009, and emergency department visits in Kentucky between 2008 and 2011.[16] In addition to the above, these records also provide detailed information on the specific diagnoses (i.e., the actual ICD-9-CM codes) associated with each emergency department visit, the expected primary payer, and the month and year of visit. For this analysis, I focus on individuals between the age of 19 and 23, inclusive, resulting in a raw sample size of 1,409,323 emergency department visits.

digestive system, (iv) upper respiratory infection, (v) acute bronchitis, (vi) asthma, (vii) bronchitis not specified as acute or chronic, (viii) kidney infections, (ix) symptoms involving skin and like tissue, and (x) inflammatory diseases of the cervix, vagina, and vulva.

[15]In 2011, 1.8 percent of the American Hospital Association (AHA) emergency department visits were not included in Arizona's emergency department discharge records. For that same year, less than 1 percent of the AHA emergency department visits were not included in Kentucky's emergency department discharge records.

[16]Recall that I selected these states because several years of emergency department and inpatient data are available, and the Medicaid eligibility cutoff for children is 19 and not 21 years old. Emergency department records are available for Arizona (via HCUP) starting in 2005, and the same data are available for Kentucky starting in 2008. For Arizona's emergency department discharge records, the last year that birth month is included in the data is 2009. Due to the implementation of the Affordable Care Act, I do not look at emergency department data beyond 2011.

Much of my analysis focuses specifically on mental health emergency department visits. To uniquely identify these types of visits, I look at the first three ICD-9-CM diagnosis codes listed on each record (i.e., the primary and first two secondary diagnosis codes); where ICD-9-CM codes 290 through 319 indicate a mental illness diagnosis. Table 3.1 lists the ten most frequent primary mental illness diagnoses for Medicaid-eligible individuals between 19 and 23 years old, inclusive. As Table 3.1 indicates, the most frequent ICD-9-CM mental illness diagnosis codes include certain forms of alcohol and drug use, and alcohol and drug dependence. To identify visits specifically related to mental health, I define three measures of mental health emergency department visits with varying degrees of restriction on drug and alcohol-related visits.

I begin with a measure of mental health emergency department visits without any restrictions on drug and alcohol-related ICD-9-CM diagnosis codes for mental illness. This measure, however, is problematic because it will also capture the effect of minimum legal drinking age in the United States. Therefore, my second measure of mental health emergency department visits excludes the diagnosis codes used in Carpenter and Dobkin (2017) (i.e., ICD-9-CM codes 291, 303, and 3050). My third measure of mental health emergency department visits is based solely on the primary diagnosis code and excludes visits with a primary diagnosis code for drugs or alcohol. Some might be concerned that this measure allows for comorbidity of mental illness and substance abuse, however, according to previous research this type of comorbidity is common (Regier et al., 1990).

3.4.1 Descriptive Statistics

Table 3.2 presents the descriptive statistics for the data used in this analysis. For female patients, comparing the fraction of non-mental health emergency department visits to mental health emergency department visits suggests that fewer women are visiting the emergency department because of a mental illness diagnosis. This pattern is consistent with Astbury (2001) who observes that men have an earlier age of onset for some mental disorders like schizophrenia. That is, for the age range considered in this analysis, we would expect to see a larger fraction of male patients visiting the emergency department because of a mental illness diagnosis.

Next, let us compare non-mental health and mental health emergency department visits by the primary expected payer. For Medicaid beneficiaries, this comparison suggests that the fraction of mental health visits is greater than the fraction of non-mental health emergency department visits. The same pattern is observed when the primary expected payer is self-pay. In other words, this pattern is consistent with Medicaid-eligible individuals experiencing a reduction in insurance coverage for inpatient psychiatric care and, as a result, turning to the emergency department for care.

3.5 Results

In this section, I present the estimates from the reduced form model described in equation (3). Figures 3.3 through 3.10 plot the age profiles for the logged count of emergency department visits, stratified by gender and expected payer (i.e., Medicaid-eligible and private insurance). Each point on these Figures represents the logged count of emergency department visits for each age in month group, and the lines are the fitted linear trends from equation (3). In the United States, when someone turns 21 two things occur. First, they can legally consume alcohol. Second, if they are a Medicaid beneficiary, they will experience a reduction in coverage for inpatient psychiatric care. Therefore, we would expect to find a discontinuity in emergency department visits for both men and women (i.e., due to alcohol use and a reduction in coverage for inpatient psychiatric care at age 21). However, given what we know about the age of onset for certain mental illness diagnoses, we would expect this discontinuity (due to the reduction in coverage) to be larger for men. Further, we would expect to observe a discontinuity (due to the reduction in coverage) for Medicaid-eligible individuals (i.e., the expected payer is either Medicaid or self-pay), but not for private insurance beneficiaries (i.e., individuals who do not experience a reduction in insurance coverage for inpatient psychiatric care at age 21).

Figures 3.3 and 3.4 plot the age profiles for the logged count of emergency department visits stratified by gender and expected payer. Table 3.3, row 1, columns 2 and 4 report the estimates for the discontinuities illustrated in Figure 3.3, and indicate that at age 21 Medicaid's IMD exclusion increases emergency department visits by 3 percent for Medicaid-eligible men. For Medicaid-eligible women, I do not observe a statistically significant effect. When the expected payer is private insurance, I find that at age 21 Medicaid's IMD exclusion increases emergency department visits by 4 and 1 percent for men and women, respectively.

One concern with the above estimates is that they do not account for the confounding effect of alcohol- and drug-related visits at age 21. For that reason, I define three measures of mental health emergency department visits with varying degrees of restriction on alcohol- and drug-related visits. Figures 3.5 and 3.6 plot the age profiles for the unrestricted logged count of mental health emergency department visits stratified by gender and expected payer. Table 3.3, row 2, columns 2 and 4 report the estimates for the discontinuities observed in Figure 3.5, and indicate that at age 21 Medicaid's IMD exclusion increases unrestricted mental health visits by 7 percent for Medicaid-eligible men. For Medicaid-eligible women, I find that at age 21 Medicaid's IMD exclusion increases unrestricted mental health visits by 2 percent. When the expected payer is private insurance, I find that at age 21 Medicaid's IMD exclusion increases unrestricted mental health visits by 7 percent for men. For women, I observe a statistically insignificant effect.

Recall that alcohol use and dependence are classified as mental illnesses. For that reason, this unrestricted measure does not adequately account for the confounding effect of alcohol use at age 21. Therefore, I proceed with my analysis using a measure of mental health emergency department visits that excludes all alcohol-related diagnosis codes (i.e., the ICD-9-CM codes used in Carpenter and Dobkin (2017)). Figures 3.7 and 3.8 plot the age profiles for the logged count of restricted mental health emergency department visits for men and women. Table 3.3, row 3, columns 2 and 4 report the estimates for the discontinuities observed in Figure 3.7, and indicate that at age 21 Medicaid's IMD exclusion increases restricted mental health visits by 4 percent for Medicaid-eligible men. Once again, for Medicaid-eligible women, I do not observe a statistically significant effect. When the expected payer is private insurance, I find that at age 21 Medicaid's IMD exclusion does not have a statistically significant effect on restricted mental health visits for men or women. This finding is consistent with my hypothesis that this restricted measure of mental health emergency department visits is not confounded by alcohol use at age 21.

Lastly, Figures 3.9 and 3.10 plot the age profiles for the logged count of mental health emergency department visits based on the primary diagnosis code only, and excluding primary diagnosis codes for drugs and alcohol. Table 3.3, row 4, columns 2 and 4 report the estimates for the discontinuities observed in Figure 3.9, and indicate that at age 21 Medicaid's IMD exclusion increases mental health visits by 14 percent for Medicaid-eligible men. As with the previous outcomes, I do not observe a statistically significant effect for Medicaid-eligible women. When the expected payer is private insurance, I do not find a statistically significant effect for men or women.

3.6 Discussion of Results

In this section, I discuss the estimated treatment effects and the potential costs associated with these effects. Up to this point, all of the results that have been discussed are reduced form estimates. In order to identify the causal effect of reduced coverage on emergency department visits, these estimates must be scaled by first-stage estimates.[17] Recall that I do not have a measure of coverage for inpatient psychiatric care, so I am unable to estimate the first-stage. However, using data from the CPS, and assuming that all Medicaid beneficiaries lose coverage for inpatient psychiatric care at age 21, via a back-of-the-envelope calculation I approximate the first-stage for Medicaid-eligible individuals.

I begin by verifying that there is no discontinuity in Medicaid coverage at age 21. Figure 3.11 plots the age profiles for the fraction of Medicaid beneficiaries in Arizona and Kentucky, and illustrates that coverage is smooth at the threshold. Since the fraction of Medicaid beneficiaries is smooth at the threshold, I can attribute any reduction in coverage for inpatient psychiatric care at age 21 to Medicaid's IMD exclusion. However, due to the fact that in Arizona and Kentucky children can qualify for Medicaid through their parents until the age of 19, there is a reduction in the fraction of Medicaid beneficiaries at age 19.

Having verified that the fraction of Medicaid beneficiaries is smooth at the threshold, I proceed with my back-of-the-envelope calculation for the relationship between Medicaid's IMD exclusion and coverage for inpatient psychiatric care. There are relatively few institutions with 16 or fewer beds, so I begin by assuming that all Medicaid beneficiaries experience a reduction in coverage at age 21. Therefore, I assume that at the threshold the reduction in coverage for inpatient psychiatric care is equal to the fraction of Medicaid beneficiaries that are just less than 21. Figures 3.12 and 3.13 illustrate my back-of-the-envelope calculation, and suggest that at age 21 coverage decreases by 32 percent for men and 30 percent for women.[18]

[17]The Wald estimator is the reduced form estimate scaled by the first-stage estimate (i.e., using the notation from equations (1) through (3), $\beta_1 = \frac{\delta_1}{\gamma_1}$) and consistently estimates the LATE.

[18]I attempt to estimate the first stage using data from California's Office of Statewide Health Planning and Development (OSHPD) Annual Survey of Hospitals. Using this data, I estimate the fraction of people in California with inpatient psychiatric care covered by Medicaid. To the left of the threshold, the fraction of people with coverage is equal to the fraction of Medicaid beneficiaries. To the right of the threshold, the fraction of people with coverage is equal to the fraction of Medicaid beneficiaries who live in a county with an inpatient psychiatric hospital with 16 or fewer beds. These estimates suggest that at age 21 Medicaid's IMD exclusion reduces coverage for inpatient psychiatric care by 9 to 10 percent. See Figures C.2 and C.3 in the Appendix.

Scaling the reduced-form estimate for restricted mental health emergency department visits by my back-of-the-envelope calculation, I find that reduced coverage for inpatient psychiatric care increases restricted mental health emergency department visits by 11 percent for Medicaid-eligible men. Repeating this exercise for the reduced form estimate for primary mental health emergency department visits, I find that reduced coverage increases these emergency department visits by 43 percent for Medicaid-eligible men. Since I assume that all Medicaid beneficiaries lose coverage at age 21, my back-of-the-envelope calculation likely overstates the true first-stage. In other words, we can think of these estimates as representing the lower bound for the treatment effect.

In terms of cost, what does the increase in emergency department visits mean for Medicaid? Using the emergency department discharge data, I determine that the average total charge for restricted mental health visits is 2,289 dollars.[19] In comparison, the average cost of inpatient psychiatric care is 5,918 dollars with an average length of stay equal to nine days.[20] That is to say that, it would take 2.6 restricted mental health emergency department visits to exceed the cost of reimbursing for inpatient psychiatric care. In fact, previous research observes that psychiatric patients have on average two repeat emergency department visits. For example, Dhossche and Ghani (1998) observe that, over a seven month period, the average number of visits for psychiatric patients was approximately 3. This observation is consistent with Mahajan et al. (2009), who observe that between 2003 and 2005 the average number of repeat visits for psychiatric patients 19 and younger is 2 (i.e., 3 visits).

In addition to the Medicaid-specific costs, reduced coverage for inpatient psychiatric care results in external costs for emergency departments. Recall that Nicks and Manthey (2012) quantify part of this cost, and suggest that psychiatric boarding in the emergency department is associated with the loss of two bed turnovers per patient. From the perspective of the

[19]This is for Medicaid-eligible men just less than 21 years old, and does not include professional fees or non-covered charges.

[20]The estimate is based on the Medicaid reimbursed amounts for schizophrenia, bipolar, and depression reported in Stensland et al. (2012).

emergency department, this reduction in bed turnovers translates to a loss of 2,264 dollars per psychiatric patient. Moreover, work by Richards et al. (2014) suggests that an increase in emergency department utilization results in patients being boarded in hallways. They argue that this practice is associated with an increase in patient morbidity and mortality for both psychiatric and non-psychiatric patients.

Mental health advocates and researchers contend that the costs of reduced coverage are potentially much larger than the Medicaid-specific and external costs discussed above. Specifically, they argue that reduced coverage for inpatient psychiatric care affects incarceration, homelessness, and an individual's ability to work on the extensive and intensive margin (Torrey et al., 2012; Fuller et al., 2016; Slade and Salkever, 2001; Wu et al., 2005). In fact, a recent report by Henry et al. (2016) suggests that 20 percent of those who are homeless or incarcerated suffer from a serious mental illness. In future research, I will study the relationship between reduced coverage for inpatient psychiatric care and these indirect and direct costs.

3.7 Conclusion

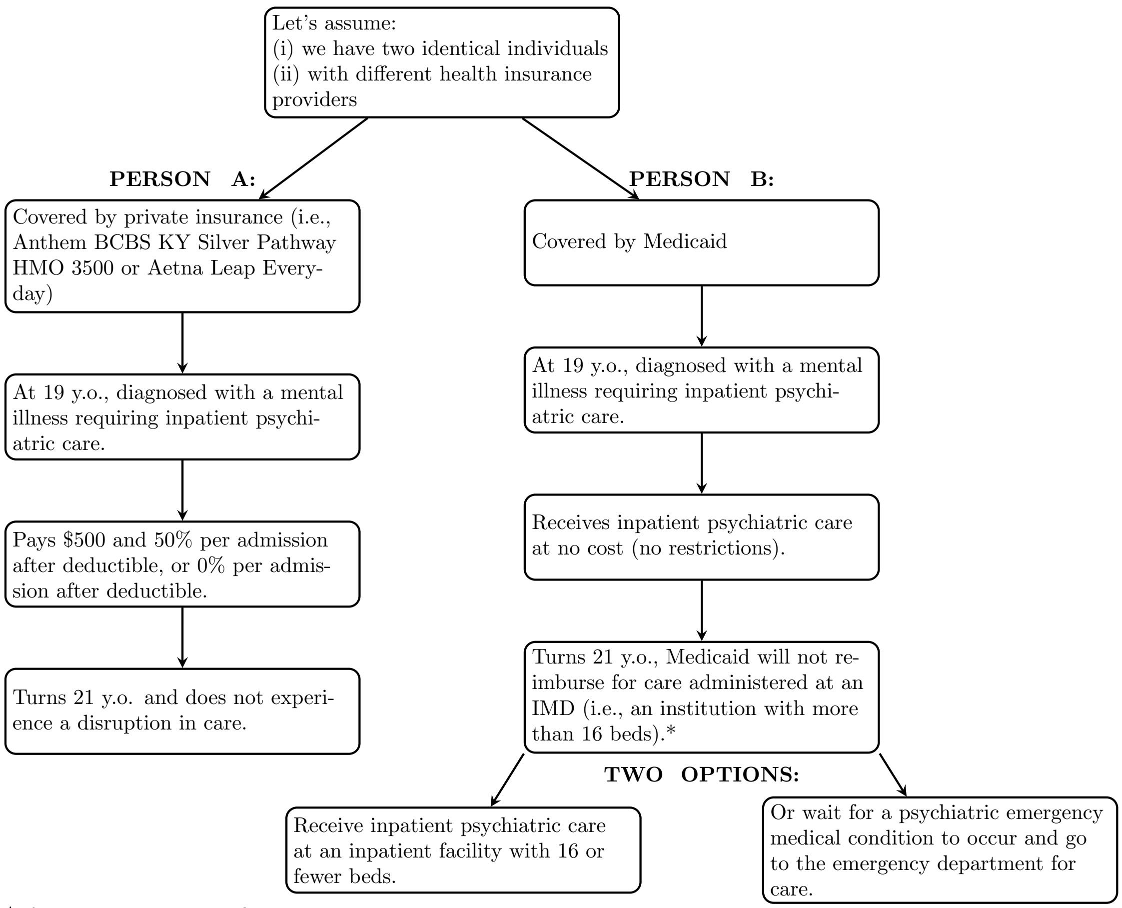
One out of every five adults in the United States has a mental illness, and according to mental health advocates and researchers, each year many do not receive medically necessary treatment (Fuller et al., 2016; Bose et al., 2016). Previous research has observed that when traditional treatment options are not available (e.g., inpatient psychiatric care), individuals experiencing symptoms associated with acute mental illness could turn to the emergency department for care (Torrey et al., 2012; Fuller et al., 2016; Szabo, 2014). If the emergency department is not equipped to stabilize their psychiatric symptoms, they could be boarded in either a private room or hallway until an inpatient psychiatric bed becomes available (Fuller et al., 2016; Torrey et al., 2012). It has been observed that this practice negatively affects the care given to emergency department patients (Nicks and Manthey, 2012; Richards et al., 2014).

Currently, there is no causal evidence regarding the effect of reduced insurance coverage for inpatient psychiatric care on emergency department utilization. I contribute to this literature by providing valuable causal evidence for the effect of reduced insurance coverage on emergency department utilization. Specifically, I exploit Medicaid's IMD exclusion to study the effect of reduced coverage for inpatient psychiatric care on emergency department visits. Using a "fuzzy" regression discontinuity framework, I find that reduced coverage for inpatient psychiatric care increases restricted mental health emergency visits (i.e., excluding alcohol-related visits) by 11 percent for Medicaid-eligible men. I also find that reduced coverage for inpatient psychiatric care increases primary mental health emergency department visits by 43 percent for Medicaid-eligible men. For Medicaid-eligible women, I do not observe a statistically significant effect for either outcome.

Given the above, the implications of my findings are twofold. First, my results provide evidence in support of the claim that untreated mental illness increases emergency department utilization. Second, we can interpret this increase in emergency department use as an external cost of reduced insurance coverage for inpatient psychiatric care. More importantly, the sum of the costs associated with increased emergency department use has the potential to exceed the cost of inpatient psychiatric care.

3.8 Tables & Figures

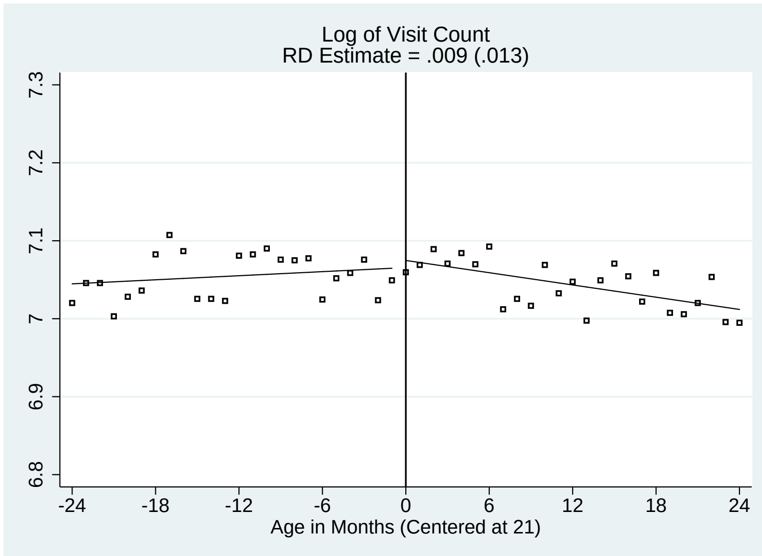
Figure 3.1: Flowchart of Mental Health Care by Insurance Provider



Note: *If a Medicaid beneficiary was receiving inpatient psychiatric care at an IMD when they turned 21, then they can complete their treatment at the IMD or leave the IMD once they turn 22 (whichever comes first).

3.8.1 Smoothness Check

Figure 3.2: Smoothness of Non-Deferable Diagnoses at the Threshold



Notes: The dependent variable is the logged count of non-deferable emergency department visits. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to age 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Table 3.1: Primary Mental Illness Diagnosis Categories (Top Ten)

Primary Diagnosis
Anxiety Disorders (300)
[26.95%]
Nondependent Abuse of Drugs (305)
[24.56%]
Depressive Disorders (311)
[11.76%]
Episodic Mood Disorders (296)
[10.07%]
Drug-Induced Mental Disorders (292)
[4.47%]
Special Symptoms or Syndromes Not Classified Elsewhere (307)
[3.19%]
Schizophrenic Disorders (295)
[3.15%]
Other Nonorganic Psychoses (298)
[3.02%]
Drug Dependence (304)
[2.50%]
Adjustment Reaction (309)
[2.50%]

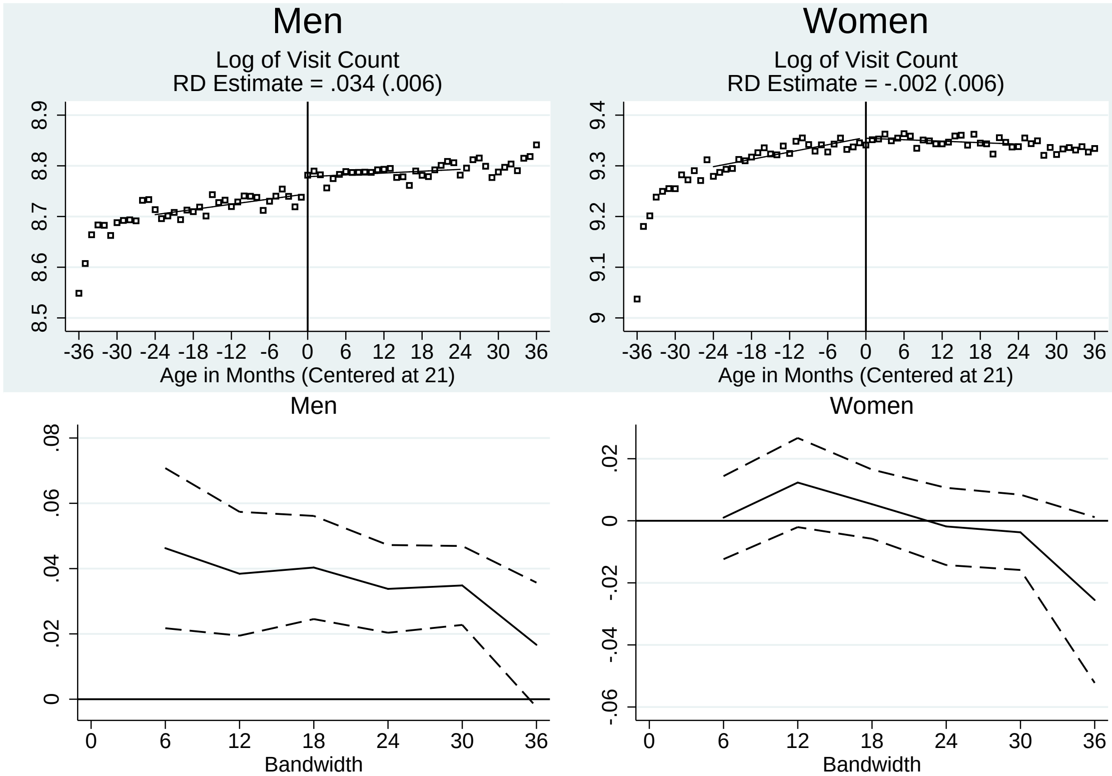
Note: This table reports the 10 most common mental illness diagnosis codes for Medicaid-eligible individuals between the ages of 19 and 23, inclusive. The diagnosis codes are reported in parenthesis, and frequencies are reported in brackets. The diagnosis codes not listed in bold text are excluded when defining restricted mental health emergency department visits (i.e., 291, 303, 3050) and primary mental health emergency department visits (i.e., 291, 292, 303, 304, and 305). Data source: Emergency department and inpatient discharge records from the Healthcare Cost and Utilization Project.

Table 3.2: Descriptive Statistics

	Pooled			Mental Health			Non-Mental Health		
	All	Male	Female	All	Male	Female	All	Male	Female
Female	0.609	–	–	0.534	–	–	0.622	–	–
	(0.487)	–	–	(0.498)	–	–	(0.484)	–	–
White	0.533	0.533	0.533	0.650	0.633	0.664	0.513	0.511	0.514
	(0.498)	(0.498)	(0.498)	(0.476)	(0.481)	(0.472)	(0.499)	(0.499)	(0.499)
Black	0.071	0.063	0.075	0.073	0.069	0.077	0.070	0.062	0.075
	(0.257)	(0.243)	(0.264)	(0.261)	(0.254)	(0.267)	(0.256)	(0.241)	(0.264)
Hispanic	0.077	0.076	0.078	0.049	0.059	0.041	0.082	0.080	0.083
	(0.267)	(0.265)	(0.268)	(0.217)	(0.235)	(0.199)	(0.274)	(0.271)	(0.276)
Age	21.008	21.019	21.001	21.063	21.056	21.070	20.998	21.011	21.990
	(1.174)	(1.174)	(1.174)	(1.169)	(1.167)	(1.170)	(1.175)	(1.176)	(1.174)
Arizona	0.523	0.531	0.518	0.411	0.456	0.389	0.543	0.551	0.537
	(0.499)	(0.499)	(0.499)	(0.492)	(0.495)	(0.487)	(0.498)	(0.497)	(0.498)
Kentucky	0.476	0.468	0.481	0.588	0.563	0.610	0.456	0.448	0.462
	(0.499)	(0.499)	(0.499)	(0.492)	(0.495)	(0.487)	(0.498)	(0.497)	(0.498)
Sample Size	1,409,323	551,045	858,278	211,260	98,423	112,837	1,198,063	452,622	745,441
Medicaid	0.318	0.198	0.395	0.303	0.227	0.370	0.320	0.191	0.399
	(0.465)	(0.398)	(0.488)	(0.459)	(0.419)	(0.482)	(0.466)	(0.393)	(0.489)
Private Insurance	0.272	0.294	0.258	0.223	0.231	0.215	0.281	0.308	0.265
	(0.445)	(0.455)	(0.437)	(0.416)	(0.421)	(0.411)	(0.449)	(0.461)	(0.441)
Self-Pay	0.297	0.366	0.253	0.355	0.404	0.311	0.287	0.371	0.254
	(0.457)	(0.481)	(0.434)	(0.478)	(0.496)	(0.463)	(0.452)	0.479)	(0.429)
Sample Size	1,409,258	551,012	858,246	211,253	98,419	112,834	1,198,005	452,593	745,412

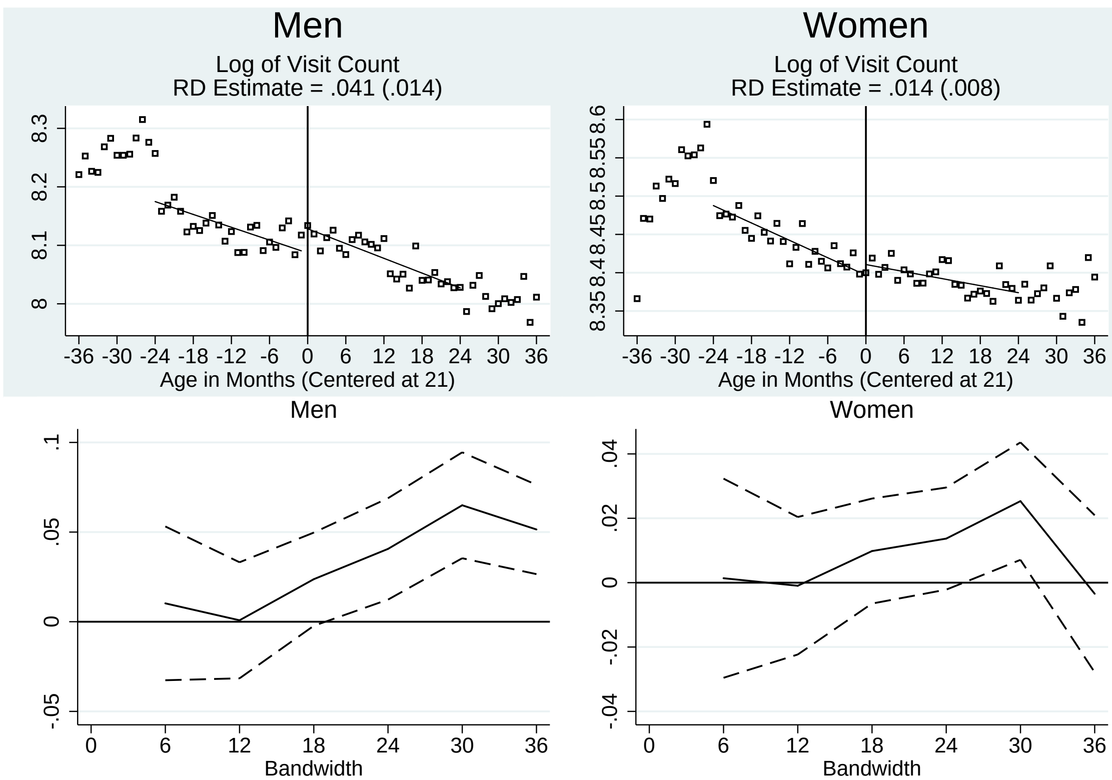
Notes: Standard deviations are reported in parenthesis. *Mental Health* represents mental health visits using the definition that excludes the ICD-9-CM codes used in Carpenter and Dobkin (2017) (i.e., excludes alcohol-related visits). Data source: Emergency department and inpatient discharge records from the Healthcare Cost and Utilization Project.

Figure 3.3: Emergency Department Visits - Medicaid-Eligible



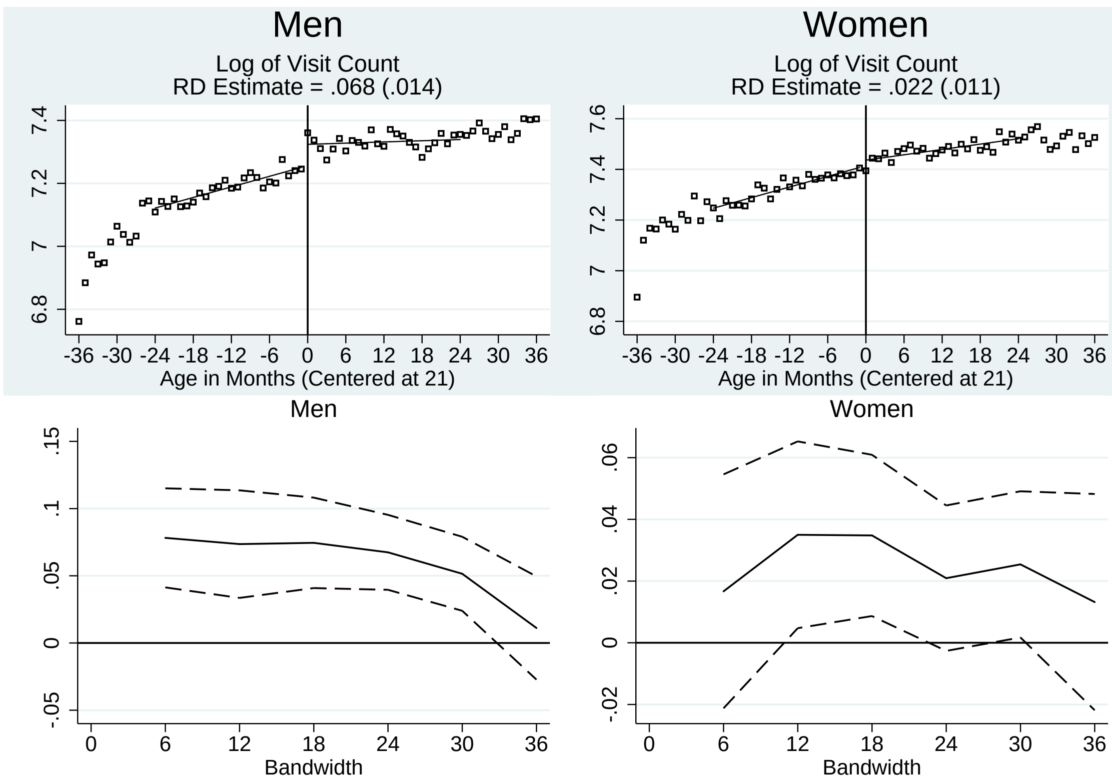
Notes: The dependent variable is the logged count of emergency department visits for Medicaid-eligible men and women (i.e., the expected payer is either Medicaid or self-pay). Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.4: Emergency Department Visits - Private Insurance



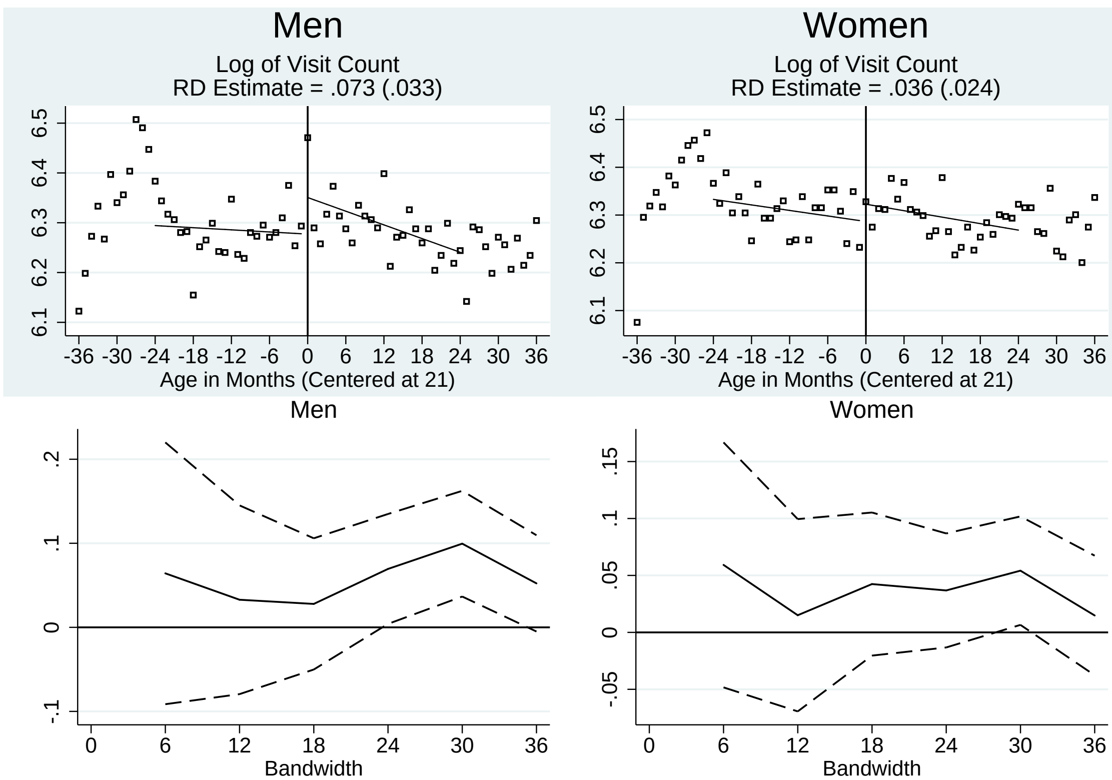
Notes: The dependent variable is the logged count of emergency department visits for men and women when the expected payer is private insurance. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.5: Mental Health Emergency Department Visits - Medicaid-Eligible



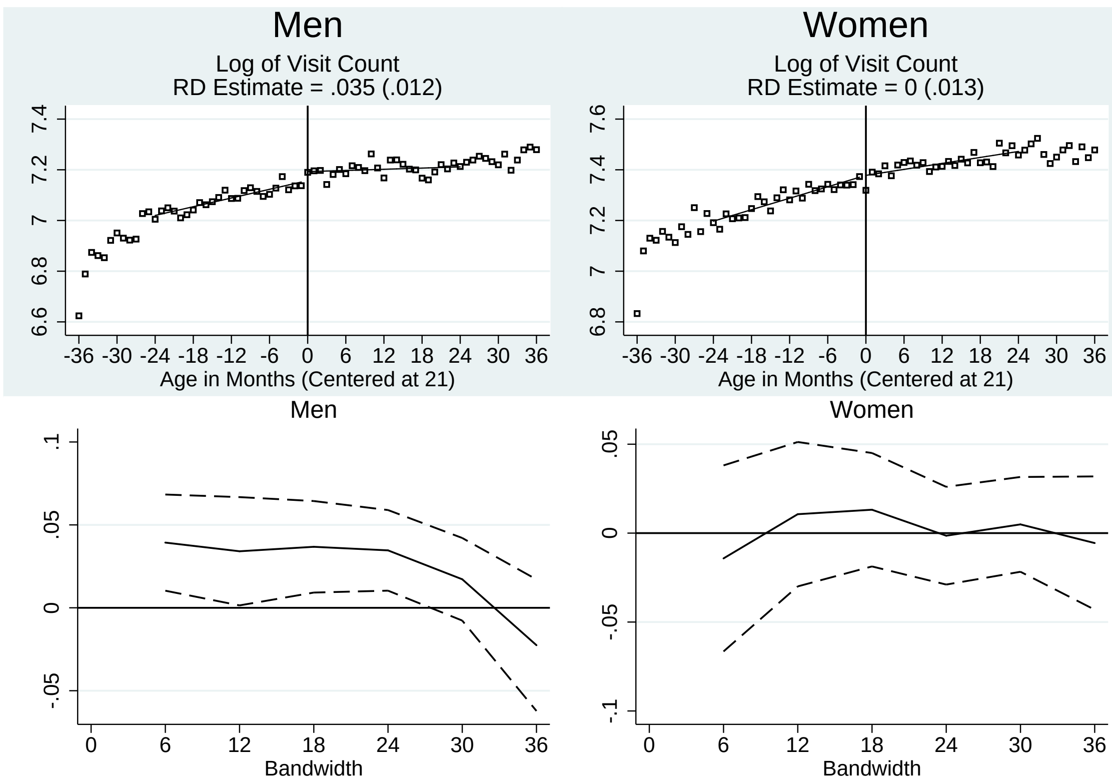
Notes: The dependent variable is the logged count of mental health emergency department visits for Medicaid-eligible men and women (i.e., the expected payer is either Medicaid or self-pay). Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.6: Mental Health Emergency Department Visits - Private Insurance



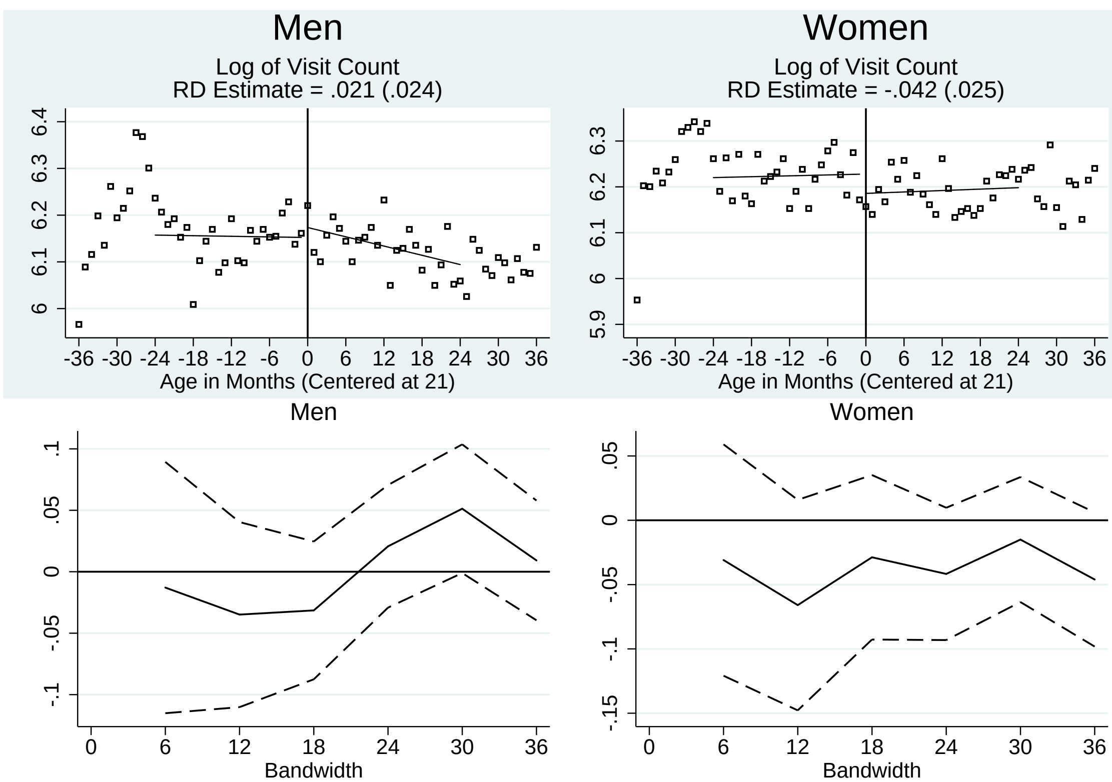
Notes: The dependent variable is the logged count of mental health emergency department visits for men and women when the expected payer is private insurance. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.7: Restricted Mental Health Emergency Department Visits - Medicaid-Eligible



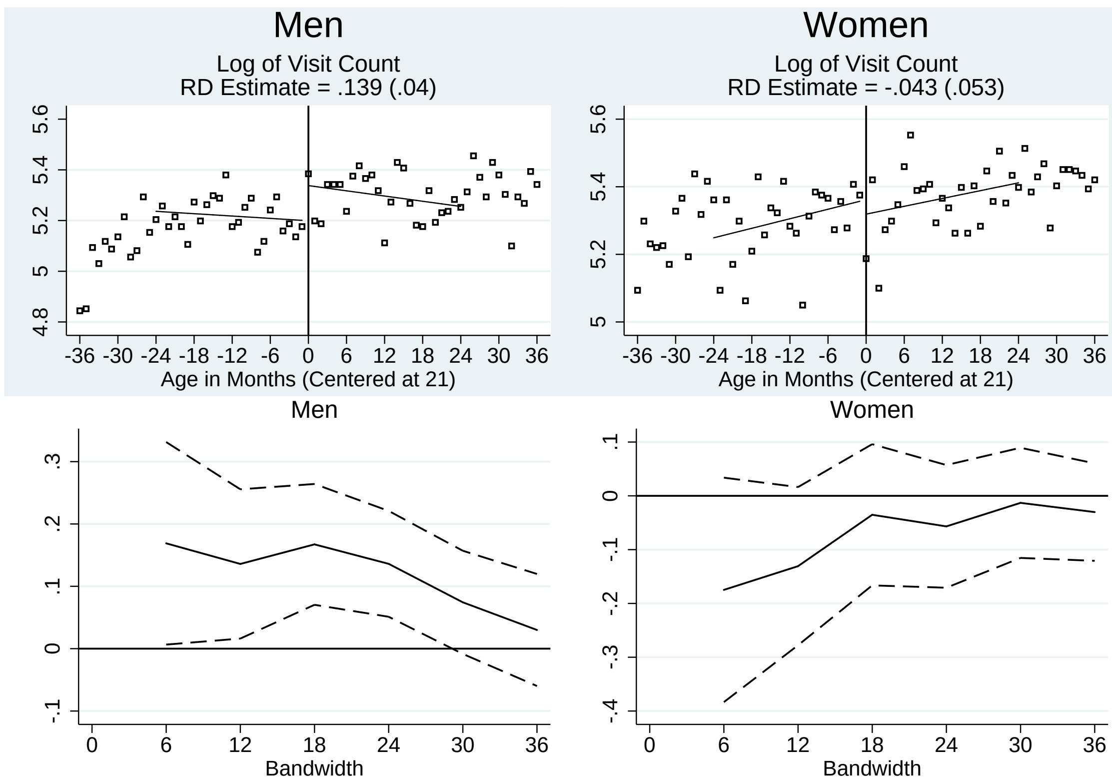
Notes: The dependent variable is the logged count of mental health emergency department visits (excluding alcohol) for Medicaid-eligible men and women (i.e., the expected payer is either Medicaid or self-pay). Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.8: Restricted Mental Health Emergency Department Visits - Private Insurance



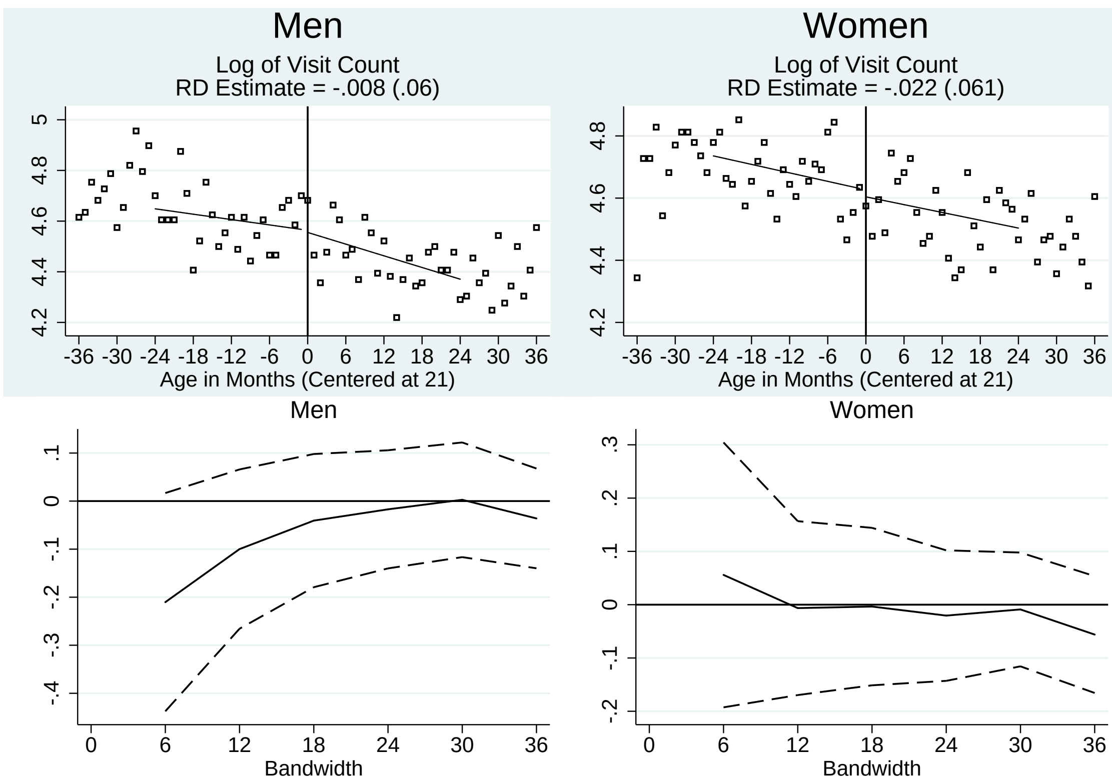
Notes: The dependent variable is the logged count of mental health emergency department visits (excluding alcohol) for men and women when the expected payer is private insurance. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.9: Primary Mental Health Emergency Department Visits - Medicaid-Eligible



Notes: The dependent variable is the logged count of primary mental health emergency department visits (i.e., based on the primary diagnosis code only and excluding codes for drugs and alcohol) for Medicaid-eligible men and women (i.e., the expected payer is either Medicaid or self-pay). Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Figure 3.10: Primary Mental Health Emergency Department Visits - Private Insurance



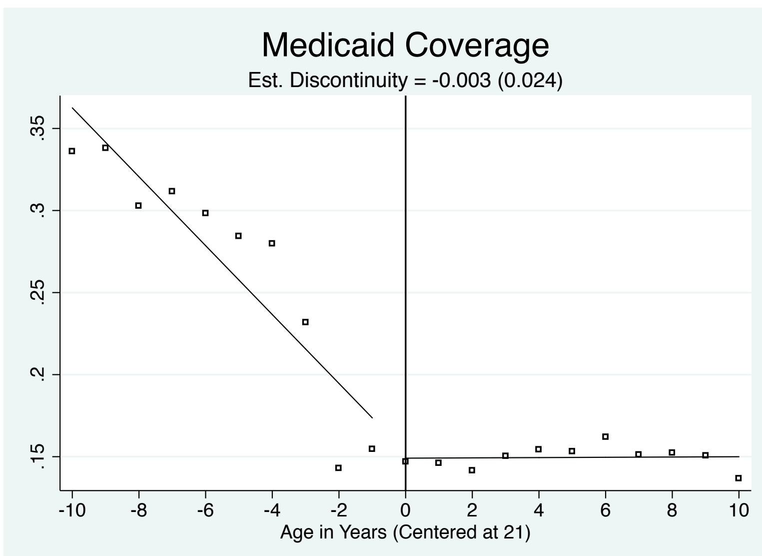
Notes: The dependent variable is the logged count of primary mental health emergency department visits (i.e., based on the primary diagnosis code only and excluding codes for drugs and alcohol) for men and women when the expected payer is private insurance. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: Healthcare Cost and Utilization Project.

Table 3.3: Reduced Form Estimates for the Logged Count of Emergency Department Visits

	Men		Women	
	Rate (< 21)	RD at 21	Rate (< 21)	RD at 21
	(1)	(2)	(3)	(4)
Medicaid-Eligible				
All Admissions	4562.345	0.034***	8405.234	-0.002
		(0.006)		(0.006)
Mental Health	1027.586	0.068***	1202.669	0.022*
		(0.014)		(0.011)
Mental Health - Excluding Alcohol	930.979	0.035***	1159.432	-0.001
		(0.012)		(0.013)
Mental Health - Primary Diagnosis	131.301	0.139***	154.484	-0.043
		(0.040)		(0.053)
Private Insurance				
All Admissions	1879.099	0.041***	2561.379	0.014*
		(0.014)		(0.008)
Mental Health	307.612	0.073***	310.205	0.036
		(0.033)		(0.024)
Mental Health - Excluding Alcohol	271.489	0.021	292.560	-0.042
		(0.024)		(0.025)
Mental Health - Primary Diagnosis	55.242	-0.008	58.615	-0.022
		(0.060)		(0.061)
Sample Size		49		49

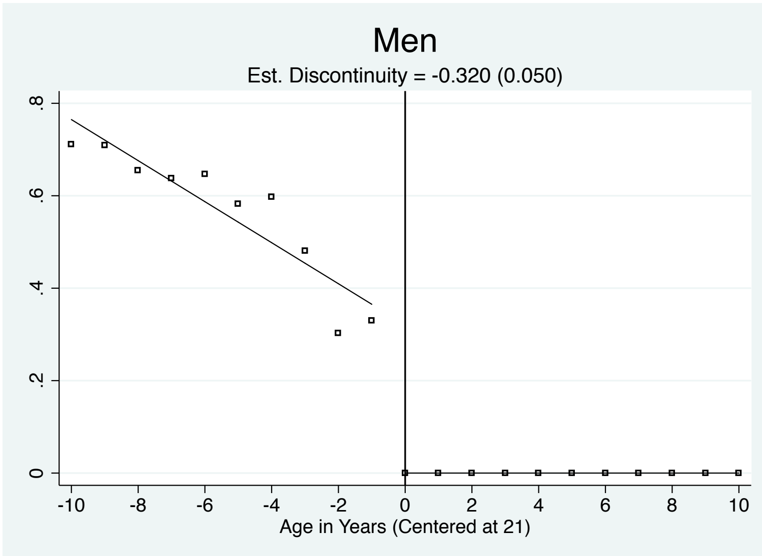
Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The dependent variable is the logged count of emergency department visits. Each entry contains the results from a single local linear regression predicting the change in the outcome variable at age 21. Robust standard errors are reported in parenthesis. The estimates presented in this table correspond to the fitted jump (i.e., β_1 from equation (3)) at age 21. The rate provides an estimate for the rate of emergency department visits just to the left of the threshold (i.e., $AGE < 0$). Data source: Emergency department and inpatient discharge records from the Healthcare Cost and Utilization Project.

Figure 3.11: Medicaid Coverage



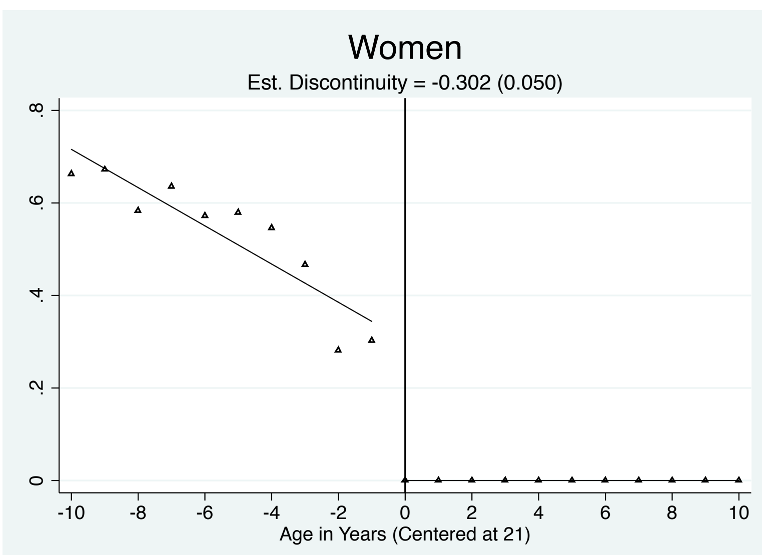
Notes: The dependent variable is the fraction of Medicaid beneficiaries. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: American Community Survey, Arizona and Kentucky, 2008 through 2011.

Figure 3.12: Expected Relationship Between Coverage & Age (Medicaid-Eligible)



Notes: This figure illustrates my back-of-the-envelope first-stage. Each point represents the fraction of people (i.e., Medicaid beneficiaries and uninsured/self-pay) with inpatient psychiatric care covered by Medicaid. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: American Community Survey, Arizona and Kentucky, 2008 through 2011.

Figure 3.13: Expected Relationship Between Coverage & Age (Medicaid-Eligible)



Notes: This figure illustrates my back-of-the-envelope first-stage. Each point represents the fraction of people (i.e., Medicaid beneficiaries and uninsured/self-pay) with inpatient psychiatric care covered by Medicaid. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data source: American Community Survey, Arizona and Kentucky, 2008 through 2011.

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Appendix A

Supplementary Tables for Universal Preschool

Table A.1: Diagnosis Counts for Children Enrolled in IDEA Part B

	4 through 13	14 through 17	Total
Intellectual Disabilities	41,673	32,544	74217
Speech or Language Impairments	147,857	4,069	151,926
Emotional Disturbance	39,453	30,449	69,902
Other Health Impairments	64,301	37,844	102,145
Specific Learning Disabilities	159,651	118,338	277,989
Multiple Disabilities	2,713	1,477	4,190

Notes: Each cell contains a count, by age group, for children served under IDEA part B in Georgia and Oklahoma. These counts are for the 2005, 2007, and 2011 academic years. Some diagnoses have been excluded from the above table (e.g., autism, visual impairments, etc.). Data source: U.S. Department of Education.

Table A.2: Estimated Effects of UPK on Preschool Enrollment (Robustness Check)

	Pooled	H.S. or Less			More than H.S.		
	Full Sample (N = 37,167) (1)	*Full Sample (N = 37,167) (2)*	*Age 4-13 (N = 26,010) (3)*	*Age 14-17 (N = 24,227) (4)*	*Full Sample (N = 37,167) (5)*	*Age 4-13 (N = 26,010) (6)*	*Age 14-17 (N = 24,227) (7)*
A: Enrolled in Preshool							
Universal Preschool	0.174***	0.198***	0.316***	0.224***	0.157***	0.178***	0.134***
	(0.012)	(0.018)	(0.025)	(0.018)	(0.010)	(0.014)	(0.017)
B: Enrolled in Public Preschool							
Universal Preschool	0.209***	0.197***	0.318***	0.167***	0.218***	0.352***	0.239***
	(0.024)	(0.017)	(0.020)	(0.016)	(0.028)	(0.010)	(0.044)
C: Enrolled in Private Preschool							
Universal Preschool	-0.035	0.001	-0.002	0.057***	-0.060*	-0.174***	-0.106***
	(0.029)	(0.015)	(0.015)	(0.016)	(0.034)	(0.013)	(0.030)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains first-stage estimates for the sample as a whole and highest level of household education sub-groups. Each cell contains the results from a single regression predicting the effect of UPK availability on preschool participation. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included in the first-stage regressions. Standard errors are clustered on state and are reported in parenthesis. New York, West Virginia, the District of Columbia and Florida have been excluded from control states. The weighted specification uses sampling weights provided by the CPS. Data Source: The October Supplement of the Current Population Survey.

Table A.3: Effect of Preschool Participation on Diagnosis Probabilities & Grade Retention (Robustness Check)

	Pooled	H.S. or Less			More than H.S.		
	Full Sample (1)	*Full Sample* (2)	*Age 4-13* (3)	*Age 14-17* (4)	*Full Sample* (5)	*Age 4-13* (6)	*Age 14-17* (7)
Learning Disability	-0.022	-0.041	0.015	-0.057	0.010	-0.340***	0.051
N = 232,459	(0.046)	(0.043)	(0.031)	(0.118)	(0.055)	(0.020)	(0.081)
ADD/ADHD	-0.002	-0.019	0.136***	0.056	0.022	-0.122***	-0.054
N = 232,391	(0.025)	(0.043)	(0.036)	(0.068)	(0.036)	(0.040)	(0.040)
Behavioral Problems	-0.020	-0.065***	-0.008	0.003	0.038	-0.022	0.011
N = 232,636	(0.013)	(0.024)	(0.040)	(0.032)	(0.030)	(0.022)	(0.029)
Special Therapy	-0.009	-0.116*	0.162***	-0.073**	0.116***	-0.067**	-0.106
N = 232,738	(0.049)	(0.070)	(0.024)	(0.029)	(0.037)	(0.029)	(0.103)
Grade Retention	-0.007	-0.019	-0.352***	-0.133**	0.025	0.019	0.134
N = 203,463	(0.033)	(0.041)	(0.048)	(0.057)	(0.063)	(0.032)	(0.084)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. Each entry contains the results from a single regression predicting the effect of preschool participation on ever being diagnosed with the conditions listed when the child was between the ages of 4 and 17, or 6 and 17 for ever repeated a grade. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included. Standard errors are clustered on state and are reported in parenthesis. New York, West Virginia, the District of Columbia and Florida are excluded from control states. The weighted specification uses sampling weights provided by the CPS and the NSCH. The first stage used is *Enrolled in Preschool* (i.e., the combination of public and private preschool enrollment). Data source: Centers for Disease Control and Prevention, National Center for Health Statistics, State and Local Area Integrated Telephone Survey, National Survey of Children's Health, 2003, 2007, and 2011. The October Supplement of the Current Population Survey.

Table A.4: Estimated Effects of UPK on Preschool Enrollment (Misclassification Check)

	Pooled	H.S. or Less			More than H.S.		
	Full Sample *(N = 38,163)* *(1)*	*Full Sample* *(N = 38,163)* *(2)*	*Age 4-13* *(N = 29,421)* *(3)*	*Age 14-17* *(N = 23,765)* *(4)*	*Full Sample* *(N = 38,163)* *(5)*	*Age 4-13* *(N = 29,421)* *(6)*	*Age 14-17* *(N = 23,765)* *(7)*
A: Enrolled in Preshool							
Universal Preschool	0.200***	0.223***	0.297***	0.252***	0.185***	0.175***	0.165***
	(0.011)	(0.006)	(0.023)	(0.015)	(0.009)	(0.015)	(0.027)
B: Enrolled in Public Preschool							
Universal Preschool	0.235***	0.225***	0.304***	0.195***	0.241***	0.347***	0.268***
	(0.029)	(0.019)	(0.019)	(0.018)	(0.034)	(0.011)	(0.057)
C: Enrolled in Private Preschool							
Universal Preschool	-0.035	-0.002	-0.007	0.058***	-0.056	-0.172***	-0.103***
	(0.032)	(0.018)	(0.013)	(0.012)	(0.039)	(0.011)	(0.034)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains first-stage estimates for the sample as a whole and highest level of household education sub-groups. Each cell contains the results from a single regression predicting the effect of UPK availability on preschool participation. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included in the first-stage regressions. Standard errors are clustered on state and are reported in parenthesis. In an attempt to correct for misclassification of treatment status, year of birth cohorts 1990 and 1991 are excluded from the analysis. The weighted specification uses sampling weights provided by the CPS. Data Source: The October Supplement of the Current Population Survey.

Table A.5: Effect of Preschool Participation on Diagnosis Probabilities & Grade Retention (Misclassification Check)

	Pooled	H.S. or Less			More than H.S.		
	Full Sample *(1)*	*Full Sample* *(2)*	*Age 4-13* *(3)*	*Age 14-17* *(4)*	*Full Sample* *(5)*	*Age 4-13* *(6)*	*Age 14-17* *(7)*
Learning Disability	-0.055**	-0.091***	0.031	-0.021	-0.008	-0.353***	-0.050
N = 230,392	(0.019)	(0.026)	(0.033)	(0.075)	(0.027)	(0.019)	(0.092)
ADD/ADHD	-0.047	-0.076	0.139***	0.104**	-0.012	-0.150***	-0.144**
N = 230,324	(0.049)	(0.059)	(0.034)	(0.050)	(0.054)	(0.043)	(0.073)
Behavioral Problems	-0.013	-0.084***	0.001	0.070	0.072***	-0.036	-0.071***
N = 230,573	(0.014)	(0.024)	(0.037)	(0.044)	(0.025)	(0.023)	(0.025)
Special Therapy	-0.006	-0.117**	0.175**	-0.028	0.120***	-0.080***	-0.082
N = 230,684	(0.036)	(0.058)	(0.023)	(0.025)	(0.025)	(0.029)	(0.089)
Grade Retention	0.032	0.001	-0.376***	0.025	0.076	0.013	0.071
N = 198,165	(0.039)	(0.041)	(0.044)	(0.053)	(0.066)	(0.029)	(0.096)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. Each entry contains the results from a single regression predicting the effect of preschool participation on ever being diagnosed with the conditions listed when the child was between the ages of 4 and 17, or 6 and 17 for ever repeated a grade. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included. Standard errors are clustered on state and are reported in parenthesis. In an attempt to correct for misclassification of treatment status, year of birth cohorts 1990 and 1991 are excluded from the analysis. The weighted specification uses sampling weights provided by the CPS and the NSCH. The first stage used is *Enrolled in Preschool* (i.e., the combination of public and private preschool enrollment). Data source: Centers for Disease Control and Prevention, National Center for Health Statistics, State and Local Area Integrated Telephone Survey, National Survey of Children's Health, 2003, 2007, and 2011. The October Supplement of the Current Population Survey.

Table A.6: Estimated Effects of UPK on Preschool Enrollment (Highest Level of Maternal Education)

	Pooled	H.S. or Less			More than H.S.		
	Full Sample *(N = 28,977)* *(1)*	*Full Sample* *(N = 28,977)* *(2)*	*Age 4-13* *(N = 18,495)* *(3)*	*Age 14-17* *(N = 17,605)* *(4)*	*Full Sample* *(N = 28,977)* *(5)*	*Age 4-13* *(N = 18,495)* *(6)*	*Age 14-17* *(N = 17,605)* *(7)*
A: Enrolled in Preshool							
Universal Preschool	0.125***	0.132***	0.169***	0.171***	0.086***	0.081***	0.075***
	(0.016)	(0.034)	(0.015)	(0.070)	(0.025)	(0.008)	(0.012)
B: Enrolled in Public Preschool							
Universal Preschool	0.194***	0.172***	0.211***	0.122***	0.183***	0.305***	0.249***
	(0.044)	(0.045)	(0.014)	(0.071)	(0.060)	(0.006)	(0.022)
C: Enrolled in Private Preschool							
Universal Preschool	-0.068*	-0.041*	-0.042***	-0.049***	-0.097**	-0.224***	-0.174***
	(0.040)	(0.021)	(0.007)	(0.012)	(0.040)	(0.008)	(0.022)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains first-stage estimates for the sample as a whole and highest level of maternal education groups. Each cell contains the results from a single regression predicting the effect of UPK availability on preschool participation. The following controls are used: race, gender, highest level of household education, and household size. State and year of birth FE are also included in the first-stage regressions. Standard errors are clustered on state and are reported in parenthesis. The weighted specification uses sampling weights provided by the CPS. Data Source: The October Supplement of the Current Population Survey.

Table A.7: Effect of Preschool Participation on Diagnosis Probabilities & Grade Retention (Highest Level of Maternal Education)

	Pooled	H.S. or Less			More than H.S.		
	Full Sample *(1)*	*Full Sample* *(2)*	*Age 4-13* *(3)*	*Age 14-17* *(4)*	*Full Sample* *(5)*	*Age 4-13* *(6)*	*Age 14-17* *(7)*
Learning Disability	0.039	0.062	0.520	-0.141	-0.013	0.245*	0.495***
N = 147,538	(0.214)	(0.161)	(0.421)	(0.244)	(0.343)	(0.130)	(0.176)
ADD/ADHD	0.122*	-0.055	0.470	-0.064	0.159	0.377***	0.168
N = 147,569	(0.074)	(0.065)	(0.310)	(0.166)	(0.104)	(0.133)	(0.279)
Behavioral Problems	0.040	-0.103	0.308	-0.001	0.218***	0.289***	0.238
N = 147,635	(0.042)	(0.092)	(0.273)	(0.301)	(0.083)	(0.082)	(0.281)
Special Therapy	0.076	-0.135	0.617	-0.178	0.284**	0.229*	0.176
N = 147,665	(0.119)	(0.161)	(0.695)	(0.265)	(0.121)	(0.125)	(0.112)
Grade Retention	-0.074	-0.157	-0.680	-0.795	-0.112	0.011	0.681***
N = 135,491	(0.206)	(0.269)	(0.564)	(0.735)	(0.402)	(0.116)	(0.144)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. Each entry contains the results from a single regression predicting the effect of preschool participation on ever being diagnosed with the conditions listed when the child was between the ages of 4 and 17, or 6 and 17 for ever repeated a grade. The following controls are used: race, gender, highest level of maternal education, and household size. State and year of birth FE are also included. Standard errors are clustered on state and are reported in parenthesis. The weighted specification uses sampling weights provided by the CPS and the NSCH. The first stage used is *Enrolled in Preschool* (i.e., the combination of public and private preschool enrollment). Data source: Centers for Disease Control and Prevention, National Center for Health Statistics, State and Local Area Integrated Telephone Survey, National Survey of Children's Health, 2003, 2007, and 2011. The October Supplement of the Current Population Survey.

Appendix B

Supplementary Table & Figure for Career Technical Education

Figure B.1: Sample Course Load for Air Conditioning Technology

SAMPLE: CAREER PATHWAY- Air-Conditioning Technology

KENTUCKY CAREER PATHWAY/PROGRAM OF STUDY 2015-2016

COLLEGE/UNIVERSITY:	College / State University	CLUSTER:	Construction
	KCTCS Community College	PATHWAY:	Domestic Air Conditioner & Furnace Installer / Refrigeration Mechanic
HIGH SCHOOL (S):	KY ATC/CTC/High School	PROGRAM:	Air Conditioning Technology

	GRADE	ENGLISH	MATH	SCIENCE	SOCIAL STUDIES	REQUIRED COURSES RECOMMENDED ELECTIVE COURSES OTHER ELECTIVE COURSES CAREER AND TECHNICAL EDUCATION COURSES			CREDENTIAL CERTIFICATE DIPLOMA DEGREE	SAMPLE OCCUPATIONS
SECONDARY	9	English I	Algebra I	Earth Space Science	World History	Health and PE	ACR 100 Refrigeration Fundamentals	ACR 130 Electrical Components		
SECONDARY	10	English II	Geometry	Biology I	World Civics	History and Appreciation of Fine Arts	ACR 170 Heat / Duct Design 480812	ARC 120 HVAC Electricity 48017		
SECONDARY	11	English III	Algebra II	Physics or Chemistry	U.S. History	Foreign Language	ARC 250 Cooling & Dehumidification	ACR 270 Heat Pump Application	NCCER HVAC Level 1	HVAC Assistant
SECONDARY	12	English IV	Math Elective	Computer Aided Drafting (elective)	World Geography	ACR 200 Commercial Refrigeration 470211	Internship (Air cond) 460883	Co-op 1 (air cond.) 460880		Refrigeration Mechanic
POSTSECONDARY	Year 13	ENG 101 Writing I	MT 110 Applied Mathematics	ASTR 104 Astronomy	College Chemistry	PSY 100 Intro Psychology	ARC 208 Chillers	Occupation Safety	Refrigeration Mechanic	Industry Apprenticeship
POSTSECONDARY	Year 14	ENG 200 Intro/Literature	Math 200	WLD 221 Certification Lab	HIS 108 US History	CAR 140 Surveying & Foundations	Materials Science	ACR 207 Commercial HVAC Systems	Associates Degree in Applied Science	HVAC Foreman / Manager
POSTSECONDARY	Year 15	ENG 200 Intro/Literature	MAT 250 CALCULUS	PHY 236 UNIV. PHYSICS I	CIV 102 WORLD CIV. II	PHY 195 METHODS OF ENG. PHYSICS	CIV 102 WORLD CIV. II	CAD 200 Intermediate Computer Aided design		
	Year 16	PHY 140 INTRO. COMPUTING APPS.	MAT 308 CALCULUS II	PHY 259 STATICS	MAT 309 CALCULUS III	MAT 411 DIFFERENTIALS EQTNS.	TECHNICAL ELECTIVE	PHY 330 DYNAMICS		
	Year 17	PHY 344 FLUID MECHANICS	PHY 370 INTRO. MODERN PHYSICS	CHE 201 GEN. COLLEGE CHEM. I	HUM 211 HUMANITIES	ITD 102 CAD APPLICATIONS	PHY 346 HEAT TRANSFER	PHY 375 MATERIALS SCIENCE	PHY 390 ENGR. MEASUREMENT	TECH.ELECTIVE
	Year 17	PHY 359 MECHANICS OF MATERIALS	PHY 470 OPTICS	PHY 498 SENIOR ENGR. DESIGN I	ECO 231 PRINC. OF MICROECONOMICS	PHY 499 SENIOR ENGR. DESIGN II	TECHNICAL ELECTIVE	MAT DEPTH ELECTIVE	FREE ELECTIVE	HUM/FA ELEC.
								BACHELORS DEGREE ENGINEERING	Western Kentucky UNIVERSITY	ENGINEER

	Other Elective Courses
Funded by the U. S. Department of Education	Career and Technical Education Courses
(V051B020001)	Credit-Based Transition Programs (e.g. Dual/Concurrent Enrollment, Articulated Courses, 2+2+2)
Revised Jan. 2005	(♦=High School to Comm. College) (• =Com. College to 4-Yr Institution) (¤ = Opportunity to test out)
October. 2006-CTE/Kentucky	Mandatory Assessments, Advising, and Additional Preparation
CCTI College and Career Transitions Initiative	TECHNICAL COLLEGE CREDIT GIVEN THROUGH THE KCTCS DUAL ENROLLMENT PROGRAM
	Certificate given through the Warren County Area Technology Center
	Degree given through the Bowling Green Technical College KCTCS
	DEGREE GIVEN THROUGH THE MURRAY STATE UNIVERSITY

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This sample course load comes from the program of studies for Air Conditioning Technology, see Borders (2015).

Table B.1: 2SLS Estimated Effects of Participation in Career Technical Education on Post-Secondary Outcomes - Years Following H.S.

	Attend Some College			Two-Year College			Four-Year College		
	Pooled	Female	Male	Pooled	Female	Male	Pooled	Female	Male
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
Total CTE Courses	0.140***	0.165***	0.122***	0.058***	0.070***	0.050***	0.082***	0.096***	0.071***
	(0.014)	(0.021)	(0.011)	(0.008)	(0.011)	(0.007)	(0.011)	(0.016)	(0.009)
N:	286,724	140,005	146,719	286,724	140,005	146,719	286,724	140,005	146,719

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The table contains the 2SLS estimates for the effect of participation in career technical education on the probability of college attendance, attending a two-year college, and attending a four-year college in the years following the completion of high school. The following controls are used: white non-Hispanic, black non-Hispanic, age, ever enrolled in special education, and gender. Dummy variables for district, and freshmen-year cohort are also included. Standard errors are clustered on district and are reported in parenthesis. Data Source: The Kentucky Center for Education and Workforce Statistics.

Appendix C

Supplementary Tables & Figures for Inpatient Psychiatric Care

Table C.1: Reduced Form Estimates for Emergency Department Visits per 10,000 Person-Years

	Men		Women	
	Rate (< 21)	RD at 21	Rate (< 21)	RD at 21
	(1)	(2)	(3)	(4)
Medicaid-Eligible				
All Admissions	4562.345	185.638***	8405.234	40.616
		(33.153)		(54.245)
Mental Health	1027.586	80.837***	1202.669	36.709**
		(15.093)		(13.746)
Mental Health - Excluding Alcohol	930.979	41.058***	1159.432	9.360
		(11.366)		(15.229)
Mental Health - Primary Diagnosis	131.301	20.148***	154.484	-6.528
		(5.924)		(8.235)
Sample Size		49		49

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The dependent variable is a rate per 10,000 person-years. Each entry contains the results from a single local linear regression predicting the change in the outcome variable at age 21. Robust standard errors are reported in parenthesis. The estimates presented in this Table correspond to the fitted jump (i.e., β_1 from equation (3)) at age 21. The constant provides an estimate for the rate of emergency department visits just to the left of the threshold (i.e., $AGE < 0$). Data source: Emergency department and inpatient discharge records from the Healthcare Cost and Utilization Project.

Table C.2: Reduced Form Estimates for Logged Count of Emergency Department Visits (with Controls)

	Men		Women	
	+ FE	+ Controls	+ FE	+ Controls
	(1)	(2)	(3)	(4)
Medicaid-Eligible				
All Admissions	0.034***	0.033***	-0.002***	-0.000***
	(0.002)	(0.002)	(0.002)	(0.002)
Mental Health	0.067***	0.067***	0.020***	0.022***
	(0.004)	(0.004)	(0.003)	(0.003)
Mental Health - Excluding Alcohol	0.035***	0.035***	-0.001***	-0.001***
	(0.003)	(0.004)	(0.004)	(0.004)
Mental Health - Primary Diagnosis	0.136***	0.139**	-0.057***	-0.058***
	(0.012)	(0.012)	(0.016)	(0.016)

Note: *** = statistically significant at the 1% level; ** = statistically significant at the 5% level; * = statistically significant at the 10% level. The dependent variable is the logged count of emergency department visits. Each entry contains the results from a single local linear regression predicting the change in the outcome variable at age 21. Robust standard errors are reported in parenthesis. The estimates presented in this Table correspond to the fitted jump (i.e., β_1 from equation (3)) at age 21. *+FE* is the baseline specification described in equation (3) plus birth month fixed effects. *+ Controls* is the baseline specification described in equation (3) plus birth month fixed effects and controls for race. Data source: Emergency department and inpatient discharge records from the Healthcare Cost and Utilization Project.

Figure C.1: Mental Health Policy in the United States

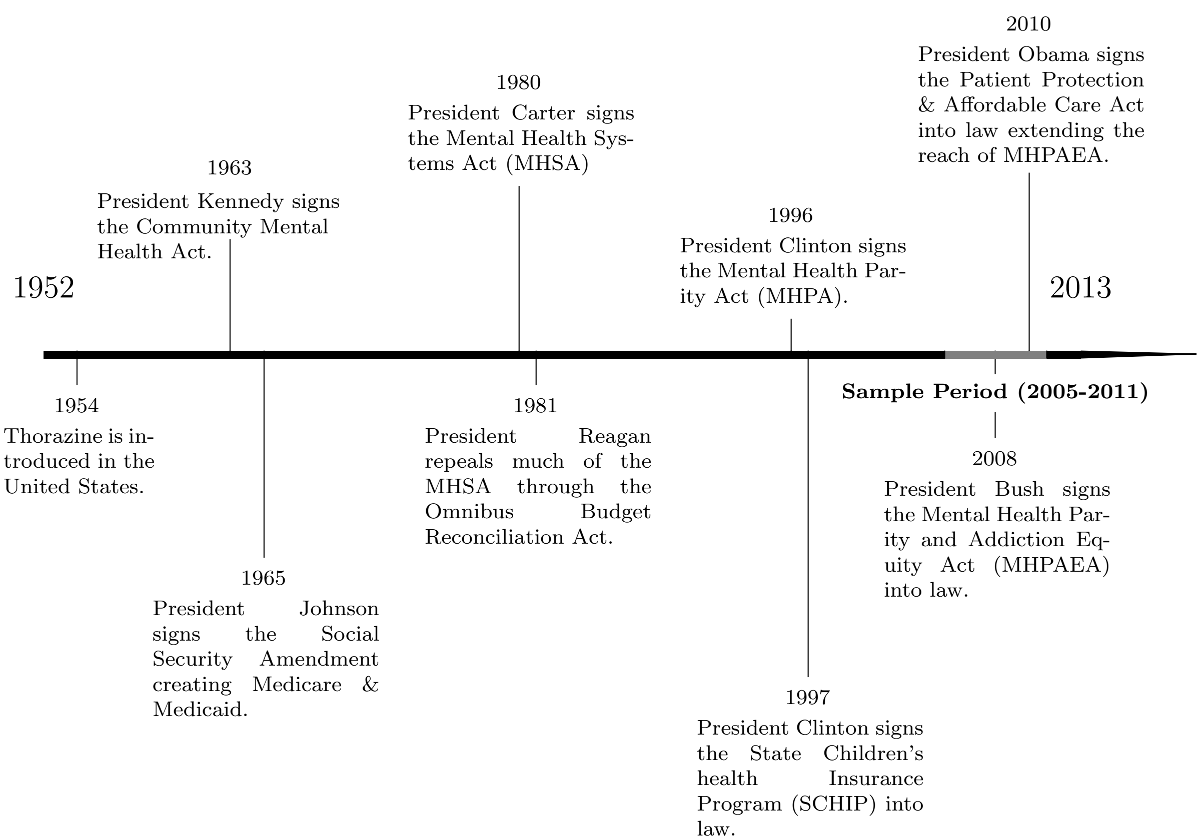
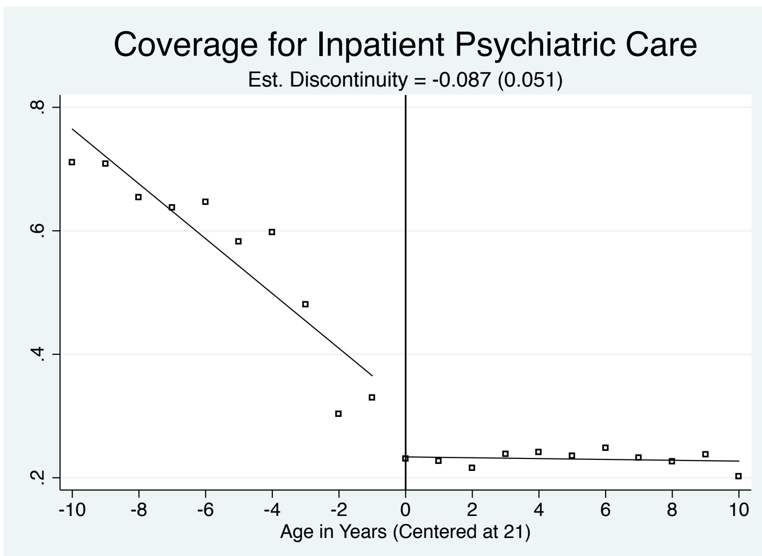
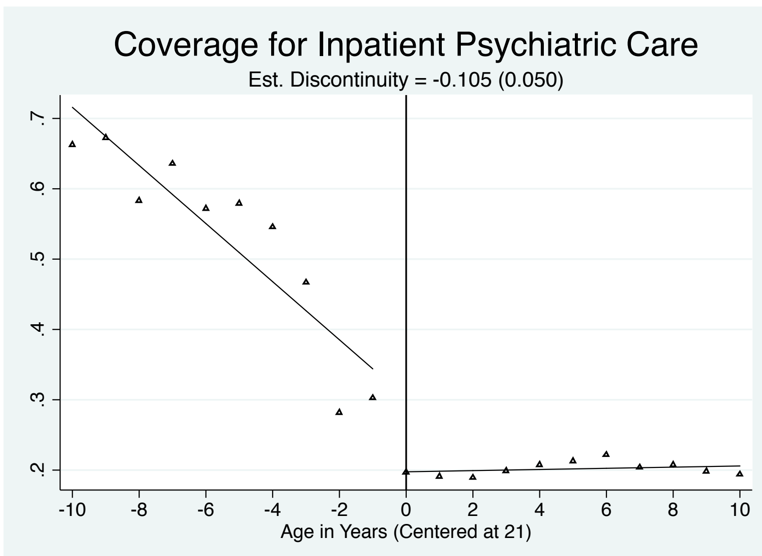


Figure C.2: First Stage Approximation (Men)



Notes: The dependent variable is the fraction of people (i.e., Medicaid beneficiaries or self-pay) with inpatient psychiatric care covered by Medicaid; where data from California is used to approximate access to an inpatient psychiatric hospital with 16 or fewer beds. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data sources: American Community Survey, and California's Office of Statewide Health Planning and Development Annual Survey of Hospitals.

Figure C.3: First Stage Approximation (Women)



Notes: The dependent variable is the fraction of people (i.e., Medicaid beneficiaries or self-pay) with inpatient psychiatric care covered by Medicaid; where data from California is used to approximate access to an inpatient psychiatric hospital with 16 or fewer beds. Age in months is relative to the age 21 threshold (i.e., age in months equal to zero corresponds to the age of 21 and zero months). Data sources: American Community Survey, and California's Office of Statewide Health Planning and Development Annual Survey of Hospitals.