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Essays on Health Insurance and Education

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy

in

Economics

by

Youjin Hahn

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2011

The Dissertation of Youjin Hahn is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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University of California, San Diego

2011

DEDICATION

To my parents, Hong-sup and Myoungjin Hahn,
for their unconditional love
and helping me stay optimistic during this journey.

To my husband Hee-Seung Yang,
for always being there for me.

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ABSTRACT OF THE DISSERTATION

Essays on Health Insurance and Education

by

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Doctor of Philosophy

University of California, San Diego, 2011

Professor Julian Betts, Co-Chair
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The goal of my research is to explore ways to improve the welfare of populations that are targeted by public programs. In particular, my thesis consists of three chapters on health insurance and education.

The first chapter looks at Medicaid take-up decisions among poor children. Medicaid is public health insurance that is available to low income individuals, and it is provided freely by the government. However, there is a puzzling observation that many low-income children are uninsured despite their eligibility for Medicaid. As one possible explanation, I propose that the low level of access to health care that Medicaid provides can explain incomplete take-up. Existing literature suggests that the low level of Medicaid fee payments to physicians reduces their willingness to see Medicaid patients, creating an access-to-care problem for these patients. Using variation in the timing of the changes in Medicaid payments across states, I find that improving Medicaid generosity increases the take-up rate and reduces the uninsured rate among poor children. These findings provide a partial explanation for why Medicaid-eligible children in poverty remain uninsured.

While my first chapter focuses on traditional means-tested public health insurance which targets mainly low income families, the second chapter explores the issues with a more recent intervention that extends beyond low income families. In recent years, several states have allowed young adults as old as 30 to remain covered under their parents' employer-provided health insurance. For those who qualify for these benefits, the expansion of parental coverage partially reduces the value of being employed by a firm that provides health insurance since adult children can now get health insurance through another channel. We employ quasi-experimental variation in the timing and generosity of states' eligibility rules to identify the effect of the policy change on young adults' labor market choices. Our results suggest that the expansion of parental coverage increases the group coverage rate and reduces labor supply among young adults, particularly in full-time employment.

The third chapter analyzes the effect of educational tracking by decomposing it into the separate roles of peer effects and coursework. The practice of tracking often results not only in grouping students by different ability, but also in providing different types of coursework for students. For instance, the advanced track may have both higher achieving peers and higher level coursework. Using detailed panel data from the San Diego Unified School district, I find that having high achieving peer is beneficial, while I do not find convincing evidence that taking more advanced math coursework predicts student's test score.

Chapter 1: The Effect of Medicaid Physician Fees on Take-up of Public Health Insurance among Children in Poverty

Abstract

I investigate how changes in fees paid to Medicaid physicians affect take-up among children in low-income families. The existing literature suggests that the low level of Medicaid fee payments to physicians reduces their willingness to see Medicaid patients, thus creating an access-to-care problem for these patients. In this paper, I use variation in the timing of the changes in Medicaid payment across states to identify whether increasing Medicaid generosity induces take-up among poor children. For the identical service, current Medicaid reimbursement rates are only about 65% of that covered by Medicare. I find that increasing these Medicaid reimbursement rates to 100% will increase the take-up rate among poor children by 4.8 percentage points. These results provide a partial explanation for the puzzling observation that many low-income children who are eligible for Medicaid remain uninsured.

1.1 Introduction

Medicaid was created in 1965 to provide virtually free public health insurance to low-income individuals in the United States. Although most children below the poverty line are eligible for public insurance through several federally mandated programs, the uninsured rate among this group has remained high, almost double that of children above the poverty line.[1] This puzzling phenomenon of 'eligible but not enrolled' under means-tested social insurance and transfer programs has motivated a good deal of research in identifying factors that affect take-up. The previous literature has proposed several explanations for why individuals do not participate in public programs even when they are eligible for benefits. Although the monetary costs of enrolling in Medicaid are almost zero since Medicaid entails virtually no out of pocket costs, individuals may face nonmonetary costs when they enroll in the public program. Stigma attached to public insurance and administrative hassles could increase the cost of enrolling in public insurance (Remler et al., 2001). There are also informational barriers, particularly if potential enrollees have not used public programs before (Aizer, 2007; Kenney and Haley, 2001).

In this paper, I offer a new perspective on the take-up of Medicaid. The previous literature on the determinants of Medicaid take-up has largely focused on the cost of enrolling in public programs. The current study departs from the previous literature by focusing on how the value of Medicaid affects take-up. In particular, I examine the relationship between take-up and patient access to care, using the

[1] For example, the uninsured rate among poor children was 16% while the uninsured rate of children above the poverty line was 9%, according to March Current Population Survey data for year 2007.

Medicaid-to-Medicare fee index as a proxy for access to care provided by Medicaid. The Medicaid reimbursement levels for physicians have been historically low. As a result, physicians are not incentivized to treat Medicaid patients, which creates access-to-care problems for Medicaid patients. In fact, twenty percent of pediatricians in the United States do not see Medicaid patients at all and forty percent limit the number of Medicaid patients in their practice (Currie and Fahr, 2005). All else being equal, increasing the Medicaid payment to physicians would lead to a higher participation rate among physicians. Past studies have both theoretically posited and empirically tested this positive relationship between Medicaid payment and physician participation (McGuire and Pauly, 1991; Perloff et al., 1995; Decker, 2007).

One valid conjecture then is how the increased physician participation, which is induced from an increase in Medicaid reimbursement, affects the decision faced by potential Medicaid beneficiaries. If the potential beneficiaries weigh the cost against the benefit of enrolling in Medicaid and decide to take-up only when the benefit exceeds the cost, then the increase in access to care would encourage higher enrollment rates among the Medicaid-eligible. This paper is the first to explore the relationship between patients' access to care and take-up.

I focus on the effect of access to care on the health insurance status among poor children since this is the population that is both most likely to suffer from access problems and most vulnerable to financial and health shocks. As figure 1 shows, the uninsured rate among poor children, which is almost double the rate among non-poor children, is still high despite nearly universal eligibility for Medicaid.

The effect of improved access to care on take-up among poor children is identified by exploiting both cross-state variation and within-state variation over time in the Medicaid-to-Medicare primary fee index. I find that increasing the Medicaid fee payments from 65% to 100% of the Medicare level increases the take-up rate among poor children by 4.8 percentage points and decreases the uninsured rate by 6.2 percentage points, thus reducing the uninsured rate in this group by almost 30%. Therefore, improving access to care through increased physician reimbursements can be an effective way of providing health insurance coverage to uninsured low-income children.

The ideal experiment would require that states be the same except for the randomly determined Medicaid reimbursement rates. In reality, however, states may endogenously change Medicaid fee, and any correlations between changes in fee and other unobserved states' efforts to increase take-up (e.g. simpler enrollment procedures or greater outreach to potential enrollees) would cause problems in identification. I will come back to this point later in the robustness section.

The paper proceeds as follows. Section 2 lays out the potential mechanisms by which the increase in Medicaid provider payment improves access to care and eventually leads to an increase in take-up. Section 3 describes the measure for access to care and the main dataset. In section 4, I specify estimation strategies. Section 5 reports results for basic specifications. Section 6 addresses potential identification issues by reporting results for robustness checks. Section 7 concludes by discussing the policy implications of the findings in this paper.

1.2 Conceptual background

In this section, I discuss the possible mechanisms through which changes in the Medicaid fee would affect the incentives physicians perceive and in turn influence take-up behavior among potential Medicaid beneficiaries.

A substantial number of office-based primary care physicians place a limit on the size of their Medicaid practices or do not see Medicaid recipients at all (Held and Holahan, 1985; Perloff et al., 1997). The primary reason for this low level of physician participation in Medicaid appears to be the low Medicaid payments to doctors. According to a statewide survey of fellows of the American Academy of Pediatrics, 58% of the pediatricians reported that the low fee was a key reason for limiting participation in Medicaid, and 53.3% of the pediatricians reported that Medicaid payments did not cover overhead (Yudkowsky et al., 2000).[2] Table 1 provides a glimpse of the access to care problem that Medicaid patients face. Although Medicaid provides superior care compared to not having any insurance (9% of Medicaid patients vs. 35% of the uninsured had no usual source of care), except for the row (6), Medicaid patients in general have greater problems in access to health care in a number of dimensions. For instance, they have harder time getting a referral to a specialist. The fraction of Medicaid patients whose usual place of care is doctor's office (as opposed to hospital outpatient clinic, other clinic/health center and hospital emergency room) is considerably lower than Medicare or private patients. They also

[2] Others, such as paperwork concerns (40.5%), unpredictable payments (39.6%), and payment delays (34.3%) are also the reasons for limiting participation in Medicaid. 11.4% reported Medicaid payments cover overhead and 35.4% do not know whether Medicaid payments cover overhead.

wait longer in the office or clinic relative to the patients with private insurance or Medicare.

In order to see how the change in Medicaid fees affects access to care, I first consider a simple case of a single payer (insurance) system where physician services are reimbursed by fee-for-service. There is an excess demand in the Medicaid health care market since Medicaid patients face almost no out-of-pocket costs once insured, while marginal costs of providing care to Medicaid patients are not zero. This unmet excess demand for health care, the access problem, is likely to be more severe since the Medicaid reimbursement is low. Thus, if the Medicaid fee increases, it would improve access to health care since total supply of health services would increase.

An increase in the Medicaid fee has several confounding effects on the supply of health care when there are multiple insurance payers. The current health insurance market in the United States can be characterized as physicians' facing multiple payers, such as private insurance, Medicaid and the State Children's Health Insurance Program (SCHIP), private insurance, Medicare and other types of public insurance (i.e. Indian health service or military health care TRICARE). Theoretically, an exogenous increase in Medicaid fee would lead to both substitution and income effects. The substitution effect would occur as an increase in Medicaid fee would make the marginal Medicaid patients more attractive relative to the marginal private patients. At the same time, a higher fee would make physicians richer so they would respond by decreasing the supply of care (income effects). McGuire and Pauly (1991) illustrate that the income effect likely dominates the substitution effect when insurance payers who cover a large volume of patients change the fee. The substitution effect dominates when insurance payers who cover a small volume of patients change the fee. Since Medicaid patients constitute a small share of total patients, the substitution effect dominates for the physicians whose practice-share of Medicaid patients is small. The increase in Medicaid fee would predict the increase in the quantity of care supplied to Medicaid patients.

Increases in the quantity of care can take several forms. First, physicians can spend more time with Medicaid patients (intensive margin). They may also accept more Medicaid patients or increase the probability of seeing Medicaid patients at all (extensive margin). Since greater physician participation means more choices for patients, it would make Medicaid a more attractive option to both existing and potential beneficiaries. Findings from earlier studies suggest that physician participation in the Medicaid program does in fact respond to Medicaid fee changes. In empirical analysis controlling for state fixed effects, Decker (2007) finds that higher Medicaid-to-Medicare fee ratios increase both the fraction of Medicaid patients seen by physicians and the number of private physicians who see Medicaid patients. Zuckerman et al. (2004) also document that physicians in states with the lowest Medicaid fees were less willing to accept new Medicaid patients in 1998 and 2003.

The increase in provider participation would indirectly improve other aspects of health care as well, such as having usual care occur in office-based settings and decreasing the travel costs involved in obtaining health care. With a lack of office-based physicians' participation, many Medicaid recipients are treated in freestanding clinics or hospital outpatient departments (Cohen, 1989; Long et al., 1986). Studies find that an increase in Medicaid payment shifts the usual place of care from clinics to private physicians' sites, which is more desirable for the continuity of care and for receiving preventive services (Cohen and Cunningham, 1995; Gruber et al., 1997). Decker (2009) also finds that cuts in fees shifted away Medicaid patients from physician offices toward hospital emergency department and outpatient departments.

In addition, the average distance to the nearest health care facility would decrease with greater physician participation. The care provided by Medicaid is practically costless to patients, but the patients may still face large travel costs relative to their income. The fact that the price elasticity of demand for health care is high for low-income people (Gertler et al, 1987) implies that potential Medicaid patients would be sensitive to changes in travel distance. Thus, reduced travel costs through increased physician participation may serve as another channel via which it increases take-up of Medicaid.

Some market characteristics and hospital policies may mitigate or confound the effect of fee changes on the supply of medical care discussed so far. One concern in particular is that the Medicaid fee policy might not be a relevant measure of access to care given the rapid growth of Medicaid managed care, where physicians are paid based on the capitation rate rather than on the fee-for-service basis. However, the fee-for-service (FFS) reimbursement continues to affect the majority of Medicaid enrollees. In 2006, about half of all Medicaid patients were enrolled in either FFS or primary care case managed (PCCM) plans, where under PCCM plans, services were still paid via FFS (Zuckerman et al. 2009). Also, the FFS reimbursement rates are highly correlated with what Medicaid health maintenance organizations (HMOs) pay physicians, as states often set capitation rates based on what they pay in the FFS part of the program (Zuckerman et al. 2004).

Another concern is that Medicaid patients are commonly served in hospitals and public clinics, and in these sites, services might not be reimbursed based on the Medicaid fee schedule. Hospital outpatient departments in most states have their own reimbursement system not tied to the Medicaid fee schedule, and Federally Qualified Health Centers (FQHCs) are paid via a cost-based reimbursement scheme which is also not tied to the Medicaid fee schedule. Also, physicians in hospitals and public clinics have less freedom in determining the supply of care since they are obligated to meet government mandates or institution goals (Baker and Royalty, 2000). Thus, the effect of the Medicaid fee changes would result mainly from private physicians who have more leeway to adjust their behavior following the fee changes.

From the beneficiaries' point of view, they will enroll when the value of health insurance exceeds the cost of receiving care and the other non-monetary enrollment costs. Health insurance mainly has two roles: 1) it makes expensive care accessible by covering the expense for unexpected catastrophic events (Nyman, 1999), and 2) it makes care for routine check-ups and preventative illnesses accessible. I expect improvements in access on the latter role of health insurance to be the more relevant mechanism through which the changes in Medicaid fee affect take-up. For unexpected catastrophic events, uninsured individuals may receive one-time care at the hospital emergency room and may not be responsible for the cost (i.e. charity care). The Medicaid-eligible patients may also enroll after receiving emergency care, since hospitals are better off enrolling the patients and being reimbursed by the government than bearing the treatment costs themselves.

In sum, increasing the Medicaid fee would raise the perceived value of Medicaid in several ways: by making routine care more accessible, by shifting the usual place of care from public clinics to doctor's offices, and by decreasing travel costs involved in receiving routine care. Using a proxy measure for Medicaid fee policy for primary care, I expect to capture all the possible channels through which the fee influences take-up.

1.3 Data

1.3.1 Proxy for access to care: Medicaid to Medicare fee ratio

Since the increase in Medicaid fees would improve access to care, I employ a summary measure of Medicaid fee policies in modeling the individual's Medicaid take-up behavior. I propose using the Medicaid-to-Medicare fee (MMF) index as a proxy for access to care of public health insurance.[3]

The Urban Institute developed the MMF by surveying the District of Columbia and 49 states that have a fee-for-service (FFS) component in their Medicaid program. The MMF reports a weighted sum of the ratios of the Medicaid fee to the Medicare fee where the weight for each service is its share in total expenditure. I use

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[3] I thank Stephen Zuckerman for kindly sharing the Medicaid-to-Medicare fee data.

data for three years, 1993, 1998 and 2003.[4] The detailed documentation of this index is available in Zuckerman et al. (2004), Norton (1995) and Norton (1999). There are four components in the fee index: overall, primary care, obstetric care and other services. These fee indexes are highly correlated. For three years when the fee indexes are available, the correlation coefficient between Medicaid-to-Medicare fee index for primary care services and (1) for all services ranges from 0.93 to 0.95, (2) for obstetric services ranges from 0.49 to 0.69, and (3) for other services ranges from 0.57 to 0.73. I use the fee index for primary care as it is likely to be the most relevant service for children and it is most useful in capturing the incentives physicians face when providing routine care. I also use other fee indexes in the falsification test (section 6.2) to see whether some fee indexes that are not expected to have effects predict take-up.

It is worth noting that the Medicaid physician fee is set by each state and exhibits substantial variability across states and within states over time. On the other hand, there is much less heterogeneity in the package of services covered from state to state. This is because federal law requires states to cover major services, such as physician and hospital care. Even for optional services such as prescription drugs or dental care for which states do not have to pay, almost all states cover these expensive optional services (Gruber, 2000).

However, eligibility standards vary both across states and within states over time. Although I look at always-eligible children by focusing on the children in

[4] As of May 2010, 2008 Medicaid to Medicare fee payment is available but this paper does not use these data in the analysis since March CPS 2010 is not available yet. Also, using Medicaid-to-private physician fee index would be more suitable but the data is not available. County level fee index would also be desirable since urban and rural payment could be different within state, but I could not find such measure available for multiple years.

poverty, the variability in eligibility poses a potential threat in estimating the relation between Medicaid fee and take-up. The increase in Medicaid fee may correlate with the increase in the eligibility standards, and this change in eligibility may affect insurance status through the crowding out of resources available to poor children. I test the robustness of the model to this concern in section 6.3.1.

Dividing the Medicaid fee by the Medicare fee adjusts the MMF to represent the relative standing of the Medicaid payment in the health insurance market. Since the Medicare fee is adjusted to take into account factors such as medical inflation in practice costs, geographic variations and general wage levels (Centers for Medicare and Medicaid Services), the MMF can be seen as a convenient summary of how well Medicaid pays physicians compared to other types of major public insurance. I expect the Medicaid fee to drive the most of differences in the MMF across states since it exhibits greater disparities than Medicare. The difference in Medicare payments between the lowest and highest-paying state for a given procedure was not more than 25 to 30% in 2002 (Public Citizen Report), while the Medicaid fee index (i.e. without dividing by Medicare fee) for 2003 ranges from 56% of the national average to 228% (Zuckerman et al., 2004). This is because Medicare is a federal program and all the states make payments according to the same fee formula,[5] while Medicaid is a state-administered program and each state can set its own payment level and formula.

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[5] The exact formula for Medicare physician fee schedule payment rates as of 2008 is:

[Work RVU*Budget neutrality adjustor (0.8806)*Work GPCI)+(PE RVU*PE GPCI)+(MP RVU*MP GPCI)]*Conversion Factor,

where Work RVU is Relative Value Units which reflect the relative levels of time and intensity associated with the service; PE RVU is to reflect Practice expense; Conversion Factor is updated on an annual basis according to a formula specified by statute; and GPCI represents Geographic Practice Cost

Figure 2 shows the Medicaid-to-Medicare primary fee index. On average, Medicaid paid only 77% of what Medicare paid in 1993, 65% in 1998, and 71% in 2003 for primary care. Except for Alaska, most states pay less for Medicaid than for Medicare. In 2003, New York had the lowest relative fee (.34) and Alaska had the highest relative fee (1.47), meaning that New York paid only 34% while Alaska paid 138% of what Medicare paid. In order to grasp how great the within-state variation is over time, I compare the overall standard deviation of the MMF in state and year cells with the standard deviation after taking out state and year fixed effects. The overall standard deviation is .206, and the standard deviation after taking out state and year fixed effects is .089. This indicates that about half of the total variation comes from across states while the other half comes from within-state variation over time. Figure 3 depicts changes in the Medicaid-to-Medicare primary fee index and shows that states change fees differentially at different points in time. Between 1993 and 1998, the majority of states decreased Medicaid payment relative to Medicare, with Alaska and New Mexico showing the greatest decrease and increase respectively. Between 1998 and 2003, more than half of the states improved Medicaid payment relative to Medicare, with Maine and Iowa showing the largest decrease and increase respectively.[6]

Figure 4 shows the graphical relation between the changes in the Medicaid-to-Medicare primary fee index and take-up. For each state, the change in take-up (i.e.

Indices, purpose of which is to account for geographic variations in the costs of practicing medicine in different areas (Medicare Physician Fee Schedule, Centers for Medicare and Medicaid Services).

[6] According to figure 3, changes in the fee ratio in Alaska (between 1993 and 1998) and Iowa (between 1998 and 2003) appear to be outliers. Excluding these two states did not change the estimates of the fee ratio much.

take-up here is defined by the fraction of poor children receiving public insurance) is plotted against the change in the fee ratio. Both the changes between 1993 and 1998 and between 1998 and 2003 are plotted. The difference in take-up rate is positively related to the difference in the fee ratio. The slope of the ordinary least square regression when I regress changes in take-up on changes in the primary fee index is 0.322 with the standard error of 0.113.

1.3.2 The March Current Population Survey

I employ the March Current Population Survey (the March CPS), 1995, 2000 and 2005 in conjunction with the Medicaid-to-Medicare fee index to identify the effect of improving access to care on take-up of public health insurance.[7] Respondents are asked about their health insurance coverage and income in the prior year, thus the data covers 1994, 1999 and 2004. Surveyed households intend to give measures of full-year uninsurance rather than point-in-time uninsurance (State Health Access Data Assistance Center and Robert Wood Johnson Foundation, 2007).

The March CPS offers a variety of information on individual characteristics including health insurance status. In addition, it has a large sample size and allows for nationally representative estimates when using sampling weights. The March CPS also identifies individuals from every state in the United States. Since my identification comes from the variation within states over time, having all states is an advantage over

[7] Data was extracted from the IPUMS website: http://cps.ipums.org/cps.

other widely used datasets for health insurance research, such as the Survey of Income and Program Participation (SIPP).

Several sample restrictions are made in the analysis. I consider only the population of children whose household income falls below the poverty level since this group is the poorest and the most vulnerable group. Several other reasons justify this restriction on income. First, doing so enables me to retain a relatively homogenous group of children who are not directly affected by the SCHIP expansion. The eligibility income limit of the SCHIP has changed drastically during the period in which this paper is interested. Thus, limiting the samples to be below the federal poverty level allows the policy variations of the Medicaid-to-Medicare fee index to be isolated from changes in the income distribution. Second reason of limiting income has to do with that the children in poverty are eligible for Medicaid rather than for SCHIP (i.e. eligibility income limit for SCHIP is usually between 100-300% of the federal poverty level, which is higher than Medicaid income limit). The Medicaid-to-Medicare fee index may be less relevant proxy for access to care for the SCHIP-eligible children, although payments for SCHIP and Medicaid are highly correlated since SCHIP payments are typically based on Medicaid payments.

Another sample restriction is that only children younger than 12 are considered. The Omnibus Budget Reconciliation Acts 1990 (OBRA 1990) required states to cover children in poverty born after September 30, 1983, so the children who are younger than 12 as of 1995 and in poverty are eligible for public insurance. In addition, older children are more likely to work, and if so they may have different channels for obtaining insurance coverage. Limiting the analysis to children below a certain age allows me to circumvent this potential issue.

Other sample restrictions include citizenship status, living arrangements, and a child's relation to the head of the household. Starting with children in poverty who are younger than 12 and who are matched to the Medicaid-to-Medicare fee data, I exclude foreign-born children (about 6% of the remaining sample is dropped) and those who live in group quarters (n=5). Lastly, I consider children who are related to the household head as child, grandchild, relative or non-relative only (about 3% of the remaining sample is dropped).[8] The resulting sample used in the analysis contains 18,635 children over three years.

Sample means for the dependent and control variables are reported in Table 2. About 64% of the poor children in sample were covered by public insurance while 20% were uninsured. The proportion of children covered by public insurance was the lowest in 1999 but it recovered in 2004. This drop in the public insurance rate is driven from the reported rate of "public only", rather than from "public and group private" or "public and non-group private" insurance coverage. The latter two coverages constitute only a small portion of overall reported public coverage.

It was also in 1999 that the uninsured rate was highest. High uninsured rate in this year may seem puzzling given that the unemployment rate was the lowest. At the same time, however, the lagged Medicaid-to-Medicare fee index for primary care services was least generous, which could partly explain the increase in uninsured rate.

[8] That is, I exclude children whose relation to the head is sibling, unmarried partner, housemate/roommate, roomer/boarder/lodger and foster children.

Both actual and simulated eligibility have steadily increased due to the SCHIP expansion. The next two time-varying state variables are related to Medicaid enrollment procedures. Most states did not require asset tests and had presumptive eligibility, but the extent to which they simplify Medicaid enrollment procedures varies across years. Child characteristics have not changed a lot across years. Some parent characteristics have varied over time, such as the proportion of parents who (1) have at least high school level of education, (2) work, and (3) work at a large size firm. Family characteristics appear to be reasonably stable over time.

1.4 Empirical Specifications

The basic specification of estimating the effect of the fee ratio on own insurance coverage status is shown in equation (1). I merge the lagged Medicaid-to-Medicare primary fee index (*Fee*) in each year and state with the sample of children. Since reported insurance status and income are for previous year, lagged fee is constructed by relating the fees in 1993, 1998 and 2003 to March CPS 1995, 2000, and 2005. I use one-year lagged Medicaid-to-Medicare fee since the current fee is likely to affect future take-up. A one-year timing lag of physician fee is also used in the past literature within a similar context (i.e. Currie et al., 1995). However, the exact time it takes to affect take-up is not known and I also experiment with other lag structures in section 6.1.

The specification of coverage for an insurance status *Y* for individual *i* in year *t* is as follows.

$$Y_{it} = \beta_0 + \beta_1 Fee_{i,t-1} + \beta_2 X_{it} + \sum_t \beta_t Year_{it} + \sum_s \beta_s State_{it} + \varepsilon_{it} \qquad (1)$$

The first outcome of interest is *Y=Public*, an indicator variable for whether a child is covered by public insurance (Medicaid). I also examine the effect of *Fee* on *Y=Private* and *Uninsured*, the indicators for being covered by private insurance and being uninsured. Effects found in these outcome variables would indicate where the change in take-up of *Public* comes from—whether from the crowding out of private insurance or from the reduction in the number of uninsured children. ε is assumed to follow a logistic distribution so equation (1) is estimated using a logit model. Standard errors are clustered by state to account for possible serial correlation over time within states. All estimates use sample weights.

The vector *X* contains demographic variables that can have independent effects on the demand for insurance coverage. For child characteristics, I include gender, race, the number of siblings, age and the relation to the household head. Parent's characteristics include age, education level, and employment information (i.e. whether either of parent works at a firm of equal or more than 100 employees, or is self-employed.) When both the mother and the father of the child are present in the data, I use a higher value between them for parent’s age and education variables. When a child does not have parents or when the parents cannot be located in data, I use the household head's characteristics instead. Family characteristics include the number of workers in the family, income as a percentage of the federal poverty level, and whether a child has a single parent. Lastly, the unemployment rate by state/year is used to account for some time-specific state effects.

I include state fixed effects and year dummies. State fixed effects would capture different time-unvarying characteristics of the state that may affect the decision to get health coverage. Likewise, year dummies would capture nationwide effects in the health market such as an increase in the price of health care that induces more people on average to be covered by public insurance upon becoming eligible.

As shown in Table 2, about 6.3% of the sample reported more than one coverage: public and private. Some of them reported public and group private insurance while the others reported public and non-group private insurance. As such, an individual's insurance status is not defined as mutually exclusive over the three outcomes; *Public*, *Private* and *Uninsured*. It is not known whether being insured by more than one type of insurance indicates 1) a misinformed answer where one is not able to differentiate between public and private insurance, 2) being covered by more than one insurance, or 3) the transition from one insurance to another over the course of a year. To address this problem, I define the types of health insurance coverage more finely and report the estimates for six mutually exclusive outcomes of public only, group coverage only, non-group coverage only, both public and group coverage, both public and non-group coverage, and uninsured. This division of insurance type is similar to what Gruber and Simon (2008) used.

1.5 Results

The average marginal effects of *Fee* for six mutually exclusive outcomes that are estimated by the logit model are listed in Table 3.[9] The panel A shows the estimates by regressing insurance coverage (by type) on Medicaid-to-Medicare fee ratio (*Fee*), without state fixed effects. Panel B presents the estimates with state fixed effects. In interpreting the table, the increase in take-up of public insurance (Medicaid) will be shown as a positive estimate of marginal effect of *Fee* in column (1), (4) and (5), where in each case the dependent variables take value 1 if the respondent is covered by public insurance. Similarly, any evidence for crowd-out will be shown as a negative estimate in column (2), (3), (4), and (5), where the dependent variables take value 1 if the respondent is covered by any type of private insurance.

As reported in panel B, all the marginal effect estimates of *Fee* in column (1), (4) and (5) are positive. The effect of *Fee* on probability of reporting "public only" (column 1) is sizeable but not statistically significant at the 10% level and there is almost no effect on the probability of reporting "public and group private" (column 4).

The largest effect in "public and non-group private" (column 5) suggests this group is likely on the margin and may be more confused in reporting their coverage—i.e. they may be new to the public insurance so may be unsure of what type of

[9] The sample average of marginal effects of *Fee* using logit and linear probability model (LPM) are qualitatively similar, albeit the marginal effect in LPM tend to be smaller in magnitude. As an alternative approach followed by Lo Sasso and Buchmueller (2004), I estimated three-stage-least-square (3SLS) in which I impose restrictions that the coefficients of *Fee* across three equations sum to zero. Without the restriction, the "both public and private" group can be treated either as public or as private insurance. The restricted 3SLS essentially provides the weighted average of these two estimates that yield minimum variance. The resulting marginal effects again are only slightly different from the logit estimates.

coverage they have. This is consistent with the study by Lo Sasso and Buchmueller (2004) in which they conclude many people in the public insurance program believed that they were covered by non-group private insurance, particularly during the SCHIP period when some state programs looked more like private insurance. Cantor et al. (2007) also find that many public coverage enrollees reported having non-group private insurance coverage. Hence, I infer that the relevant insurance type for this group is public insurance, rather than private insurance. Later I also subsume "public and group private" (column 4) under overall "public" category rather than "private". In any case, the effect of *Fee* is almost none for this group, so the result would not be sensitive to the chosen category.

There is no strong evidence that higher *Fee* promotes crowd-out once state fixed effects are added, as the estimates in column (2) and (3) in panel B suggest.[10] Although the estimate in panel A in column (2) indicates a negative correlation between *Fee* and the probability of being covered by group private insurance, this is hardly an evidence for crowd-out since the estimates in panel A may be contaminated by unobservable fixed state characteristics. Rather, the effect on take-up seems to

[10] The results in column 2 and 3 in panel B show no significant effects of *Fee* on the probability of having private insurance. One explanation for this is that *Fee* indeed has no effect on the probability of having private coverage. Another explanation is that *Fee* affects private coverage in opposing ways yielding no net effect. For instance, a higher *Fee* may attract relatively unhealthy children with private insurance to take-up Medicaid, who see greater savings in doing so. The characteristics of the remaining pool of privately insured children would then consist of healthier children, and equilibrium premiums and deductibles would decrease. These lowered premiums and deductibles can increase the probability of being covered by private insurance.

I test this hypothesis by adding interaction term of *Fee* by self-reported health status, with the caveats that only two years of data are used (since information on health status is available after 1996) and that self-reported health status can be endogenous. I do not find any convincing evidence that higher *Fee* has a differential effect across health status. Therefore, although this alternative explanation is a possibility in the long run, and perhaps possible in small firms where pooling is limited, it seems unlikely given the short time span of my analysis and given that the sample group here is poor children who is unlikely to afford private health insurance.

come from the uninsured (column 6 in panel B). Unlike other types of insurance coverage, this “uninsured” category is less subject to the reporting problem, as people may get confused about what type of coverage they have but there should be much less confusion as to whether they had health insurance at all. Therefore, I expect the “uninsured” category to be most credible amongst all the outcomes.

There is a large drop in the probability of being uninsured when *Fee* increases within states (Column 6 in panel B). Putting this into a context, when Medicaid-to-Medicare ratio increases from current 65% to 100%, then the expected drop in uninsured rate is about 6.2 percentage points. Comparing column (6) across panel A and B indicates that states with a high level of the fee ratio also have unobserved tendency to have a high uninsured rate. For instance, these states may also offer more free clinics, so that patients are less willing to get health insurance. Therefore, omitting state fixed effects may lead to an upward bias of the estimate of *Fee* on the probability of being uninsured.

For simplicity, in Table 4 and the following tables, I use comprehensive measures of public and private insurance, by grouping all the public (public only, public and group private, public and non-group private) and private insurance (group private only and non-group private only) together. The results for the three main components of *Public*, *Private only* and *Uninsured* are listed in Table 4.

The empirical results support the prediction that increasing Medicaid payment relative to Medicare increases take-up. When counting overall take-up of Medicaid, a 10-percentage-point (i.e. equivalent to roughly a half of the standard deviation of *Fee*) increase in *Fee* raises the overall Medicaid take-up among poor children by 1.38 percentage points. Since the same increase in *Fee* has no significant effect on private insurance coverage but reduces the uninsured rate by 1.77 percentage points, most of the increase in take-up seems to come from those who would have been uninsured. The average uninsured rate among poor children in the sample is about 20%, so a 10-percentage-point change in *Fee* leads almost to a 10% reduction in the uninsured rate.

The rest of the rows in Table 4 describe the relation between insurance status and individual characteristics. Although controlling for these characteristics affects the marginal effect of *Fee* minimally (not reported) and many of the results should be interpreted as correlations rather than causal, I briefly discuss the general characteristics of those who do and do not enroll in Medicaid. Whites are less likely to be covered by public insurance than non-Whites. Having more siblings is associated with higher take-up and lower uninsured rate, probably because enrolling kids in public health insurance provides greater net value in total when the marginal cost of enrolling an extra child is small. Older children are less likely to be covered by public insurance and more likely to be either privately insured or uninsured.[11]

Parent characteristics are also important factors for the child's insurance choice. The omitted employment group of the parent characteristics in Table 4 is non-working parents. Typically, parents who work at a large firm are more likely to insure a child with private insurance, as the majority of employees in large firms get group private insurance through their employer. The probability of being uninsured is higher when

[11] I also estimate using dummies for each age but it does not affect the marginal effect of *Fee* at all.

parents work in a small firm or when they are self-employed than when they work at a large firm. In particular, the uninsured rate is higher even when compared to the children whose parents do not work. This perhaps is because the parents who work in a small firm or who are self-employed face a higher opportunity cost of time than non-working parents, while they are not adequately provided with group private health insurance. Parents who work in a large firm face similarly high opportunity cost of time but they often have access to less-expensive group private insurance. It is therefore particularly important to provide public insurance to the children of the working poor whose employers do not offer group health insurance.

1.6 Robustness Tests

The basic specification in section 5 may raise concerns about the assumption made about functional forms, the use of one-year lag structure of *Fee*, and other time-varying state characteristics that may also correlate with *Fee*.

Although not reported, the results are robust to restricting the samples to be below some arbitrary multiples of the federal poverty line, such as 75% and 125%.[12] When I run a multinomial logit using a dependent variable that takes an integer for each *Public*, *Private only* and *Uninsured*, where this categorical outcome follows a multinomial distribution, the average marginal effect of *Fee* is very similar to when I use a logit model. I also run the models including various controls such as interaction terms between year dummies and age dummies, and interaction terms between state

[12] The effect of *Fee* appears to be diluted little when using the federal poverty line of 125%, possibly because some children are not eligible for Medicaid.

fixed effects and age dummies. The marginal effect of *Fee* is qualitatively similar to the baseline specification, although the p-value of the effect of *Fee* becomes larger (about 0.12) in some specifications including both interactions between year and age fixed effects and interactions between state and age fixed effects.

1.6.1 Different lag structures

I rerun the results using different lag structures of *Fee* in order to address the concern that the time it takes to affect take-up may not be exactly one year. One potential issue with the March CPS data is the lack of clarity on the timing of the health insurance, and the most relevant lag structure highlighting the relationship between *Fee* and take-up may not be one year. Since I have *Fee* measure for three years (1993, 1998 and 2003), I relate these measures using samples from different years to construct *Fee* of a different lag. For instance, the contemporaneous *Fee* is obtained by merging *Fee* data with 1993, 1998 and 2003 and one-year lag is constructed by merging *Fee* with 1994, 1999, and 2004 population and so forth. Table 5 shows the result. Not surprisingly, *Fee* does not have a strong effect on take-up. In fact, if anything, it has a negative effect on take-up. Since the reported insurance status is from the same year as *Fee*, it is likely to indicate a negative correlation between *Fee* and take-up rather than causation. One-year lag shows the strongest relationship and the average marginal effect of *Fee* seems to deteriorate as a higher-year lag is used, with the expected signs.

1.6.2 Different types of fee ratio

There are four components in the fee index: all, primary care, obstetric care and other services[13]. As discussed earlier, I expect the fee index for primary care to be the most relevant fee for children and to be most useful in capturing the incentives physicians face when providing routine care.

Table 6 presents estimates of primary *Fee* when controlling for other types of fee ratio (I exclude the fee ratio for "all services" since correlation between fee ratios of primary and all services is above 0.9). I can use other fee indexes to construct a falsification test; to the extent that the primary *Fee* is not correlated with the included fee indexes, there should be no movement in take-up in response to a less relevant fee. Table 6 indicates that it is indeed the primary fee index that drives the results. Although not reported, I also reran Panel B and C excluding primary fee index. The fee indexes for obstetric and other services were still not significant predictors of all three outcomes.

1.6.3 Controlling for other time-varying state policies

The next few robustness checks address the possibility of selective timing in Medicaid fee changes in relation to other changes in health insurance policy that could affect the take-up decision.

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[13] The main health care services of which reimbursements are used to construct primary care fee index are office visits with new and established patients. The fee index for other services include the payments for initial hospital care, initial hospital consultation, some surgeries, imaging and laboratory tests.

1.6.3.1 The SCHIP eligibility expansion

The most notable change in the public health insurance market during the period of analysis is the expansion of public health insurance to children, which occurred through the creation of the State Children's Health Insurance Program (SCHIP). The effect of *Fee* on take-up among poor children can be influenced by increased demand from the children who become eligible for public health insurance after the expansion. For instance, it is possible that states with greater SCHIP expansion increase Medicaid payment to ensure that enough health care providers participate. To the extent that the SCHIP expansion affects both *Fee* and Medicaid take-up, omitting the measure of SCHIP expansion would bias the effect of *Fee*.

I discuss possible channels through which the expansion affects the take-up of Medicaid among poor children. The SCHIP expansion would increase the demand for public health care because of many newly eligible children. Unless total health care supply in the market responses to accommodate this increased demand, which is likely in a short term, the expansion may decrease take-up of Medicaid. The reduction on take-up of Medicaid would be accentuated if physicians prefer taking SCHIP-children, who are wealthier than Medicaid-children. On the other hand, Medicaid take-up may increase if the SCHIP expansion increases overall market supply (to both Medicaid and SCHIP children) more than enough to accommodate all newly and previously eligible children. This can be done either by having more physicians relocate to the areas with the greater expansion or by increasing existing physicians' supply.[14] Lastly, if expansions result in potential coverage of children with greater (lesser) health needs, health insurance is more (less) valuable to the family so take-up propensity would be affected. I cannot test a specific channel through which the expansion operates. Here, I discuss the net effect of the expansion only.

In order to construct a proxy for the demand for public health insurance, I use state-year-age level variation in the eligibility income threshold. This measure is constructed by applying each state-year-age eligibility policy to all children under 18 and calculating the fraction of eligible children in each state and year (actual eligibility rate). This measure would closely capture actual demand. However, if the state's policy is in part driven by underlying changes in demographic characteristics that are also correlated with health insurance status—i.e. states increase eligibility rates in response to the growing uninsured rate—this measure will be endogenous. I therefore also construct a simulated eligibility rate by applying the state's eligibility policy in each year to a *constant* sample of children in 1993. This in effect is a calculation of the portion of children who would have been eligible had the population characteristics remained the same as those in 1993. Using this fixed sample, I let variation solely come from changes in policy, not from changes in population characteristics. Both the actual and simulated eligibility rates have greatly increased over time, especially after 1997 when the SCHIP was adopted.

[14] In fact, Perloff et al. (1997) found that the Medicaid eligibility rate is positively associated with the average percent of patients covered by Medicaid among participating physicians.

The first three panels A, B, and C in Table 7 show the marginal effect of *Fee* after controlling for the combinations of actual and simulated eligibility rate by state and year. In all three cases, the average marginal effect of *Fee* is very similar to the basic estimates in panel B in Table 4. The simulated eligibility rate is associated with the reduction of the uninsured rate among poor children while the actual eligibility rate is not.

1.6.3.2 Changes in enrollment procedures

The next three panels D, E, and F in Table 7 report the results after controlling for time-varying state policies that attempt to outreach potential Medicaid enrollees. There are a few examples of such policies, such as presumptive eligibility, asset and income verification requirements, requirements for face-to-face interviews, and waiting periods. Of all the policies that may affect Medicaid take-up, I could find only two policies—requirement for asset test and presumptive eligibility—that meet the data requirements (i.e. available for the same years as the data on Medicaid-to-Medicare fee ratios).

When the indicator of whether there is asset requirement is included (Panel D and E in Table 7), the estimated average marginal effect of *Fee* on take-up of public insurance does not change much but loses its precision compared to the baseline specification in Table 4. When presumptive eligibility is included, *Fee* is still statistically significant at 10%. In all cases, *Fee* is a significant predictor of probability of being uninsured. The results shown here may not perfectly eliminate omitted variable bias, but certainly mitigate some concerns about the omitted variable bias.

1.7 Policy implications and conclusion

Even though the existing literature and anecdotal evidence suggest that Medicaid's low payment hurts physician incentives to treat Medicaid patients, relatively little is known about the role of access to care on the take-up of public health insurance. In this paper, I use the Medicaid-to-Medicare fee index for primary care services (in 1993, 1998, and 2003) as a proxy for access to care to investigate the effect of Medicaid fees on the health insurance coverage. Understanding whether an increase in the Medicaid fee can be an effective policy lever to promote take-up is crucial in the current situation where states have substantial discretion over setting the fee paid to physicians and hospitals. Increases in fees have a beneficial effect on ensuring higher quality and more timely access to care, while reducing the uninsured rate.

The findings in this paper provide an additional dimension in explaining the puzzle of "eligible but not enrolled". I find that an increase in the Medicaid-to-Medicare fee index by 10 percentage points (about a half of standard deviation of the fee index) is associated with an increase in take-up by 1.38 percentage points and a decrease in the uninsured rate by 1.77 percentage points within the low-income population. About 41% of the 9 million uninsured children are in poverty (so are eligible for Medicaid), the findings indicate that a 10 percentage point increase in fee payments would lead about 51,000 low-income children to take-up Medicaid.[15]

Increasing Medicaid payment does more than simply encourage Medicaid take-up. The cost of insuring these children is incurred in the short term while the benefits of insuring children will accrue over time. Although it is hard to assess the long-term effects of increasing access to care, greater nutrition and health utilization during childhood are likely to affect human development outcomes, such as improvements in learning ability and productivity (Levine and Schanzenbach, 2009).

Although I do not find evidence that higher Medicaid fee promotes the crowd-out of private insurance among low income populations, the crowd-out may occur among a higher income group. This is particularly important since the SCHIP expansion reaches children in the higher income distribution who are more likely to afford private insurance. Therefore, to the extent that Medicaid fee is correlated with SCHIP fee, the natural next step that can be done is to evaluate the effect of the Medicaid-to-Medicare fee on take-up of SCHIP. For this higher income group, Medicaid or SCHIP is a substitute for private insurance, so the relative Medicaid fee could serve as a

[15] I did a back-of-the-envelope calculation on how much it takes to insure these children. The average Medicaid-to-Medicare fee payment ratio (at the state level) was 0.71 and the standard deviation was 0.19 in 2003. Increasing the fee index by 10 percentage points then requires the average-paying states to increase its fee ratio by 14%. In 2003, total Medicaid spending on physician services was 8.1 billion dollars (i.e. according to Financial Management Report for FY-2003 provided by Centers for Medicare and Medicaid Services, national total Medicaid expenditure on physicians' services was 8,116,481,480 dollars). In 2002, children incurred 18% of the total Medicaid expenditures (Source: The Medicaid Program at a Glance, Jan 2004, Kaiser Family Foundation). Roughly speaking, then it takes 204 million dollars to cover 51,000 children, or about 4,000 dollars per child. Movement of care from hospital-based settings (outpatient and emergency departments) to physician offices might offset some part of the costs since fees for care in hospital-based settings tend to be higher. Certain assumptions are made in the calculation. Among others, these are: (1) the fee increases are directed to children only (i.e. instead of the elderly and disabled), (2) Medicaid fee is the only policy instrument used in reducing the number of poor uninsured children, and (3) spending on marginal enrollees are the same as spending on the average enrollees.

measure of the relative quality of public over other types of insurance, rather than access to care. Examining the relation between Medicaid fee and the extent of crowd-out among higher income group reached by the SCHIP expansion would be a fruitful topic for future research.

Table 1.1: Access to care across health insurance status

	Medicaid	Medicare	Private	Uninsured
(1) Problem with getting a referral to a specialist	0.40	0.18	0.21	---
(2) Problem with delayed health care while waiting for approval	0.87	0.69	0.55	---
(3) Needed approval for any care, tests or treatment	0.30	0.13	0.25	---
(4) Usual place of care				
Doctor office	0.51	0.63	0.74	0.32
Hospital outpatient clinic	0.11	0.07	0.04	0.07
Other clinic or health center	0.22	0.11	0.08	0.16
Hospital emergency room	0.04	0.02	0.01	0.07
Other	0.03	0.07	0.05	0.03
No usual source of care	0.09	0.10	0.08	0.35
(5) Time spent waiting until seen by medical professional at last doctor visit, measured in minutes	35.25	26.13	23.26	40.05
(6) The lag time between making the appointment and the last doctor visit, measured in days	14.45	29.64	22.07	17.73
(7) Travel time at last doctor visit, measured in minutes	20.80	21.05	18.08	21.64

Notes: Author's calculation using Community Tracking Study Household Survey 2003. The reported values from (1) to (4) are the fraction of respondents who agree to the stated question given their insurance status shown in each column. The numbers from (5) to (7) report the mean given insurance status. Medicare, private insurance and no insurance have values statistically significantly different from Medicaid at the 5% significance level in all cases except for (7).

Table 1.2: Sample means of children in poverty

	1994	1999	2004	Total
Dependent Variable				
Public	0.667	0.585	0.664	0.642
Public only	0.604	0.525	0.598	0.579
Public and Group private	0.033	0.039	0.031	0.034
Public and Non-group private	0.029	0.021	0.035	0.029
Private only	0.136	0.174	0.16	0.155
Group private only	0.083	0.12	0.103	0.1
Non-group private only	0.052	0.055	0.057	0.055
Uninsured	0.198	0.241	0.176	0.203
Public and Private	0.062	0.06	0.066	0.063
Time varying state characteristics				
Medicaid-to-Medicare Primary fee index, lagged (*Fee*)	0.706	0.579	0.637	0.646
	(0.186)	(0.180)	(0.162)	(0.184)
Unemployment Rate	5.584	4.149	5.319	5.075
	(1.233)	(0.774)	(0.868)	(1.169)
Actual eligibility (for Medicaid and SCHIP)	0.084	0.336	0.377	0.255
	(0.094)	(0.124)	(0.092)	(0.169)
Simulated eligibility (for Medicaid and SCHIP)	0.1	0.377	0.425	0.288
	(0.087)	(0.102)	(0.068)	(0.171)
No asset requirement, lagged	0.813	0.946	0.855	0.866
Presumptive eligibility, lagged	0.739	0.623	0.707	0.694
Child Characteristics				
Female	0.493	0.5	0.489	0.494
White	0.613	0.627	0.631	0.623
Num. of Siblings	1.746	1.732	1.622	1.701
Age	4.84	5.174	5.052	5.008
*Parent Characteristics**				
Age	32.656	33.27	33.99	33.276
Above High School	0.253	0.267	0.314	0.277
Work	0.595	0.739	0.674	0.664
Work at a firm with less than 100 Emps	0.28	0.386	0.334	0.329
Work at a firm with more than 100 Emps	0.281	0.313	0.283	0.291
Self Employed	0.07	0.059	0.078	0.069

Table 1.2: Sample means of children in poverty, continued

	1994	1999	2004	Total
Family Characteristics				
Num. of Workers	1.069	1.262	1.171	1.159
Family Size	4.673	4.638	4.625	4.647
More than one family in the household	0.132	0.168	0.16	0.152
Single Mother	0.591	0.573	0.576	0.581
Single Father	0.051	0.049	0.056	0.052
Do not live with own parent	0.054	0.071	0.081	0.068
Income in % FPL	49.347	50.038	47.817	49.046
	(28.802)	(30.830)	(32.687)	(30.733)
Number of Observations	6488	4321	7826	18635

Notes: Data source is from the March Current Population Survey 1995, 2000, 2005 but since respondents are asked about insurance status for prior years, their insurance status refers to 1994, 1999 and 2004. Standard deviation for continuous variables is shown in parenthesis. Samples are weighted using a person-level weight: the inverse probability of selection into the sample.

* When both mother and father are present, I use higher age and education level between two. When parents of a child cannot be identified, in which case I infer as the child not living with parents, I use the household head's characteristics instead.

Table 1.3: The effect of Medicaid-to-Medicare fee ratio on health insurance coverage: all outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Pub only	Grp Priv only	Nongrp Priv only	Pub and Grp Priv	Pub and Nongrp Priv	Uninsured
	Panel A: Without State FE					
Fee	-0.038	-0.036*	0.013	-0.006	0.022**	0.043
	(0.042)	(0.019)	(0.021)	(0.011)	(0.010)	(0.050)
	Panel B: With State FE					
Fee	0.055	0.020	0.020	0.003	0.096**	-0.177***
	(0.075)	(0.050)	(0.037)	(0.029)	(0.045)	(0.062)

Notes: Average Marginal effects. All regression sample weights and standard errors are clustered by state. Robust standard errors are in parentheses. All regressions include individual characteristics and state/year fixed effects except for panel A, in which state fixed effects are not included. * p<0.10 ** p<0.05 *** p<0.01

Table 1.4: The effect of Medicaid-to-Medicare fee ratio on health insurance coverage: for three main outcomes

	(1) Public	(2) Priv only	(3) Uninsured
Fee	0.138**	0.040	-0.177***
	(0.070)	(0.054)	(0.062)
Child Characteristics			
Female (d)	-0.006	-0.000	0.007
	(0.006)	(0.004)	(0.006)
White (d)	-0.052***	0.049***	0.005
	(0.013)	(0.008)	(0.011)
Num. of Siblings	0.022***	-0.005	-0.017***
	(0.004)	(0.004)	(0.004)
Age	-0.007***	0.003***	0.005***
	(0.001)	(0.001)	(0.001)
Parent Characteristics			
Age	-0.005***	0.004***	0.002***
	(0.001)	(0.000)	(0.001)
Above High School (d)	-0.093***	0.078***	0.014
	(0.010)	(0.011)	(0.011)
Work at Frim with <100 Emps (d)	-0.045***	-0.005	0.058***
	(0.017)	(0.013)	(0.012)
Work at Firm with >=100 Emps (d)	-0.068***	0.072***	-0.001
	(0.023)	(0.015)	(0.012)
Self Employed (d)	-0.159***	0.073***	0.080***
	(0.036)	(0.020)	(0.023)
Family Characteristics			
Num. of Workers	-0.053***	0.037***	0.014**
	(0.010)	(0.005)	(0.007)
Num. of Families > 1 (d)	-0.073***	0.013	0.066***
	(0.020)	(0.010)	(0.018)
Single Mother (d)	0.125***	-0.031***	-0.096***
	(0.017)	(0.011)	(0.014)
Single Father (d)	0.020	0.012	-0.031**
	(0.022)	(0.021)	(0.013)

Table 1.4: The effect of Medicaid-to-Medicare fee ratio on health insurance coverage: for three main outcomes, continued

	(1) Public	(2) Priv only	(3) Uninsured
Family Characteristics			
Don't Live with Parent (d)	0.139***	-0.099***	-0.037*
	(0.021)	(0.012)	(0.021)
Income in %FPL	0.000	0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Relationship to the head			
Grandchild (d)	-0.140***	0.038***	0.109***
	(0.020)	(0.013)	(0.021)
Relative (d)	-0.008	-0.125***	0.041
	(0.034)	(0.025)	(0.027)
Nonrelative (d)	-0.100***	0.115***	0.001
	(0.030)	(0.025)	(0.023)
Other			
Year== 1999 (d)	-0.041	0.025	0.014
	(0.027)	(0.017)	(0.020)
Year== 2004 (d)	0.042**	0.013	-0.053***
	(0.021)	(0.011)	(0.018)
Unemployment Rate	-0.004	-0.004	0.006
	(0.013)	(0.006)	(0.013)
Mean dependent variable	0.644	0.159	0.197

Notes: Average Marginal effects. All regression sample weights and standard errors are clustered by state. Robust standard errors are in parentheses. State fixed effects are included. Omitted group for parent's work status is non-working parents. * p<0.10 ** p<0.05 *** p<0.01 (d) a dummy variable

Table 1.5: Different lag structures

	(1) Public	(2) Priv only	(3) Uninsured
Pri Fee t (n=19631)	-0.096	0.028	0.078
	(0.109)	(0.064)	(0.070)
Pri Fee t-1 (n=18635)	0.138**	0.040	-0.177***
	(0.070)	(0.054)	(0.062)
Pri Fee t-2 (n=16808)	0.096	-0.078	-0.033
	(0.083)	(0.070)	(0.048)

Notes: Average Marginal effects. All regression sample weights and standard errors are clustered by state. Robust standard errors are in parentheses. All regressions include individual characteristics and state/year fixed effects. * p<0.10 ** p<0.05 *** p<0.01.

Table 1.6: Different types of the fee ratio

	(1) Public	(2) Priv only	(3) Uninsured
		Panel A	
Primary	0.170**	0.051	-0.229***
	(0.077)	(0.065)	(0.066)
Obstetric	-0.019	-0.052	0.074
	(0.112)	(0.057)	(0.090)
Other	-0.113	0.051	0.084
	(0.109)	(0.071)	(0.081)
		Panel B	
Primary	0.148**	0.060	-0.214***
	(0.075)	(0.062)	(0.066)
Obstetric	-0.032	-0.047	0.085
	(0.105)	(0.056)	(0.086)
		Panel C	
Primary	0.163**	0.032	-0.201***
	(0.076)	(0.060)	(0.064)
Other	-0.118	0.041	0.102
	(0.100)	(0.076)	(0.073)

Notes: Average Marginal effects. All regression sample weights and standard errors are clustered by state. Robust standard errors are in parentheses. All regressions include individual characteristics and state/year fixed effects. * p<0.10 ** p<0.05 *** p<0.01.

Table 1.7: Controlling for other time-varying state policies

	(1) Public	(2) Priv only	(3) Uninsured
		Panel A	
Fee	0.128*	0.035	-0.170***
	(0.072)	(0.050)	(0.058)
Simulated eligibility	0.183	0.041	-0.250
	(0.141)	(0.124)	(0.172)
Actual eligibility	0.028	-0.127*	0.133
	(0.090)	(0.075)	(0.124)
		Panel B	
Fee	0.127*	0.045	-0.172***
	(0.071)	(0.052)	(0.063)
Simulated eligibility	0.207*	-0.069	-0.131
	(0.119)	(0.088)	(0.086)
		Panel C	
Fee	0.136*	0.038	-0.175***
	(0.075)	(0.055)	(0.062)
Actual eligibility	0.115	-0.108**	0.006
	(0.084)	(0.053)	(0.070)
		Panel D	
Fee	0.127	0.032	-0.155**
	(0.081)	(0.056)	(0.071)
No asset requirement	0.009	0.016*	-0.020
	(0.018)	(0.009)	(0.016)
Presumptive eligibility	-0.003	0.018	-0.007
	(0.048)	(0.024)	(0.022)
		Panel E	
Fee	0.128	0.030	-0.154**
	(0.080)	(0.057)	(0.070)
No asset requirement	0.010	0.014	-0.019
	(0.019)	(0.009)	(0.017)
		Panel F	
Fee	0.136*	0.043	-0.178***
	(0.072)	(0.053)	(0.063)
Presumptive eligibility	-0.005	0.015	-0.003
	(0.048)	(0.026)	(0.022)

Notes: Average Marginal effects. All regression sample weights and standard errors are clustered by state. Robust standard errors are in parentheses. All regressions include individual characteristics and state/year fixed effects. * p<0.10 ** p<0.05 *** p<0.01.

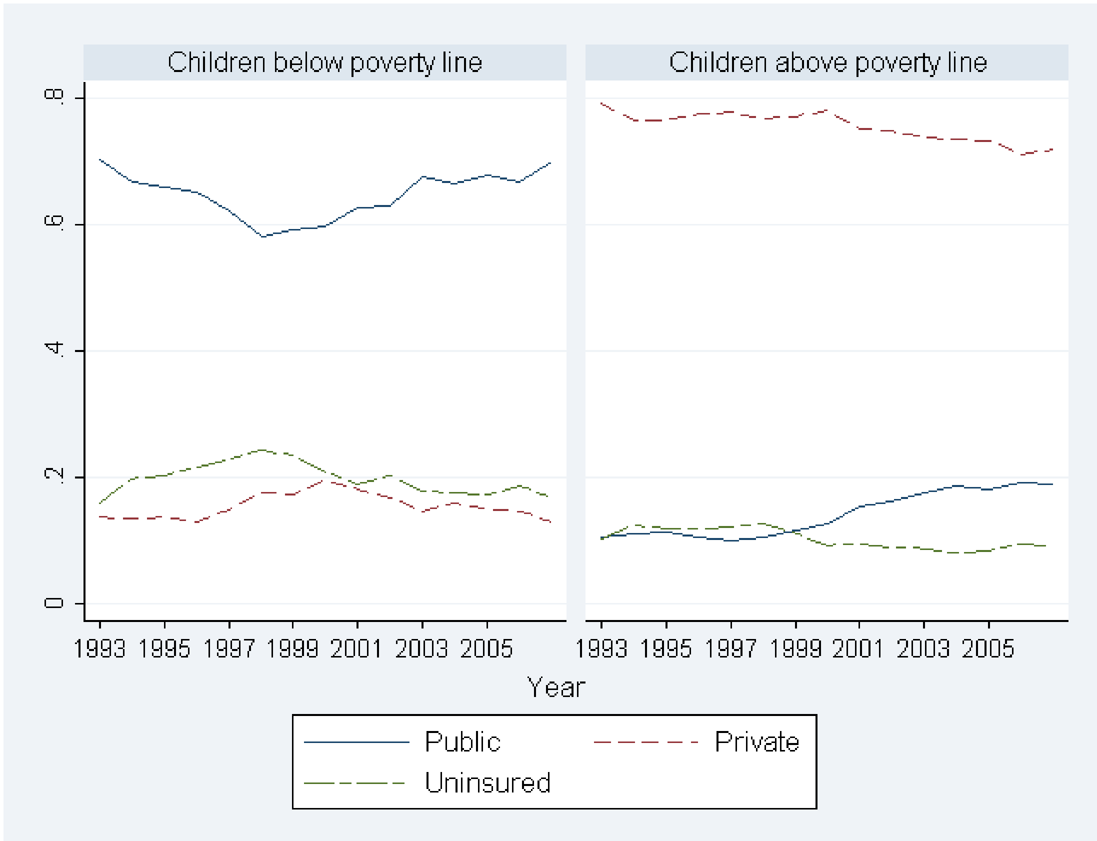

Figure 1.1: Children's insurance coverage between 1993 and 2007 by poverty status

Notes: Calculated using March CPS 1994-2008. Children below 12 years old in each year are used in the calculation.

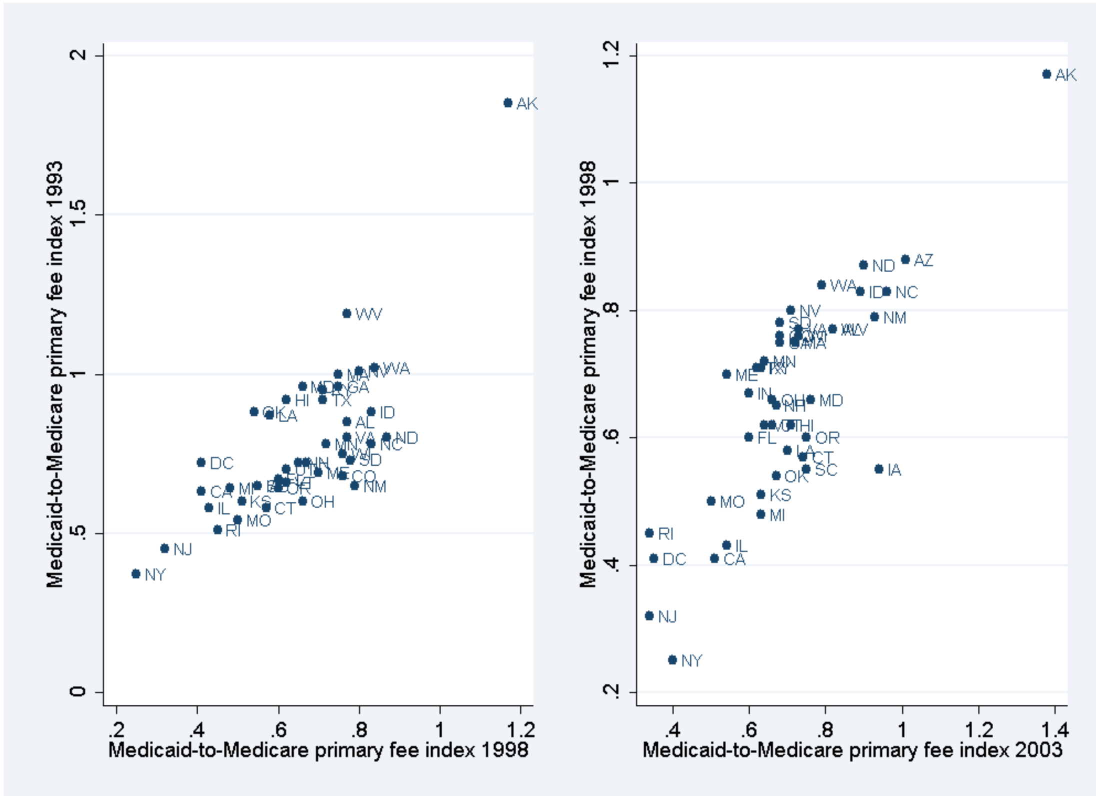

Figure 1.2: Medicaid-to-Medicare primary fee index in 1993, 1998, and 2003

Sources: Urban institute. See Norton (1995) for documentation for the 1993 index, Norton (1999) for 1998, and Zuckerman et al. (2004) for 2003. Medicaid-to-Medicare fee indexes were not available in several states. These states are: Tennessee in 2003, Arkansas, Delaware, Mississippi, Montana, Nebraska, Pennsylvania, Tennessee, and Wyoming in 1998 and Arizona and Tennessee in 1993.

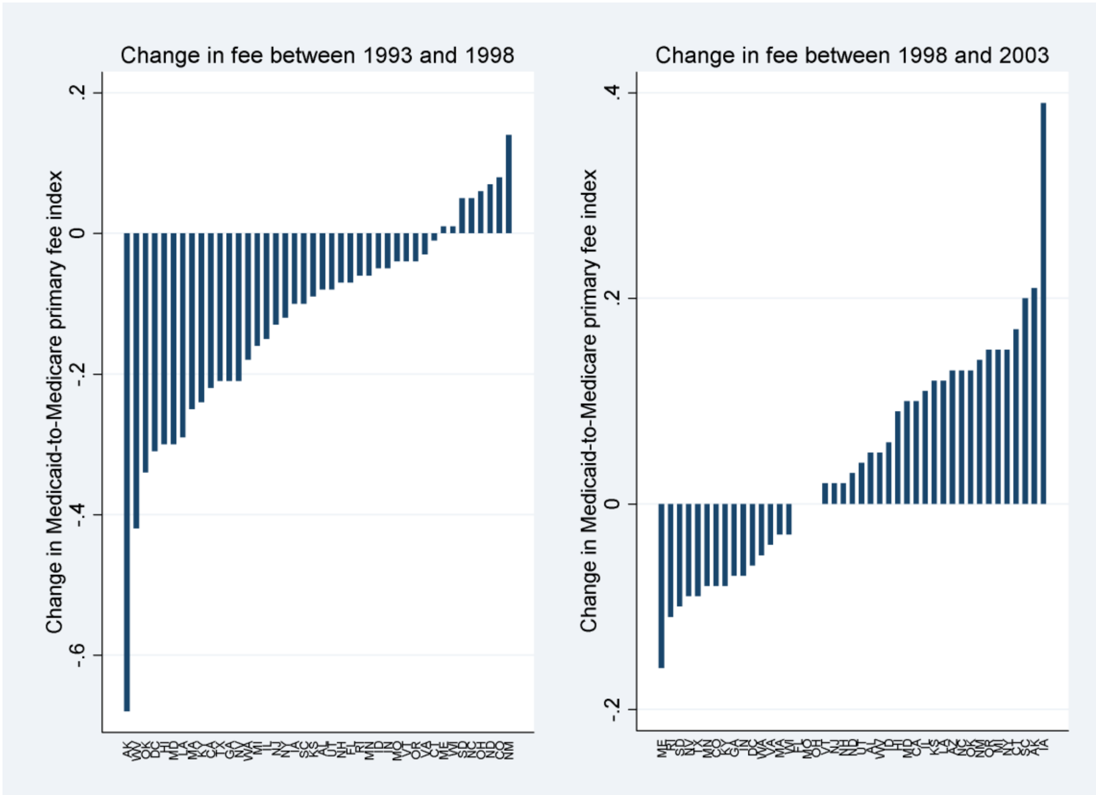

Figure 1.3: Changes in Medicaid-to-Medicare primary fee index

Sources: Urban institute. See Norton (1995) for documentation for the 1993 index, Norton (1999) for 1998, and Zuckerman et al. (2004) for 2003. Medicaid-to-Medicare fee indexes were not available in several states. These states are: Tennessee in 2003, Arkansas, Delaware, Mississippi, Montana, Nebraska, Pennsylvania, Tennessee, and Wyoming in 1998 and Arizona and Tennessee in 1993.

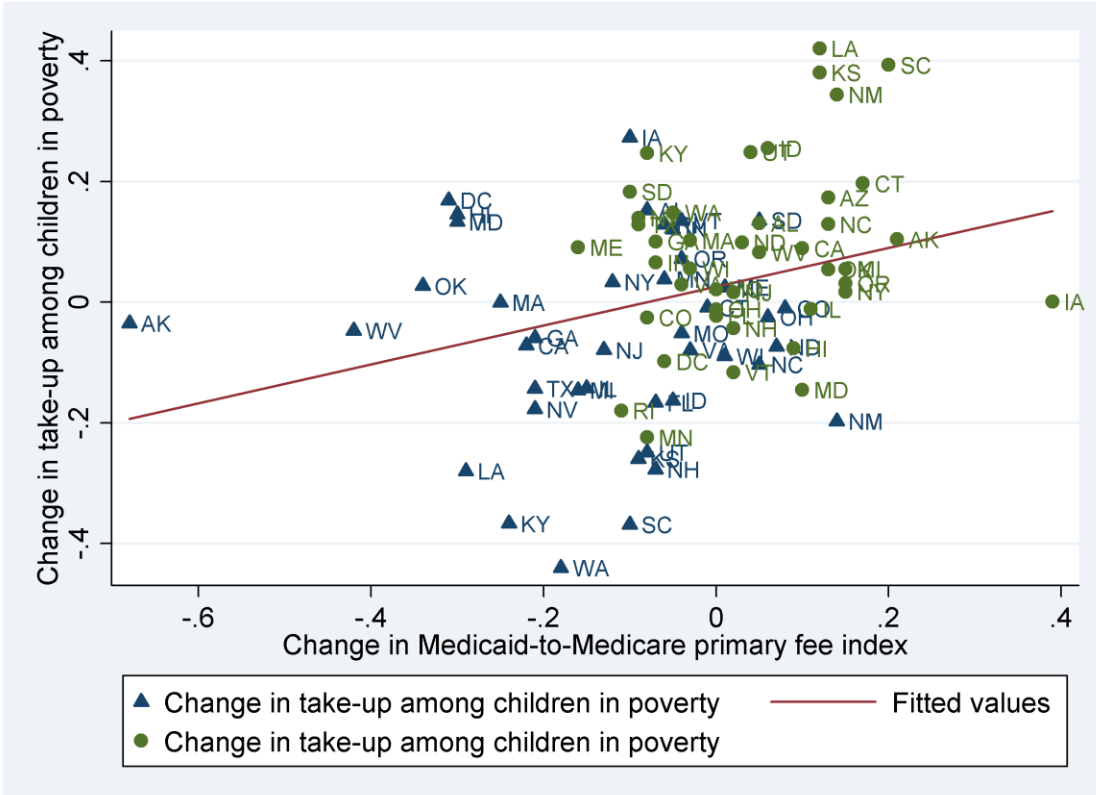

Figure 1.4: Changes in take-up and changes in the Medicaid-to-Medicare primary fee index

Notes: Slope of the linear regression of change in take-up on change in Medicaid-to-Medicare primary fee index is 0.322 and standard error is 0.113. ▲: Changes between 1993 and 1998. ●: Changes between 1998 and 2003

Sources: Urban institute. See Norton (1995) for documentation for the 1993 index, Norton (1999) for 1998, and Zuckerman et al. (2004) for 2003. Medicaid-to-Medicare fee indexes were not available in several states. These states are: Tennessee in 2003, Arkansas, Delaware, Mississippi, Montana, Nebraska, Pennsylvania, Tennessee, and Wyoming in 1998 and Arizona and Tennessee in 1993.

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Chapter 2: Do Work Decisions among Young Adults Respond to Extended Parental Coverage?

Abstract

Young adults aged 19-29 are significantly less likely to have health insurance since most family insurance policies cut off dependents when they turn 19 or finish college. In recent years, several states have expanded eligibility to allow young adults as old as 30 to remain covered under their parents' employer-provided health insurance. For those who qualify for these benefits, the expansion of parental coverage partially reduces the value of being employed by a firm that provides health insurance since adult children can now get health insurance through another channel. We employ quasi-experimental variation in the timing and generosity of states' eligibility rules to identify the effect of the policy change on young adults' labor market choices. Our results suggest that the expansion of parental coverage increases the group coverage rate and reduces labor supply among young adults, particularly in full-time employment.

2.1 Introduction

A strong linkage between health insurance and labor supply is often observed in the United States, as almost 88 percent of private insurance coverage comes through employment.[16] Given the high costs of obtaining coverage for young adults entering the labor market, health insurance could be a critical factor in labor supply decisions. Although numerous empirical studies on health insurance and labor market outcomes have been conducted, evidence on young adults is sparse if not absent.[17]

Young adults are least likely to have health insurance; only 66 percent of 22-year-olds in the U.S. have health insurance, the lowest rate of coverage among the entire population.[18] There are several reasons why young adults lack health insurance coverage. First, young adults lose dependent coverage status under a parent's private plan upon turning 19 or finishing college (up to 24). Also, most young adults after age 19 have limited access to public health insurance; eligibility for public health insurance such as Medicaid is generally restricted to very low income families or disabled adults.[19] In addition, due to their limited work experience, they often do not have an affordable employer-sponsored insurance option. Combined with high premiums for individual health insurance plans, this lack of access to coverage has made young adults the most likely to be uninsured compared to any other age group.

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[16] Income, Poverty, and Health Insurance Coverage in the United States: 2007, U.S. Census Bureau, 2008

[17] Gruber and Madrian (2002) provide a more complete review of this broader literature. Many studies have investigated the link between health insurance and labor market outcomes of other cohorts such as the elderly or lower income single mothers. More recently, Bansak and Raphael (2008) and Hamersma and Kim (2009) have examined the effect of expanding public insurance on job mobility among working parents.

[18] Annual Social and Economic (ASEC) Supplement, U.S. Census Bureau, 2008

[19] The State Children's Health Insurance (SCHIP) is available up to 19 years old.

As a result, several states have expanded the eligibility to young adults to be covered under their parents' employer-provided health insurance. Those states had taken measures to expand dependent coverage to young adults with qualifications based on age, marital status, student status, or financial dependence, which vary from state to state. This expansion provides a safety net for young adults who might not otherwise find affordable coverage in the individual insurance market, reducing risks commonly experienced at key transitions such as graduation. Since the state laws were recently implemented, there are only a few studies that have investigated the potential effects on economic outcomes. For instance, Levine et al. (2011) examine the impact of expanded eligibility on insurance coverage and find that the extended parental coverage laws increased health insurance coverage among young adults.

While these laws may help some young adults stay insured, they might have side effects. In the absence of the intervention, obtaining coverage for young adults has been difficult unless they have jobs that offer group private insurance. Thus, those who value health insurance but do not have access to health insurance other than through employment may have an incentive to work. As the implicit value of employment that provides health insurance decreases, we also expect an effect on the participation margin; those who become eligible for extended parental coverage may be less likely to work full-time.

In this paper, we examine the labor market effects of allowing young adults to remain covered under their parent's health insurance plan. Little is known about how access to health insurance affects young adults' labor supply and labor market choice.

The objective of this study is to describe the association between outside health insurance availability and labor market outcomes among young adults. In particular, this project investigates whether this recent intervention in the private health insurance market discourages young adults from participating in the labor market. We employ quasi-experimental variation in the timing and generosity of states' eligibility rules to identify the effect of the policy change on young adults' labor supply.

An analysis of state experiences with extended parental coverage informs the question of the effectiveness of federal health care reforms. The recently-passed federal health reform allows young adults to stay on their parents' plans longer by expanding dependent eligibility until age 26 (effective in late 2010), with the goal of reducing the uninsured rate among this group. It is worth asking whether this expansion has the intended effects on the young adults' health insurance coverage and how it affects their labor market outcomes.

2.2 Background

2.2.1 Expansion of parental health insurance

Young adults between the ages of 19 and 29 constitute the largest segment of the uninsured; they represent nearly one in three uninsured people in 2008, totaling approximately 13.7 million (Collins et al., 2010). In order to help those young adults stay insured, several states have expanded eligibility and allowed them to remain covered under their parent's employer-provided health insurance.[20] As of 2009, 27 states have passed laws that increased the age of dependency for insurance purposes. As shown in Table 1, the laws vary across states in the requirements for coverage. The age limit varies considerably across states, ranging from age 24 in Delaware, Indiana, Louisiana and Tennessee to age 30 in New Jersey and New York. The laws also vary by how they define dependent young adults. Some laws are restricted to full-time students, financially dependent young adults, young adults residing in the same state as their parents, unmarried young adults or young adults without their own dependents. In six states, young adults are required to be full-time students to be covered through their parent's plan.

The expansion, however, does not apply to employers that provide health benefits directly to their employees (self-insured firms). Under the Employee Retirement Income Security Acts of 1974 (ERISA), the state-level regulations apply only to employers that purchase insurance through a carrier (i.e., fully-insured plans), which are likely to be small firms. Currently, 55 percent of Americans covered by employer-based health insurance are in self-insured plans.[21] The implication for our study is that we can examine the effect of the expansion for those who are more likely

[20] The enactment of the Affordable Care Act of 2010 promises to cover approximately 32 million uninsured people over the next 10 years, including the majority of uninsured young adults. This includes the ability to remain on a parent's health plan up to age 26 beginning in September 2010. The reform applies to all adult children regardless of living situation, degree of financial independence, or marital or student status. But our research does not focus on the impact of the national reform signed in 2010 and instead, we focus on state extensions enacted before 2010.

[21] Kaiser/HRET Survey of Employer-Sponsored Health Benefits, 1999–2007. In firms with 5,000 or more employees, 86 percent of workers were covered by self-insured arrangements, while in firms with fewer than 200 employees, only 12 percent were covered by a self-insured plan in 2007.

to be affected by the law by restricting the sample to young adults whose parent works at a small firm and has private group health insurance.

For young adults, the extension of dependent coverage provides an attractive alternative to purchasing insurance through own employer or a student plan or remaining uninsured. Before the expansion of parental coverage, young adults who were not full-time students were more likely to be uninsured, following graduation from high school. Even though young adults enter the labor market after high school, they may have difficulty in finding a job with health benefits since the jobs available to them—jobs at small firms, with low wages, or part-time or temporary—are least likely to have health benefits. College graduates face risks similar to those faced by high school graduates; the insurance protections through a parent's employer policy or a student health plan are lost upon graduation and jobs offering health benefits are limited. Nearly half of employed young adults have jobs that do not provide health insurance coverage.[22] Young adults are also more likely to work in small businesses, which are less likely to offer coverage.[23] Therefore, the laws should allow young adults to easily be covered by health insurance under their parents' plan, even if they enter the labor market instead of being full-time students.

[22] Current Population Survey, March 2008

[23] Indeed, the likelihood of offering health insurance coverage among small firms (those with fewer than 10 employees) has decreased substantially, from 57 percent in 2000 to 49 percent in 2008 (Employer Health Benefits Survey by Kaiser Family Foundation).

2.2.2 Health Insurance and Labor Supply

Individuals who value health insurance but do not have access to health insurance other than through their own employment may have worked longer hours than they would otherwise, even though their marginal value of leisure exceeds that of their marginal product of labor. This is because health insurance premiums for individual plans are very high compared to a group plan, and group coverage is typically offered with full-time employment. The fact that employers tend to restrict health insurance benefits to full-time workers results in a non-convex budget constraint; once they work enough hours to be classified as full-time, workers receive health insurance and the portion of their budget constraint jumps up vertically by the consumption value of the insurance (Buchmueller and Valletta, 1998).

For young adults who worked full-time even though they would prefer to work fewer hours in the absence of the kink in the budget constraint, the expansion would effectively smooth out the kink in the budget set. In fact, the availability of parental health insurance is like an increase in unearned income, and the pure income effect predicts young adults would decrease total work hours. Also, the likelihood of working at a large firm, as opposed to working at a small-sized firm, being self-employed or not working at all, may decrease upon becoming eligible for extended parental coverage.

There is a related literature that examines the effect of spousal health insurance on own labor supply (see Gruber and Madrian 2002; Currie and Madrian 1999 for a more complete literature review). These studies indicate a strong negative effect of spouse's health insurance on own probability of working full time, either assuming that spousal health insurance and own labor supply are exogenous or using instrumental variables approaches.[24] Our estimation strategy, in which variation in the availability of alternative insurance is driven by a clear policy intervention, is less subject to the problem of assuming exogeneity of an alternative source of health insurance.

2.3 Empirical Strategy

We use a simple difference-in-differences approach where we compare the pre- and post-law change in insurance coverage and labor market outcomes of those who are and are not affected by the policy intervention.

$$Y_{ist} = \alpha + \beta_0 Treated_{is} + \beta_1 Treated_{is} \cdot Post_{st} + \beta_2 Post_{st}$$
$$+\beta_3 X_{ist} + \beta_4 Unemp_{st} + State_s + Year_t + Age_i + \varepsilon_{ist}\,, \qquad (1)$$

where *Y* refers to an outcome variable for individual *i* in state *s* at year *t*. We look at outcomes of health insurance coverage as well as labor market outcomes such as overall labor supply and full-time work. *Treated* is an imputed measure that defines

[24] Royalty and Abraham (2006) argue that even the studies that implement instrumental variables estimation, which typically predict spouse's health insurance using observable characteristics of the spouse such as human capital, are potentially biased if there is positive assortative mating on the observables. They mitigate this concern by using the degree of association between spouse's health insurance and own sick leave, which should not have any causal relationship, as a proxy for the extent of bias resulted from the assortative mating. After differencing out this bias, they find that spouse's health insurance has negative effects on the probability of working full-time at a firm providing health insurance.

who will be *newly eligible* after the policy change based on the state laws described in Section 2. Even before the coverage extensions, dependent status (and therefore eligibility) was not lost until age 24 as long as young adults continued schooling. In our analysis, we focus on *newly eligible* (= *Treated*), rather than *always eligible* (students who are age 24 or younger).[25] *Post* is a binary variable indicating whether the state and year are affected by the laws. The interaction term, $Treated \times Post$, is what we call *newly affected* group, who will be most likely to respond to policy changes. *X* is a vector of demographic characteristics that include student and marital status, female, having any children, whether residing with parents, and race dummies. *State, Year* and *Age* are state, year and age dummies, respectively. To the extent that *Post* is determined by some time-invariant state level conditions, (i.e. states that typically have had high uninsurance rate among young adults tend to adopt the laws earlier), state dummies will absorb such state differences. We also control for *Unemp*, the unemployment rate at the state and year level, in order to capture overall time-varying economic conditions for each state.

Empirically, the prediction is simple. As discussed in Section 2.2, if the expansion allows young adults to be covered by parental health insurance and lowers the incentive to work among the newly eligible, we expect β_1 to have a negative sign, especially when the dependent variable is working full-time. Whether young adults' labor market outcomes are indeed affected by the expansion is an empirical question which has not been explored before.

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[25] That is, individuals younger than 25 years old who are students are not newly eligible and *Treated* is imputed to be 0.

Our estimation strategy employs variation where the availability of alternative insurance is driven by a policy intervention. One concern with this strategy is that *Treated* may be endogenously determined if there is a behavioral response at the eligibility margin. Table 1 describes that generosity in eligibility is different across states, in terms of whether they allow young adults to be non-students, married, and to have dependents. As the intervention phases in, some young adults may find it advantageous to become eligible. We test the possibility of this behavioral response to the policy change by estimating the following regression equation:

$$\Pr(Z_{ist} = 1) = \sigma + \delta_0 Post_{st} + \delta_1 X_{ist} + State_s + Year_t + Age_i + \upsilon_{ist} \,. \qquad (2)$$

Equation (2) tests whether the young adults in the affected states changed their behavior in response to the state's eligibility rules. We are interested in the behavioral outcome *Z* that can be affected by the generosity of eligibility, which can be a binary variable of a young adult being a student, single, and having a child. We also estimate variants of the model in (2) using the lagged variable of *Post* and replacing *Post* with a variable that indicates the policy change relevant to *Z* only. For instance, four states (Indiana, South Dakota, Virginia and West Virginia) allow young adults to be married. When *Z* is being married, *Post* is multiplied by these four state dummies. In all regressions, the coefficient on *Post* is close to zero and statistically insignificant at the 10 percent level. We also run regressions where the dependent variable is having a parent who has private health insurance and works at a small firm, so that the young adults are affected even under ERISA. The coefficient on *Post* again is statistically insignificant.[26]

We also look at differential effects of *Treated* across gender on both health insurance coverage and labor supply. The effect of extended parental coverage on insurance status and labor market outcomes should be stronger among those who value insurance highly. Loosely speaking, health insurance coverage is probably more valuable for females than males. Gruber (1994) found that women of child-bearing age are likely to receive lower wages when several state and federal mandates made insurance cover childbirth costs. This finding implies that women of child-bearing age could face higher premiums for health insurance and lower wage jobs, if they were to get their own health insurance. Therefore, we expect to see greater effect for women compared to men.

2.4 Data

We use the 2001-2010 March Current Population Survey (CPS) data. The March CPS offers a variety of information on individual circumstances including health insurance status and labor market choices. In addition, it has a large sample size and allows for nationally representative estimates when using sampling weights. Among states that extended dependent insurance coverage, most adopted the laws between 1999 and 2009. Utah adopted its law exceptionally early in 1994. In order to

[26] The March CPS reports information on family members of only those residing within the same household. Thus, having parental information necessarily indicates that the young adults reside with their parents. We also estimate the regression equation (2) using a binary dependent variable of living with one's parents and find no effect of *Post*.

focus on a period when the majority of state actions occurred, our analysis ranges from 2000 to 2009, which is covered in the 2001-2010 CPS since insurance and employment information is dated back one year. We also exclude Massachusetts in our analysis since broader health insurance reform was implemented at the same time as the extended parental coverage law (Levine et al., 2011).

In our analysis we use the sample of young adults aged 19 to 24. In some states, the age limit is increased to 25 or older, but the March CPS does not have information on student status when a respondent is older than 24. Several other sample restrictions are made in the analysis. We exclude disabled individuals and those in the armed forces because they are eligible for other sources of health insurance and their work patterns are likely to be different from others.

Table 2 reports summary statistics for the sample of young adults. The first five outcomes relate to health insurance coverage. About a third of young adults in the sample are uninsured, which is very high compared to the overall uninsurance rate of 15 percent among the U.S. population. We sub-divide private insurance into three categories: group dependent, non-group dependent and own private health insurance. Group dependent coverage means a respondent is covered as a dependent on employment-based health insurance. Non-group dependent health insurance also requires the respondent to be covered as a dependent, but on a privately-purchased (non-employment-based) health insurance. Own private insurance indicates the young adult is covered by any private insurance (either group or non-group) as a policyholder for the health insurance. In terms of private coverage, most young adults are covered by either group dependent coverage or own private plan; 22 percent are covered by group dependent coverage and 31 percent had own private insurance plans, while only 2 percent are covered by non-group dependent insurance. 9 percent of young adults aged 19 to 24 are covered by public health insurance. Those on public health insurance, compared to those who are not, are likely to be married (30% vs. 23%), female (73% vs. 50%), and to have kids (54% vs. 19%).

In the sample about 72 percent are employed, which combines 51 percent of young adults who work full-time and 21 percent who work part-time. The remaining variables are the control variables in the main regressions. Those who are newly eligible constitute 26 percent of the sample (those newly affected, $Treated \times Post$, are about 8 percent of the sample). On average, 25 percent are enrolled as full-time students, whom we exclude in some regressions as they are always eligible for health insurance from parents and their labor market outcomes are heavily affected by just being a student. The majority of the young adults are still single and have no children.

Figure 1 shows the fractions of the newly affected by and the always eligible for extended parental health insurance over time. The always eligible population does not experience any time varying policy change, so their fraction is relatively stable around 25 percent. In contrast, the newly affected ($Treated \times Post$) sample, drastically increases over time, from zero percent in 2000 to 25 percent in 2009. The steep increase in slope around 2006 reflects the fact that the majority of states began implementing reforms in 2006.

2.5 Results

2.5.1 Health insurance coverage

We begin by discussing the effect of extended parental coverage laws on health insurance coverage. Table 3 presents results from the difference-in-differences approach in regression equation (1). All models are estimated by OLS. As shown in column (1), those who are newly affected by the law (*Treated*×*Post*) are 4.3 percentage points more likely to be covered by group dependent health insurance. More than 50 percent of the effect appears to come from a decrease in the likelihood of having own private health insurance, as shown in column (3). There is no evidence that those who are newly affected by the law are less likely to be uninsured as shown in column (5).[27] The probability of being covered by public insurance is not affected by the law; we find little evidence of 'reverse crowd-out', which can happen if the availability of group private insurance results in a switch from public to private insurance.[28] This finding suggests that there is no improvement on the extensive margin of increasing coverage for more young adults; rather, we see evidence of shifting between the types of insurance they get.

Table 4 examines how male and female young adults are differentially affected by the extended parental coverage. Generally, women are charged higher premiums

[27] This result is different from Levine et al. (2011), where they found that extended parental coverage was effective at increasing health insurance coverage. Our methodology is not the same, and we use more updated laws and include more recent samples in our analysis.

[28] The coefficients across the 5 models do not add to 0 because these health insurance outcomes are not defined to be mutually exclusive. The CPS asks whether a respondent holds a particular type of health insurance in the previous calendar year. Therefore, some respondents report possibly more than one type of health insurance if they switched insurance status some time during the previous year. We also estimated the regressions by assuming they have own private insurance rather than dependent private insurance in the case of having both insurance plans in the same year (and vice versa), and the results did not change much.

because as a group they tend to consume more medical services than men. Especially, women of childbearing age need more health care than men because of the combined demands of pregnancy and family planning. Partly as a result, young women typically pay more than young men for individual health insurance,[29] unless they live in one of 10 states where gender rating is illegal.[30] Therefore, women may have greater incentives than men to be covered by parental coverage instead of their own coverage. Our results show that women are indeed more likely to take advantage of this increased access to parent health insurance. The likelihood of being covered by group dependent coverage increases by 6 percentage points for women compared to 2.6 percentage points for men.[31] This increase in group dependent coverage also appears to largely come from a decrease in own private health insurance.

Table 5 shows the effects of parental coverage laws using two restricted samples. The first part is estimated using only non-students since we are eventually interested in examining the effect of expansion of parental coverage on labor market outcomes, and non-students are the relevant population. As health insurance is one channel through which labor market outcomes are affected, it is important to establish evidence that the law affects health insurance status for this group. As discussed, we do not find any change in the likelihood of being a student in response to law changes, so sample selection bias should not be a concern. The results show that those who are

[29] National Women's Law Center, 2008

[30] Maine, Massachusetts, Montana, Minnesota, New Hampshire, New Jersey, New York, North Dakota, Oregon and Washington prohibit the use of gender rating.

[31] We also restrict the sample to single females only, so that dependent coverage indeed means parental coverage rather than coverage obtained by spouse's health insurance. We obtain similar results.

not full-time students are more likely to be covered by group dependent health insurance than the overall young adults.

In the next set of columns of Table 5, we restrict the sample to young adults whose parents hold private health insurance and work at small firms with fewer than 100 employees. This restriction follows from ERISA, which exempts the self-insured firms (likely to be large firms) from the state-level regulations. As expected, we generally find that the law has a larger effect for youth whose parents work at small firms.

2.5.2 Labor market outcomes

In the previous section, we established the effects of the policy change on health insurance status, especially on the probability of being covered by group dependent health insurance. We now move on to discuss the effects of extended parental coverage on labor market outcomes.

The first panel of Table 6 presents the results with the full sample. The dependent variable in the first column of each panel is whether a young adult works or not. The law in general decreases overall labor supply. Together with the result in column (2), it shows that the decrease in the probability of working full-time is the main driver of this change; the likelihood of having a full-time job decreases (2.6 percentage points) more than that of working (2.2 percentage points), and the difference (0.4 percentage points) is the increase in the likelihood of working part-time. Lastly, as shown in column (3), young adults' propensity to work at a large size firm (thus more likely to be offered employer-provided health insurance) appears to be unaffected.

The second and third panels of Table 6 show the differential effects across gender. Columns (4) and (7) show that the decrease in employment is larger among men: 3.4 percentage points compared to 1.3 percentage points among women (not statistically significant). In terms of the probability of working full-time, shown in columns (5) and (8), the coefficient estimates are very similar although the point estimate is not statistically significant among males. We see a salient 2.8 percentage point decrease in the likelihood of working full-time among female young adults. A decrease in full-time work among women with no effect on overall labor supply suggests that women are likely to substitute away from full-time to part-time work. We again find no evidence that the law affects the probability of working at a large size firm for either men or women.

Table 7 estimates the effects of extended parental coverage laws for restricted samples. The first part shows the results for non-students. When students work, they tend to work part-time. In addition, they are always eligible for the policy change. Indeed, 38 percent of full-time students in our data worked part-time in the previous year, compared to only 16 percent of non-students. Since students' current labor market choice is likely to be very different from non-students, we obtain a more homogenous sample by limiting our analysis to non-students only. Compared to the full sample, we find a similar pattern among non-students; labor supply and full-time work decrease 1.8 and 2.3 percentage points, respectively. That is, those non-students newly affected by the law are less likely to work full-time, decreasing overall labor supply among this group. Lastly, the second panel in Table 7 uses young adults whose parent works at a small firm that offers health insurance benefits. Similar to the finding on health insurance in the previous section, we found a stronger effect (decrease in full-time work by 6.4 percentage points) among this group.

2.6 Conclusion

Young adults aged 19 to 29 are significantly less likely to have health insurance since most family insurance policies cut off dependents when they turn 19 or finish college. In recent years, several states have expanded dependent eligibility to young adults through age 24 to 30 with the intention of improving continuity of coverage for young adults as they transition from school to work in their early years. While these laws may help insure some young adults, they might have other unintended effects.

Without such laws, young adults have had difficulty in obtaining coverage unless they have a full-time job with employer-provided health insurance. Therefore, those who are in need of insurance coverage have an incentive to work or attend colleges. With the policy change, however, young adults might be less incentivized to enter the labor market or work full-time since they now have access to insurance coverage through their parents. Among those who qualified for expanded parental coverage, having continued access to health insurance through their parent’s plan partially reduces the value of being employed full-time. That is, the expansion potentially affects labor market choices by providing non-labor income.

This paper investigates the effects of recently expanded state-level dependent coverage laws on various health insurance and labor market choices. About half of the states introduced the extended parental coverage law, and we employ the quasi-experimental variation in the timing and generosity of states' eligibility rules to identify the effect of the policy change on young adults' health insurance status and labor market outcomes.

Our results suggest that the state-level expansions of parental coverage increase group dependent coverage among young adults by 4.3 percentage points. Moreover, female young adults appear to be more affected than males. This result is not surprising because women usually pay higher premiums and tend to value health insurance more than men. With easy access to health benefits, the extended parental coverage reduces young adults' labor supply by 2.2 percentage points and full-time work by 2.6 percentage points. Both men and women tend to reduce full-time work by about 2.8 percentage points (although it is statistically significant only among females). Our findings also suggest that women are more likely to substitute away from full-time work to part-time work.

We found no evidence that the expansions increased overall coverage. One possible explanation is that newly eligible young adults endogenously respond to the law changes by reducing their labor supply, decreasing the likelihood of obtaining private health insurance through their own employment. This behavioral change partially undermines the states' effort in increasing the health insurance coverage, as much of the increase in dependent private coverage was initiated by those who were previously covered. The laws were largely ineffective in attracting those who were previously uninsured, at least among the targeted group in our study.

We conclude with a discussion of the implications of the national health reform. Since young adults make up the largest share of the uninsured population, many states attempted to increase coverage by extending parental benefits. Recently-passed federal health care reform allows young adults to stay on their parents' plans longer by expanding dependent eligibility until age 26. While this reform may increase access to coverage for some young adults during a critical period of transition, it might have unintended effects. We find that the young adults are less likely to work full-time upon becoming eligible for parental health insurance, and this decrease in labor supply has feedback effects on health insurance market, mitigating the effect on increasing overall health insurance. This study suggests that such side effects should be considered in policy-making decisions.

There are other labor market outcomes that are yet to be explored. One is job-turnover. Rees (1986) notes that youth are vulnerable to layoffs and experience higher job turnover than older workers. Traditional 'job-lock,' where workers stay on the current job to secure fringe benefits like health insurance, might decrease (and job turnover increases) if extended parental coverage allows young adults to continue insurance coverage without working. Another conjecture is that the young adults' job choice sets are now less constrained, with implications beyond just the trade-off between working full-time and part-time. Would this alternative source of health insurance increase the demand for jobs that are not necessarily strongly attached to health insurance but offer other amenities, such as self-employment, free-lance type of jobs and other various part-time jobs? There are many important, unanswered questions, and we expect to see further fruitful discussions along the way.

2.7 Acknowledgement

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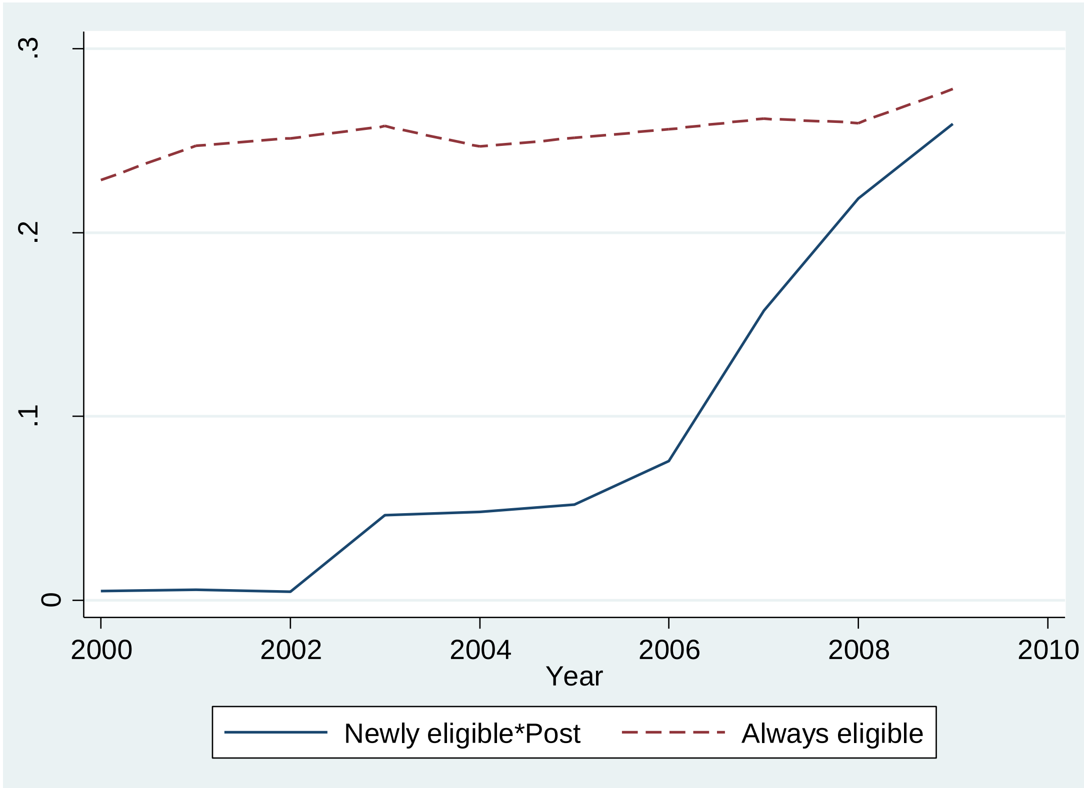

Figure 2.1: Fraction of the newly affected (*Treated*×*Post*) and the always eligible for extended parental health insurance

Notes: The newly affected population is calculated by *Treated*×*post*. Always eligible young adults are full-time students of age 24 or younger.

Table 2.1: Variations of State Dependent Coverage Laws

State	Year of passage	Age limit	Expansion		
			Non-student	Married	Dependents
Colorado	2006	25	●		●
Connecticut	2009	26	●		●
Delaware	2006	24	●		
Florida	2007	25	●		●
Idaho	2007	25			●
Illinois	2008	26	●		●
Indiana	2007	24	●	●	●
Iowa	2008	25			●
Kentucky	2008	25	●		●
Louisiana	2009	24			●
Maine	2007	25	●		
Maryland	2007	25	●		●
Minnesota	2008	25	●		●
Montana	2008	25	●		●
New Hampshire	2007	26	●		
New Jersey	2006	30	●		
New Mexico	2005	25	●		●
New York	2009	30	●		●
Rhode Island	2006	25			●
South Dakota	2005	29		●	●
Tennessee	2008	24	●		●
Texas	2003	25	●		●
Utah	1994	26	●		●
Virginia	2007	25		●	●
Washington	2007	25	●		●
West Virginia	2007	25	●	●	●

Notes: The mark (●) indicates whether a state extends parental coverage to young adults satisfying each eligibility rule (being a student, single or having children).

Source: Levine, et al. (2011), Nicholson, et al. (2009), Kronstadt, et al. (2007) and National Conference of State Legislatures (2008)

Table 2.2: Summary statistics for young adults aged 19-24

	Mean	Standard deviation
Uninsured	0.339	0.473
Group dep. health insurance	0.216	0.412
Non-group dep. health insurance	0.019	0.136
Own private insurance	0.311	0.463
Public health insurance	0.089	0.285
Work	0.723	0.447
Working full-time	0.515	0.500
Working part-time	0.209	0.406
Working at a big firm (>100)	0.455	0.498
Working at a small firm (≤100)	0.340	0.474
Fraction of the newly eligible (*Treated)*	0.256	0.436
Fraction whose Parent works at small firms with coverage	0.136	0.342
Full-time student	0.254	0.435
Single	0.781	0.413
Female	0.499	0.500
No child	0.813	0.390
Not living with parents	0.578	0.494
White	0.784	0.411
Black	0.140	0.347

Notes: Total observation is 140,629. Data are from 2001-2010 March CPS.

Table 2.3: The effects of extended parental coverage laws on health insurance coverage

Dependent Variable:	Group dependent HI	Non-group HI	Own private HI	Public HI	Uninsured
	(1)	(2)	(3)	(4)	(5)
Treated	0.043***	-0.003*	-0.024*	-0.004	-0.002
×*Post*	(0.008)	(0.002)	(0.012)	(0.008)	(0.010)
Mean	0.216	0.019	0.311	0.089	0.339
Obs.	140,629	140,629	140,629	140,629	140,629
R^2	0.282	0.016	0.151	0.081	0.109

Notes: Standard errors are clustered by state and shown in parentheses, * p<0.10, ** p<0.05, *** p<0.01. All specifications include state and year fixed effects.

Table 2.4: The effects of extended parental coverage laws on health insurance coverage by gender

	Male					Female				
Dependent variable:	Group dependent HI	Non-group HI	Own private HI	Public HI	Uninsured	Group dependent HI	Non-group HI	Own private HI	Public HI	Uninsured
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.026**	-0.006	-0.011	0.002	-0.000	0.060***	-0.001	-0.039***	-0.011	-0.001
$\times$*Post*	(0.011)	(0.004)	(0.013)	(0.010)	(0.011)	(0.012)	(0.003)	(0.014)	(0.011)	(0.013)
Mean	0.194	0.019	0.328	0.052	0.381	0.238	0.019	0.294	0.126	0.298
Obs.	67,275	67,275	67,275	67,275	67,275	73,354	73,354	73,354	73,354	73,354
R^2	0.311	0.020	0.158	0.048	0.126	0.267	0.015	0.157	0.094	0.086

Notes: Standard errors are clustered by state and shown in parentheses, * p<0.10, ** p<0.05, *** p<0.01. All specifications include state and year fixed effects.

Table 2.5: The effects of extended parental coverage laws on health insurance coverage with restricted sample

	Non-student					Parent who works at a small firm and has private HI				
Dependent variable:	Group dependent HI	Non-group HI	Own private HI	Public HI	Uninsured	Group dependent HI	Non-group HI	Own private HI	Public HI	Uninsured
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated $\times$ *Post*	0.037***	-0.004	-0.011	-0.005	-0.017	0.049**	-0.000	-0.053**	0.012	0.017
	(0.010)	(0.003)	(0.015)	(0.013)	(0.020)	(0.021)	(0.006)	(0.020)	(0.008)	(0.019)
Mean	0.143	0.013	0.371	0.097	0.381	0.549	0.021	0.240	0.038	0.200
Obs.	105,972	105,972	105,972	105,972	105,972	19,063	19,063	19,063	19,063	19,063
R^2	0.131	0.009	0.135	0.092	0.097	0.342	0.010	0.165	0.038	0.124

Notes: Standard errors are clustered by state and shown in parentheses, * p<0.10, ** p<0.05, *** p<0.01. All specifications include state and year fixed effects.

Table 2.6: The effects of extended parental coverage laws on labor market choices

	Full sample			Male			Female		
Dependent variable:	Work	Full-time	Big firm	Work	Full-time	Big firm	Work	Full-time	Big firm
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	-0.022**	-0.026***	0.006	-0.034**	-0.027	0.008	-0.013	-0.028***	0.002
$\times$*Post*	(0.009)	(0.007)	(0.011)	(0.013)	(0.016)	(0.015)	(0.014)	(0.010)	(0.018)
Mean	0.723	0.515	0.455	0.762	0.588	0.450	0.684	0.443	0.459
Obs.	140,629	140,629	140,629	67,275	67,275	67,275	73,354	73,354	73,354
R^2	0.145	0.255	0.037	0.198	0.303	0.033	0.124	0.207	0.048

Notes: Working full-time is not conditional on working. Big firms hire more than 100 employees. Standard errors are clustered by state and shown in parentheses, * p<0.10, ** p<0.05, *** p<0.01. All specifications include state and year fixed effects.

Table 2.7: The effects of extended parental coverage laws on labor market choices with restricted sample

	Non-student			Parent who works at a small firm and has private HI		
Dependent variable:	Work	Full-time	Big firm	Work	Full-time	Big firm
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.018**	-0.023*	-0.003	-0.016	-0.064***	0.038
$\times$*Post*	(0.007)	(0.012)	(0.011)	(0.031)	(0.017)	(0.038)
Mean	0.782	0.630	0.470	0.678	0.391	0.402
Obs.	105,972	105,972	105,972	19,063	19,063	19,063
R^2	0.132	0.148	0.039	0.131	0.254	0.020

Notes: Working full-time is not conditional on working. Big firms hire more than 100 employees. Standard errors are clustered by state and shown in parentheses, * p<0.10, ** p<0.05, *** p<0.01. All specifications include state and year fixed effects.

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Chapter 3: Assessing the Impacts of Educational Tracking: Peer or Coursework Effects?

Abstract

The practice of educational tracking in secondary schools in the United States often involves not only allocating students by different ability but also providing differential coursework across ability groups. In this paper, I decompose the effects of tracking into the separate roles of peer group and coursework. An identification problem arises because unobserved factors may determine selection into both a particular peer group and course-taking pattern. I address this by implementing an instrumental variables strategy, predicting a student's peer quality and coursework using variation in school policies. I also address potential endogenous school selection problems by including school fixed effects.

3.1 Introduction

Tracking is a widely used practice in American schools, which refers to a policy where schools sort students into different classes by observable ability with the intention of providing tailored instruction in a more homogenized group setting.[32] The effect of tracking has been debated. Proponents of tracking argue that it is technically efficient since educators can better tailor their teaching approaches. Opponents argue that tracking condemns students placed in the lower tracks to lower educational attainment and earnings, perpetuating economic disadvantage across generations.

Researchers have examined the effects of tracking using various techniques. It is generally agreed that high-achieving peers help (Hanushek et. al 2003; Duflo et. al 2011). If tracking leads to beneficial results for low-achieving students, then the positive effects should therefore come from increased instructional efficiency. The tailored level of instruction in a homogenized group setting is thought to improve learning, which is achieved by having teachers optimally respond to a classroom's composition (i.e. Duflo, Dupas and Kremer 2011).

Despite its important role in improving learning, the level of instruction is generally unobserved in practice and has received little attention. As such, previous studies have either focused only on the peer effects or discussed the "overall effect" of

[32] The term ability-grouping is often interchangeably used with tracking in the academic literature, although ability grouping is a subset of tracking. Tracking ranges from ability grouping alone to dividing students by academic ability with the intent of providing a different curriculum and pedagogical methods to different groups of students. For instance, especially in high schools, tracking may imply that schools provide separate classes intended for precollege vs. vocational students, and/or that schools group students into classes according to students' observed ability. Tracking in the European system sorts students into different schools rather than different classes, although it may also occur across classes (Betts 2009).

tracking, not distinguishing between the effects of peer and instruction level. This study is to my knowledge the first attempt to evaluate the role of instruction or coursework level on student's learning.

The questions I raise in this paper are twofold. First, I seek to understand the effect of taking a more advanced course, controlling for peer quality. When students are placed in a more advanced course track, they generally change peer levels as well. Therefore, tracking should be considered as a composite of various treatments. I examine two specific channels, peers and coursework, which are affected by tracking. In the empirical analysis, I use unique panel data from the San Diego Unified School District to model end-of-year standardized Mathematics test scores of middle school students as a function of their prior year's test scores and the measured level of coursework and exposure to peers. Controlling for peer quality enables the effect of coursework to be identified independently from peers, which is important as the two often exhibit a high correlation. Looking separately at the two main components of tracking, peers and coursework, provides a better understanding of the channels through which tracking affects student outcomes.

Secondly, I examine the effect of introducing a differential degree of course-tracking and ability-grouping on student outcomes. This strategy is concerned with capturing the overall effects of policy, but with unspecified channels. To do so, I use the range of math courses offered by middle schools as a formal way of differentiating instruction level at the school (as opposed to an informal way adopted freely by individual teachers in a classroom). For instance, I test whether we see a greater improvement in students' test scores when schools *formally* offer more differentiated courses (a high level of course-tracking), such as "Advanced algebra" targeted towards high achievers and "Algebra" targeted to middle and lower achievers, than when schools offer fewer courses, such as just one "Algebra" class (a low level of course-tracking).[33]

The intuition for how instruction or coursework level affects student outcomes comes from the model described in Duflo, Dupas and Kremer (2010), where students' test scores decay as the measurable distance between their initial ability and the optimal level of instruction increases. Extending this framework to the coursework-tracking environment in which schools can offer varying ranges of courses, I expect offering more courses is better if it facilitates matching between student's initial ability and the optimal level of coursework.

I focus my analysis on 7th and 8th graders in middle schools. Alexander and Cook (1982) state that the effect of curriculum assignments or track placements in high school may be too late in the presence of early tracking, as it may simply reflect the differences in achievement trajectories determined years earlier. If in fact tracking starts in middle school, the estimated effect of tracking in high school conditional on having been tracked since middle school will lead to an erroneous conclusion. Also, elementary schools do not have much variation in course-offerings within grade, so it is not an ideal environment to study coursework effects.

[33] Even if coursework is the same, pedagogy may differ across classrooms. I do not observe pedagogy within a classroom so this informal way of differentiating instruction will be partially captured by the peer effect, which is measured at the classroom level.

3.2 Existing Literature and Conceptual Framework

3.2.1 Literature Review

The primary contribution of this paper is in disentangling coursework effects from peer group effects, an issue that has not been explored much in previous studies. Tracking involves several components, and what we observe from the estimated effect is confounded by other inputs that are correlated with tracking. Although the level of peers and coursework under tracking are likely to be positively correlated with each other, previous work on the effects of peer and coursework have existed as separate literatures. The studies on peer effects have paid particular attention to the selection process governing students' exposure to a particular type of peer group.[34] Likewise, previous studies on coursework's influence on student outcomes have focused on the process through which students select into particular course-taking patterns.[35]

Because differential levels of peers and coursework are byproducts of tracking, the typical challenges to estimating the effects of tracking would apply to credibly estimating peer and coursework effects as well. So far, the empirical techniques used

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[34] The selection process may depend on schools' policies toward ability grouping and students' characteristics such as ability and motivation. Controlling for fixed individual, school, and school-grade effects, Hanushek, Kain, Markman and Rivkin (2003) find that peer achievement at the school level has a positive effect on achievement growth. Betts and Zau (2004) adopt a similar fixed effects approach but use a peer measure at the classroom level instead of the school level, and conclude that mixing students hurts top-achieving students more than it helps bottom-achieving students. Some studies have used random group assignments (Sacerdote 2001; Zimmerman 2003; Duflo et al. 2010), while others use an instrumental variables approach (Goux and Maurin 2007; Lefgren 2004) to credibly estimate peer effects.

[35] Curriculum assignments are explained by academic experiences and accomplishments (Alexander and Cook, 1982), as well as social background and academic ability (Alexander and McDill, 1976). Several studies overcome the problem of selection by using the school's average course taking to predict the individual's course taking, leaving the predicted value independent of individual ability. Altonji (1995) uses the variation in curriculum across high schools to identify the return to additional courses in academic subjects and finds the effect to be small. On the other hand, in investigating the effect of six types of high school math courses on earnings, Rose and Betts (2004) find that taking a particular math course affects students' earnings nearly a decade after graduation.

in comparing outcomes between students in tracked and non-tracked schools have evolved mainly to address the following identification issues: (1) tracking schools may be inherently different from non-tracking schools not just in the tendency to track but also in other ways (i.e. there may be correlated school inputs that could also affect student outcomes), (2) students who choose to attend more tracked schools may be different from other students (school selection), and (3) students are placed in tracks for unobservable reasons, rather than due to only their test scores (track selection) that make comparison of outcomes across tracks difficult.

Earlier non-experimental studies often use the principal's or teacher's reporting on the degree of tracking at the class or school level, and compare students in tracked classrooms or schools to students in untracked learning environments. The studies that control for school fixed effects are less likely to be subject to the first two problems cited above (e.g. Betts and Shkolnik, 2000). Figlio and Page (2002) address the school selection issue by instrumenting for tracking using county-level measures such as political preferences, which could arguably affect a school's decision to track and parents' schooling choices but not affect student achievement through other channels. They find tracking programs have a positive net effect on low-ability students' test scores.

Recent experimental evidence by Duflo, Dupas and Kremer (2010) overcomes the three empirical concerns mentioned above. In their experiments, they adopt strict ability grouping in half of the 121 sampled elementary schools in Kenya, allocating students into high and low-ability classes. In the other half of the schools, students were randomly assigned to the two classrooms. By randomly having schools adopt ability-grouping, they surmount the first and second problems, as the extent of tracking is uncorrelated with other school inputs and the underlying heterogeneity of the student-body. School selection is also independent of tracking since the student's choice of school has already been made before the experiment. They circumvent the third obstacle by having the initial score being the only determinant of the track in which a student enrolls. They find that tracking is beneficial across the entire ability distribution, increasing the average student's test score by 0.175 standard deviations. Although students placed in low-ability classrooms experienced negative peer effects, a lowered variance of student ability within classrooms may have enabled teachers to improve teaching methods, effectively counterbalancing the negative peer effects.

3.2.2 The Relationship among Ability-grouping, Mathematics Courses, Peer Quality and Student's Ability

In this section, I describe how school policies can be used to predict students' experiences with peers and coursework. For simplicity, I assume that there are two types of tracking: ability grouping and differentiated course offerings.

For simplicity of exposition, suppose that schools face discrete decisions of (1) either to group or not to group students by ability, and (2) whether to offer two courses (targeted at the difficult and easy levels) or one course (targeted at the medium level). These two choices may interact with each other to the extent that a school that chooses to group students according to ability also offers a greater breadth of math courses, and vice versa.

Differences in school policies lead to three possible types of schools. As shown in Table 1, schools that choose to have homogeneous classes in terms of student ability can either offer one course to all ability groups (School 1) or offer two courses based on students' ability (School 2). By offering several classes, a school is necessarily tracking students according to ability. Hence, schools that do not group students would offer only one math course (School 3). Few schools would fall under the case with two courses and mixed-ability case.

Table 1 also describes how these school policies lead to different middle school experiences by influencing peer quality and the coursework that students of different ability types receive. Denote H for high ability and L for low ability. The likelihood of having high quality peers increases when a student is H-type *and* when schools group students according to ability. In grouped schools (Schools 1 and 2), H-type students would have high ability peers and L-type would have low ability peers. Similarly, the level of difficulty of a student's coursework is affected by the range of math courses that a school chooses to offer. H-type students would be able to take a more difficult course when two courses are offered, but not when only one course is offered.

Comparing schools 1 and 2, conditioning on the type of student would hold peer quality constant but vary the courses, allowing one to estimate the coursework effects. Similarly, comparing Schools 1 and 3 would hold the level of coursework constant while varying peers, allowing one to estimate peer effects. My analysis is based on these types of comparisons when identifying the effects of peers and coursework, except that I use continuous measures of the range of courses offered, degree of ability grouping and own initial ability, rather than the discrete ones introduced in Table 1.

3.3 Empirical Framework

3.3.1 Identification Strategies

Middle school experiences can be summarized largely by peers, coursework, and other school resources such as a class size and teacher quality. I consider a framework that relates peers and coursework to student achievement in the following academic year or achievement at the end of middle school. In estimating the effect of peer quality (*Peer*) and the difficulty level of coursework (*C*) on student outcomes, I assume that school policies are different largely in two ways: (1) the degree to which schools group students by ability and (2) the breadth of math courses that schools choose to offer. These two policies are used to construct instrumental variables for endogenous peer groups and coursework, so they are assumed to be independent of students' unobserved abilities.

I start with a specification where the effects of own initial ability, peer quality and course content enter linearly and independently. I run a regression of the following form, using the panel dataset of middle school students (grade 7 and 8) in the San Diego Unified School District.

$$\begin{aligned} y_{it} &= \alpha_0 y_{i,t-1} + X_i\delta + \beta_0 C_{jgt} + \beta_1 Peer_{ict} \\ &+ \beta_2 Class_{ct} + Teacher_{rt} + School_{sgt}\lambda + \sigma_s + \sigma_g + \sigma_t + \varepsilon_{it} \end{aligned} \quad \text{(E1)}$$

where y_{it} is a standardized test score for student *i* at the end of academic year *t*. Other variables can also be denoted by subscript *i* and *t* only, as *i* is the finest (student) level identifier, but they are expressed in terms of school *s*, class *c*, teacher *r*, course *j*, grade *g* to indicate key sources of variation. The vector *X* includes student level characteristics: gender, race, and whether a student is an English learner. *C* is a measure of the degree of difficulty of the course that a student took (described in more detail in Section 3.3). *Peer* is measured peer quality, calculated by average *prior* year test scores of current classmates, excluding own test score.

One immediate concern is policy endogeneity. Tracking schools may be inherently different from non-tracking schools, and this school level heterogeneity can be correlated with the error term and the school policies. For instance, big schools are more likely to have ability grouping, and the schools that adopt ability grouping may have smaller class sizes, possibly increasing the teacher-student ratio. In an attempt to minimize this concern, I control for school fixed effects, so that the identifying variation comes from within school, across grade and year. Even with the school fixed effects, some school characteristics varying across time or grade are needed to be controlled for. *Class* is the number of students in the class. *School* contains observable student body characteristics: size of the school cohort (where cohort is defined by students of the same grade who attended the same school in the same year), the percentage of students on free lunch (proxy for poverty at the given school and year level), and the mean and standard deviation of lagged test scores of the cohort. *Teacher* includes whether a teacher has full credential in math, bachelor degree in math, masters degree in math, and teaching years. Lastly, I control for grade fixed effects and year fixed effects. Standard errors are clustered by the cohort and school cell.

I treat two variables as endogenous variables, peer quality (*Peer*) and coursework (*C*). Individuals tend to self-select into peer groups, so selection bias may occur in estimating peer effects. This is likely to be a problem in a secondary school setting, in which schools tend to do more ability-grouping than in elementary school. Similarly, a potential threat in identifying the effects of coursework on student outcomes arises due to unobserved selection. Unobserved selection is present when individuals self-select into a certain math course based on factors such as motivation and ability, which are unobserved by researchers. For instance, if high achievers are likely to take more advanced courses, then the estimated effect of advanced course-taking on test scores may be higher than the true causal effect. Students who are motivated enough to take advanced courses are likely to do better anyway, regardless of the difficulty level.

Prior achievement of a student largely determines a peer group and course level, so it is important to control for $y_{i,t-1}$ as well as other factors that are correlated with peer quality or course-taking behavior. Along with controlling for potentially omitted variables, I use an instrumental variables approach to address the selection problems. The two instruments I use to predict *Peer* and *C* are $AbilityGroup * y_{i,t-1}$ and

$Range * y_{i,t-1}$. As discussed in section 2.2, the peer quality that a student faces can be predicted by the degree of ability-grouping (*AbilityGroup*) and the student's own ability, which is measured by own prior year test score. If the degree of ability grouping at the school level is high, the student with high ability is exposed to a high achieving peer group, and vice versa. Thus, $AbilityGroup * y_{i,t-1}$ will be positively correlated with *Peer*. Similarly, *C* is predicted by the interaction between student's prior year test score and the range of courses (*Range*) that a school chooses to offer, so that $Range * y_{i,t-1}$ is used to instrument *C*. In this framework, to the extent that *Peer* and *C* are correlated with each other, they will be positively affected by both of the instruments to some extent but the intended instrument will show stronger effect in the first stage. The main effect of *AbilityGroup* and *Range* are included in both the first stage and second stage regressions. Thus, the model is identified from *AbilityGroup* and *Range* having a differential impact on peer quality and coursework depending on lagged test score.

The second strategy is concerned with the overall effect of school policy variables. Some schools do just ability grouping, and some schools also offer a wide range of courses. If it is proven to be helpful that students do better in schools that not just do ability grouping but also offer wide range of courses, schools that do not differentiate curriculum may not be fully using their ability to increase students' potential. While the IV regression (E1) allows us to look at the average effect of *AbilityGroup* and *Range*, it does not convey useful information on how these policies differently affect students of high and low ability.

The strategy is somewhat parallel to the instrumental variables approach. It includes the school policy variables used as instruments in the previous section, namely *Range* and *AbilityGroup*, directly in the regression and looks at the effect across varying deciles of prior year test score.

$$\begin{aligned} y_{it} &= \alpha_0 y_{i,t-1} + X_i\delta + \beta_0 Range_{jgt} + \beta_1 AbilityGroup_{ict} + Class_{ct} + Teacher_{rt} \\ &\quad + School_{st}\lambda + \varepsilon_{it} \end{aligned} \tag{E2}$$

Here I expect *AbilityGroup* to capture informal instructional efficiency that may not operate through peer effects. Positive effect of *AbilityGroup* would mean that in-class instruction is more efficient with lower variance of students' ability. The effect of *Range* (β_0) in (E2) would be positive if offering more courses leads to effective learning.

Another channel through which *Range* can affect student outcomes is through its effect on motivation or encouragement. A high degree of course differentiation may hurt low achieving students if they are condemned by being formally labeled as "easy-course takers" and differentiated from other peers. A low degree of course differentiation may also hurt high achieving students if they are not challenged enough with their assigned course. The direction of the effect is hard to predict ex-ante. Since the effect depends on where in the initial test score distribution a student is, I estimate (E2) using quantile regression.

3.3.2 Data

I use longitudinal data from the San Diego Unified School District (SDUSD). The SDUSD is the second largest district in California and the eighth largest urban district in the United States. It consists of approximately 135,000 students in pre-school through grade 12. The data consist of student academic records such as standardized test scores in math, academic grades and courses taken. It also contains information on student's gender, race, whether a student was an English learner for a given year, percentage of students on free lunch for a given school, class size, and teacher qualifications (whether a teacher has a bachelor's degree in Mathematics, credentialing, and years of math teaching experience).

This study of tracking focuses on students' middle school experiences.[36] I consider those 7th and 8th graders who attended middle schools in the SDUSD between fall 1997 and fall 2002. California mandated the Stanford 9 test in spring 1998 through spring 2002 and the California Standards Test (CST) in spring 2002 and later years. I use the Stanford 9 period only since it is a common test within the grade and year, allowing comparison of students across types of math courses taken. The CST is a subject-specific test (i.e. the students who take different math subject take different exams) therefore the course effects cannot be estimated.

To be included in the final sample, students are required to have two consecutive years of Stanford 9 test scores and information on teacher and coursework. The first requirement is needed since the dependent variable is the end of year test score and I control for the prior year test score in the regression framework.[37] The

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[36] Elementary schools in the SDUSD do not have much variation in math course offerings, so it is not an ideal environment to study the course effect. Since many elementary schools are K-6 schools, I use only 7th and 8th graders in the SDUSD.

[37] This requirement would exclude those who move to schools out of the school district. I would like to check robustness of the estimates by comparing the original results and the results from limiting samples to students who eventually switch schools after the period of my analysis.

information on teachers and coursework is needed when constructing average peer quality and the level of math coursework because a student's peers are those students with a common teacher and period. I also exclude students in charter or atypical schools, as these schools have their own coursework that may not be comparable to other schools. For a similar reason, I exclude those whose course has district-wide enrollment of fewer than 30 students and those classrooms with fewer than 5 students. Lastly, I exclude students who have taken at least one special education course during middle school.

3.3.3 Measuring school policies, peer quality and coursework

I construct measures of (1) peer quality (*Peer*), (2) the level of difficulty of a course (*C*), (3) the range of math courses offered by each school (*Range*) and (4) degree of ability-grouping (*AbilityGroup*).

Detailed descriptions of the construction of these variables are relegated to the Appendix. To briefly describe how I construct each measure, *Peer* is a class-level variable and defined as the average prior year test score of a student's current peers. For the level of difficulty of a course, I propose C_{jgt}, a data-driven measure of course difficulty constructed by the *district-wide average* of prior year test scores of those who take course j in grade g and year t. A higher value indicates a harder course. *Range* measures the range of math courses offered by each school s for grade g in year t, and is constructed by calculating the mean deviation of C, where the mean is the average difficulty of C for a given school, grade and year. Finally, *AbilityGroup* increases as the degree of ability-grouping within a school is higher. It is a continuous variable with values between 0 and 1. For each school, grade and year cell, I assign the R-squared resulting from a regression of a student's *initial* test scores on classroom fixed effects.[38] This follows Lefgren's (2004) approach.

3.4 Factors that Determine School Policy Variables

Table 2 shows summary statistics of the variables used in the regression. The average standardized math test score is greater than 0, indicating that the excluded students (i.e. special education students, transferred students and students who go to charter or atypical schools) have on average lower test scores. The mean lagged math score is close to average peer quality, as the latter is constructed by the average of the current classmate's lagged test scores. The average degree of ability grouping, *AbilityGroup* is 0.4 and varies from close to 0 to 0.7. Similarly, the average range of courses offered at the school, *Range*, shows great variation, varying from 0 to 0.75.

Figure 1 plots the degree of ability grouping (*AbilityGroup*) against the range of courses that a school chooses to offer (*Range*) for each school, grade and year. One can overlap each box with Table 1. That is, when dividing the figure into four quadrants, the top-left represents School 1, the top-right is School 2, and the bottom-left is School 3. *AbilityGroup* and *Range* show a positive relationship, indicating that schools that adopt ability-grouping are also likely to offer a wide range of courses.[39] In addition, as expected, almost no school is located in the 4th quadrant, indicating that it

[38] The initial test score for 8th graders can be either 6th or 7th grade test score (or an average thereof). I use prior year's test score as initial test score for now.

[39] The correlation between *AbilityGroup* and *Range* is 0.58.

is uncommon to have schools that do not group students yet offer a wide range of courses.

As a quick way to describe the policy variables used as instruments, I regress the policy variables on school-grade-year level characteristics. The results are shown in Table 3. Each of *AbilityGroup* and *Range* (at the school, grade and year level) is regressed on the mean and standard deviation of the lagged test score of the school cohort, school size, class size, percentage of students on free lunch (proxy for poverty), and grade, year and school fixed effects. *AbilityGroup* is positively associated with the lagged standard deviation of the school cohort's test score, indicating that schools tend to adopt a greater degree of ability grouping when the variance of the test score distribution is large and when on average student's achievement is high. Variation in *Range* cannot be explained by the control school characteristics. I include these control variables in the main regressions to address the concern that school characteristics that change over time and grades may be correlated with the school policy variables and have an independent effect on test scores.

3.5 Results: OLS and Instrumental Variables (IV) Approach

Table 4 shows results from OLS and IV estimation based on equation E1. The dependent variable is standardized Mathematics test scores at the end of the academic year. Standard errors are clustered by the school*cohort level. Looking across columns, models vary by the set of included controls.

Since there are many numbers to look at, I would like to focus on the following patterns. First, the effects of *Peer* on student test scores are positive and reasonably similar across model variations. In all 12 cases where the measure *Peer* is included (i.e. excluding column 4 where I leave *Peer* out), the effect is positive and statistically significant at the 1% level. OLS estimates of *Peer* vary from 0.117 to 0.196 (Panel A) and IV estimates vary from 0.122 to 0.224 (Panel B).

Second, the estimated coefficient of the coursework measure (*C*), interpreted as the effect of taking more difficult level of math course, is positive in all cases. If anything, students seem to do better when taking more challenged courses than easier ones. Considering internal validity and statistical significance, however, we cannot put too much emphasis on the positive estimated effect of taking advanced coursework since the results are statistically significant only for the OLS but not at all in IV. For instance, IV estimates in Panel B show that the coefficients of *C* are not precise enough to be significant at the 5% level, except in column (4) where I exclude *Peer*. The large and positive coefficient of *C* in column (4) reflects the fact that students who take harder course are also likely to have high quality peers. With the presence of positive correlation between the difficulty level of coursework and peer quality, excluding one in the regression can lead to a biased estimate of the other.

IV estimates are valid under the assumption that variation in the instruments is indeed exogenous to other factors determining student outcomes. I directly control for *AbilityGroup* and *Range* in the regression, so the direct effects coming from these school policies are taken care of. It is whether the interaction terms provide plausibly variation uncorrelated with the error term. One possible source of endogeneity is that school policies are driven by characteristics of the student body which could also affect student outcomes. I control for school fixed effects to minimize possible policy endogeneity, but the coefficient estimates do not change much (Columns 1 and 2). The observable teacher characteristics in most cases are not significant predictors. From this I infer that omitted teacher qualities correlated with both school policies and the error term pose little concern, once other variables are controlled for.

Columns (5) to (7) explore sensitivity to including more detailed controls. Column 5 and 7 control for the mean and standard deviation of lagged test scores of the school cohort (where cohort is defined by students of the same grade who attended the same school in the same year). It appears the effect of *Peer* decreases by about 27% when controlling for the past cohort characteristics. Lastly, in column 6 and 7, I include the math test score from two years ago, to flexibly control for individual's dynamic selection into a particular peer group or coursework. In all cases, the two general patterns hold.

3.6 Results: Quantile Regression

Figure 3 shows the results based on E2 in Section 3.1. In some sense it is similar to the instrumental variables strategy, where in the IV identifying variation comes from differential effect of school policy across initial academic achievements. But one difference is that $AbilityGroup * y_{i,t-1}$ and $Range * y_{i,t-1}$ are used as instruments of *Peer* and *C*, thus their effect is constrained to operate only through the peer effects and the difficulty level of coursework. Another difference is that reported coefficient in the IV regression will be the average marginal effect. On the other hand, the results in quantile regression allow us to see if the particular school policy is more effective for high or low ability students. In quantile regression, the effect of the key school policies will vary depending on where a student is in the conditional ability distribution.

Figure 3 shows *AbilityGroup* has positive effect on student's test scores and the effect persists across all ability levels. In the next figure, *Range* has no effect in all deciles. This confirms the weak evidence of the effect of coursework for both high and low ability students.

3.7 Discussions and Conclusion

Burris et al. (2006) state, "If high achievers do learn less in heterogeneous classes, is it because average-achieving and low-achieving students are also present in these classrooms or because the curriculum in heterogeneous classes may be less demanding?" Motivated by this question, the present study attempts to decompose the effect of tracking into the separate roles of peer group and coursework. Previous studies that have focused solely on the peer side or coursework side may overlook influences from each other in the presence of ability grouping that also involves differentiation in peers and coursework. To the extent that both effects are present at the same time, focusing only on one effect and its selection processes will lead to unreliable inferences of the importance of either peer groups or coursework. To my knowledge, it is the first study that explicitly acknowledges the effect of coursework in examining peer effects or educational tracking.

Peer and coursework assignments can be determined by both observable and unobservable student characteristics. To address this, I estimate peer and coursework effects by implementing an instrumental variables strategy, predicting student's peer quality and coursework using variation in school policies. Schools differ in the extent to which they practice ability grouping and in the range of courses they choose to offer to students in a given grade and year. To the extent that these policy variables are exogenous conditional on school fixed effects and various time-varying school characteristics, my strategy provides convincing estimation strategies.

My findings suggest that peer effect is a strong predictor of student test score, while taking more difficult level of coursework is not. These two general observations are robust to including school fixed effects, controlling for characteristics of the student body, and controlling for two-year lagged test score to address a potential dynamic selection into a peer group or coursework. Hence, I conclude that (1) peers matter and (2) there is weak evidence that difficulty of coursework matters in student achievement. Regarding the second point, if anything, students seem to do better when taking more difficult courses than easier ones controlling for prior year test score.

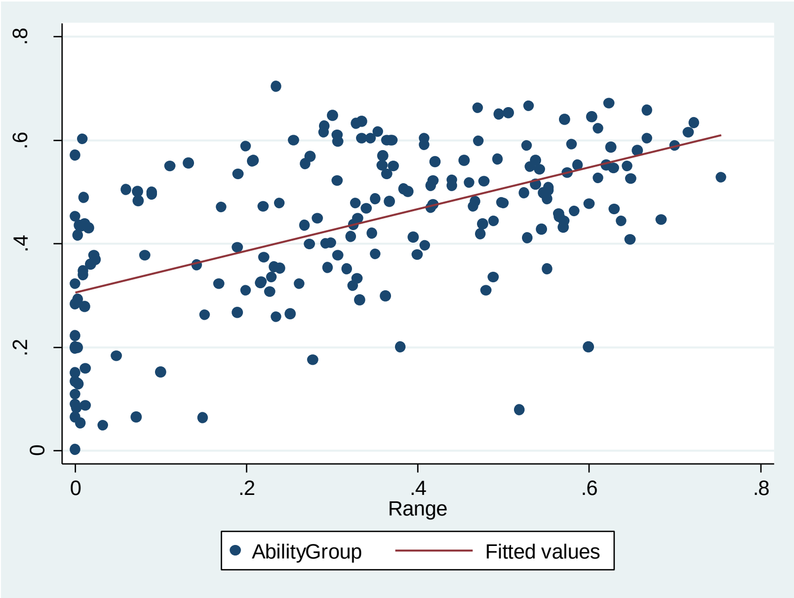

Figure 3.1: The degree of ability grouping (x-axis) and the absolute mean deviation of C_{jg} (y-axis)

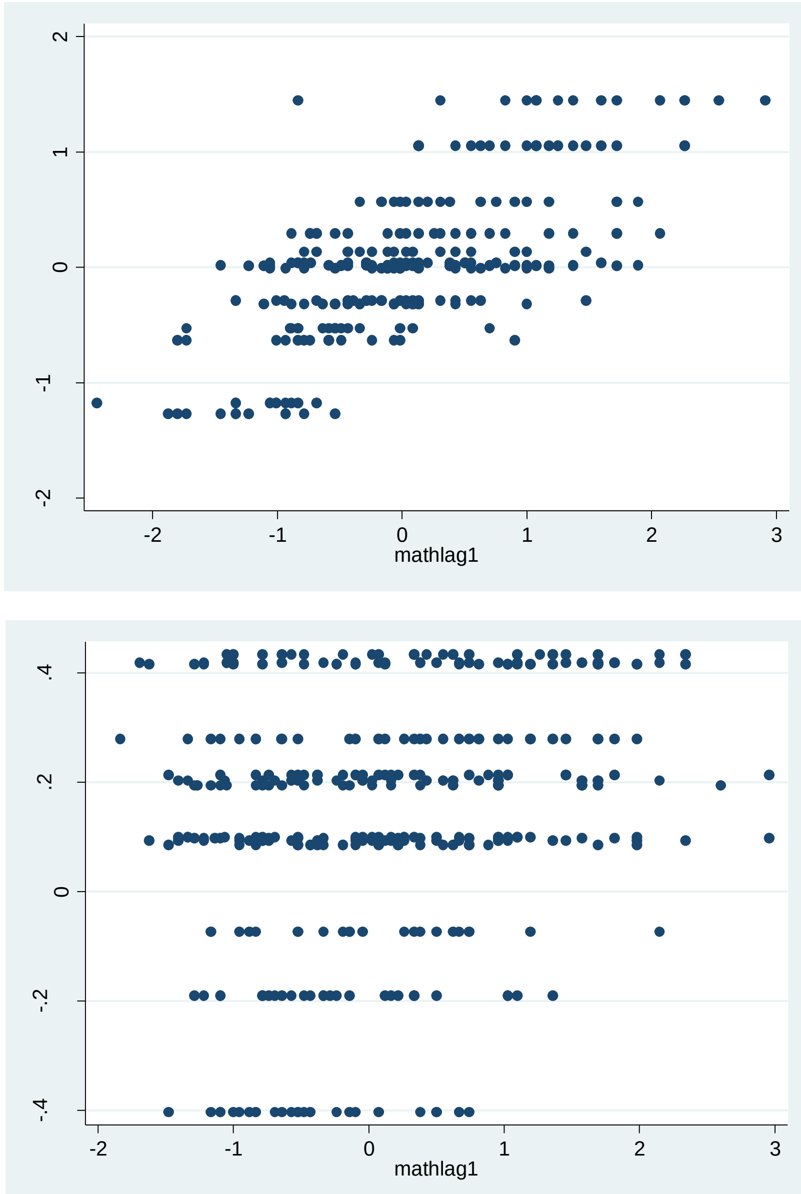

Figure 3.2: High tracking vs. Low tracking

Notes: X-axis: Student's own prior year's test score. Y-axis: average classroom level initial test score (including student him/herself) Left: a school with tracking measure (AbilityGroup) of 0.47 in grade 6 in Spring 1999, Right: the same school with tracking measure of 0.06 in grade 7 in Spring 2000.

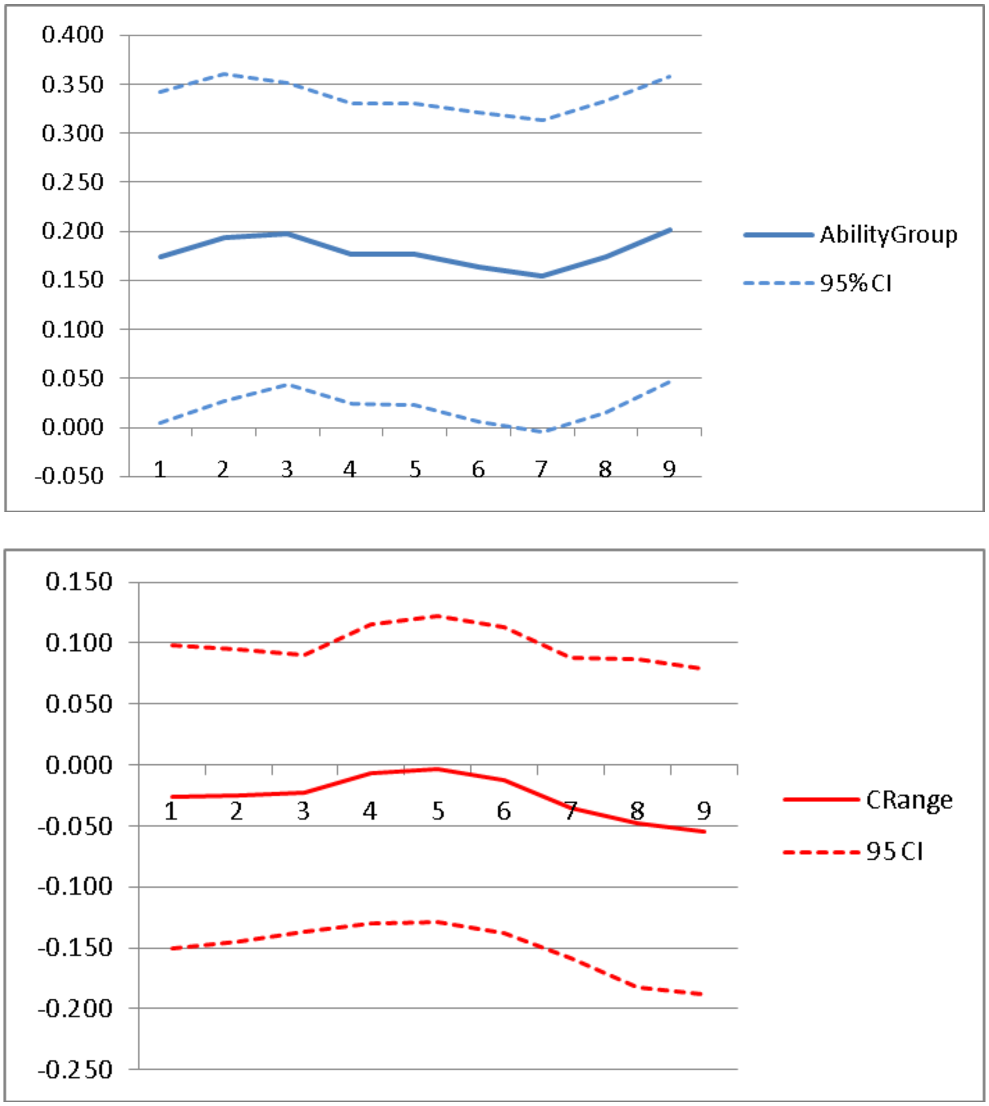

Figure 3.3: Reduced form estimates using quantile regressions

Notes: Standard errors are clustered by school-cohort using bootstrap methods based on 2000 runs for each decile.

Table 3.1: Relations among Ability-grouping, Mathematic Courses, Peer Quality and Student's Ability

	One course offered: Medium (M)	**Two courses offered: Difficult (D) and Easy (E)**
To Group (homogeneous ability within classroom)	**School 1** H-type: Peer (H) Course (M) L-type: Peer (L) Course (M)	**School 2** H-type: Peer (H) Course (D) L-type: Peer (L) Course (E)
Not to Group (mixed ability within classroom)	**School 3** H-type: Peer (M) Course (M) L-type: Peer (M) Course (M)	Not common

Table 3.2: Summary Statistics, Grade 7 and 8 (values averaged over four academic years, from 1998-1999 to 2001-2002)

Variable	Mean	SD
Math test score	0.100	0.993
Math test score t-1	0.119	0.988
Female	0.502	0.500
White	0.298	0.457
Black	0.137	0.344
English Learner	0.197	0.397
Peer	0.110	0.743
C	0.082	0.533
# Student in class	29.110	7.360
# Student in school	421.055	134.403
Avg full credential math thcrs	0.971	0.163
Avg BA in Math	0.275	0.437
Avg MS in Math	0.541	0.490
Avg teaching years	14.103	10.716
% on Free lunch	52.518	25.323
AbilityGroup	0.442	0.150
Range	0.332	0.211
AbilityGroup*test score t-1	0.075	0.479
Range*test score t-1	0.082	0.410
Observations	49373	

Table 3.3: Descriptive Analysis of the Factors that Determine School Policy Variables

	(1)	(2)
	AbilityGroup	*Range*
SD of Test score t-1 of school cohort	0.477***	0.102
	(0.126)	(0.159)
Mean of Test score t-1 of school cohort	0.005	0.048
	(0.075)	(0.094)
# Student in school	-0.000	-0.000
	(0.000)	(0.000)
# Student in class	0.001	-0.001
	(0.003)	(0.003)
% on Free lunch	0.003	0.003
	(0.003)	(0.004)
Observations	192	192
R-squared	0.616	0.668

Table 3.4: OLS and IV estimations

				Panel A: OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Peer	0.166***	0.157***	0.196***		0.168***	0.117***	0.122***
	(0.013)	(0.013)	(0.008)		(0.012)	(0.011)	(0.011)
C	0.057***	0.063***		0.174***	0.060***	0.057***	0.054***
	(0.016)	(0.016)		(0.012)	(0.014)	(0.014)	(0.014)
AbilityGroup	0.172***	0.164**	0.121*		0.181**	0.128*	0.137*
	(0.060)	(0.073)	(0.065)		(0.073)	(0.072)	(0.074)
Range	-0.035	-0.036		0.016	-0.027	0.024	0.039
	(0.057)	(0.055)		(0.051)	(0.053)	(0.078)	(0.076)
SD of Test t-1 of school cohort					0.156		0.052
					(0.123)		(0.113)
Mean of Test t-1 of school cohort					-0.326***		-0.167***
					(0.057)		(0.048)
Test t-1	0.761***	0.760***	0.762***	0.794***	0.761***	0.535***	0.537***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)
Test t-2						0.300***	0.297***
						(0.007)	(0.007)
				Panel B: IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Peer	0.170***	0.182***	0.201***		0.169***	0.122**	0.123***
	(0.063)	(0.051)	(0.036)		(0.050)	(0.048)	(0.047)
C	0.080	0.029		0.134***	0.034	0.084	0.083
	(0.063)	(0.051)		(0.036)	(0.052)	(0.052)	(0.051)
AbilityGroup	0.173***	0.148**	0.121*		0.167**	0.149*	0.160*
	(0.058)	(0.072)	(0.065)		(0.077)	(0.082)	(0.087)
Range	-0.037	-0.034		0.015	-0.025	0.017	0.033
	(0.057)	(0.055)		(0.050)	(0.052)	(0.079)	(0.076)

Table 3.5: OLS and IV estimations, continued

SD of Test t-1 of school cohort					0.163		0.050
					(0.124)		(0.118)
Mean of Test t-1 of school cohort					-0.321***		-0.173***
					(0.058)		(0.056)
Test t-1	0.753***	0.760***	0.760***	0.806***	0.767***	0.528***	0.531***
	(0.020)	(0.018)	(0.018)	(0.014)	(0.016)	(0.017)	(0.016)
Test t-2						0.296***	0.294***
						(0.008)	(0.008)
Lagged test score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE		Yes	Yes	Yes	Yes	Yes	Yes
SD/Mean of Lagged test score of school cohort					Yes		Yes
Two-year lagged test score						Yes	Yes

Notes: Standard errors in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table 3.1: First-stage results for *Peer and C*. (school, grade and year fixed effects are included but not reported)

	(1) *Peer*	(2) *Peer*	(3) *C*	(4) *C*
AbilityGroup*Test t-1	0.968***	0.989***	0.106*	0.111**
	(0.025)	(0.024)	(0.054)	(0.053)
Range*Test t-1	0.059***	0.050***	0.730***	0.726***
	(0.017)	(0.015)	(0.040)	(0.039)
AbilityGroup	-0.034	0.006	-0.528***	-0.545***
	(0.078)	(0.051)	(0.154)	(0.156)
Range	-0.027	-0.046*	-0.024	-0.021
	(0.044)	(0.025)	(0.136)	(0.137)
Female	0.036***	0.037***	0.029***	0.029***
	(0.005)	(0.005)	(0.004)	(0.005)
ethnic== 1.0000	0.025**	0.024**	-0.003	-0.003
	(0.010)	(0.010)	(0.008)	(0.008)
ethnic== 2.0000	-0.002	-0.005	-0.019***	-0.019***
	(0.008)	(0.008)	(0.007)	(0.007)
# students / class	0.006***	0.006***	0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
# students / gr-yr-school	-0.000	-0.000	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Pct of school on free lunch	-0.003	0.000	0.011*	0.011*
	(0.002)	(0.002)	(0.006)	(0.006)
English learner	-0.183***	-0.184***	-0.042***	-0.043***
	(0.017)	(0.017)	(0.011)	(0.011)
Avg full cred among math tchrs	0.059	0.048	0.044	0.044
	(0.042)	(0.042)	(0.053)	(0.053)
Avg bach among math tchrs	0.031	0.031	0.021	0.020
	(0.021)	(0.021)	(0.025)	(0.025)
Avg teaching years	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Test t-1	-0.054***	-0.065***	-0.025	-0.026
	(0.011)	(0.011)	(0.018)	(0.018)
Observations	49373	49373	49373	49373

Appendix Table 3.2: The level of course difficulty (C_{jgt}) and number of observations, by course code and school year

Code	C_{jgt}					C_{jg}
	19971998	19981999	19992000	20002001	20012002	
	Grade 7					
4001		-0.414	-0.515	-0.167	-0.109	-0.301
	5190	3094	2575	5293	4414	
4007				-0.624	-0.281	-0.452
				1433	1079	
4013					-0.036	-0.036
					213	
4014					-1.037	-1.037
					96	
4015			-0.562		-0.315	-0.439
			254		81	
4018					-0.596	-0.596
					363	
4035			-0.244	0.016		-0.114
			602	282		
4050					1.112	1.112
					105	
4051		0.235	0.165	0.809	1.062	0.568
	3410	5556	5595	1591	1394	
4063		1.563	1.608	1.477	1.614	1.566
	133	199	297	392	305	
	Grade 8					
4011		-0.479	-0.506	-0.404		-0.463
	5609	4487	916	85		
4015		-0.157	-0.563	-0.668		-0.463
	60	98	395	80		
4021					-0.357	-0.357
					183	
4041		-0.485	-0.305	-0.316	-0.302	-0.352
		162	4142	4495	5273	
4051		-0.130	-0.565	-0.523		-0.406
	189	724	375	819		

Appendix Table 3.2: The level of course difficulty (C_{jgt}) and number of observations, by course code and school year, continued

Code	C_{jgt}					C_{jg}
	19971998	19981999	19992000	20002001	20012002	
4061					0.980	0.980
					100	
4063		0.796	0.552	0.527	0.747	0.655
	2704	2939	3183	3178	2167	
4141					1.500	1.500
					61	
4175		2.223	1.803	1.958	1.790	1.944
	115	109	150	174	172	

3.8 References

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3.9 Appendix

1. Students with disability: Course codes offered in the San Diego Unified School District (SDUSD) between 7000 and 7999 are special education classes so those students took course these classes are considered to have severe disability and not included in the study. There may be students who are classified as special education children but did not take 7000 level courses. This is because their type of disability does not impede learning. I include these students in the study.

2. About 3% of students whose grade is between 6 and 8 took more than one course per semester. Most of them took extended class where they study one hour per day along with one other regular course. For course taking measure, I use the course code with higher enrollments as their main math course. Also their peer groups would be based on their peers in the regular course.

3. Measurement of four variables

(1) Peer quality (*Peer*)

Peer groups can be measured in various ways, and previous studies on peer effects have used the average characteristics of the peer in the school or the class that a student attends. Following this strategy, I use average prior year test scores of the current classmates as a measure of peer quality, excluding own prior year test score. For instance, peer quality for 8th graders is measured by current classmates' test score in 7th grade.

(2) The level of difficulty of a course *(C* and *Rank*)

San Diego Unified District offers a set of course codes, and it is up to schools to decide which courses they adopt. The district as a whole offered 6 different math courses for 8th graders in school year 20012002.[40] I construct the level of difficulty for each course *j*, grade *g*, and year *t* (C_{jgt}), where $j \in$ {Pre algebra, … , Algebra, …, Geometry}[41] and $g \in \{7,8\}$ so that $C_{jgt} \in \{C_{\text{pre-algebra,g,t}}, \ldots, C_{\text{geometry,g,t}}\}$.

The key assumption in measuring C_{jgt} is that the more difficult the course is the higher the average pretest score is for all the students in the district who take that course. C_{jgt} is meant to reflect the target level of instruction, and it is measured by calculating the average district-wide *initial* Stanford 9 test score of students who took course *j* in grade *g* and year *t* (i.e. $C_{jgt} = \dfrac{\sum_{\substack{i \in g\text{th graders} \\ \text{in the district} \\ \text{who took } j \\ \text{in year t}}} x_i}{\text{Number of } g\text{th graders in the district who took } j \text{ in year t}}$), where x_i is student *i*'s test score in prior year. For instance, $C_{8,\text{algebra},2000}$ is constructed by using average 1999 Stanford 9 test score of the students in the district who took Algebra in grade 8 in 2000. Table 3 shows C_{jgt} for each grade and year. It also shows C_{gj} (a long-run average of a degree of difficulty of a course *j*), which is measured by averaging C_{jgt} across all available years for each grade and course.

[40] The number of courses per year is calculated by the number of course codes offered in the first term of the academic year.

[41] In counting the number of courses offered, I exclude those course codes that have fewer than 30 students enrolled districtwide.

Practically, schools may have means other than what math courses to offer to improve students' learning under an ability-grouping practice. It could be that students are presented with the same course materials but taught at a different length or style. I do not observe these different pedagogical approaches across ability groups.[42] To the extent that pedagogical styles within *C* differ, difficulty level of predicted coursework would be a noisy measure of the true difficulty and it will tend to find weaker effects of coursework.

(3) The range of math courses offered by each school *(C_Range, Rank_Range)*

Schools in the SDUSD can decide which courses they offer to students given set of courses offered by the district. I construct a measure of the range of math courses (*Range*) that vary by school and grade.

One possibility is to take a simple difference between the highest and the lowest C_{jgt} within a grade/year/school ($C_{g,Hardest} - C_{g,Easiest}$). But this measure does not take into account information such as how students are allocated to different classes or what portion of students has access to the hardest and the easiest. There is difference between when the hardest course only available to top 1% of students and when it is available to half of students. Therefore, the preferred measure is to take a mean absolute deviation of C_{jgt} within a grade/year/school. I call this measure *Range*. I first

[42] Talking to Math program manager at the SDUSD, math courses such as "Advanced Algebra" and "Algebra" would have a different course code but use the same textbook. So it is likely that different course code reflects the difficulty level of instruction.

calculate school-wide average of C_{jgt} and take the mean deviation of C_{jgt} from this average.

(4) Degree of ability grouping *(AbilityGroup)*

AbilityGroup is a variable that takes a value between 0 and 1 and increases as the degree of ability-grouping within school is higher. I first construct *AbilityGroup* for each school, grade and year.

I follow Lefgren (2004)'s approach to measure the degree of ability grouping for each school/grade/year. The approach uses the degree to which the classroom of a student can explain his/her initial test score, which can be measured by R-square obtained from regressing prior student test score[43] on classroom fixed effects (I assign *AbilityGroup*=0 when there is only one class in a respective school/grade/year cell). This is effectively a measure that tells us how much variation in students' initial test score is explained by classroom fixed characteristics. Since R-square necessarily increases with the number of classrooms in a school, I also construct adjusted R-square that adjusts for degree of freedom, as an alternative measure of the degree of ability grouping. The correlation coefficient of R-square and adjusted R-square are very high, above 0.99. Figure 2 shows a graphical example of a cohort who experienced high tracking in grade 6 and low tracking in grade 7.

[43] Prior test score for 8th graders can be the test score in 5th, 6th and 7th grade, 7th graders can be in 5th and 6th grade, and 6th graders can be 5th grade test score. I constructed all measures for now.