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Essays in Education and Labor Economics

DISSERTATION

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for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Brittany Bass

Dissertation Committee:
Professor David Neumark, Chair
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ABSTRACT OF THE DISSERTATION

Essays in Education and Labor Economics

By

Brittany Bass

Doctor of Philosophy in Economics

University of California, Irvine, 2019

Professor David Neumark, Chair

This dissertation examines how sex education mandates affect teenage sexual behaviors and health, how an introduction of a paid parental leave scheme affects maternal labor market outcomes, and how school-level technology investment impacts student achievement. The data used for this dissertation include publicly available student-level data, confidential micro-data from the Australian government, and publicly available school-level data from the California Department of Education. The empirical methods used in this dissertation include difference-in-difference models, and regression discontinuity models. In the first chapter, I show that state-mandated school-based sex education has no significant impact on teenage sexual behaviors, gonorrhea rates, or birth rates. In the second chapter, I develop theoretical predictions of the impact of an introduction of a paid parental leave scheme on maternal labor market outcomes in Australia, and empirically test these predictions. I find no evidence that Australia's paid parental leave scheme impacted maternal labor market outcomes in the short-run. In the third chapter, I examine the impact of school-level technology investment on student achievement in California, and find positive effects on English test scores. I find that the effects are largest for middle schoolers, and are concentrated among the low-socioeconomic students, suggesting that technology investment can help narrow the income achievement gap.

Chapter 1

Let's Talk About Sex Education: The Effect of State-Mandated School-Based Sex Education on Teenage Sexual Behaviors and Health

1.1 Introduction

In the mid-1980s, once it was recognized that AIDS could be spread via sexual intercourse, Surgeon General Everett Koop called for increased sex education in schools beginning as early as the third grade (Cornblatt, 2009). By the late 1980s and 1990s, many states started implementing HIV and Sexually Transmitted Disease (STD) education and sex education due to concerns over HIV and teenage pregnancy. As the level of concern over HIV/STDs and teenage pregnancy steadily increased, states continued to implement and encourage sex education (Carter, 2001). As of 2018, 24 states and Washington DC mandate school-based sex education. There is, however, considerable heterogeneity in the timing and comprehensiveness of these mandates. For example, some states' sex education programs are comprehensive in nature, meaning they require information on adolescent development, conception and pregnancy, abstinence and contraception effectiveness, while others are solely abstinence-based (Mullinax et al., 2017).

Despite the potential benefits of sex education, these classes remain controversial. The debate over school-based sex education in the United States is centered on two major questions: do schools have a responsibility to teach students about issues related to sex, and if schools do teach sex education, what type of information should be presented? That is, what type of sex education is most effective at preventing teen pregnancy, and reducing disease transmission, for example. Using data from the Youth Risk Behavioral Surveys, National Vital Statistics, and the Center for Disease Control's Wonder statistics on STDs, this study presents the first examination of the effect of state-level sex education mandates on teenage sexual behaviors, teenage gonorrhea rates and teenage birth rates.

The primary goal of school-based sex education is to help young people build a foundation to mature into sexually healthy adults by assisting them in understanding a positive view of sexuality, providing them with information and skills for taking care of their sexual health, and promoting youth to make sound decisions now and in the future (Bridges & Hauser, 2014). As evidenced by the Surgeon General's call, sex education programs are also viewed as an informational policy tool intended to reduce the future costs of STDs and teen pregnancy (Sabia, 2006).

Sexually transmitted diseases are a severe public health problem in the United States. STDs cause harmful, often irreversible, and costly complications, especially among females (Weinstock et al., 2004). There are approximately 20 million new STD infections each year. Nearly half of these infections are among young people ages 15 to 24, who represent only twenty-five percent of the sexually active population (Weinstock et al., 2004). The estimated cost to the US health care system from these new infections is $16 billion annually, including HIV and HPV (human papillomavirus) diagnoses (Workowski & Bolan, 2015). Among the non-viral STDs, chlamydia and gonorrhea are the most common and costly infections, estimated at almost $517 million and $162 million in annual health care costs, respectively (Owusu-Edusei Jr et al., 2013).

Likewise, the economic costs of teenage childbearing may be sizable, especially for taxpayers and society as a whole. Several studies have found that teen childbearing is associated with declines in human capital attainment or future earnings for the teen mother (J. D. Angrist & Evans, 1999; Bronars & Grogger, 1994; Fletcher & Wolfe, 2009; Kane et al., 2013). Fletcher & Wolfe (2012) examine the consequences of teenage fatherhood by comparing young fathers to men whose partners miscarried, and find that teenage fatherhood decreased years of schooling and the likelihood of receiving a high school diploma, and had mixed short-term effects on young fathers' labor market outcomes. Evidence also suggests that the children of teenage mothers tend to fare poorly (i.e. lower cognitive development, academic achievement, health, behavior, etc.) compared to children born to older mothers (Hoffman & Maynard, 2008). The causal link between the effect of teen childbearing on the children is questioned due to these studies simply comparing children born to teenagers to children born to older individuals.

Given the public health and economic implications of teenage STD infections and births, it is important to understand the role sex education plays, if any, in reducing these occurrences. Several empirical studies have examined the relationship between sex education and teenage sexual behaviors and health. The most convincing studies implement randomized control trials (RCTs) to examine the impact of sex education. Bennett & Assefi (2005) conduct a review of all RCTs of school-based teen pregnancy prevention programs from 1980 to 2002. Among the 16 studies reviewed, 3 examined abstinence-only programs, 12 examined abstinence-plus programs (which provide information about contraception), and 1 compared an abstinence-only with an abstinence-plus program. Abstinence-only programs had no statistically significant effect on the frequency of sexual intercourse, number of sexual partners, contraception use, or pregnancy.[1] However, the majority of abstinence-plus programs analyzed significantly increased rates of contraception use in teens, with mixed results on the frequency of sexual activity.[2] Jemmott et al. (2010) is the one study that conducts a RCT which directly compares the effect of an abstinence-only intervention to comprehensive interventions on youth sexual behaviors. The authors find that the abstinence-only intervention reduced the probability of sexual intercourse, but had no effect on condom use, while the comprehensive interventions reduced reports of having multiple sexual partners. Although the RCT results of the effect of sex education on teenage sexual behaviors can be interpreted as causal, most of the RCTs were targeted toward at-risk populations, therefore lacking external validity.

Studies using a quasi-experimental approach to assess the impact of sex education have generally found no evidence that sex education significantly impacts teenage sexual behaviors and health. Sabia (2006) uses data from the National Longitudinal Study of Adolescent

[1] The one exception is Jorgensen et al. (1993) who find that abstinence-only programs increased the age of sexual initiation.

[2] See DiCenso et al. (2002); Kirby (2008) for an additional review of sex education RCTs.

Health and finds that the causal link between sex education and adverse health outcomes disappears after controlling for unobserved heterogeneity via fixed effects and instrumental variables. Carr & Packham (2016) use a difference-in-difference design to estimate the effect of state-level abstinence education mandates on teen health outcomes. The authors find that the abstinence education mandates have no effect on teen birth rates or abortion rates, and may potentially affect teen STD rates in some states. Additionally, Cannonier (2012) uses state-level data to analyze the effect of the Title V, Section 510 State Abstinence Education (SAE) on 15-17 year old birth rates. The author finds that for an average state, increasing spending by $50,000/year on SAE can help avoid approximately four teenage births. Kearney & Levine (2015) also use a difference-in-difference design to determine the role of state-level demographic changes, economic conditions, and targeted policies on recent trends in the US teen birth rate. The authors control for state-level sex education curriculum and sex education curriculums requiring the teaching of contraception in their model, and find that these policies have no effect on the teen birth rate.

This study contributes to the existing quasi-experimental literature on sex education in several important ways. First, I exploit within-state variation in state-mandated school-based sex education from 1997-2013 to estimate the effect on teenage sexual behaviors, gonorrhea rates, and birth rates. Importantly, this is the first paper to simultaneously estimate the effect of three types of state-mandated sex education - abstinence-based, comprehensive, and unspecified[3]- which allows me to directly compare the effects of each type of education on teenage sexual behaviors and health. Second, I am the first to examine the effect of sex education on teenage sexual behaviors using data drawn from repeated cross-sections of both the National and State Youth Risk Behavior Surveys (YRBS) from 1997 to 2013. The use of individual-level data allows me to estimate the "first-stage" effect of sex education mandates on measures of teenage sexual behaviors, such as sexual activity, condom use or

[3]Unspecified sex education refers to states that enact a sex education mandate but do not specify the type of sex education that is to be taught.

contraception use at last sex, and multiple sexual partners. Finally, this study is one of the first to credibly explore whether the effect of school-based sex education extends to teenage gonorrhea rates, an outcome that has both private and public health consequences.

Difference-in-difference results show that any state-mandated sex education does not significantly impact teenage sexual behaviors. Additionally, difference-in-difference-in-difference results, which leverage 20-24 year olds as a within-state control group, show no effect on teenage gonorrhea or birth rates. Exploring sex education policy heterogeneity, difference-in-difference estimates suggest that comprehensive sex education significantly decreases teenage condom use, and unspecified sex education decreases female birth control pill use, and increases engaging with multiple sexual partners. This negative effect of sex education on teenage sexual behaviors does not translate to unfavorable sexual health outcomes, as sex education has no significant impact on teenage gonorrhea or birth rates. However, the heterogeneous results are interpreted with caution due to variation and classification issues.

1.2 Background on school-based sex education

Support for sex education began in the late 1800s when mass public campaigns promoted the "regulation of sexuality" and emphasized risk-reduction practices and health care prevention in response to cholera and syphilis epidemics (Irvine, 2004). Momentum continued throughout the early and mid-1900s, until opposition towards sex education began to be organized by groups such as the John Birch Society, Christian Crusade, and Parents Opposed to Sex and Sensitivity Education (Scales, 1981).[4] By the early 1970s, twenty states had voted to restrict or abolish sex education.

As mentioned above, the 1980s brought renewed interest in sex education as a result of

[4]These groups argued that sex education was "smut", "immoral", and "a filthy communist plot" (Scales, 1981).

concerns over teen pregnancy and HIV/AIDS that motivated widespread public support for sex education in schools (SIECUS, 2010). As a response, the Reagan administration began federal funding for abstinence-only-until-marriage programs, which gained momentum during the 1990s and early 2000s, and has served as a strong incentive for states to adopt this type of curriculum. Since 1997, Congress has provided over $1.5 billion for abstinence-only programs (SIECUS, 2010).

Federal funds for such programs began easing after a report by Mathematica Policy Research was released in 2007 that found abstinence-only programs had no effect on sexual behavior outcomes (Trenholm et al., 2007).[5] In 2014, at the request of the Obama administration, Congress provided $185 million for medically accurate and age-appropriate sex education programs. In his proposed federal budget for 2017, former President Obama removed all funding for abstinence-only education.[6] Additionally, many states have responded to parents' and communities' calls to provide education on not only abstinence, but on comprehensive topics, like contraception, STDs, HIV, and the proper use of condoms (Bleakley et al., 2006). Although the federal government has provided funding for public and private sex education programs, there is no federal law or policy that requires sex education to be taught in schools. Rather, the decision to mandate school-based sex education is left up to the state and local school districts[7]. Many states that have mandated sex education in schools have developed curriculum or guidelines for local school districts to aid with the implementation of sex education (Donovan, 1998).[8]

[5] The Mathematica study examines the effect of four Title V, Section 510 grants: My Choice, My Future; ReCapturing the Vision; Families United to Prevent Teen Pregnancy; and Teens in Control, on teens' sexual abstinence, their risks of pregnancy and sexually transmitted diseases, and other behavioral outcomes.

[6] Federal funding for abstinence-only programs included funding for the Community-Based Abstinence Education grant program and the abstinence-only funding granted as part of the Adolescent Family Life Act, among others. Funding under the Obama administration was slated for "competitive contracts and grants to public and private entities to fund medically accurate and age appropriate programs that reduce teen pregnancy"(111th Congress, 2009).

[7] It may be that some states that do not mandate all schools to teach sex education have some school districts within the state that do. If this is the case, then the estimates presented in this study will be biased downward. Unfortunately, district-level data on sex education mandates and the outcomes of interest are not available.

[8] To take some examples, in 1997, the Hawaii legislature adopted a resolution to improve professional training

1.3 Conceptual Framework

According to the National Sexuality Education Standards (NSES), a representative school-based sex education mandate should include information on seven key components: anatomy and physiology, puberty and adolescent development, identity, pregnancy and reproduction, STDs and HIV, healthy relationships, and personal safety (Barr et al., 2014). However, the general requirements for school-based sex education courses vary significantly across states. Some states require abstinence-based sex education, which stresses or covers the importance of abstinence until marriage and may include some information on contraceptives, such as contraception types and failure rates. Other states are comprehensive in nature, meaning they provide information that is closely aligned with NSES's seven components.

How might school-based sex education affect teenage sexual behavior? Oettinger (1999) was the first study to develop a theoretical model of a teen's decision to be sexually active. His framework suggests that rational individuals become sexually active at the age at which the perceived benefits from sexual intercourse surpass the perceived costs (Oettinger, 1999).[9] If sex education teaches teenagers about the costs associated with pregnancy and sexually transmitted diseases, including mental, physical, and monetary costs, their expected costs and benefits of engaging in sexual behaviors may be altered. However, it is important to note that teenagers may not be making rational decisions when deciding whether or not to

in sex education and requires all health teachers be certified to teach health, take five continuing education classes in health-related areas (including teenage pregnancy, STD and HIV prevention), and be evaluated by students (Hawaii Legislature, House Resolution No. 32). In April 2012, the District of Columbia implemented the District of Columbia Comprehensive Assessment System (DC CAS) for Health and Physical Education. DC CAS is the first statewide standardized test that measures students' proficiency in physical education, sexual health, nutrition, and other health related topics (Office of the State Superintendent of Education, 2017). In 2010, North Carolina updated their abstinence-only-until-marriage sex education law to the "Healthy Youth Act", which required schools to provide sex education that was more closely aligned with parent opinion and public health best practices. The Healthy Youth Act provided clear content requirements that all schools must teach, resources for schools to aid in curriculum selection, and teacher training in general teaching skills and specific curriculum related to sex education.

[9] Oettinger (1999) uses data from the NLSY and finds that enrollment in sex education was associated with earlier sexual activity for females, and earlier pregnancy for women with fewer sources of alternative information.

engage in sexual activities, but that does not mean they will not respond in some way to sex education. Sex education should affect teens' perceptions about sex and their sexual behaviors to the extent that it reinforces existing information, and/or presents new information. Specifically, teens may already know some, or perhaps all, of the information being presented in a sex education course, but receiving the information again may change their sexual behaviors more or less than receiving the information for the first time.[10] Additionally, new information could be presented in multiple ways, and in practice typically takes one of two forms: abstinence-based or comprehensive.

Abstinence-based sex education typically stresses the following: students should abstain from sexual activity until after marriage; abstinence from sex is the only 100% effective way to avoid unwanted pregnancy, STDs and HIV; conceiving a child out of wedlock is likely to have harmful consequences for the child, the child's parents and society; and failure rates associated with condom use (Alford, 2001). Although abstinence-based sex education may include information about contraceptives, details regarding their appropriate use is not provided or encouraged. On the other hand, comprehensive sex education courses tend to be age-appropriate, and include information on topics like human development, relationships, decision making, abstinence, contraception, and disease prevention (Alford, 2001). Comprehensive sex education may also include information about how to access and properly use contraception. Consequently, abstinence-based sex education should decrease the level of sexual activity more than comprehensive sex education, but may also lead to a relatively lower take-up of contraception among teens who are sexually active. Thus, the net effect of abstinence-based sex education on teenage birth and STD rates is ambiguous. If the level of sexual activity decreases among teens who receive abstinence-based education, then teen

[10] Several studies have shown that sex education programs have increased teenagers' existing knowledge about sexual health issues (Eisen & Zellman, 1986; Kim et al., 1997; Reichelt & Werley, 1975; Sanderson, 2000). Kim et al. (1997) and Dupas (2011) find that sex education programs not only increase teens' knowledge about sex, but also affect their sexual behaviors. Additionally, Wilkinson et al. (2006) examine the correlation between the quality of sex education initiatives in the United Kingdom and teenage conceptions and abortions and find that teenage conceptions declined after the implementation of the teenage pregnancy strategy.

birth and STD rates may decrease. But, if abstinence-based education decreases contraceptive use, then teen birth and STD rates may increase. Similarly, if comprehensive sex education promotes information about contraception, it may lead to increases in sexual activity along with increases in contraception use. Again, the net effect on teenage birth and STD rates is ambiguous. If teens are engaging in sexual activity more, then birth and STD rates may increase, but if contraception use among sexually active teens is also increasing, then teenage birth and STD rates may decrease.[11] By having information on teenage sexual behaviors, as well as teenage birth and STD rates, this paper is able to shed light on how each form of sex education affects teen sexual behaviors and health.

Recall that sex education should affect teens' sexual behaviors to the extent that it reinforces existing information and/or presents new information. Sex education should have a greater impact on teens who gain a lot of new information, such as teens without low-cost alternative sources of sexual information (Oettinger, 1999). Older teenagers, age 16 and up, are more likely to have a drivers license, a job, and potentially more access to the internet and media, all of which can provide more opportunities to engage in sexual activity. Additionally, males and females may respond differently to sex education. Indeed, Measor (1996) finds that boys and girls respond differently to sex education, and that boys react more negatively, by either being more disruptive or having the teacher's attention diverted more towards them, than girls.

[11] Alternatively, improper use of contraception by teens could actually increase STD and birth rates. Buckles & Hungerman (2016) analyze the fertility effects of teenagers' access to condoms via condom distribution programs in schools and find that access to condoms in schools increases teen fertility by about 10 percent, with the effect driven by communities where condoms are provided without mandated counseling.

1.4 Data and Measures

1.4.1 Data

My primary analysis will use data drawn from three sources. First, I use repeated cross-sections of both the National and State Youth Risk Behavior Surveys (YRBS) from 1997 to 2013. The National and State YRBS surveys are conducted biennially by the Center for Disease Control and Prevention (CDC) and administered to high school students in grades 9 through 12 during the spring.[12]The National YRBS, when weighted, is representative of the population of U.S. high school students.[13] While the state surveys are coordinated by the CDC, they are usually conducted by state education and health agencies.[14] Specifically, trained data collectors travel to each participating school and administer the survey, in addition to collecting information about schools and classrooms.[15] The YRBS questionnaires assess six categories of youth health behaviors: unintentional injuries and violence, sexual behaviors, alcohol and other drug use, tobacco use, unhealthy dietary behaviors, and inadequate physical activity. Due to the sensitivity of the survey material, data collection procedures for the YRBS are designed to protect student privacy by allowing for anonymous and voluntary participation (Eaton et al., 2012).[16] Given the self-reported nature of the sur-

[12]High schools are selected to participate with probability proportional to the size of student enrollment, and then by required classes, like English, or by a specific class period of the school day (e.g. 2nd period) for all grades 9-12, and within the selected class or period, all students are eligible to participate (Eaton et al., 2012).

[13]Robustness checks of the results to omitting the National YRBS, which is not meant to be state representative, are presented in Table 1.10. The results are quantitatively and qualitatively very similar to the main results presented in Table 1.2, Panel III.

[14]The augmentation of national with state YRBS data has been employed in a number of recent studies examining the effects of many state-level public policies, including cigarette taxes (Hansen et al., 2013), medical marijuana laws (Anderson et al., 2015), anti-bullying laws (Sabia & Bass, 2016), and parental involvement laws for abortion (Sabia & Anderson, 2014), on risky behaviors.

[15]However, in some states, the questionnaires are mailed to the school and administered by the teacher of the selected class or period, then mailed to the agency conducting the survey.

[16]Participating students record their answers to the self-administered survey during the selected class or period on computer-scannable answer sheets, and are encouraged to use an extra sheet of paper provided by the data collector to cover their responses as they complete the survey. And to the extent possible, desks are spread throughout the classroom to ensure additional privacy while completing the survey (Eaton et al., 2012).

vey, one might be worried about the reliability of the data. Brener et al. (1995) and Brener et al. (2002) test the reliability of the YRBS questionnaires and find that students appear to report risky health behaviors reliably over time. Thus, the YRBS provides individual-level data that is well suited for this study because it contains several measures of student sexual behaviors, including sexual activity, condom and contraception use, and information on the number of sexual partners a respondent has had.

Using the YRBS data, I identify four key measures of teenage sexual activity. First, I create an indicator equal to one if the student indicated they had sex within the last three months, and zero otherwise.[17] Nearly 34 percent of the sample indicated they have had sex within the past 3 months (see Table 1.1). Next, respondents were asked about condom and contraception use the last time they had sex.[18] I operationalize this by creating binary variables to measure male and female condom use and female birth control pill use at last sex. Both condom and birth control pill use are conditional on having sex within the past 3 months, and birth control pill use is measured only for female respondents. The birth control pill use outcome is limited only to females because self-reports of contraceptive use by females is considered more accurate than self-reports by males (Brauner-Otto et al., 2012; Steiner et al., 2016). According to the sample, Table 1.1 shows that 56 percent of respondents indicated using a condom at last sex, while 22 percent of females used birth control pills at last sex. Finally, respondents were asked about their number of sexual partners within the past 3 months.[19] I create a binary variable equal to 1 if the respondent reports having sex

[17]Specifically, the survey question asks "During the past 3 months, with how many people did you have sexual intercourse?". Respondents' choices were (i) I have never had sexual intercourse, (ii) I have had sexual intercourse, but not during the past 3 months, (iii) 1 person, (iv) 2 people, (v) 3 people, (vi) 4 people, (vii) 5 people, (viii) 6 or more people. Students were coded as having had sex within the last 3 months if they chose any response in (iii)-(viii).

[18]Specifically, the survey questions asks "The last time you had sexual intercourse, did you or your partner use a condom", and "The last time you had sexual intercourse, what one method did you or your partner use to prevent pregnancy?". Regarding the latter, respondents could choose between (i) no method, (ii) birth control pills, (iii) condoms, (iv) Depo-Provera, (v) withdrawal, (vi) some other method, or (vii) not sure.

[19]Specifically, the survey question asks "During the past 3 months, with how many people have you had sexual intercourse?" Respondents' choices were (i) I have never had sexual intercourse, (ii) I have had sexual intercourse, but not during the past 3 months, (iii) 1 person, (iv) 2 people, (v) 3 people, (vi) 4

with more than 1 person within the past 3 months, and 0 otherwise. I find that 27 percent of the sample reported having sex with more than 1 person within the past 3 months.

Though the YRBS has rich individual-level data on sexual behaviors, the survey does not ask students questions regarding sexually transmitted diseases, pregnancy, and the outcome of a pregnancy if one occurred. Additionally, each of the above measures of sexual behavior is self-reported. If sex education mandates prompt more students to be willing to report their sexual behavior, then estimated effects of sex education mandates may be biased upward. Furthermore, STDs, pregnancy, and pregnancy outcomes are likely relatively low frequency events in the YRBS, so measures of incidence might be inaccurate. To supplement the self-reported measures of student sexual behaviors, I augment the YRBS analysis with objective STD and birth rate data. State-level gonorrhea rates for 15-19 year olds per 1,000 individuals for 1997-2013 were obtained from the CDC's STD Surveillance Data[20]. According to Table 1.1, the reported gonorrhea rate per 1,000 individuals is 4.1, with the female teenage gonorrhea rate being slightly higher with nearly 5.8 diagnoses per 1,000, and nearly 2.6 diagnoses per 1,000 for teenage males. Gonorrhea is the second most commonly reported STD in the United States, and since 2009, gonorrhea rates have been increasing among adolescents and young adults (CDC, 2015a). These data are derived from information from the official statistics for the reported occurrence of nationally notifiable STDs in the United States, test positivity and prevalence data from numerous prevalence monitoring initiatives, sentinel surveillance, and national health care services surveys (CDC, 2015b). The CDC's STD Surveillance Data only accounts for reported STDs. If sex education induces changes in STD testing rates, which in turn accounts for changes in STD rates, then the estimated results will be biased (Carr & Packham, 2016). This is especially true for chlamydia rates since the disease is typically asymptomatic, and increases in testing rates will surely lead to increases

people, (vii) 5 people, (viii) 6 or more people.

[20]STD data is available here: https://wonder.cdc.gov/std.html. State-level gonorrhea rates per 1,000 individuals were calculated using counts of gonorrhea reports to teenagers aged 15-19 divided by the population of teenagers aged 15-19, then multiplied by 1,000.

in diagnoses. In fact, the CDC reports that recent increases in chlamydia rates are likely due to expanded screening and not an increase in the disease (CDC, 2009). Gonorrhea rates, on the other hand, are less subject to this concern (CDC, 2009), and are widely accepted as the preferred measure in the prior literature that analyzes public policy and STDs (Durrance, 2013).

Since data on pregnancies and the outcome of such pregnancies is not available in the YRBS, I obtain state-level birth rates for 15 to 19 year old females for the years 1997-2013 from the National Center for Health Statistics, Division of Vital Statistics Natality Files.[21] State-level birth rates per 1,000 individuals were calculated using the number of births to female teenagers aged 15-19 divided by the population of female teenagers aged 15-19, then multiplied by 1,000. The teenage birth rate for the sample period according to Table 1.1 is 39.6 births per 1,000 individuals. In the United States, state laws require birth certificates to be completed for all births, and Federal law mandates national collection and publication of births and other vital statistics data (CDC, 2015b).

1.4.2 Sex Education Mandates

I begin by generating a binary sex education variable that measures whether a state had enacted and was enforcing a sex education mandate. Information about sex education effective dates was obtained from two sources. Policy information from 1997 to 2000 for each state was retrieved from numerous volumes of the SIECUS (Sexuality Information and Education Council of the United States) Report. The SIECUS Report was published from 1972 to 2005, and includes scholarly articles, opinion pieces, policy information, and other works regarding sexuality information and education. Sex education mandates from 2001 to 2013 were collected from the Guttmacher Institute State Policies in Brief: Sex and HIV

[21]Sex education could also affect teenage abortion rates likely through changes in the number of unintended pregnancies. However, abortion data for teenagers is not available on a consistent basis for all US states and for the sample years in this study.

Education publications, which contain detailed sex education policy information at the state level. The two sources of information were both available for the years 2001 to 2005, and the Guttmacher Institute reports were relied upon since they were published on a more consistent basis. Figure 1.1 presents the effective year for each state's sex education mandate from 1997-2013. It is important to note that variation in sex education mandates not only comes from states enacting a sex education mandate, but also from states repealing their sex education mandate during the sample period. In Figure 1.1, states marked with an "*" enacted a sex education mandate, and states marked with a "ˆ" repealed a sex education mandate. Throughout the sample period, 31 states mandated sex education, with 12 states enacting sex education, 6 state repealing sex education, and 15 states requiring sex education during the full sample period.[22]

Given the substantial heterogeneity in the type of sex education mandate enacted by each state, SIECUS Reports and the Guttmacher Institute (GI) categorize these mandates by their comprehensiveness and requirements. Given the debate among policymakers about what type of sex education should be offered in schools, identifying the most effective type of sex education, whether it be abstinence-based or more comprehensive in nature, is critical. I use the SIECUS and GI reports from 1997-2013 to indicate whether a state mandating sex education requires abstinence-based, comprehensive sex education, or "unspecified" sex education. Unspecified sex education refers to states that enact a sex education mandate but do not specify the type of sex education that is to be taught. Generally, when states do not specify the type of education to be taught, the curriculum offered often depends on the teacher's ability, training, and comfort with the subject matter (Donovan, 1998). Specifically, the SIECUS Reports and GI State Policies in Brief indicate whether a state that mandates sex education either cover or stress abstinence, and whether the state's mandate also requires coverage of contraception. I generate binary indicators for abstinence-based, comprehensive, and unspecified sex education that measure whether a state enacting a sex education mandate

[22]Florida and West Virginia both enacted and repealed a sex education mandate during 1997-2013.

requires the teaching of abstinence-based, comprehensive or unspecified education. A state is considered to have an abstinence-based curriculum if the state's mandate requires schools to cover or stress abstinence, with no requirement to cover contraception. A state is considered to have a comprehensive curriculum if the state's mandate requires schools to cover or stress abstinence, and cover contraception.[23] Fourteen states that mandate sex education throughout the sample period provide abstinence-based education, while 16 states mandating sex education require a comprehensive curriculum, and 10 states mandated sex education without providing detail on the type of education to be taught (see Figure 1.2).[24]

1.5 Empirical Approach

My econometric approach will estimate a reduced form difference-in-difference model (DD) that takes the form:

$$Y_{ist} = \alpha + \beta_1 SexEd_{st-1} + \delta' X_{ist} + \gamma' Z_{st} + \theta_s + \tau_t + \epsilon_{ist} \tag{1.1}$$

where i indexes the individual, s indexes the respondent's state, and t indexes the survey year. Y_{ist} is a measure of individual teenage sexual behavior, including sexual intercourse in the past 3 months, condom use during last sex, female birth control pill use during last sex, and having multiple sexual partners.[25] $SexEd_{st-1}$ is an indicator for whether a sex education

[23] States that provide "abstinence-plus" education (stressing abstinence education but also covering contraception, are coded as "comprehensive" states. Table 1.13 shows the results collapsing "abstinence-plus" with abstinence instead of comprehensive.

[24] It is important to note that although 14 states provided abstinence-based education, 16 provided comprehensive, and 10 provided unspecified education, some states provided more than 1 type of education throughout the sample period (see Figure 1.2). For example, Minnesota had a sex education mandate throughout the sample period, and enforced abstinence-based education only from 2011 onwards, while prior to 2011 did not specify the type of sex education that was to be taught. Thus, Minnesota is counted as having provided abstinence-based education, as well as unspecified education.

[25] Gonorrhea and birth rates are obtained at the state level, thus Equation 1 can be modified as: $Y_{st} = \alpha + \beta_1 SexEd_{st-1} + \gamma' Z_{st} + \theta_s + \tau_t + \epsilon_{st}$. This modification can be carried over to all subsequent equations where gonorrhea and birth rates are the main outcomes of interest. The vector Z includes state-level

mandate was in effect in state s in year $t-1$. Since the YRBS is typically administered in the spring of each survey year, and sex education mandates passed during a calendar year likely are not implemented by schools until the start of the school year (i.e. in August or September, months after the YRBS has been administered), I lag the sex education policy variable to better align the timing of the sexual behavior and health outcomes with the effective date of the policy. The vector X includes individual level controls, such as age, race, gender, and grade level; the vector Z includes state level economic and policy controls, such as the teenage unemployment rate, per capita income, beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Finally, θ_s and τ_t are state and year fixed effects. Individual-level regressions are weighted using sample weights provided in the YRBS[26], state-level STD regressions are weighted by the teenage population age 15-19, and state-level birth regressions are weighted by the female teenage population ages 15-19. Standard errors presented in all tables are clustered at the state level (Bertrand et al., 2004).

Identification of the parameter of interest, β_1, comes from within-state variation in sex education mandates during the 1997-2013 sample period. Estimates of β_1 in Equation 1 will only be unbiased if state-specific time-varying unobservables are uncorrelated with the adoption of sex education mandates, and if states are not enacting sex education mandates in response to unfavorable teenage sexual behaviors, STD rates, and birth rates. I pursue a number of strategies to address the possibility of policy endogeneity. First, I control for two state-level abortion policies that may have been implemented contemporaneously with sex education mandates: parental involvement laws for abortion and mandatory wait laws for abortion services. Second, I test whether sex education mandates were implemented in response to pre-existing teenage sexual behavior and health trends with the following event

demographic, economic, and policy controls, such as the percent of teenagers who are white, the percent of teenagers who are black, the teenage unemployment rate, per capita income, beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services.

[26]Linear probability models are estimated for the individual-level YRBS data. The results are quantitatively and qualitatively very similar using a Probit specification, and the results are reported in Table 1.11.

study specification:

$$Y_{ist} = \alpha + \sum_{\phi \leq -6}^{\phi=-2} \sigma_\phi SexEd_{s,t+\phi} + \sum_{\mu=1}^{\mu \geq 7} \eta_\mu SexEd_{s,t+\mu} + \delta' X_{ist} + \gamma' Z_{st} + \theta_s + \tau_t + \epsilon_{ist} \quad (1.2)$$

To operationalize the event study, I only use within-state variation from states enacting a sex education mandate during the 1997-2013 sample period. That is, I drop the six states that repeal their sex education mandate from the main event study specification, and instead show event study figures separately for states enacting a sex education mandate, and for states repealing a sex education mandate, as trends in sexual behaviors and health may lead states to alter their position on sex education.[27] Third, I add state-specific linear time trends to Equation 1 to control for unmeasured state trends unfolding linearly. Fourth, I pool 15-19 year old and 20-24 year old gonorrhea and birth rates and estimate a difference-in-difference-in-difference model (DDD), where the 20-24 year olds are used as a within-state control group to net out trends that may differentially affect treated and control states.[28] Finally, I conduct a placebo test on a number of health-related outcomes that should be unaffected by state-mandated sex education.

To capture heterogeneity across states' sex education mandates, I also estimate a reduced form difference-in-difference model similar to Equation 1, and include the policy variables "Abstinence", "Comprehensive", or "Unspecified", which indicate whether a states' sex education mandate requires the teaching of abstinence-based education, comprehensive educa-

[27] In Figure 1.4, I show the event study figures for the states that repeal their sex education mandate. That is, I drop the states that implement a sex education policy during 1997-2013. There is no evidence of significant pre-trends in any of the behavior or health outcomes.

[28] The YRBS does not survey young adults ages 20-24, therefore, the DDD analysis is only performed on STD and birth rates.

tion, or unspecified education.

$$Y_{ist} = \alpha + \beta_1 Abst_{st-1} + \beta_2 Comp_{st-1} + \beta_3 Unspecified_{st-1} + \delta' X_{ist} + \gamma' Z_{st} + \theta_s + \tau_t + \epsilon_{ist} \quad (1.3)$$

Identification of the parameters of interest, β_1, β_2, and β_3, come from within-state variation in state mandates that require abstinence-based, comprehensive, or unspecified sex education from 1997-2013 (see Figure 1.2).

1.6 Results

1.6.1 Any Sex Education Mandate

Table 1.2 presents the results from Equation 1 for the effect of state-mandated sex education on measures of teenage sexual behaviors reported in the YRBS. Panel I of Table 1.2 includes only controls for state and year fixed effects. The estimates suggest that sex education mandates significantly increase the probability of having sex by 5.8 percent. Sex education mandates are also associated with increases in condom use, birth control pill use, and having multiple sexual partners, but none of these estimates are statistically significant. Panel II includes individual-level demographic controls and state-level controls. The estimates are fairly robust to the inclusion of controls, although sex education mandates no longer significantly impact having recent sex.

The results presented in Panels I and II of Table 1.2 could be biased if states are implementing sex education mandates in response to unfavorable teenage sexual behaviors. I test the parallel trends assumption of my research strategy by estimating Equation 2 and present the results graphically in Figure 1.3. Panels (a) - (d) show the estimates of α from Equation 2 on teenage sexual activity, condom use, female birth control pill use, and multiple sexual partners, respectively. The points in the plots give the estimate of α, with years 0 to -1 excluded such that all estimates are relative to these years, while the lines extending from them represent 95% confidence intervals that are calculated using standard errors clustered at the state level.[29] Each figure also includes the F-statistic and p-value from a joint test of significance on the pre-trends.

If the adoption of sex education mandates has an effect on teenage sexual behaviors before their implementation, then the results may be driven by unobserved trends (Meer & West, 2015). This does not appear to be the case for sexual activity, female birth control pill use, or multiple sexual partners, as no evidence of pre-treatment trends are seen in panels (a), (c), and (d). However, pre-treatment trends do exist for teenage condom use, with panel (b) showing a decrease in condom use for all years prior to the adoption of sex education mandates. This suggests that pre-treatment trends in condom use may be correlated with a states' adoption of a sex education mandate. However, it is unlikely that policymakers know about the sexual behaviors of teenagers unless they are familiar with the YRBS results, or results of similar sexual behavior surveys. Policymakers are more likely to be aware of and rely on teenage STD and/or birth rate trends in their state when deciding to implement sex education mandates. Nevertheless, in order to control for the pre-treatment trends in condom use that may be correlated with a states' adoption of a sex education mandate, I augment Equation 1 and include a state-specific linear time trend. The results are presented in Panel III of Table 3. Similar to Panel II, the estimates all remain statistically insignificant, and decrease in magnitude, with the exception of multiple partners, with condom use and birth control pill use becoming slightly negative.

[29] In the event study figures for the YRBS outcomes, sets of years are grouped together to account for the biennial nature of the survey, and the variation in sex education enactment year. That is, since the YRBS is given every odd year, states that enact a sex education mandate in an even year will have data for the one year lead and lag, three year lead and lag, etc. States that enact a sex education mandate in an odd year will only have data for the two year lead and lag, four year lead and lag, etc.

Next, I explore the hypothesis that sex education mandates may not have an immediate effect on teenage sexual behaviors. The change in sexual behavior seen in Table 1.2 may take time to unfold due to teens potentially gaining new information or updating their existing knowledge, eventually changing their behavior, and finding sexual partners. Table 1.3 presents the results for the lagged effects of sex education mandates on teenage sexual behaviors. Panel I includes all individual and state-level controls, and Panel II includes all controls plus a state-specific linear time trend. The estimates are similar for each outcome across both specifications, and suggest that sex education does not significantly impact teenage sexual behaviors in the short-run or long-run.

While sex education appears to have no effect, on average, on teenage sexual behaviors, I next explore if there might be heterogeneous effects of sex education by whether a sex education mandate was enacted, or repealed. Recall Figure 1.1, which shows state-level variation in sex education mandates from 1997-2013, and indicates the states enacting a sex education mandate (marked with an "*"), and states repealing a sex education mandate (marked with a "ˆ"). To test whether an introduction or a repeal of a sex education mandate affects sexual behaviors differently, I replace $SexEd_{s,t-1}$ in Equation 1 with two dummy variables: an indicator for a sex education mandate being enacted ($SexEd - On_{s,t-1}$), and an indicator for a sex education mandate being repealed ($SexEd - Off_{s,t-1}$). The results are presented in Table 1.14, columns 1-4. The estimates suggest that neither an introduction or repeal of a sex education mandate differentially effect teenage sexual behaviors (see F-test in Panel III).[30]

[30] I also explore heterogeneous effects by age, teenagers under 16 years old and teenagers 16+ years old, a typical age cut for state-level age of consent laws. Thirty-one states have an age of consent law of 16 years old, which is the age at which a person's consent to sexual intercourse is valid in law. The results are presented in Table 1.12. The estimates show that sex education mandates do not significantly impact younger or olden teens probability of sexual activity, condom use, or female birth control pill use. However, there is some evidence that sex education mandates increase the probability of having multiple sexual partners for older teens by 3.4 percent. Heterogeneous effects by gender are also investigated, and sex education appears to have no differential effect on sexual behaviors of males and females. The results are available from the author upon request.

Although sex education courses may also present information consistent with healthy teenage behaviors, like the dangers of alcohol and drug consumption, we should not expect to see an effect of sex education on general teenage health behaviors. As a placebo test, Table 1.9 presents the results for the effect of sex education on watching television for more than 1 hour per day, taking laxatives or vomiting to lose weight, and wearing a bicycle helmet when riding a bike. The results show that sex education does not significantly effect general teenage health behaviors, providing additional confidence in the results presented above.

I next present the results for the effect of sex education on objective measures of teenage health, including teenage gonorrhea and birth rates. Table 1.4 presents the DD (columns 1, 3, and 5) and DDD (columns 2, 4, and 6) results for the effect of sex education mandates on the log of gonorrhea rates, per 1,000 individuals, for all teenagers, and for male and female teenagers separately. Panel I includes only state and year fixed effects, Panel II includes state-level controls, and Panel III includes all controls plus a state-specific linear time trend. Focusing attention on Panel III and the DD estimates in columns 1, 3, and 5, sex education appears to decrease teenage gonorrhea rates by 2 to 3 percentage points, but none of the estimates are statistically distinguishable from zero.[31]

As noted above, policymakers may be more aware of trends in the teenage STD and/or birth rates when determining whether or not to mandate sex education. If this is the case, then we will be more concerned about the validity of the estimates if pre-treatment trends emerge in teenage STD and birth rates. Figure 1.3 panel (e) shows the estimates of α from Equation 2 on the log of 15-19 year old gonorrhea rates. Gonorrhea rates appear to be slightly trending upwards, but a joint test of significance on the pre-trends is not statistically different from zero. To further confirm that gonorrhea rates were not changing systematically prior to the implementation of sex education mandates, Table 1.4 also presents the estimates from a

[31] Heterogeneous effects of sex education introduction or repeal on the teenage gonorrhea rate are also presented in Table 1.14, column 5. The estimates suggest that an introduction of a sex education mandate marginally reduces teenage gonorrhea rates by 8 percentage points, but the estimate is not significantly different from the effect of a repeal of a sex education mandate (see F-test in Panel III).

DDD specification, that uses young adults ages 20-24 as a within-state control group. The DDD results in Panel III confirm the DD estimates, though slightly larger in absolute value, and show that sex education does not significantly impact teenage gonorrhea rates.

Table 1.5 presents the DD (column 1) and DDD (column 2) results for the effect of sex education mandates on the log of the female birth rate, per 1,000 individuals. Table 1.5 also includes the results for the effect of sex education on the log of the teenage birth rate for those under 16 years old (column 3) and those ages 16 years old and over (column 4). Panel I includes only controls for state and year fixed effects, and Panel II adds state-level controls, and Panel III includes all controls plus a state-specific linear time trend. The DD estimates in Panel III show very little evidence that sex education is associated with economically or statistically significant changes in the female birth rate. Similarly, the DDD results, which use young female adults ages 20-24 as a within-state control group, provide no evidence that sex education economically or statistically significantly impacts teenage birth rates.[32]

Again, the validity of the estimates will be threatened if trends in the teenage birth rate emerge prior to the implementation of a sex education mandate. Figure 1.3 panel (f) shows the estimates of α from Equation 2 on the log of the teenage female birth rate. Teenage birth rates do not appear to be significantly trending upwards in the years prior to a sex education mandate, and a joint test of significance on the pre-trends is not statistically different from zero.

1.6.2 Exploring Heterogeneity

While the average school-based state sex education mandate appears to have no significant effect on teenage sexual behaviors or health, I next explore whether there may be hetero-

[32] Heterogeneous effects of sex education introduction or repeal on the teenage birth rate are also presented in Table 1.14, column 6. The estimates suggest that neither an introduction nor repeal of a sex education mandate significantly impact teenage birth rates.

geneity in the effect of sex education by type of law implemented. Variation in sex education mandate type is displayed in Figure 1.2. It is clear from Figure 1.2 that many states with a sex education mandate switched between types of sex education throughout the sample period. Tables 1.6-1.8 present the results from Equation 3 by type of sex education mandate on teenage sexual behaviors, teenage gonorrhea rates, and teenage birth rates, respectively, exploiting all variation in mandates presented in Figure 1.2.

Beginning with the heterogeneous results for the YRBS outcomes, Panel III of Table 1.6 shows that comprehensive and unspecified sex education mandates are associated with increases in risky teenage sexual behaviors. Comprehensive sex education mandates appear to marginally decrease teenage condom use by 3.25 percent, and unspecified sex education mandates decrease female teenage birth control pill use by 13.8 percent, and increase sex with multiple partners by 6.6 percent. Next, I report the results by sex education type on teenage gonorrhea and birth rates in Tables 1.7 and 1.8, respectively. Panel III, Column 2 (DDD) of Table 1.7 shows no evidence that the type of sex education significantly impacts teenage gonorrhea rates. The point estimates for each type of sex education are also fairly small and negative, with effects ranging from a 2.5 to 5.8 percentage point decline in teenage gonorrhea rates. Finally, Panel III, Column 2 (DDD) of Table 1.8 shows that no type of sex education economically or significantly impacts teenage birth rates.

It is important to note that although the comprehensive sex education results are inconsistent with the hypothesis that comprehensive sex education should increase contraception use, I interpret the heterogeneous results on sexual behaviors, gonorrhea, and births with caution for two main reasons. First, it is difficult to separate out effects by type of sex education, due to many states continuously switching between mandate type, and repealing their law altogether (see Figure 1.2). Second, for the main analysis, I define comprehensive sex education states as those that require coverage of abstinence and coverage of contraception, plus those that stress abstinence and cover contraception (i.e. "abstinence-plus"), since abstinence-plus programs do indeed teach students about contraception. However, it also seems plausible to combine states with abstinence-plus mandates with states that require abstinence-only sex education. The results of this recode of abstinence and comprehensive sex education are presented in Table 1.13. The effect of comprehensive sex education on decreasing teenage condom use is not robust to this reclassification. Additionally, the results now suggest that comprehensive sex education marginally increases sexual activity by 5.7 percent, and the teenage birth rate by 3 percentage points. Thus, the sensitivity of the estimates to type of sex education classification fails to provide strong evidence that abstinence-based, comprehensive, or unspecified sex education differentially effect teenage sexual behaviors and health.

1.7 Discussion and Conclusion

This study presents new evidence on the effect of state-mandated school-based sex education on teenage sexual behaviors and health. Importantly, this is the first paper to simultaneously estimate the effect of three types of state-mandated sex education, abstinence-based, comprehensive, and "unspecified", which allows me to directly compared the effects of each type of education on teenage sexual behaviors and health, unlike the previous related literature. I exploit within-state variation in sex education mandates to estimate the effect on recent sexual intercourse, condom use, female birth control pill use, having multiple sexual partners, teenage gonorrhea rates, and teenage birth rates. Difference-in-difference results show that any sex education mandate does not significantly impact teenage sexual behaviors, and difference-in-difference-in-difference results show no effect on teenage gonorrhea or birth rates. Exploring sex education policy heterogeneity, difference-in-difference estimates suggest that comprehensive sex education significantly decreases teenage condom use, and unspecified sex education decreases female birth control pill use, and increases engaging with multiple sexual partners. This negative effect of sex education on teenage sexual behaviors does not translate to unfavorable sexual health outcomes, as sex education has no significant impact on teenage gonorrhea or birth rates. However, the heterogeneous results are interpreted with caution due to variation and classification issues.

It is important to note that the null findings found in this paper potentially mask effects of school district-level sex education curriculum on teenage sexual behaviors and health. Although the state mandates the type of sex education required by all schools, the curriculum used within sex education courses is largely left up to the school districts and the teachers within those districts (Donovan, 1998). For example, Ohio does not have state health standards or a model curriculum for sex education, and instruction is left up to each school district and its staff. In 2015, five Ohio school districts brought in outside organizations, such as a trained representative from the Hamilton County Public Health office, to teach some or all of the sex education curriculum, whereas other schools districts relied on health or physical education teachers to instruct the course (Jamie Gregory, 2015)[33]. This variation in type of information given to students across school districts within a state could have differential effects on teenage sexual behaviors and health. Thus, future work should aim to exploit district-level variation in sex education curriculum to potentially uncover effects on sexual behaviors and health that are masked in the state-level analysis presented here.

Nevertheless, null effect of state-level sex education mandates on teenage sexual behaviors and health is surprising, especially from a policymakers' standpoint, given the debate about if, and what type of, school-based sex education should be taught. However, the null finding on teenage birth rates found in this study remains in line, both quantitatively and qualitatively, with the recent literature examining the effect of mandated sex education on teenage births (Carr & Packham, 2016; Kearney & Levine, 2015). Though the recent declines in

[33]To take another example, currently in Minnesota, the state law requires sex education to be taught, and it must be comprehensive, but must help students refrain from sexual activity until marriage, technically accurate, and updated. Also according to the law, it allows for each school district to interpret this law in their own way (MINN. STAT. 121A.23, 2017).

the US teen birth rate have been primarily attributed to improvements in teens' contraception use, and advocates give credit to sex education programs for their role in the decline (Boonstra, 2014), the previous literature and current study find no clear evidence that these programs are effective at decreasing the teenage birth rate.

1.8 Figures

Figure 1.1: State-level Sex Education Mandates, 1997-2013

	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
Alabama*	X																
Alaska^		X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Arizona																	
Arkansas*	X	X															
California																	
Colorado																	
Connecticut																	
Delaware	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
DC	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Florida^*					X	X	X	X	X	X	X	X	X	X			
Georgia	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Hawaii*	X	X	X	X	X	X	X	X	X	X	X	X	X	X			
Idaho																	
Illinois*	X	X	X	X	X	X	X	X	X	X							
Indiana																	
Iowa	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Kansas	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Kentucky^			X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Louisiana																	
Maine^						X	X	X	X	X	X	X	X	X	X	X	X
Maryland	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Massachusetts																	
Michigan																	
Minnesota	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Mississippi^															X	X	X
Missouri																	
Montana^											X	X	X	X	X	X	X
Nebraska																	
Nevada	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
New Hampshire																	
New Jersey	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
New Mexico^													X	X	X	X	X
New York																	
North Carolina	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
North Dakota^																X	X
Ohio^														X	X	X	X
Oklahoma																	
Oregon^												X	X	X	X	X	X
Pennsylvania																	
Rhode Island	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
South Carolina	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
South Dakota																	
Tennessee	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Texas																	
Utah	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Vermont	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Virginia																	
Washington																	
West Virginia^*	X	X	X	X	X	X	X	X	X	X			X	X	X	X	X
Wisconsin																	
Wyoming^					X	X	X	X	X	X	X	X	X	X	X	X	X

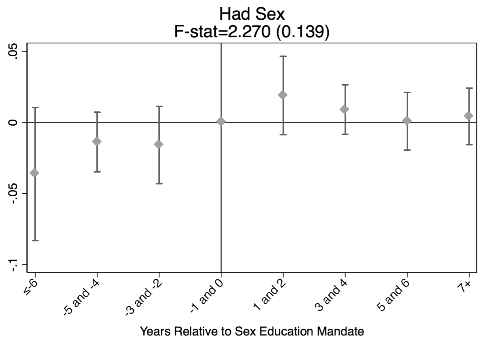
Notes: ^ indicates enactment of a state sex education mandate. * indicates repeal of a state sex education mandate.

Figure 1.2: Heterogeneity in State-level Sex Education Mandates, 1997-2013

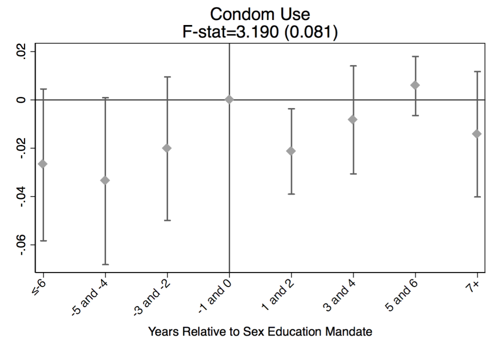
	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
Alabama	A																
Alaska		U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U
Arizona																	
Arkansas	U	U															
California																	
Colorado																	
Connecticut																	
Delaware	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
DC	U	U	U	U	U	U	U	C	C	C	C	C	C	C	C	C	C
Florida					A	A	A	A	A	A	A	A	A	A			
Georgia	C	C	C	C	C	C	C	A	A	A	A	A	A	A	A	A	A
Hawaii	C	C	C	C	C	C	C	C	C	C	C	C	C	C			
Idaho																	
Illiniois	A	A	A	A	A	A	A	C	C	C							
Indiana																	
Iowa	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U
Kansas	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U
Kentucky			A	A	A	A	A	A	A	A	A	A	A	A	A	A	A
Louisiana																	
Maine						C	C	C	C	C	C	C	C	C	C	C	C
Maryland	U	U	U	U	C	C	C	C	C	C	C	C	C	C	C	C	C
Massachusetts																	
Michigan																	
Minnesota	U	U	U	U	U	U	U	U	U	U	U	U	U	U	A	A	A
Mississippi															A	A	A
Missouri																	
Montana											A	A	A	A	A	A	A
Nebraska																	
Nevada	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U	U
New Hampshire																	
New Jersey	C	C	C	C	C	C	C	A	A	A	U	U	U	U	C	C	C
New Mexico													C	C	C	C	C
New York																	
North Carolina	C	C	C	C	C	C	C	A	A	A	A	A	A	C	C	C	C
North Dakota																A	A
Ohio														A	A	A	A
Oklahoma																	
Oregon												C	C	C	C	C	C
Pennsylvania																	
Rhode Island	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
South Carolina	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
South Dakota																	
Tennessee	C	C	C	C	A	A	A	A	A	A	A	A	A	A	A	A	A
Texas																	
Utah	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A
Vermont	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
Virginia																	
Washington																	
West Virginia	C	C	C	C	C	C	C	C	C	C			C	C	C	C	C
Wisconsin																	
Wyoming					U	U	U	U	U	U	U	U	U	U	U	U	U

Key
A=Abstinence
C=Comprehensive
U=Unspecified

Figure 1.3: Event Study Estimates of the Effect of Sex Education Mandates on Teenage Sexual Behaviors and Health



(a)



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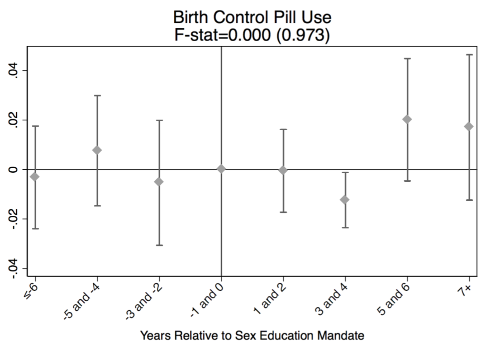

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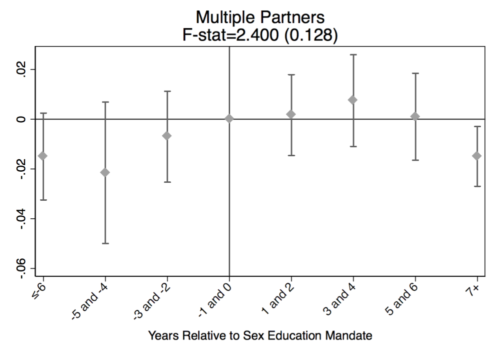

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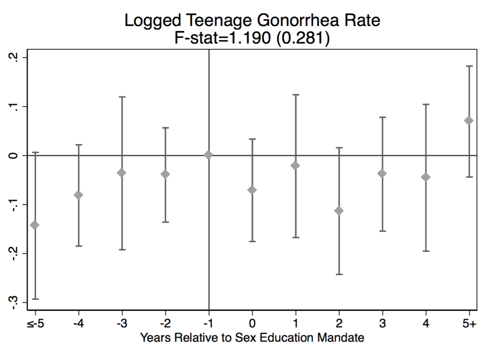
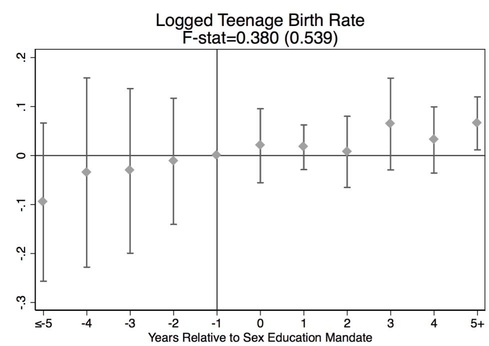


(e)



(f)

Notes: Estimates of Equation 2 described in the text. All estimates include individual controls (when applicable), state-level controls, and state and year fixed effects. 95 percent confidence intervals are shown extending from each point. All estimates are relative to years 0 and -1. The F-stat and p-value of a joint test of significance on the policy leads are also shown.

1.9 Tables

Table 1.1: Descriptive Statistics of Analysis Variables, 1997-2013

	N	Mean	SD	Min	Max
Panel I: YRBS Data					
Demographics					
Grade	997,460	10.37	1.101	9	12
Age	1,007,350	15.97	1.238	12	18
Male	1,004,800	0.490	0.500	0	1
White	1,011,009	0.540	0.498	0	1
Black	1,011,009	0.148	0.355	0	1
Hispanic	1,011,009	0.162	0.369	0	1
Outcome measures					
Sexual Activity	822,385	0.335	0.472	0	1
Condom Use	260,909	0.560	0.490	0	1
Birth Control Pill Use	137,569	0.217	0.412	0	1
Multiple Sexual Partners	275,142	0.270	0.444	0	1
Panel II: State-level Data					
State-level controls					
Parental involvement law	867	0.648	0.478	0	1
Mandatory wait law for abortion	867	0.359	0.480	0	1
Beer taxes	867	0.255	0.204	0.017	1.137
Blood alcohol content law	867	0.764	0.414	0	1
Per capita income (in 2000$)	867	31,317.9	6,588.2	20,256.3	74,513.0
Teenage unemployment rate	867	5.678	2.045	2.300	13.70
Percent White	867	0.773	0.151	0.166	0.975
Percent Black	867	0.138	0.134	0.004	0.637
State-level outcome measures					
Teenage Gonorrhea Rate	867	4.133	3.879	0.064	36.08
Teenage Female Gonorrhea Rate	867	5.761	5.116	0.019	42.92
Teenage Male Gonorrhea Rate	867	2.580	2.692	0.043	28.67
Teenage Birth Rate	867	39.62	13.01	11.93	71.84
State-level treatment measures					
Sex Education	867	0.449	0.498	0	1
Abstinence-Based	867	0.116	0.245	0	1
Comprehensive	867	0.205	0.404	0	1
Unspecified	867	0.128	0.334	0	1

Notes: Data on individual-level outcome measures and demographics come from the State and National YRBS from 1997-2013. Condom use, birth control pill use, and multiple sexual partners are all conditional on having had sex. Birth control pill use is for the sample of female respondents only. Multiple Sexual Partners is defined as having more than 1 sexual partner. The descriptive statistics for all YRBS Outcome measures are for the estimation sample in Panel III in Table 3. Data on state-level economic and policy controls come from multiple sources: abortion policies are obtained from Sabia & Anderson (2016), beer taxes - Beer Institute, blood alcohol content law - Anderson et al. (2015), per capita income - US Census Bureau, teenage unemployment rate - Bureau of Labor Statistics, percent white and black - US Census Bureau. Data on state-level STD rates for 15-19 year olds come from CDC WONDER Online Database for the years 1997-2013, and data on female birth rates for 15 to 19 year olds come from the National Vital Statistics from 1997-2013. The gonorrhea and birth rates for 15 to 19 year olds are per 1,000 individuals. Data on treatment measures come from the SIECUS Report from 1997-2000 and from the Guttmacher Institute State Policies in Brief: State Sex and HIV Education, 2001-2013.

Table 1.2: The Effect of Any Sex Education on Teenage Sexual Behaviors

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**
Panel I: No controls				
SexEd	0.0195*	0.0107	0.0079	0.0044
	(0.0113)	(0.0081)	(0.0060)	(0.0054)
N	833,091	264,905	138,680	279,551
State-level controls	No	No	No	No
Individual-level controls	No	No	No	No
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel II: All Controls				
SexEd	0.0153	0.0074	0.0049	0.0066
	(0.0128)	(0.0082)	(0.0088)	(0.0053)
N	822,385	260,909	137,569	275,142
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel III: With state-specific linear time trends				
SexEd	0.0091	-0.0047	-0.0064	0.0095
	(0.0137)	(0.0054)	(0.0090)	(0.0057)
N	822,385	260,909	137,569	275,142
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes
Outcome Means	0.335	0.560	0.217	0.270

Notes: Weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS. The variable "SexEd" is the one-year lag of the sex education indicator. Individual controls include age, grade, gender, and race. State-level controls include the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcomes means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.3: The Lagged Effect of Any Sex Education on Teenage Sexual Behaviors

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**
Panel I: All controls				
1 to 2 years after	0.0244	-0.0079	0.0107	0.0098
	(0.0229)	(0.0142)	(0.0158)	(0.0081)
3 to 4 years after	0.0054	0.0051	-0.0101	0.0273**
	(0.0177)	(0.0149)	(0.0121)	(0.0130)
5 to 6 years after	0.0023	0.0117	0.0183	0.0244**
	(0.0219)	(0.0126)	(0.0154)	(0.0111)
7+ years after	-0.0092	0.0022	0.0286*	0.0046
	(0.0199)	(0.0148)	(0.0150)	(0.0107)
N	822,385	260,909	137,569	275,142
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel II: With state-specific linear time trends				
1 to 2 years after	0.023	-0.0175	-0.0055	0.007
	(0.0209)	(0.0126)	(0.0130)	(0.0090)
3 to 4 years after	0.0269	-0.0036	-0.0119	0.0278*
	(0.0182)	(0.0137)	(0.0144)	(0.0148)
5 to 6 years after	0.0317	0.0108	0.0325	0.0197
	(0.0216)	(0.0150)	(0.0206)	(0.0186)
7+ years after	0.0364	0.0123	0.0337	0.0001
	(0.0257)	(0.0192)	(0.0284)	(0.0214)
N	822,385	260,909	137,569	275,142
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes
Outcome Means	0.335	0.560	0.217	0.270

Notes: Weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS. Sets of years are grouped together for each lag coefficient to account for the biennial nature of the survey, and the variation in sex education enactment year. Individual controls include age, grade, gender, and race. State-level controls include the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcomes means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: The Effect of Any Sex Education on Logged Teenage Gonorrhea Rates

	All - DD	**All - DDD**	**Males - DD**	**Males - DDD**	**Females - DD**	**Females - DDD**
Panel I: No controls						
SexEd	0.0261	-0.0466	0.0289	-0.0565	0.0243	-0.0375
	(0.0317)	(0.0458)	(0.0315)	(0.0444)	(0.0371)	(0.0431)
N	867	1,734	865	1,732	867	1,734
State-level controls	No	No	No	No	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel II: All controls						
SexEd	0.0103	-0.0528	0.0038	-0.0701*	0.0129	-0.0399
	(0.0265)	(0.0395)	(0.0278)	(0.0384)	(0.0313)	(0.0381)
N	867	1,734	865	1,732	867	1,734
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel III: With state-specific linear time trends						
SexEd	-0.0258	-0.0495	-0.0338	-0.0632	-0.0213	-0.0385
	(0.0424)	(0.0469)	(0.0411)	(0.0468)	(0.0440)	(0.0442)
N	867	1,734	865	1,732	867	1,734
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Means	4.133	4.430	2.580	3.407	5.761	5.510

Notes: Weighted estimates are obtained using data on STD rates for 15-19 year olds for columns 1, 3, and 5, and for 15-24 year olds for columns 2, 4, and 6, from CDC WONDER Online Database for the years 1997-2013. The variable "SexEd" is the one-year lag of the sex education indicator. State-level controls include the percentage of teens who are white, the percentage of teens who are black, the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcome means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: The Effect of Any Sex Education on Logged Teenage Birth Rates

	All - DD	**All - DDD**	**Under 16 y.o. - DD**	**16 + y.o. - DD**
Panel I: No controls				
SexEd	0.0244	0.0003	0.0327	0.0217
	(0.0180)	(0.0080)	(0.0238)	(0.0186)
N	867	1,734	859	867
State-level controls	No	No	No	No
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel II: All controls				
SexEd	0.0114	-0.0018	0.013	0.013
	(0.0146)	(0.0077)	(0.0204)	(0.0144)
N	867	1,734	859	867
State-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel III: With state-specific linear time trends				
SexEd	0.0007	-0.0001	-0.0044	-0.0017
	(0.0051)	(0.0066)	(0.0153)	(0.0056)
N	867	1,734	859	867
State-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes
Outcome Means	40.01	70.83	8.568	47.71

Notes: Weighted estimates are obtained using data on birth rates for females 15-19 year olds for columns 1, 3, and 4, and 15-24 year olds for column 2, from the National Center for Health Statistics, Division of Vital Statistics Natality Files for the years 1997-2013. The variable "SexEd" is the one-year lag of the sex education indicator. State-level controls include the percentage of teens who are white, the percentage of teens who are black, the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcome means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: The Effect of Sex Education Type on Teenage Sexual Behaviors

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**
Panel I: No controls				
Abstinence	0.0221	0.0107	0.0076	0.0004
	(0.0134)	(0.0105)	(0.0077)	(0.0071)
Comprehensive	0.0148*	0.0085	0.0102*	0.0136
	(0.0084)	(0.0093)	(0.0054)	(0.0111)
Unspecified	0.0159*	0.0168*	0.0026	0.0038
	(0.0089)	(0.0088)	(0.0100)	(0.0061)
N	833,091	264,905	138,680	279,551
State-level controls	No	No	No	No
Individual-level controls	No	No	No	No
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel II: All Controls				
Abstinence	0.0161	0.0074	0.0040	0.0046
	(0.0156)	(0.0115)	(0.0107)	(0.0070)
Comprehensive	0.0158*	0.0059	0.0099	0.0129*
	(0.0089)	(0.0101)	(0.0065)	(0.0075)
Unspecified	0.0077	0.0120	-0.0049	-0.0001
	(0.0079)	(0.0076)	(0.0172)	(0.0071)
N	822,385	260,909	137,569	275,142
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel III: With state-specific linear time trends				
Abstinence	0.0062	0.0008	-0.0105	0.0055
	(0.0147)	(0.0079)	(0.0078)	(0.0074)
Comprehensive	0.0183	-0.0182*	0.0116	0.0199
	(0.0113)	(0.0095)	(0.0129)	(0.0125)
Unspecified	0.0069	-0.0077	-0.0299*	0.0179**
	(0.0132)	(0.0084)	(0.0154)	(0.0085)
N	822,385	260,909	137,569	275,142
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes
Outcome Means	0.335	0.560	0.217	0.270

Notes: Weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS. The variables "Abstinence", "Comprehensive", and "Unspecified" are the one-year lags of the Abstinence, Comprehensive, and Unspecified sex education indicators, respectively. Individual controls include age, grade, gender, and race. State-level controls include the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcomes means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.7: The Effect of Sex Education Type on Logged Teenage Gonorrhea Rates

	All - DD	**All - DDD**	**Males - DD**	**Males - DDD**	**Females - DD**	**Females - DDD**
Panel I: No controls						
Abstinence	-0.0157	-0.0522	-0.0252	-0.0604	-0.0110	-0.0483
	(0.0288)	(0.0522)	(0.0342)	(0.0487)	(0.0351)	(0.0494)
Comprehensive	0.1058*	-0.0285	0.1235**	-0.0461	0.0960	-0.0050
	(0.0622)	(0.0365)	(0.0611)	(0.0405)	(0.0675)	(0.0389)
Unspecified	0.1035	-0.0514	0.1519	-0.0584	0.0771	-0.0229
	(0.1463)	(0.0527)	(0.1569)	(0.0591)	(0.1418)	(0.0488)
N	867	1,734	865	1,732	867	1,734
State-level controls	No	No	No	No	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel II: All controls						
Abstinence	-0.0284	-0.0615	-0.0388	-0.0765*	-0.0232	-0.0526
	(0.0257)	(0.0458)	(0.0347)	(0.0415)	(0.0294)	(0.0454)
Comp	0.0745	-0.0276	0.0755	-0.0519	0.0727	-0.0062
	(0.0594)	(0.0380)	(0.0577)	(0.0464)	(0.0652)	(0.0385)
Unspecified	0.0923	-0.0337	0.1178	-0.0540	0.0777	-0.0038
	(0.1476)	(0.0584)	(0.1641)	(0.0680)	(0.1414)	(0.0532)
N	867	1,734	865	1,732	867	1,734
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel III: With state-specific linear time trends						
Abstinence	-0.0640	-0.0584	-0.0690	-0.0710	-0.0604	-0.0510
	(0.0500)	(0.0541)	(0.0505)	(0.0514)	(0.0506)	(0.0522)
Comprehensive	0.0149	-0.0253	0.0031	-0.0436	0.0206	-0.0063
	(0.0623)	(0.0407)	(0.0643)	(0.0479)	(0.0639)	(0.0402)
Unspecified	-0.0285	-0.0342	-0.0157	-0.0444	-0.0355	-0.0097
	(0.0999)	(0.0573)	(0.1004)	(0.0619)	(0.1020)	(0.0548)
N	867	1,734	865	1,732	867	1,734
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Means	4.133	4.430	2.580	3.407	5.761	5.510

Notes: Weighted estimates are obtained using data on STD rates for 15-19 year olds for columns 1, 3, and 5, and for 15-24 year olds for columns 2, 4, and 6, from CDC WONDER Online Database for the years 1997-2013. The variables "Abstinence", "Comprehensive", and "Unspecified" are the one-year lags of the Abstinence, Comprehensive, and Unspecified sex education indicators, respectively. State-level controls include the percentage of teens who are white, the percentage of teens who are black, the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcome means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

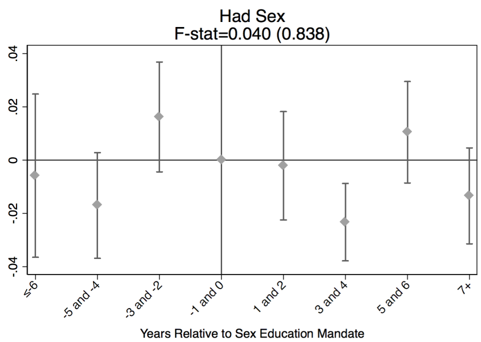
Table 1.8: The Effect of Sex Education Type on Logged Teenage Birth Rates

	All - DD	**All - DDD**	**Under 16 y.o. - DD**	**16 + y.o. - DD**
Panel I: No controls				
Abstinence	0.0235	0.0217**	0.0265	0.0211
	(0.0186)	(0.0098)	(0.0239)	(0.0192)
Comprehensive	0.0293	0.0306**	0.0571**	0.0251
	(0.0208)	(0.0151)	(0.0261)	(0.0209)
Unspecified	0.017	0.0093	0.0207	0.017
	(0.0342)	(0.0264)	(0.0535)	(0.0346)
N	867	1,734	859	867
State-level controls	No	No	No	No
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel II: All controls				
Abstinence	0.0073	0.0113	0.0044	0.0091
	(0.0143)	(0.0084)	(0.0192)	(0.0141)
Comprehensive	0.0204	0.0259*	0.0383	0.0213
	(0.0194)	(0.0131)	(0.0287)	(0.0190)
Unspecified	0.0253	0.0181	0.0306	0.0281
	(0.0406)	(0.0323)	(0.0706)	(0.0366)
N	867	1,734	859	867
State-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No
Panel III: With state-specific linear time trends				
Abstinence	0.0016	0.0039	-0.0028	-0.0029
	(0.0056)	(0.0074)	(0.0147)	(0.0061)
Comprehensive	-0.002	0.015	-0.0033	-0.0029
	(0.0068)	(0.0113)	(0.0252)	(0.0073)
Unspecified	0.0095	0.0073	-0.03	0.0124
	(0.0128)	(0.0278)	(0.0240)	(0.0134)
N	867	1,734	859	867
State-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes
Outcome Means	40.01	70.83	8.568	47.71

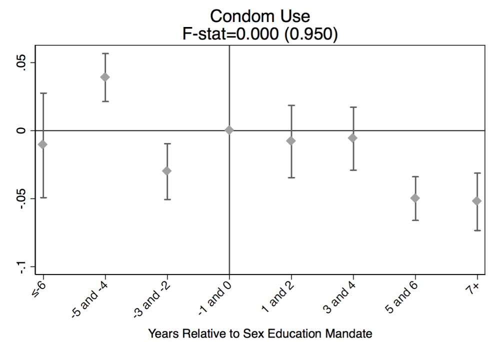
Notes: Weighted estimates are obtained using data on birth rates for females 15-19 year olds for columns 1, 3, and 4, and 15-24 year olds for column 2, from the National Center for Health Statistics, Division of Vital Statistics Natality Files for the years 1997-2013. The variables "Abstinence", "Comprehensive", and "Unspecified" are the one-year lags of the Abstinence, Comprehensive, and Unspecified sex education indicators, respectively. State-level controls include the percentage of teens who are white, the percentage of teens who are black, the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcome means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

1.10 Appendix Figures

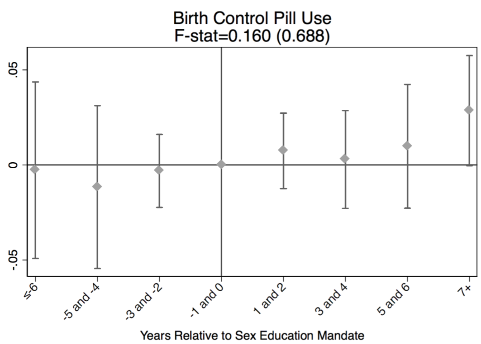
Figure 1.4: Event Study Figures of the Effect of Repealed Sex Education Mandates on Teenage Sexual Behaviors and Health



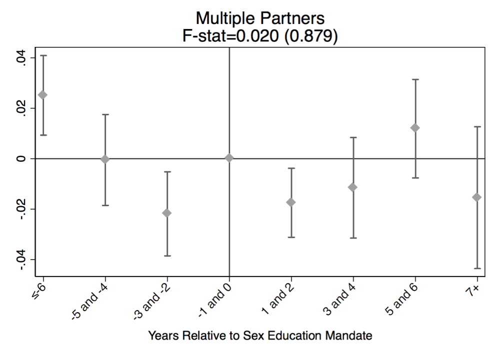
(a)



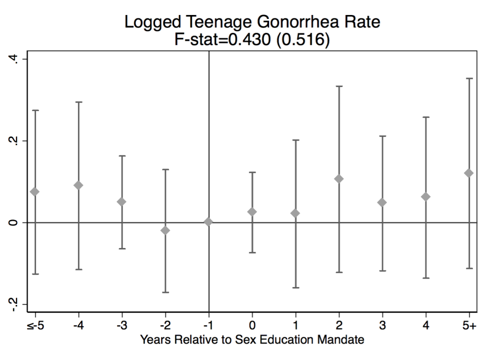
(b)



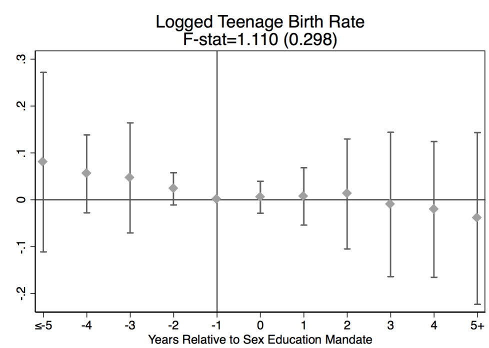
(c)



(d)



(e)



(f)

Notes: Estimates of Equation 2 described in the text. All estimates include individual controls (when applicable), state-level controls, and state and year fixed effects. 95 percent confidence intervals are shown extending from each point. The F-stat and p-value of a joint test of significance on the policy leads are also shown.

1.11 Appendix Tables

Table 1.9: The Effect of Any Sex Education on Teenage Health Behaviors

	Watch TV	**Bulemic**	**Wear Bicycle Helmet**
SexEd	-0.0069	0.0014	-0.1090
	(0.0104)	(0.0026)	(0.0750)
N	992,361	823,117	992,361
Outcome Means	0.768	0.055	0.269
State-level controls	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State-specific time trends	Yes	Yes	Yes

Notes: Weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS. Individual controls include age, grade, gender, and race. State-level controls include the unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.10: The Effect of Any Sex Education on Teenage Sexual Behaviors - Omitting NYRBS

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**
SexEd	0.0091	-0.0048	-0.0063	0.0096
	(0.0138)	(0.0054)	(0.0090)	(0.0058)
N	699,655	215,469	114,835	228,787
Outcome Means	0.327	0.598	0.226	0.266
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trends	Yes	Yes	Yes	Yes

Notes: Weighted linear probability model estimates are obtained using data from the 1997-2013 State YRBS. Individual controls include age, grade, gender, and race. State-level controls include the unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.11: The Effect of Any Sex Education on Teenage Sexual Behaviors - Probit Estimates

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**
SexEd	0.0098	-0.0046	-0.0073	0.0092
	(0.0142)	(0.0054)	(0.0090)	(0.0059)
N	822,385	260,909	137,569	275,142
Outcome Means	0.335	0.560	0.217	0.270
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trends	Yes	Yes	Yes	Yes

Notes: Weighted probit estimates are obtained using data from the 1997-2013 YRBS. Marginal effects are reported. Individual controls include age, grade, gender, and race. State-level controls include the unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Standard errors clustered at the state level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.12: The Effect of Any Sex Education on Teenage Sexual Behaviors By Age

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**
Panel I: Under 16 years old				
SexEd	0.0215	0.0005	-0.0204	0.0147
	(0.0145)	(0.0129)	(0.0139)	(0.0157)
N	308,956	58,994	31,814	62,600
Outcome Means	0.201	0.656	0.114	0.298
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trends	Yes	Yes	Yes	Yes
Panel II: 16 years old and over				
SexEd	0.0013	-0.0041	-0.0025	0.0088*
	(0.0142)	(0.0060)	(0.0112)	(0.0051)
N	513,429	201,915	105,755	212,542
Outcome Means	0.410	0.575	0.213	0.259
State-level controls	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes

Notes: Weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS. The variable "SexEd" is the one-year lag of the sex education indicator. Individual controls include age, grade, gender, and race. State-level controls include the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcomes means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.13: DD & DDD Estimates of Sex Education Type on Teenage Sexual Behaviors and Health

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**	**Gonorrhea**	**Birth rate**
Panel I: No controls						
Abstinence (w/ Abst-Plus)	0.0178	0.0088	0.0129*	0.0034	-0.0475	0.0213***
	(0.0126)	(0.0090)	(0.0069)	(0.0068)	(0.0470)	(0.0079)
Comprehensive	0.0254**	0.0122**	-0.0043	0.0086	-0.0238	0.0639**
	(0.0108)	(0.0057)	(0.0102)	(0.0165)	(0.0423)	(0.0202)
Unspecified	0.0173*	0.0171**	0.0017	0.0017	-0.0528	0.0219
	(0.0099)	(0.0082)	(0.0097)	(0.0067)	(0.0559)	(0.0222)
N	833,091	264,905	138,680	279,551	1,734	1,734
State-level controls	No	No	No	No	No	No
Individual-level controls	No	No	No	No	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel II: All controls						
Abstinence (w/ Abst-Plus)	0.0141	0.0060	0.0093	0.0082	-0.0550	0.0131*
	(0.0146)	(0.0096)	(0.0100)	(0.0068)	(0.0410)	(0.0074)
Comprehensive	0.0219*	0.0084	-0.0034	0.0050	-0.0246	0.0471**
	(0.0111)	(0.0081)	(0.0113)	(0.0133)	(0.0426)	(0.0215)
Unspecified	0.0080	0.0122*	-0.0062	-0.0014	-0.0385	0.0227
	(0.0089)	(0.0071)	(0.0170)	(0.0070)	(0.0568)	(0.0287)
N	822,385	260,909	137,569	275,142	1,734	1,734
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel III: With state-specific linear time trends						
Abstinence (w/ Abst-Plus)	0.0071	-0.0022	-0.0076	0.0087	-0.0521	0.0040
	(0.0147)	(0.0067)	(0.0091)	(0.0075)	(0.0482)	(0.0075)
Comprehensive	0.0192*	-0.0143	0.0085	0.0120	-0.0202	0.0324*
	(0.0114)	(0.0133)	(0.0126)	(0.0176)	(0.0466)	(0.0190)
Unspecified	0.0017	-0.0012	-0.0382***	0.0135*	-0.0366	0.0091
	(0.0137)	(0.0116)	(0.0139)	(0.0076)	(0.0580)	(0.0247)
N	822,385	260,909	137,569	275,142	1,734	1,734
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Means	0.335	0.560	0.217	0.270	4.430	70.83

Notes: DD weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS for columns 1-4. DDD weighted estimates are obtained using data on STD rates for 15-24 year olds for column 5 from CDC WONDER Online Database for the years 1997-2013. DDD weighted estimates are obtained using data on birth rates for females 15-24 year olds for column 6 from the National Center for Health Statistics, Division of Vital Statistics Natality Files for the years 1997-2013. The variables "Abstinence", "Comprehensive", and "Unspecified" are the one-year lags of the Abstinence, Comprehensive, and Unspecified sex education indicators, respectively. Abstinence sex education includes Abstinence-plus sex education. Individual controls include age, grade, gender, and race. State-level controls include the percentage of teens who are white, the percentage of teens who are black, the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcome means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.14: The Effect of Sex Education on Teenage Sexual Behaviors, Timing Heterogeneity

	Had Sex	**Condom Use**	**Birth Control Pill Use**	**Multiple Partners**	**Gonorrhea**	**Birth rate**
Panel I: No controls						
SexEd-On	0.0181	-0.0001	0.0084	0.002	0.0472	0.0421
	(0.0162)	(0.0092)	(0.0070)	(0.0079)	(0.0431)	(0.0369)
SexEd-Off	-0.0087	-0.0119*	0.0019	-0.0038	-0.0302	-0.0165
	(0.0080)	(0.0069)	(0.0104)	(0.0068)	(0.0751)	(0.0265)
F-test of On=Off (p-value)	5.09	1.14	0.47	0.69	1.76	6.53
	(0.0286)	(0.2907)	(0.4941)	(0.4096)	(0.1905)	(0.0137)
N	833,091	264,905	138,680	279,551	867	867
State-level controls	No	No	No	No	No	No
Individual-level controls	No	No	No	No	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel II: All Controls						
SexEd-On	0.0144	-0.0063	0.0062	0.0087	0.0237	0.0428
	(0.0197)	(0.0108)	(0.0109)	(0.0074)	(0.0459)	(0.0266)
SexEd-Off	-0.0084	-0.0151**	0.0017	0.0071	-0.0333	0.0163
	(0.0081)	(0.0061)	(0.0113)	(0.0080)	(0.0753)	(0.0202)
F-test of On=Off (p-value)	3.01	0.79	0.14	0.05	1.21	2.07
	(0.089)	(0.3788)	(0.7138)	(0.8284)	(0.2758)	(0.1565)
N	822,385	260,909	137,569	275,142	867	867
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	No	No	No	No	No
Panel III: With state-specific linear time trends						
SexEd-On	0.0118	-0.0056	-0.0044	0.0085	-0.0849*	0.0059
	(0.0178)	(0.0111)	(0.0112)	(0.0071)	(0.0467)	(0.0079)
SexEd-Off	0.0104	-0.0065	0.0014	0.0015	-0.1319	0.0075
	(0.0166)	(0.0146)	(0.0182)	(0.0089)	(0.0962)	(0.0096)
F-test of On=Off (p-value)	0.01	0.01	0.15	0.59	0.37	0.03
	(0.9303)	(0.9166)	(0.7025)	(0.4463)	(0.5449)	(0.8705)
N	822,385	260,909	137,569	275,142	867	867
State-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	No	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Means	0.335	0.560	0.217	0.270	4.133	70.83

Notes: DD weighted linear probability model estimates are obtained using data from the 1997-2013 YRBS for columns 1-4. DD weighted estimates are obtained using data on STD rates for 15-19 year olds for column 5 from CDC WONDER Online Database for the years 1997-2013. DD weighted estimates are obtained using data on birth rates for females 15-19 year olds for column 6 from the National Center for Health Statistics, Division of Vital Statistics Natality Files for the years 1997-2013. The variable "SexEd-On" is the one-year lag of an indicator equal to 1 when a sex education mandate is enacted, and 0 otherwise. The variable "SexEd-Off" is the one-year lag of an indicator equal to 1 when a sex education mandate is repealed, and 0 otherwise. Individual controls include age, grade, gender, and race. State-level controls include the teenage unemployment rate, real income per capita, state-level beer taxes, blood alcohol content laws, parental involvement laws for abortion, and mandatory wait laws for abortion services. Outcomes means are for the sample in Panel III. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Chapter 2

Does an Introduction of a Paid Parental Leave Policy Affect Maternal Labor Market Outcomes in the Short-Run? Evidence from Australia's Paid Parental Leave Scheme

2.1 Introduction

As of 2010, the United States and Australia were the only two OECD countries that did not have a comprehensive paid family leave program (International Labour Organization, 2014). Family leave programs are typically designed to provide new mothers time off of work to prepare for, or recover from, childbirth, and provide parents with time to care for their newborn or newly adopted children (Rossin-Slater, 2017). These policies aim to increase maternal employment, promote child health and development, and improve the work-family balance (Kunze, 2016). Family leave policies differ substantially across the globe, with some countries providing short, unpaid leave as in the United States, or more generous, paid family leave as in most European countries and Canada. Paid family leave is an important policy tool that can have considerable impacts on child outcomes, and maternal labor market outcomes.

Prior literature studying the effects of family leave on child outcomes and mothers' labor market outcomes is mixed, possibly due to the substantial heterogeneity in parental leave policies across the globe. Previous studies analyzing the effect of an introduction of paid family leave find positive labor market effects for the children affected by such a policy, and generally positive effects on maternal labor market outcomes. Carneiro et al. (2015) study the implementation of a four-month paid maternity leave policy in Norway in 1977, where the previous policy only granted three months of unpaid leave. The authors find that the introduction of the policy led to a two percentage point decline in high school dropout rates and a five percent increase in earnings at age 30 for the children affected by the policy. The authors also attempt to disentangle the potential mechanisms leading to these results by examining the effect of the reform on maternal labor market outcomes, and they find no long-term effects of the reform on mothers' employment 2 and 5 years after implementation or on their earnings 5 years after (Carneiro et al., 2015). Additionally, Rossin-Slater et al. (2013) examine the effect of the introduction of California's paid family leave program on leave-taking by mothers after childbirth and subsequent labor market outcomes using a difference-in-difference approach. The authors find that the overall use of maternity leave increased, and the paid family leave increased the number of usual weekly work hours. On the other hand, previous studies examining the extension of a previous family leave policy tend to find more mixed evidence on maternal labor market outcomes and child outcomes (Baker & Milligan, 2008; Kluve & Tamm, 2013; Liu et al., 2009; Rasmussen, 2010; Schönberg & Ludsteck, 2014).[1]

Given the substantial positive impacts paid family leave may have on child and maternal outcomes, more research on the causal effect of paid family leave on these outcomes is needed. This paper aims to shed additional light on the impact of introducing a paid family leave policy on maternal labor market outcomes by analyzing the effect of an introduction of a paid parental leave scheme in Australia.

In early 2010, the Australian government began ironing out the details for their forthcoming Paid Parental Leave (PPL) scheme for new parents who are the primary caregivers of a child born or adopted on or after January 1, 2011. An eligible primary caregiver would receive taxable PPL payments of the National minimum wage each week (currently $719.20), for a maximum of 18 weeks. This policy change creates variation in the receipt of paid family leave that allows me to assess how introducing a parental leave payment scheme after a child is born affects the mother's labor supply decisions. The PPL scheme is work contingent, and requires women to be in paid work and have worked continuously prior to the birth or adoption of a new child. The scheme aims to increase the average length of leave taken by employed women after childbirth by around ten weeks, and encourage increased workforce participation for women prior to having children and between pregnancies (Government, 2009).

[1] See Rossin-Slater (2017) for a review on recent empirical research related to introductions and expansions of global family leave policies.

The introduction of Australia's PPL scheme is similar to the Norwegian policy, but differs in the time of implementation, financing, length of leave, and benefit payments. This paper differs from the Carneiro et al. (2015) analysis and contributes to the existing paid family leave literature in several important ways. First, I develop theoretical predictions of the effect of an introduction of a paid leave scheme on mothers' labor supply decisions pre and post-birth. I then estimate the effect of the mother's pre and post-birth labor market outcomes using a regression discontinuity design, since PPL eligibility was based on the child's date of birth. Importantly, I test the hypothesis that the introduction of PPL impacts the labor supply decisions of mothers pre-birth – an effect that has not yet been estimated in the family leave literature. Next, I estimate the effect of the mother's post-birth labor market outcomes in the short-run, roughly 11 months after the policy passed. Finally, I am able to shed more light on the potential mechanisms leading to the significant effects on child outcomes Carneiro et al. (2015) found by estimating the effect of PPL not only on employment status, but also on hours worked, and the length of leave taken for the birth of the child.

The theoretical results imply that after the introduction of PPL, hours of work in the pre-birth period should decrease for mothers who will qualify for PPL, and increase for mothers who are attempting to qualify for PPL. Post-birth, the theoretical results imply that more mothers are out of work and on leave than would have been in the absence of PPL. The empirical results suggest that the PPL had no significant effect on the average number of hours or weeks worked pre-birth, the average age of the child when the mother returned to work, or the average number of hours worked post-birth. The empirical results appear at odds with the theoretical results, and this conflict could be due to three reasons.

First, the data used in this paper to estimate the impact of the PPL on maternal labor market outcomes is group-level data, provided as cell means for the respective variable (e.g. hours worked), instead of individual counts. Differential effects of the PPL scheme across subgroups of mothers, such as the age of the mother, race, etc. may be masked in the aggregate data. Second, using a regression discontinuity design to estimate the effect of PPL on pre-birth outcomes may not necessarily be the ideal empirical strategy. That is, mothers who are not certain if they will give birth before or after the January 1, 2011 cut point may not alter their behavior. A difference-in-difference design would likely yield more credible estimates, but pretreatment data on the pre-birth outcomes under study is not available. Third, regarding the lack of an effect on leave taking post-birth, mothers could be deciding pre-birth how much leave they will take, regardless of the leave being financed through the previous family assistance scheme or PPL scheme, and they will take as much leave as they would have in the absence of PPL.

The remainder of this paper is organized as follows. In Section 2, I provide background information on Australia's family assistance policy prior to the introduction of PPL, and more detail on the current PPL policy. In Section 3, I discuss the theoretical framework. In Section 4, I describe the empirical strategy and data used in the analysis. In Section 5, I present the empirical results. Finally, in Section 6, I present a discussion of the main findings and my conclusions.

2.2 Institutional Setting

2.2.1 Prior to Paid Parental Leave Introduction

Prior to the paid parental leave scheme implemented on January 1, 2011, employed women in Australia were entitled to 12 months of unpaid, job-protected maternity leave. All employees in Australia were eligible for the unpaid parental leave if they had completed at least 12 months of continuous service with their employer up until the time of birth. Some employers may have granted additional paid/unpaid maternity leave to their employees. Additionally, women who gave birth to a child from July 1, 2004 until December 31, 2013 and had a family adjusted taxable income of $75,000 or less in the 6 months after childbirth automatically received a non-taxable cash payment ranging between $3000 - $5,000 (i.e. the Baby Bonus)[2].

Furthermore, families may have also been eligible to receive the Family Tax Benefit (FTB). FTB Part A is a per child payment dependent upon the family's circumstances. Eligible families included those who cared for a dependent child age 0-19 years old, and who met income and residence requirements, and cared for the dependent child at least 35% of the time. The maximum rate per fortnight of the FTB Part A for each child aged 0-12 years of age was $182.84.[3] Single parents and families with one main income were also eligible for FTB Part B as a supplement to FTB Part A. FTB Part B is an income tested fortnightly payment that depends on the age of the youngest child. The maximum rate per fortnight for a child aged 0 to 5 was $155.54, and $108.64 for children aged 5 to 18.

2.2.2 Introduction of Paid Parental Leave

To be eligible for PPL, and receive taxable PPL payments of $672.70 a week (the Federal minimum wage level in Australia), for a maximum of 18 weeks, an individual must meet a number of requirements: they must be the primary caregiver of a child born or adopted on or after January 1, 2011, be in paid work and have been engaged in work continuously for at least 10 out of the last 13 months prior to the birth or adoption of the child, worked at least 330 hours in the 10 month period, not have worked between the date of birth or adoption of the child and their requested start date for PPL, and have an adjusted taxable income of $150,000 or less in the financial year prior to the date of birth or adoption of the child or the date of their claim, whichever is earlier.

PPL must be taken after the birth of the child and within 12 months of the birth or adoption

[2]When the Baby Bonus was introduced in 2004, it was worth $3,000. The Baby Bonus increased to $5,000 in July 2008.

[3]For each child aged 13 to 19 in full time secondary study, the FTB Part A fortnight maximum was $237.86, and for children aged 0-19 in an approved care organization was $58.66.

of the child. Parents who meet the eligibility requirements for PPL can choose to receive the Baby Bonus and other family assistance under the usual criteria instead of receiving PPL.[4] Parents are not eligible to change their claim to the Baby Bonus after PPL payments have begun. If eligible, families can still receive FTB Part A during receipt of PPL, but not FTB Part B. If the primary caregiver's employer provides employer-funded parental leave through an industrial agreement, they cannot withdraw the entitlement for the life of the agreement. Thus, the primary caregiver can receive PPL before, during, or after the employer-provided paid leave.

The Australian government estimated that more than eighty-five percent of families will be better off receiving PPL, and, on average, will receive roughly $2,000 more than if they chose the benefits under the previous scheme. Acknowledging that family circumstances tend to differ, the Government provides an online estimator simulating benefit amounts under the Baby Bonus, and under PPL to help families make the best family assistance decision (Government, 2009).

To provide an example of how entitlements changed after the introduction of PPL, Figure 2.1 calculates benefits provided to a working couple under the previous benefit system, and under the new system with PPL. This example is drawn from the Australian Government's "Paid Parental Leave - Information for Parents" booklet, and uses 2008-2009 rates. Let us assume a working couple, Jane and John, will birth their first child on August 5, 2011. Prior to the birth of their child, Jane and John each earned $52,000 a year. Jane is not eligible for paid maternity leave from her employer, but is entitled to 12 months of unpaid, job-protected leave. She is also eligible for PPL, and will receive taxable payments of $543.78 per week (the 2008-2009 minimum wage) for a maximum of 18 weeks, or a total of $9,788. Jane will care full-time for her child, and not return to work until July 1, 2012. In the financial year of their child's birth, Jane and John will receive $2,335 more in net family assistance and PPL

[4] In the case of multiple births (twins, triplets, etc), the parent can claim PPL for one child and the Baby Bonus for the other child/children.

than they would have without PPL. Table 1 provides these calculations. Under the previous system without PPL, Jane and John will receive the Baby Bonus, FTB Part A, and FTB Part B, totaling \$10,015. Under the new system with PPL, Jane and John will receive PPL, FTB Part A, and partial FTB Part B (which will resume after 18 weeks of PPL have been exhausted), totaling \$12,350.

2.3 Theoretical Framework

Once the PPL scheme was introduced, eligible mothers could receive 18 weeks of paid leave at the Australian minimum wage in addition to the 12 months of unpaid, job protected leave. The introduction of PPL could impact the labor market decisions of mothers who are eligible and choose to take the PPL at two different stages: pre-birth and post-birth. Figures 2.2 and 2.3 summarize the predicted effects of the PPL scheme on labor market decisions pre- and post-birth. To begin, Figure 2.2 shows the labor supply figure in income-leisure space (where hours of work are decreasing on the horizontal axis) for employed mothers' labor supply decision pre-birth, where E is the initial endowment (e.g. husband's income), L is hours of leisure, where leisure can be interpreted as time spent with the new child, and Y is consumption.[5,6] In Figure 2.2, the basic static labor supply graph is illustrated by line segment "ab" and indifference curve A. Under both family assistance regimes (i.e. with and without PPL), there is no effect on the labor supply decision of the mother in the pre-birth period. However, we can also consider the mother's intertemporal choice of labor supply in the pre-birth period.

Recall that women are required to be employed for 10 of the last 13 months prior to the

[5] As in the standard static labor supply model, the mother's budget constraint can be written as: $Y = w(T - L) + E$. This specification of the budget constraint assumes that the mother does not save, and the mother spends all of her income in the period under analysis.

[6] Also note that the FTB Part A is available to eligible families regardless of Baby Bonus of PPL receipt, and this increase in non-labor income is omitted from the figures since it is available under both regimes.

birth or adoption of the child, and have worked at least 330 hours in the 10 month period. Figure 2.2 also shows the intertemporal decision of the mother in the pre-birth period. The budget constraint will now kink once the mother works 330 hours. Once the mother works 330 hours in the 10 month period and qualifies for PPL, the budget constraint shifts up (represented by line segment "cd"), since she will now receive the PPL, and the mother moves from indifference curve A to indifference curve B. At indifference curve A, the mother was previously working L' hours (e.g. 350 hours) and already had qualified for the PPL. At indifference curve B, the mother is now working only L* (e.g. 330 hours) hours, but still qualifies for the PPL. Thus, hours of work in the pre-birth period should decrease for mothers who will qualify for the PPL. Alternatively, mothers working only 320 hours before the introduction of the PPL could increase their pre-birth work hours in order to qualify for the PPL (not pictured). Once their pre-birth work hours hit 330, their budget constraint will kink, and they will move to indifference curve B.

Figure 2.3 shows the labor supply decision of employed mothers post-birth. Post-birth, mothers can decide to take leave and return to work at some date in the future, or quit their job. If mothers value their job protection and prefer to return to work at their pre-birth employer, then they should return to work within the permitted length of leave, which is likely 12 months (given the 12 months of unpaid, job-protected leave). However, mothers also have the option to quit their job, and re-enter the labor market at a lower reservation wage. In the discussion that follows, I assume mothers choose to take leave and return to their pre-birth employer at some date in the future.[7] In Figure 2.3, I first illustrate the predicted labor supply effect before the introduction of the PPL, and upon receipt of the Baby Bonus. Mothers' non-labor income increases from receipt of the Baby Bonus, and her budget constraint shifts up by the amount BB (represented by line segment "ab"). Mothers' leisure hours and consumption increase, and her utility maximizing choice of leisure and consumption will occur at (L^{BB}, Y^{BB}), implying that Baby Bonus receipt increases the

[7]See Klerman & Leibowitz (1995) for a detailed discussion of post-birth dynamic labor supply effects.

amount of time mothers spend out of work and on leave with the child.

Next, in Figure 2.3, I also illustrate the predicted labor supply effect after the introduction of PPL. Non-labor income increases from receipt of PPL, and the budget constraint shifts up by the amount PPL (represented by line segment "cd"). It is important to note that the increase in non-labor income from PPL is larger than the increase in non-labor income from the Baby Bonus. This is the case because PPL provides 18 weeks of paid leave at the National minimum wage each week for 18 weeks, whereas the Baby Bonus provides a one-time payment of $5,000. Mothers who receive PPL increase their leisure hours more than mothers who receive the BB, since the marginal rate of substitution between work hours and leisure hours is higher. Thus, mothers who receive PPL spend more time out of work and on leave post-birth than mothers who receive the Baby Bonus, and their utility maximizing choice of leisure and consumption occurs at point (L^{PPL}, Y^{PPL}).

2.4 Empirical Strategy and Data

2.4.1 Empirical Strategy

My empirical strategy exploits the January 1, 2011 date-of-birth eligibility cutoff to identify the effect of PPL using a regression discontinuity design (RDD). In order for the effect to be identified via an RDD, it is assumed that mothers who give birth right before January 1, 2011 and mothers who give birth right after January 1, 2011 have similar characteristics in every dimension except for PPL receipt (D. S. Lee & Lemieux, 2010). The crucial identification issue is to what extent mothers could have influenced the date of birth of the current child in anticipation of the policy change. There are two reasons that would suggest mothers could not have influenced their child's birth date. First, the conception of a child is an event that cannot be perfectly timed by parents. Second, if parents could perfectly plan a conception and birth, this would require them to have been informed of the January 1, 2011 change at the time of conception. According to the Australian Government's Introduction to the PPL, full information for parents, employers and the community about how the scheme will operate, including guidelines for the program, was not available until October 2010 (Government, 2009). Therefore, births occurring around January 1, 2011 could not have been influenced by anticipation of the PPL scheme. However, it is possible that mothers could have influenced the timing of a birth by postponing induced births or planned caesarean sections.[8]

Figure 2.4 displays the daily number of births in Australia from November 1, 2010 to February 28, 2011 (2 months on each side of the cutoff), with linear fits on each side of the threshold.[9] Although visually there appears to be a slight discontinuity in births at the threshold, the estimated jump is not statistically significant.[10] Table 2.1 reports the estimates corresponding to Figure 2.4 in column 1, and estimates from varying bandwidths and polynomial choices. All estimates are statistically insignificant.

The other key identification assumption underlying the regression discontinuity procedure is that the conditional expectations of the outcomes (e.g. hours worked) with respect to the month of birth are smooth through the January 1, 2011 cut point. Although I cannot test this assumption directly, an implication is that there should be no discontinuities in predetermined variables. I test for discontinuities in predetermined characteristics of mothers, and examine if any discontinuities in the percentage of mothers who are Australian born, the average age of the mother, the average age of the mother's partner, and the percentage of mothers with only one child exist. Figure 2.5 panels (a) - (d) plot the distribution of these predetermined characteristics and the corresponding local linear regression estimates. There is no evidence of any discontinuities in the predetermined characteristics.

[8]Gans & Leigh (2009) find evidence that mothers delayed their births in response to the Baby Bonus – an Australian reform that changed fertility incentives.

[9]Daily birth count data was provided by the Australian Bureau of Statistics Customized Data and Information team.

[10]The gap in birth counts on each side of threshold is driven by more births occurring during the weekdays rather than on the weekends.

Therefore, in the absence of birth manipulation and discontinuities in predetermined characteristics, I am able to identify the effect of PPL by comparing outcomes of mothers giving birth during or after January 2011 to outcomes of mothers giving birth before January 2011. If the policy had an effect on maternal labor market outcomes, we would expect to see a sharp jump or fall in the outcome under study after the PPL cutoff date.

To analyze whether there is a discrete jump or fall in the studied outcomes, the following reduced-form equation is estimated:

$$Y_m = \alpha + \beta_1 M + \beta_2 Post + \beta_3 (M * Post) + \delta' X_m + \epsilon_m \tag{2.1}$$

where Y_m is the considered outcome in one-month age cell m, M is age in months re-centered at 0 in January 2011, $Post$ is a binary indicator equal to 1 in all months beginning January 2011, and X_m is a vector of one-month age cell covariates including the percentage of mothers who are Australian born, average age of the mother, average age of the mother's partner, and percentage of mothers with only one child. The linear term M accounts for any smooth fertility trends and is allowed to change on either side of the cutoff date.[11] The coefficient of interest, β_2, would capture a discrete jump or fall in the outcome around January 2011, and is identified by assuming no other factor affected the outcome discontinuously at the cutoff.[12]

Since the units of observation in equation 1 are sample means within cells that are defined by discrete values of age in months, the estimates and robust standard errors are obtained from regressions that are weighted by the number of mothers contributing to each outcome

[11] I also experiment with a quadratic term M, and show that the results are generally robust across specifications.

[12] No other policy changes in early 2011 applied differentially to children born before and after the January 1 cutoff date, including the cutoff birth date that determines the year when a child starts school.

cell.[13]

2.4.2 Data

My primary analysis will use data from the 2011 Australian Pregnancy and Employment Transitions Survey (PETS). The PETS is a supplement to the Australian Bureau of Statistics monthly Labor Force Survey. The survey was conducted in November 2011, and collected information on women's employment transitions during pregnancy, on starting or returning to work after the birth of their child, and job details, for birth mothers of a child living with them for which a child was under two years of age at the time of interview (Pink, 2011). The PETS data are well suited for this study because, most importantly, they contain the month and year of birth of all children, and they contain several measures of mothers' labor market outcomes, including employment status, income, number of hours worked, and the age of the child when the mother returned to work.[14]

Although the PETS data is the preferred data source for this analysis, there are two main limitations worthy of note. First, the data is available upon request as statistics in tabulated form from the Australian Bureau of Statistics.[15] That is, the data are group-level data, provided as cell means for the respective variable (e.g. hours worked), instead of individual counts. With individual-level data, I would be able to estimate the effect of PPL across different subgroups of women if we believe the effect varies across age of the mother, race, marital status, etc. Second, the PETS only includes mothers with children between the ages

[13] These estimates and standard errors are equivalent to estimates and standard errors that could be obtained by estimating unweighted regressions using individual-level data, and clustering the standard errors by month of birth (D. S. Lee & Lemieux, 2010).

[14] The HILDA (Household, Income and Labour Dynamics in Australia) survey, a household-based panel study that collects information about economic and personal well-being, labor market dynamics and family life, is also an appropriate data source that could be used for this analysis. However, the HILDA contains a very limited number of mothers who gave birth around the January 2011 threshold, and does not contain information about employment during pregnancy.

[15] The PETS data is also available as a microdata product accessible from a secure data lab in Australia. At this time, I do not have access to the microdata.

of 0 and 2 years old as of November 2011. Since the data is provided by month and year of birth of the child, this amounts to 24 month-year observations. Fewer month-year cells make it more difficult to estimate a discontinuity at the threshold (D. S. Lee & Card, 2008).

Despite these limitations, I use the PETS data to analyze the effect of PPL on PPL take-up, and three short-term maternal labor market outcomes: average number of hours worked per week immediately before stopping work to give birth, average number of weeks the mother stopped work before birth, and average number of hours actually worked per week in all jobs as of November 2011. The PETS data also include information on the number of women receiving the Baby Bonus and/or PPL. That is, in November 2011 when the survey was administered, women were asked if they received the Baby Bonus or PPL (or both in the case of multiple births) at the time of their child's birth. This information allows me to estimate the effect of PPL on Baby Bonus receipt - an indirect estimate of PPL take-up. Regarding the labor market outcomes, both hours worked outcomes and number of weeks stopped work are conditional on the mother being employed at the time of pregnancy. That is, only mothers who reported being employed while pregnant answered the survey questions about the number of hours worked before stopping work to give birth, the number of weeks stopped work before birth, and the number of hours worked at the time of survey in November 2011.

Additionally, I estimate the effect of PPL on the average age of the child when the mother returned to work. This question is asked of the PETS respondents in November 2011. That is, mothers who reported being employed and had already returned to work as of November 2011 reported a positive age of the child when they returned to work. For mothers who reported being employed but have not yet gone back to work, they reported an age of the child when they returned to work of 0 years old. All mothers, whether or not they have returned to work, are included in the estimation sample. Additionally, the sample is restricted to mothers who reported being employed while they were pregnant, as these women are most likely to be eligible for PPL.

Table 2.2 Panel I presents descriptive statistics pre- and post-PPL for each of the outcomes previously listed. Pre-PPL, the percentage of mothers receiving the Baby Bonus is 68.13, and post-PPL, this percentage substantially falls to 19. The average number of weeks that mothers stop work before giving birth is nearly 4 weeks both pre- and post-PPL. The average number of hours worked before stopping work for childbirth is similar pre- and post-PPL, with mothers working on average 30-31 hours. The age of the child when the mother returns to work differs between pre- and post-PPL, due to some mothers post-PPL just giving birth and remaining out of work and on leave (i.e. mothers who gave births in the few months before November 2011). Pre-PPL, mothers return to work when their child is 30 weeks old, and post-PPL, the average age of child is 12.5. Similarly, the average number of hours worked in all jobs per week is much lower post-PPL (4.6 weeks) than pre-PPL (16 weeks), again due to some mothers post-PPL just giving birth near the time of the survey.[16]

Table 2.2 Panel II presents descriptive statistics pre- and post-PPL for the included covariates. All covariate means are similar across both time periods. The average number of mothers who are Australian born is around 78-80 percent, the average age of the mother's partner is roughly 35 years, the average age of the mother is around 32 years, and the percentage of mothers with only one child is about 49 percent.

[16] Hours worked per week in all jobs as of November 2011 is asked of all mothers who reported being employed while pregnant, whether or not they started work since giving birth. Thus, the average number of hours worked per week as of November 2011 includes zeros.

2.5 Results

2.5.1 Paid Parental Leave Take-up

The effects of the PPL scheme on maternal labor market outcomes are summarized in Figures 2.6 through 2.10, and Tables 2.3 through 2.5. I begin by showing the effect of PPL on the percentage of mothers receiving the Baby Bonus. Recall that women who gave birth to a child from July 1, 2004 until December 31, 2013 and had a family adjusted taxable income of $75,000 or less in the 6 months after childbirth automatically received the Baby Bonus. Once the PPL scheme was introduced, mothers who gave birth on or after January 1, 2011 were able to receive either the PPL or the Baby Bonus, dependent upon eligibility for both. Thus, only mothers who had family income below $75,000 six months after childbirth and who had taxable income of $150,000 or less in the year before birth were eligible for both the Baby Bonus and PPL (but still only able to receive one or the other). Due to data limitations, I am unable to limit the Baby Bonus analysis to those that were eligible for both the Baby Bonus and PPL. Therefore, I estimate the effect of PPL on the average Baby Bonus receipt among the restricted sample (i.e. those women who reported being employed while pregnant) in the PETS.

A mother could not claim both the Baby Bonus and PPL, except in the case of multiple births. In order to determine the take-up of PPL, I look for a discontinuity in the percentage of mothers receiving the Baby Bonus. Since families receiving PPL would, on average, receive around $2000 more than under current family assistance arrangements, it is expected that receipt of the Baby Bonus should decrease once the PPL scheme was introduced (Government, 2009). Figure 2.6 panel (a) suggests that receipt of the Baby Bonus significantly decreases after the implementation of the PPL scheme. Table 2.3 reports the results from equation 1 on Baby Bonus receipt, where the dependent variable is the percentage of mothers receiving the Baby Bonus. The results show that receipt of the Baby Bonus decreases by

46 percent after the implementation of PPL.[17] Figure 2.6 panel (b) displays the estimates and 95% confidence intervals for the effect of PPL on Baby Bonus receipt using various bandwidths. The results remain stable and significant across the different bandwidths.

2.5.2 Labor Market Outcomes

Next, I report results for the effect of the PPL on logged maternal labor market outcomes in Table 2.4. Panel I includes a linear term in m, and Panel II includes a quadratic term in m. I first begin with discussing the effect of PPL on maternal labor market outcomes pre-birth, and then discuss the effect of PPL post-birth.

Given the work requirements for PPL eligibility, and the associated labor supply predictions presented in Figure 2.2, we would expect PPL to either increase or decrease pre-birth labor hours depending on the mothers' current work hours. Table 2.4 column 1, Panels I and II, suggest that the PPL scheme increases the average number of hours worked before stopping work by roughly 4 percent, but the effect is not statistically significant. Figure 2.7 panel (a) presents the corresponding regression discontinuity figure, and estimates by varying bandwidths are presented in Figure 2.7 panel (b). Across varying bandwidths, the estimates are fairly stable, and appear to hover around zero as the bandwidth increases. This effect is in line with the predictions, if mothers not meeting the work requirements to be eligible for PPL are inclined to increase their work hours pre-birth. However, this positive effect, although insignificant, is surprising. Recall that mothers are required to be employed for 10 of the last 13 months prior to the birth of the child, and have worked at least 330 hours in the 10 month period. A mother could qualify for PPL by working 8.25 hours a week in the 10 months prior to the birth of her child. Full-time, employed women in Australia work an average of 39.8 hours per week, and part-time employed women work 18.8 hours

[17] Columns 2 and 3 of Table 2.3 include quadratic and cubic terms in m, respectively. The estimated coefficients slightly decline in magnitude, but remain highly statistically significant.

per week, and women typically stop working 1 to 3 weeks before childbirth.[18] Therefore, we should expect women to decrease their work hours, since they most likely already qualify for the PPL. The positive effect could be driven by more disadvantaged mothers attempting to increase their work hours in order to qualify for the PPL. Due to data limitations, I am unable to parse out the effect by socioeconomic subgroups.

Similarly, if PPL receipt could affect the number of hours worked pre-birth, we may expect PPL to also affect the number of weeks the mother stopped work before the birth of her child. If women are attempting to qualify for the PPL, they may decrease the number of weeks they stop working before the birth of their child. Similarly, for women who are already going to qualify for the PPL, they may increase the number of weeks they stop working before the birth of their child. Table 2.4, column 2 suggests that indeed women are decreasing the number of weeks they stop working before birth, but the estimated effects are statistically insignificant. In Figure 2.8 panel (b), the estimated effects across varying bandwidths are relatively stable, but still indistinguishable from zero. However, the precision of the estimate is such that I can rule out, at the ninety-five percent confidence level, the number of weeks stopped work before the birth increasing by more than 16 percent, and decreasing by more than 50 percent. It is important to note, though, that the lack of a statistically significant effect of the PPL on the average number of hours worked before stopping work for birth and the average number of weeks stopped working before birth could be due to the empirical methodology used to estimate the effect. Mothers who are not certain if they will give birth before or after the January 1, 2011 cut point may not alter their behavior. Thus, using a regression discontinuity design to estimate the effect of PPL pre-birth may not be the most ideal empirical strategy. A difference-in-difference design would likely yield more precise estimates, but pretreatment data on the outcomes under study is not available.

Once a mother gives birth, she can either choose to go back to work right away (within a

[18] These figures were obtained from the Organization for Economic Cooperation and Development: https://stats.oecd.org/Index.aspx?DataSetCode=ANHRS accessed December 2, 2017.

reasonable amount of time post-birth), or remain at home and care for her child. After the introduction of PPL, the predictions discussed above imply that more mothers should be out of work and on leave than mothers who did not receive the PPL. Although I cannot directly measure the average length of leave the mother took for the birth of her child, I can measure the average age of the child when the mother returned to work. Table 2.4, column 3 suggests that the introduction of PPL significantly increases the average age of the child when the mother returned to work by 90.5 percent, or 27.5 weeks (30.41*0.905). However, it is clear in Figure 2.9 panel (a) (and Table 2.4, panel II, column 3) that the positive effect of PPL on this outcome is driven by the linearity imposed in the RDD model, and the downward trend on the right side of the threshold - a result of mothers just giving birth near the time of the survey still being out of work and at home with their child. If we look at a narrower bandwidth, illustrated in Figure 2.9 panel (b), it does not appear that PPL significantly increases the average age of the child when the mother returns to work. Taken together, it seems that the PPL scheme does not significantly increase the average length of leave taken by the mother post-birth. Since this effect was a primary goal of the policy, this finding is of considerable importance.

Though there is no clear aggregate effect of PPL increasing mothers' length of leave, this null finding may mask heterogeneity in subgroup effects. Specifically, it may be that the average length of leave varies by socio-economic status, with the most advantaged (disadvantaged) mothers being able (not able) to afford to take the full 18 weeks of paid leave. To test this, I collected PETS data on the average age of the child when the mother returned to work by income quintile measured at the time of pregnancy.[19] Table 2.5 reports the results for the effect of PPL on the average age of the child when the mother returned to work by income quintile. The positive effect persists across nearly all quintiles, but all estimates are statistically insignificant. Although the estimates for all quintiles are imprecise, the evidence

[19] Average income per week by quintile is as follows: 1st quintile = \$555.27; 2nd quintile = \$1,140.94; 3rd quintile = \$1,580.48; 4th quintile = \$2,083.20; 5th quintile = \$3,684.51. Mothers in the fifth income quintile have an average income that is nearly 7 times higher than mothers in the first quintile.

is suggestive that all women are increasing their average length of leave as a result of PPL.

Although there does not appear to be an effect on the average age of the child when the mother returns to work, we may still see an effect on the number of hours worked post-birth as of November 2011. Women who receive the PPL may choose to go back to work at the same time as mothers who did not receive the PPL, but the intensity of their work hours may differ. The receipt of National minimum wage each week for 18 weeks for PPL mothers may allow those mothers to return to work part-time during the 18 week period. If this is the case, then as of November 2011 these mothers will have returned to work, but work fewer hours per week than mothers who did not receive the PPL. Table 2.4, column 4, and Figure 2.10 panel (a) present the effect of PPL on average number of hours worked per week as of November 2011. In Figure 2.10 panel (a), the downward trend on the far right side of the threshold is likely a result of mothers just giving birth near the time of the survey still being out of work and at home with their child. Although there is a clear negative trend in work hours within a narrow bandwidth of the threshold, the resulting RD estimate is small and statistically insignificant. Across varying bandwidths (Figure 2.10 panel(b)), this result appears to hover around zero, suggesting that mothers are not significantly decreasing their work hours as a result of PPL receipt.

2.6 Discussion and Conclusion

Using the introduction of a paid parental leave scheme in Australia on January 1 2011, this paper presents theoretical predictions, and estimates the effect of an introduction of a family leave policy on maternal labor market outcomes. The theoretical predictions imply that after the introduction of PPL, hours of work in the pre-birth period should decrease for mothers who will qualify for PPL, and increase for mothers who are attempting to qualify for PPL. Post-birth, more mothers are out of work and on leave than would have been in the absence of PPL. By comparing the outcomes of mothers with babies born before January 1, 2011 to the outcomes of mothers with babies born on or after January 1, 2011 using a regression discontinuity design, the empirical results show that the introduction of the PPL scheme significantly decreases receipt of the Baby Bonus by 46 percent. Regarding maternal labor market outcomes pre and post-birth, the results suggest that the PPL had no effect on hours or weeks worked pre-birth, hours worked post-birth, or average length of leave taken by the mother. From a policy perspective, the last result is striking since the Australian government introduced the PPL scheme with the primary goal of increasing the average length of leave taken by the mother for the birth of her child.

The lack of an effect on length of leave taken post-birth after the introduction of PPL appears at odds with the theoretical predictions discussed above. In the conceptual framework, the post-birth labor market prediction after the introduction of PPL implies a positive income effect, where both consumption and leisure increase. However, the empirical results suggest a zero income effect, where length of leave remains unchanged, and consumption increases. This could be due to mothers deciding pre-birth how much leave they will take, regardless of the leave being financed through the Baby Bonus or PPL. Recall the average age of the child when the mother returns to work is 27.3 weeks. If mothers were already taking longer than 18 weeks of leave without pay, an introduction of paid leave of a shorter length of time would not alter their behavior. They will take as much leave as they would have in the absence of PPL, and simply be happy that some of it turns out to be paid.

The effect of an introduction of a paid family leave policy is of great importance and needs to be understood, especially given the current conversations in the United States and other countries around the world about introducing paid family leave. Although the PPL results of this paper do not appear to affect labor market outcomes for mothers on average, it is possible that the effects of PPL for some subgroups are masked. Future work would benefit from individual-level data that could be used to estimate the effect of PPL across different subgroups of women, beyond income quintiles, if we believe the effect varies across age of the mother, race, marital status, etc. Additionally, PPL may have significant positive effects on the children affected by the policy. If mothers receiving PPL are not increasing their length of leave, but are collecting PPL payments throughout some of the leave, this extra income may be used to benefit the child. This paper does not have the data necessary to explore this hypothesis, but it is worth exploring in future work related to paid parental leave policies.

2.7 Figures

Figure 2.1: Family Assistance Example

Family Income Information		
	Usual annual Salary	Annual salary in the financial year of the birth
Jane	$52,000	$4,986
John	$52,000	$52,000
Total Family Income	$104,000	$56,986

Family Assistance Calculations		
Type of Assistance	Previous System without PPL	New System with PPL
Paid Parental Leave (taxable)*	$0	$9,788
Baby Bonus (non taxable)	$5,000**	$0
Family Tax Benefit A	$1,759	$1,759
Family Tax Benefit B***	$3,256	$919
Total Assistance before tax	$10,015	$12,466
Net tax paid on PPL	na	$116
Total assistance (net of tax)	$10,015	$12,350

Notes: *PPL counts as income for determining eligibility for Family Tax Benefit; **When the Baby Bonus was introduced in 2004, it was worth $3,000. The Baby Bonus increased to $5,000 in July 2008. In ***Families are precluded from receiving FTB-B during the 18 week period of PPL. Source: Australian Government "Paid Parental Leave Scheme - Information for Parents" 2010.

Figure 2.2: Predicted Effect of Maternal Labor Supply Pre-birth

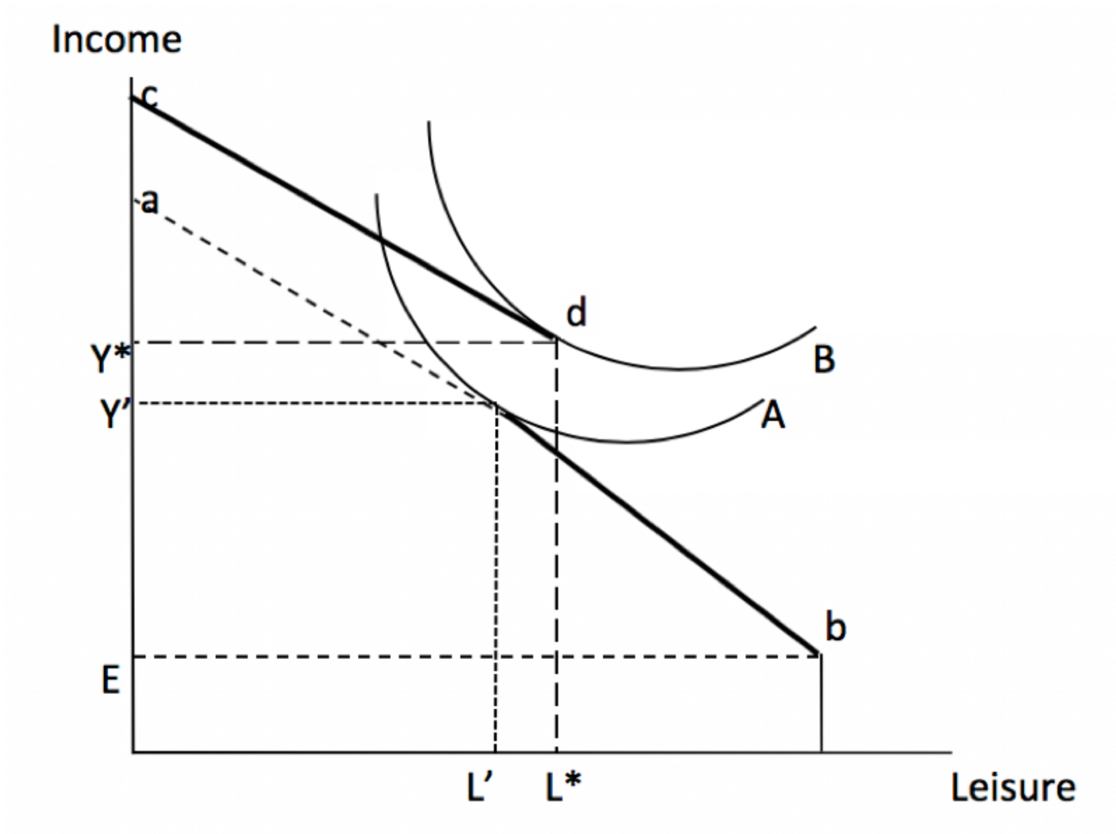


Figure 2.3: Predicted Effect of Maternal Labor Supply Post-birth

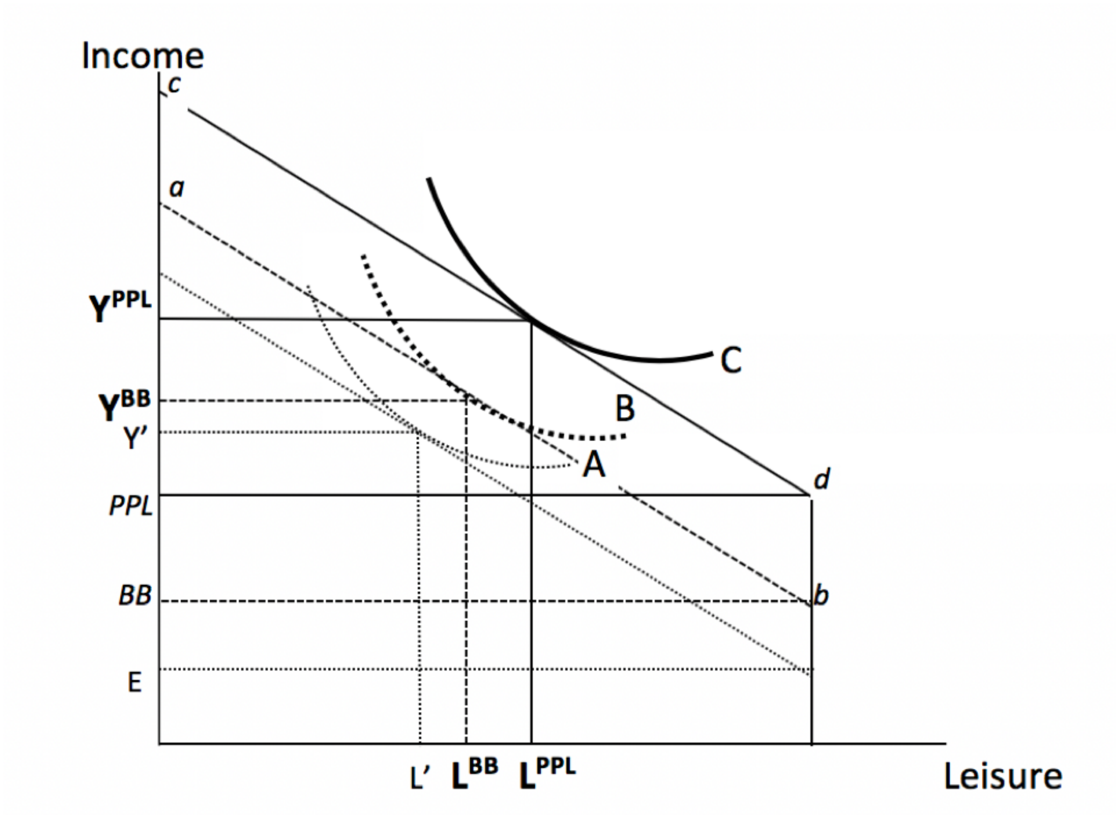
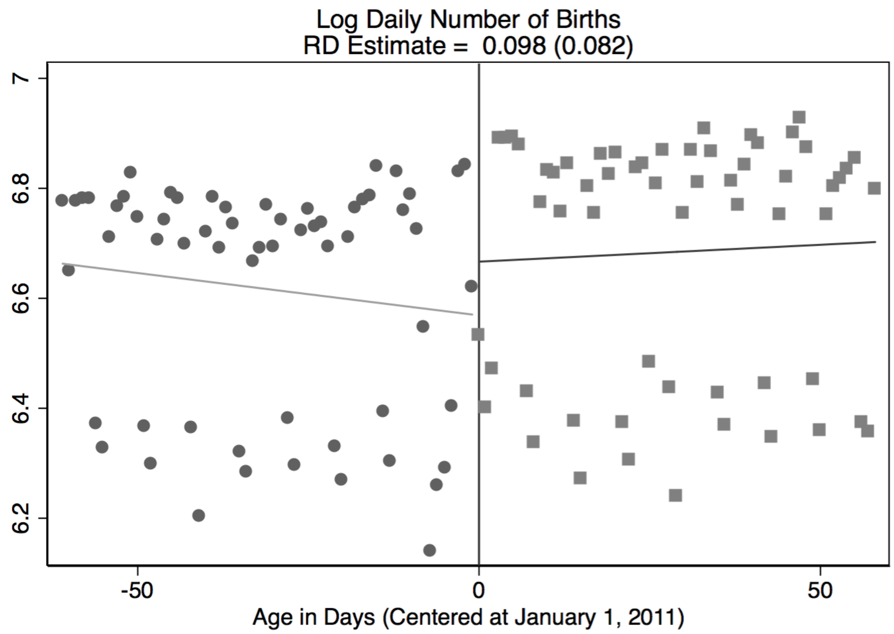
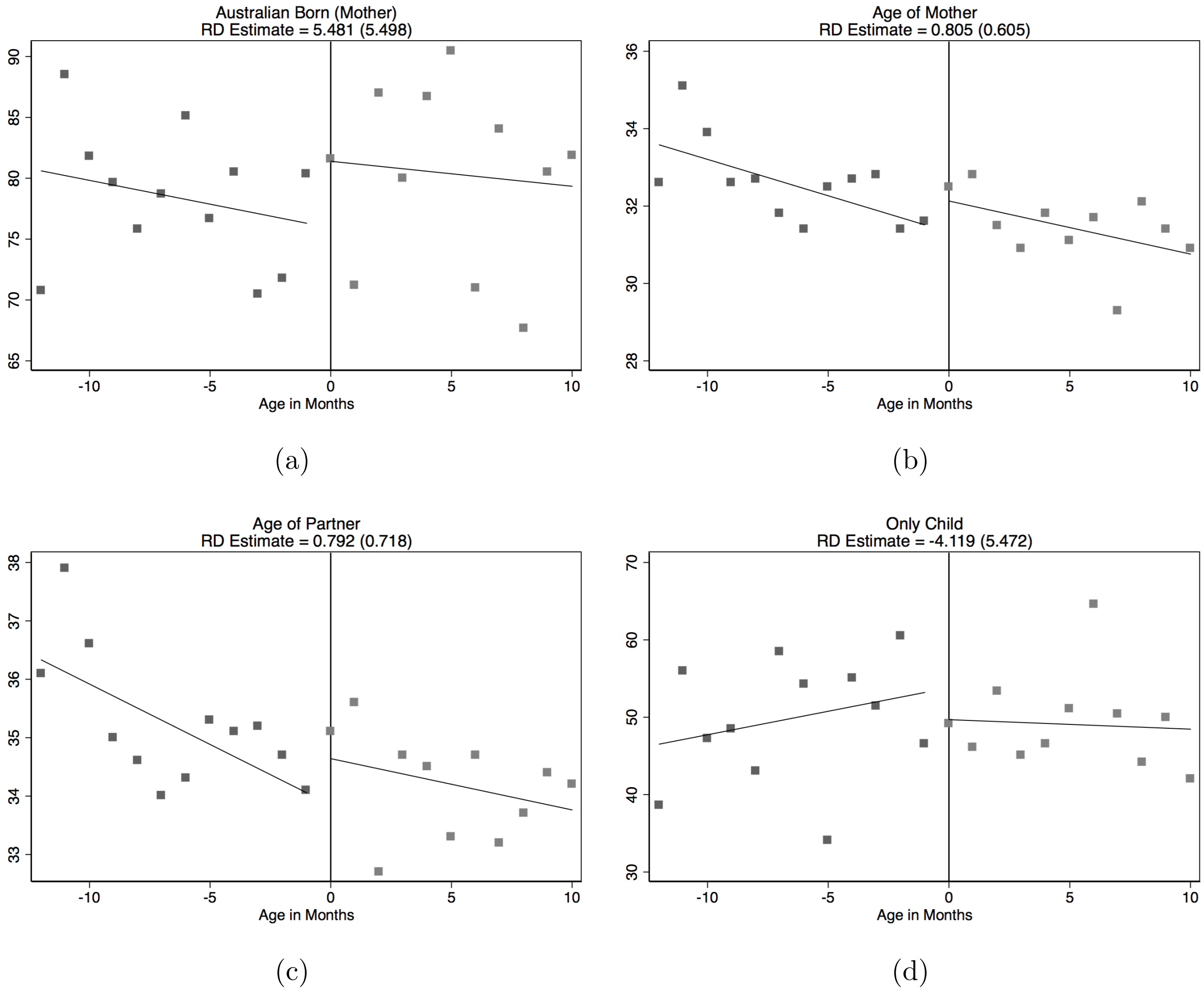


Figure 2.4: Births Discontinuity



Notes: The log of the number of children born to mothers from November 1, 2010 to February 28, 2011, and separate linear fits on each side of the January 1, 2011 cutoff. The vertical line denotes the PPL cutoff of January 1, 2011, normalized to 0.

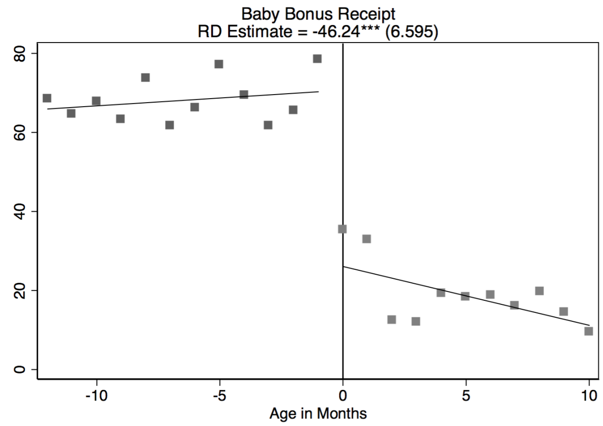
Figure 2.5: Balance in Predetermined Characteristics



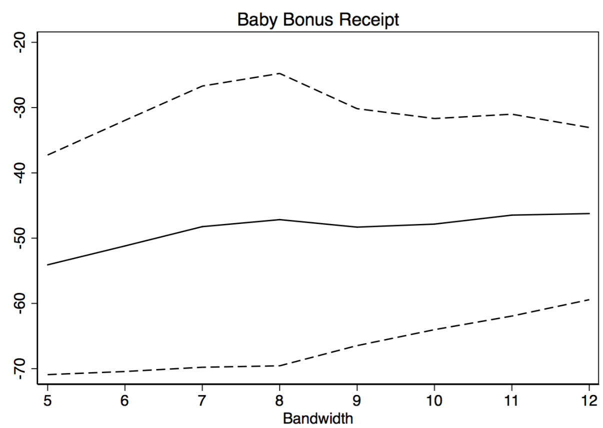
(a) (b) (c) (d)

Notes: Balance in covariates around the January 1, 2011 threshold: (a) Percentage of mothers who are Australian born; (b) Average age of the mother; (c) Average age of the mother's partner; (d) Percentage of mothers with only 1 child.

Figure 2.6: Baby Bonus Receipt



(a)

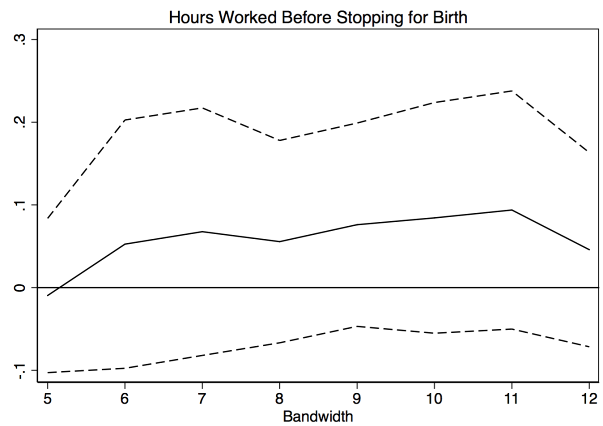


(b)

Notes: The percentage of mothers receiving the Baby Bonus. In panel (a), each data point corresponds to the average value of the percentage of mothers receiving the Baby Bonus by month of birth of the child in 1-month bins. The window includes all children born to mothers from January 2010 until November 2011.The vertical line denotes the PPL cutoff of January 1, 2011, normalized to 0. Panel (b) presents the equivalent coefficient estimates and associated 95% confidence intervals when equation 1 is estimated using 3, 4,...,12 month-of-birth cohorts on either side of the threshold.

Figure 2.7: Average Number of Hours Worked Before Stopping Work for Birth

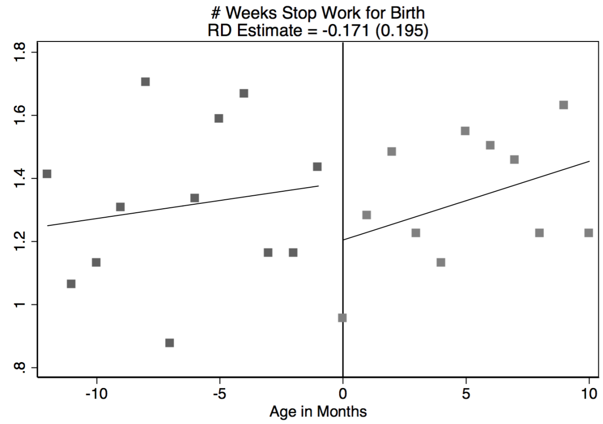
(a)



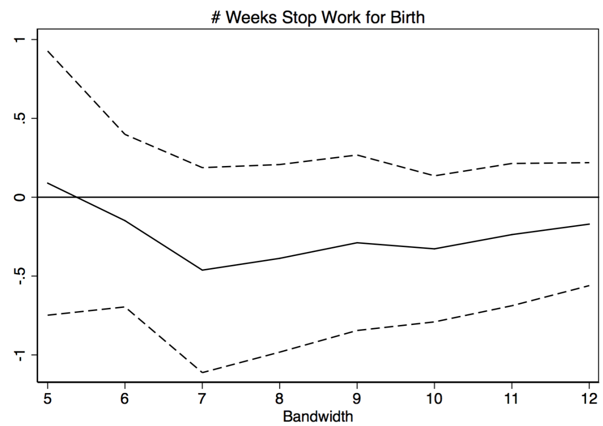
(b)

Notes: Average number of hours worked by the mother immediately before stopping work for the birth of her child. In panel (a), each data point corresponds to the average value of each outcome by month of birth of the child in 1-month bins. The window includes all children born to mothers from January 2010 until November 2011. The vertical line denotes the PPL cutoff of January 1, 2011, normalized to 0. Panel (b) presents the equivalent coefficient estimates and associated 95% confidence intervals when equation 1 is estimated using 3, 4,...,12 month-of-birth cohorts on either side of the threshold.

Figure 2.8: Average Number of Weeks Stopped Working before Birth



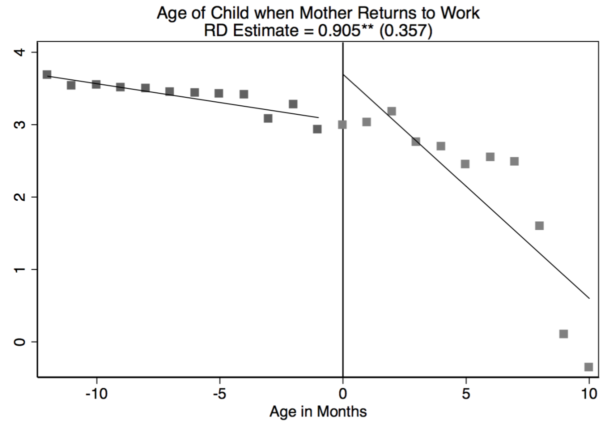
(a)



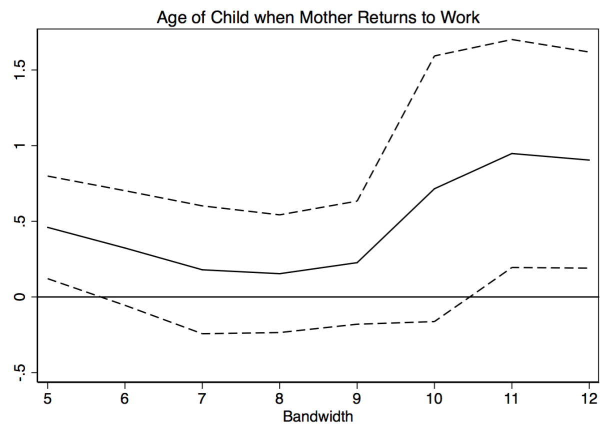
(b)

Notes: Average number of weeks the mother stopped work before the birth of her child. In panel (a), each data point corresponds to the average value of each outcome by month of birth of the child in 1-month bins. The window includes all children born to mothers from January 2010 until November 2011. The vertical line denotes the PPL cutoff of January 1, 2011, normalized to 0. Panel (b) presents the equivalent coefficient estimates and associated 95% confidence intervals when equation 1 is estimated using 3, 4,...,12 month-of-birth cohorts on either side of the threshold.

Figure 2.9: Average Age of the Child When the Mother Returned to Work



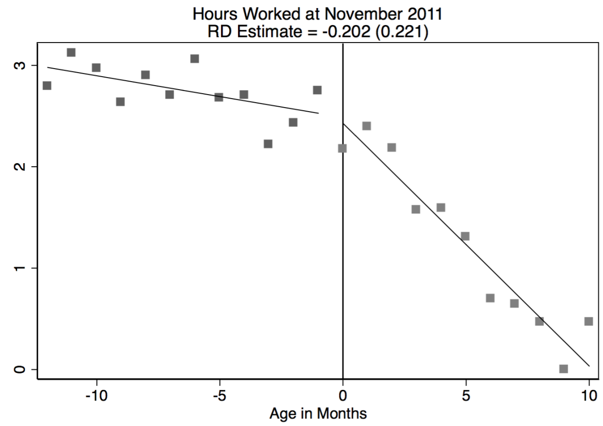
(a)



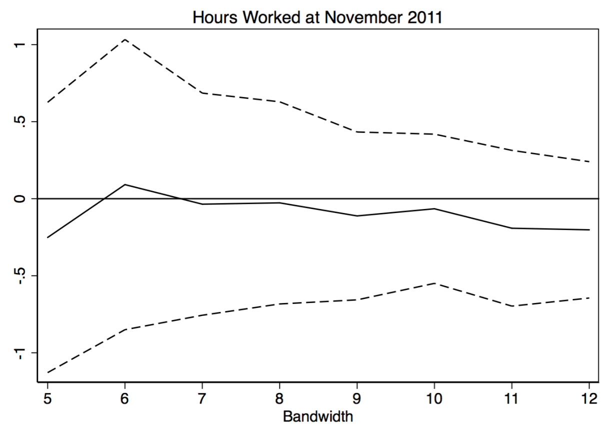
(b)

Notes: Average age of the child when the mother returned to work. In panel (a), each data point corresponds to the average value of each outcome by month of birth of the child in 1-month bins. The window includes all children born to mothers from January 2010 until November 2011. The vertical line denotes the PPL cutoff of January 1, 2011, normalized to 0. Panel (b) presents the equivalent coefficient estimates and associated 95% confidence intervals when equation 1 is estimated using 3, 4,...,12 month-of-birth cohorts on either side of the threshold.

Figure 2.10: Average Number of Hours Worked at November 2011



(a)



(b)

Notes: Average number of hours worked by the mother as of November 2011. In panel (a), each data point corresponds to the average value of each outcome by month of birth of the child in 1-month bins. The window includes all children born to mothers from January 2010 until November 2011. The vertical line denotes the PPL cutoff of January 1, 2011, normalized to 0. Panel (b) presents the equivalent coefficient estimates and associated 95% confidence intervals when equation 1 is estimated using 3, 4,...,12 month-of-birth cohorts on either side of the threshold.

2.8 Tables

Table 2.1: Discontinuity in Births at the January 1, 2011 Threshold

	2-2m	**2-2m**	**2-2m**	**1-1m**	**1-1m**	**1-1m**
Births	0.098	0.055	0.120	0.127	0.043	-0.047
	(0.082)	(0.125)	(0.166)	(0.120)	(0.168)	(0.189)
N (Number of Days)	120	120	120	62	62	62
Linear term in m	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic term in m	No	Yes	Yes	No	Yes	Yes
Cubic term in m	No	No	Yes	No	No	Yes

Notes: Each coefficient reported is the estimated discontinuity in the daily number of logged births at the threshold as a result of the PPL scheme. The 'm' in each column heading stands for months. Each coefficient is from a different regression. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.2: Descriptive Statistics of Analysis Variables

	Pre-PPL	Post-PPL
Panel I: Outcomes		
Percentage receiving Baby Bonus	68.13	19.00
	(5.647)	(8.183)
Average number of weeks stopped work before birth	3.858	3.864
	(0.979)	(0.775)
Average number of hours worked before stopped for birth (per week)	30.07	31.07
	(2.021)	(2.306)
Average age of the child when the mother returned to work (in weeks)	30.41	12.54
	(5.804)	(7.712)
Average number of hours worked in all jobs (per week)	16.04	4.564
	(3.865)	(3.507)
N (number of months)	11	12
Panel II: Covariates		
Australian born (mother)	78.33	80.18
	(5.584)	(7.324)
Age of partner	35.24	34.22
	(1.154)	(0.877)
Age of mother	32.59	31.45
	(1.067)	(0.943)
Only child	49.48	49.33
	(8.067)	(6.090)
N (number of months)	11	12

Notes: Data on each outcome was provided by the Australian Bureau of Statistics. The covariates in Panel II are: the percentage of mothers who are Australian born, the average age of the mother's partner, the average age of the mother, and the percentage of mothers with only one child. Means and standard deviations (in parentheses) are presented.

Table 2.3: Baby Bonus Receipt

	12-12m	**12-12m**	**12-12m**
Baby Bonus	-46.24***	-44.52***	-43.06***
	(6.595)	(10.11)	(7.703)
N (Number of Months)	23	23	23
Controls	Yes	Yes	Yes
Linear term in m	Yes	No	No
Quadratic term in m	No	Yes	No
Cubic term in m	No	No	Yes

Notes: The coefficient reported is the estimated discontinuity in Baby Bonus Receipt as a result of the PPL scheme. The 'm' in the column heading stands for months. An observation is the outcome rate for a one month age cell. Controls include the percentage of mothers in each age cell that are Australian born, the average age of the mother in each age cell, the average age of the partner in each age cell, and the percentage of mothers with one child in each age cell. Estimates are weighted by the population, and robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.4: The Effect of PPL on Maternal Labor Market Outcomes

	Hours Before Birth	**Weeks Stop Before Birth**	**Age of Child Return**	**Hours Worked, Nov '11**
	(1)	(2)	(3)	(4)
Panel I				
PPL	0.046	-0.171	0.905**	-0.202
	(0.059)	(0.195)	(0.357)	(0.221)
N (Number of Months)	23	23	23	23
Controls	Yes	Yes	Yes	Yes
Linear term in m	Yes	Yes	Yes	Yes
Panel II				
PPL	0.042	-0.324	0.036	-0.101
	(0.112)	(0.272)	(0.207)	(0.390)
N (Number of Months)	23	23	23	23
Controls	Yes	Yes	Yes	Yes
Quadratic term in m	Yes	Yes	Yes	Yes

Notes: The coefficient reported is the estimated discontinuity in the outcome as a result of the PPL scheme. Each coefficient is from a different regression. An observation is the outcome rate for a one month age cell. Controls include the percentage of mothers in each age cell that are Australian born, the average age of the mother in each age cell, the average age of the partner in each age cell, and the percentage of mothers with one child in each age cell. Estimates are weighted by the population, and robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.5: Average Age of the Child when the Mother Returned to Work, by Income Quintile

	1st Quintile	**2nd Quintile**	**3rd Quintile**	**4th Quintile**	**5th Quintile**
Panel I					
Avg. Age of Child	-0.153	1.012	1.248	0.171	0.529
	(0.771)	(0.675)	(1.074)	(0.529)	(0.466)
N (Number of Months)	23	23	23	23	23
Controls	Yes	Yes	Yes	Yes	Yes
Linear term in m	Yes	Yes	Yes	Yes	Yes
Panel II					
Avg. Age of Child	0.067	0.325	1.160	0.965	0.219
	(1.325)	(0.697)	(1.480)	(0.764)	(0.837)
N (Number of Months)	23	23	23	23	23
Controls	Yes	Yes	Yes	Yes	Yes
Quadratic term in m	Yes	Yes	Yes	Yes	Yes

Notes: Each coefficient reported is the estimated discontinuity in the outcome as a result of the PPL scheme. Each coefficient is from a different regression. An observation is the outcome rate for a one month age cell. Controls include the percentage of mothers in each age cell that are Australian born, the average age of the mother in each age cell, the average age of the partner in each age cell, and the percentage of mothers with one child in each age cell. Estimates are weighted by the population, and robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Chapter 3

The Effect of Technology Investment on Student Achievement

3.1 Introduction

"I can't stress enough, education is the single most important investment we can make in our future, and technology is the tool for the greatest return. Technology has the power to enhance the work of our educators and create a more immersive and engaging learning experience for students." – Margo Day, Vice President of U.S. Education for Microsoft

Educational technology in schools has substantially increased in the past decade. The number of laptops and tablets in U.S. K-12 schools grew by 363 percent over the past seven years, from roughly 3 million devices in 2010 to almost 14 million in 2017 (Bushweller, 2017). By the end of 2015, the U.S. spent $4.7 billion on instructional technology in K-12 schools (Schaffhauser, 2016). While in office, President Obama called for nearly $3 billion in commitments from the Federal Communications Commission and many private technology companies with an aim to "close the technology gap in our schools" (Bidwell, 2014). Moreover, an increasing number of schools are experimenting with one-to-one laptop programs that provide each student with a computer to use in the classroom, and often at home (Bulman & Fairlie, 2016).[1] Many proponents of providing more technology to schools hope that additional access will help close some achievement gaps, while opponents argue that such improvements are overvalued because little evidence exists that technology improves teaching and learning (McDermott & Gormley, 2016). Given the recent sizable increase in technology spending, and technology in the classroom, an understanding of whether technology improves student achievement is of critical interest to policymakers.

How can investments in technology affect student achievement? The answer to this question is not necessarily clear. To take some examples, technology investment could facilitate or

[1] According to a 2016 "Technology in the Classroom" survey by Front Row Education, educators say the increase in access to technology devices is driving the increase in technology use in the classroom, with over 50 percent of teachers indicating they have a one-to-one computer-student ratio, up 25 percent over the past year.

enhance existing computer-based instruction. To the extent that computer-based instruction is more effective than traditional classroom-based instruction - because instruction can be tailored to an individual student's needs and can proceed at a pace appropriate to an individual student - teachers might increase the amount of time devoted to computer-based instruction. However, if teachers do increase the amount of time devoted to computer-based instruction and it has smaller returns than traditional instruction (e.g. if technology is used for non-instructional purposes), technology investment could potentially harm student achievement. Technology investment could also narrow or widen some achievement gaps. In addition to the individualized, self-paced instruction that technology provides, data-driven computer-based instruction can help teachers to identify areas of weakness and work individually with struggling students, thereby improving their academic achievement. On the other hand, students at the top end of the achievement distribution may benefit more from the individualized and self-paced instruction, since they can likely obtain more subject-specific information in a computer-based classroom as opposed to a traditional classroom. These factors, in addition to the direct benefits of computer literacy, are behind the decision of schools to invest in educational technology.

A key challenge in analyzing the effect of technology investment on student achievement is that investments in technology are likely to be correlated with unobserved characteristics that affect achievement. In order to isolate the causal effect of technology investment on student achievement, exogenous variation in technology investment is necessary. This paper seeks to provide a causal estimate for the effect of technology investment on student achievement.

In this paper, I analyze the effect of technology investment on student achievement using a large scale technology expansion program in California K-12 public schools which subsidized the purchase of technology hardware and software products. The California Education Technology K-12 Voucher Program (hereafter "the voucher program") provided public schools, in which at least 40 percent of students were categorically eligible to receive free or reduced-price meals through the National School Lunch Program, with \$50.80 per pupil in technology vouchers to reimburse their purchases of qualifying computer technology hardware and software products and services. California is the largest public school system in the US, serving over 6 million students, yet continuously ranks near the bottom for technology resources in K-12 public schools (National Center for Education Statistics, 2013).[2] The primary goal of the voucher program was to improve technology access and assist California K-12 schools with implementing and supporting education technology. Since vouchers were allocated to schools based on their percentage of students who qualify for free and reduced price meals, I employ a regression discontinuity design that allows comparisons across schools that are very similar in school and student characteristics, but different in their access to technology resources. Using data on voucher eligibility and voucher spending, I estimate the causal impact of voucher spending on student achievement.

The results of this study indicate that voucher spending significantly increased academic achievement among elementary and middle school students. I find that schools which were eligible for and spent the technology voucher had significantly greater gains in achievement than schools which were not eligible for the voucher. Since policymakers typically want to implement policies that channel additional funds to where they will be used most effectively, I test for heterogeneity in treatment effects across school and student characteristics, including school type, student socio-economic status, and baseline levels of computer stock. I find the effect of voucher spending on student achievement is largest for middle school students, and is driven by the increase in test scores for low-socioeconomic students. This finding suggests that technology investment can help narrow the income achievement gap. I also find some evidence that schools with high levels of initial computer stock had significant gains in academic achievement. Finally, I explore whether voucher spending affected specific inputs to education production, including the number of instructional computers per student,

[2]In 2004, California was ranked 50th in the nation, behind Nevada, in the number of students per computer, with 5.1 compared to the national average of 3.8. California ranked 47th in the percentage of computers with Internet access, with 83 percent compared to the national average of 87 percent (Sack, 2005).

the number of classrooms with Internet, and the number of computer education courses offered. I find suggestive evidence that voucher spending marginally increased the number of instructional computers per student, and increased the number of computer education courses offered.

This study contributes to the literature on the effect of technology and student achievement in two important ways. First, very few studies have examined the effect of technology investment on student achievement, and the limited evidence that exists is mixed (J. Angrist & Lavy, 2002; Goolsbee & Guryan, 2006; Leuven et al., 2007; Machin et al., 2007).[3] These studies typically use quasi-experimental designs in settings outside the U.S.[4], and exploit governmental policies that provide subsidies or funding for schools to invest in computer technology. These studies typically find that technology investment does increase computer use in school, but has little to no positive and significant effect on most academic outcomes. The one exception to the null findings on student achievement is Machin et al. (2007), who employ an instrumental variables strategy and find that funding for technology in English primary schools significantly increases student achievement. Since our understanding of the impact of technology investments on student achievement is still limited, my study seeks to provide additional insights on this highly debated topic. Moreover, only one of the previous studies has looked at the impact of technology investments in the U.S., and this paper adds to the scant evidence in the U.S. Second, I examine heterogeneity in the effect of additional funding for technology resources on student achievement by school characteristics and student subgroups. Very little attention is given in the prior literature to heterogeneous treatment effects by school characteristics and subgroups, and this paper sheds light on how the effect of technology investment varies across these dimensions.

Unlike most previous studies, the findings of this study show that technology investment

[3] See Escueta et al. (2017) for an experimental evidence review on the effects of home computers, computer-aided instruction, technology-enable behavioral interventions, and online learning.

[4] The one exception is Goolsbee and Guryan (2006), who study the impact of the E-rate program in California.

can significantly improve student achievement. The positive results found here can likely be attributed to schools' effective use of the voucher. That is, since the voucher could be used to purchase hardware or software products, in addition to support and professional development, schools were likely using the voucher in the most effective way to improve student achievement. Moreover, much of the previous literature examined the effect of technology investment in the late 1990s or early 2000s. The educational technology climate has dramatically changed in the past decade. Not only did computers become more commonplace in the classroom, but the Internet and educational software has become increasingly more sophisticated, changing the way technology is used for teaching and learning (Svokos, 2018). Given that technology use in the classroom has significantly evolved, it is not surprising that as technology becomes more integrated into the classroom, it can have positive effects on student achievement.[5]

The remainder of this paper is organized as follows. In Section 2, I provide background on the California Education Technology K-12 Voucher Program. In Section 3, I explain the empirical strategy which underlies the analysis. In Section 4, I describe the data and how these data are used to estimate the effect of the technology voucher program on student achievement. In Section 5, I present the empirical results. Finally, in Section 6, I present a discussion of the main findings and my conclusions.

3.2 CA Education Technology K-12 Voucher Program

The California Education Technology K-12 Voucher Program is a grant opportunity that was made available through a Settlement Agreement between California consumers and Microsoft Corporation in 2003. The settlement stems from a 1999 class-action lawsuit filed

[5]In fact, as of 2011, six states expanded their definition of a "textbook" to include electronic materials, computers, or other portable personal computing devices, and two states require their Department of Education to provide technical training and professional development for teachers in the effective use of online learning resources (NCSL, 2011).

in California that accused Microsoft of overcharging customers for its software. A fund equal to two-thirds of the unclaimed and unredeemed portion of the $1.1 billion settlement was allocated to eligible K-12 public schools in the form of vouchers to reimburse their purchases of qualifying computer technology products and services. The first distribution of $250 million in technology vouchers was available to eligible schools beginning in November 2006. Subsequently, in November 2010, the second round of vouchers totaling $25.5 million was announced, and additional disbursements were announced in March 2014 and January 2015 of $188.4 million and $6.5 million, respectively. Local education agencies (LEAs) had until September 25, 2015, to redeem all vouchers from the first four disbursements.

Eligible schools included all public kindergarten through grade twelve schools, county offices of education, direct-funded charter schools, and State Special Schools in which at least 40 percent of the certified Census Day enrollment met the income eligibility guidelines or were categorically eligible to receive free or reduced-price meals (FRPM) through the National School Lunch Program.[6] Additionally, all public high schools that serve students from eligible public elementary, middle, and junior high schools in California are also eligible for the voucher. That is, if high school #1 has a FRPM percentage equal to 30% and does not qualify for the voucher on its own, but serves students from a qualifying elementary or middle school, then high school #1 will also qualify for the voucher. Given this "feeder provision", nearly all high schools in California qualify for the voucher program, and there is no longer a clear exogenous shock to technology to exploit. Therefore, this analysis will focus on elementary and middle schools, where the 40% cutoff is well-defined.

The amount of each voucher is based on the total enrollment at eligible schools. Fifty percent of the total amount is distributed in the form of General Purpose vouchers (GPV) and fifty percent is distributed in the form of Specific Category Software vouchers (SCV). For the

[6]Schools with enrollment less than 300 were given "special consideration" for the voucher, meaning that some schools with enrollment less than 300 that did not qualify for the voucher still received it. Thus, these schools are dropped from the analysis.

2006 disbursement, eligible schools could receive $50.80 per pupil in technology vouchers - nearly half of the average expenditure per student for instructional technology (California Department of Education, 2011).[7][8] Additionally, in order to receive the education technology voucher funds, county offices of education, school districts, or direct-funded charter schools must have an approved education technology plan.[9] GPVs can be used to purchase specific hardware used with any operating system platform, any non-custom software for that hardware, evaluation tools, IT support services, and professional development services. SCVs can only be utilized to purchase specific categories of software that are published or sold by any software provider.[10] Each voucher is redeemed via a redemption form that each district submits, along with invoices for every purchase, to the Settlement Claims Administrator. Districts are typically reimbursed for their qualifying purchases within 30 days of submitting the redemption form.

3.3 Empirical Methods

I exploit the 40% FRPM eligibility cutoff in the technology voucher program and use a regression discontinuity design to identify the causal effect of the voucher program on student achievement (D. S. Lee & Lemieux, 2010). To understand my estimation strategy, let us first assume that I can measure voucher spending at the school-level. With data at the school-

[7]Average expenditures per student for instructional library, media, and technology, which includes expenses for instructional technology, in California public schools are roughly $120 (California Department of Education, 2011).

[8]An obvious concern is that schools could reallocate funds that would have been used for technology in the absence of the voucher program to other school inputs, like health and counseling services, physical education, etc. To test if this is the case, I would require expenditure data at the school-level on technology-related expenses, and other school expenses. Unfortunately, expenditure data is only available at the district-level, and there is no distinct technology-related expense category. However, anecdotal evidence suggests that schools were not reallocating money initially earmarked for technology, and schools were simply supplementing their initial technology budget with the voucher funds.

[9]The purpose of an educational technology plan is to guide the use of technology, by establishing clear goals and a realistic, comprehensive strategy to improve education through technology (CA Department of Education, 2018).

[10]See Table 3.10 for examples of qualifying GPV and SCV products and services.

level, I would estimate the relationship between voucher spending and student achievement using a "fuzzy" regression discontinuity framework. The first-stage estimates the relationship between voucher program eligibility and voucher spending at the school-level:

$$VoucherSpending_s = \beta_0 + \beta_1 Eligible_s + f(FRPM_s) + \nu_s \tag{3.1}$$

where $VoucherSpending_s$ is an indicator for school s spending their voucher; $Eligible_s$ is an indicator equal to 1 if school s has a FRPM percentage $\geq$ 40%; and $f(FRPM_s)$ is a functional form relating FRPM percentage to $VoucherSpending_s$. The parameter of interest, β_1, captures the effect of the technology voucher program on school-level voucher spending.

Next, I would estimate the second-stage, which defines the relationship between voucher spending and student achievement:

$$Achievement_{gs} = \gamma_0 + \gamma_1 VoucherSpending_s + f(FRPM_s) + \epsilon_{gs} \tag{3.2}$$

where $Achievement_{sg}$ is the change in test scores (i.e. $score_{post} - score_{pre}$) for math and English for grade g in school s. The parameter of interest, γ_1, captures the effect of voucher spending on student achievement. Since voucher spending is not randomly assigned, I would use eligibility status as an instrument for voucher spending, and estimate γ_1 via two-stage least squares.

In practice, however, my measure of voucher spending is at the district-level, not the school-level. To disaggregate the district-level spending data to the school-level, I assume if a district spends nearly 100% of their voucher, then every school that was eligible for the voucher within that district must have spent their voucher. Therefore, I restrict the sample to districts (and the schools within those districts) that spent nearly 100% of their technology voucher. This procedure is described in detail in section 3.4.1. This assumption will generate a β_1=1 in equation 1, thereby reducing the above "fuzzy" regression discontinuity framework to the sharp regression discontinuity framework, where treatment status is fully determined as a function of FRPM (D. S. Lee & Lemieux, 2010). Therefore, I estimate the reduced-form effects for those schools who are eligible and spent the voucher. Specifically, I estimate the following model:

$$Achievement_{gs} = \delta_0 + \delta_1 Eligible_s + f(FRPM_s) + \psi_{gs} \tag{3.3}$$

I augment this equation with controls for school and student-specific characteristics to potentially reduce the residual variation in the outcome, and increase the precision of the estimates. I include a vector X_s of school characteristics, including enrollment, percent of schools located in rural counties, percent female students, percent white, percent black, percent hispanic, and the number of test-taking students. However, the inclusion of X should not affect the estimates of δ_1, since for schools near the 40% eligibility cutoff, the elements of X should be uncorrelated with being on either side of the threshold.

The parameter of interest is δ_1 - the effect of the technology voucher program on student achievement.[11] This parameter is estimated using a local linear regression approach (Imbens & Lemieux, 2008; D. S. Lee & Lemieux, 2010), with a bandwidth of 0.10 on each side of the FRPM percentage threshold.[12]

[11] Equation 3 will also be used to estimate the effect of the technology voucher program on other outcomes of interest, including technology stock and the number of elective courses offered.

[12] I use cross-validation procedures to determine the optimal bandwidth for each outcome. Generally, the CV plots produced an optimal bandwidth around 0.10 for each outcome. Robustness of the results to alternative bandwidth choices are also presented in the figures below.

A key assumption underlying the regression discontinuity procedure is that the conditional expectations of the outcomes (e.g. test scores) with respect to FRPM percentage are smooth through the 40% cut point. If this is the case, I can attribute any discontinuities at this threshold to the causal effect of the technology voucher program. Although I cannot test this assumption directly, an implication is that there should be no discontinuities in predetermined outcomes.[13]

I test for discontinuities in predetermined characteristics of schools, students, and teachers using the restricted sample. I examine if any discontinuities in enrollment size, the percentage of schools located in rural counties, the percentage of students who are female, white, black, and hispanic, the percentage of teachers with a minimum of five years of experience, the percentage of teachers who are female, the pupil-teacher ratio, and baseline test scores exist. Figures 3.1 and 3.2 plot the distribution of these predetermined characteristics for the 2005-2006 school year, and the corresponding local linear regression estimates. In Figure 3.1, there is no evidence of a discontinuity at the 40% FRPM cutoff for any of these outcomes. In Figure 3.2, there is evidence of a small negative discontinuity in the percentage of students who are black. This difference is controlled for, along with all other school and student characteristics, to increase the precision of the estimates.

The other key identification issue for the regression discontinuity method is to what extent schools could have influenced the percentage of students who meet the income eligibility guidelines or are categorically eligible to receive free or reduced-price meals in anticipation of the voucher program. For the 2006 voucher disbursement, school eligibility was based on the FRPM percentage from the preceding school year (i.e. 2005-2006 school year). I test for manipulation at the threshold by performing a density test of the number of schools on

[13] It is also key that no other school funding formulas adopted the 40% eligibility cut point in 2006. Title 1 funds - federal funds used to help meet the educational needs of students - used for school-wide programs designed to upgrade their entire educational programs are allocated to schools enrolling at least 40 percent of children from low-income families. However, schools in California can be granted a waiver if they do not meet the 40% low-income criteria, where schools with 30% of students from low-income families, for example, could receive Title 1 funds.

each side of the 40% eligibility threshold (McCrary, 2008). Figure 3.3 presents the density plot and corresponding local linear regression estimate. Visually there does not appear to be a discontinuity at the 40% FRPM threshold, and the estimated effect is statistically insignificant, suggesting that schools were not manipulating their FRPM percentage in order to qualify for the technology voucher. Therefore, in the absence of manipulation and after controlling for discontinuities in predetermined school and student characteristics, the estimates of δ_1 can be interpreted as the local average treatment effect - the effect of an additional $50.80 per pupil of technology spending for schools that would not have increased their technology spending in the absence of the technology voucher program.

3.4 Data

This study uses data from two sources to analyze the effect of technology investment on student achievement: the Settlement Claims Administrator (SCA) Education Technology K-12 Voucher Program Balance Statements, and the California Department of Education. These datasets are summarized in Figure 3.11. As stated above, in order to estimate the causal effect of technology spending on student achievement using the above "fuzzy" regression discontinuity framework presented with equations 1 and 2, I would require data at the school-level on technology spending made through the technology voucher program. I encounter one main obstacle in obtaining school-level technology spending data made through the voucher program. Below, I describe in detail how I address this challenge, and how I arrive at the sample used to estimate the reduced-form equation presented in equation 3. I then describe the California Department of Education outcome data, which includes school-level student achievement measures.

3.4.1 SCA Data and Sample Construction

The SCA Balance Statements provide information about each K-12 public school district in California that was eligible and received a technology voucher. A district is eligible if at least one school within the district is eligible. Information is available on which eligible districts obtained a voucher, how much each district spent, and when the voucher was spent. Specifically, the Balance Statements include the district identification number, the district name, the amount of the GPV and SCV (since half of each voucher is in the form of GPV and the other half is in the form of SCV). Additionally, the Balance Statement records the beginning balance of each voucher, and each disbursement (there were four disbursements of technology vouchers from 2006 until 2015), and reimbursement. Figure 3.12 displays a screenshot of the SCA Balance Statement website for Anaheim City School District, as an example.

Since the voucher program operates at the school-level, it would be ideal if information on the beginning balance of each voucher, each disbursement, and each voucher reimbursement was also available at the school-level. This would allow me to identify which schools spent their voucher, and when. Because this information is only available at the district-level, I make one main assumption to disaggregate it to the school-level. Since I know each district's beginning balance, and how much of the voucher a district spends and when, I can calculate the percentage of the total voucher spent beginning in 2006. If the district spent nearly 100% of their voucher at any point in time, then every school that was eligible for the voucher within the district must have spent their voucher. Therefore, I restrict the sample to districts that spent at least 90% of their voucher by October 2010.[14] That is, all schools within the districts that did not spend at least 90% of their voucher by October 2010 are dropped

[14]The 90% restriction is used, as opposed to 100%, to increase the number of observations around the 40% FRPM threshold. Estimates for the main outcomes of interest using 80% and 70% are presented in Table 3.12. The estimates become smaller in magnitude and less precise, as expected, since more schools that did not spent their voucher are included in the sample and downward biasing the estimates.

from the sample.[15] Figure 3.13 shows the distribution of percent spent by district. The distribution is bi-model, with a large fraction of districts not spending any of their voucher by October 2010, and a large, but slightly smaller fraction of districts spending all of their voucher by October 2010. Districts to the left of "0.9" are dropped from the sample.[16]

I restrict the sample to voucher spending by October 2010 for two reasons. First, the second round of vouchers were disbursed in November 2010, where schools that did not qualify for the first round of vouchers could be eligible for the second round. Since there was manipulation around the November 2010 voucher distribution 40% FRPM threshold (i.e. a violation of the regression discontinuity identifying assumptions), utilizing this cutoff and including the schools just above and just below the threshold would not yield causal estimates.[17] Second, since the focus of this paper is on the first voucher distribution that occurred in 2006, it is reasonable to assume that it may take time for schools to spend their vouchers, for teachers to adapt to new technology, or obtain the necessary training in order to effectively integrate

[15] To determine if a district spent at least 90% of their voucher, let us take the example of Anaheim City School District's SCA balance statement in Figure 3.12. To begin, I add the values in "GPV Balance" and "SV Balance" for the "Beginning Balance" action to get the beginning balance for each district. Consistent with the rules as described by the SCA for determining the total voucher amount each school receives (i.e. $50.80 per pupil), I can construct beginning balances similar to those in the SCA data using (a) number of students enrolled, and (b) whether or not the school had at least 40% of students qualify for FRPM. These calculations do not match exactly, given the feeder school provision and special consideration for small schools, which could have qualified for the voucher based on other characteristics besides the 40% FRPM cutoff. I then add together all "Payments" for the GPV and SV until October 2010. To determine percent spent, I divide the total payment by beginning balance. For Anaheim City School District, they received vouchers totaling slightly over $1 million (representing only 1% of their total budget), and spent 88% of their voucher. Thus, Anaheim City School District is dropped from the sample. The SCA Balance Statements for all eligible districts are available here: https://edtechk12vp.com/admin/statementAdmin.aspx.

[16] Since policymakers are typically interested in knowing the impact of an implemented policy on the population it targets, I report the intent-to-treat (ITT) estimates for the effect of the Technology Voucher Program on mathematics and English test scores for elementary and middle schools combined. That is, I include all schools in Figure 3.13 - schools within districts that did and did not spend at least 90% of the voucher by 2010. Tables 3.13 through 3.15 report the ITT estimates. The ITT estimates suggest that the technology voucher program does not significantly impact math or English achievement. The ITT estimates are much smaller than the estimates presented in Tables 3.3-3.5, which is expected given the sample restrictions imposed.

[17] Figure 3.14 displays the distribution of the number of schools on each side of the 40% eligibility threshold and corresponding local linear regression estimate for the 2009-2010 school year, since eligibility was based on the prior school year FRPM data. There is a clear discontinuity in the number of schools around the 40% FRPM threshold, suggesting that schools manipulated FRPM counts in order to qualify for the technology voucher program.

the technology into the classroom. Thus, we might not expect to see effects of the voucher program in the first few years after initial disbursement.

Figure 3.4 displays the evolution of the sample creation. Beginning with the district-level SCA data, 809 school districts received the first voucher disbursement. Of the 809 school districts, 180 districts (22.2%) spent at least 90% of their initial voucher distribution by October 2010 and 629 districts did not. Disaggregating this to the school-level, 8,611 schools are in the 809 school districts that received the initial voucher disbursement. There are 1,675 schools within the 180 school districts that spent at least 90% of their voucher by October 2010. Within the 629 districts, I drop 6,933 schools because they did not spend at least 90% their initial voucher disbursement by October 2010. Of the 1,675 schools, 1,026 schools were eligible for the voucher, and 649 were not. The final sample, however, is restricted to elementary and middle schools with enrollment greater than 300 students, since the primary analysis is focused on students in grades 2-8.[18] After eliminating the high schools and schools with less than 300 students enrolled, the final sample contains 1,041 elementary and middle schools, 696 of which were eligible for and spent the voucher, and 345 of which were not eligible.[19]

How do the schools within the districts that were dropped compare to schools within the districts that were not? Table 3.1 provides school-level sample means for school characteristics and student and teacher demographics for the full sample of CA elementary and middle schools, and schools within 10 percentage points of the FRPM cut point that belong to districts that were dropped from the sample, and districts that were kept in the sample, for the 2005-2006 baseline school year. The first panel reports school characteristics, including the number of schools within districts that were and were not dropped from the sample, total

[18] Recall schools with enrollment less than 300 were given "special consideration" for the voucher, meaning that some schools with enrollment less than 300 that did not qualify for the voucher still received it. Thus, these schools are dropped from the analysis.

[19] The total amount of the technology voucher spent by schools by October 2010 amounted to $42,686,360 - roughly 17% of the initial $250 million disbursement.

enrollment, the percentage of schools in rural counties, and FRPM percentage. The number of schools within the districts that spent at least 90% of their voucher is significantly less than the number of schools within the districts that were dropped. This is not surprising since smaller school districts are more likely able to spend nearly all of their voucher within the time period.[20] A slightly higher percentage of schools within districts that spent their voucher are located in rural counties, which again, is not surprising given rural counties have fewer schools per district than urban districts. The top panel also shows that total enrollment and FRPM percentage are nearly identical across the two groups.

Panel 2 of Table 3.1 compares student and teacher demographics. I find no differences in terms of the percentage of students who are female and the percentage of students who are white between the two samples. However, the schools within districts that spent their voucher have significantly less students who are black, and significantly more hispanic students, although the latter is only marginally significant. I also find no stark differences in the percentage of teachers with a minimum of five years of experience (though the difference is marginally significant), the percentage of teachers who are female, or the pupil-teacher ratio. Despite the difference in the number of schools and the percentage of students who are black within the districts that did and did not spend their voucher, panels 1 and 2 of Table 3.1 suggest that the districts that spent most of their voucher by October 2010 are very comparable to the districts that did not in terms of school, student, and teacher characteristics.

The final panel of Table 3.1 compares the main outcomes of interest. Panel 3 of Table 3.1 suggests that schools within districts that did not spend their voucher by October 2010 had significantly lower levels of technology stock (computers per student and the number of classrooms with Internet) compared to schools within districts that did spend their voucher.

[20] Los Angeles County School District has over 1,000 schools, compared to Livermore Valley Joint Unified which has 17. Livermore Valley Joint Unified School district spent nearly 100 percent of their voucher, whereas LA County School District only spent 25 percent. This difference could potentially be due to smaller districts having more coordination among schools, or stronger leadership among decision makers.

English and math test scores do not appear to differ between schools within districts that did and did not spend their voucher. Therefore, these comparisons suggest that non-spending districts had lower levels of technology stock, and may suggest that schools within districts that did spend their voucher potentially had enough existing technology stock to make more efficient use of the technology voucher. That is, given that they had significantly more computers per student and classrooms connected to the Internet, these schools may have been able to utilize their technology voucher more effectively, by for example, investing in more educational software and teacher training.

To summarize, since technology expenditures are not available at the school-level, I use district-level data from the SCA and restrict the estimation sample to only schools within districts that spent at least 90% of their voucher by October 2010. Table 3.1 compares schools within districts that did not spend at least 90% of their technology voucher by October 2010 to schools within districts that did (i.e. the "early spenders") on a number of characteristics. Panels 1 and 2 of Table 3.1 suggests that early spending districts are very comparable to the districts that did not spend their voucher by 2010 in terms of school, student, and teacher characteristics (including test scores). However, early spenders have significantly more technology stock than non-early spenders, suggesting these districts potentially had enough existing technology stock to make proper use of the technology voucher. Moreover, since districts were required to have an approved educational technology plan, early spending districts had already established clear goals and a realistic, comprehensive strategy to improve education through technology, which likely enabled them to use the technology voucher more effectively. Finally, to circle back to the empirical strategy, given the sample restrictions discussed above, the estimates of δ_1 in equation 3 can still be interpreted as local average treatment effects (LATE), but will likely be larger than typical LATE estimates, since the analysis is focused on early spenders who likely have the most to gain (in terms of student achievement) from the voucher program.

3.4.2 CA Department of Education Data

The CA Department of Education (CA DOE) provides data across many years at the district, school, and grade-level on a wide variety of topics, including test results, technology stock, student and teacher demographics, and time base allocated to courses. These data are well suited for this study for several reasons. First, the CA DOE data contains school-level California Standards Test (CST) mean scale scores for mathematics and English by grade-level, which will allow me to estimate the effect of technology spending on student performance for elementary and middle schoolers.[21] Second, school-level CST scores are available by student subgroups, including socio-economic status, which allow me to estimate the heterogeneous effects of technology spending. Third, data are available at the school-level on the number of computers per student, and the number of classrooms with Internet. With these data I am able to determine if technology stock changed as a result of the voucher program. Fourth, the CA DOE data also include staff assignment and course data, including the number of computer education courses offered, and the amount of class time in the school day allocated to mathematics and English. With these data I am able to analyze if the technology voucher affected the number of computer education courses offered or instructional time for mathematics and English - potential mechanisms through which technology investment could affect student performance. Finally, these data contain school-level information on Census Day enrollment and the percentage of students who qualify for free or reduced-price meals.

Table 3.2 provides descriptive statistics on the primary outcomes of interest, including technology stock, CST test scores for English and mathematics, and other outcomes of interest for the estimation sample population and for the subsamples of schools within 10 percentage points of the 40% FRPM threshold. The top panel displays the technology stock statistics

[21] Mean scale scores are used to equate the CST scores from year to year and to determine performance levels. The CST score is the arithmetic mean or average of the scale scores for all students who took each grade- and/or content-specific CST without modifications (California Department of Education, 2011).

for both elementary and middle schools combined for the 2010-2011 school year. Technology stock includes the number of computers per school used for instructional purposes that are less than 48 months old, and the number of classrooms per school with an Internet connection. Since the sample is restricted to schools within districts that spent at least 90% of their voucher by the end of 2010, we would expect to see changes in the technology stock in these schools by this time. The CA DOE requires schools to report specific data, such as the number of students, the number of certified and classified staff, technology stock, and a number of other counts, by October 31 of that school year. Thus, the technology stock counts for the 2010-2011 school year are reported by October 31, 2010. According to Table 3.2, schools within 10 percentage points of the 40% threshold have a similar number of computers per student, and classrooms connected to the Internet.

The following two panels display the average change from 2006 to 2011 in CST test scores for English and mathematics, by elementary and middle schools. The student achievement outcome of interest is: $testscore_{2011} - testscore_{2006}$. For English scores, the change in test scores for elementary schools just above the 40% threshold is lower, while the change in scores for middle schools is significantly higher. For mathematics scores, eligible elementary schools have a slightly smaller change in test scores compared to ineligible schools, whereas eligible middle schools have a larger change in test scores compared to ineligible schools.

The final panel displays the number of computer education courses offered, and the average instructional time spent teaching English, and mathematics for the 2010-2011 school year for middle schools.[22] Schools just above the 40% cut point offer more computer education courses and spend more slightly more instructional time in mathematics and English courses than those schools just below the cut point, but the mean differences are not statistically different.

[22] Elementary schools typically have a set curriculum where class time spent in each common core subject does not differ across schools. Middle schools, however, have more discretion regarding their time allocation to courses.

To summarize, the patterns observed in Table 3.2 suggest that eligible schools just above the 40% threshold have higher middle school English achievement. Next, I present more rigorous evidence of the effect of the voucher program on student achievement and other outcomes using the regression discontinuity empirical strategy described above.

3.5 Estimation Results

The main results presented below are estimated using local linear regressions with a bandwidth of 10 percentage points, unless otherwise noted. All regressions include controls for school characteristics, including enrollment, percent of schools located in rural counties, percent female students, percent white, percent black, percent hispanic, and the number of test-taking students (for student achievement outcomes). I show plots of the distribution of the main outcomes of interest and fitted values from local linear regressions, where the running variable, FRPM, is always normalized to be 0 at the 40% cutoff. I also show plots of the distribution of estimates obtained using varying bandwidths for the main outcomes of interest.

3.5.1 Student Achievement

I begin by presenting the results for the effect of voucher spending on the change in school-level CST mean scale scores from 2006-2011 pooling across elementary and middle schools for mathematics and English. These subjects constitute two of the four main core subjects taught in elementary and middle schools and serve as important indicators of school performance. Table 3.3 reports these estimates. Column 1 of Table 3.3 indicates that voucher spending had no statistically significant effect on mathematics test scores. Figure 3.5 panel (a) plots the distribution of the change in mathematics test scores and corresponding regression estimate from column 1 of Table 3.3. Visually, there is no clear discontinuity in test scores at the cut point. Figure 3.5 panel (c), displays the estimates and corresponding 95% confidence intervals for varying bandwidths, and the effect of voucher spending on mathematics test scores remains statistically insignificant and falls to zero as the bandwidth increases.

Column 2 of Table 3.3 reports the results for English achievement for elementary and middle schools. Voucher spending significantly increases English achievement by roughly one-third of a standard deviation. Figure 3.5 panel (b) plots the distribution of the change in English test scores and corresponding regression estimate from column 2 of Table 3.3. There is visual evidence of a positive discontinuity in English test scores at the cut point. The estimate is also fairly robust to varying bandwidths, with the estimate becoming slightly smaller as the bandwidth increases, as seen in Figure 3.5 panel (d).[23]

Given that voucher spending appears to significantly increase achievement, it is important to see if the effect was experienced equally across different school and student characteristics. In Table 3.4, I disaggregate the pooled results in Table 3.3 by school type.[24] Educational technology tends to be used differently across grades. Elementary school teachers primarily use technology to deliver instruction via interactive whiteboards, laptop computers, or projectors, whereas middle schoolers often use computers to solve math problems, write essays, and conduct research on the Internet (ASCD, 2004; Harper & Milman, 2016; Smerdon et al., 2000).[25] Table 3.4 shows that voucher spending did not significantly impact mathematics

[23]I explore the sensitivity of the results to placebo thresholds (in 10 percentage point intervals) for voucher eligibility. The results are presented in Table 3.16. The results show that there are no discontinuities in test scores at any of the placebo thresholds.

[24]All students in grades 6-8 take the English CST, and all students in grades 6-7 takes the Math CST. Thus, the number of observations is lower for middle school mathematics achievement.

[25]Indeed, educational technology is used more frequently to prepare written text, create visual displays, learn and practice skills, conduct research, and develop presentations in English/language arts, foreign languages, and social science than in mathematics, computer science, and science courses (Gray et al., 2010). Moreover, anecdotal evidence also suggests that educational technology is used more heavily by students in English/language arts classes than in mathematics in middle schools.

test scores across school type, or significantly impact elementary English scores.[26] However, Table 3.4 does show that voucher spending had large positive and statistically significant impacts on middle schoolers' English test scores. The resulting discontinuity figure is displayed in Figure 3.6 panel (d). Across various bandwidths, the estimate is robust, although slightly decreases in magnitude, suggesting that voucher spending significantly increases middle schoolers' English test scores by nearly one-half of a standard deviation.[27]

In Table 3.5, I disaggregate the results in Table 3.4 by socio-economic status. Since schools had full discretion when spending the voucher, schools could have targeted the marginal dollar towards particular student populations. Disadvantaged children typically do not perform as well in school as their more advantaged counterparts (Battle & Lewis, 2002; V. E. Lee & Burkam, 2002). As educator Benjamin S. Bloom observed, "teaching all students in the same way and giving all the same time to learn (i.e. providing little variation in the instruction) typically results in great variation in student learning" (Guskey, 2007). Educational technology can provide some of the necessary variation needed to close achievement gaps seen among students by providing self-paced and differentiated instruction targeted towards the strengths and weaknesses of the student. Moreover, children from disadvantaged households typically have less access to home technology resources than their more advantaged peers (Lynch, 2017). If the majority of technology exposure is happening in the classroom for these students, technology may have differential impacts on their achievement.

Table 3.5 shows that low-SES elementary students have higher gains in mathematics than their high-SES peers.[28] Voucher spending increases low-SES mathematics elementary scores by 0.30 standard deviations, though the estimate is not significantly different from zero. Low-SES middle school students appear to have greater gains in English compared to high-SES

[26]The corresponding discontinuity figures for elementary math and English, and middle school math are presented in Figure 3.6, panels (a)-(c). The corresponding figures with varying bandwidths are presented in Figure 3.15, panels (a)-(c).

[27]See Figure 3.15 panel (d).

[28]The corresponding discontinuity figures and varying bandwidth figures are presented in Figures 3.16 and 3.17, respectively.

middle school students, and appear to be driving the positive impact of voucher spending on middle school English achievement. There appears to be no significant difference between low and high-SES elementary students' English test scores, but a clear difference in middle school mathematics test scores between the two groups. Voucher spending increases low-SES middle school mathematics scores by nearly one-third of a standard deviation, and decreases high-SES scores by nearly one-half of a standard deviation, although both estimates are statistically insignificant.[29]

It may also be the case that schools not only targeted the marginal dollar of the voucher to particular student populations, but also according to their initial technology stock. Schools with high initial levels of computer hardware may allocate their voucher funds differently than schools with lower initial levels of computer hardware (by, for example, using the voucher to purchase computer software instead of additional computer hardware), which may have differential impacts on student achievement. To test this possibility, I interact voucher eligibility with a dummy variable equal to 1 if a school's initial computer stock (i.e. number of computers per student) is in the top half of the initial computer stock distribution for the sample. Table 3.6 reports the results from this specification. These results suggest that being in the top half of initial computer stock distribution only significantly impacts elementary English test scores. Across the other school type and subject, initial computer stock does not appear to differently affect student achievement.[30]

[29] As a falsification test, I report the results for the effect of the technology voucher program on student achievement by grade and SES for schools within districts that spent zero percent of their technology voucher from 2006-2010. The estimates are presented in Table 3.17. There is no evidence of the technology voucher program significantly impacting test scores for schools within districts that did not spend their voucher.

[30] Similar specifications were also examined using the baseline pupil-teacher ratio and baseline school district size, and no differential impact on student achievement was detected. These results are available from the author upon request.

3.5.2 Mechanisms

Given the positive effects of voucher spending on student achievement, a policy-relevant question is whether changes to observed inputs can be credited as the likely source of the achievement effects. To determine if voucher spending affected observed inputs in education production, I focus on the number of computers per student used for instructional purposes, the number of classrooms with Internet, the number of computer education and other electives courses offered, and instructional time spent in the mathematics and English courses.

I begin with the analysis of the effects of voucher spending on school-level technology stock, including the number of computers per student used for instructional purposes that are less than 48 months old, and the number of classrooms per school with Internet. Since half of the technology voucher was disbursed in the form of GPVs for specific hardware purchases, including computers, routers, wireless networks, and access points, we may expect voucher receipt to increase these two types of technology stock. Table 3.7 presents the regression discontinuity estimates of the effect of voucher spending on the number of computers per student, and the logged number of classrooms per school with Internet, pooling elementary and middle schools. For the 2010-2011 school year, column 1 reports the estimates for the number of computers per student. The voucher program appears to increase the number of computers per student by nearly 28%, but the estimate lacks precision. Figure 3.7 panel (a) plots the distribution of the number of computers per student and corresponding regression estimate from column 1 of Table 3.7. Visually, a clear change in the number of computers per student is not evident for schools within the 10 percentage point bandwidth. Figure 3.7 panel (c), displays the estimates and corresponding 95% confidence intervals for varying bandwidths. There is evidence that voucher spending does significantly impact the number of computers per student within a tight bandwidth (between 5 and 10 percentage points). However, as the bandwidth increases, the estimate falls to zero and becomes statistically insignificant.

Column 2 of Table 3.7 reports the estimate for the logged number of classrooms per school with Internet. There is no evidence that the voucher program impacted the number of classrooms per school with Internet. Figure 3.7 panel (b) plots the distribution of the logged number of classrooms per school with Internet and corresponding regression estimate from column 2 of Table 3.7. There is no clear discontinuity in the figure and as the bandwidth around the 40% threshold increases (Figure 3.7 panel (d)), the estimate falls to zero.

I next report the results for the effect of voucher spending on technology stock by elementary and middle schools separately in Table 3.8. Elementary and middle schools may have different technology needs, and the aggregate results may mask potential heterogeneity. Similar to Table 3.7, voucher spending does not appear to significantly impact the number of computers per student for elementary or middle schools, or across any bandwidth. However, voucher spending does significantly increase the number of classrooms with Internet in middle schools by 68 percent, or 23 classrooms, but the effect nearly falls to zero and becomes statistically insignificant as the bandwidth increases.

Given the achievement results, it is surprising that voucher spending had no robust effect on school-level technology stock. However, the use of GPV technology vouchers was not only limited to hardware purchases, they could also be used for any non-custom software, evaluation tools, IT support services, and professional development services. It may be the case that schools were investing in different types of technology resources to fit their specific needs, whether it be more technology hardware, software, or a combination of the two, and this is not detectable in an aggregate measure of hardware stock. Unfortunately, the SCA does not provide data on all products and services approved and reimbursed at the district or school-level, which could be used to determine exactly how schools were spending their voucher.

I next explore whether voucher spending affected the composition of computer education courses and other electives courses offered. Computer education courses may be an important input in the education production function, since the main purpose of computer education courses, generally taught in middle school, is to prepare students for the computer skills they will need to succeed both inside and outside of the classroom. Students in grades 6-8 are typically taught computer literacy and digital citizenship skills (e.g. common computer terminology, navigation of the Internet, proper citations, on-line safety, etc), and Microsoft Office Suite, including basic use of Word, Excel, PowerPoint, and Publisher.[31] Gaining an understanding of essential computer skills could lead to students becoming more productive and efficient in a computer-based classroom, which could lead to an improvement in academic achievement.

To determine if voucher spending impacts the number of computer education courses offered, I use school year 2010-2011 school-level data from the CA Department of Education on staff assignment and course data. These data include course information, like number of courses offered, and course enrollment, as well as staff assignment information, like type of staff and time base, for each type of course offered in a school. In addition to the common four core courses taught in elementary and middle school (e.g., math, English, science, and social studies or history), schools also offer elective courses. Elective courses allow students to choose classes of more interest and to customize their educational experience. While elementary schools offer electives to supplement the academic schedule, the freedom to choose specific elective courses that complement the students' own interests is typically not available until middle school. Typical elective courses offered in middle schools include art, foreign language, music (including band and chorus), computer education, and home economics.[32] Voucher spending could impact the types of electives offered. For example,

[31] See Rockaway Township Public Schools Computer Literacy Grades 6-8 Philosophy for an example of a Computer Education curriculum: https://www.rocktwp.org/domain/17.

[32] Course information disaggregated by each elective offered is not available for all schools. That is, some schools report the distinct number of computer education, art, and music classes offered, while others aggregate the number of elective courses offered into an "other" category. Therefore, this analysis is focused only on the schools that report the number of distinct elective courses offered. This limits the number of schools within 10 percentage points of the 40% FRPM threshold considerably, thus this analysis uses a bandwidth of 20 percentage points around the cut point to increase the precision of the estimates.

if schools invest in more innovative and interactive computer education software, they may be more likely to increase the number of and type of computer education courses offered. That said, if schools increase the number of computer education courses offered, in order to accommodate the increase, schools likely either decrease the number of courses provided in other elective subjects, or increase resources allocated to computer education instruction.

Table 3.9 reports the results for the effect of technology voucher spending on the number of computer education courses, and "other" elective courses offered. "Other" elective courses offered includes art, drama, dance, music, foreign language, and home economics courses. According to Table 3.9, voucher spending appears to significantly increase the number of computer education courses offered by nearly 2.5 courses. However, voucher spending does not appear to significantly decrease the number of other elective courses being offered. This evidence suggests that since the composition of elective courses offered was not changing, schools may have increased the amount of resources (e.g. teachers, classrooms, course materials, etc.) allocated to computer education instruction.[33] Figure 3.8 panels (a) and (b) plot the distribution of the number of computer education and other elective courses offered, and corresponding regression estimates from Table 3.9. Visually, in Figure 3.8 panel (a), there appears to be a discontinuity in the number of computer education courses offered.[34]

Next, I use these data to explore whether technology spending affects instructional time in mathematics and English to determine if the positive achievement effects presented above are driven by an increase in class time for these courses. Table 3.10 presents the results for the effect of technology spending on instructional time in mathematics and English. The estimates show that technology investment does not significantly impact instructional time in mathematics and English courses, suggesting that the positive effects found in previous

[33] If schools were investing in more professional development, it may be the case that more teachers may have become qualified to teach computer education courses.

[34] Across varying bandwidths, the estimate nearly falls to zero and is statistically insignificant (Figure 3.18, panel (a)).

tables are not driven by an increase in instructional time for these courses.[35]

Finally, it may be the case that the voucher program affected the teacher and student composition of eligible schools, which could be driving the results presented in the tables above. That is, if a high-technology school is more attractive for new teachers and students, we may see an increase in the number of, and potential quality of, teachers in spending schools, and a potential change in the student composition, if more involved parents choose to locate in school districts with high-tech schools. In Table 3.11, I explore these hypotheses and estimate the effect of voucher spending on a number of teacher and student characteristics: the pupil-teacher ratio, the percentage of teachers with a minimum of five years of experience, the percentage of teachers who are female, total student enrollment, and the percentage of students who are female, white, black, and hispanic. The estimates in Table 3.11 show that neither the teacher nor student composition in schools is changing as a result of voucher spending, suggesting that the positive effects found above are not driven by teacher and student characteristics.

3.6 Discussion and Conclusion

Given the substantial increase in funds targeted towards increasing technology resources in the classroom, an understanding of whether technology investment affects student achievement is of compelling importance. In this paper, I seek to provide causal estimates of the impact of technology investment on student achievement. I exploit an exogenous change in technology investment in California K-12 public schools generated by the California Education Technology K-12 Voucher Program. The voucher program provided eligible schools, in which at least 40% of students qualified for free or reduced-price meals, with general purpose and specific category software vouchers for purchasing qualifying hardware and software

[35]The corresponding regression discontinuity graphs are presented in Figure 3.9, and corresponding varying bandwidth figures are presented in Figure 3.19).

products.

Using a regression discontinuity design and focusing on the schools that spent nearly all of their technology voucher, I show that voucher spending had positive impacts on mathematics and English test scores for elementary and middle school students. Specifically, I find robust evidence that voucher spending significantly increased English test scores, especially among middle schoolers and low-socioeconomic students by one-third to nearly one-half of a standard deviation. I also explore whether measurable inputs in education production were affected as a result of voucher spending. I find suggestive evidence that voucher spending increased the number of computers per student used for instructional purposes, and increased the number of computer education courses offered. Taken together, the results of this study suggest that technology investment effectively improves student achievement.

It is important to reiterate that the large findings in this study can be interpreted as upper bounds of the effect of the technology voucher program on student achievement. Since this analysis focuses on "early spenders" that spent nearly all the voucher within the first four years of disbursement, the results of this study may not be generalizable to all schools, since these schools likely differ on unobservable characteristics which may affect the productivity of the voucher funds. Nevertheless, the findings presented here can still be used to assess the impact of technology investment on student achievement.

Though the findings of this paper contribute to our understanding of whether technology investment improves student achievement, they leave open the question of to what degree additional funds for technology affect overall spending on technology. Due to data limitations, this study is unable to determine whether the technology voucher program crowded in or crowded out existing technology expenditures. That is, it may be the case that schools were reallocating funds initially earmarked for technology to other productive inputs, such as teacher salaries, or traditional instructional materials. However, since eligible schools were required to have an approved education technology plan which clearly identified how technology was to be used to improve student performance, and how schools were going to use existing funds to achieve this goal, it is unlikely that the voucher program crowded out existing technology funds.[36] Future work analyzing the effect of voucher programs should examine if, and to what extent, voucher funds crowd in or crowd out existing technology expenditures.

This paper also provides one important lesson for understanding the impact of technology on student achievement. The findings of this study illustrate that technology investment can play a sizable role in reducing the income achievement gap. However, this paper cannot speak to the direct mechanism behind the large effects on achievement seen for low-socioeconomic students. That is, this study is unable to determine if self-paced, individualized instruction was the determining factor, or if it was teachers' specialized use of technology resources, for example, that improved achievement. Future research is needed to determine how exactly technology investments can narrow the income achievement gap.

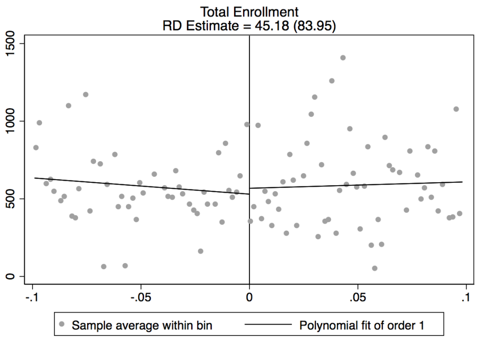
The results of this study inform the ongoing debate between those who encourage educational technology in schools and those who argue there is not enough evidence to support the costly investments. The findings show that technology is an important input in the educational production process, and can significantly improve elementary and middle school student achievement, especially among low-socioeconomic students. Moreover, the costs of technology investments are likely lower than those associated with other interventions that improve student achievement, like reductions in class size (J. D. Angrist & Lavy, 1999; Jepsen & Rivkin, 2009; Krueger, 1999). Given the enhanced efforts by education officials in the U.S. to close achievement gaps and integrate technology into the classroom, the results of this study should be acknowledged when deciding whether to implement technology in

[36]Indeed, anecdotal evidence also supports this hypothesis, with many qualifying school districts indicating that funds initially earmarked for technology were not reallocated to other school inputs. Moreover, the costs associated with maintaining technology in the classroom (e.g. replacing computers every 3-5 years, and renewing software subscriptions) are fairly substantial, suggesting again that schools are likely not reallocating funds away from technology.

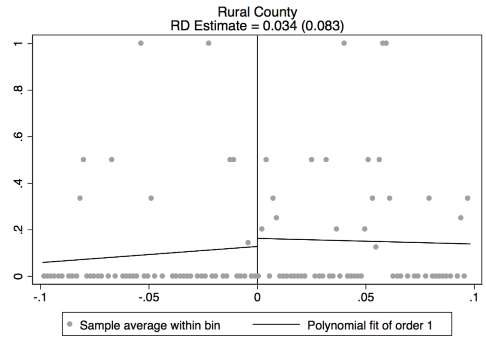
school classrooms.

3.7 Figures

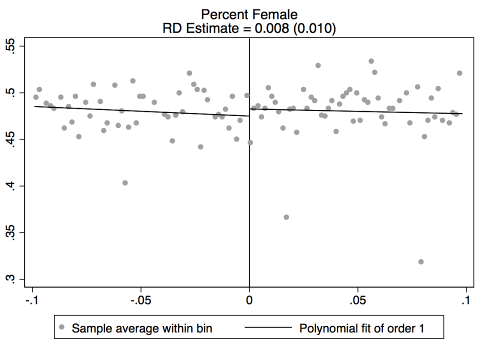
Figure 3.1: Balance in Predetermined Characteristics 1



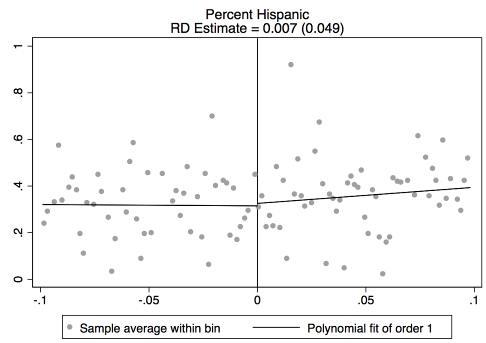
(a)



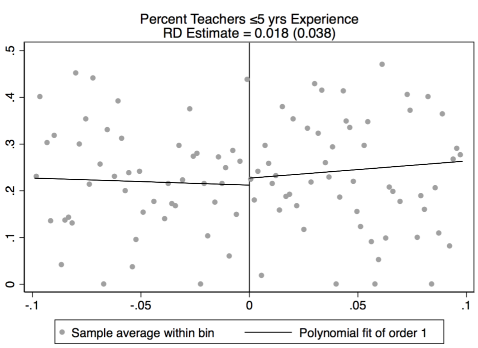
(b)



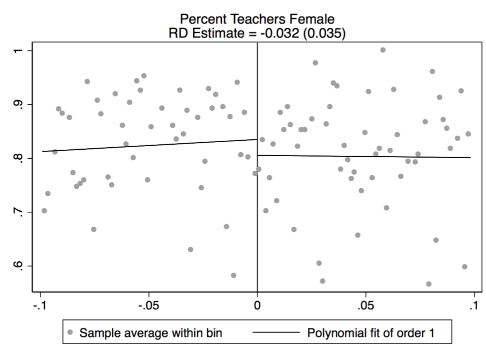
(c)



(d)



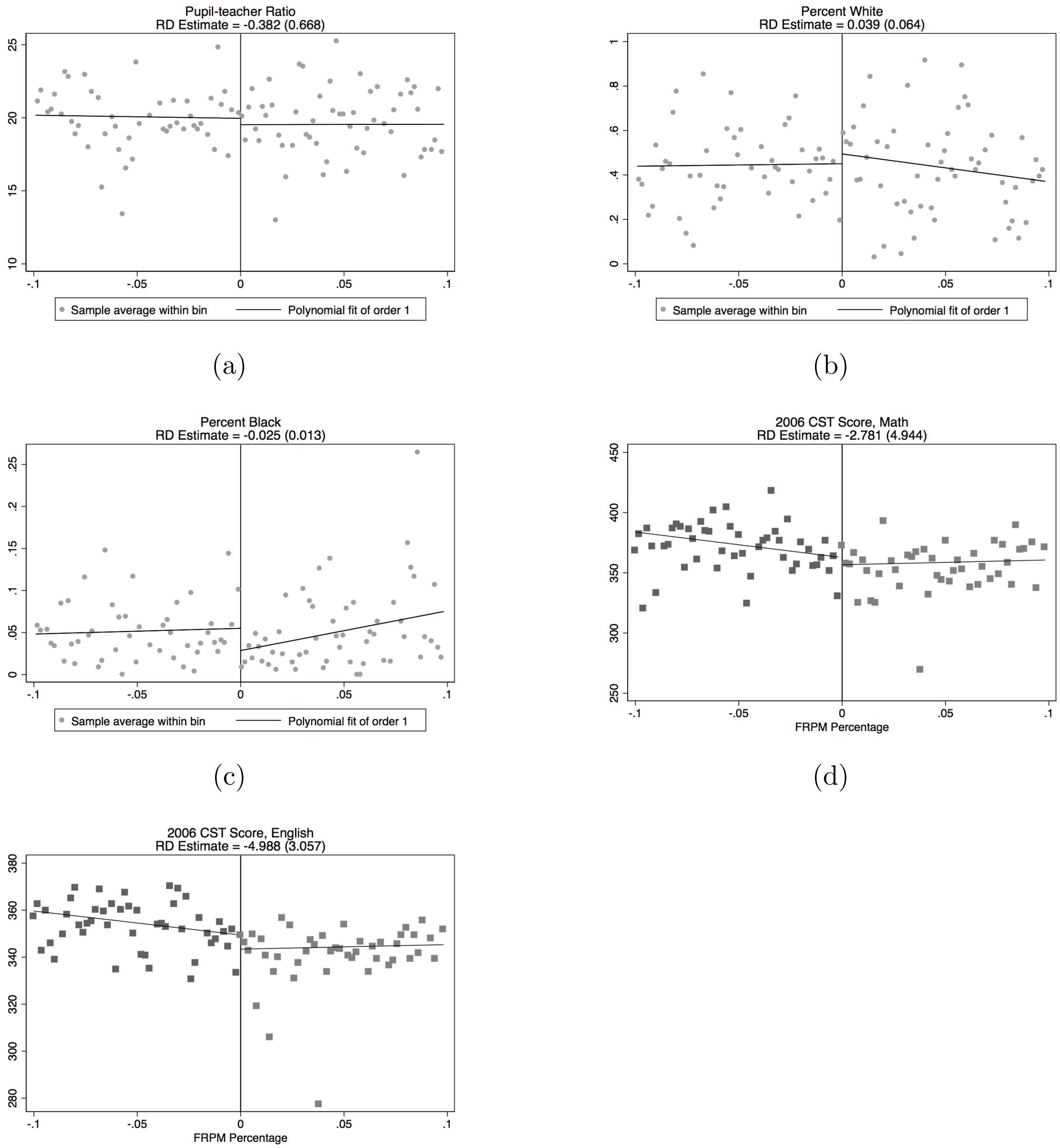
(e)



(f)

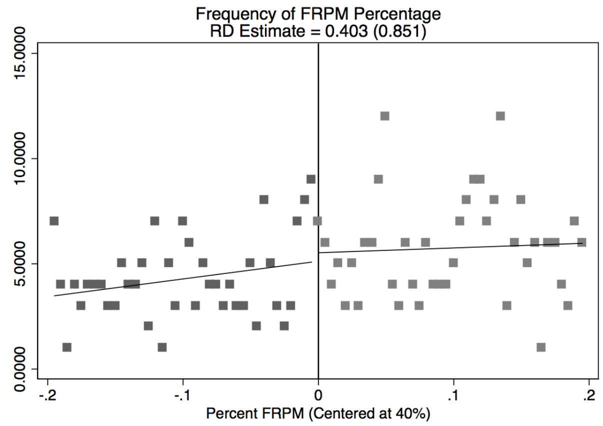
Notes: Balance in covariates around the 40% FRPM threshold: (a) School-level enrollment, (b) Percentage of schools in a rural county, (c) Percentage of students female, (d) Percentage of students Hispanic, (e) Percentage of Teachers with a minimum of 5 years of teaching experience, (f) Percentage of teachers who are female.

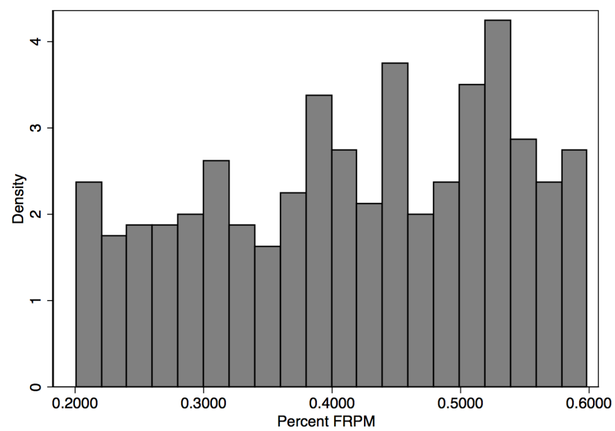
Figure 3.2: Balance in Predetermined Characteristics 2



Notes: Balance in covariates around the 40% FRPM threshold: (a) Pupil-teacher ratio (b) Percentage of students White, (c) Percentage of students Black, (d) Baseline Math CST Scores, (e) Baseline English CST Scores

Figure 3.3: Density test





Notes: Density test of the number of schools on each side of the 40% eligibility threshold for the 2005-2006 school year.

Figure 3.4: Evolution of the Sample

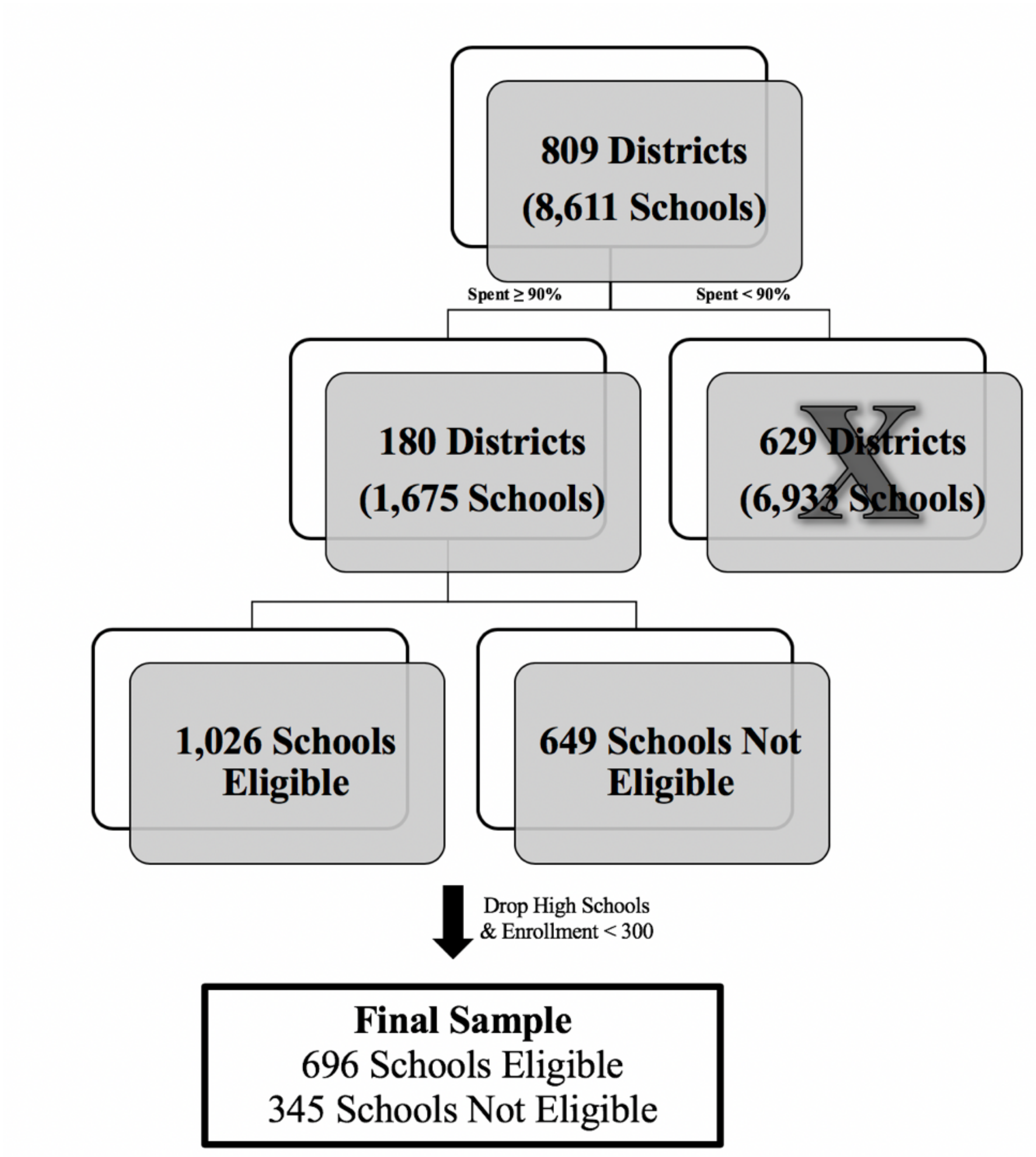
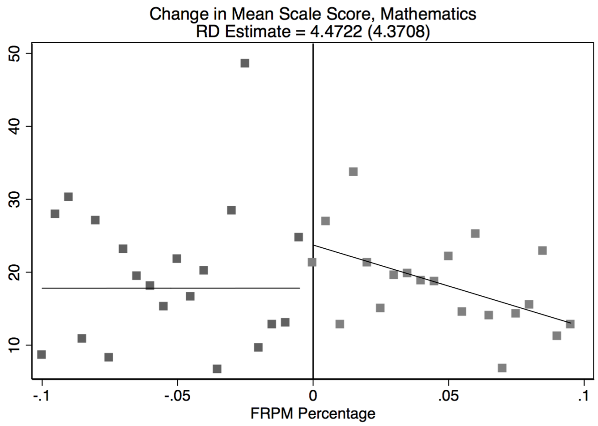
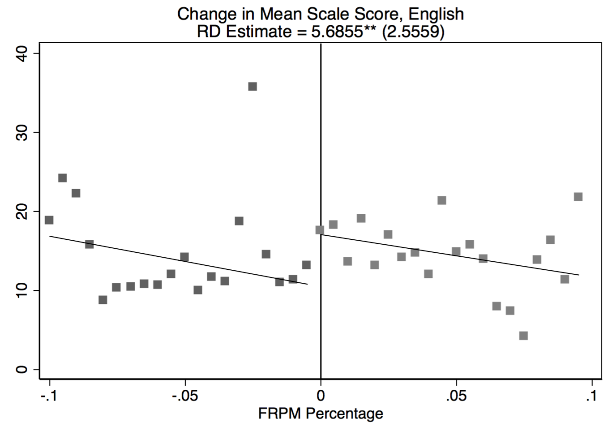


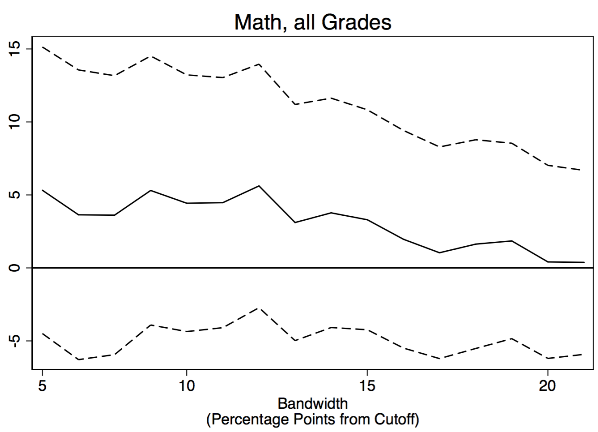
Figure 3.5: Change in Mean Scale Scores from 2006 to 2011, Mathematics and English, all Grades



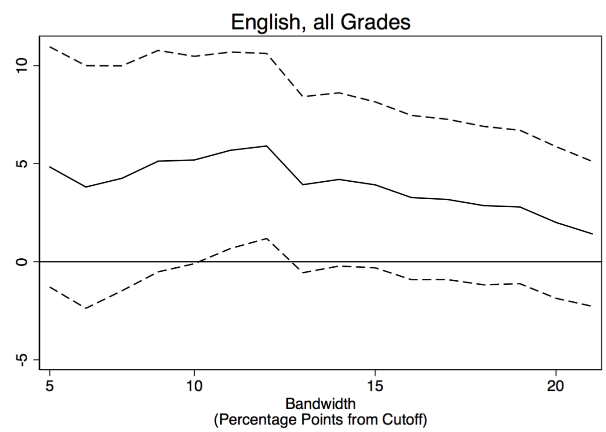
(a)



(b)



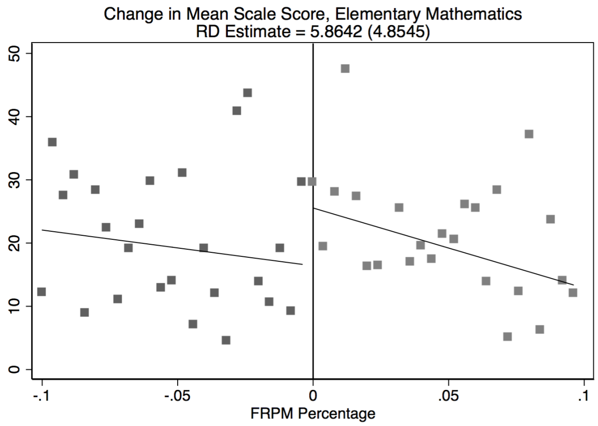
(c)



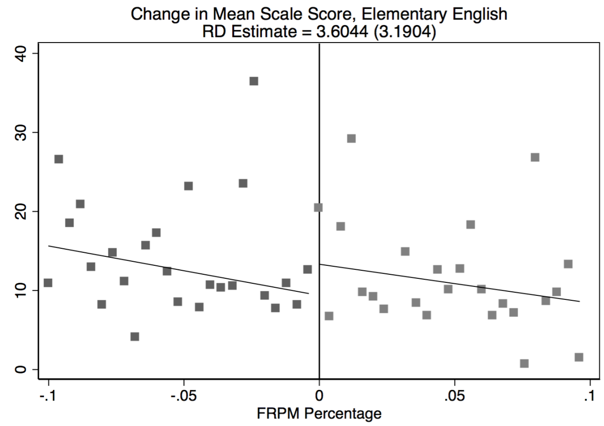
(d)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle schools combined. Free and reduced price meal (FRPM) percentage is relative to the 40% cutoff (i.e. FRPM equal to 0 corresponds to a FRPM percentage of 40).

Figure 3.6: Change in Mean Scale Scores from 2006 to 2011, Mathematics and English, by Elementary and Middle



(a)



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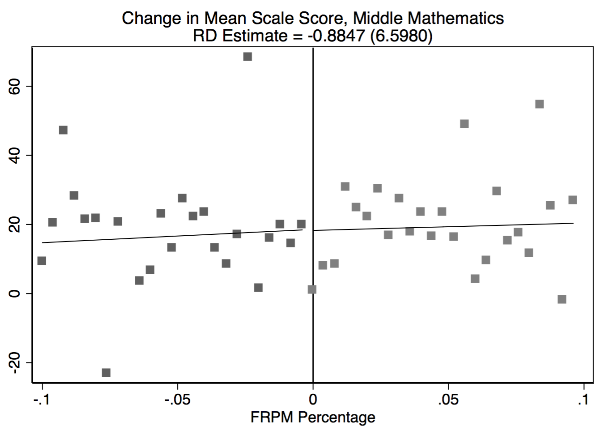
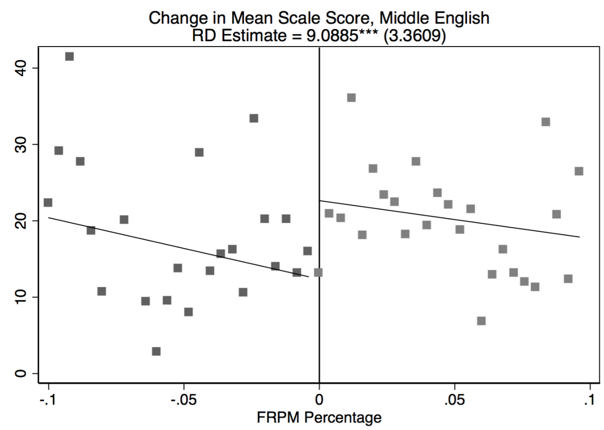


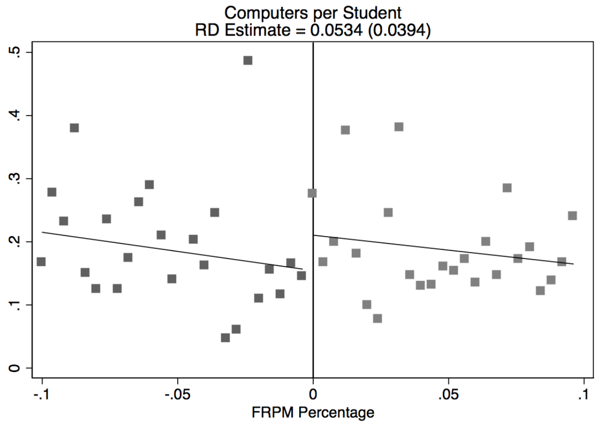
(c)



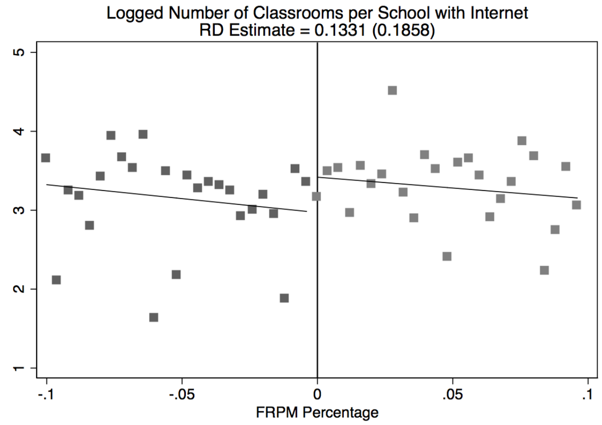
(d)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle schools. Free and reduced price meal (FRPM) percentage is relative to the 40% cutoff (i.e. FRPM equal to 0 corresponds to a FRPM percentage of 40).

Figure 3.7: Technology Stock, Elementary and Middle Schools



(a)



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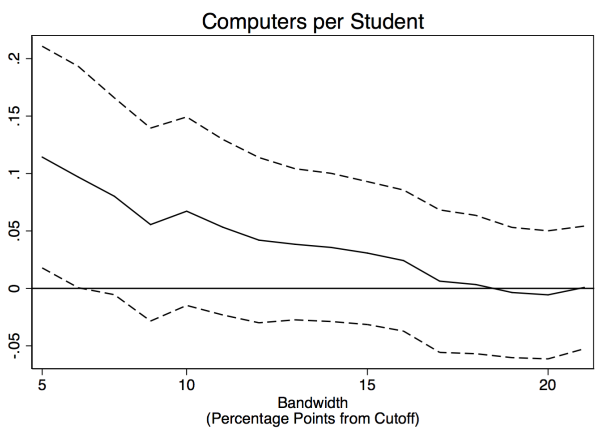

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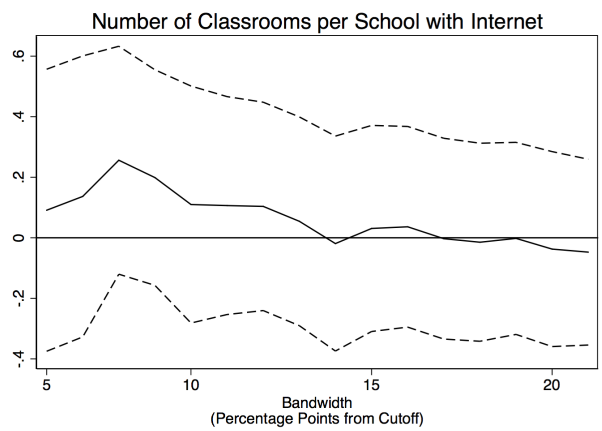
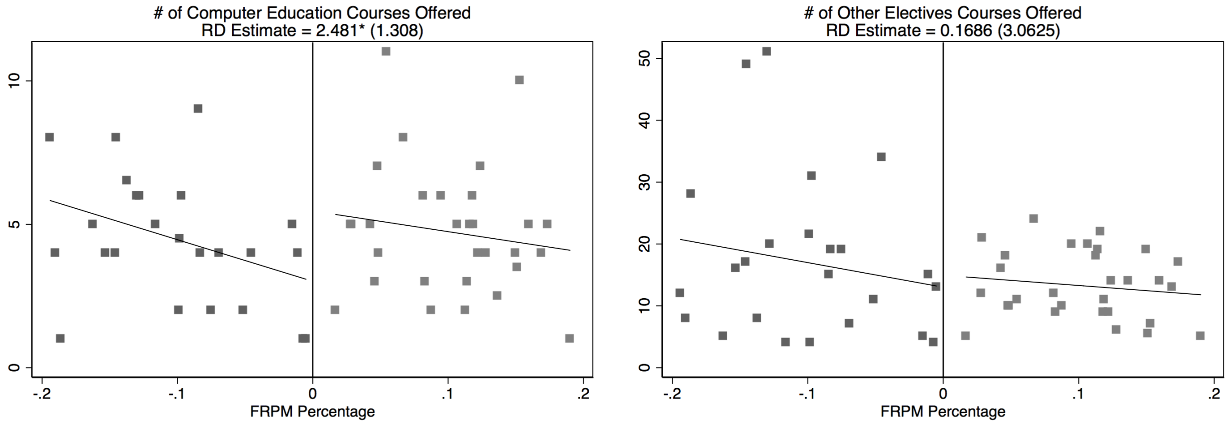


(d)

Notes: The dependent variables are the number of computers per student and the log of the number of classrooms per school with Internet. Free and reduced price meal (FRPM) percentage is relative to the 40% cutoff (i.e. FRPM equal to 0 corresponds to a FRPM percentage of 40).

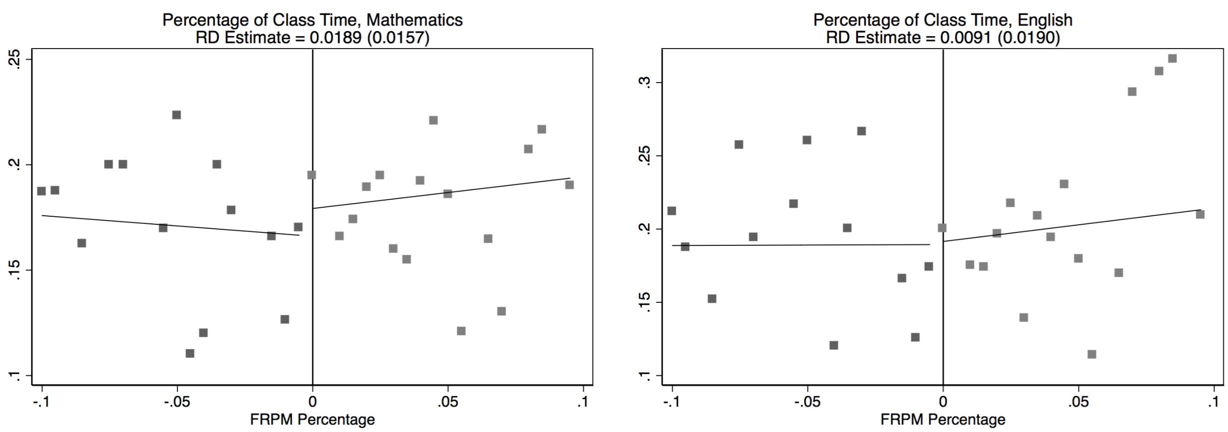
Figure 3.8: Courses Offered: Computer Education and "Other" Elective Courses



(a) (b)

Notes: The dependent variables are the number of computer education courses offered and the number of "other" elective courses offered for middle schoolers. "Other" elective courses includes art, dance, drama, foreign language, home economics, and music. Free and reduced price meal (FRPM) percentage is relative to the 40% cutoff.

Figure 3.9: Percentage of Instructional Time, Mathematics and English



(a) (b)

Notes: The dependent variables are the percentage of time spent teaching mathematics, and English for middle schoolers. Free and reduced price meal (FRPM) percentage is relative to the 40% cutoff.

3.8 Tables

Table 3.1: Sample Means for Population and School Districts, 2005-2006 SY

	Population	**Omitted Districts** (±10)	**Included Districts** (±10)	***p*-value**
	(1)	(2)	(3)	(4)
Panel I: School Characteristics				
Number of Schools	106.169	66.539	22.672	0.000
	(219.315)	(161.183)	(18.134)	
Enrollment	701.927	668.817	681.056	0.602
	(320.163)	(288.599)	(299.831)	
Rural	0.020	0.030	0.056	0.086
	(0.139)	(0.171)	(0.230)	
Percent FRPM	0.582	0.402	0.403	0.851
	(0.284)	(0.059)	(0.056)	
Observations	5,355	701	198	
Panel II: Student and Teacher Demographics				
Percent Female	0.486	0.486	0.484	0.226
	(0.023)	(0.025)	(0.021)	
Percent White	0.270	0.405	0.385	0.215
	(0.245)	(0.192)	(0.208)	
Percent Black	0.077	0.075	0.057	0.006
	(0.106)	(0.084)	(0.056)	
Percent Hispanic	0.514	0.351	0.375	0.092
	(0.286)	(0.167)	(0.169)	
Percent Teachers $\leq$ 5 yrs Exp.	0.257	0.223	0.242	0.093
	(0.144)	(0.136)	(0.137)	
Percent Teachers Female	0.817	0.826	0.813	0.190
	(0.124)	(0.119)	(0.123)	
Pupil-teacher Ratio	20.27	20.51	20.28	0.325
	(2.694)	(2.975)	(2.460)	
Observations	5,106	675	187	
Panel III: Outcomes				
Computers per Student	0.210	0.188	0.223	0.000
	(0.101)	(0.083)	(0.116)	
# Classrooms w/ Internet	35.74	32.07	38.28	0.002
	(25.81)	(17.79)	(42.43)	
Observations	5,351	701	198	
English CST Score	338.8	348.6	347.8	0.498
	(25.16)	(15.31)	(13.07)	
Observations	5,450	724	198	
Math CST Score	355.2	363.2	363.2	0.989
	(33.17)	(25.19)	(23.37)	
Observations	5,433	719	198	

Notes: Means and standard deviations (in parentheses) are presented for elementary and middle schools only. Schools with enrollment of less than 300 students are omitted. CST scores range from 150 to 600. Observations are the number of schools.

Table 3.2: Descriptive Statistics of Analysis Variables

	Population	**FRPM - 10pp**	**FRPM + 10pp**	***p*-value**
	(1)	(2)	(3)	(4)
Panel I: Technology Stock, 2010-2011 SY				
# of Computers per Student	0.185	0.183	0.187	0.857
	(0.131)	(0.124)	(0.151)	
Observations	960	86	95	
# of Classrooms with Internet	33.09	30.07	36.09	0.131
	(22.64)	(15.51)	(35.07)	
Observations	1,010	92	100	
Panel II: Δ CST English Scores, 2006-2011				
Grades 2-5	15.33	12.63	10.89	0.211
	(16.21)	(17.06)	(16.34)	
Observations	3,147	304	277	
Grades 6-8	19.36	17.03	20.21	0.058
	(13.05)	(11.78)	(13.88)	
Observations	1,047	108	137	
Panel III: Δ CST Mathematics Scores, 2006-2011				
Grades 2-5	24.58	20.01	19.74	0.909
	(29.36)	(29.73)	(27.80)	
Observations	3,147	304	277	
Grades 6-7	20.03	15.81	18.38	0.410
	(21.04)	(20.40)	(20.73)	
Observations	778	81	95	
Panel IV: Other Outcomes, 2010-2011 SY				
# Computer Education Courses	4.310	4.286	4.588	0.616
	(2.637)	(2.447)	(2.271)	
Observations	132	28	34	
Instructional time - English	0.197	0.195	0.204	0.382
	(0.074)	(0.056)	(0.067)	
Observations	620	63	92	
Instructional time - Mathematics	0.184	0.177	0.186	0.223
	(0.061)	(0.040)	(0.048)	
Observations	623	63	93	

Notes: Means and standard deviations (in parentheses) are presented for elementary and middle schools only. CST scores range from 150 to 600. Schools with enrollment of less than 300 students are omitted. CST mathematics test scores are only for grades 6-7, since students in grade 8 take the pre-algebra CST exam. Means for number of computer education courses are presented within 20 percentage points of the 40% FRPM cut point. Observations are the number of schools, with the exception of English and Math CST scores. Observations for CST Scores are at the school-grade level.

Table 3.3: Effect of CA K-12 Technology Voucher Program on Test Scores

	Mathematics	**English**
	(1)	(2)
Voucher	4.4722	5.6855**
	(4.3708)	(2.5559)
Observations	715	779
Outcome Means for Control	18.16	13.16
	(28.03)	(15.72)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011, for elementary and middle school grades combined. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Effect of CA K-12 Technology Voucher Program on Test Scores, by School Type

	Math, Elementary	**English, Elementary**	**Math, Middle**	**English, Middle**
	(1)	(2)	(3)	(4)
Voucher	5.8642	3.6044	-0.8847	9.0885***
	(4.8545)	(3.1904)	(6.5980)	(3.3609)
Observations	549	549	166	230
Outcome Means for Control	18.94	11.95	15.23	16.63
	(29.73)	(16.83)	(20.28)	(11.34)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Effect of CA K-12 Technology Voucher Program on Test Scores, by School Type & SES

	Math, Elementary	**English, Elementary**	**Math, Middle**	**English, Middle**
	(1)	(2)	(3)	(4)
Panel I: Low-SES				
Voucher	9.4545	4.5571	8.1526	11.0650***
	(5.7879)	(3.8684)	(6.6900)	(3.6545)
Observations	549	549	162	225
Outcome Means for Control	22.73	16.81	19.23	19.82
	(32.98)	(20.98)	(20.49)	(13.17)
Panel II: High-SES				
Voucher	3.142	3.8457	-9.9044	5.2183
	(4.8329)	(3.1305)	(7.3264)	(3.8915)
Observations	537	536	164	227
Outcome Means for Control	21.32	12.46	18.45	18.32
	(31.91)	(18.17)	(22.71)	(13.67)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades, by SES. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Effect of CA K-12 Technology Voucher Program on Test Scores, by Initial Computer Stock

	Math, Elementary	**English, Elementary**	**Math, Middle**	**English, Middle**
	(1)	(2)	(3)	(4)
Voucher	3.2509	0.1451	-3.093	8.4674**
	(6.1552)	(3.9295)	(6.5603)	(3.2685)
Top Half of Initial Comp	-3.4059	-3.5842	-0.306	0.69
	(3.9101)	(2.2338)	(4.9376)	(2.6955)
Voucher*Top Half of Initial Comp	6.1951	6.6285**	7.8556	2.3354
	(5.3542)	(3.3181)	(7.0864)	(4.1379)
Observations	549	549	166	230
Outcome Means for Control	18.94	11.95	15.23	16.63
	(29.73)	(16.83)	(20.28)	(11.34)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Effect of CA K-12 Technology Voucher Program on Technology Stock

	# of Computers per Student	**# of Classrooms with Internet**
	(1)	(2)
Voucher	0.0533	0.1064
	(0.0390)	(0.1836)
Observations	182	184
Outcome Means for Control	0.181	30.09
	(0.124)	(15.49)

Notes: The dependent variable is the number of computers per student or the log of the number of classrooms per school with Internet, for elementary and middle schools combined. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, and percent hispanic. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Effect of CA K-12 Technology Voucher Program on Technology Stock, by School Type

	Comp/St - Elementary	**Internet - Elementary**	**Comp/St - Middle**	**Internet - Middle**
	(1)	(2)	(3)	(4)
Voucher	0.0466	-0.2726	0.0307	0.6802**
	(0.0505)	(0.2303)	(0.0540)	(0.3267)
Observations	118	119	64	65
Outcome Means for Control	0.190	28.45	0.158	34.16
	(0.136)	(15.92)	(0.086)	(13.85)

Notes: The dependent variable is the number of computers per student or the log of the number of classrooms per school with Internet, by elementary and middle schools. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, percent female, rural, percent white, percent black, and percent hispanic. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Effect of CA K-12 Technology Voucher Program on Number of Courses Offered

	Computer Education	**Other Electives**
	(1)	(2)
Voucher	2.481*	0.1686
	(1.308)	(3.0625)
Observations	59	59
Outcome Means for Control	4.4074	16.667
	(2.4061)	(12.845)

Notes: The dependent variables are the number of courses offered for computer education, art, drama, dance, music, foreign language, and home economics courses in middle schools. Observations are the number of schools. RDD estimates are reported using LLR with a bandwidth of +/- 0.2 around the 40% FRPM cutoff. Regressions include controls for percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, and percent hispanic. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Effect of CA K-12 Technology Voucher Program on Instructional Time

	Mathematics	**English**
	(1)	(2)
Voucher	0.0189	0.0091
	(0.0157)	(0.0190)
Observations	302	300
Outcome Means for Control	0.174	0.188
	(0.040)	(0.054)

Notes: The dependent variables are the percentage of class time spent instructing mathematics, and English for middle schoolers in grades 6-8. Observations are at the school-by-grade level. RDD estimates are reported using LLR with a bandwidth of +/- 0.2 around the 40% FRPM cutoff. Regressions include controls for percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.11: Effect of CA K-12 Technology Voucher Program on Teacher and Student Demographics

	PT Ratio	**Teachers $\leq$ 5yrs Exp.**	**Pct. Teachers Female**	**Enrollment**	**Pct. Students Female**	**Pct. Hispanic**	**Pct. White**	**Pct. Black**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Voucher	0.0686	0.0104	-0.0219	-28.7068	0.0098	-0.0002	-0.0081	-0.0001
	(1.3204)	(0.0271)	(0.0183)	(30.5648)	(0.0066)	(0.0161)	(0.0193)	(0.0060)
Observations	182	182	182	182	182	182	182	182
Outcome Means	23.3	0.13	0.84	624	0.48	0.40	0.36	0.04
for Control	(2.95)	(0.10)	(0.11)	(239)	(0.03)	(0.17)	(0.20)	(0.05)

Notes: Observations are the number of schools. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for pupil-teacher ratio, percentage of teachers with minimum 5 years of experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, and percent hispanic. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

3.9 Appendix Figures

Figure 3.10: Examples of Qualifying GPV and SPV Products and Services

General Purpose Voucher Examples			Specific Category Software Voucher Examples		
Product Category	**Specific Product**	**Product or Service**	**Product Category**	**Specific Product**	**Product or Service**
Software	My Reading Coach from MindPlay (workstation license)	Product	Server Software	Microsoft Office Enterprise 2007 All Lng MVL	Product
Monitor	Acer V173b LCD Display	Product	Server Software	Business Intelligence Suite Enterprise Edition Plus	Product
Projector	Hitachi CPX2015WN Projector	Product	Server Software	L4U Enterprise Library Management Software	Product
Networking/Infrastructure	Fiber optic cable for networking and infrastructure	Product	Server Software	VMware vSphere 5 Essentials Plus Kit	Product
Service	Training in the use of Datawise	Service - PD	Server Software	EDULOG Field Trip Management Software with Web Request and Approval Rules Management (Client/Server Version) Initial License Fee/Annual License and Maintenance Fee	Product
Service	Installation of Powerpoint for 30 workstations	Service - IT	Encyclopedia	Ed1Stop web portal membership	Product
Computer	Dell Optiplex 790 desktop computer	Product	Server Software	Accelerated Reader Enterprise Special Edition - including set up fee, per year charge, and additional student charge	Product
Projector	Hitachi CP-X2011N XGA LCD Projector	Product	Server Software	Specops Password Policy by Special Operations Software	Product
Networking/Infrastructure	Apple AirPort Extreme	Product	Server Software	Microsoft Operations Server 2007 license w/SQL License and Media	Product
Projector	Sharp Notevision XR-11XCL Multimedia Projector	Product	Specific Category Software Product Bundles	Adobe Creative Suite 5.5 Master Collection	Product
Service	Upgrade of Aeries Student Information System	Service - IT	Server Software	Discover Career Planning Program (Windows Version)	Product
Software	ReadAbout 100 desktop version	Product	Server Software	VI3 Enterprise 2P Upgrade Server Software	Product
Service	Digital Classrooms: Engaging Learning Environments conference	Service - PD	Server Software	Gold Support (includes updates and upgrades) for Virtual Infrastructure	Product

Figure 3.11: Description of Datasets Used

Data source	Years	Sample	Variables used
Settlement Claims Administrator, Balance Statements	2006-Present	All eligible districts and voucher balances	Beginning balance, and payments (i.e. reimbursements) from 2006 - October 2010
California Department of Education, California Basic Educational Data System (CBEDS) Downloadable Data Files	1993-2018	All public K-12 schools in California – includes data about schools and districts including kindergarten program type, truancy, educational options enrollment, technology, educational calendars, estimated teacher hires, and graduation requirements	Number of computers per students; number of classrooms per school with Internet for the 2010-2011 school year
California Department of Education, STAR Test Results	1998-2013	All California public K-12 schools' STAR* test results	Mathematics and English CST results for the years 2006 and 2011, by school, grade, and subgroup
California Department of Education, Staff Data Files, Staff Assignment and Course Data	1997-2018	All public K-12 schools in California – includes data files for course information (e.g., course enrollment, grade level, UC/CSU indicator, and NCLB core and compliant status) as well as assignment information (e.g., type of staff and time base)	Full-time equivalent time staff spends in computer education, mathematics, and English courses for the 2010-2011 school year

Notes: *The STAR testing program includes the California Standards Tests (CST), the California Modified Assessment (CMA), the California Alternate Performance Assessment (CAPA), and the Standards-based Tests in Spanish (STS).

Figure 3.12: SCA Data Example

District Number: 3066423

District Name: ANAHEIM CITY SCHOOL DISTRICT

District Award

Total District Award:	$1,896,415.84
District Award GPV:	$952,304.85
District Award SV:	$944,110.99

Current Balance Summary

Total Current Balance:	$1,543.33
Total Current GPV:	$1,543.33
Total Current SV:	$0.00

Date	Action	GPV Activity	GPV Balance	SV Activity	SV Balance
11/3/2006	Beginning Balance		$525,559.88		$525,559.87
7/20/2007	Payment	$27,596.73	$497,963.15	$44,101.00	$481,458.87
9/26/2007	Payment	$315,301.81	$182,661.34	$70,117.24	$411,341.63
11/19/2007	Payment	$117,510.87	$65,150.47	$109,533.56	$301,808.07
3/26/2008	Payment	$924.07	$64,226.40	$15,506.26	$286,301.81
6/20/2008	Payment	$924.06	$63,302.34	$28,607.96	$257,693.85
2/23/2009	Payment	$21,381.91	$41,920.43	$53,363.81	$204,330.04
1/20/2010	Payment	$33,215.19	$8,705.24	$90,452.43	$113,877.61
11/15/2010	Distribution 2	$47,377.32	$56,082.56	$47,377.31	$161,254.92
11/19/2010	Payment	$0.00	$56,082.56	$61,445.68	$99,809.24
7/6/2011	Payment	$0.00	$56,082.56	$16,568.44	$83,240.80
7/27/2012	Payment	$8,190.67	$47,891.89	$39,694.18	$43,546.62
11/20/2012	Payment	$967.95	$46,923.94	$17,739.73	$25,806.89
7/22/2013	Payment	$14,661.57	$32,262.37	$20,332.42	$5,474.47
1/9/2014	Payment	$30,792.72	$1,469.65	$5,474.47	$0.00
2/21/2014	Payment	$1,469.65	$0.00	$0.00	$0.00
3/4/2014	Increase Allocation: Dist 1	$346,349.95	$346,349.95	$341,546.95	$341,546.95
1/27/2015	Increase Allocation: Dist 1	$10,041.84	$356,391.79	$9,906.21	$351,453.16
3/13/2015	Payment	$146,457.74	$209,934.05	$0.00	$351,453.16
6/10/2015	Payment	$182,941.71	$26,992.34	$84,222.58	$267,230.58
8/19/2015	Payment	$26,992.34	$0.00	$267,230.58	$0.00
11/20/2016	Increase Allocation: Dist 2	$21,432.53	$21,432.53	$19,720.65	$19,720.65
12/8/2017	Payment	$21,432.53	$0.00	$19,720.65	$0.00
8/29/2018	Increase Allocation: Dist 2	$1,543.33	$1,543.33	$0.00	$0.00
9/6/2018	Current Balance		$1,543.33		$0.00

Figure 3.13: District-level Distribution of Percentage of Voucher Spent by October 2010

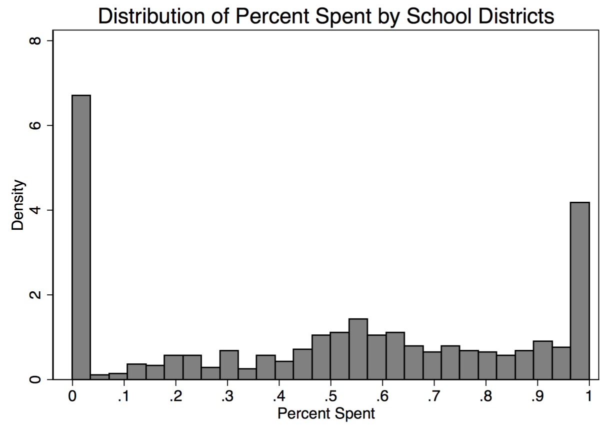
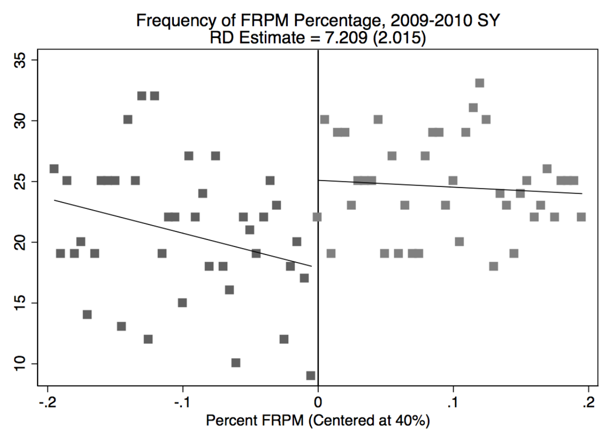
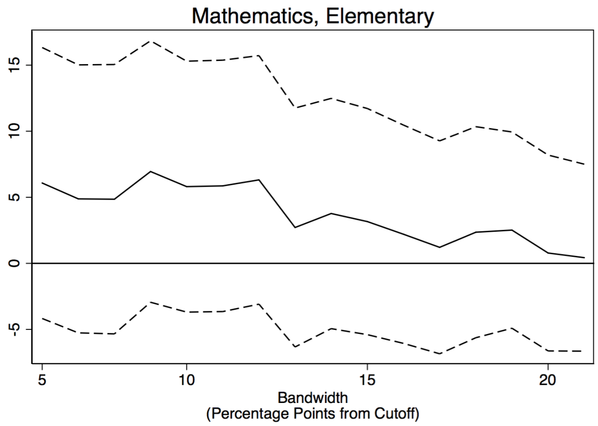


Figure 3.14: Density Test, 2009-2010 School Year

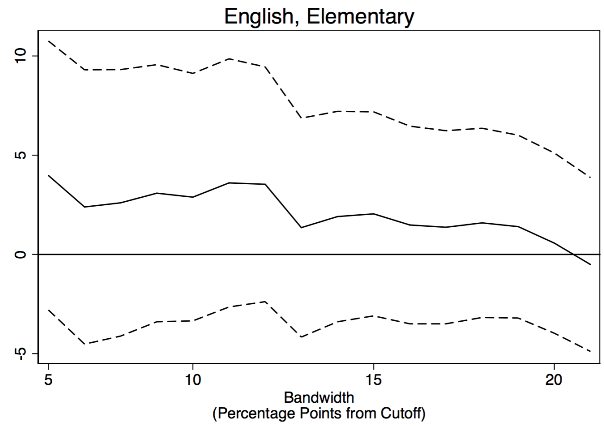


Notes: Density test of the number of schools on each side of the 40% eligibility threshold for the 2009-2010 school year.

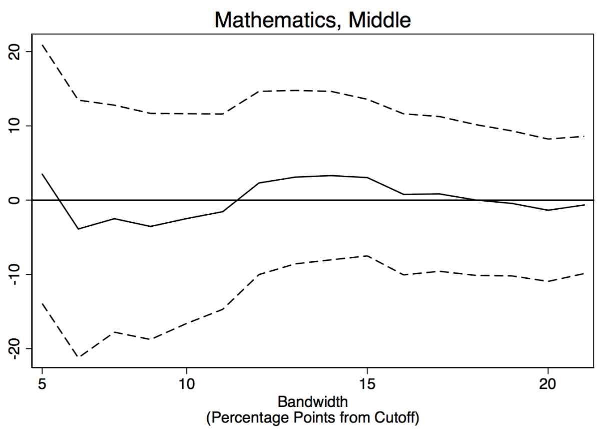
Figure 3.15: Change in Mean Scale Scores from 2006 to 2011 by School Type, with Varying Bandwidth



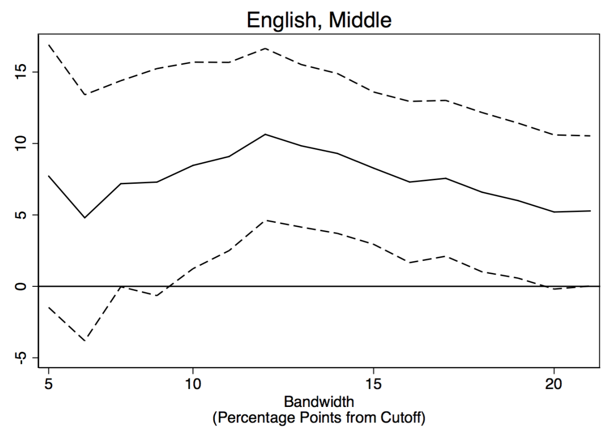
(a)



(b)



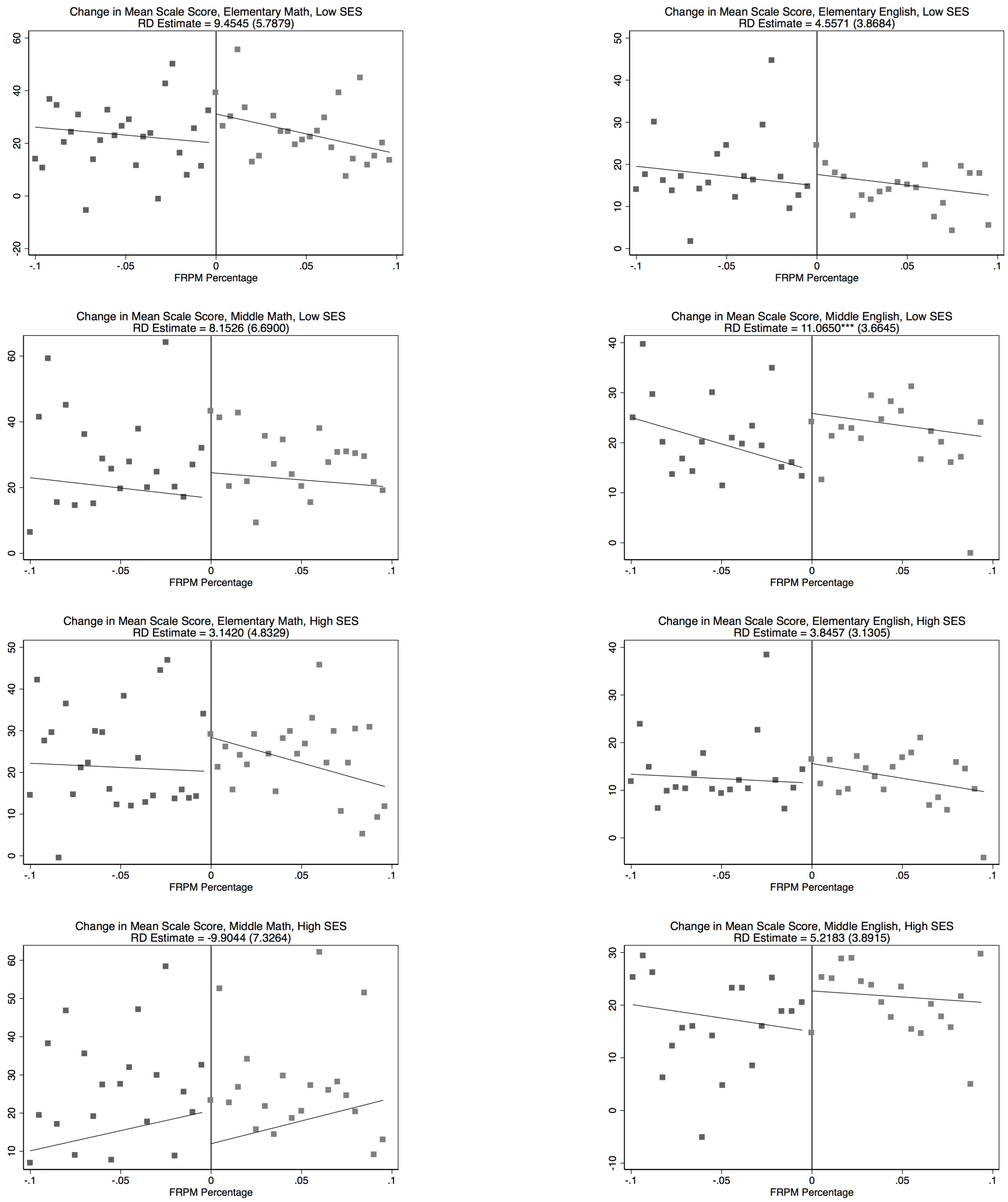
(c)



(d)

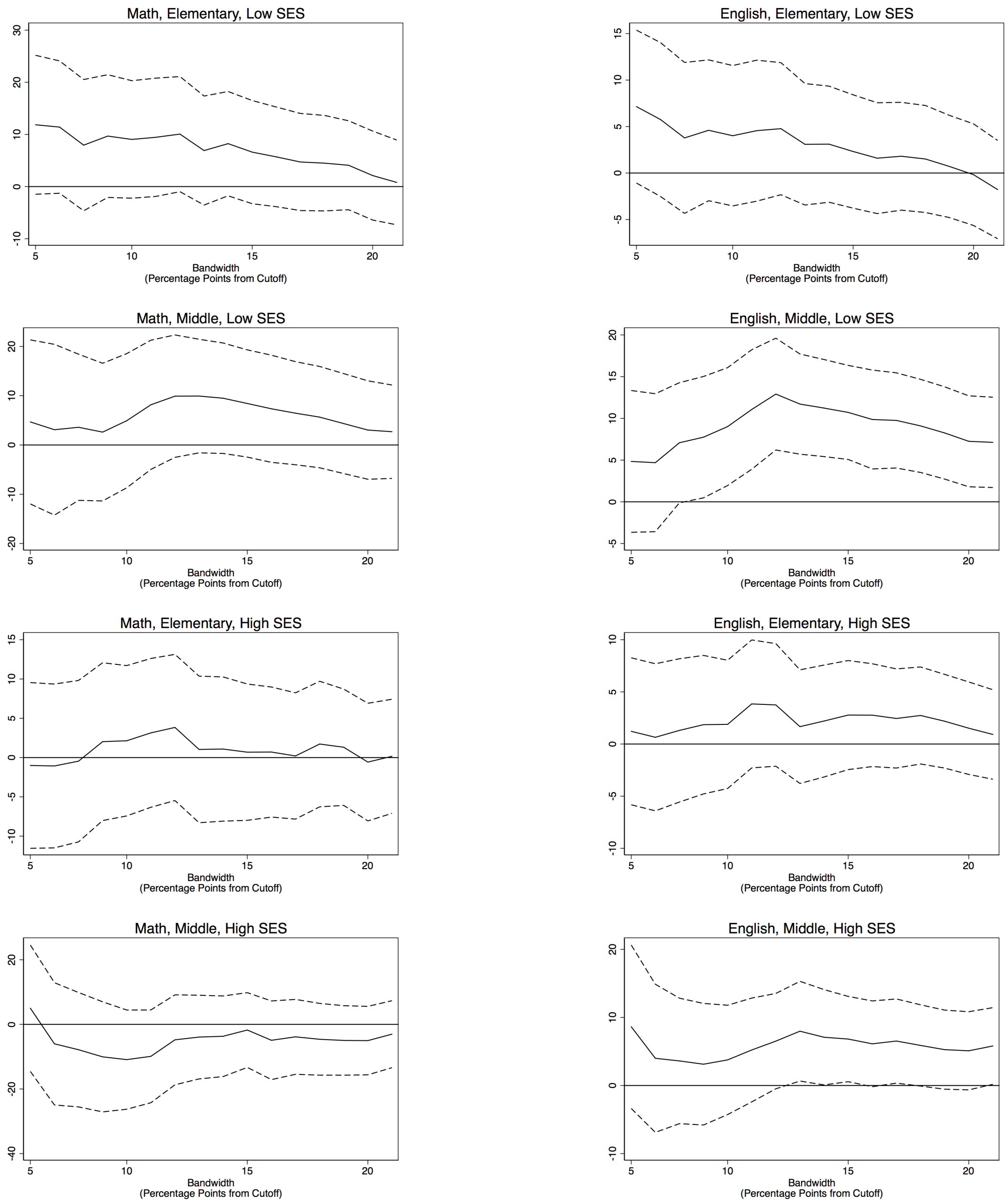
Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle schoolers. Free and reduced price meal (FRPM) percentage is relative to the 40% cutoff (i.e. FRPM equal to 0 corresponds to a FRPM percentage of 40).

Figure 3.16: Change in Mean Scale Scores from 2006 to 2011 by School Type & SES



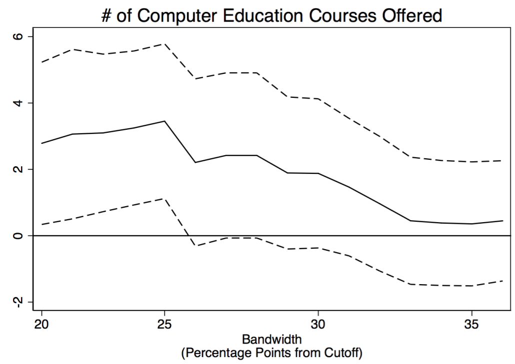
Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle schoolers, by SES. FRPM percentage is relative to the 40% cutoff.

Figure 3.17: Change in Mean Scale Scores from 2006 to 2011 by School Type & SES - Bandwidth

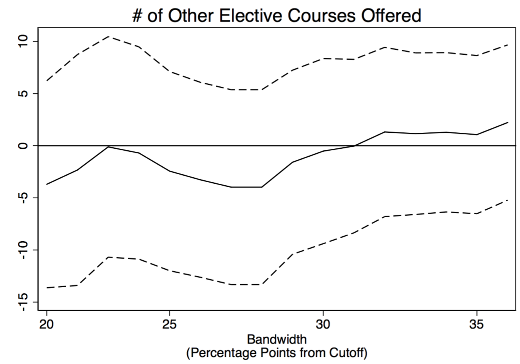


Notes: See notes to Figure 3.17.

Figure 3.18: Computer Education and "Other" Electives Courses Offered, with Varying Bandwidth



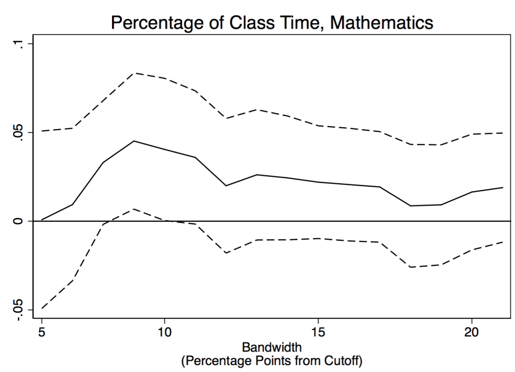
(a)



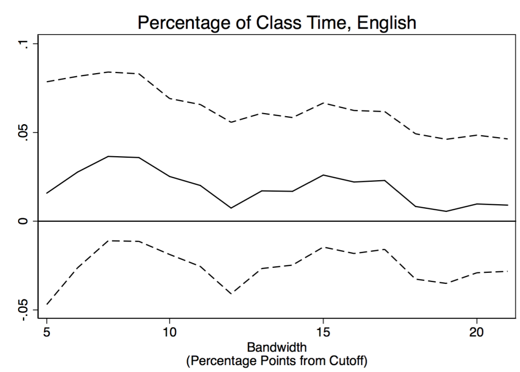
(b)

Notes: The dependent variables are the number of computer education courses offered and the number of "other" elective courses offered for middle schoolers. "Other" elective courses offered includes art, dance, drama, foreign language, home economics, and music courses. FRPM percentage is relative to the 40% cutoff.

Figure 3.19: Instructional Time in Math, and English, with Varying Bandwidth



(a)



(b)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle schoolers. FRPM percentage is relative to the 40% cutoff.

3.10 Appendix Tables

Table 3.12: Effect of CA K-12 Technology Voucher Program on Technology and Test Scores

	Computers per Student	**Classrooms with Internet**	**Mathematics**	**English**
	(1)	(2)	(3)	(4)
Panel I: 80% Spending by 2010				
Voucher	0.0261	0.2017	3.8782	4.5847**
	(0.0298)	(0.1839)	(3.3489)	(1.9395)
Observations	216	217	835	912
Outcome Means for Control	0.175	34.52	19.43	14.23
	(0.129)	(28.41)	(27.75)	(15.89)
Panel II: 70% Spending by 2010				
Voucher	0.0218	0.1360	0.8161	3.1780*
	(0.0261)	(0.1521)	(2.9258)	(1.7013)
Observations	289	290	1,111	1,213
Outcome Means for Control	0.162	34.34	19.16	14.09
	(0.121)	(24.97)	(27.11)	(15.45)

Notes: The dependent variable is the number of computers per student, the logged number of classrooms with Internet, and the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011, respectively, for elementary and middle schools combined. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.13: Effect of CA K-12 Technology Voucher Program on Test Scores, ITT

	Mathematics	**English**
	(1)	(2)
Voucher	-0.3181	0.9256
	(2.4173)	(1.4463)
Observations	3,348	3,637
Outcome Means for Control	19.63	14.37
	(26.90)	(15.19)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011, for elementary and middle schools combined. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.14: Effect of CA K-12 Technology Voucher Program on Test Scores, by School Type, ITT

	Math, Elementary	**English, Elementary**	**Math, Middle**	**English, Middle**
	(1)	(2)	(3)	(4)
Voucher	-0.789	0.478	0.5246	2.0457
	(2.7160)	(1.7076)	(3.4012)	(1.9726)
Observations	2,601	2,601	747	1,036
Outcome Means for Control	20.42	13.30	16.76	17.18
	(28.46)	(16.06)	(20.00)	(12.21)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.15: Effect of CA K-12 Technology Voucher Program on Test Scores, by School Type & SES, ITT

	Math, Elementary	**English, Elementary**	**Math, Middle**	**English, Middle**
	(1)	(2)	(3)	(4)
Panel I: Low-SES				
Voucher	-0.7926	0.6195	1.3889	1.7961
	(2.8910)	(1.8070)	(3.5642)	(2.3071)
Observations	2,567	2,567	726	1,009
Outcome Means for Control	23.97	16.79	20.96	21.53
	(32.20)	(19.79)	(20.70)	(14.95)
Panel II: High-SES				
Voucher	0.0657	1.3648	-0.9012	2.3195
	(2.9117)	(1.8020)	(3.8331)	(2.1593)
Observations	2,538	2,537	727	1,010
Outcome Means for Control	23.36	15.28	19.91	19.68
	(30.37)	(17.31)	(22.68)	(13.65)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades, by SES. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.16: Effect of CA K-12 Technology Voucher Program on Test Scores, Placebo Thresholds

	10%	**20%**	**30%**	**40%**	**50%**	**60%**	**70%**	**80%**	**90%**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel I: Mathematics									
Voucher	6.1267	-1.3122	-2.6388	4.4722	5.2479	-2.1271	6.278	-7.0245	0.6736
	(4.5732)	(4.8454)	(4.7608)	(4.3708)	(4.6661)	(5.1146)	(4.6669)	(5.4689)	(4.6323)
Observations	600	573	642	715	803	885	871	822	777
Panel II: English									
Voucher	2.4248	-1.4237	-1.4714	5.6855**	3.0503	1.5434	3.9464	-1.5813	0.5850
	(2.2690)	(2.5813)	(2.3407)	(2.5559)	(3.0022)	(3.3655)	(2.5867)	(2.8095)	(2.5690)
Observations	635	617	692	779	878	954	930	870	810

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the respective FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.17: Effect of Voucher Program on Test Scores, by School Type & SES, Non-spending schools

	Math, Elementary	**English, Elementary**	**Math, Middle**	**English, Middle**
	(1)	(2)	(3)	(4)
Panel I: All				
Voucher	-0.0545	-4.7433	-5.2047	-4.6333
	(10.0241)	(5.7748)	(11.1150)	(6.3879)
Observations	249	249	81	119
Outcome Means for Control	18.78	15.66	21.11	17.77
	(26.38)	(15.60)	(22.33)	(12.09)
Panel II: Low-SES				
Voucher	0.1469	-2.3863	-9.1932	-8.8901
	(10.6348)	(6.9453)	(16.1421)	(6.9679)
Observations	243	243	73	108
Outcome Means for Control	21.61	19.03	23.33	21.05
	(31.62)	(21.34)	(24.60)	(17.96)
Panel III: High-SES				
Voucher	-3.4198	-7.9539	-13.0733	-7.1942
	(9.6935)	(5.0952)	(11.4922)	(5.3207)
Observations	248	248	78	115
Outcome Means for Control	21.52	16.97	22.35	20.80
	(26.43)	(15.71)	(23.34)	(12.68)

Notes: The dependent variable is the change in the Mathematics or English CST Mean Scale Score from 2006 to 2011 for elementary and middle school grades, by SES. RDD estimates are reported using LLR with a bandwidth of +/- 0.1 around the 40% FRPM cutoff. Regressions include controls for the number of students tested, percentage of teachers with a minimum of 5 years experience, percentage of teachers who are female, pupil-teacher ratio, total enrollment, rural, percent female, percent white, percent black, percent hispanic, and grade fixed effects. Standard errors clustered at the school-level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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