Housing Booms and Busts, Labor Market Opportunities, and College Attendance[†]

By Kerwin Kofi Charles, Erik Hurst, and Matthew J. Notowidigdo*

We study how the recent housing boom and bust affected college enrollment during the 2000s. We exploit cross-city variation in local housing booms, which improved labor market opportunities for young men and women. We find that the boom lowered college enrollment, with effects concentrated at two-year colleges. The decline in enrollment during the boom was generally reversed during the bust; however, attainment remains persistently low for particular cohorts, suggesting that reduced educational attainment is an enduring effect of the recent housing cycle. The housing boom can account for approximately 25 percent of the recent slowdown in college attainment. (JEL I23, I25, J24, J31, R21, R31)

There is an active literature studying the consequences of the national boom and bust in housing that lasted from the late 1990s to the late 2000s, including an emerging body of work studying its effects on future economic growth. The creation of an "overhang" of debt that dampens future spending and investment is one possible mechanism by which the housing cycle may have affected future growth (Bhutta 2014; Jordà, Schularick, and Taylor 2014; Mian and Sufi 2014). Another possibility is that the cycle could have caused labor to be misallocated toward temporarily booming sectors with poor long-term growth prospects.[1] Curiously, how

Panel A. Men

Panel B. Women

FIGURE 1. FRACTION TO HAVE EVER ATTENDED COLLEGE AMONG PERSONS AGED 18–29, 1980–2013

Notes: This figure reports trends in the share of men and women (age 18–29) who have attended at least one year of college. This series is constructed from the Current Population Survey (CPS) using CPS survey weights. The dashed line is the predicted college attendance rates based on a quadratic trend that is fit to the 1980–1996 period.

the boom and bust may have affected the distribution of schooling in the population has received little attention in this literature, despite education's key role in determining future individual well-being and economic growth. This paper empirically assesses how housing demand shocks over the course of the housing cycle affected overall college attainment in the United States and adjudicates among alternative explanations for the patterns we document.

Suggestive evidence that the housing boom and bust changed individual schooling decisions comes from recent trends in overall college attainment that have received little attention. Using data from the Current Population Survey (CPS), the two panels of Figure 1 plot, separately for men and women, the share of persons aged 18–29 who reported ever having attended college. While the share of young adults who have ever gone to college rose slowly and steadily since at least 1980, there was a noticeable slowdown relative to trend, for both men and women, beginning in the late 1990s, precisely when the national housing boom started. The slowdown persisted through the peak of the national housing boom in 2006 and, despite some convergence during the bust period after 2006, attainment among young adult men and women had not fully reverted to trend as of 2013, years after the end of the housing cycle.

In their seminal work on the much larger slowdown in attainment that occurred before the period we study, Goldin and Katz (2010) show educational attainment by birth year cohort up through the 1975 cohort. We follow their specification and use CPS data between 1994 and 2014 to examine college attainment for year-of-birth cohorts from 1960 to 1990.[2] The year-of-birth effects from the Goldin-Katz-style regression models are plotted in the two panels of Figure 2, which measure the predicted fraction of a birth cohort with any college training by age 25. By extending

---

[2] These results can be interpreted as extending the Goldin and Katz (2010) results to birth cohorts after 1975, although their measure of college training is college degree completion rather than our measure of having attended college at all. The "second slowdown" in attainment that we focus on in this paper is much smaller than the very large slowdown from earlier in the century identified in earlier work. We pool the 1994 to 2014 waves of the CPS, restricting the sample to persons aged 25–54 in each year. We then estimate regressions for men and women separately on a dummy variable for whether the person has ever attended college on year-of-birth dummies, a quartic in age, and normalized year fixed effects where the first and last year effect are set to zero (as in Hall 1968).
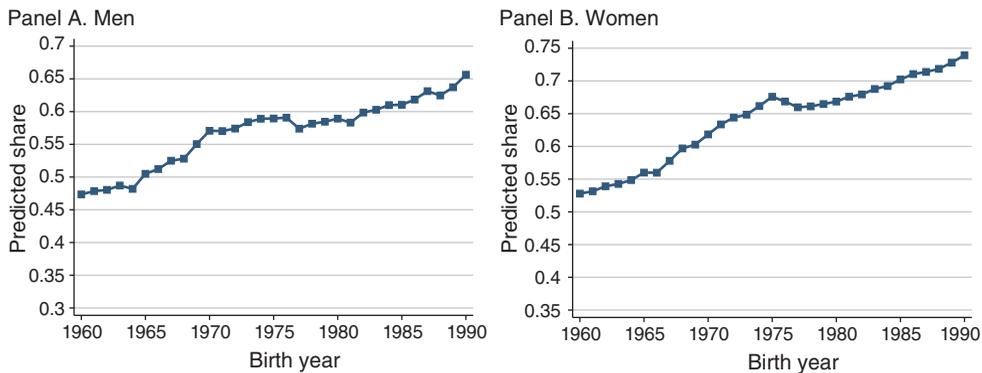
FIGURE 2. FRACTION TO HAVE EVER ATTENDED COLLEGE BY AGE 25 BY YEAR OF BIRTH FOR MEN AND WOMEN BORN BETWEEN 1960 AND 1989

Notes: This figure reports estimated birth year (birth cohort) fixed effects in education for all men and women born between 1960 and 1990 (inclusive). The sample is all individuals between the ages of 25 and 54 (based on age in survey year), pooling CPS datasets between 1994 and 2014. The birth year fixed effects are recovered from an estimated model that regresses an indicator for whether individual has attended any college on a fourth-degree polynomial in age, birth year fixed effects, and normalized year fixed effects (setting the first and last year fixed effect equal to zero and the sum of remaining year fixed effects equal to zero). The figures reported fitted values at age 25 using CPS survey weights. The sample is restricted to native-born men and women.

their work, we document that a "second slowdown" in attainment occurred somewhere between the 1970 and 1980 birth cohorts. For men, after steady cross-cohort growth of about 10 percentage points between the 1960 and 1970 cohorts, cohort-specific attainment rates were flat for the next 10 birth cohorts, before starting to rise again. For women, the slowdown started around the 1975 birth cohort. Growth in cohort-specific attainment rates for the 15 cohorts born after 1975 was about one-third the growth in cohort-specific rates for the 15 cohorts born before 1975, with the cohorts between 1974 and 1980 essentially experiencing no growth in college propensity. Although the slowdown in cohort-specific attainment by age 25 roughly lines up with the start of the boom and bust, the figure also suggests that at least some of the slowdown had nothing to do with the housing cycle since it began with cohorts that had already turned 25 before the boom began.

Figure 3 provides an initial assessment of whether the housing boom is related to the second slowdown in educational attainment. We combine the 1990 and 2000 Censuses with the 2005–2013 waves of the American Community Survey (ACS), and restrict attention to persons in this sample from the 1965–1987 birth cohorts living in their state of birth who were between the ages of 25 and 54. We then compute the share of the birth cohort who attended college, separately by whether the individual was living in a metropolitan statistical area (MSA) that was in the top tercile of the increase in housing prices between 2000 and 2006 as measured by the FHFA housing price index. The figure shows no differences in college attainment by cohort across the two groups of MSAs from the 1965 cohort through the first several cohorts of the slowdown. However, beginning with the 1979 birth cohort, who would have been 18 when the national boom began and thus at the cusp of making college-going decisions, rates for persons in MSAs with especially big price increases fell behind rates for persons from the same birth cohorts in other markets.
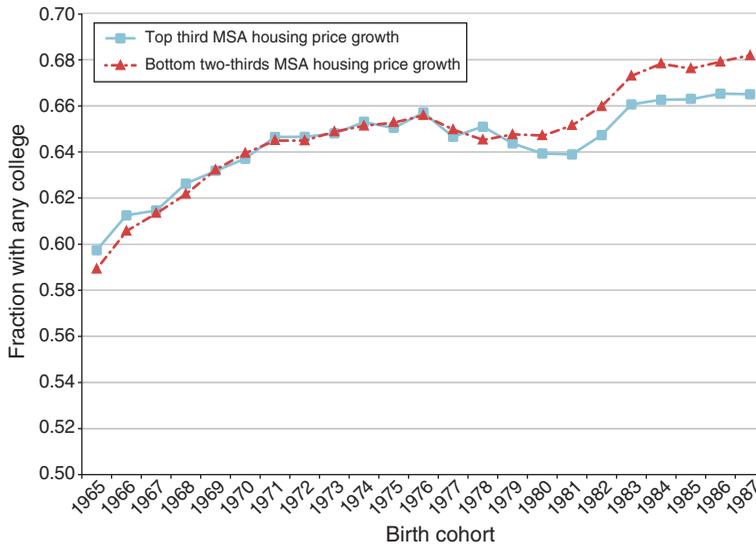
FIGURE 3. SHARE ANY COLLEGE ATTENDANCE FOR INDIVIDUALS BORN BETWEEN 1965 AND 1987, BY MSA HOUSE PRICE GROWTH

Notes: This figure reports education attainment by birth cohort for all men and women born between 1965 and 1987. The data come from the 1990 Census, the 2000 Census, and the 2005–2013 American Community Survey (ACS). The sample is restricted to all men and women between ages 25 and 54 in the survey year. The two lines are subsamples of metropolitan areas based on whether the metropolitan was in the top tercile of distribution of house price changes between 2000 and 2006. We use FHFA house price data to compute MSA-level house price changes. The data use Census/ACS survey weights.

The difference in the propensity to attend college grew to a full 2 percentage points for the 1983 cohort.

The patterns in Figures 1–3 indicate that there was a slowdown in college attainment for individuals of college-going age during the housing boom and presents the possibility that the housing boom may have reduced college attainment among both men and women. By what mechanism could this have occurred? To answer this question, we develop a simple conceptual model of college-going which shows that there are several different mechanisms through which a housing boom might affect educational attainment. Since these effects are not of the same sign, and are differentially important for different population subgroups, the overall effect of a housing boom is theoretically ambiguous. We show, however, that a boom will tend to lower attainment if it improves current labor market opportunities for young adults so much that the labor market opportunity costs of college-going (the earnings they must forgo to acquire a college education instead of working) become large enough to override any other effect of the boom that might act in the other direction, such as changes in tuition or the loosening of liquidity constraints (Deming and Dynarski 2010; Lovenheim 2011). Our conceptual framework shows that, all else equal, the housing boom should affect most the students on the margin of going to college at all (e.g., getting an associate's degree) and have little effect on investment in bachelor's-level training. Another insight from our framework is that the decision to not attend college in a given year due to a housing boom may be persistent because the time available to receive
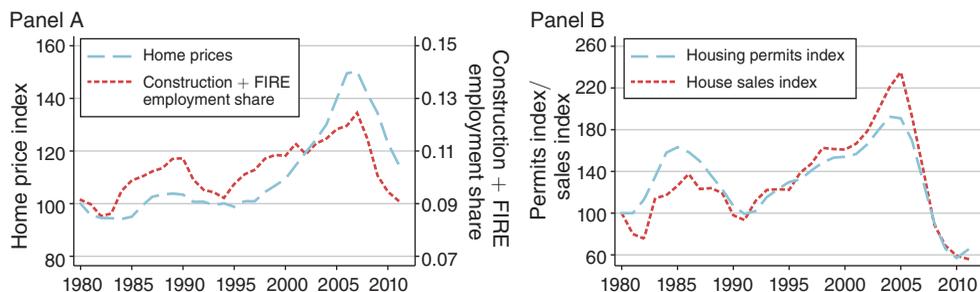
FIGURE 4. HOME PRICES, CONSTRUCTION/FIRE EMPLOYMENT, HOUSING PERMITS, AND HOUSING TRANSACTIONS IN
THE UNITED STATES, 1980–2012

Notes: These figures report trends in the FHFA National Home Price Index (1980 = 100), trends in the share of population employed in construction and FIRE (finance, insurance, and real estate), trends in the Census Housing Permits Index (1980 = 100), and trends in total new home sales from the Survey of Construction (1980 = 100). The FHFA series is a weighted, repeat-sales index that measures average changes in house prices across 363 metropolitan areas. The Census series is a building permits survey that estimates the number of new housing units (as authorized by building permits). The Survey of Construction series measures new house sales of single-family homes, whether or not building new homes in those areas requires a building permit. The population employment shares are calculated from Current Population Survey and are based on all prime-age men and women (age 18–55).

the gains from college shrinks while other costs of college attendance, such as family obligations, may rise with age.

Figure 4 plots the massive changes over the housing cycle in four measures that affect labor market opportunities for persons without college training: housing prices; housing production, as measured by new residential construction permits; total housing transactions; and employment in construction and FIRE sectors (Finance, Insurance, and Real Estate). The increases in housing production depicted in Figure 4 would not have been possible without a substantial growth in labor market opportunities in construction-related activities. Similarly, the massive surge in the number of houses bought and sold shown in the figure must have necessitated substantially greater activity in fields like real estate services, which has a large share of non-college-educated adults. Beyond these two specific sectors, demand for workers providing local nontradable services, like waitresses, gardeners, and hairdressers, has been shown to vary positively with changes in housing prices (see, e.g., Mian and Sufi 2014). Taken together, these patterns suggest that the housing demand changes during the boom may have substantially raised the opportunity costs of college-going for both young adult men and women, and could possibly partly explain the overall slowdown shown in Figures 1–3. Our main analysis assesses how MSA-level local housing demand shocks during the boom and bust affected educational attainment and labor market conditions. We use as a proxy for local housing demand the sum of changes in both local housing prices and quantities. To account for potential measurement error and endogeneity in our proxy, we isolate exogenous variation in local housing demand.

Our approach relies on the emerging consensus that much of the variation in housing prices during the boom and bust derived from a speculative "bubble" and not from changes in standard determinants of housing values such as income, population, or construction costs (Shiller 2008; Mayer 2011; Sinai 2012). Specifically, building on the work of Ferreira and Gyourko (2011) we estimate

structural breaks in the evolution in housing prices in an MSA using standard techniques from time series econometrics (Bai 1997; Bai and Perron 1998). We assume that these "sharp breaks" are exogenous to local latent confounds, such as labor supply shocks or unobserved changes in labor demand, which are likely smoothly incorporated into price changes.[3] The estimated breaks are not, in fact, systematically related to pre-period levels and changes in many observable local characteristics, and we provide several pieces of evidence consistent with them being the result of speculative activity. We also use the estimated timing and magnitude of the structural break in local house prices to carry out an "event study" analysis of employment and college attendance. The event study estimates are useful because they confirm an "on impact" effect of a sharp change in housing demand on both labor market outcomes and schooling decisions.

Beginning with labor market outcomes, we find that increases in housing demand in an MSA during the 2000–2006 boom increased employment and wages for both young adult men and women without college training, raising their opportunity cost of college-going. Among young adult men, much of the improvements in labor market opportunity occurred in construction, whereas for young women the FIRE sectors of finance, insurance, and real estate accounted for much of the gains. We also find that the boom either had no effect on, or perhaps slightly lowered, the expected future college/non-college earnings premium.

We present results for college attainment that are based on a variety of complementary data sources and different estimation methods. First, using data from the Census and ACS, we relate the 2000–2006 change in an MSA in attainment among young adults aged 18–25. Both ordinary least squares (OLS) and two-stage least squares (2SLS) estimates show that the growth in the fraction of young adults with any college training was lower the larger the MSA's housing boom. Strikingly, we find no evidence that the change in an MSA's housing demand during the boom had an effect on the change in the fraction of young adults with a bachelor's degree. The results suggest that improving labor market opportunities during the boom decreased advanced schooling attainment precisely for those persons who our conceptual model suggests should have been on the margin between obtaining associate's-level training and not going to college at all. We find similar results using rich administrative enrollment data in the Integrated Postsecondary Education Data System (IPEDS), with statistically significantly lower growth in enrollment in two-year colleges the larger the housing demand growth in the MSA, but no difference in the change in enrollment in four-year, bachelor's-degree-granting institutions over the same time.

When estimating the magnitude of the structural break in an MSA, we also identify the precise timing (year and quarter) when the break is estimated to have occurred. Using these two pieces of information and exploiting the annual frequency of the IPEDS data, we estimate that two-year college enrollment was lower after the specific year when an MSA had its structural break and was increasing in the size of the break, and we estimate no significant effect for four-year colleges and universities. We also provide "event study" estimates that show clear visual

---

[3] Our econometric approach is similar in spirit to recent work which uses structural break techniques, such as Card, Mas, and Rothstein (2008) on racial tipping points and the work of Lafortune, Rothstein, and Schanzenbach (2016), which estimates sharp breaks in school finances coming from major legal reforms.

breaks from trend in two-year enrollment right around the time of the structural break.

For our third set of education results, we obtained permission for the restricted-use version of the National Longitudinal Survey of Youth (NLSY97), a panel dataset which follows a nationally representative sample of young adults who reached late adolescence and early adulthood during the early 2000s, right around the beginning of the national boom. With observations on only a few thousand individuals, this dataset is very small compared to the Census/ACS dataset. However, the individual panel feature of the dataset allows us to track specific individuals as they age, with exact information about their MSA at each point of the housing cycle. This dataset allows us to assess bias from endogenous migration, and also contains a rich set of individual- and family-level controls not present in the other data sources. We find that NLSY97 respondents living in MSAs that experienced especially large housing demand shocks were less likely to have obtained any college training at all by 2006, but did not differ from people in other markets in their propensity to have obtained a bachelor's degree.

We next present a set of results concerning educational attainment over the housing bust and over the full course of the boom and bust cycle, all of which are consistent with predictions of our conceptual model. First, we find that the bigger the growth in an MSA's housing demand during the boom (and thus the larger its decline during the bust), the larger the *increase* in attainment between the generation of young adults who made their schooling decisions at the peak of the boom to the generation of young people whose college decisions were made after the bust. Second, we show that once the housing cycle had ended, new generations of young adults in boom markets appear to be investing in all types of college training no differently to young adults in those markets before the boom and bust cycle began. These results show that the decline in educational attainment that resulted during the housing boom was reversed during the housing bust.

Our empirical work concludes with an assessment of persistence: whether the *particular generation* of young adults who obtained less schooling during the boom reversed this pattern by obtaining more schooling during the bust as labor market opportunities collapsed. Was the reduction in attainment we find for these particular people during the boom merely a delay, or does their schooling reduction seem to be permanent? The evidence from both Census/ACS and from the individual panel NLYS97 data is that young adults who invested less in college during the nearly ten years of the housing boom did not make up their lost college-going propensity during the bust. While our precision is limited in both datasets, we find suggestive evidence that these cohorts have experienced a sort of "educational scarring" whereby their rates of attainment are permanently lower than would have been true had there been no boom. Their reduced educational attainment appears to be an enduring effect of the boom and bust cycle.

Our work is related to three distinct literatures. First, building on insights from existing models (Mincer 1958; Becker 1964), various authors have shown that labor market conditions affect college attainment (Betts and McFarland 1995; Black, McKinnish, and Sanders 2005; Atkin 2016; Cascio and Narayan 2015). We extend this literature by identifying effects for different types of colleges and universities, and by distinguishing between permanent and temporary investment responses. A second literature our work extends is the work that documents and tries to explain

longer-term changes in educational attainment in the economy (Goldin and Katz 2010). Interestingly, while the slowdown in attainment in the early 1970s has been studied, ours is the only paper of which we are aware that documents and offers a partial explanation for a "second slowdown" in college-going among both men and women starting in the mid-to-late 1990s. The third, and perhaps most important body of work the paper extends, is the literature attempting to understand the real effects of the recent boom and bust in housing, arguably the defining macroeconomic event of the early twenty-first century. Our work adds educational attainment to the set of real outcomes shown to have been possibly affected by the housing cycle, including consumption, employment, defaults, political outcomes, health, and fertility (Mian, Rao, and Sufi 2013; Mian and Sufi 2011, 2014; Mian, Sufi, and Trebbi 2010; Currie and Tekin 2015; Lovenheim and Mumford 2013). Our results suggest that, by altering schooling choices, housing booms may reduce aggregate human capital, potentially reducing both labor productivity and employment probabilities for years to come.

I. Theoretical Overview

To motivate our empirical work, we develop a simple conceptual model that illustrates how housing demand shocks affect college-going by exploring the key considerations emphasized in existing models of human capital investment. We highlight, in particular, the effect of shocks on potential students' opportunity cost of college attendance (Willis and Rosen 1979; Cameron and Taber 2004).

The potential students in our framework are young adults who have completed the minimum required amount of schooling and now can either participate in the labor market or attend one of the two types of colleges, c, in the economy: "associate's" colleges ($c = A$), or "bachelor's" colleges ($c = B$). Young adults differ in academic ability θ_i, which is distributed smoothly over the interval $[0,1]$. As a college student, a young person incurs psychic costs of learning given by $\kappa_c(1 - \theta_i)$. Training in a type-B college is inherently more difficult, and especially so for less able students, so $\kappa_B > \kappa_A$. It costs F_c in annual fees and tuition for type-c colleges, and students can borrow at an interest rate of b. In any period t, labor market participants with and without college training receive labor market income of Y_t^c and Y_t^0, respectively, which vary from one period to the next because of macroeconomic and other shocks. The college premium in a given period for persons educated at a given type of college c is thus $\prod_t^c = Y_t^c - Y_t^0 \geq 0$. We define the lifetime payoff that a person of ability θ_i gets from attending a type-c college in period t as $R_{it}^c(\theta_i)$.[4]

We focus on equilibria where some young adults enroll in each of the two types of college, and others do not attend college at all.[5] Figure 5 illustrates an equilibrium in this case. The payoff functions for the two types of colleges are negative at the lowest

---

[4] We normalize $R_{it}^0(\theta_i) = 0$ and define $R_{it}^c(\theta_i) = \sum_{k=1}^{L-t} E_t[\prod_{t+k}^c] - (1+b)F_c - \kappa_c(1-\theta_i) - Y_t^0$, where the first term is the expected future premium from college between current period t and retirement (L), and the last three terms represent the direct out-of-pocket, psychic, and opportunity cost of college, respectively.

[5] The conditions on the payoff functions for the two types of colleges to cross at some point when both become positive are the following:

$$0 > R_{it}^A(\theta_i = 0) > R_{it}^B(\theta_i = 0) \quad \text{and} \quad 0 < R_{it}^A(\theta_i = 1) < R_{it}^B(\theta_i = 1);$$

$$\overline{R} = R_{it}^A(\theta^{AB}) = R_{it}^B(\theta^{AB}) > 0.$$
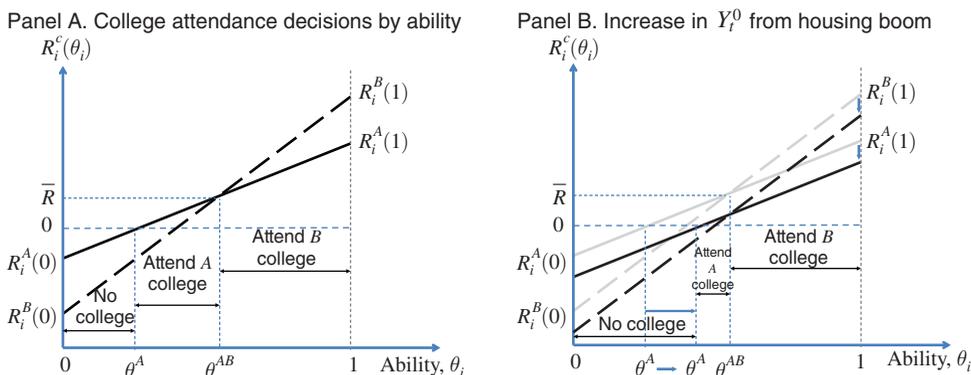
FIGURE 5. AN ECONOMIC MODEL OF COLLEGE ATTENDANCE

Notes: This figure shows the equilibrium college attendance decisions for individuals as a function of their underlying ability. The equilibrium shows individuals making choices of whether to attend college (and, if so, whether to attend type-*A* "associate's" college or type-*B* "bachelor's" college). In the equilibrium, individuals sort based on comparative advantage, with low-ability individuals not attending college, middle-ability individuals attending type-*A* college, and high-ability individuals attending type-*B* college. In panel B, there is a positive housing demand shock (housing boom), which raises income of all non-college-educated individuals by the same amount. In the new equilibrium, there is a reduction in share of population attending type-*A* college but no change in share attending type-*B* college.

levels of academic ability, with the lower intercept for the "bachelor's" colleges indicating the greater inherent difficulty of that type of college. The functions strictly increase with ability, with the steeper slope for the "bachelor's" function indicating the larger marginal benefit for this type of college for individuals with higher academic ability. Both functions eventually become positive, with the flatter "associate's" function becoming positive at a lower level of ability θ^A than the corresponding ability level for the "bachelor's" function. The two functions eventually intersect at the value $\overline{R} > 0$ at ability level θ^{AB}. A person of ability θ^A is just indifferent between attending "associate's" college and not going to college at all, and someone with ability θ^{AB} is just indifferent between going to "bachelor's" and "associate's" college. These two thresholds characterize college-going in the population in our setup.

The effect of any shock on average college-going in the population is determined by how the shock shifts payoff functions and thus the two threshold ability values. The sign and magnitude of the shift in the payoff function for type-*c* colleges from a housing demand shock, dH, is the sum of four separate effects:

$$(1) \qquad \frac{dR_{it}^c (\theta_i)}{dH} = \frac{d\sum_{k=1}^{L-t} E_t \left[\prod_{t+k}^c\right]}{dH} - \frac{db}{dH} F_c - b\frac{dF_c}{dH} - \frac{dY_t^0}{dH}.$$

The first term in (1) measures how a housing demand shock affects the expected future premium from having attended college *c*. The second term is the change in borrowing cost from the rising housing wealth associated with the housing boom. The third term is the change in the cost of college arising from the housing demand shock. The fourth term, which is the main focus of our paper, is the effect of housing shocks on potential students' opportunity costs.

If housing demand shocks increase the labor market income that a young adult forgoes by enrolling in college in period t, then, all else equal, college attendance becomes less appealing. This is reflected in the downward shift in the payoff function for both types of colleges shown in panel B of Figure 5 from increases in Y_t^0. The ability threshold defining the marginal college-goer, θ^A, rises, and some students who would have gone to an "associate's" college now forgo college altogether. By contrast, a marginal "bachelor's" student is not changed by a rise in Y_t^0. This result follows from the fact that a young person in our framework forgoes the same amount in labor market income whether he attends a type-A or a type-B school in a given period, which means that the relative attractiveness of attending one type of college versus another does not depend on the opportunity cost, Y_t^0. The two payoff functions shift downward by the same amount and the threshold for "bachelor's" college-going θ^{AB}, is unchanged.[6] Overall, the model therefore captures the intuitively appealing idea that an increase in the opportunity cost of college attendance should have a greater effect on the propensity of individuals to pursue an associate's degree as opposed to a bachelor's degree. We would expect to see these enrollment responses unless there were offsetting influences from the other three effects in (1), which we discuss in the remainder of this section.

If young adults believe that a boom today will have persistent effects, affecting the labor income of college-educated and non-college-educated persons in the *future*, when today's young adults are older, the sign of the first term in (1) will depend on people's beliefs about the relative size of the effect of the boom on future skilled versus future less-skilled labor income. Only if it is expected that the boom will increase the future labor income gap between college- and non-college-educated people could the first term possibly override the opportunity cost effect. Otherwise, an expected decline in the future college earnings premium will complement and reinforce the opportunity cost mechanism.

An effect that could, in principle, offset the negative opportunity cost mechanism is if the positive shock to housing values and family wealth reduces borrowing costs or relaxes liquidity constraints. The existing evidence of the importance of liquidity constraints is mixed. Work by Cameron and Taber (2004) suggests that in the United States most persons wishing to attend college are not liquidity constrained, which is consistent with more recent work by Hilger (2014) and Bulman et al. (2016). By contrast, Manoli and Turner (2016) find evidence that tax refunds have meaningful effects on college enrollment, and Lovenheim (2011) finds some evidence of increased college attendance among persons from low-income families experiencing increases in housing wealth during the boom.

As indicated by the third term in equation (1), the housing boom could also affect the cost of college. We report in the online Appendix that tuition rose nationally during the most recent housing boom, which would be expected to reduce college enrollment given existing empirical evidence (Deming and Dynarski 2010). Although such national trends are not the focus of our analysis, changes in tuition

---

[6] The formal statements of the claims in this paragraph are that $d\theta^A/dH > 0$ and $d\theta^{AB}/dH = 0$. If $dR^A(\theta)/dH = -dY_t^0/dH < 0$, then the first statement is true because $dR^A(\theta)$ is strictly increasing in θ. The second claim is true since θ^{AB} is implicitly defined by $R^A(\theta^{AB}) = R^B(\theta^{AB})$ and equation (1) implies that $dR^A(\theta)/dH = dR^B(\theta)/dH = -dY_t^0/dH$.

at a local level could play a role in our results. At a local level, the effect of housing booms on tuition is ambiguous: if the housing boom raises average wages, this could increase labor costs and increase tuition; on the other hand, housing booms could reduce demand for college through the opportunity cost channel, and this could reduce local tuition. We investigate this channel directly by estimating the effect of housing demand shocks on average tuition at local colleges, and we find no evidence that housing booms affect average tuition.

Overall, our empirical analysis compares the change in average outcomes *across* different MSAs, and our estimates therefore capture the opportunity cost differences between MSAs as well as any borrowing/liquidity/tuition effects. Under the assumption that local housing booms do not meaningfully affect tuition, then our estimates can provide a measure of relative importance of opportunity costs relative to borrowing/liquidity effects. In particular, if increases in housing demand cause aggregate reductions in college enrollment, then this would imply that the opportunity cost mechanism was large compared to any liquidity effect the boom might have caused.

Lastly, our conceptual framework suggests that any reductions in educational attainment from positive housing demand shocks at a point in time could, for some persons, represent permanent reductions rather than temporary delays. As in every life-cycle human capital model, young adults in our conceptual model are less likely to invest in schooling the older they get because their horizon to receive the expected lifetime earnings premium from college training shrinks over time.[7] One implication of this mechanical effect of aging is that if a share ds of the population decides not to enroll in associate's colleges in a given period as a result of a housing boom, then it is unlikely that the entire mass ds will decide to enroll in a subsequent period if there is a negative housing demand shock that is equal in size to the preceding boom. Graphically, this mechanical aging effect causes the payoff functions in Figure 5 to shift vertically downward each period as the person ages. The upward vertical shift in the two payoff functions caused by a housing bust would move them to a lower intersection point than where they intersected before a preceding boom of equal size.

Before turning to the main education results that will be interpreted using the model in this section, we first offer direct evidence about how the opportunity cost of and the expected future college earnings premium from attending college were affected by local housing demand shocks.

II. Local Housing Demand Shocks

Our empirical work exploits variation across MSAs, k, in the size of the housing demand shock that the MSA experienced during the national housing boom and bust. Many papers in the recent literature have concluded that the housing boom during the 2000s was caused primarily by large changes in housing demand (see, e.g.,

---

[7] This is only one reason for the age effect in human capital models. In addition, life events like marriage, the birth of children, infirmity of parents, consumption and expenditure commitments, and any number of similar events are all more likely to have occurred at older ages, reducing the likelihood of college-going or indeed of any type of human capital investment. This would be straightforward to capture in our framework by allowing the psychic costs of college attendance to vary with age as well as ability.

Shiller 2008). Furthermore, there is an emerging consensus that different MSAs in the United States experienced different house price appreciations during the boom primarily because of a combination of differences in the magnitude of changes in local housing demand (Davidoff 2016; Ferreira and Gyourko 2011) and differences across MSAs in the local housing supply elasticity (Mian and Sufi 2011).

To create a measure of local housing demand shocks, consider a log-linear model of housing demand and housing supply. A local housing demand shock, ΔH_k^D, produces both a housing price and quantity change given by

$$(2) \qquad\qquad \Delta H_k^D \, = \, \eta_k^D \Delta P_k + \Delta Q_k,$$

where ΔP_k is the change in the log of local housing prices in MSA k, η_k^D is the price elasticity of housing demand, and ΔQ_k is the change in log of new housing produced. Using the fact that existing estimates of the elasticity of housing demand in the literature suggest that $\eta_k^D \approx 1$, we create a proxy for the change in local housing demand over any two periods, $\widehat{H_k^D}$, as simply the sum of the log difference in local housing prices and the log difference in new housing produced in the MSA.[8]

Our proxy for changes in housing demand is a function of both changes in local housing prices (ΔP_k) and changes in local housing supply (ΔQ_k). Theory says both changes in housing prices and changes in housing supply should affect local labor markets. Increases in housing supply can directly stimulate the local construction industry. Increases in housing prices can stimulate local employment through either a housing wealth effect on consumer spending or through a relaxation of liquidity constraints (Mian and Sufi 2014). Additionally, both the housing price and housing supply channels can increase the volume of housing transactions which stimulates sectors associated with the selling and financing of housing (e.g., mortgage brokers, real estate agents, etc.). This discussion makes clear that it is theoretically ambiguous whether the housing quantity effect on local labor markets is weaker or stronger than the housing price effect on local labor markets. In our baseline specification, we combine the two effects together into one metric, which implicitly assumes that the labor market effects are similar. We describe evidence below which suggests that this assumption is approximately true in our setting.

We use local housing price information from the Federal Housing Finance Agency (FHFA) annual series on prices in FHFA metro areas. We measure local housing supply by the number of new, privately owned housing units authorized via permits within the market.[9] We match information on building permits from the

---

[8] The assumption of a unitary housing demand elasticity comes from taking the average of two widely-cited estimates of the housing demand elasticity in the literature: 0.7 from Polinsky and Ellowood (1979) and 1.2 from Houthakker and Taylor (1970). If the housing demand elasticity varies across cities, this may cause bias in our OLS results, which assume that the housing demand elasticity is constant across cities. However, our 2SLS estimates will account for this bias under the additional assumptions that our instrumental variable is uncorrelated with the (unmeasured) housing demand elasticity and that our assumed demand elasticity represents the average housing demand elasticity across cities.

[9] Using building permits as a proxy for change in quantity of housing has several important limitations. One is that housing markets are frictional and so at any given time the housing being consumed is not the same as quantity of housing available on the market. In fact, as documented in the online Appendix, we find evidence that housing booms modestly reduce the vacancy rate, suggesting that housing booms increase housing market tightness. Additionally,

FIGURE 6. CORRELATION BETWEEN CHANGES IN HOUSING PERMITS AND HOME PRICES, 2000–2006

Notes: This figure shows correlation between changes in home price index and housing permit index. The regression line is weighted regression using 18–55 adult population as weights, and the sample is the baseline sample of 275 MSAs used in main regression tables.

Census Building Permits Survey to Census/ACS metro areas using the MSA codes in the permits data. Merging the Census/ACS data with the FHFA and Building Permits Survey data produces 275 MSAs, which constitute our analysis sample of local labor markets.

Figure 6 shows a positive correlation between changes in housing prices and housing permits, which shows the predominant role for local shocks to housing demand during this time period. Figure 7 plots trends over time at the median, tenth percentile, and ninetieth percentile for our local housing demand measure (the sum of log permits and log prices in an MSA). The figure shows variation at all three percentiles, with particularly dramatic changes at the ninetieth percentile over the course of the boom and bust compared to changes at the median and tenth percentile. Our analysis exploits this large variation across MSAs. A 100 log point change in the housing demand measure between 2000 and 2006 corresponds to approximately the 90/10 percentile difference in the distribution of the 2000–2006 log changes across MSAs. The standard deviation across MSAs in the 2000–2006 changes is 0.55.

Our empirical work examines how different measures of educational attainment and labor market outcomes are affected by local housing demand shocks. A key problem we face is measurement error in our housing demand shock which could lead our estimates to be attenuated. There is some unavoidable error in the dating of the start and end of the boom in an MSA, and the information on prices and permits that we use to create the measure of housing demand are only noisy proxies of

changes in building permits may not reflect all forms of increased housing consumption (such as renovations). As an alternative to this proxy, we also construct change in housing supply by measuring change in total housing units in a metropolitan area (including vacant units). As we also show in the online Appendix, we find similar results using this alternative proxy as an instrumental variable to address bias from measurement error.
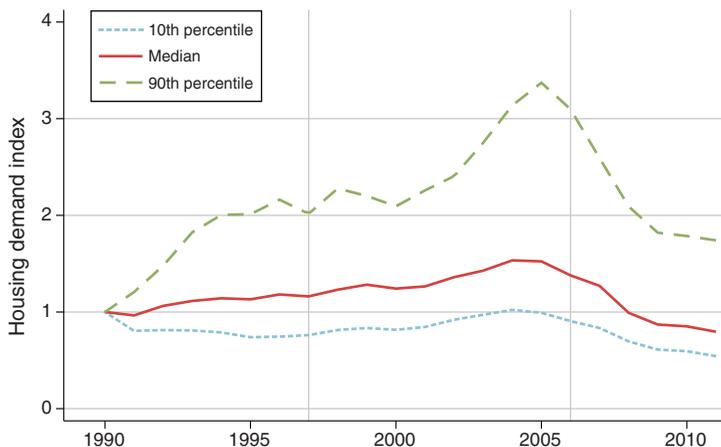
Figure 7. Trends in Housing Demand over Time and across Metropolitan Areas

Notes: This figure reports trends in our constructed housing demand index (which is the log sum of the prices and permits indexes) at the tenth percentile, median, and ninetieth percentile, normalized to 1980 values within each percentile.

underlying housing demand. A second challenge is that changes in housing demand in an MSA might be correlated with latent factors, such as latent amenity shocks, other labor demand shocks, or labor supply shocks that could independently affect education or labor market outcomes. This would cause bias of indeterminate sign in the OLS estimates. To account for both measurement error and endogeneity problems, we supplement our OLS analyses with 2SLS models that use exogenous variation in local housing demand arising from speculative activity.

Our strategy for isolating this exogenous variation draws upon the emerging consensus that much of the variation in housing prices, production, and transactions during the national boom and bust was not the result of changes in traditional fundamentals like latent productivity, income, or population, but rather was the result of factors specific to the housing market. These explanations include irrational exuberance and "bubbles" or "fads" (Shiller 2009; Mayer 2011; Chinco and Mayer 2014; Glaeser and Nathanson 2014; Burnside, Eichenbaum, and Rebelo 2016), the introduction of market products like interest-only mortgages (Barlevy and Fisher 2010), and changes in national lending standards (Favilukis, Ludvigson, and Van Nieuwerburgh 2010). The combination of these forces caused widespread speculative investment in housing assets, with dramatic increases in housing prices, production, and sales until the bubble eventually burst.

To create our instrument, we search for sharp changes in housing prices that occurred in the local area between the 2000 and 2006 period. We assume that underlying fundamentals do not change abruptly and are smoothly incorporated into prices when they do change, and we assume that sharp breaks from trend in a market's quarterly housing price reflects variation that is the result of exogenous speculative activity or other housing-specific forces, rather than unobserved changes in fundamental factors (that are the major source of endogeneity concerns in OLS analysis of labor market and education outcomes). Consistent with the work of

Panel A. Cities without structural break

Pittsburgh, PA: no break

Panel B. Cities with structural break

Portland, OR: small break

Chicago, IL: no break

Tucson, AZ: medium break

New Haven, CT: no break

Naples, FL: large break
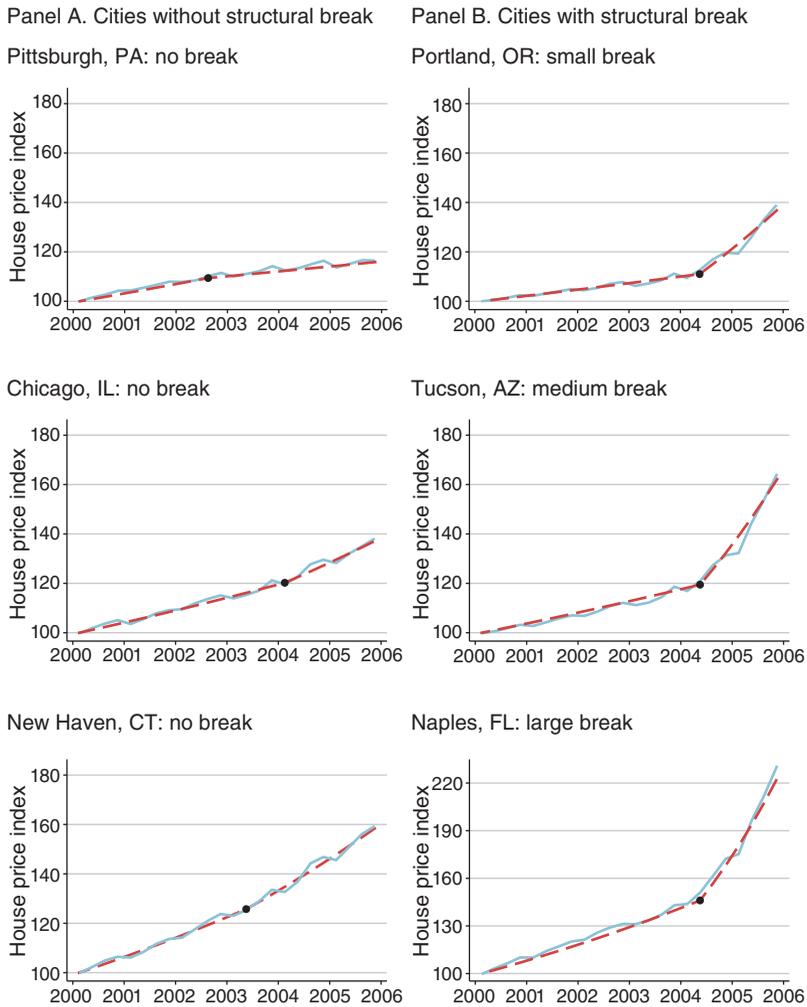
FIGURE 8. VARIATION IN STRUCTURAL BREAK ACROSS CITIES

Notes: This figure shows graphs of quarterly house price data for six MSAs. The house price index for each city is normalized so that 2000:I = 100. The solid lines report the house price series, while the dashed lines reports the structural break estimates, with a solid dot indicating the estimated quarter of the structural break. The MSAs in first column have small estimated structural breaks, and the MSAs in second column have relatively larger estimated structural breaks. The rows group MSAs based on overall house price growth up until the estimated structural break.

Ferreira and Gyourko (2011), we find that these sharp changes in housing prices occurred at different times in different locations.

Figure 8 illustrates how we use this insight to create an instrumental variable for local housing demand changes.[10] The figure plots quarterly housing prices for six MSAs between 2000:I and 2005:IV. For the three cities in the figures of panel A, the smooth evolution of prices over time suggests that all or most of this change could have been the result of latent unmeasured fundamental factors, which

---

[10] We are grateful to Edward Glaeser for discussions that encouraged us to formulate this empirical strategy.

"smoothly" affect demand. By contrast, for each of the three cities in panel B, the price series changed "sharply" at some point in the 2000s, suggesting the influence of some factor different from smooth changes in fundamentals, such as the effect of a speculative bubble.

Using the quarterly price series of each MSA between 2000:I and 2005:IV, we estimate MSA-specific OLS regressions with a single structural break, and search for the location of the break which maximizes the R^2 of the following regression:

$$(3) \qquad\qquad P_k^H(t) \ = \ \omega_k + \tau_k t + \lambda_k (t - t_k^*) \, 1\{t \ > \ t_k^*\} + \zeta_{k,t}.$$

In equation (3), $P_k^H(t)$ represents the log of the local house price index in MSA k in year-quarter t; t_k^* is the date of the structural break in the MSA's time series, restricted to be between 2001:I and 2005:I; τ_k is an MSA-specific linear time trend before the structural break; and λ_k is the size of the MSA-specific structural break: the extent to which the growth rate of MSA's quarterly house price series changed at the break.[11] This procedure follows standard practice in the time series econometrics literature for estimating structural breaks with unknown break dates (Bai 1997; Bai and Perron 1998). For MSAs whose house prices evolved nearly log-linearly over the 2000 to 2005 time period, our estimates of λ_k will be close to zero.

Our procedure for recovering an estimated structural break using equation (4) is similar to the analysis in Ferreira and Gyourko (2011) but differs in several important ways. First, our main analysis covers a shorter time period (2000–2006). This is because structural breaks that occur before 2000 will not be relevant to our first-differences analysis of changes between 2000 and 2006, which is the main time period for most of our analysis. In metropolitan areas where Ferreira and Gyourko (FG) estimate structural breaks before 2000, we tend to find breaks of negligible magnitude. Second, we use the FHFA MSA-level house price index rather than estimating equation (4) with transactions-level data. As a result, we are able to study a much larger set of metropolitan areas (275 versus 95 in FG), but at the cost of potentially introducing composition bias (beyond what can be addressed using a repeat-sales index like the FHFA house price index). In the online Appendix we show that our main results are similar using the same sample of metropolitan areas and estimated structural breaks from Ferreira and Gyourko (2011), although with the smaller sample size the results are less precise. Additionally, we show that our results are robust to alternative econometric procedures that allow for multiple structural breaks (either before or after 2000) or set the estimated structural breaks to zero if not statistically significant. Overall, we are reassured by the similarity of our results to these different ways of constructing our structural break instrument.[12]

Perhaps more important are the substantive concerns about the relevance and economic validity of our preferred structural break instrument. We address these

---

[11] The restricted range of the structural break search follows Andrews (1993), which excludes the beginning and end of sample period.

[12] We prefer our baseline procedure because it does not require either sequential or simultaneous estimation of multiple structural breaks and does not require carrying out any structural break inference (either in terms of timing, magnitude, or number of breaks). Both of these issues are known to be difficult econometric problems (Bai 1997; Bai and Perron 1998). By contrast, our instrumental variables strategy only relies on consistently estimating the magnitude of the structural break, and our procedure consistently estimates the magnitude of the structural break even if the break is nonexistent.
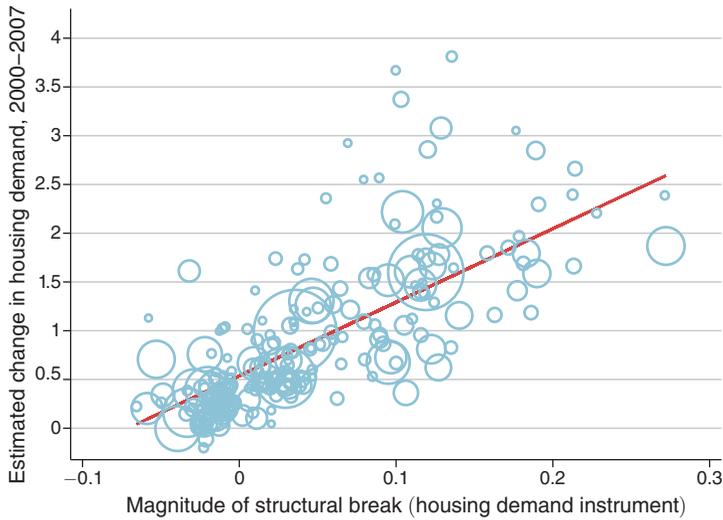
FIGURE 9. FIRST-STAGE RELATIONSHIP BETWEEN INSTRUMENT AND CHANGE IN HOUSING DEMAND

Notes: This figure shows the correlation across cities between the magnitude of structural break and the estimated housing demand change across 2000–2006. The Magnitude of Structural Break variable corresponds to the (annualized) coefficient from the city-specific structural break regression. The higher the value of the instrument, the larger the estimated structural break.

two issues in the remainder of this section. First, regarding the relevance of the instrument, Figure 9 shows the very strong positive relationship between the size of an MSA's estimated structural break and the 2000–2006 growth in housing demand in the MSA. We conduct a variety of econometric investigations of the "first-stage" relationship shown in the figure, all of which confirm the visual evidence. In particular, the structural break strongly predicts the 2000–2006 MSA change in housing demand after accounting for a full set of standard controls, with the *F*-statistic on the structural break measure in these analyses always larger than 20. The structural break also strongly predicts the housing demand change during the 2006–2012 bust period, a result that follows from the fact that the size of the boom an MSA experienced is very strongly correlated with the size of its later housing bust, as shown in Figure 10.

Regarding the validity of the instrument, we interpret the structural breaks that we identify as the result of speculative activity in the local area. It is natural to wonder if these structural breaks are actually capturing exogenous shifts in speculative activity, or if they are instead reflecting changes in some latent confound in the MSA. Formally, our assumption is that the structural break instruments are orthogonal to other latent factors that would drive local labor markets and/or educational choices. To assess the plausibility of this assumption, the eight panels in Figure 11 plot the relationship between the size of an MSA's structural break, λ_k, and preexisting features of the MSA: average housing prices in the MSA in 1990; lagged housing price growth in the MSA between 1990 and 1995; average employment in the MSA in 1990; and the lagged level and lagged growth in per capita enrollment in the MSA in both two-year colleges and four-year colleges and universities, and
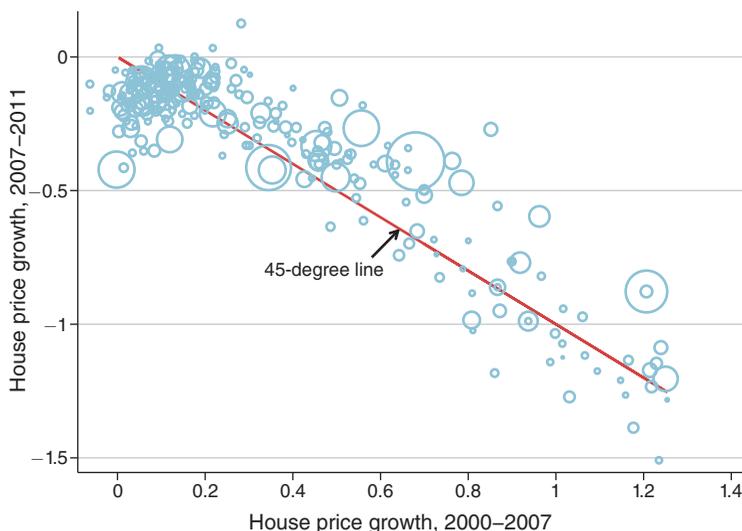
FIGURE 10. HOUSE PRICE GROWTH, 2006–2012 VERSUS 2000–2006

Notes: This figure shows the correlation between the change in house prices in 2000–2006 and the change in house prices in 2006–2012 for the 275 MSAs in our baseline sample. The dotted line is a 45-degree line (i.e., slope of −1).

the employment rate and average wages in 1990. Strikingly, the figure shows that the structural break does not systematically vary with *any* of these preexisting MSA-level variables. Of course, these patterns do not rule out the possibility that the structural break is related to *some* latent confound, but it is reassuring that λ_k exhibits no association with key preexisting observable variables that one would think are likely closely related to latent factors that would raise obvious endogeneity concerns.

Additionally, we assess validity of the instrument using County Business Patterns data to measure changes in employment and income at annual frequency by MSA. Using these data, we estimate "event study" specifications, and we find that total employment, construction employment, and average income all change sharply right around the time of the structural break. These results are consistent with a sharp change in housing demand and are reported in the online Appendix.

Lastly, the four panels in Figure 12 offer some evidence that the structural breaks indeed capture exogenous speculative activity rather than sharp changes in the underlying factors that determine labor market or education outcomes. The first graph relates the size of the structural break to the change in the price-to-rent ratio in an MSA, using data on rental price information that we have calculated for each MSA. To understand what this graph tests for, assume that there is a sudden change in amenities, productivity, or similar latent "fundamental," which immediately raises the desirability of living in an MSA. The current price of *all* housing in the MSA, whether to own or rent, should rise discontinuously in this case. In other words, there should be no relationship between λ_k and the price-to-rent ratio in an MSA if the break identified sudden changes in the latent fundamentals that give rise to endogeneity concerns regarding *current* employment, wages, and schooling. By contrast, if the structural break reflects price changes from speculative investment

Panel A. Lagged change in house price index, 1990−1995
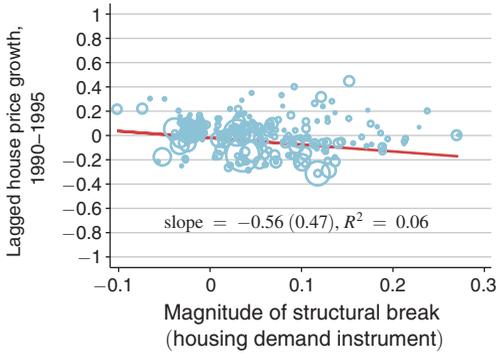
Panel B. Lagged house price index,1990
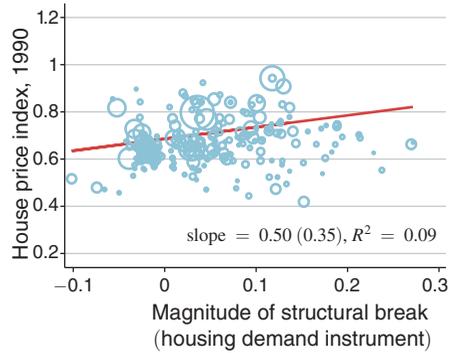
Panel C. Change in two-year enrollment per capita, 1990−1995
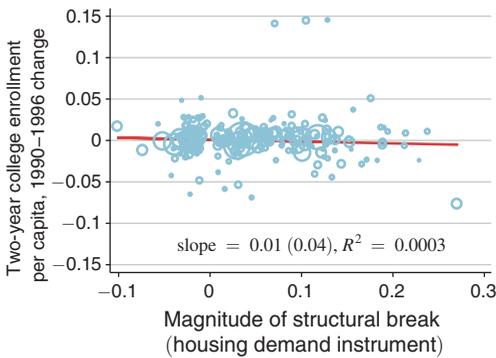
Panel D. Two-year enrollment per capita,1990
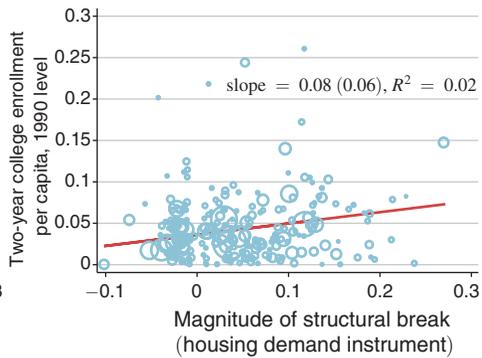
Panel E. Change in four-year enrollment per capita,1990−1995
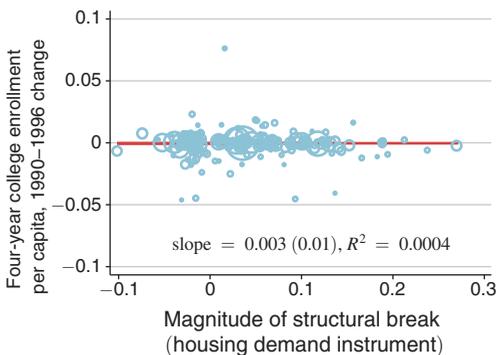
Panel F. Four-year enrollment per capita,1990

slope $= -0.56\,(0.47)$, $R^2 = 0.06$

slope $= 0.50\,(0.35)$, $R^2 = 0.09$

slope $= 0.01\,(0.04)$, $R^2 = 0.0003$

slope $= 0.08\,(0.06)$, $R^2 = 0.02$

slope $= 0.003\,(0.01)$, $R^2 = 0.0004$

slope $= 0.07\,(0.04)$, $R^2 = 0.02$

Lagged house price growth, 1990−1995

House price index, 1990

Two-year college enrollment per capita, 1990−1996 change

Two-year college enrollment per capita, 1990 level

Four-year college enrollment per capita, 1990−1996 change

Four-year college enrollment per capita, 1990 level

Magnitude of structural break (housing demand instrument)

(*Continued*)

FIGURE 11. VARIABLES NOT CORRELATED WITH STRUCTURAL BREAK INSTRUMENT

purchase, based on investors' (perhaps incorrect) judgments about the likely *future* desirability of the MSA, the price of owning should rise relative to that paid by renters, and an MSA's structural break should be positively related to growth in
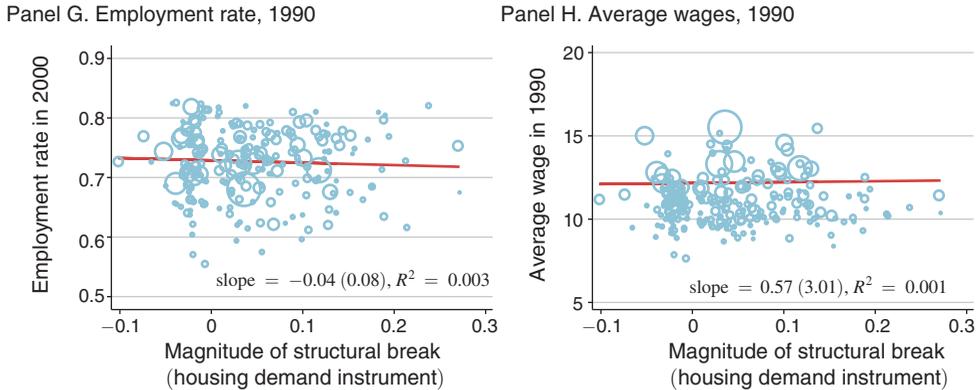
FIGURE 11. VARIABLES NOT CORRELATED WITH STRUCTURAL BREAK INSTRUMENT (*Continued*)

Note: This figure reports the correlation between the structural break instrument used in the IV specifications and lagged levels and changes in house prices, employment rate, wages, and two-year and four-year college enrollment (per capita).

its price-to-rent ratio. This is precisely what the graph shows, suggesting that the breaks do not reflect the changes in current amenities or productivity factors, at least to the extent these effects show up in rents.

Additional evidence that the structural breaks represent changes from speculative activity comes from panel B in Figure 12. In recent work, Chinco and Mayer (2014) have carefully assembled data from transaction-level deed records to identify purchases in several large housing markets made by "out-of-town buyers," individuals with a primary residence in one market who nonetheless buy a house in another market. By examining differences between local and out-of-town buyers in exit timing and realized capital gains, they present clear evidence that out-of-town buyers across most housing markets during the 2000s were disproportionately misinformed speculators. Using the data they have assembled, we analyze the 20 markets that we can match to data on housing prices and transactions. In panel B, we find, at least for this subsample of MSAs with available data, our structural break variable is strongly correlated with growth in the share of buyers who are speculative out-of-town buyers.

Lastly, panels C and D use quarterly data on local housing transactions from CoreLogic/DataQuick, using the data in DeFusco et al. (2017). We use this dataset to estimate structural break in housing transactions. Panels C and D show that the estimated structural breaks are similar in magnitude and the structural break estimated using the product of the house prices and housing transactions is also highly correlated with our main structural break measure that uses only prices. Since the transactions data are only available for a small subset of the cities (80 out of 275), we do not have a strong enough first stage to estimate 2SLS models with any of the structural break estimates that use transactions data. As a result, we focus on the structural break in housing prices, but we interpret the positive and significant correlations in the panels in this figure as indicative of sharp changes in prices and

Panel A. Growth in price-rent ratio

Panel B. Change in "out-of-town" buyers share

Panel C. Structural break in volume

Panel D. Structural break in price + volume
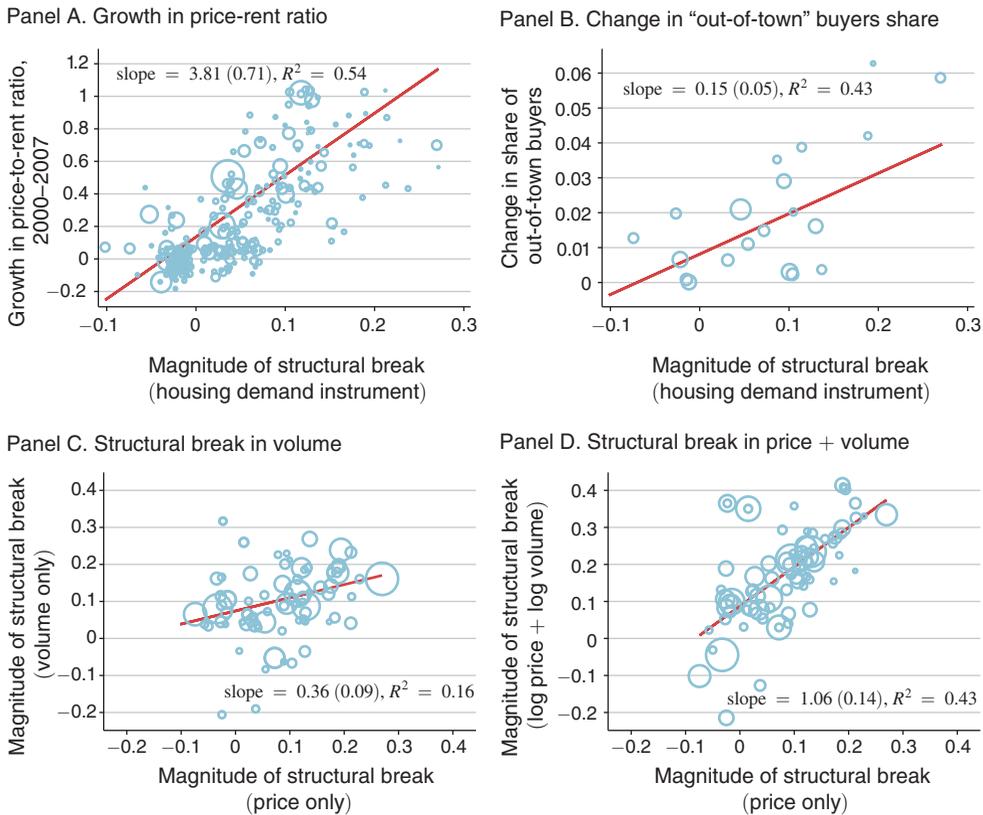
Figure 12. Variables Significantly Correlated with Structural Break Instrument

Notes: The first row in this figure reports the correlation between the structural break instrument used in the IV specifications and the growth in the price-rent ratio and the change in the share of "out-of-town" buyers, which can be interpreted as proxies for speculation. See text for details of the price-rent ratio calculation and the source of the "out-of-town" buyer share. The second row of this figure reports correlation between estimated magnitude of structural break in housing volume (from DataQuick) and house prices (from FHFA data), as well as correlation between structural break in prices and estimated structural break in Price and Volume combined. All structural breaks are estimated using log-linear models, so that Price + Volume estimates break in log(Price × Volume).

transactions going hand-in-hand, with both changes leading to changes in labor market outcomes and opportunity costs.[13]

Taken together, the evidence in Figures 11 and 12 is consistent with our assumption that the estimated structural breaks represent exogenous variation in housing demand arising from speculative beliefs. In some of our main results, we will use this assumption to estimate 2SLS first-difference models of the effect of the 2000–2006 change in housing demand on the change over the same time period in education and labor market outcomes, using the estimated structural break (converted

---

[13] See DeFusco et al. (2017) for more detail on the local housing transactions series. In addition to the significant correlation between magnitude of estimated structural breaks in prices and transactions, we also find that the timing of the structural breaks in prices and transactions is highly correlated, with the timing of the structural break in transactions slightly leading the structural break in prices, which is consistent with the results and model in DeFusco et al.

to an annualized growth rate in housing prices) as an instrumental variable for the change in housing demand.

Our preferred interpretation of the structural break variable is that it is a valid instrumental variable for changes in housing demand. However, we recognize that this is a strong assumption that may not hold exactly. As a result, we complement the main 2SLS estimates with analysis that uses the magnitude and timing of structural breaks to estimate difference-in-differences and "event study" regression models. These regression models allow us to assess whether sharp changes in local housing demand lead to an "on impact" change in trends in labor market outcomes and schooling decisions. The estimates can be interpreted as the reduced-form effect of a structural break in local house prices, which are valid whether the break is caused entirely by speculative forces or from a combination of these forces and other economic shocks (such as sharp changes in local labor demand).[14]

III. Changes in Opportunity Costs and Expected Lifetime Premium from Housing Booms

In this section, we assess how young adults' opportunity cost of (and their expected future lifetime earnings gain from) college attendance was affected by the boom, before turning to our main analysis studying different aspects of educational attainment.

We assume that a young adult who attends college in a given year forgoes the equivalent of the average labor market income received that year by persons in his MSA of roughly the same age who have no college training. His best estimate of the future lifetime premium from having gone to college is taken to be the current mean difference in labor market outcomes between older adults in his MSA with and without a college education.

We estimate mean labor market outcomes in an MSA from the 2000 Census and from several years of data from the American Community Survey (ACS), using the Integrated Public Use Microsamples (IPUMS) database (Ruggles et al. 2004). We restrict the Census/ACS sample to non-institutionalized persons living in an MSA in their state of birth, and we exclude individuals living in group quarters. This "same state" sample restriction partially accounts for the potential confounding effects of endogenous migration of the type shown to accompany other types of local demand shocks (Blanchard and Katz 1992; Bound and Holzer 2000; Notowidigdo 2013). Likewise, this restriction excludes all foreign-born individuals, mitigating the concern that our results are being driven by compositional changes in the local area due to both international migration and the intrastate migration of immigrants (Cadena and Kovak 2016). Using the Census/ACS samples, we explore three separate time periods: 2000, 2006, and 2012. Averages for the year 2000 are estimated using the 2000 Census. To compute the labor market and education averages in the years

---

[14] As described in the online Appendix, we provide a full dataset of the estimated structural breaks by metropolitan area, and future authors can decide whether they wish to follow the 2SLS specification and make the necessary exogeneity assumption, or, alternatively, authors can use the timing and magnitude of the structural breaks to estimate "event study" specifications. For settings where it is not important whether the structural break is caused by speculative activity or by sharp changes in local labor demand, the difference-in-differences and event study specifications may be preferable.

2006 and 2012, respectively, we pool ACS data from 2005 to 2007 (and refer to it as 2006) and from 2011 to 2013 (and refer to it as 2012). We pool the data in the ACS to increase precision given that our analysis is always conducted at the level of MSA observations.[15] For the Census/ACS analysis, we cannot explore the years between 2001 and 2004 because the ACS does not provide information on the individual's MSA during those years; the 2000 Census includes MSA information, as does the ACS starting in 2005.

Using the Census/ACS sample, we estimate first-difference regressions of the form

$$(4) \qquad \Delta \overline{Y}_{kt} \,=\, \gamma_0 + \gamma_1 \widehat{\Delta H_{kt}^{D}} + \mathbf{X}_{kt}\Gamma + \nu_{kt},$$

where $\widehat{\Delta H_{kt}^{D}}$ and $\Delta \overline{Y}_{tk}$ are, respectively, the change in housing demand (defined above) and the change in the average labor market conditions that proxy for opportunity costs and expected future lifetime college premium in MSA k between periods t and $t+s$. The first difference specification in (4) accounts for the effect of latent fixed MSA-specific factors. The control vector $\mathbf{X}_{kt}$ in (4) is designed to control for any factors that could cause differential trends in labor market conditions across MSAs. This vector includes controls for the share of employed workers with a college degree, the share of women in the labor force, the fraction of the MSA that is foreign-born, and the log of the MSA's total population as measured in 2000. Standard errors in all our analyses are clustered by state. Lastly, all regressions are weighted by MSA young adult population (age 18–33) in 2000.

Since we mainly focus on the education choices of 18–25-year-olds, we use the average labor market outcomes of non-college 18–25-year-olds to measure opportunity costs. This group includes all individuals with just a high school degree (or equivalent) and high school dropouts. We measure both the employment rate and average wages for this group, averaging across individuals in each MSA-year. To compute individual wages, we divide the individual's reported annual earnings from the prior year by an estimate of their reported annual hours worked over the prior year. To compute the skill premium within each MSA in each period t, we focus on the wages of 26–55-year-olds for those with and without any college education. In the language of our model outlined above, this is our estimate of the individual's expected future college premium. The online Appendix provides a further description of the construction of all variables used in the paper.

Table 1 presents estimates of the effect of the housing boom on opportunity costs, using two different measures of what a young adult gives up in terms of labor market rewards by going to college in a given year. The table presents both OLS and 2SLS results. Subsequent tables show only the preferred 2SLS results; all corresponding OLS results for all other tables are presented in the online Appendix. We show both sets of estimates in Table 1 to give a sense of the pattern of results that we consistently find across the relationships we study: strongly significant 2SLS

---

[15] When computing house price growth over the boom, we examine the change between 2000 and 2006, averaging the second- and third-quarter house price index values in each of these years. For the housing supply proxy, we calculate the change between average annual housing permits over the 2004–2006 period and average annual housing permits over the 1998–2000 period.

TABLE 1—HOUSING BOOMS AND LABOR MARKET OPPORTUNITIES FOR ADULTS WITHOUT ANY COLLEGE EDUCATION

Dependent variable is 2000–2006 change in:	Emp. rate (1)	Average wage (2)	Emp. rate × average wage (3)	Share employed in construction (4)	Share employed in FIRE (5)
Panel A. OLS estimates					
All adults age 18–25					
Housing demand change 2000–2006,	0.028	0.670	0.102	0.014	0.001
$\widehat{\Delta H_k^D}$	(0.008)	(0.100)	(0.021)	(0.003)	(0.001)
Share of total employment change				49.8%	2.6%
R^2	0.31	0.16	0.33	0.13	0.12
Men age 18–25					
Housing demand change 2000–2006,	0.028	0.067	0.105	0.023	0.001
$\widehat{\Delta H_k^D}$	(0.008)	(0.010)	(0.023)	(0.004)	(0.001)
Share of total employment change				83.6%	1.9%
R^2	0.20	0.13	0.24	0.12	0.03
Women age 18–25					
Housing demand change 2000–2006,	0.030	0.730	0.102	0.005	0.001
$\widehat{\Delta H_k^D}$	(0.009)	(0.013)	(0.022)	(0.001)	(0.002)
Share of total employment change				16.9%	3.3%
R^2	0.22	0.09	0.23	0.09	0.10
Panel B. 2SLS estimates					
All adults age 18–25					
Housing demand change 2000–2006,	0.048	0.109	0.170	0.020	0.010
$\widehat{\Delta H_k^D}$	(0.017)	(0.024)	(0.046)	(0.004)	(0.004)
Share of total employment change				41.7%	20.3%
Men age 18–25					
Housing demand change 2000–2006,	0.055	0.105	0.191	0.032	0.004
$\widehat{\Delta H_k^D}$	(0.019)	(0.027)	(0.051)	(0.007)	(0.004)
Share of total employment change				59.0%	7.6%
Women age 18–25					
Housing demand change 2000–2006,	0.041	0.111	0.144	0.005	0.017
$\widehat{\Delta H_k^D}$	(0.017)	(0.030)	(0.047)	(0.002)	(0.007)
Share of total employment change				12.7%	40.4%
First stage *F*-statistic	35.16	35.16	35.16	35.16	35.16
Observations	275	275	275	275	275
Include baseline controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports OLS and 2SLS estimates. All samples are from Census/ACS data, have been restricted to ages 18–25, have been restricted to individuals who live in same state where they were born, and excludes individuals in group quarters. Additionally, all individuals have no college education, which includes high school dropouts and high school graduates with no reported college attendance. The baseline controls included in all columns are the following: log of MSA population in 2000, share of employed adults with a college degree, the share of adults who are foreign born, and the share of women in the labor force. The average 18–25 employment rate in 2000 is 0.61 for adults, 0.64 for men, and 0.57 for women. The share of total employment change is calculated by dividing the sector-specific coefficient by the coefficient for the employment rate. Standard errors are shown in parentheses and are clustered by state.

estimates that are larger than their OLS counterparts, although the latter are consistently relatively large and generally statistically significant across all of our results.

The first column of Table 1 presents the results for the average prevailing employment rate among young adults without a college education. Both the OLS

and 2SLS results show that 2000–2006 growth in housing demand in an MSA raised employment among non-college-educated young adults overall, and for men and women separately. The OLS results in panel A suggest that in an MSA experiencing a 100 log point larger increase in housing demand between 2000 and 2006, the mean employment rate was 2.8 percentage points higher among all 18–25-year-olds and 2.8 and 3.0 percentage points higher among 18–25 non-college men and women, separately. The corresponding preferred 2SLS estimates are 4.8, 5.5, and 4.1 percentage points. These effects, which are all strongly statistically significant, are relatively large given that the mean employment rates for all non-college-educated 18-25-year-olds and for men and women separately, were 60.6, 64.3, and 56.5 percent, respectively. A one standard deviation change in housing demand across MSAs was 0.55. As a result, our 2SLS regressions imply that a one standard deviation change in housing demand was associated with a 3.0 and 2.3 percentage point increase in employment rates for 18–25-year-old non-college men and women, respectively.

The estimated effect on housing demand shocks on wages for these young non-college workers are also relatively large and strongly significant. The 2SLS results for log wage in the second column show that in an MSA experiencing a one standard deviation increase in housing demand, young adults going to college forgo 6.0 percent more in wages, with very similar effects for young men (5.8 percent) and young women (6.1 percent). Given the large increase in employment that the boom caused among non-college-educated persons, as well as the effect on college attendance, some portion of this estimated wage effect may reflect compositional effects rather than increased returns from an hour of work. Even with this caveat, however, both the OLS and 2SLS results for employment and wages suggest that the boom substantially improved labor market opportunities for both young adult men and young adult women without college educations.[16] In the third column of the table we present results for a summary measure of labor market conditions that we use elsewhere in the paper: the product of wages and the probability of employment. The estimates show that a one standard deviation change in housing demand results in a 9.4 percent increase in wages adjusted by the probability of working for the pooled sample of men and women (0.170×0.55).

Were certain sectors particularly responsible for the improved labor market opportunities for young men and women presented in the first three columns? To the extent that people associate the housing boom with large increases in the building and renovation of houses, construction probably comes naturally to mind as a sector that ought to have been profoundly affected by the housing boom. As discussed above, the boom also involved massive changes in the volume of housing transactions: the amount of houses bought and sold. Many persons performing the various tasks necessary for a sale to be consummated, things like advertising, listing, "showing," titling, insuring, procuring financing, etc., would have been employed in the so-called FIRE sector of finance, insurance and real estate. Lastly, a broad set of sectors in retail and local services likely also responded to changes

---

[16] While we interpret the effect of housing demand shocks on employment as primarily coming from an increase in local labor demand, we do not wish to rule out any role for labor supply shocks. However, the strong increase in average wages is consistent with a more important role for labor demand shifts than shifts in local labor supply.

in housing demand through consumption increases coming from housing wealth effects or reduction of liquidity constraints (Mian and Sufi 2014).

Columns 4 and 5 of Table 1 present estimates of the effect of housing booms on the employment rate in the construction sector and the FIRE sector. The table shows both the point estimates for employment changes in the two sectors, and the ratio of those estimates divided by the overall employment effect from column 1. These ratios measure how much of the total employment effect from housing demand shocks for a given type of worker can be accounted for by changes in construction employment (column 4) and changes in FIRE employment (column 5).

Focusing on the 2SLS results, our estimates show that 59.0 percent of the employment effect for young non-college men is concentrated in the construction sector while only 12.7 percent of the employment effect for young non-college women is in the construction sector. These results make intuitive sense given that young non-college men are much more likely to work in the construction sector. Conversely, our estimates show that 40.4 percent of the increase in employment for young non-college women can be traced to the FIRE sector (real estate agents, mortgage brokers, etc.). The comparable number for men is only 7.6 percent.[17]

To assess the robustness of the main 2SLS results, we report results from a wide range of alternative specifications in the online Appendix, focusing on alternative control variables (including splines and/or polynomials of the main control variables), alternative proxies for the change in local housing demand, and alternative ways of constructing the structural break instrument. We also tried to estimate whether the changes in opportunity costs were primarily driven by changes in housing prices or by changes in housing supply, since our primary housing demand measure combines them together, implicitly assuming that the employment and wage effects are similar. To assess whether this is a reasonable assumption, we carried out two exercises. First, we included both ΔP_k and ΔQ_k as separate variables in our estimation of (5) and estimated this equation via OLS since we do not have separate instruments for each component. The coefficients on ΔP_k and ΔQ_k were fairly similar, suggesting that both higher housing prices and the construction of more homes increased non-college employment and wages. Second, we explored whether housing demand changes had differential labor market effects in areas where housing supply is relatively elastic, using the local housing supply elasticities estimates from Saiz (2010), interacted with both our housing demand proxy and the structural break measure. Although our statistical precision is somewhat limited, we find no evidence that changes in local housing demand had differential effects in places where housing is inelastically supplied relative to places where housing is elastically supplied. We therefore conclude that our assumption of similar labor market effects of ΔP_k and ΔQ_k is a reasonable approximation that we carry through the rest of the analysis.

We also examined the extent to which measurement error is explaining why our 2SLS estimates are consistently larger than our OLS estimates. To do this,

---

[17] We looked at several other sectors and found no meaningful effect of changes in local housing demand on employment in manufacturing, mining, and utilities. We therefore conclude that the remaining employment effect outside of construction and FIRE is accounted for by a broad range of jobs in the local retail and service sectors. We also found no evidence of changes in local housing demand on employment in tradable sectors, which we estimate by adapting the definitions in Mian and Sufi (2014) to Census/ACS employment data.

we use the Census/ACS data to construct alternative proxies for ΔP_k and ΔQ_k. In particular, we use change in self-reported housing value as an alternative measure of change in housing prices and the change in the number of housing units in the MSA as an alternative measure of the change in housing quantity. This alternative measure can then be used as an instrumental variable instead of our preferred structural break instrument. Under the assumption that the measurement errors in each of these proxies is uncorrelated with the measurement error in the proxies used in the baseline analysis, then this provides a way of assessing the importance of measurement error. These results are reported in the online Appendix and indicate that roughly one-quarter of the difference between our OLS and 2SLS estimates could be due to measurement error. The remainder of the difference could be due either to endogeneity bias arising from unobserved shocks affecting housing market outcomes and labor market outcomes, or other sources of measurement error not addressed by these alternative proxies, such as measurement error in the local housing demand elasticity.

The results in Table 1 show clearly that the boom substantially increased the opportunity cost of college-going for both young men and women, although these increases came in different sectors. Table 2 explores how the boom changed the expected college earnings premium that a young adult could have expected to earn in the future. We estimate this by comparing the labor market outcomes of older, prime-aged persons (age 26–55) with and without any college education. We focus on the same labor market outcomes in the first three columns of Table 1, but the dependent variable is now the change over time in the difference in labor market outcomes between those with at least one year of college education to those without any college education.

Recall from the discussion in Section I that if housing booms raised the expected future college/non-college labor market premium, that effect would tend to offset any negative response to the opportunity cost changes presented in Table 1. The results in Table 2 argue strongly against this possibility. The 2SLS point estimates indicate that local housing demand shocks lowered the employment rate gap between college and non-college working adults, strictly reducing the future college/non-college gain that a younger adult might reasonably have expected from getting a college education.

The estimates in the second column show that local housing booms did not meaningfully change the expected future college/non-college wage gains. In contrast to the employment rate results, local booms did not significantly increase or reduce the college/non-college wage gap among older working adults, with estimates generally close to zero. The results for the future wages weighted by the probability of finding employment (shown in column 3) are similar to the employment rate results. In sum, the results in Table 2 show that a young adult during the boom years, trying to form a conjecture of how increasing housing demand in his local area would affect his future market returns from different education paths, would have reasonably concluded that the boom either had no effect on lifetime labor market streams from the college versus non-college path, or else potentially reduced the earnings and employment gain from becoming college-educated. Nothing about expected future gain would have tended to militate against the effect of rising opportunity costs.

TABLE 2—HOUSING BOOMS AND THE LIFETIME RETURNS TO EDUCATION

	Employment rate (1)	Average wage (2)	Emp. rate × average wage (3)
2SLS estimates for adults age 26–55			
Housing demand change 2000–2006,	−0.023	−0.001	−0.056
$\widehat{\Delta H_k^D}$	(0.006)	(0.004)	(0.019)
2SLS estimates for men age 26–55			
Housing demand change 2000–2006,	−0.019	0.004	−0.041
$\widehat{\Delta H_k^D}$	(0.006)	(0.011)	(0.021)
2SLS estimates for women age 26–55			
Housing demand change 2000–2006,	−0.027	−0.016	−0.072
$\widehat{\Delta H_k^D}$	(0.006)	(0.008)	(0.021)
First-stage *F*-statistic	35.16	35.16	35.16
Observations	275	275	275
Include baseline controls	Yes	Yes	Yes

Notes: Dependent variable is 2000–2006 change in difference between any college and no college. This table reports 2SLS estimates for alternative gender and education groups. All samples are restricted to ages 26–55 and have been restricted to individuals who live in same state where they were born and excludes individuals in group quarters. All individuals with no college education represents high school dropouts and high school graduates with no reported college attendance; all individuals with any college reported attending college for at least part of one year (which includes college graduates and college dropouts). The dependent variables are the difference in the change in labor market outcomes for those with any college relative to the same labor market change for those with no college. A negative coefficient means the labor market outcomes of those with no college improved relative to those with any college during the housing boom. The baseline controls are described in Table 1. Standard errors are shown in parentheses and are clustered by state.

IV. Housing Demand Shocks and College Attainment during the Boom

In this section, we present the paper's main results, which assess how local housing demand shocks during the boom affected young adults' college-going. We use a variety of methods and information about college-going from three different data sources. Moreover, as we show below, the particular limitations of each data source are strengths of at least one of the other two. Combining a range of estimation methods and different data sources therefore allows us to carry out a more comprehensive investigation.

A. *Census/ACS Estimates*

Our first education results use self-reports of schooling attainment in the "same state" Census/ACS sample combining the 2000 Census and the 2006 ACS. Our primary measure of educational attainment is the fraction of individuals in a given age range with any college attainment regardless of degree completion. We refer to this measure as "Any college." Our second measure of educational attainment is the fraction of individuals in a given age range who completed at

least a bachelor's degree. We refer to this measure as "Bachelor's degree or higher." We calculate the mean educational attainment rates in an MSA among 18–25-year-olds in 2000 and among 18–25-year-olds in 2006. We estimate first-difference regressions of the form

$$(5) \qquad \Delta S_{kt} = \alpha_0 + \beta_{FD} \widehat{\Delta H_{kt}^D} + \mathbf{X}_{kt}\Gamma + u_{kt},$$

where ΔS_{kt} is change in average educational attainment among 18–25-year-olds in an MSA between periods t and $t+s$. The coefficient β_{FD} is the first-difference estimate of how the growth in housing demand in an MSA affected the change in college attendance among young adults in that MSA.

Table 3 presents the 2SLS estimates using the structural break λ_k as an instrumental variable. The two columns show the results for the dependent variable defined for "Any college" and "Bachelor's degree or higher," respectively. The first three panels show the results where the education measures are defined for all individuals in the 18–25 range, just males in this range, and just females in this range. The last panel shows results for a specification where we look at changes in college attainment for all 26–33-year-olds.

The results indicate that rising local housing demand during the national housing boom sharply lowered the fraction of 18–25-year-olds with "Any college," with estimated effects that were very similar for men and women. The strongly statistically significant point estimates imply that a one standard deviation increase in local housing demand reduced the fraction of 18–25-year-olds who completed any amount of college training by about 1 percentage point: 0.9 for men and 1.0 for women. As a benchmark, roughly 43 percent of men and 51 percent of women between the ages of 18 and 25 had any college attainment in 2000. By contrast, the second column in the table shows that the growth in local housing demand had no effect on the fraction of 18–25-year-olds with at least a bachelor's degree. It is not only that the effects are statistically insignificant; the point estimates are small compared to the point estimates in column 1.

Panel D presents estimates for persons aged 26–33. The conceptual model emphasizes that older households should be less likely to respond to the housing boom. The results show, reassuringly, that a sample older than our 18–25 age group of interest did not respond to the housing demand shock. Additionally, the fact that we find no effect of housing boom on bachelor's attainment in this older group suggest that the null effect we find for 18–25-year-olds is not simply because they are too young to have completed their bachelor's degrees.

Our finding that increases in local housing demand during the national boom lowered mean college attainment, with almost all of the effect coming from schooling that is less than a bachelor's degree, is consistent with the predictions of our conceptual model emphasizing the role of opportunity costs. The large Census samples allow us to precisely estimate means at the start and peak of the boom for relatively narrow birth cohorts by MSA, which is an important advantage of this data source. However, there are some important limitations of the Census/ACS data.

One concern is that the Census/ACS education self-reports may be unreliable. This is an especially important concern because there is some evidence that the errors in self-reported education tend to be nonclassical, with people claiming

Table 3—Housing Booms and Educational Attainment: 2SLS Estimates, Census/ACS Data

	Any college (1)	Bachelor's degree or higher (2)
Panel A. 2SLS estimates for adults age 18–25		
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	−0.020	0.003
	(0.006)	(0.003)
Average for adults age 18–25 in 2000	0.468	0.102
Average for adults age 18–25 in 2006	0.506	0.117
Panel B. 2SLS estimates for men age 18–25		
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	−0.017	−0.002
	(0.007)	(0.003)
Average for men age 18–25 in 2000	0.425	0.084
Average for men age 18–25 in 2006	0.461	0.095
Panel C. 2SLS estimates for women age 18–25		
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	−0.022	0.008
	(0.008)	(0.006)
Average for women age 18–25 in 2000	0.511	0.119
Average for women age 18–25 in 2006	0.552	0.139
Panel D. 2SLS estimates for adults age 26–33		
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	0.008	0.012
	(0.010)	(0.008)
Average for adults in 2000	0.609	0.270
Average for adults in 2006	0.627	0.298
First-stage *F*-statistic	35.16	35.16
Observations	275	275
Include baseline controls	Yes	Yes

Notes: Dependent variable is 2000–2006 change in share with college education. This table reports 2SLS estimates for alternative gender and age groups. All samples are restricted to ages listed in panel heading have been restricted to individuals who live in same state where they were born, and excluded those in group quarters. All individuals with any college reported attending college for at least a portion of one year. The baseline controls are described in Table 1. Standard errors are shown in parentheses and are clustered by state.

higher educational attainment than is suggested by other types of evidence (see, e.g., Fillmore 2014). A second concern is that Census/ACS data do not allow us to determine whether a person with college training but who has not finished a degree *had* been working toward a degree at a community college or a four-year university. This makes it very difficult to accurately characterize the type of college training received by an important part of the sample, calling into question any firm conclusions about the differential responses across different types of colleges. Third, the fact that the 2001–2004 ACS samples do not record the MSA of respondents prevents us from doing high frequency analysis that better allows us to exploit the differential timing of the boom across MSAs. Finally, there is the important problem that because the Census/ACS data are pooled cross-sectional samples, we cannot definitively link people to their MSA at different points during the housing boom. Even with our sample restricted to persons living in their state of birth, we do not know their MSA at the start of the boom for persons who moved across MSAs *within*

that state during the boom. The Census/ACS results may thus still be confounded by endogenous migration, even in the "same state" sample. We address this concern with three additional exercises, which are reported in the online Appendix.

First, we use the migration information in the Census/ACS data to assess whether the selectivity of recent migrants is systematically related to the housing boom. We find no evidence across any of the observable characteristics in the Census/ACS of such composition bias. While recent migrants do look different than the rest of the local population on average, the magnitude of these differences is not related to magnitude of the housing boom. Second, we show that our main results are robust to restricting to individuals who have lived in the same residence for more than ten years (and thus by construction have not moved across MSAs during the housing boom). This sample is much more restrictive than the baseline "same state" sample, reducing the sample size by about two-thirds. We show broadly similar results in this restricted sample, with most of the main results somewhat larger in magnitude. Lastly, we follow the empirical model of Lafortune, Rothstein, and Schanzenbach (2016) and decompose our main results into a composition effect and a true "residual" effect of the housing boom, net of the estimated composition effect. As with the "same residence" sample, we again find similar (and generally somewhat stronger) results from this analysis. Overall, we interpret these exercises above as providing additional evidence against the existence of meaningful bias due to endogenous migration. The individual-level panel analysis in Section VC also addresses migration concerns and confirms these results, though with somewhat less statistical precision.

B. *IPEDS Estimates*

Our second source of information on educational attainment is the Integrated Postsecondary Education Data System (IPEDS). The IPEDS is constructed from administrative data on enrollments reported annually by most of the colleges and universities in the United States, including both community colleges and four-year colleges and universities.[18] The dataset tracks first-time, full-year enrollments, enabling us to identify persons enrolling in college for the first time during the boom. We match colleges and universities to MSAs based on street address and zip code, and we compute MSA-specific estimates of total first-time, full-year enrollments for different types of colleges and universities in each year between 1997 and 2006. In our main analysis sample, we exclude selective colleges and universities based on college rankings from Barron's.[19] Selective colleges and universities draw many students from other states. Excluding these selective schools allows us to focus on a sample of colleges and universities where a large share of students are "in-state

---

[18] The IPEDS sample includes all colleges and universities that participate in any federal financial aid program. Unfortunately, for-profit universities are underrepresented in IPEDS data, and they were growing fast during the housing boom period. In principle, we should be able to capture these educational investments in Census/ACS self-reported educational attainment variable.

[19] The Barron's ranking categories group colleges into selectivity tiers. In our main analysis sample, we exclude the top three selectivity tiers, and in the online Appendix we show robustness to alternative sample definitions, including using the full IPEDS sample.

students." This is important because we want to estimate the effect of a local housing boom on college decisions of local students.

There are several important strengths of the IPEDS data. Because the enrollment data are from administrative records, they are likely less error-ridden than the self-reported schooling attained in the Census/ACS data. In addition, the IPEDS data specifically report enrollment for different types of colleges separately, permitting a precise characterization of the type of college training for every enrollment (associate's or bachelor's, two-year or four-year). Finally, the annual IPEDS reports provide high frequency information about college-level training, which allows for some econometric specifications that cannot be done with the other data sources. For example, the IPEDS data can be used to carry out an "event study" analysis that exploits both the timing and magnitude of the structural break in local house prices.

The main shortcoming of the IPEDS is that it is not an individual-based survey, but is rather a survey of enrollments in institutions. It is not possible to measure enrollment by birth cohort using the IPEDS, as we do with the Census/ACS. In the empirical analysis, we assume that the enrollees are from the local market that houses that college or university, but this will likely not be true for some portion of enrollments if people move across MSAs for their college training.

For all of the IPEDS analysis, we use the per capita enrollment rate in the MSA, calculated by adjusting total first-time enrollment totals by the size of the 18–25 population in the MSA, using the county population estimates from the Survey of Epidemiology and End Results (SEER).[20] The analysis focuses on the 242 MSAs with enrollment information available between 1996 and 2006 and can be matched to housing market data. We divide IPEDS enrollments into community college enrollments and four-year colleges and universities. The community college category includes junior colleges and technical colleges, while the four-year colleges and universities category includes all institutions that award bachelor's degrees.

We perform two types of analysis with the IPEDS data. The first set of results follows the analysis of the Census/ACS data and focuses on changes in average enrollments during the 2002–2006 period relative to average enrollments in the 1996–2000 period. The primary advantage of this "long difference" specification is it allows us to use our 2SLS specification where we instrument for the housing demand change with our estimated structural break (λ_k). The second set of results exploits the higher frequency annual administrative data to examine whether the specific timing and magnitude of the structural break instrument lines up with the timing and magnitude of the enrollment change within each MSA.

2*SLS Estimates for Changes in Per Capita Enrollment*.—For our first analysis, we show the results from a 2SLS estimation of a first difference model of the effect of the 2000–2006 change in housing demand on the change in average annual per capita enrollment from the 1996–2000 period (when people made enrollment decisions before the boom began) to average annual per capita enrollment during the 2002–2006 period (when people made decisions during the boom). We instrument for

---

[20] The SEER data are available at http://www.nber.org/data/seer_u.s._county_population_data.html, and we use the 2000 MSA definitions to aggregate counties to MSAs to come up with annual population estimates for each MSA.

TABLE 4—HOUSING BOOMS AND COLLEGE ENROLLMENT: 2SLS ESTIMATES, IPEDS DATA

Change defined between following years:	2000 and 2006		1990 and 1996	
	2-year colleges	4-year colleges and universities	2-year colleges	4-year colleges and universities
Enrollment outcome:	(1)	(2)	(3)	(4)
Panel A. 2SLS estimates for men and women				
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	−0.012	0.002	−0.001	0.001
	(0.006)	(0.003)	(0.005)	(0.001)
Average level at start of period	0.036	0.022	0.037	0.020
Panel B. 2SLS estimates for men only				
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	−0.012	−0.001	0.001	0.001
	(0.006)	(0.002)	(0.005)	(0.001)
Average level at start of period	0.033	0.020	0.033	0.018
Panel C. 2SLS estimates for women only				
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	−0.013	0.005	−0.004	0.000
	(0.006)	(0.005)	(0.005)	(0.001)
Average level at start of period	0.038	0.024	0.041	0.022
First-stage F-statistic	33.27	32.80	27.34	27.46
Observations (number of metropolitan areas)	242	224	240	211
Include baseline controls	Yes	Yes	Yes	Yes

Notes: Dependent variable is the change in average annual enrollment per capita. The unit of observation is the metropolitan area, and the enrollment data come from the IPEDS dataset. The per capita estimates use 18–25-year-old population estimates from the SEER dataset. The dependent variable is the long difference across years reported in column headings. Each endpoint is average annual enrollment during the preceding five years. The enrollment data are matched to metropolitan areas by county, using 2000 metropolitan area definitions. Two-year colleges are defined to be any college that does not offer a four-year degree. Some four-year colleges may offer two-year degrees but they will be included in columns 2 and 4. This table reports 2SLS estimates for alternative demographic groups. The baseline controls are described in Table 1. Standard errors are shown in parentheses and are clustered by state.

the change in housing demand, $\widehat{\Delta H_k^D}$, using the structural break. This specification is identical to regression specification used previously for the various first-difference Census/ACS results aside from the change in dependent variable.

As the first column in Table 4 shows, the 2SLS results indicate that a one standard deviation increase in an MSA's housing demand from 2000 to 2006 lowered the five-year average of per capita annual enrollment in two-year colleges by about 0.7 percentage points. This statistically significant effect is similar for male and female enrollment. This is broadly similar to the effect of a one standard deviation boom on any college attendance in the Census/ACS results of about one percentage point, although a precise comparison is difficult.[21] The second column of Table 4

---

[21] Comparing the IPEDS results to the implied magnitudes from the Census/ACS results is difficult for several reasons. There is, first, the fact that the IPEDS results are based on a sample of 242 MSAs rather than 275 used in the Census analysis. This does not guarantee that the results would be quantitatively the same even if the measures used in the two studies were identical. A second issue that frustrates easy comparison across the two sets of results is that we do not know the ages of enrollees in the IPEDS, whereas all of the Census/ACS results focus on schooling completed by persons in particular age bins. Another important difference between the data sources that makes comparison of the magnitudes difficult is that whereas the Census data are limited to persons born in the same state,

presents results for enrollment in four-year colleges. The contrast with the results in the first column is very striking. We find that the 2000–2006 growth in housing demand in an MSA had no statistically significant effect on per capital four-year enrollment. For men, the four-year enrollment point estimates are very small, less than one-tenth the size of the corresponding two-year enrollment estimate. The estimated effect for women is larger but it is also imprecisely estimated. Given the large standard errors on the bachelor's results, we cannot rule out whether women actually increased their bachelor's enrollments, perhaps because of the liquidity constraint mechanism discussed in Section I.[22]

For the third and fourth columns of Table 4, we use IPEDS data from several years before the boom to assess whether the results in the first two columns actually capture the causal effect of housing boom, or whether the regressions might simply be picking up the effect of preexisting trends. In these placebo tests, we measure whether the 2000–2006 change in housing demand predicts the growth in annual average enrollment from a *previous* time period: specifically the change in average annual enrollment during the 1991–1996 period relative to average enrollment during the 1987–1990 period. Reassuringly, the results show that current booms do not predict previous changes in average annual per capita for either two-year or four-year college enrollment. This suggests that the estimates in the first two columns are not simply capturing long-term trends and indeed capture causal effects attributable to the housing demand shock.

How do these results compare to the 2SLS Census results for completed schooling in Table 3? Although enrollment is a flow measure of schooling and the Census highest schooling completed variable studied in Table 3 is a measure of the stock of college training, the two constructs should offer the same basic picture of the effect of housing booms, since the years of college that a person has completed as of given year is necessarily a function of their enrollment decisions in several separate years before year in question. It is therefore reassuring that the two sets of results give the same qualitative picture of a significant negative effect of the booms on associate's-level training, with much smaller effects for bachelor's-level training.

Difference-in-Differences and Event Study Estimates for Per-Capita Enrollments.—The second exercise we conduct with the IPEDS exploits the exogenous variation associated with the MSA-specific information about the timing and size of structural breaks in a difference-in-differences (DD) model. Specifically, using annual per capital enrollment in a given MSA during a given time period, $e_{kt,}$ we estimate

$$(6) \qquad e_{kt} = \alpha_k + \delta_t + \beta_{DD}((Post\ t_k^*) \times \lambda_k) + v_{kt},$$

---

there is no information about where IPEDS enrollees are from. Thus, IPEDS results include not only enrollment decisions of native-born persons from other states, but also immigrants. Finally, our IPEDS results are based on first-time, full-year enrollment, from which it is impossible to translate into completion rates for different types of schooling. Despite these challenges, we think the effect sizes for per capita enrollment rates in the IPEDS and any college attendance from the Census/ACS are broadly consistent.

[22] Because of space constraints, we show the OLS results in the online Appendix that accompanies the paper. The OLS results show a negative but not statistically significant association between housing demand in an MSA and enrollment in two-year colleges for both men and women, and no economically or statistically significant association between housing demand in an MSA and enrollment in four-year colleges.

where $(Post\ t_k^*)$ is an indicator variable denoting time periods after the date of the MSA-specific structural break, t_k^*: that is, all t such that $t \geq t_k^*$. The variable λ_k is size of the structural break, and α_k and δ_t are, respectively, MSA and year fixed effects.[23] The DD coefficient β_{DD} measures how per capita enrollment in an MSA in the years after the structural break differs from enrollment in the years before the break, with this post-break/pre-break difference weighted by the size of the structural break. An appealing aspect of (6) is that it tests whether there is a change in MSA enrollment that coincides with the break in housing demand. Since this DD estimate controls flexibly for time effects and for fixed features of the MSA that affect enrollment, the interaction term will yield unbiased estimates of our effect of interest as long as the timing of the break is exogenous conditional on time and MSA fixed effects.

Table 5 presents the DD results. Column 1 shows that there was a strongly statistically significant reduction in per capita enrollment in two-year colleges in an MSA in the years after the MSA's break, compared to the years before the break. These enrollment declines occurred for both men and women. To figure out the implied magnitudes of the point estimates in this table, we can scale the estimate by the first-stage relationship between structural break and the change in housing demand and the standard deviation in the housing demand measure. From this calculation, we conclude that an increase in structural break magnitude that translates into a one standard deviation housing boom causes a decline in the average annual enrollment in two-year colleges during the post-break period by about 0.6 percentage points ($0.04 \times 0.55/4.0$). This percentage point decline is similar to the implied magnitude from 2SLS first-difference estimates.

The results for per capita enrollment in four-year universities in the second column are very different. They show that there was no statistically significant change in per capita enrollment in these bachelor's-degree-granting institutions in the years after the structural break, relative to enrollment before. Although not precisely estimated, the point estimates in column 2 do suggest that there may have been a modest *increase* in enrollment in bachelor's-granting institutions in the years after the structural break. This is particularly true for women. However, none of the enrollment responses in bachelor's-granting institutions are statistically different from zero. We emphasize that this effect would be perfectly consistent with our conceptual framework, which argues that besides the opportunity cost mechanism that is this paper's main focus, housing booms may have eased liquidity constraints for some persons. To the extent that this effect exists, bachelor's-granting institutions is precisely where one would expect to observe it, since these institutions are more expensive. The relatively small number of people in an MSA whose college-going decisions are immediately changed by increases in homeowner wealth also probably

[23] For this analysis, we reestimate the timing and magnitude of the structural break following same procedure as in previous analysis, but we now allow for structural break to be anywhere between 1996:I and 2005:I (as opposed to 2001:I to 2005:I, as was done for the analysis of 2000–2006 changes). We do this because our housing price data go back to 1995 and we have annual enrollment data going back to 1990. Additionally, we restrict the sample so each MSA is observed no more than 5 years before the break and 7 years after the break. Because we allow the panel to be unbalanced and some of the estimated structural breaks occur in the 2000s, we are able to add additional MSAs to this analysis that are not part of Table 4. To maximize sample size, we include these cities but results are insensitive to including or excluding them since all of our regressions weight by population and these MSAs are relatively small.

TABLE 5—HOUSING BOOMS AND COLLEGE ENROLLMENT: DIFFERENCE-IN-DIFFERENCES
ESTIMATES EXPLOITING TIMING AND MAGNITUDE OF HOUSING BOOM, IPEDS DATA

	2-year colleges	4-year colleges and universities
	(1)	(2)
Panel A. OLS estimates for all adults		
Interaction between magnitude and timing of structural break, $\lambda_k \times (Post\ t_k^*)$	−0.045 (0.010)	0.021 (0.020)
Mean of dependent variable	0.035	0.023
Panel B. OLS estimates for men only		
Interaction between magnitude and timing of structural break, $\lambda_k \times (Post\ t_k^*)$	−0.038 (0.009)	0.010 (0.012)
Mean of dependent variable	0.032	0.020
Panel C. OLS estimates for women only		
Interaction between magnitude and timing of structural break, $\lambda_k \times (Post\ t_k^*)$	−0.054 (0.012)	0.033 (0.030)
Mean of dependent variable	0.039	0.026
Observations	2,569	2,214
Number of metropolitan areas	254	226
Metropolitan area FEs and year FEs	Yes	Yes

Notes: Dependent variable is enrollment per capita, annual data 1990–2006. The unit of observation is the metropolitan area-by-year and come from the IPEDS dataset. The enrollment data are matched to metropolitan areas by county, using 2000 metropolitan area definitions. Two-year colleges are defined to be any college that does not offer a four-year degree. Some four-year colleges may offer two-year degrees but they will be included in columns 2. This table reports OLS estimates for alternative demographic groups. All regressions include MSA and year fixed effects. The baseline controls from previous tables are not included because they are not identified when metropolitan area fixed effects are included. The right-hand-side variable is interaction of structural break variable and indicator for whether the year is after the estimated year of structural break. Standard errors are shown in parentheses and are clustered by state.

makes the effect difficult to precisely detect empirically. Why the effect, if it exists, should be bigger for women than it is for men as the point estimates suggest is unclear.

The DD results suggest that there were changes in MSA enrollments in the years after a structural break in the MSA's housing prices, and that how large that change was varied positively with the size of the structural break used by various 2SLS analyses. In Figure 13, we supplement these results with an "event study" analysis which include indicator variables for each year relative to estimated structural break interacted with structural break magnitude.[24] These figures provide visual evidence supporting the DD regression results; in particular, they show sharp changes in trends in college enrollment around the time of the structural break.

---

[24] The "event study" estimates are based on the same sample used in the DD analysis reported in Table 4. The specification is otherwise identical to the DD specification except that the single DD coefficient is replaced with a set of "event time" indicator variables for each year relative to the year of the estimated structural (normalizing the year before break to be 0).

Panel A. Change in total enrollment per capita

Panel B. Change in male enrollment per capita   Panel C. Change in female enrollment per capita
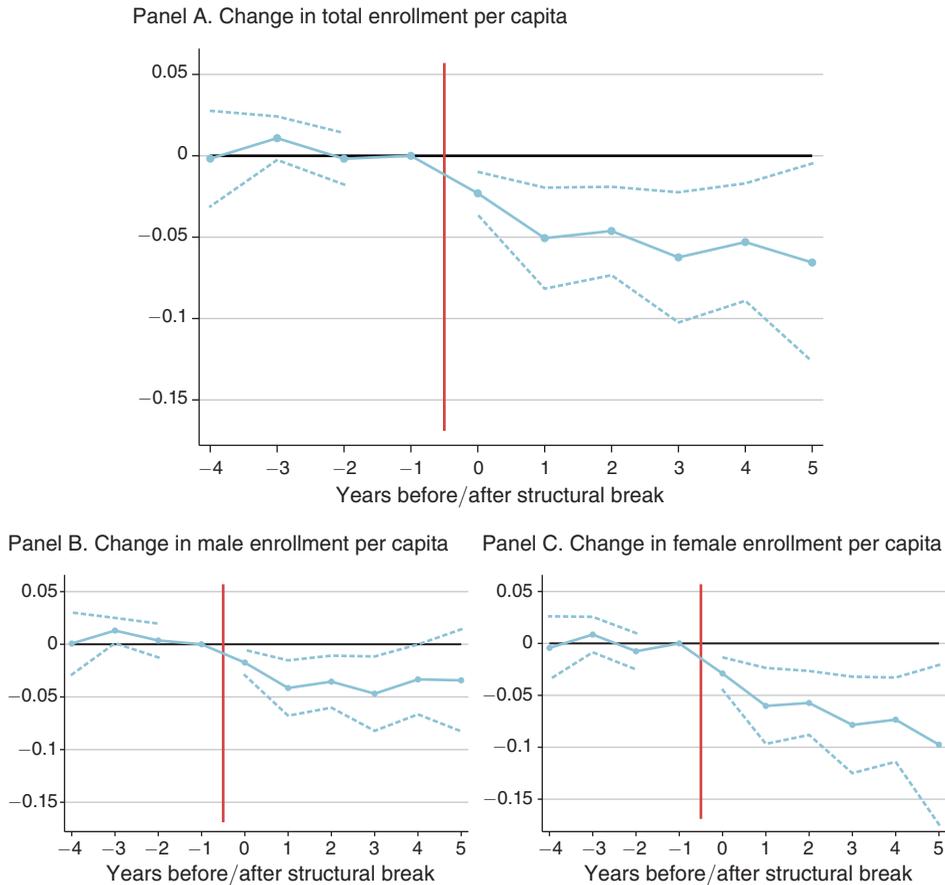

FIGURE 13. EVENT STUDY ANALYSIS OF PER CAPITA COLLEGE ATTENDANCE

Notes: This figure reports estimates of event study regressions, which include indicator variables for each year before and after year of estimated structural break, which the indicator scaled by the magnitude of the structural break. The event study regression specification includes year fixed effects and metropolitan area fixed effects and is weighted by the overall population in 1990. The structural break is allowed to be anywhere between 1995:I and 2005:I. The college enrollment data come from IPEDS and restrict to two-year colleges and universities. The population data focuses on 18–25-year-olds and are estimated using county-by-age population estimates from the Survey of Epidemiology and End Results (SEER) dataset. The sample period in each metropolitan area is restricted to six years before and after estimated structural break (if available). Standard errors are clustered by state.

C. *Individual Panel Results from NLYS97*

The third data source we use to study the effect of the boom on college attainment is the restricted-use version of 1997 National Longitudinal Survey of Youth (NLSY97). This individual-level longitudinal panel dataset initially surveyed a random sample of American youth aged 12–16 in 1997 and has followed them since.

The age range of the NLSY97 sample and the timing of the survey are ideal for our study: at 15–19 years old in 2000, these young people would have been making college-going decisions right around the time of the housing boom. Because the restricted-use NLSY97 provides information about respondents' MSA in each

survey year, we identify exactly where a person lived at the beginning of the housing boom, regardless of whether they moved subsequently.[25] As noted, this is something that is impossible to do with the individual-level Census/ACS data. More generally, uniquely among our data sources, the panel nature of the NLSY97 data allows us to track outcomes for particular persons through time. The NLSY97 is also the only one of the data sources we use that includes a very rich set of control variables measuring family background and other demographic characteristics, including parental education, parental income, race, ethnicity, and the Armed Forces Qualification Test (AFQT) score, which is often used as a proxy for cognitive ability and is a strong predictor of college attendance. We include these variables as controls in all specifications.

The big downside of the NLSY97 is its small sample size. After restricting NLSY97 sample to individuals with nonmissing data on employment, educational attainment, and control demographic variables, the final sample is 5,362 individuals (2,697 men and 2,665 women). Using this sample, we estimate a series of regressions relating individuals' outcomes in 2006 to the preferred housing instrument (structural break in local housing prices), controlling for the rich set of demographic controls described above.[26]

Table 6 presents the results. The first outcome variable, in column 1, is whether the person is employed in 2006. We show the results for this variable because the NLSY is the only dataset which allows us to directly assess not only whether people in boom markets faced situations where labor market prospects for young, non-college-educated adults in general, but whether they *personally* were more likely to participate in labor market activity. The results show clearly that this was the case. We find that young adults living just before the start of the housing boom in MSAs that subsequently experience large increases in housing demand were more likely to be employed in 2006, with a particularly pronounced effect for young men. The coefficients imply that a one standard deviation housing boom corresponds to an increase in the probability of being employed in 2006 by 1.7 percentage points. For men, the increase was a strongly statistically significant 1.5 percentage points. For women, the estimate is marginally significant and is about 2.0 percentage points. These effects represent 1.9 and 2.7 percent increases relative to mean employment rates, respectively.

Columns 2–4 of Table 6 use the available information in the NLYS97 about schooling attainment to measure the effect of being from a housing boom market on three measures of schooling attainment defined as of 2006: whether the person had attended "any college," whether they had received an associate's degree, and whether they had received at least a bachelor's degree. The results show that the effect of being from a boom MSA on having gotten "any college" training by 2006 was negative, substantial, and statistically significant. Overall, we find that adults in MSAs that had one standard deviation larger housing boom were 2.3 percentage points less likely to have attended college at all by 2006. These estimates are similar

---

[25] We use the individual's metropolitan area of residence in 1997 and assign the housing demand change of that MSA to the individual over the entire time period, even if that individual moves elsewhere during the sample period. Approximately 20 percent of the sample relocates during the 1997–2006 time period.

[26] To ease the interpretation of the reduced-form results, we scale all coefficients by the reciprocal of the first-stage coefficient from the main 2SLS estimates so that the coefficients can be interpreted as corresponding to a predicted unit change in housing demand.

TABLE 6—HOUSING BOOMS, EMPLOYMENT, AND EDUCATIONAL ATTAINMENT:
EVIDENCE FROM INDIVIDUAL-LEVEL PANEL DATA FROM NLSY

	Employed, year 2006	Has attended any college, year 2006	Has Associate's degree, year 2006	Has Bachelor's degree, year 2006	Migrated to different MSA between 2000 and 2006	Migrated to different state between 2000 and 2006
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. OLS reduced-form estimates for men and women						
Structural break instrument based on 1997 MSA of residence	0.031 (0.012)	−0.042 (0.015)	−0.033 (0.015)	−0.012 (0.014)	−0.024 (0.013)	0.019 (0.012)
Mean of dependent variable	0.762	0.569	0.433	0.233	0.166	0.140
Observations	5,362	5,362	5,362	5,362	5,362	5,362
Panel B. OLS reduced-form estimates for men only						
Structural break instrument based on 1997 MSA of residence	0.028 (0.013)	−0.039 (0.017)	−0.035 (0.018)	−0.012 (0.019)	−0.020 (0.014)	0.036 (0.014)
Mean of dependent variable	0.780	0.522	0.402	0.191	0.161	0.132
Observations	2,697	2,697	2,697	2,697	2,697	2,697
Panel C. OLS reduced-form estimates for women only						
Structural break instrument based on 1997 MSA of residence	0.037 (0.020)	−0.049 (0.019)	−0.035 (0.017)	−0.013 (0.016)	−0.026 (0.023)	0.005 (0.020)
Mean of dependent variable	0.743	0.616	0.484	0.274	0.170	0.147
Observations	2,665	2,665	2,665	2,665	2,665	2,665
Include baseline controls (metropolitan area)	Yes	Yes	Yes	Yes	Yes	Yes
Include additional individual-level controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The unit of observation is individual, and the assignment of housing demand change (between 2000 and 2006) is based on where the individual was living in 1997 at start of the NLSY97 sample. This table reports OLS estimates for alternative demographic groups, and each column reports results for a different dependent variable. The key independent variable is a dummy variable indicating whether the estimated structural break instrument was in the top tercile across MSAs. The baseline controls are the same as the controls in Table 1, and the additional individual-level controls are the following: age, demographic indicators for black, Hispanic, mixed race, non-black; separate indicators for father's and mother's education (missing, high school dropout, high school graduate, some college, and Bachelor's or greater), AFQT score (if available, 0 otherwise), indicator for missing AFQT score, log household income in 1996 (if available, 0 otherwise), indicator for missing household income. Standard errors are shown in parentheses and are clustered by state.

for both men and women. The results for having an associate's degree are smaller in magnitude but still statistically significant. The point estimates in the third column for effect of being from a housing boom MSA on bachelor's degree attainment by 2006 are small compared to the corresponding "any college" results and not statistically different from zero. These results suggest that effect of housing boom on schooling was concentrated among individuals would have been studying toward (and potentially receiving) an associate's degree in the absence of the boom.[27]

---

[27] While the results across columns 2 and 3 suggest that at least some of the individuals who do not attend college as a result of the housing boom would have gone on to receive an associate's degree, the imprecise estimates

Because the NLSY analysis tracks individuals over time, even as they move across MSAs, we can also look directly at endogenous migration due to the housing boom. The final two columns of Table 6 show no statistically significant effect of the structural break instrument on the probability of migrating between 2000 and 2006. The sign of the coefficients are not consistent, either, with an estimated decline in the probability of migrating to a different MSA but an increase in the probability of migrating to a different state. Additionally, the magnitudes are smaller than the effect on employment and college attendance, consistent with limited directed migration, as carefully documented recently in Yagan (2017). The broad similarly between the NLSY results and those presented earlier in the paper suggest that endogenous migration is unlikely to be the primary explanation for the pattern of results in the "same state" Census/ACS sample, and suggests that, despite our ignorance about the identity of enrollees in the IPEDS data, the enrollment results primarily capture the true causal effects of being from a housing boom market rather than the effect of migration.

D. *Discussion of College Attendance Results*

We find a consistent pattern of results across different data sources and methods: a negative effect of local housing boom on college attainment (or college attendance), with virtually all of the reduction coming from college training below the bachelor's-degree level, and roughly similar effects for men and women.

The patterns are consistent with the "single index" conceptual model of college choice presented in Section I. Young adults deciding between associate's-level training and labor force participation should be particularly sensitive to prevailing labor market conditions for less-skilled persons, as we find. The very small (or modestly positive) effect on bachelor's-level training could stem from either the fact that labor market conditions for young unskilled workers are irrelevant to the decisions of individuals thinking of going to bachelor's training, the larger gains from this type of college, or because the degree to which housing booms relieve liquidity constraints (and thus counteract the force of opportunity costs) is particularly important at bachelor's-granting universities and colleges compared to the much cheaper community colleges.

Another potential explanation for our results is the possibility the various estimated effects do not reflect changes in the behavior of potential students, but rather how colleges respond over the course of a boom. In particular, we would find the same pattern of results if, instead, it was simply easier for bachelor's than for associate's colleges to expand to deal with the increase in the number of students arising from population inflows into MSAs experiencing large booms. The best available evidence about the ease with which different types of colleges can expand, or even their differential *desire* to expand to accommodate interested students, suggests that this line of reasoning is unlikely to explain our results. Exploiting exogenous variation in class size arising from cohort sizes Bound and Turner (2007)

---

make it hard to draw strong conclusions. Additionally, the online Appendix reports results using IPEDS data that replace two-year college enrollment with degree completions awarded at two-year colleges. The point estimates are negative but not statistically significant, and smaller in magnitude than the corresponding enrollment estimates.

show that two-year colleges are more supply-elastic than their bachelor's-granting counterparts. Consistent with this previous work, we find no evidence that changes in housing demand affect the average cost of two-year colleges in an MSA. This suggests that the effect of local housing demand shocks on average tuition at local colleges is likely to be small. We therefore conclude that the effects we estimate during the boom were the result of the decisions of potential students in response to changes in their labor market opportunities, and not due to supply-side responses of colleges and universities to the boom.

We conclude with a rough calculation of what share of the "slowdown" in the share of young adults with any college attendance shown in Figure 2 can be explained by the housing boom. Extrapolating the pre-1996 trend to 2006 suggests a roughly 4 percentage point decline relative to trend for both men and women.[28] What share of this "slowdown" can be accounted for by our estimated effect of the housing boom? To answer this, we apply our local housing boom estimates to national housing boom. Aggregating the housing price and housing permits data, we estimate a national change in housing demand of 0.58 between 1997 and 2006.[29] Applying the 2SLS estimates in Table 3 to this national change yields a predicted decline by 2006 of 1.16 percentage points, or approximately 25 percent of the aggregate "slowdown" for men and women.[30] Of course, this rough calculation requires strong assumptions. We assume that we can scale up the "local" estimates to the national time series. This rules out the possibility of spillover effects across cities due to the housing boom. For example, our local estimates suggest that the housing boom resulted in increased in-migration, which suggests that local housing booms indirectly affected other MSAs that did not experience local housing booms. More broadly, any effect of national housing boom that is not picked up by our local estimates will not be captured by this rough calculation. This is analogous to the extrapolation of local labor market estimates to the national time series that is done in the recent international trade literature (see, e.g., Autor, Dorn, and Hanson 2013). Overall, we conclude that our estimates suggest that the housing boom may help account for a meaningful portion of the "slowdown" in college attendance, but it leaves a large share unexplained. Other factors such as the rising cost of college tuition may have also played an important role during this time period.

V. Effects during Bust and Full Housing Cycle, and Persistence of Boom Effects

Our results thus far have focused on changes during the 2000–2006 national housing boom. In this section, we study the effect of local housing demand changes during the massive national housing "bust" shown in Figure 4, and over the 2000–2012 interval spanning the entire boom and bust cycle. The goal of this section

---

[28] The exact differences in 2006 for men and women using predictions from time series model are 4.19 and 4.27 percentage points, respectively.

[29] To calculate these national changes, we use the change in the home price index and housing building permits index between 1997 and 2006 as reported in Figure 4. We calculate a log change in house price index of 0.394 and a log change in building permits of 0.185. Adding together gives the 0.58 used in the accounting exercise.

[30] The exact percentages for men and women are 23.5 percent ($= 0.58 \times 0.017/4.19$) and 29.9 percent ($= 0.58 \times 0.022/4.27$), respectively. Using either the results reported in Figure 4 or the IPEDS annual results lead to roughly similar percentages of between one-quarter and one-half of the "slowdown" can be explained by the housing boom.

is to assess whether the housing boom had persistent effects on the educational attainment of individuals. As shown in our simple theory, a housing boom when an individual is young may have a greater effect on their human capital choice than a corresponding housing bust when an individual is older. However, new cohorts may respond to the housing bust in a symmetric way as prior cohorts did when the prior cohorts experienced an equally sized housing boom.

We begin with an analysis of how changes in housing demand during these periods affected the labor market opportunity costs of attending college faced by different generations of young adults. Using data from the Census/ACS, we estimate first difference models relating the change in average labor market outcome among 18–25-year-olds without a college education between 2006 and 2012 (the bust), and between 2000 and 2012 (the full cycle) to the 2000–2006 change in housing demand in the MSA. Throughout all these analyses, our housing demand shock is defined over the 2000–2006 period. These regressions, which we estimate by 2SLS using the structural break as an instrument for the change in housing demand, assess how the size of the boom an MSA experienced affected labor market conditions in the MSA over the course of the bust, and from the beginning to end of the housing cycle.

Table 7 presents 2SLS first-difference estimates of the effect of changes in housing demand on the 2006–2012 and the 2000–2012 changes in college attendance. Columns 1 and 2 show results for the share of 18–25-year-olds in an MSA with "Any college" training as measured in the Census/ACS.[31] Although not precisely estimated, the point estimates suggest that young adult making schooling decisions during the bust were more likely to have attended college at all compared to similarly aged people at the peak of the boom. The results indicate that by the end of the bust, the size of the boom that an MSA had experienced during the 2000–2006 period had no effect on the share of 18–25-year-olds with "Any college" training compared to what had been the case for young adults in that MSA before the start of the housing boom and bust cycle. Both of these Census results are consistent with the conceptual framework about the effect of opportunity costs described above. As labor market conditions worsened during the bust, young individuals started going back to college.

The remaining columns of Table 7 show results for changes in per capita enrollment using administrative IPEDS data. Following the specification in Table 4, the outcomes variable for the bust estimates is the difference between average annual enrollment during 2007–2012 and the average of annual enrollment during 2002–2006. Similarly, for the results from the start of the housing cycle to the end of the bust, the outcome variable is the difference between average annual enrollment during 2007–2012 and the average of annual enrollment during 1996–2000. The results show that enrollment in two-year colleges was higher during the years of the bust, the bigger the size of the MSA's preceding boom, although the estimates are not precisely estimated. By the time that labor market conditions had essentially returned to levels seen before the start of the boom and bust cycle, annual enrollments were no different from what they had been in 2000, irrespective of the

---

[31] As with all of the results from the boom period, we also find that housing demand changes has no effect on bachelor's degree during the bust.

TABLE 7—HOUSING BOOMS AND HOUSING BUSTS: EDUCATIONAL ATTAINMENT AND COLLEGE ENROLLMENT, CENSUS/ACS AND IPEDS DATA

Data source:	Census/ACS		IPEDS	
Dependent variable:	Share with any college		Two-year college enrollment	
	2006 and 2012	2000 and 2012	2006 and 2012	2000 and 2012
Change defined over following periods:	(1)	(2)	(3)	(4)
Panel A. 2SLS estimates for all adults age 18–25				
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	0.015	−0.005	0.005	−0.007
	(0.010)	(0.009)	(0.005)	(0.007)
Panel B. 2SLS estimates for men age 18–25				
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	0.017	−0.000	0.006	−0.007
	(0.011)	(0.011)	(0.005)	(0.007)
Panel C. 2SLS estimates for women age 18–25				
Housing demand change 2000–2006, $\widehat{\Delta H_k^D}$	0.014	−0.009	0.005	−0.007
	(0.010)	(0.011)	(0.005)	(0.007)
First-stage *F*-statistic	35.16	35.16	32.69	32.69
Observations	275	275	241	241
Include baseline controls	Yes	Yes	Yes	Yes

Notes: This table reports 2SLS estimates for alternative demographic groups. Columns 1 and 2 report results using Census/ACS data that are analogous to results in Table 3 for alternative years. Columns 3 and 4 report results using IPEDS data that are analogous to results in Table 4 for alternative years. The baseline controls are described in Table 1. Standard errors are shown in parentheses and are clustered by state.

size of the preceding boom. Again, this is consistent with the predictions from our conceptual model.

The results in Table 7 are cross-generation comparisons: how one generation of young adults compares to another generation of young adults, making decisions at a different time and facing different labor market conditions. We study next whether the *specific* generation of young adults who invested less in college during the boom experienced persistent reductions in college training. A first piece of evidence of a persistent effect of the housing boom and bust comes from Figure 3, which shows that cohorts from "housing boom MSAs" who made their college-going decisions during the years of the boom still had reduced attainment when we observe them relative to similar persons from other markets in 2013.

While these results suggest that the effect of the decreased college investment during the boom was not reversed during the bust, the Census/ACS data do not track individual people over time. A stronger test of whether there is persistence in reduced college attainment is provided by results using individual panel data from the NLSY97, where individuals are tracked over time and where we know precisely where the person was at the start of the boom. In Table 8, we present results for the NLSY97 sample in 2013. The first column shows that as of 2013, people from MSAs that had larger housing booms were still less likely to have attended college, with estimates that are somewhat smaller than the peak of the housing boom, providing suggestive evidence of very modest "catch-up." However, the declines

TABLE 8—THE PERSISTENT EFFECTS OF HOUSING BOOMS ON EDUCATIONAL ATTAINMENT,
NLSY DATA

	Has attended any college, year 2013	Has Associate's degree, year 2013	Has Bachelor's degree, year 2013
	(1)	(2)	(3)
Panel A. OLS reduced-form estimates for all individuals			
Structural break instrument based on 1997 MSA of residence	−0.031 (0.016)	−0.022 (0.013)	−0.015 (0.012)
Mean of dependent variable	0.668	0.518	0.321
Observations	5,362	5,362	5,362
Panel B. OLS reduced-form estimates for men only			
Structural break instrument based on 1997 MSA of residence	−0.029 (0.018)	−0.028 (0.018)	−0.015 (0.019)
Mean of dependent variable	0.628	0.472	0.284
Observations	2,697	2,697	2,697
Panel C. OLS reduced-form estimates for women only			
Structural break instrument based on 1997 MSA of residence	−0.034 (0.017)	−0.017 (0.017)	−0.015 (0.016)
Mean of dependent variable	0.711	0.563	0.357
Observations	2,665	2,665	2,665
Include baseline controls (metropolitan area)	Yes	Yes	Yes
Include additional individual-level controls	Yes	Yes	Yes

Notes: The unit of observation is individual, and the assignment of housing demand change (between 2000 and 2006) is based on where the individual was living in 1997 at start of the NLSY97 sample. This table reports OLS estimates for alternative demographic groups, and each column reports results for a different dependent variable. The controls and key idenpendent variable are described in Table 6. Standard errors are shown in parentheses and are clustered by state.

are still economically significant and suggest that the likelihood of ever attending college has remained significantly depressed well after the housing boom ended.

Taken together, our various results across the different data sources suggests that there was a permanent "educational scarring" for the specific group of people who came of age during housing boom and who were from markets with especially large booms: their college training was reduced during the boom and did not recover during the bust. Their schooling investment stands in contrast to later generations of young adults in markets with large housing booms, who made college investments at rates identical to people from other markets after the bust had removed the large changes in labor market opportunities associated with the boom. The results suggest that the housing boom had a persistent effect on the human capital of younger individuals who experienced the boom.

VI. Economic Implications of Results

The paper begins by documenting a slowdown in college attainment for individuals of college age during the mid-1990s through the mid-2000s. We then show that the

housing boom can account for a part of the slowdown in college attainment for young adults during this time period. An important question left largely unanswered by this paper is what the consequences are of the persistent negative schooling effects on individual and social welfare; this is beyond the scope of this paper. This section discusses some of the economic implications of these results to help guide future work.

First, the responsiveness of college attendance to the opportunity cost of college is likely an important parameter in the design of optimal financing of higher education. For example, policymakers concerned about maintaining college enrollment may want to be particularly aggressive in increasing access to college and reducing the financial cost of college attendance when the opportunity cost of college is high. Currently, there is little theoretical work in studying how subsidies for college and other forms of financial aid should respond to local labor market conditions. Future work should tackle this problem, perhaps by drawing inspiration from other work on social insurance policy over the business cycle (Kroft and Notowidigdo 2016; Kroft et al. 2016; Landais, Michaillat, and Saez 2018).

Second, the consequences of our results for the affected individuals would appear to partly depend on the magnitude of the marginal returns to additional schooling for individuals on the margin of college attendance. Recent work suggests that this marginal return to college is very high for academically marginal students who would seem to be fairly representative of the marginal individuals whose college-going decisions are affected by local housing demand shocks (Zimmerman 2014). If true, then our results suggest a "scarring" effect of the housing boom for individuals who had the bad luck of being college-going age during the historically unprecedented boom and bust in housing. However, this conclusion is speculative; the economic return to community college attendance remains controversial, which means that the welfare implications for the affected young adults remain unclear.

Lastly, the group of men and women on the margin of college attendance are disproportionately at risk for many social problems such as teen pregnancy, crime, and poor health. A large literature using compelling quasi-experimental research designs has identified a causal link from education to crime, health, and fertility (Lochner and Moretti 2004; Lleras-Muney 2005; McCrary and Royer 2011). As a result, there are likely a range of additional social benefits from college attendance that go beyond increased earnings and employment opportunities. Our evidence of a persistent negative effect of the housing boom on college attendance thus raises the possibility of additional social costs for the affected individuals extending beyond the labor market.

VII. Conclusion

In this paper, we begin by documenting the large slowdown in college attainment that occurred nationally for cohorts who were of college age during the 1990s and early 2000s. To help assess the role of the housing boom on this slowdown, we introduce a new instrumental variable for local housing booms using the size of the structural break in housing prices during the early 2000s, and we argue that this instrument primarily captures speculative activity which generates increases in local housing demand. We use this instrument to identify the causal effect of housing

demand shocks on labor market outcomes, college attendance, and educational attainment.

Across several different complementary data sources and empirical strategies, we consistently find evidence that the housing boom reduced college attendance and educational attainment. These effects are generally similar for men and women, and seem to be concentrated among students studying at two-year colleges toward associate's degrees. Applying our local labor market effects nationally, we find that the national housing boom can account for approximately 25 percent of the observed slowdown in college-going for young men and women.

We present a simple model of college attainment during a housing boom, which highlights the separate roles of the opportunity cost of attending college, the changing skill premium, and the potential relaxation of liquidity constraints.

Using detailed labor market data, we find that the housing boom increased the employment and average wages of men and women without a college education, raising the opportunity cost of attending college. We find no evidence the housing boom altered the returns to going to college. This suggests that the estimated changes in educational attainment during the housing boom are likely coming primarily through changes in opportunity costs rather than changes in returns to education or relaxation of liquidity constraints.

Further evidence of the role of opportunity costs comes from the housing bust that followed the boom. We find that employment rates return roughly to pre-boom levels following the boom and bust in housing, and two-year college attendance in 2012 returns roughly to pre-boom levels, as well. In contrast to these results, we find evidence of persistent declines in educational attainment for birth cohorts who were of college-going age during the boom. These results may also help understand why growth has been so sluggish in the aftermath of the housing boom and bust cycle. By forgoing schooling during the housing boom, there is a set of workers with lower marginal products than they would have had otherwise. The lower level of productivity for these workers can act as a drag on aggregate labor productivity, raising the question of whether our findings can help understand the decline in labor productivity within the United States in the aftermath of the Great Recession.

REFERENCES

Andrews, Donald W. K. 1993. "Tests for Parameter Instability and Structural Change with Unknown Change Point." *Econometrica* 61 (4): 821–56.

Atkin, David. 2016. "Endogenous Skill Acquisition and Export Manufacturing in Mexico." *American Economic Review* 106 (8): 2046–85.

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *American Economic Review* 103 (6): 2121–68.

Bai, Jushan. 1997. "Estimating Multiple Breaks One at a Time." *Econometric Theory* 13 (3): 315–52.

Bai, Jushan, and Pierre Perron. 1998. "Estimating and Testing Linear Models with Multiple Structural Changes." *Econometrica* 66 (1): 47–78.

Barlevy, Gadi, and Jonas D. M. Fisher. 2010. "Mortgage Choices and Housing Speculation." Unpublished.

Becker, Gary S. 1964. *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education*. New York: Columbia University Press.

Betts, Julian R., and Laurel L. McFarland. 1995. "Safe Port in a Storm: The Impact of Labor Market Conditions on Community College Enrollments." *Journal of Human Resources* 30 (4): 741–65.

Bhutta, Neil. 2014. "The Ins and Outs of Mortgage Debt during the Housing Boom and Bust." *Journal of Monetary Economics* 76: 284–98.

Black, Dan A., Terra G. McKinnish, and Seth G. Sanders. 2005. "Tight Labor Markets and the Demand for Education: Evidence from the Coal Boom and Bust." *Industrial and Labor Relations Review* 59 (1): 3–16.

Blanchard, Olivier, and Lawrence F. Katz. 1992. "Regional Evolutions." *Brookings Papers on Economic Activity* (1): 1–76.

Bound, John, and Harry J. Holzer. 2000. "Demand Shifts, Population Adjustments, and Labor Market Outcomes during the 1980s." *Journal of Labor Economics* 18 (1): 20–54.

Bound, John, and Sarah Turner. 2007. "Cohort Crowding: How Resources Affect Collegiate Attainment." *Journal of Public Economics* 91 (5–6): 877–99.

Bulman, George, Robert Fairlie, Sarena Goodman, and Adam Isen. 2016. "Parental Resources and College Attendance: Evidence from Lottery Wins." NBER Working Paper 22679.

Burnside, Craig, Martin Eichenbaum, and Sergio Rebelo. 2016. "Understanding Booms and Busts in Housing Markets." *Journal of Political Economy* 124 (4): 1088–1147.

Cadena, Brian C., and Brian K. Kovak. 2016. "Immigrants Equilibrate Local Labor Markets: Evidence from the Great Recession." *American Economic Journal: Applied Economics* 8 (1): 257–90.

Cameron, Stephen V., and Christopher Taber. 2004. "Estimation of Educational Borrowing Constraints Using Returns to Schooling." *Journal of Political Economy* 112 (1): 132–82.

Card, David, Alexandre Mas, and Jesse Rothstein. 2008. "Tipping and the Dynamics of Segregation." *Quarterly Journal of Economics* 123 (1): 177–218.

Cascio, Elizabeth U., and Ayushi Narayan. 2015. "Who Needs a Fracking Education? The Educational Response to Low-Skill Biased Technological Change." NBER Working Paper 21359.

Charles, Kerwin Kofi, Erik Hurst, and Matthew J. Notowidigdo. 2013. "Housing Booms, Manufacturing Decline, and Labor Market Outcomes." NBER Working Paper 18949.

Charles, Kerwin Kofi, Erik Hurst, and Matthew J. Notowidigdo. 2018. "Housing Booms and Busts, Labor Market Opportunities, and College Attendance: Dataset." *American Economic Review.* https://doi.org/10.1257/aer.20151604.

Chen, Kaiji, and Yi Wen. 2014. "The Great Housing Boom of China." Unpublished.

Chinco, Alex, and Christopher Mayer. 2014. "Misinformed Speculators and Mispricing in the Housing Market." NBER Working Paper 19817.

Currie, Janet, and Erdal Tekin. 2015. "Is There a Link between Foreclosure and Health?" *American Economic Journal: Economic Policy* 7 (1): 63–94.

Davidoff, Thomas. 2016. "Supply Constraints Are Not Valid Instrumental Variables for Home Prices Because They Are Correlated with Many Demand Factors." *Critical Finance Review* 5 (2): 177–206.

DeFusco, Anthony A., Charles G. Nathanson, and Eric Zwick. 2017. "Speculative Dynamics of Prices and Volume." NBER Working Paper 23449.

Deming, David, and Susan Dynarski. 2010. "College Aid." In *Targeting Investments in Children: Fighting Poverty When Resources Are Limited*, edited by Phillip B. Levine and David J. Zimmerman, 283–302. Chicago: University of Chicago Press.

Favilukis, Jack, Sydney C. Ludvigson., and Stijn Van Nieuwerburgh. 2010. "The Macroeconomic Effects of Housing Wealth, Housing Finance, and Limited Risk-Sharing in General Equilibrium." NBER Working Paper 15988.

Ferreira, Fernando, and Joe Gyourko. 2011. "Anatomy of the Beginning of the Housing Boom: U.S. Neighborhoods and Metropolitan Areas, 1993–2009." NBER Working Paper 17374.

Fillmore, Ian. 2014. "Measuring Long Run Trends in Higher Education." Unpublished.

Glaeser, Edward L., and Charles G. Nathanson. 2014. "Housing Bubbles." NBER Working Paper 20426.

Goldin, Claudia, and Lawrence F. Katz. 2010. *The Race between Education and Technology*. Cambridge, MA: Harvard University Press.

Hall, Robert E. 1968. "Technical Change and Capital from the Point of View of the Dual." *Review of Economic Studies* 35 (1): 35–46.

Hilger, Nathaniel G. 2014. "Parental Job Loss and Children's Long-Term Outcomes: Evidence from 7 Million Fathers' Layoffs." Unpublished.

Houthakker, Hendrick S., and Lester D. Taylor. 1970. *Consumer Demand in the United States, 1929–1970.* Cambridge, MA: Harvard University Press.

Jordà, Òscar, Moritz Schularick, and Alan M. Taylor. 2014. "The Great Mortgaging: Housing Finance, Crises, and Business Cycles." NBER Working Paper 20501.

Kroft, Kory, Kavan J. Kucko, Etienne Lehmann, and Johannes F. Schmieder. 2016. "Optimal Income Taxation with Unemployment and Wage Responses: A Sufficient Statistics Approach." Unpublished.

Kroft, Kory, and Matthew J. Notowidigdo. 2016. "Should Unemployment Insurance Vary with the Unemployment Rate? Theory and Evidence." *Review of Economic Studies* 83 (3): 1092–1124.

Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach. 2016. "School Finance Reform and the Distribution of Student Achievement." Unpublished.

Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2018. "A Macroeconomic Approach to Optimal Unemployment Insurance: Theory." *American Economic Journal: Economic Policy* 10 (2): 152–81.

Lleras-Muney, Adriana. 2005. "The Relationship between Education and Adult Mortality in the United States." *Review of Economic Studies* 72 (1): 189–221.

Lochner, Lance, and Enrico Moretti. 2004. "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports." *American Economic Review* 94 (1): 155–89.

Lovenheim, Michael F. 2011. "The Effect of Liquid Housing Wealth on College Enrollment." *Journal of Labor Economics* 29 (4): 741–71.

Lovenheim, Michael F., and Kevin J. Mumford. 2013. "Do Family Wealth Shocks Affect Fertility Choices? Evidence from the Housing Market." *Review of Economics and Statistics* 95 (2): 464–75.

Manoli, Dayanand S., and Nicholas Turner. 2016. "Cash-on-Hand & College Enrollment: Evidence from Population Tax Data and Policy Nonlinearities." NBER Working Paper 19836.

Mayer, Christopher. 2011. "Housing Bubbles: A Survey." *Annual Review of Economics* 3: 559–77.

McCrary, Justin, and Heather Royer. 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth." *American Economic Review* 101 (1): 158–95.

Mian, Atif, Kamalesh Rao, and Amir Sufi. 2013. "Household Balance Sheets, Consumption, and the Economic Slump." *Quarterly Journal of Economics* 128 (4): 1687–726.

Mian, Atif, and Amir Sufi. 2011. "House Prices, Home Equity-Based Borrowing, and the US Household Leverage Crisis." *American Economic Review* 101 (5): 2132–56.

Mian, Atif, and Amir Sufi. 2014. "What Explains the 2007–2009 Drop in Employment?" *Econometrica* 82 (6): 2197–2223.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2010. "The Political Economy of the US Mortgage Default Crisis." *American Economic Review* 100 (5): 1967–98.

Mincer, Jacob. 1958. "Investment in Human Capital and Personal Income Distribution." *Journal of Political Economy* 66 (4): 281–302.

Notowidigdo, Matthew J. 2013. "The Incidence of Local Labor Market Shocks." Unpublished.

Polinsky, A. Mitchell, and David T. Ellwood. 1979. "An Empirical Reconciliation of Micro and Group Estimates of the Demand for Housing." *Review of Economics and Statistics* 61 (2): 199–205.

Ruggles, Steven, Matthew Sobek, Trent Alexander, Catherine Fitch, Ronald Goeken, Patricia Kelly Hall, Miriam King, and Chad Ronnander. 2004. Integrated Public Use Microdata Series: Version 3.0. [dataset]. Minneapolis, MN: Minnesota Population Center.

Saiz, Albert. 2010. "The Geographic Determinants of Housing Supply." *Quarterly Journal of Economics* 125 (3): 1253–96.

Shiller, Robert J. 2008. "Understanding Recent Trends in House Prices and Homeownership." In *Housing, Housing Finance and Monetary Policy,* Jackson Hole Conference Series, Federal Reserve Bank of Kansas City, 85–123.

Shiller, Robert J. 2009. "Unlearned Lessons From the Housing Bubble." *Economist's Voice* 6 (7): 6.

Sinai, Todd M. 2012. " House Price Moments in Boom-Bust Cycles." NBER Working Paper 18059.

Willis, Robert J., and Sherwin Rosen. 1979. "Education and Self-Selection." *Journal of Political Economy* 87 (5): 7–36.

Yagan, Danny. 2017. "Employment Hysteresis from the Great Recession." NBER Working Paper 23844.

Zimmerman, Seth D. 2014. "The Returns to College Admission for Academically Marginal Students." *Journal of Labor Economics* 32 (4): 711–54.