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# The Effects of Negative Equity on Children's Educational Outcomes

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## Abstract

This study examines the effects of negative equity on children's academic performance, using data on children attending Florida public schools and housing transactions from the State of Florida. Our empirical strategy exploits variation over time in the timing of family moves to Florida in order to account for household sorting into neighborhoods and schools and selection into initial mortgage terms. In contrast to the existing literature on foreclosure and children's outcomes, we find that Florida students with the highest risk of negative equity exhibit significantly higher test score growth. These effects are largest among Black students and students who qualify for free or reduced-priced lunch. We find evidence supporting two underlying mechanisms: (1) consumption patterns suggest that families in negative equity may reduce the impact of income losses on consumption by forgoing mortgage payments, and (2) mobility patterns suggest that families exposed to high levels of negative equity may move to schools that are of higher quality on average. While negative equity and foreclosure are undesirable, the changing incentives in terms of mortgage delinquency may have helped families manage the economic shocks caused by the great recession, as well as temporarily reduced the housing market barriers faced by low income households when attempting to access educational opportunities.

## I. Introduction

The extraordinary increase in housing values early in the 2000s, followed by the bursting of the housing bubble in 2006, led to large losses in housing wealth for many American families. Indeed, from peak to trough, housing prices fell by an average of 34 percent across the nation,[1] and left over one quarter of all households in negative equity (owing more on the mortgage than the value of the house) at the end of 2009.[2] When combined with the resulting recession, these high levels of negative equity led to unprecedented rates of mortgage delinquencies and home foreclosures in numerous metropolitan areas. While markets in many areas began strengthening in 2012, over 10 percent of mortgages in 2016 were still underwater.[3]

The plight of underwater households may extend well beyond household finances, affecting other domains of family and individual well-being. For example, negative equity and the risk of foreclosure have the potential to increase stress and reduce children's sense of security and stability. This study examines the effects of negative equity, as measured by the loan-to-value or LTV ratio, on children's academic performance. We apply a novel identification strategy to a unique dataset combining a statewide panel of student data on children attending Florida public schools with housing transactions data from the State of Florida for the years 2002 through 2011.

Theoretically, negative equity could have positive or negative effects on families and children. On one hand, large financial losses may lead to reduced consumption and change resource allocation decisions within the family (Bostic et al., 2009; Disney et al. 2010; Case et al., 2013). Financial losses may create stress between family members, affecting children's well-being and outcomes (Wood et al., 2012; Ananat

[1] Case Shiller - Standard and Poor's, Home Price Index Levels, Historical Price, available at: http://www.corelogic.com/research/hpi/september-2013-hpi-national-historic-data.pdf

[2] Core Logic Reports Negative Equity Decreases in First Quarter of 2012, available at http://www.corelogic.com/about-us/news/asset_upload_file365_22192.pdf. Zillow estimates an even higher number: 15.7 million, or 31.4 percent, of all mortgaged residential properties in negative equity in the first quarter of 2012. Zillow Negative Equity Report Quarter 1 2012, available at http://www.zillow.com/blog/research/reports/and Zillow Negative Equity Report Quarter 3, 2013, available at http://www.zillow.com/blog/research/2013/11/20/zillow-negative-equity-q3-2013/

[3] Zillow Negative Equity Report Quarter 3, 2016, available at https://www.zillow.com/research/q3-2016-negative-equity-report-13954/

et al., 2011; Rege et al., 2011). Negative equity may trigger foreclosure, forcing families out of their current residence and into new—and likely lower quality—schools and poorer neighborhoods (Been et al., 2011; Comey & Grosz, 2011; Cordes et al., 2019; Kachura, 2012; Schwartz et al., 2017). Residential and school mobility will have negative effects on student performance (Hanushek et al., 2004; Pribesh & Downey, 1999; Pettit & McLanahan, 2003; Rumberger & Larson, 1998; Swanson & Schneider, 1999). Renters also may face housing instability due to landlords entering into foreclosure (Johnson, 2010; Been & Glashausser, 2009).[4] [5]

On the other hand, negative equity may also produce some surprising benefits for children's well-being. For example, negative equity may reduce a family's mobility because they would have insufficient savings to pay the outstanding mortgage balance after their house is sold (Chan, 2001; Engelhardt, 2003; Ferreira et al., 2010).[6] Thus, being underwater might actually benefit the household's children academically by reducing mobility. Further, the long foreclosure timelines in many states, including Florida,[7] may actually help some households in negative equity to maintain consumption levels in the face of income losses through defaulting on their mortgage (Zhang, 2017). By contrast, households that experience income losses with positive equity in their homes have a strong incentive to make their mortgage payments and in normal times may reallocate scarce financial resources away from children in order to do so.[8] Finally, another potential positive impact of the price declines and negative equity is that this widespread reduction in housing costs may have allowed poorer families, especially renters who did

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[4] Desmond and Shollenberger (2015) show that evicted renters tend to move to poorer and higher crime neighborhoods than voluntary movers. See Vásquez-Vera et al. (2017) for a review of the literature on the health effects of eviction.

[5] The Protecting Tenants at Foreclosure Act of 2009 (PTFA) occurs in the middle of our sample, but many have argued that the PTFA was regularly violated in the years that followed (National Law Center on Homelessness and Poverty, 2012).

[6] As mentioned above, a great deal of descriptive evidence—and limited causal evidence—suggests that residential and school moves can be detrimental to children's academic and social outcomes. See Schulhofer-Wohl (2012) and Gerardi (2011) for alternative views on the question of whether negative equity reduces mobility, and Schwartz et al. (2017) for recent evidence on mobility.

[7] Past research documents a strong, positive and monotonic relationship between LTV and delinquency (Bayer et al., 2016). Further, at the end of 2011, the average time between the issuance of the first foreclosure notice and new ownership was over 1,000 days in New York and in New Jersey it was over 900 days. Florida boasted the third-longest foreclosure timeline, at 806 days (RealtyTrac, 2012).

[8] A large literature documents the positive effect of income on children's outcomes. For example, see Dahl and Lochner (2012) on the impacts of EITC expansions; Berger and Black (1992) on the impacts of child care subsidies; and Morris et al. (2001) on the impacts of welfare reform experiments.

not experience equity losses, to afford housing in wealthier neighborhoods with access to better schools. In fact, Ihlanfeldt and Mayock (2018) find that racial segregation in Florida schools fell during the housing crisis as housing prices fell in predominantly white neighborhoods.

The empirical literature on the effects of the housing crisis has generally focused on the effects of foreclosures, with several studies documenting a negative relationship between foreclosure and children's outcomes. Been et al. (2011), Comey and Grosz (2011), and Kachura (2012) examine the mobility effects of foreclosure and find that foreclosures often result in residential moves, which often take families to different neighborhoods and different schools. Dastrup and Betts (2012) and Bradbury et al. (2013) examine the effects of foreclosure notices in San Diego and Boston. Both find that students who experienced a mortgage default scored lower on standardized tests and exhibited higher absenteeism. Similarly, Swain (2013) finds that increases in county foreclosure rates over time lead to lower student test scores in North Carolina counties. In related research, Currie and Tekin (2015) find that living in a neighborhood that experiences an increase in foreclosures is associated with greater rates of emergency room visits and hospitalization.[9]

The fundamental problem faced by any study that hopes to untangle the relationship between the housing crisis and children's outcomes is that negative equity and foreclosure are influenced by the attributes or choices of the family. For example, many studies document the role of subprime lending in the foreclosure crisis (Mian & Sufi, 2009; Reid & Laderman, 2009; Gerardi & Willen, 2009), as well as the systematic concentration of subprime lending in low-income and non-white neighborhoods and among non-white borrowers (Mayer & Pence, 2008; Reid & Laderman, 2009; Fisher et al., 2010; Bayer et al., 2018). In terms of negative equity, families choose where to live and the terms of their mortgage contract. Families that faced large declines in housing prices or bought homes with little or no down payments pre-crisis may be different from households that obtained mortgages with large down payments or lived in

[9] Also see Yilmazer et al. (2015) on the negative relationship between declines in housing prices during the Great Recession and the psychological health of adult homeowners. Downing (2016) provides a complete review of the literature on health impacts of the foreclosure crisis.

locations experiencing lower home appreciation rates prior to the crisis. Analyses presented later in this paper show a strong correlation between household observables and both initial levels of LTV and selection into housing markets with rapid housing price appreciation for three large counties in Florida.

We address these endogeneity concerns by employing a reduced-form instrumental variables triple difference identification strategy to estimate the effects of negative equity on children's academic performance. Our empirical strategy accounts for the endogenous selection of households into neighborhoods and school communities, as well as into the initial terms of their mortgage contracts. First, we develop a proxy for the risk of negative equity (predicted LTV) based on county-level housing price changes, the purchase quarter by county-level distribution of initial LTV ratio, and the timing of student arrivals in the period leading up to the crisis. This proxy incorporates neither changes in housing prices at the neighborhood level nor the individual borrower's original LTV ratio. Second, we focus on recent in-movers to the state of Florida. Specifically, we use a sample of students who moved into the state before the start of the housing crisis (in the 2003-04 through 2005-06 school years). Families who move from out of state plausibly have less information than local movers about local housing markets, and their locational (purchase) decisions are more likely to be driven by labor market opportunities, rather than by an interest in housing market investment opportunities.

Third, we estimate student outcome models that include initial school by current year, arrival quarter by current academic year, and student fixed effects.[10] Our instrument varies at the county by arrival/purchase quarter by current year level, and the fixed effects yield triple-differenced estimates.[11] The inclusion of student fixed effects subsumes county by arrival/purchase quarter and means that we compare the test score growth for students exposed to different predicted LTV based on the quarter the student arrived in the county. The inclusion of school by current year fixed effects subsumes county by

[10] Purchase quarter is the same as the quarter in which families move (arrive) to Florida, and we alternatively label the quarter arrival or purchase.

[11] Schools are located within counties and all students in the same school are assigned to the same county so that initial school by current year fixed effects absorb county by current year fixed effects. Student fixed effects capture arrival quarter by county (or initial school fixed effects) because these outcomes are constant for the student over all years in the sample. We also construct all current year fixed effects using academic (rather than calendar) years.

current year fixed effects and eliminates variation associated with simple comparisons of test score growth between schools or counties, and the inclusion of arrival/purchase quarter by current year fixed effects eliminates any unique, statewide secular trends in test score growth across cohorts of students moving inot the state. Therefore, our model is identified by calculating differences in test score growth rates across arrival cohorts and comparing these differences between counties with high and low levels of housing price declines during the crisis.

Florida provides a relatively unique and empirically appealing setting for examining this phenomenon. Florida was hit hard by the Great Recession and was one of the states that saw the greatest volatility in housing prices during the housing crisis. Metropolitan area housing prices rose by as much as 88 percent between the between the third quarter of 2003 and the second quarter of 2006 (our relevant pre-period) and fell by up to 54 percent between the beginning of the crisis and the second quarter of 2011 (our sample period); see Figure 1. Florida is also one of a select group of states that provides longitudinal student data across all school districts in the state from 2000 forward, and for which quality housing transaction data are available that identify all liens used to purchase the home, allowing the calculation of a combined loan-to-value ratio.

In contrast to the existing literature that documents adverse effects of foreclosure on children's education outcomes, we find that Florida students with the *highest* risk of negative equity exhibit significantly *higher* test score growth. In fact, the relative test score gains are practically monotonic in the predicted level of negative equity. These effects are largest among disadvantaged groups, such as Black students and students who qualify for free or reduced-priced lunch. These effects are also largest in the locations with the lowest initial housing prices, which are locations where non-white and low-income households are most likely to enter the market for owner-occupied housing.

The relationship that we uncover with our triple-difference model is also observed in simple descriptive analyses examining later cohorts that bought in at a higher price level and so faced larger losses. Comparing test scores across newcomer cohorts between 2003 and 2006 reveals that the most recent cohorts exhibit the largest test score gains over time during the crisis; further, the across-cohort differences are relatively small in counties with small shocks to housing prices and significantly larger in the counties with the largest housing price shocks. Also, while there are large test score differences between households that move into high- and low-shock counties, we find no trends in these differences over cohorts. Our findings are robust to a wide variety of alternative specifications.

We find evidence supporting two of the mechanisms discussed above. First, we find evidence consistent with financially-constrained households in negative equity maintaining consumption levels by skipping mortgage payments. We examine data from the Consumer Expenditure Survey during this period and find that homeowners in Florida were better able than homeowners in other states to maintain their expenditures as a share of income relative to renters. Further, while declines in housing wealth by themselves are associated with reductions in consumption (Browning et al., 2013; Iacoviello, 2011), these declines in consumption were mitigated among homeowners in states and years where the unemployment rate was highest suggesting a reversal in the effects of housing price declines when households face income shocks. Similar to our test score effects, these consumption effects are largest among homeowners who were Black and who did not have a four-year college degree. Second, we also find evidence consistent with families being able to move to neighborhoods with better schools. Specifically, we find that exposure to high levels of predicted negative equity leads to moves between schools that raise school quality on average, as measured by pre-determined Florida school report card grades. This second mechanism might help explain the larger effects for Blacks given Ihlanfeldt and Mayock's (2018) finding of falling school segregation in Florida during this time.[12]

While our analysis does not yield obvious policy prescriptions, we believe that the study offers some important lessons. First, while a considerable literature documents the importance of income for

[12] Unlike the consumption mechanism, the effect of negative equity on school quality are similar in magnitude across student race and income. However, the higher test score effects of negative equity for non-white and low-income students could still arise if the same improvement in school quality has a larger effect on Black and free and reduced-price lunch students, perhaps because these students experience lower quality schools on average and improvements in school quality matter more at schools with the lowest initial quality.

children's outcomes, this study points to the large potential gains for children that might arise from stabilization policies that help families weather shocks to income. This finding would seem to be especially relevant given evidence on the especially large impacts of economic downturns on non-white and low education workers (Hoynes et al., 2012) and important when considering recent eviction and foreclosure moratoriums during the Covid-19 pandemic (Goodman et al., 2020; Benfer, 2020). Second, the positive impacts associated with falling housing prices and increased access to higher quality schools provide further evidence of the role housing markets can play in educational achievement (Rivkin, 1994; Bayer et al., 2014).

## II. Methodology and Identification Strategy

As noted above, the central challenge to identifying the impact of negative equity on student academic performance is the endogenous selection of households into neighborhoods and school communities, and into the initial terms of their mortgage contracts. Put simply, this selection means a parsimonious regression model linking student performance to current LTV, negative equity, or foreclosure is likely to yield biased impact estimates.

We address this challenge through several strategies. First, we focus on a sample of new entrants to the state of Florida under the premise that their housing location decisions are less likely to be driven by housing price appreciation in local housing markets. Table 1 present the fraction of households by reason for moving for households with children ages 6-11, as well as all households (including single individuals) using the March Current Population Survey (CPS) from 1996 to 2007. Columns 1 and 2 present the fractions for moves into the state of Florida, and Columns 3 and 4 present the fractions for within statae moves. For moves to Florida, job reasons are much more important, and moves related to the housing market are much less important than for moves within Florida. This finding is especially true for families with young school-age children: 54 percent of moves to Florida relate to job and only 14 percent relate to housing, as opposed to 16 and 63 percent, respectively, for within-state moves.

A second way that our identification strategy limits bias is that we develop a proxy for risk of negative equity, which does not depend upon either the endogenous change in housing prices at the neighborhood level, or the individual borrower's endogenously chosen LTV (mortgage terms) at purchase. Finally, we estimate the impact of predicted negative equity on student outcomes using a reduced-form instrumental variables triple difference identification strategy.

*A Proxy for Negative Equity*

We develop a proxy for the risk of negative equity for new students by predicting the household LTV of the student's family, based upon the timing and destination of the student's move into the state. We create this proxy for all students regardless of whether their family owns a home, because homeownership is unobserved in the student data, as well as endogenous to family unobservables.[13]

We use Florida housing transactions data on sales prices and all lien amounts used for home purchases to calculate the combined loan-to-value ratio (LTV) for each housing purchase. For computational purposes, we measure the fraction of home purchases within five-point LTV bins (e.g., 0.5-0.55 or 0.9-0.95) for each county *c* and purchase quarter-year *p*.[14] Put differently, we characterize the LTV distribution non-parametrically for each county *c* by quarter and year the housing unit was purchased *p* as the fraction of home purchases, *j*, falling in specific LTV bins. The fraction is measured as:

$$D_{bcp} = Fr(\overline{LTV}_{b-1} < LTV_{jcp} \leq \overline{LTV}_b)$$

where *Fr* represents the fraction of sales and $\overline{LTV}_b$ is the upper bound of each LTV bin *b*.

Next we use a set of county-purchase quarter-year specific fixed effects ($\delta_{ct}$) within a hedonic sales price model to create a county-level price index and to normalize LTVs over time and counties.

[13] As noted earlier, we focus on students who are new to the state of Florida because their families are likely to have relatively limited information about local housing markets and their location decisions are most likely to be motivated by labor market opportunities within the area, rather than by housing market investment opportunities. To be clear, this interpretation assumes that a household moves to the county in which their child's new school is located at approximately the same time that the child becomes a student in the State of Florida for the first time.

[14] Results are insensitive to alternative specifications of bin number or size.

Specifically, we estimate a hedonic sales price model, using the population of home purchase transactions in every county in Florida as follows:

$$P_{jct} = \beta_c Z_{jc} + \delta_{ct} + \varepsilon_{jct}$$

where *j* indexes housing units, *c* counties, and *t* purchase quarters (that is, quarter by year).[15] $P_{jct}$ is the logarithm of the sales price of house *j* in county *c* and quarter by year *t*, and $Z_{jc}$ is a vector of that house's attributes. We then construct a smoothed county-level price index ($\bar{P}_{ct}$) for county *c* and quarter by year *t* based on a moving average as:

$$\bar{P}_{ct} = \frac{Exp(\bar{\delta}^5_{ct})}{Exp(\bar{\delta}^5_{c0}\ )}$$

where $\bar{\delta}^5_{ct}$ is a five-quarter moving average of $\hat{\delta}_{ct}$ centered on *t*.

Then, for each purchase quarter and year, *p*, each academic year during the crisis, *y,* and county, *c*, we create a measure of the average LTV for all relevant housing. To do so, we calculate the current LTV for all units in a bin as the midpoint of the LTV bin ($LTV^{mid}_{\tilde{b}}$) multiplied by the ratio of the current year price index to purchase quarter price index ($\frac{\bar{P}_{c\bar{t}y}}{\bar{P}_{cp}}$) to adjust for changes in price levels. Note that we define academic years to run from the third quarter of one year to the second quarter of the next.[16] We then calculate the average of the current LTV for that county, purchase quarter and year, and current academic year, weighting by the county-purchase quarter bin frequencies, $D_{bcp}$, as follows:

$$\overline{LTV}_{cpy} = \sum_{\tilde{b}=1}^{B} D_{\tilde{b}cp}\left(LTV^{mid}_{\tilde{b}}\frac{\bar{P}_{c\bar{t}y}}{\bar{P}_{cp}}\right)$$

[15] The transactions data contain the sales price, the sales date, the amounts of up to three mortgages, and an indicator for whether the transaction is arm's length. These data can be linked to a cross-sectional assessor's file that contains housing attributes including the number of bedrooms, number of baths, number of square feet, age of structure, and property type at the time that the data was purchased at the end of the first quarter of 2011. Arm's length purchases are dropped for all analyses using the transaction data.

[16] While an academic year typically runs from September to June, the academic year that we define for these purposes runs a full calendar year, but the quarters are aligned so that the third quarter of a traditional calendar year is the first quarter of an academic year beginning in that calendar year, and the second quarter of the next calendar year is the last quarter of the academic year.

where $B$ is the number of bins and $\bar{t}$ is a fixed quarter during the current academic year, e.g. the quarter in which standardized tests are administered.

*Student Performance and the Triple Difference Identification Strategy*

We estimate a regression model relating student academic performance to predicted LTV and a full complement of fixed effects that condition on unobserved differences across students, schools, and years, and the timing and location of the student move into the state. The intuition of triple differences models is often hard to convey, but the best way to see the identification is to consider the difference between two difference-in-differences estimators. In this case, a simple difference-in-differences estimator might involve comparing changes in students' standardized test scores (difference) between students who moved to a county early in the crisis and those who moved into the county late in the crisis. Those late-moving families lost substantially more equity between the date of home purchase and the depth of the housing crisis (differences). While this variation is absorbed by the county by purchase quarter fixed effects, the triple difference model will be identified in part because this resulting difference-in-differences estimate is expected to be larger in the counties that experienced the greatest declines in housing prices and the largest accompanying increases in predicted LTV across cohorts.

We first link student educational outcomes ($Y$) to predicted LTV based upon the county of residence implied by their initial school, the quarter and year in which they moved into that school from out of state, which we match to the purchase quarter in the housing data, and the current academic year. We also include a set of arrival quarter by current academic year fixed effects ($\eta_{py}$), school (when first observed in the state) by current year fixed effects ($\nu_{scy}$), and student fixed effects ($\gamma_{iscp}$), to implement a triple difference identification strategy. Note that school by current year fixed effects ($\nu_{scy}$) subsume the county by current year fixed effects, and student fixed effects ($\gamma_{iscp}$) subsume the county by arrival quarter fixed effects. The resulting equation is

$$Y_{iscpy} = \alpha \overline{LTV}_{cpy} + \eta_{py} + \nu_{scy} + \gamma_{iscp} + \mu_{iscpy}$$

where $\mu_{iscpy}$ represents the idiosyncratic variation in the equation.

The fixed effects eliminate many possible sources of bias. The arrival quarter by current year fixed effects $\eta_{py}$ capture any heterogeneity specific to the performance in period $y$ of students who arrived in period $p$. As an example, there may be heterogeneity in the timing of how student test score performance recovers from a move. We allow that heterogeneity to vary by move-in quarter so that each combination of the quarter that a student moved into the school and the year in which they are tested has its own intercept. Therefore, our estimates are not influenced by a spurious correlation between student recovery from a move and the way in which the crisis developed and proceeded in Florida.

The school by current year fixed effects, $\nu_{scy}$ , allow for non-parametric trends in the quality of the school and the overall county environment. These fixed effects capture any direct effect of the crisis and the following recession on the operation of each school, the performance of students in that school, or the general labor market environment for the family.

Finally, the student fixed effects, $\gamma_{iscp}$, control for both the level of student performance and any unobservables associated with the school to which the student moved initially and the arrival quarter and year. For example, if parents with certain unobserved attributes were unable to afford to move into expensive neighborhoods late in the pre-crisis period, but could afford to make such moves early in the crisis, any effects of those static, unobserved attributes would be captured by the student fixed effects. Thus, our estimated impacts are not biased by time-invariant unobserved differences in students or their families.

## III. Data and Descriptive Statistics

The analyses in this paper rely on a universe of statewide longitudinal student data provided by the Florida Department of Education and housing transactions data obtained from Dataquick, Inc. Our panel of students runs from 2002 through 2011, spanning much of the housing boom and bust. Florida is one of a select group of states that collect longitudinal student data across all school districts from 2000 onward and for which quality housing transactions data are available. Further, unlike most other states, students in Florida can be linked to a school as early as kindergarten, allowing us to observe students for several years before the crisis and to observe their test scores in 3rd through 8th grades several years into the crisis.[17] The housing transactions data report both sales prices and all lien amounts used for home purchase, enabling the calculation of a combined loan-to-value ratio (LTV). To our knowledge, Florida is the only state that meets all these data requirements, and as noted earlier Florida was affected heavily by the housing crisis.

The initial dataset is a panel of students for all schools in Florida, with one observation for every grade between kindergarten and 8th grade during years 2002-2011. The sample contains quarterly information on the school attended, test scores for grades 3-8, attendance, suspensions, race/ethnicity, and free and reduced-price lunch status. We identify a subsample of children observed in grades 1-7 who are new to the state (i.e., out-of-state movers) in academic years 2003-04, 2004-05, and 2005-06, where "new to the state" is defined as not being observed in any school in the state in any previous academic year. Our analysis sample includes one observation for every observed test score for those same students during the 2006-07 through 2010-11 academic years, and students with no test scores during this period are dropped from the sample. To simplify our analyses, we use the average of each student's math and reading scores, although we also analyze effects on math and reading test scores separately.[18]

Table 2 illustrates the structure of this sample. The first column identifies a panel for each cohort, or equivalently the academic year for which the student appears in the state education data. The second column identifies the academic year in which tests scores are observed for inclusion in the analysis sample; these years are associated with the rows in each panel. The third through the eighth columns represent the student's grade when they first appeared in Florida schools, and the cells in these columns identify the tested grades observed for each combination of cohort, academic year and student's initial grade in the state. In total, 229,648 children moved in during one of these three years and remained in Florida public

[17] The use of kindergarten attendance, rather than 1st grade, raises the prospect of misclassification of new to the state 1st graders who in fact resided in Florida, but either attended private kindergartens or did not attend kindergarten. However, results are robust to dropping 1st graders and basing our sample only on students observed initially between 2nd and 8th grade.
[18] The direction of the signs, and the significance of the findings generally are the same for math and reading. The effects of negative equity generally are larger for reading scores than for math (see Table 6).

schools until the 2006-07 academic year, the first year in the analysis. Of these students, 192,513 were in tested grades of 3-8 in 2006-07, and 174,248 (90.5%) of these students reported test scores in 2006-07. Further, 67,113 of these newcomers were in tested grades in 2010-11, and 62,240 (93.7%) have test scores in 2010-11. We also create a balanced panel of 41,218 students by selecting newcomers who appear in tested grades for all five years of the analysis sample. This can only happen if students were in grade 1 in the 2003-04 cohort, grades 1 or 2 in the 2004-05 cohort, or grades 2 or 3 in the 2005-06 cohort, or equivalently students in 3$^{rd}$ or 4$^{th}$ grade in our first analysis year of 2006-07.

We purchased housing transactions data for Florida from 1998 through the first quarter of 2011 from Dataquick, Inc. Dataquick provides information on all 1-4 family housing transactions in the state during this time period. The data include the address of the housing unit, the date of the sale, the sale price, the loan amounts for up to three liens recorded at the time of home purchase, and indicators for whether each loan is an adjustable rate or fixed rate mortgage. The detailed property attributes include whether the unit is a single family property, a multi-family unit, condominium, or mobile home with an owned lot; the number of bedrooms and baths; the square feet of living space; age of the structure; and lot size. We use these data to estimate county-level housing price indices, to calculate combined LTV ratios, and to create a measure of expected LTV for each county, purchase (arrival) quarter and year and current academic year.[19] As discussed above, the expected LTV in any academic year from 2006-07 to 2010-11 is based on the county distribution of initial LTVs in each arrival quarter and year and the change in the county housing price index between the arrival quarter/year and the current academic year. For most regressions, the price index for the academic year is based on the first quarter of the calendar year, January to March, when Florida tests are administered, but we also estimate models where the expected LTVs are based on price indices between one and three quarters prior to testing.

Table 3 shows the attributes of our sample of newcomer students overall and those of newcomers to low, middle and high housing shock counties, which we partition into terciles by housing price

[19] Our results are not sensitive to the inclusion of condominiums, multifamily buildings, or mobile homes.

appreciation between the first quarter of 2002 and the second quarter of 2006. (Note that the number of observations varies depending on the variable considered; there are more observations for suspensions and absences than for test scores because suspensions and absences are observed in every grade but test scores are only observed beginning with third grade.) Test scores are lower and the populations are more disadvantaged among newcomers to the high-shock counties. The newcomers in the high-shock counties tend to be less likely to be homeowners based on a constructed index that regresses homeownership on free and reduced price lunch status and PUMA. These differences are driven by newcomers both residing in Public Use Micro Areas with lower homeownership rates and being more likely to be free or reduced-price lunch recipients.[20] Turning to the predicted LTV ratios at the bottom of Table 3, LTVs increase dramatically for all cohorts between 06-07 and 10-11, but they start at much lower levels for the earliest newcomer cohorts because those households gained the most equity in the run-up to the housing crisis. Therefore, the earlier cohorts are exposed to much lower rates of negative equity during the housing crisis.

**IV. Empirical Evidence on Identification**

In order to illustrate our concerns about the endogeneity of foreclosure and negative equity, we use a sample of home purchase mortgages from 2004 to 2006 from Miami-Dade, Broward and Palm Beach Counties, for which we have considerably more information about the homebuyers and their mortgages.[21] Tables 4 and 5 present key borrower attributes, such as race/ethnicity, income, age, and credit score (Vantage score) by LTV and by housing price appreciation at the Public Use Microdata Area (PUMA) level between the first quarter of 2002 and the second quarter of 2006. Table 4 indicates that mortgages with high initial LTV's are associated with a higher share of homebuyers who are Black, higher share who are Hispanic, as well as being associated with younger, lower income, and lower credit score borrowers. Table 5 indicates that Black homebuyers, lower income households, and lower credit score individuals

[20] The likelihood of homeownership is based on homeownership rates from the 2006 American Community Survey (ACS) using both PUMA and predicted eligibility for free and reduced-price lunch cells. We calculate eligibility using self-reported income and family structure in the ACS.

[21] See Bayer et al. (2016) for more details on this sample.

were disproportionately represented among individuals moving into PUMAs with high rates of housing price appreciation during the lead up to the crisis. These tables provide clear evidence that exposure to negative equity and risk of foreclosure during the crisis are strongly associated with systematic mortgage and location choices made by households.

Our identification strategy addresses many of the concerns raised by the patterns shown in Tables 4 and 5. The prediction for negative equity is constructed so that it does not depend upon either the changes in housing prices at the neighborhood level or the individual borrower's original LTV. The focus on recent movers assures that student fixed effects capture both student-level unobservables and any factors associated with the timing of when households moved into the neighborhood/school. The restriction of the sample to new state residents limits the information that households had concerning local housing markets and increases the likelihood that location decisions within Florida were driven by more general labor market factors. The school by county by academic year fixed effects should absorb these factors.

Further, we conduct several analyses to assure that relying on variation across newcomer cohorts is unlikely to bias our analysis. Figure 2 presents graphs representing average student demographics on race/ethnicity and free and reduced-price lunch status, by purchase cohort, i.e., the quarter and year when the students are first observed in Florida Schools. The arrival cohort is presented along the horizontal axis, with later arrival quarters represented further to the right. The graphed lines depict the percent of students in our sample who are Black (solid), Hispanic (dashed), and who are eligible for free or reduced-price lunch (short-dashed). Arrival cohort only seems to matter in terms of whether the newcomer was present at the beginning of the school year (third quarter) or moved to the school in the middle of the school year, as illustrated by changes in the share of students eligible for free or reduced-price lunch among the sample of movers (and, to a lesser extent, the share of Black students). Other than changes in demographics associated with the third quarter of each year when school begins, newcomer demographics are constant across cohorts. This third quarter variation is captured by student fixed effects that subsume county by purchase quarter effects and any unique time trends associated with this third quarter variation are absorbed by the purchase quarter by current year fixed effects.

Next, Figure 3 presents newcomer student test scores by purchase cohort for the full sample (solid lines) and for subsamples in low (dotted line), medium (short-dashed line), and high (dashed line) price shock counties. Figure 3 mirrors Table 3 in that significant test score differences exist between newcomers both across arrival counties and across cohorts based on whether they arrived at the beginning of the school year; however, for all three subsamples there is no time trend in test scores over newcomer cohorts. In summary, the only systematic differences across arrival time that we observe in our sample is between students who arrived at the beginning of the year and those who did not. Later in the paper, we demonstrate that our results are robust to both subsamples of newcomers who were present at the beginning of the school year, and subsamples of newcomers who were not.

Figure 4 illustrates the identifying variation in our sample by plotting the cumulative density distribution of predicted LTV in each year for our newcomer sample by predicted LTV in 2006-07 using 20-point LTV bins. The horizontal axis depicts predicted LTV in 2010-11. The cumulative density graphs represent initial predicted LTV levels in 2006-07 for LTV below 0.5 (solid line), between 0.5-0.7 (dashed line), between 0.7-0.9 (short dashed line) and between 0.9-1.1 (dotted line). For students with predicted LTV below 0.5 in 2006-07, which is near the mean LTV in 2006-07 for the 2003-04 cohort as shown in Table 3, the vast majority of students have either positive or at most modestly negative expected equity in 2010-11. However, as predicted LTV increases in 2006-07—primarily due to loans from later cohorts)—the fraction of students exposed to high levels of expected negative equity in 2010-11 increases dramatically.

Finally, we use housing transactions between the third quarter of 2003 and the second quarter of 2006 to demonstrate the power of our instrument. Specifically, we regress current LTV from the third quarter of 2006 through second quarter of 2011 on predicted LTV and three vectors of fixed effects: housing transaction, Zip Code by county by current year (defined as 3$^{rd}$ to 2$^{nd}$ quarter of the next calendar year), and purchase quarter by current year. This specification is nearly identical to the test score equation in terms of fixed effects structure, except that Zip Code takes the place of school. Both predicted and actual LTV ratios are based on county-level price indices in the first quarter of the calendar year, when standardized tests are administered in Florida. The first panel of Table 6 presents the estimates resulting from regressing current LTV, an indicator for whether LTV is above 1.3, and an indicator for whether LTV is above 1.5, on predicted LTV for that county, purchase quarter, and current year, together with the three vectors of fixed effects. The second panel presents estimates that allow the relationship to be non-parametric by regressing these variables on a set of dummy variables for 20-point intervals associated with predicted LTV. The estimates shown in Table 6 suggest that our instrument is very powerful, as predicted LTV has a strong positive relationship with all three variables.

The predicted LTV bin coefficients are also strongly predictive for higher levels of predicted LTV, with coefficient estimates increasing across bins beginning around the bin for LTV between 0.9 and 1.1. The failure of our instrument to explain low levels of LTV is not unexpected. When predicted LTV is low early in the crisis, much of the variation in LTV arises from variation in household down payment choices, which our instrument intentionally excludes. However, as the crisis proceeds, declines in housing prices create substantial additional variation in LTV that is explicitly captured by predicted LTV.[22]

**V. Results**

We begin by showing graphically that our results arise in a simple difference-in-differences framework, i.e., by demonstrating differences in test scores over time by differences in purchase quarter. Figure 5 shows test scores by arrival quarter and year for all newcomer cohorts separately for newcomers moving into high-shock (Panel A) and low-shock counties (Panel B). The figures are based on a subsample of students who are in 1st grade in 2003-04, 1st or 2nd grade in 2004-05, and 2nd or 3rd grade in 2005-06, so that the entire subsample is in a tested grade from the 2006-07 through the 2010-11 school year

[22] Note that negative estimates for lower predicted LTV bins arise in part because the omitted predicted LTV bin (below 0.5) represents a much smaller, more idiosyncratic set of transactions.

regardless of the newcomer's cohort. As in figures 2 and 3, the arrival quarters are shown along the horizontal axis, but in this case the graphed lines represent the academic year in which the test was administered. In both the high- and low-shock subsamples, test scores increase steadily between the academic years 2006-07 and 2010-11 (comparing across the lines), with the largest gains accruing to the later cohorts (comparing from left to right). These later cohorts faced the largest increases in negative equity over the housing crisis. Our difference-in-differences comparison is simply the difference in the test score growth between earlier and later academic years for the early and later cohorts, which suggest strong positive effects of exposure to negative equity on test scores. Note that we also observe a regular dip in test score gains in the third quarter of each calendar year, associated with the compositional differences between newcomer students who enter Florida schools at the beginning of each school year and those who enter in other quarters. However, as in Figures 2 and 3, this dip has no impact on the observed differential trends.

Further, a comparison of Figures 5A and 5B helps to illustrate the variation uncovered by the triple differences model specification. The difference-in-differences effects illustrated by Figure 5A for the high-shock county subsample, where increases in the magnitude of negative equity will be quite large, are much larger than the comparable effects for the low-shock county subsample in Figure 5B. Figure 5C depicts the difference between these two figures by calculating the differences in average gains between the high- and low-shock counties for each academic year over all cohorts. For the early cohorts, the differences over academic years between the counties are small, and there is no systematic relationship between academic year and differences between the two samples. For later cohorts, however, the differences are much larger and increase monotonically with the academic year as the housing crisis worsens.

Having found descriptive evidence of a positive effect of exposure to negative equity on student outcomes, we next present results based on our reduced form instrumental variable, triple-difference identification strategy. Table 7 presents the estimates arising from regressing standardized math and/or reading scores on the predicted level of negative equity, plus the three sets of fixed effects. The columns present the estimates for different predictions of negative equity, which differ according to whether the current housing prices are based on price indices for the quarter of the test (quarter 1) or one to three quarters before the test. The first row reports the effects for reading test scores, the second row for math, and the third row for the average of the two exams. All estimates are positive and statistically significant, suggesting improved test scores from exposure to high LTV ratios; these results are robust to the quarter used to create the current year price index. Using column 1 estimates, an increase in predicted LTV from 0.79 to 1.89—the average increase experienced by 2005-06 cohort households residing in high-shock counties (see Table 3 Column 4)—is associated with a 0.205 standard deviation increase in combined math and reading test scores. In comparison, the raw test score gains between the 2006-07 and 2010-11 academic years (comparing the solid line to the horizontal axis in Figure 5A) for our last cohort in the second quarter of 2006 in high-shock counties is 0.561 standard deviations. Thus, our estimated effect explains 37 percent of the observed test score gains during this time period. For low-shock counties, the increase in predicted LTV during the period is 0.62, implying effects of 0.116 standard deviations, or 73 percent of the test score growth during this period using the maximum of 0.159 standard deviations observed in the first quarter of 2006 in Figure 5B (again comparing the solid line to the horizontal axis on the far right hand side of the figure). These are substantively significant effects.

Figure 6 depicts the non-parametric relationship between predicted LTV ratio and the average score on the two exams. Specifically, we regress average test score on dummy variables associated with predicted LTV falling in 20-point bins and our three vectors of fixed effects. For predicted LTV ratios between 50 and 110, which would be observed early in the crisis throughout the state and in the low-shock counties through the entire period, test scores are relatively unrelated to predicted LTV. Beyond that point, however, test scores improve steadily as predicted LTV increases. The lack of effects for low predicted LTV may arise because effects only exist for substantial levels of negative equity or because of the weak power of predicted LTV at low values, as seen in Table 6.

The fourth and fifth rows of Table 7 report the effects on suspensions and attendance. We find no association with attendance, and the effect of high predicted LTV on suspensions works in the opposite direction from test score results, suggesting some disruptive effects of exposure to negative equity. The estimates are only marginally significant at the 10 percent level, however. The magnitude of the suspension estimates are relatively stable, declining only slightly as we use earlier quarters for the price index, but the estimates lose significance for two and three quarters before test administration. The effect of negative equity on attendance is positive, consistent with the effects on test score, but smaller and statistically insignificant. Thus, it seems unlikely that the positive effect of negative equity is operating through effects on intermediate outcomes such as student attendance or adverse disciplinary events.

The remaining rows of Table 7 contain a series of robustness tests using the average of the math and reading test scores. The first two rows of the bottom panel split the sample between students who attend a full year of school in their arrival year and those who enter school after the school year has begun, following our observation that students who enter mid-year exhibited lower test scores and more disadvantaged demographics. The results are robust, but larger for students who enter mid-year.[23] The next three rows address concerns arising from the structure of our sample. Namely, as seen in Table 2, the sample is triangular in student's initial grade and in the analysis academic year, with test scores observed for either early initial grades and/or early analysis years. First, we re-run the analysis dropping test scores in the latest analysis years, i.e., 2010-11 in row 4 and both 2009-10 and 2010-11 in row 5. Alternatively, we eliminate students in the highest arrival grades. Specifically, in row 6, we restrict our sample to students in 3rd or 4th grade in 2006-07 so that the students are in tested grades for our entire sample period. In row 7, we restrict the sample to 3rd, 4th and 5th grades so that students are tested in the first four years and then drop the last year, 2010-11. The estimated magnitudes are very stable. Finally, we estimate the model

[23] Note that the design assures that our regression sample only contains test scores from the year after the student first appears in Florida schools, so that the student has a full year in Florida schools for every test score included in the regressions.

dropping one purchase quarter at a time. Figure 7 presents these estimates, and they are stable across purchase quarter omissions.

Table 8 examines these effects by selected subsamples. While we find that exposure to high levels of predicted LTV increases relative test score performance for all groups, Table 8 indicates that estimated effects are much larger for free and reduced-price lunch students and for Black students, and moderately larger for Hispanic students.[24] An increase in LTV of 1.10, the change over the entire period for the most affected cohort in the high-shock counties, implies an increase in test scores for free and reduced-price lunch students of 0.303 standard deviations and an increase for full-priced lunch students of 0.105. The same increase in predicted LTV for Black students, Hispanic students, and white students implies increases of 0.281, 0.083 and 0.056, respectively. The implied effects for low-shock counties are about half this size.

While we cannot identify whether children's parents are homeowners or renters in our statewide education dataset, our largest estimates are for minority and low-income groups that have below-average homeownership rates. It is somewhat surprising that the groups least likely to own their home experience larger average test score gains from the exposure to high predicted LTV. One possible explanation for larger effects on populations that are less likely to own is that students from households in rental housing also saw increased test scores during the housing crisis. One of the mechanisms that we will test below is that falling housing prices during the crisis allowed households with less financial resources to access better schools. This mechanism could operate for renter households as well, especially if they took advantage of lower housing prices to buy housing in neighborhoods with better schools Finally, as noted in the introduction, rental households may be impacted negatively by evictions following foreclosures against landlords, or impacted positively as falling housing prices make higher quality neighborhoods and schools more accessible.[25]

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[24] Note that these estimates use predicted loan-to-value ratios based on price indices measured for the quarter of the test.

[25] Another possible mechanism could have been failure to collect rent when rental units were in foreclosure, especially given federal limitations on the eviction of renters during the foreclosure process.

However, low income and minority homeowners could in principle be driving the stronger effects for these groups. Notably, when we divide the sample of free and reduced price lunch students based on average housing prices (last three rows of Table 8), we find the largest effects in the lowest housing price PUMA's where owner-occupied housing is likely to be most affordable for these lower income families. Specifically, the effect size for free and reduced price lunch students in the lowest housing price tercile of PUMA's is more than double the size of the effect in the top tercile, 0.36 vs 0.16. Similarly, back-of-the-envelope calculations suggest that a free and reduced price lunch family could afford a mortgage on $200,000 home. Splitting our sample by PUMA's where half or more of the owner-occupied housing is below $200,000 versus less than half is below $200,000, we similarly find a large estimate of 0.23 for the PUMA where half or more of the housing meets this criteria of being affordable, and a smaller estimate of only 0.15 for the other PUMA's.

Further, in the second column of Table 8 we restrict our sample to students who live in Public Use Microdata Areas (PUMAs) in which data from the 2006 American Community Survey (ACS) indicates that at least two-thirds of households with children who belong to the subgroup identified in each row are owner-occupants. Additionally, we restrict subgroups to newcomers to the state with school-aged children and divide the sample into cells based on PUMA, race, ethnicity, and free and reduced price lunch status.[26] While the magnitude of our estimates decline, our core results are robust to imposing this restriction, again with larger effects for blacks and free and reduced price lunch students especially in the lower housing price PUMA's.

A key potential reason for larger effects among Blacks and free and reduced price lunch students is that these groups are typically more vulnerable than white and higher-income households to the housing crisis and economic downturns (Bayer et al., 2016; Hoynes et al., 2012). Therefore, even though homeownership rates are lower for these groups, the larger economic shocks they experience may lead to larger effects on student outcomes. Table 9 establishes the differential effects of recession for our sample

[26] We also restrict the sample to residents of PUMAs that are completely contained within a single county.

and time period. We use the CPS to show a much greater decline in employment rates in Florida for prime-age Black males than either white or Hispanic males, and a large decline in income for households in the $20^{th}$ percentile of household income during the period (the $60^{th}$ and $80^{th}$ percentiles exhibited no income losses). This idea of higher vulnerability may also explain why the estimated effects for blacks and free and reduced price lunch students fall in the likely homeowner sample. When we focus on locations with higher rates of homeownership for each group we almost certainly restrict ourselves to places where low-income and Black or Hispanic households are relatively well off compared to the rest of the subsample.

## VI. Mechanisms

We earlier identified three potential mechanisms by which the decline in housing equity might raise test scores. The first mechanism works through potential reductions in mobility. While negative equity can lead to default and foreclosure when associated with an economic shock like a job loss, it may reduce mobility for the majority of households who continue making housing payments because they are unwilling or unable to sell their homes at a loss. If mobility negatively affects student test scores, then declines in home equity may raise test scores.

In order to examine this phenomenon, we use our housing transactions data to estimate a model similar to the loan-to-value regressions in Table 6. The dependent variable is whether households sell their home in any given academic year. The sample includes all housing transactions during our pre-crisis period for the academic years of 2003-04, 2004-05, and 2005-06 where the household had not sold the home by the beginning of 2006-07, and includes one observation for each year between 2006-07 and 2009-10, dropping years after a housing unit has already been sold. The model includes the controls for predicted loan-to-value ratio and three sets of fixed effects: zip code by county by purchase quarter and year, zip code by county by current academic year, and purchase quarter and year by current year.[27] The

[27] Predicted LTV is based on the price level in the 1st quarter of the calendar year. Models using other quarters before the test is administered yield similar results. The model includes zip code by purchase quarter and year fixed effects rather than housing unit fixed effects because only one mobility event is included in the sample per pre-crisis transaction.

second column adds controls for mortgage terms.[28] The first panel of Table10 contains the estimates for a model including predicted LTV, and the second panel includes estimates for the same model where we replace predicted LTV with 20-point LTV bins. The coefficient on predicted LTV is consistently positive, counter to our suggested mechanism of declines in home equity reducing mobility. When we examine the results for LTV bins, we find very similar results, with housing mobility increasing strongly with predicted LTV.

In Table 11, we present the estimates of the relationship between student mobility across schools and predicted LTV. Specifically, we regress an indicator for whether the student moved from their initial school in each year of the crisis on predicted LTV and the standard set of fixed effects used in Tables 7 and 8. The rows present the estimates for the full sample and then separately for those eligible and not eligible for free and reduced-price lunch. The second column presents the estimates for a further reduced sample restricted to likely homeowners, as defined above. In all cases, the relationship between moving from the initial school and predicted LTV is positive. Therefore, we do not find any evidence of reduced mobility on average arising from higher levels of negative equity exposure.

The second mechanism through which negative equity might lead to test score improvements arises through households' use of mortgage delinquency to preserve consumption and spending on children during a job loss or other reductions in income. When households have equity in their home, they may sacrifice current consumption to make mortgage payments. But when the household owes more on the home than it is worth and foreclosure is a long, drawn-out process, mortgage delinquency may present an opportunity to live rent-free for an extended period of time, providing them with a buffer to mitigate income losses. Indeed, Bayer et al. (2016) find a strong positive correlation between LTV ratio and delinquency that is especially large in high unemployment counties. We might expect this effect to be even more pronounced in states like Florida with long foreclosure timelines. Further, Zhang (2017) finds

[28] These terms include original LTV bins, adjustable rate mortgage indicators, and indicators for whether the initial purchase included a second and/or third subordinate mortgage.

that the length of the foreclosure process decreases the incidence of short sales. He argues that the free rent received during the foreclosure process is more attractive to many borrowers than using a short sale to eliminate the obligations associated with an underwater mortgage. Similarly, Herkenhoff and Ohanian (2015) postulate that delinquent mortgages operate as a credit line to unemployed households leading to lower rates of re-employment and higher wages (job match quality) in slow foreclosure states. Calem et al. (2017) show that delinquent credit card cure rates are higher for longer foreclosure time frames.

To examine the possibility of such consumption smoothing, we turn to the Consumer Expenditure Survey between the years 2006 and 2010. We calculate the ratio of current annual consumption spending to income. Using simple state averages, we show that Florida was ranked 3rd among states in 2006 in the ratio of consumption to income for both owner-occupants and renters. Florida's rank declines modestly to 6th for owner-occupants between 2006 and 2009, but falls to 21st for renters who do not have the option of mortgage delinquency. Next, we regress the consumption to income ratio on indicator variables for residing in Florida, being an owner-occupant and the interaction of these variables (Table 12).[29] In panel 1 of column 1 in Table 12, we show that in 2006, the coefficient on Florida is positive and the coefficient on the interaction between owner and Florida is near zero. Results from 2009 in panel 2, however, show that the coefficient on Florida falls as residents reduce consumption. Yet the coefficient on the interaction is positive, implying smaller reductions for homeowners. The last row displays the differences between the 2006 and 2009 estimates. The next four columns present estimates for subsamples of Black individuals, non-Black individuals, individuals with four-year college degrees or more, and individuals without four-year college degrees, which plays the role as a proxy for low income. As with the student test score effects, our largest effects are observed for Black individuals and individuals without a college degree.

[29] The model also includes controls for race and ethnicity, education, marital status, and number of children. The inclusion of these controls has virtually no effect on the estimates.

We generalize the above analysis by pooling data across all years between 2006 and 2010 and including annual data on state unemployment rates from the Bureau of Labor Statistics and state housing price indices from the Federal Housing Finance Agency. We then regress the consumption to income ratio on housing price indices, unemployment rates, and the interaction separately for owners and renters plus standard individual controls and state and year fixed effects. In column 1 of Table 13, we report that falling housing prices are associated with reduced consumption for owner-occupants, presumably due to the wealth shock, but the coefficient on the interaction between the housing price index and unemployment is negative and significant. This implies that falling housing prices, which may make mortgage delinquency more attractive, are associated with higher consumption to income ratios when unemployment rates are high. As in Table 12, these effects are largest for Black individuals and individuals without a college degree, again consistent with our test score effects.[30]

For our third potential mechanism, we consider whether families residing in high-shock counties who move residences were more likely to move to neighborhoods with better schools, potentially because those neighborhoods had become more affordable. We first document that rents increased less in high-shock counties in the aftermath of the housing bust. Using the American Housing Survey, which is a panel survey of housing units, we calculate average changes in rents for individual units in high-shock, medium-shock, and low-shock counties. The sample sizes are relatively small, but we generally find that rent increases between 2005 and 2011 were more modest in high-shock counties. Specifically, the average rental unit saw an increase in rents of just 1.3 percent in the high-shock counties, as compared to 22.1 and 17.1 percent in the medium- and low-shock counties, respectively.

We then examine whether school children were likely to move to better schools as the prevalence of negative equity in their county increased. Specifically, we regress the 2005 state-assigned letter grade

[30] In principle, one might predict that our estimated test score effects will be larger in the counties that faced the largest employment shocks during the crisis. However, the employment shocks were in large part caused by the housing crisis, and employment growth is a prime driver of housing prices. We did attempt to estimate a model where we allowed the effects to vary with predicted employment growth based on a Bartik style index, but those estimates were statistically insignificant.

(chosen to represent the school's pre-crisis grade) of a student's current school, and the probability of moving to a higher- and lower-letter grade school, on the predicted LTV ratio conditional on the same fixed effects and using the same sample as in Table 7.[31] The inclusion of student fixed effects assures that in column 1 we are capturing the effect of changes in exposure to school quality during the crisis. In row 1 of Table 14, we report positive and significant coefficients on predicted LTV for school letter grade. Improvements in school quality operate in both directions: increasing the probability of moving to a higher-graded school, and decreasing the probability of moving to a lower-graded school. The latter result arises in spite of the increased mobility among students exposed to high levels of negative equity. We next estimate these models for subsamples of students eligible for free and reduced-price lunch and Black students. The magnitudes of the subsample estimates, especially the overall effects in column 1, are relatively uniform across the subsamples, even though the estimates for the Black subsample are statistically indistinguishable from zero.

In short, students attend better-quality schools on average as predicted LTV increases. Our results are consistent with families walking away from their underwater homes to rent in more desirable neighborhoods, possibly because they have less time in their current location time so are less attached to the neighborhood or school, and/or because they face a higher incentive to walk away from an underwater mortgage. Alternatively, our effects may be driven by renters who did not face a shock to home equity, and who now that rents or housing prices are more affordable have the opportunity to find homes in more desirable school zones. Given the school by current year fixed effects, however, our findings can only arise from children of renters if the later cohort renters are more likely to move to take advantage of improving educational opportunities.

[31] Florida assigns a letter grade from A-F to each school as part of its school accountability program. For elementary school grades, schools receive at least a C if 60 percent of students receive a 2 or better on both their FCAT reading and math tests and 50 percent receive a level 3 or above on the writing test. Schools receive a D if they fail one or more, but an F if they fail all three standards. Schools can receive a B if these standards are met by six different subgroups of students or an A if additional criteria are met related to absence rates, share of students taking the test and test score improvements.

## VII. Conclusion and Policy Implications

We examine children's educational outcomes in the State of Florida during the housing crisis among children whose families moved to the state during the years leading up to the crisis and were exposed to different levels of negative equity. Our findings are starkly different than the current literature that relies on estimating conditional correlations between foreclosure or foreclosure rates and children or family outcomes. Unlike those studies, we find positive test score effects for students in years when they are exposed to large declines in housing prices soon after moving to the state, or equivalently, high predicted LTV ratios. The estimated effects for predicted LTV ratios in our main specification are largest for relatively disadvantaged groups, including Black students and students eligible for free or reduced-price lunch. Further, these effects are concentrated in locations where housing prices are relatively low, which should have made it easier for disadvantaged populations to enter the owner-occupied housing market. While we cannot identify students living in owner-occupied housing, our results are robust to restricting our analysis to locations where two-thirds of the households in a demographic group were owner occupants, although point estimates are somewhat smaller in magnitude. Finally, we find no effects of high LTV on attendance, and a positive relationship between LTV and suspensions, so improvements in test scores cannot operate through these intermediate outcomes.

The differences between our findings and the previous literature may be driven by our attempts to address bias from family unobservables that correlate with either initial loan-to-value ratios or selection into housing markets with high levels of appreciation. Alternatively, these differences may arise because our proxy captures both the direct effect of a family's exposure to negative equity and the indirect effect of belonging to a cohort where many families are in negative equity. We find evidence of both direct and indirect effects. Owner occupants in Florida—and in states with large declines in housing prices and high unemployment rates, more generally—tend to maintain consumption during the crisis better than renters. These findings are consistent with families in negative equity reducing the impact of income loss on consumption by forgoing mortgage payments. In terms of indirect effects, we show that rents rose much less in the aftermath of the housing boom in the high housing price shock counties, and that exposure to predicted negative equity is associated with greater mobility and with moves to higher-quality schools. While no one would recommend a financial crisis as a way to improve school performance, the results of our study illustrate both the potential gains for students from policies, such a eviction or foreclosure moratoriums, that insulating low-income and non-white families from income shocks and the significance of the barriers that housing markets place on the educational opportunities of low-income and non-white families.

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**Table 1: Reasons for Moves to and within Florida**

| Type of Move | Moves to Florida | | Moves within Florida | |
|---|---|---|---|---|
| Family type | Kids age 6-11 | Any Movers | Kids age 6-11 | Any Movers |
| Job Related Reasons | | | | |
| New job or transfer | 0.478 | 0.271 | 0.149 | 0.174 |
| To look for work or lost job | 0.022 | 0.042 | 0.000 | 0.020 |
| Other job-related reason | 0.043 | 0.052 | 0.011 | 0.041 |
| Housing Related Reasons | | | | |
| Wanted to own home, not rent | 0.054 | 0.024 | 0.034 | 0.099 |
| Wanted new or better housing | 0.043 | 0.054 | 0.195 | 0.166 |
| Wanted better neighborhood | 0.000 | 0.021 | 0.172 | 0.081 |
| For cheaper housing | 0.011 | 0.027 | 0.046 | 0.041 |
| Other housing reason | 0.043 | 0.054 | 0.195 | 0.166 |
| Other Reasons | | | | |
| Change in marital status | 0.022 | 0.041 | 0.092 | 0.046 |
| To establish own household | 0.011 | 0.034 | 0.046 | 0.039 |
| Other family reason | 0.152 | 0.145 | 0.115 | 0.132 |
| For easier commute | 0.000 | 0.011 | 0.057 | 0.057 |
| Retired | 0.000 | 0.031 | 0.023 | 0.005 |
| Attend/leave college | 0.000 | 0.043 | 0.000 | 0.007 |
| Change of climate | 0.054 | 0.090 | 0.000 | 0.003 |
| Health reasons | 0.011 | 0.033 | 0.000 | 0.008 |
| Other reasons | 0.022 | 0.032 | 0.000 | 0.010 |
| Natural disaster | 0.022 | 0.003 | 0.000 | 0.000 |
| Sample Size | 92 | 2,134 | 87 | 2,178 |

Notes. The table presents the fraction of households by the reason for moving for individuals identified in the 1996-2007 March Current Population survey as having moved in the last year. Columns 1 and 2 are shares for moves to Florida with column 1 showing the results for families with kids age 6-11 and column 2 showing results for all families including single individuals. Columns 3 and 4 are shares for moves within Florida with column 1 showing the results for families with kids age 6-11 and column 2 showing results for all families.

**Table 2: Tested grades in analysis sample**

| Arrival Year | Academic Year | Grade first observed in Florida schools 1st | 2nd | 3rd | 4th | 5th | 6th | 7th |
|---|---|---|---|---|---|---|---|---|
| 2003-04 | | | | | | | | |
| | 06-07 | $4^{th}$ | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | | |
| | 07-08 | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | | | |
| | 08-09 | $6^{th}$ | $7^{th}$ | $8^{th}$ | | | | |
| | 09-10 | $7^{th}$ | $8^{th}$ | | | | | |
| | 10-11 | $8^{th}$ | | | | | | |
| 2004-05 | | | | | | | | |
| | 06-07 | $3^{rd}$ | $4^{th}$ | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | |
| | 07-08 | $4^{th}$ | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | | |
| | 08-09 | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | | | |
| | 09-10 | $6^{th}$ | $7^{th}$ | $8^{th}$ | | | | |
| | 10-11 | $7^{th}$ | $8^{th}$ | | | | | |
| 2005-06 | | | | | | | | |
| | 06-07 | | $3^{rd}$ | $4^{th}$ | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ |
| | 07-08 | $3^{rd}$ | $4^{th}$ | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | |
| | 08-09 | $4^{th}$ | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | | |
| | 09-10 | $5^{th}$ | $6^{th}$ | $7^{th}$ | $8^{th}$ | | | |
| | 10-11 | $6^{th}$ | $7^{th}$ | $8^{th}$ | | | | |

Notes: Each panel represents the year that a newcomer student first appears in Florida schools. Each newcomer student can appear in the sample multiple times once for each year during the sample that they are in a tested grade. The rows within each panel list the academic years included in our sample. The columns list the grades for which a student might first appear in Florida schools and still be contained within our sample. Each cell in these columns identifies the tested grade for each academic year that is associated with both the student's arrival year and their grade in that arrival year. Blank cells imply that such a student would typically not be tested in that academic year.

**Table 3: General table of descriptive statistics**

| | | Samples by Housing Price Appreciation | | |
|---|---|---|---|---|
| Variable | Overall | Low shock | Medium shock | High shock |
| Average test score | 0.034 | 0.109 | 0.076 | -0.080 |
| (reading+math) | (0.950) | (0.922) | (0.949) | (0.972) |
| Initial test score | -0.145 | 0.011 | -0.093 | -0.355 |
| | (1.067) | (0.992) | (1.044) | (1.132) |
| Suspension | 0.181 | 0.177 | 0.193 | 0.174 |
| Percentage of days absent | 5.837 | 6.061 | 5.631 | 5.778 |
| | (8.181) | (8.694) | (7.888) | (7.865) |
| Eligible for free or reduced price lunch in first year | 0.469 | 0.405 | 0.439 | 0.570 |
| White | 0.433 | 0.618 | 0.407 | 0.262 |
| Black | 0.173 | 0.131 | 0.221 | 0.168 |
| Hispanic | 0.309 | 0.159 | 0.278 | 0.498 |
| Born in the USA | 0.799 | 0.890 | 0.795 | 0.705 |
| Likelihood of being | 0.682 | 0.727 | 0.701 | 0.615 |
| homeowner | (0.209) | (0.186) | (0.203) | (0.223) |
| 2003 newcomers: predicted | 0.522 | 0.546 | 0.523 | 0.497 |
| LTV in 2006-07 | (0.051) | (0.065) | (0.039) | (0.031) |
| 2003 newcomers: predicted | 1.090 | 0.960 | 1.110 | 1.193 |
| LTV in 2010-11 | (0.134) | (0.133) | (0.067) | (0.071) |
| 2004 newcomers: predicted | 0.641 | 0.658 | 0.648 | 0.617 |
| LTV in 2006-07 | (0.068) | (0.074) | (0.058) | (0.064) |
| 2004 newcomers: predicted | 1.331 | 1.160 | 1.372 | 1.469 |
| LTV in 2010-11 | (0.188) | (0.159) | (0.112) | (0.132) |
| 2005 newcomers: predicted | 0.811 | 0.820 | 0.818 | 0.792 |
| LTV in 2006-07 | (0.064) | (0.064) | (0.053) | (0.069) |
| 2005 newcomers: predicted | 1.685 | 1.440 | 1.718 | 1.889 |
| LTV in 2010-11 | (0.240) | (0.184) | (0.121) | (0.147) |
| Observations (test scores) | 614,377 | 203,244 | 204,564 | 206,569 |
| Observations (suspensions) | 1,098,177 | 365,272 | 365,088 | 367,817 |

Notes: Means of variables with standard errors in parentheses for continuous variables only. The first column presents means for the entire student sample. The next three columns present means by terciles ordered by county level housing price appreciation in the period leading up to the crisis, in order bottom, middle and top tercile. Likelihood of being a homeowner is predicted using the 2006 American Community Survey (2006) based on free and reduced lunch eligibility status and PUMA. Predicted Loan to Value ratio is based on a price index for the quarter the test is administered. Number of observations varies for different variables; the table lists test scores and suspensions as two examples.

**Table 4: Borrower attributes by initial Loan to Value (LTV) ratio**

| LTV | <0.8 | 0.8-0.9 | 0.9-0.95 | >0.95 |
|---|---|---|---|---|
| Borrower Black | 0.061 | 0.088 | 0.153 | 0.174 |
| Borrower Hispanic | 0.307 | 0.404 | 0.435 | 0.554 |
| Coborrower on mortgage | 0.443 | 0.358 | 0.296 | 0.235 |
| 1st income quintile | 0.185 | 0.136 | 0.159 | 0.201 |
| 2nd income quintile | 0.165 | 0.185 | 0.229 | 0.276 |
| 3rd income quintile | 0.179 | 0.194 | 0.258 | 0.245 |
| 4th income quintile | 0.179 | 0.215 | 0.216 | 0.172 |
| 5th income quintile | 0.292 | 0.270 | 0.137 | 0.106 |
| Vantage score <700 | 0.190 | 0.274 | 0.352 | 0.465 |
| Vantage score 700-800 | 0.274 | 0.348 | 0.391 | 0.387 |
| Vantage score >800 | 0.536 | 0.378 | 0.257 | 0.148 |
| Observations | 1890 | 3214 | 1348 | 3564 |
| | | | | |
| Borrower age <40 | 0.219 | 0.328 | 0.422 | 0.444 |
| Borrower age 40-49 | 0.299 | 0.338 | 0.315 | 0.310 |
| Borrower age >50 | 0.482 | 0.335 | 0.263 | 0.246 |
| Observations | 1240 | 1811 | 730 | 1897 |

Notes: Means of subsamples based on Loan to Value (LTV) ratio at mortgage origination for a sample of home purchase mortgages in Miami-Dade, Broward and Palm Beach Counties. Race, ethnicity and coborrower identified from Home Mortgage Disclosure Act (HMDA) data. Income quintiles based on sample incomes LTV calculated using purchase price and all liens recorded in public records transaction data (Dataquick Inc.). Vantage score is a proprietary credit score developed by the major credit reporting repositories. The Vantage score and age are made available by matching a subsample from a match between HMDA and transaction data to credit repository data.

**Table 5: Borrower attributes by PUMA housing price appreciation**

| Housing Price Appreciation | Low Shock | Medium Shock | High Shock |
|---|---|---|---|
| Borrower Black | 0.075 | 0.132 | 0.157 |
| Borrower Hispanic | 0.418 | 0.459 | 0.417 |
| Coborrower on mortgage | 0.384 | 0.308 | 0.292 |
| 1st income quintile | 0.111 | 0.169 | 0.263 |
| 2nd income quintile | 0.177 | 0.232 | 0.244 |
| 3rd income quintile | 0.206 | 0.217 | 0.204 |
| 4th income quintile | 0.216 | 0.202 | 0.151 |
| 5th income quintile | 0.290 | 0.180 | 0.139 |
| Vantage score <700 | 0.283 | 0.339 | 0.368 |
| Vantage score 700-800 | 0.336 | 0.359 | 0.362 |
| Vantage score >800 | 0.381 | 0.302 | 0.270 |
| Observations | 2750 | 3431 | 2711 |
| Borrower age <40 | 0.314 | 0.363 | 0.364 |
| Borrower age 40-49 | 0.352 | 0.311 | 0.284 |
| Borrower age >50 | 0.334 | 0.326 | 0.352 |
| Observations | 1587 | 1983 | 1531 |

Notes: Means of subsamples based on housing price terciles at the PUMA level for a sample of home purchase mortgages in Dade, Broward and Palm Beach Counties. Race, ethnicity and coborrower identified from Home Mortgage Disclosure Act (HMDA) data. Income quintiles based on sample incomes. Vantage score is a proprietary credit score developed by the major credit reporting repositories. The Vantage score and age made available by matching a subsample from a match between HMDA and transaction data to credit repository data.

**Table 6: Relationship between current LTV and predicted LTV**

| Variables | Current LTV<br>LTV | <br>LTV>1.3 | <br>LTV>1.5 |
|---|---|---|---|
| Predicted LTV | 1.006*** | 1.213*** | 2.586*** |
| | (0.013) | (0.442) | (0.200) |
| Observations | 3,202,257 | 3,202,257 | 3,202,257 |
| Coefficient Estimates on predicted Loan to Value (pLTV) bins | | | |
| pLTV 50-70 | -0.0294*** | -0.031** | -0.020*** |
| | (0.006) | (0.013) | (0.007) |
| pLTV 70-90 | -0.0436*** | -0.048*** | -0.020*** |
| | (0.006) | (0.009) | (0.005) |
| pLTV 90-110 | -0.0469*** | -0.123*** | -0.053*** |
| | (0.007) | (0.015) | (0.009) |
| pLTV 110-130 | -0.0265*** | 0.080** | -0.083*** |
| | (0.009) | (0.035) | (0.011) |
| pLTV 130-150 | 0.017 | 0.282*** | 0.200*** |
| | (0.010) | (0.047) | (0.017) |
| pLTV 150-170 | 0.080*** | 0.375*** | 0.436*** |
| | (0.013) | (0.059) | (0.031) |
| pLTV 170-190 | 0.171*** | 0.346*** | 0.578*** |
| | (0.016) | (0.068) | (0.032) |
| pLTV >190 | 0.254*** | 0.242*** | 0.583*** |
| | (0.018) | (0.085) | (0.039) |
| Observations | 3,202,257 | 3,202,257 | 3,202,257 |

Notes: Regressions with a dependent variable of the current Loan to Value (LTV) ratio, whether the LTV is above 1.3 and whether the LTV is above 1.5 for the sample of home purchase mortgages originated during the pre-crisis period between the third quarter of 2003 and the second quarter of 2006. The regression model includes the predicted LTV ratio or dummy variables associated with predicted LTV bins plus the three vectors of pairwise fixed effects: transaction, zip code by county by crisis year, and purchase quarter by crisis year. The dummy variables capture whether predicted LTV falls within a specific 20 point LTV bin with current LTV below 0.5 as the omitted category. Predicted LTV ratio is based on a price index for the quarter in which the test is administered. Standard errors are clustered at the county by home purchase year level.

**Table 7: Estimated effects of loan to value (LTV) ratio on student test scores**

| Variables | Quarter in which LTV is measured | | | | |
|---|---|---|---|---|---|
| | Quarter of tests (Q1) | One quarter before (Q4) | Two quarters before (Q3) | Three quarters before (Q2) | Sample Size |
| Reading | 0.210*** | 0.210*** | 0.202*** | 0.196*** | 585,530 |
| | (0.039) | (0.037) | (0.033) | (0.031) | |
| Math | 0.162*** | 0.160*** | 0.148*** | 0.139*** | 585,928 |
| | (0.022) | (0.020) | (0.020) | (0.020) | |
| Reading+math | 0.187*** | 0.186*** | 0.175*** | 0.168*** | 585,298 |
| | (0.028) | (0.026) | (0.024) | (0.023) | |
| Suspended during academic year | 0.019* | 0.018* | 0.017 | 0.015 | 969,335 |
| | (0.010) | (0.010) | (0.011) | (0.012) | |
| Percentage of days absent | 0.091 | 0.140 | 0.210 | 0.314 | 962,146 |
| | (0.225) | (0.232) | (0.243) | (0.256) | |
| Robustness tests for Standardized reading + math score | | | | | |
| Eliminating students who spent the full year in year 1 | 0.194*** | 0.195*** | 0.186*** | 0.180*** | 429,696 |
| | (0.032) | (0.030) | (0.027) | (0.025) | |
| Only students who spent the full year in year 1 | 0.113*** | 0.111*** | 0.107*** | 0.104*** | 155,602 |
| | (0.026) | (0.025) | (0.025) | (0.026) | |
| Test scores through 09-10 only | 0.187*** | 0.185*** | 0.174*** | 0.165*** | 524,667 |
| | (0.029) | (0.027) | (0.025) | (0.024) | |
| Test scores through 08-09 only | 0.190*** | 0.188*** | 0.174*** | 0.161*** | 435,554 |
| | (0.030) | (0.028) | (0.026) | (0.024) | |
| Balanced panel: scores in 06-07 through 10-11 | 0.212*** | 0.194*** | 0.163*** | 0.151*** | 267,486 |
| | (0.055) | (0.052) | (0.048) | (0.048) | |
| Balanced panel: scores in 06-07 through 09-10 | 0.213*** | 0.219*** | 0.207*** | 0.201*** | 204,254 |
| | (0.054) | (0.052) | (0.048) | (0.048) | |

Notes: Regression of the student outcomes from newcomers to the state during the pre-crisis period on predicted Loan to Value ratio and the three vectors of pairwise fixed effects: student, school by county by crisis year, and arrival quarter and year by crisis year. The first panel shows estimations for different dependent variables in each row, and the second panel varies the sample across rows for the combined math and reading test score regressions. Each column represents estimates using price indices that are based on a different quarter during the crisis year. Standard errors are clustered at the county by newcomer arrival year level. Samples sizes listed are for the first-column specification (Q1 tests). Sample sizes differ slightly for Q4, Q3, and Q2 test specifications, but by less than one percent of the total number of observations.

**Table 8: Estimated effects by student lunch status, race/ethnicity, and housing costs**

| Variables | All students | Likely homeowners* |
|---|---|---|
| Black | 0.256*** | 0.165*** |
| | (0.026) | (0.045) |
| Hispanic | 0.075* | 0.055 |
| | (0.039) | (0.048) |
| White | 0.051*** | 0.045*** |
| | (0.017) | (0.017) |
| Not free or reduced-price lunch | 0.095*** | 0.093*** |
| | (0.022) | (0.021) |
| Free or reduced-price lunch | 0.275*** | 0.138*** |
| | (0.041) | (0.052) |
| Free or reduced-price lunch, PUMAS | 0.362*** | 0.163* |
| with lowest-priced housing | (0.059) | (0.084) |
| Free or reduced-price lunch, PUMAS | 0.198*** | -0.123 |
| with middle-priced housing | (0.040) | (0.090) |
| Free or reduced-price lunch, PUMAS | 0.163*** | 0.142 |
| with highest-priced housing | (0.071) | (0.136) |

Notes: Regression of the student outcomes from newcomers to the state during the pre-crisis period on predicted Loan to Value (LTV) ratio and the three vectors of pairwise fixed effects: student, school by county by crisis year, and arrival quarter and year by crisis year. The first column shows estimates for the full sample, and the second column restricts the sample to residents of PUMA's for whom at least two-thirds of newcomers of this type in the PUMA report being homeowners in the 2006 American Community Survey (ACS). Each row represents regressions for specific subsamples. Predicted LTV ratio is based on a price index for the quarter the test was administered. Standard errors are clustered at the county by newcomer arrival year level.

**Table 9: Changes in Economics Circumstances during Housing Crisis**

| Year | Fraction Employed Prime Age Males<br>Non-Hispanic White | Black | Hispanic | Fraction of 2006 Income Percentiles<br>20th | 40th | 60th | 80th |
|---|---|---|---|---|---|---|---|
| 2006 | 0.854 | 0.703 | 0.848 | 1.000 | 1.000 | 1.000 | 1.000 |
| 2007 | 0.846 | 0.677 | 0.837 | 1.000 | 1.000 | 1.040 | 1.041 |
| 2008 | 0.844 | 0.683 | 0.847 | 1.029 | 1.040 | 1.094 | 1.094 |
| 2009 | 0.809 | 0.620 | 0.817 | 0.909 | 0.945 | 1.000 | 1.058 |
| 2010 | 0.791 | 0.607 | 0.786 | 0.871 | 0.909 | 1.025 | 1.003 |
| 2011 | 0.774 | 0.552 | 0.768 | 0.809 | 0.891 | 1.000 | 1.049 |

Notes. Based on CPS March sample for each year. The fraction non-employed is the number of males in each the group between ages 30 and 55 who report holding a job in the March CPS divided by the total number of males in this group and age category. Fraction by percentile is the income of the specific percentile income for households in a given year and with children in specific age ranges from the March CPS divided by the income for that same percentile in the 2006 March CPS. The age range is 5-9 for 2006 and increments by one year for each year afterwards in order to compare similar cohorts over time.

**Table 10: Estimated effects of LTV on housing mobility**

| VARIABLES | Baseline | Controls |
|---|---|---|
| Predicted LTV | 0.078036*** | 0.078775*** |
| | (0.016) | (0.016) |
| Observations | 3,252,768 | 3,252,768 |
| R-squared | 0.012 | 0.015 |
| ltv5070 | 0.009*** | 0.009*** |
| | (0.003) | (0.003) |
| ltv7090 | 0.007** | 0.007** |
| | (0.003) | (0.003) |
| ltv90110 | 0.008** | 0.007** |
| | (0.003) | (0.003) |
| ltv110130 | 0.010*** | 0.010*** |
| | (0.004) | (0.003) |
| ltv130150 | 0.014*** | 0.014*** |
| | (0.003) | (0.003) |
| ltv150170 | 0.021*** | 0.021*** |
| | (0.004) | (0.004) |
| ltv170190 | 0.036*** | 0.032*** |
| | (0.004) | (0.004) |
| Ltv>190 | 0.037*** | 0.037*** |
| | (0.004) | (0.004) |
| Observations | 3,252,768 | 3,252,768 |

Notes: Regressions with a dependent variable of whether the home was sold for the first time during the crisis period for the sample of home purchase mortgages originated during the pre-crisis period between the third quarter of 2003 and the second quarter of 2006. The regression model includes the predicted LTV or dummy variables associated with predicted LTV bins plus three vectors of pairwise fixed effects: zip code by county by purchase quarter and year, zip code by county by crisis year, and purchase quarter by crisis year. The dummy variables capture whether predicted LTV falls within a specific 20 point LTV bin with current LTV below 0.5 as the omitted category. Predicted LTV ratio is based on a price index for the quarter in which the test is administered. Standard errors are clustered at the county by home purchase year level.

**Table 11: Estimated effects of LTV on student mobility**

| Sample | All students | Likely homeowners* |
|---|---|---|
| All students | 0.077*** | 0.065*** |
| | (0.018) | (0.020) |
| Black | 0.109*** | 0.092** |
| | (0.023) | (0.045) |
| Not Black | 0.071*** | 0.062*** |
| | (0.020) | (0.021) |
| Not free or reduced-price lunch | 0.071*** | 0.063*** |
| | (0.021) | (0.021) |
| Free or reduced-price lunch | 0.078*** | 0.113*** |
| | (0.019) | (0.028) |

Notes: Regression of whether the student moved from her initial school for newcomers to the state during the pre-crisis period on predicted LTV and the three vectors of pairwise fixed effects: student, school by county by crisis year, and arrival quarter and year by crisis year. The first column shows estimates for the full sample, and the second column restricts the sample to residents of PUMA's in which at least two-thirds of newcomers of this type in the PUMA report being homeowners in the 2006 ACS. Each row represents estimates for regressions using specific subsamples. Predicted LTV ratio is based on a price index for the quarter the test is administered. Standard errors are clustered at the county by newcomer arrival year level.

**Table 12: Estimated Effects of Residence in Florida and Owner-Occupancy Status on the Ratio of Consumption to Income**

| Samples | Full | Black | Not Black | 4 Yr College | No 4 Yr |
|---|---|---|---|---|---|
| Year 2006 | | | | | |
| Florida | 0.903*** | 0.521*** | 1.024*** | 1.281*** | 0.812*** |
| | (0.077) | (0.147) | (0.078) | (0.129) | (0.088) |
| Owner- | 0.004 | 0.436 | -0.050 | -0.094 | 0.046 |
| Occupant | (0.100) | (0.261) | (0.093) | (0.138) | (0.116) |
| Florida*Own | 0.112 | 0.032 | 0.028 | -0.371*** | 0.266** |
| | (0.091) | (0.246) | (0.093) | (0.131) | (0.106) |
| Observations | 15,549 | 2,062 | 13,487 | 5,067 | 10,455 |
| R-squared | 0.0135 | 0.0210 | 0.0117 | 0.0152 | 0.0148 |
| Year 2009 | | | | | |
| Florida | 0.419*** | 0.378*** | 0.412*** | 0.607*** | 0.344*** |
| | (0.069) | (0.092) | (0.069) | (0.099) | (0.075) |
| Owner- | 0.082 | 0.189 | 0.058 | -0.002 | 0.091 |
| Occupant | (0.061) | (0.181) | (0.075) | (0.100) | (0.088) |
| Florida*Own | 0.755*** | 1.389*** | 0.692*** | 0.214** | 1.032*** |
| | (0.059) | (0.193) | (0.069) | (0.083) | (0.090) |
| Observations | 15,020 | 1,957 | 13,063 | 5,174 | 9,815 |
| R-squared | 0.0127 | 0.0329 | 0.0116 | 0.0174 | 0.0167 |
| Interaction | 0.643*** | 1.357*** | 0.664*** | 0.585*** | 0.702*** |
| Difference | (0.108) | (0.312) | (0.116) | (0.155) | (0.139) |

Notes. The dependent variable is the logarithm of the ratio of four times a household's quarterly non-housing consumption divided by annual household income. The samples for panels one and two include all respondents identifying as residing in owner-occupied housing or paying cash rent in the 2006 and the 2009 Surveys of Consumer Expenditures (CEX), respectively, and in the 39 states identified in the public release version of the CEX. The first column is for the full sample, and the following columns present estimates for subsamples. Each row presents OLS estimates of coefficients on dummy variables for residing in Florida, being an owner-occupant and the interaction of those two variables. The regressions also include dummy variables for racial identity, years of education, marital status and the number of individuals under the age of 18 residing in the household. The estimates on the interaction of Florida and Owner-Occupancy are robust in both significance and magnitude for models that just include Florida, owner-occupancy and their interaction excluding all of the additional control variables. Standard errors are clustered at the state level.

**Table 13: Estimated Effects of Statewide Unemployment and Housing Prices on the Ratio of Consumption to Income**

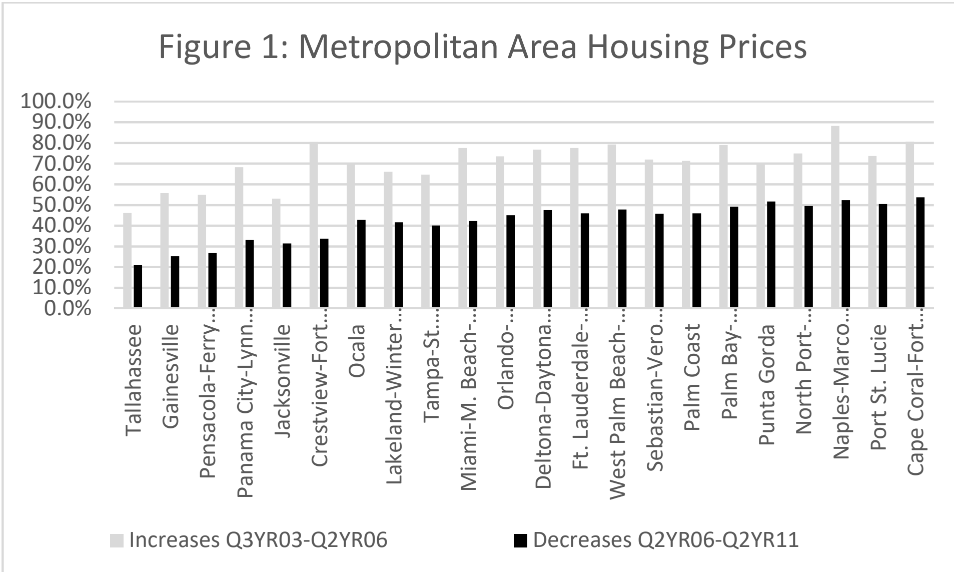
| Samples | Full | Black | Not Black | 4 Yr College | No 4 Yr |
|---|---|---|---|---|---|
| Owners | | | | | |
| Unemployment Rate | 0.162** | 0.399** | 0.148* | 0.162 | 0.177** |
| | (0.077) | (0.178) | (0.081) | (0.104) | (0.081) |
| Housing Price | 0.530** | 1.160 | 0.493* | 0.377 | 0.641** |
| Index*100 | (0.262) | (0.781) | (0.282) | (0.421) | (0.289) |
| Unemployment*Housing | -0.070*** | -0.213** | -0.063** | -0.040 | -0.095*** |
| Price | (0.022) | (0.092) | (0.024) | (0.028) | (0.029) |
| Renters | | | | | |
| Unemployment Rate | 0.064 | 0.030 | 0.068 | 0.157 | 0.031 |
| | (0.063) | (0.109) | (0.072) | (0.137) | (0.057) |
| Housing Price | 0.143 | -0.079 | 0.174 | 0.180 | 0.155 |
| Index*100 | (0.217) | (0.332) | (0.254) | (0.564) | (0.227) |
| Unemployment*Housing | -0.028 | 0.015 | -0.035 | -0.043 | -0.029 |
| Price | (0.020) | (0.044) | (0.024) | (0.051) | (0.021) |
| Observations | 71,622 | 9,470 | 62,152 | 24,190 | 47,273 |
| R-squared | 0.0241 | 0.0377 | 0.0232 | 0.0263 | 0.0268 |
| Interaction Difference | -0.042 | -0.228** | -0.028 | -0.003 | -0.066** |
| | (0.025) | (0.107) | (0.029) | (0.056) | (0.030) |

Notes. The dependent variable is the logarithm of the ratio of four times a household's quarterly non-housing consumption divided by annual household income. The sample includes all respondents identifying as residing in owner-occupied housing or paying cash rent in the 2006, 2007, 2008 and 2009 Surveys of Consumer Expenditures (CEX), and in the 39 states identified in the public release version of the CEX. The table presents OLS estimates from a single regression where the first panel presents the estimates for the subsample of owner-occupants and the second panel presents the estimates for the subsample of renters. The unemployment rate is based on the annual unemployment rate for each state averaged across all counties and weighted by county labor force using data from the Bureau of Labor Statistics, and the housing price index for each state is taken from the FHFA repeat sales price index based on home purchase mortgages averaged across all four quarters. The regressions also include dummy variables for racial identity, years of education, marital status, the number of individuals under the age of 18 residing in the household, the state of residence and the survey year. Standard errors are clustered at the state level.

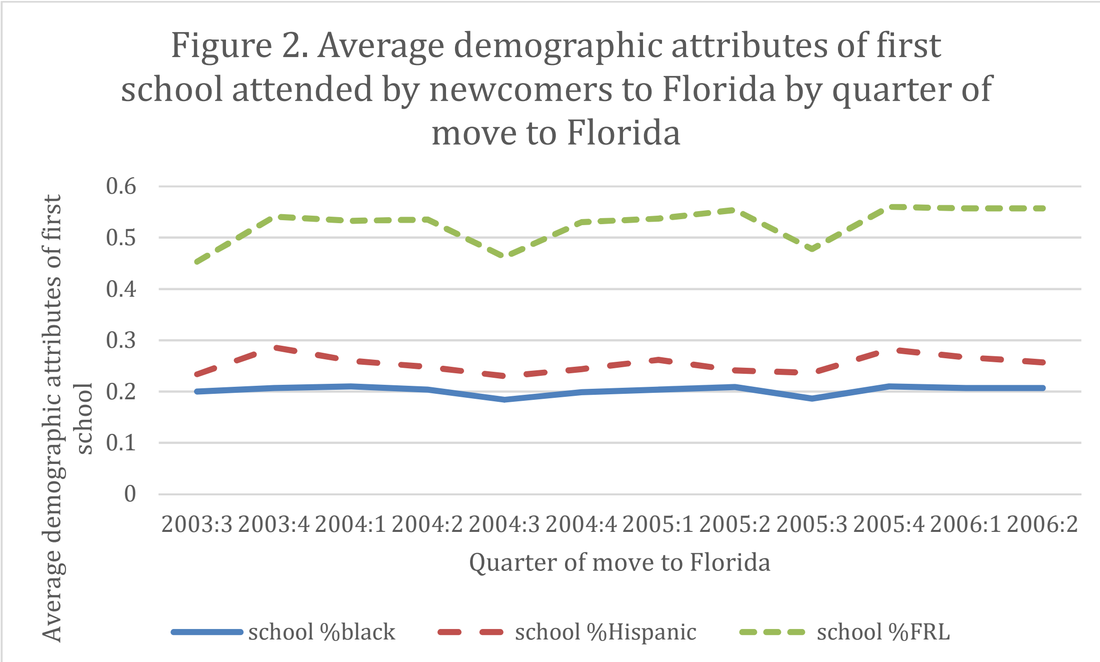
**Table 14: Estimated effects of loan to value ratio on grade of school attended**

| Dependent Variable | School grade (4-point scale) | Pr(better-graded school than 2005 school?) | Pr(worse-graded school than 2005 school?) |
|---|---|---|---|
| Full Sample | 0.122*** | 0.019** | -0.055*** |
| | (0.036) | (0.009) | (0.019) |
| Black | 0.127 | 0.013 | -0.049 |
| | (0.088) | (0.034) | (0.031) |
| Not Black | 0.122*** | 0.018** | -0.058*** |
| | (0.040) | (0.009) | (0.021) |
| Not free or reduced-price lunch | 0.125** | 0.025* | -0.051** |
| | (0.053) | (0.013) | (0.023) |
| Free or reduced-price lunch | 0.140*** | 0.012 | -0.071*** |
| | (0.041) | (0.008) | (0.025) |

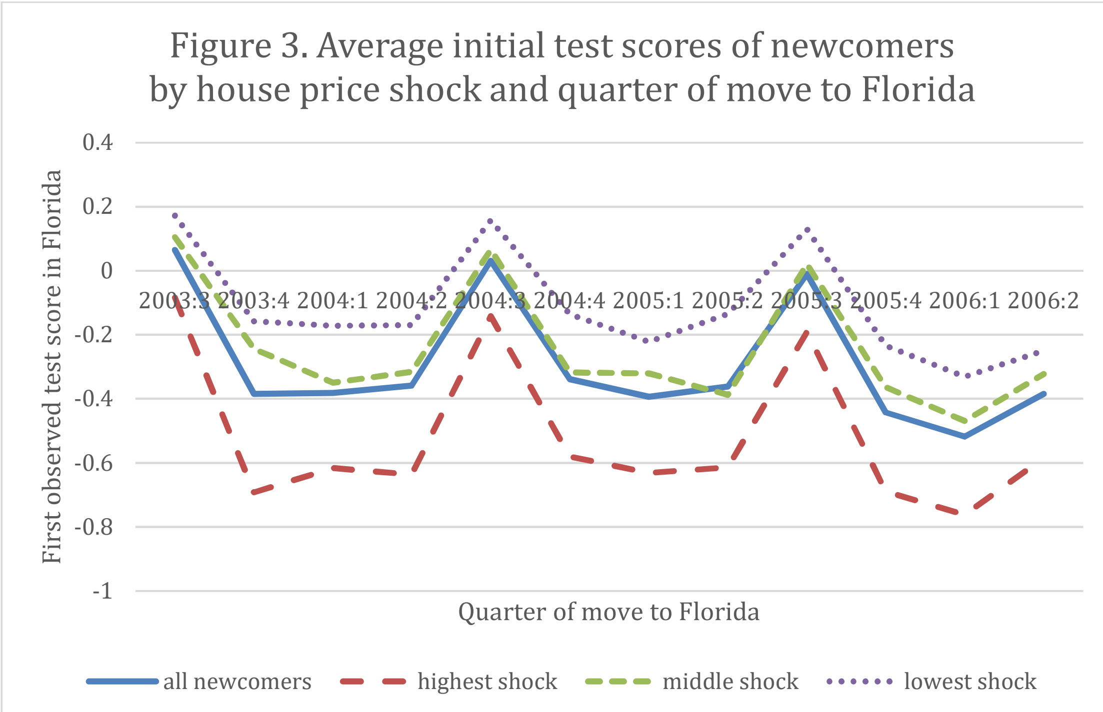
Notes: Regression of the student outcomes from newcomers to the state during the pre-crisis period on predicted LTV and the three vectors of pairwise fixed effects: student, school by county by crisis year, and arrival quarter and year by crisis year. Predicted LTV is based on a price index for the quarter the test is administered. Standard errors are clustered at the county by newcomer arrival year level.



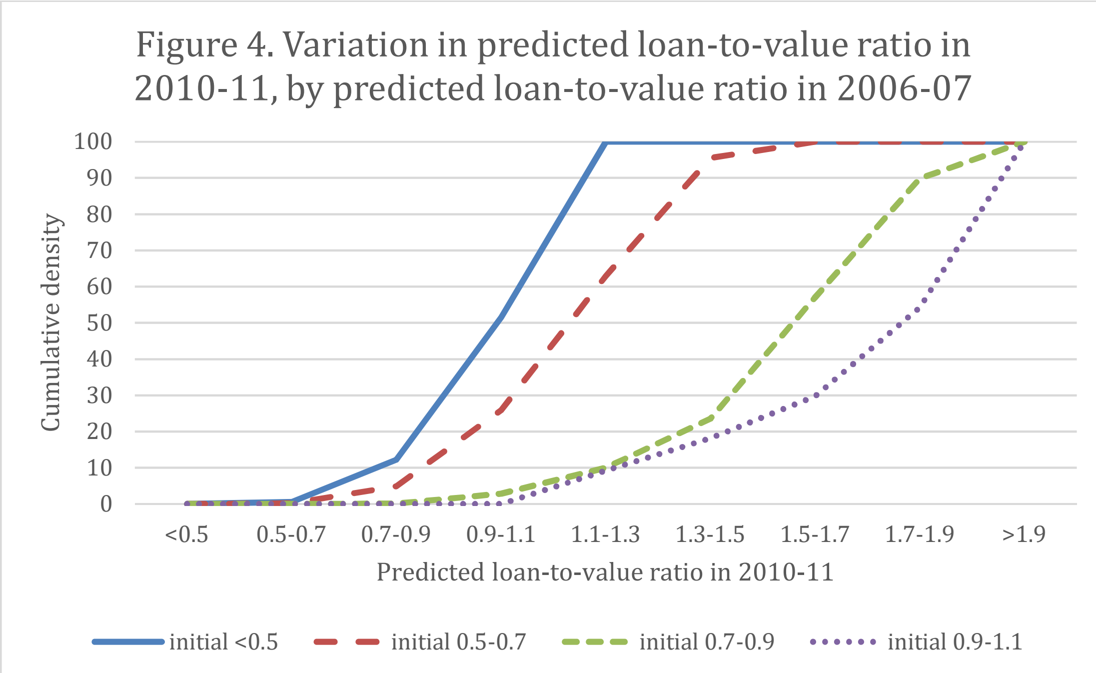

Notes: The figure presents the percentage increase (decrease) in housing prices between the third quarter of 2003 and the second quarter of 2006 (second quarter 06 and second quarter 11) as shaded (black) bars for Florida metropolitan areas based on repeat sales price indices from the Federal Housing Finance Agency.




Notes: The figure presents the fraction of newcomers to Florida schools between kindergarten and eighth grade that are black (solid), Hispanic (dashed) and free or reduced price lunch eligible (short dashed) by the quarter and year that the newcomers first appeared on the roster of Florida school students where the third quarter contains students who are first observed at the beginning of the school year or anytime in the month of September and the second quarter contains students who are observed at the end of the school year in April, May or June.




Notes: The figure presents the average of the first observed Florida standardized test score for newcomers to Florida schools between kindergarten and eighth grade where the average for the full sample is represented by a solid line, the subsamples in the counties facing the highest, middle and lowest housing price shocks are shown by the dashed, short dashed, and dotted lines, respectively. The average at each point along the horizontal axis is based on the quarter and year that the newcomers first appeared on the roster of Florida school students where the third quarter contains students who are first observed at the beginning of the school year or anytime in the month of September and the second quarter contains students who are observed at the end of the school year in April, May or June.




Notes: The figure presents the fraction of associated with a predicted Loan to Value (pLTV) ratio in the 2010-11 academic year at or below the range shown on the horizontal axis. These cumulative density functions are calculated for four subsamples: 2006-07 pLTV below 0.5 (solid), 2006-07 pLTV between 0.5 and 0.7 (dashed), 2006-07 pLTV between 0.7-0.9 (short dashed) and 2006-07 pLTV 0.09-1.1 (dotted).

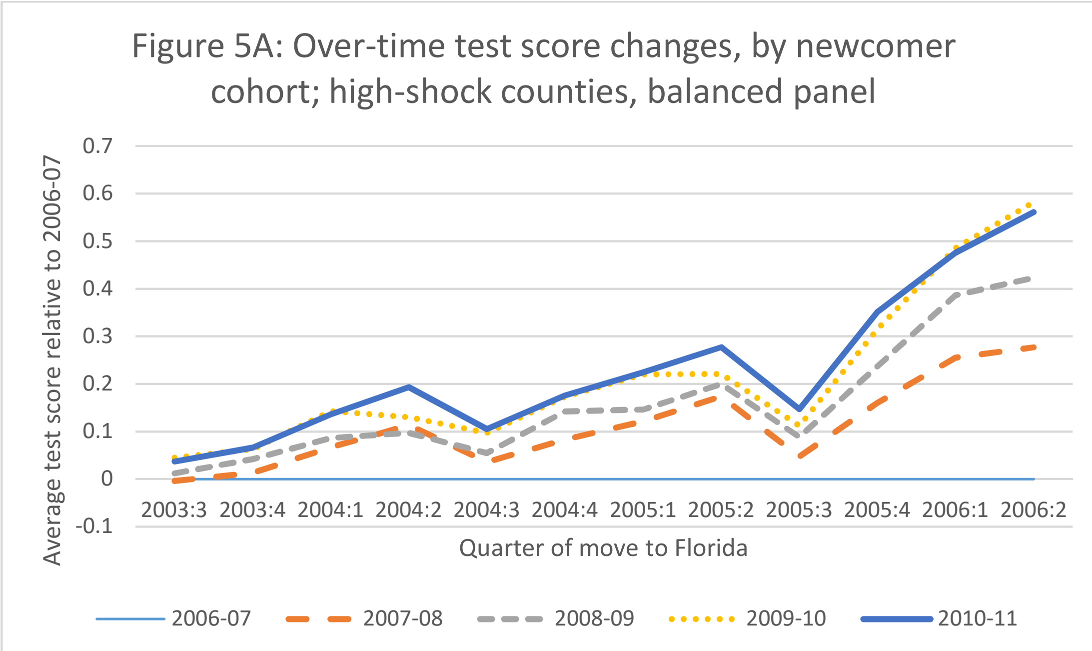


Figure 5A: Over-time test score changes, by newcomer cohort; high-shock counties, balanced panel
Average test score relative to 2006-07
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
-0.1
2003:3 2003:4 2004:1 2004:2 2004:3 2004:4 2005:1 2005:2 2005:3 2005:4 2006:1 2006:2
Quarter of move to Florida
2006-07
2007-08
2008-09
2009-10
2010-11

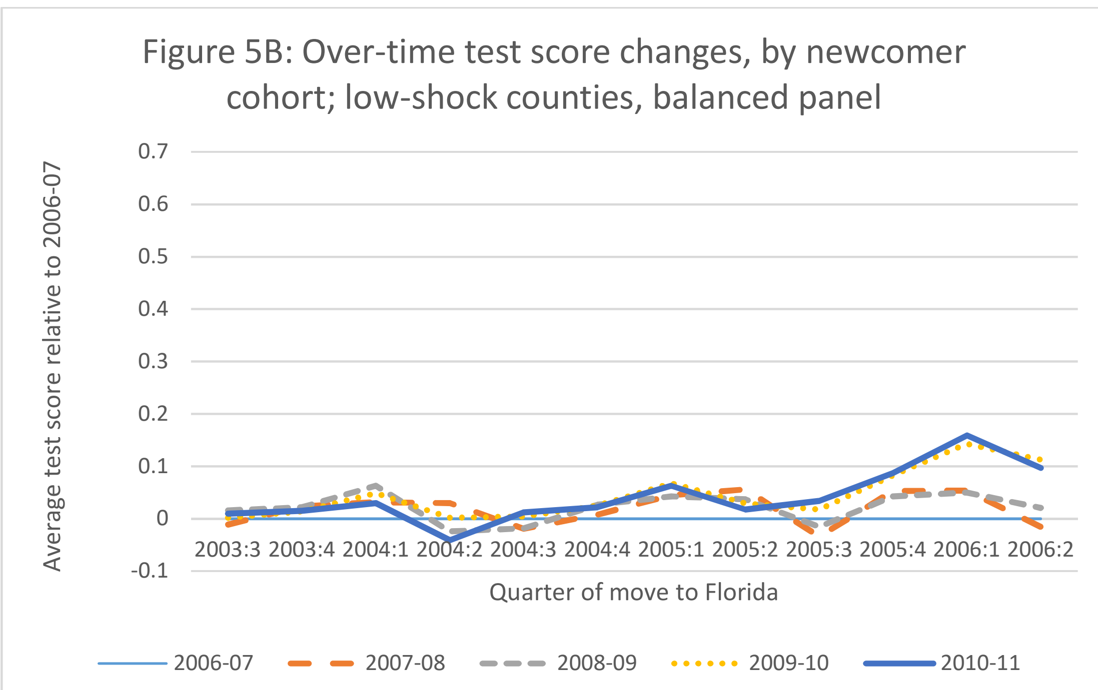
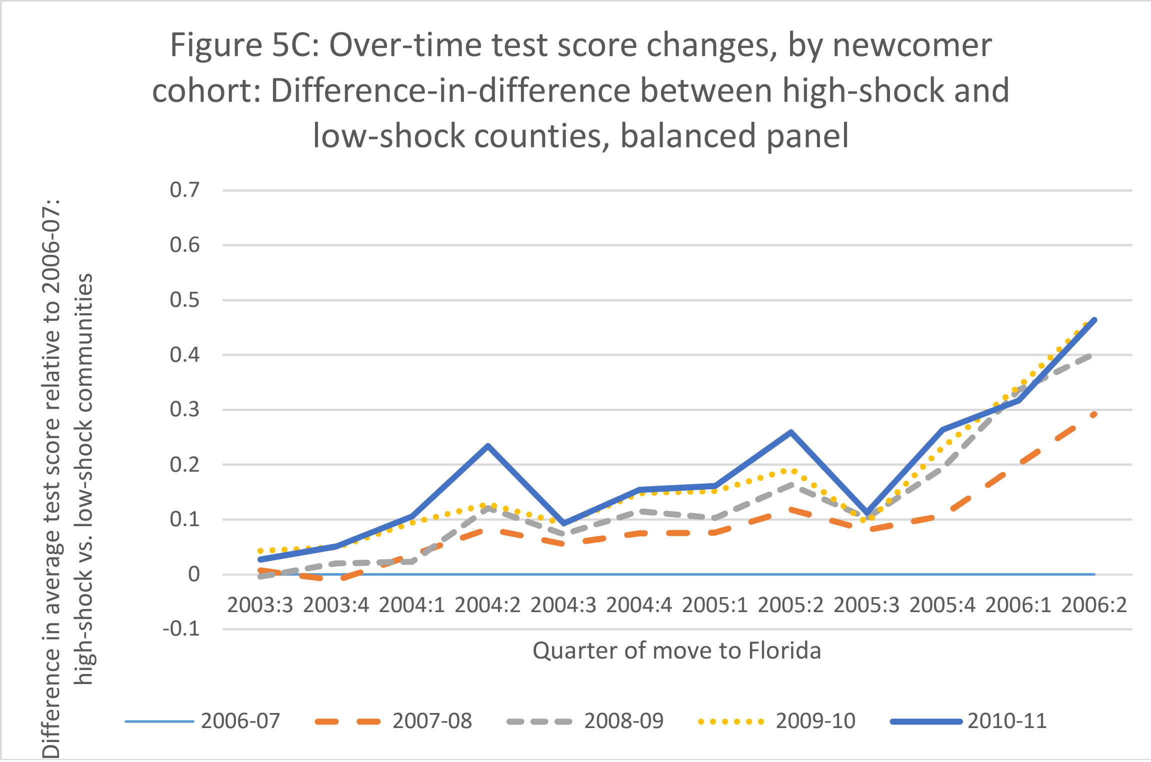
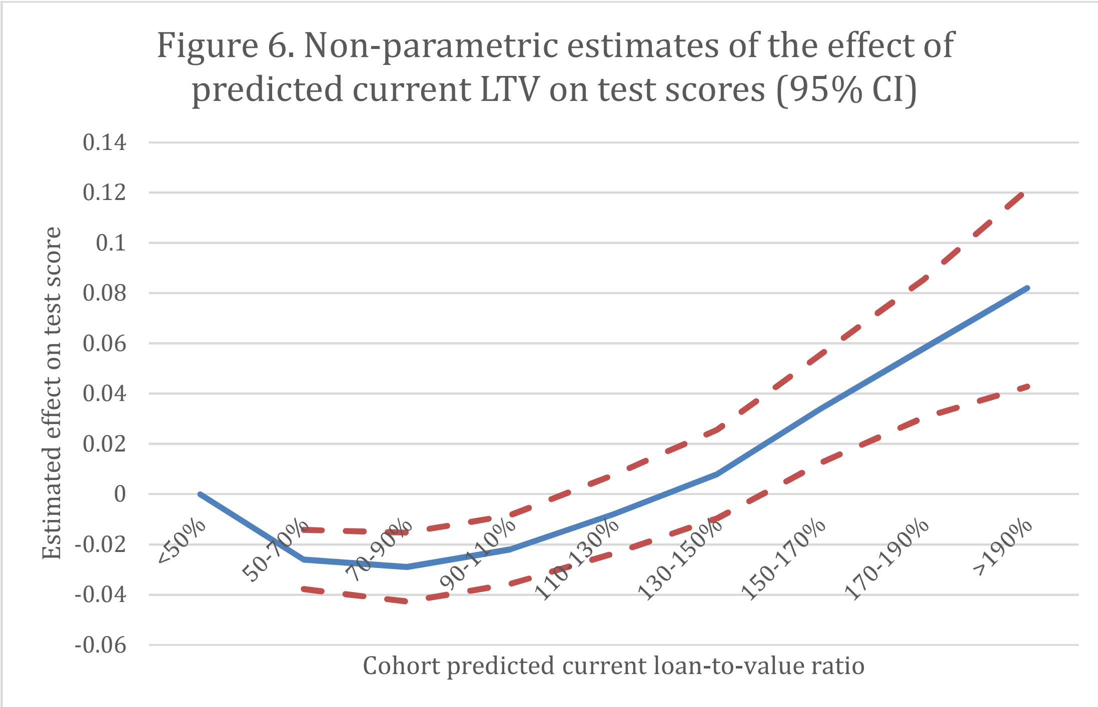


Figure 5B: Over-time test score changes, by newcomer cohort; low-shock counties, balanced panel
Average test score relative to 2006-07
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
-0.1
2003:3 2003:4 2004:1 2004:2 2004:3 2004:4 2005:1 2005:2 2005:3 2005:4 2006:1 2006:2
Quarter of move to Florida
2006-07
2007-08
2008-09
2009-10
2010-11

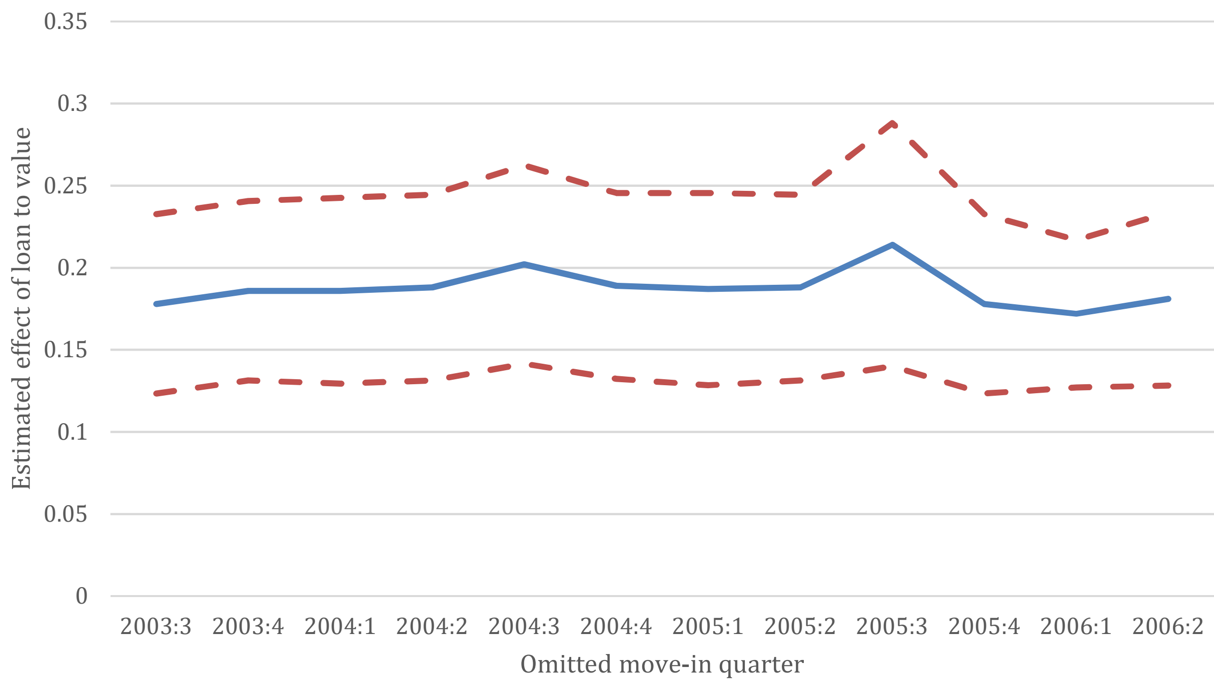



Notes: The figure presents the changes in average standardized test scores between the 2006-07 academic year (light solid horizontal line at zero) and later years during the crisis including 2007-08 (dashed), 2008-09 (short dashed), 2009-10 (dotted) and 2010-11 (dark solid) for newcomers to Florida schools between kindergarten and eighth grade. Each point along the horizontal axis represents the change in average test scores for a subsample of newcomers who arrive in Florida schools in that particular quarter and year where the third quarter contains students who are first observed at the beginning of the school year or anytime in the month of September and the second quarter contains students who are observed at the end of the school year in April, May or June. The first two panels present these changes for the high and low housing price shock counties, respectively, and the last panel presents the difference between the differences shown in the first two panels of the figure.




Notes: The figure presents the coefficient estimate and 95 percent confidence intervals for the same triple differenced model as used in Table 6 except that the linear control for predicted LTV is replaced by a series of predicted LTV bins as shown on the horizontal axis.

Figure 7. Estimated effects of loan to value ratio, omitting one move-in quarter at a time




Notes: The figure presents the coefficient estimates and 95 percent confidence intervals for the predicted LTV variable using different samples where each sample drops a single cohort of newcomers where cohorts dropped are defined based on the quarter and year of arrival, as shown along the horizontal axis.