# ABSTRACT

Title of dissertation: ESSAYS ON TARGETED PROGRAMS IN EDUCATION:

Brian Heath Witzen
Doctor of Philosophy, 2019

Dissertation directed by: Lesley J. Turner
Department of Economics

This dissertation examines three examples of education policy that affect students' decision-making at three different stages of the academic career.

In the first chapter, I examine how grant aid can affect the re-enrollment and graduation rates of bachelor's degree-seeking students. I use administrative data from the State of Maryland to study the state's largest need-based grant aid program using a regression discontinuity design. I find positive effects of grant receipt on re-enrollment beginning in the second year and a 10% increase in the rate of persistence to the fourth year, with similar-sized, but more imprecise effects on graduation within 5 years of entry.

In the second chapter, I study State Loan Repayment Programs which pay down a physician's medical school debt in exchange for a period of service in a health care provider shortage area. I gather data from individual states on the amounts that their programs offer over time and use changes in designations of health care provider shortage areas to implement a generalized differences-in-differences

strategy. I find no overall effect of the programs on the physician-to-population ratio of an area eligible for the program, though I do find evidence of a positive effect on the physician-to-population ratio when I focus on the age group where physicians are most likely to be recent medical school graduates.

In the third chapter, I examine the effect of high school Career and Technical Education coursework completion on postsecondary enrollment, degree completion, and early career earnings. I utilize two estimation strategies. The first is a propensity score matching approach and the second is an instrumental variables approach based on the distance between a student's high school and a CTE Center that offers the coursework. The two strategies generally find that CTE is associated with a substitution from four-year programs to two-year programs, and positive effects on early career earnings.

# ESSAYS ON TARGETED PROGRAMS IN EDUCATION

by

Brian Heath Witzen

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2019

Advisory Committee:
Professor Lesley J. Turner, Chair/Advisor
Professor Melissa Kearney
Professor Sergio Urzua
Professor Nolan Pope
Professor Laura Stapleton

# Dedication

To Paige, who has been a source of constant strength and support, and to my parents for giving me every conceivable opportunity to succeed.

# Acknowledgments

I owe debts of gratitude to everyone who has touched upon my graduate experience and have made it so worthwhile.

I would like to thank my advisor, Lesley Turner, for the incredible amount of help and guidance over the past six years. My passion for this area of economics research was developed by working on her research projects and through our discussions about education policy, and I look forward for that to continue in the future.

I would also like to thank Melissa Kearney and Sergio Urzua for help in developing this research. Thanks also to Nolan Pope and Laura Stapleton for agreeing to serve on my committee and review the manuscript during a busy time of the year.

The staff at the Maryland Longitudinal Data System Center have also played such a large role in the shaping of this research, from the underlying data to research advice and review, as well as financial support. Thank you to Angela Henneberger for providing much help and advice.

My fellow graduate students have contributed to making these years in graduate school a pleasure. Thomas Hegland and Yongjoon Paek deserve particular thanks for helping me make it through. I would also be nowhere without Vickie Fletcher, Terry Davis, and Mark Wilkerson in the Economic Staff.

Lastly, I would like to thank my wonderful wife for providing a constant source of strength over six years of study and my parents for having done everything possible to provide me the opportunity to reach this goal.

Table of Contents


Dedication ii

Acknowledgements iii

Table of Contents iv

List of Tables vi

List of Figures viii

1 Introduction 1

2 The Effect of Need-based Grants on Postsecondary Student Attainment 4
- 2.1 Introduction . . . . . . . . . . 4
- 2.2 Conceptual Model of Persistence . . . . . . . . . . 10
  - 2.2.1 Effect of a Price Reduction Without Borrowing Constraints . 15
  - 2.2.2 A Net Tuition Reduction When Students Face Borrowing Constraints . . . . . . . . . . 17
- 2.3 Program Description: The Educational Assistance Grant . . . . . . . . 19
- 2.4 Data and Sample . . . . . . . . . . 22
- 2.5 Empirical Strategy . . . . . . . . . . 25
  - 2.5.1 Dynamic Regression Discontinuity . . . . . . . . . . 27
  - 2.5.2 DRDD Implementation . . . . . . . . . . 31
- 2.6 Results . . . . . . . . . . 36
  - 2.6.1 Summary Statistics . . . . . . . . . . 36
  - 2.6.2 Examining the RDD Identifying Assumptions . . . . . . . . . 37
  - 2.6.3 First Stage Effects on Financial Aid . . . . . . . . . . 39
  - 2.6.4 Effects on Persistence and Graduation . . . . . . . . . . 42
  - 2.6.5 Earnings . . . . . . . . . . 44
  - 2.6.6 Robustness . . . . . . . . . . 46
- 2.7 The Effects of Program Design . . . . . . . . . . 51
  - 2.7.1 Targeting by Income . . . . . . . . . . 53
  - 2.7.2 Guarantee of Future Eligibility . . . . . . . . . . 54
  - 2.7.3 Receipt at Entry Versus Later Years . . . . . . . . . . 55
- 2.8 Conclusion . . . . . . . . . . 56
- 2.9 Figures . . . . . . . . . . 58

2.10 Tables . . . . . 66

3 How Effective Are Loan Repayment Programs at Drawing Physicians to Underserved Areas? 85
3.1 Introduction . . . . . 85
3.2 Loan Repayment Programs for Physicians . . . . . 88
3.3 Previous Literature on Location Decisions and LRPs . . . . . 92
3.4 A Model of Physician Location Decisions and the Effect of Loan Repayment Programs . . . . . 98
3.5 Empirical Strategy . . . . . 102
3.5.1 Fixed Effects Model . . . . . 103
3.5.2 Identifying Variation . . . . . 108
3.5.3 Data Sources . . . . . 111
3.6 Results . . . . . 114
3.6.1 Summary Statistics . . . . . 114
3.6.2 Evidence on the Parallel Trends Assumption . . . . . 115
3.6.3 Main Results . . . . . 116
3.6.4 Using the Overall Loan Repayment Amount . . . . . 120
3.6.5 Examining Rural Counties Only . . . . . 122
3.7 Conclusion . . . . . 123
3.8 Figures . . . . . 124
3.9 Tables . . . . . 130

4 The Effect of High School Career and Technical Education on Postsecondary Enrollment and Early Career Earnings: New Evidence from Maryland 146
4.1 Introduction . . . . . 146
4.2 CTE in Maryland . . . . . 150
4.3 Data . . . . . 153
4.3.1 Sample Selection . . . . . 158
4.4 Empirical Strategy . . . . . 161
4.5 Results . . . . . 167
4.5.1 Propensity Score Matching . . . . . 167
4.5.2 Instrumental Variables . . . . . 171
4.6 Conclusion and Future Directions . . . . . 175
4.7 Figures . . . . . 177
4.8 Tables . . . . . 178

Bibliography 193

# List of Tables


2.1 Summary Statistics Among Potentially Eligible Students . . . . . . . 66
2.2 Checking for Demographic Changes at the EA Threshold . . . . . . . 67
2.3 Effects of EA Grant Eligibility in Year 1 on Grants and Scholarships in Year 1 . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 68
2.4 Effects on loans in the first year (per $1,000 of EA Grant award) . . . 69
2.5 Main Academic Effects- Receipt of EA Grant . . . . . . . . . . . . . 70
2.6 Earnings Effects of EA Grant . . . . . . . . . . . . . . . . . . . . . . 71
2.7 Robustness of Main Effects to Bandwidth Choice . . . . . . . . . . . 72
2.8 Robustness of Main Effects to Functional Form and Controls . . . . . 73
2.9 Robustness of Main Effects to the Degree of Polynomial . . . . . . . 74
2.10 Robustness of Earnings Effects to Bandwidth Choice . . . . . . . . . 75
2.11 Robustness of Earnings Effects to the Degree of Polynomial . . . . . 76
2.12 Without Years 2011 and 2013 and Away from the Pell Threshold . . 77
2.13 Limiting the Lower Cutoff . . . . . . . . . . . . . . . . . . . . . . . . 78
2.14 Checking for Demographic Changes at the Original Threshold . . . . 79
2.15 Examining Original Cutoffs . . . . . . . . . . . . . . . . . . . . . . . 79
2.16 Academic Effects: Low Versus High Income . . . . . . . . . . . . . . 80
2.17 Heterogeneous Effects by FARMS . . . . . . . . . . . . . . . . . . . . 82
2.18 EA Vs. Pell . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 83
2.19 Receiving Grant Aid beginning Year 2 . . . . . . . . . . . . . . . . . 84

3.1 State SLRP Programs, Years of Activity, Minimum Required Years, and Repayment Amounts . . . . . . . . . . . . . . . . . . . . . . . . . 130
3.2 State SLRP Programs, Years of Activity, Minimum Required Years, and Repayment Amounts . . . . . . . . . . . . . . . . . . . . . . . . . 131
3.3 Summary Statistics . . . . . . . . . . . . . . . . . . . . . . . . . . . . 132
3.4 Summary Statistics: Rural Counties Only . . . . . . . . . . . . . . . . 133
3.5 Examining Pre-trend Evidence for the DID Assumption . . . . . . . . 134
3.6 Effect Of LRP Eligibility and Amount per Year on the Physician-to-Population Ratio . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 135
3.7 Main Effect Heterogeneity by Physician Age . . . . . . . . . . . . . . . 136

3.8 Effect Of LRP Eligibility and Amount per Year on the Physician-to-Population Ratio . . . 138
3.9 Main Effect Heterogeneity by Physician Age . . . 139
3.10 Estimates Using Full SLRP Amount . . . 140
3.11 Using Full SLRP Amount by Age . . . 141
3.12 Estimates Using Only Rural Counties . . . 143
3.13 Rural County Estimates by Age . . . 144

4.1 Summary Statistics by CTE Completion Type . . . 178
4.2 Raw Mean Outcomes by CTE Completion Type . . . 179
4.3 Breakdown of CTE Program Completion: Program Clusters . . . 179
4.4 Standardized Mean Differences Before and After Matching . . . 180
4.5 Correlations of Time Distance and Other Variables . . . 181
4.6 Enrollment Effects of CTE Education: CTE & USM . . . 182
4.7 Degree Effects: CTE & USM . . . 183
4.8 Earnings Effects: CTE & USM . . . 184
4.9 Earnings Effects: Using Multiple Imputation . . . 184
4.10 CTE Initial Enrollment Effects by Program Cluster . . . 185
4.11 CTE Degree Effects by Program Cluster: CTE & USM . . . 186
4.12 CTE Earnings Effects by Program Cluster . . . 187
4.13 CTE Earnings Effects by Program Cluster (Continued) . . . 188
4.14 First Stage, Reduced Form, and IV Estimates on Enrollment One Year Later . . . 189
4.15 First Stage, Reduced Form, and IV Estimates on Degrees and Certificates . . . 190
4.16 First Stage, Reduced Form, and IV Estimates on Enrollment One Year Later . . . 192

# List of Figures


2.1 EA Grant Threshold for Eligibility by Year . . . 58
2.2 Density of Students at the Threshold . . . 59
2.3 Changes in Demographic Thresholds . . . 60
2.4 Change in Average EA Grant Aid and Probability of Receipt at the Threshold . . . 61
2.5 Changes in Financial Aid Variables at the Threshold . . . 62
2.6 Changes in Persistence at the Threshold . . . 63
2.7 Robustness of the Persistence Estimates to Bandwidth Choice . . . 64
2.8 Density Plot at the Initial EFC Threshold . . . 65

3.1 Changing Program Eligiblity Status Over Time . . . 124
3.2 Distribution of Counties by Number of Years Eligible . . . 125
3.3 Physician-to-population Ratio in Always-Inactive and Always-Active Counties . . . 126
3.4 Changes in State Loan Repayment (SLRP) Amounts and Eligibility over time. . . . 127
3.5 Changes in State Loan Repayment (SLRP) Amounts and Eligibility over time. . . . 128
3.6 Changes in State Loan Repayment (SLRP) Amounts and Eligibility over time. . . . 129

4.1 Area of Common Support for the Propensity Score . . . 177

# Chapter 1: Introduction

In this dissertation, I examine the effect of three education policies that affect student decision-making processes at different points during the academic career. I examine how grant aid affects persistence and attainment of bachelors' degree-seeking students, how loan repayment programs can cause recent medical school graduates to locate in provider shortage areas, and how career and technical training in high school affects enrollment, degree receipt, and early career earnings among Maryland students.

In Chapter 2, I examine the effect of receiving a $3,000 grant, renewable annually, on the persistence and likelihood of graduating among bachelor's degree-seeking students attending public institutions in Maryland. I use a regression discontinuity strategy that exploits a cutoff in eligibility for the grant based on a threshold level of financial need that a student must demonstrate. Using student-level, administrative data from the Maryland Longitudinal Data System, I find that the receipt of this grant, beginning in a student's first year causes, a 8% increase in persistence to the second year and this persistence effect extends to a 10% effect on persistence to the fourth year. I find similar-sized effects on degree receipt within 5 years, though the effects are not statistically significant due to imprecision caused by limits on the number of available student cohorts in the data.

In Chapter 3, I focus on State Loan Repayment Programs (SLRPs) which provide payments towards reducing the debt of recent medical school graduates in exchange for a contract in which a physician commits to practicing in a healthcare provider shortage area. I use the Area Health Resource Files, which tracks the number of physicians by county, and information collected from individual states on the generosity and length of their SLRP programs. I use a generalized difference-in-differences approach that utilizes changes in whether or not a county is eligible for the SLRP benefits as well as changes in state program generosity over time to estimate the effects of SLRPs on the physician-to-population ratio of a county. In general I find no effect of the SLRP program on the overall physician-to-population ratio. However, when I limit to the ages of physicians likely to have recently graduated from medical school, I find suggestive evidence that the average SLRP increases the physician-to-population ratio of a county, by an amount that is equivalent to 10% of the difference between the urban-rural difference in physician-to-population ratio.

In Chapter 4, I examine the effect of completing a Career and Technical Education (CTE) program in high school on postsecondary enrollment, degree completion, and early career earnings. I utilize two complementary strategies to examine the effects of CTE programs. First, using demographic, high school standardized test scores, and school-level data for students, I use a propensity score matching (PSM) approach to match CTE program completers with students who do not complete CTE programs based on these observable characteristics. Using this method, I find that CTE completion appears to be associated with a substitution away from four-year college enrollment towards enrollment in two-year college, and positive effects on early career earnings, on average. I examine the effects separately by broad fields of study of the CTE program completed to look at program heterogeneity. I then complement this strategy with an instrumental variables strategy using the distance between a student's high school and the nearest CTE Center, which provides many CTE classes for the school district. I find that students closer to a CTE Center are significantly more likely to complete a CTE program. Using this IV method, I find that students induced to complete a CTE program because of how close their high school is to a CTE Center are more likely to enroll in two-year colleges and less likely to enroll in four-year programs, results that are consistent with the PSM strategy.

# Chapter 2: The Effect of Need-based Grants on Postsecondary Student Attainment

## 2.1 Introduction

In the United States, federal and state governments provide nearly $50 billion worth of grant aid to undergraduate students annually [College Board, 2017]. By lowering the net price of college attendance, grant programs aim to increase the number of students graduating from college. Only around 55% of bachelor's degree-seeking students complete their degree within five years [National Center of Education Statistics, 2017]. Existing research documents substantial labor market returns and other social benefits to college completion [Oreopoulos and Petronijevic, 2013] and negative consequences associated with leaving college without a degree, such as a higher likelihood of defaulting on student loans [National Center of Education Statistics, 2017]. Given the significant public investment in grant aid programs and the known benefits of completing a degree, knowing when grant aid programs affect attainment is of policy interest. Estimated effects of grant aid on attainment, vary widely, suggesting that not all programs are equally effective, and that particular features of grant aid programs may enhance or inhibit the effectiveness of grant aid.

I estimate the causal effect of Maryland's largest need-based grant aid program on persistence and degree-receipt within five years for students at public, four-year institutions. Using a discrete cut-off in eligibility based on financial need and a regression discontinuity strategy, I find that first-year students who receive a renewable award of approximately $3,000 per year are less likely to drop out. Specifically, recipients are 8%, 14%, and 10% more likely to reach the second, third, and fourth years, respectively, than non-recipients, and I also provide suggestive evidence that recipients are more likely to graduate within five years.

Besides the dollar amount provided to students, there are many other aspects of grant aid programs that could influence their effectiveness in increasing attainment. I exploit other features of the EA Program's design to understand the effects of a grant program's structure. First, I use the fact that the EA Grant eligibility thresholds vary over time to test whether the EA Grant has larger effects on lower-income students. I find suggestive evidence that lower-income students experience the largest persistence gains when receiving a grant. Second, I examine the fact that the EA Grant is renewable, meaning that eligible students are automatically eligible in subsequent years if they still have any unmet financial need. Specifically, I compare the effects of EA Grant eligibility versus eligibility for the Pell Grant, which does not have this renewable feature, and, among the same cohorts of entering college students, provide suggestive evidence that the EA Grant has larger effects on persistence. Finally, I use the eligibility thresholds a student would face in their second year to test the effect of becoming eligible to receive the EA Grant in the second year versus the effect of becoming eligible in a student's first year on persistence to year three. I provide suggestive evidence that beginning to receive the grant in the second year has little effect on persisting to the third year, in contrast to the positive effect of becoming eligible in year one.

Eligibility for Maryland's Educational Assistance (EA) Grant is determined by a student's Expected Family Contribution (EFC), a value calculated from the Free Application for Federal Student Aid (FAFSA) that indicates a student's level of need. The EFC is used as the basis for eligibility for many need-based grants, most notably the federal Pell Grant. Each year the State of Maryland sets a threshold level of EFC below which students are eligible for the EA Grant and above which they are ineligible. I use the eligibility criterion for the EA Grant as the basis for a regression-discontinuity design to estimate the effect of receiving a renewable grant in a student's first year on persistence, graduation, and early career earnings using data on students at Maryland public, four-year institutions from the Maryland Longitudinal Data System.

Students can become eligible for the EA Grant at any point during their academic career if their financial need becomes large enough for them to qualify. As a consequence, some students who are ineligible for the EA Grant in their first year may receive the grant later in their academic careers, potentially leading to an underestimate of the effect of grant aid using the standard regression-discontinuity design. Grant programs can be structured in a variety of way, but one of the most common is to provide a student support over four years of undergraduate study as long as they remain at an eligible level of income. To compare eligible first-year students against an appropriate, policy-relevant counterfactual student who is never eligible for any grant aid, I adapt a "dynamic" regression discontinuity model of Cellini et al. [2010] and use the eligibility threshold in a student's first year to estimate the probability of becoming eligible for the EA Grant in later years. Using the dynamic model, I can estimate the effects of first-year EA Grant eligibility net of any effects for initially-ineligible students by using a recursive estimation process.

My findings contribute to a growing literature on the effects of grant aid on college students' persistence, graduation, and post-college outcomes. Literature on the effects of grant aid on educational outcomes initially focused primarily on whether grants induced college attendance among students who would otherwise not have matriculated. In a survey of the existing literature Deming and Dynarski [2009] find that $1,000 of grant aid generally increases enrollment by 4 percentage points (p.p). However, enrollment effects depend on the type of program examined. The Pell Grant generally has been found to have no effect on enrollment for traditional-aged students [Hansen, 1983, Kane, 1995, Seftor and Turner, 2002, Denning et al., forthcoming], while other federal and state programs have shown positive effects on enrollment [Kane, 2003, Dynarski, 2003b, Cornwell et al., 2006, Abraham and Clark, 2006, Goodman, 2008, Dynarski, 2008]. Deming and Dynarski [2009] argue that grant programs with simple to understand eligibility criteria are most effective.

Increasing college enrollment does not necessarily lead to increasing college completion, and the returns to college completion substantially exceed the returns received by college drop-outs. Bound et al. [2010] analyzed different high school classes in the National Longitudinal Survey of Youth 1979 and the National Education Longitudinal Study of 1988, and found that the 1992 graduating cohort had higher rates of enrollment in postsecondary education than the 1972 class, but worse graduation rates. In more recent cohorts, the rate of graduation has remained largely stagnant in the four-year sector over the last 10 years [College Board, 2017]. Stagnant graduation rates are concerning, given that the net cost of college has increased by 22% in the last 10 years. Student who enroll in college, but do not finish are also particularly likely to default on their student loans [National Center of Education Statistics, 2017].

Recent research on grant aid programs have examined outcomes beyond enrollment, including persistence, degree completion, and post-college earnings and have generally found positive effects. Several studies have used regression discontinuity strategies to examine state and federal grant programs. Castleman and Long [2016] found that students who received $1,300 from Florida's Student Access Grant increased the rate of degree completion within six years by 22%. Bettinger [2015] examined a change in Ohio's need-based grant aid, which increased aid for some students while decreasing aid for others, and finds that drop-out rates fell by 2% in response to $800 additional in grant aid. Bettinger et al. [2019] study California's Cal Grant, which has both an income threshold and a high school GPA threshold for eligibility, and find that Cal Grant receipt has no effect at the income threshold, but at the GPA threshold receipt increases bachelor's degree completion by 10%. Looking at even longer run outcomes, they find a positive 3% effect effect on earnings after graduation for students at the GPA threshold. Denning et al. [forthcoming] examine students who are more likely to qualify for the maximum Pell grant by virtue of an “automatic-zero” EFC and find a 10% effect on bachelor’s degree receipt from $700 additional grant aid at entry. In addition to the quasi-experimental evidence, randomized experiments, such as that of Goldrick-Rab et al. [2016] and Angrist et al. [2014] have also found positive persistence and completion effects of randomly assigned grant aid to low-income students. Goldrick-Rab et al. [2016] found that a $3,500 annual award increased degree receipt by 21% and Angrist et al. [2014] found that a nearly $7,000 award and increased enrollment four years later at four-year institutions by nearly 19%.

I add to this growing literature by estimating the effect of a large need-based grant aid program. The positive effects on persistence that I estimate fit within the wide range of estimated effects found by prior studies. My finding that grant aid receipt appears to have larger effects for lower income students, contributes to the few existing studies that have examined heterogeneous effects by income of grant aid [Alon, 2011, Williams, 2018, Denning, 2018]. I also provide evidence that aid received earlier during a student’s academic career has a larger effect on longer-run outcomes. While other studies have not examined this effect for grant aid, Conger and Turner [2017] find that college students’ degree receipt is more responsive to tuition changes that occur earlier in their academic career. Lastly, I provide suggestive evidence that the guarantee of receipt in future years for students who receive an EA Grant aid may play a role in its positive effect using a comparison to the effects of the Pell Grant. The study of the guaranteed nature of the need-based grant is, to my knowledge, new to the literature on grant aid.

The remainder of this chapter is structured as follows. In Section 2.2, I provide a conceptual model that illustrates how changes in net tuition may affect a student's persistence, and how this effect differs when students are credit constrained. In Sections 2.3 and 2.4, I discuss the details of the EA Grant program and the MLDS. Section 2.5 describes the empirical strategy that I use, while Section 2.6 produces the results of the estimation. In Section 2.7, I provide evidence regarding the design of grant aid programs. In Section 2.8, I conclude.

## 2.2 Conceptual Model of Persistence

Grant aid may affect persistence through several mechanisms, one of which may be by relaxing credit constraints. Increased grant aid reduces how much students pay towards tuition, which might allow some students to continue who would otherwise be unable to borrow enough to continue their studies. The existing literature provides mixed evidence on the existence of credit constraints in higher education [Dynarski, 2003a, Cameron and Taber, 2004, Lovenheim, 2011, Lochner and Monge-Naranjo, 2011, Brown et al., 2011]. Related to a student's level of credit constraint, grant aid may also reduce a student's need to work while enrolled. Students who have exhausted their available credit or who face prohibitively expensive credit might require income from wages in order to finance their education. Broton et al. [2016] and Carruthers and Özek [2016] examine at the effect of grant aid on a student's propensity to work outside the classroom and found significant decreases in the probability of working or earnings received during the school year.

To illustrate how changes in net price could affect both a students' enrollment and persistence, I present a simple two-period model of student behavior adapted from Altonji [1993], modified to allow an explicit cost to education that must be paid every period (e.g. tuition and fees). At the end of high school, students have imperfect information about their ability to finish college (and thus earn a higher "graduate wage"), and must base their initial college enrollment decision on a noisy expectation of their likelihood of college completion. Once enrolled in college, students recieve updates to their information about their probability of graduation and make a decision about whether to continue or drop out. Students enter each school year comparing the expected net benefits of continued enrollment with the alternative of not enrolling and make a series of enrollment decisions. The idea that students have incomplete information about their ability before attending postsecondary education and subsequently learn about their own ability after enrollment forms the basis of similar models, such as Arcidiacono et al. [2016], and has been demonstrated to have an empirical basis by Stinebrickner and Stinebrickner [2014].

In period 0, representing the time in late high school and immediately after high school graduation, students observe a measure of their own ability, $A$, which could represent a score on a standardized test, such as the SAT or ACT. Standardized tests provide some information about a student's likelihood of graduation, but other idiosyncratic factors, such as work ethic, health factors, etc., may also contribute to a student's probability of success. In period 0, the student must make a decision about whether to enroll in college, or to begin working and receive the "high school wage". In period 1, students learn their probability of graduating college in period 2, which is a draw from a distribution that is a function of $A$. Once students observe this probability, they again make a decision to continue or to leave school and earn the wage for having completed “some college”. In period 2, all uncertainty is revealed, and remaining students either graduate and earn the graduate wage, or fail to graduate and earn the some college wage. I assume that the student does not know their draw of the probability of graduation at period 0, but does know the wages corresponding with each level of education.

This timing can be illustrated in the following timeline:

| 0 | 1 | 2 |
|---|---|---|
| Observe ability and make decision to enroll or work and receive high school wage | If enrolled, receive draw of probability of graduating in period 2. Decide to enroll in period 2 or drop out and recieve “some college” wage | All uncertainty is resolved. Enrolled students either graduate and receive graduate wages or drop out and receive the “some college” wage |

The probability of graduation, $Pr(grad) = p$, depends on ability such that $p \sim F(p|A)$, where $F(p|A)$ is the CDF of $p$. In period zero, a student observes $A$ and knows the conditional probability $F(p|A)$, but does not receive a draw of the probability of graduation until after enrolling in college. Once enrolled, a student observes their draw of $p$.

A student’s earnings are expressed in present value and depend on the amount of schooling received. $Y_0$ is the earnings of a high school graduate. The earnings for someone with some college who quits after one year is: $Y_1 = \frac{Y_0(1+r_1)}{1+R}$, which incorporates the return to the one year of education ($r_1$), and is discounted to the present by the interest rate, $R$. A student who enrolls in period 2, but fails to graduate earns $\frac{Y_1}{1+R}$, while a successful graduate earns $Y_2 = \frac{Y_0(1+r_2)}{(1+R)^2}$, which incorporates the return to graduation ($r_2$). I assume that all wages ($Y_0$, $Y_1$, and $Y_2$) are known and certain prior to the initial enrollment decision and do not vary with A or other characteristics. The net price of education, $T$, must be paid each decision-making period. This is the net cost of attending college, which is equal to the sticker price (or list price) of tuition and fees minus any grant aid received, $T = list\ price - grant\ aid$. For the sake of simplicity in the mode, I assume that $T$ does not change between periods.

In each decision making period, students receive an endowment, representing, for example, a transfer from a parent. The endowments in period 0 and 1 are $\omega_0$ and $\omega_1$, respectively, and are draws from distributions, $\omega_0 \sim \Omega_0(\omega_0)$, and $\omega_1 \sim \Omega_1(\omega_1)$, representing uncertainty over the income available to a student while enrolled in college. For convenience, I assume that the two distributions are independent, but the conclusions of the model would still hold if the two distributions were correlated. The two endowments are revealed before each decision period (i.e. $\omega_0$ is known when making the initial enrollment decision and $\omega_1$ is known prior to the re-enrollment decision).

In each period, students are able to borrow and save. In period 0, if a student decides to attend college in period 1, the amount of saving $s_0 = \omega_0 - T$. Likewise, the amount of saving to attend college in period 2 is $s_2 = (1 + R)s_0 + \omega_1 - T$. I will first present the model without any borrowing constraints on the student, and then show the results with borrowing constraints (i.e., a lower bound on $s_0$ and $s_1$.

I work backwards from the student's decision in period 1 about whether to continue their education or leave school. I assume that students are risk neutral to simplify the calculations, but this does not fundamentally affect the model's implications. In period 1, $p$ has been revealed, and the value function, or the student's expected value of each choice given the previous choices of savings and enrollment, of period 2 is:

$$V(g_2, s_0) = max\{pY_2 + \frac{(1-p)Y_1}{(1+R)} + (1+R)s_0 + \omega_1 - T,\ Y_1 + (1+R)s_0 + \omega_1\} \quad (2.1)$$

In words, a student making the decision to enroll in period 2 compares the expected net value of enrolling in period 2, versus the net value of leaving school and receiving the "some college" wage.

In period 0, the value of enrolling in period 1 is:

$$V_1(A, T) = E[V_2(g_2, s_0)|A, T, \omega_0] \quad (2.2)$$

Which is the expected value of $V_2$ with respect to the distribution of the probability of graduation and the distribution of the future endowment, conditional on ability, tuition and the initial endowment.

A student decides to initially enroll if her expected value of enrolling is greater than the high school wage, $Y_0$. A student, therefore, makes the decision to enroll if:

$$V_1(A, T) > Y_0 + \omega_0 \quad (2.3)$$

### 2.2.1 Effect of a Price Reduction Without Borrowing Constraints

Using this simple model, I now show how changes in college price could affect persistence. Increases in grant aid affect the net price of tuition, so demonstrating the implications of price reductions will be equivalent to the impact of increased grant aid.

To make the model more applicable to my setting, I focus on the decisions of students for whom Equation 2.3 holds (i.e., those who remain in college) and analyze the effect of price reductions on the probability of persistence to period 2. This effectively shuts down the effect of price reductions on initial enrollment. In Section 2.1, I discussed how some programs, like the Pell Grant, have been found to have no effect on initial enrollment, and in Section 2.6.2, I provide evidence that the EA Grant has no effect on initial enrollment. Therefore, I treat tuition reductions as if they occur after a student has already made their decision to initially enroll in college. If the model were to allow initial enrollment effects, the effects on persistence would affect initial enrollment through a student's expectations in period 0.

Students who enter college in period 1 will re-enroll in period 2 if:

$$pY_2 + \frac{(1-p)Y_1}{(1+R)} - T > Y_1 \tag{2.4}$$

$$p > \frac{Y_1(1-\frac{1}{1+R}) + T}{Y_2 - \frac{Y_1}{1+R}} \tag{2.5}$$

That is, if the expected net present value of enrolling in period 2 is greater than the net present value of leaving school. The proportion of students who decide to enroll in an additional year of school is:

$$1 - F(\Phi(T)|A) \tag{2.6}$$

Where $F(.)$ is the CDF of the distribution of the probability of graduation, $p$, and $\Phi(T)$ is the expression in the right hand side of 2.5, which expresses the ratio of the gains to leaving school after one year to the returns from a successful second year. If there is a tuition reduction, $T' < T$, then $\Phi(T') < \Phi(T)$. Therefore:

$$1 - F(\Phi(T)|A) < 1 - F(\Phi(T')|A) \tag{2.7}$$

Which means that more students will enroll in a second year when the price is lower. However, despite the increase in persistence, the expected graduation rate, or the average $p$ among students who are induced to re-enroll in the second period will be lower than the graduation rate of students whose decisions are unaffected by price, as $\Phi(T') > \Phi(T)$. Students who did not previously find the next benefit of continuing will now choose to enroll in the second period. Also, because the policy is causing students with lower expected returns to re-enroll, this also implies that there will be a reduction in the overall graduation rate.

### 2.2.2 A Net Tuition Reduction When Students Face Borrowing Constraints

The above model allowed students to borrow in order to pay the cost of education each period. However, if students face restrictions on borrowing, then net tuition reductions will also affect students at the margin of the borrowing constraint.

To illustrate the case of restricted borrowing, I simply require net saving to be higher than some arbitrary lower bound, $\gamma_1$. This credit constraint reflects the fact that there are limits on federal loan eligibility, and that private loans carry higher interest rates [Mazzeo, 2007] and might be limited in availability to students of lower ability [Lochner and Monge-Naranjo, 2011].[1]

This results in the following condition:

$$(1+R)s_0 + \omega_1 - T \geq \gamma_1 \tag{2.8}$$

I again focus on students who have already made the decision to enroll in period 1. As $p$ and $\omega_1$ are independent draws, a tuition reduction of $T' < T$ would affect the students at the margin of net benefits minus net costs according to Equation 2.5.

However, students will also face the margin of the second period credit constraint. Students will decide to enroll in period 2 if Equation 2.5 holds and if

[1]Dependent first-year students can borrow a maximum of \$5,500 in federal Direct loans, a maximum of \$3,500 of which can be subsidized. Undergraduate students face an aggregate loan limit of \$31,000 over the course of their undergraduate career. Independent students have higher borrowing limits.

Equation 2.8 also holds. A net tuition discount will reduce the number of students who drop out after period 1 due to credit constraints. The credit constraint binds on the portion of the population that do not meet condition 2.8. Using the distribution of period 2 endowments, $\Omega_1$, this portion of the population would be:

$$\Omega_1((1+R)s_0 + \omega_1 - T) \tag{2.9}$$

Therefore, the increase in the population that is not bound by the credit constraint under the new $T'$ is:

$$1 - \Omega_1((1+R)s_0 + \omega_1 - T' - \gamma_1) > 1 - \Omega_1((1+R)s_0 + \omega_1 - T - \gamma_1) \tag{2.10}$$

In this model, a tuition reduction increases both the population unbound by any credit constraints and the population that finds it beneficial to enroll in year 2. This leads to an unambiguous increase in the proportion of initially enrolled students who persist to period 2. Increases in grants will also lead to an unambiguous increase in the proportion of students who initially enroll, now through an additional credit constraint alleviation as well as an expected net benefit decision.

With the credit constraints, the effect of grant aid on the average rate of graduation, $p$ is ambiguous, as it will depend on which margin is most affected by the reduction in price. As is the case in the unconstrained model, some students who did not previously find the expected returns to a second year of education worth the investment because of the low probability of graduation and receipt of a college graduate wage will decide to enroll in period 2 as a result of the price reduction. However, in the credit constrained model, students of particularly high $p$ may be unable to enroll in period 2 due to credit constraints. If the proportion of high $p$ students that are constrained is large, then the average $p$ among persisting students could actually increase. In such a situation, a net tuition increase could lead to both an increase in persistence and an increase in the graduation rate among the students who decide to persist to period 2.

## 2.3 Program Description: The Educational Assistance Grant

The Howard P. Rawlings Educational Assistance (EA) Grant is the State of Maryland's largest need-based grant program, providing grant aid to students in two- and four-year degree programs at postsecondary institutions in the state. The amount awarded to a student is based on their level of unmet financial need, with a maximum award of $3,000, and can be renewed annually. In the state fiscal year 2015, the program disbursed 28,525 EA Grant awards to new and continuing students, with a total expenditure of $61.1 million.

Students must complete the Federal Application for Free Student Aid (FAFSA) to be eligible for federal grants and loans. Completing the FAFSA automatically places students into consideration for an EA Grant. A FAFSA application, using a complex formula, generates an Expected Family Contribution (EFC), which indicates what the family of a student could reasonably contribute toward the student's cost of education. The FAFSA uses many pieces of financial information to arrive at an EFC, but a student's EFC is generally correlated with their and their family's income (unless the student files the FAFSA as an independent student) and the number of family members at home and in college.[2] EFC is also used to determine eligibility for other programs, such as the Pell Grant, the largest federal need-based grant program, which offers a schedule of grant aid that depends on EFC and cost of attendance (COA). The cost of attendance is determined by the school and includes tuition and fees, books, expected living expenses and can vary by program and living arrangements (i.e. whether a student plans to live on campus or commute). To be eligible for the EA Grant, students must be MD residents, attend an in-state public university full-time, and be degree-seeking.

To award the EA Grant, the state first determines a student's unmet need by taking a student's cost of attendance and subtracting the student's EFC, the amount of Pell Grant awarded, and certain other state scholarships, such as the Guaranteed Access grant, a state grant provided to as small number of students, but that provides 100% of the cost of attendance to student's whose families fall under 130% of the federal poverty line.[3] A student's unmet need is then their cost of attendance, less their EFC, Pell Grants, and state scholarships received, or a measure of the amount that students are not expected to pay by federal formula, but is not met by other forms of grant aid.

[2] The definition of independent for the purposes of the FAFSA is not the same as dependent for tax purposes. Independent status is determined if a student meets certain criteria, such as being older than twenty-four, having a child that depends on them for more than half of their support, or being legally emancipated, among other criteria.

[3] The State of Maryland also adjusts each student's cost of attendance using a regional cost of living adjustment.

The State of Maryland has limited funds for the EA Grant, and cannot fill all existing unmet need. As a result, students with unmet need are sorted in ascending order by EFC, and the state exhausts EA Grant funding up to an EFC cutoff. Above this cutoff, students are no longer eligible for the EA Grant. Eligible students who attend two-year and four-year institutions can receive grants equal to 60% and 40% of their existing unmet need, respectively, up to the $3,000 maximum award.

In practice, the state sets an initial threshold EFC eligibility threshold based on estimates of how many students will accept (or renew previous) EA awards in an attempt to exhaust yearly appropriations. Students below the initial EFC cutoff are notified of their eligibility and receive the award as part of their financial aid package from their institution. Awarded students must return signed forms indicating their acceptance of the terms and conditions of the award, certifying their eligibility, accepting the length and amount of the award, and acknowledging the procedure for renewing the award in the future.[4]. Students above the initial EFC cutoff are placed on a wait list for the award and are accepted off of the wait list as the state raises the cutoff EFC to distribute additional available aid.

Students may apply to multiple institutions and Maryland first determines eligibility by the institution with the highest cost of attendance on the student's FAFSA. Students who then attend lower-cost institutions become ineligible for the aid if attending the lower-cost institution results in no unmet need. This reduction in expenditures increases the statewide EFC cutoff further, and the state offers more

[4]Based on state reports, this requirement to accept the terms and conditions is nontrivial. A significant percentage of awarded students in a year do not fill out the terms and conditions and then become ineligible for the grant aid.

students aid who are on the wait list. A final EFC cutoff is determined in the fall semester once the process of determining the pool of eligible students is complete.

In recent years, the final EFC cutoff has varied substantially from year to year. A graph of the final EFC cutoff over time can be found in Figure 2.1, where each academic year is represented by the terminal year (for example, 2013-2014 is represented as 2014). The final EFC cutoff was higher than $8,000 in 2009, 2010, and 2014, while it was lower than $6,000 in 2011, 2012, 2013 and 2015. In 2012 and 2015, the EFC cutoff was particularly low, at $1,500 and $2,610. To put this context, an student with $1,000 EFC has an average household income of $45,000 while a student at $10,000 EFC has an household income of $87,500. A combination of factors has led to this variability, including the difficulty of estimating how many students will accept the aid and attend the institution that makes them eligible. In addition, the state had awarded less than appropriated for several years prior to 2014 and decided to use that surplus in the 2014 year. The lower EFC cutoff in the 2015 year is a direct consequence of the large increase in awards in 2014.

## 2.4 Data and Sample

I use data from the Maryland Longitudinal Data System (MLDS), which contains linked longitudinal data from three Maryland agencies. The Maryland State Department of Education (MSDE) provides data on public PreK-12 students and schools. The Maryland Higher Education Commission (MHEC) provides data on Maryland public and private college students and colleges. The Department of Labor Licensing and Regulation (DLLR) provides data on Maryland employees using data from the state's unemployment insurance database. The earnings data exclude information for federal employees, military employees, individuals who are self-employed, private contractors.

I focus on students who entered four-year universities in Maryland between and including the 2008-2009 (2009) and 2015-2016 (2016) academic years. I construct postsecondary enrollment histories for all students in the sample and use financial aid data to create histories of students' financial aid awards for each year of postsecondary enrollment, and to determine eligibility for the EA Grant program. The final sample of students used for the analysis is created using the following criteria: (1) The sample is limited to students who completed a FAFSA, and thus have financial aid data available to view EFC, COA, and adjusted gross income. (2) The sample is limited to in-state, first-time, full-time, and degree-seeking students in order to be consistent with the eligibility requirements of the EA Grant program. (3) In this spirit, the sample is limited to students who have positive remaining financial need. (4) Lastly, only students with available 12th grade public school enrollment are kept in the sample. Using these filters, the overall analytical sample has 41,976 students (38%) out of the total 108,110 full-time, first-time, degree-seeking students in the Maryland higher education system over this time period.

The main outcome variables include probabilities of persisting to a given year in college, probability of graduation within 5 years, and earnings in a given academic year after entering postsecondary education. Indicator variables were created that equal 1 if a student is enrolled X years later at *any* four-year institution serve as measures of persistence. A similar indicator was created for graduation, which equals to 1 if a student receives a bachelor's degree from the same institution within 5 years of entry. The earnings data were used to create measures of earnings within an academic year. To align with academic years, earnings years were coded such that quarters roughly match the academic year, thus earnings in the 2013-2014 academic year are from quarter 3, 2013 through quarter 2, 2014. Any missing quarters were coded as zero for a student, meaning that the workforce wages measures is the sum of all observed wages (including zeros) for the academic year approximation.

Information from the FAFSA and financial aid received were used to determine EA Grant eligibility and as outcome variables to determine how a student's overall financial aid changes with EA Grant receipt. Aid awards were summed by category for the first academic year. The categories, EA Grant aid, other sources of grant aid, and loans from all sources during the first academic year were used as dependent variables. After the first year, a student's receipt of financial aid will also be determined by whether they re-enroll, which would make the examination of the effects on other sources of aid difficult.

Other demographic, test score, and income variables were included as control variables. I observe a student's race, gender, and ethnicity, as well as their level of adjusted gross income (AGI) from the FAFSA application. I also observe SAT scores for students who have taken the SAT. I also observe a student's institution and entering cohort.

## 2.5 Empirical Strategy

The parameter of interest in estimating the effect of EA Grant receipt is the effect of becoming eligible at college entry compared to a counterfactual of a student who is never eligible for EA Grant aid. Since the EA Grant is renewable, the effect of being eligible in the first year includes the promise that the grant will be available in future years, so the estimate of first year eligibility includes this guarantee of future price reductions. This is in contrast to many other grant programs, for example, the Pell Grant requires a student to have a sufficiently low EFC in each year, so the estimate of the effect of Pell Grant eligibility includes only the increase in grant aid for the particular year a student is eligible.

The EFC eligibility threshold generates a discontinuous change in grant eligibility which allows for causal estimates of this parameter using a regression discontinuity design [Hahn et al., 2001]. Consider a hypothetical student considering enrolling in her first year of postsecondary education. I denote a student's normalized EFC, or the distance between her EFC and the threshold EFC that determines EA Grant eligibility, as $E\tilde{F}C$, and an indicator for EA Grant eligibility, $g_i$ as equal to 1 if $E\tilde{F}C < 0$, and equal to 0 otherwise. Denoting the dependent variable - enrollment in year 1 - as $y_{1i}$, then I can estimate the following regression model:

$$y_{1i} = \beta g_i + f(E\tilde{F}C) + \epsilon_i, \tag{2.11}$$

where $\epsilon_i$ is unobserved variation in the probability of enrollment with $E[\epsilon_i] = 0$, and

$f(.)$ is a flexible function of the running variable. The parameter $\beta$ is interpreted as the effect of becoming eligible for the EA Grant in a student's first year on enrollment in the first year. There may be reasons why eligibility of receipt of grant aid, $g_i$ may be correlated with $\epsilon_i$. Students who are eligible for grant aid have less family income on average, which could, for instance, affect their ability to receive outside help with coursework. However, if the unobservable factors correlated with $g_i$ are continuous through the eligibility threshold, then within a neighborhood of the threshold, the effect of EA Grant eligibility, $\beta$ is identified by the discrete change in eligiblity at $E\tilde{F}C = 0$ [Hahn et al., 2001]. Using notation, if the identifying assumption holds, that, conditional on $E\tilde{F}C_i$, the unobservable component is continuous:

$$\lim_{\upsilon \to 0} E[\epsilon_i | E\tilde{F}C_i = \upsilon] = \lim_{\upsilon \to 0} E[\epsilon_i | E\tilde{F}C_i = -\upsilon] \tag{2.12}$$

Then $\beta$ is identified by:

$$\lim_{\upsilon \to 0} E[y_i | E\tilde{F}C_i = \upsilon] - \lim_{\upsilon \to 0} E[y_i | E\tilde{F}C_i = -\upsilon] = \beta \tag{2.13}$$

Eligibility for the EA Grant, which renewable in every year that a student remains enrolled, decreases expected net tuition for every subsequent year of enrollment. If eligibility for the EA Grant was only extended to first year students, a similar regression to Equation 2.11 could be used to estimate the effect of the EA Grant on subsequent years of enrollment. These regressions would simply replace $y_{1i}$ with $y_{2i}$, $y_{3i}$, $y_{4i}$, or an indicator for graduation. Each regression would identify the effect of EA Grant eligibility on persistence to a given year or graduation versus a counterfactual in which a student was never eligible for additional grant aid through the EA Grant program.

### 2.5.1 Dynamic Regression Discontinuity

One aspect of the design of the EA Grant program that affects the interpretation of the estimated parameter is that students who are ineligible for an EA Grant may subsequently become eligible for EA Grants in later years. An example of this would be a student who is just ineligible for the EA Grant in their first year, given their EFC. In their second year, the EA Grant threshold is more lenient (higher) than in their first year, and their EFC now qualifies for an EA Grant. Once a student becomes eligible, they may renew the EA Grant in any subsequent years they are enrolled regardless of EFC, as long as they have unmet need. Subsequent eligibility for students who were ineligible in their first year may be gained by a change in family circumstances or by the threshold rising in a subsequent year (e.g., Figure 2.1). Students who enter in 2013 and are just ineligible by EFC are likely to be eligible in their next year (2014) for a renewable EA Grant.

A consequence of a policy where ineligible students can subsequently become eligible is that it changes the interpretation of the "control" group (those just ineligible in their first year) compared to the treatment group. Some ineligible students can receive treatment in their later years. For the sake of notation, I will consider year 0 to be a student's first year, and year 1 the second year, etc. If I want to consider the effect of EA Grant eligibility in the first year on enrollment in year 1, then an RD estimation of:

$$y_{1i} = \beta_1 g_{0i} + f(E\tilde{F}C_{0i}) + \epsilon_i \tag{2.14}$$

identifies the effect $\beta_1$, the effect one year after the beginning of the grant's receipt. However, this $\beta_1$ has a complicated interpretation. If we express the effect $\beta_1$ as a derivative:

$$\beta_1^T \equiv \frac{dy_{1t}}{dg_{i,0}} = \frac{\partial y_{i1}}{\partial g_{i,0}} + \frac{\partial y_{i1}}{\partial g_{i,1}} * \frac{dg_{i,1}}{dg_{i,0}} \tag{2.15}$$

It is possible to see how it is the effect of becoming eligible in the first year plus a treatment effect for those in the control group that become eligible in the second year multiplied by the probability that a student in the control group received treatment. The quantity $\frac{dg_{i,1}}{dg_{i,0}}$ will always be negative in this context because a student who begins to receive the EA Grant in year 0 will be less likely to begin receiving the grant in year 1 (as they have already become eligible). Thus the effect is smaller than the effect of becoming eligible for the EA Grant in the first year versus a counterfactual in which the student never receives a decrease in price due to the EA Grant due to the addition of this negative quantity.

To estimate a policy-relevant treatment effect - the effect of becoming eligible for the EA Grant in a student's first year - I adapt the "dynamic" RDD model of Cellini et al. [2010]. In this model, eligibility for an increase in grant aid in the year of entry can affect the probability of grant aid increases in the future years. I consider a treatment indicator $g_{i,t}$ that is equal to 1 if a student $i$ is permanently eligible for an extra amount of grant aid up to \$3,000 in year $t$ and zero otherwise. To represent the renewable nature of the EA Grant, $g_{i,t}$ is an indicator for receiving an increase in grant aid in year $t$ that decreases tuition in each subsequent year after $t$. As an outcome variable, I consider $y_{it}$ as an indicator for whether a student is enrolled in year $t$. If the direct effect of receiving an increase in grant aid in year $t-\tau$ on enrollment in year $y_{it}$ depends on only the number of years since the increase in grant aid, then $y_{it}$ can be written as the sum of grant aid changes in each previous year:

$$y_{it} = \sum_{\tau=0}^{\infty} g_{i,t-\tau}\beta_{\tau}^{D} + \epsilon_{it} \tag{2.16}$$

or as the sum of the partial effects of the complete history of increases in grant aid. The coefficient $\beta_{\tau}^{D}$ is the direct (D) effect of a increase $\tau$ years prior to $t$ on $y_{it}$, holding constant any other increases in grant aid.

The direct effects are policy relevant. For example, a policymaker might want to know what is the effect of providing a student an extra \$3,000 in grant aid, beginning in year 1, where the aid is renewable and the student knows that it is guaranteed in all years, on the probability of enrolling in year 3. In a model using $y_{i3}$ as the dependent variable, this would be the effect $\beta_{3}^{D}$.

An RD regression like that of Equation 2.11 could be performed on an indicator for receiving a grant increase $\tau$ years earlier. Such a regression would take the form:

$$y_{it} = g_{i,t-\tau}\beta_{\tau}^{T} + f(E\tilde{F}C) + e_{it} \tag{2.17}$$

However, as explained above, the identified effect includes the direct effect of a grant increase plus the effects on the probability of future grant aid receipt. This regression identifies an "total" (T) effect, which includes the effect on future grant aid increases.[5] The total effect then is a combination of the direct effect of receiving an increase in a given year and the probability that subsequent treatments will be received. If receiving an increase in grant aid changes the probability of receiving a permanent increase in grant aid in the future, $\beta_1^T$ will not equal $\beta_1^D$. Equation 2.18, shows how, if the probability of receiving a permanent increase in the future depends on receiving an increase in a prior year, then the total effect of $\beta_\tau$ equals:

$$\beta_\tau^T \equiv \frac{dy_{it}}{dg_{i,t-\tau}} = \frac{\partial y_{it}}{\partial g_{i,t-\tau}} + \sum_{h=1}^{\tau} \Big( \frac{\partial y_{it}}{\partial g_{i,t-\tau+h}} * \frac{dg_{i,t-\tau+h}}{dg_{i,t-\tau}} \Big) \tag{2.18}$$

$$= \beta_\tau^D + \sum_{h=1}^{\tau} \beta_{\tau-h}^D \pi_h \tag{2.19}$$

where $\pi_h$ equals the change in probability of a grant increase in period $t-\tau+h$ due to receiving a grant increase in $t-\tau+h$.

As a concrete example, the effect in Equation 2.15 of receipt in a student's first year on persistence to the second year can be written as:

$$\beta_1^T = \beta_1^D + \beta_0^D \pi_1, \tag{2.20}$$

[5]In Cellini et al. [2010], the direct effects are called "treatment on the treated" and total effects are called "intent to treat" effects, mirroring the language used in an instrumental variables setting. Here I use "direct" and "total" to prevent confusion, because I will estimate the RD using a "fuzzy" design.

or the effect of receiving the grant in the first year on enrollment in the second year plus the effect of receiving the grant in the second year on enrollment in the second year multiplied by the change in the probability of receiving the grant in the second year after receiving the grant in the first year.

In the case of the EA Grant program, students who are ineligible in year one may receive the EA Grant if they qualify in a later year. This means that $\pi_h < 0$, and assuming that the direct effects are positive, then the $\beta^D_\tau > \beta^T_\tau$. Another way to think about this is to consider the treatment and control group when the treatment is receiving an EA Grant in the first year. If the probability of receiving an increase in year 2 is affected, then the total effect incorporates the fact that some of the initially eligible students received treatment.

### 2.5.2 DRDD Implementation

Following the method of Cellini et al. [2010], I implement a recursive estimator which estimates the direct effects of receiving increases in grant aid by incorporating total effects that can each be separately estimated. Each total effect can be written as:

$$\beta^T_0 = \beta^D_0 \tag{2.21}$$

$$\beta^T_1 = \beta^D_1 + \pi_1 \beta^D_0 \tag{2.22}$$

$$\beta^T_2 = \beta^D_2 + \pi_2 \beta^D_1 + \pi_1 \beta^D_0 \tag{2.23}$$

$$\beta_3^T = \beta_3^D + \pi_3\beta_2^D + \pi_2\beta_1^D + \pi_1\beta_0^D \tag{2.24}$$

To estimate $\beta_0^T$, I use an RDD regression of enrollment in year 1 on EA Grant eligibility in year 1. For $\beta_1^T$, I use an RDD regression of enrollment in year 2 on EA Grant eligibility in year 1, and so on. The $\pi$ effects are similarly intent to treat effects, or the overall effect of receiving a permanent increase in aid in a given year due to a change in receiving grant aid in the first year. For example $\pi_1$ can be identified by a regression of the indicator for receiving a grant aid increase on EA Grant eligibility in year 1, and all other $\pi$s estimated in a similar manner. Once the total effects and $\pi$s are estimated, then the estimates of the direct effects can be derived, and standard errors for the D estimates can be obtained by the Delta Method.

To implement the dynamic RD estimation, I first pool data from all of the 2009-2016 cohorts. I estimate the total effects via equation 2.25:

$$y_{it} = \beta_t^T \mathbb{1}\{E\tilde{F}C_i < 0\} + \psi(E\tilde{F}C_i) + \epsilon_{it} \tag{2.25}$$

I choose a subset of observations within a bandwidth around the EFC threshold. I use the Imbens and Kalyanaraman [2011] (hereafter IK) bandwidth selection procedure to obtain a bandwidth of \$3,500 EFC, but show the results are robust to a variety of bandwidths. I use the second year enrollment as the outcome for the procedure to obtain the \$3,500 figure and apply this same bandwidth to each outcome. The indicator function $\mathbb{1}\{E\tilde{F}C_{it} < 0\}$ is equal to 1 if the EFC is below the threshold and equal to 0 if the EFC is above the threshold. The function $\psi(E\tilde{F}C_{it})$

is a flexible function of $E\tilde{F}C_{it}$. In my preferred specification, this is a linear term in $E\tilde{F}C_{it}$, which is allowed to change in slope at the cutoff.[6] I also interact the EFC functions with the cohort entry year allowing the slopes to differ by cohort year. In each estimation equation, I also include fixed effects for the institution and entry cohort, as well as several control variables, including race, gender, ethnicity, and SAT math scores. Within the optimally selected bandwidths, I estimate the coefficients using local linear regressions with a rectangular kernel.

I similarly estimate the change in the probability of beginning to receive the EA Grant in a later year, corresponding to the $\pi$s of the dynamic model, using the same procedure and bandwidth. I estimate:

$$g_{it} = \pi_t \mathbb{1}\{E\tilde{F}C_i < 0\} + \phi(EFC_i) + \upsilon_{it} \tag{2.26}$$

where $g_{it}$ is an indicator for beginning to receive the EA Grant $t$ years after entry. The coefficient $\pi_t$ is interpreted as the effect of becoming eligible in year 1 on beginning to receive the EA Grant in year $t$.

I estimate the RD regressions 2.25 and 2.26 simultaneously using a seemingly-unrelated regression (SUR) procedure where the two outcome variables are stacked. This procedure obtains the $\beta_t^T$ and $\pi_t$ coefficients and a covariance matrix of the estimated coefficients. I solve the recursive model described in Section 2.5.1 for the direct effects, $\beta_t^D$, and use the Delta Method to obtain the standard errors.

Until this point, I have assumed a "sharp" RD, or that eligibility in a student's

[6] A first-order polynomial minimizes the AIC.

first year perfectly determines EA Grant receipt. In reality, some eligible students do not receive the EA Grant while a very small percentage of ineligible students appear to receive an EA Grant beginning in their first year. The requirement to complete paperwork to receive the EA Grant, as well as the late notice to students from the wait list, is likely responsible for some of the non-receipt among eligible students, while the data also may contain measurement error since student's FAFSA information is collected and provided as a snapshot in time, and can change due to verification, professional judgment, and other subsequent changes.[7] In the case of a fuzzy RD, an indicator for eligibility in the first year would serve as an excluded instrument for receipt in the first year.

In the fuzzy RD Equation 2.25, for $\beta^T$ to represent a causal effect, the relationship between unobserved variables correlated with both $Y_{it}$ and $g_{it}$ should be continuous through the cutoff. A family's ability to hire a tutor, for example, is likely related to both the student's grant eligibility and probability of graduation. For the required assumption to hold, this relationship must not change discontinuously at the eligibility threshold. If the effect of grant aid on outcomes is heterogeneous, then, under an additional monotonicity assumption, the estimated effect is a local average treatment effect (LATE), producing an estimated effect for the average type of student induced to receive treatment by becoming eligible for the grant aid.[8]

[7] Since 2012, colleges have been required to verify at least 30% of their student's FAFSAs, where they choose several items from the FAFSA for which the student must provide the documentation used to complete the application (such as income tax returns or W-2s) to the school. This can result in a change of a student's EFC. Professional judgment is a limited authority for a college to change inputs to the EFC determination in special circumstances with appropriate documentation

[8] This assumption takes on additional significance in the case of pooling students who face different eligibility thresholds. Cattaneo et al. [2016] show that the identified effect when pooling across thresholds is a weighted average of the LATEs for each threshold, weighted by the probability

I incorporate the "fuzzy" RD into the dynamic model with a first stage equation in which an indicator for receipt in the first year, $g_{i0}$, is regressed on an indicator for eligibility in year 1:

$$g_{i0} = \delta \mathbb{1}\{E\tilde{F}C_i < 0\} + \xi(EFC_i) + \kappa_{i0} \tag{2.27}$$

I include this first stage by stacking Equation 2.27 in the SUR regression, and using $\delta$, or the effect of eligibility in year 1 on receipt in year 1 to scale the direct effects. To illustrate, I compute the effect of becoming eligible in year 1 on persistence to year 2, $\beta_1^D$, by the calculation:

$$\beta_1^D = \frac{\beta_1^T - \pi_1 \beta_0^T}{\delta} \tag{2.28}$$

In Section 2.6, I provide estimates from both the static and dynamic models. In a sense, the two models provide bounds for the true direct effect on persistence. If students who become eligible for the EA Grant after the first year experience no increase in persistence due to the EA Grant, then the estimated total effect from the static will equal the direct effects. The dynamic model, on the other hand, makes the assumption that the effect of becoming eligible for the EA Grant only depends on the length of time since the start of eligibility, not the year in which the eligibility begins. This means, for instance, that the model assumes the effect on persistence to year 3 for someone beginning to receive the EA Grant in year 2 is the same as

a student faces a particular threshold and is a complier at the given threshold. Despite the local nature of the pooled effect, I continue with the estimation procedure, noting the interpretation of the effects.

the effect of persistence to year 2 for someone beginning to receive the grant in year 1. In reality, the fact that beginning to receive the EA Grant in a later year means fewer years of expected receipt would make it reasonable to assume that the direct effects could actually decrease in later years. Under this assumption, the dynamic model is providing an upper bound, in a sense, by projecting the same size effect into later years (i.e., assuming the control group students who later become eligible experience the same size persistence effects as those who begin receiving the grant in the year of entry).

## 2.6 Results

### 2.6.1 Summary Statistics

Table 2.1 shows financial and demographic characteristics of the sample of students who were potentially eligible for the EA Grant. Columns 1 and 2 show the characteristics of all eligible students and those with EFCs that placed them in the estimation sample, respectively. Columns 3 and 4 illustrate the differences between EA Grant recipients and non-recipients in the overall sample of students.

The average student in the overall sample had an AGI of nearly $77,000 and a cost of attendance of nearly $22,000. 36% of students received Pell grants and nearly 40% received institutional grant aid. Three-quarters of students had a Direct Loan, while 20% received funds from a Parent PLUS loans. Around 30% of the total sample received EA Grant awards, and 61% had positive earnings during their first academic year. The sample is 45% White, 6% Hispanic, 44% male, and had an average math SAT score of 541.

Restricting to students within the bandwidth eliminates high AGI students. Within the $3,500 bandwidth, the average AGI was around $60,000. These students were more likely to receive Pell Grants and institutional grants. While the percentages of students with Direct and Parent PLUS loans were similar, the average amount of PLUS loan was smaller for the estimating sample. Students in the estimation sample were less likely to be white and had lower math SAT scores.

Comparing EA Grant recipients versus non-recipients illustrates the necessity for utilizing a quasi-experimental design to identify the causal effects. EA Grant recipients have much lower AGIs ($50,194) than non-recipients ($87,752). They were more likely to receive other types of grant aid and were much less likely to take out Parent PLUS loans. EA Grant recipients were less likely to be white and male, and had lower SAT scores than non-recipients.

### 2.6.2 Examining the RDD Identifying Assumptions

I examine the validity of the regression-discontinuity assumptions in two ways. I first perform a McCrary [2008] density test to detect whether there are significant differences in the number of students on either side of the eligibility threshold. I then test whether there are significant differences in observable characteristics of students on either side of the threshold. Significant differences in observable characteristics at the threshold would be an indicator that correlated unobservables might also differ for eligible and ineligible students.

Looking for changes in the density of observations near the threshold is a common RD diagnostic to check for evidence of sorting behavior that could bias the estimates [McCrary, 2008]. In this particular setting, a McCrary density test takes on additional meaning, as I only observe the EFC of students who enroll in university and complete a FAFSA. If the EA Grant has an initial enrollment effect, then the estimate of EA Grant receipt could be biased by selection into enrollment in a public, 4-year colleges in Maryland.

Figure 2.2 shows a scatterplot depicting the number of students within $50 EFC bins by distance to the EA Grant eligibility threshold. The figure provides no visual evidence of a change in the density at the threshold. Implementing the McCrary test, I find a log difference in the density on each side is -0.003 with a standard error of 0.06 indicating no change in the density at the threshold.

As an additional test I examine changes in predetermined demographic variables through the threshold. To create a demographic index, I regress a model of persistence to the third year on race, ethnicity, gender, adjusted gross income, dependency status, math SAT, cohort, and institution, and use this to predict the probability of persistence to the third year for each observation. This provides a single test of demographic similarity and avoids false positive problems created by multiple hypothesis testing. I then regress the predicted dependent variable on an indicator for eligibility and the linear function of EFC. Results of this test are contained in the first column of Table 2.2. Tests for individual demographic characteristics are presented in the remaining columns. Graphical depictions with binned averages can be found in Figure 2.3.

The estimated difference in the predicted probability of persisting to the third year based on predicted observable characteristics is 0.004, and precisely estimated, with a standard error of 0.005. Eligible students who are barely eligible for the EA Grant appear to be similar to students who are barely ineligible in terms of demographics that are predictive of academic success. Columns (2)-(6) of Table 2.3 show no significant differences in individual demographic characteristics.

One potential reason why the EA Grant might not affect initial enrollment effects is due the final threshold being determined after students were admitted off of the wait list. Recipients on the wait list often learn of their eligibility after the start of the fall semester, limiting their ability to select into their first year of university based on grant eligibility. Based on this evidence, I assume that the effect of receiving EA Grant on the propensity to enroll in the first year is zero. This zero estimate is then used in the DRDD estimation of the persistence and graduation effects. To do so, I set the direct effect of EA Grant receipt in the first year on first year enrollment equal to zero.

### 2.6.3 First Stage Effects on Financial Aid

In Tables 2.3 and 2.4, I show how EA Grant eligibility affects a student's financial aid in the first year of eligibility. Figures 2.4 and 2.5 graphically show the average of the financial aid variables within $500 EFC bins by distance to the eligibility threshold. When students receive state grant aid, other changes in their financial aid can occur. Institutions offer institutional grant aid, and previous studies have found that institutions reduce their own grant aid in response to grant aid from federal, state, or private sources [Turner, 2014, Angrist et al., 2014, Bettinger, 2015]. Students also can reduce the amount borrowed through federal loan programs in response to grant aid. Understanding how a student's financial aid package changes helps understand the full impact of grant eligibility and provides a clear picture of what the actual treatment is.

The first column of Table 2.3 and subfigure (a) of Figure 2.4 show that eligible students received $1,621 more in EA Grants relative to ineligible students in their first year. The second column of Table 2.3 and and subfigure (b) of Figure 2.4 shows that EA Grant eligibility causes a 56.6 p.p. increase in the probability of EA Grant receipt.

Eligible students receive significantly less institutional grant aid ($253), which suggests that institutions react to additional grant aid from other sources by reducing the amount of aid that they provide, or "capturing" around 15% of a student's EA Grant in the form of higher prices. The 15% rate of capture is similar to the estimate found by Turner [2014]. I find a statistically significant, albeit small in magnitude, increase in Pell Grant aid for eligible students. Higher Pell Grant receipt would cause concern that eligible students are different from ineligible students in an unobservable way that is correlated with Pell Grant receipt, but in Table 2.12, I show that this difference in Pell Grant aid is due to two cohorts who faced EA Grant thresholds which were near the Pell threshold, and when I remove these cohorts the difference in Pell is no longer significant, while significant effects on other outcomes remain, including the effects on other types of financial aid, persistence, graduation, and earnings. Column (7) of Table 2.5 shows no significant difference in private scholarships received by EA Grant eligibility.

Combining the EA Grant receipt with the institutional decrease, there is a reduction of -$184 in non-EA Grant aid among EA Grant recipients, and a $1,437 increase in total grant aid received due to eligibility (Column (6)). Subfigure (c) in Figure 2.5 shows that EA Grant eligibility serves to increase the grant aid received by students above the nearly $4,000 in grant aid just-ineligible students receive. Therefore, an appropriate interpretation of EA Grant receipt is an exogenous *increase* in grant aid among students who already receive considerable amounts in grants.

Table 2.4 shows estimated effects of EA Grant (in $1,000) on student loan borrowing. These estimates are obtained by using eligibility by EFC as an instrument for the amount of EA Grant received (in $1,000). I find that $1,000 of EA Grant significantly reduces a students total amount of loans received by $347. When I disaggregate the outcome by type of loan, I find that this effect is largely driven by a decrease in the Parent PLUS loans, with a significant decrease of $232 per $1,000 of EA Grant. The effect on Direct and private loans are also negative but small and statistically insignificant.

These results indicate that first year EA Grant eligibility leads to an overall increase in the amount of grant aid received. However, understanding how a student's financial aid changes in response helps understand the overall treatment and potential mechanisms. The grant aid response indicates a substantial decrease in institutional grant aid, and thus the effect of EA Grant receipt on academic outcomes includes the fact that as student's total grant aid increase is smaller than the increase in EA Grant aid. The reduction in Parent PLUS loans may indicate a mechanism by which students' families react to additional grant aid. Parent PLUS loans are carry higher interest rates and can only be received by a dependent student's parents. This may indicate that when students receive additional grant aid, there is a shift of some of the grant aid as a transfer to the the student's parents.

### 2.6.4 Effects on Persistence and Graduation

Figure 2.6 shows graphs of the average rates of persistence to the second, third, and fourth years, as well as graduation within five years by distance to the EA Grant eligibility threshold of a student in their first year. Each of the graphs show a jump in the average probability of persisting to a given year and graduating within 5 years at the eligibility threshold.

Table 2.5 presents the results of the effects from my main specifications on academic persistence and the probability of graduating within five years of entering college. It is organized into two panels. Panel A provides the static RD estimates, and thus represents an estimate in which some of the "control" group may eventually receive the EA Grant. Panel B incorporates the dynamic model to account for control group contamination. Each panel presents the first stage estimate of the effect of eligibility on the probability of receiving an EA Grant in student's first year, a reduced form estimate of the outcome variable on EA Grant eligibility, and a Wald estimate to estimate the effect of first year EA Grant receipt on the outcome.

The dynamic estimation affects the reduced form, and as a result, the Wald estimate if there are students who receive EA Grant beginning in later years. Standard errors are provided in parentheses and a 95% confidence interval is presented for the Wald estimates in each panel. Each dependent variable is an indicator for enrollment at the same institution or graduation from a four year institution within 5 years. Persistence to the fourth year only uses data from the 2009-2015 entering cohorts, and graduation within 5 years only uses data from the 2009-2013 entering cohorts.

First year EA Grant receipt has statistically significant effects on persistence to a student's second and third years ($p < 0.05$) and the fourth year ($p < 0.10$) of 6.5 p.p, 9.1 p.p., and 5.9 p.p., respectively. EA Grant receipt leads to an increase in 5-year bachelor's degree receipt, of 4.3 p.p., but this effect is less precisely estimated and is statistically insignificant at conventional levels ($p = 0.10$). In Panel B, which reports the estimates with the dynamic estimation, the effects on persistence to the 3rd and 4th years are larger (10.2 p.p. and 7.6 p.p, respectively). The estimate for graduation within 5 years increases to 6.5 p.p., but remains statistically insignificant.

These estimates are sizable in percentage gains relative to barely ineligible students. Using the preferred dynamic estimates, the effects of receiving the EA Grant at college entry, are 7.7%, 13.6%, and 10% increases in the probabilities of persisting to the second, third, and fourth years, respectively. These results are comparable to estimated effects of need-based grants in other settings. Goldrick-Rab et al. [2016] found that an annual award of $3,500 increased graduation within four years by 4.7 p.p. (21%). Castleman and Long [2016] found that an additional $1,300 award that was rarely received past the first year led to an increase of 4.6

p.p. (22%) in bachelor's degree receipt within six years. Denning et al. [forthcoming] found that an $750 increase in first year grant aid, and a $1,012 increase in grant aid over a students academic career lead to a 3.3 p.p. (11%) increase in earning a bachelor's degree.

The EA Grant offers a max of $3,000 per year and on average spends $5,793 on each first year recipient over their entire enrollment in college.[9]. The percentage point effect of the EA Grant is comparable to the estimates of these other studies, but is smaller in percentage terms, especially when compared to the dollars spent. This may be due to, in part, the slightly higher baseline rates of persistence and graduation in my sample: 71% and 62% of my sample persist to the fourth year and graduate within five years, respectively. As described above, 55% of four-year students graduate within 5 years, and in the sample of some of the above studies, that percentage is lower.[10]

### 2.6.5 Earnings

In Table 2.6, I examine the effects of EA Grant receipt on early career earnings. Improvements in persistence and graduation are of interest to economists because of documented returns to completing a degree [Oreopoulos and Petronijevic, 2013]. Examining early career earnings allows me to test whether the increase in persistence translates into greater earnings, which, in turn, can provide insight into the type of marginal student that is being induced to continue in their education due to the

[9]To obtain this estimate, I perform the same dynamic estimation procedure as Table 2.5, but instead use the amount of EA Grant in each year as the dependent variable

[10]Castleman and Long [2016] have an average graduation rate from a four-year institution of 16% in their sample of all Florida high school graduates.

additional grant aid (e.g., like the students induced to continue education in Section 2.2).

Table 2.6 presents the results using earnings during the academic year as outcome variables.[11] These regressions implement the IV estimator without using dynamics, as in Panel A of Table 2.5, since the dynamic model cannot be applied to earnings in the same way as probabilities of persistence. Therefore these estimates are underestimates in the case of any control group students experiencing a positive effect on later earnings from EA Grant eligibility in years after the first year. Earnings in the 5th, 6th, and 7th years after entry use 2012, 2011, and 2010 as the final cohorts of the sample, respectively. This can be seen in the declining number of observations in each column. All dependent variables are inclusive of zeroes, or students who do not have any positive earnings in Maryland UI covered sectors.

Examining the first column, I first test whether EA Grant recipients have differential earnings in the first year that they receive the EA Grant. This is a test of whether EA Grant receipt changes the extent to which students work outside of school. If find a small and statistically insignificant difference of -$170, indicating that the EA Grant receipt does not appear to significantly affect how much students work during school. However, the size of the estimate and its standard error, $238, does not rule out the types of significant effects found in other studies, such as Broton et al. [2016].

Due to the decreasing sample size, the standard errors on the outcomes in

[11] Earnings are only available for students who work in the private-sector in the state of Maryland. If grant aid causes students to be more likely to remain in MD, this could have an upward bias on the earnings estimates.

columns (2)-(4) become fairly large. In column (2), the estimate on annual earnings during the 5th year is fairly small, at $526, however, columns (3) and (4) find large effects on annual earnings during the 6th and 7th years of $5,662 and $10,174, respectively. These estimates are significant at the 90% and 95% respectively. These estimates represent a 26% and 41% increases in average annual income, respectively. These estimates are large, but are also more sensitive to changes in bandwidth selection and functional form than the effects on persistence, as shown in the following section.

### 2.6.6 Robustness

I show the robustness of the main results to different bandwidths and specifications in Tables 2.7 through 2.9. Each table includes the estimated first stage effect of eligibility on receiving an EA Grant in the first year, as well as the estimated effect of EA Grant aid receipt in the first year on institutional grant aid and loans, persistence to a given year, and earnings. To keep the robustness tests simple, I examine only the effects without incorporating the dynamic framework.

In Figure 2.7, I plot the main persistence estimates by bandwidth to illustrate how the estimates vary with bandwidth choice. The solid line is the estimated effect, while the dotted line represents the 95% confidence interval. Estimates are slightly larger at smaller bandwidths, particularly less than $2,000 EFC, but are also less precise. Above a bandwidth of $2,000, the estimates are quite stable and do not show large changes with changes in the bandwidth choice.

Table 2.7 shows in greater detail how the estimate changes with bandwidth in $1,000 increments, from $1,000 to $5,000. The IK bandwidth used in the main effects estimation was $3,544. In general, it is possible to see that most estimates are fairly robust to changes in bandwidth, and, as should be expected, standard errors decrease with increases in the bandwidth. The first stage estimate ranges between 52 p.p. and 58 p.p. increase in the probability of receiving EA Grant in the first year. The amount of institutional grant captured by the institution increases with the bandwidth, from -$234 to -$512, but above $3,000 EFC find a significant decrease in institutional grant aid. The loan effect is fairly similar across specifications as well.

In all of the persistence specifications, the estimated effect decreases slightly with an increased bandwidth, but in all specifications show positive effects on persistence and graduation. The second and third year effects are statistically significant at all bandwidths, while the fourth year effect is only marginally significant at bandwidths near the IK optimally chosen bandwidth. The largest differences in point estimates due to bandwidth occur in the earnings estimates. At most bandwidths, the first year earnings estimate is small and statistically insignificant. Earnings in the 5th and 6th years varies with bandwidth choice, though the standard errors are fairly large to begin with. The earnings in the 7th year estimates are more stable across bandwidths with marginally significant effects near the IK bandwidth. From Table 2.7, most of the main results appear to be robust to large changes in the choice of bandwidth, with the exception of the earnings estimates, which vary more widely.

In Table 2.8, I show that the main effects are robust to changes in the specification and use of controls. Column (1) shows the effects without any controls and without interacting the linear EFC function with the cohort indicators. Column (2) includes the cohort interaction, Column (3) adds institution fixed effects, and Column (4) adds individual demographic, income, and test score covariates. None of the estimates of the main effects appear particularly sensitive to these functional forms and control inclusion options.

In Table 2.9, I show that my main estimates appear robust to the degree of polynomial used in the EFC function. Based on minimizing the Akaike Information Criterion, a linear function is used in the preferred specification. In Table 2.9, I show how the main results change if a quadratic or cubic polynomial is used. The use of higher order polynomials increases the standard error of the estimates, but otherwise does not appear to result in large changes in the point estimates. One exception to this generalization is the effect on institutional grant aid in the first year. The effect on institutional grant aid is positive and significant with the use of a linear function, but negative and insignificant with a quadratic, and positive but insignificant with a cubic.

In Tables 2.10 and 2.11, I show how the earnings estimates differ by choice of bandwidth and choice of polynomial. Table 2.10 shows that the effect on earnings six year after entry, for example, ranges from $11,102 at $1,000 EFC to $1,619 at $5,000 EFC. Earnings seven years after entry also vary greatly by bandwidth and the estimates at $3,000 and $4,000 EFC are even much smaller than the estimate at the $3,500 optimal bandwidth. Table 2.11 tells a similar story with the choice of polynomial. The large, significant effect on earnings in the seventh year after entry decrease to a much smaller and statistically insignificant effect at second and third order polynomials of EFC. These results suggest that the large earnings effects, while consistent with increased persistence, should nevertheless be interpreted with caution, given their sensitivity to specification.

I show additional robustness tests that test some data restrictions. In Table 2.12, I show that two years in which the EA Grant threshold was near the Pell threshold is primarily responsible for the significant difference in Pell Grant aid between EA Grant eligible and ineligible students. The main persistence results are robust to excluding these two years. After removing these two cohorts, the effect of receipt on Pell is effectively 0, while I find significant effects of 9 p.p., 10 p.p., and 7 p.p. on persistence to the second, third, and fourth years, respectively. This indicates that the main results of the paper are unlikely to be driven by any interaction with Pell Grant receipt.

Additionally, one concern with pooling all cohorts together by distance to the threshold and choosing one bandwidth is the asymmetry in cohorts represented below and above the threshold. In some years, the threshold was quite low, under $2,000, so the population of students with normalized EFCs less than $1,500 is not going to contain students from all cohorts. The same is not true above the EFC threshold. To understand whether the estimates may be sensitive to this, I limit the lower bandwidth to $1,500. Table 2.13 shows that even with the truncated lower bandwidth, I still find similar significant effects on a student's financial aid and persistence responses. This would suggest that the mix of cohorts represented below the threshold do not impact the main effect estimates.

In another robustness test, I examine the changes around the “initial” threshold, or the threshold that the state initially chooses before students are made eligible from the wait list. Students from the wait list are unlikely to see initial enrollment effects as they often do not learn about their EA Grant eligibility until the fall semester. Students initially eligible, however, find out that they are eligible during the normal financial aid cycle, or in April, and this could possibly affect their enrollment decisions.

Figure 2.8 shows the density of students around the initial threshold. I do not see visual evidence of a decrease in the density at the eligibility threshold that would indicate an enrollment effect. A McCrary [2008] density test finds a log difference in height of -0.033 with a standard error of 0.08, a large standard error that makes the estimate difficult to interpret. In Table 2.14, I show the differences in demographic characteristics of students at the initial threshold. I find no evidence of significant differences between initally eligible and wait list eligible students. Table 2.15 shows the effects of EA Grant probability, institutional grant aid, total loans, and probability of enrolling in year 2 using the initial threshold. I do not see a significant difference in EA Grant receipt or institutional grant aid, but do find a significant decrease in total loans of $867, though the effect is not statistically distinct from the effect using the wait list threshold. Learning about an award earlier might give students more of an opportunity to change the amount of loans they accept, which may become more difficult once a semester begins. I find no significant effect on persistence to the second year for learning about an award in

April rather than the fall semester. The sum total of these estimates suggest that learning about the EA Grant earlier does not lead to an initial enrollment effect, but may lead students to accept loans of smaller amounts.

## 2.7 The Effects of Program Design

Several aspects of the EA Grant program provide an opportunity to examine whether grant aid is more effective when: 1) targeted at relatively lower-income students 2) guaranteed if a student re-enrolls and 3) provided earlier in a student's academic career.

First, the varying eligibility thresholds over the years I examine offers an opportunity to examine heterogeneity by student income. Students at different levels of EFC possess different family resources, so examining differential responses for students of different thresholds amounts to examining heterogeneous effects by family resources. To do this, I implement the same dynamic estimation procedure as described in the previous subsection, but interact the EA eligibility variable with the cohort year. I use the same IK bandwidth as optimally chosen for the pooled estimation. One consequence of this bandwidth choice is a lower degree of power after looking separately at each threshold. I, therefore, group the cutoffs into two groups, "low" EFC and "high" EFC, and estimate the effects for each group. This provides an estimation of the effect of EA Grant receipt in year on persistence to a given year for low- and high-income individuals.

I supplement this analysis with another measure of income available in the data. I use a student's free-and-reduced-price meals (FARMs) status in high school as another measure of low socio-economic status and interact EA Grant eligibility with an indicator for students having FARMs status in their senior year of high school.

Second, I test for whether a grant is more effective if guaranteed in future years by comparing the effects of receipt of the EA Grant with that of receipt of the Pell Grant. The Pell Grant is the largest, and most well-known source of federal grant aid. Though there are several differences between the EA Grant and the Pell Grant, one major difference is that the Pell Grant requires an eligible EFC in every year of receipt, while the EA Grant is renewable for four subsequent years once a student becomes eligible. This enables me to estimate the RD effect of EA Grant aid and Pell Grant aid on the same sample and use the comparison to understand the effect of renewability on a program's effectiveness.

Lastly, I examine whether the EA Grant is more effective if received earlier in a student's academic career. I use an indicator for whether a student becomes eligible in their second year for the EA Grant by using their second year EFC as an instrument for beginning to receive the EA Grant in the second year and compare this estimate with estimated effects of persistence due to eligibility beginning in the first year.

### 2.7.1 Targeting by Income

In Table 2.16, I examine the differences between the "low" and "high" threshold groups as defined above to examine whether there are differential effects for students of different levels of income. Panel A corresponds to students facing low thresholds and Panel B corresponds to high thresholds. In each panel, the dynamic estimation is used.

Examining the Wald estimates, students facing low thresholds experience increases in persistence that are much larger than the point estimates for students facing high thresholds. Lower-income EA Grant recipients have an effect of 9.7 p.p., 11.3 p.p, 7 p.p, and 12.4 p.p. on persistence to the 2nd, 3rd, 4th and graduation in the 5th year, respectively. The same effects for the high thresholds are 3.1 p.p., 4.1 p.p., 5.8 p.p., and 0.001 p.p.. The estimates for the low group are statistically different from zero for the 2nd and 3rd year effect. Splitting the sample into two groups increases the standard error for each group significantly. As a result, the estimates for each group are fairly imprecise and I cannot reject the hypothesis that students of different incomes experience equal effects of grant aid.

As a supplement to the "high" versus "low" strategy, I also report the results of interacting EA Grant receipt with a students FARMS status, as a proxy for low socio-economic status. Table 2.17 shows the results of this estimation, and to keep the estimation simple, I report only the static RD results. The first row shows the effect of receiving EA Grant in the first year on persistence to years 2, 3, and 4, respectively, while the second row shows the additional effect from the interaction with receipt and FARMS status. There is a significantly larger effect for FARMS students, 8 p.p. in persistence to years 2 and 4, with a positive but statistically insignificant 4 p.p. estimate on the interaction for year 3. The positive estimates for FARMS students provide additional evidence that the effect may be larger for students of relatively lower-income backgrounds.

### 2.7.2 Guarantee of Future Eligibility

I next contrast the effects of EA Grant eligibility with those of the Pell Grant. The Pell Grant is awarded to student on the basis of their cost of attendance and EFC. As the cost of attendance is similar across students in my sample of Maryland public four-year institutions, Pell receipt is also determined according to a student's EFC. The Pell grant is awarded in decreasing amounts up to a maximum EFC threshold, but the minimum Pell award is between \$500-\$600, depending on the given year, producing a discontinuity. I compare effects using both the Pell Grant and EA Grant discontinuities in Table 2.18.

Columns (1) and (2) present the first stages for the Pell and EA Grant awards in a student's first year, respectively. I do so in dollar terms by regressing the average award on an indicator for eligibility. I find that just-Pell eligible students receive \$536 in additional Pell Grant, compared to the \$1,677 received on average by eligible EA Grant students. In Columns (3) and (4), I present the IV estimates of the effect of \$1,000 of Pell or EA Grant on persistence to the second year. I find that the EA Grant has a statistically significant ($p < .05$) 2 p.p. effect on persistence to the second year, while Pell Grant receipt has a statistically insignificant from zero -0.01 effect on persistence to the second year. A null effect on persistence for students at the upper limit of eligibility of the Pell Grant is consistent with similar work by Marx and Turner [2015], who estimate no persistence effects for Pell Grant recipients at City of New York institutions. Denning et al. [forthcoming] find positive effects of the Pell Grant recipients in Texas, but using a different margin, the "automatic zero" EFC, which affects students of such low resources that they qualify for a zero EFC. Therefore their complier population is not as directly comparable to that of this paper.

I cannot reject the null hypothesis that the two grants produce the same persistence effects, due to the size of the standard errors. Nevertheless, these results may be suggestive of a differential effect for the EA Grant versus the Pell Grant. This difference may be due to the fact that EA Grant is guaranteed for students into future years as long as they have remaining financial need and maintain full-time enrollment, while Pell Grant students must meet the eligibility threshold in any given year, making receipt of future awards less certain.

### 2.7.3 Receipt at Entry Versus Later Years

Aspects of the administration of the EA Grant program provide an opportunity to examine the effects of timing of grant aid receipt. Students who are not eligible in their first year might later become eligible if their EFC changes or the eligibility threshold falls. In Table 2.19, I use the threshold that students face in their second year, which is often different from the threshold of their first year, to identify the effects of beginning to receive the EA Grant in the second year.. In Column (1) I estimate that students who are just-eligible by EFC in their second year are 37 p.p. more likely to begin receiving EA Grant aid in their second year than students who are ineligible. This effects is not 1 due to the fact that many students receive the EA Grant in their first year, as well as for some of the other reasons of "fuzziness" identified earlier. I then estimate the effect of beginning to receive EA Grant aid in year 2 on persistence to the third year. The reduced form and IV estimates are found in Columns (2) and (3).

I find no evidence of a positive effect for students who begin receiving EA Grant aid in year 2. Beginning to receive EA Grant in the second year has a statistically insignificant -0.01 effect on persistence to the 3rd year. Though the standard error of this estimate is fairly large, at 0.04 p.p., the estimate is statistically distinct from the estimate of persistence to the 3rd year in Panel B of Table 2.5, or the effect for students who begin receiving EA Grant in the first year. This estimate would suggest that earlier receipt of the EA Grant could have larger effects on persistence.

## 2.8 Conclusion

I estimate the effect of the Maryland Educational Assistance grant on academic persistence, graduation, and early career earnings using a regression discontinuity strategy based on the level of need that the State of Maryland sets as an eligibility cutoff. Using this method, I find that receiving a renewable award in a students first year leads to a 10% increase in the probability of persisting to the fourth year, and find an statistically insignificant, but positive effect on the probability of graduation within 5 years.

Additionally, I investigate three other aspects of grant program design in order and provide suggestive evidence that other parts of a grant program's structure might influence the magnitude of the program's effect. First, using the fact that eligibility thresholds varied over time, I find that the effects for relatively lower-income students appear to be larger than those for higher-income students. Second, I compare the renewable EA Grant program, which provides a guarantee of the award in each year a student re-enrolls, with the Pell Grant, which does not have this property, and provide evidence that the EA Grant has a larger effect. Finally, I show evidence that receiving the EA Grant in a student's first year has larger effects on persistence to the third year than if a student began receiving aid in year two.

Though future studies would be necessary in providing stronger evidence about these aspects of program design, these estimates have several policy implications for the design of grant aid programs. They would suggest that a grant aid program would be best served by: (1) targeting aid towards lower-income individuals, (2) making awards renewable in subsequent years even if a student's finances were to change (and also making this salient to students when they receive the awards), and (3) structuring aid programs so that grant aid receipt begins in the first yearn in order that aid is not being directed towards those for whom there would be no persistence effect.

## 2.9 Figures

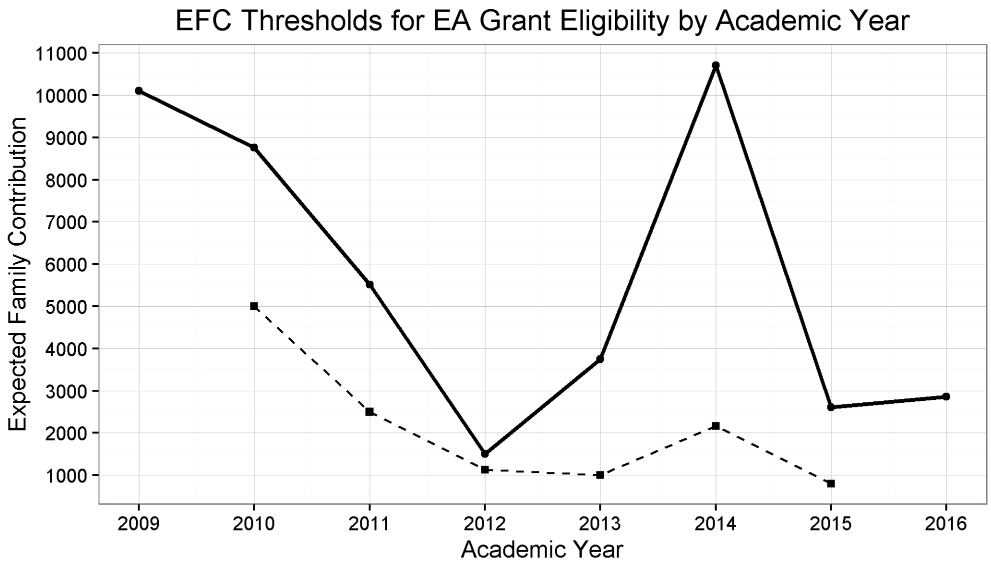



Figure 2.1: EA Grant Threshold for Eligibility by Year

Note: Figure 2.1 shows the maximum expected family contribution (EFC) for EA Grant eligibility by academic year. The solid line represents the ultimate thresholds used for the main specifications in this paper. The dashed line represents the initial cutoffs at the time that financial aid award letters are sent, and before students off of the waiting list are provided EA Grant aid awards. EFC is measured in current dollars.

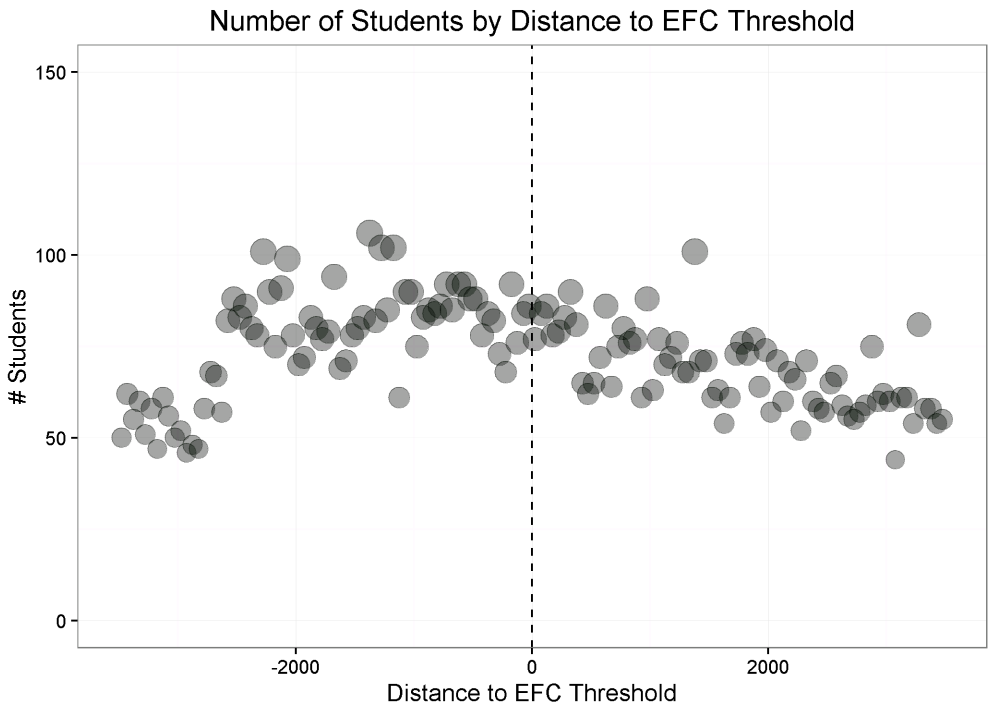
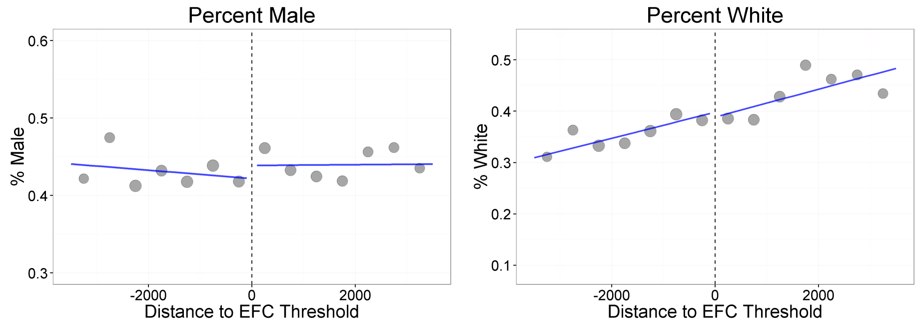
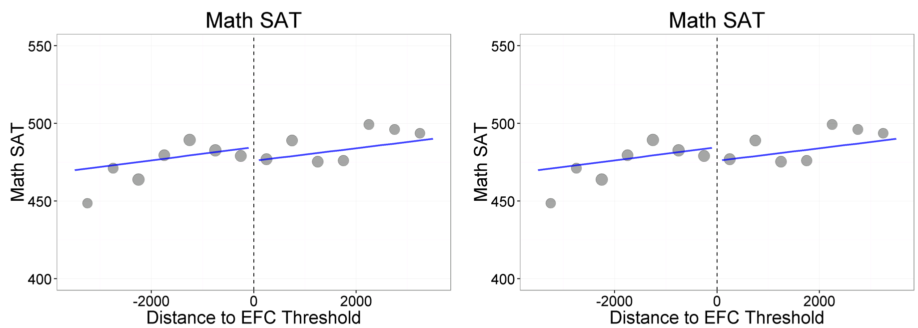



Figure 2.2: Density of Students at the Threshold

Note: Figure 2.2 shows the number of students within \$50 EFC bins on each side of the EA Grant eligibility threshold.



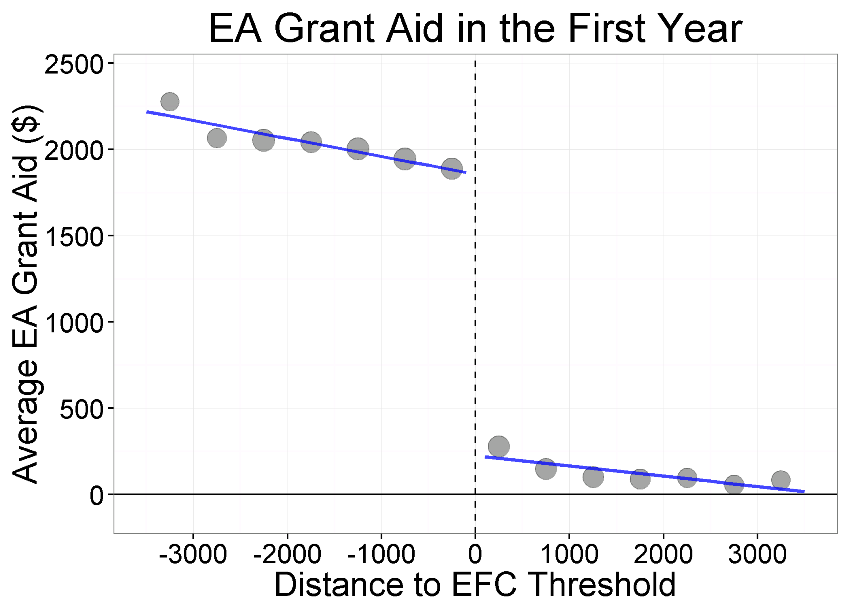

(a) Male (b) White



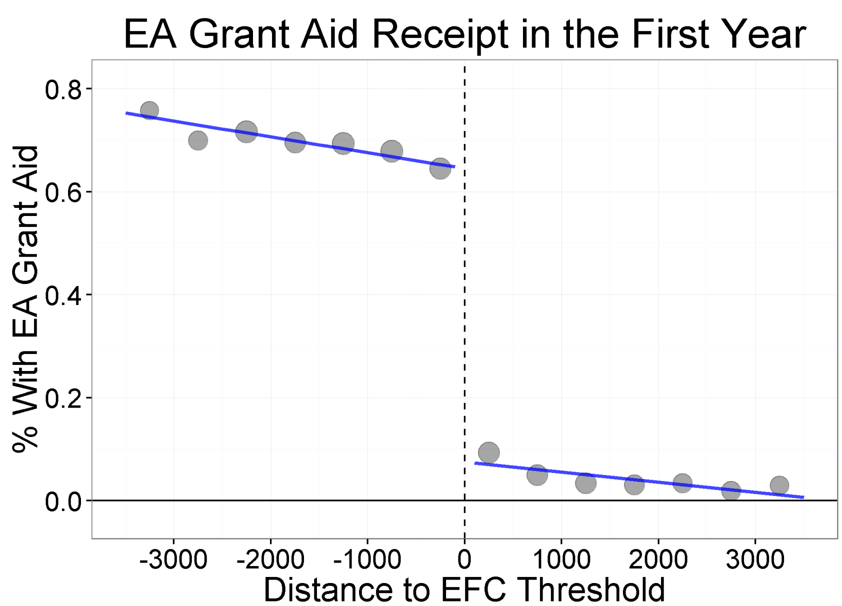

(c) SAT Math (d) Adjusted Gross Income

Figure 2.3: Changes in Demographic Thresholds

Note: Figure 2.3 shows the discontinuity in demographic characteristics at the eligibility threshold. The graph pools together all years of data. Gray dots represent the average EA Grant amount within \$500 EFC bins, while the solid line represents the estimated linear relationship estimated separately on each side of the threshold. Dollar amounts are in 2016 U.S. dollars.



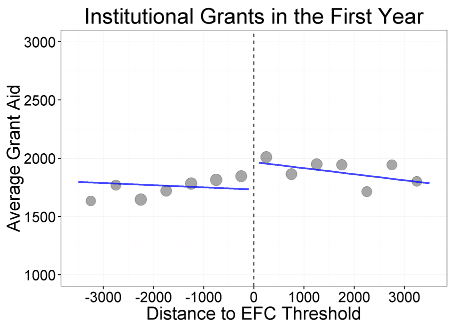

(a) Average EA Grant



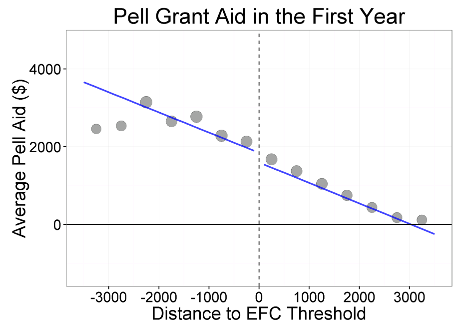

(b) Probability of an EA Grant

Figure 2.4: Change in Average EA Grant Aid and Probability of Receipt at the Threshold

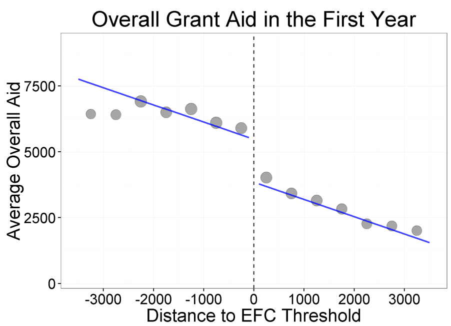
Note: Figure 2.4 shows the discontinuity in average EA Grant receipt and the probability of receiving hte EA Grant at the eligibility threshold. The graphs pool together all years of data. Gray dots represent the average EA Grant amount within $500 EFC bins, while the solid line represents the estimated linear relationship estimated separately on each side of the threshold. Dollar amounts are in 2016 U.S. dollars.



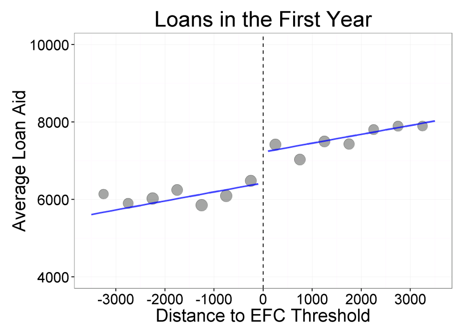

(a) Institutional Grants




(b) Pell Grants



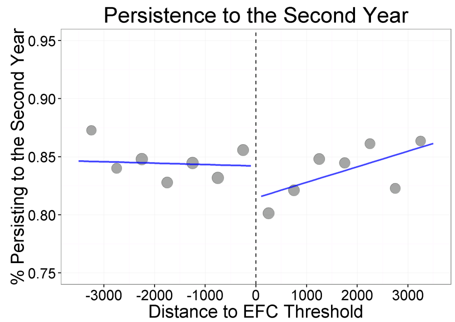

(c) Overall Grant Aid



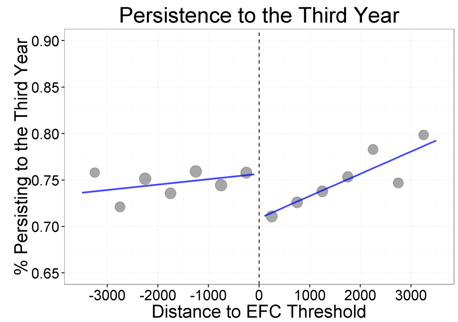

(d) Loans

Figure 2.5: Changes in Financial Aid Variables at the Threshold

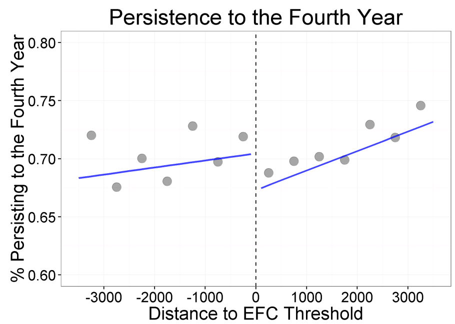
Note: Figure 2.5 shows the discontinuity in the types of financial aid at the eligibility threshold. The graph pools together all years of data. Gray dots represent the average EA Grant amount within $500 EFC bins, while the solid line represents the estimated linear relationship estimated separately on each side of the threshold. Dollar amounts are in 2016 U.S. dollars.



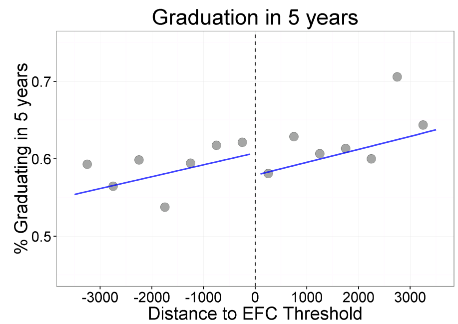

(a) Persistence to the 2nd Year




(b) Persistence to the 3rd Year



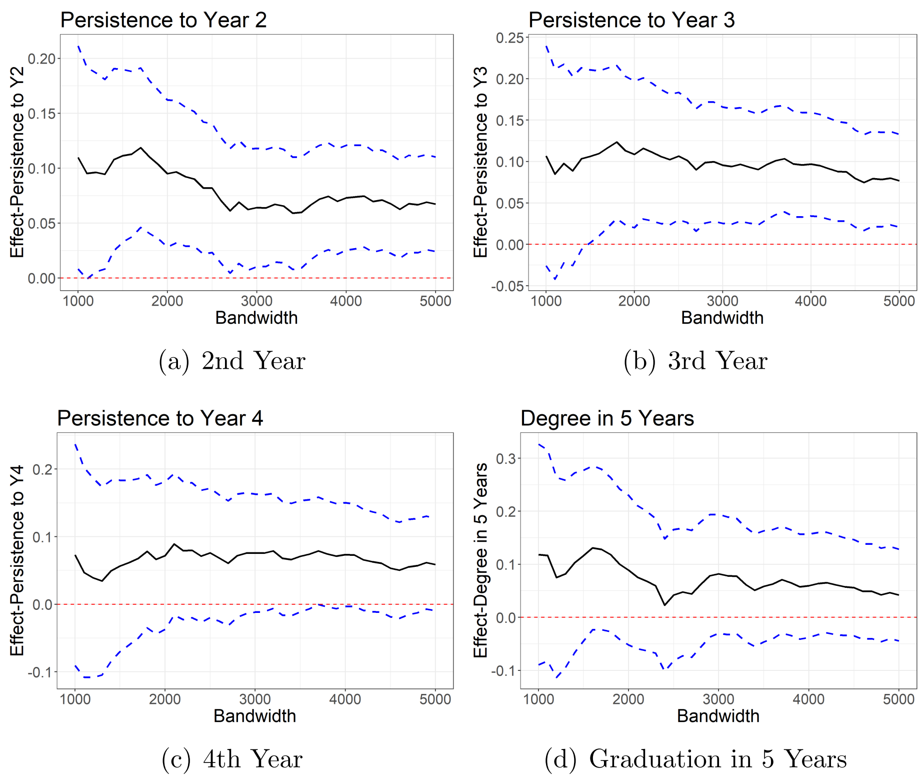

(c) Persistence to the 4th Year




(d) Graduation in 5 Years

Figure 2.6: Changes in Persistence at the Threshold

Note: Figure 2.6 shows the discontinuity in the probability of enrollment and graduation at the eligibility threshold. The graph pools together all years of data. Gray dots represent the average EA Grant amount within $500 EFC bins, while the solid line represents the estimated linear relationship estimated separately on each side of the threshold.




(a) 2nd Year

(b) 3rd Year

(c) 4th Year

(d) Graduation in 5 Years

Figure 2.7: Robustness of the Persistence Estimates to Bandwidth Choice

Note: Figure 2.7 graphs the estimated direct effect of EA Grant receipt in the first year on each persistence outcome by choice of bandwidth. The solid line represents the coefficient estimates, while the dashed lines represent 95% confidence intervals around the estimate.

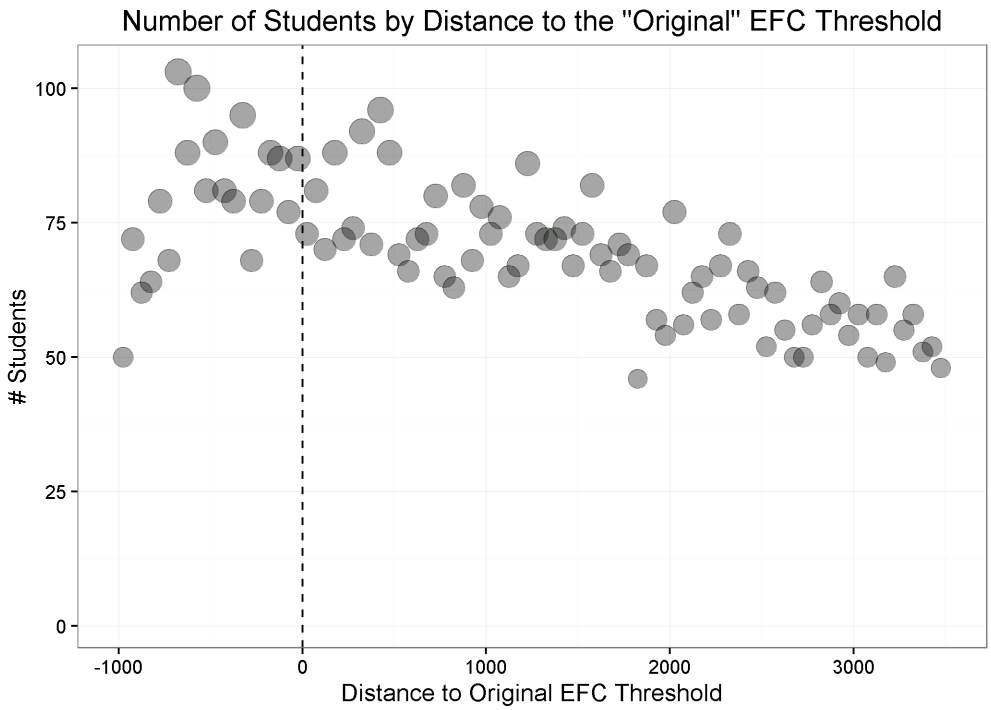



Figure 2.8: Density Plot at the Initial EFC Threshold

Note: Figure 2.8 shows the number of students, aggregated in $50 EFC bins on each side of the original EA Grant threshold applied before any students were admitted off of the wait list.

## 2.10 Tables

| | All (1) | IK Bandwidth (2) | EA Grant (3) | No EA (4) |
|---|---|---|---|---|
| *A. Financial Characteristics* | | | | |
| Expected Family Contribution | \$8,749 | 5,083 | 3,419 | 10,953 |
| Adjusted Gross Income | \$76,764 | 61,019 | 50,194 | 87,752 |
| Cost of Attendance | \$21,918 | 21,640 | 21,200 | 22,215 |
| Percentage with Pell Grants | 36 | 55 | 72 | 21 |
| Average Pell Grant | \$3,301 | 3,195 | 3,668 | 2,783 |
| Percentage with Institutional Grants | 39 | 52 | 57 | 32 |
| Average Institutional Grant | \$3,320 | 3,517 | 3,352 | 3,297 |
| Percentage with Direct Loans | 75 | 76 | 72 | 76 |
| Average Direct Loan | \$5,701 | 5,808 | 5,659 | 5,718 |
| Percentage with Parent PLUS Loans | 20 | 19 | 13 | 23 |
| Average Parent PLUS Loan | \$11,439 | 9,651 | 7,610 | 12,352 |
| Percentage with EA Grants | 29 | 39 | 100 | 0 |
| Average EA Grant | \$2,957 | 2,915 | 2,957 | NA |
| Percentage with Positive Earnings | 61 | 62 | 58 | 62 |
| Average Earnings | \$3,063 | 3,194 | 3,078 | 3,057 |
| *B. Demographic Characteristics* | | | | |
| Percentage White | 45 | 39 | 37 | 49 |
| Percentage Hispanic | 6 | 7 | 7 | 6 |
| Percentage Male | 44 | 43 | 42 | 45 |
| Average Math SAT Score | 541 | 529 | 523 | 548 |
| N | 26,170 | 10,227 | 7,656 | 18,514 |

Table 2.1: Summary Statistics Among Potentially Eligible Students

Note: Table 2.1 shows the means of financial aid and demographic variables for students in the MLDS data for those in the estimation sample (potentially eligible for the EA Grant) between the 2009 and 2016 academic years. Column (2) limits to only those within the \$3,500 EFC bandwidth. Columns (3) and (4) show the difference between EA Grant recipients and non-recipients within the full estimation sample (not restricted to the IK bandwidth).

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Predicted | Male | White | Hispanic | AGI | Math SAT |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EA Eligible | 0.004 | −0.01 | 0.002 | 0.004 | 958 | 8.11 |
| | (0.005) | (0.02) | (0.02) | (0.01) | (723) | (6.53) |
| Dep. mean \| Inelig. | 0.76 | 0.44 | 0.43 | 0.06 | 69,701 | 486 |
| Observations | 10,227 | 10,227 | 10,227 | 10,227 | 10,227 | 10,227 |

Table 2.2: Checking for Demographic Changes at the EA Threshold

Note: Table 2.2 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an RD regression using demographic characteristics as dependent variables. Each regression regresses the outcome on an indicator for having an eligible EFC, with a flexible function of EFC as a control. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of \$3,500 EFC. Refer to the methods section in the text for more details on the estimation. $^{***}$p<.01, $^{**}$p<.05, $^{+}$p<.1

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | Avg. EA Grant | EA receipt | Institutional Grant | Pell Grant | non-EA, State | Total Grants | Private Grants |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| EA Elig. | 1,621*** | 0.565*** | −253*** | 85*** | −184** | 1,437*** | 87 |
| | (45) | (0.014) | (82) | (17) | (87) | (101) | (148) |
| Dep. mean \| Inelig. | $128 | 0.05 | $1,893 | $844 | $2,769 | $2,897 | $2,132 |
| Observations | 10,227 | 10,227 | 10,227 | 10,227 | 10,227 | 10,227 | 10,227 |

Table 2.3: Effects of EA Grant Eligibility in Year 1 on Grants and Scholarships in Year 1

Note: Table 2.3 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an RD regression using grant and scholarship variables as dependent variables. Each regression regresses the outcome on an indicator for having an eligible EFC, with a flexible function of EFC as a control. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. ***p<.01, **p<.05, $^{+}$p<.1

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Total Loans | Direct Loans | Parent PLUS Loans | Private Loans |
| | (1) | (2) | (3) | (4) |
| Per $1,000 EA Grant | −347** | −56 | −232** | −37 |
| | (135) | (71) | (100) | (50) |
| Dep. mean \| Inelig. | $7,542 | $4,569 | $2,314 | $549 |
| Observations | 10,227 | 10,227 | 10,227 | 10,227 |

Table 2.4: Effects on loans in the first year (per $1,000 of EA Grant award)

Note: Table 2.4 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an "fuzzy" RD regression using loan and work-study variables as dependent variables. The first stage regresses the amount of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on the estimated EA Grant aid. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation.
***p<.01, **p<.05, +p<.1

| | *Persistence to the/Graduation in:* | | | |
|---|---|---|---|---|
| | 2nd | 3rd | 4th | Grad-5yrs |
| *A. Without dynamic estimation* | | | | |
| Effect of eligiblity | $0.037^{**}$ | $0.051^{**}$ | $0.033^{+}$ | 0.023 |
| | (0.015) | (0.017) | (0.02) | (0.025) |
| Effect of EA Grant receipt | $0.065^{**}$ | $0.091^{**}$ | $0.059^{+}$ | 0.043 |
| | (0.026) | (0.03) | (0.035) | (0.047) |
| 95% ci | (0.015 , 0.115) | (0.032 , 0.15) | (-0.01 , 0.128) | (-0.048 , 0.135) |
| *B. With dynamic estimation* | | | | |
| Effect of eligiblity | $0.037^{**}$ | $0.058^{**}$ | $0.043^{+}$ | 0.043 |
| | (0.015) | (0.019) | (0.023) | (0.023) |
| Effect of EA Grant receipt | $0.065^{**}$ | $0.102^{**}$ | $0.076^{+}$ | 0.065 |
| | (0.026) | (0.034) | (0.042) | (0.053) |
| 95% ci | (0.015 , 0.115) | (0.036 , 0.168) | (-0.005 , 0.158) | (-0.027 , 0.169) |
| Dep. mean \| Inelig. | 0.84 | 0.75 | 0.71 | 0.62 |
| Observations | 10,227 | 10,227 | 8,625 | 6,208 |

Table 2.5: Main Academic Effects- Receipt of EA Grant

Note: Table 2.5 displays the point estimates and standard errors (in parentheses) of an RD on each dependent variable. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Panel A computes the estimates without the dynamic framework of section 2.5, while Panel B incorporates dynamics in the estimation. The first stage for the effects on the 4th year and 5th year graduation are .557 and .523, respectively. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen order of polynomial. $^{***}p<.01$, $^{**}p<.05$, $^{+}p<.1$

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Earn-Y1 | Earn-Y5 | Earn-Y6 | Earn-Y7 |
| | (1) | (2) | (3) | (4) |
| EA Grant receipt | −170 | 526 | $5{,}662^{+}$ | $10{,}174^{**}$ |
| | (238) | (2,115) | (3,082) | (4,442) |
| Dep. mean \| Inelig. | 1,985 | 18,178 | 21,211 | 24,794 |
| Observations | 10,227 | 4,565 | 3,388 | 1,951 |
| *Note:* | | $^{+}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 | | |

Table 2.6: Earnings Effects of EA Grant

Note: Table 2.6 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an "fuzzy" RD regression using earnings in a given number of years after initial enrollment as dependent variables. The first stage regresses the amount of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on an indicator for EA Grant receipt. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of \$3,500 EFC. Refer to the methods section in the text for more details on the estimation. $^{***}$p<.01, $^{**}$p<.05, $^{+}$p<.1

| Bandwidth | 1000 | 2000 | 3000 | 4000 | 5000 |
|---|---|---|---|---|---|
| EA Grant | 0.52*** | 0.55*** | 0.57*** | 0.57*** | 0.58*** |
| | (0.027) | (0.019) | (0.015) | (0.014) | (0.012) |
| Institution | -234.21 | -234.53 | -328.32** | -446.71*** | -511.94*** |
| | (284.24) | (193.84) | (155.17) | (137.73) | (123.22) |
| Total Loans | -1,146.53 | -1,199.46** | -952.16** | -1,046.11*** | -805.48** |
| | (754.08) | (506.35) | (411.41) | (370.38) | (336.54) |
| First Year Earn. | -730.35 | -51.80 | -91.17 | -167.44 | -48.40 |
| | (469.49) | (318.58) | (254.83) | (225.53) | (203.87) |
| Pers.-2yr | 0.12** | 0.097*** | 0.065** | 0.071*** | 0.068*** |
| | (0.051) | (0.034) | (0.027) | (0.024) | (0.022) |
| Pers.-3yr | 0.11+ | 0.092** | 0.085*** | 0.081*** | 0.067*** |
| | (0.059) | (0.039) | (0.032) | (0.028) | (0.025) |
| Pers.-4yr | 0.071 | 0.059 | 0.062+ | 0.052 | 0.045 |
| | (0.067) | (0.045) | (0.037) | (0.033) | (0.029) |
| Degree-5yrs | 0.097 | 0.056 | 0.063 | 0.037 | 0.033 |
| | (0.089) | (0.06) | (0.048) | (0.042) | (0.037) |
| N | 3,203 | 6,269 | 8,980 | 11,106 | 13,123 |

Table 2.7: Robustness of Main Effects to Bandwidth Choice

Note: Table 2.7 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an "fuzzy" RD on each dependent variable, varied by chosen bandwidth. The first stage regresses the and indicator of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on the estimated EA Grant aid. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen bandwidth. ***p<.01, **p<.05, +p<.1

| | | | | |
|---|---|---|---|---|
| EA Grant | 0.57*** | 0.57*** | 0.56*** | 0.56*** |
| | (0.014) | (0.014) | (0.014) | (0.014) |
| Institution | -216.82 | -378.36** | -441.70*** | -448.24*** |
| | (172.50) | (177.30) | (146.12) | (145.81) |
| Total Loans | -1,554.74*** | -916.94** | -1,031.08*** | -996.75** |
| | (381.11) | (397.30) | (390.06) | (387.41) |
| First Year Earn. | -63.07 | -182.83 | -165.01 | -169.65 |
| | (228.65) | (236.84) | (240.07) | (238.91) |
| Pers.-2yr | 0.054** | 0.065** | 0.065** | 0.064** |
| | (0.025) | (0.026) | (0.026) | (0.026) |
| Pers.-3yr | 0.09*** | 0.095*** | 0.09*** | 0.09*** |
| | (0.029) | (0.03) | (0.03) | (0.03) |
| Pers.-4yr | 0.064+ | 0.063+ | 0.057+ | 0.057+ |
| | (0.033) | (0.035) | (0.034) | (0.034) |
| Degree-5yrs | 0.063 | 0.062 | 0.043 | 0.042 |
| | (0.045) | (0.047) | (0.045) | (0.045) |
| N | 10,227 | 10,227 | 10,227 | 10,227 |
| Year interaction? | No | Yes | Yes | Yes |
| Institution FE? | No | No | Yes | Yes |
| Demographic Vars.? | No | No | No | Yes |

Table 2.8: Robustness of Main Effects to Functional Form and Controls

Note: Table 2.8 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an "fuzzy" RD on each dependent variable, varied by chosen functional form and controls. The first stage regresses the amount of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on an indicator for EA Grant receipt. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen functional form. ***p<.01, **p<.05, +p<.1

| | *Degree of Polynomial:* | | |
|---|---|---|---|
| | 1 | 2 | 3 |
| EA Grant | 1,620.94*** | 1,555.77*** | 1,448.42*** |
| | (43.00) | (64.40) | (85.83) |
| Institution | -253.19*** | -12.13 | -153.11 |
| | (81.16) | (121.71) | (162.28) |
| Total Loans | -563.03*** | -515.37 | -620.27 |
| | (218.07) | (326.92) | (435.91) |
| Pers.-2yr | 0.036** | 0.051** | 0.077*** |
| | (0.014) | (0.021) | (0.028) |
| Pers.-3yr | 0.051*** | 0.045+ | 0.064+ |
| | (0.017) | (0.025) | (0.033) |
| Pers.-4yr | 0.032+ | 0.037 | 0.031 |
| | (0.019) | (0.029) | (0.038) |

Table 2.9: Robustness of Main Effects to the Degree of Polynomial

Note: Table 2.9 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an RD on each dependent variable, varied by the order of the polynomial of the EFC function. Each regression regresses the outcome on an indicator for having an eligible EFC, with a flexible function of EFC as a control. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen order of polynomial. ***p<.01, **p<.05, +p<.1

| Bandwidth | 1000 | 2000 | 3000 | 4000 | 5000 |
|---|---|---|---|---|---|
| EA Grant | 0.52*** | 0.55*** | 0.57*** | 0.57*** | 0.58*** |
| | (0.027) | (0.019) | (0.015) | (0.014) | (0.012) |
| Earn-5yrs | 1,565.24 | 1,457.82 | 780.21 | -516.91 | -714.53 |
| | (4,189.60) | (2,835.77) | (2,312.53) | (1,978.45) | (1,735.63) |
| Earn-6yrs | 11,102.28+ | 6,327.63 | 5,112.52 | 3,817.14 | 1,618.69 |
| | (6,394.65) | (4,306.89) | (3,371.38) | (2,820.89) | (2,386.03) |
| Earn-7yrs | 6,906.36 | 6,517.14 | 8,866.84+ | 7,436.27+ | 1,827.23 |
| | (10,008.74) | (6,404.14) | (4,778.99) | (3,984.55) | (3,420.38) |

Table 2.10: Robustness of Earnings Effects to Bandwidth Choice

Note: Table 2.10 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an "fuzzy" RD on each dependent variable, varied by chosen bandwidth. The first stage regresses the and indicator of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on the estimated EA Grant aid. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen bandwidth. ***p<.01, **p<.05, +p<.1

| | *Degree of Polynomial:* | | |
|---|---|---|---|
| | 1 | 2 | 3 |
| Earn.-0yr | -95.83 | -143.20 | -365.14 |
| | (129.66) | (194.50) | (259.30) |
| Earn.-5yr | 269.06 | 1,007.18 | 833.97 |
| | (1,076.98) | (1,602.63) | (2,141.44) |
| Earn.-6yr | 973.64 | 1,091.03 | 1,678.02 |
| | (631.05) | (943.46) | (1,257.17) |
| Earn.-7yr | 5,246.07** | 2,583.64 | 1,581.87 |
| | (2,248.96) | (3,369.49) | (4,483.18) |

Table 2.11: Robustness of Earnings Effects to the Degree of Polynomial

Note: Table 2.11 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an RD on each dependent variable, varied by the order of the polynomial of the EFC function. Each regression regresses the outcome on an indicator for having an eligible EFC, with a flexible function of EFC as a control. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen order of polynomial. ***p<.01, **p<.05, +p<.1

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | EA Grant | Pell | Inst. | Loans | Pers.-Y2 | Pers.-Y3 | Pers.-Y4 | Degree |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| EA Eligible | 0.58*** (60) | | | | | | | |
| EA Grant receipt | | −0.38 (35.35) | −618.26*** (167.59) | −872.89* (449.57) | 0.09*** (0.03) | 0.10*** (0.03) | 0.07* (0.04) | 0.05 (0.06) |
| Dep. mean \| Inelig. | 128 | 844 | 1,893 | 7,542 | 0.84 | 0.75 | 0.71 | 0.62 |
| Observations | 7,370 | 7,147 | 7,147 | 7,147 | 7,147 | 7,147 | 5,545 | 3,128 |

Table 2.12: Without Years 2011 and 2013 and Away from the Pell Threshold

Note: Table 2.12 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an "fuzzy" RD regression using fiancial aid, probabilities of persistence and degree receipt, and earnings as dependent variables. The first stage regresses the amount of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on the and indicator for EA Grant receipt. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. Column (1) displays the first stage estimates. Years 2011 and 2013, in which the EFC threshold is close to the Pell threshold have been removed. ***p<.01, **p<.05, +p<.1

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | EA Grant | Inst. | Loans | Pers.-Y2 | Pers.-Y3 | Pers.-Y4 | Degree |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Original EA Cutoff | 0.56*** | | | | | | |
| | (0.02) | | | | | | |
| EA Grant receipt | | −389.13** | −866.89* | 0.07** | 0.08** | 0.05 | 0.08 |
| | | (179.63) | (466.79) | (0.03) | (0.04) | (0.04) | (0.05) |
| Dep. mean \| Inelig. | 0.043 | 1,892.63 | 7,541.88 | 0.84 | 0.75 | 0.71 | 0.62 |
| Observations | 7,370 | 7,370 | 7,370 | 7,370 | 7,370 | 6,179 | 4,410 |

Table 2.13: Limiting the Lower Cutoff

Note: Table 2.13 displays the point estimates and heteroskedasticity-robust standard errors (in parentheses) of an “fuzzy” RD regression using fiancial aid, probabilities of persistence and degree receipt, and earnings as dependent variables. The first stage regresses the amount of EA Grant receipt on an indicator for having an eligible EFC. The second stage regresses the dependent variable on the and indicator for EA Grant receipt. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC, but limits the lower bandwidth to $1,500 so that the data below the threshold only includes the range in which all cohorts are present. Refer to the methods section in the text for more details on the estimation. Column (1) displays the first stage estimates. ***p<.01, **p<.05, $^{+}$p<.1

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Predicted | Male | White | Hispanic | AGI | Math SAT |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EA Eligible | −0.01 | 0.01 | 0.01 | −0.004 | 480.92 | 7.11 |
| | (0.01) | (0.03) | (0.02) | (0.01) | (654.17) | (8.96) |
| Dep. mean \| Inelig. | 0.74 | 0.43 | 0.34 | 0.068 | 51,579.68 | 473.25 |
| Observations | 6,818 | 6,818 | 6,818 | 6,818 | 6,818 | 6,818 |

Table 2.14: Checking for Demographic Changes at the Original Threshold

Note: Table 2.14 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an RD regression using demographic characteristics as dependent variables. Each regression regresses the outcome on an indicator for having an eligible EFC according to the original threshold, with a flexible function of EFC as a control. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. $^{***}$p<.01, $^{**}$p<.05, $^{+}$p<.1

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | EA Grant | Institution | Total Loans | Pers.-Y2 |
| | (1) | (2) | (3) | (4) |
| Immediately EA Eligible | −0.02 | 34.42 | −524.55$^{**}$ | 0.01 |
| | (0.02) | (103.51) | (227.48) | (0.02) |
| Dep. mean \| Inelig. | 127.85 | 1,892.63 | 7,541.88 | 0.84 |
| Observations | 6,818 | 6,818 | 6,818 | 6,818 |

Table 2.15: Examining Original Cutoffs

Note: Table 2.15 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an RD regression using demographic characteristics as dependent variables. Each regression regresses the outcome on an indicator for having an eligible EFC according to the original threshold, with a flexible function of EFC as a control. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. $^{***}$p<.01, $^{**}$p<.05, $^{+}$p<.1

| | *Persistence to the/Graduation in:* | | | |
|---|---|---|---|---|
| | 2nd | 3rd | 4th | Grad-5yrs |
| *A. Low Income* | | | | |
| first stage | 0.614*** | 0.614*** | 0.614*** | 0.562*** |
| | (0.016) | (0.016) | (0.019) | (0.024) |
| Effect of eligiblity | 0.06** | 0.07** | 0.043 | 0.043 |
| | (0.018) | (0.024) | (0.035) | (0.035) |
| Effect of EA receipt | 0.097** | 0.113** | 0.07 | 0.124 |
| | (0.029) | (0.04) | (0.056) | (0.078) |
| 95 ci | (0.04 , 0.155) | (0.035 , 0.192) | (-0.04 , 0.181) | (-0.083 , 0.277) |
| *B. High Income* | | | | |
| first stage | 0.552*** | 0.552*** | 0.552*** | 0.532*** |
| | (0.019) | (0.019) | (0.019) | (0.022) |
| Effect of eligiblity | 0.017 | 0.023 | 0.032 | 0.032 |
| | (0.018) | (0.023) | (0.024) | (0.024) |
| Effect of EA receipt | 0.031 | 0.041 | 0.058 | 0.001 |
| | (0.033) | (0.041) | (0.044) | (0.054) |
| 95 ci | (-0.034 , 0.096) | (-0.039 , 0.121) | (-0.029 , 0.145) | (-0.047 , 0.106) |

Table 2.16: Academic Effects: Low Versus High Income

Note: Table 2.16 displays the point estimates and standard errors (in parentheses) of an RD on each dependent variable, and indicator of enrollment in a given year and enrolled in a STEM major. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Each panel uses the dynamic framework of Section 2.5.

Panel A uses students that face a "low" EFC threshold for eligibility in their first year and Panel B shows estimates for students who face a "high" threshold. Refer to the methods section in the text for more details on the estimation. Each cell is a separate estimation, with the dependent variable as the row and the columns representing the chosen order of polynomial. $^{***}p<.01$, $^{**}p<.05$, $^{+}p<.1$

| | *Dependent variable:* | | |
|---|---|---|---|
| | Pers-Y2 | Pers-Y3 | Pers-Y4 |
| | (1) | (2) | (3) |
| EA Grant receipt | 0.05* | 0.08*** | 0.04 |
| | (0.03) | (0.03) | (0.03) |
| EA Grant receipt × FARMS | 0.08*** | 0.04 | 0.08** |
| | (0.03) | (0.03) | (0.04) |
| Observations | 10,227 | 10,227 | 8,625 |

Table 2.17: Heterogeneous Effects by FARMS

Note: Table 2.17 displays the point estimates and standard errors (in parentheses) of an RD of the probability of being enrolled in a given year on EA Grant receipt as well as an interaction between EA Grant receipt and whether a student had free-and-reduced-price meals (FARMS) status in high school. An indicator for FARMS status is included in the model but excluded from the table. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. ***p<.01, **p<.05, +p<.1

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Pell Y1 | EA Year 1 | Pers-Y2 | |
| | (1) | (2) | (3) | (4) |
| Pell Eligible | 535.51*** | | | |
| | (14.69) | | | |
| EA Eligible | | 1,677.38*** | | |
| | | (37.94) | | |
| Pell Grants ($1K) | | | −0.005 | |
| | | | (0.03) | |
| EA Grants ($1K) | | | | 0.02*** |
| | | | | (0.01) |
| Dep. mean \| Inelig. | $3,264 | $128 | 0.83 | 0.84 |
| Observations | 12,283 | 13,123 | 12,283 | 13,123 |

Table 2.18: EA Vs. Pell

Note: Table 2.19 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an "fuzzy" RD regression using persistence to the second year as a dependent variable. The first stage regresses the average amount Pell Grant or EA Grant on an indicator for eligibility for Pell and EA Grant respectively. The second stage regresses the dependent variable on the estimated grant aid. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC for the EA Grant and $3,200 for the Pell Grant. Refer to the methods section in the text for more details on the estimation. ***p<.01, **p<.05, +p<.1

| | *Dependent variable:* | | |
|---|---|---|---|
| | Rec. Year 2 | Pers-Y3 | |
| | (1) | (2) | (3) |
| EA Eligible | 0.37*** | −0.005 | |
| | (0.02) | (0.02) | |
| EA Grant receipt | | | −0.01 |
| | | | (0.04) |
| Dep. mean \| Inelig. | 0.24 | 0.82 | 0.82 |
| Observations | 9,363 | 9,363 | 9,363 |

Table 2.19: Receiving Grant Aid beginning Year 2

Note: Table 2.19 displays the point estimates (and heteroskedasticity-robust standard errors in parentheses) of an "fuzzy" RD regression using persistence to the second year as a dependent variable. The first stage regresses the an indicator for beginning to receive EA Grant in year 2 on an indicator for having an eligible EFC in year 2. The second stage regresses the dependent variable on the estimated EA Grant aid. Estimates are obtained by a local linear regression with a rectangular kernel within the Imbens-Karyalanaraman (2011) bandwidth of $3,500 EFC. Refer to the methods section in the text for more details on the estimation. ***p<.01, **p<.05, $^{+}$p<.1

# Chapter 3: How Effective Are Loan Repayment Programs at Drawing Physicians to Underserved Areas?

## 3.1 Introduction

The United States faces a large problem with an unequal distribution of primary medical care across regions. Less populated, rural, and poorer areas have difficulty attracting practicing primary care physicians, creating a large disparity in the availability of primary care between urban and rural areas. In 2010, rural areas averaged 6.8 primary care physicians per 10,000 residents, while urban areas averaged 8.4 [Petterson et al., 2013]. Since previous research has shown the supply of primary care physicians has been found to significantly increase the health of the population [Starfield et al., 2005], this inequity in access to primary care provides a rationale for publicly funded interventions in order to mitigate the discrepancy. In response to the unequal primary care provider distribution, federal and state loan repayment programs (LRPs) have become increasingly popular policy solutions. LRPs provide physicians with funding towards existing medical student loan debt in exchange for a commitment to provide primary care services in a federally-designated primary care shortage area for a contracted number of years.

In this paper, I estimate the effectiveness of loan repayment programs by estimating a fixed effects regression of a county's physician-to-population ratio on its eligibility for the federal loan repayment program (LRP) and on the generosity of a state loan repayment program (SLRP) in the state where the eligible county is located. This panel data approach is made possible by counties becoming eligible and ineligible for the loan repayment programs and changes in SLRP generosity over time. I estimate my model under the assumption that there are no county-level shocks correlated with eligibility or program generosity. In my main specification, I estimate that the federal and state programs do not have statistically significant effects on the physician-to-population ratio. However, when I estimate the model separately for different age ranges of physicians, I find marginally statistically significant effects of SLRPs for physicians in the "35 to 44 years-of-age" range, consistent with the prediction that loan repayment programs will be most effective at attracting recent medical school graduates, who are likely to have larger medical school debts. For this age group, I find that a county becoming eligible for an SLRP of $30,000 per year (the average among state programs) has a marginally significant (at the 10% confidence level) effect of 0.165 physicians per 10,000 residents. As this is an effect of approximately 10% of the gap between rural and urban counties, it suggests that SLRP programs may have sizable effects on the physician-to-population ratio. I find similar effects using the total amount of SLRP available, instead of simply the amount per year, and the results are also consistent when I limit my sample to only rural counties, eliminating poor urban areas that also qualify for loan repayment programs.

These estimates contribute to a greater economics literature on individuals' migration and location decisions, with particular relevance for how local labor market conditions and local government incentives enter into an agent's location utility maximization problem. I examine a unique type of place-based incentive, loan repayment, and a particular occupation, primary care physician. However, since other professions like teaching and public service law have begun using loan repayment programs similar to that of the physician loan repayment programs, this paper has relevance for the effectiveness of other loan-based incentives as well. Lastly, previous studies of physician loan repayment programs use cross-sectional surveys of participating and non-participating physicians to assess the effectiveness of the program. In this paper, I contribute to this existing literature because, while having their own strong assumptions, the fixed effects estimates are less likely to be affected by issues of selection on unobservables and omitted variables bias than the previous surveys. I also provide an estimate of the effect per $10,000 in SLRP eligibility per year, which is a new measure compared to the previous literature on LRPs, and one that is useful for evaluating the benefit of the programs.

The remainder of this paper is structured in the following way. In section 3.2, I provide more information about how LRPs and shortage areas are defined and structured. In section 3.3, I summarize previous economics literature that has examined the location decision problems as well as previous studies of LRP programs. In section 3.4, I describe a simple model of location decisions to inform the empirical strategy. In section 3.5, I describe the model used to estimate the effect of the federal and state programs on the physician-to-population ratio. In section 3.6, I present the results of the regressions, and in section 3.7, I conclude.

## 3.2 Loan Repayment Programs for Physicians

In 1972, the National Health Service Corps (NHSC) was created in response to a growing shortage of primary care physicians caused by an increasing number of graduating doctors choosing specialized fields. Initially the NHSC program consisted of scholarships to medical schools in which students contracted with the NHSC to provide primary care services in areas of need, earning a year of financial support in medical school for every year of pledged service. In the late 1980s, the program began funding loan repayment programs (LRPs) as an additional way to incentivize primary care physicians to practice in underserved areas. This national program offers a physician funds that can only be applied to outstanding medical education loan balances in exchange for service. Medical school graduates are prime candidates for these types of programs, as they accumulate a large degree of debt. In 2012, the median education debt for medical-school graduates was $170,000 [Lorin, 2013]. In the late 1980's, the NHSC also began to fund state loan repayment programs (SLRPs), which were similar programs that were administered by each state's health agency with matching funding from the NHSC. Participation in the program was optional for states, and was determined by the willingness of a state's legislature appropriate funds for the SLRP. Differences in loan repayment contracts across states is one of the major sources of variation in loan repayment exploited for this paper.

The NHSC has grown to support 9,600 primary care medical, dental, nursing and behavioral and mental health practitioners through its national loan repayment and scholarship programs. This represents a substantial federal investment that increased with the passage of the Affordable Care Act (ACA). Prior to the passage of the ACA, the total NHSC budget was $124 million but afterwards was increased to $284 million [ACA, 2013]. The NHSC funding leans heavily towards loan repayment as its primary vehicle to address the regional disparity in primary care. In 2015, 2,934 new federal LRP awards were made, compared to just 196 scholarship awards. SLRPs are smaller than the federal NSHC program, but still maintain a workforce that was approximately 600 clinicians strong in 2012 [Pathman et al., 2012]. State SLRP programs can vary in size, with some funding as few as ten physicians in a given year, and some funding more than 100. Though all NHSC funded programs offer funding to dentists, nurses, and mental health professionals, this paper focuses on program availability and funding for primary care physicians.

Both the federal loan repayment program and the state repayment programs require physicians to locate in Health Professional Shortage Areas (HPSAs), which are areas that are deemed to be underserved by primary care physicians. HPSAs can range in geographic size from a collection of a few census tracts to an entire county, and can be designated in two types: geographic- and population-based. Geographic HPSA's are primarily determined by a primary care physician-to-population ratio which is lower than 1:3,500.[1] In addition to the physician-to-population ratio requirement, the area must also show that services in contiguous areas are sufficiently inaccessible or distant. Population designated HPSAs are similar but instead are

[1] An area may also become a HPSA if it has a physician-to-population ratio of less than 1:3,000 and can demonstrate "high needs for primary care services or insufficient capacity of existing primary care providers." An abnormally high birth rate, infant death rate, or high degree of poverty could meet the "high need" requirement.

determined because a particular population group, such as a low-income population, has insufficient access to primary care, instead of the general population.[2] Population HPSAs base their physician-to-population ratios on the number of physicians that serve the population group to the group's overall population. Relevant data for the determination of a HPSA's status is converted into a score between 1 and 25, which provides a more continuous measure of the level of need.

The process of an area becoming a HPSA can begin in two ways. The first is through the HRSA's yearly process, in which the agency identifies potentially eligible HPSAs based on available physician, population, and geographic data. This list, for each state, is then sent to the appropriate state health agency (such as a state's Department of Health and Human Services or Rural Health Department) or governor, which then can approve them, at which point the regions become HPSAs. Additionally, individuals as well as state or local agencies can request or apply for regions to become HPSAs if they fit the necessary criteria. If the HRSA decides that the area fits the necessary criteria, then it is recommended by the HRSA that the region become a HPSA and the HPSA is then sent to the appropriate agencies for approval. The HRSA also re-evaluates HPSAs every three or four years to determine a new score and whether the HPSA should still be designated a HPSA.

In order to be eligible for the program, physicians must have already entered into an employment contract with a practice or hospital in the area designated as a HPSA. This is true of both the federal and state programs. Physicians can search a

[2] In explaining the barriers that define a population group's insufficient access, the HRSA states "Such barriers may be economic, linguistic, cultural, or architectural, or could involve refusal of some providers to accept certain types of patients or to accept Medicaid reimbursement."

database located at the NHSC's website, which keeps a listing of practice sites that are in eligible HPSAs. After agreeing to a contract or having been offered a position at an approved site, physicians can then apply to federal or state LRPs, but can only accept a contract with one of the two. Physicians can apply to both types of programs, but if offered a contract at both levels, they may only accept one of the offers.

This paper estimates the effect of both the federal NHSC LRP and SLRPs. The federal NHSC loan repayment program offers a maximum of $50,000 for a full-time, 2-year commitment for a site in a HPSA with a score greater than or equal to 14 or higher, and $30,000 for a score less than 14. After the initial commitment, there is also the possibility of extending the contract on a year-by-year basis for $35,000 a year. The program generosity has remained constant over the period of time studied in this paper (2000-2013). The federal program is competitive in that not all applicants are successful and an individual's contract amount depends on the HPSA score of the site. The acceptance rate of the federal LRP has varied depending on the amount of funding for the program, with nearly 100% acceptance in some years with plentiful funding and a nearly 50% acceptance rate in leaner years [APA, 2007]. Loan repayment funds are paid via a lump sum to physicians, who must show, via payment history reports from the loan servicer, that all funds have been used in payment towards outstanding educational loans. Qualifying loans include federal, state, and private loans that were used towards tuition, fees, or living expenses during undergraduate and medical school. Participating physicians cannot receive more loan repayment than outstanding loan amounts.

State loan repayment programs have a similar structure to the federal program with some major differences. States still contract with physicians to provide a loan repayment amount for a given number of contract years, and the amount must be applied towards outstanding loan balances, with similar proof. However, one major difference between the SLRPs and the federal program is an increased flexibility to set the loan repayment amount and contract years. States receive one-to-one matching funding from the NHSC for the yearly loan repayment up through $50,000 per year in loan repayment, above which states must fund all additional yearly amounts. In addition, states have the flexibility to require contract lengths longer than two years. This flexibility has lead to sizable differences between state programs in generosity, in both a total and yearly basis, which can be seen in Table 3.1. Program terms between states and within states, over time provide one of the main sources of variation used in this paper to estimate the causal effect of SLRP amounts on attracting physicians. I examine the variation in more detail in Section 3.5.2.

## 3.3 Previous Literature on Location Decisions and LRPs

A vast amount of literature in Economics studies migration and location decisions. For the sake of conciseness, I discuss only the literature with relevance to the location decision problem of physicians, and leave more general reviews of the economic literature on migration to Greenwood [1997] and Molloy et al. [2011].

Prior work on location decisions has found a substantial impact of wage differentials and local labor market conditions, like unemployment and growth. Borjas et al. [1992] shows that migration decisions depend heavily on regional differences in the return to skills, and that individuals locate in states that provide higher returns to their level of skill. In a similar fashion, Kennan and Walker [2011] examine sequences of location decisions, and find that interstate migration decisions depend heavily on geographic differences in mean wages, with workers moving toward states with higher wages, particularly if they have had an unfavorable realization of income in the current period. Local labor market conditions, such as employment and unemployment shocks, also affect migration by driving individuals out of areas with low employment towards regions with more vacancies [Blanchard and Katz, 1992].

Since the work of Tiebout [1956] the economics literature has also acknowledged the role that preferences over local public amenities, like parks, schools, and police, play in migration. Preferences for amenities can be a significant determinant of long-term migration [Mueser and Graves, 1995], and the contribution of amenities to the location decision compared to local labor market conditions increases with age [Chen and Rosenthal, 2008].

Other work has shown how particular types of migration respond to state and local spending and incentive programs. State "millionaire" taxes, for example, have been found to affect the migration of high-income individuals by causing them to move from the higher tax state to lower tax states, to some degree. [Young and Varner, 2011]. Moretti and Wilson [2013, 2015] show that state subsidies and tax incentives for biotechnology firms are effective at drawing highly productive scientists.

Some analysis of physician migration and location has previously been conducted within the medical literature. For instance, a previous study found that

51% of physicians practice in the same state in which they completed their graduate medical education, and those in general practice are more likely to do so than specialists [Seifer et al., 1995]. However, there is extensive heterogeneity by state in the percentage of physicians remaining after their education, and, interestingly, they find a weak negative correlation in the number of physicians trained per capita and the likelihood of retaining those physicians in a state. Seifer et. al. point to this as evidence that increasing the number of physicians in a state cannot be achieved as easily as by increasing the number of physicians trained at state medical schools.

Other literature has focused on what causes physicians to practice in rural locations, areas which are more likely to be underserved. Laven and Wilkinson [2003] and Ballance et al. [2009] review the medical literature on rural physicians and conclude that a physician's background and activity during medical school are major factors in the rural or urban decision. A physician having a rural background before entering medical school doubles the probability that they later locate in a rural location. Experiences during medical school can also lead to an increased likelihood of locating in a rural area, like the location of ones clerkship (learning rotations during medical school at local hospitals) and medical residency in a rural area.

Previous research on physician loan repayment and similar programs focuses on surveys of participants and analyzes their personal characteristics, motivations, and attributes of the areas in which they locate. They do this by matching loan repayment participants with non participants (or "unobligated") physicians by variables like specialty, race, and ethnicity, and observing differences. These studies provide descriptive data on they types of physicians who choose to participate in loan repayment programs.

One important finding of this literature is that physicians who are obligated, under an LRP, do typically serve in areas with more need than those who are unobligated. Pathman et al. [2000] surveyed participants of scholarship and state loan repayment programs which required service in 1999 and found that obligated physicians were more likely to work in a rural areas and with more Medicare and uninsured patients. In another paper, Pathman et al. [2004] had a similar finding concerning the physician to population ratio; obligated physicians surveyed in 1996 worked in counties with 9.1 physicians per 10,000 residents, while the average for unobligated physicians was 11.8. Jackson et al. [2003] conducted an in-depth survey of participants in West Virginia's scholarship and loan repayment programs, and similarly found that obligated physicians provided care in areas with lower physician-to-population ratios and with higher levels of uninsured patients.

These studies provided evidence that the type of physicians who participated had different characteristics than the unobligated. Obligated physicians were more likely to have higher levels of debt and report financial "concerns", and the vast majority cited the availability of the aid as a major reason for their decision to practice in that location (Pathman et al. [2004]; Jackson et al. [2003]). Jackson et al. [2003] also found that, among the West Virginia participants, LRP practitioners were more likely to have graduated from a WV medical school and had previous familiarity with their practice setting. In fact, many said that the program made it possible to work in their preferred location, which could be interpreted as a similar finding to the fact that physicians with rural backgrounds prefer rural areas.

Despite previous surveys of physicians participating in LRPs, an important question still remains with regards to the effectiveness of LRPs as a policy. To be effective at altering migration decisions and worth the cost of the programs, LRPs must be able to incentivize physicians to practice in underserved areas when they otherwise would not. Concluding that LRPs are effective based on the cross-sectional surveys above would require strong assumptions about the selection of physicians into those programs. The literature showed strongly, for example, that physicians who practice in rural areas are likely to have a rural background. If physicians would have located in the rural area without the LRP, they would be likely to do so when the LRP is in place. Then the LRP is not attracting new physicians, it is simply compensating physicians who would have chosen a rural location regardless.

To contribute to the literature on LRPs, I estimate a generalized differences-in-differences (DID) model using variations in available loan repayment amounts offered over time, which contributes to the literature on LRPs in two ways. Firstly, this model can provide an estimate of the effect on the physician-to-population ratio of an additional $10,000 in offered loan repayment amount, something not found in the previous literature, and which allows for easier evaluation of the effectiveness of the programs. Secondly, using county fixed effects controls for time-invariant aspects of counties, which, under the standard assumptions for a DID model avoids the selection bias of simply comparing participants with non-participants. This paper also contributes to the economics literature on local incentives for migration by focusing on a previously unstudied type of local incentive, loan repayment programs. Similar to Moretti and Wilson [2013, 2015] these programs are designed for a particular profession, primary care physicians.

This paper also contributes to a sparse literature on the effect of loans and the availability of loan repayment on career choice. Rothstein and Rouse [2011] study a highly selective, private institution that replaced loan assistance with grants and found that students were more likely to choose lower-pay "public interest" jobs, like teaching. Federal student loan forgiveness programs for teachers are designed to encourage individuals to become teaching professionals at high-need schools, which is similar to LRPs for primary care physicians. Literature on teacher loan forgiveness is currently limited to cross-sectional surveys of teachers, but Liou and Lawrenz [2011] find that the amount of forgiveness offered and the amount of preparation for teaching high-needs students are important determinants of program participation. Law schools also typically offer Loan Repayment Assistance Programs (LRAPs) in order to encourage students to choose public interest law. Previous literature suggests that these programs are limited in their effectiveness due to the fact that other law professions are much higher paying [McGill, 2006], but Field [2009] conducts an experiment that suggests LRAPs may be more effective if they were to be restructured as an up-front education subsidy that would have to be repaid if the student did not enter a public interest profession.

## 3.4 A Model of Physician Location Decisions and the Effect of Loan Repayment Programs

In this section, I present a simple model of how physicians decide where to locate and show how loan repayment programs enter into the decision problem. The formal model provided in this section helps shape the empirical strategy used to estimate the effect of the programs in later sections.

In each time period, physicians decide in which county to locate based on the utility provided by living and working in that county. As a utility maximizer, the physician locates in the county that provides the highest utility of all counties. Utility can be affected by county-level attributes, such as the financial incentives offered by the county (or employers of the county), population and treatment population variables, amenities offered by the county, and an individual random component.

To guide the discussion, it helps to present this model formally. Using a variant of the model of individual location decisions presented by Moretti and Wilson [2015], the utility of physician $i$ in county $c$ in state $s$ and at time $t$ can be written:

$$U_{isct} = \gamma_1 w_{ct} + \gamma_2 \max\{Fedamt_t, Stateamt_{st}\} + x'_{ct}\beta + \delta_c + \varepsilon_{isct}. \qquad (3.1)$$

In Equation 3.1 the financial incentives offered by a county are composed of the wages of the average physician in the county, $w_{ct}$, the available federal, $Fedamt_t$, and SLRP incentives, $Stateamt_{st}$. The two amounts enter through a max function, because the physician is allowed to participate in a maximum of one program. The vector $x_{ct}$ contains county-level variables that may vary over time that could influence a physicians utility, which can include indicators of the economic conditions of the county[3], like unemployment and poverty rates, and variables that affect the types patients that the physicians may treat, like the total number of births and the number of patients on medicare. In addition to these time-varying characteristics of the county, there are also time invariant characteristics of a county, $\delta_c$, which may impact residential decisions. This can be used to represent the value of a county's offered amenities, which, as an assumption, are assumed to affect all physicians' utilities in the same manner, and doesn't change over time. Lastly, there is a random, individual component of utility, $\varepsilon_{isct}$, which represents a randomly drawn preference for a particular county. Physicians then choose to locate in a county that provides the highest level of utility, or the county where $U_{isct} \geq U_{isc't}$, where $c'$ represents any other county in the U.S.

The financial incentive variables, and particularly the effects of the LRPs, are of paramount interest to this paper, and necessitate further discussion. Wages are clearly integral to any standard model of residential decisions, as with all other county aspects equal, physicians are likely to prefer a location with higher pay. They are of particular importance in studying location decisions of physicians, however, because physicians in rural and underserved areas typically earn less than in other areas [Weeks and Wallace, 2008], once controlling for work effort. To understand the effect of the LRPs, it is important to control for the fact that a county which is eligible for LRP funds is also likely to offer lower salary.

[3]This is particularly important for the time period studied in this paper, as it includes the Great Recession.

The model of Equation 3.1 allows the magnitude of the wage and the LRP effects to differ, and thus the relative contribution to $U_{isct}$ by $\gamma_1$, and $\gamma_2$ has interesting implications. It is possible, for instance, for $\gamma_1 = \gamma_2$, which would mean that a dollar of wages has the same effect as a dollar of either loan repayment incentive available. Recall that the LRP incentive is an amount paid only towards a physician's medical school loans, and so this equality would imply that a physician treats a dollar of available incentive as equivalent to a dollar of salary. In contrast, it could be the case that $\gamma_1 > \gamma_2$, or that a physician deciding on a location could place more utility weight on an additional dollar of salary compared to the offered loan repayment amount. This could be the case, for instance, if he would heavily prefer in a given year to spend less on paying down their debt, and more on consumption goods. The physician would then discount their loan repayment amount available accordingly. Discussing whether wages are preferable to physicians over loan repayment has some parallels in the literature on in-kind benefits, where the central question is whether a dollar value of an in-kind payment, like SNAP (commonly known as food stamps), is preferable to a dollar of cash [Hoynes and Schanzenbach, 2012].

In this model, it is also possible for a dollar of available LRP benefit to have a utility effect larger than the effect of wages, which would be the case when $\gamma_1 < \gamma_2$. Such an effect is less intuitive, but may exist if there are additional behavioral mechanisms through which debt repayment is highly valued. As noted in the introduction, the typical medical student graduates with a large amount of debt, combined with their undergraduate student loans, and this large amount of debt could make loan repayment more attractive than cash in several behavioral ways. In the student loan literature, there is growing evidence that students may be debt averse, or receive an additional negative utility from carrying debt [Field, 2009, Caetano et al., 2011]. Meissner [2015] tests for the presence of debt aversion in an experimental setting, finding that subjects are much less likely to borrow to smooth consumption than they are to save when faced with a utility maximization problem over many periods. LRPs may also be attractive if physicians use them as commitment devices in which they choose the contract in order to pay down their debt more quickly than they would have otherwise due to the temptation to spend earned income on consumption. Bryan et al. [2010] provide a review of the commitment device literature, and describe several evaluations of commitment device programs that are used to induce individuals to save. Similar logic could extend to the SLRP as a commitment device, which would require a participant to save, or reduce outstanding student loan debt.

The assumption of the model that physicians are choosing where to locate in each period (which in practice will be defined as year) follows a more standard model of location decisions based on per period random utility, but is fairly strong. Clearly, physicians already located in one state will both be more likely to remain in that state, and will face differential costs of migrating to different states. Loosening the assumption of the model to capture the differential effects of an origin and a destination location would be preferable, as the assumptions over the decision problem are more realistic. Such a model of location decisions is used in Moretti and Wilson [2015] to estimate the effect of taxes on location decisions of top scientists. Unfortunately, the data on physician counts is only available at the county level, and the econometric approach of section 3.5 will not be able to account for origin and destination counties, but will instead be based on county-level counts of physicians. However, the model of Equation 3.1 is more likely to describe the problem of a recently graduated physician, who is choosing where to locate. These types of physicians are likely to be participants of the LRP programs, and so the assumptions of Equation 3.1 may seem less strong in the context of recent physicians.

## 3.5 Empirical Strategy

Using the model of the previous section as a guide for how physicians choose to locate, I develop an empirical strategy utilizing county-level changes in the amounts of federal and state LRP funding available to physicians over time to estimate the effect of the programs on the county's physician-to-population.

Before discussing the empirical model, it is necessary to briefly explain why counties are used as the geographic unit. Though HPSAs, the geographic regions where practicing physicians are eligible for the LRPs, are sometimes defined as collections of census tracks, I use counties as my geographic unit of analysis for two reasons. The first is that the best data on physician counts is only available at the county level, rendering the use of data at the census track level impossible. Secondly, county borders do not generally change much over time, while HPSAs can. It is possible for some census tracts to be part of a HPSA which loses its HPSA status, and then later become a part of another HPSA. In order to keep the units constant and maintain balanced panel data, it is helpful to aggregate up to the county level. To do this, I code a county as being HPSA-active if it has an active HPSA within its borders. Many full counties are designated as HPSAs, however, a

HPSA does not necessarily cover an entire county, and a county may contain several HPSAs[4].

### 3.5.1 Fixed Effects Model

The model of Equation 3.1 shows how LRPs and variables selected based on prior research enter into the physician's utility for each county. In the model, all county-level variables are assumed to affect utility identically. Using county-level data on physician counts, I estimate the following regression to determine the effects of LRPs on the physician-to-population ratio, $MD_{ct}$ of county $c$ at time $t$:

$$MD_{ct} = \beta_1 \mathbb{1}\{Fed_{ct}\} + \beta_2 Stateamt_{st} + \beta_3 MinYears_{st} + \beta_4 wages_{ct} + X'_{ct}\Gamma + \gamma_c + \alpha_t + \epsilon_{ct} \tag{3.2}$$

Given physician-level data, the econometric implementation of the choice model in Equation (1) would produce coefficients that represent the effect of LRPs on the probability of choosing a county. In this model with county-level data, the coefficients have a slightly different interpretation, as each coefficient is interpreted as the effect of that variable on the physician-to-population ratio. Though the parameters are different, this econometric specification still estimates a measure of the effectiveness of LRPs on drawing physicians to a county, and is able to compare the effects of additional loan repayment dollars with that of wages.

[4] It is also possible for two counties to share a HPSA. This may be concerning in that the eligibility indicator for these counties are linked, and there may be some dependence between the counties sharing a HPSA, which may affect the outcome. When I analyze a sample that includes only rural counties in Section 3.6.5, it naturally eliminates many of these cases, as rural counties are much more likely to have the entire county designated as a geographic HPSA.

The dependent variable in 3.2 is the primary care physician-to-population ratio of a county, $MD_{ct}$, as increasing this ratio is the primary aim of these physician recruitment incentives. Also, using the physician to population ratio aids in interpretation of the coefficients, as counties have varying population sizes and looking at the physician to population ratio provides a more comparable measure than if I were to use the number of physicians directly. I compute the physician-to-population ratio by dividing the total number of primary care physicians in a county by the county's population.

In Equation 3.2 the main independent variables of interest are the indicator for being eligible for the federal LRP, $\mathbb{1}\{Fed_{ct}\}$, and the available state SLRP amount, $Stateamt_{ct}$, in a given county and year. For clarity, I refer to a county as being "federal eligible" if the county at least partly contains a HPSA in its borders and is thus eligible for the federal LRP, and "state eligible" if, in addition, the state has an SLRP. HPSA designations can change over time, causing the eligibility of counties for the federal and state LRPs to change as well. The federal program's existence, requirements, and program amounts do not change over this time period, which means that, when it comes to the federal program, counties can only vary in terms of eligibility, and this effect is estimated by the federal program indicator. In terms of the SLRP amount variable, there are two additional sources of variation, in addition to eligibility changes: states adopting or abandoning SLRP programs over time, and states altering the generosity and requirements of their programs. States affect the generosity of their programs by changing the total amount of loan repayment available to physicians with a minimum contract as well as the number of years required in the minimum contract. These sources of variation are examined more thoroughly in section 3.5.2.

I define the variable $Stateamt_{st}$ as the amount of SLRP dollars per year available to a physician in a county and year, obtained by using the minimum contract for physician locating in an HPSA in that state and dividing the total amount of loan repayment by the number of years of service required. Defining $Stateamt_{st}$ allows for a direct comparison to the wage variable $wages_{ct}$, which is in terms of annual salary. I also include the number of years required in a minimum contract in a state, $MinYears_{st}$ as a measure of the length of time required for a physician to complete their contract. Physicians may prefer shorter contracts which do not require as many years of service, and including the number of required years allows for a direct test of this.

I test for the equality of $\beta_2$ and $\beta_4$, the coefficients on the SLRP per year and the annual physician wages variables, respectively, using a Wald test in order to determine whether the loan repayment amount is as effective as an equivalent amount of wages. This is meant to provide some indication of whether SLRP programs are less, more, or equally effective as providing an equivalent amount of cash incentive, but there are some caveats to this exercise that make it more descriptive in nature. The first is that there is not any exogeneous variation in the wage variable, and its endogeneity means that this measure is likely to incorporate other factors that affect wages, like housing price differentials, amenity values, and competition among job seekers and employers. Secondly, because physicians must first contract with a practice or hospital in the HPSA before applying for LRP funds, there is an element of risk associated with the SLRP amount, as it is possible for a physician not to receive the SLRP amount if there are more applicants than available funds. If receiving the SLRP funds is risky, then it would be more appropriate to weight each state's program amount by the probability of receipt, in order to incorporate this uncertainty. Unfortunately, in the manually collected data on each state's program history, I do not have the number of applicants and recipients or the application success rate, because this was not something that was universally tracked by states over the period in my sample. The few states which were able to provide some of this information to me generally had very high, if not 100% acceptance rates of physicians into the SLRP program. I, therefore, make the assumption that all states have high acceptance rates, and do not weight the SLRP program amounts, but the reader should bear this assumption in mind when I test the equality of wages and SLRP amounts.

I include county and time fixed effects in model 3.2. As will be examined in more depth in section 3.5.2, a county's program eligibility and generosity change over time, and such variation enables the use of a county fixed effect. I include time fixed effects to account for general year-specific shocks in the number of physicians. As my data covers a period of time that includes the Great Recession and the passage of the Affordable Care Act, these time fixed effects are clearly needed to account for large changes that may affect physicians at the national level. In addition, I also include variables to account for some county specific characteristics that may change over time: the poverty rate and the unemployment rate. I use these variables with the intention that they can account for counties that are disproportionately affected by the Great Recession. My dependent variable of interest is uses the population of a county in its denominator, to account for population changes. I also include the birth rate as a control variable. A large number of births can be an additional criteria that makes an area a HPSA, even outside of the regular HPSA designation criteria. I include this variable as an additional regressor in order to control for the fact that counties designated under the additional birth criteria may be different from those who qualify under the normal criteria. I lastly include a measure of the percentage of the county's population that is medicare eligible, to control for the fact that physicians that treat medicare patients in a HPSA are also eligible for additional Medicare reimbursement incentives.

Equation 3.2 can be seen as a difference-in-difference-style specification, that identifies the effects of the federal LRP and SLRP from changes in program eligibility and amounts over time. Therefore, in order for the estimates of Equation 3.2 to be used as causal evidence of the effect of SLRP program amounts, I require the assumption that there are not any other county-year shocks concurrent with a county's change in eligibility status or generosity, which is the typical assumption of any difference-in-differences (DID) style estimation technique. This assumption requires that there are no other additional treatments concurrent with LRP program eligibility that would have an effect on the physician-to-population ratio. Though this "parallel trends" assumption on the counterfactual cannot be verified, research that use DID specifications typically present evidence pre-treatment of similar pre-trends in the outcome variable between the treatment and control group in order to give some credence to the strong assumptions. In section 3.6.2, I use an econometric test following Autor [2003] to provide evidence in favor of the DID assumption in the case of the model of Equation 3.2.

To analyze the overall results of the program on the physician-to-population ratio, I use physicians of all ages in the calculation of $MD_{ct}$. In additional specifications, I analyze heterogeneity of the LRP effect by physician age. The average age of loan repayment recipients in the studies by Pathman et al. [2004] and Jackson et al. [2003] is 33 and 40, respectively, and Pathman et al. [2000] find a median age of 37. This young average age is consistent with the idea that these programs would be most effective for younger physicians who have recently completed medical school, and have high amounts of debt. In the data, counts of physicians are arranged in 10-year bins beginning at age 35 (the first bin includes all physicians younger than 35). Based on this previous literature I hypothesize that the effect of the LRPs, if present, would be strongest among physicians 35 to 44 years old. Conversely, I expect the programs to be ineffective for physicians in older age ranges, as they likely have less medical school debt remaining. I examine these hypotheses by estimating the effect on each age range in separate regressions.

### 3.5.2 Identifying Variation

In order to use a county-level fixed effect specification to test the effect of LRP programs on the physician-to-population ratio, I exploit several different sources of variation in the amount of loan repayment available in a county over time: counties that change their eligibility for the federal and state LRPs, states that adopt or abolish programs over time, and changes in SLRP generosity.

Figures 3.1 and 3.2 show the variation in eligibility status over time. First, Figure 3.1 plots the number of counties that are eligible for the federal LRP and those eligible for both the federal and state programs over time. We can see that over time the number of counties that are eligible has increased substantially, with a drop after the year 2011, and that this occurs for both types of eligibility. It is also possible to observe that the number of federal eligible counties increased more quickly in the early 2000's than the state eligible programs and rose more steadily until 2011, while the number of state eligible programs had a steeper increase between 2006 and 2011.

Figure 3.2 examines this variation in a slightly different way, by looking at the number of years that a county has been eligible for each type. Immediately apparent by this graph is the large number of counties that were either ineligible or eligible for the entire length of the sample. There are substantially more programs that were never state eligible than were never federally eligible. The regression described by (2) uses the counties found in between these two values, those that have had their eligibility change at some point during the sample years, or those with 1-12 years of eligibility. Among these counties, we can see that, in general, the number of federal and state eligible are similar over time, but there are more counties federally eligible for 11 and 12 years than state eligible, while more counties state eligible for 3 and 10 years than federal eligible. This variation identifies the effects of the two programs separately.

The second source of variation is changes in SLRPs over time, which can take different forms. Some states programs do not extend over the entire 2000-

2013 period, but either begin or end during this period. In addition, the maximum amounts for a minimum contract or minimum contract length can change over time. Table 3.4 shows the list of state programs, the minimum contract lengths, and the amounts. Over the period 2000-2013, 8 states change amounts and 10 began or ended their programs. There were 6 states who never had an SLRP program over this period, and there are 12 states for which I was unable to attain the necessary information or the program was structured so differently that they cannot be used as a comparison[5]. The 6 states are included in the sample, but provide no identifying variation, but the 12 states with insufficient information are removed from the data. Two states, Texas and South Carolina, operated their own loan repayment programs outside of the federally-backed NHSC program, but still used the federal HPSA designations for determination of eligibility, and are thus included in this sample.

An explanation for this increase in the number of eligible counties over time is a general decreasing overall trend in the physician-to-population ratio. This trend can be seen in Figure 3.3, which shows the overall trend in the physician-to-population ratio across all U.S. counties, as well as the trend in counties which always have an active HPSA, and those who never have an active HPSA. Over the period 2000-2013, the physician-to-population ratio in the U.S. decreased overall, which would be an explanation for why more counties would contain active HPSAs over time, and simultaneously becoming eligible for LRP and SLRP programs. Both counties that

[5]Ten of the states are excluded because I have yet been unable to obtain sufficient information on the offered SLRP amounts between 2000-2013. Alaska is excluded because it faces a unique challenge in attracting physicians due to its location and climate, and while its loan repayment programs are fairly generous, there are also other cash incentives also included in the program. Oklahoma is also excluded as it has a state program that doesn't use federal funds, and also uses its own underserved designations besides the federal HPSA designations.

never had and always had active HPSAs have decreasing physician-to-population ratios, but the decrease among the always inactive counties is slightly steeper.

As a summary measure of all the useful variation in program eligibility and generosity, the plots in Figure 3.4 show the amount of loan repayment that each county is eligible for over the period 2000-2013. Lighter colors indicate higher loan repayment amounts, as measured by the average LRP amount used in equation 3.2. These figures demonstrate both sources of variation. Particularly in the earlier and later years, it is possible to see ineligible counties in participating states becoming eligible. Also, changes in program amounts are more visible in this fashion, and it is possible to see the states whose programs begin (New York, Idaho, or Montana, for example), end (South Carolina), or increase in generosity (Texas).

### 3.5.3 Data Sources

Each state keeps its most recent SLRP program information concerning the minimum number of years required and the maximum amount of repayment available online for physicians to view. However, to know the program specifics going back further in time, data for each state's program was collected manually by email and phone contact with each state's agency that administers the program (typically the state's Department of Health and Human Services). A table representing this information, which includes the state, years in which the SLRP program was active, the program name, the minimum years of service required, and the maximum repayment amount for that minimum contract is available in Table 3.1. The data collected in this table covers the years 2000-2013, which is the period of time used in this study, due to restrictions on the data availability of the number of physicians.

The HRSA provides a historical list of HPSAs, including the year in which they were designated as a HPSA and the year it stopped being a HPSA, if applicable. I then aggregate the HPSAs to the county level by the county FIPS code, and designate a county as having an active HPSA if either the entire county is an active HPSA or the county contains HPSAs within its borders. Ideally, I would be able to weight each county by the scores of the HPSAs inside of them, but one limitation of this data is that the HRSA only keeps the most recent HPSA score for each HPSA, and not historical scores over all available years. Since the HRSA continually reviews scores and replaces them, I cannot use the score to provide a more continuous measure of program intensity and county need. I code a county as being federal eligible if there is an active HPSA within its borders and additionally state eligible if the state has an SLRP.

The National Health Resource Administration's Area Health Resource Files (AHRF) provide county level information on the number of physicians, population, poverty, unemployment rate, and total births variables. The AHRF breaks down the number of physicians in a county by age and specialty, and I thus use the number of physicians in the fields of General Practice, Family Practice, General Internal Medicine, and Pediatrics in my count of physicians. I focus on these specialties because they are common to all state programs in terms of eligibility, and are specialties that fall under NHSC federal LRP and SLRP definitions of primary care. I also restrict to physicians that are involved directly in patient care, which excludes physicians that are only engaged in teaching and administrative positions. The

AHRF does not have a count of physicians for the year 2009, and thus 2009 was removed from the data.

Wage data for physicians at the county level is difficult to obtain, and I use two measurements of wages, separately, to incorporate wages into regression 3.2. The first is a measure from the Bureau of Labor Statistics' Occupational Employment Statistics (OES), which gives a three-year survey average of the mean annual wages for a given occupation. I use the average wages of Family and General Practitioners (occupation code 29-1062). One disadvantage of using this measure is that they are only available at the Metropolitan (and non-Metropolitan) Statistical Area level (MSA), and so I match counties to MSA and non-MSA using a Census crosswalk. This does, however, mean that the wage measure is less accurate than if it had been measured at the county level, especially for non-MSAs which can sometimes cover many counties in a state. Another disadvantage is that non-MSA wage data is only available for the years 2006-2013. In other regressions, not reported here, I use estimates of wages from the BLS's Quarterly Census of Employment and Wages (QCEW). This wage measure uses industry classification and does cover all of the years in the sample. However, the wages include an average of a physicians office, which includes non-MD staff, and the counties represented (and not all are) seem to skew towards more highly populated areas. Likely due to these two factors, the point estimates of the regression are similar, but contain much more noise, and so I focus on using the OES wages.

Lastly, I use the Urban-Rural Classification Scheme for Counties from National Center for Health Statistics (NCHS) to determine whether a county is rural or urban.

I use this variable for a robustness check on the main results. It classifies counties according to the population size and relationship to an MSA. I code counties with a classification of "small metro", "micropolitan", and "non-core" as being "rural", and those with "medium metro", "large fringe metro", or "large central metro" as being "urban".

## 3.6 Results

### 3.6.1 Summary Statistics

Summary statistics for the dependent and independent variables used in the regressions can be found in Table 3.3. The data covers the period 2000-2013, and in the full model, we can see that there are 29,522 county by year observations, which decreases to 13,793 when years before 2006 are excluded. As detailed in Section 3.5.3, the more restricted year range is used to include a measure of wages, which are only available beginning in 2006. On average, counties had 116 primary care physicians and 14.6% of its population in poverty. The average unemployment rate is 6.3%, although this variable changes significantly over this particular time period. The averages wages for primary care physicians in the data is $184,021, though this has a large standard deviation ($25,944), meaning that the wages vary across counties and through time. The physician-to-population ratio is coded in terms of physicians per 10,000 residents. Thus in Table 3.3, we can see that the average is 8 physicians per 10,000 residents, and also that this varies significantly (a standard deviation of 3 physicians). The table also shows how this physician-to-population ratio is distributed across different physician age groups. Physicians are most heavily concentrated in the 35-44 and 45-54 categories, with 2.13 and 2.34 physicians per 10,000 residents.

In some specifications, I limit the sample to counties to counties designated as "rural" based on the criteria noted in Section 3.5.3, which limits the sample to 22,453 county by year observations. In some respects, this limited sample of counties differs from the overall sample. Most strikingly, the average number of physicians and the population fall to approximately 30 and 32,000, respectively. The physician to population ratio falls from just above 8 physicians per 10,000 residents to 7.7. In the average sized county, this is a difference of about 10 physicians.

### 3.6.2 Evidence on the Parallel Trends Assumption

Though the differences-in-differences style assumption of parallel trends between counties that change eligibility or SLRP amounts and those who do not is fundamentally untestable, the empirical standard is to provide evidence that the treatment and control groups do not differ significantly in trends of the dependent variable before the treatment. When using a generalized differences-in-differences, pre-trends can be difficult to visualize, due to the fact that units are affected by the treatment at different times, which is true of LRP eligibility. In lieu of visual evidence, I follow the method of Autor [2003], who uses "leading" and "lagging" indicators of treatment indicators to test for differences in pre-trends. In a general panel set up (with unit $i$ and time $t$), this corresponds to the model:

$$y_{it} = \sum_{j=-13}^{12} \delta_j D_{ij} + \gamma_i + \alpha_t + \epsilon_{ct} \tag{3.3}$$

In equation 3.3 the fixed effects model uses a sequence of indicators, $D_{ij}$, which are equal to 1 in the year that is $j$ number of years until the treatment. The test is then whether each $D_{ij}$ is significantly greater than 0 for all $j < 0$. If so, this would be some evidence that there is a unit by year shock occurring before the treatment year, which is affecting the estimates.

To test for this in my model, I estimate a version of equation 3.2 in which I replace the federal and state treatment indicators with dummy variables designating the number of years until the county becomes eligible for the federal and the state LRPs. I include separate sequences of indicators for the federal and state programs, and leave out the time period immediately before treatment $(t-1)$ as the excluded category. In Table 3.5, I present the estimates for of coefficients on all indicators in periods before the treatment, in order to allow for a legible table. In general, the estimates are not statistically significant, and do not seem to show any type of pattern that would be concerning for the differences-in-differences specification. This provides some evidence in favor of the validity of the parallel trends assumptions.

### 3.6.3 Main Results

Table 3.6 displays the results from the regression model described by equation 3.2, and which compose the main results of this paper. The table has three columns, the first (column (1)) uses the data that includes the years 2000-2013 while the second and third columns restrict the data to the years in which the OES wages variable was available. Column (2) does not include the wage variable, while column (3) does, which allows me to see separately how the data restriction and the inclusion of the wage variable affects the estimates. Variation in the available SLRP amount varies at the state level for all eligible counties, and thus I cluster my standard errors at the state level in order to allow for within-state dependence in the error term. F-statistics for the joint significance of indicate a better fit for the models that include the 2006-2013, though none of the regressions reject the null hypothesis that all of the independent variables are equal to zero. The better fit for the 2006-2013 period may reflect the fact that eligibility status changed consistently and dramatically over this period and that several large states instituted programs during this period, relative to the 2000-2005 period.

Before discussing the coefficients and hypothesis tests, the interpretation on the "State Eligible Amount per Year" variable needs to be explained. The physician-to-population ratio is scaled to represent the number of physicians per 10,000 residents of the county, and the loan repayment variable is scaled such that an increase of one unit is an increase of \$10,000. Thus the coefficient has the interpretation of the effect of a \$10,000 per year increase on the number of physicians per 10,000 residents. Wages are similarly scaled to provide a similar interpretation of the coefficient.

Table 3.6 shows a negative estimated effect on the indicator for being eligible for the federal program, with large standard errors. The coefficient on the SLRP amount per year is 0.041, 0.031, and 0.033 in columns (1), (2), and (3) respectively, but none of these estimates are statistically significant at any conventional level of significance. The estimates for the minimum program length required for a state program are negative, but similarly statistically insignificant. Most other coefficients are also not statistically significant. This could be due to the inclusion of county and year fixed effects, which may explain most of the differences between counties and over time in these variables. The size of the medicare eligible population is significant in the first two columns, but becomes insignificant once wages are included. This could be due to the fact that physicians in underserved areas receive more of their wages through medicare reimbursements, but when I account for wages, I control for this fact.

Since previous literature found that physicians who participated in loan repayment programs were found to be younger than those who are not [Jackson et al, 2003], I hypothesize that the SLRP effect would be largest among younger physicians, or those who would be in the age range that would indicate recent graduation from medical school. In Table 3.7, I present the model of Table 3.6, Column (3) estimated separately for the age ranges, "Under 35", "35 to 44", "45 to 54", "55 to 64", "65 to 74", and "Over 75", in columns (1) through (6). In this table, the federal program eligibility is statistically insignificant in all regressions, but the state program amounts are positive and marginally statistically significant (at a 10% confidence level), with an effect size of 0.055 per $10,000 of SLRP available per year for the "35 to 44" years age group. This provides some evidence with the hypothesis that these programs are more attractive to younger physicians.

The effect of the SLRP minimum number of years required is generally negative, but statistically significant, except for the "45 to 54" group. There is a highly significant negative effect for this group, which may indicate that physicians that the next age range up from the group that seem to respond to the program may be averse to contracts that require them to stay longer in an eligible location. Since the SLRP program effect is not significant for this group, however, this results is only suggestive.

For the "35 to 44" age group, the effect of an increase in wages is positive, with a 0.015 effect. Since the program effect is marginally significant in this case, I perform a Wald test of linear restrictions to determine whether the effects are of statistically different magnitudes. As discussed in Section 3.4, a significantly higher effect for the LRPs would indicate a behavioral response to the program amounts, such as debt aversion or mental accounting. The Wald test does not reject the null hypothesis that the effect of the SLRP program amounts per year and the effect of annual wages are the same (with a p value of 0.19). However, the larger coefficient on the SLRP in conjunction with the Wald test may at least suggest that the loan repayment effects on those individuals most likely to respond are not less than that of the response to wage increases.

In order to properly understand the size of the effect on 35- to 44-year-old physicians, it is helpful to use change of a likely size given the differences in loan repayment amounts both within and between states. A change of $10,000 per year would move a state from the 50th percentile to just over the 75th percentile of per year program generosity, and thus makes for an appropriate comparison value. Based on the results in Table 3.7, this would result in a change in the number of physicians between 35-44 per 10,000 residents by 0.055. The average county's physician-to-population ratio of this age range is is 2.12, which means that the effect of a $10,000 increase would amount to a 2.5 percent increase. Using the median population in the data, which is 25,287, this would be an increase of 0.14 of a physician in the median county. A state moving from having no SLRP eligibility to a $30,000 per year SLRP (the average across state programs) would correspond to an increase in the physician-to-population ratio of 0.165. In the introduction I noted that urban areas had had approximately 8.4 physicians per 10,000 residents while rural areas had 6.8. This amounts to a gap of 1.6 physicians per 10,000 residents. The effect of a program becoming eligible for $30,000 in SLRP per year then constitutes 10% of the gap between urban and rural counties. Placed in this context, the estimate is a sizable effect.

In Tables 3.8 and 3.9, I recreate tables 3.6 and 3.7, but show the effect sizes when no other regressors are included in the model except for the SLRP amount. Table 3.8 shows that the effects are mostly unchanged in the model that is not broken down by age. A negative, but statistically insignificant effect on the Federal program indicator, and positive but statistically insignificant effects on the state program amount available. When examining by age in Table 3.9, the same positive effect is found on the location decisions of 35-44 year olds. This effect is 6.2 and is significant at the $p < 0.05$ level. This shows more evidence of a positive effect, that is somewhat diminished when wages are included in the main model specification.

### 3.6.4 Using the Overall Loan Repayment Amount

In addition to the regression results of Table 3.6, I also use the total amount of loan repayment for a minimum SLRP contract to replace the loan repayment amount per year variable. Though seemingly similar, the change from the per year variable to the overall amount variable could have slightly different effects. For example, Texas had a minimum contract of $61,000 over 3 years while Vermont had a minimum program of $40,000 over 2 years. In my previous specification, these would have appeared to have nearly the same values in the loan repayment variable, but when examined in terms of the total amount over the minimum contract years, there is a difference.

Table 3.10 shows the main regressions using this new definitions of the SLRP amount. The federal program is again estimated to have a negative effect, but the estimates are quite imprecise with large standard errors. As in Table 3.6, the effect of the SLRPs are also insignificant.

Similar to the SLRP amount per year variable, there seem to be significant effects when the model is estimated separately for different age ranges. In the "35 to 45" category and the "45 to 54" category, the effects on the total SLRP available to physicians are 0.014 and 0.011 per $10,000 in total contract, which are statistically significant at the 1% and 10% levels, respectively. The minimum program length is not statistically significant for the "35 to 45" group, but is highly significant for the "45 to 54" category, with a coefficient of -0.0910, which is the effect on the physician-to-population ratio of this age group for an additional year of required service in the minimum SLRP contract. This may again be evidence that the program does have a small effect on the next age group above the most likely respondents, but the older groups respond very strongly to longer required commitments.

Though the effects of the total SLRP amount available are smaller in estimated size, they are of a similar magnitude when placed in context. The average total minimum contract amount is almost \$55,000. Going from no SLRP to the average of \$55,000 total SLRP amount would result in an increase of 0.077 physicians between the ages of 35-44 per 10,000 residents, a 3.5% increase in the physician-to-population ratio of this group, slightly smaller than the effect of the SLRP amount per year. This is some evidence that increasing the amount of loan repayment per year has a very similar effect to increasing the total amount of loan repayment.

### 3.6.5 Examining Rural Counties Only

The summary statistics of Tables 3.3 and 3.4 show that rural locations can be very different in terms of population, the total number of physicians, or the physician-to-population ratio. As an additional examination of heterogeneous effects, I limit my sample to only rural counties to examine whether there is a different effect for rural counties. These results are presented in Tables 3.12 (all ages) and 3.13 (broken down by ages.

Table 3.12 looks very similar to the previous sections in that there is not a statistically significant effect of either the federal or SLRP programs. Again, however, looking by age groups finds heterogeneous effects. In Table 3.13 the effects of the SLRP are insignificant for the "35 to 44" age group, but are positive and statistically significant for the "45 to 54" age group. The effect estimate of \$10,000 per year is 0.060 and of a similar effect size to the effect of \$10,000 per year on 35- to 45-year old physicians in the main results of table 3.7. Interestingly, it seems that an older age group is more responsive to the state programs among rural counties. As in previous results, this age group responds quite negatively to an additional year of contract terms (an effect estimate of -0.125) indicating a preference for shorter commitments.

## 3.7 Conclusion

This study uses variation in a county's eligibility for federal and state loan repayment programs, as well as change in state program generosity over time to estimate the effects of federal and state LRPs. I find no statistically significant results for either program in the main regressions that grouped together all physician age ranges, however, when I estimate the model separately by age range, the age range that corresponds most closely to recent medical school graduates (ages 35-44) had a marginally significant effect of SLRPs on the physician-to-population ratio. Using this estimate, I calculate that the effect of the average SLRP would amount to a 10% reduction in the physician-to-population ratio gap between rural and urban areas. When I use the full SLRP amount, instead of the amount per year, I find similar results, as well as when I restrict my sample to only rural counties.

The lack of significance for federal programs, but a possible positive effect of SLRPs among young physician warrants future research into why the state program could potentially be effective when the federal program is not. It is possible that states may include more outreach and recruiting than the national program, or that states offer loan repayment programs with more attractive terms than the federal programs. Future research on SLRPs may also want to study the effects of offering a direct cash incentive versus loan repayment, as a more direct test of physicians' preference for debt repayment.

## 3.8 Figures

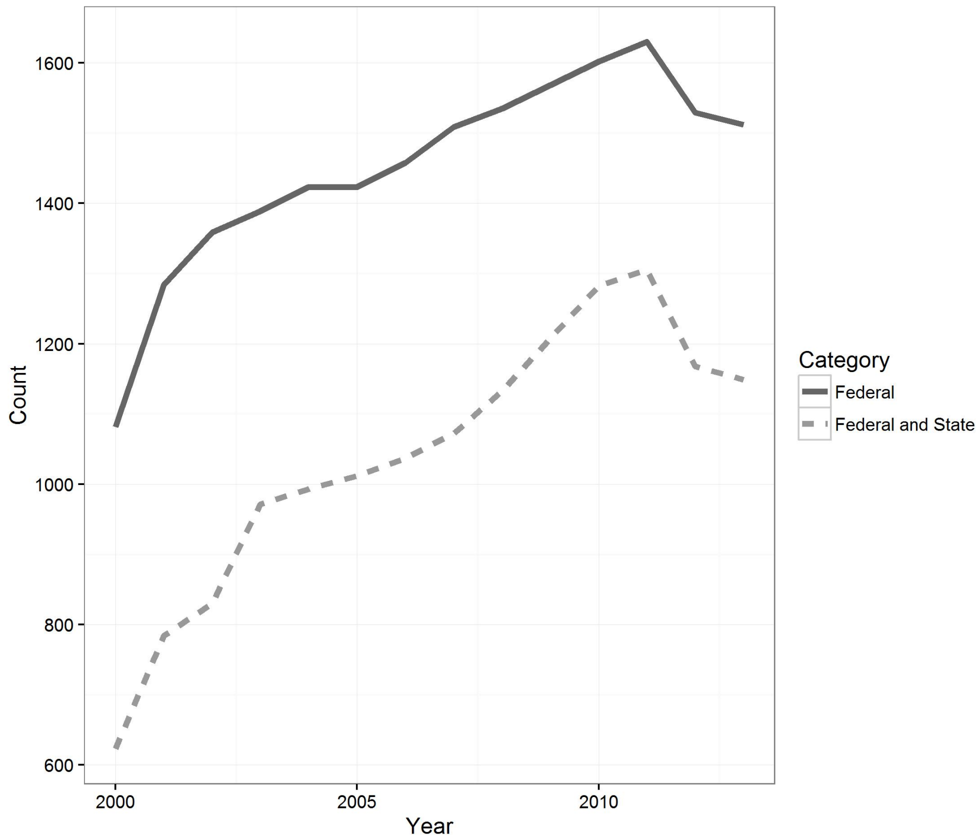



Figure 3.1: Changing Program Eligiblity Status Over Time

Notes: This figure shows the number of counties that are "eligible" for either only the federal LRP or both the federal and a state LRP over time. An eligible county contains an Health Professional Shortage Area (HPSA).

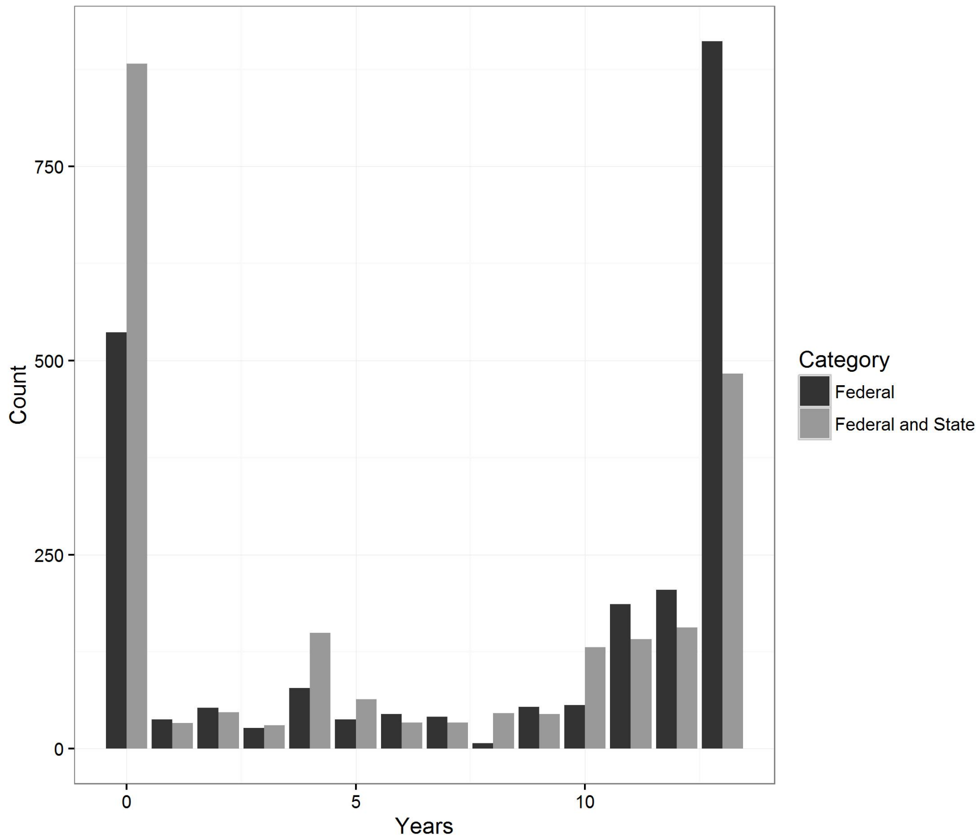



Figure 3.2: Distribution of Counties by Number of Years Eligible

Notes: This figure displays the distribution of time in which counties are eligible for the federal LRP and the federal and state LRPs. Years between 0 and 13 indicate a county that has changed its eligibility status at some point

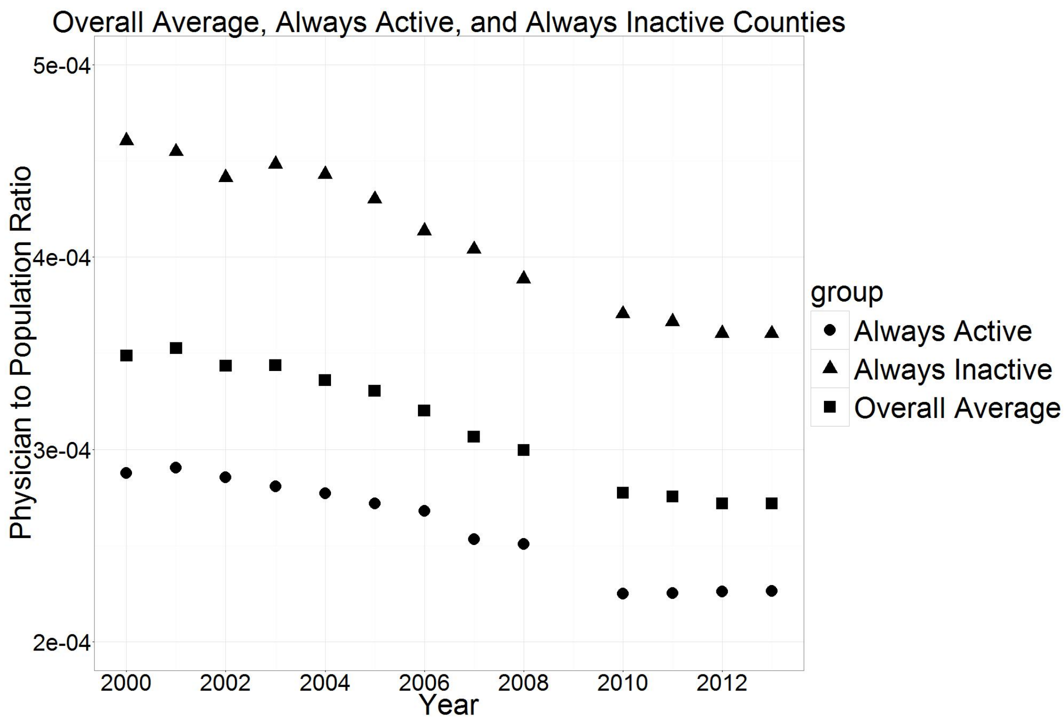



Figure 3.3: Physician-to-population Ratio in Always-Inactive and Always-Active Counties

Notes: This figure shows the trends in the physician-to-population ratio among counties that always have an active HPSA (and are thus eligible for LRPs), never have an active HPSA (always inactive), and the overall average for all counties in the sample.

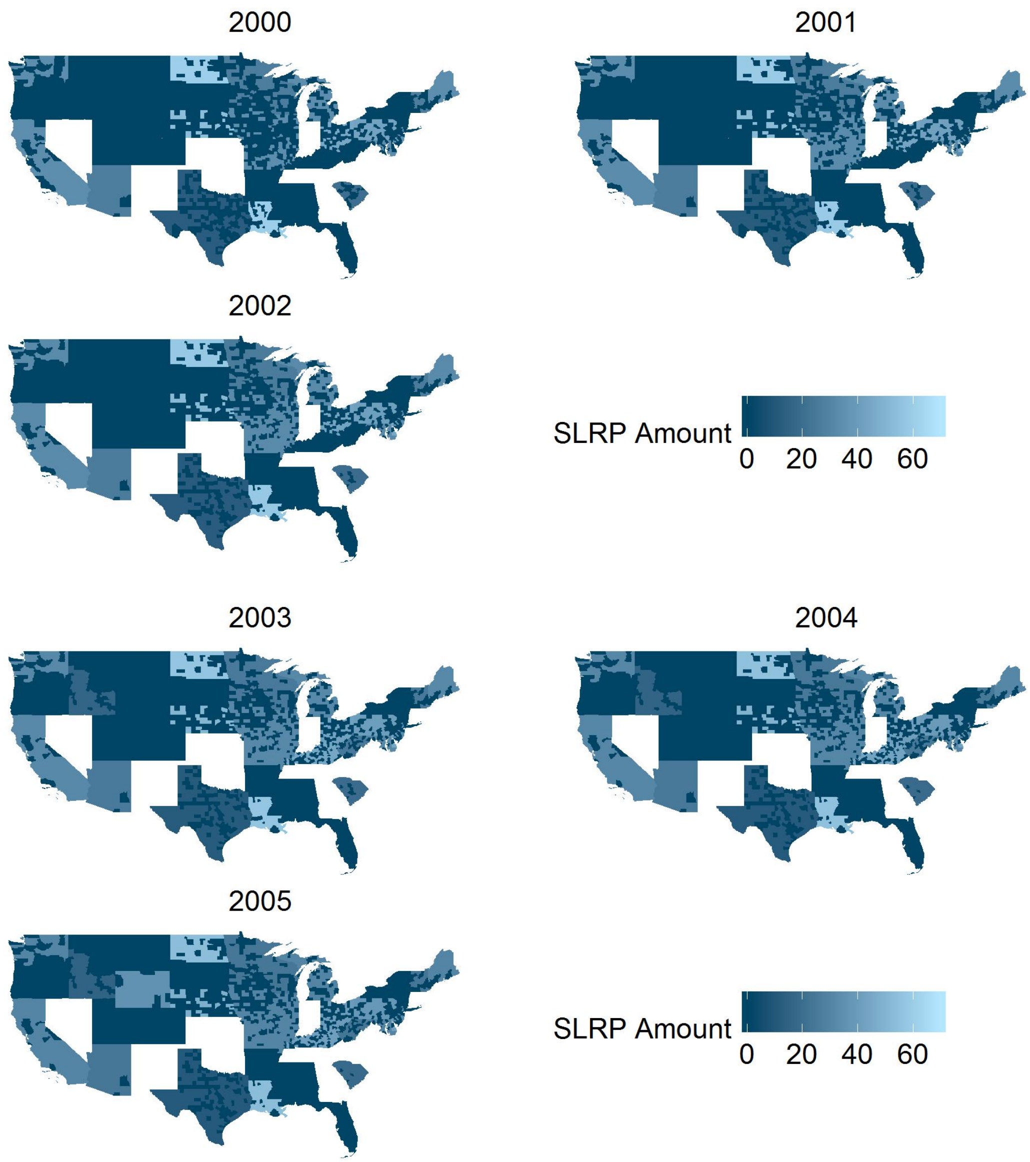



Figure 3.4: Changes in State Loan Repayment (SLRP) Amounts and Eligibility over time.

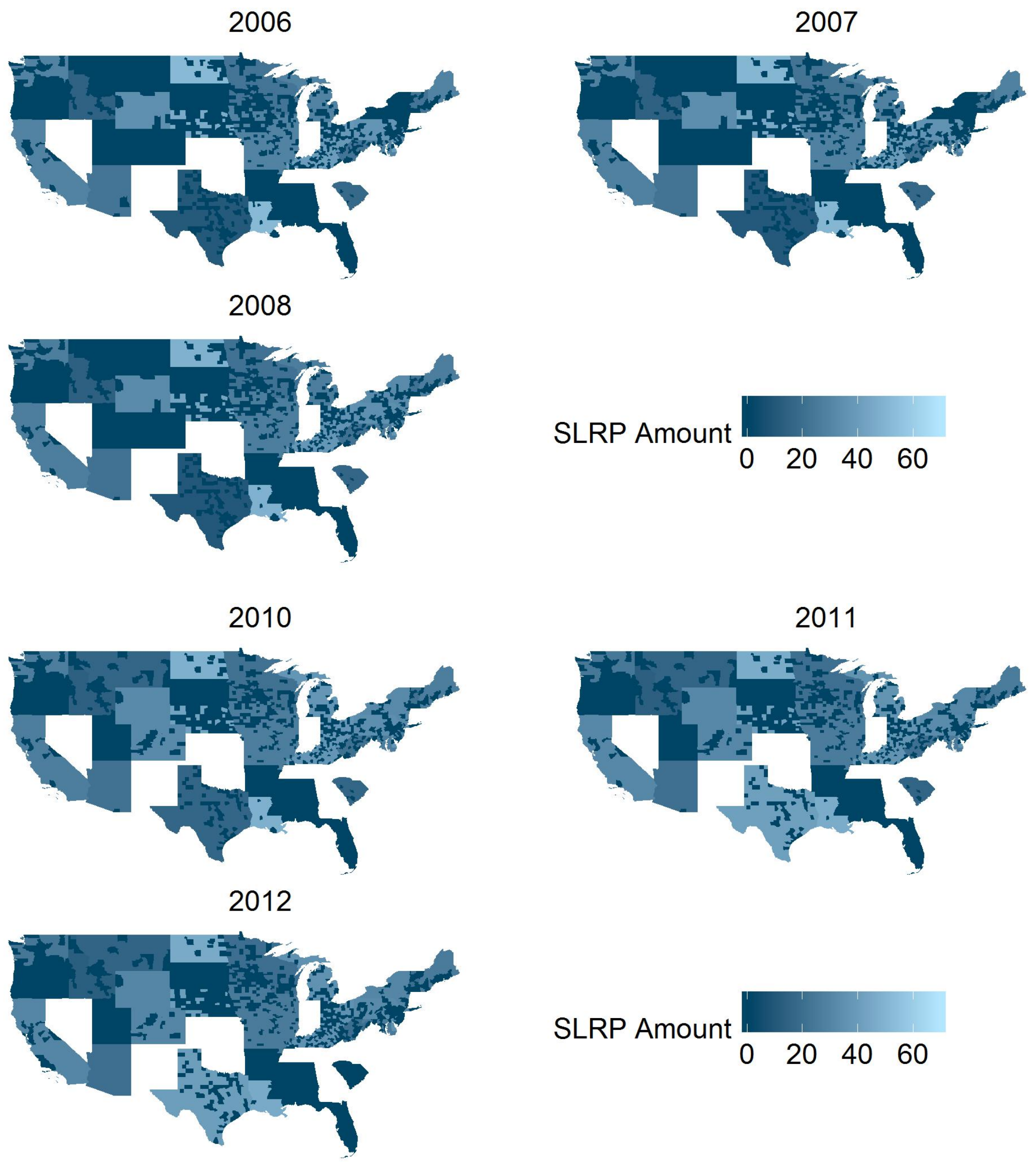



Figure 3.5: Changes in State Loan Repayment (SLRP) Amounts and Eligibility over time.

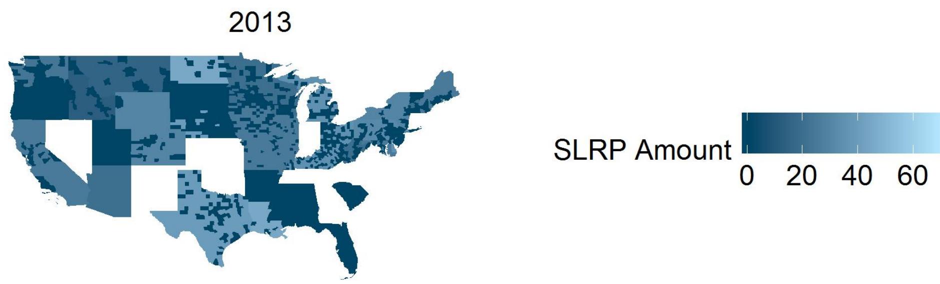



Figure 3.6: Changes in State Loan Repayment (SLRP) Amounts and Eligibility over time.

## 3.9 Tables

| State | Years | Program Name | Minimum Required Years of Service | Maximum Total Amount for Minimum-Years Contract |
|---|---|---|---|---|
| Arizona | 2000-2013 | Arizona Loan Repayment Program | 2 | $40,000 |
| California | 2000-2010, 2013 | California State Loan Repayment Program | 2 | $50,000 |
| | 2011-2012 | | 2 | $60,000 |
| Colorado | 2009-2013 | Colorado Health Service Corps | 3 | $90,000 |
| Delaware | 2001-2013 | Delaware Loan Repayment Program | 2 | $70,000 |
| Hawaii | 2010-2013 | State Loan Repayment Program | 2 | $50,000 |
| Idaho | 2003-2013 | Idaho State Loan Repayment Program | 4 | $50,000 |
| Illinois | 2000-2013 | Indiana State Loan Repayment Program | 2 | $50,000 |
| Iowa | 2000-2013 | Primary Care Recruitment and Retention Endeavor (PRIMECARRE) | 2 | $50,000 |
| Kentucky | 2003-2013 | Kentucky State Loan Repayment Program | 2 | $70,000 |
| Louisiana | 2000-2011 | Louisiana State Loan Repayment Program | 2 | $90,000 |
| Maine | 2000-2015 | Maine State Loan Repayment Program | 2 | $50,000 |
| Maryland | 2000-2016 | Maryland State Loan Repayment Program | 2 | $50,000 |
| Michigan | 2000-2009 | Michigan State Loan Repayment Program (MSLRP) | 2 | $50,000 |
| | 2010-2013 | | 2 | $70,000 |
| Minnesota | 2000-2013 | Minnesota State Loan Repayment Program | 2 | $40,000 |
| Missouri | 2000-2013 | Missouri Health Professional State Loan Repayment Program | 2 | $50,000 |
| Montana | 2009 | Montana NHSC State Loan Repayment Program | 2 | $66,000 |
| | 2010-2013 | | 2 | $30,000 |
| Nebraska | 2000-2013 | Nebraska Loan Repayment Program | 3 | $120,000 |
| New Hampshire | 2000-2007 | New Hampshire State Loan Repayment Program | 2 | $40,000 |
| | 2008-2013 | | 3 | $75,000 |

Table 3.1: State SLRP Programs, Years of Activity, Minimum Required Years, and Repayment Amounts

| State | Years | Program Name | Minimum Required Years of Service | Maximum Total Amount for Minimum-Years Contract |
|---|---|---|---|---|
| New Jersey | 2000-2013 | Primary Care Practitioner Loan Redemption Program of New Jersey | 2 | $52,800 |
| New York | 2008-2013 | Doctors Across New York Loan Repayment Program | 2 | $100,000 |
| North Dakota | 2000-2013 | North Dakota Physician Loan Repayment program | 2 | $50,000 |
| Ohio | 2000-2008 | Ohio Physician Loan Repayment Program (OPLRP) | 2 | $40,000 |
| | 2009-2013 | | 2 | $50,000 |
| Oregon | 2000-2008 | Oregon Rural Health Services Loan Repayment Program | 3 | $100,000 |
| | 2009-2013 | Oregon Partnership State Loan Repayment Program | 2 | $70,000 |
| Pennsylvania | 2000-2013 | Primary Health Care Practitioner Loan Repayment Program | 4 | $64,000 |
| South Carolina | 2000-2011 | South Carolina Rural Physician Loan Repayment Program | 4 | $36,000 |
| Texas | 2000-2008 | Physician Education Loan Repayment Program | 4 | $36,000 |
| | 2009-2010 | | 4 | $61,000 |
| | 2011-2013 | | 4 | $160,000 |
| Vermont | 2000-2013 | Vermont Educational Loan Repayment Program for Health Care Professionals | 2 | $40,000 |
| Washington | 2000-2010 | Washington Health Professional Loan Repayment Program | 3 | $75,000 |
| | 2011-2015 | | 3 | $70,000 |
| West Virginia | 2003-2015 | State Loan Repayment Program (SLRP) | 2 | $40,000 |
| Wisconsin | 2000-2013 | Rural Physician Loan Assistance Program | 3 | $50,000 |
| Wyoming | 2005-2013 | WY Healthcare Professional Loan Repayment | 3 | $90,000 |
| **States Without a Program During 2000-2013** | | | | |
| Alabama | Mississippi | | | |
| Arkansas | South Dakota | | | |
| Florida | Utah | | | |
| **States With Insufficient Information** | | | | |
| Connecticut | Kansas | New Mexico | Rhode Island | |
| Georgia | Massachusetts | North Carolina | Tennessee | |
| Indiana | Nevada | Oklahoma | Virginia | |

Table 3.2: State SLRP Programs, Years of Activity, Minimum Required Years, and Repayment Amounts

Notes: This table compiles all information gathered on state loan repayment programs including the state, the years the program was active, the name of the program, the minimum required years of service, and the maximum loan repayment amount available for that minimum contract. It also displays the states that did not have programs and states that were not included due to incomplete or insufficient information.

| Statistic | N | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|---|
| Physicians | 29,552 | 115.775 | 421.426 | 0 | 10,491 |
| Population | 29,552 | 104,888 | 349,352 | 42 | 10,017,068 |
| Annual Wages | 13,793 | 18.402 | 2.594 | 6.243 | 26.155 |
| Poverty Rate | 29,552 | 0.146 | 0.059 | 0.000 | 0.560 |
| Unemployment Rate | 29,552 | 6.319 | 2.862 | 0.000 | 29.900 |
| Total Births | 29,552 | 0.012 | 0.003 | 0.000 | 0.038 |
| Phys-Pop Ratio | 29,552 | 8.008 | 5.647 | 0.000 | 92.146 |
| PTP Ratio: und. 35 | 29,552 | 1.022 | 1.666 | 0.000 | 36.073 |
| PTP Ratio: 35-44 | 29,552 | 2.124 | 2.079 | 0.000 | 51.086 |
| PTP Ratio: 45-54. | 29,552 | 2.338 | 2.288 | 0.000 | 54.348 |
| PTP Ratio: 55-64. | 29,552 | 1.669 | 1.873 | 0.000 | 55.021 |
| PTP Ratio: 65-74. | 29,552 | 0.624 | 0.935 | 0.000 | 19.685 |
| PTP Ratio: Ovr. 75 | 29,552 | 0.231 | 0.553 | 0.000 | 14.948 |

Table 3.3: Summary Statistics

Notes: This table provides summary statistics for the dependent variables and independent variables used as controls in the regression estimates. The annual wages are scaled in terms of $10,000. Poverty Rate, Unemployment Rate, and Total Births are all scaled as rates. All physician-to-population ratios represent the number of physicians per 10,000 residents.

| Statistic | N | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|---|
| Physicians | 22,453 | 29.742 | 49.411 | 0 | 686 |
| Population | 22,453 | 32,104.570 | 37,051.960 | 42 | 247,141 |
| Annual Wages | 10,143 | 18.548 | 2.600 | 7.164 | 26.155 |
| Poverty Rate | 22,453 | 0.154 | 0.059 | 0.000 | 0.560 |
| Unemployment Rate | 22,453 | 6.374 | 2.972 | 0.000 | 29.900 |
| Total Births | 22,453 | 0.012 | 0.003 | 0.000 | 0.038 |
| Phys-Pop Ratio | 22,453 | 7.723 | 5.815 | 0.000 | 92.146 |
| PTP Ratio: und. 35 | 22,453 | 0.864 | 1.643 | 0.000 | 36.073 |
| PTP Ratio: 35-44 | 22,453 | 2.026 | 2.223 | 0.000 | 51.086 |
| PTP Ratio: 45-54. | 22,453 | 2.286 | 2.490 | 0.000 | 54.348 |
| PTP Ratio: 55-64. | 22,453 | 1.686 | 2.055 | 0.000 | 55.021 |
| PTP Ratio: 65-74. | 22,453 | 0.627 | 1.034 | 0.000 | 19.685 |
| PTP Ratio: Ovr. 75 | 22,453 | 0.234 | 0.618 | 0.000 | 14.948 |

Table 3.4: Summary Statistics: Rural Counties Only

Notes: This table provides summary statistics for the dependent variables and independent variables used as controls in the regression estimates. The annual wages are scaled in terms of $10,000. Poverty Rate, Unemployment Rate, and Total Births are all scaled as rates. All physician-to-population ratios represent the number of physicians per 10,000 residents.

| | *Dependent variable:* |
|---|---|
| | Physician-to-Population Ratio |
| Fed. Eligible t-13 | −0.287 (0.790) |
| Fed. Eligible t-12 | −0.526 (0.620) |
| Fed. Eligible t-11 | −0.031 (0.805) |
| Fed. Eligible t-10 | 0.037 (0.941) |
| Fed. Eligible t-9 | −0.079 (0.669) |
| Fed. Eligible t-8 | 0.113 (0.295) |
| Fed. Eligible t-7 | 0.253 (0.244) |
| Fed. Eligible t-6 | 0.071 (0.220) |
| Fed. Eligible t-5 | 0.197 (0.206) |
| Fed. Eligible t-4 | 0.199 (0.171) |
| Fed. Eligible t-3 | −0.011 (0.183) |
| Fed. Eligible t-2 | 0.082 (0.095) |
| State Eligible t-13 | 0.697 (1.070) |
| State Eligible t-12 | 1.285* (0.775) |
| State Eligible t-11 | 1.093 (0.873) |
| State Eligible t-10 | 0.489 (0.994) |
| State Eligible t-9 | 0.432 (0.730) |
| State Eligible t-8 | 0.289 (0.226) |
| State Eligible t-7 | 0.048 (0.243) |
| State Eligible t-6 | −0.076 (0.273) |
| State Eligible t-5 | −0.231 (0.239) |
| State Eligible t-4 | −0.078 (0.188) |
| State Eligible t-3 | 0.193 (0.137) |
| State Eligible t-2 | 0.018 (0.097) |
| Observations | 29,552 |

Table 3.5: Examining Pre-trend Evidence for the DID Assumption

This table provides evidence on the differences-in-differences assumption by using leading and lagging dummy variables for each year pre- and post-treatment, where treatment corresponds to becoming eligible for the federal program or becoming eligible for the SLRP. The table displays only the estimates of the coefficients for the pre-treatment period. The county-level covariates of the main specification are included, as well as county and time fixed effects. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | |
|---|---|---|---|
| | Physician to Population Ratio | | |
| | Without Wages | | With Wages |
| | (1) | (2) | (3) |
| Fed. Eligible | −0.188 | −0.071 | −0.074 |
| | (0.120) | (0.170) | (0.173) |
| State Eligible Amount Per Year | 0.041 | 0.031 | 0.033 |
| | (0.030) | (0.028) | (0.028) |
| Minimum Program Length | −0.025 | −0.025 | −0.024 |
| | (0.040) | (0.047) | (0.047) |
| Wages | | | −0.006 |
| | | | (0.010) |
| Poverty Rate | 0.901 | 0.005 | −0.025 |
| | (1.265) | (1.301) | (1.290) |
| Unemp. Rate | 0.014 | −0.014 | −0.015 |
| | (0.024) | (0.030) | (0.030) |
| Total Births | −18.902 | −30.862 | −30.909 |
| | (20.176) | (35.726) | (35.746) |
| Medicare Eligible Population | 4.286** | 6.039* | 5.991 |
| | (1.900) | (3.636) | (3.652) |
| Observations | 29,552 | 13,793 | 13,793 |

Table 3.6: Effect Of LRP Eligibility and Amount per Year on the Physician-to-Population Ratio

Notes: This table presents the results of the main fixed effects regression of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. Columns (1) and (2) do not include wages, while (3) does. Column (1) uses the full sample while columns (2) and (3) use the sample from 2006-2013, to correspond with the availability of wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Under 35 | 35 to 44 | 45 to 54 | 55 to 64 | 65 to 74 | Over 75 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Fed. Eligible | 0.070 | −0.131 | −0.079 | 0.119 | −0.042 | −0.011 |
| | (0.076) | (0.111) | (0.102) | (0.167) | (0.058) | (0.023) |
| State Eligible Amount Per Year | −0.012 | 0.055* | 0.035 | −0.039 | −0.009 | 0.002 |
| | (0.014) | (0.030) | (0.025) | (0.030) | (0.016) | (0.005) |
| Minimum Program Length | 0.014 | −0.002 | −0.086*** | 0.019 | 0.045* | −0.014 |
| | (0.019) | (0.030) | (0.020) | (0.020) | (0.026) | (0.011) |
| Wages | −0.004 | 0.015** | −0.023** | 0.002 | 0.005 | −0.001 |
| | (0.005) | (0.007) | (0.011) | (0.011) | (0.004) | (0.003) |
| Poverty Rate | 1.099 | −1.090 | 0.299 | −0.200 | 0.785 | −0.918** |
| | (0.675) | (0.735) | (0.767) | (0.894) | (0.672) | (0.420) |
| Unemp. Rate | 0.007 | 0.008 | 0.003 | −0.023 | −0.010 | −0.001 |
| | (0.010) | (0.017) | (0.017) | (0.015) | (0.017) | (0.005) |
| Total Births | 9.336 | −3.035 | 11.341 | −58.871** | 7.650 | 2.669 |
| | (11.287) | (14.907) | (25.677) | (23.325) | (16.964) | (5.788) |
| Medicare Eligible Population | −0.940** | 0.922 | 0.641 | 1.170 | 3.109* | 1.089 |
| | (0.447) | (1.151) | (0.745) | (1.109) | (1.775) | (1.298) |
| Observations | 13,793 | 13,793 | 13,793 | 13,793 | 13,793 | 13,793 |

Table 3.7: Main Effect Heterogeneity by Physician Age

Notes: This table presents the results of the main fixed effects regression, estimated separately by age of physician, of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. The regression uses the smaller sample that includes wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | |
|---|---|---|---|
| | Physician to Population Ratio | | |
| | Without Wages | | With Wages |
| | (1) | (2) | (3) |
| Fed. Eligible | −0.195 | −0.086 | −0.090 |
| | (0.119) | (0.174) | (0.177) |
| State Eligible Amount Per Year | 0.039 | 0.032 | 0.035 |
| | (0.029) | (0.030) | (0.030) |
| Minimum Program Length | −0.023 | −0.028 | −0.027 |
| | (0.042) | (0.049) | (0.049) |
| Wages | | | −0.009 |
| | | | (0.011) |
| Observations | 29,552 | 13,793 | 13,793 |

Table 3.8: Effect Of LRP Eligibility and Amount per Year on the Physician-to-Population Ratio

Notes: This table presents the results of the main fixed effects regression of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. Columns (1) and (2) do not include wages, while (3) does. Column (1) uses the full sample while columns (2) and (3) use the sample from 2006-2013, to correspond with the availability of wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Under 35 | 35 to 44 | 45 to 54 | 55 to 64 | 65 to 74 | Over 75 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Fed. Eligible | 0.080 | −0.145 | −0.062 | 0.095 | −0.041 | −0.013 |
| | (0.075) | (0.108) | (0.097) | (0.165) | (0.060) | (0.022) |
| State Eligible Amount Per Year | −0.016 | 0.062** | 0.025 | −0.030 | −0.010 | 0.002 |
| | (0.014) | (0.029) | (0.022) | (0.029) | (0.017) | (0.005) |
| Minimum Program Length | 0.015 | 0.001 | −0.087*** | 0.011 | 0.045 | −0.014 |
| | (0.021) | (0.031) | (0.022) | (0.022) | (0.029) | (0.011) |
| Observations | 13,793 | 13,793 | 13,793 | 13,793 | 13,793 | 13,793 |

Table 3.9: Main Effect Heterogeneity by Physician Age

Notes: This table presents the results of the main fixed effects regression, estimated separately by age of physician, of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. The regression uses the smaller sample that includes wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | |
|---|---|---|---|
| | Physician to Population Ratio | | |
| | Without Wages | | With Wages |
| | (1) | (2) | (3) |
| Fed. Eligible | −0.159 | −0.051 | −0.053 |
| | (0.118) | (0.155) | (0.156) |
| Minimum SLRP Contract Amount | 0.012 | 0.009 | 0.010 |
| | (0.009) | (0.008) | (0.008) |
| Minimum Program Length | −0.026 | −0.027 | −0.027 |
| | (0.042) | (0.055) | (0.055) |
| Wages | | | −0.006 |
| | | | (0.010) |
| Poverty Rate | 0.945 | 0.037 | 0.008 |
| | (1.254) | (1.262) | (1.255) |
| Unemp. Rate | 0.014 | −0.014 | −0.015 |
| | (0.024) | (0.030) | (0.030) |
| Total Births | −18.780 | −30.900 | −30.956 |
| | (20.140) | (35.378) | (35.407) |
| Medicare Eligible Population | 4.336** | 6.074* | 6.028 |
| | (1.919) | (3.668) | (3.680) |
| Observations | 29,552 | 13,793 | 13,793 |

Table 3.10: Estimates Using Full SLRP Amount

Notes: This table presents the results of the main fixed effects regression of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for over the entire contract. Columns (1) and (2) do not include wages, while (3) does. Column (1) uses the full sample while columns (2) and (3) use the sample from 2006-2013, to correspond with the availability of wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Under 35 | 35 to 44 | 45 to 54 | 55 to 64 | 65 to 74 | Over 75 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Fed. Eligible | 0.065 | −0.085 | −0.060 | 0.087 | −0.043 | −0.017 |
| | (0.061) | (0.076) | (0.087) | (0.136) | (0.040) | (0.019) |
| Minimum SLRP Contract Amount | −0.004 | 0.014*** | 0.011* | −0.010 | −0.003 | 0.002 |
| | (0.003) | (0.005) | (0.006) | (0.006) | (0.003) | (0.001) |
| Minimum Program Length | 0.016 | −0.003 | −0.090*** | 0.020 | 0.047* | −0.016 |
| | (0.022) | (0.034) | (0.018) | (0.019) | (0.027) | (0.014) |
| Wages | −0.004 | 0.015** | −0.023** | 0.002 | 0.005 | −0.001 |
| | (0.005) | (0.007) | (0.011) | (0.011) | (0.004) | (0.003) |
| Poverty Rate | 1.084 | −1.055 | 0.341 | −0.225 | 0.767 | −0.904** |
| | (0.682) | (0.720) | (0.774) | (0.896) | (0.672) | (0.408) |
| Unemp. Rate | 0.007 | 0.008 | 0.004 | −0.023 | −0.010 | −0.001 |
| | (0.010) | (0.017) | (0.017) | (0.015) | (0.017) | (0.005) |
| Total Births | 9.311 | −3.328 | 11.371 | −58.673** | 7.551 | 2.812 |
| | (11.418) | (14.714) | (25.663) | (23.272) | (16.907) | (5.618) |
| Medicare Eligible Population | −0.955** | 0.974 | 0.683 | 1.133 | 3.095* | 1.096 |
| | (0.448) | (1.163) | (0.741) | (1.113) | (1.779) | (1.303) |
| Observations | 13,793 | 13,793 | 13,793 | 13,793 | 13,793 | 13,793 |

Table 3.11: Using Full SLRP Amount by Age

Notes:This table presents the results of the main fixed effects regression, estimated separately by age of physician, of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. The regression uses the smaller sample that includes wage data. The coefficient on the "Minimum SLRP Contract Amount" has the interpretation of the effect of $10,000 in total SLRP amounton the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | |
|---|---|---|---|
| | Physician to Population Ratio | | |
| | Without Wages | | With Wages |
| | (1) | (2) | (3) |
| Fed. Eligible | −0.204 | −0.011 | −0.014 |
| | (0.147) | (0.219) | (0.223) |
| State Eligible Amount Per Year | 0.049 | 0.033 | 0.034 |
| | (0.040) | (0.036) | (0.036) |
| Minimum Program Length | −0.007 | 0.007 | 0.008 |
| | (0.049) | (0.085) | (0.085) |
| Wages | | | −0.004 |
| | | | (0.012) |
| Poverty Rate | 0.330 | −1.555 | −1.579 |
| | (1.439) | (1.707) | (1.686) |
| Unemp. Rate | 0.007 | −0.023 | −0.024 |
| | (0.028) | (0.036) | (0.036) |
| Total Births | −15.286 | −33.301 | −33.303 |
| | (22.215) | (40.322) | (40.331) |
| Medicare Eligible Population | 4.613** | 7.410* | 7.382* |
| | (2.201) | (4.320) | (4.343) |
| Observations | 22,453 | 10,143 | 10,143 |

Table 3.12: Estimates Using Only Rural Counties

Notes: This table presents the results of the main fixed effects regression of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. This sample is restricted to rural counties only. Columns (1) and (2) do not include wages, while (3) does. Column (1) uses the full sample while columns (2) and (3) use the sample from 2006-2013, to correspond with the availability of wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Under 35 | 35 to 44 | 45 to 54 | 55 to 64 | 65 to 74 | Over 75 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Fed. Eligible | 0.097 | −0.131 | −0.110 | 0.195 | −0.063 | −0.002 |
| | (0.099) | (0.156) | (0.132) | (0.224) | (0.073) | (0.032) |
| State Eligible Amount Per Year | −0.013 | 0.055 | 0.060** | −0.063* | −0.005 | 0.0004 |
| | (0.019) | (0.041) | (0.030) | (0.038) | (0.017) | (0.007) |
| Minimum Program Length | 0.028 | 0.023 | −0.125*** | 0.029 | 0.075** | −0.022 |
| | (0.033) | (0.047) | (0.022) | (0.028) | (0.034) | (0.020) |
| Wages | −0.006 | 0.021** | −0.026* | 0.005 | 0.003 | −0.001 |
| | (0.007) | (0.010) | (0.015) | (0.015) | (0.006) | (0.004) |
| Poverty Rate | 1.081 | −1.438 | 0.409 | −0.932 | 0.423 | −1.122** |
| | (0.810) | (0.895) | (0.817) | (1.081) | (0.762) | (0.534) |
| Unemp. Rate | 0.006 | 0.003 | 0.006 | −0.025 | −0.013 | −0.001 |
| | (0.012) | (0.022) | (0.021) | (0.019) | (0.021) | (0.006) |
| Total Births | 10.175 | −5.887 | 23.465 | −72.932*** | 7.653 | 4.222 |
| | (13.521) | (16.564) | (29.224) | (25.618) | (19.358) | (6.651) |
| Medicare Eligible Population | −0.663 | 1.096 | 0.187 | 1.998 | 3.548* | 1.216 |
| | (0.495) | (1.402) | (0.875) | (1.228) | (2.099) | (1.564) |
| Observations | 10,143 | 10,143 | 10,143 | 10,143 | 10,143 | 10,143 |

Table 3.13: Rural County Estimates by Age

Notes: This table presents the results of the main fixed effects regression, estimated separately by age of physician, of the physician to population ratio on an indicator for being eligible for the federal program and the available SLRP amount a county is eligible for per year. This model restricts the sample to only rural counties. The regression uses the smaller sample that includes wage data. The coefficient on the "State Eligible Amount Per Year" has the interpretation of the effect of $10,000 in SLRP on the physician-to-population ratio. County and time fixed effects are included. Standard errors are clustered at the state level. Significance levels denoted: *p<0.1; **p<0.05; ***p<0.01

# Chapter 4: The Effect of High School Career and Technical Education on Postsecondary Enrollment and Early Career Earnings: New Evidence from Maryland

## 4.1 Introduction

Career and Technical Education (CTE) has long been a fixture of U.S. public secondary, with federal funding of CTE existing since the first authorization of the Carl D. Perkins Career and Technical Education Act[1] in 1984. Recent incarnations of CTE education in U.S. high schools have focused on sequenced programs designed to prepare students directly for a two-year degree four-year degree, or an industry-recognized credential, with the goal of giving students pathways towards higher earning jobs after high school. Despite the history and prevalence of CTE education, the current literature concerning effects of CTE education on long-term outcomes-such as postsecondary education, degree receipt, and earnings-has found mixed results [U.S. Department of Education, 2014]. A recent Institute of Education Sciences working group on CTE noted " the need for more causal studies of CTE" [Ahn, 2017].

In this paper, I use student-level longitudinal data from the Maryland Longitudinal Data System to investigate the effects of completing a CTE program of study on postsecondary enrollment, degree receipt, as well as early career earnings.

[1]Originally the Carl D. Perkins Vocational and Technical Education Act

I use two complementary identification strategies that can be viewed as complementary and information for two cohorts of students who graduate from high school in 2010 and 2011. First, I use a propensity score matching (PSM) approach to pair CTE-completing students with students who do not complete CTE programs based on observable characteristics. In light of the possible biases and as a complimentary method, I estimate 2SLS models in order to provide an causal estimate of CTE under the relevance and exclusion restrictions assumptions. I take advantage of the fact that some CTE programs are provided at CTE Centers or technical high schools, which are separate public institutions from standard high schools that students can attend for part of their school day. I use the driving distance (in the amount of time required) from a student's high school to a CTE center or technical high school and use this as an instrument for CTE completion, as a longer distance to a separate institution might require additional direct or psychic costs in order to complete a CTE program.

Using the PSM method, I find that CTE completion is associated with a substitution effect from enrollment at four-year to two-year institutions, which persists through degree completion. Specifically, CTE completion is associated with a 5.1 percentage point (p.p.) decrease in bachelor's degree completion and a 3.5 p.p. increase in the probability of an associate degree. CTE completion also increases the likelihood of earning a certificate by 0.9 p.p. Furthermore, CTE completion is associated with higher early career earnings: CTE increases annual earnings in the sixth year after high school graduation by $2,050, and increases the earnings during the first year after the completion of education by $1,444. Estimating these effects by different program types, I find that some programs, especially those of a traditional vocational nature (e.g. construction or automotive repair, for example) are associated with larger negative effects on four-year degrees but still produce positive effect on early career earnings, while others lead students to substitute towards two-year degrees.

The 2SLS models, using driving time as an instrument, produce evidence consistent with the PSM results for the types of programs that are traditionally offered at CTE centers. I find that completing a CTE program has a positive effect on attending a 2-year institution immediately after high school, and a negative effect on attending a 4-year institution. Completing a CTE program causes a significant decrease in the probability of completing a bachelor's degree, but a significant increase in the likelihood of earning a certificate. I also find positive effects on earnings in the sixth year after high school graduation.

To implement the PSM approach, I calculate a student's propensity to complete a CTE program based on observable demographic information, as well as scores on three standardized high school assessments required to graduate high school in Maryland. I then use a nearest-neighbor matching process and examine the effect of CTE using the matched data. I also examine how the long-run effects of CTE education differ by the types of CTE programs completed in order to understand heterogeneity among the different types of CTE programs. However, in order to produce a causal effect, matching requires that there are no unobservable variables that might affect both the decision to enroll in CTE programs and the outcomes of interest. If this assumption does not hold, the matching procedure can provides descriptive information about CTE completers, but will not produce causal estimates.

I estimate the effect of CTE education by an instrumental variables procedure using two-stage least squares. The types of programs offered at CTE centers and technical high schools are often a subset of the total number of available programs to students, typically of programs that require larger amounts equipment or capital, and therefore the IV estimates provide a causal estimate of CTE completion, but for the types of programs available at CTE centers. IV estimates can then be seen as complementary method, as the IV estimates can be compared to the PSM estimates of similar types of programs to see whether the effects of CTE are similar using a different method with distinct assumptions. Using different specifications of the driving distance instrument, I find that driving distance is significantly and negatively related to the completion of a CTE program. Being within 10 minutes of a CTE Center or technical high school, for instance, increases the probability of completing a CTE program by 11 p.p., which is an increase of over 50% relative to the mean probability of completing a CTE program.

CTE education could affect postsecondary enrollment and earnings in several ways. First, CTE programs may provide easier access to industry credentials, or information to student about the availability of college programs associated with the CTE program. The latter may be valuable, especially for low-income or first generation college students who have limited information about college [Hoxby and Turner, 2015]. CTE education may also provide students with information about their own set of cognitive and mechanical skills. The dynamic process of skill revelation can influence postsecondary enrollment decisions [Arcidiacono et al., 2016] and both earnings and degree attainment may be influenced by a student's cognitive and mechanical skills. [Heckman et al., 2006, Prada and Urzúa, 2017]. In addition, CTE education could affect high school graduation by either providing students an avenue to finish high school or a reason for doing so. Receiving a high school diploma has been associated with positive career outcomes in prior research [Jaeger and Page, 1996, Arcidiacono et al., 2010].

Earlier research on vocational education found that students who take vocational courses receive higher earnings, but are less likely to complete a college degree [Bishop and Mane, 2005, Meer, 2007]. More recent evidence suggests that student outcomes can depend on the type of CTE coursework completed. [U.S. Department of Education, 2014][2] Dougherty [2016] finds positive effects of CTE on high school graduation using a regression-discontinuity design while Kreisman and Stange [2016] find positive effects of advanced CTE coursework on earnings and that there is some substitution between four- and two-year college enrollment, though this does not extend to college degrees.

## 4.2 CTE in Maryland

The United States has long funded vocational education, and the most recent iteration of this Carl D. Perkins Career and Technical Education Act of 2006, the fourth reauthorization of such funding since 1984. The Perkins Act provides grant aid to state and local education agencies to fund CTE programs in high schools and

[2]This report commissioned quasi-experimental studies in several U.S. cities to examine the effects of CTE coursework. For example, study results from San Diego [Betts et al., 2014] and Philadephia [Furstenberg and Neumark, 2005] find positive effects of CTE on postsecondary enrollment or aspirations of enrollment, while the study in Florida found no effect on college going for CTE students [Jacobson and Mokher, 2014].

community colleges. The most recent version of the Perkins Act requires funded programs to offer a sequence of nonduplicative classes that help prepare students for entering postsecondary education or obtain an appropriate industry credential.

In the State of Maryland, there are 148 programs of study for students to choose from, organized into 10 "Career Clusters" or coarse groupings of related programs. A wide variety of programs are offered, ranging from pre-engineering programs designed to prepare students for studying engineering at a four-year institution to programs like carpentry and automotive technician that prepare students for recognized industry credentials. To comply with the Perkins Act, all new programs must be approved by state and local governing bodies, and must prepare students for a postsecondary pathway or for an industry credential. Some programs include opportunities internships, shadowing, or other work experience that give students a direct opportunity to learn about the potential career options available upon completing a CTE program.[3][4]

CTE programs are typically four-course sequences that a student completes in their later years of high school. Typically, students begin with a CTE course in 10th grade, or two courses in 11th grade and complete the sequence in 12th grade with two courses that include a capstone course. Modern CTE programs are intended to be easily completed in conjunction with normal college preparation coursework, a practice intended to avoid "tracking" programs that have occurred

[3] Each Career Cluster has a Program Advisory Committee (PAC) and local districts have a Local Advisory Committee (LAC), each consisting of members of local school districts and industry representatives. LACs work in conjunction with PACs to propose new CTE programs. This can also be done at the statewide level through the Governor's Workforce Investment Board.

[4] The "Career Research and Development" is a specific cluster in which students who participate do so in assistantships in the private sector

with previous iterations of vocational education in the United States. Over 50% of students who complete CTE programs in Maryland also complete the minimum entry requirements for the University System of Maryland.

In addition to variation in the types of programs offered, there is also variation in how student access the courses. Some programs are available at a student's regular public high school, while other courses are offered at a CTE Center, an institution designed by the school system to provide CTE education for specific programs to all the schools in a school district. These programs often tend to be capital intensive programs, such as those that require specialized equipment or instruction. For example, business or marketing program may be provided at a student's high school, while an automotive technician program might instead be offered at the CTE Center. Some school districts may instead have technical high schools that provide a wider variety of CTE programs than standard high schools, but also provide standard high school curricula. These schools often function as magnet high schools where most students apply for entry in 8th grade, but also accept part-time students who commute to the school for a portion of the day. School systems typically provide transportation in the form of busing for students to attend CTE Centers, but the commute during the day may still provide both financial and psychological costs that reduces a student's likelihood of completing a program.

The three most common ways for students to complete high school in Maryland are: to complete the University System of Maryland (USM) requirements for entry into the USM system, to complete a CTE sequence of courses, or to complete both. Completing the USM requirements requires satisfying a minimum number of credits in required courses and achieving minimum scores on the High School Assessment (HSA) tests available to students once they complete their Algebra I, English 10, and Biology courses. CTE completion requires finishing the four course sequence of CTE courses. Though each of these provide pathways to a high school diploma, a student's high school diploma does not reflect which of the pathways a student used to complete high school, and therefore a student's CTE completion is only represented as an indicator on their high school transcript.

## 4.3 Data

To estimate the effect of CTE on postsecondary outcomes, degree receipt, and early career earnings, I use student-level data from the Maryland Longitudinal Data System, the State of Maryland's central repository for longitudinal student data. The MLDS data is composed from several different sources. PreK-12 enrollment data is received from the Maryland State Department of Education and contains public school enrollment information for students beginning in the 2007-2008 academic year. The PreK-12 enrollment data is then matched to data from the Maryland Higher Education Commission and the National Student Clearinghouse (NSC) to create enrollment and degree histories for students in postsecondary education. By matching to the NSC, it is possible to see enrollment and degrees for students who may have left the state to pursue higher education. Lastly, the education data is also linked to earnings data from the state Department of Labor Licensing and Regulation, which contains information on earnings from wages and salaries from the state's unemployment insurance database. The enrollment data spans the 2007-2008 to 2015-2016 academic years.

Using this data, I select a cohort of students who were in 12th grade in the 2009-2010 and 2010-2011 academic years. This is done for several reasons. First, given the time span of the data, this ensures that I can see students up to six years after high school graduation, an amount of time that is frequently used in the education literature, as it would allow students who immediately enroll in four-year degree programs 150% of the time expected to graduate. Secondly, some variables, such as the High School Assessment scores (discussed below) are not available for students before this graduating cohort. These cohorts contain over 80,000 students.

Using the data on postsecondary enrollment and earnings, I construct college enrollment and earnings histories for each student in the cohort. For college enrollment, I count a student as enrolled if they are degree-seeking and enrolled either part-time or full-time at a two-year or four-year degree granting institution in the fall of each year after high school graduation. Using the information on degrees granted, I create indicators for whether a student earns an associate degree or a bachelor's degree, where each indicator is independent, allowing students to have a value of one for each indicator if they earn both types of degrees. I also include certificate receipt as an outcomes.

Using the earnings data, I similarly construct a history of earnings after high school graduation. Earnings in the MLDS are collected quarterly, therefore I aggregate the quarters into approximations of academic years. For example, quarters 3-4 of 2015 and 1-2 of 2016 are counted as earnings during the 2015-2016 academic year. Aggregating earnings this way allows for an easy comparison to the academic year and therefore are more easily interpretable when combined with the estimates on enrollment. I focus on two measures of earnings. The first is the annual earnings six years after high school graduation. This measure is meant to indicate the earnings for a student after as sufficient length of time to complete a four-year degree is met, but may also include years of experience for students who either do not complete a degree or complete a two-year degree. I include an additional measure of earnings to exclude the effects of career experience by calculating the annual earnings for a student in the first year that they are no longer enrolled in any kind of education. This measure more directly compares students after they finish their education, but discounts the opportunity to begin a career early and earn work experience.

One important limitation of the earnings data is that by receiving earnings from the state unemployment insurance database, the earnings excludes students who move outside of the State of Maryland or federal workers. Both of these limitations are important in the case of Maryland, as there is easy travel to nearby states and federal workers make up a significant portion of the Maryland workforce.[5] Despite these limitations, the earnings data still provide an important estimate of the long-term effect of CTE education, though the results should be considered with the limitations in mind.

The data contain demographic and standardized test score information for each type of student. I view a student's race, gender, and ethnicity (Hispanic or not Hispanic), and whether a student has any special education accommodations.

[5] About 5.5% of Maryland's workforce was employed by the federal government in 2016 according to the Maryland State archives. http://msa.maryland.gov/msa/mdmanual/01glance/economy/html/labor.html

The data also contains an indicator for whether a student is eligible for free-and-reduced-price meals (FARMS), a common indicator of low-income status in K-12 education data, and the number of weeks a student is absent from school

For use as additional covariates in both the matching and instrumental variables methods, I also measure the distance (in miles) between a student's high school in 12th grade and the nearest two-year and four-year postsecondary institutions. These serve as controls for one type of cost of attending college. I also include school level covariates for students, including the percentage of students who pass their algebra HSA exams, the total number of CTE programs available at school, and the overall percentage of the school that are FARMs eligible. I use these variables to control for differences between the schools a student attends, even if the student were to be atypical within that school.

To fulfill high school graduation requirements, students in this cohort were required to score above a minimum threshold on three High School Assessment (HSA) standardized tests.[6] These tests, in Algebra, English, and Biology, typically were taken by students after finishing the corresponding high school course. Scores on each test range from 250-650 and students are required to score a combined 1208 on the three exams in order to meet the University System of Maryland requirements for entry.[7] Given the time frame of the data, students who take a course early in their education may be seen to be missing an HSA Exam score. This is particularly true of the HSA Algebra exam, where many students take Algebra I in either 8th or

[6] In 2015, the HSA exams were replaced by the PARCC assessments in the State of Maryland.

[7] Fulfilling USM requirements does not guarantee enrollment in the University System of Maryland, but is a requirement for high school students seeking to enroll.

9th grade. In this case, I code an indicator of 1 or 0 for whether a student's exam score is present. Despite the missingness of the data, this indicator provides some information on the academic ability of a student, as students with higher academic ability are more likely to take the HSA Algebra early and therefore will not have a score present.

CTE program completion is measured at high school graduation, where accompanying a student's record of high school completion is an indicator for fulfilling the USM requirements, completing a CTE program, or both. Accompanying the program completion indicator is a Classification of Instructional Program (CIP) code for the type of program that a student completes. I use the CIP code to determine to which of the Maryland CTE Career Clusters a student's CTE completion corresponds in order to provide a coarse description of the type of CTE program completed.

As described above, the CTE coursework for some programs in some counties is completed at either a technical high school or a CTE Center. Using data from the state department of education, I determine the CTE center or technical high school available to students in their school system.[8] To do this, I pull sophomore high school enrollment for the 2010 graduating cohort, and measure the distance between the student's high school as of 10th grade and the CTE center or technical high school. I use 10th grade distance as the measure, because this is the time typically before students have made their decision to enter CTE programs. As a measure of distance, I use the driving time according to Google Maps. This provides

[8] This data is publicly available at https://www.mdctedata.org.

a measure of the time cost required to attend a CTE center from your sending high school.

### 4.3.1 Sample Selection

I limit the data to only students who complete the USM requirements, including both students who complete CTE programs and do not. The rationale for doing so is a consideration of the counterfactual to completing a CTE program. In examining high school graduates, the proper control group for students who only complete high school through completing a CTE program is not clear, as the decision for some of these students is likely between completing high school by CTE or not completing high school at all. Therefore, I restrict to only students who complete the USM requirements. This has the benefit of having a reasonable control group, but does mean that both methods that I employ will likely be producing local estimates of the average treatment effect for relatively positively selected studnets. In Section 4.3.1, I show how CTE-only completers differ from students who complete both CTE and a USM program.

I examine summary statistics of students who complete and do not complete CTE programs in Table 4.1. This table breaks down several demographic and test score characteristics by the type of completion, with the three types being USM only, for students who did not complete a CTE program but fulfilled the USM requirements, CTE and USM, for students who completed both, and CTE Only, for those students who completed only a CTE program. Though students who belong to the first and last columns will be the only ones included in the analysis, it is helpful to see how they differ from student who graduate high school by only completing a CTE program.

In Table 4.1, USM Only and those who complete both are remarkably similar in demographic and test score characteristics. Those who complete both are slightly more likely to be male, white, and black. They are less likely to be Asian overall. The propensity to have an HSA Algebra test score and the score for those who do are fairly similar between the two groups, but those who complete CTE programs have slightly lower HAS English scores. These students are also much more likely to have an available HSA Biology score, suggesting that students who complete CTE programs take Biology later in their high school career, on average. The same patterns exist for CTE only relative to USM only students, just with larger magnitudes in the differences. For example, 33% of CTE only students are FARMS eligible compared to 21% of USM only students. The two types of students are generally similar in their distance to the nearest college. In general, CTE students are more likely to be male, white, of low family income, and slightly less academically prepared. CTE and USM completers, relative to non-completers, generally are the same distance from the nearest two- or four-year higher education institution, and have the same number of weeks absent. In terms of school-level characteristics, CTE and USM completers have a similar percentage of students (62% versus 61%) that pass their Algebra HSA tests. However, CTE and USM students, versus non-completers, have a slightly higher number of CTE programs available within their own high school (13 versus 12) and have a slightly higher percentage of students who are FARMs eligible (26% versus 23%).

I show raw outcomes of interest among the three groups in Table 4.2, where rates of enrollment at the two types of postsecondary institutions, rates of degree receipt and annual earnings 6 years after graduation are shown for the three types of students. Students who complete both are about 11 percentage points less likely to enroll in a four-year institution and 9 percentage points more likely to enroll in a two-year program immediately after high school graduation. This difference is also apparent in the propensity to receive a degree, with those who complete both 9 percentage points less likely to receive a bachelors degree and 5 percentage points more likely to obtain an associates degree. CTE and USM students are more likely to have positive earnings observed in the data, and among those with positive earnings, have higher earnings; $24,371 compared to $22,055 of non-completers.

To provide some idea of what CTE students study, I break down the type of program completed by each type of CTE completion (CTE and USM versus CTE Only), in order to give a percentage of completers who complete each type. Table 4.3 lists the Career Clusters on the left hand side and the percentage of completers on the right. Several patterns emerge from this comparison. While students of each type of completion complete each type of program, students who complete both CTE and USM are much more likely to complete the Business, Management, and Finance; Health and Biosciences, and Human Resource Services programs. CTE Only students, on the other hand, are much more likely to complete Career, Research, and Development; Construction and Development; and Consumer Services, Hospitality, and Tourism programs. The types of programs completed, as well as the general summary statistics suggest that students who complete CTE and USM are different from students who complete CTE only. However, since more than 50% of CTE completers complete both, the CTE and USM completers remain a highly relevant subset of students.

## 4.4 Empirical Strategy

To estimate the effect of CTE on long-run outcomes, I use two complementary strategies. First, I use propensity score matching to pair CTE completers with non-CTE completers based on observable characteristics. Under a set of strong assumptions, these estimates provide an estimate of the causal effect of CTE program completion, and allow me to examine heterogeneity in the effects of CTE by the type of program completed. Given the strong matching assumptions required for causality in the case of matching and plausible reasons why they may be violated in the case of CTE program completion, I also employ an instrumental variables (IV) strategy based on the distance from a student's high school to the CTE center or technical high school in their school district. Under the assumptions of relevance and exclusion, this instrument can be used to estimate the causal effect of CTE education, though for only the types of programs offered by CTE centers and technical high schools, which, as explained above is typically a subset of all available programs.

The first empirical strategy uses propensity score matching without replacement. Using a logistic model, I estimate the probability of completing a CTE program based on observable student characteristics. In the model, I include the demographic and test score information described in Section 4.3. In addition, I include indicators for the county of a student's high school, which corresponds to the student's school district. After examining the common support for the propensity score, I then use a nearest-neighbor matching to match each CTE completer with a non-completer of a similar propensity score. I use a caliper of 0.2 to ensure that I limit cases of extreme difference in the propensity score. There are no students eliminated from the treated sample due to not being able to find a match within .2 of the probability of completing a CTE program. After matching and checking the balance, I estimate the effect of CTE completion on each of the postsecondary enrollment, degree completion, and earnings outcomes by regressing the outcome of interest on an indicator for CTE completion.

In the case of CTE completion, it is possible to make the case that there are unobserved variables that might be associated with both CTE completion and the outcomes of interest. If we use enrollment in a four-year university as an example outcome, we can likely hypothesize that there is a degree of selection into CTE programs and that some of the variables on which a students selects, but is possibly unobserved, might also be related to the college decision. Motivation might be an example, where some students might complete a CTE program because there are significant pyschic costs to attending a four-year university, and the student may think of the jobs associated with CTE completion as another option to a four-year degree. Previous work has also shown significant returns to mechanical skills [Prada and Urzúa, 2017], and mechanical skills may be related to a student's decision to complete a CTE program as well as provides a student options relative to a four-year degree.

To match students via a propensity score, I first use a logistic regression to estimate the probability of completing a CTE program. In Figure 4.1, I show that the distributions of the propensity score for those who completed a CTE program versus those who do not have a significant region of common support, suggesting that the propensity score matching fulfills one of the crucial matching assumptions. After obtaining the propensity score, treated students are then matched via a nearest-neighbor matching with students who only completed the USM requirements with a caliper of .2.

To examine the balance, Table 4.4 shows the standardized mean difference of demographic characteristics before and after matching. The standardized mean difference has an interpretation of Cohen's d. As discussed in the prior subsection, the differences between the CTE and non-CTE students are not exceedingly large prior to the matching, with hardly any of the mean differences greater than .1. However, a $\chi^2$ test of joint significance (including the county dummies not included in the table) show that we can still reject the null hypothesis that the means of CTE students are different from non-CTE students. After matching, the standardized mean differences of the matched sample are even smaller, and the $\chi^2$ test does not reject the equivalence of the group means.

For matching to produce a causal effect of CTE completion, the following assumptions must be met. First, the propensity to complete a CTE program must be positive for each observation. Secondly, matching requires that there are no unobservable variables that may be correlated with CTE completion and the outcomes of interest.

Given the strength of assumptions necessary for the PSM method to produce causal estimates, I turn to an complementary instrumental variable strategy. As described in Section 4.2, some CTE classes and programs are offered at separate CTE Centers and technical high schools within a student's school district, that students will commute back and forth to during the school day. This extra commuting might serve as a type of cost that might prevent some student from completing a CTE program, despite school systems typically offering busing to and from the CTE center locations. I treat the distance, in terms of driving time, from a student's high school to a CTE center as an exogeneously determined cost of completing a CTE program. Using the sample of high school completers in 2010, I match students to their enrollment in the 2007-2008 academic year (their sophomore year of high school) and determine the distance from their high school in sophomore year to the nearest CTE center or technical high school in their school district, in terms of the driving distance in minutes.

I then use "time to CTE center" as an instrument for CTE program completion among the same sample as the propensity score matching. In regression form, the first stage equation looks like:

$$CTE_i = \alpha + \beta_1 Driving\ Time_i + X_i\Gamma + \epsilon_i \tag{4.1}$$

where $i$ refers to an individual student, $Driving\ Time_i$ is the minutes from the student's high school to the CTE Center and $X_i$ is a vector of individual characteristics that correspond to the variables used in the PSM technique above. Using

$Driving\ Time_i$ to instrument for CTE completion, I then estimate the effect of CTE completion on each outcome of interest, $Y_i$:

$$Y_i = \delta + \beta_2 \widehat{CTE}_i + X_i \Psi + \epsilon_i \tag{4.2}$$

where the system is estimated by two-stage least squares.

For the IV procedure to estimate the causal effect of CTE completion, two assumptions must be met. The first is that the $Driving\ Time_i$ variable must be correlated with the decision to complete a CTE program. This relevance condition is testable, and I show evidence in Section 4.5 that this appears to be true. The second is an exclusion restriction necessary for the exogeneity of the instrument, which requires that the $Driving\ Time_i$ variable is not related to the outcome through any means other than CTE completion. This is fundamentally untestable, but is likely a reasonable assumption in this case for several reasons. The first is that the measure of driving time is created in the sophomore year so that it predates the typical student's decision to engage in career and technical education. Secondly, the location of CTE centers and technical high schools is not likely to be associated with locations of community colleges or four-year universities in the State of Maryland. Thirdly, there are fewer options for school choice in Maryland compared to other states, where other schooling options like charter schools remain a very small part of the market for secondary education, and existing private-school voucher programs are small.

For the IV regressions, I use several versions of the instrument; both discretized continuous. I use an indicator for whether a student is within 10 minutes, 15 minutes, and 20 minutes as the instrument, which corresponds to the 25th, 50th, and 75th percentiles of the driving time variable. I also use a continuous measure of the driving time, $log(time)$. In Table 4.5, I provide the correlation between the continuous measure,$log(time)$ and several other variables in which high correlations would suggest that the exclusion restriction is questionable. I show the correlation with the distance to the nearest 2-year and 4-year higher education institutions, the HSA Algebra passing percentage of the student's school, and the percent of students who are FARMs eligible within the school. If driving time were significantly related to distance to college, then distance to a CTE Center might also be related to the cost of attending college, which is an outcome of interest. I look at the HSA Algebra passing percentage in order to figure out whether the driving time is related to the academic achievement of students, which could suggest that driving time would prevent less prepared students from attending CTE Centers. Lastly, I look at FARMS percentage to understand if the distance might be related to a family's level of overall resources. Each of these correlations is fairly low, with correlations of 0.03, 0.11, 0.09, and -0.05, respectively. This does not prove the exclusion restriction true, but provides some evidence that it may be reasonable.

In Section 4.3, I explain that the restrictions of the MLDS earnings data which might lead some students to not be present in the data, with the potential that this might lower for CTE students versus non-completers. I use two methods in order to try to limit the bias of these estimates. The first is that I provide lower and upper bounds on the estimated effects using the bounding procedure developed by Lee

[2009]. This uses the difference in the probability of having an observation present. In the case of CTE, CTE students are more likely to have positive observations of earnings. Using 1 minus the probability of having earnings observations as a percentile, the procedure trims the top of the CTE distribution of earnings prior to the estimation. This provides a lower bound, with a procedure for producing an upper bound in a similar fashion. I also estimate the earnings using the data from 2009-2010 where earnings have been imputed. I use a method from Rubin [2004] where values are imputed from a model that includes test score and demographic characteristics and generate 5 imputed data sets. I use Rubin [2004] to determine the average estimate, overall variance, and confidence intervals over the 5 data sets.

## 4.5 Results

### 4.5.1 Propensity Score Matching

In Table 4.6, I use the matched sample to estimate the effects of CTE on enrollment at each type of institution for four years following high school graduation. The results in Table 4.6 show the coefficient on CTE of a linear regression of an indicator for college enrollment on the CTE variable with additional controls using the matched sample. Each column corresponds to enrollment in that type of schooling for $x$ years after high school graduation. The common pattern that emerges is that there seems to be a substitution towards 2-year enrollment from 4-year enrollment. Columns (1) and (2) show a statistically significant ($p < .01$) 3.7 percentage point increase in 2-year enrollment and a 4.7 percentage point decrease in 4-year enrollment with CTE completion, respectively. This corresponds to a 10.6% increase in the likelihood of 2-year enrollment and a 12.1% decrease in 4-year enrollment, respectively. This pattern of statistically significant substitution continues through year 4, with a 3.1, 2.5, and 2.1 percentage point increases in 2-year enrollment in years 2, 3, and 4 after high school, respectively, and 5.2, 6.1, and 5.8 percentage point

Table 4.7, shows that this pattern extends to the degrees earned by each student within 6 academic years of high school graduation. Column (1) of Table 4.7 shows a student's propensity to continue to be enrolled after 6 years. CTE students appear to be no more likely, on average, to be enrolled six years after high school graduation. In terms of degree receipt, the same substitution between the two-year and four-year degrees appears to be present, when I compare the effect of CTE on the rate of associate's degree receipt and bachelor's degree receipt in columns (2) and (3). I find a 3.5 percentage point increase in the probability of an associate degree and a 5.1 percentage point decrease in the probability of a bachelor's degree, with each being statistically significant at a $p < .01$ level. This amounts to a 23% increase in the probability of attaining an associate degree and a 13.1% decrease in the probability of a bachelor's degree. CTE completion also has a positive effect on certificate receipt, with a statistically significant ($p < .01$) effect of 0.9 percentage points on the probability of receiving a certificate. Though small, this is a 30% increase over the 0.03 probability of receiving a certificate. These results suggest that CTE directs students towards associate degrees and certificates, substituting away from bachelor's degrees.

Turning to the effects of CTE on annual earnings, Table 4.8 shows the effect of CTE on the probability of observing earnings (Column (1)) and, for those with positive earnings, the effect of completing a CTE program on earnings in year 6 after high school (Column (2)) and in the first year after education (Column (3)). As the summary statistics would suggest, CTE completers are more likely to be present in the earnings data. Completers are significantly ($p < .01$) 3.2 percentage points more likely to have positive earnings, a 4.3% increase over the mean for non-completers. Restricting to only those with positive earnings, CTE completers have annual earnings $2,050 larger in the sixth year and $1,444 larger in the first year of any employment, compared to non-completers, with each results significant at the ($p < .01$) level. These represent increases of 9.1% and 8.7% above the non-completer mean, respectively. Since the data is restricted to only those with positive earnings in the sixth year, I report the Lee [2009] bounds in the bottom of the table. In each case, the lower bound of the estimate, −$256 and −$795 for the sixth year and first year of earnings, respectively, is negative, suggesting that I cannot rule out that the higher likelihood of the CTE completers to be missing is responsible for the larger estimates of earnings.

In Table 4.9, I show the same effects but for the sample in which multiple imputation was used to generate earnings values for the missing observations. I find similar sized estimates. CTE completion has a $1,905 and $1,300 effect on earnings in year 6 and at first employment, respectively, each significant at the $p < .01$ level. These effects correspond to an 8.5% and a 7.9% effect, respectively.

Table 4.10 shows the estimated effects on enrollment in 2-year and 4-year institutions in the first year after high school, using a separate PSM model for each

CTE Career Cluster. Different clusters of programs appear to have heterogeneous effects of CTE completion. Several Clusters show statistically significant evidence of the substitution from 4-year to 2-year institutions, such as: "Environmental, Agricultural, and Natural Resources", "Health and Biosciences", and "Transportation Technologies". Several are associated with decrease in the probability of attending a 4-year institution, such as: "Career Research and Development", "Construction and Development", and "Consumer Services, Hospitality, and Tourism". Others are associated mainly with a positive increase on 2-year enrollment, such as: "Business Management and Finance", "Human Resource Services", and "Information Technology". "Manufacturing, Engineering, and Technology" does not appear to have a significant effect on attendance of any type the year after a student graduates high school.

Table 4.11 provides the same PSM cluster by cluster analysis as Table 4.10, but for enrollment in the sixth year, associate degrees, bachelor's degrees, and certificates by six years after high school. The clusters that appear to shift students from bachelor's degrees to associate degrees are: "Business Management and Finance", "Consumer Services, Hospitality and Tourism", "Health and Biosciences", and "Transportation Technologies". "Career Research and Development", "Construction and Development", and "Environmental, Agricultural, and Natural Resources" have a significant, negative effect on bachelor's degree receipt. "Human Resource Services", "Information Technology", and "Manufacturing, Engineering, and Technology" increase the probability of attaining an associate degree. "Arts, Media, and Communication" and "Health and Biosciences" significantly increase the likelihood of attaining a certificate.

Tables 4.12 and 4.13 examine the earnings effects by the program cluster. Since the table is restricted to the 2009-2010 academic year, the sample sizes for each PSM are fairly small, and the standard errors fairly large. Several have positive earnings effects, but are measured too imprecisely. However, several clusters have significant positive effects on earnings. "Business Management and Finance" has positive effects of $2,321 and $2,853 on earnings after six years and earnings in the first year of employment, respectively. These effects amount to 10.5% and 17.2% effects, respectively. The Lee [2009] lower bounds (in brackets) are positive for each outcome, suggesting that the effect may not entirely be due to a difference in students who complete this cluster. "Human Resource Services" also has positive effects of $2,064 and $1,604, respectively, with the Lee lower bound being positive for the earnings after six years. "Career Research and Development", "Construction and Development", and "Transportation Technologies" have positive effects on earnings after 6 years of $2,933, $3,266, and $7,287, respectively. These clusters have smaller and statistically insignificant effects on earnings at first employment. This pattern could suggest that the way students who complete these clusters are better off six years after graduation compared to non-completers might have a tenure component.

### 4.5.2 Instrumental Variables

To present the IV estimates, Tables 4.14 through 4.16 are designed in the following way. Each of the four panels corresponds to a different version of the time distance instrument, with $A$, $B$, $C$, $D$ showing the results for the $\leq 11$ minutes, $\leq 11$

minutes, $\leq$ 11 minutes, and $log(time)$, respectively. The first column of the table displays the first stage estimates of the effect of the instrument on the probability of completing a CTE program. In each table, the following columns then show the reduced form and IV effect on the enrollment, degree, and earnings outcomes. In each Table, the continuous distance measure has opposite signed reduced-form effects to the other measures, but in the text I will describe it using the opposite sign to compare to the other measures.

In Table 4.14, it is possible to see that the driving time to a CTE Center has a negative effect on completing a CTE program. Being within 11, 16, and 20 minutes of a CTE Center increases CTE Completion by 5.1, 3.6, and 4.3 percentage points, respectively. These correspond to a 39%, 27%, and 33% increase in the likelihood of completing a CTE program, respectively. Panel D shows that a log-point increase in driving time is associated with a 2.3 percentage point, or 17.8% decrease in the probability of completing a program. The distance (in time) instruments, therefore, appear to be highly relevant to the CTE completion decision.

Columns (2) and (3) of Table 4.14 show the reduced-form and IV effects of attending a 2-year institution immediately after high school, and Columns (4) and (5) show the same effects for 4-year institutions. The estimates using the 11, 16 and continuous instruments show that shorter distances have significant positive effects on attending a two year institution, with reduced form effects of 1.5 ($p < .05$), 2.6 ($p < .01$), 0.5 ($p < .05$) percentage points, respectively. These correspond to large positive effects of CTE completion in the IV estimation of 30, 72, and 22 percentage points, respectively. These are large estimates as, each each is either near or over 100% increases over the existing mean probability of attending 2-year institutions. The 20 minute instrument is positive, but statistically insignificant. Each instrument produces negative effects on attending a four-year institution. The 11, 26, 20, and log instruments produce reduced-form effects on four-year attendance of -1.4 ($p < .05$), -2.1 ($p < .01$), -1.2 ($p < .05$), and -0.5 ($p < .05$) percentage points, respectively. These correspond to large decreases in the IV models of 28, 57, 28, and 22 percentage points, respectively, in response to completing a CTE program.

In Table 4.15, Columns (2) and (3) show the effects on associate degrees, Columns (4) and (5) show the effect on bachelor's degrees, and Columns (6) and (7) show the effect on certificate receipt. Of the four versions of the instrument, only the 16 minute instrument produces significant effects ($p < .01$) on associate degree receipt, with a reduced form effect of 1.4 percentage points, and an IV effect of 39 percentage points. Each version of the instrument produces a negative effect on bachelors degree receipt. The 11 minute, 16 minute, 20 minute, and $log(time)$ produce reduced-form effects of -1.8, -2.2, -2.1, and -0.7 percentage point effects, with each being signficant at the $p < .01$ level. These reduced form effects are associated with large IV effects of 34.3, 60.4, 47.7, and 28.5 percentage points for the effect of completing a CTE program on bachelor's degree receipt. Each version of the instrument also produces positive effects on the receipt of certificates. The reduced-form effects for the 11 minute, 16 minute, 20 minute, and $log(time)$ instruments produce positive effects of 0.6, 1.3, 1.1, and 0.4 percentage point effects, respectively, with each effect significant at the $p < .01$ level. The IV effects are quite large, and given the small percent of students who complete certificates, 2.9%, these all represent very large increases of the dependent mean in the probability of completing a certificate.

Finally, Table 4.16 shows the reduced-form and IV effects on earnings in the sixth year (Columns (2) and (3)) and earnings in the first year of any employment (Columns (4) and (5)). This table only uses data from the 2009-2010 year. Three versions of the instrument produces positive effects on earnings six years later. The 11 minute, 16 minute, and continuous instrument produce significant reduced-form effects of \$1,148 ($p < .05$), \$769 ($p < .1$), and \$418 ($p < .05$), respectively. Each of these corresponds to a very large effect on annual earnings, with each very nearly a 100% increase over the existing dependent mean. These effects magnitudes seem likely implausible large. There are smaller, but insignificant effects found on the earnings in the first year of any employment, with the exception of the continuous distance instrument, which has a positive effect of \$315 ($p < .05$) on these earnings, corresponding with an IV effect of \$15,420.

The distance instruments produce local average treatment effects that are specific to the types of students who are induced to complete a CTE program by the distance, including the types of CTE programs that students complete. This makes the comparison versus the matching results difficult, because the group of "complier" students do not produce an average treatment effect directly comparable to the average treatment effect of the matching. However, between the two sets of results, there are patterns to suggest that the two sets of estimates have some consistency. CTE Centers tend to offer programs in capital-intensive areas of education, such as "Construction and Development", "Health and Biosciences", "Consumer Services, Hospitality, and Tourism", "Manufacturing, Engineering and Technology", and "Transportation Technologies". These are likely the types of programs the instrument is inducing students to complete, and when looking at these programs between the two types of estimation, there are many consistencies. "Health and Biosciences" offers many nursing programs that can lead to certificates, and we see both a substitution between 4-year and 2-year institutions and an increase in certificate receipt in both sets of estimates, for example. "Construction and Development", "Consumer Services, Hospitality, and Tourism", and "Transportation Technologies" have large negative effects on 4-year degree completion in the matching estimates, and the IV estimates produce large negative effects on bachelor's degree completion. While these results are not directly comparable, a consistent story between the two estimation strategies could be told.

## 4.6 Conclusion and Future Directions

Using data on CTE completion from the Maryland Longitudinal Data System, propensity score matching, and IV approaches, I find that CTE education is associated with a decrease in enrollment and degree completion at four-year institutions, with substitution towards two-year institutions. This appears to have a positive effect on a students early career earnings. These results are consistent with recent research, such as that of Kreisman and Stange (2016), who find some substitution between types of institutions and positive earnings effects, though do not the substitution extending to the types of degrees received. With the matching strategy, it is reasonable to be concerned about bias due to selection on unobservables, with students possibly selecting to enroll in CTE programs based on private knowledge about variables such their unobservable skills. Using the instrumental variables approach as a complement to the matching strategy, I find patterns similar to the matching results, especially when considering the effects of the types of programs that students will typically complete at a CTE Center.

There are several possible avenues for future research using data from the MLDS on CTE completion. As additional years of earnings and enrollment data are added to the system, even longer-term effects can be estimated using similar approaches. Especially in the case of earnings, this would be useful to examine the evolution of earnings to see whether CTE completers are eventually overtaken by four-year degree completers, or whether the earnings effect of CTE completion persists. In both the matching and IV estimates, I find some evidence that would suggest that some of the earnings gains for CTE completers might be attributable to tenure effects, which could possibly fade out over time as non-completer spend more time in the workforce.

Additionally, given that the driving distance to a CTE center appears to be a reasonably strong instrument, it is possible to use this approach to estimate the effect of CTE on high school completion. This paper focuses on students who graduate high school, as the available data can only determine CTE completion among high school graduates, however, it is possible to use a reduced form approach to estimate the effect on high school completion, where high school completion would be regressed on the distance to a CTE center. This would provide a proper counterfactual analysis for students who may only complete a CTE program where dropping out of high school might be a relevant outside option, and therefore could address the types of students that are left out of the analysis in this paper.

## 4.7 Figures

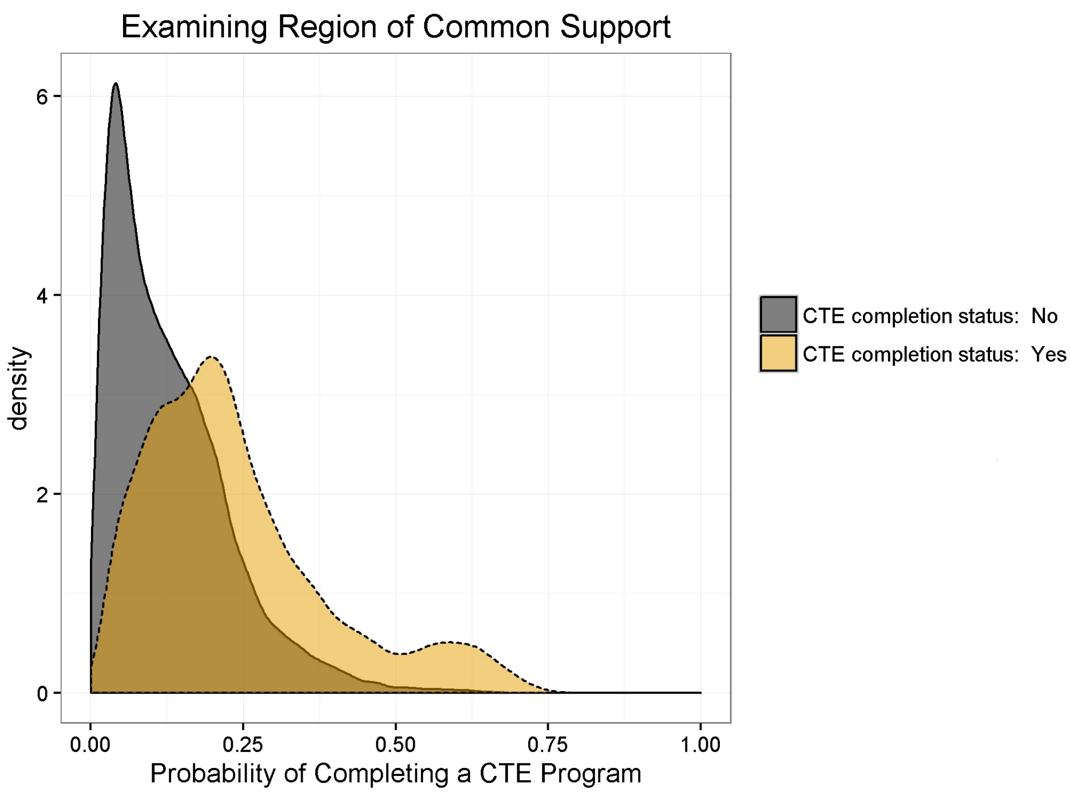



Figure 4.1: Area of Common Support for the Propensity Score

Note: This figure shows the region of common support of the propensity scores of both CTE completers and non-completers. A logistic regression was used to generate the propensity score and densities of the propensity score are shown for CTE completion and non-completion.

## 4.8 Tables

| Variable | No CTE | CTE Only | CTE and USM |
|---|---|---|---|
| % Male | 46 | 59 | 47 |
| % White | 57 | 64 | 60 |
| %Black | 28 | 29 | 30 |
| %Asian | 8 | 2 | 4 |
| % Hispanic | 7 | 4 | 5 |
| % FARMS | 21 | 33 | 24 |
| % with HSA Algebra | 29 | 69 | 33 |
| Avg. HSA Algebra | 429 | 421 | 431 |
| % with HSA English | 96 | 96 | 98 |
| Avg. HSA English | 426 | 402 | 419 |
| % with HSA Biology | 80 | 94 | 86 |
| Avg. HSA Biology | 435 | 414 | 431 |
| Four-year Distance | 10 | 13 | 11 |
| Two-year Distance | 7 | 10 | 7 |
| Weeks Absent | 2 | 3 | 2 |
| School- HSA Pass % | 61 | 66 | 62 |
| School- # CTE Programs | 11 | 15 | 13 |
| School- FARMS % | 23 | 24 | 26 |
| N | 61,838 | 9,584 | 10,476 |

Table 4.1: Summary Statistics by CTE Completion Type

Note: Table 4.1 displays demographic, test score, and school level demographic characteristics for the students in the 2009-2010 and 2010-2011 high school graduate cohort in the MLDS data. The summary statistics are given by CTE and USM completion status. Variables labeled “School-” are school level covariates.

| Variable | No CTE | CTE Only | CTE and USM |
|---|---|---|---|
| % Initial 2-year Enrollment | 28 | 27 | 37 |
| % Initial 4-year Enrollment | 48 | 7 | 36 |
| % Associate's degree earned | 12 | 7 | 17 |
| % Bachelor's degree earned | 47 | 6 | 36 |
| % Without any Wages | 31 | 36 | 35 |
| Annual Wages 6 years later | $22,055 | $21,353 | $24,371 |

Table 4.2: Raw Mean Outcomes by CTE Completion Type

Note: Table 4.2 displays raw mean outcomes for the students in the 2009-2010 and 2010-2011 high school graduate cohort in the MLDS data (earnings outcomes are restricted to the 2009-2010 graduating cohort). The table describes initial enrollment by institution, degree receipt within six years by type of degree, the percentage without any earnings, and the average earnings among those with positive earnings. The summary statistics are given by CTE and USM completion status.

| Program (% Completing) | CTE Only | CTE and USM |
|---|---|---|
| Arts, Media, and Communication | 3 | 6 |
| Business, Management, and Finance | 10 | 18 |
| Career Research and Development | 21 | 6 |
| Construction and Development | 14 | 8 |
| Consumer Services, Hospitality, and Tourism | 13 | 10 |
| Environmental, Agricultural, and Natural Resources | 4 | 3 |
| Health and Biosciences | 4 | 12 |
| Human Resource Services | 14 | 17 |
| Information Technology | 3 | 7 |
| Manufacturing, Engineering, and Technology | 3 | 8 |
| Transportation Technologies | 9 | 4 |

Table 4.3: Breakdown of CTE Program Completion: Program Clusters

Note: Table 4.3 shows the type of CTE program completed for the students in the 2009-2010 and 2010-2011 high school graduate cohort in the MLDS data who completed CTE programs. The values are percentages of all students within the type of CTE completion.

| | Standardized Mean Differences | |
|---|---|---|
| | Unmatched | Matched |
| Male | 0.008 | 0.003 |
| White | 0.036 | 0.019 |
| Black | 0.063 | 0.002 |
| Asian | 0.142 | 0.008 |
| Hispanic | 0.055 | 0.025 |
| FARMS | 0.089 | 0.001 |
| % HSA Algebra | 0.089 | 0.012 |
| HSA Algebra | 0.084 | 0.011 |
| % HSA English | 0.054 | 0.006 |
| HSA English | 0.035 | 0.001 |
| % HSA Biology | 0.083 | 0.010 |
| HSA Biology | 0.071 | 0.008 |
| Special Ed. | 0.014 | 0.007 |
| Four-Year Dist. | 0.170 | 0.022 |
| Two-year Dist. | 0.054 | 0.002 |
| Weeks Absent | 0.108 | 0.006 |
| School-HSA Pass Pct | 0.019 | 0.007 |
| School- Number of CTE Programs | 0.315 | 0.002 |
| School-FARMS Pct | | 0.008 |
| *Chi-sq test of joint significance* | | |
| Chi-sq | 6,694.030 | 52.034 |
| df | 48 | 48 |
| p-value | 0 | 0.320 |

Table 4.4: Standardized Mean Differences Before and After Matching

Note: Table 4.4 shows the standardized mean differences between CTE completers and non-completers in the unmatched and matched data. Each value has a Cohen's $d$ interpretation. At the bottom of the table, a Chi squared test of the total observable difference between the CTE completers and non-completers is shown with the degrees of freedom (df) and p-value of the test.

| | *Variable:* | | | |
|---|---|---|---|---|
| | 2-Year Dist. | 4-Year Dist. | HSA Pass Pct. | FARMs Pct. |
| | (1) | (2) | (3) | (4) |
| $log(time)$ | 0.03 | 0.11 | 0.09 | $-0.05$ |

Table 4.5: Correlations of Time Distance and Other Variables

Note: Table 4.5 shows the simple correlations between the log of the driving time between a student's high school in tenth grade and the nearest CTE Center and several school-level characteristics.

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | *Year 1* | | *Year 2* | | *Year 3* | | *Year 4* | |
| | 2-Year | 4-Year | 2-Year | 4-Year | 2-Year | 4-Year | 2-Year | 4-Year |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| CTE | $0.037^{***}$ | $-0.047^{***}$ | $0.031^{***}$ | $-0.052^{***}$ | $0.025^{***}$ | $-0.061^{***}$ | $0.021^{***}$ | $-0.058^{***}$ |
| | (0.007) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.005) | (0.006) |
| Dep. mean | 0.35 | 0.39 | 0.31 | 0.37 | 0.22 | 0.39 | 0.15 | 0.42 |
| Observations | 19,952 | 19,952 | 19,952 | 19,952 | 19,952 | 19,952 | 19,952 | 19,952 |

Table 4.6: Enrollment Effects of CTE Education: CTE & USM

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Table 4.6 shows the results from a regression of the enrollment outcome variable on an indicator for CTE completion, using the propensity score matched data. Robust standard errors in parentheses. Each column corresponds to the type of institution and the number of years after high school graduation. The sample for these regressions includes both the 2009-2010 and 2010-2011 high school graduate cohorts.

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Enrollment-6 years | Associate's | Bachelor's | Certificate |
| | (1) | (2) | (3) | (4) |
| CTE | 0.003 | 0.035*** | −0.051*** | 0.009*** |
| | (0.005) | (0.005) | (0.006) | (0.002) |
| Dep. mean | 0.19 | 0.15 | 0.38 | 0.03 |
| Observations | 19,952 | 19,952 | 19,952 | 19,952 |
| $R^2$ | 0.023 | 0.050 | 0.207 | 0.021 |

Table 4.7: Degree Effects: CTE & USM

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.7 shows the results from a regression of the outcome variable on an indicator for CTE completion, using the propensity score matched data. The outcomes are postsecondary enrollment after six years, and earnings associate's, bachelor's, or certificates within six years after graduation. Robust standard errors in parentheses. The sample for these regressions includes both the 2009-2010 and 2010-2011 high school graduate cohorts

| | *Dependent variable:* | | |
|---|---|---|---|
| | Has Earnings | Earnings 6 years | Earnings First Emp. |
| | (1) | (2) | (3) |
| CTE | 0.032*** | 2,050*** | 1,444*** |
| | (0.009) | (414) | (359) |
| Lee Bound | | (-256,3315) | (-795,2525) |
| Dep. Mean | 0.74 | 22,255 | 16,501 |
| Observations | 9,656 | 7,110 | 7,110 |

Table 4.8: Earnings Effects: CTE & USM

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.8 shows the results from a regression of the outcome variable on an indicator for CTE completion, using the propensity score matched data. The outcomes is an indicator for having any earnings, earnings six year after graduation, and earnings at the time of first employment. Robust standard errors in parentheses. The sample for these regressions includes just the 2009-2010 high school graduate cohort and restricts the sample to those with positive earnings. Lee bounds are provided according to the Lee [2009] method for each earning outcome.

| | *Dependent variable:* | |
|---|---|---|
| | Earn. 6 years | Earn.First |
| | (1) | (2) |
| CTE | 1,905*** | 1,300*** |
| | (412) | (348) |
| | [1011, 2798] | [550, 2050] |
| Observations | 9,656 | 9,656 |
| $R^2$ | 0.035 | 0.036 |

Table 4.9: Earnings Effects: Using Multiple Imputation

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.9 shows the results from a regression of the outcome variable on an indicator for CTE completion, using the propensity score matched data with multiple imputation for the earnings outcomes. The outcomes are earnings six year after graduation and earnings at the time of first employment. Robust standard errors in parentheses. The sample for these regressions includes just the 2009-2010 high school graduate cohort is the result of a multiple impuation procedure using 5 imputed data sets. Standard errors in parentheses and 95% confidence intervals in brackets.

| | *Dependent variable:* | | |
|---|---|---|---|
| | Two-year | Four-year | Observations |
| | (1) | (2) | (3) |
| Arts, Media, and Communication | 0.045* | −0.028 | 1,252 |
| | (0.026) | (0.024) | |
| Business Management and Finance | 0.029** | 0.001 | 3,980 |
| | (0.015) | (0.014) | |
| Career Research and Development | 0.044 | −0.154*** | 1,204 |
| | (0.027) | (0.022) | |
| Construction and Development | −0.032 | −0.068*** | 1,564 |
| | (0.024) | (0.021) | |
| Consumer Services, Hospitality and Tourism | 0.017 | −0.100*** | 2,240 |
| | (0.020) | (0.019) | |
| Environmental, Agricultural and Natural Resources | 0.086** | −0.134*** | 658 |
| | (0.038) | (0.033) | |
| Health and Biosciences | 0.097*** | −0.038** | 2,498 |
| | (0.018) | (0.018) | |
| Human Resource Services | 0.040** | −0.010 | 3,604 |
| | (0.016) | (0.014) | |
| Information Technology | 0.060*** | −0.006 | 1,510 |
| | (0.025) | (0.024) | |
| Manufacturing, Engineering and Technology | 0.001 | 0.022 | 1,644 |
| | (0.022) | (0.022) | |
| Transportation Technologies | 0.063** | −0.222*** | 872 |
| | (0.032) | (0.028) | |
| Dep. mean | 0.35 | 0.39 | |

Table 4.10: CTE Initial Enrollment Effects by Program Cluster

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.10 shows the results from a regression of the enrollment outcome variable on an indicator for CTE completion, using the propensity score matched data. Each row of estimates corresponds to a seperate propensity-score matching process where CTE completers of the specific CTE program were matched to the pool of non-completers. Robust standard errors in parentheses. The sample for these regressions includes both the 2009-2010 and 2010-2011 high school graduate cohorts.

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Enr. | Assoc. | Bach. | Cert. |
| | (1) | (2) | (3) | (4) |
| Arts, Media, and Communication | 0.036 | 0.022 | −0.036 | 0.016* |
| | (0.022) | (0.018) | (0.024) | (0.009) |
| Business Management and Finance | −0.0002 | 0.020* | 0.044*** | 0.003 |
| | (0.012) | (0.010) | (0.013) | (0.006) |
| Career Research and Development | −0.009 | −0.012 | −0.157*** | −0.006 |
| | (0.020) | (0.019) | (0.022) | (0.012) |
| Construction and Development | −0.058*** | −0.0003 | −0.098*** | −0.004 |
| | (0.019) | (0.017) | (0.021) | (0.009) |
| Consumer Services, Hospitality and Tourism | −0.024 | 0.081*** | −0.096*** | −0.002 |
| | (0.017) | (0.016) | (0.018) | (0.007) |
| Environmental, Agricultural and Natural Resources | −0.046 | 0.048 | −0.109*** | 0.006 |
| | (0.030) | (0.033) | (0.034) | (0.013) |
| Health and Biosciences | 0.050*** | 0.061*** | −0.053*** | 0.017** |
| | (0.017) | (0.015) | (0.017) | (0.007) |
| Human Resource Services | 0.006 | 0.028** | 0.014 | 0.006 |
| | (0.013) | (0.012) | (0.014) | (0.006) |
| Information Technology | 0.044** | 0.058*** | −0.025 | 0.017 |
| | (0.021) | (0.017) | (0.023) | (0.010) |
| Manufacturing, Engineering and Technology | 0.052*** | 0.034** | 0.008 | 0.011 |
| | (0.019) | (0.017) | (0.021) | (0.008) |
| Transportation Technologies | −0.060** | 0.055** | −0.183*** | 0.013 |
| | (0.024) | (0.024) | (0.028) | (0.011) |
| Dep. mean | 0.2 | 0.13 | 0.35 | 0.03 |

Table 4.11: CTE Degree Effects by Program Cluster: CTE & USM

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.11 shows the results from a regression of the degree outcome variable on an indicator for CTE completion, using the propensity score matched data. Each row of estimates corresponds to a seperate propensity-score matching process where CTE completers of the specific CTE program were matched to the pool of non-completers. Robust standard errors in parentheses. The sample for these regressions includes both the 2009-2010 and 2010-2011 high school graduate cohorts. For the purposes of a concise table, the Ns for each estimation correspond to the Ns of Table 4.10

| | *Dependent variable:* | |
|---|---|---|
| | Earn. 6 years | Earn. First. |
| | (1) | (2) |
| Arts, Media, and Communication | 788 | −1,781 |
| | (1,806) | (1,562) |
| | [-1774, 2096] | [-2830, -844] |
| Business Management and Finance | 2,321*** | 2,853*** |
| | (844) | (744) |
| | [695, 3385] | [925, 3389] |
| Career Research and Development | 2,933* | 1,000 |
| | (1,499) | (1,215) |
| | [-950, 3789] | [-1803, 1575] |
| Construction and Development | 3,266** | 519 |
| | (1,615) | (1,428) |
| | [1835, 4838] | [-811, 1957] |
| Consumer Services, Hospitality and Tourism | −1,286 | −1,894* |
| | (1,537) | (1,135) |
| | [-3553, 176] | [-3925, -666] |
| Environmental, Agricultural and Natural Res. | −237 | −2,532 |
| | (2,064) | (1,778) |
| | [-1321, 481] | [-4196, -2962] |

Table 4.12: CTE Earnings Effects by Program Cluster

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.12 shows the results from a regression of the earnings outcome variable on an indicator for CTE completion, using the propensity score matched data. Each row of estimates corresponds to a seperate propensity-score matching process where CTE completers of the specific CTE program were matched to the pool of non-completers. Robust standard errors in parentheses and Lee [2009] Bounds in brackets. The sample for these regressions includes just the 2009-2010 high school graduate cohort.

| | *Dependent variable:* | |
|---|---|---|
| | Earn. 6 years | Earn. First. |
| | (1) | (2) |
| Health and Biosciences | 543 | 862 |
| | (1,160) | (1,040) |
| | [-2831, 3263] | [-2959, 2025] |
| Human Resource Services | 2,064** | 1,604** |
| | (871) | (802) |
| | [68, 3228] | [-627, 2128] |
| Information Technology | 1,388 | 2,025 |
| | (1,815) | (1,589) |
| | [-4531, 4149] | [-4396, 3256] |
| Manufacturing, Engineering and Technology | −693 | 276 |
| | (1,847) | (1,675) |
| | [-5471, 6055] | [-6301, 4143] |
| Transportation Technologies | 7,287*** | 1,635 |
| | (2,330) | (1,785) |
| | [-618, 10821] | [-2190, 2499] |

Table 4.13: CTE Earnings Effects by Program Cluster (Continued)

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.13 shows the results from a regression of the earnings outcome variable on an indicator for CTE completion, using the propensity score matched data. Each row of estimates corresponds to a seperate propensity-score matching process where CTE completers of the specific CTE program were matched to the pool of non-completers. Robust standard errors in parentheses and Lee [2009] Bounds in brackets. The sample for these regressions includes just the 2009-2010 high school graduate cohort.

| | *Dependent variable:* | | | | |
|---|---|---|---|---|---|
| | Complete CTE | 2-Year | | 4-Year | |
| | (1) | (2) | (3) | (4) | (5) |
| *A. Center ≤ 11 min.* | | | | | |
| Center ≤ 11 min | 0.051*** | 0.015** | | −0.014** | |
| | (0.005) | (0.006) | | (0.006) | |
| Complete CTE | | | 0.296** | | −0.280** |
| | | | (0.118) | | (0.121) |
| *B. Center ≤ 16 min.* | | | | | |
| Center ≤ 16 min | 0.036*** | 0.026*** | | −0.021*** | |
| | (0.004) | (0.005) | | (0.005) | |
| Complete CTE | | | 0.724*** | | −0.570*** |
| | | | (0.163) | | (0.159) |
| *C. Center ≤ 20 min.* | | | | | |
| Center ≤ 20 min | 0.043*** | 0.005 | | −0.012** | |
| | (0.004) | (0.006) | | (0.006) | |
| Complete CTE | | | 0.118 | | −0.277** |
| | | | (0.134) | | (0.140) |
| *D. Log of time* | | | | | |
| *log(time)* | −0.023*** | −0.005** | | 0.005** | |
| | (0.002) | (0.002) | | (0.002) | |
| Complete CTE | | | 0.222** | | −0.219** |
| | | | (0.100) | | (0.103) |
| Dep. Mean | 0.129 | 0.284 | 0.284 | 0.513 | 0.513 |
| Observations | 29,885 | 29,885 | 29,885 | 29,885 | 29,885 |

Table 4.14: First Stage, Reduced Form, and IV Estimates on Enrollment One Year Later

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.14 displays of the result of the IV estimation on enroll by type of institution in the year after high school graduation. Each panel corresponds to a different version of the time distance instrument. For each panel, the first column is the reduced form effect on the probability of completing a CTE program, Columns (2) and (3) are the reduced form and IV effects on 2-year enrollment, and Columns (4) and (5) are the reduced form and IV effects on 4-year enrollment. Robust standard errors in parentheses.

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | CTE | Assoc. | | Bach. | | Cert. | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *A. Center ≤ 11 min.* | | | | | | | |
| Center ≤ 11 min | 0.051*** | 0.008 | | −0.018*** | | 0.006*** | |
| | (0.005) | (0.005) | | (0.006) | | (0.002) | |
| Complete CTE | | | 0.147 | | −0.343*** | | 0.126*** |
| | | | (0.092) | | (0.121) | | (0.046) |
| *B. Center ≤ 16 min.* | | | | | | | |
| Center ≤ 16 min | 0.036*** | 0.014*** | | −0.022*** | | 0.013*** | |
| | (0.004) | (0.004) | | (0.005) | | (0.002) | |
| Complete CTE | | | 0.390*** | | −0.604*** | | 0.360*** |
| | | | (0.121) | | (0.159) | | (0.068) |
| *C. Center ≤ 20 min.* | | | | | | | |
| Center ≤ 20 min | 0.043*** | −0.002 | | −0.021*** | | 0.011*** | |
| | (0.004) | (0.005) | | (0.006) | | (0.002) | |
| Complete CTE | | | −0.036 | | −0.477*** | | 0.260*** |
| | | | (0.106) | | (0.143) | | (0.058) |
| *D. Log of time* | | | | | | | |
| *log*(*time*) | −0.023*** | −0.0004 | | 0.007*** | | −0.004*** | |
| | (0.002) | (0.002) | | (0.002) | | (0.001) | |
| Complete CTE | | | 0.017 | | −0.285*** | | 0.160*** |
| | | | (0.078) | | (0.103) | | (0.040) |
| Dep. Mean | 0.129 | 0.138 | 0.138 | 0.518 | 0.518 | 0.029 | 0.029 |
| Observations | 29,885 | 29,885 | 29,885 | 29,885 | 29,885 | 29,885 | 29,885 |

Table 4.15: First Stage, Reduced Form, and IV Estimates on Degrees and Certificates

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Table 4.15 displays of the result of the IV estimation on an indicator for the type of degree received within six years after high school graduation. Each panel corresponds to a different version of the time distance instrument. For each panel, the first column is the reduced form effect on the probability of completing a CTE program, Columns (2) and (3) are the reduced form and IV effects on an associate's degree, Columns (4) and (5) are the reduced form and IV effects on a bachelor's degree, and Columns (6) and (7) are the reduced form and IV effects on certificate receipt. Robust standard errors in parentheses.

| | *Dependent variable:* | | | | |
|---|---|---|---|---|---|
| | Complete CTE | Earn. 6 Years | | Earn. First | |
| | (1) | (2) | (3) | (4) | (5) |
| *A. Center ≤ 11 min.* | | | | | |
| Center ≤ 11 min | 0.050*** | 1,148** | | 614 | |
| | (0.007) | (450) | | (400) | |
| Complete CTE | | | 23,125** | | 12,361 |
| | | | (9,743) | | (8,268) |
| *B. Center ≤ 16 min.* | | | | | |
| Center ≤ 16 min | 0.043*** | 769* | | 482 | |
| | (0.006) | (399) | | (354) | |
| Complete CTE | | | 21,707* | | 13,610 |
| | | | (11,974) | | (10,319) |
| *C. Center ≤ 20 min.* | | | | | |
| Center ≤ 20 min | 0.037*** | 434 | | 27 | |
| | (0.006) | (441) | | (392) | |
| Complete CTE | | | 12,790 | | 790 |
| | | | (13,229) | | (11,545) |
| *D. Log of time* | | | | | |
| $log(time)$ | −0.021*** | −418** | | −315** | |
| | (0.003) | (177) | | (157) | |
| Complete CTE | | | 20,434** | | 15,420* |
| | | | (9,138) | | (8,018) |
| Dep. Mean | 0.129 | $23,242 | $23,242 | $18,003 | $18,003 |
| Observations | 14,720 | 8,962 | 8,962 | 8,962 | 8,962 |

Table 4.16: First Stage, Reduced Form, and IV Estimates on Enrollment One Year Later

Note: *p<0.1; **p<0.05; ***p<0.01. Table 4.15 displays of the result of the IV estimation on the earnings outcomes. Each panel corresponds to a different version of the time distance instrument. For each panel, the first column is the reduced form effect on the probability of completing a CTE program, Columns (2) and (3) are the reduced form and IV effects on earnings six years after graduation, and Columns (4) and (5) are the reduced form and IV effects on earnings in the first year of any employment. The sample is restricted to those with positive earnings in year 6 after high school graduation. Robust standard errors in parentheses.

# Bibliography


Katharine G Abraham and Melissa A Clark. Financial aid and students college decisions evidence from the district of columbia tuition assistance grant program. *Journal of Human resources*, 41(3):578–610, 2006.

Judie Ahn. Technical working group on career and technical education meeting. meeting summary (washington, dc, september 22, 2017). *Institute of Education Sciences*, 2017.

Sigal Alon. Who benefits most from financial aid? the heterogeneous effect of need-based grants on students' college persistence. *Social Science Quarterly*, 92(3): 807–829, 2011.

Joseph G Altonji. The demand for and return to education when education outcomes are uncertain. *Journal of Labor Economics*, 11(1, Part 1):48–83, 1993.

Joshua Angrist, Sally Hudson, and Amanda Pallais. Leveling up: Early results from a randomized evaluation of post-secondary aid. Technical report, National Bureau of Economic Research, 2014.

American Psychological Association APA. NHSC loan repayment: Your Questions Answerd, 2007.

Peter Arcidiacono, Patrick Bayer, and Aurel Hizmo. Beyond signaling and human capital: Education and the revelation of ability. *American Economic Journal: Applied Economics*, 2(4):76–104, 2010.

Peter Arcidiacono, Esteban Aucejo, Arnaud Maurel, and Tyler Ransom. College attrition and the dynamics of information revelation. *National Bureau of Economic Research*, 2016.

David H. Autor. Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing. *Journal of Labor Economics*, 21(1):1–42, 2003.

Darra Ballance, Denise Kornegay, and Paul Evans. Factors that Influence Physicians to Practice in Rural Locations: A Review and Commentary. *Journal of Rural Health*, 25(3), 2009.

Eric Bettinger. Need-based aid and college persistence: The effects of the ohio college opportunity grant. *Educational Evaluation and Policy Analysis*, 37(1_suppl): 102S–119S, 2015.

Eric Bettinger, Oded Gurantz, Laura Kawano, Bruce Sacerdote, and Michael Stevens. The long-run impacts of financial aid: Evidence from california's cal grant. *American Economic Journal: Economic Policy*, 11(1):64–94, 2019.

Julian R Betts, Andrew Zau, John McAdams, and Dallas Dotter. Career and technical education in san diego: a statistical analysis of course availability, students course-taking patterns, and relationships between high school and postsecondary outcomes, 2014.

John H Bishop and Ferran Mane. Raising academic standards and vocational concentrators: Are they better off or worse off? *Education Economics*, 13(2):171–187, 2005.

Olivier J. Blanchard and Lawrence F. Katz. Regional Evolutions. *Brookings Papers on Economic Activity*, 1:1–75, 1992.

George J. Borjas, Stephen G. Bronars, and Stephen J. Trejo. Self-selection and internal migration in the United States. *Journal of Urban Economics*, 32(2):159 – 185, 1992.

John Bound, Michael F Lovenheim, and Sarah Turner. Why have college completion rates declined? an analysis of changing student preparation and collegiate resources. *American Economic Journal: Applied Economics*, 2(3):129–157, 2010.

Katharine M Broton, Sara Goldrick-Rab, and James Benson. Working for college: The causal impacts of financial grants on undergraduate employment. *Educational Evaluation and Policy Analysis*, 38(3):477–494, 2016.

Meta Brown, John Karl Scholz, and Ananth Seshadri. A new test of borrowing constraints for education. *The Review of Economic Studies*, 79(2):511–538, 2011.

Gharad Bryan, Dean Karlan, and Scott Nelson. Commitment Devices. *Annual Review of Economics*, 2:671–98, 2010.

Gregorio Caetano, Harry A. Patrinos, and Miguel Palacios. Measuring Aversion to Debt: An Experiment Among Student Loan Candidates. *The World Bank: Policy Research Working Papers*, 2011.

Stephen V Cameron and Christopher Taber. Estimation of educational borrowing constraints using returns to schooling. *Journal of political Economy*, 112(1):132–182, 2004.

Celeste K Carruthers and Umut Özek. Losing hope: Financial aid and the line between college and work. *Economics of education review*, 53:1–15, 2016.

Benjamin L Castleman and Bridget Terry Long. Looking beyond enrollment: The causal effect of need-based grants on college access, persistence, and graduation. *Journal of Labor Economics*, 34(4):1023–1073, 2016.

Matias D Cattaneo, Roc'io Titiunik, Gonzalo Vazquez-Bare, and Luke Keele. Interpreting regression discontinuity designs with multiple cutoffs. *The Journal of Politics*, 78(4):1229–1248, 2016.

Stephanie Riegg Cellini, Fernando Ferreira, and Jesse Rothstein. The value of school facility investments: Evidence from a dynamic regression discontinuity design. *The Quarterly Journal of Economics*, 125(1):215–261, 2010.

Yong Chen and Stuart S. Rosenthal. Local amenities and life-cycle migration: Do people move for jobs or fun? *Journal of Urban Economics*, 64(3):519 – 537, 2008.

College Board. Trends in student aid 2017. Technical report, 2017.

Dylan Conger and Lesley J Turner. The effect of price shocks on undocumented students' college attainment and completion. *Journal of Public Economics*, 148: 92–114, 2017.

Christopher Cornwell, David B Mustard, and Deepa J Sridhar. The enrollment effects of merit-based financial aid: Evidence from georgias hope program. *Journal of Labor Economics*, 24(4):761–786, 2006.

David Deming and Susan Dynarski. Into college, out of poverty? policies to increase the postsecondary attainment of the poor. Technical report, National Bureau of Economic Research, 2009.

Jeffrey T Denning. Born under a lucky star: Financial aid, college completion, labor supply, and credit constraints. *Journal of Human Resources*, 2018.

Jeffrey T Denning, Benjamin M Marx, and Lesley J Turner. Propelled: The effects of grants on graduation, earnings, and welfare. *American Economic Journal: Applied Economics*, forthcoming.

Shaun M Dougherty. The effect of career and technical education on human capital accumulation: Causal evidence from massachusetts. *Education Finance and Policy*, pages 1–52, 2016.

Susan Dynarski. Loans, liquidity, and school decisions. In *National Bureau of Economic Research*, 2003a.

Susan Dynarski. Building the stock of college-educated labor. *Journal of human resources*, 43(3):576–610, 2008.

Susan M. Dynarski. Does aid matter? measuring the effect of student aid on college attendance and completion. *American Economic Review*, 93(1):279–288, March 2003b.

Erica Field. Educational Debt Burden and Career Choice: Evidence from an Experiment at NYU Law School. *American Economic Journal: Applied Economics*, 1(1):1–21, 2009.

Frank F Furstenberg and David Neumark. School-to-career and post-secondary education: Evidence from the philadelphia educational longitudinal study. 2005.

Sara Goldrick-Rab, Robert Kelchen, Douglas N Harris, and James Benson. Reducing income inequality in educational attainment: Experimental evidence on the impact of financial aid on college completion. *American Journal of Sociology*, 121 (6):1762–1817, 2016.

Joshua Goodman. Who merits financial aid?: Massachusetts' adams scholarship. *Journal of Public Economics*, 92:2121–2131, 2008.

Michael J. Greenwood. Internal migration in developed countries. volume 1, Part B of *Handbook of Population and Family Economics*, pages 647 – 720. Elsevier, 1997.

Jinyong Hahn, Petra Todd, and Wilbert Van der Klaauw. Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, 69(1):201–209, 2001.

W Lee Hansen. Impact of student financial aid on access. *Proceedings of the Academy of Political Science*, 35(2):84–96, 1983.

James J Heckman, Jora Stixrud, and Sergio Urzua. The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior. *Journal of Labor economics*, 24(3):411–482, 2006.

Caroline M. Hoxby and Sarah Turner. What high-achieving low-income students know about college. *American Economic Review*, 105:514–17, 2015.

Hilary Williamson Hoynes and Diane Whitmore Schanzenbach. Work incentives and the Food Stamp Program. *Journal of Public Economics*, 96(1):151 – 162, 2012.

Guido Imbens and Karthik Kalyanaraman. Optimal bandwidth choice for the regression discontinuity estimator. *The Review of economic studies*, page rdr043, 2011.

Jodie Jackson, C. Ken Shannon, Donald E. Pathman, Elaine Mason, and James Nemitz. A Comparative Assessment of West Virginia's Financial Incentive Programs for Rural Physicians. *Journal of Rural Health*, 19(5), 2003.

Louis Jacobson and Christine Mokher. Florida study of career and technical education. final report. *CNA Corporation*, 2014.

David A Jaeger and Marianne E Page. Degrees matter: New evidence on sheepskin effects in the returns to education. *The review of economics and statistics*, pages 733–740, 1996.

Thomas J. Kane. Rising public college tuition and college entry: How well do public subsidies promote access to college? *National Bureau of Economic Research Working Paper*, 1995.

Thomas J. Kane. A quasi-experimental estimate of the impact of financial aid on college-going. *National Bureau of Economic Research Working Series*, 2003.

John Kennan and James R. Walker. The Effect of Expected Income on Individual Migration Decisions. *Econometrica*, 79(1):211–251, 2011.

Daniel Kreisman and Kevin Stange. Vocational and career tech education in american high schools: The value of depth over breadth. 2016.

Gillian Laven and David Wilkinson. Rural Doctors and Rural Backgrounds: How strong is the Evidence? A Systematic Review. *Australian Journal of Rural Health*, 11:277–284, 2003.

David S Lee. Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, 76(3):1071–1102, 2009.

Pey-Yan Liou and Frances Lawrenz. Optimizing Teacher Preparation Loan Forgiveness Programs: Variables Related to Perceived Influence. *Science Education*, 95 (1):121–144, 2011.

Lance J Lochner and Alexander Monge-Naranjo. The nature of credit constraints and human capital. *American economic review*, 101(6):2487–2529, 2011.

Janet Lorin. Medical School at $278,000 Means Even Bernanke Son Has Debt. *Bloomberg*, 2013.

Michael F Lovenheim. The effect of liquid housing wealth on college enrollment. *Journal of Labor Economics*, 29(4):741–771, 2011.

Benjamin M. Marx and Lesley J. Turner. Borrowing trouble? student loans, the cost of borrowing, and implications for the effectiveness of need-based grant aid. (20850), January 2015.

Christopher Mazzeo. Private lending and student borrowing: a primer. *Footing the tuition bill: The new student loan sector*, pages 74–97, 2007.

Justin McCrary. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics*, 142(2):698–714, 2008.

Christa McGill. Educational Debt and Law Student Failure to Enter Public Service Careers: Bringing Empirical Data to Bear. *Law & Social Inquiry*, 31(3):677–708, 2006.

Jonathan Meer. Evidence on the returns to secondary vocational education. *Economics of education review*, 26(5):559–573, 2007.

Thomas Meissner. Intertemporal Consumption and Debt Aversion: An Experimental Study. *Experimental Economics*, 2015.

Raven Molloy, Christopher L. Smith, and Abigail Wozniak. Internal Migration in the United States. *Journal of Economic Perspectives*, 25(3):173–96, September 2011.

Enrico Moretti and Daniel Wilson. The Effect of State Taxes on the Geographical Location of Top Earners: Evidence from Star Scientists. Working Paper 21120, National Bureau of Economic Research, April 2015.

Enrico Moretti and Daniel J. Wilson. State Incentives for Innovation, Star Scientists and Jobs: Evidence from Biotech. Working Paper 19294, National Bureau of Economic Research, August 2013.

Peter R. Mueser and Philip E. Graves. Examining the Role of Economic Opportunity and Amenities in Explaining Population Redistribution. *Journal of Urban Economics*, 37(2):176 – 200, 1995.

National Center of Education Statistics. The condition of education 2017. Technical report, U.S. Department of Education, 2017.

Philip Oreopoulos and Uros Petronijevic. Making college worth it: A review of the returns to higher education. *The Future of Children*, pages 41–65, 2013.

Donald E. Pathman, Thomas R. Konrad, Tonya S. King, Cora Spaulding, and Donald H. Taylor Jr. Medical Training Debt and Service Commitments: The Rural Consequences. *Journal of Rural Health*, 16(3), 2000.

Donald E. Pathman, Thomas R. Konrad, Tonya S. King, Donald H. Taylor Jr., and Gary G. Koch. Outcomes of States' Scholarship, Loan Repayment, and Related Programs for Physicians. *Medical Care*, 42(6), 2004.

Donald E. Pathman, Jennifer Craft Morgan, Thomas R. Konrad, and Lynda Goldberg. States' Experiences With Loan Repayment Programs for Health Care Professionals in a Time of State Budget Cuts and NHSC Expansion. *The Journal of Rural Health*, 28(4):408–415, 2012.

Stephen M. Petterson, Robert L. Phillips, Jr., Andrew W. Bazemore, and Gerald T. Koinis. Unequal Distribution of the U.S. Primary Care Workforce. *American Family Physician*, 2013.

Maria F. Prada and Sergio Urzúa. One size does not fit all: Multiple dimensions of ability, college attendance, and earnings. *Journal of Labor Economics*, 35(4): 953–991, 2017.

Jesse Rothstein and Cecilia Elena Rouse. Constrained after college: Student loans and early-career occupational choices. *Journal of Public Economics*, 95(12):149 – 163, 2011.

Donald B Rubin. *Multiple imputation for nonresponse in surveys*, volume 81. John Wiley & Sons, 2004.

Neil S Seftor and Sarah E Turner. Back to school: Federal student aid policy and adult college enrollment. *Journal of Human resources*, pages 336–352, 2002.

SD Seifer, K. Vranizan, and K. Grumbach. Graduate medical education and physician practice location: Implications for physician workforce policy. *JAMA*, 274 (9):685–691, 1995.

Barbara Starfield, Leiyu Shi, and James Macinko. Contribution of Primary Care to Health Systems and Health. *The Milbank Quarterly*, 83(3):457–502, 2005.

Ralph Stinebrickner and Todd Stinebrickner. Academic performance and college dropout: Using longitudinal expectations data to estimate a learning model. *Journal of Labor Economics*, 32(3):601–644, 2014.

Charles M. Tiebout. A Pure Theory of Local Expenditures. *Journal of Political Economy*, 64(5):416–424, 1956.

Lesley J Turner. The road to pell is paved with good intentions: The economic incidence of federal student grant aid. *College Park, MD: University of Maryland, Department of Economics. Retrieved April*, 15:2016, 2014.

U.S. Department of Education. National assessment of career and technical education: Final report to congress. Technical report, 2014.

William B. Weeks and Amy E. Wallace. Rural-Urban Differences in Primary Care Physicians' Practice Patterns, Characteristics, and Incomes. *Journal of Rural Health*, 24(2), 2008.

Byron Williams. Who does aid help? examining heterogeneity in the effect of student aid on achievement. *Working Paper*, 2018.

Cristobal Young and Charles Varner. Millionaire Migration and State Taxation of Top Incomes: Evidence From a Natural Experiment. *National Tax Journal*, 64 (2):255–83, 2011.