

Impacts of the Community Reinvestment Act on Neighborhood Change and Gentrification

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October 15, 2013

Abstract:

The Community Reinvestment Act (CRA) encourages bank lending in low- and moderate-income areas. We use a regression discontinuity design that exploits a relative income threshold that distinguishes CRA eligible from CRA ineligible neighborhoods (census tracts) and find little evidence that CRA has contributed to neighborhood changes associated with gentrification in eligible areas. Over the 1989 to 1999 period, we find that CRA eligible tracts had greater increases in mean income relative to ineligible tracts, but we find little evidence that the CRA caused decreases in the proportion of long-term residents, or increases in the proportion of white residents or college educated residents.

Keywords: CRA; Community Reinvestment Act; Gentrification; Housing; Low-Income Lending

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INTRODUCTION

For years, lenders redlined urban black neighborhoods, providing little credit except under restrictive terms (Schwartz, 2006). Without credit, homeowners could not rehabilitate their homes and employers could not expand their businesses. Trumpeted as a major achievement for the cause of racial equality, the Community Reinvestment Act (CRA) of 1977 encourages federally regulated banking institutions to meet the credit needs of those low- and moderate-income communities from which they draw deposits (Barr, 2005).

Since then, the CRA has been controversial. During the 1990s the CRA successfully facilitated lending in some low- and moderate-income communities (Barr, 2005; Avery et al., 2005; Gabriel and Rosenthal, 2008), but we know less about the consequences of this lending. Avery, et al.(2003) show that it improves communities by many measures: homeownership rates increase, vacancy rates decrease and property values increase. Other types of place-based policies, such as special tax zones, have also been shown to increase home prices and in some cases employment (Busso et al. 2013), but the comprehensive review by Gottlieb and Glaeser (2008) explains that the implications for communities' wellbeing are complex. Similarly, it is less clear how CRA lending ultimately affects neighborhoods' socio-economic characteristics. Because CRA lending to upper-income borrowers living in low- or moderate-income communities fulfills lenders' obligations under the law¹, the CRA could serve as a subsidy for upper-income borrowers moving into low- or moderate-income communities. Even when this does not occur, improvements paid for by

¹ “[I]f the loans are for homes or multifamily housing located in an area for which the local, state, tribal, or Federal government or a community based development organization has developed a revitalization or stabilization plan (such as a Federal enterprise community or empowerment zone) that includes attracting mixed-income residents to establish a stabilized economically diverse neighborhood, examiners may give more consideration to such loans, which may be viewed as serving the low-or moderate-income community's needs as well as serving those of the middle- or upper-income borrowers.” Such loans may also be considered if there is “other evidence of governmental support for a revitalization or stabilization project in the area” (Office of Comptroller of Currency, et al. 2010)

CRA-facilitated lending could attract new comers to the community and could spur neighborhood changes associated with gentrification. The present study seeks to learn whether or not the CRA has affected neighborhood changes in income, education, racial composition, and proportion of long-term residents in eligible communities.

The term “gentrification” is somewhat loaded. The urban studies literature has long made distinctions between stages of gentrification such as displacement of long-term residents by younger, higher income residents and “incumbent upgrading” of residents (Downs, 1981; more recently, Van Criekengen and Decroly, 2003). We mean gentrification as a general term for neighborhood changes that reflect the influx of new residents of high socio-economic status relative to incumbent residents. The term “neighborhood change,” although more ambiguous and cumbersome, might be more accurate². In any case, we use the term gentrification as a shorthand without intending to debate its problems or merits.

Freeman (2005) defines gentrification as a “process by which central urban neighborhoods that have undergone disinvestments and economic decline experience a reversal, reinvestment, and the in-migration of a relatively well-off, middle- and upper middle-class population.” The degree of displacement of long-term residents is an open question. McKinnish et al (2010), argue that gentrification does not necessarily harm minority households because it encourages in-migration of middle class blacks. Even without the mass displacement of long-time residents, Vigdor (2002) and Freeman (2006) argue that gentrification makes life uncomfortable for long-time residents, through rising housing costs and changing neighborhood cultures. Freeman (2005) argues that poor residents become less likely to move because they can no longer afford to do so within their communities. Based on the literature, gentrification has mixed effects on residents and this paper investigates how

² We thank an anonymous referee for suggesting “neighborhood change” and clarifying our thinking on this point.

the CRA has affected several neighborhood measures potentially associated with gentrification.

BACKGROUND

Redlining traces its roots back to the 1930s, when the Federal Housing Authority (FHA) instituted a policy whereby it did not insure home loans for properties located in mixed or predominantly black neighborhoods, ruling them too “risky.” Even when individual borrowers met the usual credit and asset requirements, they would be denied on the basis of their location. Schwarz (2006) argues that this process contributed to the blighting of many of America’s inner cities in the post-war period.

To counter this effect, the CRA required that bank regulators monitor banks’ dealings with borrowers within prescribed “assessment areas³.” Banks are encouraged to make loans in neighborhoods with median incomes below 80 percent of the metropolitan area median incomes. Loans in these neighborhoods contribute to banks’ CRA evaluation scores. If regulators find that a bank is in non-compliance with the CRA, they are permitted to deny the bank’s applications for a merger or acquisition. Community groups also have a role in this process. If a group believes a bank operating in its neighborhood to be in non-compliance, under the CRA, it is given a procedure by which it may go before regulators to administratively challenge the banks’ application (Schwartz, 2006; Barr, 2005)⁴. For years after its passage, however, the CRA was seen as a substantially ineffective piece of legislation. This began to change, however, with the passage of the Financial Institutions Reform, Recovery, and Enforcement Act (FIRREA) of 1989. For the first time, regulators

³ ‘One or more [metropolitan statistical areas (MSA)] ... or one or more contiguous political subdivisions, such as countries, cities or towns’ that include the census tracts ‘in which the bank has its main office, its branches, and its deposit-taking ATMs, as well as the surrounding [census tracts] in which the banks has originated or purchased a substantial portion of its loans’ (Barr, 2005).

⁴ An anonymous referee clarified for us that while community groups can file administrative challenges with financial institution regulators, they cannot bring lawsuits in court.

evaluations of banking institutions' performance under the CRA and information collected under the Home Mortgage Disclosure Act (HMDA) were made public record. These changes gave community groups access to information, which allowed them to more effectively argue against a proposed merger or acquisition (Bhutta, 2008). The same year, an application for a bank merger was denied for the first time under the CRA (Barr 2005). The HMDA was passed in 1975 and requires that home mortgage originators provide regulators with information concerning individuals who apply for a mortgage with them and their loans⁵ (Schwartz, 2006).

The CRA was made even stronger in 1995 when the Clinton Administration reformed the standards by which compliance with the law was judged. Instead of focusing on processes by which banks reviewed loan applications, as was the practice previously, regulators now looked at outcomes. Under the new standards, regulators periodically evaluated the major banks' assessment area lending, investments and services⁶. To achieve a "satisfactory" grade for the lending element of the evaluation, banks had to show that the mortgages and consumer loans they originated were both geographically and socioeconomically distributed and that no community was unduly denied credit. However, only banks that scored "outstanding" could feel confident that their mergers or acquisitions would not be slowed or stopped by a community group's challenge (Avery et al. ,2003).

Gentrification

Literature on gentrification initially focused on issues of displacement, the phenomenon of rising market rents, caused by the entry of higher income residents into a community, causing low-income long-term residents to leave the community (Smith 1996).

⁵ Among the information required is (1) the loan amount, (2) the purpose of the loan (home purchase, home improvement, refinancing), (3) the type of property involved (single-family, multifamily), (4) the location (state, county, MSA, census tract), (5) the race of the borrower, (6) the gender of the borrower, and (7) whether or not the loan was granted.

⁶ Immergluck (2007) gives a discussion of qualitative and quantitative aspects of the tests.

It is unclear, however, if displacement is actually a common phenomenon. For example, Freeman (2002) finds that poor households are actually less likely to move as housing prices in their areas increase. He reasons that this lack of displacement can be attributed partly to rent controls.

Freeman (2005) uses individual data from the American Community Survey and finds that, although low-income residents of gentrifying neighborhoods were slightly less likely to move at all than were low-income residents of non-gentrifying neighborhoods, those who did move were more likely to move out of their communities than were low-income residents of non-gentrifying neighborhoods who moved. In Freeman's view, rising housing costs reduce within neighborhood moves by indigenous residents, but may encourage immigration of wealthier residents because of improved neighborhood amenities associated with rising housing costs (Freeman, 2005). (N. Edward Coulson et al. (2003) confirm that positive neighborhood externalities from home ownership are priced into housing.

In contrast to Freeman's work, McKinnish, et al. (2010) do not find evidence within individual level census data of displacement and harm to minority households. As mentioned, they find that gentrification of black neighborhoods makes them more attractive to middle-class black families. They defined potentially gentrifying census tracts as those in the bottom quintile of their MSA in terms of median family income and gentrified tracts as those where mean income increased by ten thousand dollars between the 1990 and 2000 censuses (McKinnish, et al., 2010). Although the impacts of gentrification are yet an unsettled question, it is of interest to know the extent to which financial market policy contributes to changes in neighborhood composition.

Evaluation of Community Reinvestment Act

Recent research shows the CRA has had an impact on lending practices but the magnitude is less certain. Belsky, Schill and Yezer (2001) evaluated the impact of the Clinton Administration's 1995 revisions to the CRA enforcement standards. This study finds that CRA regulated lenders operating in their assessment area made a higher proportion of home purchase loans to low- and moderate income (LMI) neighborhoods and have lower rejection rates for loan applications from LMI neighborhoods and borrowers than do regulated lenders operating outside of their assessment area or non-regulated institutions in the same geographic area.⁷ Schwartz (1998) and Bostic and Robinson (2003) find increased CRA-eligible lending or originations for institutions subject to CRA. Avery, Bostic, and Canner (2005) conduct a survey of lending institutions and find that although CRA-related lending is not large, a majority of banks surveyed engage in some lending that they would not have undertaken without the law. Bostic, Mehran, Paulson and Saidenberg (2002) found that banks significantly increased their LMI lending prior to submitting merger and acquisition requests. Apgar and Duda (2003) report that CRA-regulated lenders originate more loans in LMI communities than they would have if the law were not in effect. Further, housing price increases and housing turnover are higher in CRA eligible neighborhoods than in similar non-eligible ones, an indication of greater credit availability. Bhutta (2008) finds an increase in application rates and lending for both home purchase and home refinancing loans in census tracts that are in regulated banks' assessment area compared to ones that are not. Barr (2005) summarizes the literature on the impact of the CRA and concludes that the CRA expanded credit in low- and moderate-income communities.

A number of studies exploit the CRA eligibility threshold and use a regression discontinuity (RD) design to assess the CRA's impacts. The papers compare a cohort of

⁷ For the purpose of CRA evaluations, moderate-income households or neighborhoods are those with median incomes between 50 and 80 percent of their MSA median. A low-income household or neighborhood is one with a median income below 50 percent of the MSA's median (Lee and Campbell, 1997).

neighborhoods just below the CRA's 80 percent eligibility threshold to those just above it (more below). Avery, Calem and Canner (2003) test for the differences in neighborhood conditions using an RD design⁸. They find that CRA-eligible communities expanded homeownership and reduced vacancy rates more than similar CRA-ineligible communities, but results vary with the specification. They decline to make conclusions about CRA's impact due to their lack of robust results. Berry and Lee (2007) evaluate the impact of CRA on loan rejection rates using individual and aggregate data in a well-presented RD design, but do not find evidence that the CRA affected loan rejection rates in eligible neighborhoods. Gabriel and Rosenthal (2008) use a regression discontinuity design and investigate the effects of the CRA and Government Sponsored Enterprises (GSEs) on lending. They find no evidence of a positive effect of GSEs on lending activity, but do find that CRA expand the availability of mortgage loans in eligible areas and find "tentative" evidence that it increases home ownership. They consider only lending and ownership outcomes.

Although the evidence is somewhat equivocal, we conclude that the overall evidence suggests that the CRA expanded access to credit, with uncertain magnitude, but that there is less evidence regarding its impacts on neighborhood characteristics. Increasing property values and home ownership rates are signs that the communities were improving economically. Our study investigates whether CRA eligibility correlates with increases neighborhood measures that are associated with gentrification. It follows Avery, et al.(2003), Berry and Lee(2007), Gabriel and Rosenthal (2008), and exploits the 80 percent of median MSA income eligibility threshold for CRA with a regression discontinuity design.

⁸ To assure that the census tracts were similar, Avery, et al. control for tract characteristics These included (1) the starting rates of each of the dependent variables in 1990, (2) the age of the tract's population under 18 and over 65, (3) the number of people per household, (4) the percent of the tract's population that was nonwhite, (5) the median income of the tract relative to the MSA, (6) the percentage change in total housing units and (7) the percentage change in the tract's median income.

METHOD AND DATA

The regression discontinuity design is well suited to study policies with strict eligibility thresholds. We follow several papers in the literature that apply this method to the CRA⁹. We estimate the size of discontinuities in neighborhood outcomes for those just above and just below the relative income threshold that defines CRA eligible and ineligible tracts¹⁰. We use the term “CRA eligible” as a shorthand only to mean that the tract’s family median income falls below the 80 percent of MSA family median income threshold relevant to CRA scoring for regulated institutions¹¹. Under assumptions discussed below, these outcome differences identify the impact the CRA. We want to test whether CRA eligibility leads to outcomes consistent with gentrification, namely reduced proportion of long-term residents, an increased proportion of college graduates, reduced share of minority residents, lower poverty rates, and higher mean incomes.

Regression Discontinuity

To use a regression discontinuity (RD) to estimate the causal impact of the CRA, we assume that CRA policy treatment is determined by tract income relative to MSA income, i.e., the assignment variable, being above or below the known 80 percent threshold. Outcomes are allowed to be influenced by the assignment variable itself, but the association is assumed to be continuous in the absence of the CRA. Thus any discontinuity in outcomes at the threshold is taken as evidence that the CRA has a causal effect. In essence, conditional on relative tract income, tracts immediately on either side of the arbitrary threshold are taken

⁹ See Berry and Lee (2007) for detailed explanation with an application to CRA or Baum-Snow and Marion (2009) for an application to low-income housing credits. Thistlethwaite and Campbell (1960) provide the first description and application.

¹⁰ Census tracts are used to demark neighborhoods.

¹¹ PMSA values came from the American Fact Finder database of the census bureau. A PMSA is defined by the Office of Management and Budget (OMB), as “a large urbanized county or a cluster of counties (cities and towns in New England) that demonstrate strong internal economic and social links in addition to close ties with the central core of the larger area.”

as similar and CRA treatment is taken as exogenous¹² at the threshold. This result also assumes that tracts cannot affect their chance of crossing the threshold and joining in the CRA treatment group. This is reasonable in our context since relative tract income is based on census data on individuals.

We predict the outcome on either side of the threshold and estimate the common treatment effect as a jump in the expected value of the outcome at the threshold.

$$y_{it} = \beta_0 + \beta_1 CRA_{it} + f(\text{relative income}_0) + \varepsilon_i \quad (1)$$

where y_{it} is an outcome in year 1999, $CRA_{it} = 1$ for tracts below the threshold as based on the 1989 census and $=0$ otherwise, and β_1 measures the impact of CRA. We estimate the impact of the CRA designation that applies through the 1990s. For outcome variables measured as 1999 levels, the coefficient β_1 can be interpreted as a cumulative impact of the CRA up to 1999 (as in Gabriel and Rosenthal, 2009; Avery, Calem, and Canner, 2003). If we measure the change in outcomes over time from 1989 to 1999 as our dependent variable, and one believes that CRA had limited impact prior to 1989, then one can interpret β_1 as the impact of the rule and enforcement changes in the 1990s. Both interpretations occur in the literature.

The function f is a nonlinear function of tract income relative to MSA income in 1989. To gain sample size and allow use of observations not immediately on either side of the threshold, we predict the outcome using a flexible form, a semi-parametric kernel linear regression to estimate f from either side of the threshold. We used a routine for Stata by Nichols(2011) to estimate the regression discontinuity model (more below), which uses a local linear regression with a triangle kernel¹³. As described below, we tested several

¹² This is the assumption of local independence (Lee 2005).

¹³ Nichols (2007) provides a detailed description and cites Cheng et al.(1997) to suggest that a triangle kernel has advantages for predictions at a boundary point.

bandwidths, a measure of the size of the sample interval around the prediction point. Small bandwidths limit bias from including observations more distant from the threshold, but at a cost of precision. Initial optimal bandwidths for the Nichols' method are based on Imbens and Kalyanaraman (2009).

Under the continuity assumption above, conditioning on variables besides relative income (the assignment variable) is not necessary to get correct estimates of the impact on outcomes, but can serve two useful purposes (Lee and Lemieux, 2010). It helps control for tract differences when we use data on tracts further from the threshold. Two, it can help improve precision of the estimate of the jump at the threshold. We present estimates using several alternate covariate sets. In all cases, we follow the literature and control for MSA effects, in our case by deviating the outcome and conditioning variables from their MSA means. Consequently, we estimate outcome variation across the threshold eliminating mean MSA differences. Tables report standard errors clustered by MSA. Later we also show results for each MSA separately. The model with covariates is simply

$$y^*_{it} = \beta_0 + \beta_1 CRA_{it0} + f(\text{relative income}) + \sum_{k=1}^n \gamma_k X^*_{kit0} + \varepsilon_i \quad (2)$$

where y^* is outcome variable deviated from its MSA mean, and X^* is other baseline covariates deviated from their MSA means¹⁴. We allow the baseline covariates to have different slopes on either side of the threshold.

To check the specification, we test for discontinuities in background covariates; such discontinuities could indicate a specification problem. We then use several alternate covariate sets, including some with lagged dependent variables. In particular, if discontinuities are not evident for lagged (1989) outcome variables, but a discontinuity occurs for the current (1999)

¹⁴ An alternate approach for conditioning on covariates is to obtain residuals from regressing the outcomes on covariates and to look for regression discontinuity in the residuals as done in Avery, Calem, Canner (2003). Lee and Lemieux (2010) note both approaches are legitimate.

outcome variable, this gives more credence to a causal effect of the CRA changes in the 1990s (Lee and Lemieux, 2010). In addition, we also use the change in the outcome variable from 1989 to 1999 as the dependent variable as has been commonly used in the literature. Berry and Lee (2007) call these first-difference RD models and suggest that it better controls for baseline tract characteristics. We also show the first difference model with alternate covariate sets and bandwidths.

Sample

This study uses tract data measured in 1989 and 1999, the same years used by Avery, et al. (2003), Freeman (2005), Gabriel and Rosenthal (2008) and McKinnish, et al. (2010). Census tracts were the unit of analysis used for this study and were taken to represent neighborhoods. Tract level variables were computed from the Neighborhood Change Database, a resource constructed by Geolytics. Geolytics normalizes data from the 1980 and 1990 censuses to conform to each tract's 2000 boundaries. This process allows for consistent comparisons of tracts over time¹⁵.

We restrict the sample to MSAs with population of two million or more, resulting in data from nine cities. Gentrification is of primary concern in central cities, and all non-central city tracts were excluded from the sample. As explained by Bhutta (2007), the lending effects of the CRA are stronger in large cities and this may be because CRA enforcement of the law may be less strenuous in smaller cities. Finally, tracts were excluded if their median family

¹⁵ Geolytics (2011) describes the normalization process this way: "The basic procedure was to use geographic information system (GIS) software to overlay the boundaries of 2000 tracts with those of an earlier year. This allowed us to identify how tract boundaries had changed between censuses. We then used 1990 block data to determine the proportion of persons in each earlier tract that went into making up the new 2000 tract. For example, if a 1990 tract split into two tracts for 2000, the population may not have been divided evenly. Our method allows us to determine the exact weight to allocate to each portion." For our purposes, it would have been better to conform the data to 1990 boundaries since this is our baseline, but this normalization was not available to us.

income relative to MSA median family income fell outside of the .7 to .9 range¹⁶. Tracts with relative median incomes between .7 and .8 (exclusive) constituted the CRA eligible treatment group and tracts with relative median incomes between .8 and .9 (exclusive) constituted the comparison group. For robustness checking, we also test narrower windows by using several alternate bandwidths in our estimation¹⁷.

Ultimately, the sample consisted of 1122 tracts— 545 in the CRA eligible cohort and 577 in the comparison cohort. Figure A2 shows maps of the relevant MSAs in our sample and census tract boundaries. The tracts in our sample are outlined in bold, and tracts with relative income below .8 are shaded.

Dependent Variables

We use five measures of neighborhood characteristics that may be associated with gentrification as outcomes: the proportion of poor residents, the proportion with college degrees, the ratio of long-term to total residents, proportion of non-white residents, and mean family income. We also use the change in each variable from 1989 to 1999. Each variable is measured as the deviation from its MSA mean to control for MSA effects. Each measure must be interpreted carefully.

The ratio of poor residents to total residents and real mean family income. Income of residents is key to a conception of gentrification that partly captures the changing economic environment of affected communities. Unfortunately, raw data on average communities' incomes ignores heterogeneity and does not allow us to distinguish whether increases in community mean income are due to income gains of long-term residents or the immigration

¹⁶ Tracts with missing median family income in either year studied were also dropped. This restriction only reduced the sample size by less than one percent of the total sample.

¹⁷ Several studies use the .7 to .9 relative income window to define the sample of tracts in studies of CRA (Berry and Lee, 2007; Avery et al, 200X; Gabriel and Rosenthal, 2008). As robustness checks, Berry and Lee and Gabriel and Rosenthal also use a .75 to .85 relative income window. Gabriel and Rosenthal find the .7 to .9 window produces somewhat stronger results than the full sample.

of higher income families. The mean is sensitive to changes from higher income residents¹⁸. Further, mean income is a delayed indicator because “first generation” gentrifiers such as students and other young people may not have high incomes, but the amenities that follow them ultimately attract higher income in-migrants (Smith, 1996; Lees, 2000; Freeman, 2005; Freeman, 2006). To account for this delay, many studies instead use educational attainment as a substitute for social class (Freeman, 2005; Freeman 2006) as we do below. Nevertheless, income is a natural indicator so we present mean income and poverty rate results as well as education results. The family income variables are adjusted by the CPI to 1999 dollars.

The ratio of residents who hold at least a bachelor’s degree to total residents.

Education changes likely reflect in-migration because long-term resident adult education levels change only slowly. It is difficult to generate large increases in a community’s average educational attainment without the immigration of more educated outsiders.

The ratio of long-time residents to total residents. Freeman (2006) suggests that educational attainment be supplemented by a variable measuring the percentage of the community’s population to have lived in that community for a number of years. Significant in-migration would reduce the proportion of long-term residents. Freeman (2006) uses three years as the cut off to indicate recent movers. Our data from the Neighborhood Change Database provides a slightly rougher indicator at five years.

The ratio of nonwhite residents to total residents. Although racial gaps in home ownership are well established, the degree to which they are due to credit constraints that might be relaxed by the CRA is less clear (Stuart A. Gabriel and Stuart S. Rosenthal, 2005). Furthermore, Freeman (2006) notes that changes in racial composition can be subtle. It is not always characterized by the more stereotypical succession of poor blacks by affluent whites.

¹⁸ Our earlier work also included real median family income. Results are similar to those for real mean family income.

Sometimes it is the succession of poor whites by affluent whites, or poor blacks by more affluent blacks (Mckinnish et al. 2010). (Freeman notes that the succession of poor blacks by affluent blacks is rarely considered to be gentrification by long-time residents.) Sometimes slower growth in the proportion of nonwhites in an already predominantly nonwhite neighborhood compared to other similar communities suggests gentrification.

Background Variables

In some specifications we include 1989 covariates that measures background characteristics of the tracts. As Avery et al. (2003) suggest, we condition on some characteristics of the housing stock that affect gentrification potential: the proportion of housing with one to four units, percentage of housing units more than 20 years old, and baseline age distribution of residents (proportion under 18 and proportion over 65).

RESULTS

Figure 1 shows graphical results for simple bin means and for the local linear regressions. The dependent variables are deviated from MSA means, but no other covariates are included. Bins are evenly spaced over the range. Except for the poverty rate, the local regressions show potential discontinuities with those just below the CRA threshold having more long-term and non-white residents with lower education levels but higher mean incomes. Tests are given below. The bin means fluctuate as we move from the threshold, indicating the desirability of small bandwidths for local regression and/or the addition of covariates to control for variation away from the threshold. Next we present tests for baseline discontinuities in covariates and lagged dependent variables. We then show test results for discontinuity in levels of our outcome variables with several alternate sets of covariates using a common bandwidth. Third we show how results vary based on different bandwidths. Finally we show results for outcome first-difference changes over time.

Test for baseline differences

Before turning to outcomes, we check that tracts on either side of the threshold look similar and check for discontinuities in other background variables, referred to in the literature as balance tests. Table A1 shows means and standard deviations for variables in the 1989 base year and changes over time for the two groups. Although several covariates show differences in means, the removal of MSA effects lessens the differences.

Of more concern is whether there is a jump in background variables at the threshold. To check this, Figure A1 shows plots of bin means for the tract housing stock and residents' age distribution variables as a function of relative tract income. The dots show simple means in the outcome for evenly spaced bins of relative tract income. The lines are the local linear regressions. The functions are not obviously discontinuous at the threshold. Yet, tests for discontinuity using local linear regressions in Table A2 show that age distribution and proportion of housing with one to four units have statistically significant discontinuities. Consequently, we present most results conditioning on these age-distribution and housing stock variables.

The next balance test is for lagged dependent variables, that is, the 1989 values. The top row of Table 1 shows estimates of the discontinuity in lagged dependent variables (MSA mean deviated) conditioning on the age distribution and housing stock variables. The table shows a 1.7 percent jump in family mean income at the threshold, but no statistically significant jump in the other lagged dependent variables. For income, this suggests that meaningful CRA impacts require a large discontinuity relative to this initial jump. We will also estimate models that condition on lagged income. For other dependent variables, the lack of discontinuity in lagged dependent variables gives greater credence to any observed discontinuity in subsequent outcomes, to which we now turn.

Results for Outcome Variables in 1999 Levels

We look at discontinuities in outcome levels in 1999 at the CRA threshold in the second row of Table 1. These can be interpreted as cumulative effects of the CRA up to 1999 (Gabriel and Rosenthal, 2009; Avery, Calem, and Canner, 2003). Table 1 shows two statistically significant discontinuities. Conditional on age distribution and housing stock variables, the proportion of long-term residents jumps about four percent for the CRA eligible tracts at the threshold. This is not consistent with the CRA inducing gentrification that displaces long-term residents. Mean family incomes jump by about eight percent (incomes are in logs), a result consistent with income gains for current residents or immigration of higher income residents. The other dependent variables do not show a statistically significant discontinuity and coefficients tend to be small. For the proportion of long-term residents and mean family income, the jump in 1999 is sizeable compared to the discontinuity observed in 1989, and this reinforces the causal interpretation of CRA induced effects during the 1990s.

Table 2 shows how estimates vary with alternate covariate sets using a common bandwidth¹⁹. In all cases the dependent variables are deviations from MSA means. As we move down the table, the estimates are conditioned on additional covariates. The top row shows that with no other covariates, we observe a significant discontinuity with those on the CRA eligible side showing a six percent increase in long-term residents and a four percent reduction in the proportion with a college degree. Neither is consistent with traditional gentrification. As we condition on more variables, the precision improves except for the model of long-term residents. The family income result is robust to conditioning on lagged family income. Overall, the specifications with at least some covariates show a fairly

¹⁹ A common bandwidth standardizes the samples for the dependent variables to allow better comparisons. We chose the middle value across the dependent variables of the optimal bandwidths provided by Nichols' Stata rd command explained above. Later we show alternate bandwidths.

consistent pattern that mean family income and the proportion of long-term residents are higher for CRA eligible tracts at the threshold but other outcomes do not show statistically significant discontinuities.

Since results from the different covariates sets are fairly robust, we now turn to different bandwidths for the local linear regression. Smaller bandwidths restrict the sample used to estimate the discontinuity to be closer to the threshold. This decreases bias from using observation too far away but at a potential cost of precision. To simplify the presentation, we present the models with the full set of covariates and MSA deviated variables. Figure 2 shows estimates of the discontinuity for bandwidths varying from .1 to .12 (tables above used .041). A vertical line shows the optimal bandwidth from Stata `rd` based on Imbens and Kalyanaraman (2009). It reveals that smaller bandwidths increase the size and statistical significance of the estimated discontinuity for the proportion of long-term residents, with CRA eligible tracts showing more long-term residents. The results for mean income generally show a positive discontinuity for the CRA eligible tracts at low to moderate bandwidths. There is no evidence of discontinuity in the other dependent variables. All graphs show that at large bandwidths the estimated discontinuities attenuate towards zero as we would expect when we include less relevant tracts. Overall, we conclude that the results for proportion of long-term residents and mean income look robust.

First-difference in Outcomes

The first-difference models show how CRA eligibility affects changes in our outcome variables from 1989 to 1999. As discussed earlier, this is the period when the CRA became more salient to lenders due to changes in rules and enforcement. We remain interested in whether there is a discontinuity in *changes* at the threshold. Such a first-difference model might be thought to more fully condition on tract background. Later we add

additional background covariates to the first-difference model with the same intention of reducing standard errors and better controlling for tract characteristics away from the threshold.

A visual representation of the first differences is shown in Figure 3. The first differenced dependent variables are MSA mean deviated but no other covariates are included. Visual inspection suggests a possible discontinuity for share non-white and for family income, although family income shows sizeable fluctuations. We turn to tests in Table 3. The pattern that emerges tells the same story as that from the levels models for family income. Precision tends to improve somewhat as we add covariates. Conditional on age distribution and housing stock variables and lagged dependent variables, tracts just below the CRA threshold have about a seven percent greater increase in mean incomes over the 1989 to 1999 period. The proportion of long-term residents no longer shows a significant discontinuity at this bandwidth, but, as before, it shows a statistically non-zero effect at smaller bandwidths. Other dependent variables show no significant discontinuities in first-differences. For different bandwidths, the results appear to have the same pattern as the levels model as can be seen in Figure 4. The models in the figure use covariates including lagged dependent variables. Generally, the first-difference models and the levels models produce reassuringly similar results.

MSA results

Because enforcement of the CRA may vary by state and by region (Barr, 2005), we also estimated models separately by city. Table 4 displays estimates of the discontinuity at the CRA relative income threshold using a local linear kernel regression in relative income as before. In these specifications, most cities show a larger decline in proportion of long-term residents in CRA eligible areas, significantly so in Philadelphia and Chicago. The impact of

CRA on the change in proportion of college graduates and incomes is mixed. Several cities show a rise in the proportion of non-white residents at the threshold for the CRA eligible tracts relative to CRA non-eligible tracts, Houston and Los Angeles significantly so.

Results vary by city and this could reflect small sample sizes, or that the role of the CRA or process of gentrification varies by geography, or that the impact of CRA is small relative to other development factors that vary across tract borders within an MSA. Overall the city results do not paint a clear picture of how the CRA changes neighborhoods. The MSA specific data are strong enough to produce some well-estimated impacts, which suggests that future MSA specific research that exploits local institutional knowledge and geography could be fruitful.

CONCLUSION

This study examines the effects of the CRA on local communities. Past research suggests that the CRA had a positive effect on lending activity in eligible areas. This credit expansion has been shown to affect home ownership and may have affected credit for business in these areas, both of which could be catalysts for neighborhood change. Based on a threshold that distinguishes CRA eligible from CRA ineligible neighborhoods (census tracts) in a sample of tracts in large MSAs, our regression discontinuity estimates imply that the CRA may raise mean incomes but did not lead to neighborhood changes that typically would be considered indicative of gentrification. There is no robust evidence that it affected racial composition, education levels or poverty rates. The most robust results show a discontinuity with tracts just below the CRA threshold tending to have about two to four percent more long-term residents and six to seven percent higher average incomes compared to similar tracts just above the threshold. Thus CRA eligibility may encourage income growth, but does not appear to induce displacement of long-term residents. This is more

consistent with CRA contributing to the improved status of incumbents or “incumbent upgrading” rather than traditional in-migrant gentrification. These results are robust to difference sets of conditioning variables and different bandwidths for our local linear regression. Admittedly, our estimates are often imprecise which recommends caution in making strong conclusions.

Why does CRA have a limited effect on neighborhood composition? Given all the relevant considerations in housing purchases and local development, including targeted GSE lending that would have applied to all tracts in our sample and expansion of subprime lending during this period, the CRA induce financing may not have produced large enough differences to significantly change neighborhood composition. Second, neighborhoods are heterogeneous and neighborhoods characteristics that give rise to different trends may overwhelm the CRA effect. But this consideration loses force in that the regression discontinuity method does not depend on a full set of background covariates, but rather only depends on continuity in the outcome variables as a function of relative income near the cutoff in the absence of CRA. Furthermore, we found results robust to changes in conditioning covariates. Finally, as mentioned by Avery, et al., the CRA might have had its biggest impacts shortly after passage, and our difference model misses the early impact. But in that case our cumulative “levels” model should pick up the early impacts.

For future research, researchers might try to determine whether some characteristics of neighborhoods make them more or less prone to change over time. That is, might CRA and other credit expansion activities have more of an impact in areas with higher mobility and how might this be related to the underlying structure of industry? Further, does the geographic arrangement of high and low-income tracts in MSAs influence the impact of credit expansions?

Overall, evidence from the literature suggests that the CRA expanded access to credit in poor neighborhoods. Our paper shows that apart from raising incomes, we find little evidence that it changed neighborhoods in ways associated with in-migrant gentrification.

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TABLE 1—REGRESSION DISCONTINUITY AT CRA THRESHOLD:
DEPENDENT VARIABLE LEVELS IN 1999 COMPARED TO LAGGED DEPENDENT VARIABLES

Variable	Prop. Long-term Residents	Poverty Rate	Proportion with College Degree	Share Non-white	Mean Family Income
Lagged Dependent Variable (1989 values)					
CRA indicator	.0224 (1.1)	.0079 (1.1)	-.00044 (.028)	.042 (.75)	.0165** (2.1)
Dependent Variable in 1999					
CRA indicator	.0392** (2.3)	-.005 (.68)	-.0098 (.44)	.0107 (.17)	.0797*** (2.6)

Notes: Sample tracts with income relative to MSA between .7 and .9. Variables are deviated from MSA means. Conditioned on 1989 age distribution and housing stock variables. Bandwidth is .041 for all models. Z values based on cluster adjusted standard errors in parentheses; * p<.1; ** p<.05; *** p<.01.

TABLE 2—REGRESSION DISCONTINUITY AT CRA THRESHOLD:
DEPENDENT VARIABLE LEVELS WITH ALTERNATE COVARIATE SETS

Variable	Prop. Long-term Residents	Poverty Rate	Proportion with College Degree	Share Non-white	Mean Family Income
No covariates, MSA deviated					
CRA indicator	.0635*	-.0025	-.0486*	.0504	.0552
	(1.75)	(.25)	(1.75)	(.67)	(1.2)
Conditioned on 1989 tract age distribution and housing stock variables, MSA deviated					
CRA indicator	.0392**	-.005	-.0098	.0107	.0797***
	(2.3)	(.68)	(.44)	(.17)	(2.6)
Conditioned on tract age distribution, housing stock variables plus own lagged dependent variable, MSA deviated					
CRA indicator	.0237	-.0085	-.0093	-.0226	.0659**
	(1.2)	(1.2)	(.59)	(.78)	(2.5)
Conditioned on tract age distribution, housing stock variables plus all lagged dependent variables, MSA deviated					
	.0221	-.0089	-.0096	-.0197	.0674***
	(1.0)	(1.5)	(.64)	(.79)	(2.9)
N	1122	1122	1122	1122	1122

Notes: Sample tracts with income relative to MSA between .7 and .9. Variables are deviated from MSA means. Bandwidth .041 for all models.
Z values based on cluster adjusted standard errors in parentheses; * p<.1; ** p<.05; *** p<.01.

TABLE 3—REGRESSION DISCONTINUITY AT CRA THRESHOLD:
FIRST-DIFFERENCES IN DEPENDENT VARIABLE WITH ALTERNATE COVARIATE SETS

Variable	Prop. Long-term Residents	Poverty Rate	Proportion with College Degree	Share Non-white	Mean Family Income
No covariates, MSA deviated					
CRA indicator	.0219 (.92)	-.0107 (1.3)	-.017 (.95)	-.036 (1.3)	.0573* (1.7)
Conditioned on 1989 tract age distribution and housing stock variables, MSA deviated					
CRA indicator	.0177 (.69)	-.0129 (1.5)	-.0091 (.56)	-.0318 (1.3)	.0644** (2.5)
Conditioned on tract age distribution, housing stock variables plus own lagged dependent variable, MSA deviated					
CRA indicator	.0247 (1.2)	-.0087 (1.2)	-.0091 (.56)	-.0231 (.81)	.0671** (2.5)
Conditioned on tract age distribution, housing stock variables plus all lagged dependent variables, MSA deviated					
CRA indicator	.0232 (1.1)	-.0091 (1.5)	-.0095 (.61)	-.0201 (.83)	.0685*** (2.9)
N	1122	1122	1122	1122	1122

Notes: Sample tracts with income relative to MSA between .7 and .9. Variables are deviated from MSA means. Bandwidth .040 for all models.

Z values based on cluster adjusted standard errors in parentheses; * p<.1; ** p<.05; *** p<.01

TABLE 4—CRA IMPACT ON CHANGES IN DEPENDENT VARIABLES FOR CITIES:
RD USING LOCAL LINEAR REGRESSION

City (N)	Change in proportion of long-term residents	Change in poverty rate	Change in proportion of college educated residents	Change in proportion non-white	Change in real mean family income
Boston (63)	0.0404 (1.07)	-0.0148 (0.37)	0.00216 (0.04)	-0.0151 (0.22)	0.211 (1.63)
Chicago (218)	-0.0706* (2.47)	-0.00753 (0.46)	0.0507 (1.77)	0.0264 (0.7)	0.0211 (0.35)
Dallas (50)	-0.0227 (0.40)	0.000514 (0.03)	0.0976* (2.45)	-0.00368 (0.04)	0.214** (2.97)
Detroit (66)	-0.0564 (1.23)	0.0471 (1.24)	0.037 (1.64)	-0.0894 (1.39)	-0.0649 (1.04)
Houston (67)	-0.0493 (1.33)	0.00734 (0.28)	-0.106* (2.34)	0.206** (2.95)	-0.00279 (0.02)
Los Angeles (156)	-0.0121 (0.41)	0.00837 (0.22)	-0.0338 (1.70)	0.225*** (5.70)	-0.151 (1.81)
New York City (359)	0.0428 (1.78)	0.0177 (0.66)	-0.00582 (0.26)	-0.0291 (0.46)	-0.108 (1.61)
Oakland (29)	-0.11* (1.81)	-0.0361 (0.51)	0.0223 (0.53)	0.00818 (0.2)	-0.498 (1.53)
Philadelphia (101)	-0.0695* (1.82)	0.0335 (0.9)	-0.00759 (0.28)	0.0851 (1.09)	-0.152 (1.66)

Notes: Sample consists of census tracts with relative income between .7 and .9 of MSA median family income. CRA eligible tracts have income less than .8 of their MSA median family income. z values in parentheses for Wald Statistic. Local Linear Regression using default optimal bandwidth. *p<.10, **p<.05, *** p<.01

TABLE A1—CHARACTERISTICS OF CRA ELIGIBLE AND INELIGIBLE CENSUS TRACTS

	CRA Eligible (Below .8 of MSA Median Income)		CRA Ineligible (Above .8 of MSA Median Income)	
	Mean	Std Dev	Mean	Std Dev
1989 Values				
Proportion of long-term residents (persons 5+ years old residing in the same house five years ago)	.543	.156	.522	.155
Poverty Rate	.138	.06	.175	.062
Proportion of college educated residents (persons 25+ years old who have a bachelors or graduate/professional degree)	.18	.116	.162	.121
Proportion of non-white residents	.412	.32	.501	.307
Log Real Median Family Income	10.7	.084	10.6	.084
Log Real Mean Family Income	10.9	.125	10.8	.117
Median tract income as proportion of median MSA income	.847	.029	.751	.028
Proportion of total housing units owner occupied	.405	.224	.38	.21
Total number of owner occupied housing units	627	506	560	451
Proportion of total housing units consisting of 1 to 4 units	.625	.283	.627	.297
Proportion of total housing units constructed prior to 1969	.826	.219	.841	.206
Proportion of total population age 18+	.226	.069	.248	.073
Proportion of total population age 65+	.134	.07	.116	.062
Proportion of total housing units vacant	.062	.045	.075	.05
Change in Variable from 1989 to 1999				
Proportion of long-term residents (persons 5+ years old residing in the same house five years ago)	-0.0269	0.0902	-0.0237	0.0835
Poverty Rate	0.0311	0.0643	0.0255	0.0837
Proportion of college educated residents (persons 25+ years old who have a bachelors or graduate/professional degree)	0.0367	0.0718	0.026	0.077
Proportion of non-white residents	0.0923	0.1264	0.0667	0.1404
Log Real Median Family Income	-0.0814	0.2265	-0.0875	0.2536
Log Real Mean Family Income	-0.0183	0.2043	.00022	0.2397
Log Median home value	-0.0515	0.34	0.0155	0.3774
Observations	577		545	

Note: Sample consists of census tracts with relative income between .7 and .9 of MSA median family income.

TABLE A2—REGRESSION DISCONTINUITY AT CRA THRESHOLD IN 1989 TRACT
AGE DISTRIBUTION AND HOUSING STOCK VARIABLES

Dependent Variable	Proportion of housing with 1-4 units	Proportion of housing 20 or more years old	Proportion of residents age 18 or less	Proportion of residents age 65 or more
CRA indicator (bandwidth .041)	.0932*** (3.0)	.0124 (.38)	.0240 (1.4)	-.0093 (.82)
CRA indicator (bandwidth .0205)	.156** (2.4)	.012 (.26)	.0317*** (2.8)	-.0265* (1.7)
CRA indicator (bandwidth .082)	.0560** (2.2)	.0248 (.93)	.0203 (1.3)	-.0039 (.54)
N	1122	1122	1122	1122

Notes: Sample tracts with income relative to MSA between .7 and .9. Variables are deviated from MSA means. Z values based on cluster adjusted standard errors in parentheses; * p<.1; ** p<.05; *** p<.01.

Figure 1 Regression Discontinuity in Outcome Levels at CRA Threshold

Figure 2 Regression Discontinuity in 1999 Outcome Levels by Bandwidths

Figure 3 Regression Discontinuity in First-differences in Outcomes at CRA Threshold

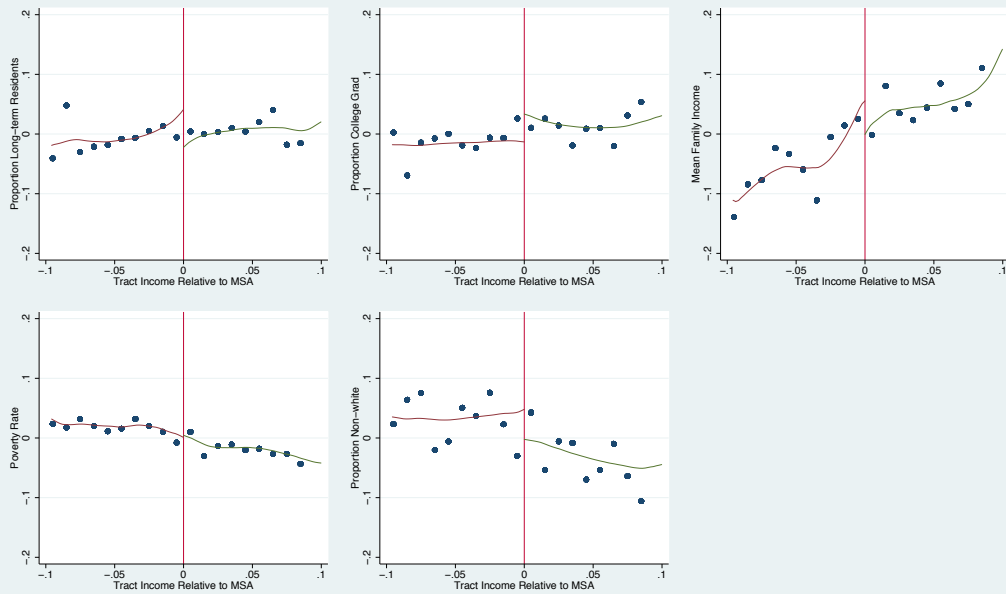
Figure 4 Regression Discontinuity in First-differences in Outcome by Bandwidths

Figure A1 Regression Discontinuity in Covariates

Figures A2a, A2b Maps of Cities

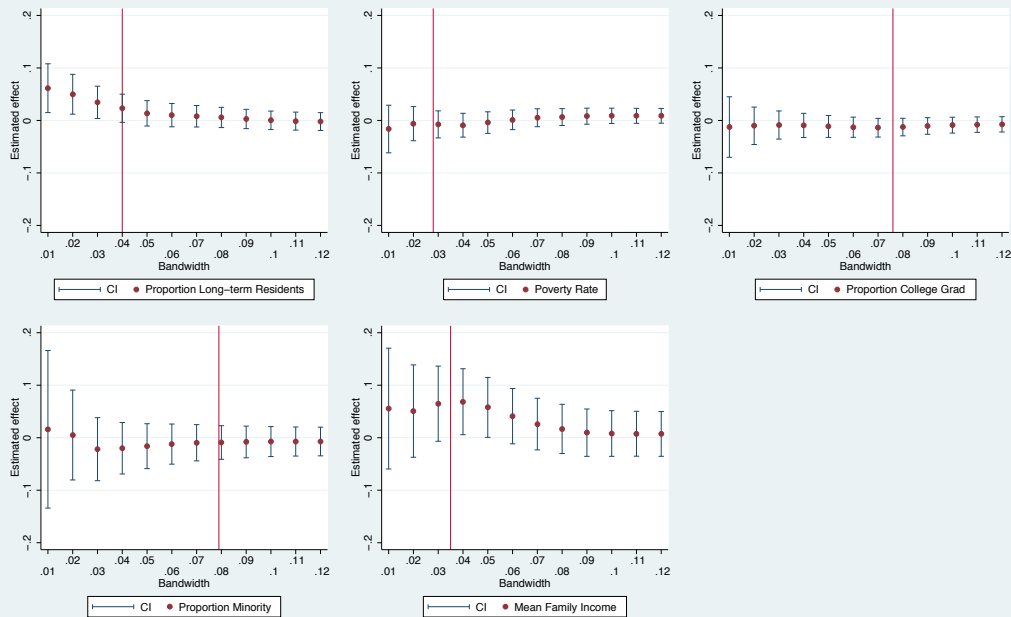
Figure 1

Regression Discontinuity in Outcome Levels at CRA Threshold



Note: Zero Corresponds to Relative Tract Income Threshold of .8
Dots are bin means, lines are local regressions

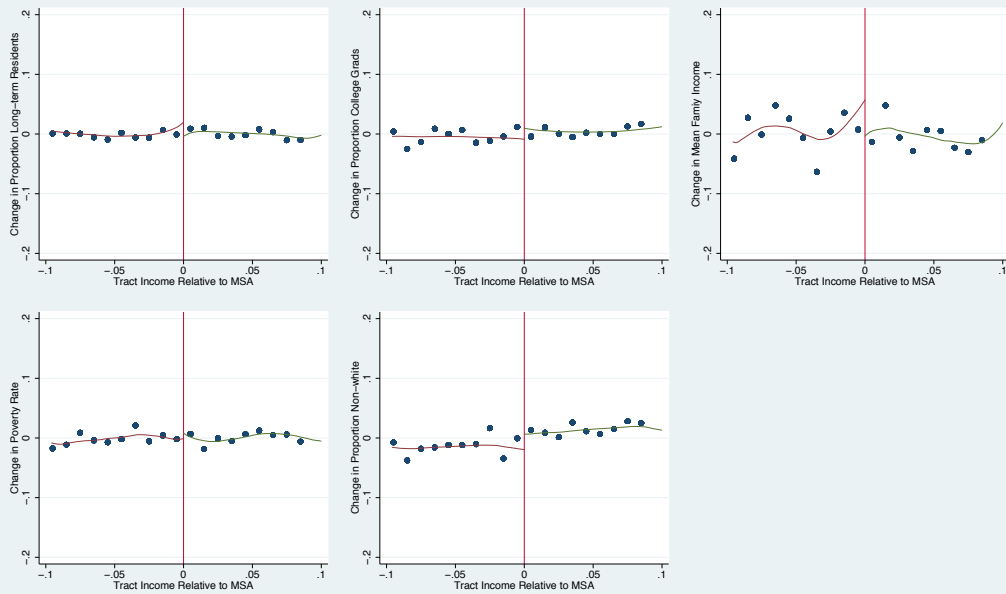
Figure 2
Regression Discontinuity in 1999 Outcome Levels by Bandwidth



Note: Discontinuity from local linear regression with covariates, outcomes demeaned by MSA. Red vertical line at optimal bandwidth.

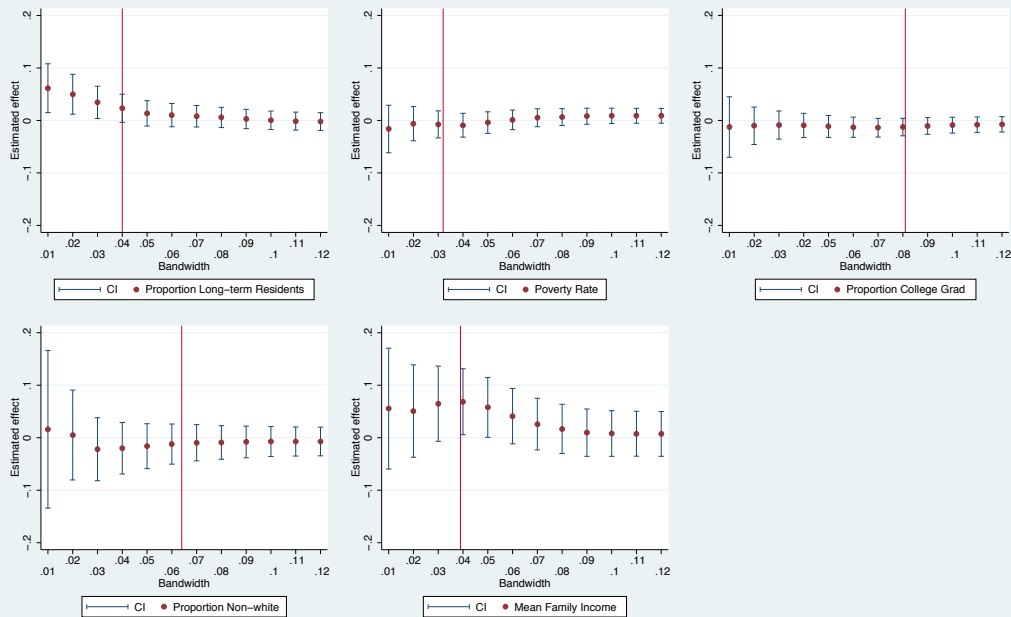
Figure 3

Regression Discontinuity in Outcome First Differences at CRA Threshold



Note: Zero Corresponds to Relative Tract Income Threshold of .8
Dots are bin means, lines are local regressions

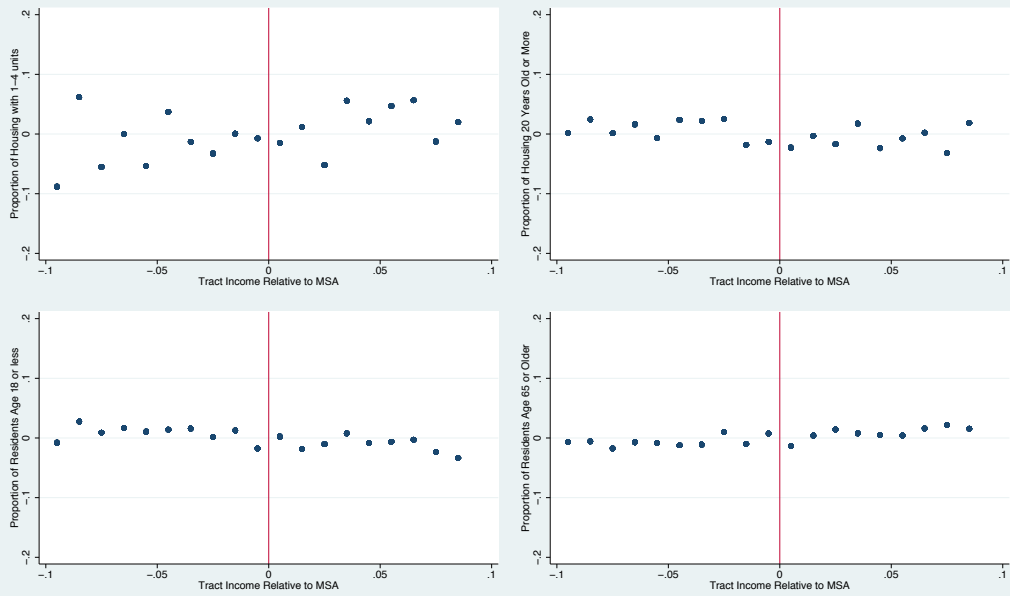
Figure 4
 Regression Discontinuity for First-differences in Outcomes by Bandwidth



Note: Discontinuity from local linear regression with covariates, outcomes demeaned by MSA. Red vertical line at optimal bandwidth.

Figure A1

Regression Discontinuity in Age and Housing Covariates at CRA Threshold



Note: Zero Corresponds to Relative Tract Income Threshold of .8
Dots are bin means



Figure A2: Boston, MA

Maps of Selected Areas within PMSAs

*Grey shading indicates tract median income below 80% of PMSA median income.

**Bolded outlines indicate tract median income between 70% and 90% of PMSA median income.

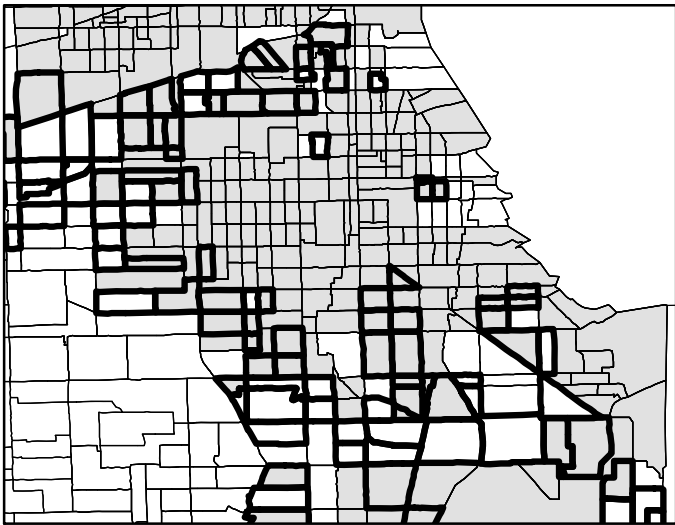


Figure A3: Chicago, IL

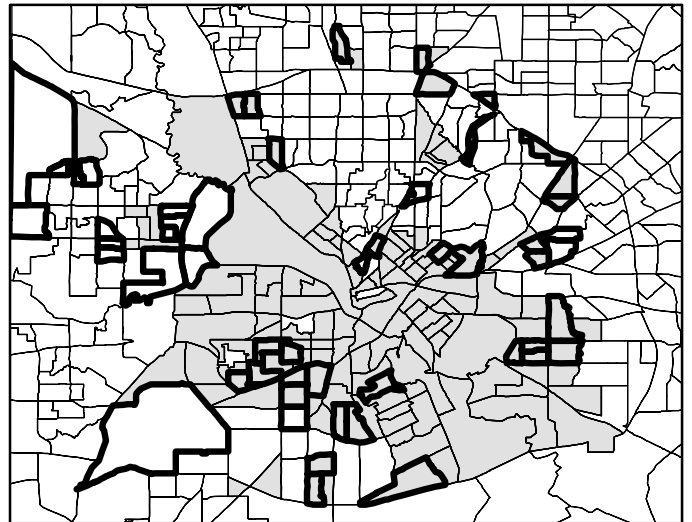


Figure A4: Dallas, TX



Figure A5: Detroit, MI

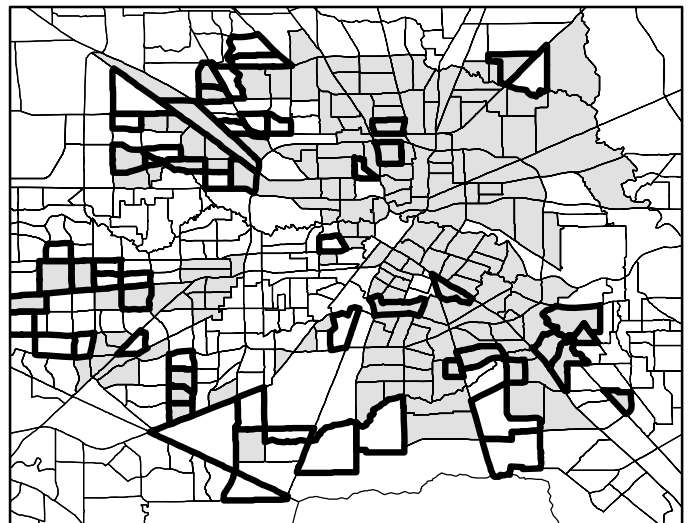


Figure A6: Houston, TX

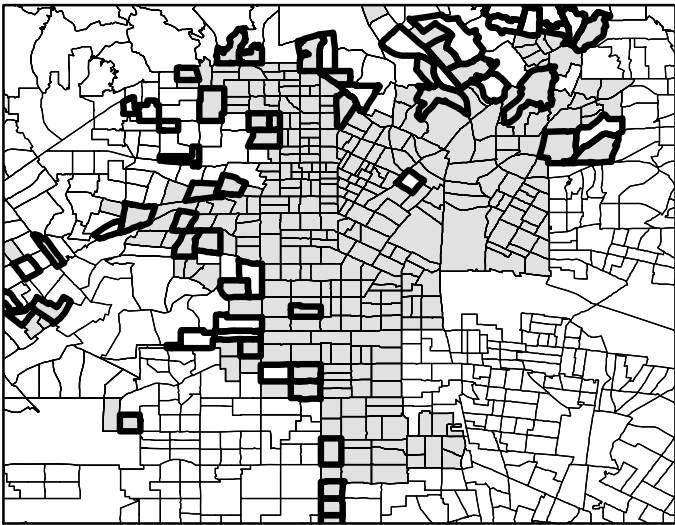


Figure A7: Los Angeles, CA

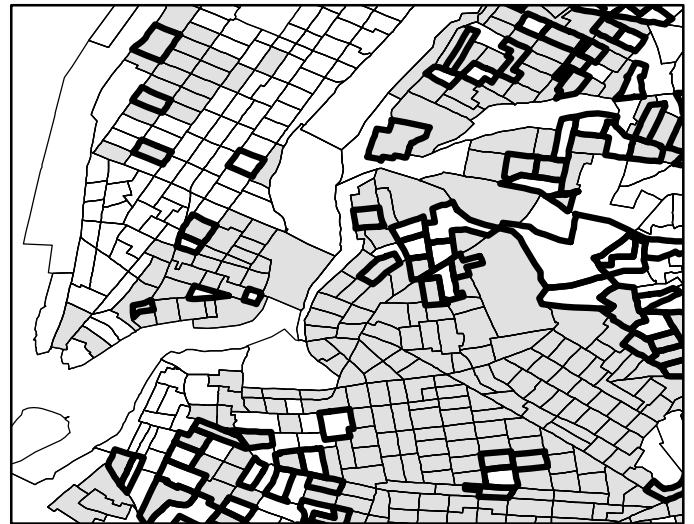


Figure A8: New York, NY



Figure A9: Oakland, CA



Figure A10: Philadelphia, PA

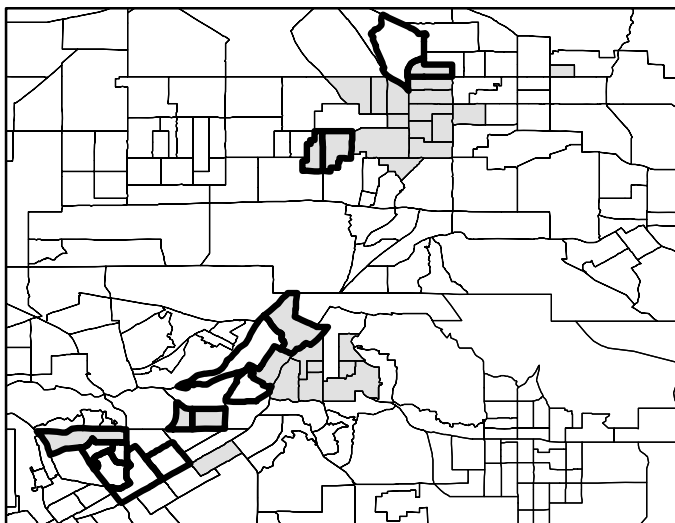


Figure A11: Riverside, CA