

Three Essays on the Impact of High School Mathematics Credits on Education and Labor Market Outcomes

by

Alfredo Sosa Gonzalez

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Higher Education and Economics)
in The University of Michigan
2017

Doctoral Committee:

Professor Brian P. McCall, Co-Chair
Professor Jeffrey A. Smith, Co-Chair
Professor John Bound
Professor Stephen L. DesJardins

Alfredo Sosa Gonzalez

asosa@umich.edu

ORCID iD: 0000-0003-1312-4937

© Alfredo Sosa Gonzalez 2017

To my inspiration, my strength, and my everything, without whom
this dissertation would not have been possible: my beautiful wife,
Mariana Sosa. Alfredo and Fernanda, this thesis is dedicated to each
one of you. I hope you always value the importance of education in
your life.

ACKNOWLEDGEMENTS

I would like to express my gratitude to Jeffrey Smith, Brian McCall, John Bound, Stephen DesJardins, Kevin Stange, Melvin Stephens, Michael Mueller-Smith and Julian Hsu for their invaluable feedback and encouragement. Thanks also to participants of the Causal Inference in Education Research Seminar (CIERS) as well as participants of the University of Michigan Labor Seminar. All the mistakes and omissions are my own. For correspondence with the author please use `asosa@umich.edu`.

TABLE OF CONTENTS

DEDICATION	ii
ACKNOWLEDGEMENTS	iii
LIST OF FIGURES	vii
LIST OF TABLES	viii
ABSTRACT	xii
CHAPTER	

I. The Impact of High School Mathematics Credits on Earnings: Evidence from Shocks to Teachers' Labor Supply	1
1.1 Introduction	1
1.2 Research Questions and Contribution	5
1.3 Related Literature	6
1.4 Identification Strategy	9
1.4.1 Identifying Variation	10
1.4.2 Data	11
1.4.3 Econometric Model	17
1.5 Results	19
1.5.1 Impact of potential exposure to STEM programs on Math Credits and Income	19
1.5.2 Impact of Math Credits on Income, College Atten- dance and Bachelor's Degree Attainment	21
1.5.3 Compliant sub-population	30
1.6 Robustness checks	32
1.6.1 Is the effect of STEM programs on income idiosyn- cratic to NLSY?	33
1.6.2 Do STEM programs increase teachers' labor supply?	35
1.6.3 Did states implement STEM teacher programs be- cause economic conditions were good or bad?	39

1.6.4	Do STEM programs induce more teachers but not different teachers?	40
1.7	Conclusions	41
1.8	References	45

II. Estimating Marginal Treatment Effects of High School Mathematics Credits on Income 61

2.1	Introduction	61
2.2	Brief Review of the Literature on the Returns to High School Math Credits	64
2.3	Methods for Estimating Marginal Treatment Effects	65
2.4	Estimated Marginal Treatment Effects	73
2.4.1	Data	73
2.4.2	Sample Characteristics	78
2.4.3	First Stage and Reduced Form	78
2.4.4	Parameter estimates from the normal selection model	81
2.4.5	Estimates of the Returns to Advanced Mathematics Credits: ATE, TT, TUT, MTE.	83
2.4.6	Sensitivity Analysis	87
2.5	Conclusions	91
2.6	References	93

III. Financial Incentives for Teachers in STEM fields: A National Data Set 98

3.1	Motivation	98
3.1.1	Teacher Shortages in the US	98
3.1.2	States' Response to Teacher Shortages	100
3.1.3	Research Questions and Contribution	101
3.2	Related Literature - Financial Incentives and Teachers' Labor Supply	103
3.3	Construction of a National Data Set	105
3.3.1	Existing data on financial incentives for teachers	105
3.3.2	A New Data Set of Financial Incentives for Teachers	106
3.3.3	Examples of STEM Programs	108
3.3.4	Definition of Variables - Program Level Data	109
3.3.5	Definition of Variables - State Level Data	111
3.4	STEM Programs Characteristics	112
3.4.1	STEM Program Characteristics - Variation Across Programs	112
3.4.2	STEM Program Characteristics - Variation Across States	117
3.4.3	STEM Program Characteristics - Variation Across States: Maps	130

3.5	Conclusions	137
3.6	References	139
APPENDICES		141

LIST OF FIGURES

Figure

2.1	Marginal Treatment Effects of Advanced Mathematics Credits - Normal Selection Model	86
2.2	Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n2 and n3	88
2.3	Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n4 and n5	89
2.4	Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n6 and n7	89
2.5	Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n8 and n9	90
2.6	Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n10 and n11	90
3.1	Duration in Years of STEM Programs	115
3.2	Across-State variation on: Duration, Recipients and Expenditures .	132
3.3	Across-State variation on: Program Type	133
3.4	Across-State variation on: Program Focus	134
3.5	Across-State variation on: Recipients	135
3.6	Across-State variation on: Expenditures	136

LIST OF TABLES

Table

1.1	Summary Statistics Analysis Sample N= 4,219. Controls.	50
1.2	Summary Statistics Analysis Sample N= 4,219. Treatment, Outcome, High School Graduation Requirements and Math Reforms and Instrument.	51
1.3	First Stage: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on advanced mathematics credits controlling for demographics, household characteristics, high school math graduation requirements, and state and cohort fixed effects. Please refer to the Appendix for the complete table.	52
1.4	First Stage: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on Total Credits controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects. Please refer to the Appendix for the complete table.	53
1.5	Reduced Form: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on ln(Income age 28) controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects. Please refer to the Appendix for the complete table.	54
1.6	Impact of mathematics credits on log income. N=4,219	55
1.7	Impact of mathematics credits on college attended and bachelors degree attainment	56
1.8	Characteristics of compliers using Abadie's (2003) kappa method. Demographic and household characteristics.	57
1.9	Impact of mathematics credits on log income. N=4,771. Specification 2: Exclude household gross income 1996-1999 and household income to poverty ratio 1996-1999	58

1.10	Impact of mathematics credits on log income. N=4,219. Specification 3: Include interactions of both biological parents and household gross income 1996-1999 and household income to poverty ratio 1996-1999	59
1.11	Impact of mathematics credits on log income. N=4,771. Specification 4: Set missing values of household gross income 1996-1999 and household income to poverty ratio 1996-1999 to zero.	60
2.1	Definitions of the Variables Used in the Empirical Analysis	77
2.2	Summary Statistics of Analysis Sample N= 4,219	79
2.3	First Stage and Reduced Form. Impact of interactions of exposure to STEM programs and state dummies on advanced math and log income. Please refer to the appendix for the complete table. . .	80
2.4	Parameter Estimates - Normal Sample Selection Model . .	81
2.5	Returns to one credit of advanced math	84
2.6	Test of Constancy of MTE. $H_0 : \sigma_{1D} = \sigma_{0D}$	85
2.7	Returns to one credit of advanced math. All instruments.	87
2.8	First Stage / Reduced Form Impact of interactions of potential years of exposure to STEM programs on advanced math and natural logarithm of income.	95
3.1	Distribution of Program Types. N=87	113
3.2	Proportion of Programs with Specific Focus. N=87	113
3.3	Program Type vs Program Focus. N=87	114
3.4	Average annual recipients. N=87	115
3.5	Distribution of Start and End years. N=87	116
3.6	Distribution of Annual Expenditures (2011 USD). N=87	117
3.7	State Level Program Characteristics. N=87	120
3.8	Across States Distribution of STEM Programs Characteristics . . .	122
3.9	Across States Distribution of STEM Program Type	123
3.10	Across States Distribution of STEM Program Focus	125
3.11	Across States Distribution of STEM Program Normalized Characteristics	127
A.1	First Stage: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on advanced mathematics credits controlling for demographics, household characteristics, high school math graduation requirements, and state and cohort fixed effects.	143
A.2	First Stage: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on Total Credits controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects.	147

A.3	Reduced Form: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on ln(Income age 28) controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects.	150
A.4	Reduced Form American Community Survey 2009. Impact of the potential years of exposure to STEM programs on the natural logarithm of personal yearly wages controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.	154
A.5	Reduced Form Survey of Income and Program Participation 2008. Impact of the potential exposure to STEM programs on the natural logarithm of household monthly income controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.	155
A.6	Reduced Form American Community Survey 2009. Impact of the interactions of potential years of exposure to STEM programs and state dummies on the natural logarithm of personal yearly wages controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.	156
A.7	Reduced Form: Survey of Income and Program Participation 2008. Impact of the interactions of potential exposure to STEM programs and state dummies on the natural logarithm of household monthly income controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.	158
A.8	NLSY 97. Impact of potential years of exposure during college on the probability of teaching. Teaching is measured with two dummy variables: ever been a teacher and, individual is a teacher in 2013. .	160
A.9	ACS 09. Impact of potential years of exposure during college on the probability of teaching.	161
A.10	SIPP 08. Impact of potential years of exposure during college on the probability of teaching.	162
A.11	CCD. Impact of implementing a STEM teacher recruitment program on the number of teachers.	163
A.12	CCD. Impact of implementing a STEM teacher recruitment program on the log of the number of teachers.	164
A.13	NLSY 97. Impact of the interactions of potential years of exposure during college and state dummies on the probability of teaching. Teaching is measured with two dummy variables: ever been a teacher and, individual is a teacher in 2013.	165
A.14	ACS 09. Impact of the interactions of potential years of exposure during college and state dummies on the probability of teaching. .	168
A.15	SIPP 08. Impact of the interactions of potential years of exposure during college and state dummies on the probability of teaching. .	171

A.16	BLS. Impact of Economic Conditions on States' decisions of implementing a STEM teacher recruitment program.	173
A.17	BLS. Impact of Economic Conditions on States' decisions of implementing a STEM teacher recruitment program. The role of occupation wages on states' decisions to implement STEM programs. . . .	174

ABSTRACT

Three Essays on the Impact of High School Mathematics Credits on Education and Labor Market Outcomes

by

Alfredo Sosa Gonzalez

Co-Chairs: Jeffrey A. Smith and Brian P. McCall

This dissertation focuses on the role of high school mathematics credits on shaping college access and graduation as well as future earnings. In the first chapter, I exploit state and time variation in shocks to teachers' labor supply to identify the effect of high school mathematics credits on education and labor market outcomes. The results indicate that, on average, each additional year of math increases yearly labor income by about 3%. Other results show that math credits during high school also increase the probability of college attendance and bachelors' degree completion.

In the second chapter, I estimate marginal treatment effects of advanced high school mathematics credits on total labor income at age 28. The results indicate that the average gain from obtaining advanced math credits during high school for a randomly selected individual is about 4%. For people who already enrolled in advanced math credits the gain from another year of math is smaller of about 2.45% whereas that for individuals who never obtained mathematics credits the potential increase is the highest of about 7.39%. To this date, this is the first study of marginal treatment effects in the literature of the impact of high school math credits on labor

market outcomes.

Finally, in the third chapter, I describe the process to obtain a national data set of state-sponsored financial incentives utilized to recruit and retain teachers in STEM fields. Between 1983 and 2016, 41 states implemented 87 unique financial programs aimed at increasing the supply of teachers, especially in shortage subject and geographic areas. The data generated in this chapter helped me to identify and estimate different program evaluation parameters such as the weighted Local Average Treatment Effect in chapter 1, and, in chapter 2, the Average Treatment Effect (ATE), the Average Treatment Effect on the Treated (TT), the Average Treatment Effect on the Untreated (TUT) and, the Marginal Treatment Effect (MTE).

CHAPTER I

The Impact of High School Mathematics Credits on Earnings: Evidence from Shocks to Teachers’ Labor Supply

1.1 Introduction

Examining the benefits derived from high school math credits is important for numerous reasons. First, the amount of mathematics credits students earn during high school is a strong predictor of college readiness and success (Adelman 1999, 2006; Long, Iatarola, and Conger, 2009). Furthermore, mathematics and science test scores of young individuals, which in turn are correlated with the number of math and science credits earned during high school, have been linked not only to economic benefits to individuals (Mitra, 2002) but also to society (Hanushek and Woessmann, 2010; 2012).

Despite the efforts made by state governments and school districts to increase student achievement of college-bound students, some scholars argue that many recent high school graduates are not only unprepared for a college education but also for the workforce (McCormick & Lucas, 2011). For instance, for the class of 2016, 52 percent of all ACT test-takers did not attain college ready status (ACT, 2016). Additionally, some economists claim that the relative position of the US in international assessment

tests (e.g., PISA) is problematic. In 2009, compared to other developed nations, in science the US ranked in the middle, and, below the middle in math (Moretti, 2012).

The number of mathematics credits that high school students obtain is an important policy vehicle that could help state governments and school districts to improve, among other outcomes, college readiness and, state, national and international achievement test scores of the student population. For individuals, obtaining additional advanced math credits during high school might be a ticket to better chances to attend and succeed in college, to enroll and get credits in high-paid majors, to work in more economically rewarding occupations; in short, to enjoy higher income in adulthood. Understanding what influences the course-taking choices of high school students as well as the benefits of obtaining additional math credits is paramount. In this study, I examine the private economic benefits of mathematics credits earned during high school; however, as previously expressed, the social benefits might also be sizable.

Research indicates that the number of high school mathematics credits is a strong predictor of: (a) higher achievement test scores during high school¹; (b) increased college admission test scores²; (c) greater chances of college entry and completion³; (d) better financial market participation and credit management⁴, and, (e) higher earnings in adulthood⁵.

Even when the number of studies that examine the association between high school math credits and education and labor market outcomes is relatively large, most

¹Attewell & Domina, (2008); Lee, Croninger & Smith (1997); Long, Cogner & Iatarola (2012); Gamoran & Hannigan (2000) ; Gamoran (1987); Jones (1987); Lee, Burkam, Chow-Hoy, Smerdon & Goverdt (1998); Welch, Anderson & Harris (1982); Hoffer, Rasinski & Moore (1995); Rock & Pollack (1995); Madigan (1997); Bozick & Ingels (2008); Laing, Engen & Maxey (1987)

²Alexander & Pallas (1984); Sebring (1987)

³Schneider, Swanson & Riegle-Crumb (1998); Attewell & Domina (2008); Long, Cogner & Iatarola (2012); Dougherty, Mellor & Jian (2006); Horn & Kojaku (2001); Adelman, 1999; Adelman 2006; Alexander, Riordan, Fennessey & Pallas, 1982; Clotfelter, Hemelt & Ladd, 2016

⁴Cole, Paulson, & Shastri (2015)

⁵Altonji (1995); Levine & Zimmerman (1995); Rose & Betts (2004); Joensen & Nielsen (2009); Goodman (2012)

studies do not identify and estimate parameters with a causal interpretation. The main motivation of this paper is to contribute to the extant literature that examines the impact of mathematics course taking during high school on education and labor market outcomes by exploiting shocks to teachers labor supply as determinants of course taking behavior. By using the variation in math credits induced by shocks to teacher labor supply, in this study I attempt to provide a causal interpretation of the parameter estimates.

The identification strategy consists on exploiting shocks to teachers labor supply as determinants of math course-taking choices during high school. In the US K-12 education system, there is a longstanding problem of teacher shortages, especially in math and science. In response, many states have implemented financial incentive programs aimed to increase the supply of teachers in hard-to-staff schools. Throughout this paper, I call these programs STEM teacher recruitment programs, or just, STEM programs.

The hypothesized mechanism through which STEM programs induce students to earn additional math credits is the following. When states implement STEM programs, new teachers are recruited to work in some of the most disadvantaged schools. The schools that receive the additional teachers might expand the number and types of courses offered to students. Given the increase in the courses choice set, some individuals might be induced to enroll, and subsequently, to earn credits from subjects that were not available before the implementation of the STEM programs.

The results indicate that, for each additional year of advanced math (i.e., algebra 2 and above), there is an increase in total labor income of about 3 percent. Also, the probability of attending college increases by 8 pp, the probability of attending a 4-year college or university rises by 11 pp, and, finally, the probability of obtaining a bachelors degree increases by 9 pp. All these results are consistent with the current literature.

This study offers various contributions to at least three different bodies of literature. For the literature that examines the returns to high school math credits, this paper is the first that exploits variations in supply-side features of the education system as determinants of education choices. This assertion is relevant since, unlike other papers in the literature, I present evidence of the link between state-level programs aimed to recruit and retain teachers (a policy lever) and high school mathematics course taking behavior. If states want to increase the number of math credits students earn during high school, an alternative might be to design and implement financial incentives for college students with a teaching commitment component, especially in hard-to-staff schools. Moreover, in contrast to comparable papers in this literature⁶, I do control for credits earned in other subjects while estimating the returns to math credits. The decisions to control - or not - for credits in other subjects have important implications when interpreting the results.

For the literature that examines the impact of teacher recruitment programs on teachers' labor supply decisions, even though this study does not evaluate specific programs, I do provide an overall estimate of the effect of such programs on the probability to teach. Also, this study provides a comprehensive picture of the different financial aid programs utilized to recruit and retain math and science teachers in the US. Finally, to the literature that examines the effects of high school math credits on college attendance and bachelors degree attainment, to my knowledge, this is the first study that presents evidence of such effects using instrumental variables estimators.

The remainder of the paper is organized as follows. In section (1.2), I state the research questions and contribution; section (1.3) is devoted to the current literature on the effects of math credits on various measures of earnings. Section (1.4) is devoted to the identification strategy, sample description, definitions of treatment, controls, instruments and outcomes; and the econometric models. The results are presented

⁶Joensen and Nielsen, (2009); Goodman, (2012)

in section (1.5); also, a characterization of the compliant sub-population is provided. Section (1.6) addresses a number of threats to identification. Finally, in section (1.7) conclusions are provided.

1.2 Research Questions and Contribution

The main research question of this paper is, *what are the effects of high school mathematics credits on total labor income?* I also examine the impact of high school math credits on college attendance and bachelor's degree attainment. Since I utilize **Instrumental Variables** estimators, I can only hope to recover a measure of the impact of math on income, **only** for the population of individuals induced by the instrument to change their course taking behavior; in other words, compliers.

This paper contributes to the literature on the impact of high school mathematics courses on education and labor market outcomes by utilizing an institutional feature of the US teacher labor market: the widespread problem of teacher shortages and the responses from states to address this issue. By compiling a complete list of financial incentives aimed at increasing the supply of teachers in hard-to-staff schools and shortage subjects such as mathematics and science, this study provides two important conclusions. First, I estimate a causal effect of mathematics credits on education and labor market outcomes by instrumenting for mathematics credits with state-level financial incentives aimed to attract teachers. Second, the study also examines the impact of financial incentives on the probability of teaching; given that the literature on the impact of financial incentives on teacher recruitment and retention is slim, this study will provide an aggregate estimate of the impact of such efforts in the US context.

1.3 Related Literature

Many studies in many different countries have demonstrated that better-educated individuals earn higher wages, experience less unemployment and work in more prestigious occupations than their less-educated counterparts (Card, 1999). Most of these studies have focused on the number of years of education as the variable of interest. Less attention has been given to the study of which components of the education black box impact labor market outcomes.

Only a handful of studies have attempted to estimate the causal impact of high school math courses on earnings. In all cases, the authors do not distinguish which estimands their estimators are associated to; in other words, these studies do not use the parameters of interest commonly utilized in the program evaluation literature such as the Average Treatment Effect (ATE), Treatment on the Treated (TT), Treatment on the Untreated (TUT), and, Marginal Treatment Effect (MTE.)

Altonji (1995) uses data from the National Longitudinal Survey of the High School Class of 1972 (NLS72) to identify the effect of high school curriculum on wages. As an instrument for course-taking in each subject he uses the high school average number of courses taken in that subject. He defined the outcome as wages which were calculated as earnings divided by hours worked in 1977, 1978 and 1979. He finds that one more year of the combination of science, math, English, social studies and foreign language leads to an increase of wages of only 0.3 percent.

Levine & Zimmerman (1995) used two data sources: the National Longitudinal Survey of Youth (NLSY-79) and the 1980 cohort of the High School and Beyond (HSB) survey, to examine the impact of the number of high school math courses on log weekly wages around 10 years after high school graduation. They also used the high school average number of math and science courses taken as instruments. All the IV models led to statistically insignificant effects; -0.017 for men and -0.060 for

women⁷.

By using the sophomore cohort (1980) of the High School and Beyond (HSB) data, Rose & Betts (2004) estimated the effect of high school mathematics courses on earnings. The outcome was the natural log of annual earnings in 1991, approximately 10 years after high school graduation. This study also used the high school average number of math courses as an instrument. Credits earned in algebra/geometry increased earnings by about 8%. No statistically significant effects were found for intermediate algebra (-0.107), advance algebra (-0.77) and calculus (-0.132)⁸.

In a two-sample instrumental variables (TSIV) framework, Goodman (2012) identified the impact of mathematics courses taken during high school on earnings using the differential timing of state-level increases in high school graduation requirements as a source of exogenous variation. The outcome was the natural log of total earnings from last year. He found that each additional year of math increases black males' earnings by 5-9 %. The impacts on white males are around the same magnitude but statistically insignificant. The results for black (0.035) and white (0.005) women are also statistically insignificant. Finally, by exploiting a national curricular reform in Denmark, Joensen & Nielsen (2009) identified the causal effect of advanced high school mathematics courses on earnings. The authors concluded that math and chemistry - together - increased earnings by around 20 percent.

Currently there is a gap in the literature that investigates the impact of math course taking on labor market outcomes. First, unlike the returns to years of schooling literature which includes hundreds of studies and many countries, the number of studies that seek to measure the impact of math courses on labor market outcomes is very limited. Second, most of the extant studies - Altonji (1995), Rose & Betts (2004),

⁷Even when the estimates are statistically insignificant, they are quite large since they are raw estimates from regressing the dependent variable log weekly earnings on the treatment, the number of math courses.

⁸The numbers in parentheses are the coefficients coming from the IV estimation of the impact of math credits on log earnings.

Levine & Zimmerman (1995) - use an ill-conceived instrument - the per-high school mean of the number of mathematics courses taken - to instrument for math course taking. There are a number of reasons why the average number of math courses at the high school level might be correlated with labor market outcomes, thus violating the exclusion restriction. For example, if the high school is located in an affluent neighborhood with many resources, including teachers, it is likely that the per high school average courses taken in math is correlated to local economic conditions; since income of person i is also correlated with local economic conditions the IV estimates would be biased upward. Under these conditions the instrument is not valid.

Third, Goodman's (2012) estimates restrict the sample to individuals with a high school degree; since high school graduation is an outcome that might be correlated with the reforms and course taking decisions, conditioning on high school graduation (a potential outcome), might bias the results upward⁹. Fourth, none of the studies in the literature present evidence of the strength of the instruments. At the very minimum, the *F-statistic* of a test of the joint significance of all the coefficients of the excluded instruments must be presented. If the F-statistic is included, in the case of one endogenous regressor, it should be at least 10 (Staiger & Stock, 1997). Finally, none of the studies state clearly which parameter they are estimating (e.g, Average Treatment Effect).

This paper attempts to provide solid evidence of the impact of high school mathematics credits on education and labor market outcomes via the following contributions: first, by constructing a data set of all the state-level financial incentives aimed at increasing the supply of teachers to hard-to-staff schools and to subjects with high shortage rates such as mathematics and science, I am able to measure the impact of these exogenous changes on mathematics credits and earnings as well as other out-

⁹Let \mathbf{R} be the reform dummy which for exposition purposes is equal to 1 if individuals were exposed to a reform and 0 otherwise; let HS be an indicator of high school graduation. Since $E[\epsilon|R, HS = 1] > E[\epsilon|R] = 0$ then the IV estimates will be biased upward.

comes and to recover the 2SLS estimates of the impact of mathematics credits on college attendance, bachelor's degree attainment and earnings. To my knowledge this is the first paper - in the returns to math credits literature - that utilizes state level variation in financial aid incentives for teachers as an instrument for math credits.

Second, I also provide estimates of the aggregate impact - across states - of STEM teacher recruitment programs on the probability of teaching. Similar to the returns to math credits literature, the literature that examines the impact of financial incentives on teacher recruitment and retention is sparse. The estimates of the impact of STEM programs on the probability of teaching are fairly consistent across all the data sets utilized in this paper. Third, I provide a local interpretation of the parameter estimates; I do not provide a measure of the Average Treatment Effect on the Treated (ATET), instead, I estimate a weighted measure of Local Average Treatment Effects (LATE's). Fourth, evidence of the relationship between the instrument and earnings in larger data sets is provided¹⁰. This provides strong evidence that the impact of STEM programs on earnings is not idiosyncratic to NLSY 97. Finally, I address potential violations to the exclusion restriction assumption.

1.4 Identification Strategy

The purpose of this study is to identify and estimate the impact of high school mathematics credits on education and labor market outcomes. It is important to place the estimates in the context of the parameters of interest of the program evaluation literature (Cameron & Trivedi, 2005). Because I use 2SLS estimators, I am only able to recover a weighted measure of Local Average Treatment Effects (LATE's)¹¹; i.e., the average gain in total labor income per each additional year of advanced math-

¹⁰Whereas the IV estimates use NLSY97 data $\approx 9,000$ individuals, the reduced form equation was also estimated using the Survey of Income and Program Participation 2008 $\approx 400,000$ individuals each, and American Community Survey, 2009 $\approx 1,500,000$ individuals.

¹¹As Angrist and Pischke (2009) argue, when the 2SLS estimator is calculated using covariates, the parameter of interest represents a weighted average of causal effects for instrument-specific compliers.

ematics credits *only* for the group of individuals who are induced by the instruments to change their course taking behavior. Since by definition LATE is only recovered in a model with no covariates in which both treatment and instrument are binary variables, the 2SLS estimates presented in this paper represent weighted averages of LATE's that result from marginal changes in both - instruments and treatment.

As previously mentioned, in this paper I do not provide measures of other estimands such as the Average Treatment Effect (ATE), Treatment on the Treated (TT), Treatment on the Untreated (TUT), Marginal Treatment Effects (MTE) or Quantile Treatment Effects (QTE). In a separate paper, I estimate ATE, TT, TUT and MTE of high school advanced mathematics credits on total labor income (Sosa, 2017a). In this paper, when I refer to the impact of math credits on education and labor market outcomes, the parameter I estimate is a weighted average of LATEs, and consequently, only pertains to the group of individuals who are induced by the instrument to change their choices.

1.4.1 Identifying Variation

To identify the effect of high school mathematics credits on education and labor market outcomes I use a feature of the US education system - teacher shortages especially in math and science -, and the corresponding policy response from state governments when trying to address this issue.

I use a national data set created by Sosa (2017b) that includes all state-sponsored financial incentive programs aimed at increasing the supply of teachers, especially in math and science and/or critical geographic shortage areas. I call these programs, STEM programs (Sosa, 2017b). Forty one states have implemented at least one program between 1983 and 2016; 87 unique programs have been identified. There is a huge variation across programs in terms of program characteristics such as duration, participants and expenditures, among others.

The mechanism by which STEM programs might influence high school mathematics course-taking behavior is the following. Given the teacher shortages problem, especially in high-poverty neighborhoods, schools and school districts have struggled to hire qualified math and science teachers. Therefore, not all students have equal access to the same number of math teachers per student. Some schools have more resources and can afford hiring extra teachers. Other schools are located in better neighborhoods and might have fewer positions than the number of teachers willing to teach at these schools.

Conversely, some schools might not even have a trigonometry or calculus teacher. The STEM programs try to recruit and retain math and science teachers to work in hard-to-staff schools. Some students that, before the STEM program implementation, did not have access to qualified math teachers, once the STEM program is in place and additional teachers work in their new positions, might have been exposed to a higher number of math teachers, and, thus, to new course offerings and, consequently, alternative course-taking choices. I fully describe the construction of the instruments and exclusion restrictions later in the paper. For a complete description of the STEM Programs Data Set please refer to Sosa (2017b).

1.4.2 Data

The principal source of individual-level data is the National Longitudinal Survey of Youth (NLSY) 1997 cohort, which includes high school transcript information, total labor income as well as a rich set of controls. NLSY 97 is a nationally representative sample of around 9,000 individuals who were 12 to 16 years old as of 12/31/1996.¹² The participants are re-surveyed on a biannual basis. Thus, NLSY 97

¹²Quote from NLSY web page March 1, 2016: "A transcript information is available on the NLSY97 data file for 6,232 respondents, or about 69 percent of the 8,984 respondents who participated in the initial round of the NLSY 97. The respondents for whom transcripts were not obtained mainly include those who did not sign written consent forms to contact their schools, respondents whose schools would not or could not provide transcripts, and respondents who were home-schooled and thus did not have transcripts."

allows researchers to construct an individual-level panel that spans from 1994 to 2013. The NLSY 97 data includes high school transcript information that was collected in two separate waves, the first in 2000, and the second in 2004.

Around 70 percent of the individuals in the NLSY 97 sample have high school transcript information.¹³ One key element of the transcript data are the Carnegie units in mathematics and science. The National Center for Education Statistics (NCES) defines a Carnegie unit as the number of credits a student receives for a course taken every day, one period per day, for a full school year.¹⁴

Treatment, Outcome and Controls

Recently, researchers and policy commentators have been interested in the role of advanced mathematics on college access and success and career readiness. In particular, they agree that Algebra 2 is important not only because it is a pre-requisite for college preparation courses such as Pre-Calculus, Calculus, AP calculus and AP Statistics, but also because it benefits students' general development by improving logical thinking, cognitive capacity, and complex problem solving.

In this study, the treatment is the total number of advanced mathematics credits earned during high school. I define **advanced math** as the sum of Carnegie units earned in Algebra 2 through Pre-Calculus, Calculus, AP/IB and Advanced Mathematics-Other. The main outcome is total labor income¹⁵ at age 28. The rationale behind this age is that all members of the sample turn 28 within the observation period. In order to boost the sample size, instead of using measures of income at exactly age 28, I use a weighted measure around this age. The numerator of the weights is the inverse of the distance between each year and 28; for example, the weights for 27 and 29 years are 1, whereas the weights for 26 and 30 years are 1/2

¹³<http://www.bls.gov/nls/y97hstran.htm>

¹⁴<https://nces.ed.gov/nationsreportcard/glossary.aspx?nav=y>

¹⁵The verbatim question is "During last year, how much income did you receive from wages, salary, commissions, or tips from all jobs, before deductions for taxes or anything else?"

and the weights for 25 and 31 years are $1/3$, and so on and so forth. I chose 2 for the weight at exactly 28. The denominator is the sum of all these quantities.

In addition to labor income, in this paper, I also examine the impact of math credits on college outcomes such as **Ever attended any college**, **Ever attended a four-year college**, and **Ever Received a Bachelors Degree**. To control for variation in total labor income due to variation in demographic characteristics, in all the models, I included: a dummy for female, a dummy for white, age in years as of 12-31-1996 (age at the beginning of the study), the average of non-missing values of household gross income between 1996 and 1999 (in 1997 real USD), the average of non-missing values of household income to poverty ratio between 1996 and 1999, household size in 1997, a dummy that indicates whether the household had both biological parents in 1997, state-level number of Carnegie units (years) of math required to obtain a high school diploma in 1997, number of years of exposure to a high school math reform¹⁶. State and cohort¹⁷ fixed effects were also included.

Analysis Sample

The NLSY 97 sample includes 8,984 individuals of which 6,120 have transcript information. After dropping records with missing values of income the resulting sample included 4,841 individuals. By dropping missing observations on the following variables: average household gross income between 1996 and 1999 (545 observations), average household income to poverty ratio between 1996 and 1999 (17 observations), and the number of years of math required to obtain a high school diploma (60 observations), the final sample size for all the analyses is 4,219. None of the remaining

¹⁶Reform indicates whether a state changed the number of years of mathematics required for high school graduation between 1995 and 2005. Exposure to math reforms was based on the first year of high school enrollment using the following rules: if the reform year occurred before first year of enrollment, exposure to math reforms is equal to 4; if the reform year occurred after the last year of high school enrollment, exposure to math reforms is equal to zero; finally, if the reform year occurred in between the first and last years of enrollment, exposure to math reforms equals the difference between the last year of high school enrollment and reform year.

¹⁷Cohort is defined as the year individuals entered high school.

controls have missing values. The weights utilized are the 1997 weights.

Instruments

In this paper I call *instruments* individual-level measures of exposure to STEM programs. When the instruments are *interacted* with state dummies, I call these variables *exclusion restrictions*. The first instrument is the number of years of potential exposure to STEM programs while individuals were enrolled in high school. Throughout this paper this variable is called **expo**. First, I calculated the first and last years of potential enrollment in high school; the first year of enrollment is equal to the birth year plus 17¹⁸ and the last year of high school enrollment is the first year of enrollment plus 3. In this way, the instrument does not depend on *actual* enrollment which is endogenous but only on potential enrollment which depends on the year individuals were born. For each individual in the sample, I created a row vector $enrollment_{is}$ which is a 1X34 vector with zeros in the years of no enrollment and ones in the years of potential enrollment. The 34 columns refer to all the years between 1983 to 2016 utilized in the STEM programs data. Thus, expo was calculated by the following formula:

$$expo = enrollment_{is} * A'_s \quad (1.1)$$

It is important to notice that some STEM programs - e.g, scholarship-loans - will induce individuals to alter their course taking behavior a few years after they are implemented since they serve individuals who are currently pursuing a teaching degree. Only after they graduate from college, they work as math and science teachers in hard-to-staff schools. Other STEM programs (loan forgiveness, salary bonus, and tuition reimbursement) place teachers at schools immediately.

¹⁸Conditional on enrolling in high school, the average age at the year of first enrollment in high school is a little bit above 16.

This is an important distinction that will be included in the process of publication of this paper. For this version, as I will show in the results section, the strength of association between the exclusion restrictions and the treatment is strong, therefore, sufficient to estimate the impact of mathematics credits on education and labor market outcomes.

Recall that A_s is a 1X34 row vector with ones on the years in which state s had STEM programs from 1983 to 2016; 0 otherwise. To assess whether, not only the presence of STEM programs but also the intensity of the programs induced variation on math credits and total labor income, I also calculated instruments based on the number of recipients (R_s) and expenditures (E_s) using the following formulas:

$$expo_recipients = enrollment_{is} * R'_s \quad (1.2)$$

$$expo_expenditures = enrollment_{is} * E'_s \quad (1.3)$$

In fact, I calculated potential exposure with all possible variations of the instruments as described in section 4.4. The formula was similar:

$$expo_z = enrollment_{is} * z'_s \quad (1.4)$$

Here z_s represents any of the following instruments: recipients per 1,000 teachers, recipients per 1,000 secondary school teachers, recipients per 1,000 students, recipients per 1,000 high school students, expenditures per teacher, expenditures per secondary school teacher, expenditures per student and expenditures per high school student. Because of space limitations, I only include five instruments in this paper: $in1 = \mathbf{expo}$, $in2 = expo_recipients$, $in3 = expo_expenditures$, $in4 = expo_recipients/1,000teachers$, and $in5 = expo_expenditures/teachers$. In section 7 (Robustness checks) I only use **expo** ($in1$).

Since the instruments are time-varying measures of exposure to STEM programs (years, recipients, expenditures), and also since some instruments also consider the size of the education system per state-year (number of teachers, number of students, etc), the instruments already consider the impact of STEM programs on the flow of teachers, and how this flow induces variations on math credits and income.

Descriptive Statistics

[Table 1 here]

Table 1 includes summary statistics of the controls utilized in all the models. The analysis sample consists of N=4,219 individuals of whom 50% are women; about 73% are white with an average age at the beginning of the study of about 14.68 years. The average gross income per household between 1996 and 1999 is about \$56,141, and the household income to poverty ratio, also between 1996 and 1999 was about 3.56. The average household size in 1997 was about 4.36 and 55 percent of the households had both biological parents. All the summary statistics are weighted.

[Table 2 here]

Table 2 includes descriptive statistics of math credits, total number of credits¹⁹, total labor income at age 28, state-level high school math graduation requirements in 1997, exposure to math reforms between 1995 and 2005, college attendance and bachelor's degree attainment, as well as the number of potential years of exposure to STEM programs.

On average, the number of Carnegie units of advanced math credits is about 1.03 and the average total number of academic credits is about 16.05. The mean income (total labor income) at age 28 is about \$26,894. On average, the number of years

¹⁹The rationale for including total number of credits is that, the interpretation of the estimates is different depending on whether or not the total number of credits are included.

of math required to obtain a high school diploma in 1997 was 2.4 and individuals were exposed 0.65 years to changes in high school math graduation requirements. Moreover, the average number of years of potential exposure during high school is 2.14. It is noteworthy that the potential exposure to STEM programs varies greatly from 0 to 4 with a standard deviation of 1.82 years. Finally, 77% of the sample attended college; 48% attended a 4-year college, and 32% received a bachelor's degree.

1.4.3 Econometric Model

Exclusion Restrictions

States implemented STEM teacher recruitment programs in different years. Since the data (NLSY 97) includes individuals from different cohorts²⁰, exposure to these programs varies within state and across cohorts. In regard to STEM programs, since there is a huge heterogeneity in benefits (loan forgiveness, financial aid for teachers, etc), individual eligibility requirements, participating schools' eligibility requirements, etc, the most sensible way to predict mathematics credits from exposure to STEM programs, is by interacting the variable **expo** (and the other measures of exposure) with state dummies. In this way, the effects of exposure to STEM programs on math credits are allowed to vary by state. By including state fixed effects in the first stage equation and in the outcome equation, these interactions capture the within-state across-exposure variation; thus, I use the interactions of exposure and state dummies as **exclusion restrictions**.²¹

²⁰Individuals in NLSY 97 were first surveyed when they were 12, 13, 14, 15 and 16 years old as of 12/31/1996.

²¹Exclusion restrictions are the components of the first stage equation that do not "belong" in the outcome equation.

Instrumental Variables (IV/2SLS) Equations

To identify the impact of high school mathematics credits on income, I use variation that is both within-state and across potential exposure to STEM teacher recruitment programs. In order to measure the impact of math credits on income, I estimated the following equations in an IV/2SLS framework:

First Stage

$$Math_i = X_i\alpha + \sum_s expo_z_{is}S_{is}\eta_s + \delta_c + \delta_s + \epsilon_i \quad (1.5)$$

Outcome Equation

$$\ln(Income_i) = X_i\beta + \rho\widehat{Math_i} + \delta_c + \delta_s + \mu_i \quad (1.6)$$

In equations (1.5) and (1.6), X_i is the matrix of controls; δ_c and δ_s are, correspondingly, cohort and state fixed effects. The cohorts are defined based on the year individuals enrolled for the first time in high school.²² In equation (1.5), $expo_z$ represents each of the five measures of exposure previously described (e.g. years of potential exposure, potential exposure to recipients, etc.). The exclusion restriction is the term $\sum_s expo_z_{is}S_{is}$ in equation (1.5). The variable and parameter of interest are, correspondingly, $Math_i$ and ρ ; the outcome is $\ln(Income_i)$. Finally, ϵ_i and μ_i are error terms. By including state fixed effects in both, the first stage and outcome equations identification of ρ results from within-state and across-time variation in exposure to STEM teacher recruitment programs.

²²Cohorts are 1994 or before, 1995, 1996, 1997, 1998 and 1999 or after.

1.5 Results

1.5.1 Impact of potential exposure to STEM programs on Math Credits and Income

To measure the impact of potential exposure to STEM programs (and other variations of the instrument), on math credits (first stage) I estimated the following equation:

First Stage

$$Math_i = \sum_s expo_{is} S_{is} \eta_s + X_i \alpha + \delta_c + \delta_s + \epsilon_i \quad (1.7)$$

[Table 3 here]

Table 3 includes the parameter estimates of equation (1.7). For expositional reasons, I included a trimmed version of table 3; the complete table is located in the Appendix associated with this paper. In all the columns the dependent variable is **advanced math**; the controls are the same as those described in section (1.4). In each column I utilize a different instrument. For instance, in column 1, I use **expo**; in column 2, the instrument is exposure to STEM programs' recipients; in column 3, the instrument is exposure to expenditures; in column 4 the instrument is exposure to recipients per 1,000 teachers, and, in column 5, the instrument is exposure to expenditures per teacher.

As Table 3 indicates, the impact of *expo* and the other instruments, on math credits varies considerably by state. In most cases, the impact is positive, although some states present negative effects. For each column, I tested the null hypothesis that all the coefficients of the interactions of the instrument and state dummies are jointly equal to zero. Because in the 2SLS models I include both math credits and total credits as endogenous variables, I use the multivariate F-statistic described in Angrist and Pischke (2009). This F-statistic allows measuring the strength of

association between each endogenous variable and the exclusion restrictions when there is more than one endogenous variables. For the five columns, the multivariate F-statistics were at least 11. These results rule out a weak instruments problem. A potential concern when clustering the standard errors at the state level is to find large effects in each state. These effects might be mechanically large due in part to clustering. Nevertheless, this study is not concerned to provide state-by-state results. Instead, the important results rely on the average (national) measure of the impact math on total labor income. For the first stage regression, the most important part is the AP F-Statistic, instead of individual coefficients by state. In addition to assessing the impact of the exclusion restrictions on math credits, I also tested whether the instruments influence total academic credits. Table 4 includes the parameter estimates of the impact of the interactions of the instruments and state dummies on total academic credits.

[Table 4 here]

Table 4 presented here is also a trimmed version of the complete table which can be found in the Appendix. Overall, Table 4 indicates that there is a strong and positive effect of exposure to STEM programs on total academic credits. In summary, the evidence presented indicates that exposure to STEM programs during high school is a strong predictor of both math and total credit accumulation.

To measure the impact of exposure to STEM programs on total labor income, I estimated the following equation:

Reduced Form

$$\ln(Income_i) = \sum_s expo_z_{is} S_{is} \eta_s + X_i \alpha + \delta_c + \delta_s + \epsilon_i \quad (1.8)$$

In equation (1.8), $\ln(Income_i)$ is the natural logarithm of total labor income at age 28.

[Table 5 here]

Table 5 includes the parameter estimates of equation (1.8). For expositional purposes, I included a trimmed version of table 5; the complete table is located in the Appendix. The impact of exposure to STEM programs varies greatly by state and is positive and statistically significant in most cases.

1.5.2 Impact of Math Credits on Income, College Attendance and Bachelor's Degree Attainment

Impact of Math Credits on Income

Table 6 includes the main results of the paper. To model the impact of math credits on total labor income, I proceeded as follows. First, I assume (momentarily) that math credits and total credits are exogenous and estimated equation (1.6) using Ordinary Least Squares (OLS). In column 1, the treatment is math, and, in column 2, the treatment variables are math and total credits. In column 3, I assume that math credits are endogenous and estimate equations (1.5) and (1.6) using the 2SLS estimator. In column 4, building upon column 3, I assume that total credits are exogenous and include it as control. Finally, in column 5, I assume that both, math and total credits are endogenous.

[Table 6 here]

The estimates of the impact of math credits on income at age 28 are included in Table 6. Each coefficient of math (and its corresponding standard error), or combination of coefficients of math and total academic credits, represents a separate regression. Table 6 includes 25 separate regressions.

Column (1) indicates that each additional Carnegie unit of math is associated with an increase in income of about 13%. When the total number of credits is included, the coefficient of math decreases to 10.2%. Since the estimates in columns (1) and

(2) are obtained via Ordinary Least Squares (OLS), the results are likely to be biased due to the endogeneity of math credits and total credits.

In column (3), each panel represents a different 2SLS estimate that depends on the instrument utilized. For instance, in panel A, the instrument is number of potential years of exposure to STEM programs. The parameter estimate of math is about 0.0520 (0.258). In panel B, the instrument is the potential exposure to recipients, and the coefficient of math is about 0.0805 (0.0653). When the instrument is potential exposure to expenditures, as in panel C, the coefficient of math is 0.0809 (0.0638). In panel D, the instrument is potential exposure to recipients per 1,000 teachers and the parameter estimate of math is 0.00154 (0.0613), and, finally, in panel E, the instrument is potential exposure to expenditures per teacher, and the coefficient of math is 0.0103 (0.0671). The intention to present coefficients and standard errors in parentheses is due to the fact that, for panels B and C, the standard errors are borderline to statistical significance whereas in panels A, D and E, the coefficients are statistically insignificant.

Column (3) of Table 6 includes parameter estimates of the impact of math credits on income when the total number of credits is excluded. The decision of including the total number of credits is relevant for the following reasons. First, the interpretation of the coefficient of math credits changes depending on whether we control for other subjects' credits. Second, whereas some studies in the literature control for credits earned in other subjects (e.g., Altonji, 1995; Levine and Zimmerman, 1995; Rose and Betts, 2004), other studies do not control for such credits (Joensen and Nielsen, 2009; Goodman, 2012). Specifically, the estimates in Table 6 Column (3) compare to the coefficients in Joensen and Nielsen (2009) and Goodman (2012). The coefficients of table 6, columns 4 and 5, compare to the parameter estimates from Altonji (1995), Levine and Zimmerman (1995) and Rose and Betts (2004).

In Table 6 column (3), panels B and C, I found that for each additional Carnegie

unit (year) of math during high school, there is an increase in total labor income of about 8 percent. These results are consistent with Goodman's (2012) estimates which are between 5-9% for males but not with the estimates for females. Also, the estimates in Joensen and Nielsen (2009) are much larger than those in this study (about 20%); this could be due to differences in education contexts between the US and Denmark.

Including credits earned in other subjects changes the interpretation of the coefficient of math. In columns (4) and (5), I control for the total number of credits. Altonji (1995) included courses taken in math, science, foreign language, commercial courses, industrial arts, social studies, and fine arts. Levine and Zimmerman (1995) included math and science courses, and, Rose and Betts (2004) included credits earned in math subjects such as vocational, pre-algebra, algebra/geometry, intermediate algebra, advanced algebra, and calculus. Even when none of the previous studies included total number of credits, I will compare the estimates of columns (4) and (5) to those in Altonji (1995), Levine and Zimmerman (1995) and, Rose and Betts (2004).

In Table 6, column (4), I control for total credits but view them as exogenous. When using the instruments: expo years, expo recipients, expo expenditures, expo recipients/1,000 teachers, and, expo expenditures/teacher, the corresponding parameter estimates are 0.051 (0.276), 0.0253 (0.0730), 0.0312 (0.0698), -0.0137 (0.0826) and -0.00231 (0.0886). Again, the first instrument provides large estimates and standard errors. For the other four instruments, the increase in income that follows from an additional year of math varies between -1.3% and 5% although imprecisely measured.

The estimates that are comparable to those in Altonji (1995), Levine and Zimmerman (1995) and Rose and Betts (2004) are those in table 6, column 5 since both math credits and total credits are considered endogenous and, therefore,

instrumented for using the interactions of the instruments and state dummies. For the instruments, expo, expo recipients, expo expenditures, expo recipients/1,000 teachers, and, expo expenditures/teacher the corresponding coefficients of math credits are 0.0831 (0.302), 0.0322 (0.0876), 0.0386 (0.0858), 0.0144 (0.0955) and 0.0281 (0.102). The estimates are smaller than the coefficients of algebra/geometry in Rose and Betts (2004), but larger to those in Altonji (1995) and Levine and Zimmerman (1995). My preferred set of specifications is column 5 because, in all cases, math credits and total credits are considered endogenous.

The main results of the paper are: for each additional Carnegie unit of advanced math earned during high school, *holding the total number of credits constant*, there is an increase in total labor income between 1.4% to 8%. More specifically, three out of five instruments provide a consistent return of about 3%.

Impact of Math Credits on College Attendance and Bachelor's Degree Attainment

A potential mechanism that explains the strong positive relationship between math credits during high school and total labor income at age 28 is college attendance and degree attainment. Individuals who are induced to obtain additional math credits may be more likely to attend, and subsequently, to graduate from college. Given that the instruments produce variation in math credits, in other words, the first stage relationship is not zero, in this section, I examine the impact of advanced mathematics credits on college attendance and bachelor's degree attainment.

I estimate the impact of math credits on three (binary) college outcomes: ever attended college, ever attended a 4-year college and, bachelor's degree attainment. First, I estimated a probit model with endogenous variables via the Stata command `ivprobit`. With the estimated coefficients, I calculated the probability of a positive outcome for all members of the sample $\hat{P}_i[Y = 1|math, X, Z]$. Next, I calculated a new variable which adds 1 unit to the actual number of math credits ($math + 1$),

and calculated the probability of a positive outcome for each person in the sample $\hat{P}_i[Y = 1|math + 1, X, Z]$ conditional on $math + 1$ and, holding all other variables constant. Finally, calculated the difference between these two probabilities $\hat{P}_i[Y = 1|math + 1, X, Z] - \hat{P}_i[Y = 1|math, X, Z]$ for all i . The average of $\hat{P}_i[Y = 1|math + 1, X, Z] - \hat{P}_i[Y = 1|math, X, Z]$ across all i represents the average marginal derivative of the impact of $math$ on Y . The standard errors were obtained by bootstrapping 50 repetitions²³. Let Y be any of the three college outcomes:

$$\frac{dY}{dmath} = \frac{1}{N} \sum_{i=1}^N [\hat{P}_i[Y = 1|math + 1, X, Z] - \hat{P}_i[Y = 1|math, X, Z]] \quad (1.9)$$

Table 7 includes the average marginal derivatives for the three college outcomes.

[Table 7 here]

The results indicate that for each additional Carnegie unit of math, the probability to attend college increases by 0.0792 (0.0389), the probability to attend a 4-year college raises by 0.1127 (0.0472) and, the probability to obtain a bachelor's degree increases by 0.0882 (0.0488). Even though these results might seem large, they are consistent with the current, although scarce literature that examines the impact of high school math credits on college outcomes. For instance, Aughinbaugh (2012), by implementing a household fixed-effects identification strategy in NLSY97 data, examined the impact of advanced high school math credits (algebra 2 and up) on college attendance. She found that students who take advanced math during high school are 17 percent points more likely to attend college and 20 percentage points more likely to start college at a 4-year institution.

In addition, Long, Conger and Iatarola (2012), by using propensity score matching techniques in data from the state of Florida, examined the impact of high school curriculum on a number of education outcomes. They found that students who take

²³Due to time limitations, this process was only performed with the first instrument: expo years.

level 3 math (mix of honors, upper-level, AP, International Baccalaureate (IB)) are 10 to 15 percentage points more likely to attend a 4-year college. Finally, Levine and Zimmerman (1995) found smaller results. They examined the impact of the number of math and science courses on the probability to attend and to graduate from college. They found that, for each additional math course taken during high school the probability to attend college increases by 0.02 for males and 0.027 for females. Also, the probability to graduate from college increases by 0.027 for men and 0.046 for women.

Sensitivity Analysis

In this section, I examine how the parameter estimates of the impact of advanced math on income change when using different model specifications. The preferred specification was presented in Table 6. In Table 9 the model specification does not include the two household income measures, household gross income between 1996-1999 and household income to poverty ratio between 1996-1999.

[Table 9 here]

As Table 9 indicates, the 2SLS estimates in panel A are much bigger than the corresponding OLS estimates. In panels B and C the estimates are slightly smaller than their OLS counterparts. Panels D and E present more credible results in which the parameter estimates vary between 0.5% and 5%. The rationale of this specification is to gauge the sensitivity of the estimates and standard errors when the variables that measure household income when individuals were enrolled in high school were not included.

Compared to the preferred specification (table 6), by not controlling for household income variables we incur in an omitted variable bias problem which it is not solved by the instruments at hand. If the error term in equation (6) has the form

$\mu_i = f_{amincome_i} + \nu_i$, where $f_{amincome_i}$ and ν_i are orthogonal, by not including

$famincome_i$, we could still solve the problem of the correlation between ν_i and $math_i$ but the correlation between $famincome_i$ and $math_i$ will induce bias in the parameter estimates. By including family income variables, I net out the impact of math on individual income from the impact of the family income on individual income.

[Table 10 here]

Since the variables household gross income 1996-1999 and income to poverty ratio 1996-1999 by construction depend on whether the household includes two parents or only one parent (widow, separated or divorced), these variables might have measurement error. In the third model specification, I interacted the variable **both biological parents** with both variables, household gross income 1996-1999 and income to poverty ratio 1996-1999. The results are included in Table 10. In this specification, the sample size is the same as in the preferred specification (4,219). When using expo as the instrument, panel A shows that for each additional year of advanced math there is an increase of total labor income between 1% and 3%. The standard errors are quite large. When the instruments are exposure to STEM program recipients and exposure to expenditures, as shown in panels B and C, the 2SLS estimates vary between 3% to 8%. Unlike, panel A, the standard errors are much smaller. Interestingly, panels D and E include negative 2SLS estimates. This specification provides very similar results to those from the first (preferred) specification.

[Table 11 here]

Finally, in table 11 instead of discarding the observations for which the variables household gross income 1996-1999 and household income to poverty ratio 1996-1999 are missing, I replaced the missing values by zeros. The sample increased to 4,771

and the results indicate that, for panel A, the increase of income for each additional year of advanced math is about 30%. Similar to table 9, table 11-Panel B indicates that the 2SLS estimates are slightly below their corresponding OLS estimates. In addition, panels D and E present 2SLS estimates that vary between 2% and 5%. Even when replacing missing by zeros might increase efficiency by increasing the sample size, by including these observations the parameter estimates vary greatly across the different instruments. Assuming that these missing values are randomly distributed the parameter estimates in table 6 are the most credible.

Impact of math credits on total labor income via college attendance and degree attainment

In this section, I attempt to measure the proportion of the effect of math on income that is due to the effect of math on bachelor's degree attainment. The idea is to combine information on returns to obtaining a bachelor's degree with the impact of math credits on the probability of bachelor's degree attainment presented in this paper.

Define $E_i[\widehat{math}]$ to be the earnings of individual i who obtains the average amount of advanced math credits in high school. For simplicity, assume²⁴ that there are only two groups in the population: high school graduates and bachelor's degree recipients. Then, earnings take the form:

$$E[Y|\widehat{math}]_i = E[Y|\widehat{math}]_{BA}Pr[BA|\widehat{math}] + E[Y|\widehat{math}]_{HS}Pr[HS|\widehat{math}]$$

For simplicity define $E[Y|\widehat{math}]_{BA} = E_{BA}$, $E[Y|\widehat{math}]_{HS} = E_{HS}$ as the average earnings of four-year college graduates and high school graduates respectively. Also, $Pr[BA]$ and $Pr[HS]$ are the probabilities of being in each group. The previous equation can be transformed to:

²⁴This assumption is reasonable because it is unlikely that the instrument will induce high school dropouts to change their course-taking behavior.

$$\frac{E_i[\widehat{math}]}{E_{HS}} = \frac{E_{BA}}{E_{HS}} Pr[BA] + Pr[HS]$$

Since the returns to education literature estimates that each year toward a bachelor's degree raises earnings by about 10% (Card, 1999; Carneiro, Heckman and Vytlačil, 2011), then $E_{BA}/E_{HS} = 1.4$ assuming 4 years of college. We want to calculate the increase in earnings that is due to increases in the probability of receiving a bachelor's degree. Adding one year of advanced math credits we have:

$$\frac{E_i[\widehat{math} + 1]}{E_{HS}} = 1.4Pr'[BA] + Pr'[HS]$$

$$\frac{E_i[\widehat{math} + 1]}{E_{HS}} - \frac{E_i[\widehat{math}]}{E_{HS}} = 1.4(Pr'[BA] - Pr[BA]) + (Pr'[HS] - Pr[HS])$$

By assumption: $Pr'[BA] - Pr[BA] = -(Pr'[HS] - Pr[HS])$ because there are only two groups in the population.

$$\frac{E_i[\widehat{math} + 1]}{E_{HS}} - \frac{E_i[\widehat{math}]}{E_{HS}} = 0.4(Pr'[BA] - Pr[BA])$$

Considering column (3) in table 7 we have:

$$\frac{E_i[\widehat{math} + 1]}{E_{HS}} - \frac{E_i[\widehat{math}]}{E_{HS}} = 0.4(0.0882) = 0.035$$

The increase in earnings that is channeled via the increases in the probability of attaining a bachelors degree would be about 3.5%. In table 6, the preferred estimates are about 3%. Thus, the effect of math on income can be explained through the effect of math on bachelor's degree attainment.

1.5.3 Compliant sub-population

In this paper I estimate weighted Local Average Treatment Effects (LATEs); in other words, the average effect of an additional credit of math courses only for the population of **compliers**. Compliers are defined as individuals who are induced by the instrument to change their course-taking behavior. Unlike compliers, another set of people are called *always-takers* since no matter if states implement STEM teacher recruitment programs or the number of years they have been exposed to such programs; they always will choose a certain level - presumably high - of math courses. The last group, called *never-takers* will not change the number of math courses - probably low - regardless of whether or not States implemented teacher recruitment programs or the number of years they have been exposed to such programs. In general, it is not possible to identify individual compliers. However, it is plausible to characterize the complier sub-population.

Given that, the estimates in this study are weighted Local Average Treatment Effects (LATE), in order to understand what are the characteristics of the individuals for which the results in this paper are relevant, I characterize the sub-population of compliers.

Complier Characteristics; Abadie's (2003) Kappa Weighting Method

I use Abadie's (2003) Kappa weighting method to determine the average characteristics of the complier sub-population. Abadie demonstrated that, under certain conditions:

$$E[x_i|complier] = \frac{E[\kappa_i X_i]}{E[\kappa_i]} \quad (1.10)$$

In equation (1.10), $E[x_i|complier]$ represents the average of x_i only for compliers; κ_i are the weights that allow the characterization of the complier sub-population.

Since neither the instrument nor the treatment are binary, in order to estimate the

kappa weights I recoded both as binary. The kappa weights are obtained as follows:

$$\kappa_i = 1 - \frac{D_i(1 - z_i)}{1 - Pr(z_i = 1|x_i)} - \frac{(1 - D_i)z_i}{Pr(z_i = 1|x_i)} \quad (1.11)$$

In (1.11) z_i is a dummy variable equal to 1 if the individual has ever been exposed to STEM teacher recruitment programs and 0 otherwise; D_i is a dummy variable equal to 1 if the number of math credits is larger than or equal to its mean; 0 otherwise. By using Abadie's kappas, I estimated the mean of several variables and compared them with the mean of the same variables for the entire population.

[Table 8 here]

Table 8 includes the means of some variables for the entire sample and for the sub-population of compliers. The third column is the ratio of the two. These results should be interpreted as follows: for example, the probability of randomly choosing a women from the group of compliers is 53% whereas the probability of randomly choosing a women from the general population is 50%; thus, it is slightly more likely to find a women in the population of compliers than in the entire sample. The proportion of white individuals in the sub-population of compliers is 60% whereas the same proportion for the entire sample is about 73%. Compliers are slightly younger than the entire sample; their families have more family members; the proportion of families with both biological parents is almost the same as in the entire population; they enjoy less household income between 1996 and 1999; and the income to poverty ratio between 1996 and 1999 is slightly smaller for compliers. Roughly speaking, the compliers are more likely to be non-white and to come from more disadvantaged backgrounds. These preliminary results are consistent with the underlying mechanism through which the instruments induce students to earn additional math credits. Some STEM programs are intended to increase the supply of teachers in low-performing and hard-to-staff schools. These schools serve a

disproportionately non-white and poorer population when compared to schools that don't receive STEM programs participants.

1.6 Robustness checks

In this section I address some threats to the validity and generalizability of the results. First, is the effect of STEM programs on total labor income also present outside NLSY 97? The STEM programs should impact earnings when examined in alternative data sets.

Second, the hypothesized mechanism through which STEM programs impact high school math credits is via the increase in the number of teachers available. A natural question is, what is the impact of STEM programs on the probability of teaching of a random individual? In other words, do STEM programs increase the supply of teachers? If STEM programs do not increase the number²⁵ of available teachers, it would be hard to argue that students will increase their math course-load, and consequently, there would be a violation of the exclusion restriction since there exists another mechanism - different from increasing math credits - through which STEM programs influence earnings.

Third, another potential violation of the exclusion restriction is the possibility that states implemented STEM teacher programs **because** economic conditions such as poverty or unemployment were good or bad. A similar argument is that states implemented STEM teacher programs because the wages in (STEM) occupations were good. In this case, the implementation or elimination of STEM programs would not be exogenous, and therefore, these decisions would be correlated to economic conditions, and consequently, there would exist an alternative mechanism that explains the impact of STEM programs on income, different from the increase

²⁵STEM programs might also change the distribution of teacher quality of the teaching body; although it would be hard to think that quality can go up via STEM programs.

in math credits.

Fourth, a final mechanism that is not addressed in this paper is that STEM programs not only increase the number of teachers but also the composition of teacher characteristics. For instance, if STEM programs place not only more teachers but also better teachers and the new teachers provide students with additional motivation and information regarding their college application processes, this would violate the exclusion restriction. If this is the case, the increase in income would not be driven exclusively by increases in math teachers and the corresponding increases in math credits, but also by other factors such as motivation and information, which excellent teachers tend to provide to their students. Unfortunately, the available data does not allow me to address this potential violation of the exclusion restriction.

1.6.1 Is the effect of STEM programs on income idiosyncratic to NLSY?

How can we be sure that the impact of STEM teacher recruitment programs on income prevails when using larger data sets? We could think that the sample from which the estimates of this paper are drawn includes some idiosyncratic features that prevent the generalization of the results. The NLSY 97 sample includes less than 9,000 individuals out of which the estimation sample utilizes 4,219. In this section, I address this concern by measuring the impact of STEM teacher programs on different measures of earnings at different stages in the life cycle, by estimating the *reduced form* equations on larger data sets. To measure the impact of STEM programs on earnings, I use two sources of data: Survey of Income and Program Participation (SIPP) and the American Community Survey (ACS).

SIPP 2008

The SIPP is a study administered by the Census Bureau which surveys households to form a continuous series of national panels. Each panel includes a nationally

representative sample interviewed over a period of about four years. The main goal of the SIPP is the examination of the distribution of income and participation in government programs. Even though the SIPP permits longitudinal analyses, I use a cross-section of the base year 2008. As measure of earnings, I use total household monthly income. The data also includes controls such as year of birth, gender, race, state and poverty level.

ACS 2009

The American Community Survey (ACS) also provides information on education and income. In this study, I use data from 2009. The output is personal yearly wages, and the controls are educational attainment, year of birth, gender, race, state and poverty level.

Impact of exposure to STEM programs during high school on earnings

For the two samples, SIPP 2008 and ACS 2009, I estimated the following equation²⁶:

$$\ln(Earnings_i) = expo_{is}\eta + X_i\alpha + \gamma_s + \gamma_c + \epsilon_i \quad (1.12)$$

In equation (1.12) the treatment is *expo* which is defined as the number of years of potential exposure to STEM teacher recruitment program for individual *i*. To calculate *expo*, I followed the same method as in section (1.4).²⁷ In ACS 09, the measure of earnings is personal yearly wages and the matrix X_i includes female, white, black, Asian (the reference category is other), birth year and poverty; the number of years required for graduation in 1997 and the number of years exposed to changes to these requirements. For SIPP 08, the measure of earnings is household

²⁶The equation with interactions of *expo* and state dummies was also estimated; the estimates are located in the Appendix.

²⁷The vector of potential high school enrollment is based on the birth year; the result is a 1X34 row vector with ones on the years of potential enrollment and 0 otherwise. The 34 columns refer to the 34 years from 1983 to 2016 for which I have information of STEM programs.

monthly income; the matrix X_i includes female, white, birth year, poverty, high school graduation requirements in 1997 and number of years exposed to changes to high school graduation requirements.

To measure the impact of potential exposure to STEM teacher recruitment programs on earnings at different stages in the life cycle, I estimated equation (1.12) for the following samples: between 28 and 29 years old (to be consistent with the NLSY97 results), between 30 and 35 years old, and between 36 and 40 years old.

[Tables 4-5 Appendix here]

Table 4 in the Appendix includes the parameter estimates of equation (1.12) for ACS. For the cohort 28-29, each additional year of potential exposure during high school to STEM programs is associated with an increase in personal yearly wages of about 0.021%. For the cohorts 30-35 and 36-40, the corresponding estimates are 0.0083% and 0.00016%. When using SIPP 08, as table 5 in the Appendix indicates, the increases in monthly household income for the cohorts 28-29, 30-35 and 36-40, associated with a unit increase in *expo* are, respectively, 0.15%, 0.046%, and 0.13%. The estimates vary widely by state as indicated in tables 6 and 7 in the Appendix.

The main conclusion of this section is that potential exposure to STEM teacher programs **does** influence earnings in bigger and more robust data sets. For ACS 09, the estimate for the cohort of 28-29 years old is about 0.02% and for SIPP 08, the corresponding result is about 0.16%. These estimates rule out the possibility that the impact of exposure to STEM programs on earnings, is **specific** to NLSY 97.

1.6.2 Do STEM programs increase teachers' labor supply?

The working hypothesis of this paper is that the mechanism through which STEM programs influence mathematics credits is via increasing the number of teachers available. Assuming all else remains the same, (e.g., current math teachers are not assigned

to teach other subjects or current teachers do not decrease the number of hours they teach), the influx of new teachers should increase course offerings within receiving schools. This might increase students' math course taking. Some students (compliers) will be induced to take math courses that they otherwise would not take. If STEM programs do not increase teachers' supply, then, there is a violation of the exclusion restriction since the impact of STEM programs on earnings cannot be channeled via increases in math credits.

To measure the impact of STEM programs on the probability of teaching I estimate the following equation:

$$Teach_i = expo_college_{is}\eta + X_i\alpha + \gamma_s + \gamma_c + \epsilon_i \quad (1.13)$$

I estimate equation (1.13) using NLSY 97, ACS 09, and SIPP 08. For NLSY 97, there are two outcomes - represented by $Teach_i$. First, $Teach_i$ is a dummy variable that indicates whether or not individual i has ever taught. Second, $Teach_i$ indicates whether or not individual i taught during 2013 (last observation period). For ACS 09, $Teach_i$ equals 1 if individual i , during 2009, has one of the following occupations: Elementary and Middle School Teacher, Secondary School Teacher, Special Education Teacher, Other Teachers and Instructors and Teacher Assistants; 0 otherwise. For SIPP 08, $Teach_i$ indicates whether the occupation of individual i is one of the following: Preschool and kindergarten, Elementary and middle school, Secondary school teachers, Special education teachers, Other teachers and instructors, Teacher assistants, Other education occupations.

The treatment is *expo_college* which measures the potential years of exposure to STEM programs *during* college and is defined similar to *expo* with the only difference that the first year of college enrollment is calculated by adding 19 to the birth year. The matrix X_i includes the same controls as in the previous section. State fixed effects were included.

[Tables 8-10 Appendix here]

As table 8 in the Appendix indicates, for NLSY 97, the impact of *expo_college* on *ever teaching* is positive, although statistically insignificant of about 1 percentage point; whereas when the outcome is *teacher in 2013* the results are much smaller, about 0.18 percentage points. For ACS 09, as Table 9 in the Appendix indicates, the impacts of exposure during college on teaching for the three groups [28-29], [30-35] and [36-40] are positive, statistically significant of about 0.6, 0.5 and 0.26 percentage points respectively. Table 10 in the Appendix includes the parameter estimates of the impact of exposure during college on teaching using the SIPP 08 sample; the coefficients for the [28-29] and [30-35] groups are positive and statistically significant of about 1.1 and 1 percentage points respectively. For the group of 36-40, the impact is still positive but insignificant of about 0.4 percentage points.

In the Appendix, in tables 13-15, the effects are allowed to vary by state; in most cases the impacts are positive, and, in some states, statistically significant. In summary, potential exposure *during college* to STEM programs is associated with a higher probability of teaching. The impacts vary across samples, but consistently they lie between 0.1 and 1.1 percentage points. In fact, when the effects vary by state, the estimates are much bigger. Since some STEM programs intend to recruit new teachers, while individuals are enrolled in college via incentives such as loan forgiveness, signing bonuses, etc. the impacts of exposure to these programs *while in college* are observed in the data, not only in NLSY but also in bigger and more robust data sets.

Impact of STEM programs on the number of teachers

To examine the impact of STEM programs on the per-state number of teachers, I built a panel dataset from 1983 to 2016 with the following variables: state, year, $STEM_{st}$ (1 indicates whether state s has at least one active STEM program in year

t ; 0 otherwise), number of teachers in K-12, number of secondary teachers, number of students in K-12 and number of high school students. The data was obtained from the public files of the National Center of Education Statistics (NCES), Common Core of Data (CCD). By exploiting the panel nature of these variables, I estimated the following equation using the state fixed-effects estimator:

$$y_{st} = \beta_0 + \beta_1 STEM_{st} + \beta_2 X_{st} + \gamma_s + \gamma_t + \nu_{st} \quad (1.14)$$

In equation (1.14), y_{st} represents one of the following three outcomes: total number of teachers, total number of secondary teachers and total number of elementary teachers; X_{st} includes two controls: the total number of students and the total number of high school students. State and year fixed effects were included.

[Table 11 Appendix here]

Table 11 in the Appendix includes the parameter estimates of equation (1.14). The impact of STEM programs on the number of elementary teachers is negative of about -25 (-0.09%); the impact on the number of secondary teachers is positive although insignificant of about 1,443 (6.9%²⁸) and the impact on the total number of teachers is also positive and insignificant of about 1,357 (2.6%). Table 11 in the Appendix includes the estimates of equation (1.14) when the outcome is the natural logarithm of the three different measures of the number of teachers. The results indicate that only for secondary teachers the estimate is positive although statistically insignificant.

In summary, STEM programs increase the number of teachers available, especially secondary school teachers. These results will contribute to the literature on the impact of financial incentives aimed to recruit and retain teachers. Even though this study does not address any specific financial aid program, it provides an aggregate measure of the impact of financial incentives on both the probability of teaching and on the

²⁸When compared to the mean.

number of teachers.

1.6.3 Did states implement STEM teacher programs because economic conditions were good or bad?

Were states' decisions to implement STEM teacher programs influenced by the wage rates of occupations that inherently use high levels of mathematics? For example, if some occupations were booming, and thus, the wage rates were high, some state officials might be induced to implement STEM teacher recruitment programs to increase the number of teachers in the market and, consequently, to better prepare their student population. Similarly, were states' decisions to implement or to eliminate STEM programs influenced by the state's economic conditions such as the unemployment rate and percent in poverty? For instance, if the economy is healthy and states have more money to spend on education, do they do it by increasing the supply of teachers?

In any event, if states' decisions to implement STEM programs were based either on the wage rates of math-oriented occupations or state-level economic conditions, the instrument would not be valid. The impact of STEM programs on earnings would be driven by the correlation of STEM programs with other factors rather than by increases in math credits, thus violating the exclusion restriction.

To empirically address these two questions, I built a panel of states between 1983 to 2016 with the following variables: mean hourly wage rates, median hourly wage rates, mean annual wages and median annual wages for all occupations. Also, I calculated the same variables for the following occupations: Engineering, Mathematics, Business, Health, Education and Law. All the wages data were obtained from the BLS and adjusted for inflation (\$2011 USD). In addition to the wage data, I also included the percent in poverty from the Census Bureau and the unemployment rate from the BLS. Finally, I merged the variable $STEM_{st}$ which as previously mentioned, is equal to 1 in the years that states had at least one STEM program active and 0 otherwise.

In this analysis I model states' choices to implement STEM programs as a function of states' economic measures. To do so, I estimate the following equation by using the within fixed effects estimator:

$$STEM_{st} = \delta_0 + \delta_1 Econ_{st} + \gamma_s + \gamma_t + \mu_{st} \quad (1.15)$$

In equation (1.15), $Econ_{st}$ represents each one of the economic variables detailed above. In addition to $STEM_{st}$, I also led this variable one and two years into the future; in this way, I would capture any ramp-up effect. As table 16 Appendix indicates, there is no statistically significant effect of any of the economic indicators included on states' decisions to implement or eliminate STEM programs. The only statistically significant parameter is the contemporaneous effect of percent on poverty on STEM. Again, since the two variables are measured in the same year, it is unlikely that an increase in the proportion of poor people in the state, would lead to state governments to implement STEM programs.

[Tables 16-17 Appendix here]

When $Econ_{st}$ represents field-specific wages - as shown in table 17 Appendix - the results are mixed. There is a positive effect of wages in Business occupations on the probability to implement STEM programs. For Law the effect is negative. No statistically significant effect was found for Engineering, Math, Health and Education. In conclusion, there is no discernible effect of economic conditions on states' decisions to implement STEM programs.

1.6.4 Do STEM programs induce more teachers but not different teachers?

If a STEM program besides increasing the *quantity* of math teachers, also raises the *quality* of teachers, this could be problematic. In an extreme case, assume that

a state implements a STEM program which for various reasons did not increase the number of teachers. Instead, it changed the composition of teachers such that, the new group of teachers is highly motivated, has access to more resources, and, has up-to-date knowledge. Also, the new set of teachers also has more updated information about college application procedures and financial aid options than the pre-STEM teachers. Most likely, individuals exposed to the post-STEM teachers would change their behaviors in ways that will lead them to higher earnings later in life, *without* increasing their math credits. In this case, the STEM program influenced earnings via another mechanism other than boosting the number of math credits; this is a direct violation of the exclusion restriction.

If STEM programs induce variations in both, the quantity and quality of teachers, then an alternative interpretation of the results is pertinent. As long as the impact of STEM programs on earnings is channeled via the gain in math credits, either because there are more teachers, or because there are better teachers who induce individuals to earn more math credits, the results are valid. Unfortunately, with the data at hand, it is not possible to test whether STEM programs have changed the distribution of teacher characteristics.

1.7 Conclusions

By exploiting variations in the supply side of the education system as determinants of education choices (Card, 2001), this study contributes to the literature that examines the impact of high school math credits on education and labor market outcomes in a number of ways. First, this study shows that programs that aimed to recruit teachers to work in shortage (geographic and subject) areas do impact high school math choices. Second, this study presents evidence of the private economic returns of high school math credits (3%). Third, this paper also provides evidence of the causal impact of high school math credits on college attendance (8 pp), 4-year college

attendance (11 pp) and bachelor's degree attainment (9pp). Finally, although it is not the primary purpose of this study, this paper presents estimates of the aggregate impact of STEM teacher recruitment programs on the probability of teaching (0.5%-1%).

The main result of the paper is that each additional Carnegie unit of advanced math increases total labor income at age 28 by about 3%. To get an idea of the relative magnitude of the estimates in this study, I compare them with the conventional wisdom from the returns to education literature. In particular, the returns to college education is a well-studied topic.

By using NLSY79, Carneiro, Heckman and Vytalacil (2011) estimated the returns to college education by comparing earnings of college graduates versus earnings of high school graduates. They utilized various instruments such as the the presence of a college in the county of residence at 14, local earnings and local unemployment in the area of residence at 17, and average tuition in public 4 year colleges in the county of residence at 17 (interacted with AFQT, mothers education and number of siblings). In their preferred specification, the return to one year of college education is 9.51%. Other estimates vary between 5.6% and 17.36%.

Also, Card (2001) reviewed the literature of the returns to college education and included 11 studies that exploited variations on the supply side of the school system as predictors of education choices. The IV estimates of the return to a year of college education vary between 0.06 to 0.245 although the majority of the estimates lie between 0.10 and 0.15. Finally, in a Bookings Institution study, Greenstone and Looney (2011) compared the returns to a college education to other investments such as the stock market, bonds, gold and Treasury bills. They concluded that investment in college provides the best return of all of about 15%. For comparison purposes, I consider that the return to one year of college education varies between 10% and 15%.

Taking the estimates in Table 6 one additional Carnegie unit of math yields a return of 3%. Let's recall that the treatment variable is **advanced** math which includes Algebra 2, trigonometry, pre-calculus, calculus, statistics, AP-calculus and AP-statistics. These courses are college preparatory, and, in many cases college students take them. Thus, since obtaining an extra year of advanced math content, knowledge and skills during high school yields a 3% return, this estimate is consistent with the return to the average return to a year worth of college education: between 10% and 15%. I acknowledge that I am comparing high school math versus college education which by definition reference different populations of individuals. The main goal of this comparison is to place the results obtained in this study within the range of a well identified and consistently estimated parameter: the returns to college education.

The results obtained in this paper are consistent with some studies in the literature in terms of both, magnitude and precision. As previously mentioned, from Table 6, column (3), the returns to math credits when total credits are excluded is about 5%. This estimate is consistent with Goodman's (2012) estimates for males (5%-9%), but smaller than the coefficients in Joensen and Nielsen (2009) of about 20%. When including total credits, as Table 3, column (5) the results indicate that the return to math credits is about 3%. This value is smaller than the returns to algebra/geometry in Rose and Betts (2004) of about 8%, but inconsistent with all other math courses. Finally, the results differ from those in Altonji (1995) and Levine and Zimmerman (1995).

In addition to the estimates of the economic returns to high school math credits, this study also provides evidence of the impact of math credits on college outcomes. The impacts on: college attendance (8 pp), 4-year college attendance (11 pp), and, bachelor's degree attainment (9 pp) are also consistent with the literature. Aughinbaugh (2012) found that students who take advanced math increase their probability

to attend college by 17 percentage points, and the probability to attend a 4-year college by 20 percent points. Long, Conger and Iatarola (2012) also found increases in the probability to attend a 4-year college of about 10 to 15 pp when taking advanced math.

Finally, unlike Goodman (2012) who studies the impact of high school math reforms on mathematics course-taking and earnings - an imposition of a constraint -, this study examines the opposite: the effects of a relaxation of a constraint faced by some students: the availability of teachers, courses, course-sections, etc. Since some states invest heavily on recruiting and retaining teachers in shortage areas, the estimates presented in this study are more similar to the studies presented in Card (2001) than to studies in the current returns to math credits literature.

1.8 References

- [1] Abadie, A. (2003). Semiparametric instrumental variable estimation of treatment response models. *Journal of Econometrics*. 113 (2003) 231-263.
- [2] Adelman, C.(1999). Answers in the Tool Box: Academic Intensity, Attendance Patterns, and Bachelor's Degree Attainment – June 1999. US Department of Education.
- [3] Adelman, C.(2006). The Toolbox Revisited. Paths to Degree Completion from High School Through College – 2006. US Department of Education.
- [4] Alexander, K. & Pallas, A. (1984). Curriculum Reform and School Performance: An Evaluation of the "New Basics". *American Journal of Education*. Vol. 92, No. 4 (Aug., 1984), pp. 391-420
- [5] Alexander, K., Riordan, C., Fennessey, J. & Pallas, A. (1982). Social Background, Academic Resources, and College Graduation: Recent Evidence from the National Longitudinal Survey. *American Journal of Education*, Vol. 90, No. 4 (Aug., 1982), pp. 315-333
- [6] Altonji, J. (1995). The Effects of High School Curriculum on Education and Labour Outcomes. *The Journal of Human Resources*, Vol. 30, No. 3 (Summer, 1995), pp. 409-438
- [7] Angrist, J. & Imbens, G. (1995). Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity, *Journal of the American Statistical Association*, 90:430, 431-442
- [8] Angrist, J. & Krueger, A. (1991). Does compulsory school attendance affect schooling and earnings. *Quarterly Journal of Economics*, Vol. 106, No. 4 (Nov, 1991), pp. 979-1014.
- [9] Angrist, J. & Pischke, J. (2009). Mostly Harmless Econometrics, An Empiricist's Companion. Princeton University Press.
- [10] Attewell, P. & Domina, T. (2008). Raising the Bar: Curricular Intensity and Academic Performance. *Educational Evaluation and Policy Analysis*, March 2008, Vol. 30, No. 1, pp. 51-71
- [11] Aughinbaugh, A. (2012). The effects of high school math curriculum on college attendance: Evidence from the NLSY97. *Economics of Education Review*, 31(2012) 861-870.
- [12] Bozick, R. & Ingels, S. (2008). Mathematics Coursetaking and Achievement at the End of High School: Evidence from the Education Longitudinal Study of

- 2002 (ELS:2002). Statistical Analysis Report, National Center for Education Statistics (NCES), January 2008
- [13] Card, D. (1999). The Causal Effect of Education on Earnings. Handbook of Labor Economics, Volume 3, Edited by O. Ashenfelter and D. Card, 1999 Elsevier Science B.V.
 - [14] Card, D. (2001). Estimating the Returns to Schooling: Progress on Some Persistent Econometric Problems. *Econometrica*, Vol. 69, No. 5 September, 2001, 1127-1160
 - [15] Carneiro, P., Heckman, J., & Vytlacil, E. (2011). Estimating Marginal Returns to Education. *American Economic Review*, 101 (October 2011): 2754-2781
 - [16] Clotfelter, C., Glennie, E., Ladd, H., & Vigdor, J. (2007). Would higher salaries keep teachers in high-poverty schools? Evidence from a policy intervention in North Carolina. *Journal of Public Economics* 92 (2008) 13521370
 - [17] Clotfelter, C., Glennie, E., Ladd, H., & Vigdor, J. (2008). Teacher Bonuses and Teacher Retention in Low-Performing Schools Evidence from the North Carolina \$1,800 Teacher Bonus Program. *Public Finance Review*, Volume 36 Number 1, January 2008 63-87
 - [18] Clotfelter, C., Hemelt, S., & Ladd, H. (2016). Raising the bar for college admission: North Carolinas increase in minimum math course requirements. National Bureau of Economic Research, Working Paper 21926.
 - [19] Cole, S. Paulson, A. & Shastri, G. (2015). High School Curriculum and Financial Outcomes: The Impact of Mandated Personal Finance and Mathematics Courses. *Journal of Human Resources*, Vol. 51, issue 3; August 2016 pp. 656-698
 - [20] Dougherty, C., Mellor, L. & Jian, S. (2006). The Relationship between Advanced Placement and College Graduation. 2005 AP Study Series, Report 1, February 2006. National Center for Educational Accountability.
 - [21] Gamoran, A. (1987). The Stratification of High School Learning Opportunities. *Sociology of Education*. Vol. 60, No. 3 (Jul., 1987), pp. 135-155
 - [22] Gamoran, A. & Hannigan, E. (2000). Algebra for Everyone? Benefits of College-Preparatory Mathematics for Students With Diverse Abilities in Early Secondary School. *Educational Evaluation and Policy Analysis*. Fall 2000, Vol. 22, No. 3, pp. 241-254
 - [23] Goodman, J. (2012). The Labor of Division: Returns to Compulsory Math Coursework. Faculty Research Working Paper Series. Harvard Kennedy School. August 2012.

- [24] Greenstone, M. & Looney, A. (2011). Where is the Best Place to Invest \$102,000 In Stocks, Bonds, or a College Degree?. Brookings Institution. Retrieved <https://www.brookings.edu/research/where-is-the-best-place-to-invest-102000-in-stocks-bonds-or-a-college-degree/>
- [25] Hanushek, E. & Wmann, L. (2010), Education and Economic Growth. In: Penelope Peterson, Eva Baker, Barry McGaw, (Editors), International Encyclopedia of Education. volume 2, pp. 245-252. Oxford: Elsevier.
- [26] Hanushek, E. & Wmann, L. (2012), Do better schools lead to more growth? Cognitive skills, economic outcomes, and causation. *J Econ Growth* (2012) 17:267-321
- [27] Hoffer, T., Rasinski, K. & Moore, W. (1995). Social Background, Differences in High School Mathematics and Science Course-taking and Achievement. Statistics in Brief, National Center for Education Statistics (NCES), August 1995
- [28] Horn, L. & Kojaku, L. (2001). High School Academic Curriculum and the Persistence Path Through College Persistence and Transfer Behavior of Undergraduates 3 Years After Entering 4-Year Institutions. Statistical Analysis Report, National Center for Education Statistics (NCES), August 2001
- [29] Joensen, J. & Nielsen, H. (2009). Is there a Causal Effect of High School Math on Labor Market Outcomes? *The Journal of Human Resources*, 44 (2009).
- [30] Jones, L. (1987). The Influence on Mathematics Test Scores, by Ethnicity and Sex, of Prior Achievement and High School Mathematics Courses. *Journal for Research in Mathematics Education*. (May, 1987) Vol. 18, No. 3. pp. 180-186
- [31] Laing, J., Engen, H. & Maxey, J. (1987). Relationships between ACT test scores and High School Courses. *ACT Research Report Series*, 83-7. January, 1987
- [32] Lee, V., Burkam, D., Chow-Hoy, T., Smerdon, B. & Goverdt, D. (1998). High School Curriculum Structure: Effects on Course-taking and Achievement in Mathematics for High School Graduates. An Examination of Data from the National Education Longitudinal Study of 1988. Working Paper Series. National Center for Education Statistics (NCES), 1998
- [33] Lee, V., Croninger, R. & Smith, J. (1997). Course-Taking, Equity, and Mathematics Learning: Testing the Constrained Curriculum Hypothesis in U.S. Secondary Schools. *Educational Evaluation and Policy Analysis*. Summer 1997, Vol. 19, No. 2, pp. 99-121
- [34] Levine, P. & Zimmerman, D. (1995). The Benefit of Additional High-School Math and Science Classes for Young Men and Women. *Journal of Business & Economic Statistics*. Vol. 13, No. 2, JBES Symposium on Program and Policy Evaluation (Apr., 1995), pp. 137-149.

- [35] Levine, P. & Zimmerman, D. (1995). The Benefit of Additional High-School Math and Science Classes for Young Men and Women. *Journal of Business & Economic Statistics*. Vol. 13, No. 2, JBES Symposium on Program and Policy Evaluation (Apr., 1995), pp. 137-149.
- [36] Long, M., Conger, D., & Iatarola, P. (2009). Explaining Gaps in Readiness for College-level Math: The Role of High School Courses. *Education Finance and Policy*.
- [37] Long, M., Conger, D., & Iatarola, P. (2012). Effects of High School Course-Taking on Secondary and Postsecondary Success. *American Educational Research Journal* April 2012, Vol. 49, No. 2, pp. 285-322
- [38] McCormick, N. & Lucas, M. (2011). Exploring mathematics college readiness in the United States. *Current Issues in Education*, 14(1).
- [39] Madigan, T. (1997). Science Proficiency and Course Taking in High School: The Relationship of Science Course-taking Patterns to Increases in Science Proficiency Between 8th and 12th Grades. Statistical Analysis Report, National Center for Education Statistics (NCES), March 1997
- [40] Mitra, A. (2002). Mathematics skill and malefemale wages. *Journal of Socio-Economics* 31 (2002) 443-456
- [41] Rock, D. & Pollack, J. (1995). Mathematics Course-Taking and Gains in Mathematics Achievement. Statistics in Brief, National Center for Education Statistics (NCES), June 1995
- [42] Rose, H. & Betts, J. (2004). The Effect of High School Courses on Earnings. *The Review of Economics and Statistics*, May 2004, 86(2): 497-513
- [43] Schneider, B., Swanson, C. & Riegle-Crumb, C. (1998). Opportunities For Learning: Course Sequences and Positional Advantages. *Social Psychology of Education* 2: 25-53, 1998
- [44] Sebring, P. (1987). Consequences of Differential Amounts of High School Course work: Will the New Graduation Requirements Help? *Educational Evaluation and Policy Analysis*. (Fall 1987). Vol 9y No. 3, pp. 258-273
- [45] Sosa, A. (2017a). Estimating Marginal Treatment Effects of High School Mathematics Credits on Income. *Dissertation Chapter*, University of Michigan May 2017.
- [46] Sosa, A. (2017b). Financial Incentives for Teachers in STEM fields: A National Data Set. *Dissertation Chapter*, University of Michigan May 2017.
- [47] Staiger, D. & Stock, J. (1997). Instrumental Variables Regressions with Weak Instruments. *Econometrica*, Vol. 65, No. 3 (May, 1997), pp. 557-586

- [48] Welch, W., Anderson, R. & Harris, L. (1982). The Effects of Schooling on Mathematics Achievement. *American Educational Research Journal*. (Spring 1982). Vol. 19, No. 1, Pp. 145-153

Table 1.1: Summary Statistics Analysis Sample N= 4,219. Controls.

Variable	Mean	Sd	Min	Max
Demographics				
female	0.50	0.50	0	1
white	0.73	0.44	0	1
age as of 12-31-1996	14.68	1.10	13	16
Family				
household gross income 1996-1999	\$56,141	\$46,039	\$233	\$417,074
household income to poverty ratio 1996-1999	3.56	3.05	0.01	32.27
household size 1997	4.36	1.42	2	16
both bio parents in household	0.55	0.50	0	1
Cohort				
cohort1995	0.28	0.45	0	1
cohort1996	0.25	0.43	0	1
cohort1997	0.21	0.41	0	1
cohort1998	0.08	0.28	0	1
cohort1999	0.01	0.12	0	1

All the means are calculated using the 1997 weights.

Table 1.2: Summary Statistics Analysis Sample N= 4,219. Treatment, Outcome, High School Graduation Requirements and Math Reforms and Instrument.

Variable	Mean	Sd	Min	Max
Mathematics Credits				
Advanced math credits	1.03	1.11	0	7.50
Total Credits				
Total Academic Credits	16.05	6.20	0	33.50
Income age 28				
income age 28	\$26,894	\$19,194	\$2.70	\$128,535
College attendance and BA/BS attainment				
Ever attended any college	0.77	0.42	0	1
Ever attended a 4-year college	0.48	0.50	0	1
Received BA/BS diploma	0.32	0.47	0	1
HS Math Graduation Requirements and Reforms				
high school graduation req (mathematics)	2.40	0.58	1	4
years exposed to math reforms	0.65	1.35	0	4
Instrument				
<i>expo</i> : years of potential exposure to STEM programs (during high school)	2.14	1.82	0	4

All the means are calculated using the 1997 weights.

Table 1.3:

First Stage: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on **advanced mathematics credits** controlling for demographics, household characteristics, high school math graduation requirements, and state and cohort fixed effects. Please refer to the Appendix for the complete table.

	(1) expo years	(2) expo recipients	(3) expo expend.	(4) expo recip/ 1,000 teach	(5) expo exp/teacher
in*state 7	0.224*** (0.0359)	0.00282*** (0.000578)	6.74e-07*** (1.37e-07)	0.107*** (0.0217)	0.0257*** (0.00561)
in*state 19	0.0233 (0.0594)	0.00121 (0.00241)	1.64e-07 (3.24e-07)	-0.000307 (0.110)	0.000790 (0.0160)
in*state 34	0.147*** (0.0315)	6.85e-05*** (1.60e-05)	1.91e-08*** (4.02e-09)	0.00533*** (0.00132)	0.00154*** (0.000362)
⋮	⋮	⋮	⋮	⋮	⋮
N	4,219	4,219	4,219	4,219	4,219
Math	1.026	1.026	1.026	1.026	1.026
AP F stat	11.39	101.89	95.8	115.54	106.9
p-value	0.0015	0.000	0.0000	0.0000	0.0000

State-level clustered robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions in which the dependent variable is **advanced math credits**; the treatment variables are the interactions of **in** and state dummies. in1-in5 measure potential years of exposure to: (1) STEM programs, (2) recipients, (3) expenditures, (4) recipients/1,000 teachers, and (5) expenditure/teacher. State and cohort fixed effects were also included. The F-stat tests the null hypothesis of joint significance of the coefficients of the interactions of *in* and state dummies. All regressions use the 1997 weights.

Table 1.4:

First Stage: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on **Total Credits** controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects. Please refer to the Appendix for the complete table.

	(1) expo years	(2) expo recipients	(3) expo expend.	(4) expo recip/ 1,000 teach	(5) expo exp/teacher
in*state 7	0.853*** (0.167)	0.0112*** (0.00269)	2.67e-06*** (6.41e-07)	0.390*** (0.111)	0.0891*** (0.0282)
in*state 19	2.068*** (0.296)	0.0667*** (0.0120)	8.99e-06*** (1.62e-06)	2.979*** (0.580)	0.389*** (0.0825)
in*state 34	0.438*** (0.156)	0.000189** (7.85e-05)	4.54e-08** (2.04e-08)	0.0122* (0.00665)	0.00270 (0.00182)
⋮	⋮	⋮	⋮	⋮	⋮
N	4,219	4,219	4,219	4,219	4,219
Total credits	16.05	16.05	16.05	16.05	16.05

State-level clustered robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions in which the dependent variable is **total credits**; the treatment variables are the interactions of **in** and state dummies. in1-in5 measure potential years of exposure to: (1) STEM programs, (2) recipients, (3) expenditures, (4) recipients/1,000 teachers, and (5) expenditure/teacher. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table 1.5:

Reduced Form: Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on **ln(Income age 28)** controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects. Please refer to the Appendix for the complete table.

	(1) expo years	(2) expo recipients	(3) expo expend.	(4) expo recip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.0547* (0.0289)	0.0505*** (0.00274)	3.51e-06*** (1.87e-07)	0.747*** (0.0945)	0.132*** (0.0158)
in*state 5	-0.0218 (0.0136)	0.00131*** (7.52e-05)	1.49e-07*** (7.87e-09)	0.0434*** (0.0156)	0.0119*** (0.00416)
in*state 6	0.0297 (0.0187)	0.000147* (8.37e-05)	8.12e-08* (4.69e-08)	0.00297 (0.00434)	0.00205 (0.00230)
in*state 7	-0.104*** (0.0250)	-0.00115*** (0.000343)	-2.76e-07*** (8.21e-08)	-0.0611*** (0.0154)	-0.0140*** (0.00348)
in*state 8	0.0446*** (0.0153)	0.00230 (0.00307)	3.25e-07 (4.42e-07)	0.00437 (0.0241)	-9.26e-05 (0.00376)
⋮	⋮	⋮	⋮	⋮	⋮
N	4,219	4,219	4,219	4,219	4,219
Mean Income	\$26,894	\$26,894	\$26,894	\$26,894	\$26,894

State-level clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. OLS regressions in which the dependent variable is **ln(income age 28)**; the treatment variables are the interactions of **in** and state dummies. in1-in5 measure potential years of exposure to: (1) STEM programs, (2) recipients, (3) expenditures, (4) recipients/1,000 teachers, and (5) expenditure/teacher. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table 1.6: Impact of mathematics credits on log income. N=4,219

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	2SLS	2SLS	2SLS
Panel A. instrument: expo years					
math	0.130*** (0.0179)	0.102*** (0.0212)	0.0520 (0.258)	0.0501 (0.276)	0.0831 (0.302)
total cred-its		0.00948** (0.00389)		0.0107 (0.0352)	-0.00621 (0.0656)
Panel B. instrument: expo recipients					
math	0.130*** (0.0179)	0.102*** (0.0212)	0.0805 (0.0653)	0.0253 (0.0730)	0.0322 (0.0876)
total cred-its		0.00948** (0.00389)		0.0367** (0.0145)	0.0304 (0.0312)
Panel C. instrument: expo expenditures					
math	0.130*** (0.0179)	0.102*** (0.0212)	0.0809 (0.0638)	0.0312 (0.0698)	0.0386 (0.0858)
total cred-its		0.00948** (0.00389)		0.0351** (0.0141)	0.0276 (0.0313)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.130*** (0.0179)	0.102*** (0.0212)	0.00154 (0.0613)	-0.0137 (0.0826)	0.0144 (0.0955)
total cred-its		0.00948** (0.00389)		0.0239 (0.0161)	-0.00207 (0.0279)
Panel E. instrument: expo expenditures/teacher					
math	0.130*** (0.0179)	0.102*** (0.0212)	0.0103 (0.0671)	-0.00231 (0.0886)	0.0281 (0.102)
total cred-its		0.00948** (0.00389)		0.0259 (0.0169)	-0.00673 (0.0302)

State-level clustered robust standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.001. Regressions are weighted. Monetary measures in 2011 dollars. In all the 2SLS equations the exclusion restrictions are the interactions of the instruments and state dummies. State and cohort dummies were included in all the models.

Table 1.7: Impact of mathematics credits on **college attended and bachelors degree attainment**.

	(1)	(2)	(3)
	Ever college	Ever 4-year college	BA attainment
$dPr[Y = 1 X, Z]/d\text{math}$	0.0792	0.1127	.0882
Bootstrap s.e.	(0.0389)	(.0472)	(.0488)

Average marginal derivatives of three measures of college outcomes. In column 1, the outcome is a dummy variable that indicates whether or not individuals ever attended any college. In column 2, the outcome is a dummy variable that indicates whether or not individuals attended a 4-year college and, in column 3, the outcome is a dummy variable that indicates whether or not individuals obtained a bachelors degree. I utilized the **ivprobit** Stata command to estimate the coefficients of the probit model of the impact of interactions of *in1* (*expo*) and state dummies on the outcome, controlling for the same background variables as before. With the coefficients, I predicted for all the members of the sample, (1) the probability of positive outcome, $Pr_i[y = 1, X, \text{math}]$, and, (2) by increasing the number of math credits by one unit, I predicted the probability of a positive outcome $Pr_i[y = 1, X, \text{math} + 1]$. The difference is the derivative evaluated at each individual. The average marginal derivative is the average of $Pr_i[y = 1, X, \text{math} + 1] - Pr_i[y = 1, X, \text{math}]$ across all the members in the sample. The standard errors were obtained by bootstrapping 50 repetitions.

Table 1.8: Characteristics of compliers using Abadie’s (2003) kappa method. Demographic and household characteristics.

	(1) $E[x complier]$	(2) $E[x]$	(3) $\frac{E[x complier]}{E[x]}$
Demographic controls			
Female	0.53	0.50	1.07
White	0.60	0.734	0.811
Age as of 12/31/1996	14.60	14.68	0.994
Family			
hh income 96-99	\$54,529	\$56,140	0.97
hh income poverty ratio 96-99	3.46	3.56	0.97
hh size 1997	4.38	4.36	1.00
both bio parents	0.54	0.55	0.98

The values of $E[x|complier]$ and $E[x]$ were obtained by following a variation of the method proposed in Abadie (2003). Since Abadie’s (2003) method applies when the endogenous variable and the instrument are binary I calculated dummy variables of math and expo. For math, the dummy is equal to 1 if the math is greater or equal to its mean; for the instrument, the dummy is equal to 1 when the number of years of potential years of exposure is above zero.

Table 1.9:

Impact of mathematics credits on log income. N=4,771. Specification 2: Exclude **household gross income 1996-1999** and **household income to poverty ratio 1996-1999**.

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A. instrument: expo years					
math	0.145*** (0.0154)	0.104*** (0.0187)	0.307 (0.187)	0.305 (0.208)	0.319 (0.199)
total cred-its		0.0136*** (0.00366)		0.0135 (0.0286)	-0.00323 (0.0461)
Panel B. instrument: expo recipients					
math	0.145*** (0.0154)	0.104*** (0.0187)	0.182 (0.114)	0.116 (0.161)	0.133 (0.164)
total cred-its		0.0136*** (0.00366)		0.0335 (0.0204)	0.0155 (0.0294)
Panel C. instrument: expo expenditures					
math	0.145*** (0.0154)	0.104*** (0.0187)	0.187 (0.121)	0.131 (0.165)	0.149 (0.168)
total cred-its		0.0136*** (0.00366)		0.0310 (0.0205)	0.0109 (0.0299)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.145*** (0.0154)	0.104*** (0.0187)	0.00838 (0.104)	0.00616 (0.125)	0.00538 (0.129)
total cred-its		0.0136*** (0.00366)		0.0260 (0.0155)	-0.000169 (0.0289)
Panel E. instrument: expo expenditures/teacher					
math	0.145*** (0.0154)	0.104*** (0.0187)	0.0437 (0.116)	0.0552 (0.140)	0.0514 (0.144)
total cred-its		0.0136*** (0.00366)		0.0230 (0.0177)	-0.00675 (0.0309)

State-level clustered robust standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.001. Regressions are weighted. Monetary measures in 2011 dollars. In all the 2SLS equations the exclusion restrictions are the interactions of the instruments and state dummies. State and cohort dummies were included in all the models.

Table 1.10:

Impact of mathematics credits on log income. N=4,219. Specification 3: Include interactions of **both biological parents** and **household gross income 1996-1999** and **household income to poverty ratio 1996-1999**.

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A. instrument: expo years					
math	0.131*** (0.0181)	0.105*** (0.0214)	0.0302 (0.191)	0.0139 (0.210)	0.0119 (0.223)
total cred-its		0.00883** (0.00392)		0.00950 (0.0390)	0.0153 (0.0554)
Panel B. instrument: expo recipients					
math	0.131*** (0.0181)	0.105*** (0.0214)	0.0886 (0.0694)	0.0437 (0.0787)	0.0373 (0.0866)
total cred-its		0.00883** (0.00392)		0.0324** (0.0150)	0.0366 (0.0314)
Panel C. instrument: expo expenditures					
math	0.131*** (0.0181)	0.105*** (0.0214)	0.0862 (0.0669)	0.0460 (0.0748)	0.0398 (0.0848)
total cred-its		0.00883** (0.00392)		0.0313** (0.0146)	0.0340 (0.0319)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.131*** (0.0181)	0.105*** (0.0214)	-0.0350 (0.0790)	-0.0304 (0.101)	-0.0185 (0.108)
total cred-its		0.00883** (0.00392)		0.0255 (0.0177)	-0.00543 (0.0270)
Panel E. instrument: expo expenditures/teacher					
math	0.131*** (0.0181)	0.105*** (0.0214)	-0.0189 (0.0840)	-0.0100 (0.107)	0.00247 (0.114)
total cred-its		0.00883** (0.00392)		0.0265 (0.0185)	-0.0103 (0.0293)

State-level clustered robust standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.001. Regressions are weighted. Monetary measures in 2011 dollars. In all the 2SLS equations the exclusion restrictions are the interactions of the instruments and state dummies. State and cohort dummies were included in all the models.

Table 1.11:

Impact of mathematics credits on log income. N=4,771. Specification 4: Set missing values of **household gross income 1996-1999** and **household income to poverty ratio 1996-1999** to zero.

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A. instrument: expo years					
math	0.134*** (0.0160)	0.0964*** (0.0187)	0.275 (0.218)	0.288 (0.236)	0.307 (0.219)
total cred-its		0.0127*** (0.00361)		0.00900 (0.0317)	-0.0167 (0.0468)
Panel B. instrument: expo recipients					
math	0.134*** (0.0160)	0.0964*** (0.0187)	0.127 (0.0980)	0.0582 (0.131)	0.0970 (0.143)
total cred-its		0.0127*** (0.00361)		0.0352* (0.0185)	0.0105 (0.0297)
Panel C. instrument: expo expenditures					
math	0.134*** (0.0160)	0.0964*** (0.0187)	0.132 (0.103)	0.0715 (0.136)	0.112 (0.146)
total cred-its		0.0127*** (0.00361)		0.0327* (0.0189)	0.00585 (0.0302)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.134*** (0.0160)	0.0964*** (0.0187)	0.0181 (0.0629)	0.00160 (0.0810)	0.0244 (0.0987)
total cred-its		0.0127*** (0.00361)		0.0229* (0.0133)	-0.00320 (0.0306)
Panel E. instrument: expo expenditures/teacher					
math	0.134*** (0.0160)	0.0964*** (0.0187)	0.0325 (0.0754)	0.0297 (0.102)	0.0503 (0.113)
total cred-its		0.0127*** (0.00361)		0.0217 (0.0161)	-0.0117 (0.0331)

State-level clustered robust standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.001. Regressions are weighted. Monetary measures in 2011 dollars. In all the 2SLS equations the exclusion restrictions are the interactions of the instruments and state dummies. State and cohort dummies were included in all the models.

CHAPTER II

Estimating Marginal Treatment Effects of High School Mathematics Credits on Income

2.1 Introduction

Several studies have attempted to measure the impact of high school mathematics credits on labor market outcomes (Altonji, 1995; Levine & Zimmerman, 1995; Rose & Betts, 2004; Joensen & Nielsen, 2009; Goodman, 2012; Gaertner, Kim, DesJardins & McClarty, 2014; Kim, Kim, DesJardins & McCall, 2015). In all cases the starting point is the following equation:

$$Y_i = \alpha_0 + \beta Math_i + \mathbf{X}_i\gamma + \epsilon_i \quad (2.1)$$

If equation (2.1) is estimated via Ordinary Least Squares (OLS), the estimate of β is inconsistent since $Math_i$ is endogenous, i.e., it is correlated with ϵ_i . This literature has addressed this problem by using instrumental variables approaches. Therefore, these studies have provided estimates of the Local Average Treatment Effects (*LATE*) that are, by construction, instrument-specific (Angrist and Pischke, 2009). The problem with this approach as stated in Carneiro, Heckman and Vytlačil (2011) is that people induced by the instrument to change their choice might not be the same people that change their choice in response to a policy change; the returns might be different for these two groups.

There is another problem that this literature has overlooked: β is assumed to be

constant in the population, and, consequently, implicitly these studies assume that individuals act as if they don't know their idiosyncratic returns or, if they know, they do not use this information when choosing the optimal level of math credits. As pointed out by Carneiro, Heckman and Vytlačil (2011), selection on gains complicates the estimation of the impacts of education on earnings.

The idea of heterogeneous returns to education is not new to the literature. Card (2001) used heterogeneous returns to education to explain why the 2SLS estimates are generally larger than the corresponding OLS estimates. He concluded that the marginal returns to education among low-education groups tend to be relatively high, reflecting their high marginal cost of schooling.

A concept that is suited to measure heterogeneous effects is the Marginal Treatment Effect (MTE) which was introduced in the literature by Björklund and Moffitt (1987) and extended in Heckman & Vytlačil (1999, 2001, 2005, 2007). Moffitt (2008) estimated marginal treatment effects of higher education in the UK by using power series or splines. Carneiro, Heckman & Vytlačil (2011), provided two methods for estimating marginal treatment effects of college attendance in the US: the first is based on a normal selection model, and the second utilizes Local Instrumental Variables (LIV). In addition, Heckman and Li (2004) utilized marginal treatment effects to identify heterogeneous returns to college in China. For a comprehensive treatment of marginal treatment effects theory and applications please refer to Heckman and Vytlačil (2007).

By borrowing the methods in Carneiro, Heckman and Vytlačil (2011), and Heckman, Tobias and Vytlačil (2001), in this paper I estimate marginal treatment effects of obtaining advanced mathematics credits during high school on income. The treatment is a dummy variable that indicates whether or not individuals earned credits in advanced mathematics during high school. Following the definitions of NLSY 97, advanced mathematics include Algebra 2 through Pre-Calculus, Calculus, AP/IB and

Advanced Mathematics-Other.

These methods allow me to estimate not only the marginal treatment effects (MTE), but also other relevant parameters commonly presented in the literature of returns to education such as the Average Treatment Effect (ATE), Treatment on the Treated (TT), and, Treatment on the Untreated (TUT). The results indicate that for each Carnegie unit of advanced mathematics earned during high school, the Average Treatment Effect (ATE) is about 4.4%, the Treatment on the Treated (TT) is about 2.45% and the Treatment on the Untreated (TUT) is about 7.39%. The Marginal Treatment Effect (MTE) varies between -0.01% and 10%.

Interestingly, the results indicate that individuals who are already enrolled in advanced math courses would benefit the least from taking an additional year of advanced math courses. Also, individuals who are not enrolled in advanced math courses, would benefit the most from taking advanced math during high school. Finally, the results also indicate that obtaining an additional year of advanced math courses is beneficial for all individuals.

These results are consistent with Heckman, Tobias and Vytlačil (2001) who found that, for a randomly chosen individual, the average gain from obtaining some form of college is about 9%, whereas the average gain for those who actually select into college is about 4%. Even when they do not report TUT, implicitly these estimates need to be higher than 9%.

The paper is organized as follows. In section (2.2), I briefly describe the literature that examines the returns to high school math credits. In section (2.3), I describe the methods for estimating marginal treatment effects. Section (2.4) is devoted to describing the data, the first stage and reduced form relationships and the parameter estimates of ATE, TT, TUT and MTE from the normal selection model. Conclusions are presented in section (2.5).

2.2 Brief Review of the Literature on the Returns to High School Math Credits

All the studies that have attempted to provide a causal impact of high school mathematics credits on earnings utilize instrumental variables. Although these studies do not mention which parameter they are estimating, by using instrumental variables, they provide an estimate of the Local Average Treatment Effect or LATE (Angrist & Pischke, 2009). In other words, they gauge the impact of an additional unit of mathematics credits during high school on earnings *only* for the subgroup of individuals who are induced by the instrument to change their course-taking behavior.

In this study, I depart from the traditional IV framework approach utilized in the returns to math credits literature and, based on the work of Carneiro, Heckman and Vytlačil (2011) and Heckman, Tobias and Vytlačil (2001), I present four parameters of interest in the treatment evaluation literature: Average Treatment Effect (ATE), Treatment on the Treated (TT), Treatment on the Untreated (TUT) and Marginal Treatment Effect (MTE). The rationale of presenting the results of previous studies is to compare the magnitudes and place this study's estimates in context.

An important difference of this study with respect to other studies in the returns to high school math credits literature is that, instead of using a continuous treatment, I use a binary treatment. In this way, I can use the methods presented in Carneiro, Heckman and Vytlačil (2011) and Heckman, Tobias and Vytlačil (2000).

In this section, I briefly describe the results of the studies that examine the causal impact of high school mathematics credits on labor market outcomes¹. For a complete review of the literature please refer to Sosa (2017a).

Altonji (1995) concluded that one more year of the combination of science, math, English, social studies and foreign language leads to an increase of wages of about 0.3%. Levine & Zimmerman (1995) found that the number of high school math

¹Earnings or income

courses does not impact individuals' weekly wages ten years after high school graduation. The magnitudes of the estimates was -0.017 for men and -0.060 for women; both statistically insignificant. In addition, Rose & Betts (2004) estimated that credits earned in algebra/geometry increased earnings by 8%. No statistically significant effects were found for intermediate algebra (-0.107), advanced algebra (-0.77) and calculus (-0.132).

Goodman (2012) calculated that each additional year of math increases black males' earnings between 5-9 %. The impact for white males is about the same magnitude but statistically insignificant. The results for women are small and statistically insignificant at about 0.035 for black women and 0.005 for white women. Finally, departing from the US context, Joensen & Nielsen (2009) concluded that taking advanced math credits coupled with advanced chemistry, increases earnings by about 20%.

In Sosa (2017a), I estimate that each additional Carnegie unit (year) of high school advanced mathematics increases total labor income by about 3%. For comparison purposes, I use Goodman (2012) and Sosa (2017a) as the references to benchmark the estimates found in this study. The reasons behind choosing these two studies are the following: first, they both provide convincing instruments; second, they depart from the traditional instrument proposed by Altonji (1992,1995) - the per-high school average number of math credits - and followed by Levine & Zimmerman (1995) and Rose & Betts (2004). Finally, Joensen & Nielsen's (2009) study was conducted in Denmark, and, thus, might not be relevant to the US context.

2.3 Methods for Estimating Marginal Treatment Effects

In this section, I present the methods to identify and estimate four parameters of interest in the program evaluation literature: Average Treatment Effect (ATE), Average Treatment Effect on the Treated (TT), Average Treatment Effect on the Untreated

(TUT), and, Marginal Treatment Effect (MTE). Specifically, I estimate these parameters in the context of the effects of high school math credits. To my knowledge, this is the first study of marginal treatment effects of high school mathematics credits on labor market outcomes.

Mostly, the methods presented in this paper are based on the study by Heckman, Tobias and Vytlacil (2001) who presented the aforementioned four parameters - ATE, TT, TUT and MTE in the context of returns to college education. The main takeaway on their methods is that these parameters can be estimated using the normal selection model. For the case of the Marginal Treatment Effect (MTE), I combine the methods of Heckman et al (2001), with those of Carneiro, Heckman and Vytlacil (2011). The rationale behind this choice is due to the fact that, in Carneiro et al. (2011), it is easier to interpret the MTE as a function of the propensity to receive the treatment in a support bounded between 0 and 1.

Since the treatment is binary, the starting point in Heckman et al. (2001) is the Generalized Roy Model. If individual i obtains a positive (> 0) number of advanced math credits during high school, then $D = 1$; $D = 0$ otherwise. The selection equation is:

$$D_* = Z\theta + U_D \quad \text{where} \quad D = 1[Z\theta + U_D \geq 0] \quad (2.2)$$

In equation (2.2), D_* represents a latent index of the propensity to obtain advanced math credits during high school. The support of D_* is $(-\infty, +\infty)$. The potential outcome equations are:

$$Y_1 = X\beta_1 + U_1 \quad \text{and} \quad Y_0 = X\beta_0 + U_0 \quad (2.3)$$

It is worth noting that equations (2.3) are *potential* outcome equations instead of outcome equations because we observe individuals only in one of the two states,

$D = 1$ or $D = 0$ but not in both. For example, if individual i earned advanced math credits during high school, and hence, $D = 1$, then, Y_1 represents her measure of log income, and, Y_0 represents what would her log income be if she did not earn advanced math credits during high school ($D = 0$). Combining the outcome and treatment in the same equation we have $Y = DY_1 + (1 - D)Y_0$ for all individuals.

In equations (2.3) X is a matrix of variables that influence income assumed to be uncorrelated with (U_1, U_0) . Also, in equation (2.2), Z includes some or all elements of X but also, it includes at least one variable that induces individuals into the treatment but is not included in any of the potential outcome equations. U_D is an unobserved scalar component that induces individuals into the treatment.

Define $\Delta \equiv Y_1 - Y_0$. The ATE Average Treatment Effect is defined as the expected gain from the treatment for a randomly chosen individual. The ATE conditional on x is:

$$ATE(x) = E[\Delta|X = x] = x(\beta_1 - \beta_0)$$

Assuming that the joint distribution of X is $F_X(X)$ then, the unconditional ATE is:

$$ATE = E[\Delta] = \int ATE(X)dF(X) \approx \frac{1}{n} \sum_{i=1}^n ATE(x_i) = \bar{x}(\beta_1 - \beta_0) \quad (2.4)$$

The Treatment on the Treated (TT) is the average gain from treatment for those who actually select into the treatment.

$$TT(x, z, D[z] = 1) = E[\Delta|X = x, Z = z, D[z] = 1]$$

In the previous equation $D[z] = 1$ indicates that the treatment is equal to 1 and also that it depends on the vector of instruments z .

$$TT(x, z, D[z] = 1) = x(\beta_1 - \beta_0) + E[U_1 - U_0 | U_D \geq -z\theta, X = x, Z = z]$$

Assume that (U_D, U_1, U_0) is independent from (X, Z) then

$$TT(x, z, D[z] = 1) = x(\beta_1 - \beta_0) + E[U_1 - U_0 | U_D \geq -z\theta]$$

Integrating over the joint distribution of (X, Z) conditional on $D = 1$ we have:

$$TT = E[\Delta | D[Z] = 1] = \int TT[X, Z, D[Z] = 1] dF(X, Z | D[Z] = 1)$$

Then, define n_t as the number of observations for which $D = 1$ then

$$TT \approx \frac{1}{n_t} \sum_{i=1}^n D_i TT[x_i, z_i, D[z_i] = 1] \quad (2.5)$$

Finally, the Marginal Treatment Effect, MTE is defined as the treatment effect for individuals with a given value of U_D .

$$MTE(x, u_D) = E[\Delta | X = x, U_D = u_D]$$

$$MTE(x, u_D) = x(\beta_1 - \beta_0) + E[U_1 - U_0 | X = x, U_D = u_D]$$

$$MTE(x, u_D) = x(\beta_1 - \beta_0) + E[U_1 - U_0 | U_D = u_D]$$

The unconditional (of X) version is

$$MTE(u_D) = \int MTE(X, u_D) dF(X) \approx \frac{1}{n} \sum_{i=1}^n MTE(x_i, u_D) \quad (2.6)$$

$$MTE(u_D) = \bar{x}(\beta_1 - \beta_0) + E[U_1 - U_0|U_D = u_D]$$

Treatment parameters assuming joint normality

Similar to Heckman, Tobias and Vytlacil (2001) assume joint normality of (U_D, U_1, U_0)

$$\begin{bmatrix} U_D \\ U_1 \\ U_0 \end{bmatrix} \sim N \left[0, \begin{bmatrix} 1 & \sigma_{1D} & \sigma_{0D} \\ \sigma_{1D} & \sigma_1^2 & \sigma_{10} \\ \sigma_{0D} & \sigma_{10} & \sigma_0^2 \end{bmatrix} \right]$$

The variance parameter in the selection equation has been normalized to unity. The estimation procedure, according to Heckman, Tobias and Vytlacil (2001) is as follows:

1. Obtain $\hat{\theta}$ from a probit model on the decision to take the treatment, i.e., estimate equation 2.2 using a probit model.

$$D = 1[Z\theta + U_D \geq 0]$$

2. Compute the appropriate selection correction term evaluated at $\hat{\theta}$ (i.e., $\widehat{\lambda}_{1i} = \phi[Z_i\hat{\theta}]/\Phi[Z_i\hat{\theta}]$ when $D_i = 1$ and $\widehat{\lambda}_{0i} = \phi[Z_i\hat{\theta}]/(1 - \Phi[Z_i\hat{\theta}])$ when $D_i = 0$). For each individual in the sample calculate either $\widehat{\lambda}_{1i}$ or $\widehat{\lambda}_{0i}$.
3. Run treatment-outcome specific regressions for the groups $[i : D_i = 1]$ and $[i : D_i = 0]$ with the inclusion of the appropriate selection-correction term obtained from the previous step.

$$Y_1 = X\beta_1 + \sigma_{1D}\widehat{\lambda}_1 + U_1$$

$$Y_0 = X\beta_0 + \sigma_{0D}\widehat{\lambda}_0 + U_0$$

4. Given $\hat{\beta}_1$, $\hat{\beta}_0$, $\widehat{\sigma_{1D}}$ and $\widehat{\sigma_{0D}}$ obtained from step 3, and $\hat{\theta}$ from step 1, calculate the following equations:

Conditional ATE

$$ATE(x_i) = x_i(\hat{\beta}_1 - \hat{\beta}_0)$$

Unconditional ATE

$$ATE = \bar{x}(\hat{\beta}_1 - \hat{\beta}_0) \quad (2.7)$$

$$ATE = \frac{1}{n} \sum_{i=1}^n ATE(x_i) \quad (2.8)$$

Conditional TT

$$TT[x_i, z_i, D[z_i] = 1] = x_i(\hat{\beta}_1 - \hat{\beta}_0) + (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}}) \frac{\phi(z_i \hat{\theta})}{\Phi(z_i \hat{\theta})}$$

Unconditional TT

$$TT = \frac{1}{n_t} \sum_{i=1}^n D_i TT[x_i, z_i, D[z_i] = 1] \quad (2.9)$$

Conditional TUT

$$TUT[x_i, z_i, D[z_i] = 0] = x_i(\hat{\beta}_1 - \hat{\beta}_0) - (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}}) \frac{\phi(z_i \hat{\theta})}{(1 - \Phi(z_i \hat{\theta}))}$$

Unconditional TUT

$$TUT = \frac{1}{n - n_t} \sum_{i=1}^n (1 - D_i) TUT[x_i, z_i, D[z_i] = 0] \quad (2.10)$$

Conditional MTE

$$MTE[x_i, U_D = u_D] = x_i(\hat{\beta}_1 - \hat{\beta}_0) + (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}})u_D \quad (2.11)$$

Unconditional MTE

$$MTE[U_D = u_D] = \bar{x}(\hat{\beta}_1 - \hat{\beta}_0) + (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}})u_D \quad (2.12)$$

Since the Heckman, Tobias and Vytlacil (2001) study does not present charts of marginal treatment effects, for comparison purposes I use the same unobserved component of the propensity to participate in the treatment (U_S) as in Carneiro, Heckman and Vytlacil (2011). Transforming (2.11) to correspond to Carneiro, Heckman and Vytlacil (2011). Under Carneiro et al (2011), in the choice model I_S is defined as the net benefit from choosing $S = 1$ ². The latent variable I_S is a function of a vector of observable characteristics (\mathbf{Z}) and an unobserved scalar component V in the following way:

$$I_S = \mu_S(\mathbf{Z}) - V \quad (2.13)$$

Thus, we observe

$$S = 1 \quad \text{if} \quad I_S \geq 0; \quad S = 0 \quad \text{otherwise} \quad (2.14)$$

Similarly to Heckman, Tobias and Vytlacil (2001) each choice is associated with a potential outcome equation:

²In Carneiro et al. (2011) S represents a binary treatment of whether or not individuals were ever enrolled in college.

$$Y_1 = \mu_1(\mathbf{X}) + U_1 \quad \text{and} \quad Y_0 = \mu_0(\mathbf{X}) + U_0 \quad (2.15)$$

Again, in equations (2.15), \mathbf{X} is a vector of observed characteristics that influence income. In equation (2.13), \mathbf{Z} is a vector of observable characteristics which might include some (or all), elements of \mathbf{X} but also includes at least one instrument, i.e., a variable that induces individuals to change their advanced math credits but does not belong to any of the potential outcome equations (2.15).

In the net benefit equation, (2.13), V is assumed to be a continuous random variable with a cumulative distribution function F_V . The rationale behind this transformation is the following: whereas the support of V is $(-\infty, +\infty)$, the support of $F_V(V)$ is $[0, 1]$. This transformation allows working with a more mathematically tractable variable $F_V(V)$ instead of V . Define $U_S = F_V(V)$, then, the MTE is defined as:

$$MTE(x, u_s) \equiv E[\beta | \mathbf{X} = \mathbf{x}, U_s = u_s] \quad (2.16)$$

According to Carneiro et al. (2011), in their context "the marginal treatment effect is the mean return to schooling for individuals with characteristics \mathbf{X} and $U_S = u_s$." (pp. 2727). In this paper, the marginal treatment effect is interpreted as the average return of having earned advanced math credits during high school for persons who are indifferent between obtaining or not advanced math credits.

By comparing equations (2.2) and (2.13) we know that $U_D = -V$. Thus, equation (2.12) becomes

$$MTE[V = v] = \bar{x}(\hat{\beta}_1 - \hat{\beta}_0) - (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}})V \quad (2.17)$$

Similar to Carneiro et al (2011), I use the transformation $U_S = F_V(V)$ to be able to work with a more tractable variable U_S instead of V . Finally, the marginal

treatment effect formula that I use in this study is:

$$MTE[U_S = u_S] = \bar{x}(\hat{\beta}_1 - \hat{\beta}_0) - (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}})F^{-1}(u_S) \quad (2.18)$$

Implications of the joint normality assumption

The estimates generated in this study depend on whether or not the joint normality assumption holds in the population. Since the error terms are by construction unobserved, no test can tell us whether or not the assumption of joint normality holds. Even when this is a strong assumption, it is also true that this is the first paper that explores marginal treatment effects in the context of returns to high school math credits. In further versions of the paper, I am planning to estimate marginal treatment effects by the semiparametric method of Local Instrumental Variables as presented in Carneiro, Heckman and Vytlacil (2011). For now, this paper is the first step into the integration of the Marginal Treatment Effect framework and the returns to high school math credits literature.

2.4 Estimated Marginal Treatment Effects

2.4.1 Data

Treatment, Outcome and Controls

The individual-level data for this paper were drawn from the National Longitudinal Survey of Youth 1997 (NLSY 97) which is a nationally representative sample of around 9,000 individuals who were 12 to 16 years old as of 12/31/1996. This survey allows researchers to link individual choices such as high school mathematics course taking behavior to labor market outcomes later in life. NLSY 97 includes transcript information for about 70 percent of all individuals in the sample. Transcript information has been made homogeneous across all schools and years via the Carnegie units

system.

According to NCES, one Carnegie unit is defined as the number of credits a student receives for a course taken every day, one period per day, for a full school year³. In addition to high school Carnegie units, NLSY 97 also includes information about labor market outcomes such as employment, income, earnings and wages. The dependent variable or outcome in this paper is the total labor income, which is obtained by asking "During last year, how much income did you receive from wages, salary, commissions, or tips from all jobs, before deductions for taxes or anything else?". Since this question is asked repeatedly, I constructed a measure of income, in 2011 real USD, centered around age 28. I chose 28 because all individuals in the sample turn 28 within the observation period.

In this paper I define advanced mathematics (*advanced_math*) as a dummy variable equal to 1 if individual i obtained credits in advanced mathematics as defined by NLSY 97; that is, in any of the following courses: Algebra 2 through Pre-Calculus, Calculus, AP/IB and Advanced Mathematics-Other. As controls, in all the models, I included: a dummy for female, a dummy for white, age in years as of 12-31-1996 (age at the beginning of the study), the average of non-missing values of household gross income between 1996 and 1999 (in 1997 real USD), the average of non-missing values of household income to poverty ratio between 1996 and 1999, household size in 1997, a dummy that indicates whether the household had both biological parents in 1997, state-level number of Carnegie units (years) of math required to obtain a high school diploma in 1997, number of years of exposure to a high school math reform⁴. State

³<https://nces.ed.gov/nationsreportcard/glossary.aspx?nav=y>

⁴Reform indicates whether a state changed the number of years of mathematics required for high school graduation between 1995 and 2005. Exposure to math reforms was based on the first year of high school enrollment using the following rules: if the reform year occurred before first year of enrollment, exposure to math reforms is equal to 4; if the reform year occurred after the last year of high school enrollment, exposure to math reforms is equal to zero; finally, if the reform year occurred in between the first and last years of enrollment, exposure to math reforms equals the difference between the last year of high school enrollment and reform year.

and cohort⁵ fixed effects were also included.

The sample was restricted to individuals who ever enrolled in high school and who had non-missing values of advanced mathematics, total labor income at age 28 as well as in all the aforementioned controls. A total of 4,219 individuals were included in the analysis sample.

Instrument / Exclusion Restrictions

In the formulation presented in section (2.3), the matrix Z includes some or all elements in X but also includes at least one instrument, i.e., a variable that induces individuals to modify their course taking choices but does not otherwise affect outcomes. The instruments in this paper are state-level measures of shocks to teacher labor supply. Specifically, as instruments, I use the implementation of state-level financial incentive programs that induce current or future teachers and/or recent college graduates into teaching in shortage subject areas such as math and science, and/or geographic critical shortage areas (e.g., schools located in high-poverty neighborhoods). Throughout this paper I call these programs STEM Programs. For a comprehensive description of the STEM programs please refer to Sosa (2017b).

For clarity, I call *instruments* to individual-level measures of exposure to STEM programs. When the instruments are *interacted* with state dummies, I call these variables *exclusion restrictions*. The main instrument utilized in this paper is the number of years of potential exposure to STEM programs while individuals were enrolled in high school; this variable is called **expo**.

To construct expo, first, for each individual, I calculated the first and last years of potential enrollment in high school; the first year of enrollment is equal to the birth year plus 17 and the last year of enrollment is the first year of enrollment plus 3. In this way, the instrument does not depend on *actual* enrollment which is endogenous

⁵Cohort is defined as the year individuals entered high school.

but only on potential enrollment which depends on the year individuals were born. For each individual, I created a row vector - $enrollment_{is}$ - which is a 1X34 vector with 0's in the years of no enrollment and ones in the years of potential enrollment.

Also, for each state, I constructed a row vector A_s with ones on the years state s had at least one active STEM program, and 0's on the years state s did not have an active STEM program. The 34 columns refer to all the years between 1983 to 2016 utilized in the STEM programs data. Thus, $expo$ was calculated by the following formula: $expo = enrollment_{is} * A'_s$.

In addition to $expo$, I also used other STEM program characteristics such as number of recipients, expenditures, recipients per 1,000 teachers, expenditures per teacher, recipients per 1,000 secondary teachers, expenditures per secondary teacher, recipients per 1,000 students, expenditures per student, recipients per 1,000 high school students, and, expenditures per high school student.

Let $stem_s$ be any of the mentioned STEM program characteristics. For the sake of explanation, let $stem_s$ be $expo_recipients_s$ which is a 1X34 row vector with zeros in the years state s did not have any STEM program, and, the average number of recipients on the years state s did have at least one active STEM program. Thus, to calculate potential exposure to all STEM program characteristics I used the following formula: $expo_stem_s = enrollment_{is} * stem'_s$.

To abbreviate, for the remaining of the paper, I call exposure to STEM program characteristics $in1, \dots, in11$. Please refer to table (2.1) for the definition of all the instruments $in1, \dots, in11$. Please refer to Sosa (2017b) to learn more about the different STEM Program characteristics like program type (e.g., loan forgiveness, scholarship, etc), program focus (e.g., math and science, shortage geographic areas, minorities) as well as the magnitude of the programs in terms of the number of participants and expenditures. I also provide maps that capture the variation across states regarding the aforementioned STEM program variables. Finally, in table (2.1), I define all the

variables included in this paper.

Table 2.1: Definitions of the Variables Used in the Empirical Analysis

Variable	Definition
Y	Log of total labor income at age 28
D=1	if respondent obtained high school advanced math credits; 0 otherwise.
X	Female, white, age in years as of 12-31-1996, average household gross income between 1996 and 1999, average household income to poverty ratio between 1996 and 1999, household size in 1997, household both biological parents in 1997, state-level years of math required to obtain a high school diploma in 1997, years of exposure math reforms.
Instruments	
in1	Years of potential exposure to STEM programs during high school.
in2	Years of potential exposure to STEM program recipients.
in3	Years of potential exposure to STEM program expenditures.
in4	Years of potential exposure to STEM program recipients per 1,000 teachers.
in5	Years of potential exposure to STEM program expenditures per teacher.
in6	Years of potential exposure to STEM program recipients per 1,000 secondary teachers.
in7	Years of potential exposure to STEM program expenditures per secondary teacher.
in8	Years of potential exposure to STEM program recipients per 1,000 students.
in9	Years of potential exposure to STEM program expenditures per K-12 student.
in10	Years of potential exposure to STEM program recipients per 1,000 high school students.
in11	Years of potential exposure to STEM program expenditures per high school student.

All the analyses utilize the same controls. The exclusion restrictions are the interactions of the instruments and state dummies. State and cohort fixed effects are also included.

2.4.2 Sample Characteristics

The NLSY 97 sample includes 8,984 individuals of which 6,120 have transcript information. After dropping records with missing values of income the resulting sample included 4,841 individuals. By dropping missing observations on the following variables: average household gross income between 1996 and 1999 (545 observations), average household income to poverty ratio between 1996 and 1999 (17 observations), and the number of years of math required to obtain a high school diploma (60 observations), the final sample size for all the analyses is 4,219. None of the remaining controls have missing values.

Table (2.2) includes descriptive statistics of the variables employed in the empirical analyses. The analysis sample consists of $N=4,219$ individuals of whom 50% are women; about 73% are white with an average age at the beginning of the study of about 14.68 years. The average household gross income between 1996 and 1999 is about \$56,141, and, the household income to poverty ratio, also between 1996 and 1999, is about 3.56. The average household size in 1997 was about 4.36 and, 55 percent of the households had both biological parents. All the summary statistics are weighted.

On average, the number of Carnegie units of advanced math credits is about 1.02, and 59% of the sample obtained advanced math credits. The mean income (total labor income) at age 28 is about \$26,894. Moreover, the average years of potential exposure during high school is 2.14. It is noteworthy that potential exposure to STEM programs varies greatly from 0 to 4 with a standard deviation of 1.82 years.

2.4.3 First Stage and Reduced Form

One condition for identification of marginal treatment effects is the existence of at least one variable that influences individuals choices without being included in

Table 2.2: Summary Statistics of Analysis Sample N= 4,219

Variable	Mean	Sd	Min	Max
Treatment, Outcome, Instrument				
Advanced math	1.02	1.11	0	7.50
Advanced math (binary)	0.59	0.49	0	1
income	\$26,894	\$19,194	\$2.70	\$128,535
expo	2.14	1.82	0	4
Demographics				
female	0.50	0.50	0	1
white	0.73	0.44	0	1
age as of 12-31-1996	14.68	1.1	13	16
Family				
household gross income	\$56,141	\$46,039	\$233	\$417,074
1996-1999				
household income to poverty	3.56	3.05	0.01	32.27
ratio 1996-1999				
household size 1997	4.36	1.42	2	16
both bio parents in household	0.55	0.50	0	1

All the means are calculated using the 1997 weight. The analysis sample includes individuals who ever enrolled in high school who also have transcript information and whose income at age 28 is non-missing. State and cohort fixed effects are also included in the analyses but not shown in this table.

the potential outcome equations. The exclusion restrictions ($Z|X$) are measures of potential exposure to STEM program characteristics (e.g., *expo*) interacted with state dummies.

In table (2.3), I estimated separate OLS regressions in which the dependent variables are, in column (1), advanced math, and, in column (2), the log of total labor income. I only present estimates of the first stage and reduced form relationships for the first instrument (*expo*). For more information about the F – *statistics* regarding the association between $Z|X$ and advanced math, please refer to Sosa (2017a). In all cases (instruments), the F – *statistics* are much bigger than the rule of thumb of 10.

As table (2.3) indicates, the impact of *expo* on treatment and outcome vary greatly by state and it is positive and statistically significant in most cases. For space limitations, I present a trimmed version of the first stage and reduced form table; the complete table is located at the end of this chapter.

Table 2.3:
First Stage and Reduced Form. Impact of interactions of exposure to STEM programs and state dummies on advanced math and log income. Please refer to the appendix for the complete table.

	(1) advanced math	(2) ln(income)
expo*state 4	0.115*** (0.0202)	0.707*** (0.0531)
expo*state 5	0.293*** (0.0335)	1.502*** (0.0747)
⋮	⋮	⋮
expo*state 50	0.0836*** (0.0193)	0.353*** (0.0391)
expo*state 51	0.304*** (0.0333)	1.159*** (0.0766)
N	4,219	4,219

State-level cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions in which the dependent variable are for column 1, a dummy variable that indicates whether students earned a positive number of credits in advanced math, and 0 otherwise. In column 2, the dependent variable is the natural logarithm of income at age 28. State and cohort fixed effects were also included. All regressions use the 1997 weight.

2.4.4 Parameter estimates from the normal selection model

As mentioned in section (2.3), in order to estimate ATE, TT, TUT and MTE, under the assumption of joint normality of the error terms, I estimated an OLS regression for each group $D = 1$ and $D = 0$ with the corresponding sample correction term. Table (2.4) includes the parameter estimates of such regressions.

Table 2.4: **Parameter Estimates - Normal Sample Selection Model**

	(1) $E[Y D$ $1, X, lamda1]$	(2) $E[Y D$ $0, X, lamda0]$	=
female	-0.297*** (0.0545)	-0.603*** (0.0565)	
white	-0.0295 (0.0682)	0.135 (0.0881)	
age as of 12-31-1996	-0.0296 (0.130)	0.0239 (0.0916)	
hh gross income 1996-1999	1.56e-07 (2.34e-06)	-1.74e-06 (2.06e-06)	
hh income to poverty ratio 1996-1999	0.0181 (0.0261)	0.0483 (0.0299)	
household size 1997	0.0101 (0.0285)	0.0295 (0.0216)	
both biological parents	-0.0238 (0.102)	-0.0600 (0.0992)	
expo reforms	-0.0708 (0.0755)	-0.0294 (0.0725)	
high school math grad req 1997	-0.00887 (0.209)	-0.119 (0.199)	
λ_1	-0.236 (0.404)		
λ_0		0.492* (0.292)	
Constant	10.79*** (1.497)	9.717*** (1.270)	
N	2,354	1,859	
R^2	0.060	0.165	

Column 1 includes the parameter estimates of the OLS regression of log income on X and λ_1 conditional on $D = 1$. Column 2 includes the parameter estimates of the OLS regression of log income on X and λ_0 conditional on $D = 0$.

The important contribution of table (2.4) is that, the parameter estimates of

β_1 , β_0 , σ_{1D} and σ_{0D} along with θ are the basis for calculating ATE, TT, TUT and MTE using the formulas presented in section (2.3). For instance, to calculate ATE, I first calculated $ATE(x_i) = x_i(\hat{\beta}_1 - \hat{\beta}_0)$ for all the members in the sample. Next, I calculated the average across all individuals.

Similarly, to calculate TT, first, I calculated $TT[x_i, z_i, D[z_i] = 1] = x_i(\hat{\beta}_1 - \hat{\beta}_0) + (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}}) \frac{\phi(z_i\hat{\theta})}{\Phi(z_i\hat{\theta})}$, or equivalently, $TT[x_i, z_i, D[z_i] = 1] = x_i(\hat{\beta}_1 - \hat{\beta}_0) + (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}})\lambda_{1i}$ for each individual with $D = 1$, and, next, I calculated the average across all treated individuals. For the TUT, I calculated $TUT[x_i, z_i, D[z_i] = 0] = x_i(\hat{\beta}_1 - \hat{\beta}_0) - (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}}) \frac{\phi(z_i\hat{\theta})}{(1-\Phi(z_i\hat{\theta}))}$ for all individuals with $D = 0$, and, next, I averaged it across all the untreated.

In order to calculate MTE, first, I generated a grid of U_S ranging from 0.05 to 0.95 in intervals of 0.05. Next, for each i and U_S , I calculated $MTE[x_i, U_s = u_s] = x_i(\hat{\beta}_1 - \hat{\beta}_0) - (\widehat{\sigma_{1D}} - \widehat{\sigma_{0D}})\Phi^{-1}(u_s)$. This step resulted in 19 columns: $MTE_{05}, MTE_{10}, \dots, MTE_{95}$. Finally, the graph (figure (2.1)) showing $MTE(U_S)$ is generated averaging each of the 19 columns across all members of the sample. The 90% standard errors were obtained by bootstrapping this process 50 repetitions⁶.

Similar to Carneiro, Heckman and Vytlacil (2011) who divided by their estimates by four in order to measure the average gain of one year of college, I also divided the estimates in this paper by 20 since most states require 20 units to obtain a high school diploma. By doing so, I assume that all of the credits are concentrated in advanced mathematics.

It is important mentioning the parameter estimates of λ_1 and λ_0 in table (2.4). These parameters are estimates of the covariances between the error terms in the outcome equations (Equation 2.3) and the error term in the selection equation (Equation 2.2) in Heckman, Tobias and Vytlacil (2001). In particular $\sigma_{1D} = Cov(U_1, U_D)$ and $\sigma_{0D} = Cov(U_0, U_D)$. Since U_D is positive in equation (2.2)), a higher value of U_D

⁶According to the Stata Bootstrap Manual, N=50 is an adequate number of repetitions for normal approximation confidence intervals.

leads to a higher propensity to select into the treatment. Now, the fact that $\hat{\sigma}_{1D} < 0$ indicates that, for individuals who select into the treatment, an increase in U_D is associated with a decrease in U_1 , an unobserved component of income. Furthermore, $\hat{\sigma}_{0D} > 0$ also indicates that, for individuals who do not select themselves into the treatment, an increase in U_D is associated with an increase in U_0 ; in other words, for individuals who are not taking advanced math courses, increasing the propensity to obtain advanced math credits is associated with a higher unobserved component of income.

2.4.5 Estimates of the Returns to Advanced Mathematics Credits: ATE, TT, TUT, MTE.

The main results of the paper are presented in this section. These results were obtained using the first instrument: *expo*. Table (2.5) includes the estimates of the returns to advanced math credits for different groups. For each Carnegie unit of advanced math, for a random individual in the population, the Average Treatment Effect (ATE) is about 4.47%. For individuals who actually obtained advanced math credits the effect (TT) is about 2.45%. Finally, for the group who did not earn advanced math credits, the average effect (TUT) is about 7.39%.

The results are consistent with those in Heckman, Tobias and Vytlačil (2011) who examined the impact of college education on log wages. They found that for a randomly chosen person, receiving some form of higher education leads to an increase in hourly wages of about 9%. For individuals who select into college, the average gain is about 4%. They conclude that "individuals with unobservables making them most likely to enroll in college receive the smallest return to a college education", Heckman, Tobias and Vytlačil (2001, p.221).

In addition, the results presented in this study are consistent with other studies in the literature of returns to high school math credits. For example, Goodman

(2012) found that the returns to math credits for males varied between 5% and 9%. In Sosa (2017a), I found that one Carnegie unit of advanced math increases total labor income by about 3%. Let’s recall that both Goodman (2012) and Sosa (2017a) present estimates of weighted Local Average Treatment Effects (LATE’s). Unlike all other studies in the literature of returns to high school math credits, this is the first study that provides measures of different estimands other than weighted LATE’s.

Table 2.5: Returns to one credit of advanced math

$ATE = E(\beta)$	0.0447 (0.0002345)
$TT = E(\beta D = 1)$	0.0245 (0.000387)
$TUT = E(\beta D = 0)$	0.0739 (0.0196)

The parameters ATE, TT and TUT are calculated according to the formulas presented in this paper which are based on Heckman, Tobias and Vytlačil (2001).

An important concern is whether the average gain to advanced math credits is constant in the population. If this is the case, then individuals either do not know their idiosyncratic returns to math credits, or if they do know, this information does not play any role when choosing whether or not to take advanced math during high school. When individuals act on the information about their idiosyncratic returns to math credits, selection on gains complicates the estimation of returns to math credits. On top of the endogeneity problem characterized by the correlation between $Math_i$ and ϵ_i in equation (2.1), we also have a selection on gains problem because of the correlation between $Math_i$ and β .

The framework presented in this study allows testing whether or not β is constant in the population. This is accomplished by testing the null hypothesis of equality of covariances: $H_0 : \sigma_{1D} = \sigma_{0D}$. Table (2.6) includes the results of this test. By

bootstrapping the difference in covariances $(\widehat{\sigma}_{1D} - \widehat{\sigma}_{0D})$ 250 times, I calculated the standard error. The conclusion is that I cannot reject H_0 .

Even when we cannot reject the null hypothesis of equality of covariances, and consequently, constancy of MTE , the p-value (0.133) is very close to 0.1, therefore we cannot conclude that MTE is constant. Heckman and Vytlačil (2007) summarize the evidence regarding the constancy of MTE in different contexts. In their analysis, studies such as Lee (1978), Farber (1983) and Duncan and Leigh (1985) do not reject the null hypothesis of equality of covariances in the normal selection model. Most studies, however, reject the null hypothesis, and conclude that MTE is not constant.

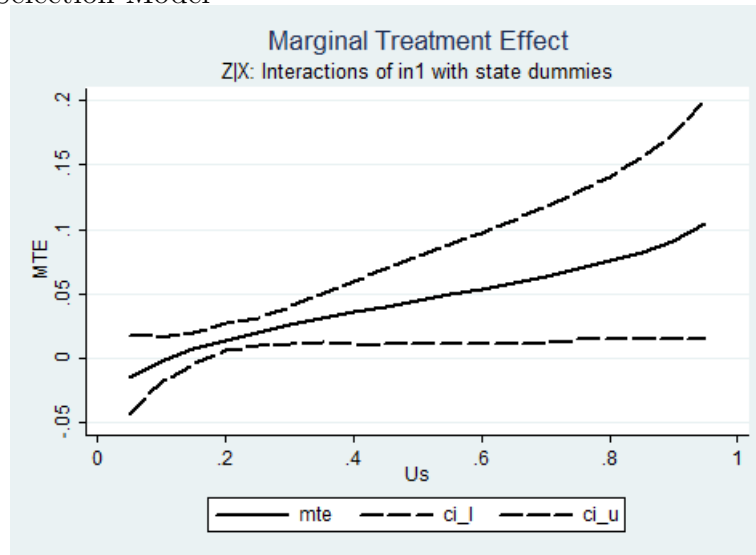
Table 2.6: Test of Constancy of MTE. $H_0 : \sigma_{1D} = \sigma_{0D}$	
$\widehat{\sigma}_{1D} - \widehat{\sigma}_{0D}$	-0.7273
Bootstrap standard error	(0.485)
<i>P – value</i>	0.133
Decision	Cannot Reject H_0
The standard error was calculated bootstrapping 250 repetitions.	

Figure (2.1) shows the mapping of the marginal treatment effect onto the unobserved propensity to receive the treatment, U_S . The 90% confidence intervals were estimated by bootstrapping 50 repetitions. Since I transformed the unobserved component of the propensity to receive the treatment from U_D to U_S , higher values of U_S decrease the propensity to receive the treatment, and viceversa, lower values of U_S increase the propensity to receive the treatment.

Figure (2.1) reflects some important features. First, the average gain varies across the distribution of the propensity to select into the treatment. On the one hand, individuals with high values of U_S , those who are less likely to participate in the treatment, are the ones who gain more. On the other hand, individuals with low values of U_S ; i.e., those with high propensity to obtain advanced math credits are the

ones who benefit the least. Second, the average gain for individuals who are indifferent between $D = 1$ and $D = 0$ varies between -0.05% and 10%. For individuals at the margin the effects can be substantial, especially for those with high values of U_S .

Figure 2.1: Marginal Treatment Effects of Advanced Mathematics Credits - Normal Selection Model



Notes: The 90% confidence intervals were calculated using bootstrap. The exclusion restrictions $Z|X$ are the interactions of in1 (years of exposure to STEM programs) and state dummies. State and cohort fixed effects were also included in the outcome and selection equations.

2.4.6 Sensitivity Analysis

In this section I estimate three parameters of interest, ATE, TT, TUT and MTE using the entire set of instruments (one by one) defined at the beginning of this section. As table (2.7) indicates, the ATE ranges from 3.35% to 4.77%. Also, the TT varies between 1.95% and 2.67%, and, finally, the TUT varies between 5.37% and 7.90%.

Table 2.7: Returns to one credit of advanced math. All instruments.

Instrument Definition: exposure to		ATE	TT	TUT
in1	years of STEM programs	0.0447 (0.000235)	0.0245 (0.000387)	0.0739 (0.000456)
in2	Recipients	0.0456 (0.000239)	0.0266 (0.000414)	0.0729 (0.0215)
in3	Expenditures	0.0456 (0.000240)	0.0267 (0.000416)	0.0727 (0.000502)
in4	Recipients/1,000 teachers	0.0335 (0.000237)	0.0195 (0.00034)	0.0537 (0.000373)
in5	Expenditure/teacher	0.0378 (0.000235)	0.0218 (0.000359)	0.0609 (0.000407)
in6	Recipients/1,000 sec teachers	0.0470 (0.000235)	0.0254 (0.0004004)	0.0781 (0.000481)
in7	Expenditures/sec teacher	0.0428 (0.000235)	0.0241 (0.000384)	0.0696 (0.000452)
in8	Recipients/ 1,000 students	0.0460 (0.000237)	0.0260 (0.000407)	0.0748 (0.000490)
in9	Expenditure/student	0.0436 (0.000235)	0.0244 (0.000387)	0.0712 (0.000456)
in10	Recipients/1,000 HS students	0.0477 (0.000236)	0.0260 (0.000408)	0.0790 (0.00050)
in11	Expenditure/HS student	0.0448 (0.000237)	0.0258 (0.000404)	0.0721 (0.000483)

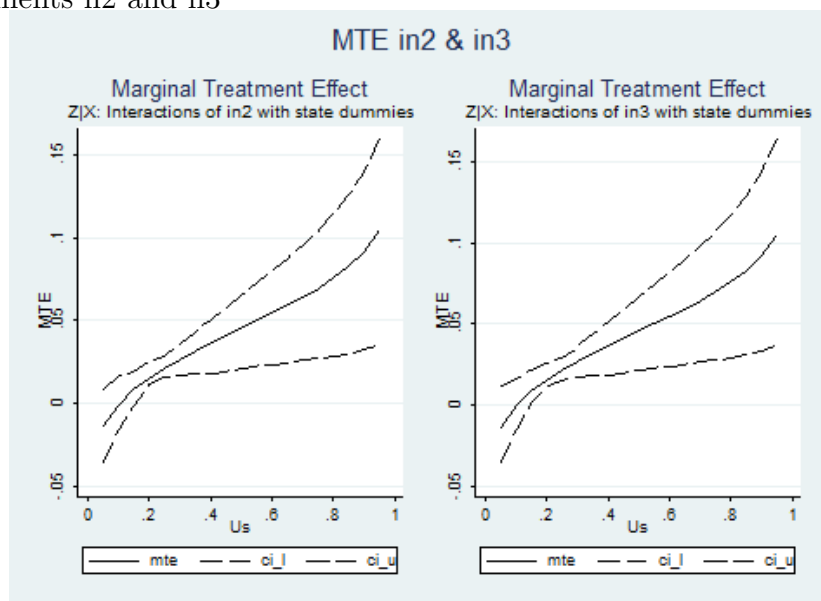
The parameters ATE, TT and TUT are calculated according to the formulas presented in this paper which are based on Heckman, Tobias and Vytlačil (2001).

The small variation in ATE, TT and TUT across the different instruments is remarkable. In contrast to Sosa's (2017a) study in which the variation in weighted

LATE's across the first five instruments is very large, in this study, the parameters estimates are more robust to the selection of the instrument. This is not surprising since the weighted LATE estimates are instrument dependent (Angrist & Pischke, 2009) whereas the Marginal Treatment Effects framework presented here helps to dissociate the instruments from the parameter estimates. Thus, it is a more robust framework to examine the gains from advanced math courses.

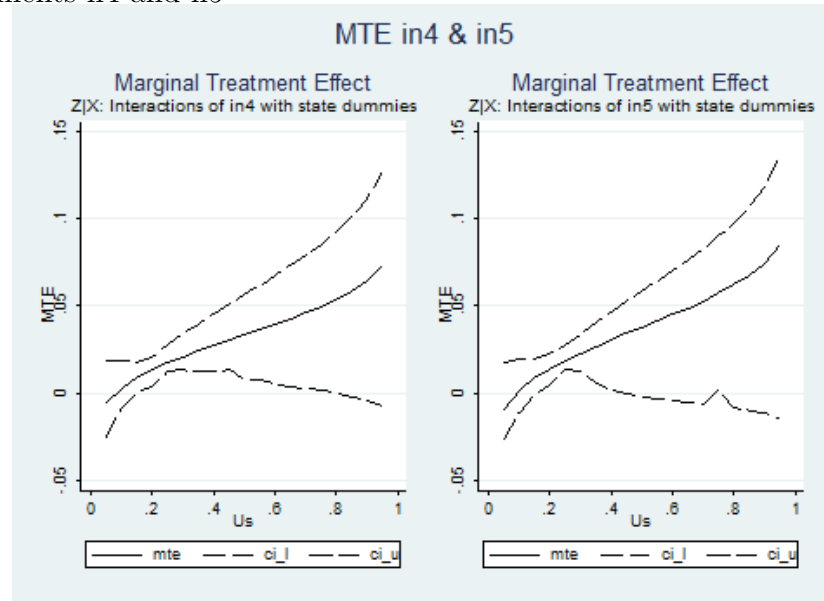
In figures (2.2) to (2.6), I estimated the marginal treatment effects using instruments 2 through 11. All the graphs follow the same pattern as shown in figure (2.1). Regardless of the instrument utilized, the average gain of advanced math credits is not constant in the population. In fact, it is a increasing function of U_S which implies that, for those with high values of U_S , and, hence, those who are less likely to earn advanced math credits, the gains are high whereas for those with low values of U_S , i.e., those more likely to earn advanced math credits, the returns are close to zero.

Figure 2.2: Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n2 and n3



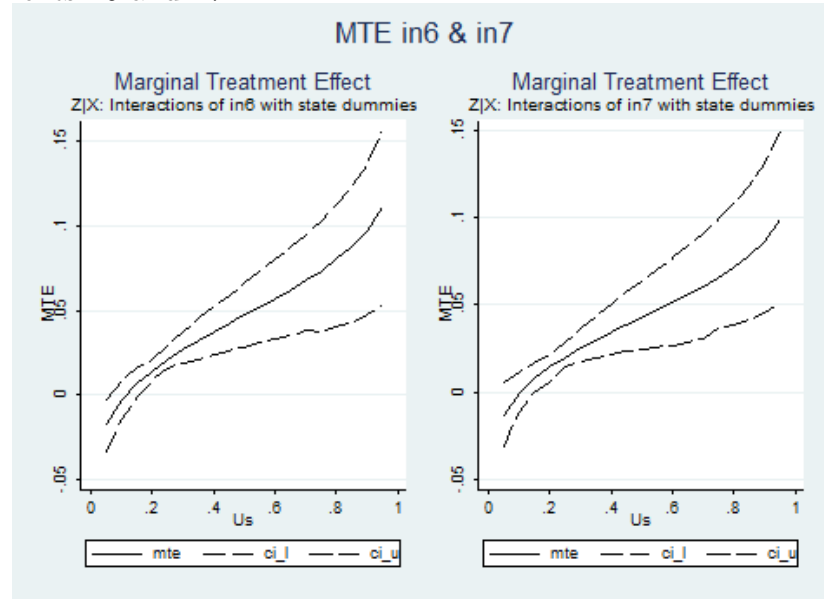
Notes: The 90% confidence intervals were calculated using bootstrap. The exclusion restrictions $Z|X$ are the interactions of in2 & in3 respectively and state dummies. State and cohort fixed effects were also included in the outcome equations and in the selection equation.

Figure 2.3: Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n4 and n5



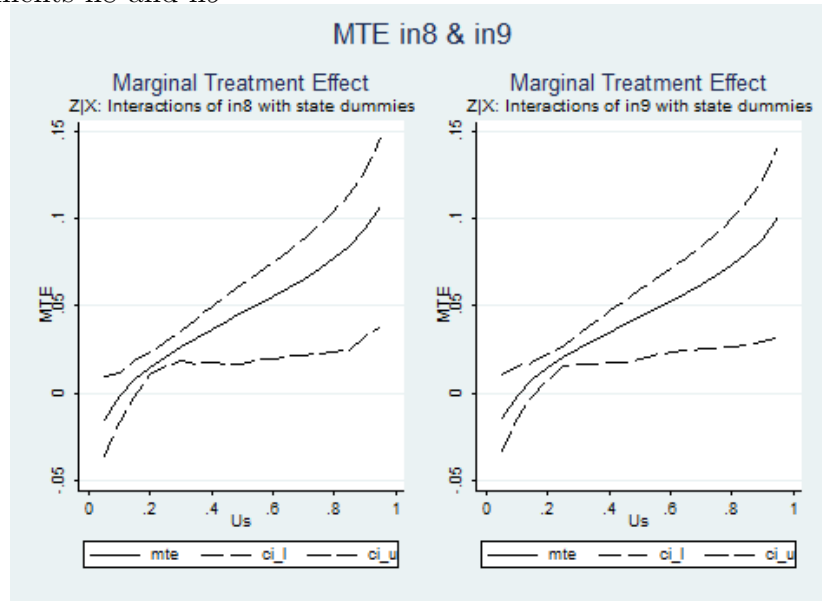
Notes: The 90% confidence intervals were calculated using bootstrap. The exclusion restrictions $Z|X$ are the interactions of in4 & in5 respectively and state dummies. State and cohort fixed effects were also included in the outcome equations and in the selection equation.

Figure 2.4: Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n6 and n7



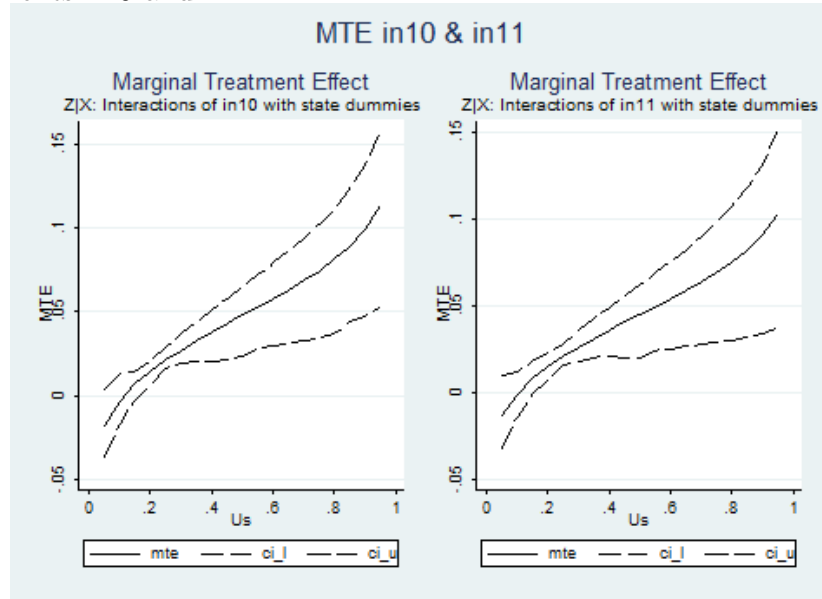
Notes: The 90% confidence intervals were calculated using bootstrap. The exclusion restrictions $Z|X$ are the interactions of in6 & in7 respectively and state dummies. State and cohort fixed effects were also included in the outcome equations and in the selection equation.

Figure 2.5: Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n8 and n9



Notes: The 90% confidence intervals were calculated using bootstrap. The exclusion restrictions $Z|X$ are the interactions of in8 & in9 respectively and state dummies. State and cohort fixed effects were also included in the outcome equations and in the selection equation.

Figure 2.6: Marginal Treatment Effects of Advanced Mathematics Credits: Instruments n10 and n11



Notes: The 90% confidence intervals were calculated using bootstrap. The exclusion restrictions $Z|X$ are the interactions of in10 & in11 respectively and state dummies. State and cohort fixed effects were also included in the outcome equations and in the selection equation.

2.5 Conclusions

This study is the first to estimate marginal treatment effects in the context of the returns to high school math credits. Several conclusions and contributions are worth mentioning.

First, the framework presented in this paper allows estimating different parameters relevant to the program evaluation literature: ATE, TT, TUT and MTE. The average gain in total labor income from obtaining advanced math credits during high school varies between 3.35% and 4.77%. These results are consistent with the parameter estimates obtained in Sosa (2017a) of about 3%.

Second, for individuals who are already enrolled in advanced mathematics credits during high school, increasing one more year of advanced math yields a return of about 2.45%. In other words, even when this group benefits the least, there is still some gain to increase the amount of advanced math credits.

Third, for individuals who are not enrolled in advanced math credits, by adding one year of advanced math credits to their transcripts, they have the potential to increase earnings by about 7.39%. From the public policy perspective this result is the most important because, the potential to benefit individuals from low-income backgrounds who do not have access to advanced mathematics due to lack of resources, teachers in particular, is very large.

Fourth, unlike Carneiro, Heckman and Vytlacil (2011), this study suggests that: (1) both treated and untreated individuals could benefit from one additional year of high school advanced mathematics. (2) There is no selection on gains, in other words, individuals with the highest potential gains do not select themselves into the treatment. This might be associated with the fact that, whereas in Carneiro, Heckman and Vytlacil (2011), individuals choose whether or not to attend college, and therefore, most of them are high school graduates, in this study, individuals are

choosing whether or not to enroll in advanced math, thus, high school dropouts are included.

Also, because marginal individuals in Carneiro, Heckman and Vytlačil (2011) are older than those in this study, they might be more prone to include future earnings into their cost-benefits analyses of the decisions to attend college, when compared to the cost-benefits analyses done by high school students when choosing which courses to take. It is likely that high school students are not thinking on future earnings when deciding whether or not to take algebra II.

Finally, unlike the previous literature on returns to math credits, this study advances our understanding of the distribution of average benefits across different groups of the population. No other study in the returns to high school math credits literature discusses the parameters of interest in the program evaluation literature: ATE, TT, TUT and MTE.

2.6 References

- [1] Altonji, J. (1995). The Effects of High School Curriculum on Education and Labour Outcomes. *The Journal of Human Resources*, Vol. 30, No. 3 (Summer, 1995), pp. 409-438
- [2] Angrist, J. & Pischke, J. (2009). Mostly Harmless Econometrics, An Empiricist's Companion. Princeton University Press.
- [3] Bjorklund, A. & Moffitt, R. (1987). The Estimation of Wage Gains and Welfare Gains in Self-Selection. *Review of Economics and Statistics*, 69(1): 49-49.
- [4] Card, D. (2001). Estimating the Returns to Schooling: Progress on some Persistent Econometric Problems. *Econometrica*, Vol. 69, No. 5 September, 2001., 1127 - 1160
- [5] Carneiro, P., Heckman, J. & Vytlacil, E. (2011). Estimating Marginal Returns to Education. *American Economic Review*, 101 October 2011, pp.2754-2781.
- [6] Duncan, G.M., & Leigh, D.E. (1985). The endogeneity of union status: An empirical test. *Journal of Labor Economics*, 3(3), 385-402 (July).
- [7] Farber, H.S. (1983). Worker preferences for union representation. *Research in Labor Economics*, Volume Supplement 2: New Approaches to Labor Unions. JAI Press, Greenwich, CT.
- [8] Gaertner, M., Kim, J., DesJardins, S. & McClarty, K.(2014). Preparing Students for College and Careers: The Causal Role of Algebra II. *Research in Higher Education*, (2014) 55:143 - 165.
- [9] Goodman, J. (2012). The Labor of Division: Returns to Compulsory Math Coursework. Faculty Research Working Paper Series. Harvard Kennedy School. August 2012.
- [10] Heckman, J. & Li, X. (2004). Selection Bias, Comparative Advantage and Heterogeneous Returns to Education: Evidence from China in 2000. *Pacific Economic Review*, 9:3 (2004) pp. 155-171.
- [11] Heckman, J., Tobias, J. & Vytlacil, E. (2001). Four Parameters of Interest in the Evaluation of Social Programs. *Southern Economic Journal*, 68:2 (Oct 2001) pp. 210-223.
- [12] Heckman, J., Urzua, S. & Vytlacil, E. (2006). Understanding Instrumental Variables with Essential Heterogeneity. *The Review of Economics and Statistics*, August 2006, 88(3): pp.389-432.
- [13] Heckman, J. & Vytlacil, E. (1999). Local instrumental variables and latent variable models for identifying and bounding treatment effects. *Proc. Natl. Acad. Sci. USA*, Vol. 96, pp. 4730 - 4734, April 1999, Economic Sciences.

- [14] Heckman, J. & Vytlačil, E. (2001). Policy-Relevant Treatment Effects. *The American Economic Review*, Vol. 91, No. 2, Papers and Proceedings of the Hundred Thirteenth Annual Meeting of the American Economic Association (May, 2001), pp. 107-111
- [15] Heckman, J. & Vytlačil, E. (2005). Structural Equations, Treatment Effects, and Econometric Policy Evaluation. *Econometrica*, Vol. 73, No. 3 (May, 2005), 669 - 738.
- [16] Heckman, J. & Vytlačil, E. (2007). Econometric Evaluation of Social Programs, Part II: Using the Marginal Treatment Effect to Organize Alternative Econometric Estimators to Evaluate Social Programs, and to Forecast their Effects in New Environments. *Handbook of Econometrics* Volume 6B
- [17] Joensen, J. & Nielsen, H. (2009). Is there a Causal Effect of High School Math on Labor Market Outcomes? *The Journal of Human Resources*, 44 (2009).
- [18] Kim, J., Kim, J., DesJardins, S. & McCall, B. (2015). Completing Algebra II in High School: Does It Increase College Access and Success? *The Journal of Higher Education*, Vol. 86, Number 4, July/August 2015 pp.628-662.
- [19] Lee, L.-F.(1978).”Unionism and wage rates: A simultaneous equations model with qualitative and limited dependent variables”. *International Economic Review*, 19(2), 415-433 June
- [20] Levine, P. & Zimmerman, D. (1995). The Benefit of Additional High-School Math and Science Classes for Young Men and Women. *Journal of Business & Economic Statistics*. Vol. 13, No. 2, JBES Symposium on Program and Policy Evaluation (Apr., 1995), pp. 137-149.
- [21] Moffitt, R. (2008). Estimating Marginal Treatment Effects in Heterogeneous Populations. *Annales d’Economie et de Statistique*, No.91/92, Econometric Evaluation of Public Policies: Methods and Applications (JULY-DECEMBER 2008), pp. 239-261.
- [22] Rose, H. & Betts, J. (2004). The Effect of High School Courses on Earnings. *The Review of Economics and Statistics*, May 2004, 86(2): 497-513
- [23] Sosa, A. (2017a). The Impact of High School Mathematics Credits on Earnings: Evidence from Shocks to Teachers Labor Supply. *Job Market Paper*, University of Michigan May 2017.
- [24] Sosa, A. (2017b). Financial Incentives for Teachers in STEM fields: A National Data Set. *Dissertation Chapter*, University of Michigan May 2017.

Table 2.8: **First Stage / Reduced Form** Impact of interactions of potential years of exposure to STEM programs on advanced math and natural logarithm of income.

	(1) advanced math	(2) ln(income)
expo*state 4	0.115*** (0.0202)	0.707*** (0.0531)
expo*state 5	0.293*** (0.0335)	1.502*** (0.0747)
expo*state 6	-0.0943*** (0.00973)	0.0297 (0.0187)
expo*state 7	0.170*** (0.0110)	-0.104*** (0.0250)
expo*state 8	0.346*** (0.0318)	1.568*** (0.0719)
expo*state 10	0.0984*** (0.0200)	0.732*** (0.0521)
expo*state 11	0.141*** (0.0185)	0.705*** (0.0455)
expo*state 14	0.335*** (0.0339)	1.473*** (0.0753)
expo*state 15	0.351*** (0.0347)	1.499*** (0.0758)
expo*state 18	0.0906*** (0.0207)	0.561*** (0.0540)
expo*state 19	0.171*** (0.0217)	0.314*** (0.0518)
expo*state 20	0.164*** (0.0329)	1.526*** (0.0691)
expo*state 21	0.119*** (0.0196)	0.754*** (0.0517)
expo*state 25	0.425*** (0.0321)	1.542*** (0.0796)
expo*state 26	-0.137*** (0.0238)	-0.152*** (0.0432)
expo*state 32	0.128*** (0.0196)	0.784*** (0.0539)
expo*state 33	0.0332*** (0.0108)	0.00890 (0.0279)
expo*state 34	0.0425*** (0.0101)	0.128*** (0.0226)
expo*state 35	0.00435 (0.00826)	0.190*** (0.0233)

expo*state 37	0.220*** (0.0169)	0.825*** (0.0416)
expo*state 39	0.118*** (0.0205)	0.687*** (0.0522)
expo*state 41	-0.0361*** (0.000873)	-0.0365*** (0.00184)
expo*state 42	-0.320*** (0.0263)	0.0604 (0.0486)
expo*state 43	0.192*** (0.0163)	0.786*** (0.0411)
expo*state 44	0.0451*** (0.0117)	-0.0375 (0.0243)
expo*state 45	0.284*** (0.0338)	1.540*** (0.0734)
expo*state 47	0.0687*** (0.0136)	0.154*** (0.0371)
expo*state 48	0.314*** (0.0344)	1.543*** (0.0765)
expo*state 49	0.258*** (0.0360)	1.598*** (0.0762)
expo*state 50	0.0836*** (0.0193)	0.353*** (0.0391)
expo*state 51	0.304*** (0.0333)	1.159*** (0.0766)
female	0.0603*** (0.0182)	-0.370*** (0.0329)
white	0.0761*** (0.0257)	0.105** (0.0422)
age as of 12-31-1996	-0.159*** (0.0153)	-0.159*** (0.0365)
hh gross income 1996-1999	2.29e-06*** (8.17e-07)	1.32e-06 (1.35e-06)
hh income poverty ratio 1996-1999	-0.0124 (0.0115)	0.0193 (0.0190)
hh size 1997	-0.0280*** (0.00750)	-0.00636 (0.0150)
both bio parents	0.152*** (0.0157)	0.0903*** (0.0315)
expo reforms	-0.0857*** (0.0114)	-0.118** (0.0554)
high school grad reg 1997	0.880*** (0.0630)	3.236*** (0.162)
N	4,219	4,219

State-level clustered robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions in which the dependent variable are for column 1, a dummy variable that indicates whether students earned a positive number of credits in advanced math, and 0 otherwise. In column 2, the dependent variable is the natural logarithm of income at age 28. State and cohort fixed effects were also included. All regressions use the 1997 weight.

CHAPTER III

Financial Incentives for Teachers in STEM fields: A National Data Set

3.1 Motivation

3.1.1 Teacher Shortages in the US

There is a longstanding view that there is a serious shortage of mathematics and science teachers in the US K-12 public education system (Guthrie & Zusman, 1982; Darling-Hammond & Skyes, 2003; Murphy, DeArmond, & Guin, 2003; Chin, Young, & Floyd, 2004; Moin, Dorfield, & Schunn, 2005; NCCTQ, 2007; Podolsky and Kini, 2016). The teacher shortage problem has two dimensions. The first is **geographical** in which hard-to-staff schools are usually located in high-poverty neighborhoods; these hard-to-staff schools and districts often have difficulties attracting and retaining qualified and fully licensed teachers (Chin, Young, & Floyd, 2004; Podlsky and Kini, 2016; Ingersoll & Perda, 2010; Darling-Hammond & Skyes, 2003). Shortages also exist in **subject areas** such as mathematics and science (Chin, Young, & Floyd, 2004; Podlsky and Kini, 2016; NCCTQ, 2007). Individuals with knowledge and experience in these areas can earn significantly higher starting salaries in private job sectors.

A deeper look into the problem reveals that, the shortage problem does not emerge because the supply does not match demand. The real problem lies in the **unequal distribution** of high quality teachers with surpluses in some areas and shortages in others (Darling-Hammond & Skyes, 2003). Ingersoll & Perda (2010) point out that

teacher shortages result from an uneven production of new teachers across locales.

Darling-Hammond & Skyes (2003) state that there are longstanding shortages in particular fields which result from more attractive earnings opportunities outside teaching. For example, when compared with what could have been their salary outside teaching, mathematics and science teachers suffer larger wage disparities than those teaching English and social studies. The opportunity cost for college graduates trained in mathematics and science is higher if they want to teach. Moin et al. (2005) point out that, nationally, between 1993 and 1999, 39 percent of school districts reported having math and science teacher vacancies.

Moreover, teacher pay is not only relatively low but, during the 1990's has even declined relative to other professions. Even after adjusting for the shorter work year in teaching, teachers earn 15 to 30 percent less than college graduates who enter other occupations (Darling-Hammond & Skyes, 2003). In a similar venue, Podlsky & Kini, (2016) point out that even after adjusting for a shorter work year, beginning teachers, nationally earn about 20% less than individuals with college degrees who enter other occupations; the gap widens to 30% by mid career.

Another reason behind the teacher shortages is the pre-retirement teacher attrition/turnover (Ingersoll & Perda, 2010). According to Darling-Hammond & Skyes (2003), since 1990 the annual outflow of teachers has surpassed the influx by increasingly large margins. As many as 20 percent of new teachers may leave teaching after 3 years and around 30 percent after five years. In addition, teacher turnover is 50 percent higher in high-poverty schools than in more affluent ones.

In sum, the evidence suggests that, the shortage problem is due to, (1) an uneven distribution of teachers; while some regions have shortages other regions have surpluses; (2) the shortage in fields like math and science is driven by wage differentials that favor occupations outside teaching, therefore, increasing the opportunity costs for individuals who have math and science knowledge and are willing to teach

(Darling-Hammond & Skyes, 2003; Ingersoll and Perda, 2010). Also, (3) the wage differentials between teaching and non-teaching occupations is another driver of teacher shortages, and finally, (4), the high levels of attrition characteristic of the teaching profession, especially within the first five years, contributes to the teacher shortages problem.

Indeed, more research is needed to better understand the sorting mechanisms of teachers within schools. If teachers are allocated into schools according to the price choice theory, then it should suffice for school districts to increase the wages high enough for teachers to fill up the positions. If teachers are sorted into schools by other mechanisms not related with pecuniary compensation then other solutions must be sought. Apparently, the responses by state governments to teacher shortages imply that the price choice theory is the prevailing sorting mechanism of teachers into schools. If this is the case, the shortages are artificially generated by the school districts when they do not increase wages high enough.

3.1.2 States' Response to Teacher Shortages

In response to teacher shortages, in recent decades, states have implemented a wide range of initiatives aimed to recruit new teachers - especially in mathematics and science (Arfin, 1986; Ingersoll & Perda, 2010; Darling-Hammond & Hudson, 1990). These programs include emergency certification, out of field assignments to fill vacancies, alternative certification programs, scholarships, bonuses and student loan forgiveness, among others (Moin et al., 2005).

Typically, the financial incentives are in the form of scholarship/loans, forgivable loans and tuition reimbursement for new teachers (Chin, Young, & Floyd, 2004; Podolsky & Kini, 2016), and signing bonuses for current teachers (Clotfelter, Glennie, Ladd, & Vigdor, 2007; 2008). The program recipients must commit to teaching in hard-to-staff schools and/or to teach in subjects areas such as mathematics and

science (Chin, Young, & Floyd, 2004; Podolsky & Kini, 2016).

This study focuses on financial incentives implemented by states aimed to increase teachers' labor supply, especially in mathematics and science and critical shortage areas. Some states might address the teacher shortage problem by using non-financial incentives such as out of field certification programs. In these cases, this data do not capture that activity. Instead, the data and analyses presented here only involve financial incentives.

3.1.3 Research Questions and Contribution

Given the intense efforts made by state governments to recruit highly qualified teachers across all locales and subjects, the research questions guiding this study are the following:

1. Starting in 1980, which states have implemented financial incentive programs aimed to recruit and retain teachers in STEM fields?
2. What is the purpose of each program?
3. What is the size of each program in terms of the number of recipients?
4. What is the size of each program in terms of expenditures?
5. What is the average duration of the programs?
6. What are the different types of programs (e.g., loans, scholarship, etc)?
7. What are the focus of the programs (e.g., math and science, shortage areas, etc)?
8. When normalized to account for state education systems' size (number of teachers and students), what is the distribution of STEM program characteristics across states?
9. Is there a geographic pattern across the US states in terms of the above variables?

This study intends to contribute to the literature of teachers' labor supply decisions by creating a data set that includes all state-sponsored financial incentive

programs aimed to recruit and retain teachers, especially in mathematics and science, and/or critical shortage areas. A goal of this study is the creation of a national data set that allows researchers, by merging this data with other data sources, to answer the following research questions:

- What is the impact of financial incentives for teachers on student education outcomes such as academic achievement, high school graduation, college access and success, etc.?
- What is the impact of financial incentives for teachers on teacher labor supply decisions?
- What is the impact of financial incentives for teachers on the distribution of teaching credentials across districts?
- Do financial incentives for teachers play a role in shaping equality of opportunity to high quality teachers?
- Do financial incentives for teachers impact the stock of teachers at the state and district levels?

The above questions, are only a small set out of all possible questions that researchers might answer with these data. One of the main contributions of this study is to put forward this data set, and encourage researchers to use it in their research endeavors.

This paper is organized as follows. In section (3.2), I review the literature of financial incentives and teachers' labor supply. In section (3.3), I describe the existing data sources of financial incentives for teachers and present the process I followed to construct a new data set of financial aid programs aimed at increasing the supply of teachers in math and science and/or critical shortage geographic areas. In this section, I also define the variables included in the data set. In section (3.4), I present the main results of the paper such as the program characteristics at the program level, and, variation of program characteristics across states. In this section, I also include a visual representation of the distribution of program characteristics in a US map. Conclusions are presented in section (3.5).

3.2 Related Literature - Financial Incentives and Teachers' Labor Supply

Even when many states have implemented financial incentive programs aimed at increasing the supply of teachers, especially in under-privileged geographic areas and/or in content areas such as mathematics and science, there is little evidence of the impact of these initiatives (Berry & Hirsch, 2005).

To my knowledge, there are only three studies that assess the impact of financial incentives aimed at recruiting teachers to understaffed schools. Steele, Murnane and Willett (2010) exploited a natural experiment in California to evaluate the impact of the Governors Teacher Fellowship (GTF) on the probability to teach in low-performing schools. The GTF was created in 2000 and subsequently eliminated in 2002, provided \$20,000 to newly licensed teachers who work for four years at schools in the bottom half of the state's Academic Performance Index (API). The purpose of the study was to estimate the impact of GTF on the decisions of new teachers to begin their careers in low performing schools.

The sample included individuals who were enrolled in a teacher licensure program during the academic years 1998-1999, 1999-2000, 2000-2001, 2001-2002 and 2002-2003, and who were also APLE¹ recipients. Only students enrolled in post-baccalaureate teacher licensure programs during the 2000-2001 and 2001-2002 academic years were eligible to apply. The outcome was a binary variable that indicates whether or not individuals began working in low-performing schools within two years after the licensure program's first enrollment. The treatment variable was equal to 1 if individuals received the GTF, and 0 otherwise.

Since the acquisition of GTF is endogenous, they used eligibility to receive GTF as instrument for receiving GTF. Eligibility was equal to 1 if individuals were enrolled during the academic years 2000-2001 and 2001-2002; the active years of the program.

¹APLE stands for Assumption Program of Loans for Education.

The results indicate that receiving the GTF increased the probability of starting working in a low-performing school by 28 percentage points.

Clotfelter, Glennie, Ladd and Vigdor (2008) examined the impact of the Math, Science and Special Education (MSSE) Teacher Recruitment Program in North Carolina. Between 2001 and 2004, the state of North Carolina offered a \$1,800 annual salary bonus to certified math, science, and special education teachers who worked at low-performing and high-poverty middle schools and high schools. By using a triple difference-in-differences analyses within a discrete time hazard model, the authors estimated the impact of the bonus program on the hazard rates of teachers.

The triple difference exploited three types of eligibility: schools, subjects and time. The hazard rate was defined as the probability of ending a teaching spell during year $t + 1$ conditional on having taught in year t . The results indicate that the bonus program reduced turnover rates by about 17%.

Finally, Feng and Sass (2015) analyzed the impact of the Florida Critical Teacher Shortage Program (FCTSP) on teacher attrition. One arm of the FCTSP provided loan forgiveness to certified teachers who worked in designated shortage areas. The other arm provided tuition reimbursement² to teachers who pursued certification.

In 2011, the maximum amount of tuition reimbursement was \$2,808, and the maximum amount forgiven, for up to four years, was \$2,500 per year for undergraduates and \$5,000 per year for graduate students. Similarly to Clotfelter et al (2008), Feng and Sass also estimated a difference-in-differences model within a discrete hazard approach. The hazard rate represents the probability that a teaching spell ends at period $t + 1$ conditional on being active in period t . They also used subject and time eligibility to perform the difference-in-differences approach. The results indicate that the loan forgiveness arm of the program reduced the probability of leaving teaching by 8.6% for science high school teachers, and, by 11% for middle and high school

²Tuition reimbursement was applied directly to tuition costs of the certification.

math teachers.

In sum, the evidence presented suggests that there is a positive effect of the above mentioned programs on recruitment and retention of teachers in shortage areas. Teachers respond to financial incentives. Nevertheless, more research is still needed to understand whether or not the financial incentives aimed at increasing teachers' labor supply are effective in providing equality of access to high quality teachers to all students, regardless of their socioeconomic background.

3.3 Construction of a National Data Set

3.3.1 Existing data on financial incentives for teachers

To my knowledge, there are only two institutions that collect information about state-sponsored financial incentives aimed to recruit and retain teachers: The Education Commission of the States (ECS) and the National Association of State Student Grant and Aid Programs (NASSGAP). In both cases, the information is collected and analyzed at the state level.

According to Aragon (2016) from ECS, most states have implemented financial incentives to improve teacher recruitment and retention. She divides the financial incentives in three types: **salary requirements** (e.g., minimum salary and salary schedules), **diversified pay**, i.e., differential payment aimed to attract teachers to work in hard-to-staff schools and/or shortage subject areas, and, **pay-for-performance**, to recognize excellence in teaching. Based on these categories, 7 states have implemented a minimum salary requirement, 17 states have programs based on salary schedules³, 23 states have implemented diversified pay, and 16 states have pay-for-performance programs.

Zinth (2008), also from ECS, conducted a 50-state analysis of state recruitment

³Differential pay according to credentials and experience

efforts of high school teachers in STEM fields. She concluded that 37 states provide loan-forgiveness, scholarship or tuition reimbursement to individuals seeking certification who agree to teach in shortage subject areas such as Math and Science. Also, she found that 12 states offer supplemental pay for teachers in STEM fields.

The information from ECS, although rich in content it was last updated in 2008. In addition, the report is presented as a snapshot in time and not as a time series and therefore, it is difficult to analyze the dynamics of creation or elimination of the programs. In any case, as I will mention later in this paper, I utilized the ECS report as the seminal list of all the financial incentives aimed at recruiting STEM teachers in high school.

The National Association of State Student Grant and Aid Programs (NASSGAP) conducts an annual survey about state-sponsored financial aid programs. Unlike ECS, NASSGAP collects the information in a year-by-year basis. Among other elements, it collects program name, expenditures, recipients, and type. The reports span from 1977 to 2015. Unlike, ECS, the information from NASSGAP does not include program description, eligibility requirements, participant commitments, and, law statutes.

Even though this paper utilizes information from ECS and NASSGAP, many financial aid programs that I eventually found using the process described later in the paper, were not included in either ECS or NASSGAP data sets.

3.3.2 A New Data Set of Financial Incentives for Teachers

For this study, I define STEM Teacher Recruitment Programs or just STEM programs as any financial aid utilized to recruit teachers, with emphasis on, but not limited to, mathematics and science and/or critical shortage geographic areas. First, I identified the websites that contained information about state-sponsored financial aid programs aimed at recruiting math and science teachers as well as teachers to work in critical shortage geographic areas.

Even though there might be additional websites with this information, in the observation period from May 2016 to September 2016, this is the smallest set that includes all the programs: Education Commission of the States www.ecs.org, www.iteachamerica.org, www.mathteaching.org, www.teachtomorrow.org, www.teachingdegree.org, www.collegegrant.net, www.credible.com, www.collegescholarship.org and www.collegeinvestor.com.

The outcome of the first step is a list of all the state-sponsored financial aid programs aimed at recruiting math and/or science teachers as well as teachers to work in critical shortage geographic areas; in other words, a list of all STEM programs. This list included only three variables: state, program name and program description.

Next, for each program, I searched for the following information: the first year of implementation, last year the program was active or observed on the internet, the per-year budget allocated⁴, and, the per-year number of recipients. For some programs, I also collected contact information, eligibility requirements, and responsibilities. In this search process I relied on www.google.com, www.yahoo.com, and www.bing.com.

This process led me to informational websites like the ones described above, websites of higher education state institutions (e.g., Alabama Commission On Higher Education <http://www.ache.alabama.gov/>, California Student Aid Commission <http://www.csac.ca.gov/>), state legal statutes (e.g., Louisiana <http://law.justia.com/codes/louisiana/2011/rs/title17/rs17-427-2>, Missouri <http://law.justia.com/codes/missouri/2015/title-xi/chapter-173>), state departments of education (e.g., Mississippi [http://www.mde.k12.ms.us/OTC/SLF\\$](http://www.mde.k12.ms.us/OTC/SLF$), Oklahoma <http://www.okhighered.org/state-system/overview/part3.shtml>), and state assistance authorities (e.g., North Carolina http://www.ncseaa.edu/pdf/FTNC_Current_Announcement.pdf). Finally, I complemented the information with reports from the National Association of State Student Grant and Aid Programs (NASSGAP).

⁴The budget allocated includes overhead costs. Therefore, the recipients did not necessarily receive all the money

Even when the search was conducted exclusively using the Internet, which appear in the mid 1990's, most historical records and documents have been digitalized and made available to the public. Thus, if a STEM program was implemented during the early 1980's or 1990's the probability to find it on the Internet is still very high.

Finally, the program-level data set includes the following variables: state, program name, program description, the first year of implementation, the last year observed, the average per-year expenditures, and the average per-year number of recipients. It is noteworthy that, for the programs with more than one year of information on expenditures and recipients, after adjusting for inflation⁵, I calculated the average across all available years. The program level data set can be found in this link. All the programs found were implemented as early as 1983; therefore, the time frame I use in this paper covers all the years between 1983 and 2016.

3.3.3 Examples of STEM Programs

There is a sizable variation across all programs regarding program types, assigned budgets, the number of participants and eligibility requirements. For example, in 2001 Alabama implemented the Mathematics and Science Scholarship-Loan Program for Alabama Teachers (MSSPAT) which is awarded to Alabama residents attending an Alabama university and seeking a teaching degree in math or science. Recipients must commit to teaching for five years in a critical shortage school.

In 2007, Arizona launched the Mathematics, Science, and Special Education Teacher Student Loan Program to defray in-state tuition, instructional materials and mandatory fees for students pursuing a teaching degree. Loans may be taken out for five years, and are forgiven on a one year of loan basis for one year of service in a public school teaching mathematics or science.

California implemented the Assumption Program of Loans for Education (APLE),

⁵In 2011 real USD

which is a competitive teacher incentive program that encourages outstanding students to become teachers in subject areas where teacher shortages have been identified. Examples of these fields are 7th to 12th grade mathematics and life/physical sciences. In order to be eligible for loan reimbursement, the prospective teacher must agree to teach in a public school for four years.

Finally, Colorado, in 2001 started the Colorado Loan Incentive for Teachers (LIFT) which is a program that offers up to \$2,000 in loan forgiveness per year, for up to four years for teachers who began teaching math, science, special education, or linguistically diverse education on or after June 11, 2001.

Without being comprehensive, the programs above give the reader a notion of the types of programs included in this study. Indeed, over the last three decades many states have implemented financial incentives aimed at increasing the supply of teachers. The complete list of all the programs can be downloaded [here](#).

3.3.4 Definition of Variables - Program Level Data

Since the program-level data includes the start and end years of each program as well as recipients and expenditures, for each program, I constructed the following vectors. The vector `expenditures` is a 1X34 row vector that includes the elements `expenditures_1983`, `expenditures_1984`, ..., `expenditures_2016`, which are equal to the average expenditure per program for the years between the start year and end year; 0 otherwise. Similarly, the vector `recipients`, is also a 1X34 row vector that includes the variables `recipients_1983`, `recipients_1984`, ..., `recipients_2016`; these elements are equal to the average number of recipients for the years the program was active and 0 otherwise. Since the data includes all the states regardless of whether or not they implemented STEM programs, I created a variable named `stem_program` which is equal to one if the state implemented at least one STEM program between 1983 and 2016 and 0 otherwise.

Based on the variable `program description`, I coded the variable `program type` which includes the following categories: 1) `loan forgiveness` is a financial aid program in which the recipient already has debt and the hiring institution will pay some or all the loan to the lender according to the program rules. Under this program type individuals are expected to teach in public schools, especially in shortage areas. 2) `scholarship`; this program type induces individuals into teaching by paying tuition costs to obtain teaching degrees. There is no obligation for the recipient to teach. 3) `scholarship loan`, this is the most commonly utilized program in which states finance college tuition and fees with the expectation that recipients will teach in public schools for a number of years after college graduation. If the recipient does not teach as agreed, the scholarship is converted into a loan which the recipient must pay. 4) `tuition reimbursement`, is a program type in which individuals are reimbursed part of all their college or graduate tuition costs with the expectation that the recipient will teach in a subject or geographic shortage area. Finally, 5) `salary bonus` is utilized to induce current teachers into shortage subject or geographic areas. Each program was assigned to only one category.

Again, based on the variable `program description`, I coded four dummy variables that provide information about the program `focus`. Unlike program type, one program could have more than one focus. The `program focus` variables are: `math & science`, `critical shortage area`, `low performing schools` and, `minorities`. These variables are equal to one if, in the program description these themes (e.g., math and science, low performing schools, etc) are mentioned; 0 otherwise.

In sum, the program-level data set includes the following variables: `state`, `program name`, `program description`, `start year`, `end year`, `duration`⁶, `average per-year recipients`, `average per-year expenditures` and the vectors `recipients` and `expenditures`.

⁶Duration = end year - start year.

3.3.5 Definition of Variables - State Level Data

Based on the program-level data, I created the state-level data by collapsing the variables at the state level. In this process, the variables program name and program description are no longer relevant. The variable program type was converted to dummy variables. The variable `star year` is the earliest year in which states implemented their first STEM program; `end year` is the latest year in which states have at least one active STEM program. The variable `duration` is the difference between `end year` and `start year`.

All the variables `recipients_1983`, ..., `recipients_2016`, `expenditures_1983`, ..., `expenditures_2016`, `average per-year recipients`, `average per-year expenditures`, `loan forgiveness`, `scholarship`, `scholarship-loan`, `tuition reimbursement`, `salary bonus`, `math and science`, `critical shortage area`, `low performing schools`, and `minorities` were collapsed from program level to state level by adding rows within each state.

Consequently, for example, the state-level variable `recipients_2000` represents the sum across all programs within each state of the average per-year number of recipients in 2000. Similarly, the variable `math and science` represents the number of different STEM programs focused on math and science each state has ever implemented.

To account for the size of the states' education systems, I combined the state-level data of STEM programs with information from the National Center for Education Statistics (NCES) - Common Core of Data (CCD). For each state-year between 1983 to 2016, I collected the following quantities: `number of students K-12`, `number of high school students`, `number of teachers` and `number of secondary school teachers`.

By combining the state-level STEM programs data with data from the CCD, I calculated the following variables per state-year: `recipients per 1,000 teachers`,

recipients per 1,000 secondary school teachers, recipients per 1,000 students, recipients per 1,000 high school students, expenditures per teacher, expenditures per secondary school teacher, expenditures per student and expenditures per high school student. This is the link to the state-level data.

3.4 STEM Programs Characteristics

3.4.1 STEM Program Characteristics - Variation Across Programs

In this section, I describe the characteristics of the STEM programs both at the program and state levels. Between 1983 and 2016, 41 states implemented at least one STEM program; in total 87 unique programs were found. As table (3.1) indicates, the most common program type is scholarship-loan (64%). This implies that most participants obtain financial aid to pay their college education, especially to become teachers. Once graduated, they are required to teach in a public school, most likely math and science and/or in a shortage geographic area. This incentive intends to increase the supply of new teachers by inducing individuals who are currently enrolled in college or who will start college, to become math and science teachers in public schools.

The second most common STEM program type is loan forgiveness (20%). This program type serves mainly current teachers who want to pay their student debt by teaching in a hard-to-staff school and/or teaching math and science.

Table 3.1: Distribution of Program Types. N=87

Program Type	N	%
Loan Forgiveness	17	20
Scholarship	7	8
Scholarship/Loan	56	64
Tuition Reimbursement	1	1
Salary Bonus	6	7
Total	87	100

Source: `STEM Programs - Program Level.dta`.

There are seven STEM programs that are scholarships, six salary bonuses and only one tuition reimbursement. Regarding program focus, table (3.2) includes the proportion of programs, that focus on math and science, critical shortage areas, low performing schools and minorities.

For instance, 47% of the programs focus on math and science; in other words, they are intended to induce individuals to teach mathematics and science. Also, 41% of the programs focus on critical shortage geographic areas. In fact, some programs focus on both. The third focus, minorities, as its name indicates, induces individuals from minority groups into teaching. Finally, only 4.6% of the programs focus on low performing schools. Notably, the California Assumption Program of Loans for Education, APLE, focuses on low performing schools as well as on math and science.

Table 3.2: Proportion of Programs with Specific Focus. N=87

Program Focus	Mean	Sd
Math and Science	0.47	0.50
Critical Shortage Areas	0.41	.50
Low Performing Schools	0.046	0.21
Minorities	0.10	0.31

Source: `STEM Programs - Program Level.dta`.

In table (3.3), I present combined information of program type and program focus. Of the 17 loan forgiveness programs, 65% focus on math and science and 47% focus on critical shortage areas. In addition, of the 56 scholarship-loan programs, 41%

emphasize math and science; 39% aim to increase teachers' supply in critical shortage areas; 9% focus on minorities and 3.6% focus on low performing schools. The two most popular program types - scholarship-loan and loan forgiveness - address shortages in math and science as well as shortages in critical geographic areas.

Table 3.3: Program Type vs Program Focus. N=87

Program Type	Math and Science	Critical Shortage Areas	Low Performing	Minorities	N
Loan Forgiveness	.65	.47	0	0	17
Scholarship	.29	.14	0	.57	7
Scholarship/Loan	.41	.39	.036	.089	56
Tuition Reimbursement	0	1	0	0	1
Salary Bonus	.83	.67	.33	0	6
Total	.47	.41	.046	.10	87

Source: STEM Programs - Program Level.dta.

In order to gauge the size of the programs in terms of the number of recipients, table (3.4) includes the distribution of program grantees across the 87 programs. As shown, 54% of the programs serve, on average, 250 or less individuals per year. The percentages above the first bracket is too scattered to note any pattern. Noticeably, there are three programs with 2,000 grantees or more and eight with no information about how many individuals they serve.

Moreover, between 1983 and 2016, some programs were implemented and eliminated. For instance, as figure (3.1) indicates, 15 programs were observed for a period of less than five years, 29 lasted less than 10 years; 8 were observed to last exactly 10 years; 22 programs lasted between 10 and 20 years, and, 18 programs lasted 20 years or more. In fact, 5 programs were observed throughout the entire observation period.

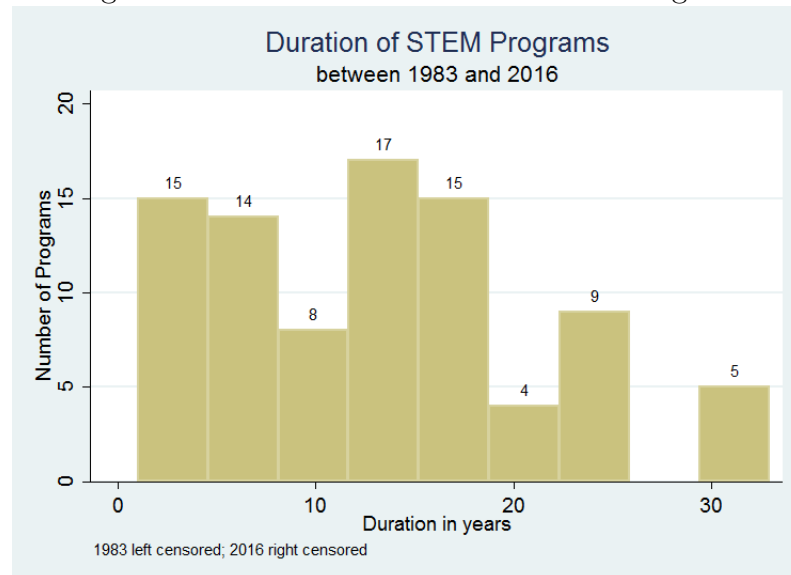
Importantly, 1983 and 2016 are the censoring years, which means that, even when some programs might have started before 1983, they are coded as starting in 1983. Similarly, there might be programs that are active after 2016; nevertheless, for those

Table 3.4: Average annual recipients. N=87

Average Annual Recipients	N	%
[0, 250]	47	54.02
(250,500]	14	16.09
(500,750]	2	2.30
(750,1000]	5	5.75
(1000,1250]	2	2.30
(1500,1750]	4	4.60
(1750,2000]	2	2.30
(2000,+]	3	3.45
missing	8	9.20
Total	87	100

Source: STEM Programs - Program Level.dta.

Figure 3.1: Duration in Years of STEM Programs



programs 2016 is the last year they were observed in the data.

Regarding when the programs were first and last observed, table (3.5) includes the distribution of start and end years for all the programs. There are 3 programs that started in 1983 or before. Also, in 1991, 11 programs started, and combining 2001, 2002 and 2003, 25 programs were born. These patterns require further explanation because starting in 2001, many states were interested in increasing teachers' labor supply. A potential reason might be the passing of the No Child Left Behind (NCLB) Law in 2001 which entered into effect in 2002. After this period, some states started STEM programs but not with the 2001-2003 intensity. Regarding the end year of the programs, 2014 seems to be the year in which many states decided to drop the support for these programs as 32 programs disappeared. In addition, 25 programs were last observed in 2016, which implies that they might still be active.

Table 3.5: Distribution of Start and End years. N=87

Year	Start	End	Year	Start	End
1983	3		2001	9	
1984	3		2002	9	
1985	1		2003	7	
1986	1		2004	2	2
1988	1		2005	3	1
1989		1	2006	3	
1990		1	2007	3	1
1991	11		2008	2	2
1992	1		2009	4	2
1993	1	1	2010		6
1994	2		2011	1	2
1995	2		2012	1	5
1996	1		2013	4	2
1997	1	1	2014	1	32
1998	3		2015		3
1999	3		2016		25
2000	4		Total	87	87

Source: STEM Programs - Program Level.dta.

An alternate measure of the programs' size is annual expenditures. Table (3.6) shows the distribution of annual expenditures across the 87 programs. All the monetary measures were inflation adjusted to represent 2011 real dollars. As table (3.6)

indicates, almost half of the programs spent 0.5 Million or less per year. There is a large variation across the programs regarding the budget allocated.

Table 3.6: Distribution of Annual Expenditures (2011 USD). N=87

Annual Expenditures (2011USD)	N	%
[0, \$0.5M]	41	47
(\$0.5M, \$1M]	9	10
(\$1M, \$1.5M]	8	9
(\$1.5M, \$2M]	4	5
(\$2M, \$2.5M]	4	5
(\$2.5M, \$3M]	5	6
(\$3M, \$3.5M]	2	2
(\$3.5M, \$4M]	1	1
(\$4M, \$4.5M]	2	2
(\$4.5M, 5M]	1	1
(\$5M, \$5.5M]	2	2
(\$5.5M, +]	8	9
Total	87	100

Source: STEM Programs - Program Level.dta.

3.4.2 STEM Program Characteristics - Variation Across States

The above mentioned tables and graphs refer to individual programs and do not account for the size of the different education systems. For instance, having 250 grantees per year is a large number for Wyoming but a very small for New York or California. In the following paragraphs, I discuss STEM program characteristics normalized to account for state by year number of teachers and students. By doing so, I compare STEM program characteristics across states.

Table (3.7) includes summary statistics of all the variables collapsed at the state level. For instance, **duration** was calculated as the difference between the latest year each state had an active STEM program minus the earliest year the same state had an active STEM program. Recall that some states have more than one STEM program and the start and end years of these programs within each state need not to coincide. Of the 34 observed years, the "life expectancy" of the programs is about 13 years. In

this case, both the mean and the median coincide.

Some states have been working during a long time in recruiting teachers through STEM programs. For instance, California, Delaware, Oklahoma and South Carolina all have at least one active STEM program for at least 30 years. Other states (and DC) don't have any STEM programs for the same time period: Alaska, DC, Michigan, Minnesota, Nevada, New Hampshire, New Jersey, Oregon, Rhode Island and Vermont.

The average number of **recipients** is about 892; however, the median is 136. This implies that there are some states that invest in their STEM programs very heavily. For example, New York's programs serve about 12,000 individuals per year, North Carolina's programs impact about 5,400 individuals, and California's program impacts about 4,800 individuals each year. The programs of Idaho, Wyoming and Hawaii impact less than 30 individuals per year.

The mean **expenditures** is about 4 Million and again, the median is about 0.5 Million. This implies that there are large states that invest heavily. The four states with the highest investments are New York, North Carolina, Iowa and California. It is important to consider that the budgets assigned might or might not include overhead costs, thus, not all the money is assigned to program participants.

The variables **recipients per 1,000 teachers**, **expenditures per teacher**, **recipients per 1,000 secondary teachers**, **expenditures per secondary teachers**, **recipients per 1,000 students**, **expenditures per student**, **recipients per 1,000 high school students**, and **expenditures per high school student** were calculated as follows. First, the state-level variables, **recipients_1983** ,..., **recipients_2016**, **expenditures_1983** ,..., **expenditures_2016** were divided by the corresponding (by year) measure of **teachers_1983** ,..., **teachers_2016**, **students_1983** ,..., **students_2016**, **secondary_ teachers_1983** ,..., **secondary_ teachers_2016**, and **high_school_ students_1983** ,..., **high_school_ students_2016**.

Next, for each state, I calculated the average (mean of elements in the row) of all non-missing values of the recipients per 1,000 teachers, expenditures per teacher, recipients per 1,000 secondary teachers), expenditures per secondary teachers, recipients per 1,000 students), expenditures per student, recipients per 1,000 high school students), and, expenditures per high school student. Table (3.7) includes the summary statistics using across-state variation. The resulting statistics account now for the education system size across states.

For instance, for every 1,000 teachers around 6 individuals participate in STEM programs, and, for each teacher, the expenditures in STEM programs is about \$30 USD. For every 1,000 secondary teachers, about 16 individuals participate in STEM programs. Also, for each secondary teacher, expenditures in STEM programs are about \$83, and for every 1,000 students, 0.37 individuals are STEM programs grantees. Additionally, for each student, states spend about \$2 in STEM programs and for each 1,000 high school students, about 1.27 individuals participate in STEM programs. Finally, for each high school student, states invest about \$7 in STEM programs.

Table 3.7: State Level Program Characteristics. N=87

	mean	sd	min	max	p10	p25	p50	p75	p90
Duration (years)	13	10	0	33	0	2	13	22	24
Annual recipients	892	2,025	0	12,229	0	23	136	807	2,686
Annual expenditures	\$4,098,705	\$8,056,020	\$0	\$45,100,000	\$0	\$144,050	\$538,598	\$4,623,649	\$9,365,375
Recipients / (1,000 teachers)	5.63	9.86	0.00	54.80	0.00	0.06	1.92	6.40	14.24
Expenditures / teacher	\$29.46	\$45.88	\$0.00	\$191.82	\$0.00	\$0.46	\$9.31	\$37.48	\$69.00
Recipients / (1,000 secondary teachers)	16.16	28.58	0.00	150.31	0.00	0.18	3.91	16.47	37.57
Expenditures / secondary teacher	\$83.18	\$131.86	\$0.00	\$523.63	\$0.00	\$1.50	\$23.19	\$130.69	\$232.66
Recipients / (1,000 students)	0.37	0.64	0.00	3.43	0.00	0.00	0.14	0.41	0.94
Expenditures / student	\$1.94	\$3.01	\$0.00	\$12.49	\$0.00	\$0.03	\$0.65	\$2.81	\$4.94
Recipients / (1,000 high school students)	1.27	2.30	0.00	12.91	0.00	0.01	0.48	1.44	3.40
Expenditure / high school student	\$6.66	\$10.49	\$0.00	\$45.37	\$0.00	\$0.12	\$2.18	\$9.31	\$17.45
Loan Forgiveness	0.33	0.48	0.00	1.00	0.00	0.00	0.00	1.00	1.00
Scholarship	0.14	0.35	0.00	1.00	0.00	0.00	0.00	0.00	1.00
Scholarship / Loan	1.10	1.20	0.00	5.00	0.00	0.00	1.00	2.00	2.00
Tuition Reimbursement	0.02	0.14	0.00	1.00	0.00	0.00	0.00	0.00	0.00
Salary Bonus	0.12	0.38	0.00	2.00	0.00	0.00	0.00	0.00	0.00
Math and Science	0.80	0.85	0.00	3.00	0.00	0.00	1.00	1.00	2.00
Critical Shortage Area	0.71	0.97	0.00	4.00	0.00	0.00	0.00	1.00	2.00
Low Performing Schools	0.08	0.34	0.00	2.00	0.00	0.00	0.00	0.00	0.00
Minorities	0.18	0.39	0.00	1.00	0.00	0.00	0.00	0.00	1.00

Source: STEM Programs - State Level.dta.

Furthermore, the variables `loan forgiveness`, `scholarship`, `scholarship-loan`, `tuition reimbursement`, and `salary bonus` were calculated as follows. When the data was at the program level, these variables were dummy variables. When the data was transformed to state-level, these dummies were collapsed by adding the values of these dummies within each state. If, for instance, a state has two programs in the `loan forgiveness` category, then its state-level value on the variable `loan forgiveness` is equal to 2.

As table (3.7) indicates, the average number of loan forgiveness programs is about 0.33. The corresponding statistics for `scholarship`, `scholarship-loan`, `tuition reimbursement`, and `salary bonus` are, respectively 0.14, 1.1, 0.02 and 0.12. Across all the states, the conclusion is the same, the most popular program type is `scholarship-loan`, followed by `loan forgiveness`.

The variables `math and science`, `critical shortage areas`, `low performing schools` and `minorities` were calculated similarly to the program type variables. When collapsing from program-level to state-level each variable was calculated as the sum of the dummy variables within each state. Across all states, the number of programs focused on math and science is about 0.8 and focusing on critical shortage areas is about 0.71.

Table (3.8) presents state-level information regarding STEM program characteristics. As mentioned, `start` refers to the earliest year in which each state implemented at least one STEM program. Some states have implemented STEM programs since the beginning of the observation period. For example, California (1983), Connecticut (1983), Delaware (1984), Pennsylvania (1984), Oklahoma (1985), South Carolina (1984) and Washington (1983). The states with the largest program size in terms of program recipients are: New York, North Carolina, California, Georgia and Florida. Regarding average annual expenditures the states with the largest investments are New York, North Carolina, Iowa, California, Mississippi, Georgia, Illinois, South Car-

olina and Utah.

Table 3.8: Across States Distribution of STEM Programs
Characteristics

State	Start	End	Duration	Recipients ⁷	Expenditures ⁸
Alabama	2001	2016	15	176	\$846,652
Alaska			0	0	\$0
Arizona	2011	2014	3	36	\$193,677
Arkansas	1994	2014	18	827	\$3,104,066
California	1983	2016	33	4,788	\$16,591,672
Colorado	2001	2008	7	248	\$443,040
Connecticut	1983	2014	12	383	\$3,059,283
Delaware	1984	2016	32	34	\$395,875
DC			0	0	\$0
Florida	1991	2014	19	3,109	\$4,580,539
Georgia	1995	2011	16	3,551	\$9,365,375
Hawaii	2002	2004	2	23	\$119,000
Idaho	1992	1993	1	28	\$84,864
Illinois	1986	2016	30	1,207	\$8,652,812
Indiana	1991	2014	23	214	\$317,478
Iowa	2001	2016	13	639	\$16,884,974
Kansas	2005	2007	2	36	\$191,500
Kentucky	1991	2014	23	1,550	\$4,623,649
Louisiana	2003	2005	2	33	\$244,000
Maine	1998	2014	16	340	\$1,099,799
Maryland	1991	2014	19	433	\$2,027,210
Massachusetts	1999	2016	17	807	\$6,403,656
Michigan			0	0	\$0
Minnesota			0	0	\$0
Mississippi	1991	2016	23	2,686	\$9,713,505
Missouri	2003	2015	11	300	\$538,598
Montana	2007	2016	9	120	\$364,000
Nebraska	2000	2012	12	0	\$3,429,000
Nevada			0	0	\$0
New Hampshire			0	0	\$0
New Jersey			0	0	\$0
New Mexico	1994	2016	22	40	\$144,050
New York	2000	2016	16	12,229	\$45,119,421
North Carolina	2001	2014	12	5,428	\$29,482,719
North Dakota	2001	2014	13	285	\$298,125
Ohio	2009	2016	7	0	\$6,825,000

⁷ Average annual recipients

⁸ Average annual expenditures

Oklahoma	1985	2016	31	136	\$494,325
Oregon			0	0	\$0
Pennsylvania	1984	2008	24	2,168	\$6,576,526
Rhode Island			0	0	\$0
South Carolina	1984	2016	32	1,405	\$8,651,061
South Dakota	2003	2016	13	117	\$1,859,350
Tennessee	1991	2014	23	293	\$1,290,925
Texas	2001	2016	15	820	\$3,899,257
Utah	1996	2016	18	465	\$8,426,077
Vermont			0	0	\$0
Virginia	2002	2014	12	277	\$928,490
Washington	1983	2014	23	87	\$386,308
West Virginia	1991	2014	23	64	\$269,828
Wisconsin	1998	2014	16	91	\$244,258
Wyoming	1997	2015	18	24	\$864,000

All monetary measures are presented in inflation adjusted in 2011 USD. The average annual recipients and expenditures were calculated as the average of non-zero quantities across row after transforming from Program Level to State Level data. Source: `STEM Programs - State Level.dta`.

Table (3.9) includes the number of programs by type that each state has implemented. As previously mentioned, the most popular type is scholarship-loan. For instance, North Carolina and Mississippi have implemented 5 scholarship-loan programs; Illinois, South Carolina and Missouri implemented 3 scholarship-loan programs. Other program types appear 2 times or less in each state.

Table 3.9: Across States Distribution of STEM Program Type

State	Loan For- giveness	Scholarship	Scholarship Loan	Tuition Reim- bursement	Salary bonus
Alabama	0	0	2	0	0
Alaska	0	0	0	0	0
Arizona	0	0	1	0	0
Arkansas	1	1	1	0	0
California	0	0	1	0	0
Colorado	1	0	0	0	0
Connecticut	0	1	1	0	0
Delaware	0	0	2	0	0
DC	0	0	0	0	0
Florida	0	0	2	1	0

Georgia	0	0	2	0	0
Hawaii	0	0	1	0	0
Idaho	0	0	1	0	0
Illinois	1	0	3	0	0
Indiana	0	1	0	0	0
Iowa	1	0	1	0	1
Kansas	0	0	1	0	0
Kentucky	0	1	1	0	0
Louisiana	0	0	0	0	1
Maine	0	1	0	0	0
Maryland	1	0	2	0	0
Massachusetts	0	0	2	0	0
Michigan	0	0	0	0	0
Minnesota	0	0	0	0	0
Mississippi	1	1	5	0	0
Missouri	0	0	3	0	0
Montana	1	0	0	0	0
Nebraska	0	0	1	0	0
Nevada	0	0	0	0	0
New Hampshire	0	0	0	0	0
New Jersey	0	0	0	0	0
New Mexico	1	0	1	0	0
New York	0	0	2	0	0
North Carolina	0	0	5	0	2
North Dakota	1	0	0	0	0
Ohio	1	0	0	0	1
Oklahoma	1	0	1	0	0
Oregon	0	0	0	0	0
Pennsylvania	1	0	1	0	0
Rhode Island	0	0	0	0	0
South Carolina	1	0	3	0	0
South Dakota	0	0	2	0	0
Tennessee	1	0	2	0	0
Texas	1	0	1	0	0
Utah	0	0	2	0	1
Vermont	0	0	0	0	0
Virginia	0	0	1	0	0
Washington	1	0	1	0	0
West Virginia	0	1	0	0	0
Wisconsin	1	0	0	0	0
Wyoming	0	0	1	0	0

Each cell represents the number of programs each state has from each program type. Source: STEM Programs - State Level.dta.

Regarding program focus, table (3.10) presents the distribution of programs by state. North Carolina and Mississippi have both three programs focused on increasing teacher labor supply in math and science, and, New York, Utah, Maryland, Delaware, Arkansas, Connecticut, Washington and Ohio have all two programs focused on math and science teachers. Mississippi is the state with the largest number of programs focused on critical shortage areas (4), followed by South Carolina (3), and, North Carolina, Arkansas, Illinois, South Dakota, Alabama, Iowa, Georgia, Florida and New Mexico have all two programs focused on critical shortage areas.

Table 3.10: Across States Distribution of STEM Program Focus

State	Math & Science	Critical Short- age Area	Low Performing Schools	Minorities
Alabama	1	2	0	0
Alaska	0	0	0	0
Arizona	1	0	0	0
Arkansas	2	2	0	1
California	1	0	1	0
Colorado	1	0	0	0
Connecticut	2	1	0	1
Delaware	2	0	0	0
DC	0	0	0	0
Florida	0	2	0	1
Georgia	0	2	0	0
Hawaii	1	0	0	0
Idaho	0	0	0	0
Illinois	1	2	0	1
Indiana	0	0	0	1
Iowa	1	2	0	0
Kansas	1	1	0	0
Kentucky	1	0	0	1
Louisiana	1	1	0	0
Maine	0	0	0	0
Maryland	2	0	0	0
Massachusetts	1	0	0	0
Michigan	0	0	0	0
Minnesota	0	0	0	0
Mississippi	3	4	0	0

Missouri	0	0	1	1
Montana	1	1	0	0
Nebraska	0	1	0	0
Nevada	0	0	0	0
New Hampshire	0	0	0	0
New Jersey	0	0	0	0
New Mexico	0	2	0	0
New York	2	1	0	0
North Carolina	3	2	2	1
North Dakota	1	1	0	0
Ohio	2	1	0	0
Oklahoma	1	1	0	0
Oregon	0	0	0	0
Pennsylvania	1	1	0	0
Rhode Island	0	0	0	0
South Carolina	1	3	0	0
South Dakota	1	2	0	0
Tennessee	0	0	0	1
Texas	1	1	0	0
Utah	2	0	0	0
Vermont	0	0	0	0
Virginia	1	0	0	0
Washington	2	0	0	0
West Virginia	0	0	0	0
Wisconsin	0	0	0	0
Wyoming	0	0	0	0

Each cell represents the number of programs each state has from each program focus. Source: **STEM Programs - State Level.dta**.

When STEM program characteristics are normalized to account for the size of the states' education systems the results are presented in table (3.11). Mississippi, South Carolina, New York, California and Kentucky are the states with the largest ratio of STEM program recipients per 1,000 teachers. In addition, the states with the highest ratio of STEM program expenditures per teacher are Mississippi, Iowa, South Carolina, Utah and New York. The states with the largest ratio of STEM program recipients per 1,000 students are Mississippi, New York, South Carolina, North Dakota and Kentucky. Finally, the states with the largest ratio of STEM program expenditures per student are Iowa, Mississippi, South Carolina, New York and Wyoming.

Table 3.11: Across States Distribution of STEM Program
Normalized Characteristics

State	Recip. / 1000 teachers	Exp / teacher	Recip. / 1000 sec teachers	Exp / sec teach- ers	Recip. / 1000 stu- dents	Exp / student	Recip. / 1000 hs stu- dents	Exp / hs students
Alabama	1.18	\$5.82	3.21	\$15.95	0.08	\$0.39	0.27	\$1.34
Alaska	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
Arizona	0.09	\$0.46	0.29	\$1.56	0.00	\$0.02	0.01	\$0.07
Arkansas	5.80	\$23.69	13.74	\$55.68	0.41	\$1.68	1.44	\$5.84
California	19.26	\$66.76	67.14	\$232.66	0.86	\$2.96	2.97	\$10.30
Colorado	1.27	\$2.26	2.65	\$4.74	0.08	\$0.13	0.26	\$0.46
Connecticut	2.79	\$21.58	6.87	\$50.41	0.21	\$1.60	0.70	\$5.44
Delaware	2.17	\$36.51	4.33	\$72.94	0.14	\$2.37	0.48	\$8.03
DC	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
Florida	11.35	\$14.50	29.99	\$38.27	0.66	\$0.86	2.35	\$3.01
Georgia	14.24	\$37.48	35.00	\$92.13	0.94	\$2.47	3.40	\$8.95
Hawaii	0.18	\$0.95	0.39	\$2.01	0.01	\$0.06	0.04	\$0.20
Idaho	0.14	\$0.42	0.30	\$0.90	0.01	\$0.02	0.02	\$0.07
Illinois	6.40	\$48.31	22.80	\$172.22	0.40	\$3.00	1.38	\$10.43
Indiana	2.57	\$3.81	5.85	\$8.66	0.15	\$0.22	0.51	\$0.75
Iowa	4.46	\$173.78	13.13	\$498.67	0.32	\$12.49	1.05	\$40.67
Kansas	0.09	\$0.49	0.18	\$0.98	0.01	\$0.04	0.02	\$0.12
Kentucky	17.73	\$58.70	70.09	\$228.25	1.10	\$3.63	3.87	\$12.78
Louisiana	0.06	\$0.45	0.20	\$1.50	0.00	\$0.03	0.02	\$0.12
Maine	10.50	\$34.01	33.25	\$107.69	0.87	\$2.81	2.80	\$9.07
Maryland	3.14	\$15.59	7.52	\$37.08	0.21	\$1.02	0.69	\$3.42
Massachusetts	6.00	\$47.65	16.47	\$130.69	0.44	\$3.51	1.49	\$11.83
Michigan	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00

Minnesota	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
Mississippi	54.80	\$191.82	150.31	\$523.63	3.43	\$12.05	12.91	\$45.37
Missouri	1.06	\$2.06	2.19	\$4.26	0.08	\$0.15	0.25	\$0.50
Montana	3.41	\$10.35	10.90	\$33.07	0.25	\$0.75	0.82	\$2.48
Nebraska	0.00	\$60.70	0.00	\$162.22	0.00	\$4.51	0.00	\$14.58
Nevada	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
New Hampshire	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
New Jersey	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
New Mexico	1.00	\$3.81	3.59	\$13.79	0.06	\$0.25	0.22	\$0.84
New York	28.70	\$105.66	75.15	\$276.67	2.18	\$8.04	7.33	\$27.00
North Carolina	10.61	\$68.07	30.32	\$193.25	0.71	\$4.60	2.49	\$16.00
North Dakota	14.18	\$14.82	37.57	\$39.29	1.18	\$1.23	3.69	\$3.86
Ohio	0.00	\$10.61	0.00	\$23.19	0.00	\$0.66	0.00	\$2.18
Oklahoma	2.80	\$7.24	6.52	\$16.76	0.18	\$0.47	0.65	\$1.70
Oregon	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
Pennsylvania	6.69	\$20.35	15.40	\$46.84	0.40	\$1.23	1.38	\$4.21
Rhode Island	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
South Carolina	28.84	\$158.24	90.23	\$496.88	1.83	\$10.09	6.42	\$35.44
South Dakota	3.77	\$36.80	13.89	\$137.02	0.28	\$2.70	0.94	\$9.31
Tennessee	2.42	\$11.21	8.69	\$40.26	0.15	\$0.71	0.54	\$2.51
Texas	0.99	\$4.71	2.51	\$11.94	0.07	\$0.32	0.24	\$1.14
Utah	10.88	\$112.80	26.87	\$279.18	0.48	\$4.94	1.66	\$17.53
Vermont	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00	0.00	\$0.00
Virginia	1.26	\$4.22	2.34	\$7.82	0.09	\$0.29	0.28	\$0.95
Washington	1.21	\$5.37	2.95	\$13.14	0.06	\$0.27	0.20	\$0.90
West Virginia	2.21	\$9.31	5.69	\$23.93	0.16	\$0.65	0.52	\$2.21
Wisconsin	0.77	\$2.06	1.97	\$5.29	0.05	\$0.14	0.16	\$0.44

Wyoming	1.92	\$69.00	3.91	\$140.78	0.15	\$5.39	0.48	\$17.45
---------	------	---------	------	----------	------	--------	------	---------

All monetary measures are presented in inflation adjusted in 2011 USD. All measures were calculated as the average of non-zero quantities across row after transforming from Program Level to State Level data. Source: **STEM Programs - State Level.dta**.

3.4.3 STEM Program Characteristics - Variation Across States: Maps

The following analyses rely on maps generated using state-level information. Figure (3.2) shows across-state variation in duration, recipients and expenditures. For duration, the pattern is clear; the states with the largest duration of STEM programs are Illinois, Indiana, Kentucky, Tennessee, Mississippi, South Carolina, West Virginia, Pennsylvania, Oklahoma, California and Washington. Interestingly, most states are near each other. Regarding annual recipients, the states with the greatest number of STEM program recipients are located in the South (Texas) and South East as well as California. Finally, in terms of expenditures, the states with the highest investments in STEM programs are located in the East and South East.

Figure (3.3) shows the between-state variation in program type. For instance, states with loan forgiveness STEM programs are located in the North (Oregon, Montana, North Dakota), the South (New Mexico, Texas, Oklahoma, Arkansas, Colorado, Tennessee and Mississippi), as well as in the Midwest (Wisconsin, Illinois, Iowa, Ohio), and one state in the East (Pennsylvania). The states that concentrate most of the scholarship-loan STEM programs are Illinois, Missouri, Mississippi, North Carolina and South Carolina. This finding is important since most of the STEM programs are scholarship-loans. Tuition reimbursement programs are located in Florida and scholarships are located in Indiana, Kentucky, West Virginia, Arkansas, Mississippi and Maine.

Figure (3.4) shows the variation across states in terms of program focus. Notably, most states have at least one program focused on math and science. Some states have more than one program devoted to attract math and science teachers (Washington, Utah, Arkansas, Mississippi, Ohio, North Carolina and New York). Conversely, the states that invest in STEM programs focused on critical shortage areas are concentrated mainly in the South-East and in the Mid-West. STEM Programs with focus on low performing schools are scarce, and are located in California, Missouri and

North Carolina. Finally, states with STEM Programs aimed to attract minorities into teaching are located in the Mid-West.

In terms of the normalized number of recipients, figure (3.5) shows the between-state variation in four measures: recipients per 1,000 teachers, recipients per 1,000 secondary teachers, recipients per 1,000 students and recipients per 1,000 high school students. In the four cases, the states with the highest number of recipients per unit of either teachers or students are located in the East and South-East, although there are also states in the West like California and Utah.

Finally, regarding the normalized version of expenditures, figure (3.6) shows the between state variation in expenditures per teacher, expenditures per secondary teacher, expenditures per student and expenditures per high school student. In the four cases, the states that invest the heaviest are located across all the mid-range of the US (California, Utah, Wyoming, Nevada, Iowa, Illinois, Kentucky, Missouri, North Carolina and South Carolina and New York).

Figure 3.2: Across-State variation on: Duration, Recipients and Expenditures

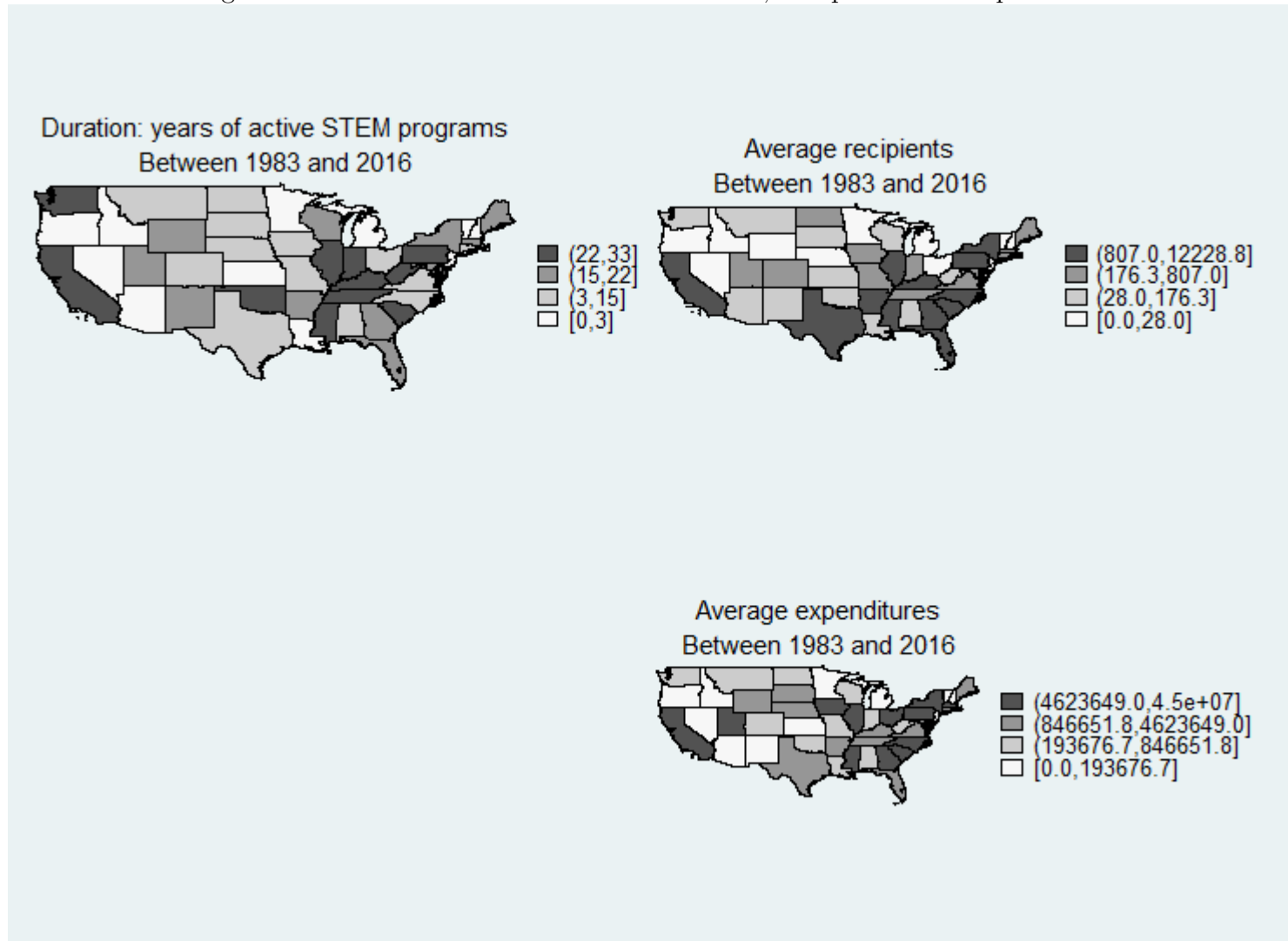


Figure 3.3: Across-State variation on: Program Type

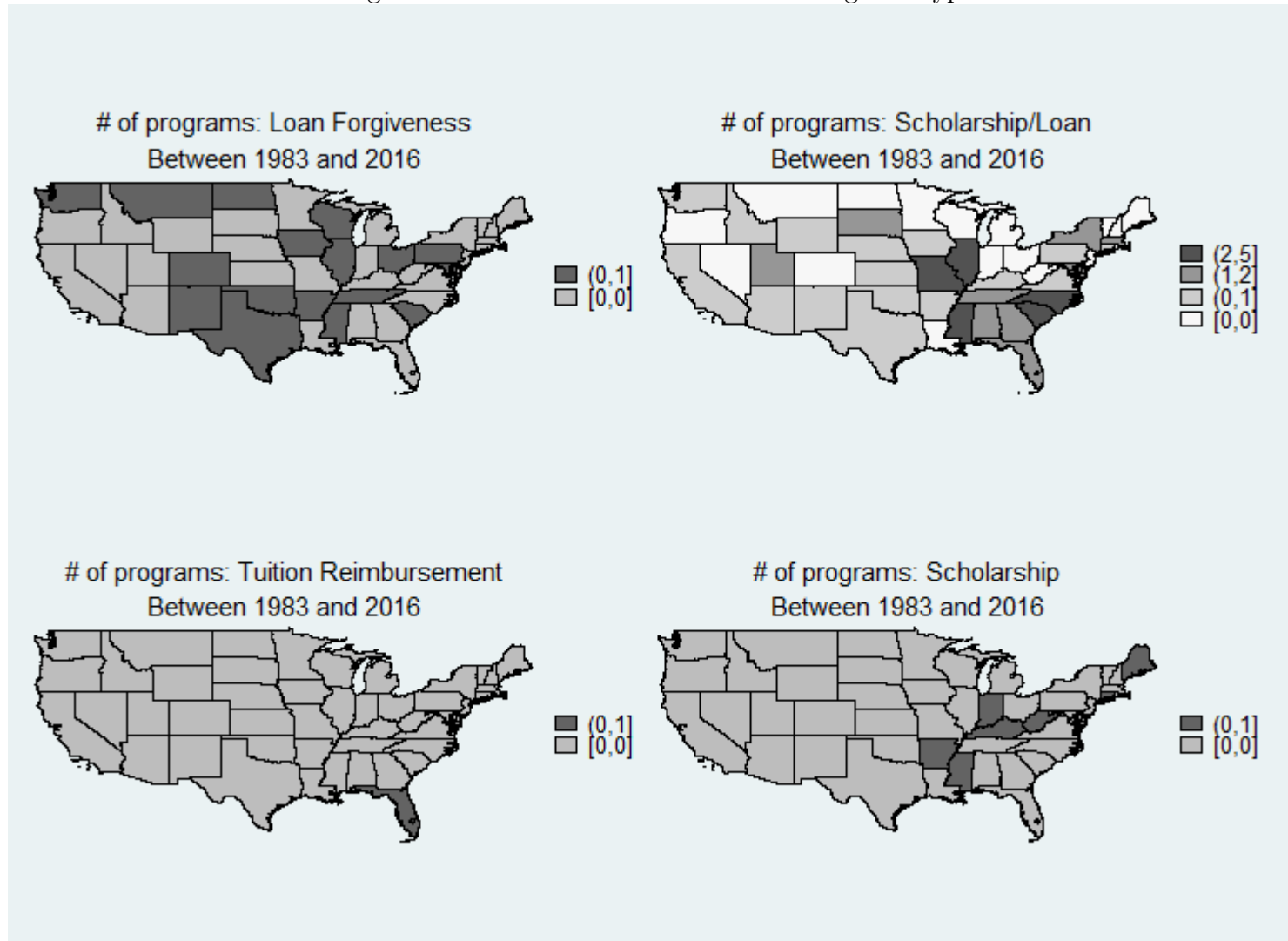


Figure 3.4: Across-State variation on: Program Focus

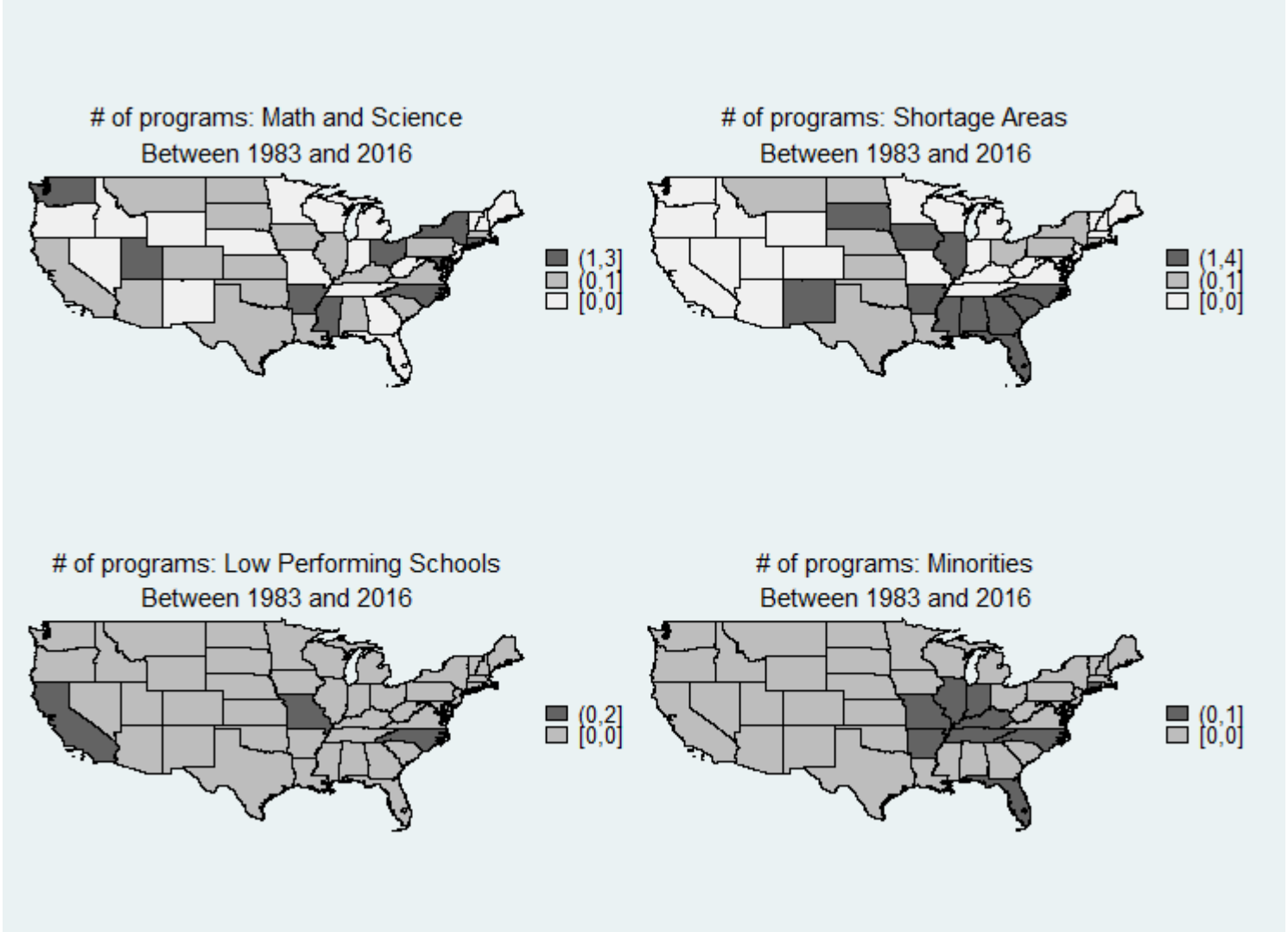


Figure 3.5: Across-State variation on: Recipients

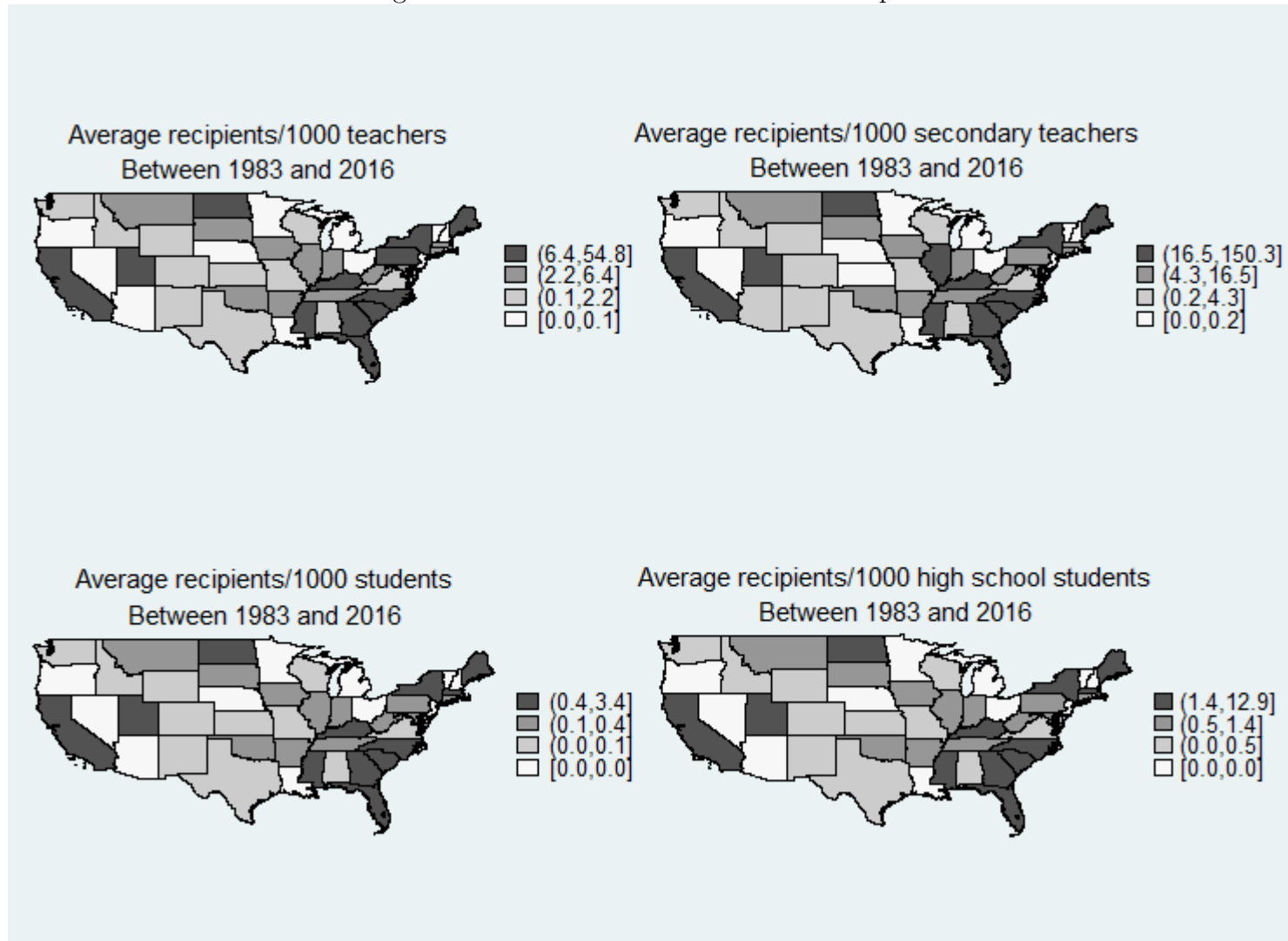
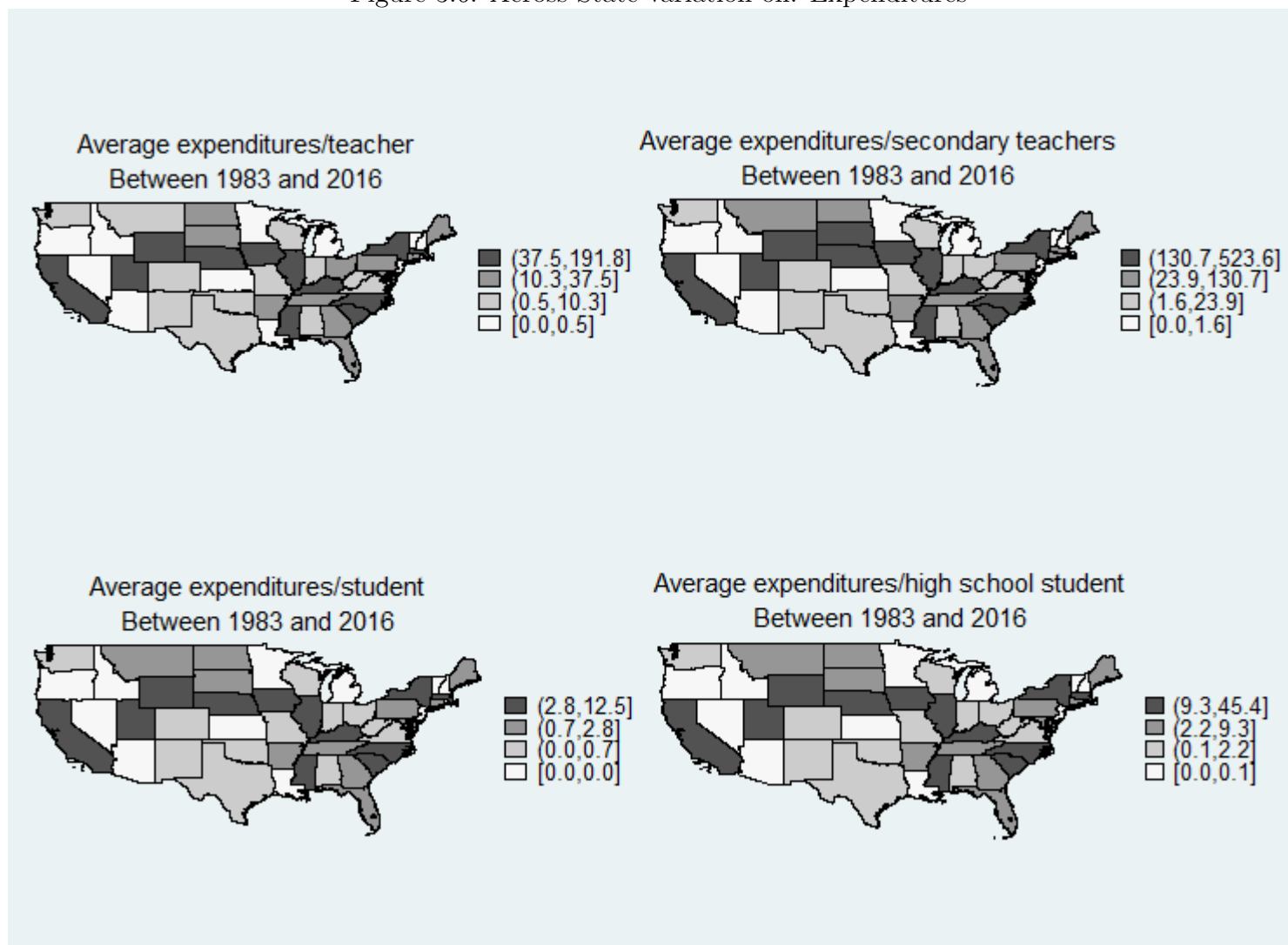


Figure 3.6: Across-State variation on: Expenditures



3.5 Conclusions

This study serves two purposes. First, to inform the reader the process of gathering the data and coding the variables of financial incentives aimed to recruit and retain teachers in STEM fields and/or critical shortage areas. In doing so, I encourage researchers to utilize and improve the data that is available to the public. A contribution derived from this purpose is the promotion of research that involves financial incentives of the nature described in this study. A suggested set of research questions is offered in section (3.1).

Second, this study provides a comprehensive (national) view of all the financial incentive programs aimed to recruit and retain teachers in STEM fields. The most popular type of program is scholarship-loan and the two main focus are math and science and critical shortage areas. There is a sharp variation on states' approaches to solve teacher shortages in math and science and/or critical shortage areas. On the one hand, some states invest heavily and for a long period of time on these programs whereas other states haven't even started.

Geographically, there are some interesting patterns to discuss. The states with the highest expenditures per unit of education system (e.g., students, teachers) are located across the center of the US and are adjacent in most cases. In contrast, the states with the largest ratios of recipients per unit of education system are mostly located in the East Coast and California. Finally, whereas loan forgiveness programs are located in the South and North, the Scholarship-Loan programs prevail in the South-East.

Indeed, more research is needed to assess the role of financial incentive programs for teachers on a number of education outcomes. Large sums of money are invested in a yearly basis and programs are born and disappear often. Nevertheless, the decisions to implement or eliminate programs could be done more systematically

if these programs were evaluated appropriately in terms of the marginal costs and marginal benefits to individuals and society.

3.6 References

- [1] Aragon, S. (2016). Mitigating Teacher Shortages: Financial Incentives. *Education Commission of the States*, Teacher Shortages Series, May 2016.
- [2] Arfin, D. (1986). The Use of Financial Aid to Attract Talented Students to Teaching: Lessons from Other Fields. *The Elementary School Journal*, Vol. 86, No. 4, Special Issue: Policy Initiatives for Developing a Teaching Profession (Mar., 1986), pp. 404-423
- [3] Berry, B. & Hirsh, E. (2005). Recruiting and Retaining Teachers for Hard-to-Staff Schools. Issue Brief, NGA Center for Best Practices. October 27, 2005.
- [4] Chin, E., Young, J. & Floyd, B. (2004). Placing Beginning Teachers in Hard-to-Staff Schools: Dilemmas Posed by Alternative Certification Programs. Paper presented at the American Association of Colleges of Teacher Education (AACTE). Chicago, Illinois, February 9, 2004
- [5] Clotfelter, C., Glennie, E., Ladd, H., & Vigdor, J. (2007). Would higher salaries keep teachers in high-poverty schools? Evidence from a policy intervention in North Carolina. *Journal of Public Economics* 92 (2008) 13521370
- [6] Clotfelter, C., Glennie, E., Ladd, H., & Vigdor, J. (2008). Teacher Bonuses and Teacher Retention in Low-Performing Schools Evidence from the North Carolina \$1,800 Teacher Bonus Program. *Public Finance Review*, Volume 36 Number 1, January 2008 63-87
- [7] Darling-Hammond, L. & Hudson, L. (1990). Precollege Science and Mathematics Teachers: Supply, Demand, and Quality. *Review of Research in Education*, 16 (1990) 223-264.
- [8] Darling-Hammond, L. & Skyes, G. (2003). Wanted: A National Teacher Supply Policy for Education: The Right Way to Meet The "Highly Qualified Teacher" Challenge. *Education Policy Analysis Archives*, 11(33).
- [9] Feng, L. & Sass, T. (2015). The Impact of Incentives to Recruit and Retain Teachers in Hard-to-Staff Subjects: An Analysis of the Florida Critical Teacher Shortage Program. National Center for Analysis of Longitudinal Data in Education Research, Working Paper 141, September 2015.
- [10] Guthrie, J. & Zusman, A. (1982). Teacher Supply and Demand in Mathematics and Science. *The Phi Delta Kappan*, 64 (1982) 28-33.
- [11] Ingersoll, R. & Perda, D. (2010). Is the Supply of Mathematics and Science Teachers Sufficient? *American Educational Research Journal*, 47 (3) 563-594.

- [12] Moin, L., Dorfield, J., & Schunn, C. (2005). Where Can We Find Future K-12 Science and Math Teachers? A Search by Academic Year, Discipline, and Academic Performance Level. 2005 Wiley Periodicals, Inc.
- [13] Murphy, P., DeArmond, M., & Guin, K. (2003). A National Crisis or Localized Problems? Getting Perspective on the Scope and Scale of the Teacher Shortage. *Education Policy Analysis Archives*. 11(23).
- [14] NCCTQ (2007). Recruiting Quality Teachers in Mathematics, Science and Special Education for Urban and Rural Schools. National Comprehensive Center for Teacher Quality, 2007.
- [15] Podolsky, A. and Kini, T. (2016). How Effective are Loan Forgiveness and Service Scholarships for Recruiting Teachers? Learning Policy Institute, Policy Brief, 2016.
- [16] Steele, J., Murnane, R. & Willett, J. (2010). Do Financial Incentives Help Low-Performing Schools Attract and Keep Academically Talented Teachers? Evidence from California. *Journal of Policy Analysis and Management*, Vol. 29, No. 3, 451478 (2010)
- [17] Zinth, K. (2008). High School Level STEM Initiatives: State Recruitment Efforts for STEM Teachers. *Education Commission of the States*, June 2008.

APPENDICES

APPENDIX A

Two-Dimensional Crank-Nicolson Scheme for a Uniform Spherical Grid

Table A.1: **First Stage:** Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on **advanced mathematics credits** controlling for demographics, household characteristics, high school math graduation requirements, and state and cohort fixed effects.

	(1) expo years	(2) expo recipients	(3) expo expend.	(4) expo re- cip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.301*** (0.0154)	0.0266*** (0.00590)	1.62e-06*** (4.25e-07)	-0.00625 (0.146)	-0.00210 (0.0284)
in*state 5	-0.510*** (0.0142)	0.000699*** (0.000151)	7.23e-08*** (1.73e-08)	0.0998*** (0.0226)	0.0285*** (0.00701)
in*state 6	-0.255*** (0.0309)	- (0.000165)	-5.57e-07*** (9.21e-08)	-0.0483*** (0.00647)	-0.0268*** (0.00392)
in*state 7	0.224*** (0.0359)	0.00282*** (0.000578)	6.74e-07*** (1.37e-07)	0.107*** (0.0217)	0.0257*** (0.00561)
in*state 8	-0.404*** (0.0160)	0.0102*** (0.00336)	1.48e-06*** (4.82e-07)	0.0711*** (0.0256)	0.0137*** (0.00428)
in*state 10	-0.288*** (0.0153)	9.31e-05 (9.28e-05)	2.72e-08 (2.71e-08)	0.00183 (0.0199)	0.00222 (0.00440)
in*state 11	-0.209*** (0.0102)	0.000112*** (2.39e-05)	4.19e-08*** (8.88e-09)	0.0105*** (0.00237)	0.00399*** (0.000938)
in*state 14	-0.446*** (0.0144)	-0.000605 (0.000559)	-1.29e-07 (1.18e-07)	-0.402*** (0.0634)	-0.0319*** (0.0114)
in*state 15	-0.416*** (0.0151)	0.0161*** (0.00337)	4.08e-06*** (8.98e-07)	0.0524 (0.326)	0.0294 (0.239)
in*state 18	-0.327*** (0.0167)	- (4.90e-05)	-1.23e-07*** (2.08e-08)	-0.0128*** (0.00168)	- (0.000772)
in*state 19	0.0233 (0.0594)	0.00121 (0.00241)	1.64e-07 (3.24e-07)	-0.000307 (0.110)	0.000790 (0.0160)
in*state 20	-0.751***	0.00914***	8.74e-07***	-1.351***	-0.405***

	(0.0188)	(0.00216)	(2.73e-07)	(0.175)	(0.0525)
in*state 21	-0.230***	0.000351	8.41e-08	0.0125	0.00323
	(0.0157)	(0.000245)	(5.83e-08)	(0.0130)	(0.00339)
in*state 25	-0.297***	0.00148***	1.62e-07***	-0.0717***	0.00304***
	(0.0329)	(0.000300)	(3.25e-08)	(0.00555)	(0.00100)
in*state 26	-0.244***	-	-6.26e-07**	-0.0629***	-0.0476***
	(0.0629)	(0.000313)	(2.41e-07)	(0.0198)	(0.0165)
in*state 32	-0.241***	0.0560***	5.26e-06***	0.00767	0.000574
	(0.0164)	(0.0120)	(1.25e-06)	(0.300)	(0.0817)
in*state 33	-0.106***	-7.56e-06**	-2.08e-09**	-	-
	(0.0329)	(3.34e-06)	(8.98e-10)	(0.000643)	(0.000184)
in*state 34	0.147***	6.85e-05***	1.91e-08***	0.00533***	0.00154***
	(0.0315)	(1.60e-05)	(4.02e-09)	(0.00132)	(0.000362)
in*state 35	-0.0510*	-0.000150	-1.44e-07	-0.00181*	-0.00168*
	(0.0268)	(0.000127)	(1.21e-07)	(0.000967)	(0.001000)
in*state 37	-0.161***	0.00201*	1.85e-07*	0.0752*	0.00603
	(0.0184)	(0.00112)	(1.03e-07)	(0.0416)	(0.00421)
in*state 39	-0.293***	-1.19e-05	-3.63e-09	0.000859	0.000249
	(0.0157)	(3.81e-05)	(1.25e-08)	(0.00358)	(0.00126)
in*state 41	-0.0244***	0.000955***	6.97e-08***	0.0530***	0.00319***
	(0.00255)	(0.000175)	(1.38e-08)	(0.0111)	(0.000737)
in*state 42	-0.249***	-	-5.23e-07***	-0.0335***	-
	(0.0626)	(0.00115)	(1.94e-07)	(0.0108)	(0.00196)
in*state 43	-0.240***	-0.000243	-5.75e-08	-0.0220*	-0.00503*
	(0.0178)	(0.000219)	(5.11e-08)	(0.0114)	(0.00291)
in*state 44	-0.0418	-3.72e-05	-7.86e-09	-0.0204	-0.00408
	(0.0371)	(6.13e-05)	(1.28e-08)	(0.0164)	(0.00373)
in*state 45	-0.427***	0.0233***	1.16e-06***	0.895***	0.0275***
	(0.0213)	(0.00119)	(5.87e-08)	(0.0479)	(0.00153)
in*state 47	0.184***	0.000731***	2.18e-07***	0.0629***	0.0191***
	(0.0398)	(0.000177)	(5.22e-08)	(0.0154)	(0.00486)
in*state 48	-0.474***	0.0389***	3.20e-06***	-1.039***	-0.242***
	(0.0136)	(0.00833)	(7.48e-07)	(0.321)	(0.0797)
in*state 49	-0.561***	0.0515***	4.27e-06***	-8.624***	-1.991***

	(0.0125)	(0.0113)	(1.07e-06)	(1.122)	(0.259)
in*state 50	-0.179***	-0.00177*	-6.65e-07*	-0.137**	-0.0503**
	(0.0660)	(0.000920)	(3.40e-07)	(0.0525)	(0.0209)
in*state 51	-0.419***	0.144***	1.51e-06***	-7.524***	-0.203***
	(0.0345)	(0.0299)	(3.26e-07)	(0.985)	(0.0266)
female	0.134***	0.135***	0.135***	0.135***	0.135***
	(0.0390)	(0.0391)	(0.0391)	(0.0386)	(0.0388)
white	0.235***	0.234***	0.233***	0.232***	0.233***
	(0.0484)	(0.0489)	(0.0492)	(0.0495)	(0.0497)
age as of 12-31-1996	-0.351***	-0.341***	-0.342***	-0.359***	-0.360***
	(0.0374)	(0.0470)	(0.0467)	(0.0437)	(0.0459)
hh gross income 1996-1999	6.00e-06***	5.96e-06***	5.96e-06***	5.94e-06***	5.96e-06***
	(1.90e-06)	(1.91e-06)	(1.91e-06)	(1.86e-06)	(1.87e-06)
hh income poverty ratio 1996-1999	-0.0249	-0.0245	-0.0244	-0.0233	-0.0238
	(0.0273)	(0.0276)	(0.0276)	(0.0264)	(0.0266)
hh size 1997	-0.0564***	-0.0568***	-0.0568***	-0.0570***	-0.0571***
	(0.0146)	(0.0149)	(0.0149)	(0.0147)	(0.0147)
both bio parents	0.356***	0.358***	0.358***	0.355***	0.355***
	(0.0371)	(0.0368)	(0.0369)	(0.0373)	(0.0373)
expo re-forms	-0.118***	-0.131***	-0.130***	-0.134***	-0.133***
	(0.0300)	(0.0264)	(0.0264)	(0.0256)	(0.0259)
high school grad req 1997	-0.553***	7.185***	2.890***	-53.21***	-51.66***
	(0.0717)	(1.448)	(0.575)	(7.004)	(6.809)
Constant	9.125***	-21.92***	-4.725	219.9***	213.7***
	(0.682)	(6.396)	(2.923)	(28.51)	(27.77)
N	4,219	4,219	4,219	4,219	4,219
math	1.026	1.026	1.026	1.026	1.026
AP F stat	11.39	101.89	95.8	115.54	106.9
p-value	0.0015	0.000	0.0000	0.0000	0.0000

State-level clustered robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions in which the dependent variable is **advanced math credits**; the treatment variables are the interactions of **in** and state dummies. in1-in5 measure potential years of exposure to: (1) STEM programs, (2) recipients, (3) expenditures, (4) recipients/1,000 teachers, and (5) expenditure/teacher. The F-stat tests the null hypothesis of joint significance of the coefficients of the interactions of *in* and state dummies. All regressions use the 1997 weight.

Table A.2: **First Stage:** Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on **Total Credits** controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects.

	(1) expo years	(2) expo recipients	(3) expo expend.	(4) expo re- cip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.335*** (0.125)	0.0165 (0.0246)	-1.55e-06 (1.73e-06)	-6.416*** (0.766)	-1.142*** (0.148)
in*state 5	0.617*** (0.0798)	0.000699 (0.000645)	1.72e-08 (7.26e-08)	0.365*** (0.115)	0.109*** (0.0354)
in*state 6	-0.999*** (0.137)	- (0.000725)	-2.12e-06*** (4.05e-07)	-0.194*** (0.0320)	-0.111*** (0.0191)
in*state 7	0.853*** (0.167)	0.0112*** (0.00269)	2.67e-06*** (6.41e-07)	0.390*** (0.111)	0.0891*** (0.0282)
in*state 8	1.345*** (0.108)	0.0788*** (0.0185)	1.13e-05*** (2.67e-06)	0.562*** (0.139)	0.104*** (0.0227)
in*state 10	-0.399*** (0.121)	0.000118 (0.000429)	3.10e-08 (1.26e-07)	0.0370 (0.0976)	-0.0105 (0.0214)
in*state 11	-0.0448 (0.0713)	0.000557*** (0.000130)	2.08e-07*** (4.85e-08)	0.0502*** (0.0127)	0.0182*** (0.00496)
in*state 14	0.530*** (0.0759)	0.00388 (0.00254)	8.08e-07 (5.38e-07)	-0.318 (0.349)	-0.0459 (0.0589)
in*state 15	1.070*** (0.0816)	0.0178 (0.0144)	2.33e-06 (3.78e-06)	2.283 (1.552)	1.716 (1.131)
in*state 18	0.271** (0.131)	0.000352 (0.000233)	1.48e-07 (9.93e-08)	0.00785 (0.00890)	0.00284 (0.00403)
in*state 19	2.068*** (0.296)	0.0667*** (0.0120)	8.99e-06*** (1.62e-06)	2.979*** (0.580)	0.389*** (0.0825)
in*state 20	0.153 (0.102)	0.00856 (0.00927)	-1.39e-07 (1.16e-06)	-14.83*** (0.822)	-4.341*** (0.242)
in*state 21	-0.216* (0.127)	-0.000405 (0.00119)	-1.03e-07 (2.84e-07)	-0.0540 (0.0684)	-0.0127 (0.0175)
in*state 25	2.249*** (0.283)	0.00209 (0.00131)	2.23e-07 (1.43e-07)	-0.647*** (0.0373)	-0.0137*** (0.00493)

in*state 26	-1.496*** (0.351)	- (0.00167)	-3.82e- 06*** (1.28e-06)	-0.387*** (0.111)	-0.310*** (0.0900)
in*state 32	-0.427*** (0.125)	0.0304 (0.0502)	-5.33e-06 (5.08e-06)	-10.27*** (1.454)	-2.536*** (0.397)
in*state 33	0.555*** (0.175)	5.25e- 05*** (1.66e-05)	1.41e- 08*** (4.50e-09)	0.00863** (0.00345)	0.00224** (0.000979)
in*state 34	0.438*** (0.156)	0.000189** (7.85e-05)	4.54e-08** (2.04e-08)	0.0122* (0.00665)	0.00270 (0.00182)
in*state 35	0.592*** (0.130)	0.00229*** (0.000620)	2.18e- 06*** (5.91e-07)	0.0145*** (0.00526)	0.0133** (0.00535)
in*state 37	2.026*** (0.151)	-0.00932* (0.00539)	-8.77e-07* (4.97e-07)	-0.468** (0.218)	-0.0506** (0.0216)
in*state 39	-0.186 (0.127)	0.000437*** (0.000155)	1.46e- 07*** (5.07e-08)	0.0608*** (0.0184)	0.0207*** (0.00646)
in*state 41	-0.277*** (0.0130)	0.00719*** (0.000793)	5.47e- 07*** (6.29e-08)	0.421*** (0.0531)	0.0254*** (0.00353)
in*state 42	0.665 (0.399)	0.0109 (0.00667)	1.82e-06 (1.12e-06)	0.0691 (0.0642)	0.0102 (0.0112)
in*state 43	1.118*** (0.150)	0.00253** (0.00102)	5.87e-07** (2.38e-07)	0.103* (0.0601)	0.0219 (0.0151)
in*state 44	-0.432** (0.175)	-0.000427 (0.000290)	-9.14e-08 (6.07e-08)	-0.187** (0.0858)	-0.0417** (0.0192)
in*state 45	1.193*** (0.109)	0.0483*** (0.00556)	2.41e- 06*** (2.75e-07)	1.752*** (0.260)	0.0535*** (0.00816)
in*state 47	0.456** (0.222)	0.00204** (0.000931)	5.99e-07** (2.78e-07)	0.140* (0.0820)	0.0384 (0.0255)
in*state 48	0.347*** (0.0650)	0.0354 (0.0355)	4.88e-08 (3.13e-06)	4.770*** (1.658)	1.137*** (0.406)
in*state 49	-0.547*** (0.0819)	0.0345 (0.0486)	-3.17e-06 (4.54e-06)	-95.58*** (5.282)	-21.53*** (1.198)
in*state 50	-0.734*** (0.252)	-0.00697* (0.00360)	-2.65e-06* (1.33e-06)	-0.594** (0.256)	-0.230** (0.101)
in*state 51	1.156*** (0.276)	0.164 (0.131)	1.00e-06 (1.45e-06)	-83.38*** (4.636)	-2.195*** (0.123)
female	1.636*** (0.154)	1.636*** (0.153)	1.637*** (0.153)	1.626*** (0.151)	1.634*** (0.152)
white	1.089*** (0.329)	1.087*** (0.327)	1.085*** (0.328)	1.089*** (0.330)	1.089*** (0.330)

age as of 12-31-1996	-2.524*** (0.266)	-2.447*** (0.287)	-2.454*** (0.286)	-2.569*** (0.261)	-2.597*** (0.267)
hh gross income 1996-1999	3.33e-05** (1.32e-05)	3.31e-05** (1.33e-05)	3.31e-05** (1.33e-05)	3.32e-05** (1.33e-05)	3.31e-05** (1.33e-05)
hh income poverty 1996-1999	-0.161 (0.192)	-0.157 (0.194)	-0.157 (0.194)	-0.157 (0.193)	-0.156 (0.193)
hh size 1997	-0.366*** (0.0870)	-0.365*** (0.0876)	-0.365*** (0.0876)	-0.368*** (0.0872)	-0.369*** (0.0871)
both bio parents	1.866*** (0.213)	1.879*** (0.209)	1.879*** (0.209)	1.880*** (0.209)	1.882*** (0.209)
expo re- forms	-1.657*** (0.269)	-1.707*** (0.266)	-1.702*** (0.267)	-1.728*** (0.260)	-1.721*** (0.262)
high school grad req 1997	5.126*** (0.619)	10.69* (6.331)	4.557* (2.541)	-590.1*** (32.95)	-558.9*** (31.42)
Constant	41.10*** (4.943)	17.84 (28.22)	42.47*** (13.30)	2,423*** (132.7)	2,298*** (126.8)
N	4,219	4,219	4,219	4,219	4,219
Total cred- its	16.05	16.05	16.05	16.05	16.05

State-level clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. OLS regressions in which the dependent variable is **total credits**; the treatment variables are the interactions of **in** and state dummies. in1-in5 measure potential years of exposure to: (1) STEM programs, (2) recipients, (3) expenditures, (4) recipients/1,000 teachers, and (5) expenditure/teacher. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table A.3: **Reduced Form:** Impact of interactions of potential years of exposure to STEM programs time-varying characteristics and state dummies on **ln(Income age 28)** controlling for demographics, household characteristics, high school math graduation requirements and state and cohort fixed effects.

	(1) expo years	(2) expo recipients	(3) expo expend.	(4) expo re- cip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.0547* (0.0289)	0.0505*** (0.00274)	3.51e-06*** (1.87e-07)	0.747*** (0.0945)	0.132*** (0.0158)
in*state 5	-0.0218 (0.0136)	0.00131*** (7.52e-05)	1.49e-07*** (7.87e-09)	0.0434*** (0.0156)	0.0119*** (0.00416)
in*state 6	0.0297 (0.0187)	0.000147* (8.37e-05)	8.12e-08* (4.69e-08)	0.00297 (0.00434)	0.00205 (0.00230)
in*state 7	-0.104*** (0.0250)	- (0.000343)	-2.76e-07*** (8.21e-08)	-0.0611*** (0.0154)	-0.0140*** (0.00348)
in*state 8	0.0446*** (0.0153)	0.00230 (0.00307)	3.25e-07 (4.42e-07)	0.00437 (0.0241)	-9.26e-05 (0.00376)
in*state 10	-0.0298 (0.0283)	5.01e-05 (4.84e-05)	1.43e-08 (1.42e-08)	-0.0428*** (0.0133)	-0.000540 (0.00256)
in*state 11	-0.0570*** (0.0178)	8.43e-05*** (1.91e-05)	3.15e-08*** (7.14e-09)	0.00749*** (0.00214)	0.00292*** (0.000761)
in*state 14	-0.0511*** (0.0129)	- (0.000307)	-2.42e-07*** (6.51e-08)	-0.00451 (0.0486)	0.0129* (0.00740)
in*state 15	-0.0246* (0.0126)	0.0292*** (0.00170)	7.76e-06*** (4.17e-07)	-0.0476 (0.203)	-0.0699 (0.129)
in*state 18	-0.201*** (0.0300)	0.000156*** (2.74e-05)	6.64e-08*** (1.17e-08)	0.00544*** (0.00115)	0.00239*** (0.000467)
in*state 19	0.314*** (0.0518)	0.00986*** (0.00177)	1.33e-06*** (2.40e-07)	0.440*** (0.0853)	0.0609*** (0.0111)
in*state 20	0.00232 (0.0173)	0.0185*** (0.00110)	2.27e-06*** (1.31e-07)	2.747*** (0.102)	0.791*** (0.0283)

in*state 21	-0.00827	-	-1.87e-07***	-0.0527***	-0.0127***
	(0.0289)	(0.000139)	(3.33e-08)	(0.00831)	(0.00190)
in*state 25	0.0187	0.00262***	2.86e-07***	0.00909*	0.00950***
	(0.0496)	(0.000161)	(1.75e-08)	(0.00509)	(0.000648)
in*state 26	-0.152***	-	-3.90e-07***	-0.0426***	-0.0311***
	(0.0432)	(0.000167)	(1.29e-07)	(0.0122)	(0.00883)
in*state 32	0.0217	0.105***	1.09e-05***	0.319	0.0723
	(0.0302)	(0.00557)	(5.50e-07)	(0.210)	(0.0487)
in*state 33	0.00890	1.84e-06	4.76e-10	5.79e-05	3.53e-05
	(0.0279)	(2.50e-06)	(6.78e-10)	(0.000570)	(0.000145)
in*state 34	0.128***	5.67e-05***	1.52e-08***	0.00436***	0.00124***
	(0.0226)	(1.00e-05)	(2.61e-09)	(0.000956)	(0.000230)
in*state 35	0.190***	0.000693***	6.60e-07***	0.00500***	0.00488***
	(0.0233)	(8.91e-05)	(8.53e-08)	(0.000732)	(0.000661)
in*state 37	0.0629**	-	-2.73e-07***	-0.149***	-0.0129***
	(0.0277)	(0.000548)	(5.08e-08)	(0.0257)	(0.00219)
in*state 39	-0.0747**	-5.36e-05***	-1.74e-08***	-0.00372	-0.00133*
	(0.0285)	(1.68e-05)	(5.52e-09)	(0.00234)	(0.000708)
in*state 41	-0.0365***	5.39e-05	3.69e-10	-0.00594	-0.000351
	(0.00184)	(8.18e-05)	(6.52e-09)	(0.00663)	(0.000374)
in*state 42	0.0604	0.000961	1.61e-07	0.00432	0.000931
	(0.0486)	(0.000690)	(1.16e-07)	(0.00705)	(0.00114)
in*state 43	0.0237	0.000634***	1.48e-07***	0.0309***	0.00753***
	(0.0274)	(0.000123)	(2.90e-08)	(0.00804)	(0.00177)
in*state 44	-0.0375	-3.49e-05	-7.50e-09	-0.0190*	-0.00361*
	(0.0243)	(3.34e-05)	(7.03e-09)	(0.0106)	(0.00208)
in*state 45	0.0161	0.000402	1.86e-08	-0.000928	-5.14e-06
	(0.0190)	(0.000927)	(4.58e-08)	(0.0399)	(0.00121)
in*state 47	0.154***	0.000615***	1.83e-07***	0.0520***	0.0162***
	(0.0371)	(0.000150)	(4.45e-08)	(0.0148)	(0.00421)
in*state 48	0.0197*	0.0724***	6.49e-06***	0.927***	0.203***
	(0.0115)	(0.00416)	(3.43e-07)	(0.223)	(0.0471)
in*state 49	0.0743***	0.0994***	9.51e-06***	17.69***	3.921***

in*state 50	(0.0173) 0.353***	(0.00585) 0.00405***	(5.31e-07) 1.50e-06***	(0.660) 0.217***	(0.141) 0.0822***
in*state 51	(0.0391) -0.365***	(0.000424) 0.247***	(1.57e-07) 2.47e-06***	(0.0340) 15.40***	(0.0117) 0.399***
	(0.0474)	(0.0162)	(1.79e-07)	(0.581)	(0.0145)
female	-0.370*** (0.0329)	-0.374*** (0.0329)	-0.374*** (0.0329)	-0.371*** (0.0333)	-0.371*** (0.0333)
white	0.105** (0.0422)	0.105** (0.0424)	0.104** (0.0427)	0.105** (0.0424)	0.106** (0.0428)
age as of 12-31-1996	-0.159***	-0.149***	-0.150***	-0.169***	-0.167***
hh gross income 1996-1999	(0.0365) 1.32e-06	(0.0377) 1.33e-06	(0.0380) 1.34e-06	(0.0356) 1.28e-06	(0.0359) 1.30e-06
hh income poverty ra- tio 1996-1999	(1.35e-06) 0.0193	(1.35e-06) 0.0189	(1.35e-06) 0.0189	(1.37e-06) 0.0202	(1.36e-06) 0.0197
hh size 1997	(0.0190) -0.00636	(0.0190) -0.00693	(0.0190) -0.00693	(0.0194) -0.00582	(0.0193) -0.00583
both bio parents	(0.0150) 0.0903***	(0.0149) 0.0910***	(0.0149) 0.0911***	(0.0150) 0.0891***	(0.0149) 0.0891***
expo re- forms	(0.0315) -0.118**	(0.0321) -0.131**	(0.0321) -0.130**	(0.0321) -0.134**	(0.0322) -0.135**
high school grad req 1997	(0.0554) 0.189*	(0.0600) 12.78***	(0.0599) 5.191***	(0.0610) 110.4***	(0.0609) 103.0***
Constant	(0.110) 12.19*** (0.635)	(0.785) -38.27*** (3.225)	(0.316) -7.902*** (1.447)	(4.136) -428.6*** (16.43)	(3.712) -398.9*** (14.72)
N	4,219	4,219	4,219	4,219	4,219
Mean Income	\$26,894	\$26,894	\$26,894	\$26,894	\$26,894

State-level clustered robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions in which the dependent variable is **ln(income age 28)**; the treatment variables are the interactions of **in** and state dummies. in1-in5 measure potential years of exposure to: (1) STEM programs, (2) recipients, (3) expenditures, (4) recipients/1,000 teachers, and (5) expenditure/teacher. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table A.4:

Reduced Form American Community Survey 2009. Impact of the potential years of exposure to STEM programs on the natural logarithm of personal yearly wages controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo	0.0209*** (0.00574)	0.00826** (0.00389)	0.000163 (0.0154)
female	-0.222*** (0.0164)	-0.300*** (0.0128)	-0.399*** (0.0128)
white	0.0204** (0.00995)	0.0196** (0.00790)	0.0101 (0.00798)
black	0.0108 (0.0193)	0.0284** (0.0123)	0.0641*** (0.0116)
Asian	0.0267 (0.0165)	0.0744*** (0.0136)	0.0918*** (0.0139)
birthday year	-0.0291*** (0.00888)	-0.0268*** (0.00194)	-0.000525 (0.00169)
poverty	0.00369*** (3.89e-05)	0.00394*** (3.61e-05)	0.00416*** (4.04e-05)
N	54,465	159,168	146,922
State FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Mean Income	\$26,981	\$32,456	\$38,100

State-level clustered robust standard errors in parentheses. ***p<0.01, ** p<0.05, * p<0.1. OLS regressions of the impact of expo on ln(personal yearly wages) controlling for race, gender, birthday year, poverty, high school math graduation requirements and number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects were included. All regressions are weighted.

Table A.5:

Reduced Form Survey of Income and Program Participation 2008. Impact of the potential exposure to STEM programs on the natural logarithm of household monthly income controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo	0.156*** (0.0287)	0.0459** (0.0213)	0.133*** (0.0486)
female	-0.100** (0.0406)	-0.0708*** (0.0157)	-0.0996*** (0.0139)
white	0.152* (0.0779)	0.220*** (0.0631)	0.163** (0.0694)
birth year	-0.0510 (0.0305)	-0.0272*** (0.00699)	-0.00934 (0.00586)
poverty	0.000131*** (3.63e-05)	5.70e-05 (3.89e-05)	0.000207*** (3.32e-05)
N	10,068	29,512	31,702
State FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Mean Income	\$5,614	\$6,064	\$6,463

State-level clustered robust standard errors in parentheses. ***p<0.01, ** p<0.05, * p<0.1. OLS regressions of the impact of potential exposure to STEM programs on the natural logarithm of household monthly income controlling for race, gender, birth year, poverty, high school math graduation requirements and number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects were included. All regressions are weighted.

Table A.6: **Reduced Form American Community Survey 2009.** Impact of the interactions of potential years of exposure to STEM programs and state dummies on the natural logarithm of personal yearly wages controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo*state 1	0.0742*** (0.00954)		
expo*state 4	0.0138*** (0.00195)	0.0120*** (0.00245)	
expo*state 5	0.0292*** (0.00199)	0.0100*** (0.00210)	0.0246*** (0.00441)
expo*state 6	0.0126 (0.00943)		
expo*state 8	-0.0270*** (0.00246)	0.0525*** (0.00229)	-0.137*** (0.00424)
expo*state 10	0.0148*** (0.00201)	0.0228*** (0.00220)	-0.0172*** (0.00438)
expo*state 11	-0.00979*** (0.00246)	0.0173*** (0.00205)	
expo*state 13		-0.0593*** (0.00570)	-0.249*** (0.00429)
expo*state 14	0.0321*** (0.00191)	-0.00259 (0.00240)	0.0839*** (0.00432)
expo*state 15	-0.0451*** (0.00221)	0.00310 (0.00238)	-0.0143*** (0.00418)
expo*state 18	0.0536*** (0.00179)	0.0399*** (0.00282)	-0.0697*** (0.00458)
expo*state 20	0.0875*** (0.00721)	-0.0848*** (0.00371)	
expo*state 21	0.0407*** (0.00285)	0.00592** (0.00227)	-0.0150*** (0.00432)
expo*state 25	0.178*** (0.00169)	0.0397*** (0.00253)	0.0562*** (0.00455)
expo*state 32	0.0144*** (0.00166)	0.0263*** (0.00210)	
expo*state 33	0.0439*** (0.00796)		
expo*state 34	-0.0371*** (0.00946)		

expo*state 35	0.139*** (0.00989)		
expo*state 37	0.00720*** (0.00191)	-0.0202*** (0.00237)	-0.0795*** (0.00437)
expo*state 39	-0.0724*** (0.00184)	-0.000387 (0.00300)	-0.0571*** (0.00443)
expo*state 41	0.00923*** (0.00171)	0.0113*** (0.00242)	0.0175*** (0.00412)
expo*state 43	0.0304*** (0.00163)	0.0216*** (0.00244)	-0.0287*** (0.00442)
expo*state 45	0.00268* (0.00146)	-0.0362*** (0.00217)	
expo*state 48	-0.0409*** (0.00226)	0.00273 (0.00234)	0.0238*** (0.00441)
expo*state 49	6.003*** (1.605)	-0.0215*** (0.00330)	0.0337*** (0.00436)
expo*state 50	0.110*** (0.00554)	-0.0213*** (0.00361)	
expo*state 51	-0.0614*** (0.00351)	0.0174*** (0.00266)	
female	-0.222*** (0.0164)	-0.300*** (0.0129)	-0.399*** (0.0128)
white	0.0200* (0.00994)	0.0196** (0.00782)	0.00999 (0.00801)
black	0.0103 (0.0194)	0.0282** (0.0121)	0.0637*** (0.0116)
asian	0.0262 (0.0166)	0.0746*** (0.0135)	0.0913*** (0.0139)
bday_year	-0.0318*** (0.00972)	-0.0267*** (0.00195)	-0.000511 (0.00169)
poverty	0.00369*** (3.92e-05)	0.00394*** (3.63e-05)	0.00416*** (4.03e-05)
N	54,465	159,168	146,922
State FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Mean Income	\$26,981	\$32,456	\$38,100

State-level clustered robust standard errors in parentheses. ***p<0.01, ** p<0.05, * p<0.1. OLS regressions of the impact of interactions of expo and state dummies on the natural logarithm of personal yearly wages controlling for race, gender, birthday year, poverty, high school math graduation requirements and number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects are included. All regressions are weighted.

Table A.7: **Reduced Form: Survey of Income and Program Participation 2008.** Impact of the interactions of potential exposure to STEM programs and state dummies on the natural logarithm of household monthly income controlling for demographic characteristics, high school graduation requirements and state and cohort fixed effects.

	(1)	(2)	(3)
	cohort 28-29	cohort 30-35	cohort 36-40
expo*state 4	0.348*** (0.0114)	0.0334*** (0.00626)	
expo*state 5	0.191*** (0.00709)	0.0948*** (0.00572)	0.208*** (0.0180)
expo*state 8	-0.789*** (0.0227)	0.256*** (0.0122)	0.542*** (0.0295)
expo*state 10	0.429*** (0.0122)	0.0368*** (0.00623)	0.128*** (0.0196)
expo*state 11	0.0651*** (0.0118)	-0.0418*** (0.00762)	
expo*state 13		0.142*** (0.00842)	0.180*** (0.0246)
expo*state 14	0.0205** (0.00810)	0.0488*** (0.00584)	0.164*** (0.0217)
expo*state 15	0.144*** (0.00389)	0.0708*** (0.00631)	-0.138*** (0.0181)
expo*state 18	-15.01** (7.263)	-0.0147 (0.0100)	0.418*** (0.0189)
expo*state 20	-0.0209 (0.0387)	-0.235*** (0.0362)	
expo*state 21	0.153*** (0.0126)	0.0455*** (0.00713)	-0.0890*** (0.0210)
expo*state 25	-0.0636*** (0.00987)	0.0480*** (0.00492)	-1.473*** (0.0277)
expo*state 32	-15.10** (7.262)	0.178*** (0.00688)	
expo*state 33	0.412*** (0.0427)		
expo*state 37	0.267*** (0.00506)	0.0908*** (0.00602)	-0.0226 (0.0240)
expo*state 39	0.0611*** (0.00798)	0.0772*** (0.00716)	-0.135*** (0.0199)
expo*state 41	0.121*** (0.00750)	-0.0884*** (0.00734)	0.447*** (0.0202)

expo*state 43	0.183*** (0.00818)	-0.0634*** (0.00708)	0.442*** (0.0185)
expo*state 45	-0.107*** (0.0395)	-0.0873*** (0.00969)	
expo*state 48	0.199*** (0.00642)	-0.0638*** (0.00703)	0.0645*** (0.0190)
expo*state 49	-0.718*** (0.0256)	-0.0106 (0.0105)	0.344*** (0.0230)
expo*state 50	-0.101*** (0.0241)	-0.123*** (0.0212)	
expo*state 51	-0.874*** (0.0347)	0.263*** (0.0279)	
female	-0.0981** (0.0401)	-0.0708*** (0.0158)	-0.0994*** (0.0140)
white	0.153* (0.0788)	0.219*** (0.0626)	0.161** (0.0689)
birth year	-0.0522* (0.0295)	-0.0247*** (0.00679)	-0.00941 (0.00585)
poverty	0.000125*** (3.69e-05)	6.08e-05 (3.90e-05)	0.000208*** (3.32e-05)
N	10,068	29,512	31,702
State FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Mean Income	\$5,614	\$6,064	\$6,463

State-level clustered robust standard errors in parentheses.***p<0.01,** p<0.05,* p<0.1. OLS regressions of the impact of interactions of expo and state dummies on the natural logarithm of household monthly income controlling for race, gender, birth year, poverty, high school math graduation requirements and number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects are included. All regressions are weighted.

Table A.8:

NLSY 97. Impact of potential years of exposure **during college** on the probability of teaching. Teaching is measured with two dummy variables: ever been a teacher and, individual is a teacher in 2013.

	(1) teacher ever	(2) teacher in 2013
expo_college	0.0101 (0.0131)	0.00179 (0.00913)
female	0.0966*** (0.00902)	0.0376*** (0.00537)
white	0.0168* (0.00995)	0.00663 (0.00629)
age as of 12-31-1996	-0.0257*** (0.00890)	-0.00728 (0.00441)
hgc bio dad	0.0103*** (0.00235)	0.00307** (0.00130)
hgc bio mom	0.0115*** (0.00267)	0.00369** (0.00171)
household gross income 1997	9.34e-07** (3.76e-07)	6.32e-07** (2.88e-07)
household income poverty ratio 1997	-0.000123** (5.66e-05)	-8.89e-05** (4.22e-05)
reforms_expo	-0.000855 (0.00811)	-0.00164 (0.00362)
high school math requirements	0.0309 (0.0387)	0.00980 (0.0214)
N	5,139	5,139
R-squared	0.199	0.080
Mean Teach	0.129	0.0396

OLS regressions that measure the impact of potential exposure during college on the probability of teaching. The first outcome is a dummy variable that indicates whether individuals have ever taught; the second outcome is a dummy variable that indicates whether the individual is teaching during 2013. The controls included are female, white, age as of 12-31-1996, parental education, household income and poverty as well as high school math graduation requirements and changes to high school math graduation requirements. State and cohort fixed effects were included. Standard errors are robust and clustered at the state level. All regressions are weighted. ***p<0.01, **p<0.05, * p<0.1.

Table A.9:

ACS 09. Impact of potential years of exposure **during college** on the probability of teaching.

	(1)	(2)	(3)
	cohort 28-29	cohort 30-35	cohort 36-40
expo college	0.00602*** (0.000603)	0.00503*** (0.000583)	0.00264*** (0.000896)
female	0.0458*** (0.00386)	0.0463*** (0.00180)	0.0475*** (0.00250)
white	0.0114*** (0.00369)	0.0130*** (0.00325)	0.0148*** (0.00161)
black	-0.00338 (0.00359)	0.00133 (0.00334)	0.00404* (0.00229)
asian	-0.0216*** (0.00402)	-0.0216*** (0.00263)	-0.0114*** (0.00176)
birthday year	-0.00727*** (0.00170)	7.13e-05 (0.000362)	-0.000418 (0.000530)
poverty	0.000122*** (1.06e-05)	0.000115*** (6.67e-06)	9.93e-05*** (6.47e-06)
math reform	-4.763*** (1.121)	0.0445 (0.239)	-0.782 (1.046)
high school math requirements	4.782*** (1.121)	-0.0526 (0.239)	0.397 (0.523)
N	66,126	196,346	183,023
R-squared	0.069	0.066	0.063
Mean Teach	0.0427	0.0424	0.0409

OLS regressions in which the dependent variable is a dummy variable that indicates whether the individual has one of the following occupations: Elementary and Middle School Teacher, Secondary School Teacher, Special Education Teacher, Other Teachers and Instructors and Teacher Assistants. The treatment variable is the number of years of potential exposure during college to STEM teacher programs. The controls include female, white, black, Asian, birthday year, poverty, high school math graduation requirements in 1997, changes to math graduation requirements between 1996 and 2008 and state fixed effects. Robust standard errors are clustered at the state level. All regressions are weighted. ***p<0.01, ** p<0.05, * p<0.1

Table A.10:

SIPP 08. Impact of potential years of exposure during college on the probability of teaching.

	(1)	(2)	(3)
	cohort 28-29	cohort 30-35	cohort 36-40
expo college	0.0113*** (0.00207)	0.00972*** (0.00203)	0.00401 (0.00414)
female	0.0513*** (0.00760)	0.0390*** (0.00555)	0.0471*** (0.00529)
white	0.0291* (0.0162)	0.00490 (0.00824)	0.00995** (0.00442)
birthday year	0.000884 (0.00440)	-0.00239** (0.00105)	3.94e-05 (0.00150)
poverty	-3.15e-05*** (7.09e-06)	-4.37e-06 (5.70e-06)	-2.06e-06 (5.00e-06)
math reform	1.906 (8.706)	-4.631** (2.071)	-0.0922 (0.985)
high school math requirements	-0.904 (4.353)	2.342** (1.036)	0.0113 (0.983)
N	10,303	30,186	32,485
R-squared	0.085	0.060	0.066
Mean Teach	0.0525	0.0422	0.0426

OLS regressions in which the dependent variable is a dummy variable that indicates whether the individual has one of the following occupations: Preschool and kindergarten, Elementary and middle school, Secondary school teachers, Special education teachers, Other teachers and instructors, Teacher assistants, Other education occupations. The treatment variable is potential years of exposure during college to STEM teacher programs. The controls include female, white, birthday year, poverty, high school math graduation requirements in 1997, changes to math graduation requirements between 1996 and 2008 and state fixed effects. Robust standard errors are clustered at the state level. All regressions are weighted. ***p<0.01, ** p<0.05, * p<0.1

Table A.11:

CCD. Impact of implementing a STEM teacher recruitment program on the number of teachers.

	(1)	(2)	(3)
	Elementary Teachers	Secondary Teachers	Total Teachers
STEM Program	-24.55 (840.3)	1,443 (898.5)	1,357 (1,283)
total students	0.0249*** (0.00457)	0.00657 (0.00655)	0.0613*** (0.0109)
high school students	0.0121 (0.0102)	0.0433*** (0.0136)	0.0120 (0.0366)
N	1,428	1,428	1,428
R-squared	0.561	0.528	0.845
Number of states	51	51	51
Mean Elementary Teachers	26,830		
Mean Secondary Teachers		20,761	
Mean Total Teachers			55,170

Fixed Effects estimates in which the dependent variables are: the number of elementary teachers, number of secondary school teachers and the total number of teachers. The panel of state-year data spans from 1983 to 2013. The treatment variable, STEM Program is equal to 1 in the year in which states implemented at least one teacher recruitment program and zero otherwise. The controls are the state-year total number of students and the state-year total number of high school students. Year effects were included. Robust standard errors were clustered by state. ***p<0.01, **p<0.05, * p<0.1

Table A.12:

CCD. Impact of implementing a STEM teacher recruitment program on the log of the number of teachers.

	(1)	(2)	(3)
	ln Elementary Teachers	ln Secondary Teachers	ln Total Teachers
STEM program	-0.0173 (0.0266)	0.000120 (0.0313)	-0.0119 (0.0173)
total students	3.08e-07** (1.16e-07)	2.28e-07 (1.62e-07)	3.79e-07*** (9.74e-08)
high school students	-1.97e-07 (2.71e-07)	8.56e-08 (3.39e-07)	-3.60e-07 (2.29e-07)
N	1,428	1,428	1,428
R-squared	0.368	0.397	0.714
Number of states	51	51	51

Fixed Effects estimates in which the dependent variables are: the natural logarithms of the number of elementary teachers, number of secondary school teachers and the total number of teachers. The panel of state-year data spans from 1983 to 2013. The treatment variable, STEM Program is equal to 1 in the year in which states implemented at least one teacher recruitment program and zero otherwise. The controls are the state-year total number of students and the state-year total number of high school students. Year effects were included. Robust standard errors were clustered by state. ***p<0.01, ** p<0.05, * p<0.1

Table A.13: **NLSY 97**. Impact of the interactions of potential years of exposure **during college** and state dummies on the probability of teaching. Teaching is measured with two dummy variables: ever been a teacher and, individual is a teacher in 2013.

	(1) teacher ever	(2) teacher 2013
expo_college*state 4	0.0213 (0.0131)	0.00406 (0.00578)
expo_college*state 5	0.0254 (0.0207)	0.000159 (0.0101)
expo_college*state 6	0.0187 (0.0113)	0.0451*** (0.00680)
expo_college*state 7	0.0114* (0.00610)	-0.0275*** (0.00381)
expo_college*state 8	0.0375* (0.0201)	-0.00834 (0.0101)
expo_college*state 10	0.0143 (0.0129)	-0.000573 (0.00563)
expo_college*state 11	0.0137 (0.0115)	-0.00541 (0.00543)
expo_college*state 12	0.000101 (0.0172)	-0.00641 (0.00836)
expo_college*state 14	0.0235 (0.0209)	0.00513 (0.0102)
expo_college*state 15	0.0105 (0.0210)	-0.00577 (0.0103)
expo_college*state 17	0.0417*** (0.00576)	0.0422*** (0.00251)
expo_college*state 18	0.0409*** (0.0124)	0.0217*** (0.00521)
expo_college*state 19	-0.00625 (0.00667)	-0.0196*** (0.00337)
expo_college*state 20	0.0128 (0.0248)	0.00101 (0.0119)
expo_college*state 21	-0.00157 (0.0129)	-0.00891 (0.00569)
expo_college*state 25	0.0371* (0.0208)	0.0235** (0.0110)
expo_college*state 26	-0.00892 (0.00535)	-0.00929*** (0.00280)
expo_college*state 27	0.0713***	-0.0412***

	(0.0142)	(0.00968)
expo_college*state 32	0.0358**	0.0113*
	(0.0135)	(0.00592)
expo_college*state 33	0.0223	0.00605
	(0.0205)	(0.0102)
expo_college*state 34	0.122***	0.0725***
	(0.0124)	(0.00646)
expo_college*state 35	0.0220*	0.0206***
	(0.0113)	(0.00671)
expo_college*state 37	0.0107	-0.00575
	(0.0105)	(0.00551)
expo_college*state 39	0.00999	-0.00162
	(0.0129)	(0.00549)
expo_college*state 41	-0.0126***	-0.00730***
	(0.000719)	(0.000434)
expo_college*state 42	-0.0893***	-0.0764***
	(0.00618)	(0.00337)
expo_college*state 43	0.00652	0.00566
	(0.0105)	(0.00559)
expo_college*state 44	0.0177**	0.0132***
	(0.00707)	(0.00361)
expo_college*state 45	0.0300	-0.0114
	(0.0219)	(0.0104)
expo_college*state 47	0.0466***	0.0214***
	(0.00919)	(0.00423)
expo_college*state 48	0.0340	0.000137
	(0.0215)	(0.0103)
expo_college*state 49	0.00282	-0.00192
	(0.0226)	(0.0112)
expo_college*state 50	0.0162	0.0105
	(0.0213)	(0.0100)
expo_college*state 51	-0.0137	-0.00765
	(0.0205)	(0.0109)
female	0.0958***	0.0369***
	(0.00898)	(0.00524)
white	0.0167*	0.00608
	(0.00990)	(0.00642)
age as of 12-31-1996	-0.0247***	-0.00679
	(0.00899)	(0.00445)
hgc bio dad	0.0104***	0.00308**
	(0.00234)	(0.00129)
hgc bio mom	0.0115***	0.00375**
	(0.00267)	(0.00170)

household gross income 1997	9.68e-07** (3.74e-07)	6.69e-07** (2.90e-07)
household income poverty ratio 1997	-0.000130** (5.61e-05)	-9.48e-05** (4.26e-05)
reforms_expo	-0.00415 (0.00955)	-0.00376 (0.00399)
high school math requirements	0.0411 (0.0405)	0.0124 (0.0214)
N	5,139	5,139
R-squared	0.201	0.084
Mean Teach	0.129	0.0396

OLS regressions that measure the impact of potential exposure during college on the probability of teaching. The first outcome is a dummy variable that indicates whether individuals have ever taught; the second outcome is a dummy variable that indicates whether the individual is teaching during 2013. The controls included are female, white, age as of 12-31-1996, parental education, household income and poverty as well as high school math graduation requirements and changes to high school math graduation requirements. State and cohort fixed effects were included. Standard errors are robust and clustered at the state level. All regressions are weighted. ***p<0.01, ** p<0.05, * p<0.1

Table A.14: **ACS 09.** Impact of the interactions of potential years of exposure **during college** and state dummies on the probability of teaching.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo_college*state 4	0.00777*** (0.000398)	0.00845*** (0.000340)	0.0132*** (0.000854)
expo_college*state 5	0.00579*** (0.000415)	0.00574*** (0.000373)	0.00276*** (0.000660)
expo_college*state 6	-0.00486*** (0.000758)	0.00413*** (0.00109)	
expo_college*state 7	0.0273*** (0.00120)		
expo_college*state 8	0.000759 (0.000508)	0.00876*** (0.000241)	0.00502*** (0.000682)
expo_college*state 10	0.00563*** (0.000460)	0.00441*** (0.000303)	0.00186*** (0.000637)
expo_college*state 11	0.00561*** (0.000605)	0.00590*** (0.000424)	-0.00256* (0.00145)
expo_college*state 12	0.00929*** (0.00188)		
expo_college*state 13		-0.00203* (0.00106)	-0.00595*** (0.00139)
expo_college*state 14	0.00624*** (0.000531)	0.00496*** (0.000452)	0.00593*** (0.000654)
expo_college*state 15	0.00771*** (0.000454)	0.00574*** (0.000283)	-0.00125** (0.000610)
expo_college*state 18	0.00589*** (0.000455)	0.00637*** (0.000274)	-0.00100 (0.000631)
expo_college*state 19	-0.00566*** (0.00190)		
expo_college*state 20	0.00783*** (0.000364)	0.00248*** (0.000393)	
expo_college*state 21	0.00496*** (0.000652)	0.00524*** (0.000397)	0.00188*** (0.000645)
expo_college*state 25	0.00761*** (0.000432)	0.00738*** (0.000313)	0.00245*** (0.000620)
expo_college*state 26	0.00336* (0.00194)		
expo_college*state 32	0.00834*** (0.000282)	0.00662*** (0.000231)	0.0106*** (0.000881)
expo_college*state 33	0.00800***	0.00229***	

	(0.000612)	(0.000684)	
expo_college*state 34	0.00661***	0.00526***	
	(0.000873)	(0.00113)	
expo_college*state 35	0.0130***	-0.0356***	
	(0.00102)	(0.00101)	
expo_college*state 37	0.00942***	0.00613***	-0.000761
	(0.000443)	(0.000294)	(0.000639)
expo_college*state 39	0.00668***	0.00389***	0.00177***
	(0.000493)	(0.000350)	(0.000619)
expo_college*state 41	0.00456***	0.00475***	0.00984***
	(0.000464)	(0.000320)	(0.000625)
expo_college*state 42	0.0152***		
	(0.00209)		
expo_college*state 43	0.00213***	0.00677***	0.00216***
	(0.000450)	(0.000290)	(0.000635)
expo_college*state 44	0.0118***		
	(0.00110)		
expo_college*state 45	0.00854***	0.000466	
	(0.000255)	(0.000420)	
expo_college*state 47	0.00665***		
	(0.00126)		
expo_college*state 48	0.000737	0.00418***	0.00426***
	(0.000510)	(0.000358)	(0.000602)
expo_college*state 49	0.00410***	0.00380***	-0.00918***
	(0.000364)	(0.000250)	(0.000672)
expo_college*state 50	-0.00233***	0.00175***	
	(0.000507)	(0.000400)	
expo_college*state 51	0.0149***	-0.00500***	
	(0.000651)	(0.000410)	
female	0.0458***	0.0463***	0.0475***
	(0.00386)	(0.00179)	(0.00251)
white	0.0113***	0.0130***	0.0148***
	(0.00366)	(0.00327)	(0.00161)
black	-0.00353	0.00120	0.00396*
	(0.00357)	(0.00337)	(0.00230)
asian	-0.0217***	-0.0218***	-0.0114***
	(0.00407)	(0.00264)	(0.00176)
birthday year	-0.00757***	0.000228	-0.000404
	(0.00210)	(0.000364)	(0.000540)
poverty	0.000122***	0.000114***	9.93e-05***
	(1.05e-05)	(6.70e-06)	(6.48e-06)
math reform	-5.019***	0.183	-0.225
	(1.383)	(0.239)	(0.356)
high school math	4.999***	-0.164	0.251

requirements	(1.385)	(0.240)	(0.355)
N	66,126	196,346	183,023
R-squared	0.069	0.067	0.063
Mean Teach	0.0427	0.0424	0.0409

OLS regressions in which the dependent variable is a dummy variable that indicates whether the individual has one of the following occupations: Elementary and Middle School Teacher, Secondary School Teacher, Special Education Teacher, Other Teachers and Instructors and Teacher Assistants. The treatment variables are the interactions of the years of potential exposure during college to STEM teacher programs and state dummies. The controls include female, white, black, Asian, birthday year, poverty, high school math graduation requirements in 1997, changes to math graduation requirements between 1996 and 2008 and state fixed effects. Robust standard errors are clustered at the state level. All regressions are weighted. ***p<0.01, ** p<0.05, * p<0.1

Table A.15: **SIPP 08.** Impact of the interactions of potential years of exposure **during college** and state dummies on the probability of teaching.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 35-40
expo college*state 4	0.0198*** (0.00203)	-0.0177*** (0.000646)	0.00967*** (0.00311)
expo college*state 5	0.0154*** (0.000743)	0.0141*** (0.000973)	0.00458** (0.00192)
expo college*state 6	-0.0166*** (0.00388)		
expo college*state 7	-0.124*** (0.00770)		
expo college*state 8	0.00262 (0.00167)	0.0228*** (0.00163)	0.212*** (0.00191)
expo college*state 10	0.00601*** (0.000828)	0.0105*** (0.000723)	2.33e-05 (0.00219)
expo college*state 11	0.0131*** (0.000540)	0.0138*** (0.000935)	-0.0122** (0.00509)
expo college*state 12	0.00146 (0.0103)		
expo college*state 13		-0.00402 (0.00270)	-0.0316*** (0.00386)
expo college*state 14	0.00707*** (0.00115)	0.0103*** (0.000725)	-0.0169*** (0.00218)
expo college*state 15	0.0142*** (0.000765)	0.0130*** (0.000730)	0.00448** (0.00209)
expo college*state 18	0.0117*** (0.00103)	0.0310*** (0.00112)	0.0344*** (0.00210)
expo college*state 20	0.0812*** (0.00907)	0.00898*** (0.00194)	
expo college*state 21	0.00740*** (0.00118)	0.00224 (0.00146)	0.0204*** (0.00205)
expo college*state 25	-0.00247 (0.00185)	0.00178*** (0.000513)	0.0151*** (0.00215)
expo college*state 32	0.0263*** (0.00233)	0.00927*** (0.000553)	-0.0212*** (0.00284)
expo college*state 33	0.0104** (0.00407)	-0.0163*** (0.00400)	
expo college*state 34	0.0194*** (0.00406)		
expo college*state 35	-0.0222**		

	(0.00972)		
expo college*state 37	0.00892*** (0.00136)	0.0180*** (0.000637)	-0.00856*** (0.00208)
expo college*state 39	0.0157*** (0.00142)	0.0137*** (0.000892)	-0.00130 (0.00246)
expo college*state 41	0.0178*** (0.00111)	0.00801*** (0.000410)	-0.0181*** (0.00227)
expo college*state 43	0.00450*** (0.00127)	0.00366*** (0.000751)	0.00788*** (0.00220)
expo college*state 44	0.0328*** (0.00606)		
expo college*state 45	0.0162*** (0.00101)	-0.00402*** (0.00122)	
expo college*state 47	-0.0317*** (0.00683)		
expo college*state 48	0.0134*** (0.00117)	-0.0159*** (0.00117)	0.0163*** (0.00220)
expo college*state 49	-0.211 (0.919)	0.00444*** (0.000739)	0.0334*** (0.00260)
expo college*state 50	-0.00999*** (0.00188)	0.00520*** (0.00147)	
expo college*state 51	-0.249 (0.919)	-0.000595 (0.00140)	
female	0.0514*** (0.00764)	0.0395*** (0.00561)	0.0467*** (0.00529)
white	0.0296* (0.0163)	0.00516 (0.00824)	0.00978** (0.00435)
birthday year	0.00148 (0.00557)	-0.00222** (0.00107)	0.000186 (0.00152)
poverty	-3.14e-05*** (7.13e-06)	-4.20e-06 (5.73e-06)	-1.60e-06 (5.04e-06)
math reform	1.012 (3.680)	-1.394* (0.700)	0.00453 (1.002)
high school math requirements	-0.971 (3.677)	1.448** (0.704)	-0.0852 (1.000)
N	10,303	30,186	32,485
R-squared	0.087	0.061	0.070
Mean Teach	0.0525	0.0422	0.0426

OLS regressions in which the dependent variable is a dummy variable that indicates whether the individual has one of the following occupations: Preschool and kindergarten, Elementary and middle school, Secondary school teachers, Special education teachers, Other teachers and instructors, Teacher assistants, Other education occupations. The treatment variables are the interactions of the years of potential exposure during college to STEM teacher programs and state dummies. The controls include female, white, birthday year, poverty, high school math graduation requirements in 1997, changes to math graduation requirements between 1996 and 2008 and state fixed effects. Robust standard errors are clustered at the state level. All regressions are weighted. ***p<0.01, ** p<0.05, * p<0.1

Table A.16:

BLS. Impact of Economic Conditions on States' decisions of implementing a STEM teacher recruitment program.

	(1)	(2)	(3)
	STEM	STEM lead 1 year	STEM lead 2 years
annual median wages	0.000000457 (0.00000124)	0.000000748 (0.00000148)	0.00000174 (0.00000146)
annual mean wages	0.000000805 (0.000000989)	0.00000063 (0.00000111)	0.00000122 (0.00000111)
hourly median wages	0.000948 (0.00258)	0.00155 (0.00308)	0.00361 (0.00303)
hourly mean wages	0.00167 (0.00206)	0.00131 (0.00230)	0.00255 (0.00232)
Percent on poverty	0.0155* (0.00865)	0.0130 (0.00967)	0.00835 (0.00968)
Unemployment rate	0.0140 (0.0212)	0.00806 (0.0207)	-0.00524 (0.0202)
Number of states	51	51	51

Fixed Effects estimates in which the dependent variables are: STEM is equal to 1 in the year in which states implemented at least one STEM teacher recruitment program and 0 otherwise. In columns (2) and (3) STEM was led one and two years correspondingly. The treatment variables are the per state-year wages across all occupations, annual median wages, annual mean wages, hourly median wages and hourly mean wages, percent on poverty and unemployment rate. The panel of state-year data spans from 1983 to 2013. Each cell represents a separate fixed effects regression. Year effects were included. Robust standard errors were clustered by state. ***p<0.01, ** p<0.05, * p<0.1

Table A.17:

BLS. Impact of Economic Conditions on States' decisions of implementing a STEM teacher recruitment program. The role of occupation wages on states' decisions to implement STEM programs.

	(1)	(2)	(3)	(4)
	hourly mean	hourly median	annual mean	annual median
Engineering	-0.00260 (0.0147)	-0.000699 (0.0144)	-0.00000124 (0.00000704)	-0.000000313 (0.00000693)
Math	0.0125 (0.0115)	0.0115 (0.0102)	0.000006 (0.00000554)	0.00000553 (0.00000489)
Business	0.0449** (0.0204)	0.0512** (0.0247)	0.0000216** (0.0000098)	0.0000246** (0.0000119)
Health	-0.00661 (0.0160)	-0.0364 (0.0240)	-0.00000314 (0.00000769)	-0.0000175 (0.0000115)
Education	0.00666 (0.0173)	-0.00235 (0.0152)	0.00000321 (0.00000831)	-0.0000011 (0.0000073)
Law	-0.0119* (0.00653)	-0.0153** (0.00673)	-0.00000571* (0.00000314)	-0.00000735** (0.00000323)
Number of states	51	51	51	51

Fixed Effects estimates in which the dependent variables are: STEM is equal to 1 in the year in which states implemented at least one STEM teacher recruitment program and 0 otherwise. The treatment variables are the per state-year wages the fields Engineering, Math, Business, Health, Education and Law; these variables were measured hourly (mean and median) and annually (mean and median). The panel of state-year data spans from 1983 to 2013. Each cell represents a separate fixed effects regression. Year effects were included. Robust standard errors were clustered by state. ***p<0.01, ** p<0.05, * p<0.1

Why are the OLS and 2SLS estimates so different?

Consider the following model assuming homogeneous returns.

$$y_i = \beta_0 + \beta_1 x_i + \epsilon_i \quad (\text{A.1})$$

In this case, the estimate of β_1 is:

$$\hat{\beta}_1 = \frac{Cov(y_i, x_i)}{Var(x_i)} = \frac{Cov(\beta_0 + \beta_1 x_i + \epsilon_i, x_i)}{Var(x_i)} = \beta_1 + \frac{Cov(\epsilon_i, x_i)}{Var(x_i)} \quad (\text{A.2})$$

In a constant returns world, i.e., β_1 is the same for all individuals in the sample, an OLS estimate of β_1 would be biased upwards because, if we assume that ϵ_i captures motivation, persistence, etc, then $Cov(\epsilon_i, x_i) > 0$.

Now consider the model of heterogeneous returns:

$$y_i = \beta_0 + \beta_{1i} x_i + \epsilon_i \quad (\text{A.3})$$

In this model β_{1i} is different for each individual i . The OLS estimator of (7) is

$$\widehat{\beta_{1OLS}} = \frac{Cov(y_i, x_i)}{Var(x_i)} = \frac{Cov(\beta_0 + \beta_{1i} x_i + \epsilon_i, x_i)}{Var(x_i)} = \frac{Cov(\beta_{1i} x_i, x_i)}{Var(x_i)} + \frac{Cov(\epsilon_i, x_i)}{Var(x_i)} \quad (\text{A.4})$$

Now suppose that the β_{1i} takes the form:

$$\beta_{1i} = \gamma_0 + \gamma_1 x_i \quad (\text{A.5})$$

$$\widehat{\beta_{1OLS}} = \frac{Cov(\beta_{1i} x_i, x_i)}{Var(x_i)} + \frac{Cov(\epsilon_i, x_i)}{Var(x_i)} = \frac{Cov(\gamma_0 x_i + \gamma_1 x_i^2, x_i)}{Var(x_i)} + \frac{Cov(\epsilon_i, x_i)}{Var(x_i)} \quad (\text{A.6})$$

$$\widehat{\beta_{1OLS}} = \gamma_0 + \gamma_1 \frac{Cov(x_i^2, x_i)}{Var(x_i)} + \frac{Cov(\epsilon_i, x_i)}{Var(x_i)} \quad (\text{A.7})$$

Thus, assuming a positive correlation between returns and course taking ($\gamma_1 > 0$), as stated in equation (9), the true value of the OLS estimate would be strictly greater than that under the constant returns assumption.

$$\gamma_0 + \gamma_1 \frac{Cov(x_i^2, x_i)}{Var(x_i)} > \beta_1 \quad (\text{A.8})$$

Since the LATE parameter estimates recover the effect of an increase on x_i on y_i **only** for the population of compliers we can conclude that, (1) the constant returns assumption is not valid in this study, and, (2) the compliers in this study are those who have high returns.