The Community Reinvestment Act after Thirty Years

Christopher R. Berry
crberry@uchicago.edu

Sarah L. Lee
sllee@uchicago.edu

The University of Chicago
Harris School of Public Policy

Abstract: The Community Reinvestment Act of 1977 (CRA) is the most important federal legislation designed to increase the availability of credit to low-income and minority borrowers and their communities. Proponents point to nearly $1 trillion in CRA-eligible lending as evidence that the act has had a significant impact. However, critics counter that other factors—such as technological changes, market restructuring, and the decline of racial discrimination generally—account for the lending growth in low-income markets, making CRA irrelevant. In this paper, we apply regression discontinuity (RD) analysis, a design that arguably is stronger for causal inference than any other except a randomized experiment, to identify the effects of CRA. The discontinuity we identify is based on the rule that gives banks CRA credit for lending to individuals and census tracts with income less than 80 percent of their MSA median. Using a data set with millions of loan application records from 1995 to 2002, we analyze CRA's effect on both individual and neighborhood lending and related outcomes. For individual loans, we use a pure RD design. At the neighborhood level, we use a matched-pair RD design based on comparing geographically neighboring pairs of census tracts in which one tract is just above the CRA eligibility threshold and its neighbor is just below it. We find no evidence that CRA has causally influenced any of the neighborhood- or individual-level outcomes that we examine, except possibly in Los Angeles.

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1. Introduction

The Community Reinvestment Act (CRA) is the most important piece of federal legislation addressing access to credit for low-income families and neighborhoods. Enacted in 1977 in response to concerns about *redlining*, CRA established "continuing and affirmative obligations" for lenders to meet the credit needs of low- and moderate-income (LMI) neighborhoods. Advocates point to nearly $1 trillion in CRA-eligible lending and rapid growth in credit for minority and LMI borrowers as evidence that the act has had a significant impact on its intended communities. However, critics counter that other factors, such as technological changes and market restructuring, account for the growth in lending to LMI markets, making CRA irrelevant. Existing empirical studies have not convincingly disentangled the effects of the law from other social and economic trends, and the controversy over CRA continues.

In this paper, we apply regression discontinuity analysis, a design that arguably is stronger for causal inference than any other except a randomized experiment, to identify the effects of CRA on lending to LMI individuals and neighborhoods. Specifically, the regulation uses a strict cut-off of 80 percent of metropolitan area (MSA) median family income to identify LMI neighborhoods and individuals, loans to which count toward a lending institution's CRA performance rating. Using this RD design and a data set comprising millions of mortgage applications from 1995 to 2002, we are able to distinguish the causal effects of CRA from other (observable and unobservable) variables correlated with residential lending rates. To preview our results, we do not find significant effects of CRA on the outcomes we examine.

The paper proceeds as follows. The next section reviews the history of the act and explains the relevant discontinuity in the regulatory criteria used for CRA ratings. Section 3 surveys the related literature and discusses the limitations of methods used in existing studies for making causal inferences about CRA. Our regression discontinuity design is explained in section 4, while section 5 describes the data used in our analysis. The main results are presented in section 6, and section 7 discusses the implications of our findings and directions for future research.

2. History and Overview of CRA

2.1 Regulatory History

CRA was passed in 1977 as part of a package of laws, including the Home Mortgage Disclosure Act (HMDA) and the Equal Credit Opportunity Act (ECOA), intended to improve credit availability and homeownership opportunities in LMI neighborhoods. Supporters of CRA argued that the law was needed to combat "redlining," a practice by which banks refused to make loans in LMI or minority neighborhoods. The act stated that all federally insured depository institutions (a category that does not include credit unions and independent mortgage companies) have a "continuing and affirmative obligation to help meet the credit needs of the local communities in which they are chartered" (12 USC § 2901). CRA required federal banking regulators, in the course of their supervisory examinations of depository institutions, to evaluate a bank's history of lending in LMI neighborhoods. The regulatory bite of the statute came from the requirement that the regulators consider a bank's CRA evaluation when reviewing applications for such activities as establishing or relocating a branch office or engaging in a merger or acquisition. However, the regulation, as originally written, included little direction as to the specific criteria that should be used to assess how well a bank met the credit needs of its local community. Evaluators tended to focus on the procedures a bank used to evaluate loan applications and on their plans to enhance lending in LMI neighborhoods rather than on the record of loans actually extended. Moreover, external scrutiny of banks' lending records was limited because CRA ratings were not made public (Apgar and Duda 2003).

For the first decade after its passage, CRA was derided even by its supporters as ineffectual and cumbersome (Zinman 2002). The regulation was strengthened in 1989 with the passage of the Financial Institutions Reform, Recovery, and Enforcement Act (FIRREA), which required regulators to publicly disclose banks' CRA performance evaluations and ratings. FIRREA also expanded the scope of data that banks must disclose through HMDA to include the race and income of borrowers and the disposition of their mortgage applications. Additionally, it adopted a simple four-tier rating system for CRA exams that replaced the original regulation's relatively complicated numeric system. Banks received a rating of either *outstanding*, *satisfactory*, *needs to improve*, or *substantial noncompliance*. At the same time that Congress strengthened the letter of the law, CRA-regulators began to strengthen their enforcement of it. 1989 marked the first ever denial of a merger application on CRA grounds, when regulators blocked Continental Bank's acquisition of Grand Canyon Bank of Scottsdale.

The most significant reform of CRA began in 1993, when President Clinton directed banking agencies to revise and strengthen the regulation. Final changes were issued in 1995 and phased in through 1998. The 1995 reforms changed the focus of CRA evaluation from process to performance (Barr 2005). As detailed in the next section, the reforms instituted objective performance measures for CRA ratings based on a three-part test of lending, investments, and services. Importantly, the 1995 reforms also allowed banks to get CRA credit for lending to LMI *borrowers* regardless of their location, a shift from the original law's exclusive focus on LMI *neighborhoods*. In addition, banks were allowed discretion in choosing whether the activities of their affiliated mortgage companies would be included in their CRA evaluation.

Finally, two other financial regulatory reforms in the 1990s indirectly strengthened CRA. The 1994 Riegle-Neal Interstate Branching and Efficiency Act allowed interstate branching, which set the stage for a wave of mergers and acquisitions throughout the industry in the following years. Because a satisfactory CRA rating is required for institutions wishing to take advantage of these new opportunities, Riegle-Neal enhanced the incentives for lenders to meet CRA performance tests. Next, the Gramm-Leach-Bliley Financial Modernization Act of 1999 (GLBA) expanded the ability of banks to offer certain securities and insurance products. Again, banks wishing to enter these lucrative markets had to satisfy regulators' CRA performance exams.

2.2 CRA Evaluations and Enforcement

Under the 1995 CRA reforms, large depository institutions[1] are subject to a specific lending test, as well as a service test and investment test. The lending test consists of five components: (1) the number and amount of loans in an institution's assessment area; (2) the geographic distribution of loans; (3) the distribution of loans

[1] Large depository institutions are defined as having assets over $250 million or being part of a bank holding company with assets over $1 billion.

across borrower characteristics; (4) community development lending; and (5) innovative lending practices. The 'geographic distribution of loans' component is based specifically on the distribution of loans to LMI neighborhoods[2]; while the borrower characteristics component looks specifically at the distribution of loans to LMI individuals (12 CFR § 25.22).

For both neighborhoods and individuals, income categories are defined by the regulation as follows: low-income is less than 50% of MSA median family income; moderate-income is at least 50% but less than 80% of MSA median income; middle-income is at least 80% but less than 120% of MSA median income; and upper-income is at least 120% of MSA median income (12 CFR § 25.12).[3] Banks get credit in their CRA ratings for the number and amount of loans originated in LMI neighborhoods and to LMI borrowers; i.e., those with incomes less than 80 percent of the MSA median. It is this 80 percent threshold in the CRA evaluation criteria that we exploit for our RD design, explained below. The lending test is given the most weight in the CRA evaluation, accounting for 50 percent of the overall rating. No bank can receive an overall rating of satisfactory or outstanding if it does not receive a rating of at least satisfactory on its lending test; conversely, any bank that obtains an outstanding rating on the lending test is guaranteed an overall rating of at least satisfactory (Federal Reserve Bank of Dallas 1997, Barr 2005).

Depository institutions are only evaluated on the basis of loans made in their *assessment area*. An assessment area consists of the geography (MSA, county, city or town) in which a bank has its main office, branch offices, or deposit-taking ATM's and

[2] 'Neighborhoods' and 'Census tracts' are used interchangeably.
[3] A neighborhood's income is defined by the median family income for that Census tract.

may also include "surrounding geographies in which the bank has originated or purchased a substantial portion of its loans." A bank may adjust its assessment area boundary if it includes areas that it may not "reasonably … be expected to serve," however the assessment area must still contain whole geographies and cannot arbitrarily exclude LMI neighborhoods (12 CFR § 25.41).

CRA is enforced by the same four federal agencies charged with safety-and-soundness supervision of the banking sector (Zinman 2002). Depending on its type of charter, a bank is regulated by the Federal Reserve System (FRB), the Office of the Comptroller of the Currency (OCC), the Federal Deposit Insurance Corporation (FDIC), or the Office of Thrift Supervision (OTS).[4] Staff of these agencies evaluate banks based on the regulatory criteria described above and issue CRA ratings and written performance evaluations.[5] Although the agencies are charged with enforcing the same regulation and have made formal and informal efforts to coordinate their practices, there is evidence that the strictness of the evaluations varies by agency and also by region (Thomas 1998).

The regulators consider CRA performance when reviewing a bank's application for a merger, acquisition, opening or closing of branches, or expanding line of business. In addition, public watchdog groups rely on CRA ratings to pressure banks for increased community lending and may lodge CRA-based protests of bank applications for the activities just mentioned. Johnson and Sarkar (1996) demonstrate that CRA protests are harmful to a bank's stock price.

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[4] National banks are regulated by the OCC, state-chartered banks are regulated by either the FRB or the Federal Deposit Insurance Corporation FDIC, and savings associations are regulated by the OTS. Credit unions are exempt from CRA.

[5] Frequency of CRA exams varies by agency and bank circumstances, but most banks are evaluated biannually.

Although the 1995 reforms went a long way toward making CRA evaluations quantitative and objective, there is no hard-and-fast rule that translates a bank's share of lending to LMI communities into a rating on the lending test. Bank examiners are allowed discretion in considering a bank's "performance context" when assigning a CRA rating. Performance context may include factors such as economic and market conditions, a bank's business plan or operating constraints, as well as the performance of other similar institutions (Barr 2005). Thus, while the number and amount of a bank's loans to LMI individuals and neighborhoods are the basis for its rating on the lending test, the process of translating lending activity into CRA ratings is not formulaic.[6]

3. Related Literature

Although CRA remains controversial among scholars, bankers, and policymakers, there is little disagreement over the basic fact that lending to LMI individuals and neighborhoods increased substantially in the 1990s. For instance, a study by Harvard's Joint Center for Housing Studies (JCHS) found that home purchase loans to LMI borrowers and neighborhoods increased by 77 percent from 1993 to 2000, exceeding the 53 percent overall increase in home purchase lending. Furthermore, home purchase loans made to black borrowers increased 94 percent from 1993 to 2000, while the increase for white borrowers was only 27 percent (JCHS 2002).

Beyond the generally accepted evidence that mortgage lending has been increasing among the individuals targeted by CRA lies a vigorous debate about the origins of this increase and, even, whether CRA has helped or hindered the underlying

[6] For the purposes of our analysis, as explained below, the only necessary condition is that a bank's CRA rating is monotonically increasing in LMI lending, which is the case under the present system.

trend.[7] During this period, a number of factors external to CRA contributed to the lending increases, making it particularly difficult to isolate the effect of the regulation. The most important of these external factors included automated underwriting and credit scoring systems, low down payment mortgages, Riegle-Neal, the growth of independent mortgage companies, and increased liquidity in the secondary market. In fact, CRA critics contend that the regulation has contributed little or nothing to increase credit for LMI borrowers and many support its elimination (e.g., Lacker 1995, Macey and Miller 1993).

The most influential empirical study of the effects of CRA is JCHS (2002), whose findings have been widely cited by CRA proponents (e.g., Apgar and Duda 2003, Barr 2005). The empirical strategy of the JCHS study is to treat CRA as a "natural experiment" by constructing a treatment group and two ersatz control groups. The treatment group is composed of CRA-regulated banks operating in their assessment areas. The first control group includes CRA-regulated banks operating outside their assessment areas. The second control group is composed of non-CRA-regulated lenders; i.e., independent mortgage companies and credit unions. The JCHS study finds that: a) CRA-regulated lenders operating in their assessment area make a higher proportion of home purchase loans to LMI neighborhoods than do out-of-area lenders or non-regulated institutions; b) CRA-regulated lenders in their assessment areas make a higher proportion of loans to LMI borrowers living in non-LMI neighborhoods than either of the control groups; and c) rejection rates for loan applications from LMI neighborhoods and borrowers are lower in the assessment areas of CRA-regulated lenders than outside assessment areas or for non-regulated lenders. The authors interpret these differences

[7] Barr (2005) provides a comprehensive analysis of the CRA literature.

between the treatment and control groups as the causal effects of CRA. We believe this identification strategy is fundamentally flawed, and we show why in section 6.4 below.

Avery, Calem and Canner (2003) exploit the LMI definition, to compare outcomes (such as changes in homeownership rates, vacancy rates, and housing values) in neighborhoods just below the 80% LMI threshold to neighborhoods just above the 80% threshold. Although they use the LMI cutoff to motivate their analysis, they do not use the regression discontinuity design that we describe below. Instead, they predict outcomes in neighborhoods just below the threshold on the basis of coefficients from models estimated for neighborhoods just above, and vice versa. Their findings are mixed and do not consistently point to a clear CRA effect.

4. Regression Discontinuity Design

Our empirical strategy is to identify the effects of CRA using a regression discontinuity (RD) analysis.[8] The intuition behind RD is simple: if individuals are assigned to a treatment based on a threshold value of a continuous variable, then comparison of outcomes for individuals just above and just below the threshold yields an unbiased estimate of the effect of the treatment. In other words, individuals just below the threshold serve as the control group and individuals just above serve as the treatment group.

[8] The first published example of an RD design is Thistlewaite and Campbell (1960). Rubin (1977) provided formal statistical foundations for RD estimates of treatment effects. More recently, Hahn et al. (2001) establish continuity restrictions for identification in RD, and Lee (2005) establishes a local independence result in which identification by RD is analogous to a randomized experiment. Trochim (1984) provides an accessible introduction to RD. Imbens and Lemieux (2007) offer a guide to practice.

More formally, three variables characterize the RD design.[9] Let Z be a known assignment variable such that an individual receives treatment if and only if Z crosses a cutting point z_0. Let T be a binary variable that equals one for those who are treated and zero for those who are not, and let Y be an outcome variable on which we want to estimate the effect of the treatment. Following Rubin's (1977) potential outcomes framework, suppose that Y_1 is the outcome that will result if the individual receives treatment and Y_0 is the outcome without treatment. Ideally, we would like to estimate the difference between Y_1 and Y_0, but we never observe both outcomes simultaneously for a single individual. However, we are able to observe $Y = TY_1 + (1-T)Y_0$. If the treatment has no effect, then $E[Y \mid Z = z_0 + e] \cong E[Y \mid Z = z_0 - e]$, where e is an arbitrarily small positive number. Under certain existence and continuity assumptions, it follows that the effect of the treatment, β, is identified by:

$$\beta = \lim_{Z \to z_0^+} E[Y \mid Z] - \lim_{Z \to z_0^-} E[Y \mid Z]. \tag{1}$$

In practice, β is often estimated by the mean difference for persons just above and just below the cutting point, which is $E[Y \mid Z = z_0 + e] - E[Y \mid Z = z_0 - e]$,[10] although any nonparametric estimator can be used to estimate the limits (Hahn et al. 2001).

To identify the effect of CRA, we exploit the discontinuity in the definition of LMI status for CRA-eligible lending. As described above, the CRA lending test uses a strict cut-off of 80 percent of MSA median family income to classify both neighborhoods

[9] This discussion follows Hahn et al. (2001).

[10] In the data, we observe $E[Y_1 \mid Z = z]$ when $z \geq z_0$, as well as $E[Y_0 \mid Z = z]$ when $z < z_0$. $E[Y_0 \mid Z = z_0]$ can be approximated arbitrarily well by $E[Y_0 \mid Z = z_0 - e]$ (Lee and Card 2006).

and individuals as LMI, the CRA treatment group. CRA-regulated institutions can improve their performance on the lending test by lending in LMI neighborhoods and to LMI individuals. The definition of these groups is strict; as a result, loans made in a neighborhood with 79.9% of MSA median income will improve an institution's performance on the lending test, but loans made in a neighborhood with 80.1% of MSA median income will not.[11] Classification is fully known and strictly observed. Using RD in this context, we will compare outcomes for individuals and neighborhoods just above and just below the 80 percent cutting point. The large volume of lending data available through HMDA, explained below, allows us to focus our analysis at a very fine level of resolution around the threshold.

Another advantage of RD is that no additional covariates are needed to obtain unbiased estimates of β. *Local independence* (Lee 2005) implies that, in the neighborhood of the cutting point, all variables determined prior to treatment will be independent of treatment status, meaning that estimates of β will not be biased by (observable or unobservable) omitted variables. This result is particularly important in the CRA context because relatively few covariates are available in the HMDA data that form the basis for most CRA analyses. For instance, some analysts have been cautious about analyzing the effect of CRA on loan rejection rates because borrowers' credit histories and scores are unknown. In a conventional regression analysis, these omitted variables are likely to bias estimates of the effect of CRA on loan approval for LMI borrowers. With the RD design, on the other hand, we can obtain unbiased estimates of the CRA effect on loan approval without credit scores or any other covariates.

[11] Although, as explained above, the CRA rating process is not formulaic, the only assumption we need for our identification to be valid is that the CRA rating is monotonically increasing in lending to LMI borrowers and neighborhoods. This assumption is clearly satisfied under the system in place since 1993.

Additional covariates, when available, do serve two useful purposes in RD analysis, comparable to their uses in the analysis of a randomized experiment (Lee 2005). First, covariates can be used to verify local independence. There should be no treatment-control differences in pre-determined covariates. These restrictions can be tested empirically by examining whether any covariate changes discontinuously at the cutoff point. Second, covariates can be included in the analysis to reduce sampling variability, yielding more precise estimates of β. In the analyses presented below, we employ available covariates for both purposes.

This RD design allows us to make strong causal inferences about the contribution of CRA to increases in lending in LMI neighborhoods or to LMI borrowers. Indeed, under fairly weak conditions, causal inferences from RD analysis have been shown to be as good as from a randomized experiment (Lee 2005).

5. Data

Our primary source for the data used in this analysis is the loan application-level data released by loan-making institutions following the Home Mortgage Disclosure Act (HMDA). This information is augmented with individual income eligibility cut-offs from HUD, as well as bank data from the FDIC's Summary of Deposits database and the FRB's Commercial Bank Database.

5.1 HMDA Data

The Home Mortgage Disclosure Act (HMDA) was originally passed in 1975; at the time, the regulation compelled lenders to report the number and dollar value of mortgage loans originated and purchased. Reforms have expanded reporting requirements, so that lenders are now required to report more detailed information about each loan application received, including the race and income of the applicant. Publicly available HMDA datasets contain all loan applications to large lenders; small institutions are not required to report under HMDA. Additionally, HMDA contains only metropolitan loan applications; non-metropolitan loans are reported separately to CRA-regulators.

Four categories of mortgage loans are classified by and included in each HMDA yearly file. They are: one-to-four family home purchase loans; one-to-four family home improvement loans; one-to-four family refinancing loans; and multifamily purchase, improvement, or refinancing loans. For the purpose of this analysis, we limit our scope to one-to-four family home purchase loans[12].

We pool data from the years 1995 to 2002. There are roughly 70 million records for one-to-four family home purchase loans in this period. The data are filtered to include only those records whose regulating agency is one of the four CRA regulators (OCC, FDIC, FRB and OTS); this eliminates roughly 16 million loan applications across all years. To avoid confounding effects of CRA with other federal lending programs, we also limit the data to include only those loans which are classified as "conventional." This eliminates an additional 4 million Federal Housing Authority (FHA)-insured loans, Veterans' Administration (VA)-guaranteed loans, and Rural Housing Service or Farm Service Agency (RHS/FSA) loans.[13]

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[12] Bhutta (2008) suggests that the effectiveness of CRA may vary by category of loan. Our own analysis of Bhutta's results indicate that the effect of CRA he identifies may be, in larger part, driven by a home improvement loan-specific effect. We intend to examine this further in subsequent research.
[13] In analyses not reported, we included both conventional and government-backed loans and found that results were unchanged from those presented here.

Because one of our outcomes of interest is the loan rejection rate, we also eliminate 3 million records where we do not observe a rejection/origination decision. The majority of records eliminated through this filter are "purchased loans", which are included in the HMDA data when a loan, previously originated by one institution, is sold to another HMDA-regulated institution. Loan applications which are withdrawn by the applicant prior to a rejection/origination decision and those which are never completed are also eliminated.

We also exclude all other non-depository lending institutions, such as bank-owned mortgage brokers, which eliminates another 6 million records.[14] Finally, we limit our analysis to those loans which are in the lender's CRA assessment area, which excludes an additional 2 million records. After applying all filters, we create two separate sub-sets of these records: (1) 1.7 million loan application records from tracts with a tract-to-MSA median family income ratio of 0.70 to 0.90; and (2) 1.4 million loan application records from individuals with an individual income-to-MSA median family income ratio of 0.70 to 0.90.[15] The sub-set of records selected based on the tract-to-MSA income ratio is subsequently aggregated by census tract, giving us tract-level measures of the loan application rejection rate, average loan amount, and other variables described below. Thus, we use individual- and tract-level data sets for different components of our analysis.

5.2 HUD Data

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[14] We also estimated all models without excluding non-depository lenders. Results did not change substantively.

[15] Note that the individual income-to-MSA median family income ratio is calculated using information provided by both HMDA and HUD. The use of HUD is described in greater detail below.

Although the HMDA loan records include tract-to-MSA median family income ratios, as well as individual borrower incomes, the individual income-to-MSA median family income ratios cannot be directly determined from the HMDA data. Fortunately, the Department of Housing and Urban Development (HUD) prepares annual estimates of the median family income for each MSA. We matched these estimates to the HMDA records for the appropriate year, and then derived the individual income-to-MSA median family income ratio for each loan application record. These are the same data and methodology used by CRA regulators to determine the LMI status of individual loans.

5.3 Bank Data

We use two databases to facilitate identification of: (1) whether a particular loan application is made to a non-depository lending institution; and (2) whether a particular loan application is within or outside of the lender's CRA assessment area. The FDIC's Summary of Deposits (SOD) annual data provide branch location data, while the FRB's commercial bank databases facilitate the matching process between HMDA and the SOD.

Following the JCHS (2002) study, we assume that an individual lender's assessment area is defined as all counties in which its bank holding company operates deposit-taking branches. We match each HMDA record to the bank holding company using the lender ID, and then determine whether the loan application is in a county in which the bank holding company operates a deposit-taking branch.

5.4 Census Data

CRA regulators defined neighborhood eligibility according to 1990 Census tract boundaries up until 2002. This poses a challenge for the three outcome variables we draw from the census: the home ownership rate; the vacancy rate; and the median value of owner-occupied housing units. The difficulty is that the 2000 Census data are reported according to different tract boundaries than the 1990 data. Yet, we wish to understand the outcomes for CRA eligible tracts in 2000, defined in 1990 boundaries. To obtain the 2000 census data in 1990 boundaries, we purchased a custom data set from Geolytics, a private-sector data vendor. Geolytics is perhaps best known in academia for its *neighborhood change database*, developed in association with the Urban Institute, which translates 1970, 1980, and 1990 census data into 2000 census boundaries (Tatian 2003). We asked Geolytics to do essentially the reverse and translate 2000 census data into 1990 boundaries.[16]

6. Results

We present our results in four steps. First, we present our analysis of the effect of CRA on several neighborhood-level outcomes. Second, we present our analysis of individual loan rejection probabilities and average loan amounts. Third, we discuss additional sensitivity analyses for our major results. Finally, we show why previous attempts to estimate CRA's effect by comparing in- and out-of-assessment area lending practices are flawed.

6.1 Neighborhood Analysis

To analyze the effect of CRA on neighborhood-level outcomes, we incorporate a matched-pair design into the RD framework. We begin by identifying all neighboring pairs of census tracts within each US metropolitan statistical area (MSA), according to 1990 Census boundaries. Defining neighboring tracts as those that share a common

[16] Further information about the company and its methodology can be found on its website: www.geolytics.com.

boundary or vertex[17], we find a total of 137,455 tract-pairs. Next, we reduce our sample to only those tract pairs in which one member of the pair is CRA eligible and the other is not; that is, one member's median family income is less than 80 percent of the MSA average, while the other's is greater. In total, we find 30,783 of these CRA-mismatched tract pairs. Finally, we restrict our analysis to those pairs in which the CRA-eligible member's median family income is between 70 and 79.99 percent of the MSA median and the non-eligible member's income is between 80 and 90 percent of the MSA median. In other words, we focus on pairs in which one member is just below the CRA eligibility threshold of 80 percent of MSA income while the other member is just above the threshold. We have a total of 3,956 tract pairs within this narrow range around the CRA threshold, and it is on this sample that we run the RD analysis[18]. Thus, we identify the CRA effect by comparing outcomes for neighboring pairs of tracts in which one tract barely qualifies for CRA and the other barely exceeds the CRA threshold.

We analyze a variety of neighborhood-level outcomes that could plausibly be related to CRA. Most obviously, we analyze the loan rejection rate—that is, the fraction of loan applications from the tract that are rejected—to test the primary hypothesis that CRA incentivizes banks to approve loans that they otherwise would have denied. We also analyze the loan application rate—the number of loan applications received per household—to test for the possibility that banks improve outreach to CRA-eligible neighborhoods to boost the number of applications, regardless of whether they alter their underwriting criteria conditional on receiving an application, as well as the total number

[17] Neighbors are defined using the "queen contiguity" algorithm in GeoDa 0.95i.
[18] We also drop one observation with an outlying value of the difference between the 1990 population of the CRA-eligible tract and the ineligible tract. Specifically, the eligible tract had a population of 19,000 whereas the non-eligible tract had a population of 1,000. In principle, census tracts are designed to contain roughly even populations, generally between 2,500 and 8,000 residents.

of home purchase loan originations in a neighborhood (normalized by the number of households). We analyze the change in the aggregate annual dollar volume of lending, which follows from the argument that CRA increased aggregate credit flows to LMI neighborhoods. In addition, we use Census data to investigate indirect effects of CRA on the following neighborhood outcomes: the change in the home ownership rate from 1990 to 2000; the change in the vacancy rate; and the change in the median value of owner-occupied housing. The latter three variables have been suggested as outcomes that are negatively affected by redlining, and therefore should improve if CRA has its intended effect (Avery, Calem and Canner 2003).

Table 1 presents summary statistics for each of the main outcome variables and the tract-level covariates. Statistics are computed separately for the treatment and control groups, tabulated for two different windows around the threshold—70 to 90 percent of MSA median income and 78 to 82 percent—and tests for differences between treatment and control tracts are presented for each variable and window size[19]. Starting with the covariates, Table 1 shows that there are statistically significant treatment-control differences for many variables when the 70-90 percent sample is used; however, most differences are substantively quite small. For instance, 13.6 percent of applicants from treatment tracts were of Hispanic origin, while 12.5 percent in control tracts were of Hispanic origin, a difference that is significant statistically but not practically. Narrowing in to the 78-82 percent sample, nearly all of the covariate differences are statistically insignificant and substantively small, even minute. Only two treatment-control differences are significant at the 5% standard in the 78-82 percent sample: CRA

[19] Standard errors for the test of whether the differences between treatment and control tracts are significantly different from zero are clustered by the treatment tract.

applicants come from tracts with slightly lower median family incomes (a difference which is constructed by definition); and the housing stock in CRA neighborhoods is slightly older (53 percent built before 1960 compared to 49 percent). We note further that because we have run 20 tests of treatment-control differences for the 78-82 percent sample, we should expect to find some significant differences just by chance. If we make a Bonferroni adjustment to our critical values to account for the multiple hypothesis tests we have run (e.g., Miller 1986), the housing age difference no longer attains statistical significance. Moreover, even without the Bonferroni adjustment, both the housing age and family income differences disappear when we control for the treatment-control difference in the tract-to-MSA income ratio, which is the assignment variable. These results lead us to conclude that the local independence condition is satisfied in our data.

Turning to the outcome variables, three differences are significant in the 70-90 percent sample, all in a direction opposite that which would be expected if CRA had its intended effect. Specifically, CRA-eligible tracts had higher loan application rejection rates, smaller declines in housing vacancy, and less appreciation in housing values between 1990 and 2000. However, none of these differences remain significant in the 78-82 percent sample. Instead, the difference in the loan volume growth is statistically significant, but in a direction contrary to CRA's intent.

Our neighborhood-level RD analysis for matched tract pairs is presented in Table 2. For each outcome, we present the analysis for 70-90, 75-85, and 78-82 percent windows. Each model is a regression of the difference in the outcome variable between the treatment and control tract. The first specification controls only for the difference in the assignment variable; that is, the tract-to-MSA income ratio. In addition, we run a second version of each model that also includes the full set of covariates listed in Table 1, expressed as differences between the treatment and control tract. Standard errors account for clustering by CRA-eligible tract.[20]

The results of the matched-pair RD analysis provide no evidence of a significant CRA effect at the neighborhood level for any of the outcomes we examined. Results for only two of the dependent variables are significant in Table 2, all in the opposite direction from what would be expected if CRA had its intended impact. The annual appreciation rate in lending is about two percentage points lower for CRA tracts in the 75-85 percent sample, while the change in median housing value from 1990 to 2000 is 14 percentage points lower for CRA tracts in the 75-85 percent sample and 23 percentage points lower for CRA tracts in the 78-82 percent sample. Again, we must emphasize that, having conducted 42 hypothesis tests for CRA effects in Table 2, we should not be surprised to find about four results significant at the 10 percent level purely by chance. Therefore, we are hesitant to make too much of the two results just described, especially because neither appears to be consistent across the various samples and model specifications.

With so many null results reported in Table 2, should we be concerned that our tests are not powerful enough to detect effect sizes of practical, if not statistical, significance? In fact, the coefficients are fairly precisely estimated and we can reject even relatively small effects in most cases. Models of the rejection rate, homeownership rate,

[20] Each tract may appear in multiple pairs. For instance, a given CRA-eligible tract may have more than one non-CRA-eligible neighbor. In addition, a given non-qualifying tract may be the neighbor to more than one qualifying tract. Observations for multiple pairs including a common tract cannot be treated as independent. We account for this potential non-independence by clustering according to the CRA-eligible tract. This clustering strategy also accounts for repeated observations of the same tract pair in multiple years. In principle, there may be some residual correlation in the errors attributable to repeated observations on non-eligible tracts. It is not obvious to us how to account for both types of clustering simultaneously. Therefore, we stipulate that our standard errors are, if anything, likely to be too small. Nevertheless, we still find no significant effects of CRA at the neighborhood level.

and vacancy rate produce fairly precisely estimated zeroes, especially in the larger windows around the threshold. However, in models of the application rate, origination rate, and dollar amount of lending, the point estimates are, while all substantively small or in the "wrong" direction, imprecisely estimated. The imprecision of these models may be due to the fact that we have normalized these dependent variables by the number of households in 1990, which is a noisy measure of the actual values from 1995 through 2002. It is conceivable that CRA affects application and origination rates but not the rejection rate; for instance, this could happen if banks made outreach to increase applications from LMI consumers, but did not alter their underwriting standards. While our data certainly do *not* show that this is the case, we cannot completely reject the possibility either, given the imprecision in the application and origination estimates.

We emphasize that the only statistically significant results from Table 2 run *counter* to CRA's desired effect, as explained above, so the overall pattern of results does not suggest that there is an important effect of CRA which our tests are not sufficiently powerful to detect. Rather, our main conclusion from the results shown in Table 2 is that there is little support for the argument that CRA has had a positive, causal effect on any of these neighborhood-level outcomes.

6.2 Individual Analysis

As explained above, under the 1995 reforms banks receive CRA credit for lending to LMI neighborhoods, and to LMI individuals regardless of their neighborhood. Therefore, our neighborhood-level analysis may fail to detect any effect CRA has on lending to LMI individuals in non-LMI tracts. To estimate the individual-level CRA effect, we use a pure RD design, ignoring the tract-matching step employed in the neighborhood-level analysis. The RD analysis takes the form of a regression in which the unit of analysis is the individual loan application, and the outcome of interest is regressed against: the CRA eligibility indicator, the applicant's individual-to-MSA income ratio, whose effect is allowed to vary on either side of the threshold, and a set of control variables explained below. We estimate the effect of CRA on the probability that a loan is approved and on the size of the loan. We also estimate the interaction between individual-level and the neighborhood-level CRA eligibility. That is, we test whether the probability that a loan is approved, or its size, varies for LMI individuals within and outside of LMI neighborhoods.

Because the CRA eligibility threshold is a function of MSA median income, the actual income cutoff varies from one MSA to another, although the threshold income *ratio* is the same in all MSAs. Therefore, it is problematic in the RD context to pool observations from different MSAs. For instance, a tract that is just below 80 percent of MSA income in New York might actually have a higher median income than a tract that is just above 80 percent of MSA income in Detroit. For this reason, we conduct the individual-level analysis separately for 10 of the largest MSAs.[21] The huge HMDA data base, described above, delivers thousands of observations around the CRA discontinuity even in an individual MSA. For example, when restricting our analysis to loan applicants with income ranging from 75 to 85 percent of the MSA median, our sample size ranges from 8,200 in New York to 33,300 in Chicago. When we further restrict our focus to applicants with income between 78 and 82 percent of the MSA median, we still have at least 3,000 observations in each of the 10 MSAs analyzed.

[21] Alternatively, we could pool the observations and run a model with MSA-level interactions. When we do so, our results do not change.

Table 3 presents tests of covariate balancing for the individual application data. The large quantity of data allows us to estimate small differences very precisely, so we are attentive to both substantive and statistical significance when interpreting these results. We test for balancing in the attributes of the loan applicants as well as the attributes of their neighborhoods (i.e., census tracts). In the vast majority of cases, treatment-control differences are statistically insignificant and/or substantively unimportant. However, the results for two MSAs show enough differences to raise concerns. In Houston, CRA-eligible applicants are more likely to be female, less likely to be Asian, and come from tracts with higher home ownership rates; all of these differences are statistically significant and substantively large. In Phoenix, CRA-eligible applicants are more likely to be female, come from larger and more affluent tracts (as measured by both the tract-to-MSA median family income ratio and the median housing value) with smaller minority populations. Again, the differences are both substantively and statistically significant. We conclude that the local independence assumption is satisfied for the eight remaining MSAs, but we must be circumspect in our interpretation of the RD results for Houston and Phoenix.

Table 4 presents our logit models of the probability that a loan application is rejected. We report odds ratios (i.e., exponentiated coefficients), which have the interpretation that a value greater than one implies that a CRA-eligible applicant is more likely to be rejected, while a value less than one implies that a CRA-eligible applicant is less likely to be rejected. We show results of RD models for windows of 75-85 and 78-82 percent of MSA median income. In each case, we show three specifications of the model. The first includes controls for the applicant characteristics listed in Table 3. The second includes an interaction between tract and individual CRA-eligibility; that is, an interaction between individual eligibility dummy and a dummy equal to one if the applicant is also in a CRA-eligible tract. The third specification adds the tract-level covariates shown in Table 3. All models include year dummies and the applicant's individual-to-MSA income ratio, which is allowed to vary on either side of the CRA eligibility threshold. Standard errors are clustered by census tract.

The results of the applicant-level models do not reveal an important effect of CRA on individual loan application rejection rates in most MSAs. The estimated CRA effect is insignificant in most of the specifications shown in Table 4. In Atlanta, the individual CRA effect is significant and in the expected direction, but only in the 75-85 percent sample. When we narrow into to the discontinuity, the effect is no longer significant and takes on a value near one. Both the individual CRA effect and the interaction with the neighborhood level effect are close to one and not significant in Boston and Chicago. In Detroit, New York, and Washington DC-Baltimore, the CRA effect generally takes the "wrong" sign—that is, CRA eligible applications are more likely to be rejected— and is never significant. (In New York, the interaction effect - LMI borrowers in LMI neighborhoods - is significant in the 78-82 percent range, but is again takes on the "wrong" sign.) In Philadelphia and Phoenix, the models suggest that LMI borrowers in non-LMI neighborhoods are more likely to be rejected, while LMI borrowers in LMI neighborhoods are less likely to be rejected; however, the pattern is not robust. For none of these eight MSAs, then, does there appear to be robust, persuasive evidence of a CRA effect, one way or another, on loan rejection rates.

Among the remaining two MSAs, evidence of a CRA effect is more powerful. Los Angeles demonstrates the clearest evidence of the expected CRA effect: borrowers just below the CRA eligibility threshold are significantly less likely to have their applications rejected than borrowers just above the threshold. This result becomes significant and larger as we narrow from the 75-85 percent sample to the 78-82 percent sample, and indicates that the odds of rejection for CRA-eligible borrowers are 0.64 to 0.68 times the odds of rejection for non-eligible borrowers. Although not significant, these models also suggest that CRA-eligible borrowers in CRA-eligible neighborhoods are less likely to have their applications rejected. CRA-eligible borrowers are also less likely to be rejected in Houston, where the results are highly significant in the 78-82 percent sample.[22]

Finally, Table 5 presents our models of CRA's effect on the average dollar amount of loans to LMI borrowers. In other words, we ask whether CRA has induced lenders to increase the amount they are willing to lend to eligible applicants. In almost every case, the answer is *no*. The notable exception is again Los Angeles, where as the results are universally positive, and become larger and significant as we narrow to the final, fully specified model for the 78-82 percent sample. Less consistently, the results in Chicago suggest that LMI borrowers in LMI neighborhoods receive larger loans on average, but there is no indication that LMI borrowers alone receive larger loan amounts than non-LMI borrowers.

The results shown in Tables 4 and 5 do not support the contention that CRA has a pervasive effect on loan amounts or approval rates for LMI borrowers. In most MSAs we

[22] Again, we are cautious in interpreting the results for Houston, given the failure of the balancing tests reported in Table 5.

examined, the weight of the evidence points to the absence of any effect of CRA. In Los Angeles and Houston, CRA may have had its intended effect in reducing loan rejections for LMI borrowers; and CRA may also have increased loan volume in LA. Additional research focused on these two MSAs may shed light on the conditions under which CRA is more or less effective.

6.3 Sensitivity Analysis

We conducted a number of additional analyses to confirm the robustness of our conclusions. To check the robustness of our neighborhood-level results, we re-analyzed the tract eligibility sub-set of data using a pure RD set-up identical to that used for the individual-level results discussed above. In other words, rather than using neighboring pairs of qualifying and non-qualifying tracts, we simply pooled *all* tracts in the 70 to 90 percent range of MSA median income. Models were estimated using both a pooled dataset of loan records from all MSAs and MSA-specific datasets. As with the matched-pair design, we did not find any evidence of an effect of CRA on neighborhood-level outcomes.

However, we have strong reason to believe that the matched-pair design is better suited to identify the effect of neighborhood eligibility than the pure RD set-up. Unlike the individual income-to-MSA median family income ratio, the tract-to-MSA median family income ratio is not at all continuously distributed in each MSA, meaning that there are relatively few tracts at any point in the tract-to-MSA median family income ratio distribution. When we narrow in to the smallest window of data around the tract-level discontinuity, inferences about the effect of CRA are made almost entirely on the basis of rejection rate differentials across the two neighborhoods closest to the discontinuity, often from different MSAs. As a result, the local independence assumption is more difficult to maintain in the pure RD framework at the neighborhood level.

We also analyzed both the tract eligibility and the individual eligibility sub-sets of data (pooled across all MSAs) using a pure RD set-up, in which the assignment variable was specified as a series of dummy variables (for instance, all loan applications with an income ratio of at least 0.70 and up to 0.71 were coded as having the same income ratio indicator) rather than as a continuous variable. This was done to allow a completely flexible functional form in the assignment variable. We found no specification, in either sub-sample, where we could reject the hypothesis that the probability of rejection for applications just below the CRA cut-off is equal to the probability of rejection for applications just above the CRA cut-off.

While these robustness checks strongly suggest that the average effect of CRA across the years 1995 to 2002 is null, we have lingering concerns that our specifications may obscure year-to-year variation in the CRA effect, particularly given the changing regulatory environment over the period of analysis. To address these concerns, we tested for year-specific effects at both the neighborhood level (using the matched-pair RD design) and at the individual level (using the pure RD design in 10 MSAs). At the neighborhood level, we do not find evidence of any year-specific CRA effect on any outcome examined. At the individual level, the balance of the evidence indicates that there are no year-specific CRA effects. In LA and Houston (where we find fairly consistent evidence that borrowers just below the eligibility threshold are less likely to be rejected than borrowers just above the threshold), we do find that the CRA effect is statistically significant in only two years (2000 and 2002 for Houston; 1996 and 2000 for

LA). For both MSAs, however, the point estimate of the CRA effect has the expected sign in every year. Further, the results provide no strong reason to conclude that the CRA effect is changing systematically throughout this period in these two MSAs.

Finally, we note that our neighborhood-level results may be biased toward zero if CRA encourages lenders to adopt practices whose benefits spill-over to non-LMI neighborhoods. There is anecdotal evidence to suggest that lenders adopt "special lending programs" to meet their CRA obligations, but that these programs do not strictly target borrowers in LMI neighborhoods (Avery, Bostic and Canner 2000). We contend that, if this is the case, we should observe declining rejection rates and rising loan volumes in non-LMI neighborhoods that are located in close proximity to LMI neighborhoods. We test for such patterns by regressing tract-level rejection rates and loan volumes on the share of a tract's neighbors which are LMI (holding constant the tract's own median income ratio and the share of loan applicants from the tract who are themselves LMI). We find no evidence that tract-level rejection rates and aggregate loan volume are related to proximity to LMI neighborhoods. We conclude that spill-overs to non-LMI neighborhoods are not biasing our results toward zero.

6.4 Assessment Area Effects?

Our results run counter to JCHS (2002), who conclude that CRA has causal effects on lending to LMI neighborhoods and individuals. Although, the JCHS findings have not appeared in a peer-reviewed academic publication, they are regularly cited by scholars (e.g., Barr 2005) and prominent policymakers (e.g., Bernanke 2007). Therefore, we conclude by showing that the JCHS study suffers from a basic identification problem, which explains why our results differ from theirs.

As discussed above, the JCHS (2002) study relies on differences between in- and out-of-assessment area lending to make causal inferences about CRA. Consider Figure 1, which shows average annual loan application rejection rates from 1995 to 2002. The top panel of Figure 1 shows rejection rates in CRA-eligible tracts; that is, tracts with median family income less than 80 percent of their MSA median. Average rejection rates are shown for three different groups of lenders: CRA-regulated lenders operating in their CRA assessment area; CRA-regulated lenders operating outside their assessment area; and non-CRA-regulated lenders. Clearly, CRA-regulated lenders are less likely to reject loans from CRA-eligible tracts within their assessment area than they are to reject applications from eligible tracts outside their assessment area. Also, CRA-regulated lenders operating in their assessment area are less likely than non-CRA-regulated lenders to reject applications from CRA-eligible neighborhoods. The JCHS study interprets these differential rejection rates as the causal effects of CRA.

The problem with this interpretation is evident upon consideration of the bottom panel of Figure 1 (which has no equivalent in the JCHS paper). The bottom panel shows average rejection rates for non-CRA-eligible tracts among the same three groups of lenders. Here too, CRA-regulated lenders operating in their assessment area have consistently lower rejection rates than the other two categories of lenders. In other words, CRA-regulated lenders operating in their assessment areas are less likely to reject loan applications, regardless of whether those applications are from CRA-qualifying tracts or not.

Table 6 formalizes this analysis using linear probability models of loan rejection rates, including a set of control variables comparable to those employed by JCHS.[23] The main variable of interest is the CRA assessment area dummy, which indicates the differential probability that a loan from the lender's assessment area will be rejected compared to a loan from outside the assessment area, all else equal. Model (1) shows that CRA-eligible individuals in CRA-eligible tracts are roughly 12 percentage points less likely to have their applications rejected if they are in the lender's assessment area. Model (2) shows that applications from non-LMI borrowers in CRA-eligible tracts are about 6 percentage points less likely to be rejected when in the lenders assessment area, while model (3) shows that eligible individuals in non-eligible tracts are about 14 percentage points less likely to be rejected. However, model (4) demonstrates that applications from *non-eligible individuals* in *non-eligible tracts* are roughly 13 percentage points less likely to be rejected in a lender's CRA assessment area.

To further address the question of whether differences in outcomes between in-assessment area loans and out-of-assessment area loans can be interpreted as causal CRA effects, we explicitly incorporate assessment area into our RD models at both the neighborhood and individual level. We restore the out-of-assessment area loan application records filtered from the datasets (as described in Section 5.1), and test whether the difference between in- and out-of-assessment area outcomes varies on either side of the discontinuity. For example, with the assignment variable specified as a series of dummy variables, we conducted a differences-in-differences test and were unable to reject the hypothesis that the difference between in- and out-of-assessment area rejection

[23] See section 5, exhibit 27 in JCHS (2002). We use a linear probability model following JCHS, however we note that our results do not change substantively if we use probit instead.

rates is equal for loans just above (80.0 to 80.99) and just below (70.0 to 70.99) the LMI cut-off. We could not reject the null in either the individual eligibility sample or the tract eligibility sample.[24]

In summary, while we agree with JCHS that loan rejection rates are lower for banks operating in their CRA assessment areas than operating outside, we find no evidence that this difference is causally related to CRA. In this context, it is important to note that *assessment area* simply means an area where a bank has a deposit-taking operation. A comparison of in- and out-of-assessment area lending practices therefore amounts to a comparison of areas where a bank has bricks-and-mortar operations and those where it does not. Lender practices and strategies likely differ in these two areas for reasons that have nothing to do with CRA. In any event, because the assessment area differences exist for both LMI and non-LMI loans, we conclude that they have no apparent causal connection with CRA.

7. Discussion

Using a matched-pair regression discontinuity design, we find no evidence that CRA had an effect on any of the neighborhood-level outcomes that we examined: the average loan rejection rate; the average loan application rate; the average annual dollar amount of lending; the home ownership rate; the housing vacancy rate; or the change in median housing value. Moreover, using a regression discontinuity design to analyze individual-level loan applications for the top 10 MSAs, we find no consistent or widespread effect of CRA on loan approval or size. The most charitable interpretation of

[24] Results available on request.

the individual-results results is that CRA may have had an effect in one of the MSAs, namely Los Angeles.

Although we do observe that loan rejection rates are lower for banks operating in their CRA assessment areas, we emphasize that this difference holds for both LMI and non-LMI loans. Therefore, we think the difference between assessment area and non-assessment area lending more likely reflects different business practices in areas where banks have a physical presence compared to where they do not, rather than any impact of CRA.

We are hesitant to conclude based on this evidence that CRA has not made *any* difference in credit availability for LMI communities. Future research may show CRA effects operating through different channels than those analyzed here. The secondary market and the ability it provides to regulated lenders to purchase loans that meet CRA eligibility criteria may play an important role in increasing the credit available among LMI neighborhoods.. Further, future research into the relationship between the regulatory agencies and regulated lenders and into the strategies that lenders use to meet their CRA obligations may reveal other channels through which CRA has an effect on its intended beneficiaries.

In conclusion, while our results do not eliminate the possibility that CRA has had an impact on some neighborhood- or individual-level outcome not analyzed here, we believe the burden must shift to CRA supporters to demonstrate convincingly a causal relationship between CRA and some outcome of interest. Because of its strength for making causal inferences without the need a rich set of covariates, we believe the RD design is particularly promising for future CRA studies.

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Figure 1: Overall Rejection Rate by Year

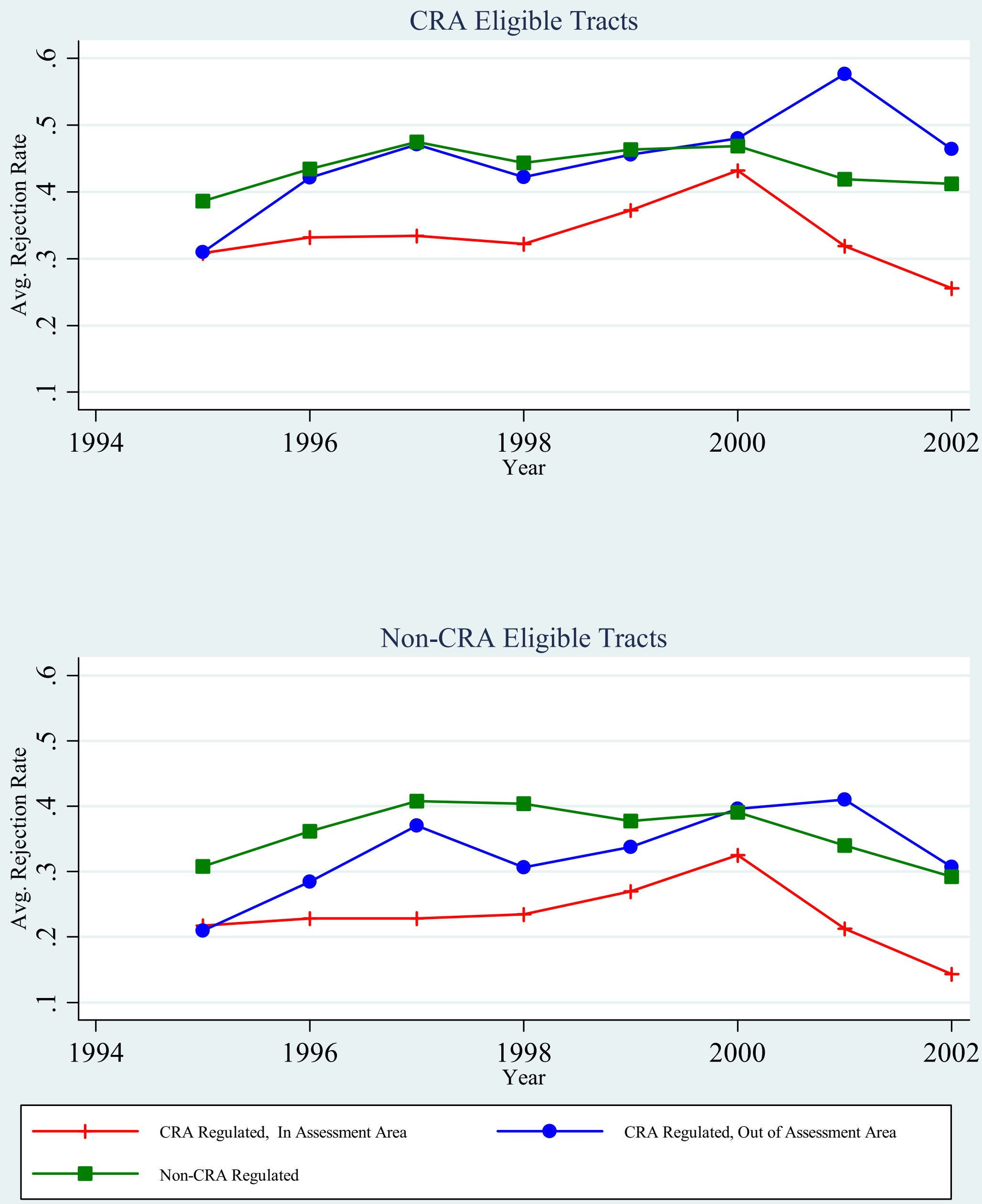

Figure 2: Total Loan Volume by Year

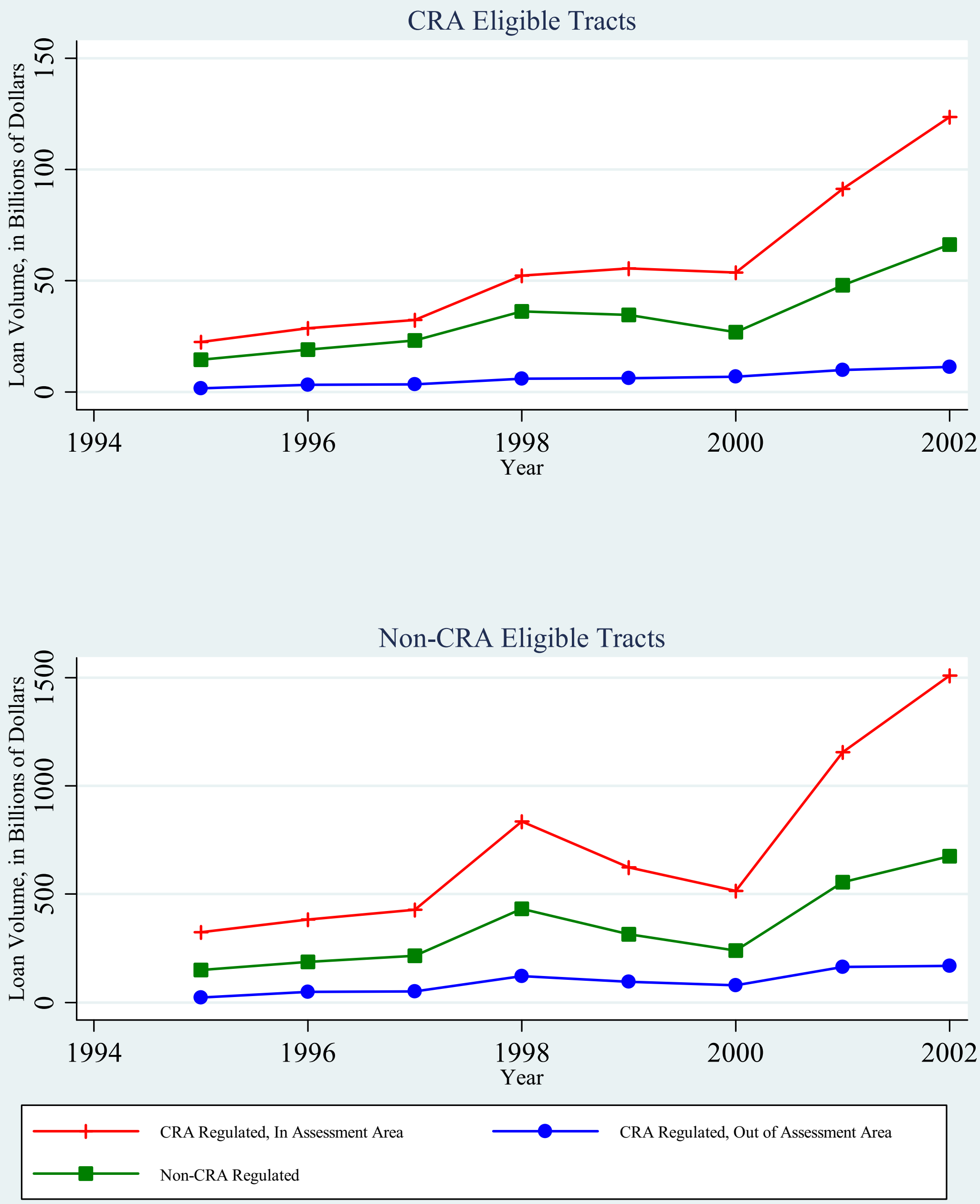

Table 1: Matched-Pair Tract-Level Summary Statistics

	CRA-Eligible Tract	Non-CRA-Eligible Tract	Difference	N of Pairs
70-90% TRACT-TO-MSA INCOME RATIO				
Outcomes				
Average Annual Application Rate (per thousand Households, 1990)	14.477	15.648	-1.171	3956
Average Annual # of Originations (per thousand Households, 1990)	10.927	12.224	-1.297	3956
Average Annual Rejection Rate	0.264	0.240	0.025 ***	3933
Average Annual Dollar Amount of Lending (per Households, 1990)	$1,120.20	$1,529.57	-$409.38	3956
Average Annual Percent Change in Dollar Amount of Lending	0.090	0.088	0.002	3917
Change in Homeownership Rate, 1990 to 2000	0.000	0.001	-0.001	3954
Change in Vacany Rate, 1990 to 2000	-0.006	-0.009	0.003 **	3954
Percent Change in Median Value of Housing, 1990 to 2000	0.550	0.585	-0.034 *	3881
Covariates				
Average Income (Constant 1995 Dollars) of Applicant Pool	$54,097.37	$55,089.36	-$992.00 **	3933
Percent Black, Applicant Pool	0.140	0.126	0.014 ***	3933
Percent Hispanic, Applicant Pool	0.136	0.125	0.011 ***	3933
Percent Asian, Applicant Pool	0.056	0.058	-0.002	3933
Percent Other Race, Applicant Pool	0.023	0.022	0.002 **	3933
Percent Female, Applicant Pool	0.295	0.292	0.003	3933
Percent Below Individual CRA Eligibility Ratio, Applicant Pool	0.462	0.434	0.027 ***	3933
Tract Population, 1990	4404.512	4448.794	-44.281	3956
Percent Non-White, 1990	0.324	0.283	0.041 ***	3956
Median Family Income, 1990	$29,131.66	$32,768.90	-$3,637.24 ***	3956
Median Housing Value, 1990	$82,889.20	$88,377.93	-$5,488.73 ***	3956
Homeownership Rate, 1990	0.546	0.607	-0.061 ***	3956
Percent of Housing Built before 1960	0.557	0.535	0.023 ***	3956
78-82% TRACT-TO-MSA INCOME RATIO				
Outcomes				
Average Annual Application Rate (per Households, 1990)	13.728	14.717	-0.989	180
Average Annual # of Originations (per Households, 1990)	10.413	11.193	-0.780	180
Average Annual Rejection Rate	0.248	0.255	-0.006	180
Average Annual Dollar Amount of Lending (per Households, 1990)	$1,032.93	$1,150.17	-$117.24	180
Average Annual Percent Change in Dollar Amount of Lending	0.094	0.116	-0.022 **	180
Change in Homeownership Rate, 1990 to 2000	-0.001	-0.003	0.002	180
Change in Vacany Rate, 1990 to 2000	-0.007	-0.008	0.001	180
Percent Change in Median Value of Housing, 1990 to 2000	0.476	0.490	-0.015	177
Covariates				
Average Income (Constant 1995 Dollars) of Applicant Pool	$53,460.92	$54,461.41	-$1,000.49	180
Percent Black, Applicant Pool	0.141	0.138	0.003	180
Percent Hispanic, Applicant Pool	0.119	0.111	0.007	180
Percent Asian, Applicant Pool	0.069	0.075	-0.006	180
Percent Other Race, Applicant Pool	0.020	0.021	-0.001	180
Percent Female, Applicant Pool	0.297	0.290	0.006	180
Percent Below Individual CRA Eligibility Ratio, Applicant Pool	0.455	0.440	0.015	180
Tract Population, 1990	4795.278	4540.061	255.217	180
Percent Non-White, 1990	0.312	0.312	0.000	180
Median Family Income, 1990	$31,087.05	$31,852.63	-$765.58 ***	180
Median Housing Value, 1990	$89,945.55	$90,939.45	-$993.89	180
Homeownership Rate, 1990	0.573	0.562	0.011	180
Percent of Housing Built before 1960	0.531	0.489	0.042 ***	180

Notes: Standard errors are clustered by the CRA eligible tract. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 2: Matched-Pair RD Model of CRA Effect on Tract-Level Outcomes

Range of Tract-to-MSA Median Income Ratio:	70-90 %		75-85%		78-82%	
Controls?	N	Y	N	Y	N	Y
Average Annual Application Rate (per thousand Households, 1990)						
CRA Effect	-3.191	-3.111	1.034	0.799	-0.225	0.057
	(2.030)	(1.964)	(1.901)	(1.882)	(1.584)	(1.464)
N of Pairs	3956	3933	1185	1181	180	180
Average Annual # of Originations (per thousand Households, 1990)						
CRA Effect	-2.556	-2.538	1.224	0.992	0.137	0.369
	(1.716)	(1.705)	(1.693)	(1.672)	(1.275)	(1.145)
N of Pairs	3956	3933	1185	1181	180	180
Average Annual Rejection Rate						
CRA Effect	0.000	0.003	-0.002	0.001	-0.022	-0.013
	(0.005)	(0.004)	(0.009)	(0.007)	(0.029)	(0.018)
N of Pairs	3933	3933	1181	1181	180	180
Average Annual Dollar Amount of Lending (per Households, 1990)						
CRA Effect	-$755.08	-$832.56	$180.24	$156.23	-$76.52	-$33.33
	(562.284)	(655.775)	(238.768)	(235.247)	(175.391)	(150.744)
N of Pairs	3956	3933	1185	1181	180	180
Average Annual Percent Change in Dollar Amount of Lending						
CRA Effect	-0.003	-0.004	-0.022	-0.025	-0.004	-0.009
	(0.006)	(0.006)	(0.011)**	(0.011)**	(0.024)	(0.024)
N of Pairs	3917	3917	1178	1178	180	180
Change in Homeownership Rate, 1990 to 2000						
CRA Effect	0.000	0.000	-0.003	-0.002	0.013	0.014
	(0.003)	(0.003)	(0.006)	(0.006)	(0.013)	(0.013)
N of Pairs	3954	3931	1185	1181	180	180
Change in Vacancy Rate, 1990 to 2000						
CRA Effect	0.003	0.003	0.001	0.000	-0.005	-0.005
	(0.003)	(0.003)	(0.004)	(0.004)	(0.008)	(0.009)
N of Pairs	3954	3931	1185	1181	180	180
Percent Change in Median Housing Value, 1990 to 2000 (Owner-Occupied Housing)						
CRA Effect	-0.021	-0.032	-0.091	-0.143	-0.190	-0.229
	(0.050)	(0.042)	(0.088)	(0.085)*	(0.148)	(0.134)*
N of Pairs	3881	3858	1163	1159	177	177

Notes: Standard errors, clustered by CRA eligible tract, are reported in parentheses. The unit of observation is a pair of neighboring Census tracts, where one tract is below the CRA eligibility threshold and the other tract is above the CRA eligiblity threshold (limited to pairs of tracts in the same MSA). Each dependent variable is the difference in the relevant characteristic between the CRA-eligible tract and the non-CRA-eligible tract in the pair. All models include the tract-to-MSA income ratio difference between tracts in a pair. Controls include pair differences in: average real income (in 1995 dollars) of the applicant pool, ethnic and gender composition of the applicant pool, percent of applicant pool with incomes below the individual CRA eligiblility threshold, 1990 tract population, 1990 non-white percent of population, 1990 percent of housing built before 1960, 1990 median family income, 1990 homeownership rate (except when change in homeownership rate is the dependent variable), and 1990 median housing value (except when change in median housing value is the dependent variable).

* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 3: Tests for Balancing of Covariates around Individual Discontinuity, by MSA

	PANEL A: INDIVIDUAL-LEVEL COVARIATES							PANEL B: TRACT-LEVEL COVARIATES					
Dependent Variable:	Female	Black	Asian	Hispanic	Other Race	Race Missing	Gender Missing	Tract-to-MSA Median Family Income Ratio	Tract Population, 1990	Percent of Tract Population, Non-White, 1990	Tract Median Housing Value, 1990	Tract Homeownership Rate, 1990	Percent of Houses Built before 1960
Atlanta													
Threshold Effect	0.044	-0.020	0.017	0.007	-0.009	0.027	0.002	2.109	659.1	-0.017	$2,439.31	0.009	-0.023
	(0.041)	(0.036)	(0.021)	(0.021)	(0.009)	(0.029)	(0.025)	(2.831)	(340.001)*	(0.026)	(3,258.29)	(0.021)	(0.020)
Observations	4,307	4,307	4,307	4,307	4,307	4,307	4,307	4,307	4,307	4,307	4,307	4,307	4,307
Boston													
Threshold Effect	0.013	-0.016	-0.005	-0.032	-0.020	0.003	0.001	1.974	63.8	-0.027	$2,978.91	0.012	0.021
	(0.033)	(0.015)	(0.014)	(0.016)**	(0.009)**	(0.015)	(0.013)	(1.862)	(113.6)	(0.013)**	(3,894.56)	(0.015)	(0.015)
Observations	4,696	4,696	4,696	4,696	4,696	4,696	4,696	4,696	4,696	4,696	4,696	4,696	4,696
Chicago													
Threshold Effect	-0.031	-0.005	-0.020	0.018	-0.003	-0.002	0.002	-3.572	-116.9	0.000	-$5,274.19	-0.002	0.014
	(0.026)	(0.017)	(0.015)	(0.021)	(0.007)	(0.013)	(0.010)	(2.120)*	(144.9)	(0.016)	(3,888.58)	(0.014)	(0.019)
Observations	14,832	14,832	14,832	14,832	14,832	14,832	14,832	14,832	14,832	14,832	14,832	14,832	14,832
Detroit													
Threshold Effect	-0.005	-0.004	-0.003	-0.002	0.011	0.007	0.022	-2.494	-35.1	0.002	-$2,693.30	0.007	0.016
	(0.036)	(0.026)	(0.013)	(0.008)	(0.011)	(0.022)	(0.020)	(2.752)	(142.2)	(0.020)	(3,194.51)	(0.013)	(0.026)
Observations	6,518	6,518	6,518	6,518	6,518	6,518	6,518	6,518	6,518	6,518	6,518	6,518	6,518
Houston													
Threshold Effect	0.274	0.122	-0.180	0.054	-0.028	-0.025	-0.040	7.407	-658.6	-0.012	-$6,917.47	0.140	0.060
	(0.134)**	(0.094)	(0.072)**	(0.119)	(0.042)	(0.052)	(0.046)	(10.723)	(1274.1)	(0.071)	(9,364.91)	(0.061)**	(0.061)
Observations	4,893	4,893	4,893	4,893	4,893	4,893	4,893	4,893	4,893	4,893	4,893	4,893	4,893
Los Angeles													
Threshold Effect	0.002	-0.016	-0.027	0.024	0.003	0.000	-0.004	-0.612	-330.5	-0.023	-$315.36	-0.005	-0.011
	(0.041)	(0.019)	(0.029)	(0.045)	(0.012)	(0.027)	(0.022)	(3.011)	(339.1)	(0.025)	(6,643.47)	(0.021)	(0.023)
Observations	6,710	6,710	6,710	6,710	6,710	6,710	6,710	6,710	6,710	6,710	6,710	6,710	6,710
New York City													
Threshold Effect	0.099	0.023	0.023	0.041	-0.015	-0.044	-0.048	-4.114	128.3	0.043	-$2,388.24	0.008	-0.025
	(0.038)***	(0.024)	(0.026)	(0.027)	(0.015)	(0.026)*	(0.024)**	(3.517)	(257.2)	(0.025)*	(7,701.31)	(0.018)	(0.020)
Observations	4,652	4,652	4,652	4,652	4,652	4,652	4,652	4,652	4,652	4,652	4,652	4,652	4,652
Philadelphia													
Threshold Effect	0.023	-0.014	-0.013	-0.018	0.026	0.047	0.052	-4.142	260.6	0.041	-$5,469.12	-0.015	0.029
	(0.061)	(0.035)	(0.027)	(0.019)	(0.013)**	(0.039)	(0.031)*	(3.891)	(471.3)	(0.026)	(6,671.40)	(0.021)	(0.040)
Observations	4,417	4,417	4,417	4,417	4,417	4,417	4,417	4,417	4,417	4,417	4,417	4,417	4,417
Phoenix													
Threshold Effect	0.132	-0.003	-0.005	-0.037	-0.021	-0.060	-0.061	7.769	549.6	-0.029	$8,315.11	0.028	-0.010
	(0.051)***	(0.015)	(0.016)	(0.035)	(0.015)	(0.036)	(0.031)**	(3.982)*	(269.719)**	(0.016)*	(4,276.796)*	(0.020)	(0.015)
Observations	4,312	4,312	4,312	4,312	4,312	4,312	4,312	4,312	4,312	4,312	4,312	4,312	4,312
Washington DC - Baltimore													
Threshold Effect	-0.010	-0.016	0.007	0.015	-0.015	0.001	0.013	1.241	98.4	-0.006	$4,838.81	0.001	0.000
	(0.028)	(0.019)	(0.018)	(0.014)	(0.009)*	(0.015)	(0.012)	(1.706)	(177.5)	(0.013)	(4,488.01)	(0.012)	(0.018)
Observations	7,379	7,379	7,379	7,379	7,379	7,379	7,379	7,379	7,379	7,379	7,379	7,379	7,379

Notes: Standard errors, clustered by Census tract, are reported in parentheses. All models include the individual-to-MSA income ratio (allowed to vary on either side of the discontinuity) and year fixed effects. Models are estimated using the 78-82% window aroud error individual-level discontinuity. * significant at the 10% level, ** signifcant the 5% level, *** significant at the 1% level.

Table 4: Applicant-Level Logit Model of Effect of CRA on Applicant-Level Rejection Rate for Home Purchase Loans, by MSA

Range of Tract-to-MSA Income Ratio:		75-85%			78-82%	
Controls for Applicant Characteristics?	Y	Y	Y	Y	Y	Y
Control for Tract Eligibility Status?	N	Y	Y	N	Y	Y
Controls for Tract Characteristics?	N	N	Y	N	N	Y
Atlanta						
CRA Effect	0.790	0.794	0.779	0.989	1.038	0.978
(Individual Threshold)	(0.046)**	(0.055)*	(0.038)**	(0.963)	(0.879)	(0.929)
CRA Effect		0.985	0.997		0.975	0.988
(Individual*Tract Threshold)		(0.926)	(0.983)		(0.913)	(0.958)
Observations	11,172	11,172	11,172	4,307	4,307	4,307
Boston						
CRA Effect	1.007	1.036	1.031	0.949	1.016	1.015
(Individual Threshold)	(0.955)	(0.771)	(0.799)	(0.818)	(0.944)	(0.948)
CRA Effect		0.928	0.928		0.809	0.813
(Individual*Tract Threshold)		(0.551)	(0.548)		(0.298)	(0.313)
Observations	12,393	12,393	12,393	4,696	4,696	4,696
Chicago						
CRA Effect	0.976	0.985	0.981	1.082	1.070	1.092
(Individual Threshold)	(0.756)	(0.848)	(0.812)	(0.702)	(0.748)	(0.683)
CRA Effect		0.966	0.981		0.935	0.941
(Individual*Tract Threshold)		(0.682)	(0.822)		(0.600)	(0.643)
Observations	37,244	37,244	37,244	14,832	14,832	14,832
Detroit						
CRA Effect	1.100	1.110	1.104	1.221	1.230	1.218
(Individual Threshold)	(0.304)	(0.283)	(0.311)	(0.390)	(0.384)	(0.415)
CRA Effect		0.937	0.917		1.066	1.067
(Individual*Tract Threshold)		(0.564)	(0.464)		(0.725)	(0.736)
Observations	17,227	17,227	17,227	6,518	6,518	6,518
Houston						
CRA Effect	1.030	0.994	0.991	0.234	0.253	0.227
(Individual Threshold)	(0.690)	(0.939)	(0.905)	(0.023)**	(0.033)**	(0.024)**
CRA Effect		1.236	1.278		1.192	1.200
(Individual*Tract Threshold)		(0.027)**	(0.011)**		(0.238)	(0.223)
Observations	13,195	13,195	13,195	4,893	4,893	4,893
Los Angeles						
CRA Effect	0.893	0.901	0.905	0.640	0.673	0.676
(Individual Threshold)	(0.153)	(0.213)	(0.232)	(0.040)**	(0.074)*	(0.079)*
CRA Effect		0.971	0.971		0.832	0.831
(Individual*Tract Threshold)		(0.731)	(0.736)		(0.131)	(0.129)
Observations	15,246	15,246	15,246	6,710	6,710	6,710
New York City						
CRA Effect	1.036	1.011	1.024	1.116	1.046	1.066
(Individual Threshold)	(0.740)	(0.923)	(0.827)	(0.556)	(0.810)	(0.736)
CRA Effect		1.165	1.169		1.505	1.485
(Individual*Tract Threshold)		(0.329)	(0.319)		(0.051)*	(0.060)*
Observations	10,098	10,098	10,098	4,652	4,652	4,652
Philadelphia						
CRA Effect	0.856	0.883	0.876	2.235	2.271	2.168
(Individual Threshold)	(0.181)	(0.299)	(0.268)	(0.067)*	(0.067)*	(0.085)*
CRA Effect		0.847	0.830		0.811	0.748
(Individual*Tract Threshold)		(0.240)	(0.196)		(0.340)	(0.208)
Observations	11,640	11,640	11,640	4,417	4,417	4,417
Phoenix						
CRA Effect	0.843	0.867	0.863	1.066	1.206	1.230
(Individual Threshold)	(0.097)*	(0.182)	(0.171)	(0.832)	(0.543)	(0.507)
CRA Effect		0.866	0.860		0.731	0.726
(Individual*Tract Threshold)		(0.215)	(0.194)		(0.094)*	(0.088)*
Observations	11,165	11,165	11,165	4,312	4,312	4,312
Washington DC - Baltimore						
CRA Effect	1.011	0.996	0.993	1.377	1.371	1.368
(Individual Threshold)	(0.925)	(0.969)	(0.951)	(0.118)	(0.128)	(0.132)
CRA Effect		1.122	1.143		1.133	1.168
(Individual*Tract Threshold)		(0.411)	(0.337)		(0.570)	(0.483)
Observations	17,028	17,028	17,028	7,379	7,379	7,379

Notes: The dependent variable is a binary variable equal to "1" if the loan application is denied. Coefficients are odds-ratios from logistic regression models. P-values are reported in parentheses; standard errors are clustered by Census tract. All models include the individual-to-MSA income ratio (allowed to vary on either side of the discontinuity) and year fixed effects. Individual controls include gender and ethnicity. Tract controls include the tract-to-MSA median family income ratio, 1990 tract population, 1990 non-white percent of population, 1990 percent of housing built before 1960, 1990 homeownership rate and 1990 median housing value. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 5: Applicant-Level Model of Effect of CRA on Applicant-Level Loan Amount, by MSA

Range of Tract-to-MSA Income Ratio:		75-85%			78-82%	
Controls for Applicant Characteristics?	Y	Y	Y	Y	Y	Y
Control for Tract Eligibility Status?	N	Y	Y	N	Y	Y
Controls for Tract Characteristics?	N	N	Y	N	N	Y
Atlanta						
CRA Effect	-$1,893.46	-$1,751.17	-$1,334.34	-$822.08	-$1,464.63	-$607.55
(Individual Threshold)	(1,745.00)	(1,750.69)	(1,720.51)	(4,182.43)	(4,208.55)	(4,121.56)
CRA Effect		-$2,023.28	-$2,453.18		-$6,269.43	-$7,051.81
(Individual*Tract Threshold)		(2,770.37)	(2,760.84)		(3,974.36)	(3,957.621)*
Observations	9,305	9,305	9,305	3,593	3,593	3,593
Boston						
CRA Effect	$680.23	$410.19	$261.62	-$4,446.54	-$5,345.84	-$6,245.41
(Individual Threshold)	(1,964.36)	(2,000.00)	(1,958.57)	(4,153.43)	(4,111.36)	(3,981.54)
CRA Effect		$905.61	$748.53		$4,097.11	$2,332.43
(Individual*Tract Threshold)		(2,540.72)	(2,570.20)		(4,273.20)	(4,099.79)
Observations	11,105	11,105	11,105	4,192	4,192	4,192
Chicago						
CRA Effect	$778.41	$443.31	$462.65	-$357.66	-$950.87	-$553.58
(Individual Threshold)	(1,096.31)	(1,101.48)	(1,098.67)	(2,740.31)	(2,771.69)	(2,757.75)
CRA Effect		$2,477.13	$2,152.03		$4,939.54	$4,803.20
(Individual*Tract Threshold)		(1,781.08)	(1,758.69)		(2,806.485)*	(2,779.509)*
Observations	33,269	33,269	33,269	13,204	13,204	13,204
Detroit						
CRA Effect	-$112.44	$145.59	-$15.83	-$7,472.03	-$7,806.98	-$6,698.51
(Individual Threshold)	(1,429.19)	(1,407.57)	(1,378.93)	(4,758.70)	(4,618.940)*	(4,449.74)
CRA Effect		-$2,061.68	-$1,238.40		$249.01	$815.54
(Individual*Tract Threshold)		(2,517.02)	(2,321.86)		(3,419.73)	(3,316.03)
Observations	14,792	14,792	14,792	5,568	5,568	5,568
Houston						
CRA Effect	$1,797.75	$1,589.97	$1,716.41	$12,288.99	$10,836.99	$11,684.19
(Individual Threshold)	(1,571.70)	(1,626.30)	(1,625.05)	(10,770.41)	(10,681.84)	(10,745.45)
CRA Effect		-$624.13	-$940.21		$878.73	$760.15
(Individual*Tract Threshold)		(1,856.91)	(1,853.99)		(2,594.95)	(2,592.62)
Observations	9,053	9,053	9,053	3,371	3,371	3,371
Los Angeles						
CRA Effect	$1,975.75	$1,671.37	$2,029.78	$9,142.82	$9,427.48	$11,219.37
(Individual Threshold)	(1,847.07)	(1,968.95)	(1,878.22)	(5,000.547)*	(5,124.402)*	(4,896.599)**
CRA Effect		$1,029.84	-$24.91		-$320.53	-$1,225.27
(Individual*Tract Threshold)		(1,831.86)	(1,754.68)		(2,843.21)	(2,771.88)
Observations	11,304	11,304	11,304	4,951	4,951	4,951
New York City						
CRA Effect	$3,734.47	$4,037.76	$2,889.91	$2,956.17	$2,451.36	$1,708.53
(Individual Threshold)	(2,933.94)	(2,942.81)	(2,899.61)	(5,635.62)	(5,583.68)	(5,435.12)
CRA Effect		-$1,667.14	-$1,494.83		-$3,424.55	-$6,174.75
(Individual*Tract Threshold)		(4,484.06)	(4,405.23)		(6,963.03)	(6,686.54)
Observations	8,225	8,225	8,225	3,779	3,779	3,779
Philadelphia						
CRA Effect	-$1,029.27	-$647.84	-$213.59	$3,292.42	$5,581.18	$5,763.71
(Individual Threshold)	(1,718.84)	(1,659.19)	(1,568.77)	(5,840.75)	(5,689.91)	(5,399.93)
CRA Effect		$1,451.88	$1,726.05		-$4,438.62	-$4,276.45
(Individual*Tract Threshold)		(2,216.19)	(2,023.34)		(4,138.86)	(3,996.80)
Observations	9,841	9,841	9,841	3,733	3,733	3,733
Phoenix						
CRA Effect	-$281.95	-$200.12	$62.61	$7,032.63	$6,142.43	$4,780.09
(Individual Threshold)	(1,392.19)	(1,410.24)	(1,327.12)	(4,709.39)	(4,663.49)	(4,599.82)
CRA Effect		$613.71	-$645.57		-$3,778.16	-$4,972.10
(Individual*Tract Threshold)		(2,066.37)	(2,033.09)		(3,267.92)	(3,179.28)
Observations	8,963	8,963	8,963	3,447	3,447	3,447
Washington DC - Baltimore						
CRA Effect	$3,408.48	$3,423.39	$3,330.35	$2,678.20	$3,066.68	$2,884.33
(Individual Threshold)	(2,235.00)	(2,245.98)	(2,270.83)	(3,784.43)	(3,808.85)	(3,846.32)
CRA Effect		-$1,002.44	-$806.96		-$3,155.78	-$2,653.39
(Individual*Tract Threshold)		(3,769.75)	(3,857.37)		(5,155.92)	(5,289.13)
Observations	15,481	15,481	15,481	6,713	6,713	6,713

Notes: The dependent variable is the loan amount for originated loans. Standard errors, clustered by Census tract, are reported in parentheses. All models include the individual-to-MSA income ratio (allowed to vary on either side of the discontinuity) and year fixed effects. Individual controls include gender and ethnicity. Tract controls include the tract-to-MSA median family income ratio, 1990 tract population, 1990 non-white percent of population, 1990 percent of housing built before 1960, 1990 homeownership rate and 1990 median housing value. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 6: Loan-Level Model of Loan Application Rejection Rates by Loan Eligiblity Status

Tract Eligible?	Y	Y	N	N
Individual Eligible?	Y	N	Y	N
Loan Inside Assessment Area	-0.123	-0.056	-0.136	-0.125
	(0.001)**	(0.001)**	(0.001)**	(0.001)**
MSA Median Household Income	0.000	0.000	0.000	0.000
	(0.000)**	(0.000)**	(0.000)**	(0.000)**
90-00 growth -8-0%	-0.038	-0.011	-0.025	-0.022
	(0.002)**	(0.001)**	(0.001)**	(0.001)**
90-00 growth 0-10%	-0.033	-0.003	0.004	0.005
	(0.001)**	(0.001)**	(0.001)**	(0.001)**
90-00 growth 10-20%	-0.038	0.003	0.017	0.015
	(0.002)**	-0.001	(0.001)**	(0.001)**
90-00 growth >20%	-0.011	-0.006	0.011	0.006
	(0.002)**	(0.001)**	(0.001)**	(0.001)**
% Tract Housing Built pre1960	-0.147	-0.043	-0.068	-0.045
	(0.002)**	(0.002)**	(0.001)**	(0.001)**
1990 Ratio of Tract to MSA Median Family Income	-0.002	-0.001	-0.002	-0.001
	(0.000)**	(0.000)**	(0.000)**	(0.000)**
Loan Amount/Applicant Income	-0.001	-0.007	-0.001	-0.001
	(0.000)**	(0.000)**	(0.000)**	(0.000)**
Black Applicant	0.064	0.150	0.111	0.114
	(0.001)**	(0.001)**	(0.001)**	(0.001)**
Hispanic Applicant	-0.011	0.093	0.037	0.046
	(0.001)**	(0.001)**	(0.001)**	(0.001)**
Constant	0.889	0.456	0.808	0.576
	(0.004)**	(0.003)**	(0.002)**	(0.002)**
Observations	1,060,393	1,187,808	3,877,789	4,653,933

Notes: Standard errors in parentheses. Dependent variable is coded as "1" if loan was denied; "0" if loan was originated. All models also include year fixed effects. All models estimated using Ordinary Least Squares. * significant at 5%; ** significant at 1%