Essays on Access to Education

Harold Stolper

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ABSTRACT

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This dissertation uses survey data and administrative data to explore persistent barriers in access to education. The first chapter explores how constraints on credit supply can impact the level and distribution of higher education, including access to selective and 4-year colleges. I exploit a 2003 Texas constitutional amendment that provided plausibly exogenous variation in access to home lending markets for Texas homeowners, without affecting credit access for renters, or homeowners in other states. By comparing outcomes between groups, I show that this led Texas homeowners to send their children to more selective colleges and spend $4,500 more in tuition (net-of-aid) per line of credit. In the presence of college supply constraints, homeowners' increased demand for institutions higher in the college selectivity hierarchy forced some renters to attend less selective colleges, and others to forgo college altogether instead of attending less selective colleges. In addition, selective colleges capture some of the private credit supply shock through price-discrimination, raising tuition and shifting aid towards remaining renters. On net, the availability of home equity financing reinforced gaps in access to higher education.

These results inform our understanding of how inequality in college access is generated and transmitted from parent to child: the availability of home equity credit reinforces gaps between homeowning and renting families, and it does so through two distinct mechanisms. First, constraints in credit access are relaxed for homeowners, allowing them to ascend the college quality hierarchy. Second, due to college supply constraints, the gains to homeowners crowd out some renters from making otherwise privately optimal investments. By documenting important distributional effects on renters, this paper informs our interpretation of previous research: increases in college choice for one group may come in part at the expense of another group.

The results of the first chapter also demonstrate how the more selective colleges are able to capture some of the gains from cheaper credit by price-discriminating by homeownership status. The net effects of subsidized home lending markets and federal aid policy on college access are not immediately clear: on one hand, homeowners are sending their children to better colleges, but they are paying higher net prices at these colleges than they would in the absence of the private credit supply shock. On the other hand, tuition increases for renters who remain enrolled at selective colleges are offset by increases in institutional aid, but some renters are pushed down the college quality hierarchy and displaced from college altogether.

The relationship between housing markets and access to higher education is also explored in chapter three, which examines the effect of metropolitan house price shocks on college enrollment patterns across cities. This chapter begins by presenting a simple theoretical model to illustrate the mechanisms through which parental housing wealth can ease educational borrowing constraints for their children. The model highlights how house price growth can reinforce inequality in future generations: increases in the value of parental housing collateral can ease educational borrowing constraints for children, but the indivisibility in owner-occupied housing limits exposure to this externality to higher-income families. The second part of this chapter presents estimates of the relationship between house price shocks and changes in college enrollment at the MSA level. The empirical results confirm that house price growth leads to higher college enrollment rates (and vice versa for declining house prices), but these effects are concentrated in metropolitan areas with lower house price levels. There is only weak evidence that house price growth leads to increases in housing-related employment among college-aged individuals.

The second chapter of this dissertation examines the effect of policies used to reclassify non-native English speakers (English Learners, or ELs) in the Oakland Unified School District into mainstream classes. Despite a heightened policy debate surrounding appropriate instructional policies for the large and growing number of non-native English speaking students nationwide, policymakers have limited causal research available on the effects of reclassification policy. This paper addresses some of the gaps in the empirical literature on reclassification by exploiting exogenous variation in the probability of reclassification introduced by the multiple criteria students must meet to be eligible for reclassification. It begins with a conventional regression discontinuity (RD) design that estimates the short and long-term effects of reclassification for non-native English speakers who have met all reclassification criteria except potentially one. These students exhibit large jumps in the probability of reclassification around relevant test score cutoffs. The RD estimates suggest that reclassification has very limited effects on students at the margin, but that the timing of reclassification may indeed matter, though not necessarily through effects on student learning. There is some suggestive evidence of increases in SAT-taking and four-year college enrollment, but limited statistical power prevents definitive conclusions regarding small changes in long-term outcomes.

Motivated by the limitations inherent to RD designs that estimate treatment effects for students at relevant cutoffs, chapter two also presents an extension to the conventional RD design in order to draw conclusions about the effects of reclassification for students whose reclassification scores place them well above the cutoff. The framework we present exploits the fact that some students who meet the first cutoff will remain untreated due to being below the cutoff for a second running variable. These untreated students provide additional information on the relationship between reclassification test scores and outcomes, which can then be used to inform our expectation of counterfactual outcomes for reclassified students in the absence of reclassification. More specifically, we can use this information for EL students who were not reclassified to estimate outcomes for reclassified students in the absence of reclassification under a straightforward separability assumption that can be examined in the data. We show this assumption holds in the data, before estimating the relationship between outcomes and reclassification test scores for non-reclassified students and using these estimates to predict outcomes for reclassified students in the absence of reclassification. Estimates of the effect of reclassification for any score above the cutoff can then be obtained by comparing the prediction to the observed value for reclassified students. Beyond the immediate application to reclassification policy in Oakland Unified, the framework we introduce for estimating treatment effects above the cutoff can apply to any setting where treatment status is based on multiple criteria.

The resulting estimates imply that for all students in elementary school who were above the cutoffs, the average effect of reclassification into mainstream classes on English language arts (ELA) scores is a 0.182 standard deviation increase in their ELA score in the following year. These results imply that the CST ELA cutoff should not be raised for students in grades 3 through 5, as benefits accrue to students above the current cutoffs and in mainstream classrooms. Without knowing how students below the current cutoff are impacted by reclassification, however, we cannot say whether policymakers should consider lowering the criteria.

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1 Home Equity Credit and College Access: Evidence from Texas Home Lending Laws

1.1 Introduction

A growing literature suggests that college quality affects labor market earnings (e.g. Black and Smith 2004, 2006; Hoekstra 2009; Andrews, Li, and Lovenheim 2012). This can reinforce inequality in lifetime earnings between individuals who are able to access more selective colleges and those who are not. At the same time, tuition growth at selective colleges has outpaced financial aid, and most families must borrow to finance college.[1] Thus insufficient access to low-cost credit may constrain some students' ability to access selective colleges.

For policymakers aiming to remove barriers to college degrees that offer high returns, the key question is which barriers are most salient, and how much would it cost to reduce them? There is some evidence that access to credit is an important barrier. For instance, federal student loans—loans that are typically disbursed through colleges—have been shown to have positive impacts on college attendance in general, and at 4-year colleges in particular (Reyes 1996; Dynarski 2002; Dunlop 2013).[2] But there is also substantial variation in students' access to private credit through home lending markets, and comparatively little is known about the effects of this type of credit channel.[3] In addition, institutions may be aware of variation in private credit supply and make strategic adjustments to financial aid that affect college access for some groups.[4]

This paper explores how constraints on credit supply can impact the level and distribution of higher education, including access to selective and 4-year colleges. Specifically, it estimates how enrollment decisions across all potential students respond to an increase in private credit access for one group. I exploit a sharp change in access to private credit markets in Texas;

[1]According to data from the Dept. of Education's National Postsecondary Student Aid Study, the net price of attendance (defined as tuition and fees less institutional aid) at colleges with selective admissions rose 30 percent from 1999-2000 to 2007-08. Between 2003-04 and 2007-08, the share of undergraduates who borrowed to pay for college rose from 46% to 51%.

[2]Federal students loans are offered to eligible college students through Title IV of the Higher Education Act of 1965 (and subsequent amendments), which guarantees repayment to the lender if the student defaults.

[3]In the 2007 Survey of Consumer Finance, more than one-third of homeowners with college-aged children had loans issued through private borrowers and secured by their home equity ("home equity loans").

[4]Long (2004) and Turner (2012) show that colleges capture a portion of federal/state aid by price-discriminating based on need-based eligibility for grant aid. This paper explores whether colleges price discriminate based on eligibility for private loans.

this was triggered by a 2003 constitutional amendment that allowed homeowners to secure home equity lines of credit (HELOCs). HELOCs are revolving lines of credit secured by the value of one's home equity, and are frequently utilized by homeowning families to finance college. Despite widespread use of HELOCs in the rest of the country, they were effectively unconstitutional in Texas until the passage of Proposition 16 in September of 2003.

I exploit this policy change as a source of plausibly exogenous variation in the availability of HELOCs along three key dimensions: temporal, geographic and by homeownership status. These three dimensions define different groups that vary in their exposure to HELOC eligibility—homeowners in Texas, homeowners in other states, and renters—which allows for several counterfactual exercises. First, I use individual-level data to compare the evolution of college investment levels among homeowners (renters) in Texas to homeowners (renters) in other states using difference-in-difference (DID) methods. Second, I consider the gap between homeowners and renters (e.g. the gap in average college sticker price) and compare the evolution of this gap in Texas to that in other states using triple difference (DDD) methods.[5]

I rely on data from two separate national surveys (the Current Population Survey and the National Postsecondary Aid Study) which provide repeated cross-sectional data on parents' homeownership status linked to their child's college enrollment and college choice. While I cannot identify which homeowners take out HELOCs, I can estimate average impacts among all homeowners (and renters) in an intent-to-treat approach. On the extensive margin, the enrollment data permit estimation of the effect of HELOC-availability on overall enrollment rates by homeownership status. On the intensive margin, the college choice data permit estimation of the effects on college choice conditional on attending college.

The first contribution of this paper is descriptive: I document how home-owning families can lower their cost of capital using HELOCs. Among the 18% of families who had HELOCs in 2007, more than three-quarters had HELOCs with nominal interest rates lower than those they faced on federal student loans.[6] The tax deductibility of HELOC interest

[5]Sticker price is defined as tuition and fees paid; net price subtracts institutional aid from sticker price.
[6]Seventy-eight percent had HELOCs with interest rates lower than fixed-rate federal PLUS Loans at

payments lowers the effective rate by an additional 2 percent for the median HELOC-holder. Additionally, a line of credit can save on the fixed costs (time and fees) associated with arranging multiple lump-sum loans over the duration of a college degree. These observations support the longstanding view that home equity is likely the cheapest source of capital after subsidized federal loans (Dynarski 2005 and Kane 1998), and for some families can provide even cheaper credit than subsidized loans.

The second contribution is to present arguably causal estimates of the effects of access to home lending markets on college enrollment, college choice, and net price. I find that despite no change in their overall enrollment rate, access to HELOCs increases the likelihood that homeowners enroll at 4-year colleges (7.2 percentage points relative to other states), the most selective college tier (2.1 percentage points relative to other states), and Texas flagships (3.1 percentage points relative to Texas renters).[7] As they ascend the college selectivity hierarchy, homeowners spend more on college—roughly $4,500 per line credit.[8]

In other words, HELOCs shift out the demand for better colleges among homeowners. In the absence of college capacity constraints, this might have little effect on renters. Specifically, if prices increased in response to greater demand then some renters might be priced out. However, selective colleges can price discriminate based on homeownership status. Thus renters may not in fact experience higher net prices, and their enrollment may be unaffected.

In a more realistic model, capacity constraints place limits on enrollment in the 4-year college sector; Bound and Turner (2007) argue that college capacity has remained largely fixed since the 1970s, a reversal of earlier expansion during the 1950s and 1960s. Stagnant capacity coupled with a growing pool of Texas high school graduates suggests the existence of supply constraints at Texas colleges.[9]

I show that in the presence of these capacity constraints, increased demand for selective

8.5%; and 23 percent faced HELOC rates below the fixed Stafford rate of 6.8%.

[7]The University of Texas at Austin and Texas A&M University are the two Texas flagship universities.

[8]This assumes that Texas homeowners take up HELOCs at the same rate as homeowners with college-aged children nationwide in the 2007 SCF (26.4%).

[9]The number of rising high school graduates in Texas increased by 25.8% from 2001-02 to 2005-06, based on NCES' Common Core of Data.

colleges among homeowners pushes some renters down the college quality hierarchy.[10] I find that some renters even forgo college altogether. Specifically, the overall renter enrollment rate drops by 6 percentage points relative to other states, and conditional on attending any college their likelihood of enrollment drops at the most selective colleges by 0.6 percentage points, and by 4.6 percentage points at non-selective 4-year colleges.

The finding that some renters forgo college altogether rather than attending less selective colleges could arise for several reasons. One explanation is that the return to college may fall with reduced college selectivity more quickly for low-income students than for higher income students (Dale and Krueger 2002). It is also possible that low-income renters only apply to a limited set of colleges (Hoxby and Avery 2012), and if they aren't admitted because of college supply constraints then employment may be their only option. The finding of renter crowd-out is consistent with Zimmerman (2014), which argues that supply constraints at public colleges bind, preventing students from making investments that would have high economic returns.[11]

Thus increases in homeowner enrollment at the top and bottom of the 4-year college quality hierarchy are being partially offset by decreases in renter enrollment (even though renters do not face higher net prices). Additional results show that the most selective Texas colleges are able to price-discriminate based on homeownership status: they raise tuition for all students by roughly $2,000 (relative to colleges in other states), simultaneously re-allocating institutional aid from homeowners to renters to cover this tuition increase.[12]

The identifying assumption behind the above results is that there are no confounding

[10]Similarly, Bound and Turner (2007) show that increases in college demand owing to larger cohort sizes may crowd some individuals out of college when public funding doesn't increase in proportion to demand.

[11]Feigenberg (2014) finds crowd-out in a different educational setting by showing how community-level income shocks in Chile generate decreases in enrollment for middle-income students at higher quality private schools (primary and secondary). He finds that the drop in enrollment is driven by school market power and parental preferences for quality, characteristics which are shared by the U.S. higher education system.

[12]While the Federal Application for Student Aid (FAFSA) does not ask students for information on their home equity, many selective colleges rely on alternative aid calculators that incorporate home equity, such as the College Board's College Scholarship Service PROFILE (CSS) and Section 568's "Consensus" formula. As of the 2014-15 academic year, Texas colleges that use CSS for domestic students include Baylor University, Rice University, Southern Methodist University, Texas Christian University and Trinity University.

shocks affecting college investment among Texas homeowners that are not shared with Texas renters or homeowners in control states, conditional on family income, individual ability and state-level shocks to housing prices and mortgage rates. It also assumes that the homeownership rate in Texas does not respond to the policy change.

To explore these assumptions, I perform comparisons with the counterfactual drawn from a set of control states designed to mimic the evolution of income inequality in Texas between homeowners and renters. To test the robustness of the results against an alternative counterfactual, I also use a weighted combination of states to construct a "synthetic" control group using the methods of Abadie, Diamond, and Hainmueller (2010). This synthetic control group is constructed to match relevant characteristics of Texas prior to Proposition 16. Additional robustness tests comes from comparing Within-Owner DID results that exclude renters (and thus renter neighborhoods) to DDD results estimated over homeowners and renters. An alternative explanation posits that Texas colleges faced confounding budget pressures, prompting them to raise net price and target homeowners with a greater willingness to pay; however, it is reassuring that public college funding in Texas evolves very similarly to in other states. To confirm that the estimates reflect the effects of HELOC-eligibility through a borrowing channel, I also show that impacts are concentrated among homeowners most likely to have HELOCs, and analyze impacts on non-college spending.

These results inform our understanding of how inequality in college access is generated and transmitted from parent to child: the availability of home equity credit reinforces gaps between homeowning and renting families, and it does so through two distinct mechanisms. First, constraints in credit access are relaxed for homeowners, allowing them to ascend the college quality hierarchy. Second, due to college supply constraints, the gains to homeowners crowd out some renters from making otherwise privately optimal investments.

This paper also relates to a recent literature finding positive effects of housing price shocks on college enrollment and choice for homeowners (Lovenheim and Reynolds 2013; Lovenheim 2011). These studies exploit local housing booms that generate wealth effects in addition to easing liquidity constraints. In contrast, this paper emphasizes the effects of credit access by exploiting variation in home equity loan eligibility. By documenting important distributional effects on renters, this paper also informs our interpretation of previous research: increases in college choice for one group may come in part at the expense of another group.

The rest of the paper is organized as follows. Section two describes how HELOCs impact college financing costs. Section three describes the policy change, discusses identification, and presents a simple model to inform empirical predictions about the response of college decisions to a credit supply shock affecting a subset of the student population. Section four describes the data, tests the empirical predictions, and describes the empirical methods. The results are presented in sections five (household responses) and six (institutional responses) before concluding in section seven.

1.2 HELOCs and the Cost of Capital

The three most commonly cited advantages of HELOCs in the 1997 Survey of Consumer Finance were “convenient to use,” “tax advantage,” and “low interest rate.” This section demonstrates these advantages in further detail.

1.2.1 Fixed Cost Savings

HELOCs function like credit cards but with lower interest rates and different default provisions. Brito and Hartley (1995) emphasize that lines of credit can save borrowers on the fixed transaction costs of arranging loans (in the form of origination fees and time costs). Once a line of credit is secured, consumers can finance spending over time without repeatedly incurring fixed loan costs (e.g. when securing student loans at multiple points in time over the duration of a 4-year college education).[13] The introduction of HELOCs thus reduces college financing costs in the face of uncertain consumption or income flows and fixed loan trans-

[13]Marx and Turner (2014) introduce a conceptual model of students' human capital decisions that formalizes the role of fixed loan costs in the presence of loan and grant aid programs. Their empirical findings suggest these fixed costs have economically meaningful impacts on educational attainment and indebtedness.

action costs. Because uncertainty is greater when looking further into the future, HELOCs should lower financing costs for large investment goods spread out over several years, such as college degrees and vehicle purchases. On the other hand, small predictable purchases (e.g. food) are less sensitive to uncertainty in consumption or income. This reasoning predicts that food expenditures should not be affected by HELOC-availability whereas vehicle purchases may increase; these predictions are confirmed in Section 1.9.2 of the Appendix.

1.2.2 HELOCs versus Student Loans

In addition to fixed cost savings, HELOCs can offer several other advantages relative to federal student loans: (1) interest rate savings; (2) tax deductible interest payments; and (3) bankruptcy provisions that are less restrictive than on federal student loans. Table 1.1 compares the features of HELOCs to student loans offered through the two main federal undergraduate loan programs: Stafford loans and PLUS loans.[14] Federal student loans consist of subsidized Stafford loans up to the allowable amount, beyond which students can take out unsubsidized Stafford loans up to the aggregate Stafford limit, followed by PLUS loans up to the cost of attendance. The interest on subsidized loans is paid by the federal government while the student is in school. Dependent students faced an aggregate limit for all Stafford loans of $23,000 from October 1993 through July 2008.[15] PLUS loans are the only federal student loans that can be taken out in either the student's or a parent's name.[16]

Among homeowning families with college-aged children (17-25 years old), 23.2% of homeowning families with college-aged kids have HELOCs, not far below the 27.2% with student loans. Table 1.2 shows that HELOCs are most common among middle and upper income quintiles. Table 1.3 confirms that HELOCs allow families to borrow large amounts if nec-

[14]Loan data comes from the 2007 SCF. Historical student loan information was taken from finaid.org.

[15]Since July 1, 1994, independent undergraduate students and dependent students whose parents were denied a PLUS loan were allowed an additional $23,000 in unsubsidized Stafford loans, facing a combined aggregate Stafford limit of $46,000. Roughly 37% of PLUS loan applicants were denied in 2007-08, representing 6.3 percent of all undergraduates (Kantrowitz 2009).

[16]PLUS Loan rejection rates spiked in 2012 after the Department of Education changed their underwriting standards, with loan denials disproportionately affecting students at historically black colleges (Fishman 2014). This has implications for the importance of private credit going forward.

essary, with a median limit of $50,000. While federal student loans have had fixed interest rates since the 2006-07 academic year, HELOC interest rates vary with credit history. Table 1.3 also summarizes the distribution of HELOC rates across the income distribution in 2007; the median HELOC interest rate for all families is 8 percent, and within every income quintile more than half of HELOC-holders have lower rates than fixed rate PLUS loans at 8.5%.

The value of the tax deduction rises with family income: the median effective interest rate (inclusive of the federal income tax savings) falls from 8 percent in the bottom income quintile to 6 percent in the top income quintile.[17] Among HELOC-holders with college-aged children, median household income in 2007 was $95,702 (the 78th percentile of household income in the US). At this income level, deducting interest payments of $960 (based on the median HELOC balance of $12,000 times the median interest rate of 8 percent) for an itemizing married couple reduces the effective HELOC interest by 2 percent.

HELOCs can thus lower the cost of capital for many homeowning families, especially for students at more costly degree programs who must rely on more than subsidized student loans; and for upper-middle class families who stand to benefit the most from the tax savings.

1.3 Identification

This section discusses the plausibility of interpreting Proposition 16 as a natural experiment. It begins with a brief history of home lending laws in Texas, before reviewing the impact of Proposition 16 on HELOC activity and some of the major forces driving home lending reform in Texas. Lastly, it presents a simple motivating model to inform empirical predictions about the response of college investment levels to a credit supply shock affecting a subset of the student population.

[17] These figures are based on annual HELOC interest payments totalling $960 (8 percent × $12,000) for a family at the median household income within each income quintile. Federal income tax savings were computed using NBER's TAXSIM tax calculator for 2007, a married family in Texas with one 18 year old child and other itemized deductible expenses totaling $10,000 (exceeding the standard deduction of $9,600).

1.3.1 Home Lending Reform in Texas

Texas has a history of legal restrictions on home lending dating back to the Texas Homestead Act of 1839 which exempted the family home from the claims of creditors. This protection was carried over to the State's founding Constitution in 1845. Article XVI, Section 50 of the Texas Constitution of 1876 protected homesteads from foreclosure except for failure to pay the original home purchase loan or debt incurred to finance home improvements. This effectively prohibited home lending by eliminating the collateral value of housing for creditors. Abdallah and Lastrapes (2012) explain that because this restriction was embedded in the state constitution, it has been difficult to relax, in spite of home lending innovations available to homeowners in the other 49 states.[18] Homes were protected from forced sale until Proposition 8 was approved by voters in 1997, allowing lump-sum home equity loans (but not lines of credit) without restriction on how the proceeds were to be used. Prior to the passage of Proposition 8, homeowners were allowed to refinance only their current loan balance, thus ruling out "cash-out" refinance.

By the fall of 2002, the Texas Credit Union League (TCUL) had begun a campaign to push for further home equity reform, including lines of credit. Other features of the reform efforts included reverse mortgages, designation of a state agency to issue home equity rules and interpretations, and allowing for more flexible loan repayment options. In March of 2003, the state comptroller issued a report in support of home equity lines of credit, and the legislature passed the TCUL proposal with SJR 42 and SB 1067. The corresponding amendment, Proposition 16, was approved with 65 percent of the vote on September 13, 2003. Subsequent amendments in 2005 and 2007 made minor changes to reverse mortgage agreements and instituted additional consumer protections, respectively.[19]

[18] Section 50 was only amended prior twice before 1997, extending homestead foreclosure protections to single adults in 1973, and exempting from protection debts related to purchasing an undivided interest in the homestead (related to divorce proceedings) in 1995.

[19] Other constitutional amendments were reviewed for confounding policy changes. Between 2001 and 2005, only three amendments concern home lending; all three concern reverse mortgages which only affect elderly households whose children are beyond the typical age of college entry.

1.3.2 "First-Stage" Impacts on Home Lending in Texas

The ban on HELOCs was lifted effective September 29, 2003, though administrative interpretations by the Texas Finance Commission and Credit Union Commission were adopted on December 18, 2003 and February 20, 2004. While there is no publicly available data that reliably reports information on the number of HELOCs in Texas, regulatory data maintained by the Federal Deposit Insurance Commission (FDIC) can be used to track home equity lending at banks based in Texas.[20] Figure 1.1 plots the sum of outstanding HELOC loans and unused HELOC commitments by quarter for Texas and the rest of the nation, indexed to pre-Proposition 16 levels (the third quarter of 2003). It is also restricted to "small" institutions (with less than $1 billion in assets) more likely to lend in-state, though results are not particularly sensitive to this asset threshold. A stable pattern of HELOC growth is evident in other states over the period, while Texas exhibits a pronounced increase beginning around the second quarter of 2004 and peaking towards the end of the housing boom in 2006.

If households (and colleges) were not anticipating the passage of Proposition 16, then from the standpoint of college investment the effective date of the policy change would be the following academic year (Fall 2004). Results presented in Section 1.5 confirm sharp changes in college enrollment patterns beginning in the 2004-05 academic year, with no anticipatory effects in earlier years. Accordingly, the remainder of the paper generally focuses on the period spanning from the 1999-2000 academic year through the 2007-08 year, with treatment beginning in 2004-05. This allows for the analysis to begin several years after lump-sum home equity loans were introduced in Texas in 1997, while largely excluding college-going decisions made after dramatic housing market changes towards the end of 2007.

[20] The U.S. Census Bureau asks households about home equity lines of credit in the American Housing Survey, but the data suggests there was confusion by respondents in terms of differentiating between lump-sum home equity loans and lines of credit. This view was also articulated in Special Report issued by the Texas comptroller's office in March 2003.

1.3.3 Proposition 16 and Identification

In order to determine whether Proposition 16 provides plausibly exogenous variation in HELOC-eligibility, it is important to consider the forces behind the policy change. A review of government press releases and news reports suggest that the push for home equity lines of credit—and home equity reform in general—was largely framed as an issue of consumer choice: why shouldn't Texas citizens have access to the same home lending innovations available in the other 49 states?[21] Abdallah and Lastrapes (2012) argue that the timing of home equity reform was heavily influenced by the Tax Reform Act of 1986, a 1994 circuit ruling, and growing Republican influence in Texas—none of which have any obvious relationship to college investment of local credit demand. After practical issues with lump-sum home equity loans and reverse mortgages were resolved around the end of the decade, the ban on home equity lines of credit remained as the last major home lending restriction for Texas citizens. I argue that lifting the restriction on HELOCs was in a sense the inevitable conclusion to home equity reform in Texas, irrespective of local credit demand. While it is impossible to definitively determine whether the last round of major reforms was accelerated by rising local credit demand, publicly the reform efforts emphasized equity in consumer choice.

1.3.4 Testable Predictions

How might the distribution of higher education change in response to a credit supply shock for homeowners? Consider a model in which 4-year colleges engage in third-degree price discrimination by conditioning net price on homeownership; the elasticity of demand for homeowners exceeds that of renters, which 4-year colleges (but not 2-year colleges) are able to observe. In practice, colleges may be maximizing some quality function that takes student characteristics as inputs. For present purposes, I abstract away from the quality tradeoff and assume that colleges admit all students above some ability admissions threshold (as in Epple, Romano, Sarpça and Sieg 2013). Four-year colleges act as monopolistic competitors,

[21] See, for example, Combs (2003) and Sopensky (2003)

segmenting the market into homeowners and renters. Marginal costs are assumed to rise with college selectivity, and are thus highest at colleges in the most selective 4-year college sector and lowest at 2-year colleges. To fix ideas, I make the simplifying assumption that there are a fixed number of enrollment slots available at each 4-year college, while 2-year colleges are perfectly enrollment elastic and cannot price discriminate; in practice, Bound and Turner (2007) argue that enrollment elasticities rise as college selectivity falls.

In this basic setup, the marginal cost of a student does not depend on whether the student comes from a family that owned or rented. In this case, the institution would allocate its given number of slots sequentially to the higher marginal revenue sector (own vs rent), as shown in the top panel of Figure 1.8. The final allocation will equalize the marginal revenue from each sector. Net prices would be then given by the respective demand curves at this allocation (NP_O^* and NP_R^*). The last step is for the institution to verify that the final MR^* equals or exceeds marginal cost at the full allocation of students. If not, the institution should leave slots unfilled until this condition is satisfied.

The bottom panel of Figure 1.8 depicts the equilibrium after an outward shift in homeowner demand and marginal revenue, as the reduction in borrowing costs raises homeowners' return to college (net of financing costs). In the new equilibrium, final marginal revenue is higher as institutions charge a higher net price to both homeowners ($NP_O^{'} > NP_O^*$) and renters ($NP_R^{'} > NP_R^*$) and admit a greater share of homeowners ($ADMIT_O^{'} > ADMIT_O^*$).

Renters who are displaced from the top of the hierarchy will filter down the college selectivity hierarchy. If increased homeowner demand displaces renters from the 4-year college sector altogether, this will shift out renter demand for open enrollment 2-year colleges as depicted in Figure 1.8. Any renters who are displaced from the 4-year sector will be accommodated in the enrollment-elastic 2-year sector as 2-year renter enrollment increases from $ADMIT_R^0$ to $ADMIT_R^1$ with no effect on net price. The impact of the credit supply shock on homeowner demand in the cheaper 2-year sector is less obvious; it depends on whether the return to these non-selective colleges (net of financing costs) rises for individuals who would otherwise enter the workforce. Homeowners thus filter upwards, with increases in homeowner enrollment at 4-year colleges and an ambiguous effect on enrollment at 2-year colleges and overall enrollment rates. These predictions are tested in the following section.

Previous research by Dale and Krueger (2002, 2011) finds that the return to college quality is greatest for students from disadvantaged backgrounds (e.g. renters). To the extent this holds true, then renters who are displaced from more selective colleges face lower returns in less selective colleges. Zimmerman (2014) finds empirical support for displacement effects in the presence of binding supply constraints: individuals who score just below the admissions threshold at a large state university experience lower levels of college attainment and subsequent earnings. It is thus possible that for some renters displaced from 4-year colleges, their return to (a less selective) college would fall below their return to full-time employment, resulting in decreases in the overall renter enrollment rate.

1.4 Empirical Strategy and Data

This section provides a description of the data sources as well as the difference-in-difference (DID), triple difference (DDD) and synthetic control methods. Under the assumption that Proposition 16 can be interpreted as a natural experiment and the timing of the event is well-defined, the effects of introducing HELOCs can be estimated by comparing college-going outcomes in Texas before and after the amendment to an appropriate control group.

1.4.1 Data

The empirical strategy requires individual-level data on college-going decisions, including college enrollment, sticker price and net price, institutional type and selectivity, coupled with enough family background information to determine homeownership status and state-of-residence.

Enrollment Data (Extensive Margin) College enrollment data is taken from the March Supplements of the Current Population Survey (CPS), with enrollment defined as part- or full-time enrollment in any college (or completion of at least a 2-year degree program). I observe 18-22 year olds in March of every year, assigning them to cohorts based on their anticipated year of college entry: e.g. 18 year olds observed in March of 2005 are assigned to the 2004-05 college entry cohort (the first treated cohort), while 19 year olds observed in March of 2005 are assigned to the 2003-04 college entry cohort (the last pre-treatment cohort).[22] The top one percent of the national income distribution is dropped in order to focus on students who are constrained in their ability to finance college out of current income. The enrollment estimation sample begins with the 1999-2000 college entry cohort and ends with the 2007-08 cohort, totaling 73,355 students across all states, 2,337 Texas homeowners and 1,743 Texas renters.

College Choice Data (Intensive Margin) Data on college choice is taken from the National Postsecondary Aid Study (NPSAS), a national survey of enrolled college students released by the National Center for Education Statistics (NCES) every four years. NPSAS provides a rich set of information on students' college experience and finances (including an institutional identifier), with sufficient sample size to focus on the state of Texas. The survey collects student information from a variety of institutional sources and the FAFSA. I use the 2003-04 wave as the pre-treatment period (excluding students who began college in the Spring semester of 2004 after HELOCs were available in Texas) and the 2007-08

[22]This is a conservative approach, because any impacts on the children of homeowners who were observed after the credit supply shock but turned 18 before the credit supply shock are assigned to the pre-treatment group. In other words, if a 20-year old observed in March of 2005 is enrolled it will boost pre-treatment enrollment rates, even if they were induced to enroll after the credit supply shock. The extent of any downward bias in the corresponding estimates is likely minimal, however, because homeowners are less likely to be on the margin of enrollment. The major disadvantage to this approach is that treatment status is also based on homeownership status, which may have changed between the time of the CPS observation and the timing of college enrollment. The potential for bias introduced by measurement error in homeownership is discussed further in Section 1.5.

wave for the post-treatment period (excluding children who began college before 2004).[23,24] For dependent students, their homeownership status and state of residency is determined by their parents; for independent students their own information is used.[25] Because the HELOC option is only relevant for households that cannot finance entirely out of their savings and current income, I only consider students who applied for some sort of financial aid.[26] The analysis is also restricted to students aged 25 and younger who permanently reside in the United States or Puerto Rico.[27] The 2007-08 NPSAS wave covers 113,500 undergraduate students in total, up from 79,900 in the 2003-04 wave, 50,000 in 1999-2000 and 41,500 in 1995-96. DID and DDD models are only estimated over the 2007-08 and 2003-04 data in order to obtain sufficient sample sizes to support a rich set of fixed effects. The synthetic control group method uses data from all four NPSAS waves, aggregated to the state level.

Outcome variables derived from the NPSAS data include sticker price and net price. NPSAS also includes an institutional identifier which is used to merge additional data sources: (1) IPEDS provides information on median composite SAT scores for an admitted class; and (2) the Barron's Selectivity Index assigns an ordinal selectivity category to 4-year institutions based on a function of SAT/ACT scores among accepted students, admission rates, and class rank and GPA required for admission.[28] For the student-level college

[23]All specifications include a control for class year to address imbalances in class composition between NPSAS waves in the analysis sample.

[24]While it is unfortunate that there NPSAS data is not available between the 2003-04 and 2007-08 waves, it is fortunate that the timing of these waves covers a period before and after Proposition 16 without being subject to the effects of confounding policy changes from the previous decade. Affirmative action in admissions was banned in 1996. Beginning in 1998, the Ten Percent Rule guaranteed admissions to all public universities for Texas high school graduates in the top ten percent of their graduating class. It was followed shortly after its adoption with various targeted scholarship and recruitment programs (e.g. the Longhorn Opportunity Scholarship and Century Scholarship programs).

[25]The response rate to the NPSAS homeownership question does not appear to be influenced by strategic concerns; there are no missing observations in the 2003-04 wave, and only a small number of missing observations in the 2007-08 wave for independent students only (representing 3.3% of respondents).

[26]The general pattern of the estimates is similar when estimated over all students and not just aid applicants, but precision is reduced. Table 1.5 confirms that treatment status is not correlated with the financial aid decision.

[27]For federal aid purposes, all students under the age of 24 are considered dependents unless they are enrolled in graduate school, are married or have their own dependents, are an orphan, have active/veteran military status or are emancipated minors. For the final analysis sample of younger aid applicants, 82 percent are listed as dependents.

[28]Additional details on these variables can be found in the Data Appendix, Section 1.9.1.

spending data, the final estimation sample of students in all states with non-missing tuition and homeownership status includes 85,460 students for the 2003-04 and 2007-08 waves (rounded to the nearest 10, per NCES requirements). This includes 2,190 Texas homeowners in 2007-08 and 2,080 in 2003-04, as well as 1,010 Texas renters in 2007-2008 and 900 Texas renters in 2003-04.

1.4.2 Descriptive Results

Before describing the empirical strategy in detail, consider the empirical predictions from the stylized model of Section 1.3.4. The available data supports estimation of the effect of Proposition 16 on overall enrollment rates for homeowners and renters, and measures of college choice conditional on any college enrollment. It does not, however, support estimation of 4-year (or 2-year) college enrollment rates without conditioning on any enrollment. Instead, sampling weights from NPSAS can be used to obtain estimates of the number of homeowners (and renters) enrolled at 4-year colleges and at 2-year colleges (in Texas and in other states). To proxy for enrollment rates, these figures are divided by the number of 18-25 year olds (in homeowning or renting families) obtained from the March Supplements of the CPS.

Evidence of a homeowner demand shock can be found in Figure 1.4, which shows the change in imputed enrollment rates between 2003-04 and 2007-08 at 4-year colleges by homeownership status. The homeowner 4-year college enrollment rate rises by 1.4 percentage points in Texas, while only rising by 0.04 percentage points for non-Texas homeowners. On the other hand, the renter 4-year college enrollment rate fell by 1.2 percentage points in Texas, but exhibited no change in other states. These findings are consistent with renter crowd out at 4-year colleges in favor of homeowners. Additional evidence of the homeowner demand shock is found in Figure 1.5, which shows a roughly $1,000 increase in net price for Texas homeowners, with smaller increases of roughly $600 for all other groups.

The change in imputed enrollment rates at 2-year colleges is presented in Figure 1.6, which shows that renters who leave the 4-year sector are not being absorbed by the 2-year sector. Renter enrollment at 2-year colleges actually falls by 1.3 percentage points at 2-year colleges (a drop of similar magnitude is observed in other states), despite previous evidence that the 2-year sector is enrollment-elastic. On the homeowner side, the 2-year enrollment rate drops by 5.1 percentage points in other states, but only drops by 0.5 percentage points in Texas.

Texas homeowners thus exhibit modest increases in 4-year college enrollment and smaller decreases in 2-year enrollment. Figure 1.7 shows that the combined effect is a small increase in the overall homeowner college enrollment rate of 0.9 percentage points. Renters exhibit a decrease in 4-year enrollment that is not offset by gains in the 2-year college sector, resulting in a decrease in the overall renter enrollment rate of 2.5 percentage points. The next section introduces the main empirical strategy, which permits estimation of the effect of Proposition 16 on overall enrollment rates, and on college choice (conditional on any enrollment).

1.4.3 Empirical Strategy

Difference-in-Difference and Triple Difference Methods

First, I consider homeowners in other states as the counterfactual for Texas homeowners. This "Within-Owner" DID compares the before-after change in outcomes for homeowning families with college-aged children in Texas to those in other states. A "Within-Renter" DID specification can be estimated in similar fashion. Second, a DDD model is estimated to compare before-after changes in the owner-renter gap between Texas and other states. The DDD estimates combine any gains to homeowners and losses to renters into a single "wedge" that can be interpreted as the gap (in sticker price, for example) between homeowners and renters induced by the policy change. The full DDD specification is estimated with the following equation,

$$y_{ijst} = \beta_1 own_{ijst} post_t texas_s + \beta_2 own_{ijst} post_t + \beta_3 post_t texas_s + \beta_4 own_{ijst} + \beta_5 post_t$$

$$+ \theta_s + \theta_s own_{ijst} + \theta_s post_t + \phi_j + \phi_j own_{ijst} + \psi_{sj} + \beta_6 X_{ijst} + \beta_7 Z_{st} + \sum_k \delta_k class_{ijst} + \varepsilon_{ijst} \tag{1}$$

where the subscript i denotes the individual student, j their college-entry cohort, s their permanent state of residence, and t the year of observation (from 1 to T). The dependent variable y is the outcome of interest (e.g. an enrollment dummy, the log of sticker price, or measures of college quality). The right-hand side variables for *own*, *post* and *texas* are dummy variables set equal to one for students whose parents own their homes, who are observed in the post-period, or who permanently reside in Texas, respectively. Here the parameter β_1 is the parameter of interest, the triple-difference estimator. In addition to the usual triple difference terms, this specification allows us to control for: (1) time-invariant differences shared by homeowners (and renters) in a given state ($\theta_s own_{ijst}$); (2) state-period shocks ($\theta_s post_{ijst}$); (3) national shocks shared by all homeowners (and renters) in a given cohort ($\phi_j own_{ijst}$); and (4) state-cohort shocks (ψ_{sj}). Other controls include a vector of individual controls for class year ($\sum_k \delta_k class_{ijstc}$ where k indexes class year from first through fifth and unclassified), household income and SAT scores (X_{ijst}); and a vector of state-level credit and house price controls (Z_{st}) including median state home mortgage rates (to control for the price of home equity credit), median state house prices and 3-year state house price growth (to control for housing wealth effects). The enrollment data also supports the inclusion of metropolitan area fixed effects to confirm that the results are not being driven by omitted time-invariant factors unique to metropolitan areas. All observations are weighted according to the individual-level weights provided in the survey data.

For the CPS enrollment data, I use cohort as the time variable (t) (precluding separate fixed effects for cohort and time). This data includes annual observations, permitting estimation of an event study model that breaks up the treatment effect by cohort,

$$y_{ist} = \sum_{r \neq t_b}^{T} \alpha_r own_{ist} texas_s + \phi_t own_{ist} + \theta_s own_{ist} + \psi_{st} + \beta_6 X_{ist} + \beta_7 Z_{st} + \varepsilon_{ist} \tag{2}$$

where t_b is an omitted base period (the 2003-04 academic year).

Estimation proceeds using a set of control states designed to mimic the evolution of owner-renter income inequality in Texas. This is done in order to rule out the possibility that widening owner-renter income inequality is driving the results; among all states, Texas exhibited the 8th highest increase in the owner-renter income gap between pre- and post-periods. The restricted state control group consists of all states within a fifteen percentile band of Texas (in the population-weighted pre-post change in log owner-renter income inequality), fifteen states in total.[29] Appendix Figure 1.16 confirms that this restricted set of control states follows the evolution of owner-renter income inequality in Texas quite closely.

Estimates are reported along with robust standard errors clustered at the state level, except for the Within-Texas estimates which report unclustered robust standard errors.[30] A more detailed discussion of alternative statistical inference methods is provided in Appendix Section 1.9.3, which demonstrates that state-level clustering is a more conservative approach than clustering by state-year or state-own cells.

Synthetic Control Methods

The DID and DDD methods specify a counterfactual that is effectively a population-weighted average over the fifteen states included in the control group. Ideally the control group isn't simply matched to Texas in terms of owner-renter income inequality, but other relevant determinants of college investment as well. The idea behind the synthetic control methods developed by Abadie, Diamond and Hainmueller (2010) is that a weighted combination of untreated units can provide a better comparison for the treated unit than either a single untreated unit or a simple population-weighted average across untreated units. This method

[29]The restricted set of control states includes: Alaska, Colorado, Connecticut, District of Columbia, Georgia, Indiana, Iowa, Kentucky, Maryland, Massachusetts, Minnesota, Mississippi, Missouri, New Mexico, and North Dakota.

[30]It should be noted that estimation of college choice impacts proceeds with a single pre-treatment period and single post-treatment period, which produces consistent standard errors in the face of serially correlated outcomes even when the number of states is small; see Bertrand, Duflo and Mullainathan (2004) for a discussion of inference in the presence of serial correlation.

uses state-level data to construct a "synthetic control" that resembles relevant characteristics of Texas prior to Proposition 16.

To implement this approach, I aggregate the data to the state-level and estimate the college sticker price gap between homeowners and renters in each state (y_{st}). First consider a standard state-level DID equation,

$$y_{st} = \delta post_t texas_s + X_{st}\Gamma + \theta_s + \varphi_t + \varepsilon_{st} \tag{3}$$

where X_{st} is a vector of state-level controls, θ_s and φ_t are state and year fixed effects respectively, and states are weighted by the sum of individual survey weights in each state. To construct the synthetic control, the variables used for matching are stacked into a vector X_1 for the treated unit and a matrix X_0 for the potential control units. The weights W that are used to construct the synthetic control are chosen to minimize $\sqrt{(X_1 - X_0W)' V (X_1 - X_0W)}$. The matrix V weights the variables used in the matching and is chosen to minimize the mean squared prediction error over the pretreatment period. The synthetic control estimate of δ_{SC} is the difference between the outcome in the treatment unit and the synthetic control unit for the post-treatment period: $\hat{\delta}_{SC} = y_{1t} - \sum_{j=2}^{J+1} w_j y_{jt}$, where the treated state is the first of the $J+1$ states in the donor pool and w_j is the weight put on the jth state in the synthetic control. The synthetic control is thus a weighted average of the college sticker price gap in the states in the donor pool during the post-period. Abadie, Diamond and Hainmueller (2010) show that under the optimal weights δ_{SC} is an unbiased estimator of the treatment effect.

I form the synthetic control by matching on pre-treatment values of the sticker price gap, homeownership rates, the Mexican-American population share, the change in public college funding per student, and three-year changes in housing prices.[31] Estimate proceeds over three periods (1999-2000, 2003-04 and 2007-08).[32] The donor pool consists of the 32 states

[31] The data sources used for these variables are described in Appendix 1.9.1.

[32] Data from earlier NPSAS waves (1995-96 and 1999-2000) is added to the later waves used in the main student-level analysis.

with at least 20 renters that appear in the analysis sample for each NPSAS wave, in order to minimize measurement error when aggregating to the state level. The results that follow will report estimates of δ and δ_{SC}. Inference proceeds by applying the synthetic control method to every potential control state, as if each state were subject to a similar intervention. This allows for inference on whether the treatment effect estimated by synthetic control methods for Texas is large relative to the effect estimated for a state selected at random.

1.5 Enrollment Effects

How does the introduction of HELOCs affect overall college enrollment rates among homeowners and renters? This section presents estimates of the effect on enrollment rates using the yearly event study (equation 2) and before-after mean shift specifications (equation 1), before discussing the robustness of the results to alternative assumptions.

1.5.1 Event Study Estimates of the Effect on College Enrollment

The event study estimates from Within-Owner and Within-Renter specifications (for the restricted state control group and for all states) are reported in Figures 1.8 and 1.9, respectively. For homeowners, no discernible pattern emerges between the cohort-specific estimates before and after the policy change, confirming the descriptive finding that overall homeowner enrollment in Texas is unaffected. For renters, enrollment is lower for the cohorts after the introduction of HELOCs. While the post-period cohort effects are generally not statistically significant for the restricted control states, cohort effects based on all states range from -2.8 to -10.4 percentage points after the policy change, and are statistically significant for all cohorts except 2006. Cohort effects for all states prior to the policy change are not distinguishable from zero. This pattern is confirmed by Figure 1.10, which reports estimates of the DDD specification of equation 2; the difference in the gap in college enrollment rates between homeowners and renters is not distinguishable from zero for unexposed cohorts, but rises by between 3.1 and 16.3 percentage points for exposed cohorts. The estimated effects are smallest for the first and last cohorts exposed to the treatment. This is consistent with homeowners taking some time to respond to HELOC marketing from creditors and incorporate newly available HELOCs into their college financing decisions, followed by a tightening of available home equity credit in 2007 as housing prices began to drop.

1.5.2 Mean-Shift Estimates of the Effect on College Enrollment

The conclusions from the event study results on overall enrollment are supported by estimates of a mean-shift averaged between pre- and post-periods (as in equation 1). Table 1.4 presents the Within-Owner DID, Within-Renter DID, and DDD estimates on overall enrollment rates. The Within-Owner estimates show an insignificant drop in the homeowner enrollment rate of just over 1 percentage point (relative to in other states), while the Within-Renter estimates show a statistically significant drop in the enrollment rate of 5.7 percentage points. These findings are confirmed by the DDD estimates of columns 3 and 4, which show that the enrollment gap between homeowners and renters in Texas widened by 6.3 to 7.7 percentage points compared to other states. Column 4 also confirms that the results are robust to time-invariant metropolitan-level variation (such as local labor market specialization or metropolitan variation in home prices) captured by MSA fixed effects. A remaining concern is that the estimates are driven in part by local housing wealth shocks over time, but it is reassuring that the estimates are not responsive to the inclusion of controls for state housing prices and state housing price growth. Drawing the control group from all states also reveals a similar pattern of renter crowd-out but no homeowner enrollment effects (Appendix Table 1.10).[33]

[33]It is worth noting the scope for measurement error in the enrollment results, owing to mismeasurement in homeownership status in particular. Recall that an individual's college enrollment status is assigned to a cohort based on their year of anticipated college entry, but it is possible that homeownership status has changed between the age of 18 and the time it is observed, in ways that are related to college enrollment. This bias might work in two different ways: the less common transition from owning to renting, or the relatively more common transition from renting to homeownership. This amounts to measurement error in homeownership status that is correlated with financial distress, with mismeasurement of some of the "worst" homeowners as renters and some of the "best" renters as homeowners. To rule out the possibility that the drop in renter enrollment is driven by measurement error, I turn to the NPSAS data of enrolled students which does not suffer from the same measurement lag in homeownership status; similar to the CPS results,

The results confirm that the credit supply shock resulted in the crowd-out of renters (this is explored further in section 1.6). While these enrollment changes seem large at first glance, it is important to consider the proportion of renters among enrolled students; depending on assumptions about the distribution of crowd-out effects across age levels, the drop in renter enrollment represents between 1 out of 18 and 1 out of 35 students enrolled in 2003-04.[34] In contrast, if Texas homeowners with college-aged children took up HELOCs at the same rate as in the rest of the country, it would imply that roughly 1 in 5 students enrolled in 2003-04 secured HELOC financing.[35] Moreover, roughly 3 out of every 5 enrolled Texas students are in the non-selective college sector and thus may have less attachment to college.

1.5.3 Additional Robustness Checks

Panel A of Table 1.5 explores the identifying assumption by examining the relationship between treatment status and observable measures of family background for the CPS sample of college-aged individuals. The left-side of Panel A reports estimates of the DDD coefficient from a baseline specification (equation 1), but with the dependent variable replaced with selected family background measures. The last column shows DID estimates comparing the change in homeownership rates in Texas to other states. The results indicate that there is no significant relationship between treatment status and family income, mother's education, race and homeownership. While it is impossible to rule out changes in unobserved factors, the fact that treatment status is uncorrelated with family background among all 18-22 year olds helps to mitigate concerns about an unobserved shock to Texas renters. Back

the raw number of enrolled renters under the age of 26 who applied for financial aid drops by 9.7% between 2003-04 and 2007-08.

[34] A 6 percentage point drop in renter enrollment corresponds to 2.4% of all Texas 18-22 year olds in the 2003 CPS, or 5.5% of all enrolled Texas students. Students between the ages of 18 and 22 represent 51.8% of enrolled students in the 2003-04 NPSAS. Under the extreme assumption that there is no effect on students older than 22, the drop in renter enrollment represents 1 in every 35 students. Under the alternative assumption that students of all ages experience similar crowd-out effects, the drop in renter enrollment represents 1 in every 18 students.

[35] In the 2007 SCF, 26.4% of homeowning families with college-aged children had HELOCs. Multiplying this figure times the 75.9% of enrolled students in homeowning families in the 2007-08 NPSAS wave implies that roughly 17.8% of enrolled students were in families with HELOCs.

of the envelope calculations demonstrate that even under extreme assumptions about the enrollment behavior among marginal homeowners, any bias introduced from compositional effects owing to increasing homeownership rates in Texas cannot explain changing enrollment patterns.[36,37]

Taken as a whole, the enrollment results show that renters are crowded out of college despite only small absolute increases in the homeowner enrollment rate (that are not distinguishable from zero when compared to homeowners in other states). This finding is robust to: (1) identification within-renters only, and between homeowners and renters; (2) confounding shocks unique to homeowner-state, homeowner-cohort and state-cohort cells; (3) time-invariant differences across metropolitan areas; and (4) state-level variation in the price of home mortgage credit and home prices. As an additional robustness check that the results are not driven by shocks to the returns to any college education, Appendix Table 1.11 confirms that the identifying variation is not associated with any differential impacts on high school enrollment between homeowners and renters in Texas.

1.6 College Choice Impacts

This section explores the effect of HELOC-eligibility on the intensive margin of college choice, conditional on any college enrollment. For renters (homeowners), the estimates should be interpreted as the effect of the credit supply shock on college choice for the renters (homeowners) that remain enrolled.

Before proceeding, it is worth noting that the renters who remain enrolled after the policy change do not look substantively different in terms of student ability and family

[36]Previous research has shown that the entire home mortgage interest deduction, which targets the wealthy who are almost always homeowners, has had no effect on homeownership rates (Glaeser and Shapiro 2002).

[37]The homeownership rate among Texas families with children between the ages of 18 and 22 rose 1.9 percentage points between 2003 and 2007 (from 52.2 to 54.1 percent). First, even under the assumption that these marginal Texas homeowners were among the "worst" students and enrolled at the lower enrollment rates exhibited by Texas renters (33.1% in 2007-08 compared to 49.9% among Texas homeowners), this would only account for a very small drop in homeowner enrollment equal to $(.499 - .331) \cdot (.019/.541) = 0.006$, or 0.6 percentage points. Second, even under the assumption that marginal homeowners were among the "best" students and enrolled at the higher rates exhibited by Texas homeowners, this would only account for a very small drop in renter enrollment equal to $(.499 - .331) \cdot (.019/(1 - .541)) = 0.007$, or 0.7 percentage points.

background. Panel B of Table 1.5 reports estimates of the relationship between treatment status and family background measures for the NPSAS sample (household income, mother's education, race, SAT scores, dependency status and homeownership rates). As in Panel A, none of the relationships are statistically significant and no pattern emerges. At first glance, this seems at odd with the notion that renters with less attachment to college may be crowded out; however, subsequent results will show that renters who forgo college entirely are predominantly leaving from non-selective 4-year colleges, and these individuals are in fact the modal renters in terms of college selectivity.[38]

1.6.1 Difference-in-Difference and Triple Difference Estimates

Sticker Price and Net Price Impacts

Table 1.6 reports DID and DDD estimates of the effect of HELOC eligibility on the log of college sticker and net price. Columns 1 and 2 show statistically significant increases in sticker price and net price among homeowners (14.1% and 19.8% respectively). This corresponds to annual increases in sticker price and net price of roughly $1,200, on pre-treatment means for Texas homeowners of $6,260 and $4,440, respectively. Columns 3 and 4 show that sticker price and net price are unaffected among renters, consistent with the notion that the drop in renter enrollment comes from modal renters (in terms of college selectivity). The DDD estimates of columns 5 and 6 echo the Within-Owner DID estimates, with significant increases in sticker price and net price gaps of 14.7% and 15.2%, respectively.[39]

As with the overall enrollment results of the previous section, the Within-Owner DID estimates are very similar in magnitude to the DDD estimates. Unfortunately the NPSAS data does not include a finer geographic identifier than state of residence to control for local housing price growth, but once again the estimated treatment effect is virtually un-

[38] The distribution of college selectivity at Texas colleges by homeownership status is shown in Appendix Figure 1.17.

[39] Appendix Table 1.12 confirms that the results are nearly identical when measuring sticker price and net price in levels rather than logs. Expanding the control group to include all states also yields similar conclusions but with estimates that are slightly smaller (Appendix Table 1.13).

affected by the inclusion of state-level housing price controls. Moreover, the fact that the estimated treatment effect persists when renters (and renter neighborhoods) are excluded suggests that the results are not driven by confounding factors correlated with neighborhood or neighborhood type.

Table 1.7 presents the Within-Owner DID coefficients for the log of sticker price and net price by income quintile (computed over the analysis sample). The results show that college choice impacts are generally limited to families in the top three income quintiles of the analysis sample (with household income above $69,000); these are the same families who are most likely to have been approved for a HELOC as in Figure 1.2.

College Selectivity Impacts

To the extent that sticker price is associated with institutional quality, spending increases among the children of homeowners should translate into attendance at institutions with more selective admissions criteria and higher-ability peers. Panel A of Table 1.8 shows that the owner-renter gap in median peer SAT scores increased by more than twelve points, with a statistically significant increase of more than 26 points for homeowners.[40] Panel B of Table 1.8 shows the effect of HELOC-eligibility on college selectivity, using different selectivity tiers as the dependent variable. The first column reports treatment effects from the preferred DDD specification with controls (equation 1), reflecting the change in the gap in the likelihood of enrollment between homeowners and renters; columns two and three show Within-Owner and Within-Renter DID estimates, respectively.

Several patterns emerge from Panel B. First, the enrollment gap between Texas homeowners and renters is widening at the top (most competitive colleges) and at the bottom (non-selective) of the 4-year college hierarchy.[41] Second, both of these gaps are widening

[40] The SAT estimates are not computed over the full analysis sample but rather the subset of colleges with selective admissions that require and report median SAT scores for admitted students.

[41] Appendix Table 1.14 shows that the likelihood that homeowners are enrolled in one of Texas' two flagship universities (The University of Texas at Austin and Texas A&M University) rises by more than 3 percent relative to renters.

due to increases in the likelihood of homeowner enrollment coupled with decreases in the likelihood of renter enrollment (relative to in other states). Third, Texas homeowners are significantly more likely to leave the 2-year college sector (relative to other states) as they ascend the college quality hierarchy into the 4-year college sector, while Texas renters are not significantly more likely to exit the 2-year sector (relative to in other states). This is consistent with the notion that the reduction in financing costs makes a 4-year college degree worthwhile for some homeowning households who would otherwise enroll in less costly 2-year colleges. It is also consistent with college supply constraints for renters at non-selective 4-year colleges; enrollment in the non-selective 4-year college sector expanded by almost fifty percent from 2003-04 to 2007-08 (see Figure 1.11), but these colleges enroll greater numbers of homeowners (in absolute terms) while rationing supply for renters (relative to homeowners).

The interpretation of supply constraints for renters is supported by Figure 1.12, which shows the share of enrollment slots allocated to homeowners within each college selectivity tier and across NPSAS waves. If renters are being crowded out of most competitive and non-selective 4-year colleges, then the share of students at these college coming from homeowning families should increase. Indeed, while the homeowner share is rising over time within every selectivity tier in other states, the increase in Texas homeowner share is considerably larger across all 4-year college selectivity tiers, especially at the most competitive and non-selective 4-year colleges. Figure 1.12 also suggests that renter supply constraints do not exist at 2-year colleges; the Texas homeowner share at 2-year colleges rises by the same amount as it does in other states (5 percentage points).

Thus the 4-year college prospects for Texas renters fall; some of the more able renters are still enrolling at more selective 4-year colleges, while some of the less competitive renter applicants may be opting to work rather than attend a 2-year college (or less selective 4-year college). This result is consistent with previous research which finds that the return to college for lower income families falls as tuition—and college quality, on average—decreases (Dale and Krueger 2002). Because average family income is lower among renter families, some children in renter families may opt for full-time employment rather than the prospects of facing lower returns from the cheapest college sector (2-year colleges).

The reduction in renter enrollment at non-selective 4-year colleges is also higher for males (-0.080, *p-value* = 0.043) than for females (-0.034, *p-value* = 0.014). This is consistent with higher college returns for women than men (Dougherty 2005). It also supports the notion that the discouragement effect is stronger among males who may face greater employment prospects in the construction sector in the midst of the housing boom; similarly, Charles, Hurst and Notowidigdo (2012) show that local housing booms can reduce college enrollment while increasing construction employment. While it is difficult to definitively determine causality between construction employment and renter discouragement, the fact that the Texas housing boom was stronger during the pre-period casts doubt on the notion of a confounding post-period shock.[42]

Because homeownership status is highly correlated with race and ethnicity, most of the reduction in renter enrollment at the most selective colleges is experienced by minorities. Estimating the preferred DDD specification with minority status used in place of homeownership status implies that the gap in enrollment at the most selective colleges between minorities and non-Hispanic whites widens by 1.4 percentage points (*p-value* = 0.011).

Additional Robustness Checks

The question remains whether the drop in renter enrollment is occurring at the same colleges where homeowner enrollment is rising (consistent with renter crowd-out by homeowners), or if the drop among renters is greater at colleges where homeowner enrollment is also falling (consistent with a contraction at colleges disproportionately attended by renters). Unfortunately, raw headcounts by college are not informative in the NPSAS sample, as there is substantial variation across survey waves in the number of sampled students at a particular college. All that can be said definitively with this data is that renter enrollment

[42]The three-year change in Texas housing prices peaked in 2001 (author's calculations based on the FHFA's Housing Price Index).

is dropping, with the greatest drop at non-competitive 4-year colleges. If the drop in renter enrollment was driven by an unobserved shock impacting the college prospects of renters, one might also expect to see a drop in the relative likelihood of enrolled renters attending college full-time rather than part-time. Additional results show this is not, in fact, the case (see Appendix Table 1.15).

Similarly, in the event of an unobserved shock affecting renters, one might also expect to see a drop in college applications from renters. While application data is not disaggregated by homeownership status, IPEDS data can be used to track the total number of applications at the top 5 Texas colleges in terms of pre-treatment renter share (with a combined student body that was 45.7% renter) and pre-treatment owner share (94.7% owner). The total number of applications from the three-year periods just before and just after Proposition 16 grew by 23.6% at the top renter colleges and 19.7% at the top homeowner colleges. Under the assumption that homeowner applications were not rising faster at historically renter colleges than at historically homeowner colleges, then the increase in applications at top renter colleges is inconsistent with a negative demand shock on the part of renters.

One remaining threat to identification concerns the coincidental timing of tuition deregulation in Texas. Prior to 2003, public undergraduate institutions in Texas charged statutory and designated tuition components that were set by the state legislature, and were generally identical across institutions. Public institutions were, however, able to set mandatory and course fees at the discretion of their own governing board, using these fees to maintain substantial variation in the net cost of tuition plus fees across public institutions. In 2003 the Texas Legislature passed a tuition deregulation bill (HB 3015) that allowed governing boards of public institutions to set their own designated tuition rates, effective in the spring semester of 2004. Because public institutions could still impact net student costs prior to tuition deregulation through fees, it is not obvious what causal impact, if any, this change may have had on student costs inclusive of fees. While Appendix Figure 1.18 shows that tuition and fees at 4-year public colleges in Texas do not exhibit any pronounced break from trend over this period, one cannot rule out the possibility that tuition deregulation allowed greater tuition increases at colleges disproportionately attended by homeowners. To confirm that estimated sticker price impacts for homeowners are not just the result of rising tuition at colleges disproportionately attended by homeowners, I estimate the preferred DDD specification using the log of sticker price held constant at pre-treatment levels as the dependent variable.[43] The estimate is 13.1 percentage points ($p-value = 0.037$), only slightly smaller than the actual sticker price estimate of 14.7 percentage points; homeowners were thus induced to attend colleges that were more costly before Proposition 16 took effect.

In order to rule out any concerns that the results are driven by policy changes at public colleges (e.g. lingering effects of the Texas ten percent rule), I show that the preferred DDD specification yields similar results when estimated over students enrolled at private colleges (18.2 percentage points, $p - value = 0.018$).[44] Appendix Figure 1.19 also confirms that the level of state and local funding to public colleges (appropriations and grants) evolved similarly in Texas as in other states.

Lastly, what of the predictions of Section 2 that access to lines of credit can increase spending on large multi-period expenditures? Appendix Figure 1.20 confirms that increases in college spending by homeowners are echoed by increased spending on vehicle purchases but not on predictable food-related expenditures.

1.6.2 Synthetic Control Methods

The synthetic control method relies on an alternative counterfactual constructed to match relevant pre-treatment characteristics in Texas. The optimal weights place zero weight on all states except Arizona, California, Colorado, North Carolina and Washington. Figure 1.13 plots the evolution of the actual owner-renter college sticker price gap in Texas to its synthetic

[43] This is implemented using published tuition for the 2003-04 year based on full-time and residency status.

[44] Davis, Saenz and Tienda (2010) show that enrollment at the state flagship University of Texas-Austin among previously underrepresented students from rural high schools rose steadily from 2001 through 2007 (Davis, Saenz and Tienda 2010). If rural enrollees are more likely to come from homeowning families than non-rural enrollees, this could bias the current estimates upwards.

control, yielding a treatment effect of $1,317 (close to the implied estimate of $1,200 from the individual-level analysis). This also compares closely to the estimate from the standard state-level DID (equation 3) drawing on the full donor pool with state weights determined by the sum of NPSAS survey weights in each state and including the aforementioned set of state-level predictors; the corresponding estimate is $1,079 ($p\text{-}value = 0.051$).[45]

Inference is based on the placebo study outlined in Abadie, Diamond and Hainmueller (2010): the synthetic control method is applied to every potential control state, as if each state were subject to a similar intervention. Consider the pre-intervention root mean square prediction error (RMSPE) for Texas (the average of the squared discrepancies between Texas and synthetic Texas prior to Proposition 16). If synthetic Texas were poorly fitted prior to Proposition 16, then the post-2003 gap may be artificially generated by a lack of fit rather than by the effect of Proposition 16. To limit the risk of drawing conclusions that are influenced by poor fit in the pre-period, states which are poorly fitted by their synthetic control are excluded.[46] The results are plotted in Figure 1.14. Because two of the 17 states that remain would generate larger treatment effects even in the absence of an intervention, we cannot rule out a zero effect. An alternative approach to inference that avoids having to choose of a cutoff for ill-fitting placebo runs is to consider the ratio of RMSPE in the post-period to the pre-period for each state as a more precise measure of the relative rarity of observing a large post-period gap. Figure 1.15 shows the distribution of these ratios for all 32 states in the donor pool. No state achieves a ratio as large as Texas. If the intervention were randomly assigned to another state in the data, the probability of obtaining a pre/post-RMSPE ratio as large as the one obtained for Texas would be $1/33 = 0.03$.

[45]The synthetic control method can also be applied in similar fashion to perform the analogous Within-Owner comparisons between homeowners in Texas and in other states, yielding an estimate of $967. Additional results are available upon request.

[46]States are excluded if the RMSPE in the post-period is more than 15 times that of Texas.

1.7 Strategic Institutional Responses

What of the possibility that some institutions are aware of the increase in private credit supply and are making strategic adjustments to their own tuition prices or financial aid packages? While it is impossible to definitively distinguish between tuition deregulation and home equity reform as the cause of any changes in tuition, this section will specifically explore whether colleges are treating homeowners differently. To investigate, variants of the aforementioned DID and DDD specifications are estimated with fixed effects included for each institution. This serves to identify the effects of the policy change conditional on college choice, isolating changes within institutions over time. Two outcomes are considered: sticker price and institutional aid (including merit aid, non-merit aid, tuition waivers and work-study). The sample is restricted to only include students attending college in-state and not attending exclusively part-time. The in-state restriction abstracts away from potential cross-subsidies between in-state and out-of-state students, while still focusing on the majority of college students.[47] The restriction on attendance intensity abstracts away from differences in the mix of part- and full-time students across institutions.

First consider a DID specification estimating the difference in the pre-post change between students attending Texas institutions at in-state rates and those attending institutions in other states at in-state rates:

$$y_{ijstc} = \beta_1 post_t texas_s + \beta_2 post_t + \beta_3 EFC_{ijstc} + \beta_4 SAT_{ijstc} + \phi_j + \theta_s + \varphi_c + \sum_k \delta_k class_{ijstc} + \varepsilon_{ijstc} \quad (4)$$

where the subscript c denotes institution, with institution fixed effects (φ_c) included along with cohort (ϕ_j) and state (θ_s) fixed effects. In addition to class year dummies, expected family contribution (EFC) is included as a single measure of an individual student's financial need, and composite SAT scores are included to proxy for student ability (for selective colleges only). The coefficient β_1 gives the difference in the average change in in-state sticker price

[47]NPSAS data indicate that more than 90% of college students with Texas residency attend in-state.

(or institutional aid) between Texas institutions and institutions in other states.[48]

Note that because homeowners and renters are filtering through the college selectivity hierarchy, the relevant characteristics of homeowners and renters may be changing even conditional on college choice, EFC and SAT scores.[49] Thus equation 4 identifies the effect of the policy change on average price levels within colleges, but it does not speak to the question of whether a given Texas student is treated differently in the post-period.

Next, I extend this DID model into a DDD model that incorporates variation between homeowners and renters, in order to investigate any changes in the allocation of institutional aid by homeownership status:

$$y_{ijstc} = \beta_1 own_{ijstc} post_t texas_s + \beta_2 own_{ijstc} post_t + \beta_3 post_t texas_s + \beta_4 own_{ijstc} + \beta_5 post_t$$

$$+\varphi_c + \theta_s + \theta_s own_{ijstc} + \theta_s post_t + \phi_j + \phi_j own_{ijstc} + \psi_{sj} + \beta_6 EFC_{ijstc} + \beta_7 SAT_{ijstc} + \sum_k \delta_k class_{ijstc} + \varepsilon_{ijstc} \quad (5)$$

This specification is similar to the baseline DDD of equation (1), but with institution fixed effects, EFC instead of household income, and excluding state-level housing market controls that shouldn't matter within-schools. The coefficient β_1 gives the difference between Texas colleges and colleges in other states in the average increase in institutional aid provided to homeowning families relative to renters.

Table 1.9 presents estimates of β_1 for equations 4 and 5, broken down by level of institutional selectivity with standard errors clustered at the institution level (the top three selectivity tiers are labeled as "more selective," the bottom four tiers are labeled as "less selective"). The DID estimates of columns 1 and 2 show a statistically significant increase in tuition at the more selective colleges of more than $2,000, but no tuition effects at less

[48]Note that to the extent students and parents have residency in the same state, restricting the sample to students attending college in-state is equivalent to assigning treatment status based on institution state. For students with information on their parents state of residency, state of residency only differs between parent and student in 5.6% of cases.

[49]Conditioning on EFC sweeps away differences in need-based aid across students due to the effect of financial need averaged across all colleges. To the extent that SAT scores determine merit-based aid, then conditioning on SAT scores sweeps away differences in merit-based aid across students due to average effects across colleges.

selective colleges. Column 3 shows a smaller statistically significant increase in the overall amount of aid ($960) among students at more selective Texas colleges relative to colleges in other states, with a small and insignificant decrease at less selective colleges in Column 4 ($200). Columns 5 and 6 confirm that colleges don't use tuition and fees to price discriminate between homeowners and renters. Instead colleges price discriminate using institutional aid. Despite only modest changes in the overall amount of aid at more selective Texas colleges, there is a large shift in the recipients of institutional aid. Column 7 shows that renters at more selective Texas colleges experience a statistically significant increase in institutional aid of more than $2,400 relative to homeowners, effectively offsetting the tuition increase. For less selective colleges, on the other hand, there is a much smaller decrease in the amount of institutional aid that renters receive relative to homeowners of $613 (this may be because homeownership status is proxying for program type within college).

1.8 Conclusion

Researchers have debated the importance of borrowing constraints on college investment (e.g. Carneiro and Heckman 2002). While insufficient access to credit may prevent college enrollment altogether in extreme cases (especially for lower income families), this paper demonstrates how higher borrowing costs may also lead families to enroll in less expensive and less selective colleges (even for more affluent homeowning families). Because more costly and selective colleges are associated with higher lifetime earnings (e.g. Hoekstra 2009), inequality in college access is likely transmitting inequality across generations through differences in lifetime earnings.

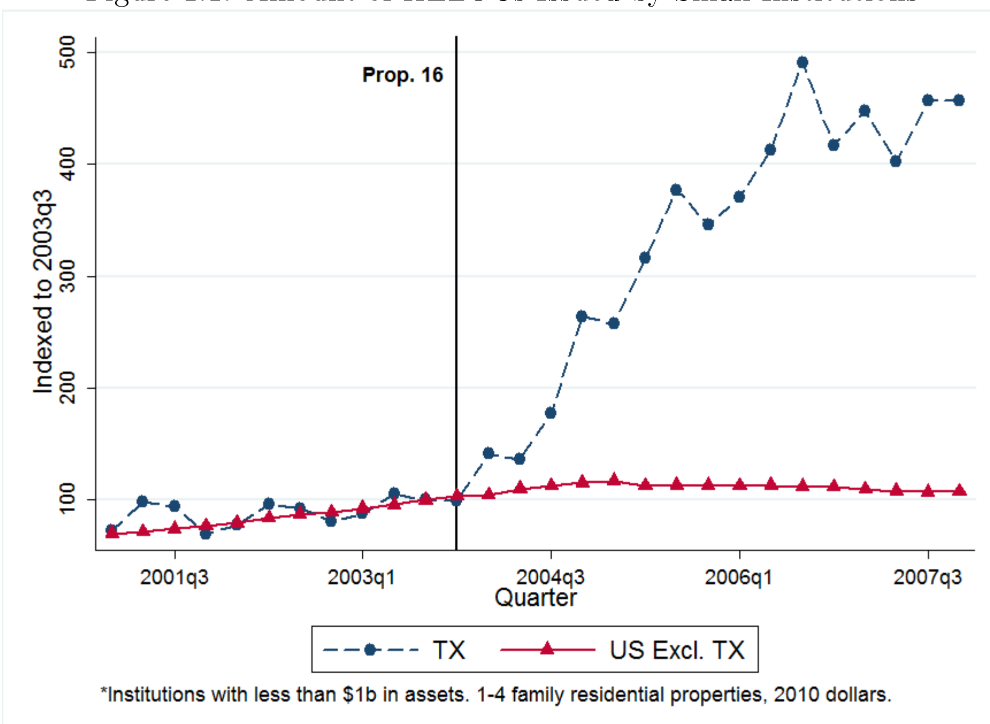
The findings of this paper also demonstrate how gains in college access for one group may come at the expense of reduced access for other groups. While the available data does not allow for the imputation of any foregone earnings of those renters displaced from college, previous research suggests that as important as these effects may be, the foregone earnings of those renters that remain enrolled but at lower quality colleges may be just as important (e.g. Saavedra 2008). Andrews, Li and Lovenheim (2012) and Dale and Krueger (2011) argue that these college choice effects are larger for minorities and disadvantaged families who may not otherwise have access to social networks; not only are minorities more likely to be displaced from the most selective colleges, but they may also have the most to lose from displacement in terms of foregone earnings.

The fact that homeowners are going to better colleges and paying higher net prices suggests that colleges prefer to admit homeowners on account of a greater willingness to pay. However, the data does not permit a more rigorous examination of this crowd-out mechanism; 4-year colleges may also prefer homeowners because they are better prepared and contribute more to their objective functions (e.g., by fostering positive peer effects). From a social standpoint, it is not immediately clear whether allocating scarce 4-year college enrollment slots to homeowners over renters represents a more or less efficient allocation. Regardless, it would not be very costly to target federal student aid in such a way that provides renters with a similarly low cost of capital as homeowners, and then let colleges decide which students to admit (irrespective of financing costs). Moreover, Title IV federal aid was originally intended to benefit those families "with financial or cultural need... from low-income families," which certainly includes many renter families. It is also possible that the benefits of targeting public funds to ease supply constraints at 4-year colleges outweigh the costs; indeed, Zimmerman (2014) argues that expanding supply along this margin is likely welfare improving, provided that any adverse effects on infra-marginal students are small.

This paper also demonstrates how the more selective colleges are able to capture some of the gains from cheaper financing by price-discriminating by homeownership status. The net effects of subsidized home lending markets and federal aid policy on college access are not immediately clear: on one hand, homeowners are sending their children to better colleges, but they are paying higher net prices at these colleges than they would in the absence of the private credit supply shock. On the other hand, tuition increases for renters who remain enrolled at selective colleges are offset by increases in institutional aid, but some renters are pushed down the college quality hierarchy and displaced from college altogether. This may have important implications for President Obama's plans to link federal aid to college value: if selective, high-value institutions respond to federal aid in a similar manner as they do to increases in private credit supply, they may raise tuition and re-allocate institutional aid in ways that improve accessibility for some groups at the expense of others.

Lastly, this line of research highlights one way in which federal tax policy may be undermining federal aid policy. The introduction of the home mortgage interest deduction with the Tax Reform Act of 1986 was clearly not intended to impact college access, yet it triggered dramatic increases in home equity lending nationwide that improve college access for some families at the expense of others.

Figure 1.1: Amount of HELOCs Issued by Small Institutions



Source: Author's calculations using Call Report data from the FFIEC.

Figure 1.2: The Effect of a Credit Supply Shock in the Most Selective 4-Year College Sector

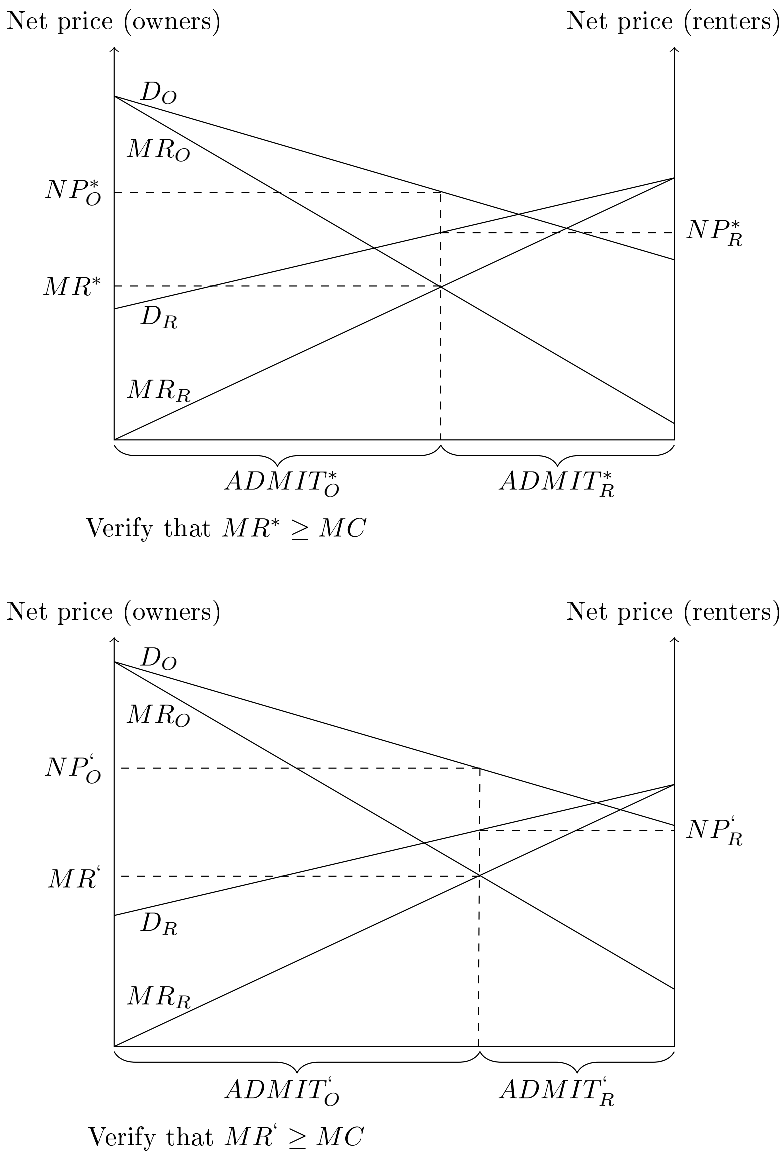


Figure 1.3: The Effect of a Credit Supply Shock in the 2-Year College Sector

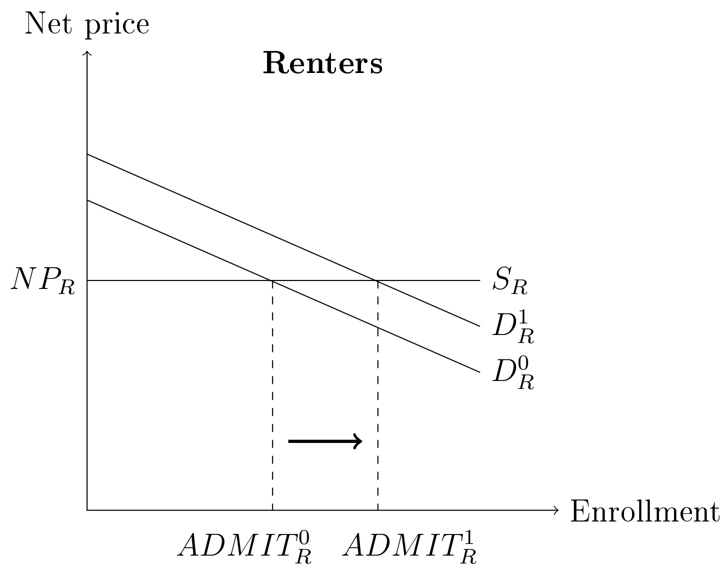
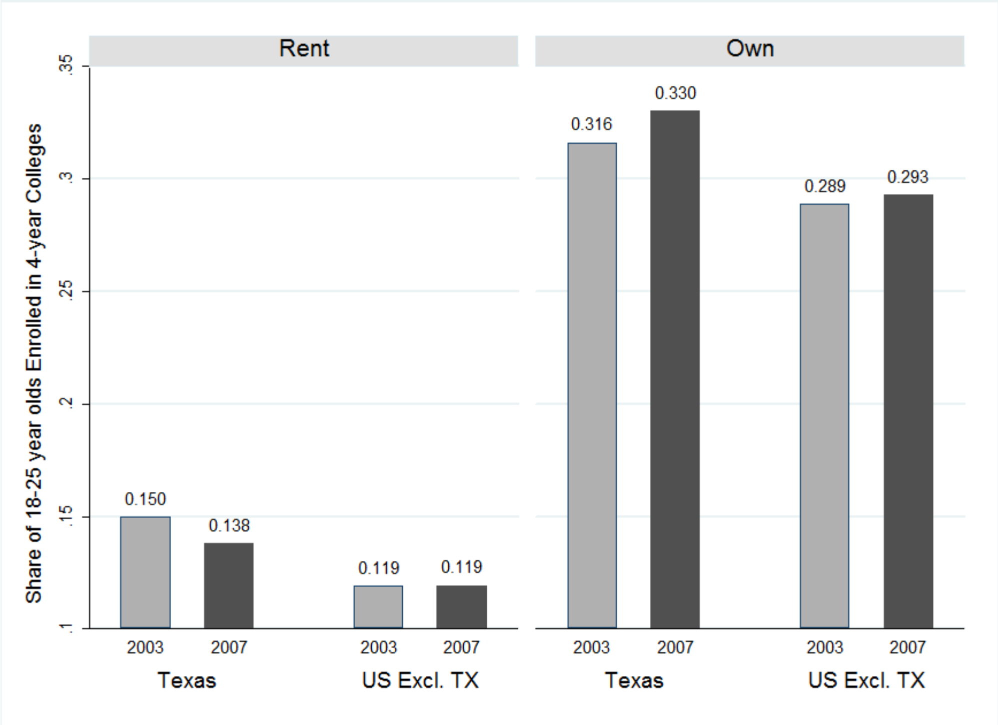
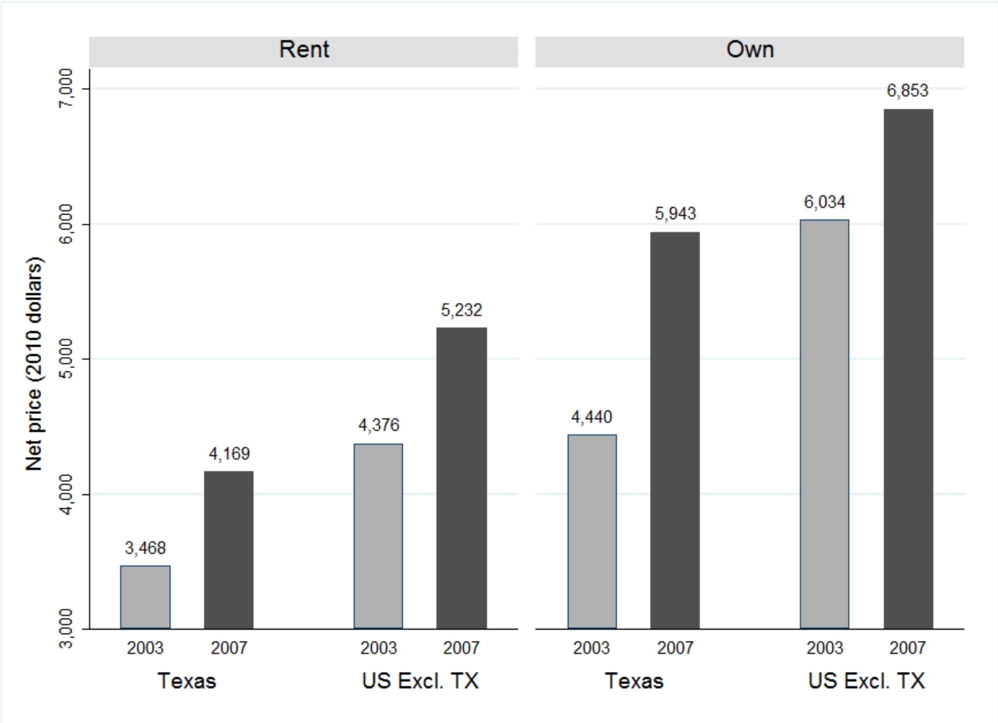


Figure 1.4: The Change in 4-year College Enrollment Rates by Homeownership Status



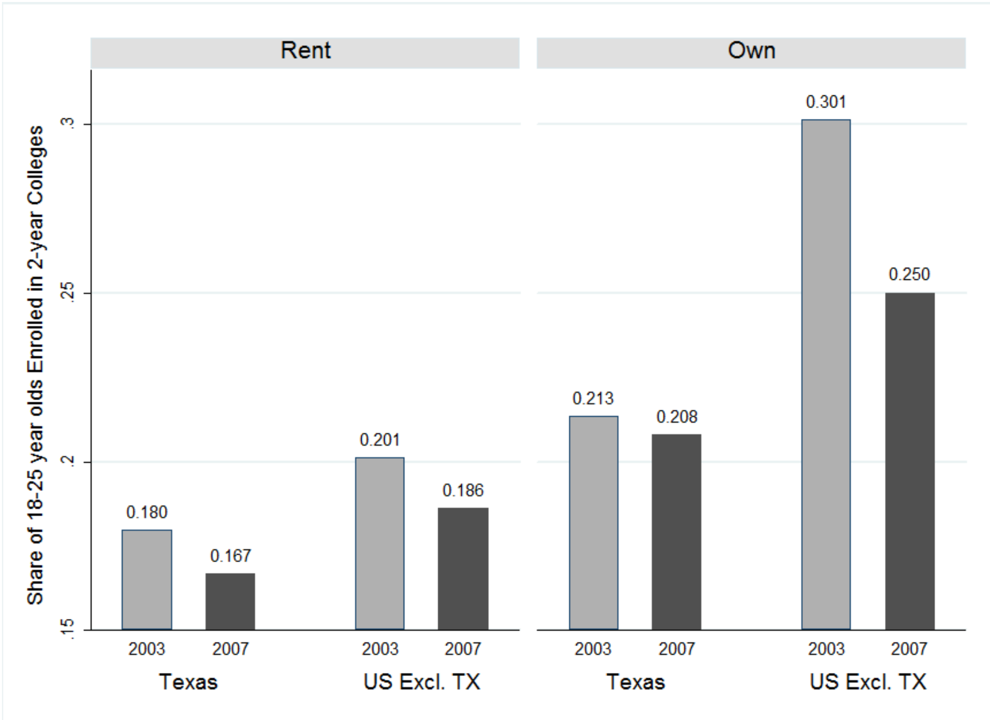
Source: Author's calculations based on weighted calculations from NPSAS and the CPS.

Figure 1.5: The Change in Net Price by Homeownership Status



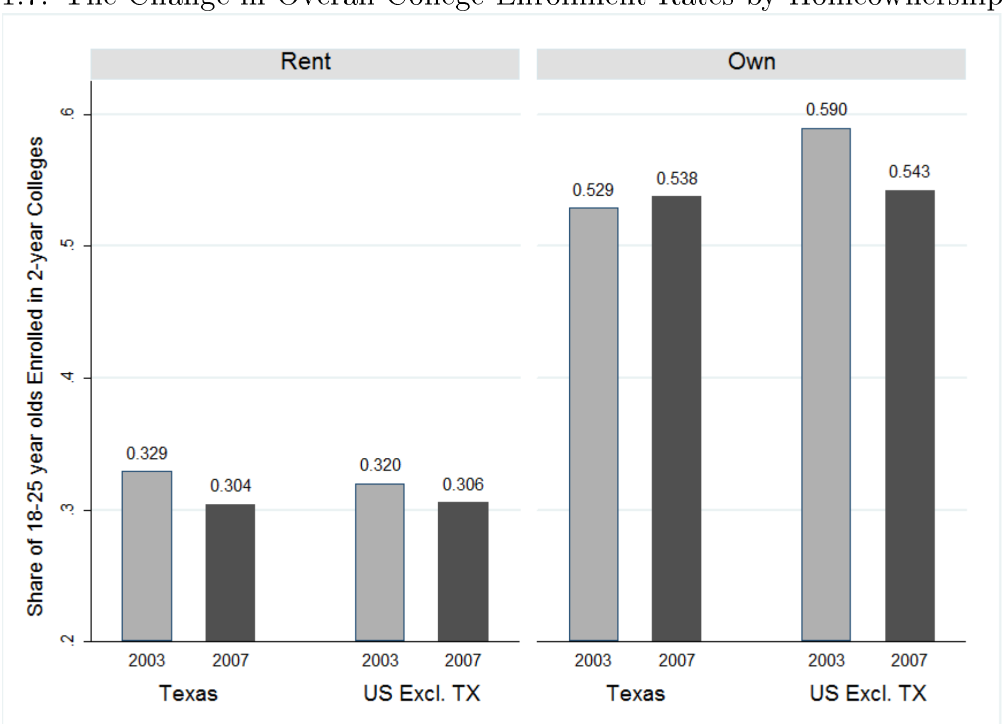
Source: Author's calculations based on weighted calculations from NPSAS.

Figure 1.6: The Change in 2-year College Enrollment Rates by Homeownership Status



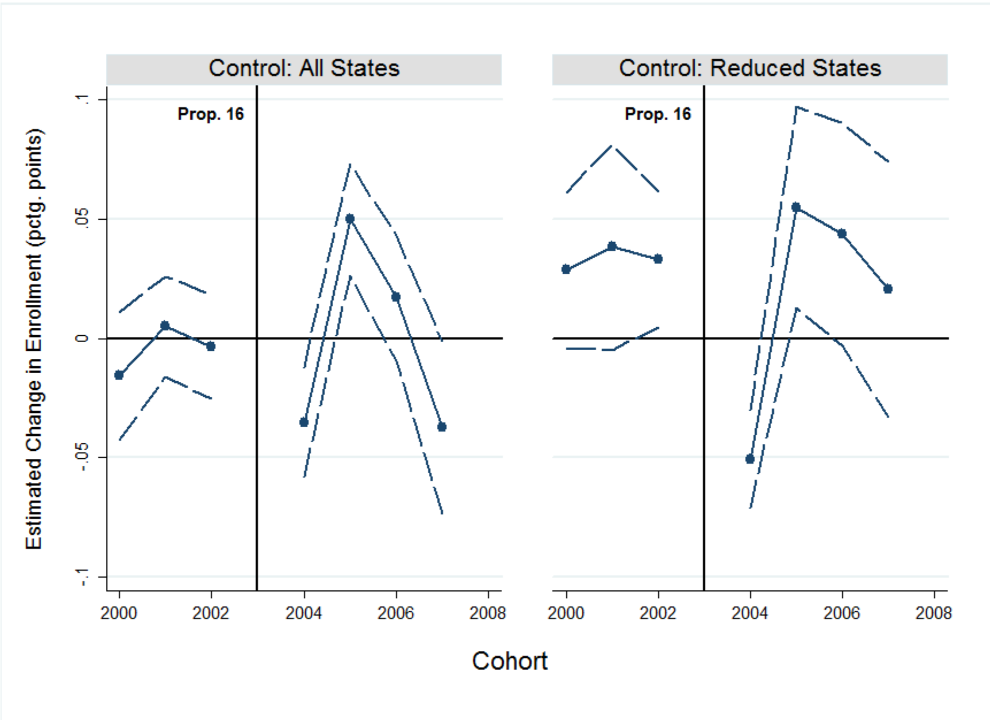
Source: Author's calculations based on weighted calculations from NPSAS and the CPS.

Figure 1.7: The Change in Overall College Enrollment Rates by Homeownership Status



Source: Author's calculations based on weighted calculations from NPSAS and the CPS.

Figure 1.8: Enrollment Effects by Cohort (Within-Owner DID)

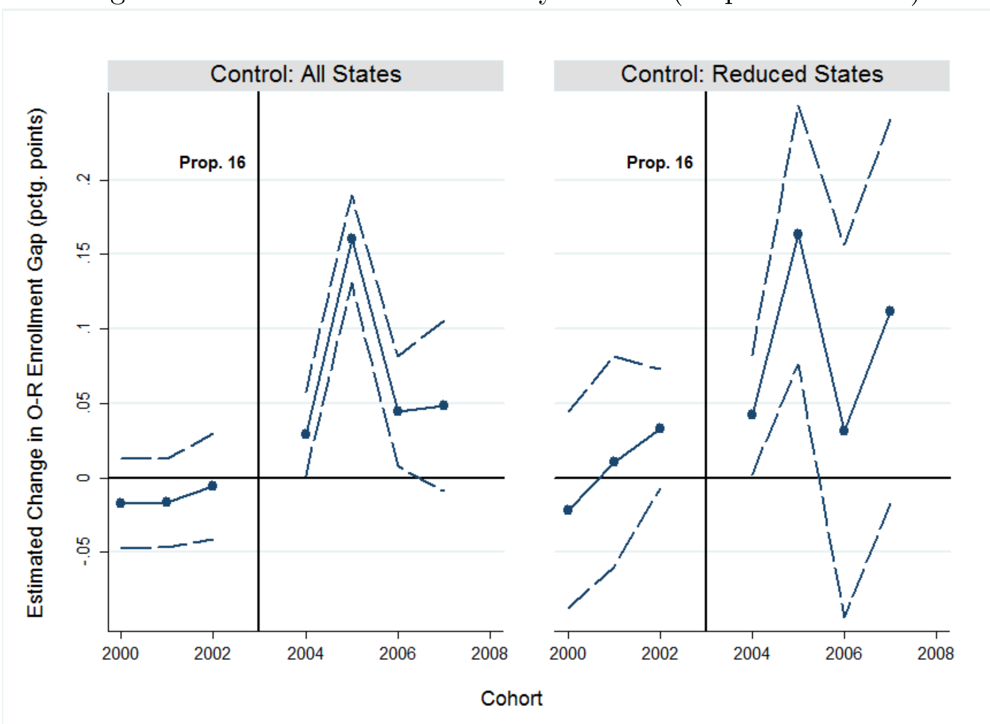


Source: Author's calculations based on the CPS analysis sample.
Notes: Estimates are from a Within-Owner DID specification with controls and FEs for state and cohort. Omitted base year is 2003, dashed lines represent 90% CIs.

Figure 1.9: Enrollment Effects by Cohort (Within-Renter DID)

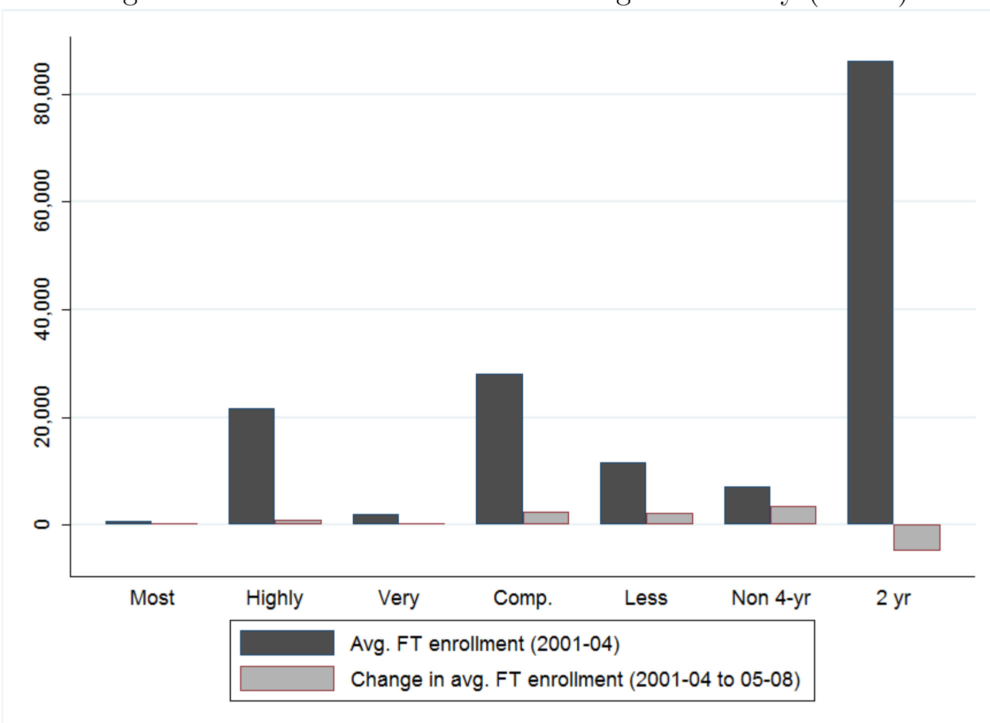
Source: Author's calculations based on the CPS analysis sample.
Notes: Estimates are from a Within-Renter DID specification with controls and FEs for state and cohort. Omitted base year is 2003, dashed lines represent 90% CIs.

Figure 1.10: Enrollment Effects by Cohort (Triple Difference)



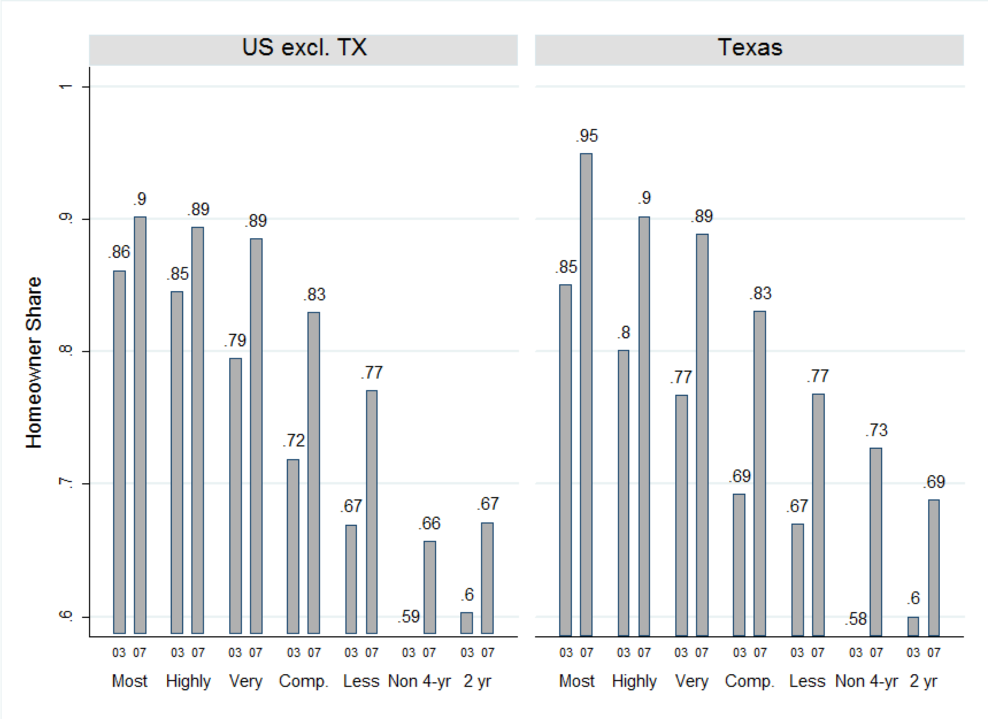
Source: Author's calculations based on the CPS analysis sample.
Notes: Estimates are from the preferred DDD specification with controls and FEs for state-own, cohort-own and state-cohort cells. Omitted base year is 2003, dashed lines represent 90% CIs.

Figure 1.11: The Distribution of College Selectivity (Texas)



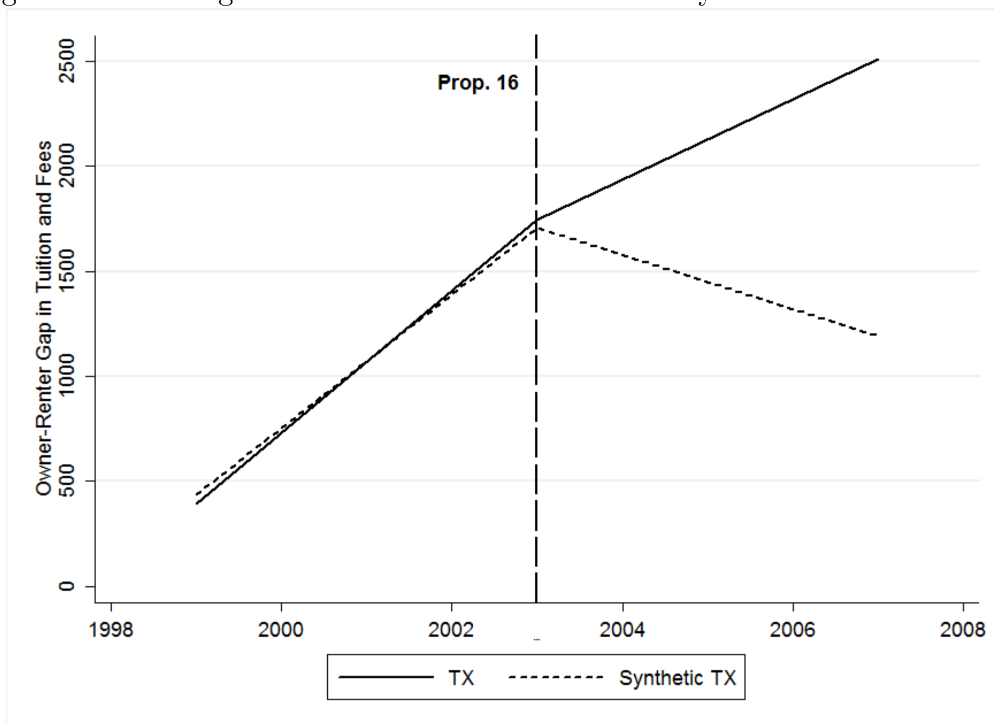
Source: Author's calculations based on IPEDS data merged to the Barron's Selectivity Index.

Figure 1.12: The Change in Homeowner Share by College Selectivity



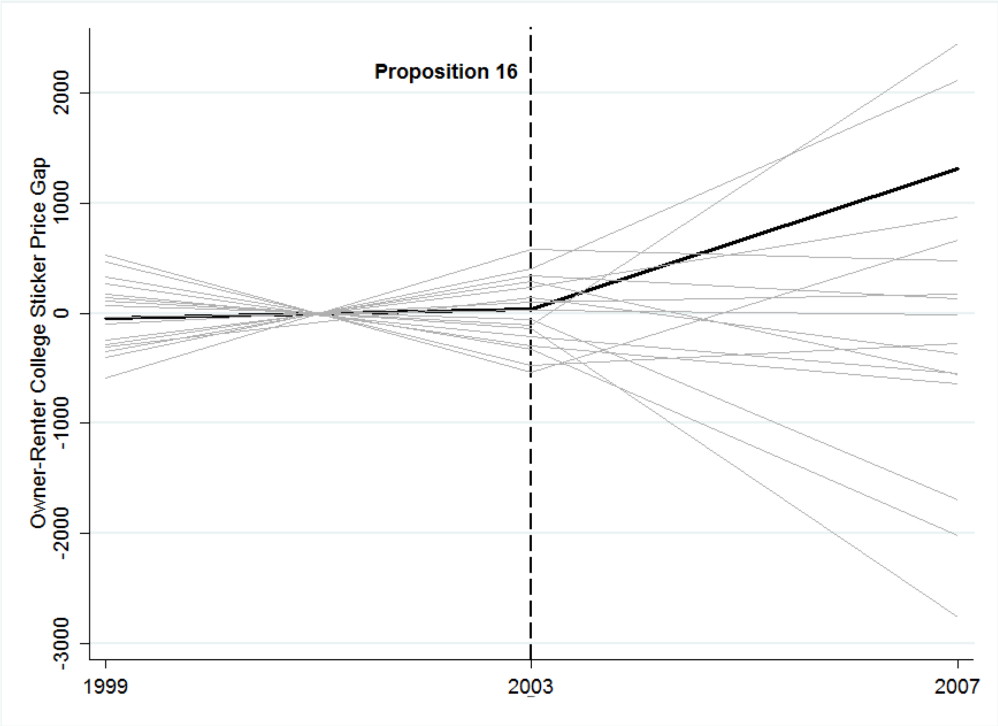
Source: Author's calculations based on NPSAS data merged to the Barron's Selectivity Index.

Figure 1.13: College Sticker Price Estimates Under Synthetic Control Method



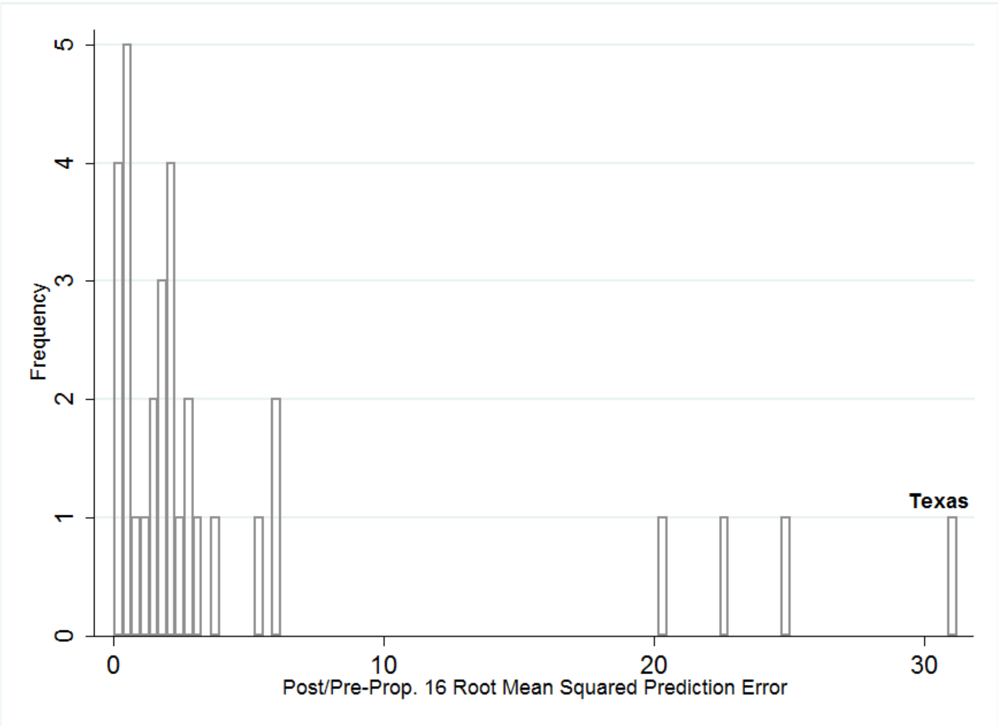
Source: Author's calculations using data from NPSAS, IPEDS, CPS, and the 2000 Census.

Figure 1.14: Distribution of College Sticker Price Placebo Gaps



Source: Author's calculations using data from NPSAS, IPEDS, CPS, and the 2000 Census.

Figure 1.15: Ratio of Pre/Post Prop. 16 RMSPE



Source: Author's calculations using data from NPSAS, IPEDS, CPS, and the 2000 Census.

Table 1.1: Comparing HELOCs and Student Loans

	HELOCs	PLUS Loans	Stafford Loans	
			Unsubsidized	Subsidized
Interest rates (2007)	3-12.75%[a]	7.9-8.5%[b]	6.8%	6.8%
Subsidized interest	No	No	No	Yes
Tax deductible interest	Yes	Sometimes[c]	Sometimes[c]	Sometimes[c]
Aggregate limits	No	Cost less other aid	$23,000 combined[d]	
Re-apply every year	No	Yes	Yes	Yes
Discharge in bankruptcy	Yes	Rarely[e]	Rarely[e]	Rarely[e]

[a] Among households with college-aged kids, the average APR of a household's HELOCs ranged from 3 to 12.75% in the 2007 SCF.

[b] The interest rate on fixed rate PLUS Loans through the FFEL Program was 8.5%, and 7.9% through the Direct Loan Program.

[c] Up to $2,500 of student loan interest is deductible if adjusted income is less than $75,000 ($155,000 for joint filers).

[d] For dependent students whose parents were denied a PLUS loan, the aggregate Stafford limit is extended to $46,000.

[e] Discharged through bankruptcy only if proven that repayment imposes undue hardship on the loanholder and dependents.

Table 1.2: Loan Frequency

All Homeowners	Income Quintile (Analysis Sample) 1	2	3	4	5	All
Upper Income Bound	20,214	41,114	69,034	104,310	–	–
Proportion with Loans						
HELOCs	6.1%	12.6%	16.2%	20.9%	29.1%	18.4%
Education Loans	5.7%	5.8%	13.7%	19.5%	15.7%	12.7%
N	1,232	2,054	2,506	2,357	8,306	16,455

Source: Statistics are computed from the 2007 Survey of Consumer Finance using supplied sampling weights.
Notes: Income quintiles are determined from the NPSAS analysis sample.

Table 1.3: HELOC Characteristics

HELOC-Holders	Income Quintile (Analysis Sample) 1	2	3	4	5	All
Upper Income Bound	20,214	41,114	69,034	104,310	–	–
Limit (median)	70,000	30,000	40,000	50,000	80,000	50,000
Utilization (median)	17.9%	25.0%	31.3%	25.0%	22.0%	24.0%
HELOC Interest Rate Percentile						
10	5.9	6.0	6.0	6.0	6.0	6.0
50	8.5	8.0	8.0	8.0	7.5	8.0
90	12.0	9.8	10.0	11.8	9.0	10.0
Median Effective Interest Rate	8.0	7.3	6.8	6.5	6.0	6.8
Percentage of Families w/HELOC Rates Below						
6.8% Stafford Loan Rate	14	22	16	23	27	23
8.5% PLUS Loan Rate	60	81	70	74	85	78
N (Interest Rate Sample)	69	202	286	336	1,204	2,097

Source: Statistics are computed from the 2007 Survey of Consumer Finance using supplied sampling weights.
Notes: Income quintiles are computed from the NPSAS analysis sample. Median effective interest rates are calculated as the overall median HELOC interest rate (8 percent) minus the value of the tax deduction on interest payments for the median HELOC balance of $12,000 at different points in the income distribution. The value of the tax deduction is calculated as the savings in 2007 federal income tax liability when adding $960 of interest payments (8 percent times $12,000) to another $10,000 in itemized deductions for a married family in Texas with one 18 year old child and the mean household income within each income quintile.

Table 1.4: The Effect of HELOC-Eligibility on College Enrollment

	DID		DDD	
	Within Owners	Within Renters		
	(1)	(2)	(3)	(4)
Post*TX	-0.012	-0.057**		
	(0.011)	(0.027)		
Post*TX*Own			0.077***	0.063**
			(0.023)	(0.024)
Fixed Effects:				
Cohort	X	X		
State	X	X		
State*Cohort			X	X
State*Own			X	X
Cohort*Own			X	X
MSA				X
N	12,694	8,202	20,896	19,350
R-squared	0.064	0.022	0.082	0.105

Source: Author's calculations using the CPS analysis sample of 18-22 year olds, excluding the top 1% of the national income distribution.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is a dummy variable for college enrollment or any degree completion. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.5: Treatment Status and Family Background

Dependent variable	ln(Income)	Mom BA	White	SAT	Dependent	Apply for Aid	Own Home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: CPS Sample							
coef.	0.033	-0.022	0.007				0.021
se	(0.036)	(0.024)	(0.028)				(0.013)
N	24,934	13,944	24,934				24,934
Panel B: NPSAS Sample							
coef.	0.027	-0.022	0.028	5.30	0.006	-0.022	0.003
se	(0.069)	(0.021)	(0.030)	(12.96)	(0.016)	(0.023)	(0.006)
N	28,750	27,510	28,750	20,910	28,750	33,100	28,750

Source: Panel A uses the CPS analysis sample with survey year minus one as the time variable (rather than birth cohort). Panel B uses the NPSAS analysis sample (except for column 6 which also includes non-aid applicants).

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Columns 1 through 6 provide OLS estimates for the *post*own*texas* coefficient from a baseline triple difference specification without individual controls and with fixed effects for state-time, state-own and time-own cells (Panel B includes fixed effects for state-time, state-cohort, own-cohort and state-own cells). Column 7 provides OLS estimates for the *post*texas* coefficient in a DID specification with state and time fixed effects (plus class year control for Panel B). Robust standard errors are in parentheses, clustered at the state level. For the enrollment sample, observations are weighted by the CPS person-level supplement weight. For the NPSAS sample, observations are weighted by the NSPAS study weight, normalized to sum to one in each study wave.

Table 1.6: The Effect of HELOC-Eligibility on College Sticker/Net Price

	DID: Within Owner		DID: Within Renter		DDD	
	Sticker	Net	Sticker	Net	Sticker	Net
	(1)	(2)	(2)	(3)	(4)	(5)
Post*TX	0.141**	0.198***	-0.030	0.037		
	(0.061)	(0.043)	(0.139)	(0.124)		
Post*TX*Own					0.147***	0.152***
					(0.048)	(0.053)
Fixed Effects:						
Cohort	X	X	X	X		
State	X	X	X	X		
State*Time					X	X
State*Cohort					X	X
State*Own					X	X
Cohort*Own					X	X
N	21,550	21,550	7,210	7,210	28,750	28,750
R-squared	0.307	0.151	0.206	0.119	0.314	0.168

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is the log of sticker price or net price. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.7: The Effect of HELOC-Eligibility by Income Quintile

Quintile	Upper Bound	N	Tuition	Net Price
	(1)	(2)	(3)	(4)
1	$20,214	2,430	-0.029	-0.064
			(0.088)	(0.047)
2	$41,114	4,040	-0.034	-0.209**
			(0.054)	(0.086)
3	$69,034	4,900	0.308**	0.438***
			(0.107)	(0.090)
4	$104,310	4,890	0.126*	0.157***
			(0.059)	(0.053)
5	–	5,300	0.145***	0.317***
			(0.045)	(0.056)

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. Coefficients in columns 3 and 4 are from the Within-Owner DID specification with the full set of controls and fixed effects for state and birth cohort, and the log of tuition and net price as the dependent variable, respectively. Income quintiles are computed over the analysis sample of aid applicants aged 25 or under in the restricted set of control states. Robust standard errors clustered at the state level are in parentheses. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.

Table 1.8: The Effect of HELOC-Eligibility on College Quality

	DDD	DID: Owners	DID: Renters
Panel A: Peer Ability			
Median SAT	12.37	26.31**	-4.52
(Admitted Class)	(10.21)	(9.15)	(21.06)
N	12,720	10,600	2,090
Panel B: College Selectivity			
Most Competitive	0.016**	0.021***	-0.006**
	(0.007)	(0.003)	(0.003)
Highly Competitive	0.008	0.019	0.013
	(0.015)	(0.012)	(0.009)
Very Competitive	-0.018	-0.006	0.019
	(0.014)	(0.003)	(0.021)
Competitive	0.005	0.024	0.038
	(0.023)	(0.036)	(0.046)
Less Competitive	0.015*	0.001	0.010
	(0.007)	(0.017)	(0.025)
Non-Competitive 4-year	0.033**	0.011	-0.046***
	(0.013)	(0.007)	(0.014)
2-year	-0.059**	-0.069***	-0.028
	(0.029)	(0.020)	(0.058)
N	28,400	21,280	7,120

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. Estimates are from the preferred specifications with full set of controls and fixed effects. Median SAT scores are estimated as the midpoint between the 25th and 75th percentiles. Robust standard errors, clustered at the state-own level, are in parentheses. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.

Table 1.9: Institutional Responses

Dependent Var.	DID				DDD			
	Tuition		Institutional Aid		Tuition		Institutional Aid	
	More Selective	Less Selective	More Selective	Less Selective	More Selective	Less Selective	More Selective	Less Selective
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	2,046.7***	123.5	959.5*	-200.0	-774.9	61.1	-2,421.1**	613.0***
	(615.2)	(251.1)	(576.2)	(149.8)	(754.7)	(226.7)	(1,124.4)	(175.0)
Fixed Effects:								
Institution	X	X	X	X	X	X	X	X
Cohort	X	X	X	X				
State	X	X	X	X				
State*Time					X	X	X	X
State*Cohort					X	X	X	X
State*Own					X	X	X	X
Cohort*Own					X	X	X	X
N	4,990	16,450	4,990	16,450	4,990	16,450	4,990	16,450
R-squared	0.952	0.904	0.540	0.488	0.961	0.906	0.577	0.502
2003-04 TX Mean	13,330	4,351	4,563	801				

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.

Notes: ***Indicates significance at the 1% level, **5%, and *10%. The sample includes students of all ages attending college in-state who applied for financial aid and are not missing imputed tuition, and excludes exclusively part-time students. Controls include EFC (for all colleges) and composite SAT score (for selective colleges only). Robust standard errors clustered at the institution level, are in parentheses. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.

1.9 Appendix

1.9.1 Data Appendix

NPSAS NPSAS reports tuition and fees paid (sticker price) for all students attending one institution in a given year, but not for students who transferred or stepped up (from a two- to four-year institution) in the middle of the academic year; these students represent roughly 8% of the entire NPSAS sample. For present purposes, however, the last institution attended is of primary concern rather than all tuition payments made at previous institutions. In order to include these students in the analysis, tuition is imputed by using the mean for cells determined by institution, attendance intensity (full- or part-time) and jurisdiction (public in-jurisdiction, public out-of-jurisdiction, and non-public) when there are at least ten observations in a cell. Sticker price and net price used in the analysis are based on this imputed tuition variable.

In order to include the log of household income as a control for families with non-positive income, I follow Altonji and Doraszelski (2005) and take household income as the maximum of the natural log of 50 and the natural log of reported income.

Barron's Selectivity Index Selectivity categories were obtained by merging the NPSAS data to the 2008 Barron's Admissions Competitive Index by institution. If an institution was missing from the 2008 Barron's data, the 2004 data was used. Colleges classified as "Special" by Barron's were omitted because they are generally art or music colleges with very different admissions criteria. Institutions that were missing from both years of Barron's were coded as non-selective (4-year or 2-year).

IPEDS As a measure of student peer quality, I merge NPSAS to IPEDS to obtain institution-level information on standardized test scores among admitted students. I estimate median composite SAT scores for an admitted class as the midpoint between the 25th and 75th percentiles, summed over Critical Reading and Math components. The synthetic control method relies on measures of state and local college funding per student, based on IPEDS measures of state and local appropriations and grants, and total enrollment figures. Tuition and fees used in Appendix Figure 1.18 are based on published in-state tuition for full-time, full-year undergraduates. See Jaquette and Parra (2014) for a discussion of relevant data issues with institutional analysis using IPEDS.

CPS All data on enrollment and completion, homeownership and household income are taken from the March Supplements of the CPS and accessed via IPUMS. High school enrollment rates are computed over 16 and 17 year olds.

Additional Data Sources Effective interest rates were taken from the Federal Housing Finance Agency's (FHFA) Monthly Interest Rate Survey (Table 15: Terms on Conventional Single Family Mortgages by State). The three-year change in statewide housing prices is computed as the the three-year change in the FHFA's non-seasonally adjusted state Housing Price Index ending in the second quarter of the beginning year in each academic year (i.e. three years ending in 2007Q2 for the 2007-08 academic year). Median statewide housing prices are taken from FHFA statistics for one-unit, non-condominium properties (also for the second quarter). The synthetic control method also relies on state-level Mexican-American population shares computed from the 2000 Decennial Census (accessed via IPUMS).

Appendix Table 1.20 is based on data from the Consumer Expenditure Survey. New vehicle purchases are taken as outlays for new vehicles including the purchase price or down payment, principal and interest paid for financed purchases.

1.9.2 Non-Education Spending Impacts

Section 2.1 argues that HELOCs can reduce financing costs for large consumption commitments spread out into the future and thus subject to greater income uncertainty. If this is indeed the case, then HELOC-eligibility should be associated with increased spending on costly durables such as new vehicle purchases, but not on small predictable expenditures such as food purchases. [50] To test this proposition, I turn to the Consumer Expenditure Survey to track spending on these household items in Texas and in other states.

Figure 1.20 plots the yearly treatment effects on owner-renter spending gaps from the preferred DDD specification with controls using all states for the control group. The graph on the left-hand side plots yearly treatment effects for the owner-renter gap in the log of food expenditures, revealing no significant increase in the spending gap after the introduction of HELOCs; yearly effects are significantly less than zero in 2000 and 2002, but the pattern in the pre-period is similar to the post-period. The graph on the right-hand side plots yearly treatment effects for new vehicle purchases (including down payments, principal and interest paid), revealing a pronounced jump in the spending of homeowners relative to renters after the introduction of HELOCs (though the annual jumps are only statistically significant in 2005). These results are consistent with the argument that HELOCs can reduce financing costs for large expenditures spread out over multiple years.

1.9.3 Alternative Methods of Statistical Inference

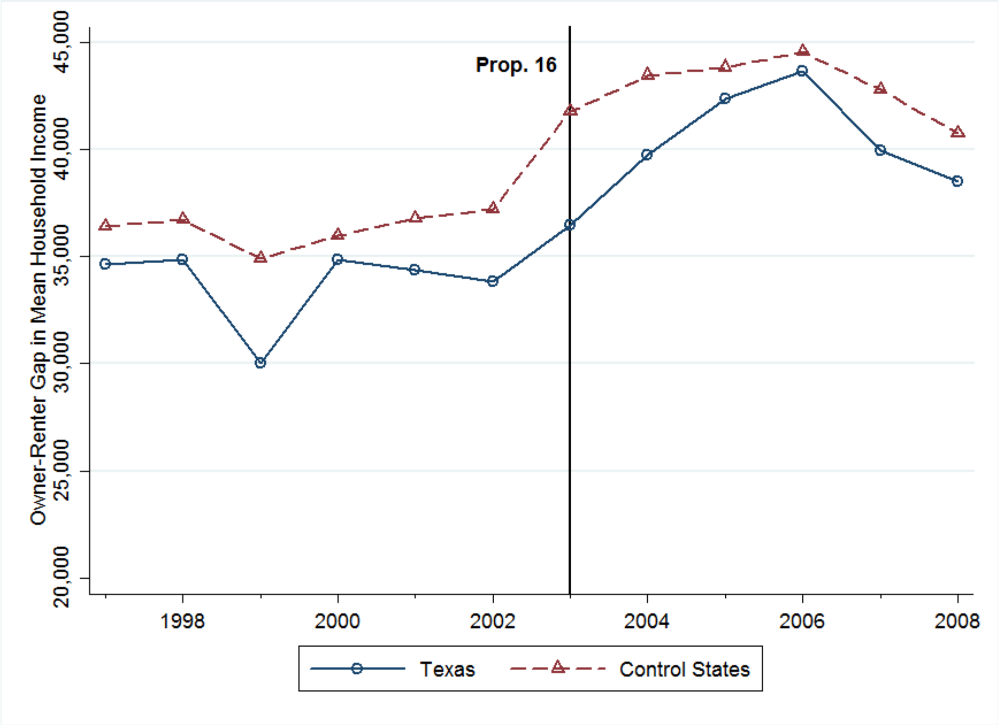
Table 1.16 shows that the conclusions from inference on sticker price impacts are not sensitive to alternative error structures. Column 1 presents 90 percent confidence intervals for the treatment effect for the preferred Within-Owner DID, column 2 presents estimates for an unweighted Within-Owner DID estimated over all states, and column 3 presents estimated for the preferred DDD specification. State-level clustering is intended to allow for arbitrary correlation between observations in the same state. Clustering within state-year cells provides a more restrictive error structure that allows for correlation between observations in the same state and year. Clustering within state-homeownership cells allows for correlation between homeowners in the same state and between renters in the same state (but does not allow correlation between homeowners and renters in the same state). The conclusions are

[50] There may be an income effect from a reduction in average financing costs that leads to increases on spending for normal goods, but it seems likely that this effect is of second-order importance relative to any price effects (including financing costs).

unchanged across alternative error structures, and state clustering yields the most conservative confidence intervals. Because clustered standard errors converge to their true values as the number of clusters increases, robustness of the conclusions to alternative error structures (with greater numbers of clusters) mitigates any concerns that the conclusions may be impacted by small-sample bias in the standard errors.

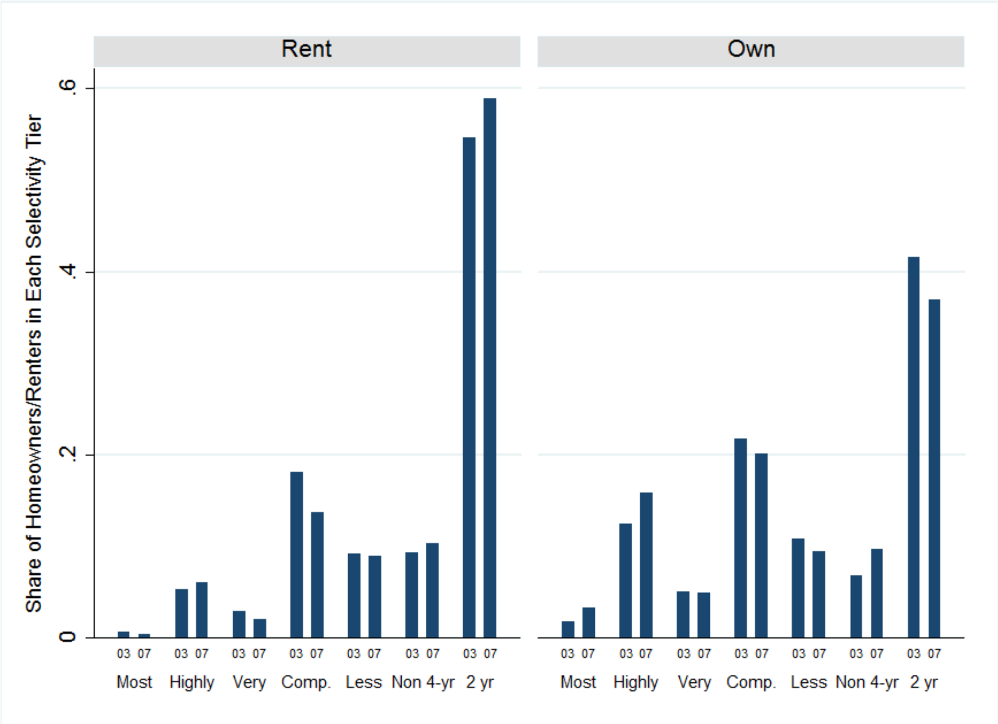
While clustering is intended to allow for inference in the face of correlated errors, a more general concern is whether inference is based on the appropriate small-sample distribution. Conley and Taber (2009) illustrate that for cases such as this where the policy change is restricted to a single cluster, the difference-in-difference estimator itself is inconsistent and thus subject to small-sample bias. They key idea behind their approach is that even though a consistent estimator of the treatment effect is unavailable, the larger number of control states can be used to estimate the distribution of the small-sample bias term (under the assumption of a common error distribution for treated and control states). The null hypothesis of no effect can then be rejected if the estimated treatment effect is a sufficiently unlikely event according to the empirical distribution derived from control states. This approach can provide an additional check on the robustness of the conclusions to alternative inference methods, by trading off assumptions about the appropriate small sample error distribution with the assumption of a common error distribution for treated and control states. Column 1 of Table 1.16 shows that the 90% Conley-Taber confidence interval is imprecisely estimated and does in fact include zero (though the one-tailed null hypothesis that the treatment effect is equal to 0 can be rejected with 90% confidence). Because of small sample sizes within many states, however, this approach is ill-suited for the present application because it makes no adjustment for imprecisely estimated control states.

Figure 1.16: Trends in Owner-Renter Income Inequality



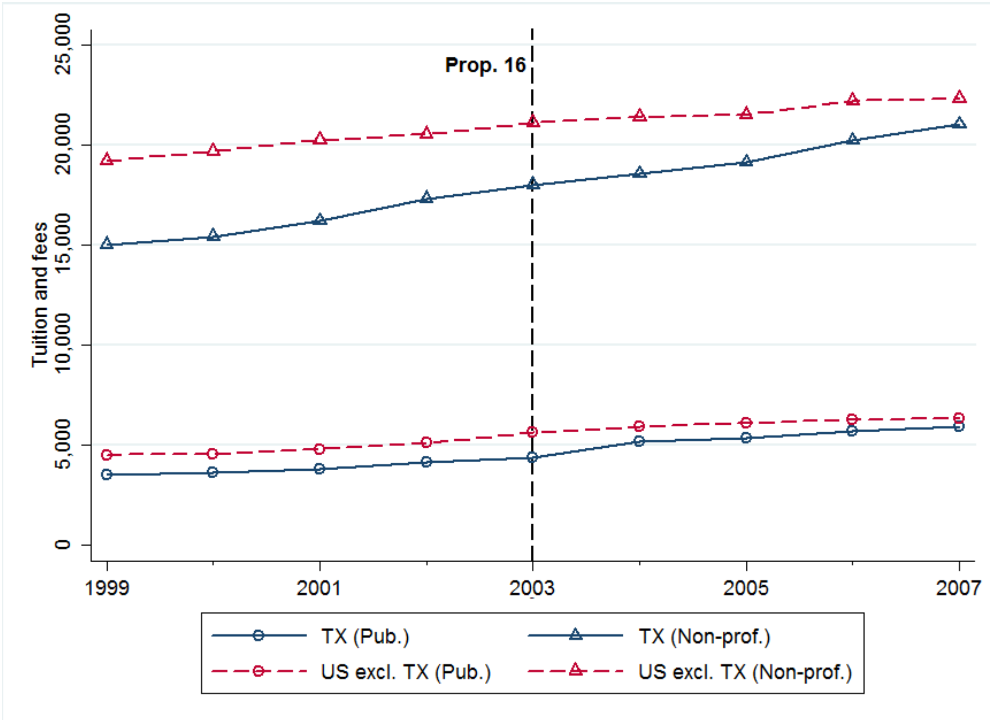
Source: Author's calculations using data from the CPS March Supplements.

Figure 1.17: Distribution of College Selectivity By Homeownership Status (Texas Colleges)



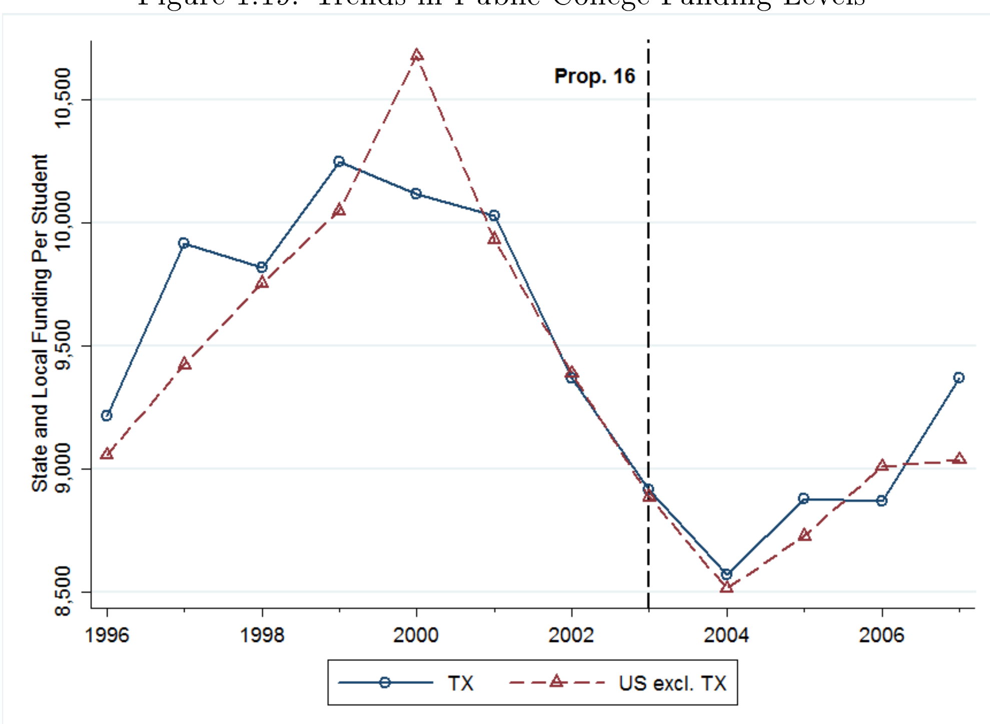
Source: Author's calculations using NPSAS data merged to the Barron's Selectivity Index.

Figure 1.18: Tuition Trends



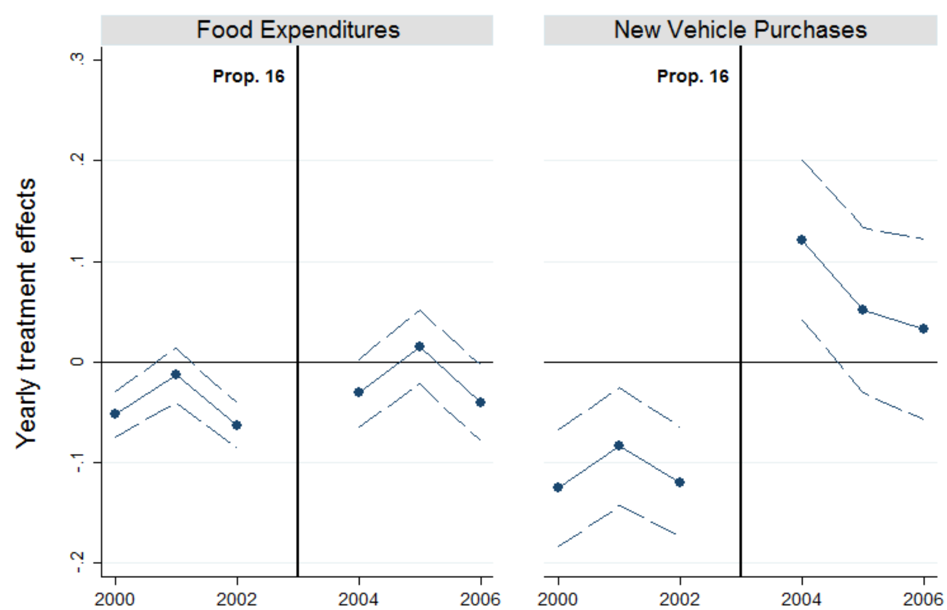
Source: Author's calculations using IPEDS data.

Figure 1.19: Trends in Public College Funding Levels



Source: Author's calculations using IPEDS data.

Figure 1.20: Owner-Renter Spending Gaps by Spending Type



Baseline triple difference specification with controls. 90% CI shown, the omitted base year is 2003.

Source: Author's calculations using Consumer Expenditure Survey data.

Table 1.10: The Effect of HELOC-Eligibility on College Enrollment (All States)

	DID		DDD	
	Within Owners	Within Renters		
	(1)	(2)	(3)	(4)
Post*TX	0.003	-0.071***		
	(0.009)	(0.012)		
Post*TX*Own			0.081***	0.068***
			(0.012)	(0.011)
Fixed Effects:				
Cohort	X	X		
State	X	X		
State*Cohort			X	X
State*Own			X	X
Cohort*Own			X	X
MSA				X
N	44,641	28,714	73,355	69,875
R-squared	0.059	0.020	0.076	0.102

Source: Author's calculations using the CPS analysis sample of 18-22 year olds, excluding the top 1% of the national income distribution.
Notes: The dependent variable is a dummy variable for college enrollment or any degree completion. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.11: The Effect of HELOC-Eligibility on High School Enrollment

	DDD	
	(1)	(2)
Post*TX*Own	-0.016	-0.004
	(0.016)	(0.017)
Fixed Effects:		
State*Cohort	X	X
State*Own	X	X
Cohort*Own	X	X
MSA		X
N	16,808	15,673
R-squared	0.032	0.043

Source: Author's calculations using the CPS analysis sample of 17 year olds.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is a dummy for HS enrollment among 16 and 17 year olds. Controls include the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.12: The Effect of HELOC-Eligibility on College Sticker/Net Price (Levels)

	DID: Within Owner		DID: Within Renter		DDD	
	Sticker	Net	Sticker	Net	Sticker	Net
	(1)	(2)	(3)	(4)	(5)	(6)
Post*TX	1101.2**	1251.8***	-143.0	36.7		
	(430.4)	(295.4)	(585.8)	(490.7)		
Post*TX*Own					1141.0**	1235.5***
					(441.9)	(373.2)
Fixed Effects:						
Cohort	X	X	X	X		
State	X	X	X	X		
State*Time					X	X
State*Cohort					X	X
State*Own					X	X
Cohort*Own					X	X
N	21,550	21,550	7,210	7,210	28,750	28,750
R-squared	0.232	0.189	0.163	0.119	0.249	0.199

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is sticker price or net price. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.13: The Effect of HELOC-Eligibility on College Sticker/Net Price (All States)

	DID: Within Owner		DID: Within Renter		DDD	
	Sticker	Net	Sticker	Net	Sticker	Net
	(1)	(2)	(3)	(4)	(5)	(6)
Post*TX	0.109***	0.154***	-0.015	0.026		
	(0.037)	(0.035)	(0.050)	(0.058)		
Post*TX*Own					0.109***	0.090*
					(0.036)	(0.049)
Fixed Effects:						
Cohort	X	X	X	X		
State	X	X	X	X		
State*Time					X	X
State*Cohort					X	X
State*Own					X	X
Cohort*Own					X	X
N	62,490	62,490	22,970	22,970	85,460	85,460
R-squared	0.291	0.169	0.241	0.217	0.310	0.207

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is the log sticker price or net price. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.14: The Effect of HELOC-Eligibility on Flagship Attendance

	DID: Within TX (N=6,180)	
	(1)	(2)
Post*Own	0.031**	0.031**
	(0.015)	(0.015)
Household Income		X
Cohort FEs	X	X
R-squared	0.057	0.059

Source: Author's calculations using the NPSAS analysis sample of Texas aid applicants under the age of 26. Notes: ***Indicates significance at the 1% level, **5%, and *10%. Estimates are from the Within-Texas specification with controls for each class year. Robust standard errors are in parentheses. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.

Table 1.15: The Effect of HELOC-Eligibility on Renter Attendance Intensity

	DID: Within Renters	
	All Renters	Selective Colleges
	(1)	(2)
Post*TX	0.118**	-0.019
	(0.051)	(0.053)
Fixed Effects:		
Cohort	X	X
State	X	X
N	7,210	2,670

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26. Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is a dummy variable for exclusively attending college full-time. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered at the state level, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 1.16: Comparing Alternative Inference Procedures–Sticker Price

	DID: Within Owner		DDD
	Reduced States	All States	Reduced States
	(1)	(2)	(3)
Coefficient estimate	0.141	0.227	0.147
Survey weights	X		X
90% confidence interval			
Cluster by state	(0.034, 0.248)	(0.140, 0.315)	(0.026, 0.268)
Cluster by state-year	(0.067, 0.215)	(0.165, 0.290)	(0.065, 0.229)
Cluster by state-own	–	–	(0.048, 0.246)
Conley-Taber	–	(-0.083, 0.533)	–
N	21,550	62,490	28,750

Source: Author's calculations using the NPSAS analysis sample of aid applicants under the age of 26.
Notes: For all columns, the dependent variable is the log of college sticker price. Column 1 presents estimates from the preferred Within-Owner specification over all states with dummies for class year, the standard set of controls, and fixed effects for state and cohort. Column 2 relies on the same specification as Column 1 but is estimated over all states and without survey weights. Column 3 presents estimates from the preferred DDD specification over the reduced set of control states with dummies for class year, the standard set of controls, and fixed effects for state-time, state-cohort, own-cohort and state-own cells. Conley-Taber confidence intervals are estimated using publicly available Stata code used in Conley and Taber (2009) with 5% tails of the 90% confidence interval computed from an empirical error distribution weighted by the number of observations within each control state. Observations are weighted by the NPSAS study weight (except for column 2), normalized to sum to one in each study wave.

2 Do English Learners Benefit from Mainstream Schooling? Evidence from Oakland Public Schools (with Naihobe Gonzalez[51])

[51]Department of Economics, Columbia University. ndg2109@columbia.edu.

2.1 Introduction

There are sizable gaps in academic achievement and attainment between Hispanic and non-Hispanic white students (e.g. Heckman and Lafontaine, 2010; Reardon and Galindo, 2009; Todd and Wolpin, 2007; Fryer and Levitt, 2004). These gaps may in part be explained by the fact that many Hispanic students do not speak English as their primary language at home, leading them to face the dual challenge of developing English fluency at the same time they are learning other material taught in English. A lack of English proficiency can not only limit progress in schooling, but can also hurt labor market outcomes down the road, especially for recent immigrants and their children (Trejo, 1997; Kossoudji, 1988). While school districts are required by federal law to assist English Learners (ELs), policymakers debate the timing and criteria used to transition students away from EL services and into mainstream classrooms.

ELs and former ELs who have been reclassified as Fluent English Proficient (RFEPs) represent a large and growing share of the public school student population in the U.S. Almost 1 out of every 9 students nationwide was an EL in 2007-08, with the number of ELs growing ten times faster than the number of English proficient students over the last decade (NCELA, 2010). California, where this study takes place, is home to the largest concentration of ELs in the country. The nearly 1.5 million ELs and 1.3 million RFEPs in California make up 43 percent of the K-12 student population and are distributed across 1,028 school districts. Each of these school districts is responsible for setting reclassification criteria that adequately transitions students from EL into mainstream education. Recent changes in California's school accountability and financing rules have established goals for increasing reclassification rates while also tying school funding to the number and concentration of ELs in a district, bringing the reclassification of these students to the forefront of K-12 education policy discussions in the state. Despite heightened attention to the issue, policymakers have limited causal research available on the effects of reclassification, particularly for long-run outcomes and for inframarginal students.

In this paper, we address some of the gaps in the empirical literature on reclassification by exploiting exogenous variation in the probability of reclassification introduced by the multiple criteria students must meet to be eligible for reclassification. We begin with a conventional regression discontinuity (RD) design that estimates the short and long-term effects of reclassification along both cognitive and non-cognitive measures for ELs who have met all criteria except potentially one and thus exhibit large jumps in the probability of reclassification around that cutoff. The analysis focuses on EL students in the Oakland Unified School District, a public school district in northern California with an enrollment of approximately 47,000 students during the 2013-14 school year, 49 percent of whom spoke a language other than English at home.[52] Oakland Unified is the 12th largest school district in California and has a high share of minority, socioeconomically disadvantaged students. Academically, the average Oakland student scores in the 28th percentile in math and 25th percentile in reading compared to students nationwide (Global Report Card, 2011).

Our RD estimates suggest that reclassification has very limited effects on students at the margin. We first estimate short-term reclassification effects (one year out) separately for students in elementary and secondary grades. These groups of students receive different EL services and are widely thought to respond differently to reclassification. We explore a number of cognitive and non-cognitive outcomes, but find few statistically significant effects. What effects we do find suggest that the timing of reclassification may indeed matter, though not necessarily through effects on student learning. First, cumulative GPA increases by 0.20 grade points for students reclassified the year before entering middle school. Despite no cognitive gains as measured by scores on the California Standards Test (CST), these students receive better grades in their first middle school classes, possibly due to increased motivation or effort. Second, attendance rates for students in secondary grades decrease by about 0.5 percentage points, equivalent to missing one more day of school each year based on the maximum 180 days of enrollment. Because no effects are found for cognitive achievement,

[52]About two-thirds of ELs in 2013-14 spoke Spanish, and 16 percent spoke Cantonese. The remaining 20 percent of EL students represented 44 different home languages.

this attendance result does not appear to affect student learning.

Even though we do not find significant short-term effects, it is possible that benefits of reclassification accrue and intensify over time, or that long-term outcomes like college enrollment are influenced by non-cognitive factors impacted by reclassification, such as exposure to different peer groups. To date, there is very little empirical evidence on the effects of reclassification in the long term. We examine a number of longer run outcomes, including CST English Language Arts (ELA) and Math scores in grade 11, high school graduation, meeting college prep course requirements, and enrolling in a four-year college, and find little evidence of long-term effects for EL students between grades 6 and 10 who were up for reclassification in our sample. There is some weak evidence of increases in SAT-taking and four-year college enrollment, but limited statistical power hinders our ability to draw definitive conclusions regarding small changes.

Of the limited body of work exploring the causal effects of reclassification, all existing estimates are local to students on the margin of reclassification. While this group is quite narrow, estimates based on marginal students can have important policy implications. For example, a positive effect for students at the margin would imply that otherwise similar students just below the reclassification cutoff would also benefit from being reclassified, suggesting that criteria should be lowered until the marginal effect of reclassification is zero. The policy implications of null effects, like those we find in our paper, are more difficult to discern without knowing how reclassification affects students away from the cutoff. If treatment effects are constant across all values of CST ELA reclassification scores, for example, then our null effects would imply that reclassification does not benefit any students. Alternatively, if treatment effects vary with reclassification scores, then it is possible that inframarginal students do benefit from reclassification and the cutoff is appropriately set at the point where the performance of the marginal student would be the same in either setting.

Motivated by this limitation, we present an extension from the conventional RD design in order to draw conclusions about the effects of reclassification for students whose reclassification scores place them well above the cutoff. The framework we present exploits the fact that some students who meet the first cutoff will remain untreated due to being below the cutoff for a second running variable. These untreated students provide additional information on the relationship between reclassification test scores and outcomes, which can then be used to inform our expectation of counterfactual outcomes for reclassified students in the absence of reclassification. More specifically, we can use this information for EL students who were not reclassified to estimate outcomes for reclassified students in the absence of reclassification under a straightforward separability assumption that can be examined in the data.

Formally, the identifying assumption requires that the relationship between outcomes and CST ELA reclassification test scores is not impacted by a second reclassification test (the Overall score for the California English Language Development Test, or CELDT), after conditioning on additional pre-treatment information such as CELDT subtest scores and student background measures. We show that this assumption holds in the data for students who are below either cutoff (CST ELA or CELDT Overall). Given this assumption, we can then estimate the relationship between outcomes and CST ELA reclassification test scores for these non-reclassified EL students and use it to predict outcomes for reclassified students in the absence of reclassification. Estimates of the effect of reclassification for any CST ELA score above the cutoff can then be obtained by comparing this prediction to the observed value for reclassified students.

Our estimates imply that for all students in elementary school who were above the CST ELA cutoff (and all other reclassification test score cutoffs), the average effect of reclassification into mainstream classes on CST ELA scores in the following year is an increase of 0.182σ. These results imply that the CST ELA cutoff should not be raised for students in grades 3 through 5, as benefits accrue to students above the current cutoffs. However, without knowing how students below the current cutoff are impacted by reclassification, we cannot definitively say whether policymakers should consider relaxing the reclassification criteria.

Beyond the immediate application to reclassification policy in Oakland Unified, the framework we introduce for estimating treatment effects above the cutoff can apply to any setting where treatment status is based on multiple criteria. Thus, our paper joins a growing literature that exploits additional sources of variation to draw conclusions away from treatment cutoffs that conventional RD designs are bound to (e.g. Angrist and Rokkanen, 2015; Wing and Cook, 2013; Mealli and Rampichini, 2012; and Jackson, 2010). For example, Jackson (2010), who studies the benefits of attending better schools, exploits a school assignment mechanism that depends on both a deterministic rule and family preference. Mealli and Rampichini (2012) evaluate the effects of university grants in a setting where deterministic rules restrict both students' eligibility to apply and ultimate chances of receiving the grant. Similarly, our approach highlights how the existence of additional factors that affect treatment status (such as a second running variable in settings with multiple criteria) provides useful information for an otherwise unobserved counterfactual. In the case when assignment depends on multiple running variables, which is common across many interventions, we argue that the plausibility of the identifying assumption can be convincingly tested using data from the various untreated groups that arise. Estimation proceeds using one of two approaches: a difference-in-difference (DID) style estimator that is nested within an RD, or direct extrapolation in the spirit of Rokkanen (2014).

To the best of our knowledge, this paper provides the first estimates of treatment effects from reclassification for students away from the cutoff, and the first estimates of long-term effects on degree completion and postsecondary enrollment for students at the cutoff. The rest of the paper proceeds as follows. Section 2 describes the EL program and reclassification policies in Oakland Unified and summarizes existing research on the effects of EL programs. Section 3 introduces the RD design used to estimate reclassification effects at the cutoff. The data and sample selection are described in Section 4. The fifth section presents results for EL students at the reclassification cutoff. In Section 6 we describe the implications of the RD results for optimal reclassification policy, before presenting a framework for estimating treatment effects for students above the cutoff and the corresponding results. Section 7 concludes.

2.2 Background

2.2.1 Institutional Background

Of the nearly 1.5 million ELs in California who make up a quarter of all K-12 students in the state, about 10 percent become "Reclassified as Fluent English Proficient" (RFEP) each year (Jacobs, 2008). Whether this rate is too high or too low is a hotly contested question, with one camp advocating for less stringent reclassification criteria that moves more students into mainstream education and another warning that students reclassified too soon may suffer without English language support. On one hand, easing reclassification requirements may mean students enter mainstream education earlier, avoiding the risk that they will be tracked into classes with lower expectations. On the other, it may raise the risk that they will struggle with school work without the special help provided to ELs. The argument for increased reclassification has received greater support in recent years, as it is buttressed by descriptive studies showing that RFEP students perform better than ELs and as well as, or better, than native English speakers (see Hill et al., 2014a; Saunders and Marcelletti, 2012; Hill, 2012; Gándara and Rumberger, 2006). For example, RFEP students are the least likely of all language fluency groups, including native speakers, to drop out of high school and the most likely to graduate within four years, even after controlling for individual and district characteristics (Hill et al., 2014a).

Of particular concern to policymakers are so-called long-term ELs (LTELs), students who remain ELs for six or more years. These students have especially poor outcomes. Even when they are reclassified, LTELs do not perform as well, on average, as RFEPs who are reclassified at younger grades (Hill et al., 2014a). Despite the fact that LTELs, who were unable to meet the reclassification criteria in earlier grades, are a negatively selected group, many see their lower performance as evidence that EL students should be reclassified before they reach middle school. Officials from the California education department have said that "the optimal zone for reclassification is second through fifth grade." Some stakeholders take this to mean that later reclassification hurts students relative to early reclassification.[53] Taylor (2004) finds that about half of EL students who begin school in California as kindergarteners are not reclassified by sixth grade and about 60 percent of ELs in grades 6-12 are LTELs.

Aside from facing uncertainty about what is best for students, districts receive conflicting incentives from policymakers. In California, federal and state accountability systems hold districts accountable for increasing reclassification rates, yet under the state's new school funding formula, districts with significant EL populations receive additional funding.[54][55] Finally, because the CST will be replaced by new Common Core-aligned assessments beginning in the 2014-15 academic year, districts across the state are being forced to redesign their reclassification criteria.[56] These dynamics are also present in Oakland Unified, which qualifies for concentration funding due to its high rate of EL and low-income students (about 75 percent of students are ELs and/or qualify for free or reduced price lunch) but has made increasing reclassification rates one of its priorities. In 2013, Oakland Unified set a goal in its state LCAP of increasing reclassification of ELs by 11 percent annually from a baseline rate of 11.7 percent, and increasing reclassification of LTELs by 20 percent annually from a baseline rate of 6.9 percent.

[53]The head of a parent advocacy non-profit was quoted in Kuznia (2012) as saying: "If kids haven't been reclassified by fifth grade, they have pretty much been tracked, and are not going to be able to go to college."

[54]The Local Control Accountability Plan, California's new accountability system, requires districts to set their own goal for increasing the EL reclassification rate. The California Office to Reform Education (CORE) waiver from the mandates of the No Child Left Behind law, in which districts like Los Angeles Unified, San Francisco Unified, and Oakland Unified participate, requires districts to increase the reclassification rates of long-term ELs.

[55]For each EL, districts receive additional funding equal to 20 percent of the per-pupil base rate. For example, a 9th grade EL student generates an additional $1701. In addition to this supplemental funding, districts with high shares of EL and low-income students receive concentration funds. Each EL/low-income student above 55 percent of enrollment generates an additional 50 percent of the base rate, equal to an extra $4252 per 9th grade EL in this example (Taylor, 2013).

[56]Districts can also choose to adjust their reclassification criteria at any point. For example, in 2006, Long Beach Unified decided to increase the CST ELA requirement from 300 to 325 following concerns that some reclassified students were struggling to succeed academically (Jacobs, 2008).

2.2.2 Criteria Used for Reclassification

While the state provides guidelines for reclassification, individual school districts have a significant amount of leeway in determining reclassification criteria. As a result, reclassification standards vary widely across the state. Hill et al.'s (2014a) survey of school districts documents the disparities in criteria and finds that 90 percent of districts set standards that are more stringent than the state's recommendations. California requires that districts use four criteria to determine reclassification of ELs: 1) an assessment of basic English skills, 2) an assessment of English proficiency, 3) teacher evaluation, and 4) parental input. To meet the basic English skills requirement, the state recommends students score at least a score of 300 on the California Standards Test in English language arts (CST ELA). For the English proficiency requirement, the state recommends students achieve a minimum overall proficiency level of 4 (Early Advanced) on the California English Language Development Test (CELDT), and minimum scores on each subtest—listening, speaking, reading, and writing—of 3 (Intermediate) or higher. Districts with stricter criteria tend to require higher performance on the CST ELA and/or a minimum GPA in English and/or mathematics classes.

Like most other districts in California, Oakland Unified employs stricter reclassification standards than those recommended by the state. Until the 2014-15 academic year, when CST scores were no longer available, the district required a minimum CST ELA score of 325 for reclassification. This is the minimum score associated with reaching a Mid-Basic performance level on the CST, a category pre-defined by the state. Primary students must also receive a teacher recommendation, and secondary students are required to be successful in their English courses, though this requirement has been assessed both subjectively and objectively (based on course grades) at different points in time. Until 2011-12, high school students who did not meet the CST ELA requirement but passed the California High School Exit Exam in ELA (CAHSEE ELA) were allowed to replace their CST ELA score with the CAHSEE ELA. Finally, students have to meet the CELDT requirements recommended by the state. Note that Oakland Unified does not permit reclassification in grades K-2, when students do not yet have CST ELA scores. Roughly half of districts in the state do not permit reclassification prior to grade 3 (Hill et al, 2014a).

Despite the multitude of requirements students must meet to be reclassified, not all impact the likelihood of reclassification equally. In Hill et al.'s (2014a) statewide survey, districts reported that the CST ELA requirement was the most difficult for students to meet across all grade levels, but especially so for secondary students. Which criterion is hardest to meet is clearly impacted by where districts set the CST ELA cutoff score. For example, in Los Angeles Unified, which requires a minimum score of 300, the CST ELA constrains 40 percent of 5th graders. This rate is nearly 60 percent at San Diego Unified, where the minimum CST ELA score is set at 333 (Hill et al., 2014b). The criterion that is "hardest to meet" for each student is the one with the greatest distance between that student's test score and the minimum score required for reclassification. In Oakland Unified, which set the CST ELA cutoff score at 325, the CST ELA is overwhelmingly the main obstacle for most students in all grades above grade 3 (see Figure 2.1).

2.2.3 Factors Affected by Reclassification

Of course, how many EL students are reclassified and how well they do following reclassification depends not just on whether criteria are set appropriately, but also on the quality of EL services provided. High-quality EL services should succeed in helping more students reach the level of English proficiency necessary for both reclassification and success in mainstream education. While identifying the specific services a student receives is difficult, ELs in California generally receive instruction in one of three settings. The first is Structured English Immersion (SEI), a classroom setting where ELs who have not yet acquired reasonable fluency in English, as defined by the school district, receive instruction through an English language acquisition process in which nearly all classroom instruction is in English but with a curriculum and presentation designed for children who are learning the language. All districts in the state are required to offer an SEI program. The second, and most common, setting is known as English Language Mainstream (ELM), in which ELs who have acquired reasonable fluency in English, as defined by the school district, receive English Language Development (ELD) instruction either integrated into mainstream instruction (known as Integrated ELD) or during a protected time during the school day (known as Designated ELD).

The third category of EL services is known as alternative programs. Particular schools offer programs like early-exit bilingual programs, where teachers utilize students' native language for early reading and clarification for 2-3 years before students are mainstreamed into English-only classrooms, and one-way dual language programs, in which ELs are taught to become bilingual in English and their native language. Student placement into these programs is based on a particular district's offerings and parental choice. In Oakland Unified, 14 elementary schools offer early-exit bilingual programs and six elementary schools offer dual language programs. In addition, ten secondary schools offer special newcomer programs for EL students who have recently arrived to the U.S. Across all categories, teachers of ELs must possess or be in training for Crosscultural Language and Academic Development (CLAD) or Bilingual CLAD (BCLAD) certificates. This requirement restricts the allocation of students across classrooms as well as ELs' access to courses not taught by CLAD or BCLAD teachers. Finally, some ELs may receive no services at all. Olsen (2010) estimates the share at 12 percent statewide, while data from Oakland Unified suggest about 9 percent of ELs there receive no services.

Evidence on the quality of these offerings is sparse, but what exists does not paint a positive picture. Based on survey data from 40 California districts and reviews of the literature, Olsen (2010) characterizes the instructional experience of ELs in the state as one in which students receive elementary school curriculum and materials not designed to meet EL needs; are enrolled in weak language development program models; experience inconsistent programs as they move across grades, schools, and districts; and are socially segregated and linguistically isolated. In secondary grades, Olsen (2010) further documents inappropriate placement into mainstream, unprepared teachers, and limited access to the full college preparatory curriculum. A 2015 study of EL services in Oakland Unified conducted by the Stanford University Graduate School of Education found that there was no clear language instruction in about 75% of classrooms, that ELs lacked consistent access to college preparatory course offerings (known as A-G), and that ELD teachers were among the newest and least experienced teachers in the district. Nevertheless, there is no guarantee that being reclassified will lead ELs into a much better track: Parrish et al. (2006) quote an administrator who questioned the practical difference between students being in "an ELD-style program or if they're sitting at the lowest-level of the English-only classes."

While the instructional change is arguably the biggest difference reclassified students experience, other conditions like classroom peer composition change as well. If educational achievement and attainment are reinforced by interactions between students (both inside classrooms and within social networks outside of classrooms), segregating ELs into or within classrooms may have impacts on longer-run educational outcomes irrespective of any in-class instructional differences. Moreover, the effects of social capital on attainment may not be apparent from short-term impacts on test scores. Recent studies have shown that social capital is positively related to academic achievement in high school (Fryer and Torelli, 2010; Calvo-Armengol et al., 2009), and may also affect college enrollment (Furstenberg and Hughes, 1995; Yan 1999) and college choice (Person and Rosenbaum, 2006). While previous studies have found that exposure to ELs can reduce achievement among native English speakers (Chin et al., 2013; Cho, 2011), little is known about longer-term impacts or peer effects on EL students reclassified into mainstream settings. The estimates presented in this paper reflect the causal effect of reclassification which captures the totality of changes ELs experience when their status changes, both over shorter time horizons (one year out) and longer horizons (grade 12 outcomes).

2.2.4 Previous Research

There is a lengthy history of empirical work producing mixed results on the relative effects of EL education versus immersion into mainstream education (see Slavin et al. 2011 for a summary). Most of these studies have a limited causal interpretation on account of their reliance on matching techniques rather than random assignment. In addition, most of these studies date back to before 2000, yet EL education in California has changed dramatically since the passage of Proposition 227 in 1998, which required that ELs be taught "overwhelmingly in English." There are a smaller number of recent studies that account for the fact that program assignment is not random but in fact correlated with other determinants of English proficiency and academic achievement like student ability, motivation and background, as well as other factors that vary across parents, peers, schools, and districts.

Among recent studies evaluating the effects of initial classification as an EL and reclassification as an RFEP with a causal interpretation, little consensus has emerged regarding the impacts of classification into and out of EL programs. Two recent studies use the regression discontinuity design to evaluate classification impacts in the state of California. Pope (forthcoming) estimates the marginal effects of classification and reclassification using data from Los Angeles Unified. Pope (forthcoming) exploits cutoffs in the overall CELDT scores to examine the effects of being initially classified as an EL in kindergarten as well as the effects of being reclassified out of EL education. He finds that marginal kindergarteners receive small gains in ELA standardized test scores from 2nd to 8th grade, and no gains in math scores, GPA, attendance, or grade retention. For reclassification, Pope (forthcoming) finds that marginal students reclassified in 2nd to 4th grade obtain large benefits, which persist over time, in their ELA test scores and English GPA. Notably, most of the effects are accrued by boys. Marginal students reclassified in 5th to 10th grade, however, obtain no benefit. On the surface his results suggest that earlier reclassification may, in fact, help students. However, reclassification effects that vary by grade level may also be driven by differing reclassification rates by grade, differing language acquisition capability of older versus younger students, and differences in the distribution of ability for reclassified students at different grade levels. Nevertheless, Pope (forthcoming) argues that achievement gains are possible by increasing the inflow and early outflow of ELs.

Robinson (2011) estimates the marginal effects of reclassification for a large urban (and anonymous) school district in California. Instead of only looking at assignment around the CELDT cutoff, this study standardizes scores from all of the criteria, recenters each around its respective cutoff, and uses the minimum value for each student as the running variable in a fuzzy regression discontinuity design. This approach allows the author to include more students in his estimation, but as we discuss in Section 2.3, also implies that his estimates reflect a weighted average of the treatment effects for each of the triggered reclassification criteria. The weights are arbitrary because they depend on the scale or distribution of the criteria involved, and can therefore somewhat limit interpretation, particularly if treatment effects are not homogeneous across the various criteria and cut-points. In the district he studies, Robinson (2011) finds that CELDT Reading is the minimum reclassification test score among 4th graders, but by 7th grade the CST ELA test becomes the main obstacle for most students. In contrast to the results from Pope (forthcoming), Robinson (2011) does not find strong evidence of short-term (year or year-after) reclassification effects on CST ELA scores or other outcomes for elementary or middle-school ELs, and finds negative effects on CST ELA for high school students. His results suggest that action should be taken to modify the criteria and/or instruction in place for high school ELs, since the non-reclassified setting is more effective than the reclassified setting for these students.

An earlier regression discontinuity study by Matsudaira (2005) examines the marginal effects of reclassification for 4th graders at a large, urban school district in the northeast. Despite striking differences in classroom environments, Matsudaira (2005) finds negligible differences in achievement in both reading and math across up to four years after reclassification, consistent with the findings in Robinson (2011). In a study evaluating the impacts of different types of EL instruction, Slavin et al. (2011) rely on an experimental design using three cohorts of students in six cities across the U.S., beginning with kindergartners in 2004. Spanish-dominant students with parent approval were randomly assigned to either transitional bilingual education (TBE) or SEI and then followed for five years. Vocabulary and reading tests revealed that TBE students initially performed worse than SEI students in Spanish and better in English, but no significant differences remained by 4th grade after all TBE students had transitioned to traditional instruction in a mainstream setting.

Because the availability and implementation of EL services can vary widely across schools, even those within the same state, it is not altogether surprising that studies based in different districts have found inconsistent results. Nevertheless, existing studies with causal interpretations appear to rarely find any significant results. As Robinson (2011) points out, finding anything other than a null effect for reclassification is problematic because it suggests a poor transition between instructional settings. If reclassifying students at the margin leads them to perform better, it implies that gains are available by easing the reclassification criteria and reclassifying more students. The converse argument also applies. However, a null treatment effect can imply many scenarios leading to different policy implications. We return to this discussion of optimal reclassification policies in Section 2.6. Furthermore, because all of the papers with a causal interpretation use an RD design, the effects they estimate are limited to those students at the margin of the two programs. Section 2.6 also examines outcomes for students away from the cutoff. Finally, our paper contributes what may be the first causal estimates of the effects of reclassification on long-term outcomes like high school graduation and college enrollment. Even in the absence of cognitive gains, these outcomes may be mediated not just by instructional differences but also by exposure to different groups of peers and social networks.

2.3 Empirical Strategy

Although the non-random assignment of reclassification decisions poses challenges to identifying causal effects, the criteria described above yield large discontinuities in reclassification probabilities around the cutoffs that can be exploited in a regression discontinuity (RD) design. The RD uncovers the local average causal effect for students with reclassification test scores arbitrarily close to the cutoffs, provided that nothing else changes discontinuously at the cutoff other than the probability of reclassification. While a traditional RD estimates a single average treatment effect around a single test score cutoff, EL reclassification utilizes up to seven test scores and cutoffs (CST ELA, CELDT Overall, CELDT Listening, CELDT Writing, CELDT Reading, CELDT Speaking, and CAHSEE ELA). When there are multiple treatment criteria to consider, each one can be exploited in a separate RD design based on individuals who have met all other criteria except the one being examined, thus yielding multiple estimates of treatment effects (one for each criterion). Alternatively, multiple treatment criteria can be collapsed into a single running variable by taking the minimum (standardized) value across all criteria, thus generating a single estimate of the average treatment across all criteria (henceforth referred to as the "minimum score method").

The tradeoff between these two methods is primarily between statistical power and policy-relevant interpretation: the average treatment effect uncovered by the minimum score method is a weighted average of all treatment effects across the various criteria with weights that are arbitrary (and have no inherent meaning) because they depend on the scale or distribution of the running variables involved, and can therefore somewhat limit interpretation. However, the minimum score method always utilizes more data. Furthermore, in the case when criteria are highly correlated, there will be relatively little data available for estimation by individual criteria. Estimation by criteria is performed on individuals who meet all criteria except potentially one. If correlations between criteria are strong, then most observations will fall on the right side of the cutoff, leaving relatively little data on the left for estimation. Simulations suggest the two methods yield similarly low rates of estimation bias, though the minimum score method minimizes mean squared error (see Porter et al., 2014 for a detailed discussion of RD estimators when multiple running variables apply). Thus, the minimum score method appears preferable if there is reason to believe that the treatment effects are homogenous across criteria.

For the RD design to be valid, there should be no discontinuity in the distribution of the reclassification test scores just above or below the cutoff (Imbens and Lemieux, 2007). Finding such a discontinuity would suggest students just around the cutoff differ in ways other than their scores (as in the case of manipulation around the cutoff). Figure 2.2 shows the distribution of potential running variables around the relative cutoffs for all EL students in the two samples we construct (and describe in more detail in the following section). Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Recall from Figure 2.1, which showed which tests were the minimum score for students in the district, that the CST ELA is by far the most common reclassification obstacle. The only other test that would provide a sufficient number of observations for estimation by criteria is CELDT Overall. Thus, we examine the distribution of these two tests, as well as the distribution of students' single minimum score score, in Figure 2.2. Visual inspection and results from the statistical density test proposed by McCrary (2008) suggest that while the CST ELA is smooth around the cutoff, CELDT Overall and other CELDT components exhibit evidence of potential manipulation.

As a result, the distribution of the minimum score also appears to fail the smoothness assumption. A key difference between the CST and CELDT is that parts of the CELDT (for example, the speaking section and a subset of the reading section) are administered separately for individual students and responses are recorded by the examining teacher. In addition, districts are allowed to perform preliminary scoring of the CELDT before sending answer sheets to the state for official scoring, whereas CST ELA answer sheets are sealed before being sent to be scored offsite. Due to the prospect of manipulation of the CELDT, we thus choose to primarily focus our empirical strategy on students who have reached all other reclassification test score cutoffs except potentially the CST ELA. Henceforth, this group of students is referred to as being "on the CST ELA frontier."

While the smoothness assumption of CST ELA scores appears to be validated, there are other conditions that must be met for the RD approach using the CST ELA frontier to yield unbiased estimates. For one, the CST ELA cut-point must be exogenous. As detailed in Section 2.2.2, the cut-point selected by Oakland Unified is the minimum score associated with reaching a Mid-Basic performance level on the CST, a category pre-defined by the state. In addition, there must be no other relevant ways in which students on one side of the cut-point are treated differently from those on the other side, other than reclassification. The concept of "Mid-Basic" is not commonly used, since the standard performance levels reported for the CST are Far Below Basic, Basic, Proficient, and Advanced. Figures 2.9 and 2.10 graph a sampling of non-outcome variables (U.S. born, special education, and Asian) against CST ELA scores for each of our samples as a check for the orthogonality of unobservables. No discontinuities are visible.

To confirm that students on the CST ELA frontier experience jumps in the probability of being reclassified around the CST ELA cutoff, we plot the relationship between the probability of reclassification and standardized, re-centered CST ELA scores for our two samples in the first row of Figures 2.7 and 2.8.[57] The probability of reclassification for students in grades 3-5 observed one year after jumps from zero to almost 90 percent. For students in grades 6-10 in this sample, the probability of reclassification jumps from close to zero to approximately 82 percent. Students in grades 6-10 in our second sample, whom we observe through 12th grade, have a higher probability of reclassification left of the cutoff than in the other sample but still experience a significant jump in the probability of being reclassified, from approximately 18 to 98 percent. In this setting, even for EL students on the CST ELA frontier who have met all other testing criteria, reaching the CST ELA cutoff does not perfectly predict reclassification primarily due to additional unobserved requirements for parent and teacher approval, changing course grade criteria, and data entry errors. It should also be noted that students are subject to reclassification every year until they are reclassified or complete high school.

[57] A bin size of 0.1 standard deviations is used in all graphs.

When thinking about the effects of reclassification on outcomes measured at various future points in time, the treatment may best be viewed as a continuous variable reflecting the duration of RFEP status, rather than the usual binary variable indicating treatment status in a given year. For instance, the effects of reclassification may be different for students who have spent more years in mainstream education than for those who were recently reclassified. Using this continuous measure of treatment exposure also reveals pronounced discontinuities around the cutoff (see the second row of Figures 2.7 and 2.8). The range of this variable naturally depends on the time horizon for measuring outcomes. When looking at outcomes at the end of the following year, for example, RFEP duration will range from zero to a maximum of 2 academic years or about 1.8 calendar years (since the final academic year does not span the summer months). Students whom we observe through 12th grade naturally spend a higher number of years as RFEPs on average.

Using the continuous measure of treatment exposure does not directly allow us to test whether treatment effects fade away or intensify as the number of years spent reclassified increases. However, we estimate treatment effects on both short-term outcomes up to two years after reclassification and long-term outcomes occurring at the end of high school.

While RFEP duration is used as a continuous measure of treatment exposure in the subsequent estimation, another way to understand how treatment exposure is accumulated over a range of years is to consider the likelihood of remaining an EL student at the end of a particular period of time (e.g. one year out, or in 12th grade). The third row of graphs in Figures 2.7 and 2.8 illustrate how the likelihood of maintaining EL status one year out varies along with CST ELA reclassification scores. For this measure, the probability of remaining an EL student a year later falls from roughly 40 percent to 5 percent at the CST ELA cutoff.

Because RFEP duration is not perfectly determined by CST ELA score, this setting lends itself to estimation of treatment effects around the reclassification test score cutoffs using the fuzzy RD design (also known as the instrumental variables RD). The fuzzy RD design allows for an estimate of the effect of the treatment (an additional year of RFEP status) for individuals at the cutoff who become RFEPs by crossing the CST ELA cutoff:

$$\delta = \frac{\lim_{x \downarrow c} E\left[Y \mid X = x\right] - \lim_{x \uparrow c} E\left[Y \mid X = x\right]}{\lim_{x \downarrow c} E\left[T \mid X = x\right] - \lim_{x \uparrow c} E\left[T \mid X = x\right]} \tag{1}$$

where X denotes the recentered, standardized CST ELA score used for reclassification; c is the cutoff used for reclassification (here all scores are recentered such that $c = 0$); Y is the outcome of interest (e.g. CST ELA score in the following year, or high school graduation); and T is a continuous variable measuring the length of time a students spends as an RFEP student. In equation 1, the numerator is the difference in average outcomes for individuals right above and right below the cutoff. The denominator is the difference in RFEP duration for individuals right above and right below the cutoff.

Estimates of δ are obtained from a two-stage least squares (2SLS) specification which first predicts RFEP duration using the CST ELA reclassification test score discontinuity, and then uses the predicted value of RFEP duration to estimate the effect of an additional year of RFEP status on a given outcome:

$$T_i = \beta above_i + \pi_0 CSTELA_i below_i + \pi_1 CSTELA_i above_i + \phi \mathbf{X}_i + \sum_s s_i + \sum_g g_i + \sum_t t_i + \varepsilon_i \tag{2}$$

$$y_i = \delta \hat{T}_i + \gamma_0 CSTELA_i below_i + \gamma_1 CSTELA_i above_i + \theta \mathbf{X}_i + \sum_s s_i + \sum_g g_i + \sum_t t_i + \varepsilon_i \tag{3}$$

where i denotes an individual EL student; *below* and *above* are indicators for being below or above the relevant CST ELA cutoff; $\mathbf{X}$ is a vector of demographic controls including dummies for being born in the United States, ethnicity (African-American, Asian, Latino, white, or other), female, and being a special education student; the last three terms are fixed effects for school, grade, and year; and ε is an idiosyncratic error term. In this specification, δ gives the difference in outcomes at the cutoff, adjusting for differences in the number of years a student spends as an RFEP student until the time the outcome is observed. Results are presented along with robust standard errors clustered at the student level. Observations are weighted by an edge kernel restricted to a bandwidth computed using Imbens and Kalyanaraman (2011) "plug-in" procedure. This type of non-parametric estimation reduces the chances that bias is introduced by using a much smaller portion of the data where the relationship between the running variable and the outcome is more likely to be linear (or another order polynomial, in the case of local quadratic regressions and other higher order fits) and utilizing kernels with compact support that reduce sensitivity to observations farther from the cut-point. The baseline specification of equations 2 and 3 includes linear controls for the rating variable (CST ELA test scores) both below and above the cutoff. Alternative specifications include quadratic controls for the rating variable, alternative bandwidths ($\pm.25\sigma$), and a uniform kernel (equivalent to unweighted OLS) rather than the edge kernel.

2.4 Data

The data used in this paper span all students enrolled in non-charter schools in Oakland Unified between the 2005 (2005-06) and 2013 (2013-14) academic years. [58] Students are tracked longitudinally across years. The data consist of administrative data on student demographics, EL status, course enrollment and grades, attendance, and suspensions; standardized test results and cohort graduation outcomes from the California Department of Education and the College Board; and postsecondary enrollment from the National Student Clearinghouse, a non-profit organization that provides degree and enrollment verification for more than 3,300 colleges.

There are 20,709 students who make up the complete set of EL students observed in the data with non-missing reclassification test scores, of whom 35 percent are reclassified at some

[58] Of the 118 schools in the district in 2013, 32 of them were charter schools enrolling about 22 percent of all students. The share of students enrolled in charter schools is lower in earlier years (12 percent in 2005). Since charter schools are run independently, they may establish reclassification criteria different from that used by all other schools in the district.

point in the data. Because we observe students over time, we obtain 93,425 student-year records. Figure 2.3 shows reclassification rates by grade for all EL students as well as the subset of students who meet all the testing reclassification criteria in that grade. Recall that in addition to the CELDT and CST ELA requirements, teachers must recommend students for reclassification, grades in English courses must be deemed adequate, and parental input must be requested. No data are recorded for these additional factors. EL students in 3rd and 5th grade have the highest rates of reclassification, at approximately 30 and 28 percent, respectively. For all other grades, reclassification rates are at or below 15 percent. As expected, reclassification rates for students meeting the testing criteria are significantly higher, ranging approximately between 75 and 90 percent. However, they consistently fall below 100 percent, likely due to the other unobserved criteria in place. The share of students meeting all the testing criteria who are reclassified falls in later grades, consistent with course grade requirements affecting secondary students. Note in Figure 2.4 that over the years spanned by the data, reclassification rates have remained fairly constant, with the exception of a dip in 2007. However, the rate of reclassification for students who meet all the testing criteria has fallen from a high of 95 percent in 2006 to a low of 81 percent in 2012. The reasons for this change are unknown, but may be due to increased use of non-testing criteria in the reclassification decision.

As discussed earlier, ELs reclassified in earlier grades tend to have better outcomes than those who are reclassified in later grades. Figure 2.5 shows select student outcomes in 11th and 12th grade by grade of reclassification together with select student background characteristics for ELs observed through grade 12 (which does not necessarily reflect the population of EL students in the district). As the first graph indicates, students who are reclassified earlier have higher CST ELA scores in 11th grade than students who are reclassified later, and reclassified students outperform ELs who are never reclassified regardless of the grade of reclassification (no pattern in CST Math performance is discerned). Looking at 12th grade outcomes (graduation, meeting A-G requirements, and enrolling in college), the relationship between grade of reclassification and achievement is not as clear, though it appears students reclassified in grades 7-11 have better outcomes than students reclassified in grades 5-6 and grade 12. Reclassified students outperform students never reclassified in all three outcomes, regardless of when they were reclassified. However, as the first panel indicates, students differ markedly by their grade of reclassification. Intuitively, as students fail to reclassify in earlier grades, they become more negatively selected. Students reclassified in later grades are more likely to be born outside of the U.S. and are somewhat more likely to be identified as special education students, as are students who are never reclassified. Figure 2.5 highlights the danger in comparing outcomes between reclassified and non-reclassified ELs and between ELs reclassified in early versus later grades.

2.4.1 Sample Selection

Since we are interested in measuring both the short and long-term effects of reclassification, we must follow students into the years following their reclassification to RFEP. In making decisions about sample selection, we wish to maximize the number of students observed in the data both at the time of reclassification and at the time of relevant outcomes. Because many of the recent younger students have not yet progressed through their primary and secondary education, and because mobility and attrition are both prevalent in the data, tracking future outcomes (particularly in grade 12) reduces the sample size of ELs that can be studied. Thus, while there are benefits to utilizing the same sample throughout the paper, we choose to create two main analysis samples: one which consists of EL students observed the year after reclassification for our estimations of short-term effects, and one which consists of students observed in the year of reclassification and in grade 12. The first sample, which we term the "one year out" sample, consists of 33,931 observations for EL students between grades 3 and 10 who are observed in the data one year after (recall that Oakland Unified does not reclassify students prior to 3rd grade). The second sample, which we term the "12th grade" sample, consists of 5,273 observations for EL students between grades 6 and 10 who are also observed in the data in 12th grade. We are currently unable to examine long-term outcomes like graduation and college enrollment for students reclassified prior to grade 6 due to the range of years spanned by the data. Figure 2.6 depicts the grades and years for which we observe reclassification and outcomes for both samples. Further details are how variables were constructed for these outcomes and RFEP duration is included in the Data Appendix.

As described in Section 2.3, our primary empirical strategy relies on students on the CST ELA frontier–that is, students who have met all testing criteria except potentially the CST ELA requirement. Because we are again narrowing our focus to a subset of all data, our sample sizes are reduced once more, and may reflect students who are different from other ELs. To explore whether these differences, we present descriptive statistics in Table 2.1 for students in both the one year out and 12th grade samples who are "on" and "off" the CST ELA frontier. There are 11,093 student-year observations in the one year out sample who are on the CST ELA frontier. These students are more likely to be female, Asian, and U.S. born and less likely to be Latino and in special education than students who are off the frontier. Because they have met all other reclassification criteria, students on the frontier are higher performing and much more likely to be reclassified (students off the frontier have a zero probability of reclassification). Students on the CST ELA frontier in the one year out sample spend an average 0.92 years as RFEPs and exhibit higher ELA and math test scores and GPA one year later than students off the frontier.

Turning to ELs in the grade 12 sample, the 1,438 student-year observations on the CST ELA frontier are more likely to be born in the U.S. and less likely to be in special education than students off the frontier. They also obviously score higher on reclassification criteria and have a much higher probability of being reclassified (zero versus 40 percent). These students spend an average 2.26 years reclassified, which reflects the greater length of time they are followed in the data. When we compare 12th grade outcomes for students on the CST ELA frontier to those off the frontier, we find that students on the frontier have higher ELA and math test scores and GPA, and are more likely to graduate, meet A-G requirements, take the SAT, and attend college. Again, it is not surprising that students who have met more criteria exhibit better outcomes. However, comparing students on the frontier across the two samples shows that the students in the grade 12 sample have similar demographics, though are less likely to be U.S. born, than those in the one year out sample. They also perform worse on the CST ELA used for reclassification and are less likely to be reclassified. Students who remain enrolled in the district in grade 12 are composed both of those who have not dropped out of high school and those who have not left the district for charter schools, private schools, or other districts. Students in the grade 12 sample are also older, as the earliest possible grade for a student to appear in this sample is 6th grade. As a result of these various dynamics, the descriptive statistics in Table 1 suggest that, on average, students whom we can follow through grade 12 are negatively selected compared to those whom we follow for just one year.

Table 2.2 further splits students in each of the CST ELA frontier samples by grade level (elementary, middle, and high). Because students who are not reclassified in earlier grades are necessarily those with test scores that fell below the reclassification cutoffs, we find that they become more negatively selected in later grades (consistent with Figure 2.3). Students not yet reclassified in later grades in the one year out sample are less likely to be female and U.S. born. They also have lower scores on the CST ELA used for reclassification, although they perform as well as earlier grades on the CELDT Overall test (note these students have all reached the CELDT Overall cutoff). Even though all students in this table are on the CST frontier, the probability of being reclassified falls in later grades in which additional reclassification requirements around ELA course grades are introduced. EL students in later grades also exhibit lower ELA and math test scores and lower GPA. The relationship between year of reclassification and outcomes is less clear in the grade 12 sample, which only contains middle and high school students. Students not yet reclassified in high school are less likely to be U.S. born but also have higher graduation and college enrollment rates. The descriptive statistics in Table 2.2 suggest that there are important composition effects introduced into the two samples by virtue of the grade levels they contain, with younger students performing better both in and following reclassification. Finally, note that as we split the sample further, sample sizes continue to fall, which will hinder our ability to estimate heterogeneous treatment effects across grades of reclassification despite the clear differences that exist between grade level groups.

2.5 Regression Discontinuity Results Around CST Cutoff

2.5.1 Short-Term Effects

We explore the short-term effects of an additional year of being reclassified for students on the CST ELA frontier on the one year out sample of students, which we further divide into elementary and secondary grades (3-5 and 6-10, respectively). Figure 2.11 graphs average CST ELA scores, CST Math scores, cumulative GPA, and attendance rates against the reclassification CST ELA score for these two groups net of fixed effects for grade, year, and school. No discrete jumps are noticeable around the cutoff score except for cumulative GPA for students reclassified in elementary grades. We find similar results when implementing the estimation strategy described in Section 2.3, the results of which are presented in Table 2.3 for grades 3-5 and Table 2.4 for grades 6-10. Focusing first on the students observed in earlier grades, we find consistently small and statistically insignificant results for CST ELA and Math scores and attendance rates one year out. However, we confirm the positive effect of reclassification on cumulative GPA the following year for these students, which consistently remains at around 0.2 grade points across specifications. Because only students in grades 6 and up receive GPAs, this effect only applies to students who are reclassified the year before entering middle school and suggests that despite no cognitive gains as measured by CST scores, these students receive better grades in their first middle school classes.

Turning to the estimates of short-term effects on students reclassified in grades 6-10 in Table 2.4, we again find limited evidence of any effects. Estimates are generally small in magnitude and statistically insignificant, although more consistently negative, for this sample of students. We do estimate a statistically significant negative effect on attendance rates, which is equivalent to students attending one fewer day of school in the academic year following reclassification (based on the maximum 180 days of enrollment). Because no effects are found for cognitive achievement, this attendance result does not appear to be affecting student learning. In results not shown, we also explore suspensions and on-time grade progression as short-term outcomes and find no effects from reclassification for either group of students.

While sample sizes decrease rapidly as we focus on students with valid outcomes on the CST ELA frontier and further divide them by grade of reclassification, effect sizes would have to be quite small (on the order of less than $\frac{1}{20}\sigma$ for CST ELA) for statistical power to fall below the typically minimum accepted level of 80 percent, based on a 0.05 significance level.[59] In summary, being reclassified appears to have limited impacts on students' short-term cognitive and non-cognitive outcomes. What effects we do find suggest that the timing of reclassification may indeed matter, though not necessarily through effects on student learning: ELs reclassified in elementary school have higher GPAs in middle school, while students reclassified in later grades are more likely to miss school as a result of being reclassified.

2.5.2 Long-Term Effects

One of the gaps in the previous literature studying the effects of reclassification has been the examination of long-term outcomes such as graduation and college enrollment. In this section, we estimate treatment effects at the end of high school on the sample of students observed through grade 12. Even though we do not find significant short-term effects, it may be possible that benefits of reclassification accrue and intensify over time. Further, even in the absence of cognitive effects, outcomes like graduation and college enrollment may be influenced by non-academic factors impacted by reclassification into mainstream education

[59] $Power(\delta) \simeq 1 - \Phi(1.96 - \delta\frac{\sqrt{N}}{\hat{\sigma}})$. We utilize the sample variance of the outcome measure and the sample size to estimate power for different effect sizes.

such as peer groups. Recall that EL students in this sample could have been reclassified at any point between grade 6 and grade 10 and spend an average of 2.26 years reclassified.

Figure 2.12 graphs average grade 11 CST ELA and CST Math scores, grade 12 cumulative GPA and attendance rates, as well as the probabilities of meeting A-G requirements, taking the SAT, graduating from high school, and enrolling in a four-year college, against students' reclassification CST ELA score. On the surface, the graphical analysis presented in Figure 2.12 provides little evidence of effects for students on the CST ELA frontier in this sample. The absence of statistically significant treatment effects is confirmed in the estimates, presented in Table 2.5. Most estimates are positive (with the exception of high school graduation), but their magnitude is generally small and they are not statistically significant. There is only one significant estimate among all grade 12 outcomes and specifications: there is a 3 percentage point increase in the probability of enrolling in a four-year college (significant at the 0.1 level) when using the rectangular kernel (but observations farther away from the cut-off may be impacting this estimate relative to the edge kernel estimates). In additional results not shown, we also explore potential effects on suspensions in grade 12, on-time grade progression to grade 12, on-time graduation, high school dropout, and enrollment in any college (including two-year and vocational) and find no significant effects.

Note that sample sizes in this analysis are significantly smaller. Not only are we able to track fewer students through the end of high school, but because they are older EL students, they are lower performing and thus we have even fewer observations for students on the CST ELA frontier around the cutoff. In the case of four-year college enrollment, we have an even smaller number of observations because enrollments that occurred after 2012 were not yet available in the data. In this case, we may be lacking sufficient statistical power. For instance, power calculations suggest that we have less than an 80 percent chance of detecting treatment effects on the probability of four-year college enrollment that are smaller than 5 percentage points at a 0.05 significance level using this sample. When taken together, the fact that all specifications yield positive estimates of a similar magnitude for the effects of reclassification on taking the SAT and enrolling in a four-year college (which generally require SAT scores for admission) may be suggestive of positive long-run effects operating through non-cognitive channels, but we are unable to draw definitive conclusions because of limited statistical power.

2.6 Optimal Reclassification Policy: Moving Beyond the Cutoff

2.6.1 Policy Implications of Null Effects

The results from the previous section indicate that there is no discontinuity in the relationship between past and future CST ELA scores for students just above the reclassification cutoff (the same holds true for most other outcomes we explore). If treatment effects are constant across all values of CST ELA reclassification scores, then null effects would imply that there are no gains of reclassification for any students. Alternatively, if treatment effects vary with reclassification scores, then it is possible that inframarginal reclassified students benefit from reclassification and the cutoff is appropriately set at the point where the performance of the marginal student would be the same in either setting, ensuring a smooth transition for these students. Null effects could also reflect insufficient statistical power. As Robinson (2011) explains, a null effect is consistent with the existence of a better alternative, but does not necessarily imply that a better alternative exists. Only by understanding how reclassification affects students away from the cutoff can we interpret the policy implications of a null result for students at the cutoff.

To illustrate alternative hypotheses, we consider various scenarios for the true relationship between the running variable (past CST ELA scores) and outcomes (future CST ELA scores) under reclassified and non-reclassified settings. Drawing heavily on Robinson (2011), Figure 2.7 depicts these relationships for a subset of possible cases, with the relationship between past and future CST ELA scores plotted as a solid line when reclassification occurs, and as a dashed line when reclassification does not occur. The top graph illustrates the scenario where these two lines intersect at the cutoff c. If one were to raise the cutoff above c, students who would have benefited from reclassification would fall below the new cutoff and experience lower future CST ELA scores in the absence of reclassification. Lowering the cutoff below c, on the other hand, would mean reclassifying students who would be better served with continued EL program support. Thus, an appropriate reclassification cutoff exists at point c. If this scenario holds, then our findings of null effects imply that the CST ELA cutoff is correctly set and no further action on the part of policymakers is necessary.

The second graph in Figure 2.7 presents the scenario where the marginal effect of reclassification is constant and positive, implying that all students should be reclassified and the CST ELA criterion should be eliminated. The third scenario describes the situation where the effect of reclassification is always negative (here shown as an increasing negative marginal effect), implying that no students should be reclassified. If either of these cases were true, we would find non-null effects for students at the margin. The fourth scenario, which is also consistent with a null result, describes a situation where reclassification has no effect on future CST ELA scores at all, as the gradient between past and future CST ELA scores is unaffected by the reclassification regime. This last scenario would suggest there are no differences between services provided to EL and RFEP students. Even if CST ELA scores improve between periods (which is not necessarily the case), this scenario should prompt examination of the relative quality and cost-effectiveness of EL services compared to mainstream education.

For the finding of null effects to offer meaningful policy guidance, we must distinguish between the first scenario where reclassification cutoffs are optimally set and the fourth scenario where reclassification has no effect for any students. More generally, even when RD designs yield positive (or negative) effects at the cutoff, scenario 2 (or 3) could apply, suggesting there would be benefits from significant changes to EL reclassification policies and services. To provide meaningful interpretation that can help policymakers decide when an appropriate cutoff exists or when reclassification is always or never preferred given the current programs in place, we must understand how reclassification affects students away from the cutoff.

2.6.2 Estimating Reclassification Effects Above the Cutoff

The RD analysis of Section 2.5 exploits variation in a single running variable (CST ELA) at the cutoff. The existence of multiple running variables, however, provides additional variation in treatment status for students who are above the CST ELA reclassification cutoff. Figure 2.15 plots the relationship between CST ELA and CELDT Overall reclassification scores for students in grades 3 through 5 who also met all other reclassification test score criteria (for each CELDT subtest). The two test scores exhibit a correlation of 0.50, but there is still considerable independent variation in the two test scores; the two tests offer noisy measurements of different sorts of ability (English language arts skills such as an understanding of written grammar, versus functional English proficiency), though such abilities are no doubt correlated in the underlying student population. The traditional RD analysis compares students to the right of the vertical axis that are just above and below the horizontal axis in Figure 2.15, but it ignores all of the information provided by students to the left of the vertical axis (i.e. students who are off of the CST ELA frontier due to low CELDT Overall scores). These students may provide additional information on the relationship between reclassification test scores and outcomes. For simplicity and sample size limitations, the remainder of this section examines short-run English proficiency (CST ELA scores one year out) for EL students in grades 3 through 5.

In order to utilize this additional information and identify the effect of reclassification for students above the CST ELA cutoff, an additional assumption about the counterfactual for treated students above the CST ELA cutoff is required. We make the assumption that outcomes in the absence of the treated are additively separable in the first two running variables, henceforth denoted as R^1 (CST ELA) and R^2 (CELDT Overall) respectively:

$$E\left[Y_0 \mid R^1, R^2\right] = m_1\left(R^1\right) + m_2\left(R^2\right) \quad (4)$$

where additional covariates have been omitted for ease of exposition. This assumption implies that students' CELDT Overall scores do not affect the relationship between their past and future CST ELA scores. While the appropriateness of such an assumption will generally depend on the particular setting, the appeal of this simplifying assumption in a multiple RD setting such as this is that it can be tested in the data for untreated students (i.e. students who are not in the northeast quadrant of Figure 2.15). We test whether this assumption holds by testing the statistical significance of the interaction between R^1 and R^2 in following specification estimated over all untreated students who passed the other four reclassification tests (CELDT subtests):

$$y_i = m_1 R^1 + m_2 R^2 + m_3 R^1 R^2 + \phi \mathbf{W}_i + \theta \mathbf{X}_i + \sum_s s_i + \sum_g g_i + \sum_t t_i + \varepsilon_i \quad (5)$$

where $\mathbf{W}_i$ is a vector of other reclassification test scores (CELDT subtests), and the other terms control for student background and fixed effects as before. Unsurprisingly, after conditioning on additional pre-treatment information such as CELDT subtest scores, R^2 has no independent explanatory power; the coefficient estimates for m_2 and m_3 are not statistically significant (with t-values of 0.38 and 1.29, respectively), and the specification explains the same amount of variation in future CST ELA scores (R-square = 0.393) as a reduced specification excluding the R^2 terms.

Under this simple separability assumption, we can then estimate the conditional expectation of future English language arts scores conditional on R^1 and R^2 for students who remain untreated because they are below either cutoff (or both), and then use these estimates to predict outcomes for reclassified students (who are above both cutoffs) in the absence of reclassification. This extrapolation is justified by the data, which indicates that the relationship between CST ELA reclassification scores and outcomes is not dependent on CELDT

Overall reclassification scores, after conditioning on other pre-treatment information. We obtain estimates of m_1 and m_2 from a specification that is identical to equation 5 save for the omission of the interaction term between R^1 and R^2. These estimates are then used to predict outcomes for reclassified students in the absence of reclassification. Specifically, for any value of $R^1 = \widetilde{r^1}$ and $R^2 = \widetilde{r^2}$ above their respective cutoffs, we predict outcomes for students who have met all other reclassification test score criteria based on the following equation:

$$\hat{E}\left[Y_0 \mid R^1 = \widetilde{r^1}, R^2 = \widetilde{r^2}\right] = \hat{m}_1\left(R^1\right) + \hat{m}_2\left(R^2\right) \tag{6}$$

where additional covariates have again been omitted for ease of exposition.

The intent-to-treat effect (ITT) can then estimated for any value of $R^1 = \widetilde{r^1}$ and $R^2 = \widetilde{r^2}$ above their respective cutoffs by comparing the observed value of the outcome to the prediction:

$$\begin{aligned} I\hat{T}T &= \hat{E}\left[Y_1 - Y_0 \mid R^1 = \widetilde{r^1}, R^2 = \widetilde{r^2}\right] \qquad (7) \\ &= E\left[Y^{obs} \mid R^1 = \widetilde{r^1}, R^2 = \widetilde{r^2}\right] - \left(\hat{m}_1\left(\widetilde{r^1}\right) + \hat{m}_2\left(\widetilde{r^2}\right)\right) \end{aligned}$$

The above estimation framework relies on estimation of the conditional expectation for students who are in any of the three untreated quadrants of Figure 2.15. An alternative approach to estimation is a difference-in-difference (DID) style estimator, where the relevant differences are between students above and below the first cutoff, and above and below the second cutoff. Compared to the previous approach of equation 7, the DID approach requires an estimate of the difference in outcomes between two students who are above and below the CELDT Overall cutoff but have the same CST ELA score below the cutoff (students to the left of the vertical axis in Figure 2.15). This difference is then subtracted from an estimate of the difference in outcomes between two students who are above and below the CELDT Overall cutoff but have the same CST ELA score above the cutoff. Under the same separability assumption, the DID estimator provides an unbiased estimate of the effect of reclassification for any value of R^1 above the cutoff. Formally, for any value of $R^1 = \widetilde{r}^1$and $R^2 = \widetilde{r}^2$ above their respective cutoffs, and for any value of $R^1 = \widetilde{\widetilde{r}}^1$ and $R^2 = \widetilde{\widetilde{r}}^2$ below their respective cutoffs, an unbiased estimate of the ITT can be obtained from the following equation:

$$\begin{aligned} I\hat{T}T &= \hat{E}\left[Y_1 \mid R^1 = \widetilde{r}^1, R^2 = \widetilde{r}^2\right] - \hat{E}\left[Y_0 \mid R^1 = \widetilde{r}^1, R^2 = \widetilde{\widetilde{r}}^2\right] \\ &= \hat{E}\left[Y_0 \mid R^1 = \widetilde{\widetilde{r}}^1, R^2 = \widetilde{r}^2\right] - \hat{E}\left[Y_0 \mid R^1 = \widetilde{\widetilde{r}}^1, R^2 = \widetilde{\widetilde{r}}^2\right] \end{aligned} \tag{8}$$

2.6.3 Results Away from the CST ELA Cutoff

Estimates of the ITT effect on CST ELA scores one year out (from equation 7) are reported graphically in Figure 2.16. After controlling for other reclassification test scores, student background and fixed effects, the average ITT effect for students above the CST ELA cutoff (and above all other cutoffs) is 0.149σ. The graph confirms null effects at the CST ELA cutoff, but the treatment effect increases linearly with CST ELA reclassification scores, reaching a gain of roughly 0.3σ for students at 2σ above the CST ELA cutoff.[60] Note that below the CST ELA cutoff, the predicted relationship between past and future CST ELA scores is nearly identical to the observed relationship. Because the probability of reclassification among students above the CST ELA cutoff and on the frontier jumps from 4.1 percent to 85.8 percent relative to students off the frontier, the ITT estimate corresponds to an average reclassification effect on all reclassified students of 0.182σ.

The same pattern of results is obtained using the DID approach of equation 8, plotted in Figure 2.17. The DID approach yields an average ITT effect for students above the cutoff of 0.175σ, significant beyond the 0.01 level. Figure 2.17 also includes a plot of first stage reclassification outcomes against the running variable for both groups of students (students

[60]Inference on the ITT estimate of equation 7 should proceed by relying on a bootstrapping procedure.

who are above and below the CELDT Overall cutoff). The first stage plots confirm that only students who are above the CELDT Overall cutoff (and thus on the CST ELA frontier) exhibit a sharp jump in reclassification probability at the CST ELA cutoff.

For elementary school EL students, the estimated relationship between reclassification scores and future scores for our treatment and control groups thus resembles the scenario depicted in the first panel of Figure 2.7, in which the cutoff is appropriately set. We cannot, however, draw any conclusions about the effects of reclassification for ELs below the cutoff. Lowering the CST ELA cutoff should only be considered in the unlikely alternative scenario where there is a kink in the relationship between reclassification scores and future scores at the cutoff, such that students with reclassification scores below the cutoff benefit from reclassification (as do students above the cutoff) even though students located at the cutoff do not benefit.

2.7 Conclusion

The introduction of new Common Core-aligned tests in California, which will replace the CSTs, means every school district in the state will need to redesign its reclassification criteria in the coming years. Without rigorous evidence on the effects of reclassification for students across the distribution of English proficiency, however, it is difficult for policymakers to make informed decisions about the appropriateness of existing criteria or the relative effectiveness of EL services compared to mainstream education. We address some of the gaps in the empirical literature on reclassification by exploiting exogenous variation in the probability of reclassification introduced by the various criteria that students must meet to be eligible for reclassification.

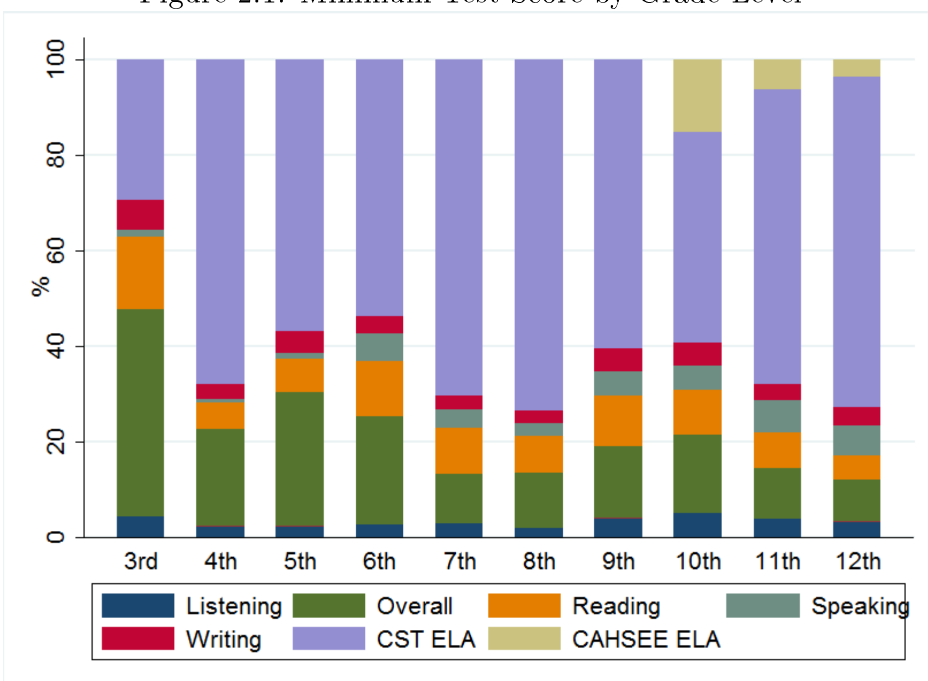
Our RD estimates suggest that reclassification has very limited effects on students at the margin. We explore a number of cognitive and non-cognitive outcomes, but find few statistically significant effects. What effects we do find suggest that the timing of reclassification may indeed matter, though not necessarily through effects on student learning. Future work should explore whether timing of reclassification matters for students above and below the cutoff as well, as it would inform whether criteria should differ for younger and older students. We also explore long-term effects, which could be impacted by non-academic factors that change with reclassification, such as exposure to different peer groups, even in the absence of short-term effects. We examine a number of outcomes, including CST ELA and Math scores in grade 11, high school graduation (ever and on-time), and four-year college enrollment, and find limited evidence of long-term effects for the students in our sample, who could have been reclassified at any point between grades 6 and 10. There is some suggestive evidence that reclassification impacts the probability of taking the SAT and enrolling in a four-year college (which generally requires SAT scores for admission), but we are unable to draw definitive conclusions due to limited statistical power. As additional years of data become available, future research will revisit long-term outcomes for students reclassified in both early and later grades.

In order to interpret the RD findings of predominantly null short-run reclassification effects, we also exploit variation in reclassification exposure across multiple criteria. To do so, we present an extension from the conventional RD design that exploits the fact that some students who meet the first cutoff will remain untreated due to being below the cutoff for a second running variable. Under a straightforward identifcation assumption that is supported in the data, we estimate the average treatment effect for reclassified EL students in elementary school to be a 0.182σ increase in CST ELA scores one year out. Pope (forthcoming) is the only other paper to find a non-null result for ELs in elementary grades, with an estimated treatment effect of 0.25σ on future CST ELA scores for marginal students in grades 2-4 around the CELDT Overall cutoff. On average, our estimates reflect a relatively higher performing group of students than Pope's estimates at the cutoff, which may in part explain why our estimate is somewhat lower in magnitude.

Our results imply that the CST ELA cutoff should not be raised for students in grades 3 through 5, as benefits accrue to reclassified students scoring above the current cutoffs. It remains an open question, however, whether students further below the CST ELA cutoff benefit from not being reclassified and continuing to receive EL support. One opportunity for estimating reclassification effects for students below the cutoff may arise from the introduction of the Common Core, which will soon provide a new dimension of variation for students who would have been reclassified under one reclassification regime but not the other. In future work we will also utilize the above-the-cutoff estimation framework for older students above reclassification cutoffs in order to draw conclusions about the effectiveness of EL policy for LTELs. This requires sufficient independent variation in CST ELA and CELDT Overall reclassification scores among our smaller sample of middle and high school students.

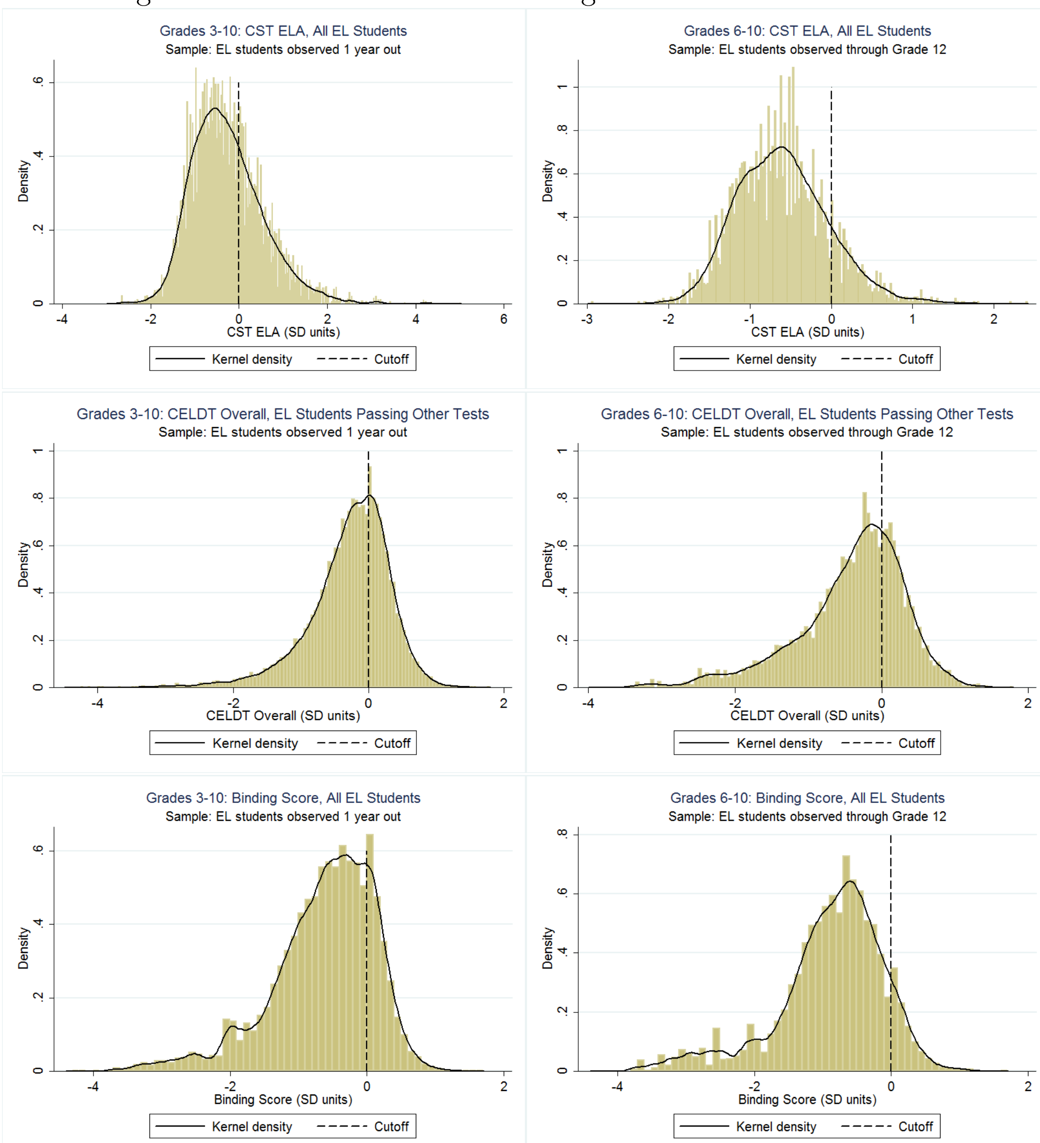
Even in the presence of positive reclassification effects for young students above the cutoff, however, reclassifying these students may not yield a net positive benefit for the entire school district. On the one hand, reclassified students exhibit positive learning gains in ELA, which have been linked to higher future wages (e.g. Cawley et al., 2001; Bishop, 1989; Tainer, 1988; Trejo, 1997). On the other hand, reclassification could impart a negative externality on other students by lowering the average ability in the EL setting from which they are removed and potentially also lowering the average ability in the mainstream setting they are placed into (see Sacerdote, 2011 for a review of the literature on peer effects in student outcomes). These are ideas for future projects. Finally, note that there are financial disincentives to reclassifying students, which can impact districts' budgets. Average per student expenditures in California are about 6 to 9 percent higher for ELs than mainstream students (Alth and Jepsen, 2005), yet under the state's new school finance law, districts would lose at least 20 percent of per pupil funding for each student they reclassify (Taylor, 2013). Based on current expenditure and funding levels, reclassification would result in a minimum net loss of about $750 per reclassified student each academic year.

Figure 2.1: Minimum Test Score by Grade Level



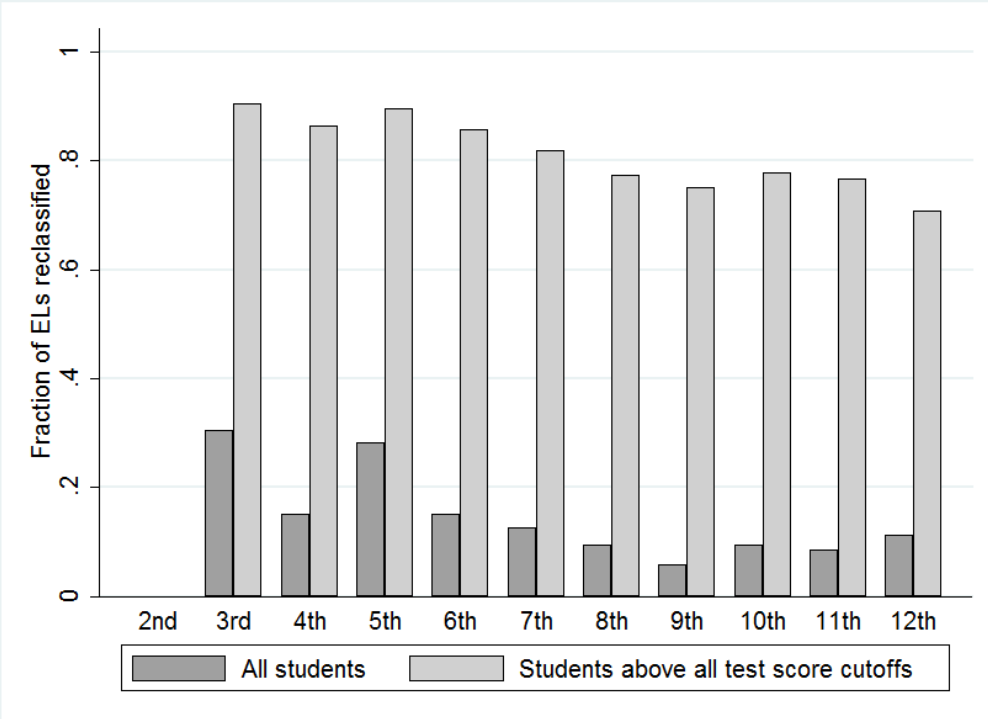
Notes: Statistics are calculated over all EL students with test scores for all required reclassification tests. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff.

Figure 2.2: Distribution of Running Variables around Cutoffs



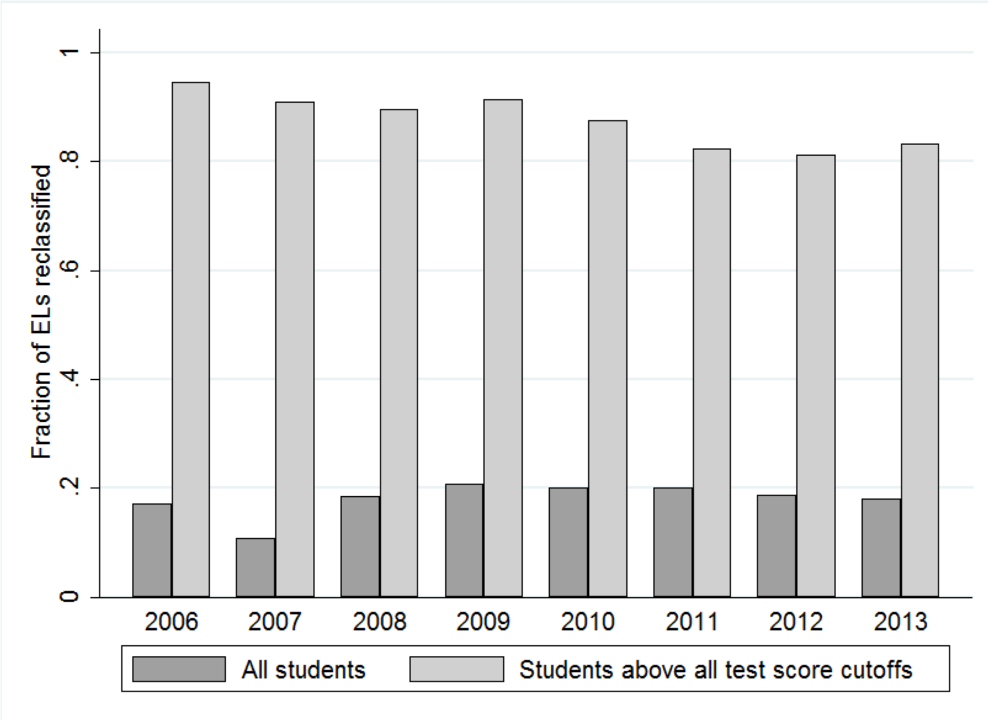
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff.

Figure 2.3: Reclassification Rates by Grade



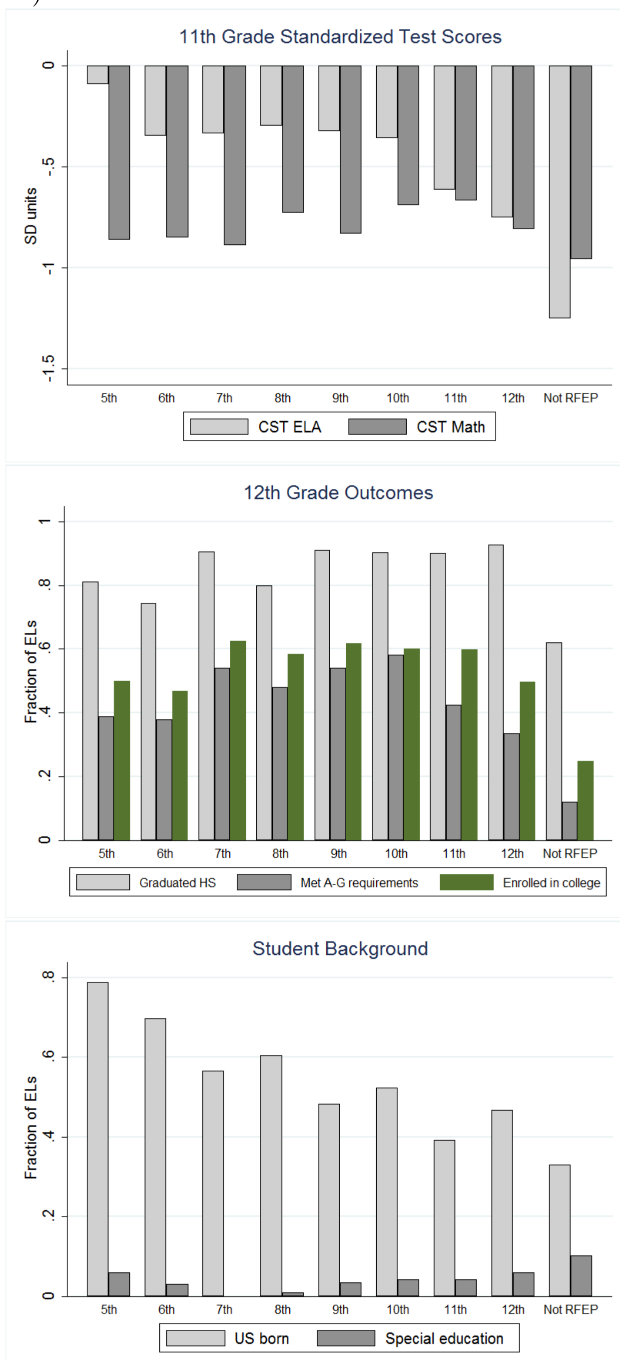
Notes: Statistics are calculated over all EL students with test scores for all required reclassification tests.

Figure 2.4: Reclassification Rates by Year



Notes: Statistics are calculated over all EL students with test scores for all required reclassification tests.

Figure 2.5: Student Background and Outcomes by Grade of Reclassification (Students Observed through Grade 12)



Notes: Statistics are calculated over all EL students with test scores for all required reclassification tests and observed in the data in Grade 12.

Figure 2.6: Sample Selection

Sample: EL Students Observed 1-Year Out

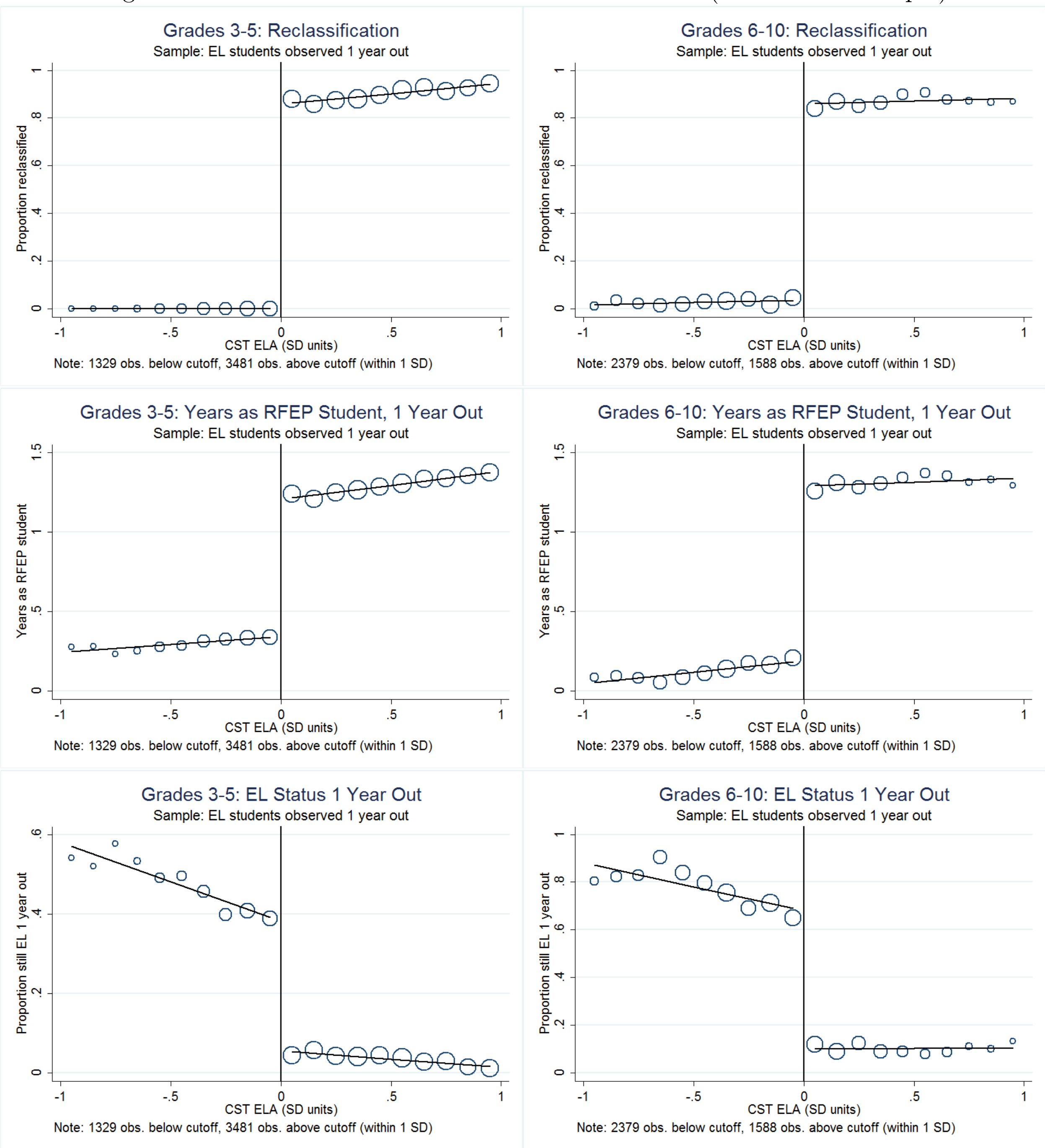
2006	2007	2008	2009	2010	2011	2012	2013
			K	1	2	3	4
		K	1	2	3	4	5
	K	1	2	3	4	5	6
K	1	2	3	4	5	6	7
1	2	3	4	5	6	7	8
2	3	4	5	6	7	8	9
3	4	5	6	7	8	9	10
4	5	6	7	8	9	10	11
5	6	7	8	9	10	11	12
6	7	8	9	10	11	12	
7	8	9	10	11	12		
8	9	10	11	12			
9	10	11	12				
10	11	12					
11	12						
12							

Sample: EL Students Observed through Grade 12

2006	2007	2008	2009	2010	2011	2012	2013
K	1	2	3	4	5	6	7
1	2	3	4	5	6	7	8
2	3	4	5	6	7	8	9
3	4	5	6	7	8	9	10
4	5	6	7	8	9	10	11
5	6	7	8	9	10	11	12
6	7	8	9	10	11	12	
7	8	9	10	11	12		
8	9	10	11	12			
9	10	11	12				
10	11	12					
11	12						
12							

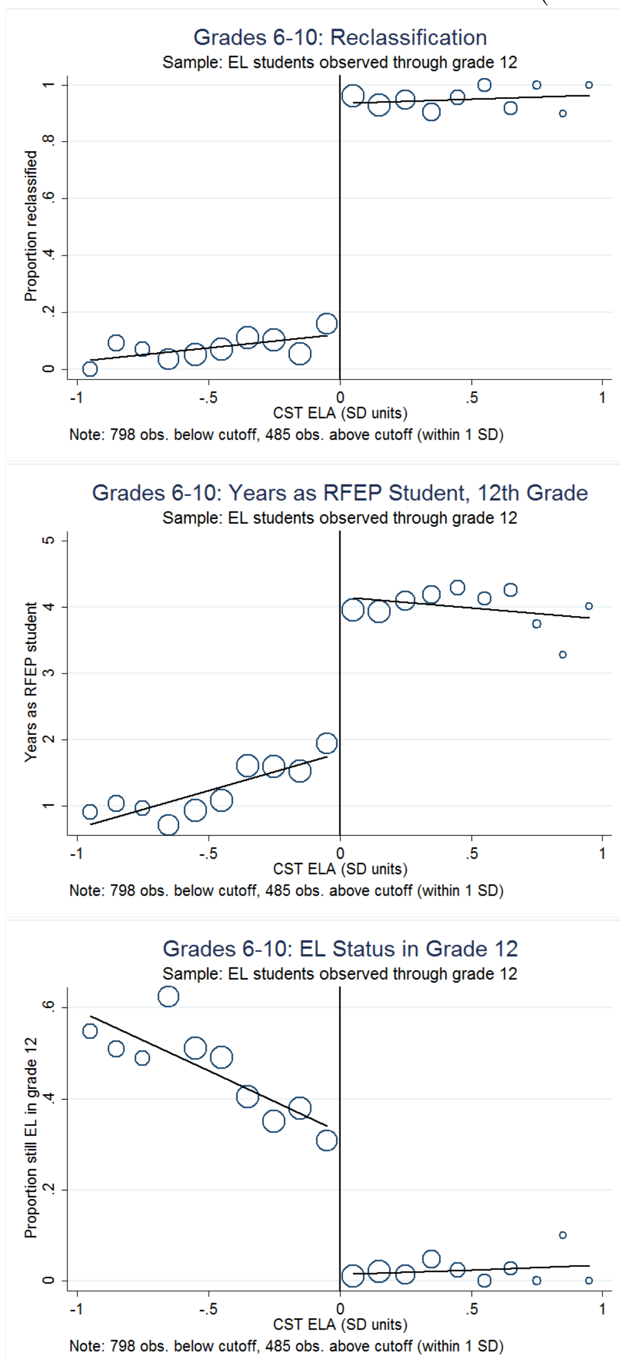
Notes: Cells highlighted in dark gret indicate yearly observations included in each sample. Cells highlighted in light grey indicate additional years when student outcomes are measured.

Figure 2.7: Reclassification around CST ELA Cutoff (1-Year Out Sample)



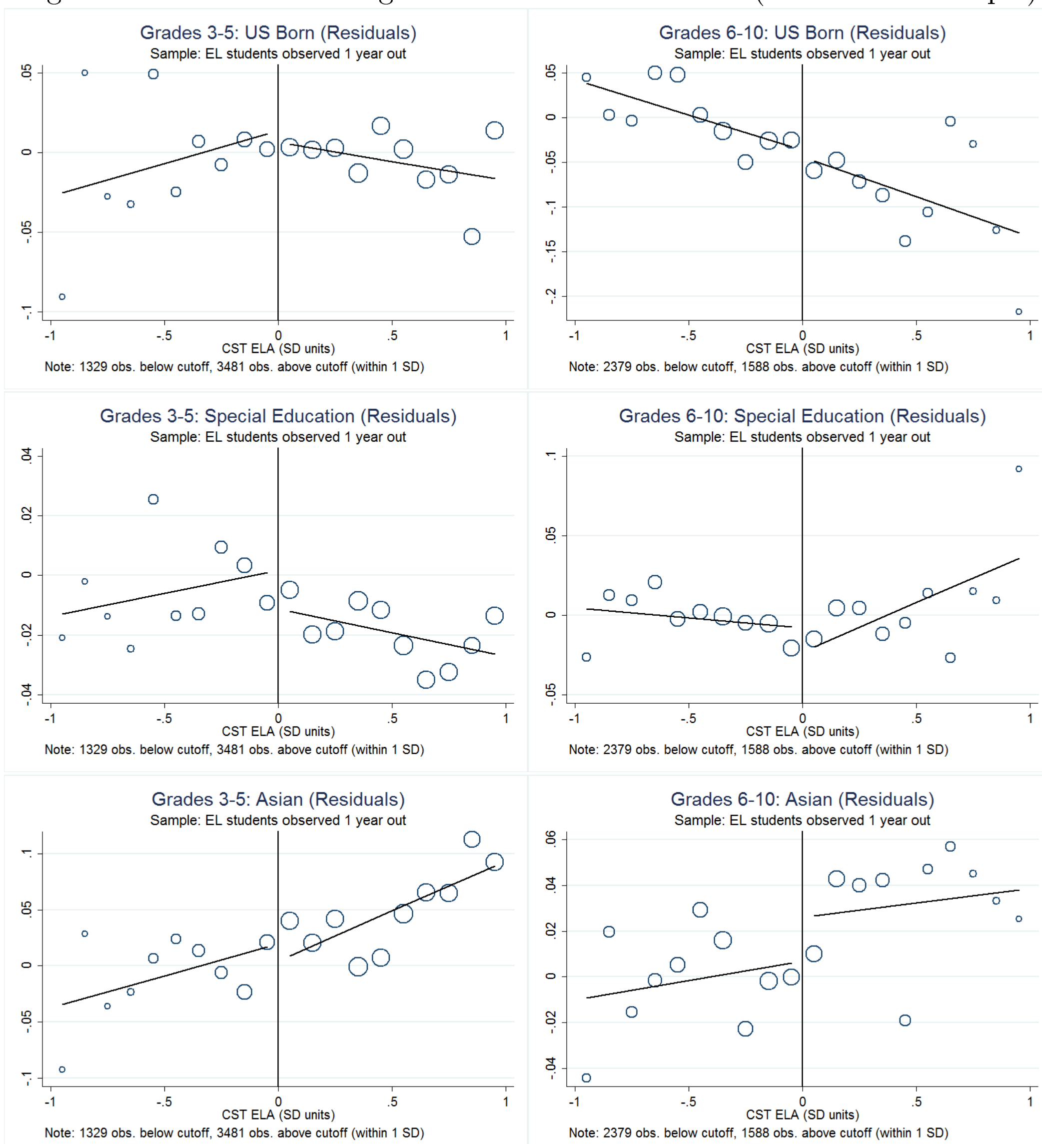
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff.

Figure 2.8: Reclassification around CST ELA Cutoff (12th Grade Sample)



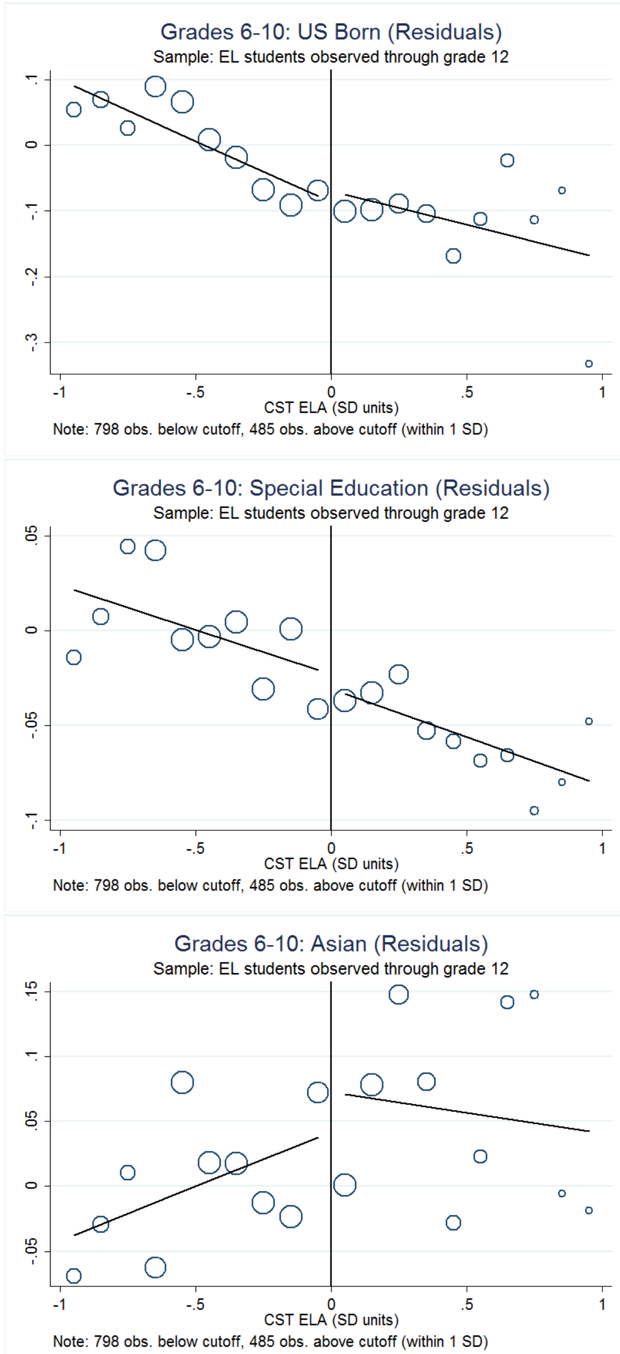
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff.

Figure 2.9: Student Sorting around CST ELA Cutoff (1-Year Out Sample)



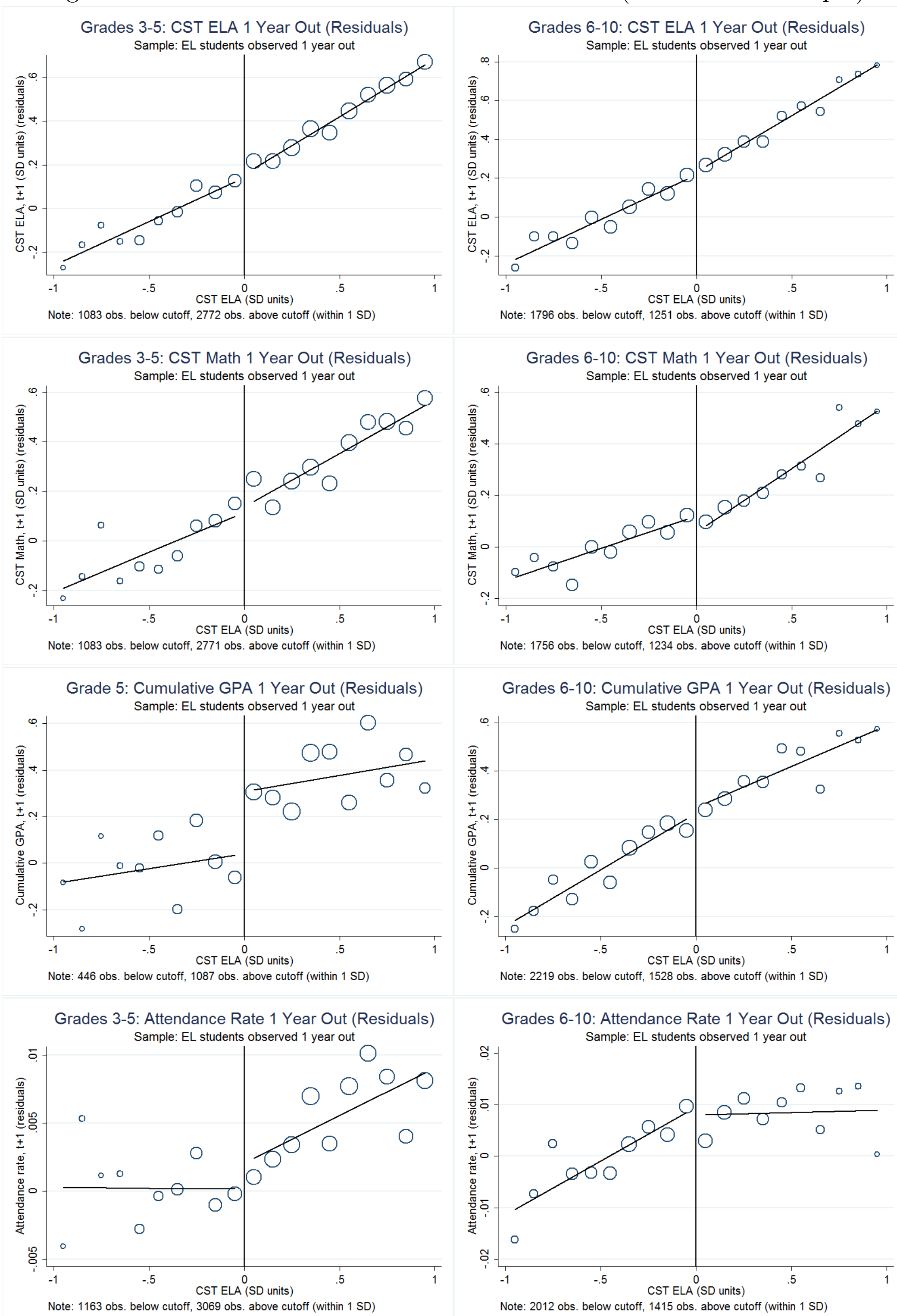
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Residuals are computed using the coefficients from a regression of the outcome variable on a set of fixed effects for grade, year, and school for students below the cutoff in the sample only.

Figure 2.10: Student Sorting around CST ELA Cutoff (12th Grade Sample)



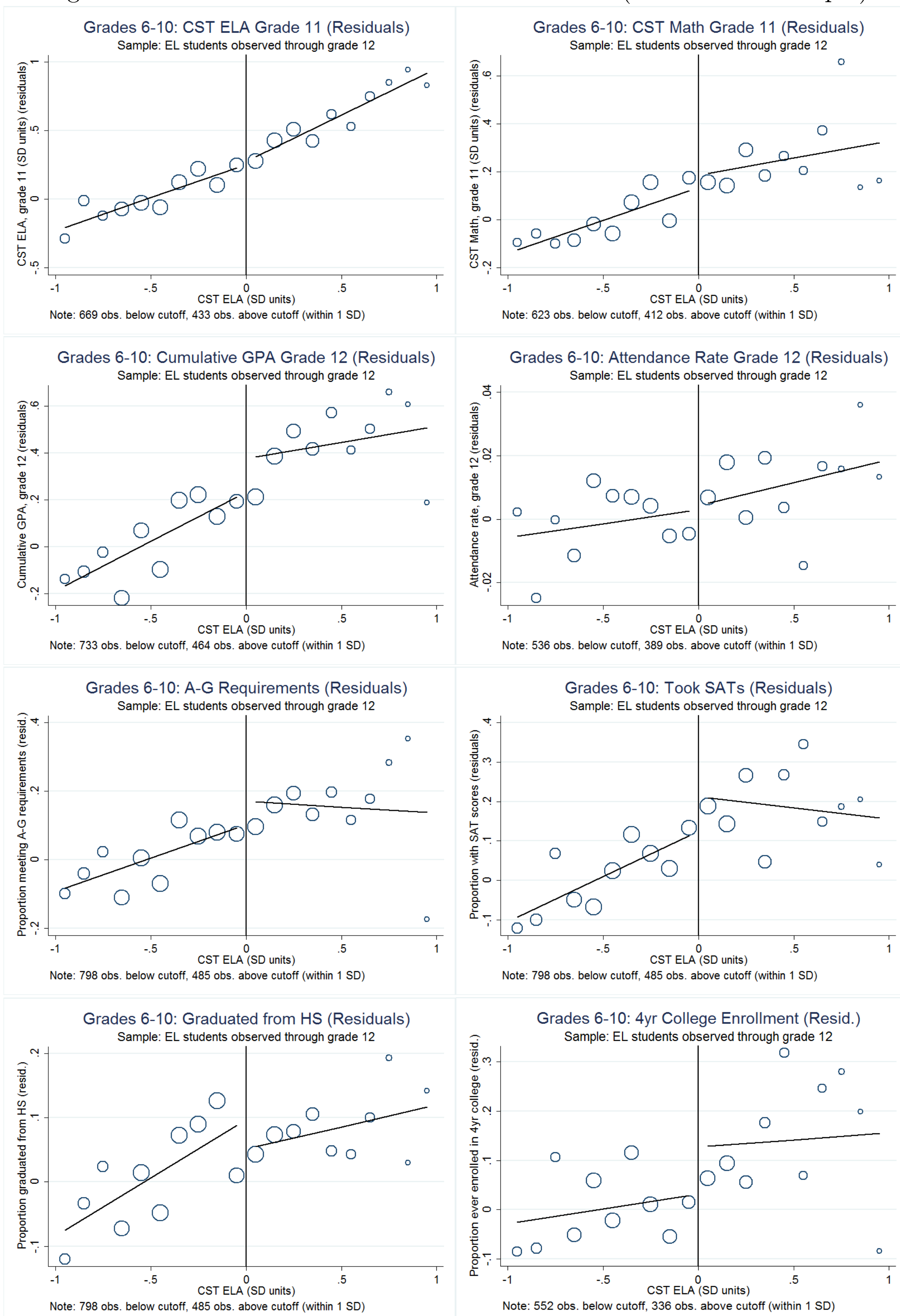
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Residuals are computed using the coefficients from a regression of the outcome variable on a set of fixed effects for grade, year, and school for students below the cutoff in the sample only.

Figure 2.11: Outcomes around CST ELA Cutoff (1-Year Out Sample)



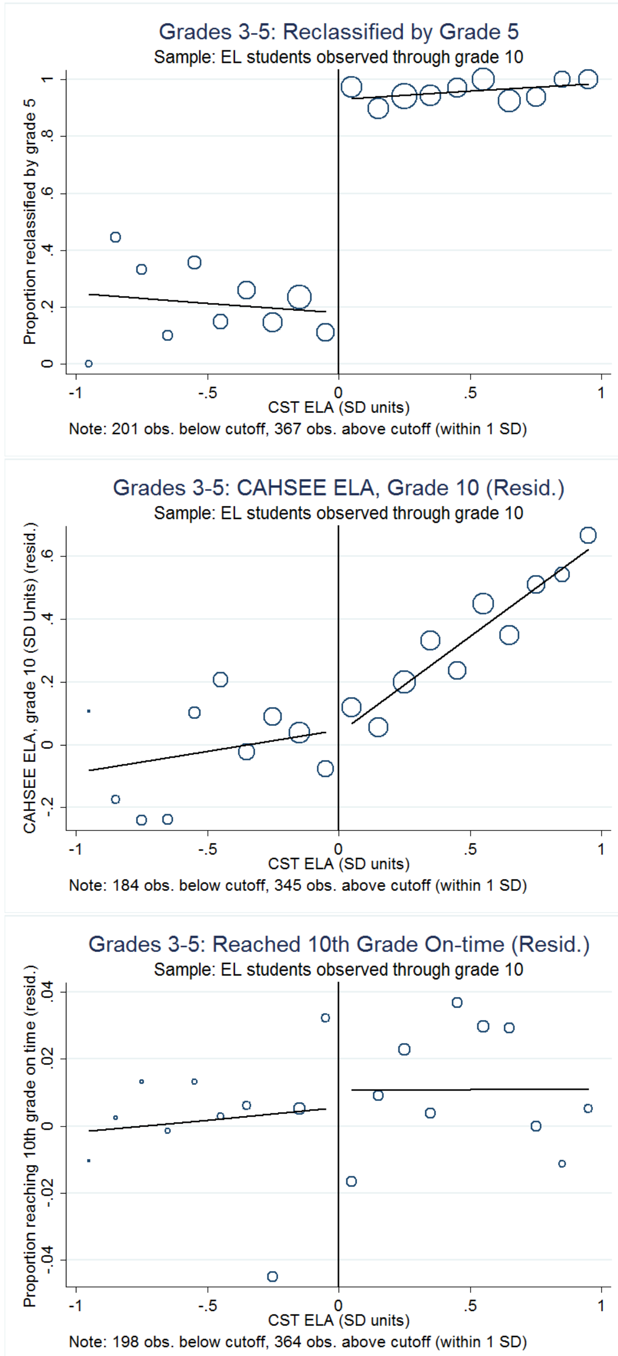
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Residuals are computed using the coefficients from a regression of the outcome variable on a set of fixed effects for grade, year, and school for students below the cutoff in the sample only.

Figure 2.12: Outcomes around CST ELA Cutoff (12th Grade Sample)



Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Residuals are computed using the coefficients from a regression of the outcome variable on a set of fixed effects for grade, year, and school for students below the cutoff in the sample only.

Figure 2.13: Outcomes around CST ELA Cutoff (10th Grade Sample)



Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Residuals are computed using the coefficients from a regression of the outcome variable on a standardized CELDT Overall reclassification test scores, and fixed effects for grade, year, and school, for students below the cutoff in the sample only. The group of EL student on the CST ELA Frontier is the group of students passing all other reclassification tests, as in the preceding analysis. The group of EL students off of the CST ELA Frontier is those students passing all other reclassification tests except for CELDT Overall.

Figure 2.14: Potential Scenarios for Shifting Reclassification Cutoffs

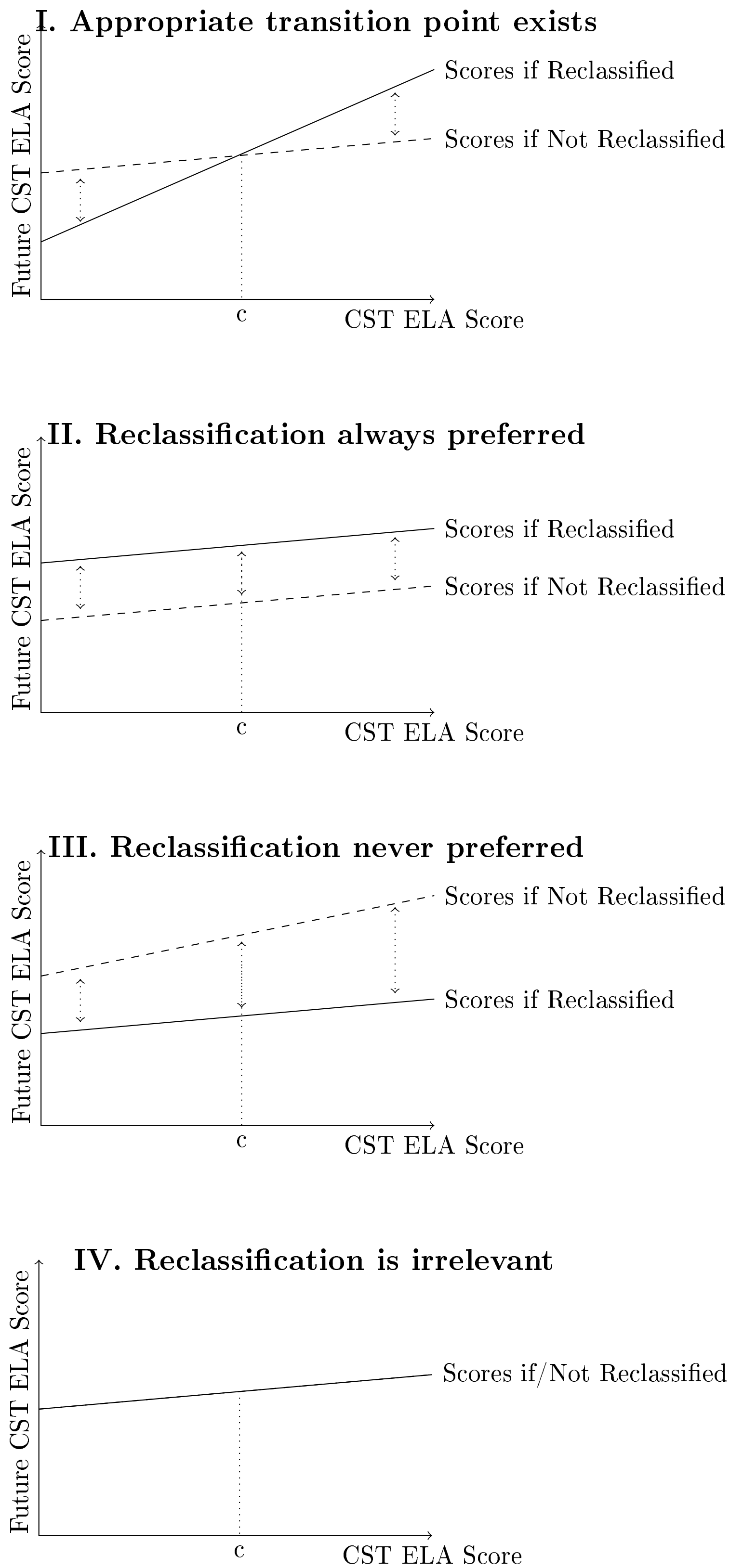
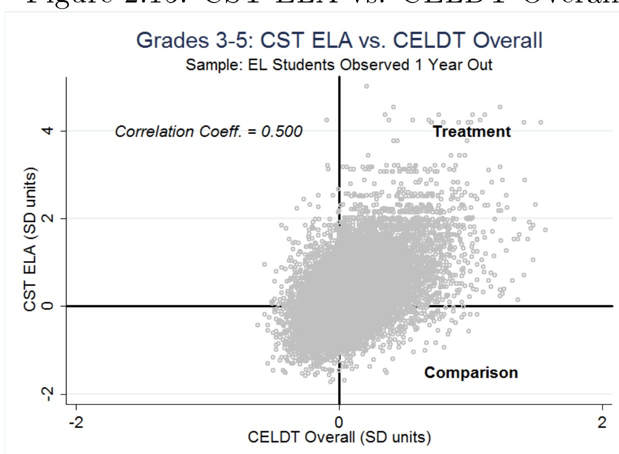
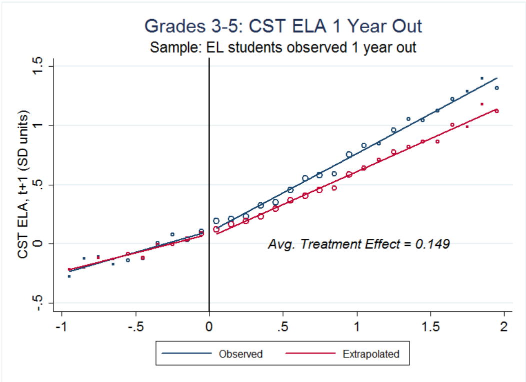


Figure 2.15: CST ELA vs. CELDT Overall



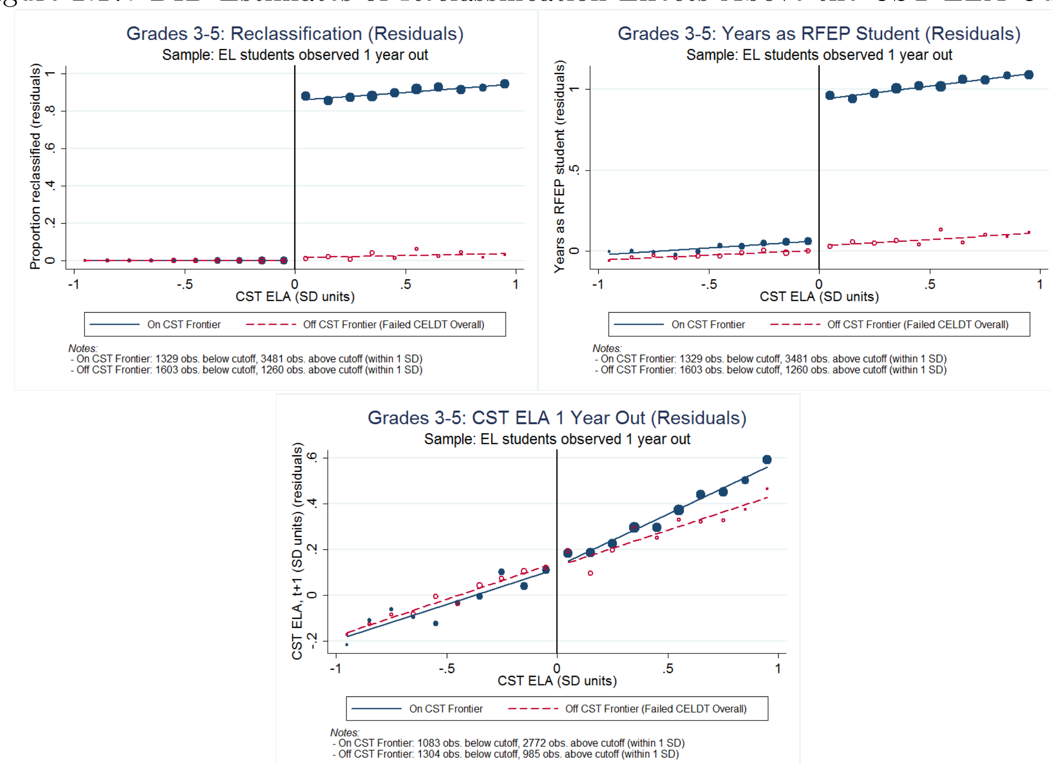
Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff.

Figure 2.16: Estimates of Reclassification Effects Above the CST ELA Cutoff



Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Extrapolated outcomes are obtained based on estimates of equation 6 as described in the text.

Figure 2.17: DID Estimates of Reclassification Effects Above the CST ELA Cutoff



Notes: Test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Residuals are computed based on the coefficients from a regression of the outcome variable on all reclassification test scores (other than CST ELA) and fixed effects, for students below the cutoff in the sample only.

Table 2.1: Summary Statistics on and off the CST ELA Frontier

EL Sample	Grades 3-10, Observed 1 Year Out		Grades 6-10, Observed through Grade 12	
	On Frontier	Off Frontier	On Frontier	Off Frontier
Student characteristics:				
Female	.48 (.50)	.44 (.50)	.45 (.50)	.47 (.50)
Asian	.25 (.43)	.15 (.36)	.25 (.43)	.24 (.43)
Latino	.68 (.47)	.78 (.41)	.68 (.46)	.69 (.46)
US born	.74 (.44)	.65 (.48)	.61 (.49)	.44 (.50)
Special education	.03 (.18)	.14 (.34)	.04 (.21)	.13 (.33)
Reclassification information:				
CST ELA	.33 (.79)	-.55 (.65)	-.19 (.56)	-.76 (.48)
CELDT Overall	.29 (.24)	-.55 (.52)	.33 (.26)	-.68 (.64)
Reclassified	.59 (.49)	.00 (.06)	.40 (.49)	.00 (.04)
Years as RFEP	.92 (.65)	.03 (.13)	2.26 (2.05)	.32 (.88)
Still EL	.27 (.44)	.90 (.30)	.30 (.46)	.78 (.41)
Outcomes, 1-year out:			Outcomes at end of HS:	
CST ELA	.20 (.81)	-.55 (.72)	-.59 (.66)	-1.09 (.61)
CST Math	.24 (1.00)	-.34 (.79)	-.91 (.56)	-1.03 (.51)
GPA (cumulative)	2.58 (.98)	2.27 (.96)	2.58 (.81)	2.41 (.77)
Attendance rate	.97 (.05)	.96 (.06)	.95 (.07)	.95 (.08)
Graduated HS			.79 (.41)	.64 (.48)
Met A-G req.			.41 (.49)	.25 (.44)
Took SAT			.39 (.49)	.24 (.43)
4-yr college enrollment			.17 (.38)	.08 (.27)
n	11,093	22,645	1,438	3,696

Notes: RFEP = Reclassified fluent English proficient. The CST Frontier indicates whether a student has passed all required reclassification tests other than the California Standards Test (CST) English Language Arts (ELA) component. Means for EL (English Language Learner) students with test scores for all required reclassification tests are reported for each variable, along with standard deviations in parentheses. Reclassification test scores are the most recent scores available. A-G requirements is an indicator meeting the minimum course requirements for admission into a 4-year public college in California. Further details on outcomes measured at the end of high school are provided in the Data Appendix. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Sample sizes reflect the number of students in the data without missing reclassification test scores, but some students are missing information for certain outcomes.

Table 2.2: Summary Statistics by Grade Level

EL Sample on CST Frontier	Grades 3-10, Observed 1 Year Out			Grades 6-10, Observed through Grade 12	
	Grades 3-5	Grades 6-8	Grades 9-10	Grades 6-8	Grades 9-10
Student characteristics:					
Female	.50 (.50)	.48(.50)	.44 (.50)	.45 (.50)	.45 (.50)
Asian	.30 (.46)	.14(.35)	.23 (.42)	.24 (.42)	.26 (.44)
Latino	.62 (.49)	.79(.40)	.70 (.46)	.70 (.46)	.67 (.47)
US born	.80 (.40)	.67(.47)	.59 (.49)	.65 (.48)	.58 (.49)
Special education	.02 (.15)	.05(.21)	.05 (.22)	.04 (.20)	.05 (.21)
Reclassification information:					
CST ELA	.63 (.78)	-.08(.55)	-.24 (.56)	-.16 (.54)	-.21 (.57)
CELDT Overall	.27 (.23)	.33(.25)	.30 (.25)	.35 (.26)	.31 (.26)
Reclassified	.73 (.44)	.37(.48)	.36 (.48)	.37 (.48)	.44 (.50)
Years as RFEP	1.13 (.53)	.61(.69)	.56 (.69)	2.91 (2.39)	1.58 (1.31)
Still EL	.11 (.32)	.51(.50)	.50 (.50)	.32 (.47)	.29 (.45)
Outcomes, 1-year out:				Outcomes at end of HS:	
CST ELA	.54 (.73)	-.28(.58)	-.59 (.63)	-.59 (.65)	-.60 (.66)
CST Math	.67 (.93)	-.36(.63)	-.85 (.58)	-.93 (.55)	-.89 (.58)
GPA (cumulative)	2.79 (1.00)	2.55(.95)	2.31 (.96)	2.54 (.85)	2.62 (.78)
Attendance rate	.97 (.03)	.96(.06)	.95 (.07)	.95 (.08)	.95 (.06)
Graduated HS				.76 (.43)	.83 (.38)
Met A-G req.				.41 (.49)	.41 (.49)
Took SAT				.37 (.48)	.42 (.49)
4-yr college enrollment				.18 (.39)	.17 (.37)
n	6,723	3,255	1,115	733	705

Notes: RFEP = Reclassified fluent English proficient. The CST Frontier indicates whether a student has passed all required reclassification tests other than the California Standards Test (CST) English Language Arts (ELA) component. Means for EL (English Language Learner) students with test scores for all required reclassification tests are reported for each variable, along with standard deviations in parentheses. Reclassification test scores are the most recent scores available. A-G requirements is an indicator meeting the minimum course requirements for admission into a 4-year public college in California. Further details on outcomes measured at the end of high school are provided in the Data Appendix. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Sample sizes reflect the number of students in the data without missing reclassification test scores, but some students are missing information for certain outcomes.

Table 2.3: RD Estimates of Effect of an Additional Year of RFEP Status (1 Year Out Sample, Grades 3-5)

	Optimal Bandwith (h^*)		Linear (Edge)	Linear (Rect.)	Quadratic (Edge)	Linear, Alt. Bandwiths $h^* + .25$sd	$h^* - .25$sd
Outcomes (1 yr out)			(1)	(2)	(3)	(4)	(5)
CST ELA	1.16	coeff.	.000	.009	.012	.003	-.001
		se	(.034)	(.032)	(.049)	(.032)	(.037)
		n	4229	4229	4229	4614	3630
CST Math	0.94	coeff.	-.040	-.009	-.080	-.028	-.052
		se	(.054)	(.05)	(.079)	(.049)	(.061)
		n	3725	3725	3725	4268	2952
Cumulative GPA	0.91	coeff.	.221**	.209 **	.266*	.218**	.231*
(5th graders only)		se	(.105)	(.095)	(.154)	(.096)	(.119)
		n	1481	1481	1481	1622	1226
Attendance rate	1.40	coeff.	.002	.002	.000	.002	.002
		se	(.003)	(.002)	(.004)	(.003)	(.003)
		n	5028	5028	5028	5342	4620

Source: Authors' calculations using OUSD data.
Notes: ***Indicates significance at the 1% level, **5%, and *10%.
Robust standard errors clustered at the individual level are reported in parentheses. All estimates are from the two-stage least squares estimates of equation 3, with linear controls for the running variable (recentered, standardized CST ELA scores) above and below the cutoff (except column 3 which uses quadratic control functions). All specifications include the baselines set of covariates and fixed effects for grade, year, and school. Observations are weighted using an edge kernel for all columns except column 2, which uses a rectangular kernel equivalent to unweighted OLS. The optimal bandwith is computed using Imbens and Kalyanaraman (2009) "plug-in" procedure. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. CST ELA test scores used in the first stage to predict reclassification are the most recent scores available.

Table 2.4: RD Estimates of Effect of an Additional Year of RFEP Status (1 Year Out Sample, Grades 6-10)

	Optimal Bandwith (h^*)		Linear (Edge)	Linear (Rect.)	Quadratic (Edge)	Linear, Alt. Bandwiths	
						$h^* + .25$sd	$h^* - .25$sd
Outcomes (1 yr out)			(1)	(2)	(3)	(4)	(5)
CST ELA	0.79	coeff.	-.003	-.001	.011	.001	-.006
		se	(.031)	(.029)	(.038)	(.028)	(.036)
		n	2804	2804	2804	3119	2242
CST Math	0.77	coeff.	-.038	-.059*	.002	-.045	-.024
		se	(.037)	(.033)	(.046)	(.033)	(.043)
		n	2737	2737	2737	3048	2156
Cumulative GPA	1.17	coeff.	-.040	-.018	-.060	-.035	-.052
		se	(.045)	(.041)	(.058)	(.042)	(.050)
		n	3950	3950	3950	4062	3692
Attendance rate	1.11	coeff.	-.005*	-.004	-.007*	-.005*	-.006**
		se	(.003)	(.003)	(.004)	(.003)	(.003)
		n	3571	3571	3571	3697	3295

Source: Authors' calculations using OUSD data.
Notes: ***Indicates significance at the 1% level, **5%, and *10%.
Robust standard errors clustered at the individual level are reported in parentheses. All estimates are from the two-stage least squares estimates of equation 3, with linear controls for the running variable (recentered, standardized CST ELA scores) above and below the cutoff (except column 3 which uses quadratic control functions). All specifications include the baselines set of covariates and fixed effects for grade, year, and school. Observations are weighted using an edge kernel for all columns except column 2, which uses a rectangular kernel equivalent to unweighted OLS. The optimal bandwith is computed using Imbens and Kalyanaraman (2009) “plug-in" procedure. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. CST ELA test scores used in the first stage to predict reclassification are the most recent scores available.

Table 2.5: RD Estimates of Effect of an Additional Year of RFEP Status (12th Grade Sample)

	Optimal Bandwith (h^*)		Linear (Edge)	Linear (Rect.)	Quadratic (Edge)	Linear, Alt. Bandwiths $h^* + .25$sd	$h^* - .25$sd
Outcomes (12th Grade)			(1)	(2)	(3)	(4)	(5)
CST ELA	0.82	coeff.	.015	.008	.031	.015	.020
(11th grade)		se	(.039)	(.037)	(.042)	(.034)	(.044)
		n	1041	1041	1041	1143	850
CST Math	1.03	coeff.	.011	.020	.031	.012	.014
(11th grade)		se	(.035)	(.031)	(.039)	(.032)	(.040)
		n	1062	1062	1062	1114	967
Cumulative GPA	0.93	coeff.	.001	.020	-.001	.008	-.001
		se	(.04)	(.034)	(.045)	(.035)	(.045)
		n	1187	1187	1187	1273	1037
Attendance rate	1.83	coeff.	.003	.002	.004	.003	.003
		se	(.004)	(.003)	(.004)	(.004)	(.004)
		n	1026	1026	1026	1026	1020
Graduated from HS	1.49	coeff.	-.017	-.015	-.014	-.016	-.019
		se	(.016)	(.015)	(.019)	(.016)	(.018)
		n	1424	1424	1424	1434	1385
A-G requirements	1.11	coeff.	.004	.005	.007	.004	.003
		se	(.023)	(.020)	(.026)	(.021)	(.026)
		n	1349	1349	1349	1407	1226
Took SAT	1.29	coeff.	.026	.020	.036	.026	.031
		se	(.021)	(.018)	(.023)	(.019)	(.023)
		n	1395	1395	1395	1427	1321
4-yr college enrollment	1.22	coeff.	.029	.034 *	.042	.028	.031
		se	(.020)	(.018)	(.032)	(.019)	(.023)
		n	949	949	949	984	877

Source: Authors' calculations using OUSD data.
Notes: ***Indicates significance at the 1% level, **5%, and *10%.
Robust standard errors clustered at the individual level are reported in parentheses. All estimates are from the two-stage least squares estimates of equation 3, with linear controls for the running variable (recentered, standardized CST ELA scores) above and below the cutoff (except column 3 which uses quadratic control functions). All specifications include the baselines set of covariates and fixed effects for grade, year, and school. Observations are weighted using an edge kernel for all columns except column 2, which uses a rectangular kernel equivalent to unweighted OLS. The optimal bandwith is computed using Imbens and Kalyanaraman (2009) “plug-in" procedure. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. CST ELA test scores used in the first stage to predict reclassification are the most recent scores available.

Table 2.6: Estimates of Reclassification Effects above the CST ELA Cutoff (Grades 3-5, 1 Year Out Sample)

Outcomes (1 yr out)		CST ELA		CST Math	
		Mean-shift	Slope-shift	Mean-shift	Slope-shift
Diff. in conditional expectation		(1)	(2)	(3)	(4)
On-frontier vs. off	coeff.	.048	.060	.080	.116
(below cutoff)	se	(.040)	(.068)	(.053)	(.085)
	n	2520	2520	2519	2519
Break at cutoff	coeff.	.018	.059	.004	-.021
(off-frontier)	se	(.027)	(.062)	(.036)	(.073)
	n	5174	5174	5173	5173
ITT (Diff. above cutoff)	coeff.	-.027	.162***	.011	.090
	se	(.029)	(.041)	(.040)	(.056)
	n	5217	5217	5216	5216
ITT (DID)	coeff.	-.001	.132*	-.045	-.012
	se	(.040)	(.080)	(.055)	(.100)
	n	7737	7737	7735	7735

Source: Authors' calculations using OUSD data.
Notes: ***Indicates significance at the 1% level, **5%, and *10%.
This table presents OLS estimates for differences in the linear conditional expectations of the outcome variable as a function of CST ELA reclassification scores, and conditioning on CELDT Overall reclassification scores, the baseline set of covariates, and fixed effets for grade, school, and year. Estimation is limited to students whose CST ELA reclassification scores place them within 2 standard deviations of the cutoff. The first row gives the difference (in intercept and slope) between the conditional expectation functions for students on and off the CST ELA frontier, only for students below the CST ELA cutoff. The second row gives the difference (in intercept and slope) between the conditional expectation functions for students below and above the CST ELA cutoff, only for students off the CST ELA frontier. The ITT estimates of the third row give the difference (in intercept and slope) between the conditional expectation functions of student on and off the CST ELA frontier, for students above the CST ELA cutoff. The ITT (DID) estimates of row four give the difference (in intercept and slope) between students who are on and off the frontier and above the CST ELA cutoff, minus the difference for students who are below the CST ELA cutoff. All test scores are standardized within year across all students district-wide, and recentered around the relevant cutoff. Robust standard errors clustered at the individual level are reported in parentheses.

2.8 Data Appendix

The outcomes considered for the one year out sample include CST ELA and CST Math scores (standardized within each year among all students district-wide), weighted cumulative GPA, and attendance rates. The continuous measure of treatment intensity (duration as an RFEP student) is measured as the number of days between a student's reclassification date if reclassified during the current or following academic year and the end of the following academic year (June 30th). Otherwise RFEP duration is set to zero.

For the 12th grade sample, RFEP duration is computed separately for each outcome to reflect the point in time when the corresponding outcome is observed, as described below:

- *CST ELA and Math scores (11th grade):* test scores are taken as the maximum across each student's grade 11 observations (in the event a student repeated grade 11), with RFEP duration computed based on the end of the corresponding test score's academic year.

- *Cumulative GPA (12th grade):* GPA is taken from each student's first non-missing grade 12 observation, with RFEP duration computed based on the end of the corresponding academic year.

- *Attendance rate (12th grade):* attendance rates are taken from each student's first non-missing grade 12 observation, with RFEP duration computed based on the end of the corresponding academic year.

- *Graduated from high school*: graduation is set equal to one for any student recorded as having graduated at any point in the data, and zero for any student who did not graduate but was still observed in grade 12. RFEP duration was computed based on the end of the academic year corresponding to each student's first year of 12th grade (in order to cap RFEP duration for students who repeated grade 12 as part-time students).

- *Met A-G requirements:* this is set equal to one for any student recorded as having satisfied A-G course requirements at any point in the data, and zero for any student who did not but was still observed in grade 12. RFEP duration was computed based on the end of the academic year corresponding to each student's first year of 12th grade (in order to cap RFEP duration for students who repeated grade 12 as part-time students).

- *Took SAT:* this is set equal to one for any student with available SAT scores, and zero for any student who did not have SAT scores but was still observed in grade 12. RFEP duration was computed based on the end of the academic year corresponding to each student's first year of 12th grade (in order to cap RFEP duration for students who repeated grade 12 as part-time students).

- *Four-year college enrollment:* this is set equal to one for any student recorded as attending a four-year college in the National Student Clearinghouse data, and zero for all other students observed in grade 12 up to 2013, the last year captured by the college enrollment data. RFEP duration was computed based on the end of the academic year corresponding to each student's first year of 12th grade (in order to cap RFEP duration for students who repeated grade 12 as part-time students).

3 The Distributional Effects of House Price Shocks on College Enrollment

3.1 Introduction

Understanding the forces that limit human capital accumulation is a longstanding topic of interest in economics. There is a growing body of evidence that a lack of family resources can limit college attendance, and that this effect is stronger for recent cohorts (e.g. Lochner and Monge-Naranjo 2011; Belley and Lochner 2007). Wealthier families not only have more resources with which to finance educational investments, but may also provide their children with better academic preparation, have different preferences and information regarding their educational choices, and have enhanced access to credit, among other advantages. Understanding the role of each of these advantages in the human capital investment decision is not only important for understanding barriers to college access and subsequent labor market success, but can also shed light on which public policies may lead to more efficient and equitable outcomes.

For all but the wealthiest families, the majority of household wealth exists in the form of home equity. Home equity is also frequently used as collateral to secure household debt at interest rates that can be considerably lower than on unsecured household debt and federal education loans (Stolper 2015). A number of recent studies have documented that homeowning families who experience rising home values are more likely to send their children to college (Lovenheim 2011; Johnson 2012), in particular the more selective colleges (Lovenheim and Reynolds 2013).

Housing market cycles cause large fluctuations in housing wealth, but relatively little is known about the causal mechanisms through which home equity appreciation affects college decisions in general equilibrium. Recent evidence finds that increased consumption from home equity appreciation is concentrated in borrowing constrained families, suggesting important collateral effects (Cooper 2013). Stolper (2015) shows that increased access to home lending markets allows homeowning families to send their children to more selective colleges. For families on the margin of college enrollment, however, it remains an open question whether lower levels of current family resources or a lack of access to affordable credit is the more salient constraint for homeowning families.

While house price appreciation does not impact borrowing constraints for renters, they may be affected through other mechanisms. First, the rental costs of housing tend to rise with house prices, resulting in a negative wealth effect as renters have to consume fewer non-housing goods in order to offset higher housing costs (Cooper 2013). Second, local housing booms can trigger increases in construction employment that can reduce college enrollment (Charles, Hurst and Notowidigdo 2012), which may disproportionately affect lower income renters. Distinguishing between wealth and credit channels for both renting and homeowning families is of particular interest because they suggest different roles for policy. If wealth constraints are a salient barrier then targeted grant aid and tuition subsidies may improve college access (though it remains an open question whether such investments would be socially desirable). Credit constraints, on the other hand, imply a larger role for subsidized education loans to correct market failures in household credit markets.

The first part of this paper presents an intergenerational model of the college investment decision in which families can borrow against their housing wealth in order to finance college for their children. The model builds on the characterization of capital markets from a landmark paper in the macro inequality literature (Galor and Zeira 1993) in which lenders must track borrowers at a cost, thus driving up the borrowing rate to cover these tracking costs. I extend this model to allow the lender tracking technology to depend on the value of housing collateral in an intuitive way (to reflect foreclosure by lenders), and introduce this collateral-based borrowing into an overlapping generations model where children borrow against their parents housing collateral to finance educational investments.

The contribution of this modeling exercise is two-fold. First, the model provides insight into the underlying credit market imperfection and the role of housing collateral to further our understanding of intergenerational mobility and optimal educational policies. Operating as a dual consumption-collateral good, owner-occupied housing can lower educational financing costs for the next generation. As a result, the distribution of owner-occupied housing can influence intergenerational mobility through two key mechanisms: a positive intergenerational externality from housing collateral, and in indivisibility in owner-occupied housing that limits exposure to this externality to higher-income families. Second, the model generates testable predictions for the impact of house price shocks on college enrollment operating through a credit channel to guide the empirical analysis: home equity growth is expected to ease borrowing constraints for homeowning families with lower housing wealth, but this effect will diminish as the value of home equity rises; renters, on the other hand, should be unaffected by borrowing constraints.

The second part of the paper presents estimates of the relationship between house price shocks and changes in college enrollment at the MSA level. Aggregating to the MSA level abstracts away from the effects of differences in the characteristics of homeowners and renters within metropolitan areas, while first differences eliminate the effect of time invariant differences in relevant characteristics across metropolitan areas. I also isolate plausibly exogenous variation in house price growth by instrumenting with a measure of metropolitan housing supply elasticity from Saiz (2010) that is based on local land topography. MSAs with elastic housing supply should experience only modest increase in house prices (and home equity) in response to large shifts in the demand for housing because housing supply can be expanded relatively easily. MSAs with inelastic housing supply, on the other hand, should experience large house price changes in response to the same demand shock (Glaeser, Gyourko and Saiz 2008). To implement this empirical strategy, college enrollment data was obtained from the Current Population Survey (CPS), and house price shocks were calculated based on data from Zillow, an online real estate database based on information from real estate agents and prospective home buyers.

The empirical results are consistent with the predictions from the theoretical model in the presence of credit constraints, as house price growth has a positive effect on college enrollment but the effect diminishes as house prices rises. In other words, there are positive college enrollment effects from house price growth in metropolitan areas with lower past housing prices where credit constraints are more likely to bind for more homeowning families. Metropolitan areas with higher house prices, on the other hand, where credit constraints are less likely to bind, are largely unresponsive to house price growth. With respect to previous research, an additional contribution is that this relationship also holds over a period of declining house prices, whereas previous studies have focused primarily on the housing boom. The results presented in this paper are also robust to isolating housing price shocks driven by the topography-based measure of metropolitan housing supply elasticity. Additional results provide only weak evidence that house price growth is associated with increases in housing-related employment.

To draw stronger conclusions about the relative importance of wealth effects versus collateral effects for homeowners would require additional data. One approach would be to estimate the impacts of house price shocks in a setting where home equity cannot be used as collateral, such as in the state of Texas prior to 1997 when home lending was effectively prohibited by the state constitution. Another approach would be to disaggregate metropolitan level data by homeownership status. Estimating the effects of house price shocks on renter enrollment and housing-related employment would help to assess the relative importance of negative wealth effects versus housing employment effects. Unfortunately, CPS data disaggregated by homeownership status is limited to the March Supplements, leaving too few observations to disaggregate college enrollment rates by homeownership status within metropolitan areas.

The remainder of this paper is structured as follows. Section 2 introduces the theoretical model, characterizes short-run equilibrium and generates simple testable predictions for the effects of shocks to the value of home equity on college enrollment. The empirical strategy and data are described in section 3 and 4, respectively. Section 5 presents the empirical results for college enrollment across metropolitan areas. Section 6 concludes and outlines possible extensions.

3.2 Housing Collateral and Borrowing Constraints

This section introduces a theoretical model to explore the role of shocks to the value of home equity in an intergenerational context where children borrow against their parents housing collateral to finance educational investments. The aim is to consider how house price shocks affecting the value of housing collateral can impact educational borrowing constraints and college investment decisions. This section concludes with a set of testable predictions on the relationship between house price shocks and college investment.

3.2.1 Model Setup

Consider a small open economy in a two-good world (housing services and a composite consumption good). Individuals in this economy live for two periods each in overlapping generations. During the first period individuals are considered to be "young" and borrow an amount e_t to invest in their education. The investment is realized in period two, when individuals become "old" and supply one unit of labor, pay back their educational loans with interest, choose the amount of housing services (h_t) to consume, their tenure type, and their consumption of non-housing goods (c_t).[61] Individuals also have children in their second period of life that invest in their own education. Children, however, are restricted in how much they can borrow. It is through this borrowing constraint that children are linked to their parents: educational loans must be fully backed by the housing collateral of parents. The owner-occupied housing of generation t (h_t) thus serves as collateral for generation $t+1$.

A few comments on this setup are instructive at the outset. First, it may seem unintuitive that a child's borrowing environment is entirely determined by her parents yet the child still shoulders the entire burden of financing the investment. This strict dichotomy is made to simplify the subsequent analysis, but there need not be a complete intergenerational division of these functions, and children may contribute a portion of the loan payments through

[61] Housing services are measured in "quality units" of housing that implicitly capture both quantity and quality dimensions)

informal arrangements with their parents. In practice, home equity loans taken out by the parents are likely to be one in a portfolio of several loans used to finance college—the larger the home equity loan take out by the parents for the purposes of college investment, the lower the average financing costs faced by the student for their loan portfolio. The critical assumption here is that average educational financing costs are influenced by the amount of collateral held by parents, and that children shoulder some portion of the financing costs. The decision to only consider loans secured by parental collateral and to have children pay *all* of their educational financing costs is made with the objective of simplifying the analysis and abstracting away from purposeful giving. Here we are free to consider the effects of an intergenerational capital market externality without the confounding effects of a bequest motive; the bequest motive not undo the key results from this model, but it may mask the effects from easing borrowing constraints if bequests are large relative to the financing burden.

Educational Investment Educational investment is treated as a continuous variable representing "quality units" of education. The numeraire good is produced by individuals who convert their labor into output (housing and non-housing) according to the following technology:

$$Y_t = w\left(e_t\right) L_t \tag{1}$$

where Y_t and L_t are output and labor input respectively, and marginal productivity $w\left(e_t\right) > 0$ is a concave function of education:

$$w\left(e_t\right) = \theta e_t^{\gamma}, \qquad 0 < \gamma < 1 \tag{2}$$

Household Preferences Each individual has one parent and one child so that there is no population growth. In each generation there is a continuum of individuals of size L. In this basic setup there is no explicit parental altruism; that is, parents neither leave their children bequests nor have preferences over their children's economic status. Parents only derive utility from consumption of housing services and other consumption goods in the second period of life:

$$U = \alpha \log h_t + (1 - \alpha) \log c_t \tag{3}$$

I thus abstract away from heterogeneity in preferences for homeownership, assuming that all individuals have identical preferences for housing consumption that are independent of tenure type. As discussed further in the next section, the housing tenure decision is determined by appealing to an intuitive result based on established observations from the urban economics literature: tenure sorting by level of housing consumption. Cobb-Douglas preferences imply that housing expenditures are a fixed share of household income. This assumption is supported by Davis and Ortalo-Magne (2011), who show that in the United States the expenditure share on housing is stable both across MSAs and within MSAs over time.

In this setup parents are thus purely self-interested agents. By their choice of housing, however, they also influence the extent of capital market imperfections that limit the scope of educational investment for their children, as discussed below.

Housing Tenure Sorting by Income The primary purpose of this modeling exercise is to consider the short-run impacts of a positive intergenerational externality from the consumption of owner-occupied housing (on borrowing rates for children's educational loans). Accordingly, I abstract away from many practical issues in the individual housing tenure decision, including life cycle issues, the dual function of housing as an investment good (housing does not earn a return in this model), and housing inheritances (housing completely depreciates by the time the child becomes an adult). Instead, I rely on previous results in the urban economics literature, which under minimal additional assumptions yield the prediction of tenure sorting based on the quantity of housing services consumed.

As a first approximation, tenure sorting by the quantity of housing services consumed is supported empirically in the United States (Glaeser 2011)[62] When households have monotonic preferences, this yields tenure sorting by income, with households sorting into homeownership based on willingness-to-pay. Thus it is an assumption of the model that there a critical income level ($\underline{y}$) above which households own their homes and below which they rent. Similarly, there is a critical level of housing services ($\underline{h}$) above which families own and below which they rent.

Capital Markets Capital markets function as in Galor and Zeira (1993), as described below, where borrowers can evade debt payments by moving to other places, but this activity is costly. Lenders thus track borrowers to impose evasion costs on borrowers and deter default, and factor these tracking costs into the lending rate. The main modification introduced here is that borrowers' evasion costs are an increasing function of their (parents') housing collateral. Specifically, borrowers' evasion costs are a linear function of tracking expenditures, but these evasion costs are scaled up by the level of housing collateral. The positive relationship between evasion costs and housing collateral is intuitive, as lenders foreclose on collateral in the event of default and borrowers lose their homes. Note that here the unique feature of housing collateral is that it is perfectly immobile, unlike other household assets. It is also possible that housing collateral influences the tracking technology in other ways that serve to lower mobility for those owning more valuable homes (and thus further reducing the tracking

[62]First, note that there is an obvious correlation between structure type (e.g. detached single family home) and the value of housing services provided by a housing unit; for a given level of land costs, detached single family homes tend to reflect "nicer" homes than do multifamily dwellings. Using data from the 2000 Census, Glaeser (2011) shows that ownership rates for structures with more than 4 units range between 9.1 and 13.2 percent; among the bottom income quintile, the ownership rate for these structure types falls even further to between 4.7 and 5.9 percent. On the other hand, the ownership rate for single family detached homes is 86.8%, and rises to 95.6% among the top income quintile. These numbers also mask a good deal of heterogeneity across metropolitan areas, as the relationship between structure type and unit quality undoubtedly differs across metropolitan areas along with variation in land costs. To this point, Glaeser (2011) focuses on detached structures and examines the relationship between the homeownership rate and the share of households that are single-family detached at the MSA level, finding a correlation of 0.7. Thus there is a strong correlation between tenure type and structure type (and between tenure type and quantity of housing services).

burden on the part of lenders).[63] To simplify the an analysis, I assume that evasion costs are multiplicatively separable in lender tracking expenditures and parental housing collateral, which enters linearly. Extending this setup to allow evasion costs to be a concave function of housing collateral does not change the spirit of the results, as discussed when characterizing the capital market equilibrium.

Formally, the market functions as follows. For simplicity time subscripts are suppressed in the discussion of capital markets, but housing always enters as a lagged variable since it is parental housing that serves as collateral for loans that their children will repay as adults. Capital is assumed to be perfectly mobile, so that individuals have free access to international capital markets. The world rate of interest is equal to $r > 0$ and is assumed to be constant over time. Lenders (who are outside the economy) can lend any amount at this rate. Borrowers can evade debt payments but this activity is costly, so lenders incur tracking costs (z) to prevent evasion and default. In the event of successful evasion and default, borrowers who rent their homes incur evasion costs that are a linear function of lenders' tracking expenditures, given by βz where $\beta > 1$. Homeowning borrowers secure their loans with housing collateral (valued at ph), incurring evasion costs that are scaled up by a linear function of the value of their housing collateral (Aph); their evasion costs are thus given by $\beta z(Aph)$ where β and A are the tracking technology parameters with $\beta > 1$ and $Aph > 1$.

This tracking technology is intuitive: (1) borrowers' evasion costs rise as lenders' tracking expenditures increase; and (2) borrowers' evasion costs rise as they own more housing, reflecting the consequences of foreclosure. As shown in the next section, the presence of

[63]There is ample evidence that homeowners are less mobile then renters (see Dietz and Haurin 2003 for a summary). To my knowledge there is no empirical research on the specific effects of the level of home equity on mobility, but there are several observations about the nature of transaction costs that support this assumption. First, the financial costs of moving (e.g. realtor commission payments based on sale price) and non-financial costs (e.g. psychic costs of giving up more "home") both seemingly rise with the value of one's home. Second, capital gains taxation of home sales has been shown to unambiguously reduce household mobility (Lundberg and Skedinger 1998). In progressive tax regimes, capital gains tax liabilities will generally rise with income, which is correlated with home values. In the United States, however, capital gains on real property sales are tax exempt up to $250,000 for single filers and $500,000 for married couples. Nonetheless, wealthier families that exceed these limits face transaction costs that rise with home values once their tax liability kicks in.

tracking and evasion costs create a capital market imperfection where individuals can only borrow at an interest rate i that is higher than the world rate r (as in Galor and Zeira, 1993) . The current model has the additional feature that the borrowing rate falls with the level of housing collateral.

Notice that while all individuals are born with the same returns to education (ability) and same preferences, they differ only in terms of the borrowing environment surrounding their educational investment as dictated by their parent's housing decisions. Families with greater housing wealth can secure credit at lower borrowing rates because they are able to provide more valuable collateral that reduces lenders' tracking burden.[64]

3.2.2 Capital Market Equilibrium

The capital market equilibrium follows as in Galor and Zeira (1993). It is clear that lenders to individuals must have positive tracking costs for each borrower otherwise everyone defaults. Hence the individual borrows at a rate higher than r for the lender to cover their tracking costs. An individual who borrows an amount d pays an interest rate $i = i\,(d, h)$ that covers the lenders' interest rate and tracking costs such that competitive financial markets operate on zero profits:

$$d \cdot i = d \cdot r + z \tag{4}$$

Lenders then choose z to be high enough to prevent evasion on the part of borrowers:

$$d\,(1+i) = \begin{cases} \beta z & \text{if } \; h_{t-1} < \underline{h} \\ \beta z(Aph) & \text{if } \; h_{t-1} \geq \underline{h} \end{cases} \tag{5}$$

[64]Empirical support for this feature of the model—that uncollateralized debt is offered at higher interest rates than collateralized debt—can be found in the 2007 Survey of Consumer Finances (SCF). Families with a home equity line of credit (HELOC) faced considerably higher interest rates on consumer loans than other loan types (11.5 percent compared to 8 percent on HELOCs and 6.7 percent on closed home equity loans).

Equation (5) is an incentive compatibility constraint. Together, equations (4) and (5) determine the borrowing rate:

$$i = \begin{cases} \frac{\beta r+1}{\beta-1} \equiv \bar{i} & \text{if } h_{t-1} < \underline{h} \\ \frac{\beta rAph+1}{\beta Aph-1} \equiv i(h) & \text{if } h_{t-1} \geq \underline{h} \end{cases} \tag{6}$$

In order for the borrowing rate to be well-defined and exceed the world rate of interest r, the following condition must hold:

$$ph \geq p\underline{h} > \frac{1}{\beta A} \tag{7}$$

In words, this condition says that the value of the smallest owner-occupied home must be sufficiently large relative to the inverse of the product of the evasion technology parameters A and β. Given assumption (7), the capital market imperfection can be seen from the equilibrium interest rate: (1) $\bar{i} > i\,(h) > r$, as the borrowing rate exceeds the world rate of interest, and borrowing rates are lower on home equity credit than on unsecured debt; and (2) $i' < 0$ and $i'' > 0$, so the borrowing rate falls with increases in housing collateral but at a decreasing rate.

3.2.3 Household Equilibrium

Next consider the household problem. Individuals borrow when young to invest in their education. Optimal investment occurs at point where the marginal return to education equals the additional debt payments:

$$\gamma\theta e_t^{\gamma-1} = 1 + i(h_{t-1}) \tag{8}$$

Plugging in for the equilibrium interest rates for owner and renter families, this yields optimal investment levels given by the following:

$$e_t = \begin{cases} \left(\frac{1+\bar{i}}{\gamma\theta}\right)^{\frac{1}{\gamma-1}} = k_1 & \text{if } h_{t-1} < \underline{h} \\ \left(\frac{1+i(h_{t-1})}{\gamma\theta}\right)^{\frac{1}{\gamma-1}} = k_2\left(\frac{ph_{t-1}}{\beta Aph_{t-1}-1}\right)^{\frac{1}{\gamma-1}} & \text{if } h_{t-1} \geq \underline{h} \end{cases} \tag{9}$$

where k_1 and k_2 are constants such that $k_2 > k_1$. Equation (9) says that for children in renter families, optimal investment is constant at a level below the optimal investment level for children in homeowning families. Moreover, for children in homeowning families, optimal investment rises with increases in the value of parental housing collateral but at a decreasing rate. Note that altering the underlying tracking technology so that housing collateral increases evasion costs at a decreasing rate does not change this conclusion, it merely accelerates children's diminishing returns to parental housing collateral.

Given optimal educational investment levels, the optimal housing consumption of the younger generation is completely determined by the parent's housing. From (3), optimal housing consumption is given by $h_t = \frac{\alpha y_t}{p}$ where income y_t is the difference between wage earnings and debt payments:

$$y_t = w(e_t) - [1 + i(h_{t-1})]\, e_t$$

Thus the distribution of parental housing completely determines the distribution of education levels, wages and housing in the next generation:

$$h_t = \begin{cases} k_3 & \text{if } h_{t-1} < \underline{h} \\ k_4\left(\frac{ph_{t-1}}{\beta Aph_{t-1}-1}\right)^{\frac{\gamma}{\gamma-1}} & \text{if } h_{t-1} \geq \underline{h} \end{cases} \tag{10}$$

where k_3 and k_4 are constants such that $k_4 > k_3$.

Before considering the effect of shocks to the value of home equity, let us summarize the lessons from the model in a static context. First, the model clarifies the nature of the underlying credit market imperfection; that is, lender tracking costs drive up aggregate financing costs that limit educational investment. Operating as a dual consumption-collateral good, owner-occupied housing can lower educational financing costs for the next generation. As a result, the distribution of owner-occupied housing can influence intergenerational mobility and trends in inequality through two key mechanisms: a positive intergenerational externality from housing collateral, and in indivisibility in owner-occupied housing that limits exposure to this externality to higher-income families.

Second, the nature of the underlying credit market imperfection has implications for policy. Output and income would be higher in the long-run if government intervention were able to lower the aggregate cost of educational financing, but this is not a Pareto improvement. As discussed in Galor and Zeira (1993), a potential Pareto improvement only exists if intertemporal exchange can be achieved at a lower cost than the cost of monitoring borrowers. In the current setup, potential Pareto improvements are those that mimic the effects of scarce collateral on the subsequent generation (by influencing the tracking technology) but at a lower cost. One approach would be to redistribute income through educational subsidies, such as a universal tuition voucher financed by a progressive income tax in the next period. Such a policy would enable people to purchase more housing (and non-housing) which in turn serves as collateral and lowers tracking costs for the subsequent generation. This would only represent a potential Pareto improvement if the aggregate savings in financing costs outweighs the costs of tax collection. Similar to Galor and Zeira (1993), this is likely to be the case for two reasons: (1) the government avoids the need to keep track of individual borrowers by providing the subsidy to everybody and by taxing everybody based on income; and (2) the tax system already operates for other reasons, so this policy only raises the taxes collected.

3.2.4 Testable Predictions

While households endogenously sort into homeownership, unanticipated housing shocks provide potentially exogenous variation in the value of housing collateral. At the boundary of homeownership, it may be that the benefits of any anticipated home equity appreciation are partially capitalized into home price, potentially undoing any investment effects resulting from anticipated home equity growth. In any event, since home equity growth is the sum of both anticipated and unanticipated components, here it is desirable to abstract away from any anticipated effects to focus on the effects of unanticipated shocks to the value of home equity (as in Johnson 2012).

Consider a shock to the value of home equity (through house price shocks) at the end of the second period, after parental housing and consumption decisions but before educational investments are determined. From equation (9), optimal educational investment rises with the price of housing. Owner families are thus susceptible to house price shocks, with $\frac{\partial e_t}{\partial p} > 0$. Moreover, the effects diminish as home equity increases, with $\frac{\partial^2 e_t}{\partial h_{t-1} \partial p} < 0$. Renter families, on the other hand, are shielded from the credit effects of house price shocks.

Recall that the model does not allow for direct parental altruism through bequests or direct educational investments, thereby restricting house price shocks to a credit effect but not a parental wealth effects. This simplified setup is intended to keep the characterization of short-run equilibria tractable. Allowing for parental wealth effects on educational investments would require, for example, constant elasticity of substitution (CES) preferences over three goods: housing, educational investments for children, and other expenditure goods. It is instructive to informally consider the predictions of such an expanded model, to see how parental effects may impact the distribution of education.

In addition to the expanded CES preferences just described, further divide the second period into multiple periods corresponding to different rental contract periods. House price shocks are assumed to impact both renter- and owner-occupied housing, though in practice there may be independent variation in rental prices. A house price shock will thus raise rental house prices in subsequent sub-periods. For renters, a positive house price shock in the middle of the second period, after initial parental housing decisions have been made, will translate into higher rental prices for the remainder of the second period, triggering a negative income effect on all goods, including educational investments. Thus if parental wealth effects are important, optimal educational investment for the children of renters will fall with the price of housing $\frac{\partial e_t}{\partial p} < 0$. However, a negative correlation between house price growth and college enrollment for renters would also be consistent with an increase in the demand for housing construction labor that disproportionately comes from renter families.

If owner-occupied housing is not treated as an investment good and owners are locked into their more valuable homes until death, then the above predictions remain unchanged. If owners can sell their homes at a profit and move to cheaper homes, then this profit will be used to finance increased consumption of all goods, including both owner-occupied housing and educational investments. These wealth effects would magnify the earlier prediction for homeowners, $\frac{\partial e_t}{\partial p} > 0$.

3.3 Empirical Strategy

The empirical strategy tests the predictions from the theoretical model by relating 3-year house price shocks to college enrollment rates of 18-21 year olds at the MSA level. The theoretical model predicts that house price growth may impact educational borrowing constraints for homeowning families (through effects on the value of their home equity), but with effects that diminish as home equity (and house price) levels rise. For renting families, house price shocks may also have a wealth effect (in the opposite direction as for homeowners), or influence renter college decisions through changing labor market prospects (i.e. increased demand for construction labor) and college supply constraints (i.e. re-allocating scarce enrollment slots from renters to homeowners). All of these effects can be traced to house price shocks; the central challenge to estimating the causal effects of house price shocks on college enrollment is that individuals are not randomly assigned to areas with different house price growth profiles.

A credible research design must address the possibility that children who benefit from house price growth are different from children who do not live in appreciating homes, or they face different college and labor market conditions. First, parents that sort into appreciating neighborhoods may value education more than parents who don't. Second, neighborhoods may be appreciating because their primary and secondary school preparation is improving. Third, Mian and Sufi (2009) show that house price growth is not independent of underlying income dynamics or changes in the supply of mortgage credit, both of which may be correlated with changes in the returns to college. Thus local home values may reflect current or future local labor market conditions that may be correlated with college enrollment decisions.

To address the first two concerns—sources of unobserved heterogeneity at the local level—I aggregate to the metropolitan level and relate the difference in average house prices in a given MSA between the beginning and end of three-year periods to differences in college enrollment shares among 18-21 year olds. Aggregating to the MSA level eliminates the effects of differences in the characteristics of homeowners and renters within metropolitan areas. Differencing eliminates the effect of time invariant differences in relevant characteristics across metropolitan areas; similarly, MSA fixed effects can also be considered in some specifications.[65] Region-year fixed effects can also be included, to avoid identifying college enrollment effects off of regional trends.

To address the third concern—sources of unobserved heterogeneity at the metropolitan level—I isolate plausibly exogenous variation in house price growth by instrumenting with a measure of metropolitan housing supply elasticity that is based on local land topography. MSAs with elastic housing supply should experience only modest increase in house prices in response to large shifts in the demand for housing because housing supply can be expanded relatively easily. MSAs with inelastic housing supply, on the other hand, should experience large house price changes in response to the same demand shock (Glaeser, Gyourko and Saiz 2008). I confirm this relationship using a measure of MSA housing supply elasticity developed by Saiz (2010) that is based on land topography constraints.

Because house price growth in the last decade is associated with the expansion and col-

[65]Note that the identifying variation—variation in housing prices across MSAs and within MSAs over time—is similar to that exploited in Lovenheim (2011).

lapse of subprime mortgage securitization, as an additional instrument I also use a dummy variable for the three states with the greatest concentrations of subprime mortgage originations in 2005 (Nevada, Arizona, and California)[66]—in future work I will aim to use a continuous, time-varying measure of subprime mortgage activity in place of this time-invariant state-level variation. I allow housing supply elasticity to have a different effect on house price growth across high- and low-subprime states and in each period. One cause for concern about the exogeneity of subprime mortgage share would be its relation to other types of household credit, but Mian and Sufi (2009) show that expansions in the supply of mortgage credit between 2002 and 2006 were restricted to mortgage credit and not other types of credit. I argue that the extent of subprime mortgage originations is a valid instrument that is conditionally exogenous when examining first differences in college enrollment at the MSA level.

The motivating theoretical model predicts that the effect of house price shocks will diminish as home equity increases, $\frac{\partial^2 e_t}{\partial h_{t-1} \partial p} < 0$. At the MSA level, this implies that MSAs with higher home equity levels should be less responsive to house price shocks. The main Two-Stage Least Squares specification thus includes the log of average lagged home price (to proxy for home equity levels), instrumented home price growth, and the instrumented interaction between house price growth and the log of average lagged home price:

$$\Delta Enroll_{crt} = \phi_{rt} + \beta_1 HomePrice_{cr,t-1} + \beta_2 \Delta \widehat{HomePrice}_{crt} + \beta_3 \Delta HomePrice_{crt}\widehat{H}omePrice_{cr,t-1} + \beta_4 \Delta X_{crt} + \varepsilon_{crt} \quad (11)$$

$$\Delta HomePrice_{crt} = \phi_{rt} + \alpha_1 HomePrice_{cr,t-1} + \textstyle\sum_t \alpha_2^t \tau_t Elasticity_{cr} + \sum_t \alpha_3^t \tau_t Subprime_{cr} +$$

$$\sum_t \alpha_4^t \tau_t Elasticity_{cr} Subprime_{cr} + \alpha_6 \Delta X_{crt} + u_{crt} \quad (12)$$

$$\Delta HomePrice_{crt}\widehat{H}omePrice_{cr,t-1} = \phi_{rt} + \alpha_1 HomePrice_{cr,t-1} + \textstyle\sum_t \alpha_2^t \tau_t Elasticity_{cr} + \sum_t \alpha_3^t \tau_t Subprime_{cr} +$$

$$\sum_t \alpha_4^t \tau_t Elasticity_{cr} Subprime_{cr} + \alpha_6 \Delta X_{crt} + u_{crt} \quad (13)$$

[66] See Mayer and Pence (2008)

where $\Delta Enroll_{crt}$ denotes the change in college enrollment among 18-21 year olds living in MSA c in region r between the end of the current three-year period t and the end of the previous period $t-1$; τ_t is an indicator variable for each time period; $HomePrice_{cr,t-1}$ is the log of average lagged home price; $\triangle HomePrice_{crt}$ is the percentage change in home prices in MSA c in region r over the last three-year period t; $\triangle X_{crt}$ is a vector of first-differenced time-varying MSA-level controls (e.g. average household income); and ϕ_{rt} are region-year effects. In the first stage equations 12 and 13, the instruments include a dummy variable for being in a subprime state ($Subprime_{cr}$), housing supply elasticity ($Elasticity_{cr}$), and their interaction, with effects on house price growth that vary in each time period depending on the direction and magnitude of the underlying housing market shock. Note that each time period is a distinct three-year period to allow for housing supply elasticity to have opposing effects on house price growth in boom and bust periods.

3.4 Data

3.4.1 Data Sources

The final dataset is a panel of MSAs observed over three consecutive three-year periods spanning the housing boom and bust. Each period ends at the end of August, just prior to the following academic year. The analysis period is split into three periods designed to coincide with two bust periods followed by a boom period: September 1999 through August 2002, September 2002 through August 2005, and September 2005 through August 2008. The final dataset contains 393 observations across 131 MSAs. Data was merged across several sources: house price data from Zillow (a real estate pricing information provider), metropolitan population data from the 2000 Census, college enrollment data from the Current Population Survey (CPS) monthly supplements, and a measure of housing supply elasticity computed by Saiz (2010). Housing prices and household income are converted to 2000 dollars using the Bureau of Labor Statistics' CPI-U.

House Price Data Median home sales price by zip code by month was obtained from Zillow, an online real estate database by real estate agents and prospective home buyers. Sales price data is available for all states except non-disclosure states (Idaho, Indiana, Kansas, Maine, Mississippi, North and South Dakota and Vermont), for all zip codes located in counties with at least 30 transactions in the previous 3 months. Zip codes were assigned to MSAs based on 2000 Census Zip Code Tabulation Areas (ZCTAs) and the 1999 County-based MSA or NECMA definitions. A monthly weighted average sales price for each MSA was computed by weighting each zip code in a given MSA by the number of housing units in the 2000 Census. Over the analysis period, median home sales price was available for 56% of all metropolitan zip codes that appear in the 2000 ZCTAs within the 42 available states. The dependent variable of interest is the cumulative percentage change in the weighted average home price for each MSA over each three-year period.

Housing Supply Elasticity Data One of the instrumental variables is a measure of housing supply elasticity computed by Saiz (2010) using satellite-generated data. His measure covers 269 metropolitan areas (using the 1999 county-based MSA or NECMA definitions) and is based on a precise measure of exogenously undevelopable land (e.g. owing to bodies of water and elevation). Land-constrained cities with large populations, such as in coastal California, Miami, New York City, Boston and Chicago, are among the most inelastic; Houston, Atlanta, San Antonio and St. Louis are among the most elastic large metropolitan areas. As will be shown in the following section, this elasticity correlates very strongly with recent changes in house prices.

College Enrollment and Other Metropolitan-Level Data For each MSA, CPS microdata was pooled from the monthly CPS supplements (September through April) to compute an annual weighted average college enrollment percentage (full- or part-time) for all 18-21 year olds. MSAs with less than 20 observations were dropped from the analysis sample, leaving 131 MSAs that appear in each three-year period. Other outcomes considered include the share of 18-21 year olds employed, and the share employed in the housing sector (construction and real estate industry codes). Median household income for 18-21 year olds is also computed as a control variable.

3.4.2 Summary Statistics

Table 3.1 shows that on average the magnitude of the housing boom in the second period (9/02 - 8/05) was roughly twice that of the first period (9/99 - 8/02), and twice that of the bust period that followed (9/05 - 8/08). Figure 3.1 shows the distribution of house price shocks within each period, and confirms that the break between the second and third periods naturally splits the majority of MSAs into boom and bust periods. Table 3.1 also shows that college enrollment rates among 18-21 year olds rose in every period, but average gains were largest during the second period when house price growth was also strongest.

3.4.3 Assessing the Instrument

The relevance of the instrument for predicting house price growth is illustrated in Figure 3.2. The top panel plots the growth in average house prices relative to 2000 for MSAs in the highest and lowest deciles based on the Saiz (2010) measure of housing supply elasticity. The most elastic housing supply MSAs experience very little house price appreciation from 1999 to 2008. Inelastic housing supply MSAs, on the other hand, experience strong growth of roughly 75 percent percent from 2000 to 2006 before house prices began to drop. A similar pattern is shown in the second panel, which examines year-to-year house price growth rather than cumulative changes.

The relationship between house price growth and housing supply elasticity is also evident from the first stage estimates of the IV specification (equation 2). Figure 3.3 plots the first-stage relationship between housing supply elasticity and house price growth separately for high- and low-subprime states in each period. There is a significant negative relationship between house price growth and housing supply elasticity for low subprime states during the first two boom periods, as more elastic MSAs exhibit smaller increases in house prices. The opposite relationship is observed during the bust period, as more elastic MSAs are resistant to the drop in house prices. In all periods, the positive relationship between housing supply elasticity and the magnitude of house price shocks does not hold for high subprime states.

In support of the exclusion restriction, Mian and Sufi (2011) present evidence that is inconsistent with a general non-housing related credit demand shift in inelastic MSAs. They show that there were no differences in borrowing between elastic and inelastic housing supply MSAs for unconstrained households (with low credit card utilization or high credit scores), but significant differences for constrained households.

3.5 The Effects of House Price Shocks Across Metropolitan Areas

This section presents estimates of equation 1 and related specifications to examine the effect of house price shocks on college enrollment. Columns 1 and 2 of Table 3.2 present estimates of the effect of 3-year house price growth from a basic specification without heterogeneous effects by house price levels, revealing no significant gains to college enrollment from house price growth among all MSAs. Subsequent columns show that the lack of an overall effect masks enrollment gains to low house price MSAs and that diminish as existing house price levels rise. Columns 3 and 4 report a positive coefficient estimate on 3-year house price growth in the split sample of MSAs with lagged house prices in the bottom tercile at some point during the analysis period. Given the smaller sample size from the split sample, these results are generally less precise; the magnitude of the estimate for low-price MSAs falls substantially with the inclusion of MSA fixed effects and is no longer statistically significant. Columns 5 and 6 report a negative coefficient estimate for the effect of 3-year house price growth for higher price MSAs, though the estimate is much larger and only statistically significant with the inclusion of MSA fixed effects.

Columns 7 and 8 of Table 3.2 present OLS estimates of β_2 and β_3 from equation 1. The results are consistent with the predictions from the theoretical model, yielding positive effects from house price growth for low-price MSAs that fall as lagged house prices rise. The magnitude of the estimates do respond to the inclusion of MSA fixed effects, but the general pattern of the results is unchanged. A similar relationship is found in the IV estimates of equations 1 and 2, presented in columns 4 and 5 of Table 3.3 (weighted by MSA population and unweighted) alongside the corresponding OLS estimates in columns 1 through 3. The first-stage F statistics on the excluded instruments are not indicative of a weak instrument problem. The fact that the general pattern of results does not change substantively when instrumenting for house price growth helps to mitigate any concerns that the OLS results are driven by endogenous variation in house prices.

To interpret the results, Figure 3.4 plots the estimated relationship between house price growth and changes in college enrollment in each period for low-price MSAs (at one standard deviation below mean lagged house price levels) and high-price MSAs (one standard deviation above). The estimated relationship is based on OLS estimates from a variant of equation 1, with all variables demeaned within each region-period pairing, and with the lag of logged housing prices re-centered around one standard deviation above or below the mean in each period. The estimated relationship is similar for the two boom periods: there is a positive relationship between house price growth and the change in college enrollment for low price MSAs, though most of these MSAs experienced only modest house price growth. But for low price MSAs that exhibited house price growth equivalent to one standard deviation above the region-period mean, the average increase in college enrollment was 3.1 percentage points during the second boom period, and 0.8 percentage points during the first boom period. High price MSAs, on the other hand, are largely unresponsive to house price growth, with enrollment effects close to zero.

A similar relationship holds between house price changes and college enrollment in the bust period. For low price MSAs, a drop in house prices of one standard deviation below the region-period mean led to a drop in enrollment of 1.5 percentage points. For high price MSAs in the bust period, falling house prices were associated with a slight increase in college enrollment of 0.7 percentage points.

This pattern of results is consistent with the predictions from the theoretical model in the presence of credit constraints, as house price growth has a positive effect on college enrollment but the effect diminishes as house prices rises. It is somewhat surprising, however, that there are no enrollment gains from house price growth in high price MSAs, but small gains from large house price declines in the bust period. One explanation for this finding would be that some individuals opt to leave college in order to work in the expanding housing sector, but return to college during the housing bust. Another contributing factor could be wealth effects for renters that work in the opposite direction as the model predicts for homeowners; renters who are forced to pay higher rents during housing booms may be less likely to enroll in college, for example.

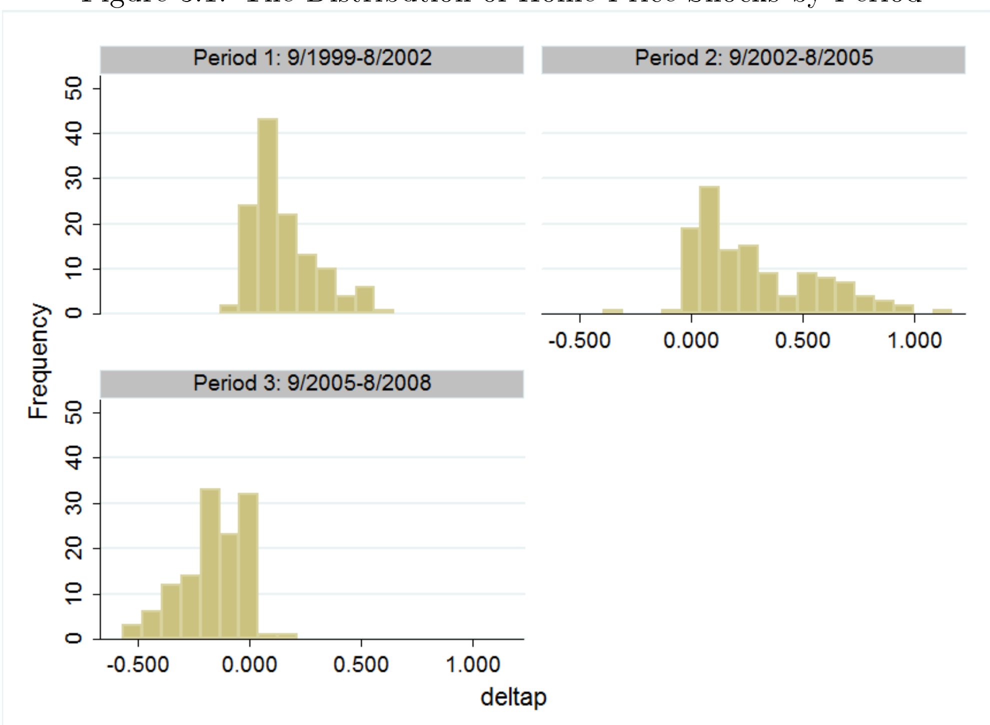
To begin to explore these mechanisms, Table 3.4 estimates a series of models using the share of 18-21 year olds employed in housing jobs as the dependent variable (defined as employment in either the "construction" or "real estate" sectors). The results present only weak support for housing employment effects; the estimated relationship between house price growth and housing employment is positive in all specifications, but very imprecise in some cases and only significant for high price MSAs when MSA fixed effects are included. For the full sample with heterogeneous effects, house price growth has small (and statistically insignificant) positive effects on housing employment that rise along with housing prices.

3.6 Discussion

This paper presents a simple theoretical model to illustrate the mechanisms through which parental housing wealth can ease educational borrowing constraints for their children. The model highlights how home equity appreciation can reinforce inequality in future generations: increases in the value of parental housing collateral can ease educational borrowing constraints for children, but the indivisibility in owner-occupied housing limits exposure to this externality to higher-income families. The empirical results confirm that house price growth leads to higher college enrollment rates (and vice versa for declining house prices), but these effects are concentrated in metropolitan areas with lower house price levels. There is only weak evidence that house price growth leads to increasing housing-related employment among college-aged individuals.

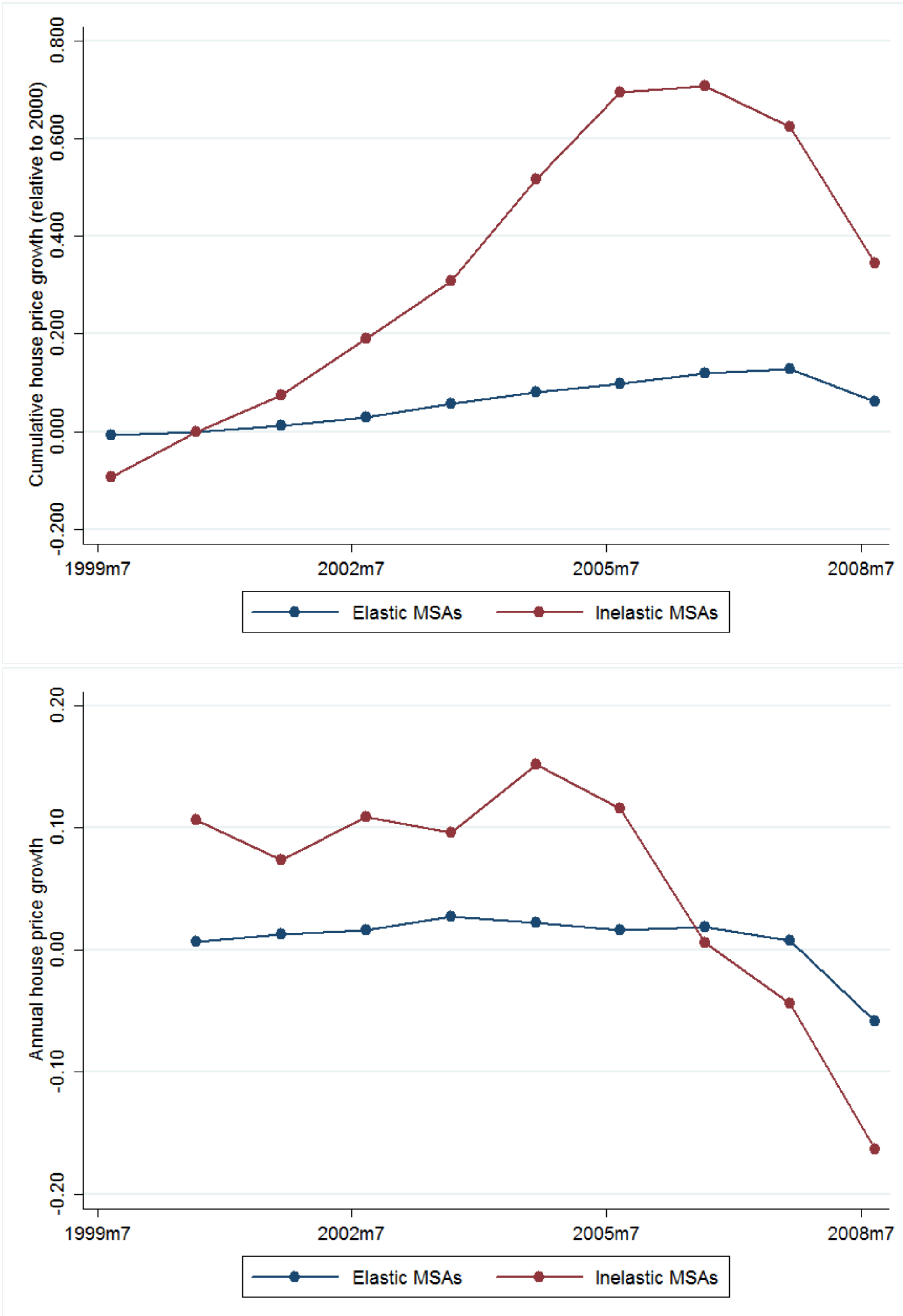
The major limitation of the empirical work presented here is the lack of college enrollment data disaggregated by homeownership status. More specifically, the fact that college enrollment rates do not rise along with house prices (and home equity) in higher-priced metropolitan areas could reflect a lack of any effect, or reductions in renter enrollment that mask enrollment gains to homeowners. Indeed, Chapter 1 of this dissertation has demonstrated that increases in access to home lending markets for homeowners may displace some renters from scarce college enrollment slots. There is also evidence that local housing booms increase the demand for construction labor and reduce college enrollment rates at community colleges in particular (Charles, Hurst and Notowidigdo 2012). Extending the current line of research to shed more light on the breadth of general equilibrium effects from house price growth across homeowners and renters alike is an area for future work. In order to draw stronger conclusions about the relative importance of collateral effects and wealth effects, a similar empirical strategy can be used but would require college enrollment data at the metropolitan level that can be disaggregated by homeownership status within metropolitan areas.

Figure 3.1: The Distribution of Home Price Shocks by Period



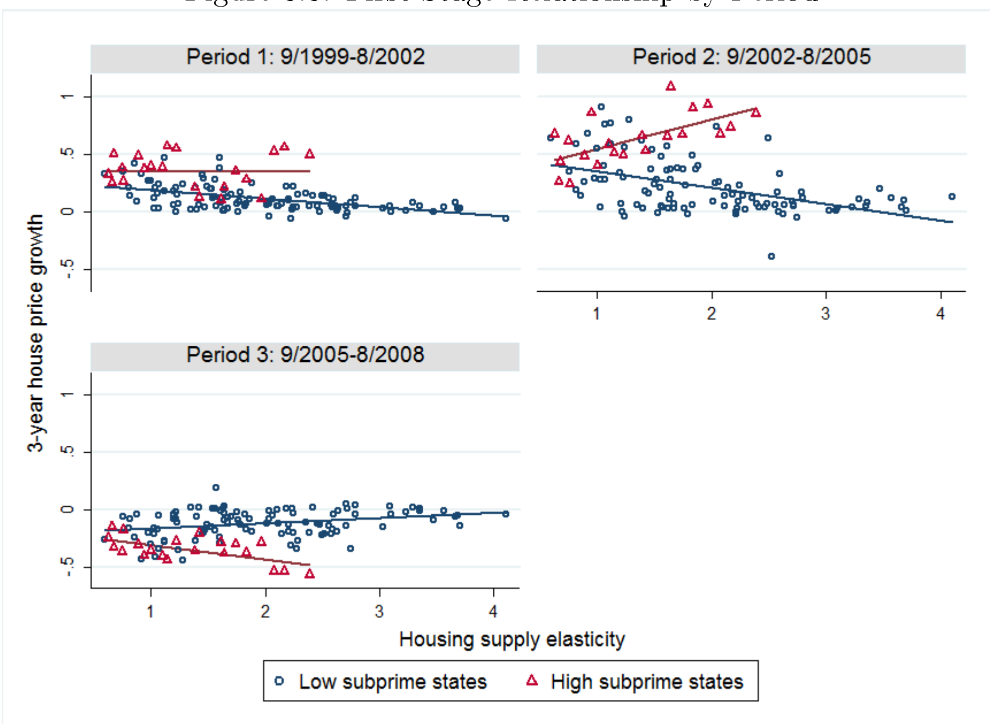
Source: Author's calculations based on Zillow data.

Figure 3.2: House Price Trends, Inelastic versus Elastic Housing Supply MSAs



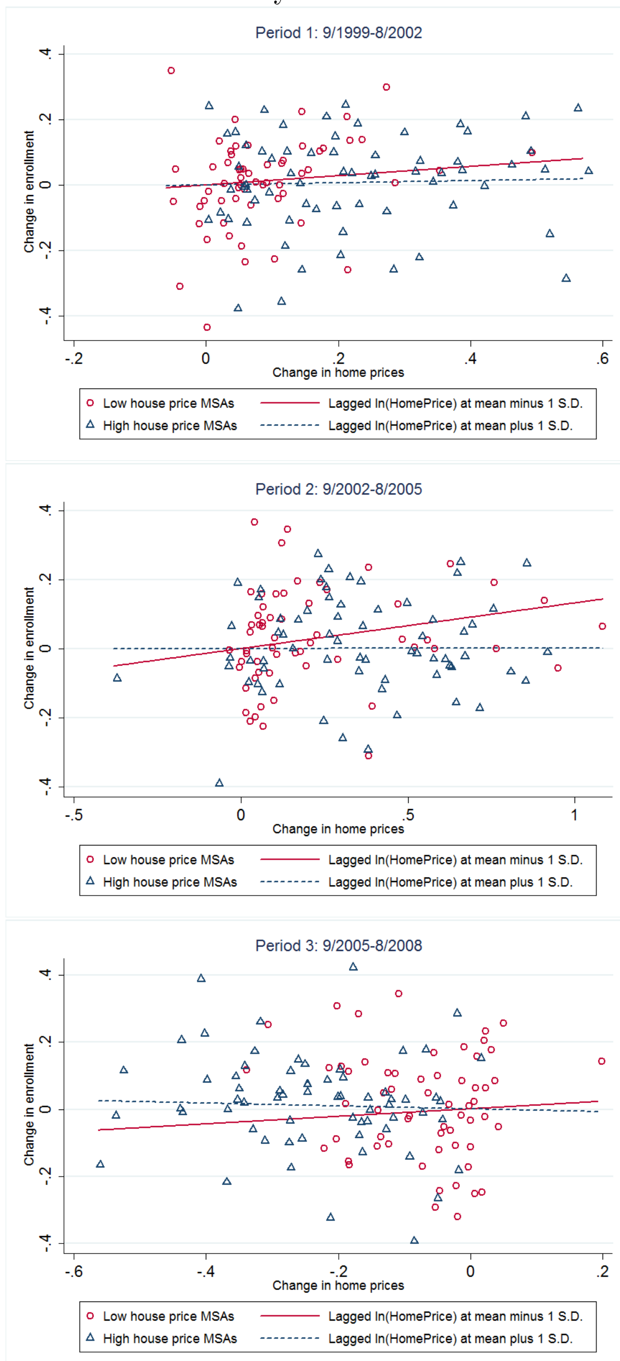
Source: Author's calculations based on Zillow data merged with the Saiz (2010) measure of housing supply elasticity.

Figure 3.3: First Stage Relationship by Period



Source: Author's calculations based on Zillow data merged with the Saiz (2010) measure of housing supply elasticity.

Figure 3.4: IV Estimates by House Price Levels and Period



Source: Author's calculations based on data from Zillow, CPS, and the Saiz (2010) measure of housing supply elasticity.
Notes: Regression estimates are based on OLS estimates of a variant of equation 1 de-meaned by region-period.

Table 3.1: Summary Statistics

	Period 1: 9/99-8/02 (n= 125)		Period 2: 9/02-8/05 (n= 125)		Period 3: 9/05-8/08 (n= 125)	
	Mean	SD	Mean	SD	Mean	SD
House prices (t-1)	132,599	66,172	158,215	94,788	208,239	140,452
△house prices	.158	.148	.287	.275	-.157	.144
College enrollment	.496	.118	.517	.128	.535	.144
△college enrollment	.009	.142	.021	.139	.018	.152

Source: Author's calculations based on data from Zillow and the CPS.

Table 3.2: The Effect of House Price Shocks on College Enrollment—OLS Estimates

	All MSAs		Low Price MSAs		High Price MSAs		All MSAs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\triangle HousePrice_t$	0.033	-0.075	0.255***	0.061	-0.034	-0.153**	0.187***	0.155*
	(0.042)	(0.056)	(0.094)	(0.109)	(0.045)	(0.075)	(0.066)	(0.083)
$\triangle HousePrice_t$							-0.151***	-0.209***
$\times ln(HomePrice)_{t-1}$							(0.053)	(0.063)
Fixed effects:								
Region-period	X	X	X	X	X	X	X	X
MSA		X		X		X		X
N	375	375	159	159	216	216	375	375
R-squared	0.076	0.240	0.157	0.331	0.108	0.281	0.098	0.270

Source: Author's calculations based on data from Zillow and the CPS.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. All standard errors clustered by MSA. MSAs are weighted by their 2000 Census population.

Table 3.3: The Effect of House Price Shocks on College Enrollment—IV Estimates

	OLS			IV	
	(1)	(2)	(3)	(4)	(5)
$\triangle HousePrice_t$	0.187***	0.155*	0.223***	0.175**	0.259*
	(0.066)	(0.083)	(0.076)	(0.082)	(0.133)
$\triangle HousePrice_t$	-0.151***	-0.209***	-0.196***	-0.135*	-0.196*
$\times ln(HomePrice)_{t-1}$	(0.053)	(0.063)	(0.068)	(0.073)	(0.107)
Region-period FEs	X	X	X	X	X
MSA FEs		X			
Weighted	X	X		X	
R-squared	0.098	0.264	0.058	0.098	0.0506
F-test of excluded IVs					
$\Delta HomePrice$				42.2	26.9
$\Delta HomePrice_{crt} \times HomePrice_{cr,t-1}$				84.0	30.9

Source: Author's calculations based on data from Zillow, the CPS, and the Saiz (2010) measure of housing supply elasticity.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. All standard errors clustered by MSA.

Table 3.4: The Effect of House Price Shocks on Housing-Related Employment

	Low Price MSAs		High Price MSAs		All MSAs		
					OLS		IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\triangle HousePrice_t$	0.042	0.014	0.022	0.042*	-0.008	-0.006	-0.013
	(0.049)	(0.076)	(0.019)	(0.025)	(0.036)	(0.054)	(0.052)
$\triangle HousePrice_t$					0.030	0.027	0.054
$\times ln(HomePrice)_{t-1}$					(0.031)	(0.040)	(0.043)
Fixed effects:							
Region-period	X	X	X	X	X	X	X
MSA		X		X		X	
N	159	159	216	216	375	375	375
R-squared	0.330	0.359	0.659	0.694	0.491	0.525	0.487

Source: Author's calculations based on data from Zillow, the CPS, and the Saiz (2010) measure of housing supply elasticity.
Notes: ***Indicates significance at the 1% level, **5%, and *10%. All standard errors clustered by MSA. MSAs weighted by population.

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