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

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Abstract

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. Many states have implemented programs aimed to recruit recent college graduates to fill teaching positions especially in underprivileged geographic areas, and in high-returns fields such as mathematics and science. Students who enroll in these programs must teach math and science courses in public schools. The influx of new teachers, which is often accompanied by increases in course offerings, induces marginal students to take math and science courses that otherwise would not have been taken. The results indicate that, on average, each additional year of math increases yearly labor income by about 3%. Since the results presented in this paper were obtained using instrumental variables methods, the estimates represent weighted measures of Local Average Treatment Effects (LATE's); in other words, the effects of math credits on labor income, *only* for the group of individuals induced to change their course taking behavior *because* of the implementation of teacher recruitment programs. Other results show that math credits during high school also increase the probability of college attendance and bachelors' degree completion.

I 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).

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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 school[1]; (b) increased college admission test scores[2]; (c) greater chances of college entry and completion[3]; (d) better financial market participation and credit management[4], and, (e) higher earnings in adulthood[5].

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 studies do not identify and estimate parameters with a causal interpretation. The main motivation of this paper is to

[1] Attewell & 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)

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

[3] Schneider, 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

[4] Cole, Paulson, & Shastri (2015)

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

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 teacher's 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 bachelor's 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 literature[6], 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 bachelor's 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 (II), I state the research questions and contribution; section (III) is devoted to the current literature on the effects of math credits on various measures of earnings. Section (IV) is devoted to the identification strategy, sample description, definitions of treatment, controls, instruments and outcomes; and the econometric models. The results are presented in section (V); also, a characterization of the compliant sub-population is provided. Section (VI) addresses a number of threats to identification. Finally, in section (VII) conclusions are provided.

II 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

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

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.

III 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 women[7].

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), 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

[7] Even 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.

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

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 upward[9]. 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 outcomes 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

[9] Let **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.

sets is provided[10]. 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.

IV 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 mathematics 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.

[10]Whereas 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.

[11]As Angist 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.

A 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).

B 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 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 income[15] 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 and the weights for 25 and 31 years

[12] Quote 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."

[13] http://www.bls.gov/nls/y97hstran.htm

[14] https://nces.ed.gov/nationsreportcard/glossary.aspx?nav=y

[15] The 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?"

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 reform[16]. State and cohort[17] 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 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

[16]Reform 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.

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

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 17[18] 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:

$$expo = enrollment_{is} * A'_s \tag{1}$$

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.

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:

$$expo\_recipients = enrollment_{is} * R'_s \tag{2}$$

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

$$expo\_expenditures = enrollment_{is} * E'_s \tag{3}$$

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

$$expo\_z = enrollment_{is} * z'_s \tag{4}$$

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: $in1 = \textbf{expo}$, $in2 = expo\_recipients$, $in3 = expo\_expenditures$, $in4 = expo\_recipients/1,000teachers$, 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 credits[19], 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 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.

C Econometric Model

Exclusion Restrictions

States implemented STEM teacher recruitment programs in different years. Since the data (NLSY 97) includes individuals from different cohorts[20], 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]

[19] The 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.

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

[21] Exclusion 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

$$Math_i = X_i\alpha + \sum_s expo\_z_{is} S_{is} \eta_s + \delta_c + \delta_s + \epsilon_i \tag{5}$$

Outcome Equation

$$ln(Income_i) = X_i\beta + \rho\widehat{Math_i} + \delta_c + \delta_s + \mu_i \tag{6}$$

In equations (5) and (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 (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 (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.

V Results

A 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

$$Math_i = \sum_s expo_{is} S_{is} \eta_s + X_i\alpha + +\delta_c + \delta_s + \epsilon_i \tag{7}$$

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

[Table 3 here]

Table 3 includes the parameter estimates of equation (7). For expositional reasons, I included a trimmed version of table 3; the complete table is located in the Web 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 (IV). 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 Web 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

$$ln(Income_i) = \sum_s expo\_z_{is} S_{is} \eta_s + X_i \alpha + +\delta_c + \delta_s + \epsilon_i \tag{8}$$

In equation (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 (8). For expositional purposes, I included a trimmed version of table 5; the complete table is located in the Web Appendix. The impact of exposure to STEM programs varies greatly by state and is positive and statistically significant in most cases.

B 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 (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 (5) and (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 repetitions[23]. Let Y be any of the three college outcomes:

$$\frac{dY}{dmath} = \frac{1}{N}\sum_{i=1}^{N}[\hat{P}_i[Y = 1|math + 1, X, Z] - \hat{P}_i[Y = 1|math, X, Z]] \tag{9}$$

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

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

(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 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 $\mu_i = famincome_i + \nu_i$, where $famincome_i$ 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, assume[24] 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:

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

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 Vytlacil, 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:

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

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

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

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.

C 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:

$$E[x_i|complier] = \frac{E[\kappa_i X_i]}{E[\kappa_i]} \tag{10}$$

In equation (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:

$$\kappa_i = 1 - \frac{D_i(1 - z_i)}{1 - Pr(z_i = 1|x_i)} - \frac{(1 - D_i)z_i}{Pr(z_i = 1|x_i)} \tag{11}$$

In (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.

VI 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 number[25] 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 - though 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

[25] STEM 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.

the impact of STEM programs on income, different from the increase 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.

A 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 equation[26]:

$$ln(Earnings_i) = expo_{is}\eta + X_i\alpha + \gamma_s + \gamma_c + \epsilon_i \quad (12)$$

In equation (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 (IV).[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 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 (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 Web Appendix here]

Table 4 in the Web Appendix includes the parameter estimates of equation (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

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

[27] The 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.

36-40, the corresponding estimates are 0.0083% and 0.00016%. When using SIPP 08, as table 5 in the Web 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 Web 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.

B 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:

$$Teach_i = expo\_college_{is}\eta + X_i\alpha + \gamma_s + \gamma_c + \epsilon_i \tag{13}$$

I estimate equation (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 Web Appendix here]

As table 8-Web 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 Web 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 Web 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 Web 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:

$$y_{st} = \beta_0 + \beta_1 STEM_{st} + \beta_2 X_{st} + \gamma_s + \gamma_t + \nu_{st} \tag{14}$$

In equation (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 Web Appendix here]

Table 11 in the Web Appendix includes the parameter estimates of equation (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 Web Appendix includes the estimates of equation (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 number of teachers.

[28] When compared to the mean.

C 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:

$$STEM_{st} = \delta_0 + \delta_1 Econ_{st} + \gamma_s + \gamma_t + \mu_{st} \tag{15}$$

In equation (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 Web 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 Web Appendix here]

When $Econ_{st}$ represents field-specific wages - as shown in table 17 Web 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.

D 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.

VII 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 Vytlacil (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, mother's 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.

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Table 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. The analysis sample includes individuals who at least completed 9th grade who also have transcripts information. The reference category is Cohort 1994 which constitutes a 17% of the sample.The variable *household gross income 1996-1999* was calculated as the 1997 inflation-adjusted average of non-missing observations between 1996 and 1996. The variable *household income to poverty ratio 1996-1999* was also calculated as the average of non-missing observations between 1996 and 1996.

Table 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				
expo: 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. The analysis sample includes individuals who at least completed 9th grade who also have transcripts information. Income at age 28 is measured in 2011 real dollars.

Table 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 Web 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***	0.00282***	6.74e-07***	0.107***	0.0257***
	(0.0359)	(0.000578)	(1.37e-07)	(0.0217)	(0.00561)
in*state 19	0.0233	0.00121	1.64e-07	-0.000307	0.000790
	(0.0594)	(0.00241)	(3.24e-07)	(0.110)	(0.0160)
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)
⋮	⋮	⋮	⋮	⋮	⋮
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	-0.0249	-0.0245	-0.0244	-0.0233	-0.0238
ratio 1996-1999	(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)
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 controls are female, white, age, household gross income between 1996-1999, household income to poverty ratio between 1996-1999, household size in 1997; a dummy indicating whether the household had two biological parents in 1997; number of math units (years) required for high school graduation, number of years of exposure to changes in high school math graduation requirements. 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 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 Web 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***	0.0112***	2.67e-06***	0.390***	0.0891***
	(0.167)	(0.00269)	(6.41e-07)	(0.111)	(0.0282)
in*state 19	2.068***	0.0667***	8.99e-06***	2.979***	0.389***
	(0.296)	(0.0120)	(1.62e-06)	(0.580)	(0.0825)
in*state 34	0.438***	0.000189**	4.54e-08**	0.0122*	0.00270
	(0.156)	(7.85e-05)	(2.04e-08)	(0.00665)	(0.00182)
⋮	⋮	⋮	⋮	⋮	⋮
female	1.636***	1.636***	1.637***	1.626***	1.634***
	(0.154)	(0.153)	(0.153)	(0.151)	(0.152)
white	1.089***	1.087***	1.085***	1.089***	1.089***
	(0.329)	(0.327)	(0.328)	(0.330)	(0.330)
age as of 12-31-1996	-2.524***	-2.447***	-2.454***	-2.569***	-2.597***
	(0.266)	(0.287)	(0.286)	(0.261)	(0.267)
hh gross income 1996-1999	3.33e-05**	3.31e-05**	3.31e-05**	3.32e-05**	3.31e-05**
	(1.32e-05)	(1.33e-05)	(1.33e-05)	(1.33e-05)	(1.33e-05)
hh income poverty 1996-1999	-0.161	-0.157	-0.157	-0.157	-0.156
	(0.192)	(0.194)	(0.194)	(0.193)	(0.193)
hh size 1997	-0.366***	-0.365***	-0.365***	-0.368***	-0.369***
	(0.0870)	(0.0876)	(0.0876)	(0.0872)	(0.0871)
both bio parents	1.866***	1.879***	1.879***	1.880***	1.882***
	(0.213)	(0.209)	(0.209)	(0.209)	(0.209)
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. The controls are female, white, age, household gross income between 1996-1999, household income to poverty ratio between 1996-1999, household size in 1997; a dummy indicating whether the household had two biological parents in 1997; number of math units (years) required for high school graduation, number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table 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 Web 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*	0.0505***	3.51e-06***	0.747***	0.132***
	(0.0289)	(0.00274)	(1.87e-07)	(0.0945)	(0.0158)
in*state 5	-0.0218	0.00131***	1.49e-07***	0.0434***	0.0119***
	(0.0136)	(7.52e-05)	(7.87e-09)	(0.0156)	(0.00416)
in*state 6	0.0297	0.000147*	8.12e-08*	0.00297	0.00205
	(0.0187)	(8.37e-05)	(4.69e-08)	(0.00434)	(0.00230)
in*state 7	-0.104***	-0.00115***	-2.76e-07***	-0.0611***	-0.0140***
	(0.0250)	(0.000343)	(8.21e-08)	(0.0154)	(0.00348)
in*state 8	0.0446***	0.00230	3.25e-07	0.00437	-9.26e-05
	(0.0153)	(0.00307)	(4.42e-07)	(0.0241)	(0.00376)
⋮	⋮	⋮	⋮	⋮	⋮
female	-0.370***	-0.374***	-0.374***	-0.371***	-0.371***
	(0.0329)	(0.0329)	(0.0329)	(0.0333)	(0.0333)
white	0.105**	0.105**	0.104**	0.105**	0.106**
	(0.0422)	(0.0424)	(0.0427)	(0.0424)	(0.0428)
age as of 12-31-1996	-0.159***	-0.149***	-0.150***	-0.169***	-0.167***
	(0.0365)	(0.0377)	(0.0380)	(0.0356)	(0.0359)
hh gross income 1996-1999	1.32e-06	1.33e-06	1.34e-06	1.28e-06	1.30e-06
	(1.35e-06)	(1.35e-06)	(1.35e-06)	(1.37e-06)	(1.36e-06)
hh income poverty ratio	0.0193	0.0189	0.0189	0.0202	0.0197
1996-1999	(0.0190)	(0.0190)	(0.0190)	(0.0194)	(0.0193)
hh size 1997	-0.00636	-0.00693	-0.00693	-0.00582	-0.00583
	(0.0150)	(0.0149)	(0.0149)	(0.0150)	(0.0149)
both bio parents	0.0903***	0.0910***	0.0911***	0.0891***	0.0891***
	(0.0315)	(0.0321)	(0.0321)	(0.0321)	(0.0322)
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. The controls are female, white, age, household gross income between 1996-1999, household income to poverty ratio between 1996-1999, household size in 1997; a dummy indicating whether the household had two biological parents in 1997; number of math units (years) required for high school graduation, number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table 6: Impact of mathematics credits on log income. N=4,219

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A. instrument: expo years					
math	0.130***	0.102***	0.0520	0.0501	0.0831
	(0.0179)	(0.0212)	(0.258)	(0.276)	(0.302)
total credits		0.00948**		0.0107	-0.00621
		(0.00389)		(0.0352)	(0.0656)
Panel B. instrument: expo recipients					
math	0.130***	0.102***	0.0805	0.0253	0.0322
	(0.0179)	(0.0212)	(0.0653)	(0.0730)	(0.0876)
total credits		0.00948**		0.0367**	0.0304
		(0.00389)		(0.0145)	(0.0312)
Panel C. instrument: expo expenditures					
math	0.130***	0.102***	0.0809	0.0312	0.0386
	(0.0179)	(0.0212)	(0.0638)	(0.0698)	(0.0858)
total credits		0.00948**		0.0351**	0.0276
		(0.00389)		(0.0141)	(0.0313)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.130***	0.102***	0.00154	-0.0137	0.0144
	(0.0179)	(0.0212)	(0.0613)	(0.0826)	(0.0955)
total credits		0.00948**		0.0239	-0.00207
		(0.00389)		(0.0161)	(0.0279)
Panel E. instrument: expo expenditures/teacher					
math	0.130***	0.102***	0.0103	-0.00231	0.0281
	(0.0179)	(0.0212)	(0.0671)	(0.0886)	(0.102)
total credits		0.00948**		0.0259	-0.00673
		(0.00389)		(0.0169)	(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 column 1, I present OLS estimates of the impact of math on income controlling for the set of characteristics previously mentioned. In column 2, building on column 1, I include the total number of credits as control. In column 3, the 2SLS estimates of the impact of math on income are presented. Column 4 builds upon column 3 and includes total credits as control. Finally, in column 5, both, math credits and total credits are assumed endogenous. 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 7: Impact of mathematics credits on **college attended and bachelors degree attainment**.

	(1) Ever college	(2) Ever 4-year college	(3) BA attainment
$dPr[Y = 1\|X, Z]/dmath$	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 **ivprovit** 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, $Pr_i[y = 1, X, math]$, and, (2) by increasing the number of math credits by one unit, I predicted the probability of a positive outcome $Pr_i[y = 1, X, math + 1]$. The difference is the derivative evaluated at each individual. The average marginal derivative is the average of $Pr_i[y = 1, X, math + 1] - Pr_i[y = 1, X, math]$ across all the members in the sample. The standard errors were obtained by bootstrapping 50 repetitions.

Table 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 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***	0.104***	0.307	0.305	0.319
	(0.0154)	(0.0187)	(0.187)	(0.208)	(0.199)
total credits		0.0136***		0.0135	-0.00323
		(0.00366)		(0.0286)	(0.0461)
Panel B. instrument: expo recipients					
math	0.145***	0.104***	0.182	0.116	0.133
	(0.0154)	(0.0187)	(0.114)	(0.161)	(0.164)
total credits		0.0136***		0.0335	0.0155
		(0.00366)		(0.0204)	(0.0294)
Panel C. instrument: expo expenditures					
math	0.145***	0.104***	0.187	0.131	0.149
	(0.0154)	(0.0187)	(0.121)	(0.165)	(0.168)
total credits		0.0136***		0.0310	0.0109
		(0.00366)		(0.0205)	(0.0299)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.145***	0.104***	0.00838	0.00616	0.00538
	(0.0154)	(0.0187)	(0.104)	(0.125)	(0.129)
total credits		0.0136***		0.0260	-0.000169
		(0.00366)		(0.0155)	(0.0289)
Panel E. instrument: expo expenditures/teacher					
math	0.145***	0.104***	0.0437	0.0552	0.0514
	(0.0154)	(0.0187)	(0.116)	(0.140)	(0.144)
total credits		0.0136***		0.0230	-0.00675
		(0.00366)		(0.0177)	(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 column 1, I present OLS estimates of the impact of math on income controlling for the set of characteristics previously mentioned. In column 2, building on column 1, I include the total number of credits as control. In column 3, the 2SLS estimates of the impact of math on income are presented. Column 4 builds upon column 3 and includes total credits as control. Finally, in column 5, both, math credits and total credits are assumed endogenous. 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 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***	0.105***	0.0302	0.0139	0.0119
	(0.0181)	(0.0214)	(0.191)	(0.210)	(0.223)
total credits		0.00883**		0.00950	0.0153
		(0.00392)		(0.0390)	(0.0554)
Panel B. instrument: expo recipients					
math	0.131***	0.105***	0.0886	0.0437	0.0373
	(0.0181)	(0.0214)	(0.0694)	(0.0787)	(0.0866)
total credits		0.00883**		0.0324**	0.0366
		(0.00392)		(0.0150)	(0.0314)
Panel C. instrument: expo expenditures					
math	0.131***	0.105***	0.0862	0.0460	0.0398
	(0.0181)	(0.0214)	(0.0669)	(0.0748)	(0.0848)
total credits		0.00883**		0.0313**	0.0340
		(0.00392)		(0.0146)	(0.0319)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.131***	0.105***	-0.0350	-0.0304	-0.0185
	(0.0181)	(0.0214)	(0.0790)	(0.101)	(0.108)
total credits		0.00883**		0.0255	-0.00543
		(0.00392)		(0.0177)	(0.0270)
Panel E. instrument: expo expenditures/teacher					
math	0.131***	0.105***	-0.0189	-0.0100	0.00247
	(0.0181)	(0.0214)	(0.0840)	(0.107)	(0.114)
total credits		0.00883**		0.0265	-0.0103
		(0.00392)		(0.0185)	(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 column 1, I present OLS estimates of the impact of math on income controlling for the set of characteristics previously mentioned. In column 2, building on column 1, I include the total number of credits as control. In column 3, the 2SLS estimates of the impact of math on income are presented. Column 4 builds upon column 3 and includes total credits as control. Finally, in column 5, both, math credits and total credits are assumed endogenous. 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 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***	0.0964***	0.275	0.288	0.307
	(0.0160)	(0.0187)	(0.218)	(0.236)	(0.219)
total credits		0.0127***		0.00900	-0.0167
		(0.00361)		(0.0317)	(0.0468)
Panel B. instrument: expo recipients					
math	0.134***	0.0964***	0.127	0.0582	0.0970
	(0.0160)	(0.0187)	(0.0980)	(0.131)	(0.143)
total credits		0.0127***		0.0352*	0.0105
		(0.00361)		(0.0185)	(0.0297)
Panel C. instrument: expo expenditures					
math	0.134***	0.0964***	0.132	0.0715	0.112
	(0.0160)	(0.0187)	(0.103)	(0.136)	(0.146)
total credits		0.0127***		0.0327*	0.00585
		(0.00361)		(0.0189)	(0.0302)
Panel D. instrument: expo recipients/1,000 teachers					
math	0.134***	0.0964***	0.0181	0.00160	0.0244
	(0.0160)	(0.0187)	(0.0629)	(0.0810)	(0.0987)
total credits		0.0127***		0.0229*	-0.00320
		(0.00361)		(0.0133)	(0.0306)
Panel E. instrument: expo expenditures/teacher					
math	0.134***	0.0964***	0.0325	0.0297	0.0503
	(0.0160)	(0.0187)	(0.0754)	(0.102)	(0.113)
total credits		0.0127***		0.0217	-0.0117
		(0.00361)		(0.0161)	(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 column 1, I present OLS estimates of the impact of math on income controlling for the set of characteristics previously mentioned. In column 2, building on column 1, I include the total number of credits as control. In column 3, the 2SLS estimates of the impact of math on income are presented. Column 4 builds upon column 3 and includes total credits as control. Finally, in column 5, both, math credits and total credits are assumed endogenous. 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.

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

Web Appendix

Alfredo Sosa

May 13, 2018

Table 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 recip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.301***	0.0266***	1.62e-06***	-0.00625	-0.00210
	(0.0154)	(0.00590)	(4.25e-07)	(0.146)	(0.0284)
in*state 5	-0.510***	0.000699***	7.23e-08***	0.0998***	0.0285***
	(0.0142)	(0.000151)	(1.73e-08)	(0.0226)	(0.00701)
in*state 6	-0.255***	-0.000994***	-5.57e-07***	-0.0483***	-0.0268***
	(0.0309)	(0.000165)	(9.21e-08)	(0.00647)	(0.00392)
in*state 7	0.224***	0.00282***	6.74e-07***	0.107***	0.0257***
	(0.0359)	(0.000578)	(1.37e-07)	(0.0217)	(0.00561)
in*state 8	-0.404***	0.0102***	1.48e-06***	0.0711***	0.0137***
	(0.0160)	(0.00336)	(4.82e-07)	(0.0256)	(0.00428)
in*state 10	-0.288***	9.31e-05	2.72e-08	0.00183	0.00222
	(0.0153)	(9.28e-05)	(2.71e-08)	(0.0199)	(0.00440)
in*state 11	-0.209***	0.000112***	4.19e-08***	0.0105***	0.00399***
	(0.0102)	(2.39e-05)	(8.88e-09)	(0.00237)	(0.000938)
in*state 14	-0.446***	-0.000605	-1.29e-07	-0.402***	-0.0319***
	(0.0144)	(0.000559)	(1.18e-07)	(0.0634)	(0.0114)
in*state 15	-0.416***	0.0161***	4.08e-06***	0.0524	0.0294
	(0.0151)	(0.00337)	(8.98e-07)	(0.326)	(0.239)
in*state 18	-0.327***	-0.000288***	-1.23e-07***	-0.0128***	-0.00538***
	(0.0167)	(4.90e-05)	(2.08e-08)	(0.00168)	(0.000772)
in*state 19	0.0233	0.00121	1.64e-07	-0.000307	0.000790
	(0.0594)	(0.00241)	(3.24e-07)	(0.110)	(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***	-0.000813**	-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**	-0.00197***	-0.000529***
	(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***	-0.00312***	-5.23e-07***	-0.0335***	-0.00554***
	(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	-0.0249	-0.0245	-0.0244	-0.0233	-0.0238
ratio 1996-1999	(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 reforms	-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 controls are female, white, age, household gross income between 1996-1999, household income to poverty ratio between 1996-1999, household size in 1997; a dummy indicating whether the household had two biological parents in 1997; number of math units (years) required for high school graduation, number of years of exposure to changes in high school math graduation requirements. 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 weight.

Table 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 recip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.335***	0.0165	-1.55e-06	-6.416***	-1.142***
	(0.125)	(0.0246)	(1.73e-06)	(0.766)	(0.148)
in*state 5	0.617***	0.000699	1.72e-08	0.365***	0.109***
	(0.0798)	(0.000645)	(7.26e-08)	(0.115)	(0.0354)
in*state 6	-0.999***	-0.00378***	-2.12e-06***	-0.194***	-0.111***
	(0.137)	(0.000725)	(4.05e-07)	(0.0320)	(0.0191)
in*state 7	0.853***	0.0112***	2.67e-06***	0.390***	0.0891***
	(0.167)	(0.00269)	(6.41e-07)	(0.111)	(0.0282)
in*state 8	1.345***	0.0788***	1.13e-05***	0.562***	0.104***
	(0.108)	(0.0185)	(2.67e-06)	(0.139)	(0.0227)
in*state 10	-0.399***	0.000118	3.10e-08	0.0370	-0.0105
	(0.121)	(0.000429)	(1.26e-07)	(0.0976)	(0.0214)
in*state 11	-0.0448	0.000557***	2.08e-07***	0.0502***	0.0182***
	(0.0713)	(0.000130)	(4.85e-08)	(0.0127)	(0.00496)
in*state 14	0.530***	0.00388	8.08e-07	-0.318	-0.0459
	(0.0759)	(0.00254)	(5.38e-07)	(0.349)	(0.0589)
in*state 15	1.070***	0.0178	2.33e-06	2.283	1.716
	(0.0816)	(0.0144)	(3.78e-06)	(1.552)	(1.131)
in*state 18	0.271**	0.000352	1.48e-07	0.00785	0.00284
	(0.131)	(0.000233)	(9.93e-08)	(0.00890)	(0.00403)
in*state 19	2.068***	0.0667***	8.99e-06***	2.979***	0.389***
	(0.296)	(0.0120)	(1.62e-06)	(0.580)	(0.0825)
in*state 20	0.153	0.00856	-1.39e-07	-14.83***	-4.341***
	(0.102)	(0.00927)	(1.16e-06)	(0.822)	(0.242)
in*state 21	-0.216*	-0.000405	-1.03e-07	-0.0540	-0.0127
	(0.127)	(0.00119)	(2.84e-07)	(0.0684)	(0.0175)
in*state 25	2.249***	0.00209	2.23e-07	-0.647***	-0.0137***
	(0.283)	(0.00131)	(1.43e-07)	(0.0373)	(0.00493)
in*state 26	-1.496***	-0.00492***	-3.82e-06***	-0.387***	-0.310***
	(0.351)	(0.00167)	(1.28e-06)	(0.111)	(0.0900)
in*state 32	-0.427***	0.0304	-5.33e-06	-10.27***	-2.536***
	(0.125)	(0.0502)	(5.08e-06)	(1.454)	(0.397)
in*state 33	0.555***	5.25e-05***	1.41e-08***	0.00863**	0.00224**
	(0.175)	(1.66e-05)	(4.50e-09)	(0.00345)	(0.000979)
in*state 34	0.438***	0.000189**	4.54e-08**	0.0122*	0.00270
	(0.156)	(7.85e-05)	(2.04e-08)	(0.00665)	(0.00182)
in*state 35	0.592***	0.00229***	2.18e-06***	0.0145***	0.0133**
	(0.130)	(0.000620)	(5.91e-07)	(0.00526)	(0.00535)
in*state 37	2.026***	-0.00932*	-8.77e-07*	-0.468**	-0.0506**

	(0.151)	(0.00539)	(4.97e-07)	(0.218)	(0.0216)
in*state 39	-0.186	0.000437***	1.46e-07***	0.0608***	0.0207***
	(0.127)	(0.000155)	(5.07e-08)	(0.0184)	(0.00646)
in*state 41	-0.277***	0.00719***	5.47e-07***	0.421***	0.0254***
	(0.0130)	(0.000793)	(6.29e-08)	(0.0531)	(0.00353)
in*state 42	0.665	0.0109	1.82e-06	0.0691	0.0102
	(0.399)	(0.00667)	(1.12e-06)	(0.0642)	(0.0112)
in*state 43	1.118***	0.00253**	5.87e-07**	0.103*	0.0219
	(0.150)	(0.00102)	(2.38e-07)	(0.0601)	(0.0151)
in*state 44	-0.432**	-0.000427	-9.14e-08	-0.187**	-0.0417**
	(0.175)	(0.000290)	(6.07e-08)	(0.0858)	(0.0192)
in*state 45	1.193***	0.0483***	2.41e-06***	1.752***	0.0535***
	(0.109)	(0.00556)	(2.75e-07)	(0.260)	(0.00816)
in*state 47	0.456**	0.00204**	5.99e-07**	0.140*	0.0384
	(0.222)	(0.000931)	(2.78e-07)	(0.0820)	(0.0255)
in*state 48	0.347***	0.0354	4.88e-08	4.770***	1.137***
	(0.0650)	(0.0355)	(3.13e-06)	(1.658)	(0.406)
in*state 49	-0.547***	0.0345	-3.17e-06	-95.58***	-21.53***
	(0.0819)	(0.0486)	(4.54e-06)	(5.282)	(1.198)
in*state 50	-0.734***	-0.00697*	-2.65e-06*	-0.594**	-0.230**
	(0.252)	(0.00360)	(1.33e-06)	(0.256)	(0.101)
in*state 51	1.156***	0.164	1.00e-06	-83.38***	-2.195***
	(0.276)	(0.131)	(1.45e-06)	(4.636)	(0.123)
female	1.636***	1.636***	1.637***	1.626***	1.634***
	(0.154)	(0.153)	(0.153)	(0.151)	(0.152)
white	1.089***	1.087***	1.085***	1.089***	1.089***
	(0.329)	(0.327)	(0.328)	(0.330)	(0.330)
age as of 12-31-1996	-2.524***	-2.447***	-2.454***	-2.569***	-2.597***
	(0.266)	(0.287)	(0.286)	(0.261)	(0.267)
hh gross income 1996-1999	3.33e-05**	3.31e-05**	3.31e-05**	3.32e-05**	3.31e-05**
	(1.32e-05)	(1.33e-05)	(1.33e-05)	(1.33e-05)	(1.33e-05)
hh income poverty 1996-1999	-0.161	-0.157	-0.157	-0.157	-0.156
	(0.192)	(0.194)	(0.194)	(0.193)	(0.193)
hh size 1997	-0.366***	-0.365***	-0.365***	-0.368***	-0.369***
	(0.0870)	(0.0876)	(0.0876)	(0.0872)	(0.0871)
both bio parents	1.866***	1.879***	1.879***	1.880***	1.882***
	(0.213)	(0.209)	(0.209)	(0.209)	(0.209)
expo reforms	-1.657***	-1.707***	-1.702***	-1.728***	-1.721***
	(0.269)	(0.266)	(0.267)	(0.260)	(0.262)
high school grad req 1997	5.126***	10.69*	4.557*	-590.1***	-558.9***
	(0.619)	(6.331)	(2.541)	(32.95)	(31.42)
Constant	41.10***	17.84	42.47***	2,423***	2,298***
	(4.943)	(28.22)	(13.30)	(132.7)	(126.8)
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. The controls are female, white, age, household gross income between 1996-1999, household income to poverty ratio between 1996-1999, household size in 1997; a dummy indicating whether the household had two biological parents in 1997; number of math units (years) required for high school graduation, number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table 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 recip/ 1,000 teach	(5) expo exp/teacher
in*state 4	-0.0547*	0.0505***	3.51e-06***	0.747***	0.132***
	(0.0289)	(0.00274)	(1.87e-07)	(0.0945)	(0.0158)
in*state 5	-0.0218	0.00131***	1.49e-07***	0.0434***	0.0119***
	(0.0136)	(7.52e-05)	(7.87e-09)	(0.0156)	(0.00416)
in*state 6	0.0297	0.000147*	8.12e-08*	0.00297	0.00205
	(0.0187)	(8.37e-05)	(4.69e-08)	(0.00434)	(0.00230)
in*state 7	-0.104***	-0.00115***	-2.76e-07***	-0.0611***	-0.0140***
	(0.0250)	(0.000343)	(8.21e-08)	(0.0154)	(0.00348)
in*state 8	0.0446***	0.00230	3.25e-07	0.00437	-9.26e-05
	(0.0153)	(0.00307)	(4.42e-07)	(0.0241)	(0.00376)
in*state 10	-0.0298	5.01e-05	1.43e-08	-0.0428***	-0.000540
	(0.0283)	(4.84e-05)	(1.42e-08)	(0.0133)	(0.00256)
in*state 11	-0.0570***	8.43e-05***	3.15e-08***	0.00749***	0.00292***
	(0.0178)	(1.91e-05)	(7.14e-09)	(0.00214)	(0.000761)
in*state 14	-0.0511***	-0.00113***	-2.42e-07***	-0.00451	0.0129*
	(0.0129)	(0.000307)	(6.51e-08)	(0.0486)	(0.00740)
in*state 15	-0.0246*	0.0292***	7.76e-06***	-0.0476	-0.0699
	(0.0126)	(0.00170)	(4.17e-07)	(0.203)	(0.129)
in*state 18	-0.201***	0.000156***	6.64e-08***	0.00544***	0.00239***
	(0.0300)	(2.74e-05)	(1.17e-08)	(0.00115)	(0.000467)
in*state 19	0.314***	0.00986***	1.33e-06***	0.440***	0.0609***
	(0.0518)	(0.00177)	(2.40e-07)	(0.0853)	(0.0111)
in*state 20	0.00232	0.0185***	2.27e-06***	2.747***	0.791***
	(0.0173)	(0.00110)	(1.31e-07)	(0.102)	(0.0283)
in*state 21	-0.00827	-0.000780***	-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***	-0.000504***	-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**	-0.00293***	-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***
	(0.0173)	(0.00585)	(5.31e-07)	(0.660)	(0.141)
in*state 50	0.353***	0.00405***	1.50e-06***	0.217***	0.0822***
	(0.0391)	(0.000424)	(1.57e-07)	(0.0340)	(0.0117)
in*state 51	-0.365***	0.247***	2.47e-06***	15.40***	0.399***
	(0.0474)	(0.0162)	(1.79e-07)	(0.581)	(0.0145)
female	-0.370***	-0.374***	-0.374***	-0.371***	-0.371***
	(0.0329)	(0.0329)	(0.0329)	(0.0333)	(0.0333)
white	0.105**	0.105**	0.104**	0.105**	0.106**
	(0.0422)	(0.0424)	(0.0427)	(0.0424)	(0.0428)
age as of 12-31-1996	-0.159***	-0.149***	-0.150***	-0.169***	-0.167***
	(0.0365)	(0.0377)	(0.0380)	(0.0356)	(0.0359)
hh gross income 1996-1999	1.32e-06	1.33e-06	1.34e-06	1.28e-06	1.30e-06
	(1.35e-06)	(1.35e-06)	(1.35e-06)	(1.37e-06)	(1.36e-06)
hh income poverty ratio	0.0193	0.0189	0.0189	0.0202	0.0197
1996-1999	(0.0190)	(0.0190)	(0.0190)	(0.0194)	(0.0193)
hh size 1997	-0.00636	-0.00693	-0.00693	-0.00582	-0.00583
	(0.0150)	(0.0149)	(0.0149)	(0.0150)	(0.0149)
both bio parents	0.0903***	0.0910***	0.0911***	0.0891***	0.0891***
	(0.0315)	(0.0321)	(0.0321)	(0.0321)	(0.0322)
expo reforms	-0.118**	-0.131**	-0.130**	-0.134**	-0.135**
	(0.0554)	(0.0600)	(0.0599)	(0.0610)	(0.0609)
high school grad req 1997	0.189*	12.78***	5.191***	110.4***	103.0***
	(0.110)	(0.785)	(0.316)	(4.136)	(3.712)
Constant	12.19***	-38.27***	-7.902***	-428.6***	-398.9***
	(0.635)	(3.225)	(1.447)	(16.43)	(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. The controls are female, white, age, household gross income between 1996-1999, household income to poverty ratio between 1996-1999, household size in 1997; a dummy indicating whether the household had two biological parents in 1997; number of math units (years) required for high school graduation, number of years of exposure to changes in high school math graduation requirements. State and cohort fixed effects were also included. All regressions use the 1997 weight.

Table 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)	(2)	(3)
	cohort 28-29	cohort 30-35	cohort 36-40
expo	0.0209***	0.00826**	0.000163
	(0.00574)	(0.00389)	(0.0154)
female	-0.222***	-0.300***	-0.399***
	(0.0164)	(0.0128)	(0.0128)
white	0.0204**	0.0196**	0.0101
	(0.00995)	(0.00790)	(0.00798)
black	0.0108	0.0284**	0.0641***
	(0.0193)	(0.0123)	(0.0116)
Asian	0.0267	0.0744***	0.0918***
	(0.0165)	(0.0136)	(0.0139)
birthday year	-0.0291***	-0.0268***	-0.000525
	(0.00888)	(0.00194)	(0.00169)
poverty	0.00369***	0.00394***	0.00416***
	(3.89e-05)	(3.61e-05)	(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 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***	0.0459**	0.133***
	(0.0287)	(0.0213)	(0.0486)
female	-0.100**	-0.0708***	-0.0996***
	(0.0406)	(0.0157)	(0.0139)
white	0.152*	0.220***	0.163**
	(0.0779)	(0.0631)	(0.0694)
birth year	-0.0510	-0.0272***	-0.00934
	(0.0305)	(0.00699)	(0.00586)
poverty	0.000131***	5.70e-05	0.000207***
	(3.63e-05)	(3.89e-05)	(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 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***	0.0120***	
	(0.00195)	(0.00245)	
expo*state 5	0.0292***	0.0100***	0.0246***
	(0.00199)	(0.00210)	(0.00441)
expo*state 6	0.0126		
	(0.00943)		
expo*state 8	-0.0270***	0.0525***	-0.137***
	(0.00246)	(0.00229)	(0.00424)
expo*state 10	0.0148***	0.0228***	-0.0172***
	(0.00201)	(0.00220)	(0.00438)
expo*state 11	-0.00979***	0.0173***	
	(0.00246)	(0.00205)	
expo*state 13		-0.0593***	-0.249***
		(0.00570)	(0.00429)
expo*state 14	0.0321***	-0.00259	0.0839***
	(0.00191)	(0.00240)	(0.00432)
expo*state 15	-0.0451***	0.00310	-0.0143***
	(0.00221)	(0.00238)	(0.00418)
expo*state 18	0.0536***	0.0399***	-0.0697***
	(0.00179)	(0.00282)	(0.00458)
expo*state 20	0.0875***	-0.0848***	
	(0.00721)	(0.00371)	
expo*state 21	0.0407***	0.00592**	-0.0150***
	(0.00285)	(0.00227)	(0.00432)
expo*state 25	0.178***	0.0397***	0.0562***
	(0.00169)	(0.00253)	(0.00455)
expo*state 32	0.0144***	0.0263***	
	(0.00166)	(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***	-0.0202***	-0.0795***
	(0.00191)	(0.00237)	(0.00437)
expo*state 39	-0.0724***	-0.000387	-0.0571***
	(0.00184)	(0.00300)	(0.00443)

expo*state 41	0.00923***	0.0113***	0.0175***
	(0.00171)	(0.00242)	(0.00412)
expo*state 43	0.0304***	0.0216***	-0.0287***
	(0.00163)	(0.00244)	(0.00442)
expo*state 45	0.00268*	-0.0362***	
	(0.00146)	(0.00217)	
expo*state 48	-0.0409***	0.00273	0.0238***
	(0.00226)	(0.00234)	(0.00441)
expo*state 49	6.003***	-0.0215***	0.0337***
	(1.605)	(0.00330)	(0.00436)
expo*state 50	0.110***	-0.0213***	
	(0.00554)	(0.00361)	
expo*state 51	-0.0614***	0.0174***	
	(0.00351)	(0.00266)	
female	-0.222***	-0.300***	-0.399***
	(0.0164)	(0.0129)	(0.0128)
white	0.0200*	0.0196**	0.00999
	(0.00994)	(0.00782)	(0.00801)
black	0.0103	0.0282**	0.0637***
	(0.0194)	(0.0121)	(0.0116)
asian	0.0262	0.0746***	0.0913***
	(0.0166)	(0.0135)	(0.0139)
bday_year	-0.0318***	-0.0267***	-0.000511
	(0.00972)	(0.00195)	(0.00169)
poverty	0.00369***	0.00394***	0.00416***
	(3.92e-05)	(3.63e-05)	(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 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) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo*state 4	0.348***	0.0334***	
	(0.0114)	(0.00626)	
expo*state 5	0.191***	0.0948***	0.208***
	(0.00709)	(0.00572)	(0.0180)
expo*state 8	-0.789***	0.256***	0.542***
	(0.0227)	(0.0122)	(0.0295)
expo*state 10	0.429***	0.0368***	0.128***
	(0.0122)	(0.00623)	(0.0196)
expo*state 11	0.0651***	-0.0418***	
	(0.0118)	(0.00762)	
expo*state 13		0.142***	0.180***
		(0.00842)	(0.0246)
expo*state 14	0.0205**	0.0488***	0.164***
	(0.00810)	(0.00584)	(0.0217)
expo*state 15	0.144***	0.0708***	-0.138***
	(0.00389)	(0.00631)	(0.0181)
expo*state 18	-15.01**	-0.0147	0.418***
	(7.263)	(0.0100)	(0.0189)
expo*state 20	-0.0209	-0.235***	
	(0.0387)	(0.0362)	
expo*state 21	0.153***	0.0455***	-0.0890***
	(0.0126)	(0.00713)	(0.0210)
expo*state 25	-0.0636***	0.0480***	-1.473***
	(0.00987)	(0.00492)	(0.0277)
expo*state 32	-15.10**	0.178***	
	(7.262)	(0.00688)	
expo*state 33	0.412***		
	(0.0427)		
expo*state 37	0.267***	0.0908***	-0.0226
	(0.00506)	(0.00602)	(0.0240)
expo*state 39	0.0611***	0.0772***	-0.135***
	(0.00798)	(0.00716)	(0.0199)
expo*state 41	0.121***	-0.0884***	0.447***
	(0.00750)	(0.00734)	(0.0202)
expo*state 43	0.183***	-0.0634***	0.442***
	(0.00818)	(0.00708)	(0.0185)
expo*state 45	-0.107***	-0.0873***	
	(0.0395)	(0.00969)	
expo*state 48	0.199***	-0.0638***	0.0645***
	(0.00642)	(0.00703)	(0.0190)

expo*state 49	-0.718***	-0.0106	0.344***
	(0.0256)	(0.0105)	(0.0230)
expo*state 50	-0.101***	-0.123***	
	(0.0241)	(0.0212)	
expo*state 51	-0.874***	0.263***	
	(0.0347)	(0.0279)	
female	-0.0981**	-0.0708***	-0.0994***
	(0.0401)	(0.0158)	(0.0140)
white	0.153*	0.219***	0.161**
	(0.0788)	(0.0626)	(0.0689)
birth year	-0.0522*	-0.0247***	-0.00941
	(0.0295)	(0.00679)	(0.00585)
poverty	0.000125***	6.08e-05	0.000208***
	(3.69e-05)	(3.90e-05)	(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 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	0.00179
	(0.0131)	(0.00913)
female	0.0966***	0.0376***
	(0.00902)	(0.00537)
white	0.0168*	0.00663
	(0.00995)	(0.00629)
age as of 12-31-1996	-0.0257***	-0.00728
	(0.00890)	(0.00441)
hgc bio dad	0.0103***	0.00307**
	(0.00235)	(0.00130)
hgc bio mom	0.0115***	0.00369**
	(0.00267)	(0.00171)
household gross income 1997	9.34e-07**	6.32e-07**
	(3.76e-07)	(2.88e-07)
household income poverty ratio 1997	-0.000123**	-8.89e-05**
	(5.66e-05)	(4.22e-05)
reforms_expo	-0.000855	-0.00164
	(0.00811)	(0.00362)
high school	0.0309	0.00980
math requirements	(0.0387)	(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 9: **ACS 09**. Impact of potential years of exposure **during college** on the probability of teaching.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo college	0.00602***	0.00503***	0.00264***
	(0.000603)	(0.000583)	(0.000896)
female	0.0458***	0.0463***	0.0475***
	(0.00386)	(0.00180)	(0.00250)
white	0.0114***	0.0130***	0.0148***
	(0.00369)	(0.00325)	(0.00161)
black	-0.00338	0.00133	0.00404*
	(0.00359)	(0.00334)	(0.00229)
asian	-0.0216***	-0.0216***	-0.0114***
	(0.00402)	(0.00263)	(0.00176)
birthday year	-0.00727***	7.13e-05	-0.000418
	(0.00170)	(0.000362)	(0.000530)
poverty	0.000122***	0.000115***	9.93e-05***
	(1.06e-05)	(6.67e-06)	(6.47e-06)
math reform	-4.763***	0.0445	-0.782
	(1.121)	(0.239)	(1.046)
high school math	4.782***	-0.0526	0.397
requirements	(1.121)	(0.239)	(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 10: **SIPP 08**. Impact of potential years of exposure **during college** on the probability of teaching.

	(1) cohort 28-29	(2) cohort 30-35	(3) cohort 36-40
expo college	0.0113***	0.00972***	0.00401
	(0.00207)	(0.00203)	(0.00414)
female	0.0513***	0.0390***	0.0471***
	(0.00760)	(0.00555)	(0.00529)
white	0.0291*	0.00490	0.00995**
	(0.0162)	(0.00824)	(0.00442)
birthday year	0.000884	-0.00239**	3.94e-05
	(0.00440)	(0.00105)	(0.00150)
poverty	-3.15e-05***	-4.37e-06	-2.06e-06
	(7.09e-06)	(5.70e-06)	(5.00e-06)
math reform	1.906	-4.631**	-0.0922
	(8.706)	(2.071)	(0.985)
high school math	-0.904	2.342**	0.0113
requirements	(4.353)	(1.036)	(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 11: **CCD**. Impact of implementing a STEM teacher recruitment program on the number of teachers.

	(1) Elementary Teachers	(2) Secondary Teachers	(3) Total Teachers
STEM Program	-24.55	1,443	1,357
	(840.3)	(898.5)	(1,283)
total students	0.0249***	0.00657	0.0613***
	(0.00457)	(0.00655)	(0.0109)
high school students	0.0121	0.0433***	0.0120
	(0.0102)	(0.0136)	(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 12: **CCD**. Impact of implementing a STEM teacher recruitment program on the log of the number of teachers.

	(1) ln Elementary Teachers	(2) ln Secondary Teachers	(3) ln Total Teachers
STEM program	-0.0173	0.000120	-0.0119
	(0.0266)	(0.0313)	(0.0173)
total students	3.08e-07**	2.28e-07	3.79e-07***
	(1.16e-07)	(1.62e-07)	(9.74e-08)
high school students	-1.97e-07	8.56e-08	-3.60e-07
	(2.71e-07)	(3.39e-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 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	0.00406
	(0.0131)	(0.00578)
expo_college*state 5	0.0254	0.000159
	(0.0207)	(0.0101)
expo_college*state 6	0.0187	0.0451***
	(0.0113)	(0.00680)
expo_college*state 7	0.0114*	-0.0275***
	(0.00610)	(0.00381)
expo_college*state 8	0.0375*	-0.00834
	(0.0201)	(0.0101)
expo_college*state 10	0.0143	-0.000573
	(0.0129)	(0.00563)
expo_college*state 11	0.0137	-0.00541
	(0.0115)	(0.00543)
expo_college*state 12	0.000101	-0.00641
	(0.0172)	(0.00836)
expo_college*state 14	0.0235	0.00513
	(0.0209)	(0.0102)
expo_college*state 15	0.0105	-0.00577
	(0.0210)	(0.0103)
expo_college*state 17	0.0417***	0.0422***
	(0.00576)	(0.00251)
expo_college*state 18	0.0409***	0.0217***
	(0.0124)	(0.00521)
expo_college*state 19	-0.00625	-0.0196***
	(0.00667)	(0.00337)
expo_college*state 20	0.0128	0.00101
	(0.0248)	(0.0119)
expo_college*state 21	-0.00157	-0.00891
	(0.0129)	(0.00569)
expo_college*state 25	0.0371*	0.0235**
	(0.0208)	(0.0110)
expo_college*state 26	-0.00892	-0.00929***
	(0.00535)	(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**	6.69e-07**
	(3.74e-07)	(2.90e-07)
household income poverty ratio 1997	-0.000130**	-9.48e-05**
	(5.61e-05)	(4.26e-05)
reforms_expo	-0.00415	-0.00376
	(0.00955)	(0.00399)
high school math	0.0411	0.0124
requirements	(0.0405)	(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 requirememnts 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 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***	0.00845***	0.0132***
	(0.000398)	(0.000340)	(0.000854)
expo_college*state 5	0.00579***	0.00574***	0.00276***
	(0.000415)	(0.000373)	(0.000660)
expo_college*state 6	-0.00486***	0.00413***	
	(0.000758)	(0.00109)	
expo_college*state 7	0.0273***		
	(0.00120)		
expo_college*state 8	0.000759	0.00876***	0.00502***
	(0.000508)	(0.000241)	(0.000682)
expo_college*state 10	0.00563***	0.00441***	0.00186***
	(0.000460)	(0.000303)	(0.000637)
expo_college*state 11	0.00561***	0.00590***	-0.00256*
	(0.000605)	(0.000424)	(0.00145)
expo_college*state 12	0.00929***		
	(0.00188)		
expo_college*state 13		-0.00203*	-0.00595***
		(0.00106)	(0.00139)
expo_college*state 14	0.00624***	0.00496***	0.00593***
	(0.000531)	(0.000452)	(0.000654)
expo_college*state 15	0.00771***	0.00574***	-0.00125**
	(0.000454)	(0.000283)	(0.000610)
expo_college*state 18	0.00589***	0.00637***	-0.00100
	(0.000455)	(0.000274)	(0.000631)
expo_college*state 19	-0.00566***		
	(0.00190)		
expo_college*state 20	0.00783***	0.00248***	
	(0.000364)	(0.000393)	
expo_college*state 21	0.00496***	0.00524***	0.00188***
	(0.000652)	(0.000397)	(0.000645)
expo_college*state 25	0.00761***	0.00738***	0.00245***
	(0.000432)	(0.000313)	(0.000620)
expo_college*state 26	0.00336*		
	(0.00194)		
expo_college*state 32	0.00834***	0.00662***	0.0106***
	(0.000282)	(0.000231)	(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 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***	-0.0177***	0.00967***
	(0.00203)	(0.000646)	(0.00311)
expo college*state 5	0.0154***	0.0141***	0.00458**
	(0.000743)	(0.000973)	(0.00192)
expo college*state 6	-0.0166***		
	(0.00388)		
expo college*state 7	-0.124***		
	(0.00770)		
expo college*state 8	0.00262	0.0228***	0.212***
	(0.00167)	(0.00163)	(0.00191)
expo college*state 10	0.00601***	0.0105***	2.33e-05
	(0.000828)	(0.000723)	(0.00219)
expo college*state 11	0.0131***	0.0138***	-0.0122**
	(0.000540)	(0.000935)	(0.00509)
expo college*state 12	0.00146		
	(0.0103)		
expo college*state 13		-0.00402	-0.0316***
		(0.00270)	(0.00386)
expo college*state 14	0.00707***	0.0103***	-0.0169***
	(0.00115)	(0.000725)	(0.00218)
expo college*state 15	0.0142***	0.0130***	0.00448**
	(0.000765)	(0.000730)	(0.00209)
expo college*state 18	0.0117***	0.0310***	0.0344***
	(0.00103)	(0.00112)	(0.00210)
expo college*state 20	0.0812***	0.00898***	
	(0.00907)	(0.00194)	
expo college*state 21	0.00740***	0.00224	0.0204***
	(0.00118)	(0.00146)	(0.00205)
expo college*state 25	-0.00247	0.00178***	0.0151***
	(0.00185)	(0.000513)	(0.00215)
expo college*state 32	0.0263***	0.00927***	-0.0212***
	(0.00233)	(0.000553)	(0.00284)
expo college*state 33	0.0104**	-0.0163***	
	(0.00407)	(0.00400)	
expo college*state 34	0.0194***		
	(0.00406)		
expo college*state 35	-0.0222**		
	(0.00972)		
expo college*state 37	0.00892***	0.0180***	-0.00856***
	(0.00136)	(0.000637)	(0.00208)
expo college*state 39	0.0157***	0.0137***	-0.00130
	(0.00142)	(0.000892)	(0.00246)

expo college*state 41	0.0178***	0.00801***	-0.0181***
	(0.00111)	(0.000410)	(0.00227)
expo college*state 43	0.00450***	0.00366***	0.00788***
	(0.00127)	(0.000751)	(0.00220)
expo college*state 44	0.0328***		
	(0.00606)		
expo college*state 45	0.0162***	-0.00402***	
	(0.00101)	(0.00122)	
expo college*state 47	-0.0317***		
	(0.00683)		
expo college*state 48	0.0134***	-0.0159***	0.0163***
	(0.00117)	(0.00117)	(0.00220)
expo college*state 49	-0.211	0.00444***	0.0334***
	(0.919)	(0.000739)	(0.00260)
expo college*state 50	-0.00999***	0.00520***	
	(0.00188)	(0.00147)	
expo college*state 51	-0.249	-0.000595	
	(0.919)	(0.00140)	
female	0.0514***	0.0395***	0.0467***
	(0.00764)	(0.00561)	(0.00529)
white	0.0296*	0.00516	0.00978**
	(0.0163)	(0.00824)	(0.00435)
birthday year	0.00148	-0.00222**	0.000186
	(0.00557)	(0.00107)	(0.00152)
poverty	-3.14e-05***	-4.20e-06	-1.60e-06
	(7.13e-06)	(5.73e-06)	(5.04e-06)
math reform	1.012	-1.394*	0.00453
	(3.680)	(0.700)	(1.002)
high school math requirements	-0.971	1.448**	-0.0852
	(3.677)	(0.704)	(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 16: **BLS**. Impact of Economic Conditions on States' decisions of implementing a STEM teacher recruitment program.

	(1) STEM	(2) STEM lead 1 year	(3) STEM lead 2 years
annual median wages	0.000000457	0.000000748	0.00000174
	(0.00000124)	(0.00000148)	(0.00000146)
annual mean wages	0.000000805	0.00000063	0.00000122
	(0.000000989)	(0.00000111)	(0.00000111)
hourly median wages	0.000948	0.00155	0.00361
	(0.00258)	(0.00308)	(0.00303)
hourly mean wages	0.00167	0.00131	0.00255
	(0.00206)	(0.00230)	(0.00232)
Percent on poverty	0.0155*	0.0130	0.00835
	(0.00865)	(0.00967)	(0.00968)
Unemployment rate	0.0140	0.00806	-0.00524
	(0.0212)	(0.0207)	(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 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) hourly mean	(2) hourly median	(3) annual mean	(4) annual median
Engineering	-0.00260	-0.000699	-0.00000124	-0.000000313
	(0.0147)	(0.0144)	(0.00000704)	(0.00000693)
Math	0.0125	0.0115	0.000006	0.00000553
	(0.0115)	(0.0102)	(0.00000554)	(0.00000489)
Business	0.0449**	0.0512**	0.0000216**	0.0000246**
	(0.0204)	(0.0247)	(0.0000098)	(0.0000119)
Health	-0.00661	-0.0364	-0.00000314	-0.0000175
	(0.0160)	(0.0240)	(0.00000769)	(0.0000115)
Education	0.00666	-0.00235	0.00000321	-0.0000011
	(0.0173)	(0.0152)	(0.00000831)	(0.0000073)
Law	-0.0119*	-0.0153**	-0.00000571*	-0.00000735**
	(0.00653)	(0.00673)	(0.00000314)	(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.

$$y_i = \beta_0 + \beta_1 x_i + \epsilon_i \tag{1}$$

In this case, the estimate of β_1 is:

$$\hat{\beta_1} = \frac{Cov(y_i, x_i)}{Var(x_i)} = \frac{Cov(\beta_0 + \beta_1 x_i + \epsilon_i, x_i)}{Var(x_i)} = \beta_1 + \frac{Cov(\epsilon_i, x_i)}{Var(x_i)} \tag{2}$$

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:

$$y_i = \beta_0 + \beta_{1i} x_i + \epsilon_i \tag{3}$$

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

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

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

$$\beta_{1i} = \gamma_0 + \gamma_1 x_i \tag{5}$$

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

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

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.

$$\gamma_0 + \gamma_1 \frac{Cov(x_i^2, x_i)}{Var(x_i)} > \beta_1 \tag{8}$$

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.