Strategically Staying Small: Regulatory Avoidance and the CRA*

Jacelly Cespedes† Jordan Nickerson‡ Carlos Parra§

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Using the introduction of an asset based two-tiered evaluation scheme in the 1995 CRA reform, we examine the consequences of regulatory avoidance. Banks exploit the attribute-based regulation by strategically slowing asset growth, bunching below the $250M threshold. The regulatory avoidance also produces real effects. Banks near the threshold experience an increase in the rejection rate of LMI loans, while areas they serve experience a decline in county-level small establishment shares and independent innovation. These results highlight a bank's willingness to take costly actions to avoid regulatory oversight and subsequent credit reduction for individuals whom the CRA is designed to benefit.

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†Carlson School of Management, University of Minnesota. Email: cespe013@umn.edu.

‡Foster School of Business, University of Washington. Email: jnick@uw.edu.

§Pontificia Universidad Católica de Chile. Email: parra.c.carlos.r@gmail.com.

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Abstract Using the introduction of an asset based two-tiered evaluation scheme in the 1995 CRA reform, we examine the consequences of regulatory avoidance. Banks exploit the attribute-based regulation by strategically slowing asset growth, bunching below the $250M threshold. The regulatory avoidance also produces real effects. Banks near the threshold experience an increase in the rejection rate of LMI loans, while areas they serve experience a decline in county-level small establishment shares and independent innovation. These results highlight a bank's willingness to take costly actions to avoid regulatory oversight and subsequent credit reduction for individuals whom the CRA is designed to benefit.

1. Introduction

Banks operate in arguably one of the most heavily regulated industries, where policy intervention is used to achieve wide-ranging goals such as regulating risk via capital requirements, dictating permissible operating activities, protecting consumers, and ensuring fair treatment of individuals through equal access to credit (see Kroszner (2008) for a thorough review). One notable example studied extensively in the extent literature is the Community Reinvestment Act (CRA), originally enacted in 1977. Counter to most banking regulations which restrict actions, the CRA encourages a bank to extend credit to targeted groups within its community. As such, there exists a mature literature examining the extent to which the CRA mandate encourages risky lending.[1] While much attention has been paid to the promotion of risky lending, this represents just one potential consequence associated with the CRA. In this paper, we instead measure the impact of a discrete jump in regulatory burden and the consequences of regulatory avoidance. In the context of the CRA, banks avoid the step-up in regulatory requirements by taking strategic but costly growth-slowing actions.[2]

Our strategy centers on a feature embedded in a 1995 CRA revision that increased regulatory intensity and monitoring for banks with assets greater than \$250M. Arguably, the existence of a discrete increase in regulatory requirements tied to this threshold suggests a perceived compliance cost attached to the CRA. Our approach exploits this threshold by examining the tendency of a bank to strategically manage its assets in order to fall just shy of \$250M. As the strategic slowing of a bank's growth is plausibly a costly action, our strategy is built on a revealed preference argument, whereby banks bunching below \$250M view the increase in regulatory cost triggered upon crossing the threshold as being greater than the cost of slowing growth. A key advantage of this strategy is the ability to evaluate

1. Prior studies yield somewhat mixed results on the CRA's effect on risky lending. For example, using different identification strategies Agarwal, Benmelech, Bergman and Seru (2012) and Saadi (2020) find that CRA-induced originations default at a higher rate. In contrast, Ringo (2017) finds no measurable increase in default rates while Canner and Passmore (1997) find that lenders specializing in low-income areas are not less profitable.

2. Studying the CRA is particularly important in light of the recent proposal for a strengthened and modernized CRA framework (FRB (2020)).

the overall cost of the regulation on banks and the areas they serve, rather than focusing on a specific component. Through the lens of Ito and Sallee (2018), the incorporation of a size threshold qualifies the CRA as an "attribute-based regulation." Ito and Sallee (2018) develop a theoretical model of the welfare implications of attribute-based regulation which weighs the distortionary costs against potential benefits. While we do not quantify potential benefits of the CRA, our strategy does leverage the increase in regulation at the threshold to evaluate the consequences of regulatory avoidance in the context of the CRA.[3] As such, we join other recent works which draw insights from attribute-based regulation to evaluate regulatory costs in different settings (Anderson and Sallee (2011), Kisin and Manela (2016), Bouwman, Hu and Johnson (2018), Alvero, Ando and Xiao (2020), Ewens, Xiao and Xu (2020), Ballew, Iselin and Nicoletti (2021), Fuster, Plosser and Vickery (2021)).

We begin by describing key institutional details regarding the 1995 changes made to the CRA. While the stated motivation for the reform was to "replace paperwork and uncertainty with greater performance, clarity, and objectivity," the revisions also included the creation of two bank classifications, *small banks* and *large banks*. Determined by year-end assets being greater or less than $250M, banks in each group faced significantly different regulatory requirements. Among others, *small banks* faced a streamlined evaluation process with a more narrow scope, were not required to disclose the geographic distribution of small business loans, and were evaluated at a 5-year interval compared to the 2-year frequency of *large banks*. Taken together, this collection of preferential treatments offered to *small banks* provides an incentive for a bank to strategically manage assets to stay below the $250M asset threshold.

Consistent with this idea, we document significant bunching of banks at the $250M asset threshold over the period from 1996 to 2004.[4] As a first step, we provide simple visual evidence of bunching by plotting a histogram of year-end total assets over the period. In contrast, we find no evidence of bunching in the pre-reform period (1986-1993) nor signs of

3. An extensive literature evaluates potential benefits of the CRA. Notable examples include Ding, Lee and Bostic (2018) and Chakraborty, Chhaochharia, Hai and Vatsa (2020), among others.

4. The CRA faced a major revision in 2005, both reducing some benefits of being classified as a small bank while also complicating our identification strategy. More details of the change are provided below.

bunching at other salient asset values ($150M and $350M) which were not tied to CRA regulations. We follow this with further evidence of excess bunching by applying techniques from the public finance literature (e.g., Saez (2010), Kleven and Waseem (2013)). In particular, we use two approaches to construct the counterfactual distribution of bank assets that would have prevailed in the absence of the $250M asset threshold. The first approach relies on the distribution prior to the introduction of the cutoff (e.g., Londoño-Vélez and Ávila-Mahecha (2018), DeFusco, Johnson and Mondragon (2020)), while the second approach generates the counterfactual by fitting a high-order polynomial to the observed distribution (Kleven and Waseem (2013)). Both approaches confirm that the discrete change in regulatory requirements tied to the $250M threshold led to a significant excess mass below it.

To identify how a bank circumvents the more rigorous CRA assessment and to estimate possible effects on the area it serves, we exploit the introduction of the CRA asset threshold in a difference-in-differences approach akin to Bartik (1991). Here, the first differential captures the difference in outcomes for banks with *pre-reform* assets (e.g., measured prior to the 1995 reform) falling just below the threshold (treated) relative to similarly sized banks (assets less than $350M), while the second differential captures how this difference changes following the 1995 reform (post). We find that banks with 1994-measured assets between $200M and $250M experienced a 4.4pp reduction in post-reform asset growth. Reassuringly, this estimate is stable across various lower bounds for the treated group (e.g., $220M). Moreover, we find no evidence of a pre-trend, with the effect immediately being realized in 1995.[5] This result is consistent with banks strategically managing asset growth to retain the reduction in regulatory costs associated with a *small bank* classification. In addition, we find a stronger slowing of asset growth among banks facing less attractive investment prospects, and an increase in loan profitability for bunching banks.[6]

Counter to the standard view that the CRA encouraged more lending, the previous re-

5. Our results are also robust to using banks with 1993 assets falling just below the $250M threshold.

6. This latter find is consistent with the regulation-driven inefficient investment of Gong and Yannelis (2018).

sults suggest that banks near the regulatory threshold responded by strategically slowing their growth. Given this, we shift our attention to the potential real effects of the CRA on areas served by treated banks. We begin by considering a possible shift in the distribution of credit extended to households, examining potential heterogeneity in a bunching bank's rejection rate for loans that qualify for CRA credit ("low- and moderate-income" or LMI loans) compared to other loans. We find that banks falling just below the $250M threshold experience a 1.3pp to 2.2pp increase in rejection rates for LMI-qualifying loans. Importantly, these estimates control for local economic conditions with county-year fixed effects and include bank-LMI and LMI-year fixed effects to account for general differences across banks and loans over time. This result is particularly interesting, suggesting that the increase in regulatory costs accompanying the *large bank* test is associated with a reduction in credit offered to a specific group of potential borrowers targeted by the CRA. In contrast, we find no evidence that non-bunching banks (assets outside the range [$200M,$250M]) responded by increasing either originations, in general, or LMI-qualifying originations, in particular, in the census tracts served by bunching banks.

In a final series of tests, we examine the equilibrium effects on areas served by bunching banks. We begin by examining the composition of business establishments across counties. Both Berger and Udell (1995) and Weston and Strahan (1996), among others, note the positive correlation between the size of a firm and its lending bank. Since the specific threshold we exploit reduces the growth of relatively smaller banks, we would expect any negative effect on business establishments to be more concentrated in smaller firms. Consistent with this idea, the post-CRA reform share of small businesses (less than 20 employees) decreases by 0.07% for a one-standard-deviation increase in the county-level share of bank branches falling just below the $250M threshold. Economically, compared to the secular decline in small businesses witnessed over our sample period, this effect represents roughly 0.7 years of small business decline. Finally, we consider the potential effects on innovation. Babina, Bernstein and Mezzanotti (2020) find a reduction in independent (non-firm) innovation in areas more severely hit by the Great Depression. To the extent that independent inventors behave like small firms and also rely disproportionately on lending from smaller banks, slower bank growth by this group may hamper innovation. Consistent with this idea, we find a reduction in independent innovation in counties served by bunching banks, with a post-reform reduction in the rate of individually-assigned patents of 4.1% to 4.4%.

Taken together, these results suggest that rather than promoting lending and economic growth in the areas they serve, banks elected to reduce their economic footprint in response to the CRA reform to avoid an increase in regulatory oversight, a cost that was partially borne by the borrowers whom the act was designed to benefit. This response is consistent with either an on-going cost of being classified as a *large bank*, perhaps due to additional scrutiny stemming from the increase in disclosure, or alternatively, a bank incurring a one-time cost to build the necessary loan assessment, reporting, and compliance infrastructure needed to comply with the more thorough large bank test. Ultimately, we are unable to empirically distinguish between the two possible channels in a satisfactory manner. Instead, the finding that banks that bunch just below the threshold exhibit greater loan profitability is consistent with the first interpretation. While at the same time, we find no evidence of bunching near the newly introduced $1B threshold in a second major reform in 2005, which reclassified a subset of (previously large) banks into an intermediate category which also faced a more streamlined evaluation process, consistent with the second view.

Our paper contributes to two strands of the literature. First, our findings add to the literature examining the distortionary effects of regulation across a broad collection of settings. This includes prior works exploiting attribute-based regulation to measure the cost of fuel-economy standards (Anderson and Sallee (2011)), being publicly listed (Ewens, Xiao and Xu (2020)), CFPB oversight (Fuster, Plosser and Vickery (2021)), lending subsidies (Bachas, Kim and Yannelis (2021)), and capital holding requirements (Kisin and Manela (2016)). Specific to credit outcomes, Campbell, Ramadorai and Ranish (2015) and Cerulli, Fiordelisi and Marques-Ibanez (2021) document regulatory effects on the supply and performance of loans in India and Europe, respectively, while Di Maggio, Kermani and Korgaonkar (2016) examine the effect of deregulation on the prevalence of complex mortgage products. Finally, recent works focusing on the CRA include the examination of strategic branch closures (Hendrickson and Nichols (2010)), origination of under-performing loans due to public pressure (Dou and Zou (2019)), and deposit sensitivity to CRA ratings (Chen, Hung and Wang (2019)). We complement this strand by evaluating the overall increase in regulatory costs associated with the *large bank* test in the context of foregone bank growth. As such, our results are consistent with recent works documenting slowing asset growth as banks near the $10B threshold written into Dodd-Frank (Bouwman, Hu and Johnson (2018), Alvero, Ando and Xiao (2020), Ballew, Iselin and Nicoletti (2021)). Moreover, we provide evidence that the effects of regulatory avoidance extend to the real domain, through a reduction in small business and independent innovation activity.

Second, we contribute to a mature literature examining the effects of the CRA. Prior works generally find a CRA-induced increase in the supply of credit.[7] Credit expansion is felt across varying credit types, with an increase in the supply of residential mortgages (Ding and Nakamura (2017), Lee and Bostic (2020)), as well as small business loans (Ding, Lee and Bostic (2018), Chakraborty, Chhaochharia, Hai and Vatsa (2020)). In contrast, Begley and Purnanandam (2021) find a reduction in the quality of credit, with greater incidents of mis-selling and poor customer service. Moreover, the literature is generally divided when evaluating a change in credit riskiness. Using different identification strategies, both Agarwal, Benmelech, Bergman and Seru (2012) and Saadi (2020) find an increase in origination rates and defaults as a result of the CRA. In contrast, Ringo (2017) finds no evidence of an increase in default rates while Avery and Brevoort (2015) conclude that the CRA did not play a significant role in the sub-prime crisis. Rather than focusing on a regulatory-induced change in lending standards, we complement prior works by instead estimating the resulting

7. In a notable exception, Dahl, Evanoff and Spivey (2000) do not find evidence of an increase in credit supply over a relatively short time horizon, while Bhutta (2011) finds evidence of a small increase in loan originations.

cost imposed on a bank and the area it serves following the strategic avoidance of the CRA.

The remainder of the paper is organized as follows: Section 2 provides institutional details for the Community Reinvestment Act, describes the data used and presents summary statistics. We follow this with a description of our empirical strategy and a presentation of our main findings in Section 3. We then estimate the local impact the regulation avoidance in Section 4, before concluding in Section 5.

2. Institutional Details & Data

This section provides institutional details on the motivation and implementation of the Community Reinvestment Act (CRA), describes the data used in the analyses, discusses our sample selection process, and describes our final sample.

2.1. Details of the CRA

At the time of its enactment in 1977, the stated goal of the CRA was to "encourage depository institutions to help meet the credit needs of the communities in which they operate." This goal was achieved through legislation by way of regulatory supervisory agencies (e.g., the Federal Deposit Insurance Corporation (FDIC)), each tasked with monitoring banks under their respective purview to ensure banks comply with CRA guidelines. Accordingly, regulatory agencies take a bank's past CRA performance into consideration when evaluating applications for both new branch openings as well as bank mergers and acquisitions (FRB (n.d.), FDIC (n.d.)). Such considerations place a potentially large cost of non-compliance on banks subject to CRA guidelines.

Over the course of its life, the CRA has experienced multiple significant alterations. One major revision to the CRA was initiated by President Clinton in a July 1993 memo to the supervisory agencies. Bierman, Fraser and Zardkoohi (1993) make notes that the regulatory directive contained in the memo was for "cleared guidance as to how the regulatory agencies will evaluate CRA performance," pushing to "reform the CRA enforcement system by replacing paperwork and uncertainty with greater performance, clarity, and objectivity."

In response, regulatory agencies proposed more structure for the measurement of CRA performance in April 1995 by way of a set of objective standards by which banks would be evaluated. For example, one such metric which has garnered attention in the related literature is the share of loans originated in LMI census tracts (Agarwal, Benmelech, Bergman and Seru (2012)). Importantly, the proposed refinements to the CRA called for a tiered-evaluation scheme in which banks were partitioned into groups based on reported assets. This classification, which effectively bisected financial institutions into *small* and *large* banks, determined the set of evaluation standards by which a bank would be measured. Details of the tests faced by each bank classification are described in Figure 1.

[Insert Figure 1 Near Here]

The ultimate determination of what qualifies as a *small* bank versus a *large* bank is a function of the bank's total reported assets. Specifically, a bank is classified as a *small* bank if it has total assets less than $250M on December 31 of either of the prior two years. Federal Register (1995) makes note of two motivations for the consideration of a two-year measurement window. First, the wider window provides more stability in bank classifications. Second, this choice "ensures that institutions that exceed the asset limits have adequate time to prepare to meet the requirements applicable to larger institutions." This motivation is of particular interest, as it suggests a non-trivial burden placed on banks to prepare for the large bank test.

We now turn to the differentiating factors between the two evaluation tests. First, small and large banks both face the well-known *Lending Test*, which evaluates the geographic distribution of loans made, with particular attention paid to low- and moderate-income (LMI-qualifying) loans. However, FRB of Atlanta (1995) notes that small banks are evaluated under "more streamlined standards," suggesting a reduction in the regulatory burden of the lending test. In addition, large banks face two additional tests, the *Investment Test* and the *Service Test*. The investment test evaluates how a bank meets the credit needs of its community through qualified investments, such as projects which qualify for a low-income housing credit (Hossain (2004)). Finally, the service test examines how a bank's branches, ATM, etc. vary across geographic areas. This includes a bank's history of opening and closing branches in LMI areas and its overall distribution of branches throughout the community.

CRA requirements for small and large banks also differed in other ways. First, financial institutions classified as large banks were required to disclose the geographic distribution and size of the small business loans they originated. Dou and Zou (2019) make note of the potential use of this disclosure by community organizations. They find that non-performing rates decline when banks are exempted from this disclosure requirement. Finally, small banks are evaluated much less frequently than large banks, undergoing an examination every five years versus two years for large banks. Taken together, these examples represent a potentially substantial increase in the cost of complying with the CRA as banks transition from the small bank to large bank classification. Recorded bank objections are consistent with a non-trivial difference in the regulatory burden between the two classifications. Marsico (2005) notes that "Several banks asserted, ...that banks with assets slightly above the threshold had a difficult time competing with much larger institutions for investments, rarely qualified for an outstanding CRA rating, invested in projects inconsistent with their business strategy and financial interests, and faced disproportionately higher data collection and reporting costs." While anecdotal, such complaints are consistent with the notion that banks consider the consequences of crossing the $250M threshold which partitions small and large banks. Ultimately, this discrete classification of banks and the corresponding change in reporting and monitoring standards serves as the basis for our identification strategy, which we describe in detail in Section 3.

Finally, the CRA faced a second major revision in 2005. The revision created a third bank category ("intermediate-small banks") with a transition to dynamic thresholds designed to track Consumer Price Index growth. Under this reform, intermediate small banks (defined as those banks with assets between $250M and $1B) are no longer evaluated on the lending, investment, and service tests. Instead, they face the same streamlined lending test as small banks along with a new community development test. As a result of this amendment, 1,508 banks with 13,643 branches and total assets of $679B ceased to be subject to the more rigorous lending, investment, and service tests for large banks (Marsico (2005)). In untabulated results, as expected, we find that the bunching around the $250M disappeared following the 2005 reform. In addition, we find no evidence of bunching around the $1B threshold following the 2005 revision. However, recall that banks just below the $1B threshold in 2005 were previously classified as large banks under the 1995 reform. One explanation for the lack of bunching is that those banks falling just below the $1B threshold had previously invested resources in reporting requirements and loan assessment necessary to comply with the more rigorous large bank test. As such, having already adjusted lending standards and built up any necessary infrastructure, such banks face little benefit from strategically slowing growth to achieve an intermediate-small bank classification. For this reason, we focus on the period prior to this second major revision.

2.2. Data and Sample Selection

The majority of our analysis is based on yearly Call Report data disclosed at the bank level, which we obtain in a cleaned format from Philipp Schnabl's website (Drechsler, Savov and Schnabl (2017), Drechsler, Savov and Schnabl (2021)). We augment this dataset with information on branch locations from FDIC. In additional analysis, we utilize loan-level residential mortgage lending activity from HMDA. Finally, to consider the real effects of the CRA on small business activity and independent innovation, we utilize the Census-provided County Business Patterns and patent issuance data, respectively.

Our primary analysis is conducted at the annual level and is based on the fourth quarter Call Report disclosed by each bank. The Call Report data begins in 1986. We truncate the sample to end in 2004, corresponding to the final year before the second major revision to CRA evaluation standards (see the discussion above). The result is an unbalanced yearly panel of 154k observations comprised of 10.3k unique banks over a 16 year span. While we consider the comprehensive set of banks in some analysis, the majority of our tests restrict the sample to banks with reported assets less than $350M.

From this set of unique banks, we collect branch location data from FDIC Summary of Deposit reports. We then use the resulting set of branch locations as a filter for our auxiliary data sources, keeping data from the 3.1k counties with at least one bank in our sample. This results in application and lending decisions for 1.47M residential mortgages reported in HMDA from 1990 to 2004. To measure small business activity, we rely on the County Business Patterns series provided by the U.S. Census. The data series reports the annual number of establishments at the county level, as measured on March 12th, across varying levels of employed workers (e.g., 1-5 employees, 6-10 employees). The CBP data begins in 1991, resulting in a balanced panel that covers all counties with at least one bank in our sample. Finally, to evaluate potential effects on independent innovation, we use patent-level grant data from the PatentsView dataset provided by the USPTO. This data contains information on the inventor, location, and assignee (if applicable) over the period from 1985 to 2005 for counties in our sample.

2.3. Summary Statistics

Table 1 presents summary statistics for key variables considered. Panel A presents statistics collected from call reports for the bank-year level. The size distribution of banks in our sample is right skewed, with a mean asset value of $534M and a median value of $65M. A similar relationship exists for loans reported on the balance sheet at year-end. Moreover, banks in our sample exhibit a 6% mean CPI-adjusted asset growth rate (difference in natural logs). Finally, the average bank is highly levered, financed by approximately 10% equity.

[Insert Table 1 Near Here]

Panel B of Table 1 presents summary statistics on real outcomes we consider. When considering the effects on small business growth, the unit of observation is the county-year level.

Small businesses, proxied by establishments employing less than 20 or 50 employees, respectively make up 90% and 96% of all establishments in the average county-year. Similarly, between 10% and 15% of counties have a bank falling just below the CRA size threshold, depending on the classification considered. We also consider potential effects on the reallocation of credit using HMDA application-level data. The average mortgage application in our sample is for a loan of $90k, with an approval rate of 87.4%. Of these applications, approximately 39% meet the criteria necessary to earn credit towards the lending test (e.g., LMI loans).[8]

Before moving to a formal bunching framework, we briefly examine the potential bunching of banks around the CRA size threshold using a reduced-form approach. Panel A of Figure 2 reports the histogram of year-end assets reported in Call Reports from 1996 to 2004, aggregated to bins of $2.5M. If banks are strategically managing assets to avoid narrowly crossing the $250M barrier, we would expect a discrete change in the frequency of bank-year observations at this threshold, with a larger count of bank-year observations in the bins slightly to the left of the boundary relative to their counterparts to the right. Two patterns emerge from the panel. First, while the frequency of banks generally declines with asset size, there is a modest flattening of this trend as bank assets approach the $250M mark from the left. Second, the frequency count of bank-year observations exhibits a noticeable drop as bank assets cross the *Large Bank* threshold, consistent with the notion of banks bunching in an attempt to avoid the increase in regulatory compliance costs.

[Insert Figure 2 Near Here]

While purely motivational, we consider a number of placebo groups to alleviate concerns that the histogram in Panel A is being generated by coincidence. These results are presented in the remaining panels. First, we consider an earlier time period in which the threshold was

8. To be classified as a Low- and moderate-income (LMI) loan, an individual has to either: a) have a reported income that is less than 50 percent of the area median income, or b) reside in a census tract with a median family income that is less than 50 percent of the area median income.

not considered. Panel B of Figure 2 repeats the previous analysis for the period from 1986 to 1993, with no noticeable change in the frequency of observations around the threshold. Next, we ensure our results are not explained by a behavioral effect related to salient boundaries. Again, we find no obvious change in the distribution of bank assets when considering placebo thresholds of \$150M (Panel C) or \$350M (Panel D). Overall, the results depicted in Figure 2 present motivating evidence that banks near the boundary are strategically managing their asset size in response to the regulation. We now turn to a more rigorous means by which to quantify this bunching behavior.

3. Methodology & Main Results

To determine the extent to which banks strategically manage their assets to avoid the discrete jump in compliance costs associated with the large bank test, we use a pair of distinct empirical approaches. First, we describe and implement the bunching analysis pioneered in Saez (2010). To complement this approach, we develop a Bartik-style difference-in-differences approach similar to Greenstone, Mas and Nguyen (2020).

3.1. Excess Mass Estimation

A simple examination of the bank size distribution in Figure 2 suggests that this regulatory threshold influences bank behavior. To formally measure the excess mass in the observed distribution, we first need to construct the counterfactual distribution that would exist in the absence of the regulatory threshold. The standard approach in Chetty et al. (2011) and Kleven and Waseem (2013) involves fitting a high-order polynomial to the observed distribution while excluding a region around the threshold and then extrapolating this polynomial through the omitted area.

However, an important advantage of our setting is that we observe the bank size distribution before the introduction of the \$250M threshold. This allows for an alternative approach, which uses the observed pre-period distribution of bank sizes between 1986 and 1994 as the counterfactual density (e.g., Londoño-Vélez and Ávila-Mahecha (2018), DeFusco, Johnson and Mondragon (2020)). This methodology alleviates concerns about implicit functional form assumptions (Blomquist, Newey, Kumar and Liang (2017)).

In order to utilize the pre-reform distribution of bank sizes, we begin by centering each bank's asset size around the CRA threshold. A value of zero corresponds to a bank with an asset size equal to \$250M, whereas all other values are percentage deviations from the threshold. Subsequently, we group each bank-year observation a_j into bins. We define the bins as $j \in [l, u]$, where l and u are the lower and upper levels of the region most affected by the regulatory threshold. Then, from 1996 to 2004, we count the number of banks in each bin, n_j.

The main assumption of this approach in our setting is that there must be some maximum bank size $\bar{j}$ below which the distribution is unaffected. The intuition for this assumption is that imposing the CRA threshold should only move banks from above the cutoff to below and/or alter the behavior of banks below the threshold but sufficiently close to be at risk of crossing it in the near future. In either case, the assumption is that banks sufficiently far from the threshold should be unaffected and thus, the policy should not affect the entire distribution. This assumption provides a useful normalization that allows us to compare the pre-period distribution with that after the CRA threshold was put in place.

Since the number of banks differs across periods, it is not informative to directly compare the number of banks in a given asset bin between the 1986-1994 and 1996-2004 periods. To account for any difference in the number of banks across time, we follow DeFusco et al. (2020) and divide each bin count by the corresponding level of activity up to $\bar{j}$, resulting in readily comparable ratios. In particular, if $\sum_{i=0}^{\bar{j}} n_j = N_{\bar{j}}$ and $\sum_{i=0}^{\bar{j}} n_j^{pre} = N_{\bar{j}}^{pre}$, then we can make the asset bin counts comparable:

$$\frac{\hat{n}_j}{N_{\bar{j}}} = \frac{n_j^{pre}}{N_{\bar{j}}^{pre}} \triangleq \hat{\pi}_j.$$

In particular, $\frac{n_j^{pre}}{N_{\bar{j}}^{pre}}$ is the average ratio of the number of banks in bin j to the total banks to the left of $\bar{j}$ for a specific year between 1986 and 1994. Therefore, we can rewrite $\hat{n}_j = \hat{\pi}_j \times N_{\bar{j}}$.

To measure excess bunching, we take the sum of the difference between the normalized counterfactual and empirical distributions for $\bar{j} \leq j < 0$

$$\hat{B} = \sum_{\bar{j} \leq j < 0} (n_j - \hat{n}_j).$$

We calculate standard errors by bootstrapping from the observed sample of banks, drawing 1,000 random samples with replacement, and re-estimating the excess mass parameter at each iteration.

[Insert Figure 3 Near Here]

For our main analysis, we set $\bar{j}$ =\$200M or 20% relative to the threshold. This limit is informed from Figure 3, which suggests the pre- and post-period distributions are roughly similar for bins less than this cutoff. We also show that our results are robust to alternative choices for $\bar{j}$.

In our second approach, we construct the counterfactual distribution by fitting the following regression to the count of banks in each bin:

$$n_j = \sum_{i=0}^{p} \beta_i (a_j)^i + \sum_{k=l}^{u} \gamma_k \mathbb{1}(a_k = a_j) + \varepsilon_j$$

where a_j is the standardized asset size in bin j, and p is the polynomial order. The counterfactual bin counts are obtained as the predicted values from the above equation omitting the contribution of the dummies in the excluded region.

$$\hat{n}_j = \sum_{i=0}^{p} \hat{\beta}_i (a_j)^i.$$

Excess bunching is estimated as the difference between the observed and counterfactual bin counts within and to the left of the excluded region:

(1) $$\hat{B} = \sum_{l \leq j < 0} (n_j - \hat{n_j}).$$

This procedure relies on specifying the excluded region $[a_l, a_u]$. The lower bound of the excluded region (a_l) is determined visually, which in our case is set at $200M (or 20%). For the upper bound, we use the "point of convergence" approach, in which the difference between the excess mass to the left of the threshold and the missing mass to the right is minimized (Kleven and Waseem (2013)). We calculate standard errors for all estimated parameters using a bootstrap procedure, as in Chetty et al. (2011).

There are two key identifying assumptions behind this approach (Kleven (2016)). First, the counterfactual distribution, in the absence of the CRA reform, is smooth around the $250M threshold. Figure 2 supports this assumption by showing that banks do not bunch below the notch in the period before introducing the CRA threshold.[9] A potential threat to this assumption is the existence of another policy change at the same threshold. To the best of our knowledge, this is not the case in our setting. Second, the counterfactual distribution can be well approximated by a flexible polynomial fitted over the manipulation-free section of the distribution when excluding the bunching window. The shape of the counterfactual distribution is important, as this approach relies on the extrapolation of a large range when estimating the counterfactual distribution. Following the literature, we perform a sensitivity analysis concerning the order of the polynomial p and the bin-width.

9. The fact that banks do not cluster below the threshold prior to the change in the regulation alleviates the concern that the $250M threshold serves as a reference point for banks.

3.1.1. Excess Mass Estimates

We now apply these two approaches to estimate the number of banks that adjust their asset size to potentially circumvent the regulation. In Figure 3, we plot the empirical distribution between 1996 and 2004, along with the pre-period normalized distribution. Each dot represents the share of bank-year observations by one-percent-wide bins. The vertical lines delimit the region affected by the threshold. In our preferred specification, we choose \$200M (20%) as our lower bound as there is a clear excess mass at this bin. Alternatively, we also show estimates using \$220M (12%). The empirical distribution shows a discontinuity at the \$250M threshold and excess mass that starts at \$200M relative to the counterfactual distribution.

Consistent with the findings in Figure 3, estimates in Table 2 indicate a significant excess mass below the threshold. In the first column, the point estimate of 598 indicates that close to 600 bank-year observations that would have otherwise had assets larger than \$250M strategically managed their asset size to move from above to below the threshold. The estimates are smaller but still statistically significant when setting the lower bound of the excluded region at \$220M.

[Insert Table 2 Near Here]

Next, we follow the standard approach and build the counterfactual distribution by fitting a 6th degree polynomial to the observed number of banks in each bin, omitting the bins in the excluded region used to estimate the excess mass. Internet Appendix Figure IA.1 confirms the findings in Figure 3, showing an excess mass to the left of the \$250M threshold. In Columns 3 and 4 of Table 2, we repeat the excess mass calculation using the polynomial approach. When considering a lower bound of \$200M, the point estimate indicates that 410 bank-years respond to the CRA threshold by manipulating their size. Again, the estimate decreases but remains significant when considering the more narrow bandwidth. In Internet Appendix Table IA.1, we explore robustness to various choices of parameters. Columns 1-3 present the estimates using a \$1.0M bin-width for 5th, 6th, and 7th degree polynomials. The excess bunching magnitudes are qualitatively similar to those in Table 2. Similarly, Columns 4 and 5 also demonstrate the robustness of the main result when using a \$2.5M bin-with in conjuction with 5th and 7th degree polynomials, yieliding comparable results.

Altogether, the estimates in Table 2, Figure 3 and Internet Appendix Figure IA.1 confirm that banks responded to the discrete change in regulatory requirements tied to the \$250M threshold. It is important to note that our approach captures the actions (and thus revealed-preferences) taken by the agents who control the banks in our sample. As such, to the extent that banks face a principle-agent problem where agents experience the additional cost of being classified as a *large* bank, our approach is unable to partition the potential cost imposed on the bank from that borne by its manager. Nevertheless, our estimates are still able to speak to the overall cost of the regulation on banks, regardless whether this is partially the result of a principle-agent problem faced by a bank.

3.2. Means of Strategic Avoidance

The results from the previous section are consistent with a bank strategically managing its asset size to reduce the regulatory burden it faces, where the tiered nature of the evaluation process induces banks to bunch just below a discrete jump in regulatory oversight. However, while well-suited to measure the excess mass of banks just below the CRA evaluation threshold, the excess bunching analysis is unable to evaluate the means by which banks avoid the increase in regulatory oversight costs. To this end, we turn to a reduced-form framework similar to that of Greenstone, Mas and Nguyen (2020). Intuitively, the approach segments banks by asset size prior to the implementation of the 1995 CRA reforms and then tests for a differential response following the enactment of the threshold across bins of *pre-regulation* bank assets. Thus, the approach is akin to the shift-share methodology of Bartik (1991), measuring the differential response to treatment based on pre-treatment differences in characteristics.

More formally, we implement this approach using OLS regressions of the following form:

$$y_{it} = \beta Assets^{\tau}_{i,LB-250} \times 1(t > 1995) + \eta_i + \phi_t + \varepsilon_{it} \tag{2}$$

where y_{it} is the outcome of interest for bank i in year t. $Assets^{\tau}_{i,LB-250}$ is an indicator variable that takes on a value of one if the end-of-year assets of bank i, measured in year τ, lie within the region $[LB, \$250M]$. The primary variable of interest is the interaction of $Assets^{\tau}_{i,LB-250}$ and $1(t > 1995)$, an indicator variable that takes on a value of one in the years following the enactment of the reform.[10] Thus, β captures any change in the actions of banks that fall just below the CRA size threshold in response to the regulation. We control for general differences across banks and years with the inclusion of bank (η) and time (ϕ) fixed effects, which subsume the un-interacted terms associated with the interaction. Standard errors are clustered at the bank level. Our identifying assumption is that the post-1995 outcome for a bank with pre-reform assets just below the threshold would have been identical to the outcomes of other banks had the \$250 million threshold not been introduced.

With this empirical approach in hand, we begin by examining the potential effect of the CRA's tiered evaluation scheme on bank growth. Here, the dependent variable is a bank's total assets as reported in year-end Call Reports, which we adjust for inflation using a CPI-deflator and take the first-difference of logged values. Thus, point estimates reflect a change in the real growth rate of a bank's assets.

Table 3 presents results of OLS regressions of the form described in Equation (2). If banks are attempting to strategically avoid the additional regulation that accompanies a large bank classification, this would predict a negative coefficient on the interaction term of interest as banks just below the threshold slow their asset growth. We begin by estimating the differential response of asset growth rates following the CRA reform for banks with

10. We define the post period as 1996 onward, to allow banks sufficient time to respond to the 1995 rule change. We show robustness to this choice below.

assets between \$200M and \$250M, as measured in 1994.[11] In the first specification, we contrast growth rates for this set of banks against a control group consisting of all other reporting banks. The coefficient of -0.024 (t-stat=-3.73) on $Assets_{200-250} \times 1(yr > 1995)$ indicates that those banks falling just below the CRA bank size threshold prior to the 1995 reform experience a relative decrease in real growth rates of 2.4pp following the reform's enactment. Relative to the average real growth rate in our sample of 5.6pp, this represent a 43% reduction in asset growth. To alleviate concerns that the previous estimate is partially due to the comparison of growth rates among banks with very large differences in initial sizes, we restrict the sample in the second specification to banks with assets less than \$500M. Following this change, the point estimate on the interaction term increases in magnitude and statistical significance, implying a relative decrease in growth rates of 3.7pp (t-stat=-5.41) for banks falling just below the size threshold. When further restricting the sample to banks with less than \$350M in the third specification, we see another increase in the magnitude and statistical significance of the estimated effect.

[Insert Table 3 Near Here]

Note, our selection of \$200M for the lower bound of the treated group is designed to capture banks that both: 1) fall below the CRA threshold prior to the reform and 2) face a reasonable chance of approaching the threshold in subsequent years, absent intervention. Nevertheless, the specific value chosen for the lower bound is somewhat arbitrary. To this end, we repeat the previous analysis while considering an alternate lower bound of \$220M in the final three specifications of Table 3. Although we generally find a decrease in the estimated magnitude of the effect following the change, point estimates continue to indicate a relative decrease in asset growth rates for those banks that fall just below the size threshold in the year prior to the CRA reform.[12] For instance, when benchmarked against banks with

11. For ease of exposition, going forward we refer to a bank's assets when measured in the year prior to the CRA reform as "pre-reform" assets.

12. Note, the point estimate is not statistically significant at traditional levels in the least restrictive sample (Column 4).

assets less than \$350M in the final specification, banks with pre-reform assets between \$220M and \$250M experience a relative decrease in real asset growth of 3.5pp.

While the results in Table 3 suggest that banks falling below the CRA size threshold strategically slow growth to avoid a step-up in regulatory compliance costs, they are silent regarding the timing of the effect. To this end, we make a slight modification to Equation (2), replacing $1(yr > 1995)$ with a vector of indicator variables corresponding to each calendar year from 1990 to 2004 (with 1989 making up the base case). Thus, the interaction of each time indicator with $Assets_{LB-250}$ allows us to see the differential response in growth rates of potentially bunching banks through time. Figure 4 graphically presents the resulting coefficients (and corresponding 95% confidence intervals) following this change for specifications analogous to Column 3 (hollow triangles) and Column 6 (solid squares). Two patterns emerge from the figure. First, the growth rate of banks in the bunching region does not appear to differ from other banks prior to enactment of the CRA reform. More importantly, while the relative growth rate of banks in the bunching region is statistically indistinguishable from other banks compared to the 1989 base case, more importantly, the relative growth rate is stable and does not exhibit a linear trend in the years leading up the 1995 reform. This similarity in growth is reassuring, alleviating concerns that our approach could pick up a pre-existing trend or difference across the two groups, in support of our identifying assumption. Second, the growth rate of banks falling just below the large bank size threshold experiences a sharp drop in 1995, coinciding with the year of the CRA reform. Moreover, while exhibiting some variability across years, the difference is relatively stable over the sample period.

[Insert Figure 4 Near Here]

Overall, the results presented in Table 3 and Figure 4 are consistent with the excessive bunching estimated in Section 3.1, with banks that fall below the size threshold experiencing a relative slowing of asset growth rates following the enactment of the 1995 CRA reform. Before continuing, we ensure that results are robust to alternative modeling choices along multiple dimensions. First, Internet Appendix Figure IA.2 reports the coefficients from OLS regressions analogous to Column 3 of Table 3 when varying the lower bound used to segment banks by pre-reform assets. Coefficients are relatively stable when considering a lower bound that ranges from \$200M to \$240M. In contrast, the 95% confidence intervals reported in the figure generally widen as the lower bound increases, suggestive of a reduction in the precision of the estimator as the number of banks in the treated group decreases. Second, although inconsistent with Figure 4, we consider the possibility that banks began to strategically manage asset sizes prior to 1994 in anticipation of the CRA reform. Internet Appendix Table IA.2 repeats the analysis in Table 3 when instead classifying banks by their year-end reported assets as of 1993. Results are relatively similar to the baseline analysis following this change. Finally, Internet Appendix Table IA.3 reports the results of OLS regressions when adapting Equation (2) to consider the contemporaneously-measured assets of a bank. Intuitively, as a bank crosses the CRA asset threshold (possibly due to a natural increase in local banking demand), the bank may find it excessively costly to shrink in size to again fall below the threshold. Results following this change demonstrate point estimates similar to those in Table 3, albeit slightly larger in magnitude in some specifications, with tighter confidence intervals. However, we caution against drawing strong inferences from this test. The use of contemporaneous assets likely over-weights the sample of treated banks towards those for which either the increase in regulatory oversight is excessively costly or the opportunity cost of foregone growth is relatively small.

The previous results suggest that banks are actively slowing their *asset* growth rates. However, this objective can be achieved by numerous means which vary considerably in their impact on local economic growth. For instance, a bank may slow asset growth by either trimming cash positions or altering core business operations, such as loan origination and retention. To this end, we now examine the effect on growth in specific components of a bank's assets reported in the Call Reports.

[Insert Table 4 Near Here]

Panel A of Table 4 considers the effect of the CRA reform on banks with pre-reform assets between \$200M and \$250M. Each column re-estimates the final specification from Table 3 when considering growth in a different outcome, beginning with cash holdings in the first column. The point estimate on the interaction term indicates that banks falling below the threshold reduce their growth in cash holdings by 6.6pp (t-stat=-4.82). The second column indicates a similar response with respect to a bank's marketable security holdings. While providing flexibility in managing year-end assets, and possibly increasing a bank's risk exposure through a reduction of its protective buffer, these asset components do not represent a bank's core business. To this end, we turn our attention to the growth in loan holdings in the remaining columns. Point estimates indicate that banks falling just below the \$250M threshold experienced a significant slowing of loan growth rates, with an estimated decrease of 5.2pp for all loans collectively. A similar effect is present for the growth of real-estate loans (Column 4) and C&I loans (Column 5).

Panel B of Table 4 repeats the previous specifications when contrasting the growth rate for the set of banks with pre-reform assets bounded below at \$220M against the control group. In general, we continue to find evidence consistent with a slowing of growth rates across each asset type, albeit with a general reduction in the precision of estimates to the point of losing statistical significance in the final specifications which consider specific loan types. Moreover, estimates from Panel B suggest a stronger response by treated banks with respect to cash and security holdings relative to portfolio loans. This is somewhat unsurprising, as cash holdings likely exhibit a greater degree of liquidity while not impacting core operations to the same degree as loans, making it easier to slow overall asset growth by managing cash holdings relative to loan holdings. Taken together, the results in this table suggest that banks taper growth in portfolio loans while also reducing excess cash deposits to manage asset levels. The former response has potential implications for local economic growth, a point we revisit in Section 4.

One limitation of using Call Report data is that we cannot observe the flow of loans originated by banks and must instead draw inferences from the year-end stock held on a bank's balance sheet. However, to the extent that the two are correlated, a reduction in originated loans by banks near the policy threshold provides other interesting testable predictions. Specifically, if banks just below the threshold respond by adjusting the selection criteria of the marginal loan originated, it is plausible this has implications for loan performance.

To this end, Table 5 examines loan profitability and non-performing rates of banks near the CRA threshold relative to other banks. Specifically, the first two columns examine the differential response of the CRA reform on the profitability of banks falling just below the threshold, where profitability is defined as the ratio of net interest income to year-end loan values. In the first specification, we find a statistically significant increase in profitability for banks falling just below the threshold when considering the more broadly defined classification of potentially bunching banks (pre-reform total asset between $200M and $250M). The coefficient on the interaction term indicates that following the CRA reform, banks just below the threshold exhibit an increase in profitability of 2.7% (t-stat=3.79) relative to other banks. This relation continues to hold when considering the more narrowly defined segment of banks near the CRA threshold.

[Insert Table 5 Near Here]

In the latter pair of specifications, we turn to the rate of non-performance among portfolio loans. Here, we follow the construction of the non-performing loan index provided by the St. Louis Federal Reserve and define the non-performing rate as the sum of *Total Loans and Lease Finance Receivables, Nonaccrual* and *Total Loans and Lease Finance Receivables, Past Due 90 Days and More and Still Accruing* divided by total loans outstanding. The coefficients suggest a modest, but statistically significant decrease in the post-reform change in non-performing rate for banks falling just below the threshold. Specifically, following the 1995 CRA reform, the non-performing rate for banks with pre-reform assets just below $250M decreases by between 0.1% and 0.3% relative to similarly sized banks. In sum, this evidence from loan profitability and non-performance rates is consistent with an increase in lending selectivity for a bank falling just below the $250M threshold, resulting in the provision of a marginal loan that is on average more profitable and less likely to be non-performing.

Interestingly, while a bank falling just below the threshold adjusts its lending behavior in a way that yields more profitable loans, recall from Table 4 that the bank also experiences a slower growth rate in its cash holdings. Before continuing, we consider two potential implications of this reduction in cash held on the balance sheet. First, to the extent that cash holdings provide a protective buffer against negative shocks, a bank may offset the reduction of such buffers by adjusting its capital structure. In this case, we would expect a treated bank to respond by reducing the relative amount of leverage used. Second, if a bank is trimming excess cash from its balance sheet, a natural means by which this is accomplished is through dividend disbursements. Thus, it is plausible that to fall below the threshold, a bank would respond to the CRA reform by increasing its dividend payout policy.

[Insert Table 6 Near Here]

Table 6 considers these two possibilities in turn, beginning with a bank's choice of capital structure. Specifically, the first two columns consider the effect of the CRA reform on the capital structure of banks falling just below the $250M size threshold relative to other banks with assets less than $350M. Here the dependent variable is the share of equity financing relative to total assets. For both classifications of treated banks, we see that banks falling just below the size threshold decrease leverage and increase the share of equity financing in response to the 1995 reform. In the latter pair of specifications, we turn our focus to a bank's dividend payout policy. Here, the dependent variable is the yearly dividend payout total relative to equity value. When considering the effect on banks with pre-reform assets between $200M and $250M, the interaction suggests an increase in dividends paid of 4.3pp (t-stat=2.54). However, when considering the more narrowly defined definition of treated banks in the final specification, we see a reduction in the coefficient and precision, to the point where the coefficient loses statistical significance at traditional levels. Taken together, the results presented in this table suggest a means by which a bank is able to trim its cash holdings, while remaining consistent with the ensuing response following the reduction in its protective buffer.

3.3. Cross-Section of Strategic Avoidance

While banks may position themselves to avoid larger compliance costs embedded in the CRA's tiered evaluation scheme, this response is not without its own cost. Instead, the strategic slowing of growth rates likely represents otherwise profitable maturity-transforming operations passed up by a bank. Thus, if a bank trades off the two costs, it is plausible that it is more likely to taper asset growth when there is a lower cost of forgone projects.

This intuition motivates our next test, in which we consider potential heterogeneity in profitable lending opportunities available to a bank. Specifically, we consider two proxies designed to capture variation in lending opportunities, the prior growth rate of a bank's assets and loans. As these proxies are also outcomes of interest, we classify banks based on pre-1989 growth rates while the regressions are estimated using Call Report data from 1989 onward, to avoid using the same data to both partition banks and form the pre-treatment sample. For each proxy, we construct an indicator variable, $1(BelowMed.Growth)$, which partitions the sample into banks with a median growth rate that is either below or above that of the full sample. We then extend Equation (2) by interacting the indicator variable with our key variable of interest, $Assets^{1994}_{i,LB-250} \times 1(t > 1995)$, yielding a traditional triple difference-in-differences framework.[13] If banks are weighing the cost of regulatory compliance against forgone operations, this would predict a negative coefficient on the triple interaction, indicating a larger effect of the CRA reform on banks just below the threshold when the bank has a slower rate of past growth.

[Insert Table 7 Near Here]

Table 7 presents results of OLS regressions based on this triple difference-in-differences

13. We also include the interaction of $1(BelowMed.Growth)$ and $1(t > 1995)$, while all the other double-interaction and un-interacted terms are subsumed by the fixed effects.

estimator, where the outcome of interest is again CPI-deflated asset growth. In the first pair of specifications, we proxy for variation in profitable banking operations available to a bank using past asset growth. In the first column, the point estimate on the triple interaction term is -0.019 (t-stat=-1.87), indicating a larger effect of the CRA reform for banks potentially facing less attractive lending opportunities. We see a similar effect in the second specification which uses a $220M lower bound to classify the set of banks approaching the bank size threshold. We see consistent results when considering the proxy based on loan growth in the second pair of specifications, albeit not statistically significant at traditional levels for one column. Taken together, the results presented in Table 7 are consistent with the notion that banks trade-off the cost of regulation with that of passed up lending opportunities. Interestingly, if past bank growth is correlated with the economic health of an area, counter to the CRA's stated goal, this highlights a potentially out-sized effect of the policy on lending in those areas most in need of economic growth.

4. Impact on Residential Lending and Local Growth

The results in Section 3 demonstrate an adverse effect of the 1995 CRA reform in which banks strategically slow their growth rates to avoid the compliance cost brought on by increased regulatory oversight. Interestingly, to the extent that this response hampers either credit supply or economic growth in areas served by an affected bank, this unintended consequence runs counter to the stated purpose to "encourage depository institutions to help meet the credit needs of the communities in which they operate" (Federal Register (1995)). In this section, we explore the potential impact of the regulatory threshold on residential lending and two forms of local economic activity arguably reliant on the affected banks: residential mortgage lending and local economic growth.

4.1. Residential Mortgage Lending

We begin by considering the effect of the CRA reform on the distribution of credit provided by bunching banks in the mortgage lending market. Specifically, our focus is on potential heterogeneity in mortgage lending decisions across qualifying loans used in the evaluation process (e.g., LMI loans) relative to non-qualifying loans. In contrast to the other real outcomes considered in the latter half of this section, the theoretical prediction regarding a differential response of banks falling just below the threshold is somewhat ambiguous.

First, a bank may strategically slow growth to avoid the greater regulatory cost accompanying the large bank evaluation criteria. At the same time, both the small bank test and large bank test consider the share of a bank's mortgage lending activity that services either: 1) low- and moderate-income census tracts or 2) individuals in other census tracts with a reported income that falls within either the low- or moderate-income categories. Thus, it is plausible that a bank falling below the threshold, when only the bank's mortgage lending record is considered, may respond by increasing the share of credit available to qualifying loan applicants.

In contrast, Agarwal, Benmelech, Bergman and Seru (2012) find that banks increase the share of lending made to qualifying applicants precisely when they are undergoing a CRA evaluation. This suggests that banks typically have a weaker preference to extend credit to CRA qualifying loans, potentially reflecting the profitability of such loans. Thus, if a strategically bunching bank is sacrificing otherwise profitable growth, it may respond by reducing the amount of credit it provides to qualifying applicants.

To evaluate these two potential responses of banks falling just below the CRA size threshold, we turn to application-level data reported in the yearly HMDA disclosures. For each loan application, the administrative records report information on the loan type (e.g., origination vs. refinance), size, reported borrower income, property census tract, and lending decision, among others. Starting with all HMDA-reported loan applications from 1990 to 2004, we restrict the sample to origination applications made to banks with 1994-measured assets below \$350M, the most restrictive sample considered in Section 3. The result is a sample covering 1.23M loan applications and slightly more than 2k unique banks. Using this sample, we estimate linear probability models of the likelihood of a loan application being accepted. To estimate potential heterogeneity in the effect of the 1995 reform, we extend the difference-in-differences empirical model outlined in Equation (2) by including an additional interaction term, yielding a triple diff-in-diff. Specifically, we introduce $1(LMI)$, an indicator denoting a loan application that qualifies for CRA credit. Here, our interest is on the triple-interaction, which captures the differential change in post-reform acceptance rates by banks with 1994-measured assets slightly below the threshold for LMI-loans relative to other loans. Given the granularity of the data, we are able to control for potentially confounding effects with a more comprehensive set of fixed effects. Specifically, we allow for time-invariant differences in acceptance rates at the bank-LMI level. Moreover, we control for general time-varying differences in acceptance rates of qualifying and non-qualifying loans with an LMI-year fixed effect. Finally, we allow for spatial heterogeneity in acceptance rates with a county fixed effect.

[Insert Table 8 Near Here]

Table 8 presents the results of OLS regressions, where the outcome is an indicator for an acceptance decision. In the first column, the point estimate on the triple-interaction term indicates that a bank just below the size threshold exhibits a post-reform decrease in the acceptance rate of LMI loans of -2.2pp (t-stat=-3.15) relative to non-qualifying loans. This result continues to hold in the second specification, which allows for time-varying effects of local economic conditions with a county-year fixed effect. Point estimates are relatively unchanged when we allow for general time-varying differences in acceptance rates that vary with loan size (rounded to the nearest \$5k) with a loan amount-year fixed effect. Finally, these results continue to hold when considering the more narrowly defined set of potentially bunching banks with pre-reform assets between \$220M and \$250M.[14]

14. Note that coefficients are less precisely measured using the more narrowly defined group, with the final

Overall, while theoretically ambiguous about what effect the CRA reform would have on the distribution of credit, the results in Table 8 suggest a reduction in credit to the group of applicants specifically named in the legislation. This result is consistent with the notion that a bank responds to the cost of strategically slowing growth by reducing credit extended to applicants at the margin defined by the CRA, in line with Agarwal, Benmelech, Bergman and Seru (2012).

Before continuing, we briefly examine the extent to which the market ameliorates the reduction in LMI lending by bunching banks, where non-bunching banks fill the void by increasing residential lending. Specifically, using the HMDA application-level data, we construct 1) a panel of loan origination dollar totals at the bank-year-census tract level for each non-bunching bank and 2) the census tract-level share of loans originated by bunching banks in the pre-reform period. Following the construction of these variables, we estimate OLS regressions (similar to Equation (2)) of the following form:

$$y_{ict} = \beta TractShare_{c,LB-250} \times 1(t > 1995) + \eta_{(i)c} + \phi_{it} + \varepsilon_{ist} \tag{3}$$

where y_{ict} is the outcome of interest for bank i in census tract c and year t. Now, $TractShare_{c,LB-250}$ is a continuous variable set equal to the census tract-level share of loans originated by banks with assets that lie within the region $[LB, \$250M]$ in 1994. We winsorize $TreatedShare$ at the 1% level to account for extremely large origination shares present in less populated regions. To control for differential lending across census tracts, we include a tract-level fixed effect (η) which we allow to vary at the bank level in some specifications. Finally, as our interest is in the potential differential change in lending by non-bunching banks in treated areas relative to non-treated areas, we include a bank-year fixed effect (ϕ) to control for an overall shift in residential lending by a bank through time.

specification becoming statistically insignificant at traditional levels. The loss in precision can be explained by the fact that banks with assets between \$200M-\$220M, that also have incentives to bunch, are now in the control group under this more narrow definition.

[Insert Table 9 Near Here]

Panel A of Table 9 reports the results of OLS regressions of the form described in Equation (3), where the outcome of interest is the natural log of the total dollar amount of originated loans, measured at the bank-year-census tract level. Importantly, as our interest is in the response of non-bunching banks, we only consider banks which lie outside of the bunching region in the test. For ease of interpretation, we standardize the explanatory variable of interest $TractShare$ to be mean-zero and a standard deviation of one. Overall, we find no evidence that non-bunching banks exhibit a differential lending response to the CRA reform in census tracts previously served by treated (bunching) banks. Point estimates are statistically insignificant and demonstrate economically small magnitudes regardless of the definition of a bunching bank (e.g., $[\$200M, \$250M]$ vs. $[\$220M, \$250M]$) or the inclusion of bank-tract fixed effects. Panel B repeats the previous regressions when considering the natural log of dollars lent to *LMI-qualifying* loan originations. The results are consistent with the previous panel, failing to find a differential response by non-bunching banks in treated census tracts.[15]

4.2. Local Economic Growth

In a final series of tests, we shift our focus from the actions of banks specifically impacted by the size threshold to the local effects on the areas they serve. However, this requires a slight modification to our empirical approach. As a first step, we identify the geographic area served by each bank by collecting branch location data from the annual Summary of Deposit reports provided by the FDIC. While highly detailed data on branch locations is disclosed, we aggregate locations to the county level to reflect the granularity for some of our outcome variables. With this data, we redefine our estimating equation to consider observations at

15. We note that one out of the eight specifications is marginally significant at the 10% level before adjusting for multiple hypothesis testing. However, even here the point estimate is economically small. In a robustness test, we repeat the previous analysis when replacing the outcome variable with the natural log of one plus dollar totals so as to include bank-tract-years with zero originations. We continue to find no evidence for an increase in supply by other banks following this change.

the county-year level, rather than the bank-year level, yielding the following reduced-form:

$$y_{ist} = \beta BranchShare^{\tau}_{i,LB-250} \times 1(t > 1995) + \eta_i + \phi_{st} + \varepsilon_{ist} \tag{4}$$

where y_{ist} is the outcome of interest for county i in state s and year t. Now, $BranchShare_{i,LB-250}$ is a continuous variable set equal to the county-level share of branches belonging to banks with assets that lie within the region $[LB, \$250M]$ in year τ.[16] We winsorize $BranchShare$ at the 1% level to account for extremely large branch shares present in some small counties, while also measuring bank branch locations as of December 1994 to avoid capturing endogenous bank expansion due to local economic conditions. We also continue to evaluate banks using reported assets as of December 1994. The only other modification relative to Equation (2) is the ability to consider a state-year fixed effect, which allows us to control for broader economic effects caused by other changing factors (e.g., state-level bank regulation reforms (Jayaratne and Strahan (1996))). With this modified framework, we now consider the impact of the 1995 CRA reform on two facets of local economic growth arguably dependent on credit from smaller banks: small business growth and independent innovation.

If lending is geographically segmented (Becker (2007)), it is plausible that a decrease in credit access as a result of the CRA reform hindered local business growth. Both Berger and Udell (1995) and Weston and Strahan (1996), among others, make note of a positive correlation between firm and bank sizes in lending relations, where small businesses are more likely to be serviced by smaller banks. This would suggest that a reduction in growth for banks falling below the \$250M size threshold is likely to have a negative impact on small businesses in particular. To consider this possibility, we turn to administrative data on business establishment counts reported in the annual County Business Patterns data, provided by the U.S. Census Bureau. Importantly, the series reports establishment counts at the county level, broken out by establishment-level employment ranges (e.g., 5-9 employees,

16. We collect bank branch locations from the FDIC Summary of Deposit reports.

10-19 employees, etc.).

Panel A of Table 10 reports the results of OLS regressions examining potential distributional effects that the CRA had on establishment sizes in treated counties. Here, the outcome is the county-year count of establishments employing less than 20 or 50 employees, scaled by the count-year total establishment count to explain general changes in local economic conditions, multiplied by 100 for ease of illustration. The primary variable of interest is the interaction of $BranchShare$ and an indicator which takes the value of one in the years following the CRA reform. To more easily interpret economic magnitudes, we standardize $BranchShare$ to be mean-zero and a variance of one. We begin by considering the effect of the CRA reform on the share of establishments employing less than 20 workers. The point estimate of -0.057 (t-stat$=-2.73$) on $BranchShare_{200-250} \times 1(yr > 1995)$ indicates that following the CRA reform, the share of small businesses in a county declined by approximately 0.06pp for a one-standard-deviation increase in the county-level share of bank branches falling just below the size threshold. To provide context, the share of small businesses demonstrates a systematic decline in our sample, with an average annual decrease of 0.09pp. Measured against this downward trend, the effect of a one-standard deviation increase in the share of treated banks represents roughly two-thirds of a year of small business decline over our sample.

The second specification considers the more narrowly defined bunching region of pre-reform assets between \$220M and \$250M. Economic and statistical significance increase following this change, with an estimated decrease in the share of small business of -0.068 (t-stat$=-3.32$). Next, we consider the potential impact on a more broadly defined collection of "small firms," expanding the criteria to include those with less than 50 employees. We repeat the first two empirical specifications following this change. The reduction in point estimates and statistical significance following this change suggests that the reduction in bank growth for banks falling just below the threshold had a particularly large impact on smaller establishments who are potentially more likely to borrow from smaller banks.[17]

[Insert Table 10 Near Here]

The results in Panel A of Table 10 suggest that the post-reform share of small businesses declined in counties served by treated banks relative to counties without such banks. Before continuing, we ensure that this result is not driven by counties serviced by only extremely large banks. Panel B narrows the control group by restricting the sample to counties served by a bank with less than $350M in assets, mirroring the final specification of Table 3. Results remain quantitatively similar across all specifications following this change.

Finally, we turn our attention to a related economic outcome possibly influenced by a reduction in growth among smaller banks: independent innovation. Leveraging the strain on the banking system caused by the Great Depression, Babina, Bernstein and Mezzanotti (2020) find that harder hit areas are associated with a reduction in patenting activity associated with individuals rather than firms. If individuals are more likely to have credit extended by local, smaller banks, a reduction in lending activity by these financial intermediaries may impair innovation rates for this group. In light of our previous findings regarding the effect on smaller establishments, it is plausible that the innovation rate among small firms may also be negatively impacted by a reduction in lending by treated banks. To evaluate this possibility, we consider grant-level patent data disseminated by the USPTO.[18] We classify a patent as being independent if either: 1) the patent is not assigned to another entity (e.g., a firm), 2) the patent is assigned, but the assignee has no previous patents, or 3) the name of the assignee contains the inventor's last name.[19] Thus, this classification plausibly

17. Internet Appendix Figure IA.3 breaks out the effect of *BranchShare* by calendar year, showing no noticeable pre-trend. Instead, the effect appears to materialize beginning in March 1997 (recall establishments are measured on March 12th of each year).

18. Specifically, we use the PatentsView dataset provided by the USPTO. For each patent grant, the data includes the name and location of all named inventors, as well as any assignee information. Moreover, the data includes a disambiguated unique identifier for each inventor and assignee.

19. While we consider patents with an application date between 1985 and 2004, our grant data runs back to 1973. The second criteria incorporates this information, taking the value of one if the assignee has no grant activity dating back to 1973. The final criteria is designed to identify instances in which an inventor forms an LLP or another legal entity to house the intellectual property.

captures innovative activity of individuals and small and/or new firms. For convenience, we refer to these groups collectively as "independent patents." Next, we define our measure of independent innovation as the count of independent patent applications at the county-year level, which we lag by one year to reflect a likely delay between an idea's time of inception and development and the date a patent application is submitted.

[Insert Table 11 Near Here]

With this, we estimate empirical models similar to those of Table 10, with one exception. Here, as the outcome is the count of applications for future patent grants for a given county-year, we estimate a Poisson count model.[20] Table 11 presents the results from the count models, where the coefficient on an indicator variable can be interpreted as the difference between the natural log of *expected* patent counts for a one standard deviation increase in the county-level share of treated bank branches. The first pair of specifications present results from models analogous to those estimated in Panel A of Table 10. In the first specification, when defining treatment as those banks with assets between $200M and $250M, the rate of independent patenting fell by 4.1% (t-stat=-1.97) for a one standard deviation increase in the county share of branches belonging to banks falling just below the CRA size threshold. Similar to the impact on small business growth, this effect increases when narrowing the pre-reform asset range to $220M in the second specification. We confirm these findings in the second pair of specifications, which mirror Panel B of Table 10 and restrict the sample to counties serviced by at least one bank with assets less than $350M. Overall, this table suggests that a regulatory-driven slowing of bank growth led to a reduction in innovative activity among independent inventors. More broadly, this result complements the other results in this section, in which the type of local economic growth likely to be more dependent on smaller financial institutions is negatively impacted by the CRA.[21]

20. See Correia, Guimarães and Zylkin (2019) for details. Given the existence of county-years with zero patents, an alternate approach is to instead consider the transformation $ln(1 + x)$. However, as Cohn, Liu and Wardlaw (2021) note, this transformation is not innocuous, where a change in the constant (e.g., .5 vs 1) can yield potentially large changes in inferences.

21. Internet Appendix Figure IA.4 breaks out the effect of *BranchShare* by calendar year. While point

5. Conclusion

The banking industry is one shaped by a collection of regulations, each designed to achieve a specific goal. A prime example is the legislative effort to bring about lending parity embodied in the Community Reinvestment Act. However, perhaps as a partial acknowledgement that the CRA would impose a cost on banks, the implementation of the 1995 reform to the act was designed as an attribute-based, tiered-evaluation scheme. While designed to reduce the potential burden placed on smaller banks, this exception introduced the perverse incentive for a bank to strategically manage growth to avoid a step-up in regulatory costs.

We evaluate the perceived cost of this increase in regulatory oversight by the revealed preferences of banks, finding considerable bunching below the CRA asset size threshold. This action is achieved through the strategic slowing of asset growth rates, which include a slowing of cash and security growth rates but also of loans held on the balance sheet. More importantly, we find evidence of real effects on areas served by banks near the threshold. First, banks falling below the threshold experience an increase in the rejection rate of LMI loans following the 1995 reform. We find no evidence that a competitive lending market ameliorates this effect, finding no change in the lending behavior of non-bunching banks. Second, local areas with pre-reform exposure to banks just below the threshold experience a decline in the share of small businesses and independent innovation. These results are particularly important, as they stand in stark contrast to the CRA's stated objective to "encourage depository institutions to help meet the credit needs of the communities in which they operate." Instead, banks elected to take costly action to avoid the regulatory cost of the CRA, a price that was partially borne by the very borrowers whom the act was designed to benefit.

Finally, given the recent efforts to modernize and expand the CRA to institutions currently not obligated to comply, our results provide evidence that banks attempt to avoid the CRA and the resulting negative consequences which should be taken into consideration.

estimates are somewhat noisy, we again see no noticeable pre-trend prior to the CRA reform.

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Small banks	**Large banks**
Lending test:	*Lending test:*
• Loan-to-deposit ratio.	• Number and dollar amount of home mortgage, small business, and small farm loans.
• Percentage of loans in its community.	• Geographic distribution of loans and number and dollar amount of loans in LMI, and upper income census tracts.
• Record of lending to borrowers at different income levels and farms and businesses of different sizes.	• Loans to borrowers at different income levels, including home mortgage loans, small businesses and small farms with annual revenue less than or equal to $1 million, and small-business and small farm loans by amount at origination.
• Geographic distribution of loans.	• Community development loans, including their innovativeness.
• Responsiveness to complaints.	• Complexity and innovative or flexible credit practices.
	Investment test:
	• Dollar amount of community development investments.
	• Investment innovation and complexity.
	• Investment responsiveness to credit and community development needs.
	• The extent to which they are not provided by other investors.
	Service test:
	• Branch distribution by neighborhood income level.
	• Record of opening and closing branches, particularly in LMI neighborhoods.
	• Alternative means, such as automated teller machines, for providing banking services to low- and moderate-income neighborhoods.
	• Range of services provided in neighborhoods by income level.
	• Community development banking services.

Fig. 1. Tests Faced by each Bank Classification
This figure describes the different tests faced by small banks (assets lower than $250M) and large banks (assets greater than $250M).

Panel A: Q4 Assets from 1996-2004

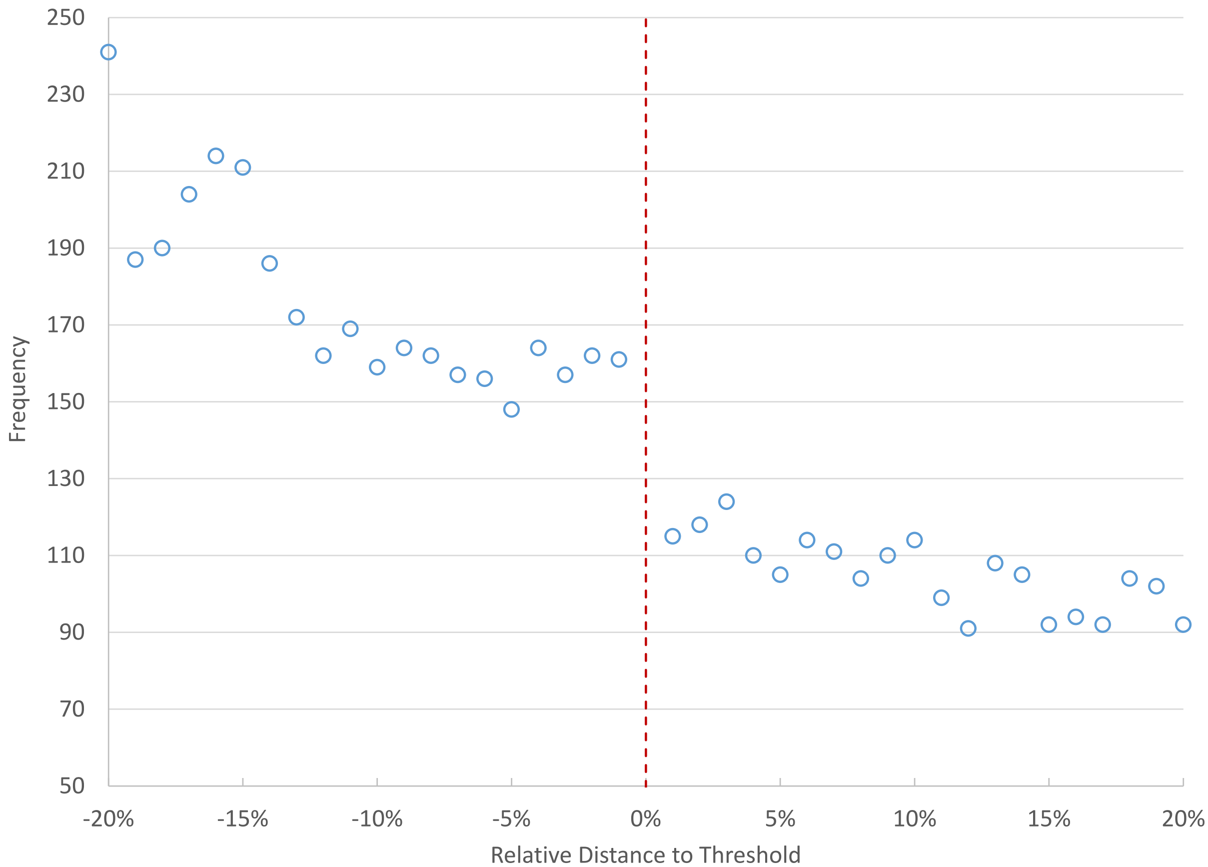
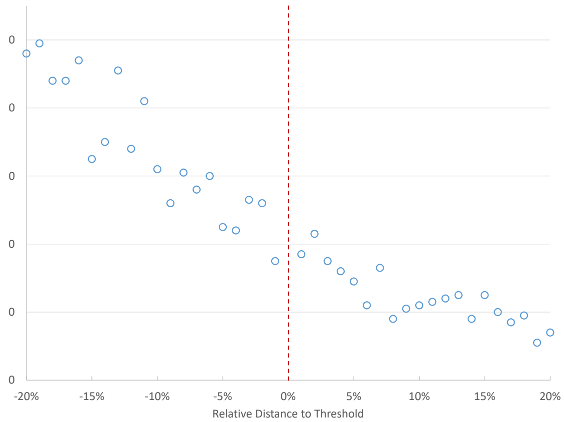
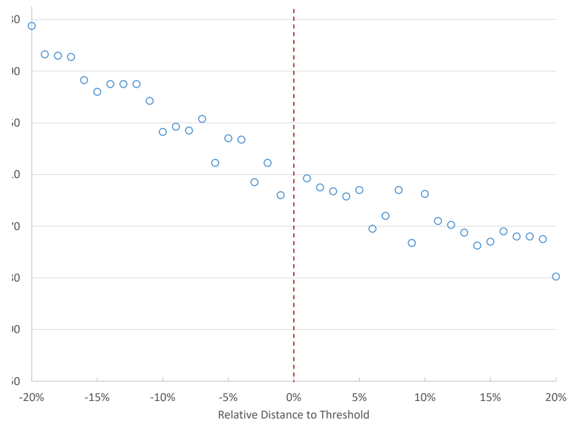


Fig. 2. Bank Size Distribution

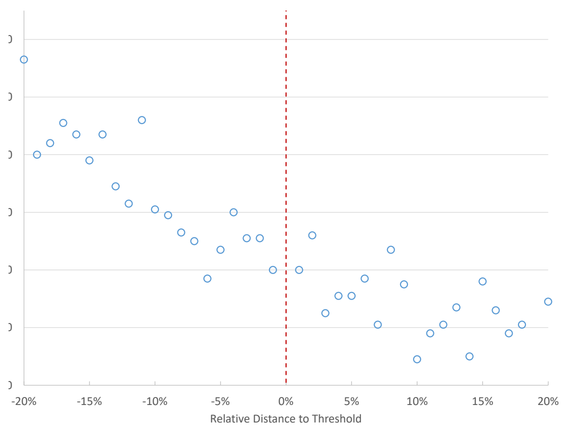
This figure reports histograms of the count of bank-year observations based on year-end reported assets. Panel A reports the number of observations for each \$2.5M wide bin over the period from 1996 to 2004. The remaining panels repeat the procedure in placebo tests. This includes the number of observations over the period from 1986-1993 (Panel B), relative to a \$150M threshold (Panel C), and relative to a \$350M threshold (Panel D).



Panel B: Q4 Assets from 1986-1993



Panel C: Threshold = $150M



Panel D: Threshold = $350M

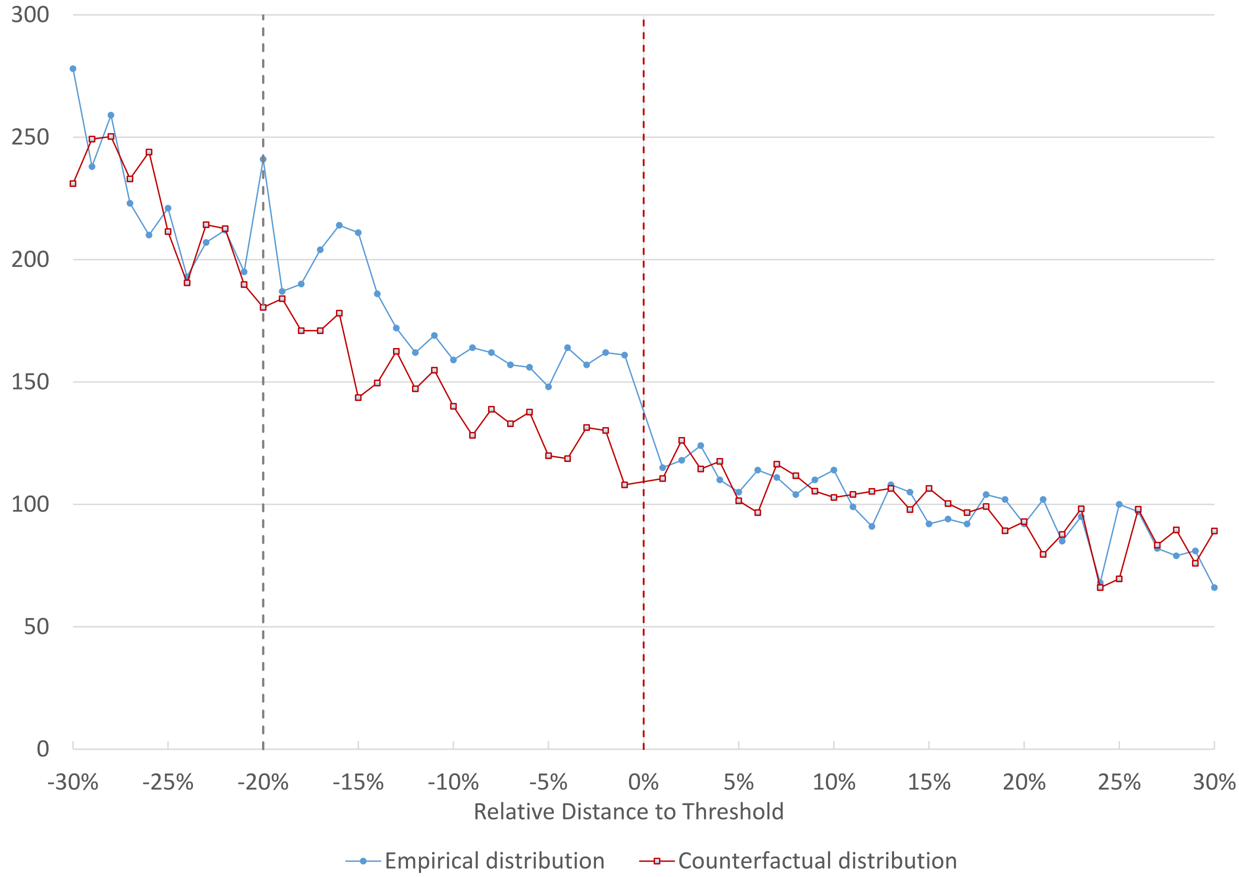


Fig. 3. Comparison of the Empirical and Counterfactual Bank-Asset Distribution

This figure plots the empirical (solid blue circles) and counterfactual distribution (hollow red squares) over the period from 1996 to 2004. The counterfactual distribution was generated as described in Section 3.1 using 1986-1994 as the pre-period. The vertically dashed grey line marks the lower bound of the area affected by the threshold. Each dot represents the count of bank-year observations based on year-end reported assets

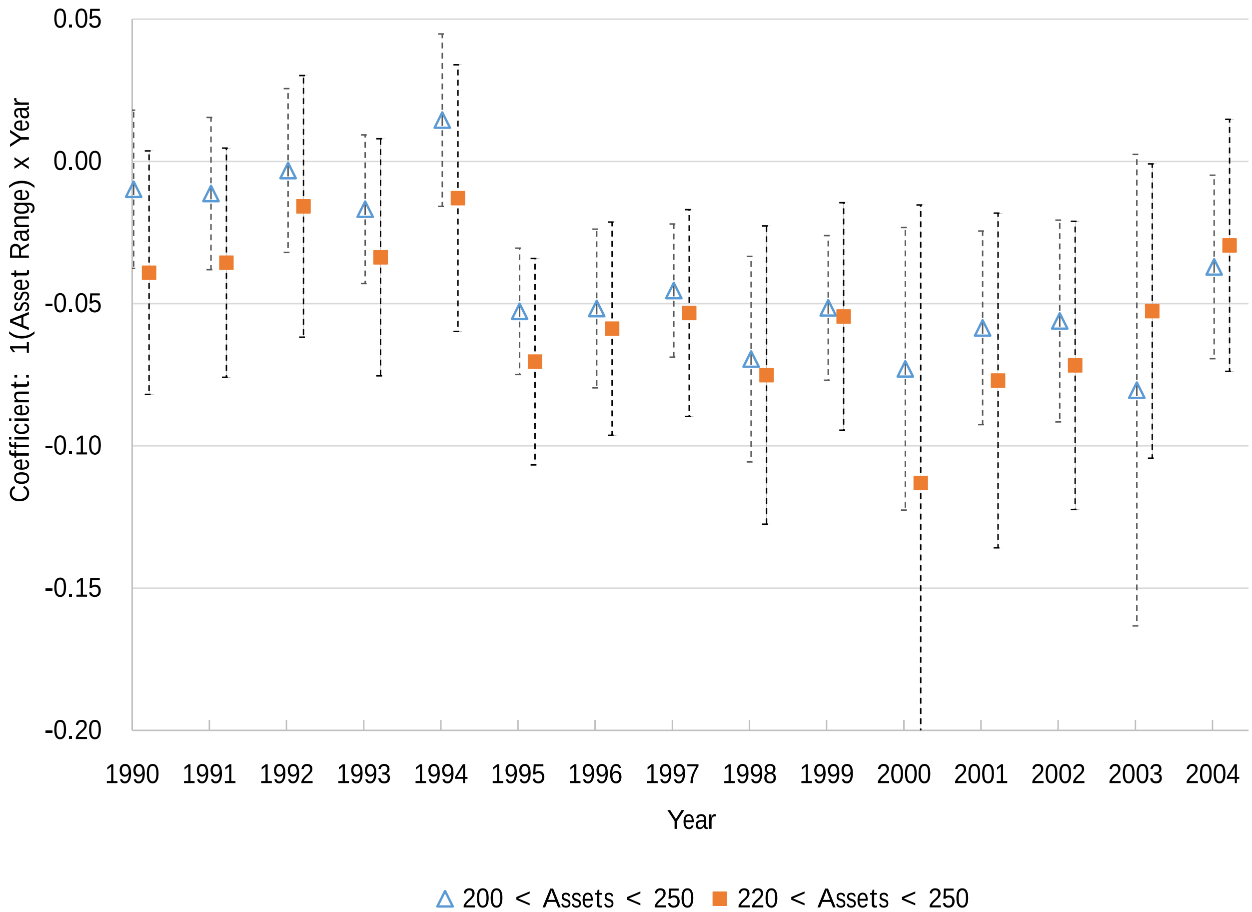


Fig. 4. Effect of CRA Threshold on Asset Growth

This figure presents OLS point estimates from a modified version of Equation (2). Here, we replace $1(yr > 1995)$ with a vector of indicator variables corresponding to each calendar year from 1990 to 2004. As such, the base case is 1989. Following this change, we re-estimate analogous regressions to Column 3 (hollow triangles) and Column 6 (solid squares) of Table 3. Reported are 95% confidence intervals, where standard errors are heteroscedasticity-robust and clustered by bank.

Table 1.
Summary statistics

	N	Mean	SD	p25	Median	p75
Panel A: Bank Characteristics						
Assets ($M)	151,869	534.40	8101.42	32.63	65.12	141.20
Loans ($M)	151,868	318.90	4427.91	16.67	36.42	85.58
Cash ($M)	151,868	35.97	551.20	1.62	3.21	7.01
Asset Growth	151,869	0.06	0.19	-0.02	0.03	0.09
Loan Growth	151,867	0.07	0.30	-0.02	0.04	0.12
Cash Growth	151,867	0.01	0.42	-0.21	0.00	0.22
Equity (%)	151,869	9.96	5.63	7.62	8.94	11.00
Panel B: County & HMDA Outcomes						
Branch Share [$200M,$250M] (%)	43,481	2.80	9.39	0	0	0
Branch Share [$220M,$250M] (%)	43,481	1.56	7.33	0	0	0
1(200 $<$ Assets $<$ 250) (%)	43,481	15.70	36.40	0	0	0
1(220 $<$ Assets $<$ 250) (%)	43,481	9.25	29.00	0	0	0
Establishments w. $<$ 20 employees (%)	43,481	89.71	3.53	87.33	89.60	91.96
Establishments w. $<$ 50 employees (%)	43,481	96.30	1.70	95.18	96.31	97.45
Independent Patent Count	54,789	2.12	9.92	0	0	1
Mortgage Approval Rate (%)	1,471,869	87.40	33.20	100	100	100
1(LMI) (%)	1,339,272	39.10	48.80	0	0	100
Mortgage Loan Amount ($k)	1,471,869	89.85	212.10	40	71	115

This table describes the final sample. *Bank Characteristics* are collected from year-end Call Reports over the period from 1989 to 2004. *County & HMDA Outcomes* are sourced from the County Business Patterns series (1991-2005), USPTO patent grant XML files (1985-2004), and HMDA disclosures (1990-2004). *LMI* denotes a mortgage application from either 1) a low- or moderate-income qualifying census tract or 2) a reported income that is LMI-eligible.

Table 2.
Excess Mass Estimates

	(1)	(2)	(3)	(4)
$Bunching\,Assets_{200-250}$	598***		410***	
	(4.17)		(7.63)	
$Bunching\,Assets_{220-250}$		333***		201***
		(3.41)		(4.47)
Counterfactual	Pre-period	Pre-period	Poly	Poly
Bootstrap replications	1,000	1,000	1,000	1,000
Bin-width	$2.5M	$2.5M	$2.5M	$2.5M

This table shows the excess mass estimates of the effect of the discrete change in regulatory requirements tied to the $250M threshold. $Bunching\,Assets_{200-250}$ corresponds to the excess count of bank-year observations based on year-end reported assets, between the interval $200M and $250M during the 1996-2004 period, calculated using the procedure described in Section 3.1. $Bunching\,Assets_{220-250}$ is similarly constructed using the interval bounded by $220M and $250M. Columns 1 and 2 report the excess mass when the counterfactual distribution comprises the normalized bank asset distribution for the period 1986-1994 ($pre-period$). In Columns 3 and 4, the counterfactual distribution is constructed by fitting a 6th degree polynomial (*poly*). Reported t-statistics in parentheses correspond to the standard errors calculated by bootstrapping from the observed sample of banks, drawing 1,000 random samples with replacements and re-estimating the parameters at each iteration. ***p<0.01, **p<0.05, *p<0.1.

Table 3.
Effect of CRA Threshold on Asset Growth

	(1)	(2)	(3)	(4)	(5)	(6)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	-0.024***	-0.037***	-0.044***			
	(-3.73)	(-5.41)	(-5.76)			
$Assets_{220-250} \times 1(\text{yr} > 1995)$				-0.012	-0.025***	-0.035***
				(-1.55)	(-2.85)	(-3.37)
Sample	Full	< \$500M	< \$350M	Full	< \$500M	< \$350M
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	137,051	127,192	123,148	137,051	127,192	123,148
R-squared	0.180	0.200	0.216	0.180	0.200	0.216

This table shows OLS regressions where the dependent variable is the yearly log change in asset values. $Assets_{200-250}$ is an indicator variable which takes on the value of one if a bank's reported assets in 1994 are between \$200M and \$250M. $Assets_{220-250}$ is similarly constructed using the interval bounded by \$220M and \$250M. *Sample* denotes the sample selection criteria in which we exclude banks with assets greater than the corresponding value. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table 4.
Growth in Components of Total Assets

Panel A: Treated = [$200M, $250M]

Growth:	Cash	Securities	Loans	R.E. Loans	C&I Loans
	(1)	(2)	(3)	(4)	(5)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	-0.066***	-0.052***	-0.052***	-0.050***	-0.049***
	(-4.82)	(-3.44)	(-3.36)	(-3.24)	(-2.63)
Bank FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Number of observations	123,146	123,148	123,146	123,146	123,148
R-squared	0.072	0.104	0.174	0.155	0.092

Panel B: Treated = [$220M, $250M]

Growth:	Cash	Securities	Loans	R.E. Loans	C&I Loans
	(1)	(2)	(3)	(4)	(5)
$Assets_{220-250} \times 1(\text{yr} > 1995)$	-0.088***	-0.060**	-0.042**	-0.025	-0.044
	(-4.07)	(-2.36)	(-1.98)	(-1.44)	(-1.56)
Bank FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Number of observations	123,146	123,148	123,146	123,146	123,148
R-squared	0.072	0.104	0.174	0.155	0.092

This table shows OLS regressions where the dependent variable varies across specifications, reported in the column heading. *Cash* is the total amount of cash held on the balance sheet. *Securities* corresponds to the value of marketable securities held on the balance sheet, while *Loans* represents total loans held at year-end. Similarly *R.E. Loans* and *C&I Loans* correspond to real estate and commercial loans, respectively. All dependent variables are constructed as the yearly log change in the value. Panel A reports the results where the variable of interest is the interaction of $Assets_{200-250}$ and $1(yr > 1995)$. Panel B instead considers banks with 1994-measured assets between $220M and $250M. The sample is restricted to banks with assets less than $350M. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table 5.
Bank Profitability and Loan Performance

	Profitability		Non-Performance	
	(1)	(2)	(3)	(4)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	0.027***		-0.001*	
	(3.79)		(-1.88)	
$Assets_{220-250} \times 1(\text{yr} > 1995)$		0.032***		-0.003***
		(3.17)		(-2.72)
Bank	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Number of observations	123,420	123,420	123,420	123,420
R-squared	0.758	0.758	0.420	0.420

This table shows OLS regressions where the dependent variable varies across specifications, reported in the column heading. *Profitability* is the ratio of net interest income to year-end loan value. *Non-Performance* is the ratio of the sum of *Total Loans and Lease Finance Receivables, Nonaccrual* and *Total Loans and Lease Finance Receivables, Past Due 90 Days and More and Still Accruing* to year-end loan value. Each dependent variable is winsorized at the 1% level. The sample is restricted to banks with assets less than $350M. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table 6.
Effects on Equity Financing and Payout Policy

	Pct. Equity		Div. Payout	
	(1)	(2)	(3)	(4)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	0.009***		0.043**	
	(3.37)		(2.54)	
$Assets_{220-250} \times 1(\text{yr} > 1995)$		0.009**		0.013
		(2.44)		(1.28)
Bank	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Number of observations	123,724	123,724	123,722	123,722
R-squared	0.739	0.739	0.202	0.202

This table shows OLS regressions where the dependent variable varies across specifications, reported in the column heading. *Pct. Equity* is the ratio of equity (common plus preferred) to total assets. *Div. Payout* is the cumulative amount of dividends issued over the year divided by year-end equity. The sample is restricted to banks with assets less than $350M. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table 7.
Heterogenous Effects across Past Growth Prospects

Prev. Bank Growth:	Asset Growth		Loan Growth	
	(1)	(2)	(3)	(4)
1(Below Med. Growth) × 1(yr > 1995)	0.014***	0.014***	0.013***	0.013***
	(7.64)	(7.81)	(6.98)	(7.22)
$Assets_{200-250}$ × 1(yr > 1995)	-0.028***		-0.031***	
	(-3.74)		(-4.05)	
× 1(Below Med. Growth)	-0.019*		-0.013	
	(-1.87)		(-1.26)	
$Assets_{220-250}$ × 1(yr > 1995)		-0.019**		-0.019**
		(-2.42)		(-2.38)
× 1(Below Med. Growth)		-0.024*		-0.024*
		(-1.70)		(-1.79)
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of observations	118,130	118,130	118,130	118,130
R-squared	0.191	0.191	0.191	0.191

This table shows OLS regressions where the dependent variable is the yearly log change in asset values. The table augments the regressions reported in Table 3 with the inclusion of interaction terms involving $1(BelowMed.Growth)$. $1(BelowMed.Growth)$ is an indicator variable which takes on the value of one if the median value for our proxy of a bank's investment prospects is less than the median value across all banks over the period from 1986-1989. We consider two proxies of investment prospects: growth in loans held on the balance sheet (Loan Growth) and growth in total assets (Asset Growth). The sample is restricted to banks with assets less than $350M. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table 8.
Effects on the Distribution of Residential Mortgage Credit Supplied

	(1)	(2)	(3)	(4)	(5)	(6)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	-0.001	0.012**	0.012**			
	(-0.24)	(2.53)	(2.46)			
$\times$ 1(LMI)	-0.022***	-0.019***	-0.018***			
	(-3.15)	(-2.90)	(-2.77)			
$Assets_{220-250} \times 1(\text{yr} > 1995)$				-0.008	0.006	0.005
				(-1.29)	(0.73)	(0.66)
$\times$ 1(LMI)				-0.022**	-0.014*	-0.013
				(-2.51)	(-1.69)	(-1.61)
Bank-LMI FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-LMI FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	$\times$ Year	$\times$ Year	Yes	$\times$ Year	$\times$ Year
Loan Amt-Year FE	No	No	Yes	No	No	Yes
Number of observations	1,233,816	1,231,151	1,230,582	1,233,816	1,231,151	1,230,582
R-squared	0.097	0.121	0.125	0.097	0.121	0.125

This table shows OLS regressions where the dependent variable is an indicator variable which takes on the value of one if a loan application is accepted and zero if the application is rejected. The sample includes all applications for new loan originations reported in HMDA for banks with total assets less than $350M. $1(LMI)$ is an indicator variable which takes on the value of one if an application qualifies as an low-or-middle-income loan. This includes applications in census tracts with a median income less than 80% of the MSA median or loans in other census tracts with a reported income less than 80% of the MSA median. The $CountyFE$ row denotes fixed effects that vary at either the county level (Yes) or county-year level $\times Year$. $LoanAmt - Year$ is a fixed effect made up of the interaction of yearly indicator variables and a vector of indicator variables partitioning loan amounts into bins of $5k. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table 9.
Potential Response by Other Banks

Panel A: All Originated Loans

	(1)	(2)	(3)	(4)
$TractShare_{200-250} \times 1(\text{yr} > 1995)$	-0.004		0.001	
	(-1.33)		(0.35)	
$TractShare_{220-250} \times 1(\text{yr} > 1995)$		0.002		0.002
		(0.50)		(0.58)
Tract FE	Yes	Yes	× Bank	× Bank
Bank-Year FE	Yes	Yes	Yes	Yes
Number of observations	11,357,130	11,357,130	8,574,287	8,574,287
R-squared	0.436	0.436	0.734	0.734

Panel B: LMI-Qualifying Originated Loans

	(1)	(2)	(3)	(4)
$TractShare_{200-250} \times 1(\text{yr} > 1995)$	-0.000		0.005*	
	(-0.01)		(1.85)	
$TractShare_{220-250} \times 1(\text{yr} > 1995)$		-0.001		0.002
		(-0.27)		(0.63)
Tract FE	Yes	Yes	× Bank	× Bank
Bank-Year FE	Yes	Yes	Yes	Yes
Number of observations	5,209,807	5,209,807	3,604,328	3,604,328
R-squared	0.388	0.388	0.671	0.671

This table shows OLS regressions where the dependent variable is the natural log of the total dollar amount of loans originated, which varies at the bank-year-census tract level. Panel A considers the total dollars lent to all new originations, while Panel B only considers dollars lent to LMI-qualifying loans. The sample includes all new loan originations from non-bunching banks (1994-measured assets outside the range [$200M, $250M]) in tracts with at least one bank with total assets less than $350M prior to 1995. $TractShare_{LB-250}$ denotes the dollar share of loans originated in a census tract prior to the CRA reform by banks with 1994-measured total assets between LB and $250M. The variable is winsorized at the 1% level and standardized to have variance of one. The $TractFE$ row denotes fixed effects that vary at either the census tract level (Yes) or tract-bank level $\times Bank$. $Bank - Year$ is a fixed effect made up of the interaction of yearly indicator variables and bank indicator variables. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by county. ***p<0.01, **p<0.05, *p<0.1.

Table 10.
Effects on Small Business Prevalence

Panel A: All Counties (Full Sample)

Share:	< 20 employees		< 50 employees	
	(1)	(2)	(3)	(4)
$BranchShare_{200-250} \times 1(\text{yr} > 1995)$	-0.057***		-0.009	
	(-2.73)		(-0.88)	
$BranchShare_{220-250} \times 1(\text{yr} > 1995)$		-0.068***		-0.016*
		(-3.32)		(-1.65)
County FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Number of observations	43,480	43,480	43,480	43,480
R-squared	0.917	0.917	0.891	0.891

Panel B: Require 1+ Banks with Assets < \$350M

Share:	< 20 employees		< 50 employees	
	(1)	(2)	(3)	(4)
$BranchShare_{200-250} \times 1(\text{yr} > 1995)$	-0.059***		-0.011	
	(-2.91)		(-1.03)	
$BranchShare_{220-250} \times 1(\text{yr} > 1995)$		-0.064***		-0.016
		(-3.27)		(-1.64)
County FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Number of observations	40,980	40,980	40,980	40,980
R-squared	0.919	0.919	0.893	0.893

This table shows OLS regressions where the dependent variable is the share of establishments with fewer than 20 employees ($< 20employees$) or 50 employees ($< 50employees$), measured at the county-year level. Panel A considers all counties, while Panel B restricts the analysis to counties with at least one bank with 1994-measured assets less than \$350M. $BranchShare_{200-250}$ is the county-level share of branches associated with banks with 1994-measured assets between \$200M and \$250M. $BranchShare_{220-250}$ is similarly constructed using the interval bounded by \$220M and \$250M. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by county. ***p<0.01, **p<0.05, *p<0.1.

Table 11.
Effects on Independent Innovation

Sample:	All Counties		Has < \$350M	
	(1)	(2)	(3)	(4)
$BranchShare_{200-250} \times 1(\text{yr} > 1995)$	-0.041**		-0.042**	
	(-1.97)		(-2.02)	
$BranchShare_{220-250} \times 1(\text{yr} > 1995)$		-0.044***		-0.046***
		(-3.15)		(-3.23)
County FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Number of observations	51,611	51,611	48,495	48,495
R-squared	-	-	-	-

This table shows results from a Poisson count model where the dependent variable is the number of patent applications not assigned to an organization, measured at the county-year level. Application dates are lagged one year. Columns 1 and 2 consider the full sample of all counties, while Columns 3 and 4 restrict the analysis to counties with at least one bank with 1994-measured assets less than \$350M. All remaining variables are described in Table 10. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by county. ***p<0.01, **p<0.05, *p<0.1.

Internet Appendix For "Strategically Staying Small: Regulatory Avoidance and the CRA"

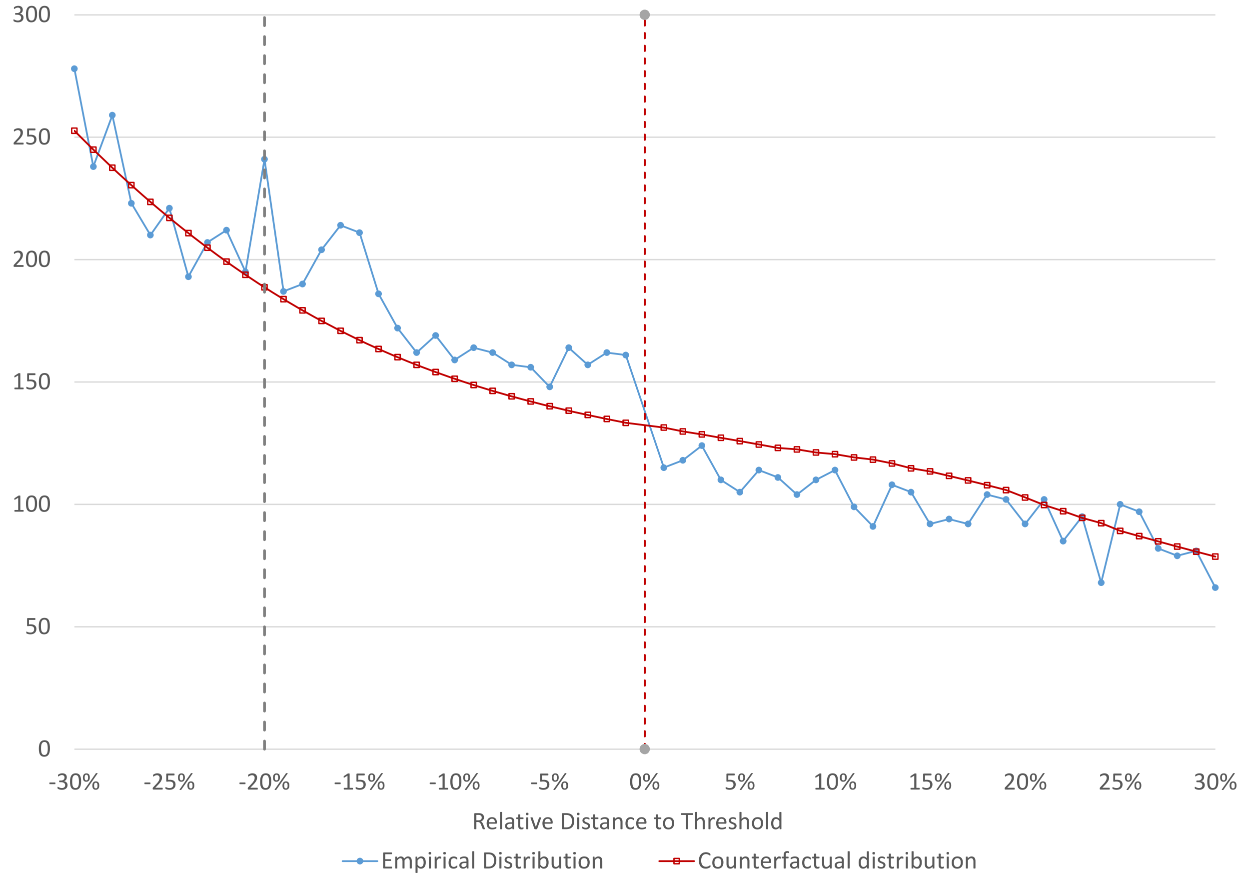


Figure IA.1. Comparison of the Empirical and Counterfactual Bank-Asset Distribution

This figure plots the empirical (solid blue circles) and counterfactual distribution (hollow red squares) over the period from 1996 to 2004. The counterfactual distribution was generated as described in Section 3.1 fitting a 6th degree polynomial to the bin counts, omitting the contribution of the bins in the region marked by the vertical dashed gray line. Estimation was carried out in the sample of banks with asset size between \$150M and \$500, but the figure shows only loans within 30% of the CRA asset threshold. The vertically dashed grey line marks the lower bound of the area affected by the threshold. Each dot represents the count of bank-year observations based on year-end reported assets.

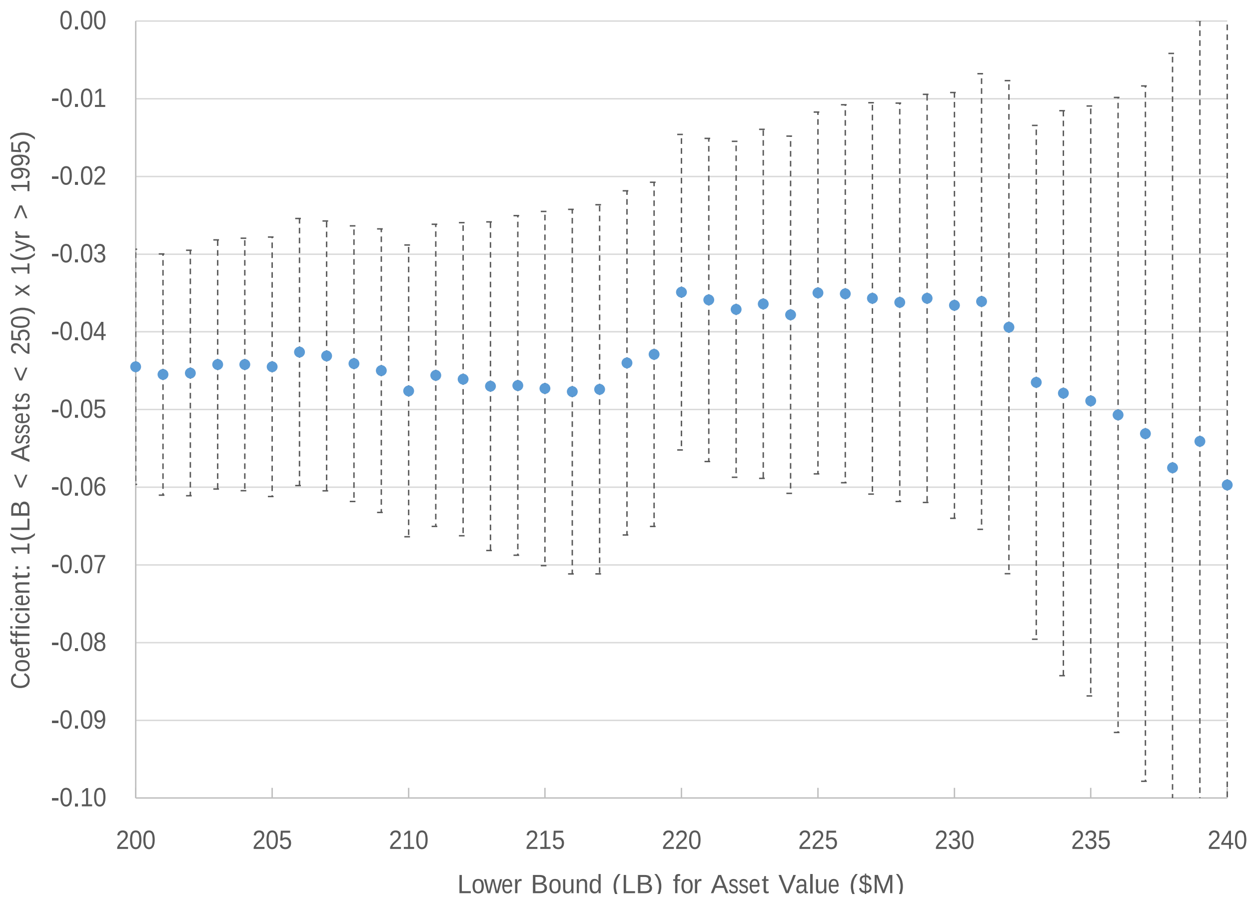


Figure IA.2.
Robustness of Main Effect to Various Definitions of Treated Banks
This figure reports the point estimate and corresponding 95% confidence interval for variations of the final specification of Table 3. Specifically, we vary the lower bound used to define a treated bank (e.g., \$200M) and report the interaction of the resulting interaction and $1(yr > 1995)$. Standard errors are heteroscedasticity-robust and clustered by bank.

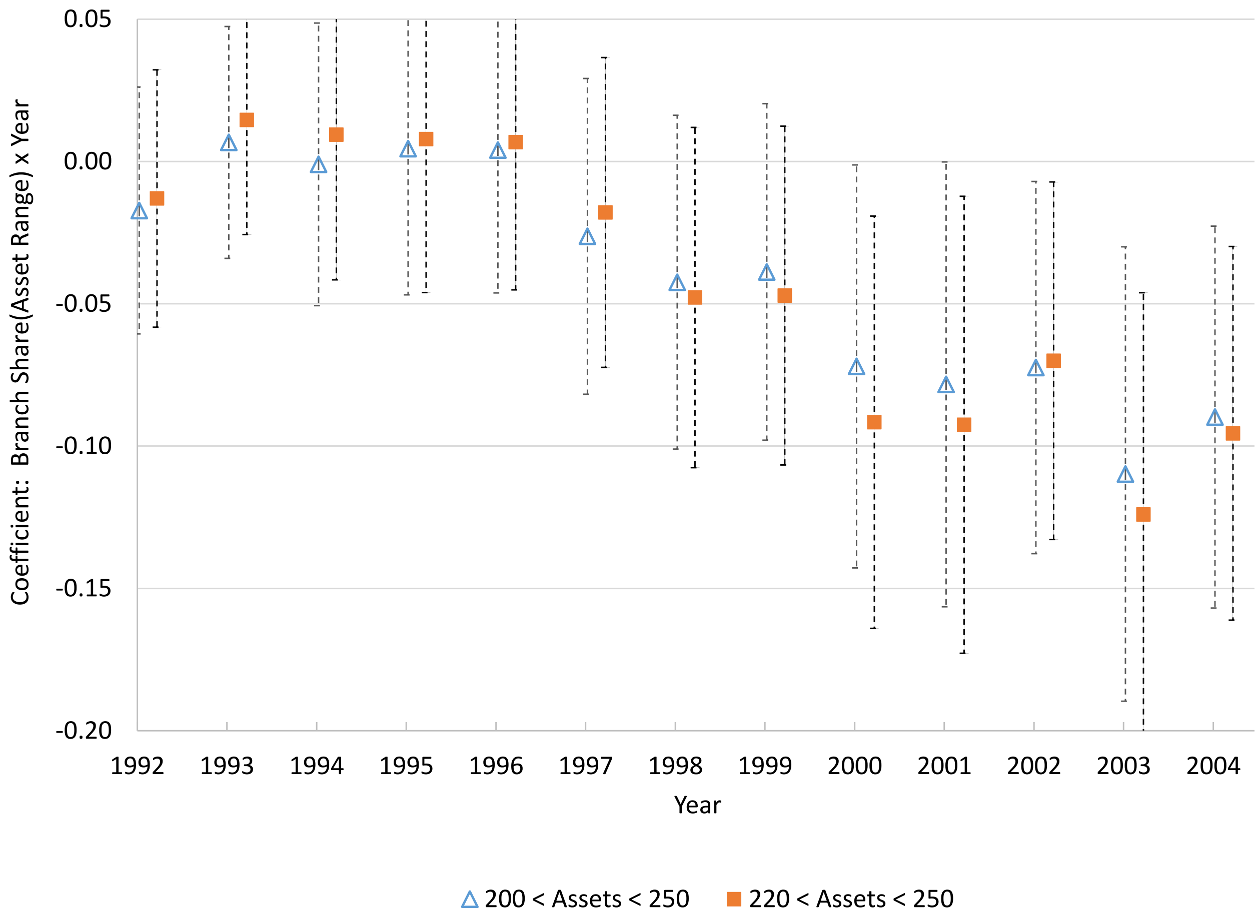


Figure IA.3.
Effect of CRA Threshold on Small Businesses
This figure presents OLS point estimates from a modified version of Equation (2). Here, we replace $1(yr > 1995)$ with a vector of indicator variables corresponding to each calendar year from 1992 to 2004. As such, the base case is 1991. Following this change, we re-estimate analogous regressions to Column 1 (hollow triangles) and Column 2 (solid squares) of Table 10. Reported are 95% confidence intervals, where standard errors are heteroscedasticity-robust and clustered by county.

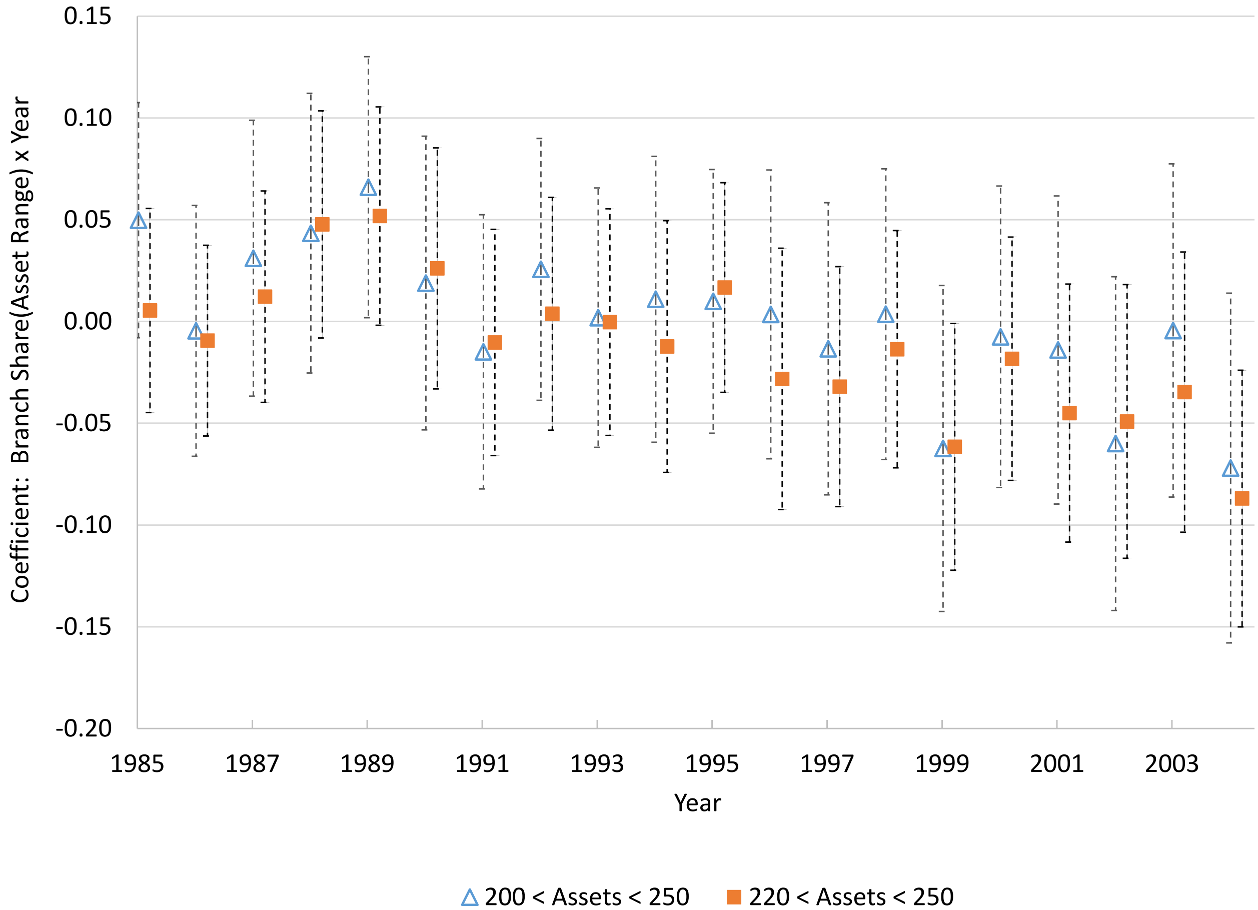


Figure IA.4.
Effect of CRA Threshold on Independent Innovation
This figure presents OLS point estimates from a modified version of Equation (2). Here, we replace $1(yr > 1995)$ with a vector of indicator variables corresponding to each calendar year from 1992 to 2004. As such, the base case is 1991. Following this change, we re-estimate analogous regressions to Column 1 (hollow triangles) and Column 2 (solid squares) of Table 11. Reported are 95% confidence intervals, where standard errors are heteroscedasticity-robust and clustered by county.

Table IA.1.
Excess Mass Estimates using Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
$Bunching\ Assets_{200-250}$	322***	531***	393***	345***	386***
	(6.18)	(7.21)	(6.64)	(6.37)	(6.82)
Degree polynomial	5th	6th	7th	5th	7th
Bootstrap replications	1,000	1,000	1,000	1,000	1,000
Bin width	\$1.0M	\$1.0M	\$1.0M	\$2.5M	\$2.5M

This table shows the excess mass estimates of the effect of the discrete change in regulatory requirements tied to the \$250M threshold using alternative specifications. The table differs from Table 2 in that the counterfactual distribution is constructed by fitting polynomials of different degrees and using alternative bin-widths. Reported t-statistics in parentheses correspond to the standard errors calculated by bootstrapping from the observed sample of banks, drawing 1,000 random samples with replacements and re-estimating the parameters at each iteration. ***p<0.01, **p<0.05, *p<0.1.

Table IA.2.
Main Effect when using 1993-Measured Assets

	(1)	(2)	(3)	(4)	(5)	(6)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	-0.019***	-0.023***	-0.037***			
	(-3.15)	(-4.01)	(-5.23)			
$Assets_{220-250} \times 1(\text{yr} > 1995)$				-0.018**	-0.025***	-0.045***
				(-2.14)	(-3.07)	(-4.13)
Sample	Full	< \$500M	< \$350M	Full	< \$500M	< \$350M
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	137,051	127,192	123,148	137,051	127,192	123,148
R-squared	0.180	0.200	0.216	0.180	0.200	0.216

This table shows OLS regressions where the dependent variable is the yearly log change in asset values. The table differs from Table 3 in that $Assets_{200-250}$ and $Assets_{220-250}$ are calculated using assets measured as of 1993 (rather than 1994). All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.

Table IA.3.
Main Effect when using Contemporaneously Measured Assets

	(1)	(2)	(3)	(4)	(5)	(6)
$Assets_{200-250} \times 1(\text{yr} > 1995)$	-0.026***	-0.036***	-0.043***			
	(-4.33)	(-6.09)	(-7.10)			
$Assets_{220-250} \times 1(\text{yr} > 1995)$				-0.018**	-0.027***	-0.034***
				(-2.46)	(-3.67)	(-4.44)
Sample	Full	$<$ \$500M	$<$ \$350M	Full	$<$ \$500M	$<$ \$350M
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	137,051	127,192	123,148	137,051	127,192	123,148
R-squared	0.180	0.200	0.216	0.180	0.200	0.216

This table shows OLS regressions where the dependent variable is the yearly log change in asset values. The table differs from Table 3 in that $Assets_{200-250}$ and $Assets_{220-250}$ are calculated using contemporaneously measured assets, rather than those measured in 1994. All remaining variables are described in Table 3. Reported t-statistics in parentheses are heteroscedasticity-robust and clustered by bank. ***p<0.01, **p<0.05, *p<0.1.