The Efficacy and Efficiency of Credit Market Interventions: Evidence from the Community Reinvestment Act

(Job Market Paper)

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ABSTRACT

Economies around the world are marked by major interventions in credit markets. Institutions ranging from central banks to the Grameen Bank operate under the assumptions that credit markets are imperfect, that these imperfections can be ameliorated, and that doing so increases output. There is surprisingly little empirical support for these propositions. This paper develops evidence on related questions by exploiting changes to a major intervention in U.S. credit markets, the Community Reinvestment Act (CRA). Using data on both banks and potential commercial borrowers, I find evidence that CRA does increase credit to small businesses as intended. I then exploit these CRA-induced supply shocks to identify the impact of credit increases on county-level payroll and bankruptcies. There is some evidence of real benefits at plausible implied rates of return on CRA borrowing, and little suggestion of crowd-out or adverse effects on bank performance. The findings therefore appear consistent with a model where targeted credit market interventions can improve efficiency. Ongoing work seeks to identify whether CRA does in fact ameliorate any particular type of credit market failure.

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I. Introduction

Economies around the world are characterized by major interventions in credit markets. Institutions ranging from central banks to the Grameen Bank operate under the assumptions that credit markets are imperfect, that these imperfections can be ameliorated, and that doing so increases output. There is surprisingly little empirical support for these propositions.

The existence of important credit market failures is uncertain. A substantial body of work on investment-cash flow sensitivity concludes that many firms are liquidity constrained (Hubbard, 1998; Fazzari, et. al. 2000).[1] Yet whether the observed liquidity sensitivity actually implies *financing* (e.g., credit) constraints has been questioned on both theoretical and empirical grounds (Kaplan and Zingales, 1997 and 2000).[2] More direct tests of theoretical models of credit constraints (e.g., Stiglitz and Weiss 1981, Hart and Moore 1994) are rare, and they have produced little evidence of empirically important imperfections (e.g., Berger and Udell, 1992).[3]

Finding "real" (as opposed to merely "financial") effects of finance might offer indirect evidence of underlying market failures and motivate interventions.[4] But there is little to suggest that increasing credit (as many interventions seek to do) would increase output in steady-state; on the contrary, the finance literature suggests that banks may be the second-best solution to credit

[1] Holtz-Eakin, Joulfaian, and Rosen (1994) and Blanchflower and Oswald (1998) find that self-employment exit and entry are sensitive to liquidity shocks (specifically inheritances).

[2] Indeed, Lamont (1997) finds investment responses to a plausibly exogenous oil shock in very large companies with access to public markets, suggesting that liquidity sensitivity may not be driven by financial constraints.

[3] Petersen and Rajan (1995) is a notable exception-- it develops and finds empirical support for a model where limited contracting and asymmetric information combine to create credit constraints. Evans and Jovanovic (1989) find evidence of entrepreneurial behavior consistent with a reduced-form model of credit constraints. Evidence of differential access to credit by race (Blanchflower et. al. 1998; Cavaluzzo et. al. 2001; Munnell, et. al. 1996) is suggestive but has not been linked to any specific underlying market failure. Tootell (1996) finds little evidence of discrimination by geography (a.k.a. "redlining").

[4] The distinction between real and financial decisions can be traced to Modigliani and Miller (1958). Interest in links between financial markets and the macroeconomy dates at least to Schumpeter (1911) and Robinson (1952).

market frictions.[5] A growing body of evidence does suggest that aggregate output increases with the quality of financial intermediation (Jayaratne and Strahan 1996; Rajan and Zingales 1998), but little is known about the effects of changes to credit supply; e.g., the existence of a bank lending channel for monetary policy remains relatively controversial.[6,7]

Even presuming that policy *should* target credit markets, knowledge of what it *can* accomplish is modest. There is little evidence that instruments other than blunt mandates (e.g., Banerjee and Duflo, 2001) or costly subsidies (e.g., Gale, 1991) can alter capital allocation. The key players in capital markets are sophisticated, and might engage in gaming or offsetting behavior when presented with even carefully constructed incentives to alter their investment decisions. Policies that rely on regulator discretion to assess efficiency may be undermined by agency problems. Interventions that target certain institutions (e.g., banks) may merely change the composition of finance rather than net access to capital.

This paper exploits changes in a major intervention in U.S. credit markets, the Community Reinvestment Act (CRA), to identify evidence on the related questions of whether regulation can allocate credit, whether regulation should allocate credit, and whether credit has an independent effect on real activity. Using data on both banks and potential commercial borrowers, I find evidence that CRA does increase lending to small businesses as intended. I then exploit these

[5] Black (1975) and Fama (1985) posit that banks possess a comparative advantage in lending to small businesses thanks to the private information provided by deposit accounts. Petersen and Rajan (1994) and Mester, et. al. (2001) find empirical evidence in support of this type of model.

[6] Peek and Rosengren's (2000) work on the international transmission of Japanese banking sector shocks to U.S. borrowers, and the "credit crunch" literature (e.g., Sharpe 1995, Bernanke and Lown 1991), find some evidence that regulation-induced *negative* supply shocks to bank lending *decrease* output. However, forcing banks to *increase* lending will not *increase* output if banks produce a constrained Pareto optimum when left to their own devices (see footnote 5).

[7] In contrast, the existence of a balance sheet (demand-side) channel is relatively well-established (Bernanke and Gertler 1995; Hubbard 1994). My paper provides evidence on whether two of the three necessary conditions for a bank lending channel hold: the imperfect substitutability of bank and nonbank finance (i.e., whether policy-induced changes in bank lending produce equilibrium changes in net access to financing), and finance impacting output (i.e., whether equilibrium changes in credit markets produce real effects). Of course, this paper does not speak to the question of whether monetary policy actually induces changes in bank credit supply. Ashcraft (2001) describes the challenges facing identification of a lending channel and develops and implements a new empirical approach.

CRA-induced supply shocks to identify the impact of credit flows on county-level real activity. There is some evidence of real benefits at plausible rates of return, and little suggestion of crowd-out or adverse effects on bank performance. The findings therefore appear consistent with a model where targeted credit market interventions can improve efficiency. Ongoing work seeks to identify whether CRA does in fact ameliorate any particular credit market failure(s).

CRA is a reasonable place to look for identifiable supply shocks to lending because its effects are plausibly large and its incentives have varied idiosyncratically across banks, space, and time. CRA provides banks with incentives for lending to small businesses and in low-income areas generally. As detailed in Section II, I exploit the fact that the bite of these incentives changed dramatically but somewhat haphazardly due to regulatory reforms-- certain banks faced newly binding CRA incentives to increase lending beginning in 1996, while otherwise similar banks experienced no change in CRA incentives. Equally importantly, CRA has potentially large but poorly understood effects on credit markets. Over $400 billion in business lending qualified as CRA loans in 1998, but the existing literature provides little guidance on whether CRA has any causal effects (Gramlich, 1999; Litan, et. al., 2000). Nevertheless informal estimates of CRA's impacts often start in the billions of dollars, as in conjectures by economists Edward Gramlich (1999) and Lawrence Lindsey (1995) and by CRA expert Kenneth Thomas (1998).

The rest of the paper proceeds as follows. The next section details the CRA institutions and enforcement practices that create incentives for banks to increase certain types of lending. Section III develops and estimates models of CRA's effects on potential borrowers. The results suggest that CRA increases access to credit for approximately five percent of firms. Section IV provides some confirmation that CRA accomplishes this by inducing small business lending

increases of perhaps twelve to fifteen percent by affected banks. Non-CRA lending does not appear to increase, and there is little evidence of adverse effects on bank profits or loan quality. Section V finds some evidence that CRA-induced credit increases produce real benefits at the county-level, with payroll increasing by perhaps one percent (this estimate is relatively imprecise) and bankruptcies decreasing by four to five percent in counties where banks faced newly binding CRA incentives. These findings do not appear to be driven by redistribution across counties. An estimate of the gross rate of return on marginal borrowing implied by CRA's effects on borrowing, lending, and payroll is shown to range from 20 to 58 percent. Returns in this neighborhood would be plausible, in light of the loan prices faced by CRA borrowers (almost certainly less than 20 percent) and the potentially high price of pre-existing outside options (possibly 70 percent or greater), if CRA actually mitigates an underlying credit market failure. Section VI concludes with a brief discussion of models where CRA could improve efficiency through either a blunt or surgical intervention, ongoing research that seeks to identify whether any of these models help explain CRA's reduced-form impacts, and related avenues for future work that bears on the welfare implications of credit market interventions.

II. How CRA Works

In this section I detail how CRA works. This institutional detail will help establish that CRA could induce changes in bank lending and motivate empirical strategies for estimating its effects.

A. History and Overview

CRA was enacted by the United States Congress in 1977 in response to concerns about bank redlining of poor communities. It established a "continuing and affirmative obligation" for a federally-insured depository institution to meet "the credit needs of its entire community,

including low- and moderate-income (LMI) neighborhoods, consistent with the safe and sound operation" of the institution. The CRA statute directs federal banking regulators to consider a bank's CRA record when it applies for permission to expand. Otherwise the law provides little guidance on how CRA performance should be evaluated and no other direct enforcement authority. As such regulators have always had substantial latitude in how they evaluate CRA performance, but limited powers to compel banks to meet CRA objectives. CRA imposed few if any binding constraints on bank lending during its early years;[8] to the extent that it did, CRA appears to have impacted residential mortgage lending primarily, in accordance with the preferences of early CRA proponents**.**

Regulatory reforms changed CRA in 1995, instituting new incentives for lending to small businesses and in LMI areas. I outline below how these incentives were binding only for certain banks, effectively creating CRA regimes that were newly tough for certain banks and unchanged (i.e., still easy) for others. Insights from industry experts, trade press, and focus groups conducted by the Joint Center for Housing Studies at Harvard University will help highlight the *de facto* as well as the *de jure* workings of the law.

B. The Enforcers

Four separate federal agencies have responsibility for implementing CRA, and a bank is assigned to the same agency for both CRA and supervisory (safety-and-soundness) purposes.[9]

[8] Among the statistical and anecdotal evidence supporting this contention is the fact that in some years during the 1980s certain regulators conducted no CRA exams (Matasar and Pavelka, 1998). Today regulators perform thousands of CRA exams per year.

[9] A bank is assigned to an agency based on its charter type: the Federal Reserve System for bank holding companies and for state-chartered banks that are members of the System, the Office of the Comptroller of the Currency for national banks, the Federal Deposit Insurance Corporation for state-chartered nonmember banks (that are FDIC-insured), and the Office of Thrift Supervision for thrifts (a.k.a. savings and loans). These four agencies cover virtually the entire universe of federally insured depository institutions with the exception of credit unions, which are exempt from CRA and supervised by the National Credit Union Administration.

Third-party watchdogs-- typically nonprofit community development organizations— use the CRA data and regulatory process described below to extract lending from banks (see, e.g., http://www.woodstockinst.org/craexplained.html).

C. Exams and Applications: the Process of Evaluating CRA Performance

The four agencies evaluate and rate banks on CRA performance through periodic (typically annual or biannual) exams conducted by staff stationed at one of 31 regional offices scattered throughout the country. Exams are conducted at the individual bank level— even when a bank is controlled by a bank holding company-- and based on some combination of publicly available data reported by the bank for supervisory or CRA purposes, samples of bank loan files, interviews with market participants, and discretionary data collected by banks and/or regulators on "market context". The regulator evaluates this data based on the criteria outlined below, writes a detailed evaluation, and assigns the bank a rating.[10] The evaluation and rating are then posted online and published.

A bank's CRA performance must then be considered when a bank applies to its regulating agency, as it must, for permission to merge, acquire, or otherwise expand. Regulators look first at past ratings in evaluating CRA performance, and often consider lending activity since the most recent exam as well. By law, they must also consider substantive public comments (more commonly known as "CRA protests"). Such actions are often enough to derail applications from "fast-track" processing, and they may delay approval by months or jeopardize the application entirely. Regulators also independently delay, reject, or impose conditions on application

[10] The rating categories and 1998 frequencies (as compiled by the National Community Reinvestment Coalition) are as follows: outstanding (19.1%), satisfactory (78.9%), needs-to-improve (1.8%), and substantial noncompliance (0.2%).

approvals with nontrivial frequency.[11] These adverse application outcomes are costly for banks (Johnson and Sarkar, 1996)— they require substantial allocations of senior management time for negotiations with regulators and watchdogs, impose goodwill losses, and increase transaction costs.

Perhaps most importantly, bankers appear to believe that the *threat* of costly application outcomes is credible, and that CRA lending can prevent these outcomes. They point to seminal rejections as signals from regulators to the market (see, e.g., Belsky, et. al.), and many appear to operate on the premise articulated by one banker: "an outstanding [CRA] rating is insurance against being put in an unmergeable category" (Belsky, et. al.).

Aside from this ability to impact bank applications, regulators have no direct authority to impose sanctions on CRA grounds. More generally watchdog groups use the CRA process and data to impose public relations or goodwill costs on targeted banks, for as one banker noted, "Lenders care about what the Wall Street Journal writes about your lending institution. It's a big deal." (Belsky, et. al.).

D. CRA criteria: what counts?

But what counts as a CRA loan? The CRA defines small business lending as loans of less than $1 million made for business purposes and/or loans made to businesses with less than $1 million in revenues. LMI areas are defined at the census tract level; accordingly, a loan counts as an LMI loan if it is to a business (borrower) located in (residing in) a census tract where the 1990 median family income is less than 80 percent of median family income in the tract's metropolitan statistical area (MSA). There is no provision, as some have suggested, granting

[11] Statistics compiled by Thomas (1998), Litan, et. al. (2000), the National Community Reinvestment Coalition, and Gramlich (1999) suggest that perhaps five percent of merger and acquisition applications receive CRA scrutiny that is costly to banks. Most of these are protests and/or delays, with perhaps 0.5 percent subjected to conditional approvals based on future CRA performance, and only a tiny fraction rejected.

"extra credit" for loans that qualify as both small business *and* LMI. "CRA lending" is thus comprised of small business lending generally, plus LMI home mortgage and LMI business lending. Banks are evaluated on CRA lending only in markets where they have a bricks-and-mortar presence, and largely on lending during the time elapsed since the previous exam. Flows are emphasized more than stocks, although the latter may be considered at the regulator's discretion. More generally CRA relies heavily on regulator discretion to balance its stated goals of increased access to credit against the costs of credit allocation. Consequently the letter and practice of evaluating CRA lending has never been formulaic; e.g., CRA regulations and examination procedures (http://www.ffiec.gov/cra/about.htm) outline several tests designed to assess a bank's small businesses and LMI lending, but individual regulators have substantial leeway in deciding which tests to use and how to interpret them. Equally interesting for our purposes is the fact that many of these tests represented dramatic *changes* to CRA lending criteria when they were enacted in May 1995.

Most generally, the 1995 CRA reforms emphasized performance over process. The previous criteria had been very complex and multifaceted, rewarding banks for activities such as conducting market studies and meeting with community groups. In contrast, the new regulations make lending the clear focus of the CRA evaluation (e.g., Broome 1996).

More particularly, the 1995 reforms provided new incentives for small business lending. This reflected an evolution of community development practice in LMI areas-- which was expanding from an often singular focus on LMI housing provision to concerns with "economic development"-- and a concern that consolidation in the banking industry was tightening credit availability to small businesses generally.[12] The new lending tests made clear reference to the importance of small business lending within and outside of LMI areas, and political will backed

[12] The literature on this question is inconclusive—see Strahan and Weston (1998) for a review.

these new directives (e.g., Ludwig, 1998). CRA also began to require larger banks to report the location of small business loans. This data reduced the cost of evaluating banks, drew attention to small business as a target market, and thus provided regulators and watchdogs with new leverage (e.g., Belsky, et. al., 2000).

All in all, it seems plausible that the 1995 reforms "raised the bar" on the level of CRA lending required for good ratings and expeditious approval of applications. Figure 1 shows that banks at least increased their CRA *commitments* in the wake of the 1995 reforms (commitments are nonbinding but typically well-publicized pledges to engage in future CRA lending).

E. Different Bite for Different Banks

CRA incentives vary not only across space (by targeting LMI areas), and across time (by providing newly binding incentives, particularly for small business lending, post-1995), but also across banks. This subsection details how the 1995 reforms appear to have had more bite for larger banks, banks intending to expand, and banks with tough regulators (see Table 1a for summary statistics).

Several factors suggest that the 1995 changes had differential effects by bank size. The CRA regulations define a "small bank" on the basis of bank asset size and holding company affiliation. A bank is considered small if it was independently owned and had assets of less than $250 million on December 31st of either of the two prior calendar years, or if it was owned by a holding company that had total assets of less than $1 billion on December 31st on either of the two prior calendar years. Small banks qualify for streamlined examination procedures that reward them, almost inevitably, for lending they would do in the absence of CRA (Thomas 1998; Barefoot 1998). Small banks are moreover exempt from the new small business data collection requirements that became effective in 1996; as discussed above, this data reduces enforcement

costs. Large banks, on the other hand, are subject to both the reporting requirements and a more substantive battery of tests (e.g., Cocheo, 1996). Large banks evidently believed the new standards would bind— those with exams scheduled between January 1, 1996 and June 30, 1997 had the option of choosing to be examined under either the new or old criteria, and only a tiny fraction opted for the new (Thomas, 1998).[13] Figure 2 presents a first bit of evidence that the new large bank standards did produce lending increases: these graphs show simply that while banks just above the CRA size cutoff did less small business lending than their counterparts just below the cutoff before the reforms (the dashed line), they did more small business lending after the reforms (the solid line).

The limited scope of CRA enforcement powers suggests that CRA also binds more for the many banks interested in expanding (summary statistics in Table 1), since regulators may only take formal action on CRA grounds around an expansion application and must consider watchdog input at that time. The threat of costly application outcomes appears credible, as discussed in sub-section C.

Regulator tastes also critically determine whether CRA incentives bind for a given bank. Although the regulating agencies coordinate almost perfectly on the letter of CRA criteria through the Federal Financial Institutions Examination Council (FFIEC), in practice there is wide variation in how CRA is enforced both within and across agencies.[14] Anecdotal evidence of this phenomenon abounds, and the General Accounting Office (1995) found inconsistent grading in a review of 40 evaluations. More recently Thomas (1998) documented variation in

[13] Anecdotes relate that several large banks with exams scheduled just after the July 1st, 1997 deadline lobbied to move their exams forward in time, presumably so that they could be evaluated one last time under the old, easier criteria.

[14] Variation across agencies appears to stem from political factors and the preferences of lead officials (see e.g., Thomas, 1998); variation within agencies may be driven by agency problems inherent in the decentralized administration of CRA.

CRA grading propensities by systematically re-rating the first 1,407 small bank evaluations conducted by the 31 examining regional offices in 1996 (the Thomas procedure and resulting data are described in greater detail in Section III, part C.). Thomas' simulated ratings can be compared to actual CRA ratings and used to compute each office's propensity to follow the letter of CRA criteria. There is substantial variation in these propensities— regulator-regions grade fairly as little as 9 percent of the time and as often as 89 percent of the time-- and they appear to be idiosyncratic. For example, variance decompositions show that there is actually more variation in grading propensities within regions than across them. This is not surprising given that any geographic area is home to at least three different regulator-region offices, by dint of the fact that there are three different regulating agencies, all with substantial representation in the bank population (Table 1). Moreover, each agency defines its geographic regions somewhat differently (Figure 3).

So there is an empirically supported consensus that CRA regulator diligence varies idiosyncratically. Figure 4 presents a preliminary bit of evidence that "regulator toughness" impacts CRA lending by comparing lending by banks supervised by regulator-regions most likely to grade CRA exams fairly (i.e., the "toughest regulators") in the Thomas data to lending by banks supervised by regulator-regions most likely to give banks better grades than they deserve (i.e., the "easiest regulators"). This figure suggests that while banks with tough regulators broke from trend and increased their small business lending sometime after the 1995 reforms, banks with easy regulators appear unmoved.

F. Summary

The entire CRA incentive scheme can be summarized as follows: banks engage in CRA lending if they expect to receive net benefits from doing so. Net benefits may be linked to

lending indirectly, via the CRA rating assigned by regulators, or directly, since lending helps a bank makes its own case to the public, regulators, or watchdogs, regardless of its rating. Regulators and watchdogs help determine the strength of the relationship between CRA lending and net benefits. If these actors care about CRA lending, they can extract it by (threatening to) impose costs on banks; e.g., by delaying or rejecting a merger application, or generating negative publicity. Enforcer diligence is thus an essential element of CRA incentives. Enforcement leverage— the ability to extract CRA lending effort conditional on enforcer diligence— differs across banks due to provisions in the CRA law and regulation. Beginning in 1995, CRA regulations provided new incentives for CRA lending, especially for banks that are considered "large" and intending to expand.

In sum, CRA incentives vary across bank characteristics, space, and time. The next sections detail how this variation can be mapped into available data and used to identify the effects of CRA on borrowing, lending, and real activity.

III. The Effect of CRA on Potential Borrowers

I begin by motivating a model that will identify CRA's effects on debtholding by potential borrowers. This is, in an important sense, the "1st-stage" of interest, since any aggregate real effects presumably work through CRA-induced changes in borrower access to credit. Data on potential borrowers is also less subject to functional form issues than data on banks.

A. Model and Identification

If CRA is effective, then a small business should be more likely to hold a (CRA) loan if a local bank faces newly binding CRA incentives. "Local" is defined at the county level because this is arguably the best geographic description of small business credit markets (86% of bank

borrowers in the 1993 National Survey of Small Business Finances had a loan from a bank in their home county). County-level analysis also provides substantial statistical power, with over 3000 units. The discussion in Section II highlighted that "big" banks with tough regulators faced newly binding CRA incentives after the CRA reforms in 1995.[15] This motivates the following model:

$$(1)\ Y_{ict} = \alpha + \beta(Post_t*Big_c*ToughRegulator_c) + n_1 Post_t*Big_c + n_2 Post_t*ToughRegulator_c + n_3 Big_c*ToughRegulator_c + n_4 Post_t + n_5 Big_c + n_6 ToughRegulator_c + \gamma_t + \varepsilon_{ict}$$

where Y is a measure of whether potential borrower i has a loan at time t (see sub-section B below), and c indexes counties. Later Y will be a county-level measure of real activity (see Section V). $Post_t*Big_c*ToughRegulator_c$ is the regressor of interest and takes the value of one if the observation on i is recorded after the CRA reforms took effect, *and* i is located in a county that has a bank that is: a) considered big for CRA purposes, *and* b) has a tough CRA regulator (the finer points of measuring these three dimensions of CRA incentives are detailed in sub-section C). We are interested in testing whether β is positive; e.g., if CRA has an effect, potential borrowers located near big banks with tough regulators should become more likely to have a loan post-1995.

$Post_t*Big_c$ and $Post_t*ToughRegulator_c$ control for "component trends"-- differential time trends in Y for potential borrowers located near big banks (relative to firms located near only small banks) and banks with tough regulators (relative to firms located near only banks with easy regulators), respectively. $Big_c*ToughRegulator_c$, Big_c, and $ToughRegulator_c$ condition out the main effects (i.e., the conditional means of Y) for potential borrowers that are located in counties

[15] I have not yet attempted to exploit bank expansion status as a dimension of CRA incentives due to various measurement problems. One of these problems, the difference between *potential* expansion (which creates the CRA incentive) and *actual* expansion (which we observe), does lessen with the passage of time, however, as data on bank expansion activity after the end of my sample period (1998) becomes available.

that have an "affected" bank (i.e., a bank that is both big for CRA purposes and has a tough regulator), a big bank, and a bank with a tough regulator, respectively. γ_t partials out year effects-- these of course capture any time series shocks that affect the entire sample. ε_{ict} is the observation's error term and allows for correlation among observations located in the same county, the locus of CRA incentives.

β will capture the effect of CRA incentives if there is no unobserved shock that affects potential borrowers located near big banks with tough regulators relative to other potential borrowers (conditional on the other observables included in the model), is contemporaneous with the CRA reform time shock, and is correlated with the outcome of interest. In other words, (1) assumes only that there is no unobserved shock to our measure of debtholding during the post-1995 period that hits "affected firms" (potential borrowers located near big banks with tough regulators) differently than unaffected firms.

The identifying assumption might not hold if the CRA incentive scheme somehow targets banks or regional economies that broke from trend in the post-reform period for secular reasons; e.g., if policymakers assign tough regulators to the most promising subset of large banks. More mechanically, regulators might be tough where it is cheapest for them extract lending— and this might hold where there are banks that are both big (giving the regulator binding incentives at her disposal) and secularly growing. Such concerns are mitigated by several factors. The length and unpredictability of the reform process alleviates the typical policy endogeneity concern— even if CRA reform (which started in mid-1993) was somehow driven by increasing demand in areas with big banks and tough regulators, the timing of any related secular break from trend would have almost certainly preceded the implementation of the reforms (which began only in 1996 and

was not completed until mid-1997).[16] Recall, moreover, that the regulator toughness measure is based on grading propensities for *small* bank exams, reducing the probability that it somehow reflects secular trends in big bank behavior. Furthermore the forecasting technology required to bias the model in favor of finding a positive CRA effect is quite sophisticated-- the regulator toughness measure is based on data collected at only one point in time, and regulators therefore would need to be targeting *prospectively* growing big banks on average. Targeting transitorily growing big banks would likely bias against finding a CRA effect due to mean reversion. Finally, evidence presented in Section II suggests that regulator toughness is distributed idiosyncratically.

I also will present results from models based on the CRA incentive components, largely for expositional purposes:

(2) $Y_{ict} = \alpha + \beta(Post_t * Z_c) + n_1 Post_t + n_2 Z_c + \gamma_t + \varepsilon_{ict}$

Where Z_c is either Big_c or $ToughRegulator_c$. Identification here requires the stronger assumption of no unobserved differential trends in Y by Z_c.

B. Data on Debtholding by Potential Borrowers

I estimate (1) and (2) using Internal Revenue Service data that captures the financial structure of potential CRA borrowers. The Statistics of Income Corporate File is a restricted-access, nationally representative sample of corporate tax returns that affords limited detail on firm balance sheets but great statistical power, with 80,000 or more observations annually from 1993 through 1998. Y_{ict} is constructed here as I(notes, mortgages, or bonds payable); i.e., it measures

[16] President Clinton ordered an overhaul of the CRA regulation July 15, 1993, and banking regulators issued two sets of controversial draft regulations for public comment before settling on the final version in May 1995. The new regulations became effective for small banks beginning with exams on January 1, 1996, and for large banks beginning with data collection on January 1, 1996.

whether the firm holds *any* debt exclusive of trade credit (see Table 2 for summary statistics). This measure of Y proxies for changes in access to credit and permits estimation of CRA effects that are net of any crowd-out. One important limitation is that the IRS codes unreported debt as zero. This mismeasurement should bias against finding significant results in the IRS data, since we expect that CRA's effect, if any, would be to change some firms from no debtholding to some debtholding, and some of these changes will not be captured.

The IRS data also provides information on firm location needed to match firm records to measures of CRA incentives, which vary at the county and/or census tract level.

C. Measuring CRA Incentives

As equation (1) highlights, we wish to test whether Y increases after the CRA reforms take effect, for firms located in affected counties. Capturing the time variation in CRA incentives is easy— the reforms of interest took effect in 1996.[17] Affected counties are identified by flagging affected banks from publicly available data on bank size and CRA regulator-region grading propensities, and then matching banks to counties using data on the universe of bank office locations. Specifically, the universe of "big" commercial banks is derived from the 1993 Call reports by applying the CRA asset size cutoffs for the large bank standards. "Regulator toughness" is extracted from Thomas' (1998) data on the grading propensities for each of 19 relevant regional offices that conduct CRA exams.[18] A regulator-region is labeled "tough" if it

[17] Mismeasuring this time shock due to the gradual phase-in of the 1995 reforms will generally bias against finding CRA effects.

[18] Thomas (1998) and a team of analysts under his direction "examined the examiners" by comparing simulated to actual ratings for the first 1,407 small banks evaluated under the new regulations in 1996. Thomas and team prepared for their simulated exams as regulators do, collecting data from the banks themselves and from outside data sources where publicly available data proved insufficient. Three different members of Thomas' team then graded each bank, following the written CRA examination procedures, before a simulated rating was assigned. These simulated ratings therefore represent "one man's (or one team's) view" of how the letter of the CRA law should be applied. The simulated ratings can then be compared to the actual ratings assigned by each of the 31 regional offices

grades fairly more than 50% of the time, and a bank is then matched to its regulator-region (and consequently to a 1/0 measure of regulator toughness) based on its charter and headquarters location. These bank-specific measures of CRA size and regulator toughness are matched to the universe of bank office locations captured in the Federal Deposit Insurance Corporation's 1994 Summary of Deposits (SOD). The SOD is then aggregated to identify which counties had one or more big banks and one or more banks with tough regulators. These county-level measures of CRA status are subsequently matched to firms using IRS zip codes. An IRS firm's LMI status is ascertained, albeit with some measurement error, by using its zip+4 code to identify the census tract code and accompanying income category from the 1990 Census. Table 2 lists the number of observations on firms located in affected and unaffected counties, in counties with and without big banks, in counties with and without banks with tough regulators, and in LMI and non-LMI tracts.

D. Estimation Samples

I create pooled IRS estimation samples based on active, nonfinancial firms filing continuing, full-year returns for the tax years 1993, 1994, 1997, and 1998. An additional 28,000 observations with questionable zip codes are excluded, although results are robust to including them. These rules produce a "full sample" of 236,579 observations. In specifications including regulator toughness, I also omit observations where small sample sizes in the Thomas data produce uncertain regulator toughness for the firm's county; this eliminates another 10% of the sample for discrete specifications of regulator toughness, and 40% of the sample for continuous

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that conduct CRA exams to create estimates of regulator-region grading propensities; for example, one can calculate (as Thomas does) the frequency with which an office gives a bank a better rating than it deserves. I ignore the 5 Office of Thrift Supervision regions since I do not include thrifts in my lending analysis, and discard 7 (out of 13 total) Fed regions due to small samples of exams (9 or less). Grading propensities for the remaining regulator-regions are based on a total of 1,139 exams, with regulator-region sample sizes ranging from 19 to 300.

specifications. Each equation is then estimated on three different samples: the full sample described above (including any adjustments for uncertain regulator toughness), and two sub-samples designed to limit the analysis to small firms. The "small shareholder" sample keeps only firms with less than 36 shareholders (this cutoff is established by the IRS). The "small assets" sample keeps firms with less than $6 million in assets.[19]

E. Exploiting Variation in CRA Incentives Across Bank Characteristics

Table 3 presents weighted linear probability estimates of equations (1) and (2) for the three different samples described above, using two different parameterizations of CRA incentives. The first row features results for the variable of interest, $Post_t*Big_c*ToughRegulator_c$; these coefficients estimate the effect of changes in CRA incentives on the probability that a firm in the IRS sample holds any debt. The first three columns of results present estimates for each of the three different samples using a discrete parameterization of CRA incentives-- a firm is defined as affected if it is located in a county that had one or more affected banks located there pre-reform. These results suggest that the probability of holding debt increases by approximately seven percentage points due to CRA among affected firms relative to unaffected firms. This implies an approximate 14% increase on the weighted mean debtholding probability of 0.5. The final three columns present a specification based on smooth measures of both the bank size and tough regulator incentives, with bank size measured as the pre-reform deposit share held by big banks in the firm's county and regulator toughness constructed as a deposit-weighted grading propensity across all of the regulator-regions operating in a given county (i.e., for a given county, toughness is the sum over the propensities to grade fairly for each of the three regulator-regions

[19] The assets cutoff is an attempt to make the sample comparable to that featured in the Survey of Small Business Finances, which is representative of firms with less than 500 employees (the IRS lacks employment data). 90% of firms in the SSBF have less than $6 million in assets.

operating in that county, weighted by the total pre-reform deposits owned by banks supervised by each regulator-region.) These results appear comparable to those obtained with the discrete specification-- a one standard deviation increase in the CRA incentive variable, $Post_t*Big_c*ToughRegulator_c$, produces debtholding increases of eight to ten percentage points.

Estimates of equation (2), presented in the second and third rows of results, show debtholding increases where there are tough regulators but not where there are large banks. One should view these results skeptically, however, as both the bank size and regulator toughness classifications seem prone to bias from secularly differential trends.[20] These concerns are addressed directly in (1) with the inclusion of component trends.

CRA presumably impacts debtholding through improved access to bank credit that does not fully crowd-out alternative sources of finance. Section IV provides some indirect confirmation of this channel with evidence suggesting that CRA does induce lending increases by affected banks along intended margins; moreover, there is little evidence of crowd-out between banks. Estimates of (1) from joint work with Alicia Robb using the confidential version of the Survey of Small Business Finances (SSBF) provide more direct but statistically weaker confirmation (see the last two rows of Table 3). Point estimates suggest eleven or twelve percentage point increases in the probability of holding a bank loan, but the small SSBF samples produce large standard errors.

Taken together, the results in Table 3 suggest that, in locations where CRA binds, it improves access to credit for perhaps seven to ten percent of firms.

[20] Others have suggested, for example, that merging behavior and technology adoption may have changed differentially by bank size beginning in the mid- to late-90s. One might also worry that regulator toughness will be correlated with bank or regional growth if, for example, regulators enforce CRA only where there are secularly capable banks. Indeed, Thomas' data shows that regulators err on the side of laxity: 36.8% of the exams he sampled were graded too easily, while less than 1% were graded too stringently. Note the direction of the bias probably depends on whether regulators bite prospectively or transitorily growing banks.

F. The LMI Incentive

Section II suggested that CRA *may* induce banks to increase lending to businesses in LMI census tracts. Although CRA ostensibly rewards small business lending regardless of location, banks may find efficient to focus on loans that qualify as both small business and LMI lending if there are fixed search costs and/or enforcers care more about LMI loans. Moreover LMI loans to big businesses should also boost CRA performance. Accordingly, I begin by testing whether debtholding increases for firms located in LMI tracts (relative to non-LMI tracts) after 1995. These estimates of equation (2) are presented in row 1 of Table 4 and show no effect. Rows 2 and 3 add bank size or regulator toughness, respectively, to create models that incorporate the LMI incentive into equation (1). These too show no significant effects. Rows 4 and 5 split the sample into firms located in LMI and non-LMI tracts and estimate equation (1). These results suggest that equation (1)'s results (Table 3) are driven by *non*-LMI firms. This finding jibes with contentions by LMI advocates that banks find it easiest to maximize CRA performance by increasing small business lending in non-LMI areas, if at all (e.g., Immergluck 1997).

G. Summary

Taken together, results from tests on potential borrowers suggest that, where it binds, CRA increases the number of firms that hold debt by seven to ten percentage points. Since affected counties house approximately 60% of all firms (see Appendix), this suggests that CRA increases the total number of firms holding debt by four to six percentage points**.** The results provide no support for the hypothesis that CRA successfully targets firms in LMI areas.

IV. The Effects of CRA on Bank Lending and Performance

CRA presumably increases the probability of holding debt among the firms studied in Section III by inducing (small) business lending increases among affected banks. I now test this hypothesis, and whether CRA increases lending along intended margins more generally, using data on banks. Bank microdata also permits falsification exercises and tests for potential distortions that are not possible in the data on potential borrowers.

A. Bank Lending Models

CRA's effects on lending are estimated with bank-level analogs of (1):

$$(3)\quad Y_{brt} = \alpha + \beta(Post_t * Big_b * ToughRegulator_r) + n_1 Post_t * Big_b + n_2 Post_t * ToughRegulator_r + n_3 Big_b * ToughRegulator_r + n_4 X_{bt} + n_5 Post_t + n_6 Big_b + n_7 ToughRegulator_r + \phi_b + \gamma_t + \varepsilon_{brt}$$

Where b indexes banks, t time, and r regulator-regions. Y is now a measure of (CRA) bank lending, with levels of small business lending, home mortgage lending, and total lending the primary outcomes of interest (summary statistics in Table 1). The lending functional form is motivated by the assumption that the *level* of credit available in a county is what potentially affects the ultimate outcome of interest, real activity. Therefore bank lending changes that are small in percentage terms but large in levels could have real impacts. Moreover, it seems plausible that CRA produces this very pattern of lending changes, given its stronger incentives for (discretely) larger banks and the apparent propensity of watchdogs to target (continuously) larger banks.[21]

[21] CRA lending tests focus on both proportional and absolute measures. CRA commitments (which are often extracted by watchdogs) are almost always in levels (see National Community Reinvestment Coalition, 2000).

CRA incentives are now measured at the bank or regulator-region level, with affected banks defined as those for which $Big_b * ToughRegulator_r = 1$. β again is the coefficient of interest and should be positive for CRA lending; i.e., if CRA has an effect, we should see large banks with tough regulators increase their CRA lending post-1995. Conversely, β should *not* be positive for "non-CRA" lending, which is comprised of loan types that could not possibly qualify for CRA credit (e.g., loans to other banks or to governments, and unsecured consumer loans in most cases). ϕ_b captures bank fixed effects, and X_{bt} is a vector of bank assets and interactions of assets with year effects and the two components of affected status. These additional variables are designed to purge the bank lending heterogeneity evident in Table 1; X_{bt} does so by exploiting the discontinuous shift in CRA incentives at the big bank cutoff.

Measurement of the LMI incentive is limited here due to the absence of data on loan location. Nevertheless one can use data on *bank* location to construct another falsification test—banks with offices only in counties without any LMI tracts should have little incentive to increase their big business lending, since banks are evaluated on CRA performance only in markets where they have offices, and a big business loan counts as CRA lending only when it is made to an LMI borrower.

B. Bank Lending Data

Data on loans outstanding at all U.S.-based commercial banks is extracted from the June 30th Reports of Condition and Income ("Call Reports") for each year beginning in 1993 and ending in 1998, a universe of 11,673 banks and 59,030 bank-year observations. The Call Reports permit precise measurement of CRA small business lending, since the CRA regulation borrows the Call definition of loans of less than $1 million that are secured by commercial real estate or used more generally for commercial and industrial purposes. The Call provides the number as well as

the original dollar amount of small business loans outstanding, but only the original dollar amount for other lending types. Other unique details in the small business data can be used to construct internal consistency checks that flag 1,597 observations with reporting errors. Home mortgage lending is also constructed to match the CRA definition, which simply encompasses all residential mortgage loans.

C. Effects on CRA and non-CRA lending

I begin with a "full sample" of commercial banks from the pooled 1993-98 June 30th Call reports; this is simply the universe of nearly 60,000 bank-year observations, excluding approximately 5,000 observations with reporting errors and/or unknown regulator toughness. Next I limit the sample to banks just above and below the CRA own asset size cutoff-- this sampling-based "regression discontinuity" approach moves beyond the heterogeneity controls included in (3) by limiting the analysis to a set of plausibly homogeneous banks. Summary statistics for both samples are presented in Table 1.

Estimates based on these two samples are presented in Table 5 for each of the 7 types of lending listed in the rows. The effect of the variable of interest, $Post_t*Big_b*ToughRegulator_r$, is shown in the first column for the full sample and the fourth column for the regression discontinuity sample. Full sample results suggest that CRA indeed increases lending along intended margins, with small business lending and home mortgage lending increasing along with total lending. Moreover, there is no evidence that my measure of CRA incentives predicts increases along unintended margins-- affected banks that have offices only in counties with no LMI tracts do not increase big business lending, and there appear to be significant decreases in other lending types which could not possibly contribute to CRA performance ("non-CRA" lending). The small business lending coefficient implies a $21.6 million increase by affected

banks after the CRA reforms, or a 12% increase over base period lending by affected banks. The regression discontinuity sample provides additional evidence of a small business lending increase-- one that is quite similar when scaled, at 15% of base period lending, to that of the full sample-- and again shows no increases in non-CRA lending. Home mortgage and total lending no longer show significant responses to CRA incentives, although the latter result may simply be a precision issue due to the small sample. Estimates using the bank analogs of equation (2) (presented in the "$Post_t$*Big_b" and "$Post_t$*$ToughRegulator_r$" columns) differ in places from those obtained using equation (3), but again should be interpreted cautiously given concerns about confounding trends.

The full sample results do not appear to be driven by outliers or mechanical changes in bank ownership-- windsorizing the top and bottom percentiles, eliminating dfbeta-influential observations, and eliminating merging (but not acquiring) banks from the full sample do not change the qualitative nature of the key results (available upon request). In all, the effects on total lending suggest that CRA lending increases may not be completely offset by other lending decreases within affected banks. Nor is there clear evidence of crowd-out across banks-- augmented versions of equation (3) show no significant effects for the CRA incentives of neighboring banks, and leave the effect on own incentives unchanged.

The full sample findings do appear sensitive to functional form assumptions, however. These results change markedly if lending is parameterized in logs rather than levels, with estimates suggesting no effect on small business lending and a significant *negative* effect on total lending. Logs and levels do not produce appreciably different results in the regression discontinuity sample.

D. Effects on Bank Financing

Banks can finance CRA-induced lending increases by substituting from other assets (including non-CRA lending, cash, or other assets that can sold for cash used to issue CRA loans), assuming new liabilities, and/or drawing on equity. Banks might also change their capital structure if CRA induces them to assume new systematic risk that must then be hedged. These dynamics motivate estimating equation (3) with financing margins as the outcomes of interest. Preliminary tests depict no clear picture of CRA impacts on bank capital structure, but this topic merits further exploration.

E. Effects on Bank Performance and Consolidation

Table 6 shows no evidence of adverse effects on bank performance and weak evidence of decreases in merger and acquisition (M&A) activity. Profitability— whether measured by unscaled profits, return on equity, or return on assets— appears unaffected by CRA. Bad loans also appear unaffected in general, although there is weak evidence of a *decrease* in the regression discontinuity sample. The lack of significant effects on bank performance is unsurprising given the relatively small size of the estimated CRA lending responses.[22] Cross-section regressions suggest that merger and acquisition activity decreases by six to thirteen percent among affected banks, but these coefficients are not significantly different from zero.

F. Summary of CRA's effects on Bank Behavior

The results presented in this section provide some confirmation that CRA induces bank lending increases along intended margins, and thereby increases credit availability for targeted

[22] Estimates suggest that the upper bound of CRA's effects on total lending is a six percent increase by affected banks. If CRA increased lending by this amount and CRA-induced loans were only half as profitable as the average loan (e.g., a chargeoff rate of .006 instead of .003), then chargeoffs would increase by seven percent and profits would decrease by three percent. The observed standard errors could not differentiate effects this small from zero.

borrowers. Small business lending in particular appears to increase with CRA incentives, while non-CRA lending does not. CRA lending does not appear to be *completely* offset by decreases in non-CRA lending, either within or across banks, and in fact there is no clear evidence of *any* crowd-out. In general, however, the full sample lending results appear sensitive to functional form assumptions. CRA does not appear to have large adverse effects on bank profits or loan performance, suggesting that any benefits attributable to CRA may come cheaply (see Sections V and VI).

V. The Real Effects of Bank Lending

This section tests whether the observed CRA-induced increases in credit cause county-level real activity to increase. Several factors suggest that such a finding would be surprising. Marginal loans may cost banks more than they benefit borrowers. The marginal borrower may be unproductive on average but willing to gamble with loan proceeds due to limited liability or limited enforcement. Observed increases in real activity may represent redistribution rather than net gains. Or banks and borrowers could simply collude, with banks paying borrowers to hold loan proceeds in safe securities. In fact, I find some evidence that CRA does increase real activity and proceed to calculate implied rates of return on CRA borrowing.

A. CRA's Effects on Real Activity

The impacts of CRA on county-level measures of real activity are estimated by adding county fixed effects to (1) and changing Y-- the outcomes of interest are now logged business or nonbusiness bankruptcy counts from the Administrative Office of the U.S. Courts, and logged mid-March employment or annual payroll from the Census' County Business Patterns (CBP). The Courts data covers the universe of filings, and the CBP covers virtually the entire universe

of businesses with employees.[23] This setup will identify any reduced-form effects of CRA on real activity; I explain below (in sub-section B) how its results can then be combined with those from the IRS and/or bank microdata to calculate any impact of credit flows on real activity. Summary statistics for the outcomes and CRA variables of interest are presented in Table 7.

Dropping the 43 counties with CBP disclosure issues and 122 counties with uncertain regulator toughness produces a sample of 2,973 counties and 17,838 county-year observations from 1993-98. Running OLS on this "full sample" with the fully discrete parameterization of CRA incentives produces the results in the first "$Post_t$*Big_c*$ToughRegulator_c$" column of Table 8.[24] The point estimate suggests that CRA increases payroll by nearly one percent in affected counties, although this increase is not statistically significant (p-value = 0.146). Personal (nonbusiness) bankruptcies drop by 3.6%-- this could be driven by the improved ability of closely held businesses to "stick it out", a la Holtz-Eakin, Joulfaian, and Rosen (1994), and/or the improved ability of households to smooth adverse shocks due to increased access to mortgage credit.[25] Effects on business bankruptcies are insignificant but noteworthy because bankruptcy counts are unscaled. This is necessary because the contemplated denominator of interest, the total number of business extant in county c at the beginning of year t, is difficult to measure and potentially endogenous to CRA. Its omission probably biases the results in favor of

[23] County Business Patterns does surpress an occasional county-year for disclosure reasons. More importantly, it excludes businesses without employees, or the "self-employed". But CBP businesses account for approximately 97% of revenues and 25% of businesses in the United States (Census Bureau, Nonemployer Statistics, http://www.census.gov/epcd/nonemployer/index.html).

[24] Using the smoother parameterizations of CRA incentives (as in Section V) does not materially alter the results.

[25] This result could be interpreted as weak evidence against strategic explanations for bankruptcy filing by consumers (see Fay, Hurst, and White 2001), since if the marginal CRA home mortgage borrower is a first-time homebuyer, she will in virtually all cases have new access to the shelter provided by a bankruptcy homestead exemption and therefore experience *increased* financial incentive to file. Alternately, the observed decrease in filings may simply be driven by positive survival effects on unincorporated businesses (which drive down filings) dominating strategic effects on households (which increase filings).

finding a positive effect on business bankruptcies.[26] The negative sign thus again broaches the possibility that CRA increases firm survival.

The second and third columns present the results obtained when (1) is run separately for counties with and without any LMI census tracts, respectively (the latter counties are almost exclusively rural). The results on employment and payroll suggest that the full sample results are driven by improvements in affected no-LMI *counties*. This is consistent with results from the IRS data showing that debtholding increases only for firms in affected counties and non-LMI *tracts*.[27] In contrast, the personal bankruptcy decreases appear to be driven by affected LMI counties. This is consistent with CRA successfully targeting home mortgage borrowers and/or unincorporated businesses in LMI areas; testing this hypothesis is a topic for future research using more detailed data on mortgage loan location (from the Home Mortgage Disclosure Act) and business life-cycles (from various restricted or private sources).

Estimates of the real activity analogs of equation (2) (presented in the "$Post_t$*Big_c" and "$Post_t$*$ToughRegulator_c$" columns) differ in places from those obtained using equation (1), but again should be interpreted cautiously given concerns about confounding trends (see Section III).

There is little evidence suggesting that the observed changes in real activity are driven by redistribution across counties rather than net gains for the national economy. Shifting could occur if affected banks reallocated lending from unaffected to affected counties. *Ex-ante*, there is little reason to believe that affected banks face incentives to engage in such behavior, since banks are evaluated on CRA performance essentially wherever they do business. And indeed I find no direct evidence that affected banks with offices in multiple counties increase CRA or

[26] There are no publicly available counts of firm populations by county. If CRA increases business formation in affected counties— either through direct lending or otherwise— then raw bankruptcy counts will increase mechanically (in partial equilibrium, at least) if any of the new businesses fails.
[27] The IRS results do not imply that we could not observe real activity increases in affected LMI *counties*, however, since these counties include non-LMI tracts as well as LMI tracts.

total lending by less than affected banks with offices in only a single county (results available upon request). Nor do I find any direct evidence of negative regional spillovers in the real activity effects; additional variables that capture CRA incentives in neighboring counties show little evidence of a significant effect and generally leave the own-county CRA effects unchanged.[28] Finally, one should note that the size differences between affected and unaffected counties suggest that rather large real dislocations would be needed for redistribution to explain the results; e.g., the level shift implied by the observed 1 percent change in payroll in affected counties, $11.6 million, would represent a 2.3% change in payroll in unaffected counties.

B. Estimating the Effect of Bank Lending on Real Activity

The point estimates in Table 8 suggest the possibility of economically meaningful effects of CRA on real activity.[29] If one scales by base-period outcome levels in affected counties, then the coefficients imply $11.6 million increases in payroll (recall however that the effect on payroll was not statistically significant), and decreases of 13 nonbusiness bankruptcies, per affected county. The question then becomes how to compare these levels to credit increases to produce estimates of the effect of (CRA) credit on real activity. Unfortunately, the absence of data on loan location precludes direct scaling via a Wald or instrumental variables estimate. Accordingly I simply multiply average effects on affected units by the number of affected units to obtain

[28] Results available upon request. I have run these tests only on the subset of counties located within Metropolitan Statistical Areas (MSAs) thus far— rural counties lack a comparably natural grouping for the local/regional marketplace, and in fact are often considered as their own markets (at least for banking antitrust purposes).

[29] Attempts to explore the microfoundations of these results in the IRS and SSBF data were hindered by precision issues. Although these data provide extensive information on firm outcomes and input decisions— covering firm profitability and sales as well as the labor input decisions captured by the CBP— CRA's effects on these measures prove far too noisy to estimate with any precision. Given the standard errors one would need ridiculously high rates of return on CRA borrowing-- perhaps 800%-- to observe a significant effect on any firm outcome or hiring decision. Moreover micro estimates based on samples of firms will be biased against finding improvements in firm performance if CRA effects the composition of firms such that the average firm becomes relatively weak. This may well be the case if CRA prevents failures (as the bankruptcy results suggest) and/or induces starts. Of course aggregate output could still increase in this world because there would be more firms to sum over. All told, the IRS and SSBF estimates can neither rule out nor confirm effects that would aggregate to what we observe in the CBP.

aggregate estimates that can be used for scaling. The presence of approximately 1,370 affected counties then implies an aggregate payroll increase of $15.9 billion and personal bankruptcy decrease of 18,000 filings due to CRA.[30] Analogous aggregation using the small business lending or IRS results generate estimated borrowing increases ranging from $4.0 billion to $11.5 billion (see Appendix). These estimates imply payroll increases of between $1.40 and $4.00 for every dollar borrowed due to CRA, and one personal bankruptcy prevented for every $220,000 to $640,000 borrowed.

C. Implied Rates of Return on CRA Borrowing

Of course, the above calculations are not particularly informative without some notion of the implied rates of return (both social and private) to CRA borrowing. The motivation for calculating these is twofold: they will help estimate the welfare effects of CRA, and they will provide a plausibility check on the results. In particular, the gross private rate of return should be bounded below by the borrower's cost of CRA funds and above by the cost of pre-existing alternatives to (previously unavailable) bank credit-- if gross returns to CRA borrowing fall short of the cost of funds then marginal projects should not be undertaken, and if gross returns exceed the cost of outside options the marginal projects should have been undertaken already. Available evidence suggests that borrowers pay around 10% annual interest for small business loans—firms paid an average (median) of 9% (10%) on their most recent loan in the 1998 SSBF. Pre-existing alternatives to bank credit are more difficult to pin down. Nonbank institutional sources of finance are scarce-- banks provided 61% of small business loans and 77% of lines of credit in the 1993 SSBF. Trade credit generally is easier to obtain than capital from financial institutions

[30] There are 1,316 known affected counties in the sample; imputing affected status for counties omitted due to unknown regulator toughness based the proportion in the estimation sample yields 1,370 affected counties.

but carries an (implicit) average rate of about 70% (Petersen and Rajan, 1994). One problem with presuming that trade credit offered a viable outside option, however, is that it typically must be tied to purchases of inventory or intermediate inputs. This suggests that firms are relatively credit constrained on the labor margin we observe in the CBP, and that many firms may have lacked any legitimate pre-existing option in the absence of bank credit. The upper bound on plausible CRA returns is therefore uncertain but plausibly high.

Estimating rates of return requires an additional set of assumptions to translate the observed changes in real activity into changes in profits. Payroll seems the logical place to start, and if we assume that the marginal CRA borrower's marginal production function looks like the national aggregate average production function, then an additional dollar in payroll would produce $1.43 in sales (since 70% of national income is due to labor). Similarly, if the marginal profitability of new CRA borrowers can be approximated by the average profitability of small businesses, then we might conclude that the relevant net margin (i.e., profits/sales) is about 10% (median net margins were 7% in the 1993 SSBF and 13% in the 1998 SSBF). Under these assumptions, CRA increases profits by $15.9 billion*(1.43)*(0.10)= $2.3 billion, where $15.9 billion is our earlier (and admittedly imprecise) estimate of the aggregate payroll increase. Scaling this by the estimated borrowing increase of $4.0 billion to $11.5 billion implies that the gross rate of return on CRA borrowing falls in the range of 20 to 58 percent.

This range will provide a better approximation of the private return than the social return if some of the observed real gains are due to redistribution from unaffected to affected counties, or if CRA increases mortgage lending and these increases are distortionary.[31] Conversely, 20 to 58

[31] The available evidence suggests little reason to suspect that CRA mortgage lending is driving a wedge between the private and social returns: there is only mixed evidence that CRA affects mortgage lending (Table 5) and little evidence that CRA is distortionary (related evidence is summarized in Section VI). CRA mortgage lending may

percent will be a more accurate estimate of the social return than the private return if affected borrowers themselves do not realize some of the gains from CRA lending; i.e., if there are positive spillovers to real activity financed by CRA.

Returns to CRA borrowing in the neighborhood of 20 to 58 percent appear plausible given the nature of small business credit markets— where alternatives to bank credit may be quite expensive-- but would imply a nontrivial wedge between available profits and the cost of funds. One explanation is that the wedge is illusory; e.g., perhaps the estimated real benefits are the transitory result of CRA lending that inefficiently props up failing businesses. (Related work is examining the dynamic and longer-run effects of CRA as more data becomes available.) An alternative explanation is that something deterred arbitrageurs from financing (socially) profitable investments *ex-ante*. The concluding section outlines two models of credit constraints that could explain such a wedge, and ongoing research seeks to test whether amelioration of any particular credit market imperfection(s) drives the results in this paper.

VI. Conclusion

This paper presents some evidence that a major intervention in U.S. credit markets increases bank lending in a targeted market and access to capital for targeted firms. "Affected" banks facing binding CRA incentives appear to increase their small business lending by approximately twelve to fifteen percent, and the number of firms holding debt increases by perhaps fifteen percent in counties with affected banks. Evidence suggests that these financial changes produce aggregate real changes in affected counties, with an (somewhat imprecisely) estimated one percent payroll increase and significant bankruptcy decreases. A rough estimate of the gross rate

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also produce nonfinancial welfare gains if, as is often argued, it reaches credit constrained would-be homebuyers and/or improves neighborhoods by internalizing spillovers.

of return on CRA borrowing implied by these effects ranges from 20 to 58 percent. Returns in this neighborhood would be plausible— they are almost certainly greater than the borrower's cost of CRA funds, but may not be so large that the marginal projects should have been undertaken with previously available, expensive financing.

The efficiency implications of these results are not entirely clear. On one hand, there is little direct evidence of distortions. There is no strong evidence of crowd-out *in* bank lending, either within or across banks, or *by* bank lending of other sources of finance. Nor do the results suggest that the observed changes in real activity are due to shifting from unaffected to affected counties. Moreover there is little indication that CRA adversely affects bank profitability or loan performance (although power issues preclude identifying small effects). There is some suggestion that CRA discourages mergers and acquisitions, however.

On the other hand, important unresolved questions remain. One is whether the observed real "benefits" are illusory. The estimate of CRA's effect on payroll is imprecise, and the fact that we observe payroll, not profits, sparks concerns that there may be unobserved distortions even in the presence of a payroll increase. Furthermore the observed bankruptcy decreases raise the possibility that CRA inefficiently props up marginal borrowers. Future work will address these issues by examining CRA's effects on real activity (including business starts) in the longer-run, and by studying its effects on bank outcomes in greater detail— including the question of how banks finance marginal CRA loans— in an attempt to identify the presence or absence of additional distortions.

A second unresolved question is whether CRA ameliorates any particular credit market imperfection(s). Answering this question is critical to understanding the efficiency implications of CRA and other credit market interventions, and also offers the potential for more general

insight into the nature of credit markets. Ongoing research attempts to identify whether the results observed in this paper are driven by CRA impacting one or more commonly postulated sources of credit constraints, e.g., credit rationing/redlining or spillovers.[32,33]

In all, the findings in this paper appear consistent with a world where targeted credit market interventions can improve efficiency but provide little direct evidence that this is actually the case. Much work remains to be done to ascertain the causes and real effects of credit constraints.

[32] Models beginning with Stiglitz and Weiss (1981) show that credit rationing and redlining can result when prices have incentive effects due to asymmetric information. If prices change the distribution or behavior of borrowers, then the bank's profit-maximizing price may be lower than the market-clearing price. If this occurs some observationally equivalent agents will be "rationed" and some observationally distinct agents will be "redlined". Specifically, rationed or redlined agents will be denied loans *at any price*—they cannot obtain loans simply by bidding more. Ordover and Weiss (1981) show that a redlining equilibrium may exclude borrowers with positive (and even *relatively* high) returns, and that a government regulation forcing banks to lend to excluded types may increase the expected total return per dollar loaned. CRA thus could ameliorate credit rationing in one of two ways— through a blunt intervention that succeeds by simply forcing banks to lend more to excluded types (e.g., certain small businesses), or through a more surgical intervention that somehow address the underlying information problems.

[33] Positive spillovers could create credit constraints if a bank's return on loans in a given area increases with market thickness, as in Lang and Nakamura (1993). Negative spillovers could generate credit constraints if competition undermines privately optimal solutions to information and contracting problems, as in Petersen and Rajan (1995). CRA would mitigate spillovers if it provided an effective commitment device to coordinate lending in the positive spillovers case, or simply forced reluctant banks to make socially productive (but privately unprofitable) loans in the negative spillovers case.

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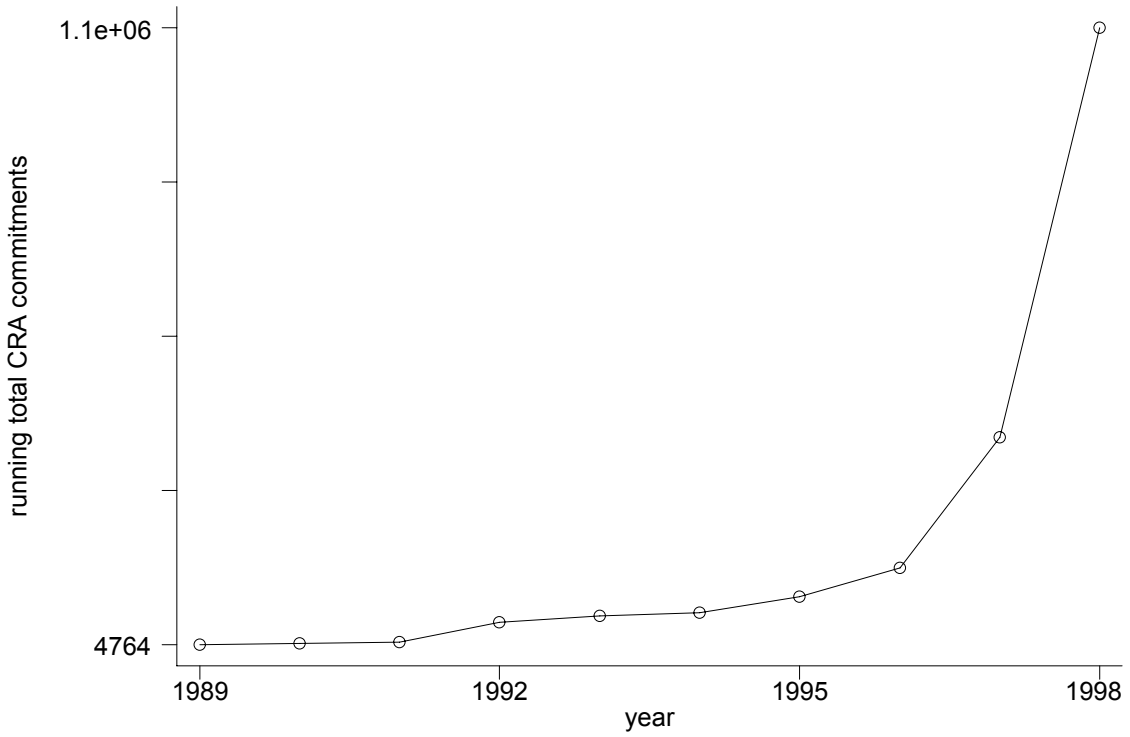
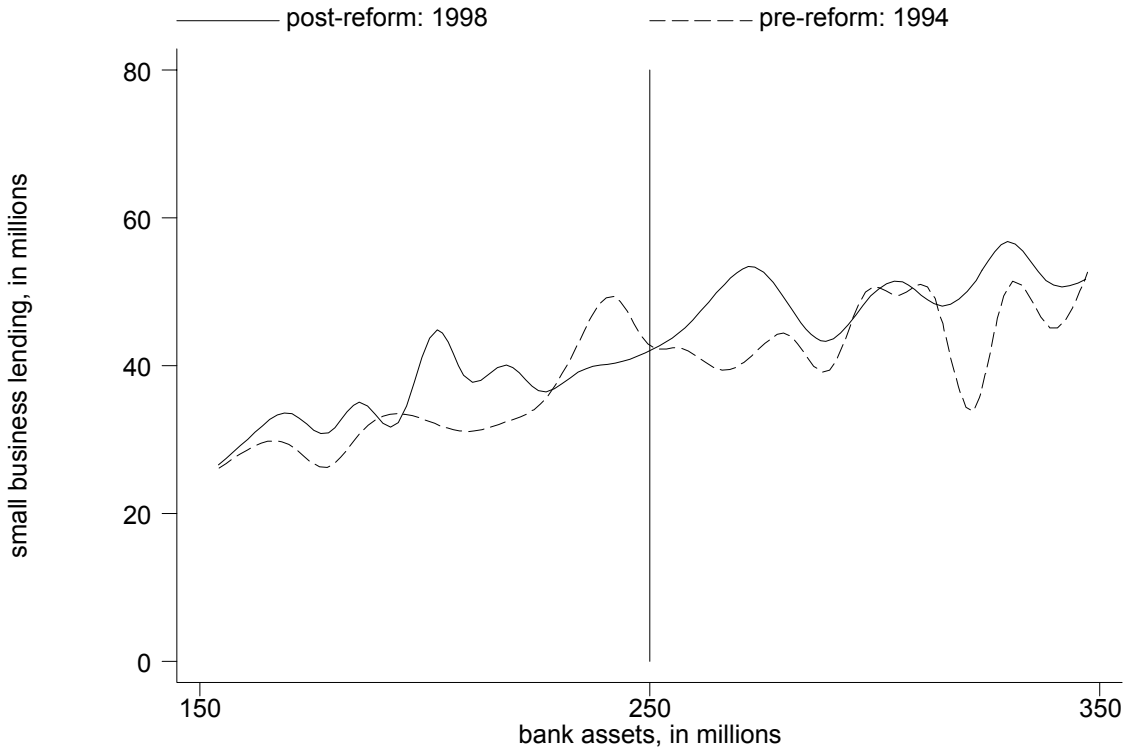
Figure 1. CRA Lending Commitments



Source: National Community Reinvestment Coalition (2000). CRA commitments are in millions of dollars and are defined as bank pledges to engage in future lending (primarily LMI mortgage and small business lending). Commitments are tallied in the year pledged and graphed as a running total. Regulatory changes providing new incentives for CRA lending took effect beginning in 1996 and were fully phased in by 1998.

Figure 2. Small Business Lending By Bank Size,
Around the CRA Big Bank Cutoff

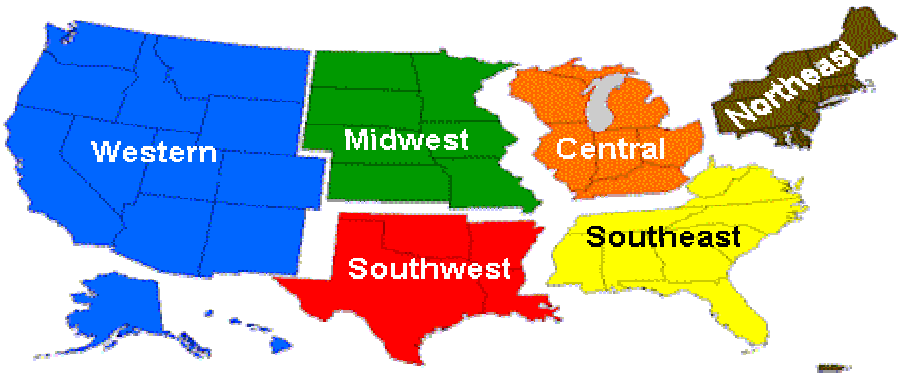


Smoothed cubic spline from 1,102 and 1,178 individual observations on commercial bank small business lending dollars outstanding from the June 30th, 1994 and June 30th, 1998 Consolidated Reports of Condition and Income ("Call Reports"), respectively. CRA provided plausibly binding incentives for banks at or above $250 million in assets, beginning with reforms that were enacted in 1995 and fully effective by 1998. Consequently we do not expect to see a discrete jump in lending above the asset cutoff in 1994 (and do not), but might expect to see a discrete jump in 1998 (and do).

Figure 3. CRA Regulator-Regions

Each regulating agency defines its geographic regions somewhat differently; e.g., whereas an OCC bank from Michigan shares its CRA regulator-region with banks from Kentucky but not from Iowa, a FED bank from Michigan shares its regulator-region with banks from Iowa but not from Kentucky, and an FDIC bank from Michigan does not share its regulator-region with banks from either Iowa or Kentucky.

5a. Office of the Comptroller of the Currency (OCC) Districts



5b. Federal Deposit Insurance Corporation (FDIC) Regions

5c. Federal Reserve Districts

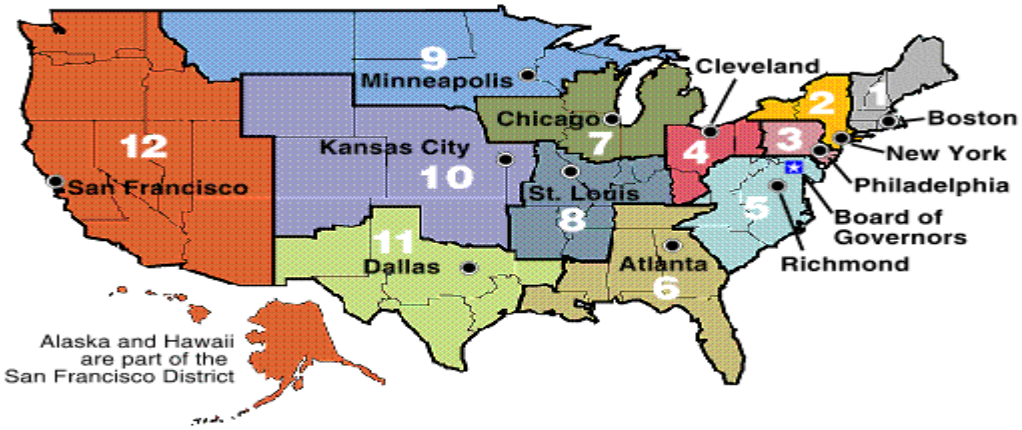
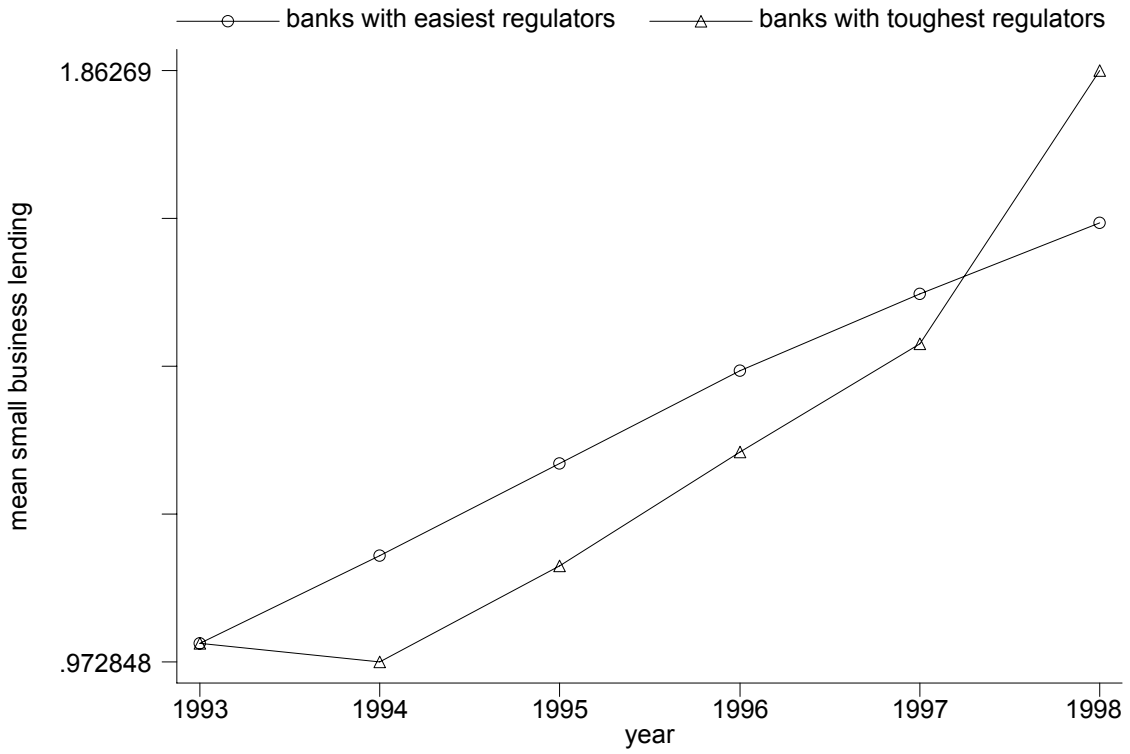


Figure 4. Small Business Lending:
Banks with Tough CRA Regulators vs. Banks with Easy CRA Regulators



Graph presents small business lending from the June 30th Call reports for two groups of commercial banks: those assigned to the four easiest CRA regulator-regions (28% of the full sample), and those supervised by the four toughest CRA regulator-regions (9% of the full sample). Gradations of regulator toughness are defined based on the observed propensity to grade CRA evaluations fairly in the Thomas (1998) data, for the 19 regulator-regions included in my estimation samples (see Section III). Banks are assigned to regulator-regions based on pre-reform charter type and headquarters location. Small business lending here is the mean annual group average, scaled by the 1993 group average. CRA reforms became effective beginning in 1996 and were fully phased in by 1998.

Table 1. Bank Summary Statistics, 1993-1998

	Full Sample			Regression Discontinuity Sample		
	All	**Affected**	**Unaffected**	**All**	**Affected**	**Unaffected**
Lending type						
Small business #	563 (3582)	3086 (12297)	392 (1738)	726 (588)	755 (684)	720 (568)
Small business $	32.4 (156)	193 (508)	21.6 (82.0)	47.1 (31.1)	52.1 (35.4)	46.1 (30.2)
Home mortgage	62.7 (650)	553 (2404)	29.6 (206)	58.1 (62.5)	77.6 (83.1)	54.4 (57.0)
Big business lending	49.4 (615)	524 (2194)	17.4 (250)	17.2 (26.8)	23.5 (31.7)	16.0 (25.6)
Big business lending, no-LMI banks	1.0 (6.4) 17135	6.5 (12.3) 258	0.9 (6.2) 16877	9.7 (18.9) 556	14.1 (14.1) 49	9.2 (19.3) 507
Non-CRA lending	87.6 (1554) 54380	930 (6023) 3442	30.7 (278) 50938	43.5 (45.6) 5219	56.8 (65.5) 837	41.0 (40.2) 4382
Total lending	236 (2648)	2212 (9900)	103 (762)	170 (123)	213 (165)	161 (112)
Bank characteristic						
"Big" bank	0.14 (0.35)	1.0 (0.0)	0.09 (28)	0.40 (0.49)	1.0 (0.0)	0.28 (0.45)
Tough regulator	0.30 (0.46)	1.0 (0.0)	0.25 (0.43)	0.43 (0.50)	1.0 (0.0)	0.32 (0.47)
FDIC	0.63 (0.48)	0.35 (0.48)	0.64 (0.48)	0.50 (0.50)	0.36 (0.48)	0.53 (0.50)
FED	0.07 (0.26)	0.17 (0.37)	0.07 (0.25)	0.08 (0.27)	0.15 (0.36)	0.06 (0.24)
OCC	0.30 (0.46)	0.49 (0.50)	0.29 (0.45)	0.42 (0.49)	0.49 (0.50)	0.42 (0.49)
p(M&A) 1996-99	0.42 (0.49)	0.77 (0.42)	0.39 (0.49)	0.58 (0.49)	0.69 (0.46)	0.56 (0.50)
Observations	54421	3445	50976	5226	840	4386

Means, standard deviations (in parentheses), and number of observations (where different from last row) from pooled 1993-1998 bank microdata from the June 30th Consolidated Reports of Condition and Income ("Call Reports"). The first three columns present results based on the "full sample" of commercial banks; the last three columns limit the analysis to a "regression discontinuity sample" of banks with 1993 assets between $150 million and $350 million. "Affected" banks are those for which Big_b*$ToughRegulator_r$ = 1. All lending variables are in millions of dollars, except for small business loan counts ("#"). The FDIC, FED, and OCC variables present the proportion of bank-year observations supervised by each of those three regulating agencies. "p(M&A)" captures the proportion of bank-year observations on banks that were involved in merger or acquisition activity during 1996-1999.

Table 2. Potential Borrower Summary Statistics

Measure of CRA incentives	**full sample**	**small shareholders sample**	**small assets sample**
All	0.49	0.51	0.48
	(0.50)	(0.50)	(0.50)
	240608	180439	135477
Big_c*$ToughRegulator_c$ = 1	0.48	0.50	0.47
	(0.50)	(0.50)	(0.50)
	179343	132649	100199
Big_c*$ToughRegulator_c$ = 0	0.58	0.59	0.57
	(0.49)	(0.49)	(0.49)
	37799	30328	23428
Big_c = 1	0.49	0.51	0.49
	(0.50)	(0.50)	(0.50)
	232749	174052	129674
Big_c = 0	0.60	0.62	0.60
	(0.49)	(0.48)	(0.49)
	7859	6387	5803
$ToughRegulator_c$ = 1	0.48	0.50	0.48
	(0.50)	(0.50)	(0.50)
	182306	135016	102352
$ToughRegulator_c$ = 0	0.57	0.59	0.57
	(0.49)	(0.49)	(0.50)
	34836	27961	21275
LMI_a = 1	0.53	0.56	0.52
	(0.50)	(0.50)	(0.50)
	57758	43711	31127
LMI_a = 0	0.49	0.51	0.48
	(0.50)	(0.50)	(0.50)
	173496	129914	99063

Mean, standard deviation, and number of observations for the probability that a firm-year observation holds any debt. Each column presents statistics for one of three samples constructed by pooling 1993, 1994, 1997, and 1998 IRS Statistics of Income Corporate Files. The first row presents statistics for all of the firms in a given sample. Each succeeding row presents means and standard deviations based on pre-reform observations for a sample split based on a different definition of CRA incentives; e.g., the Big_c*$ToughRegulator_c$= 1 row captures firms defined as "affected" when estimating equation (1).

Table 3. Effect of CRA on Debtholding by Potential Borrowers: Exploiting Variation in CRA Incentives Across Banks

Parameterization of CRA incentives	>=1 big banks, >=1 tough banks			Big bank deposit share, Deposit-weighted % of tough CRA grades		
sample	**Full sample**	**Small share-Holder**	**Small assets**	**Full Sample**	**Small share-Holder**	**Small assets**
measure of CRA incentives						
$Post_t*Big_c*ToughRegulator_c$	0.071	0.058	0.074	0.309	0.373	0.316
	(0.039)	(0.044)	(0.039)	(0.159)	(0.186)	(0.161)
	217142	162977	123627	137248	107318	81159
$Post_t*Big_c$	-0.022	-0.048	-0.022	0.009	-0.007	0.009
	(0.019)	(0.021)	(0.019)	(0.016)	(0.018)	(0.016)
	240608	180439	135477	240608	180439	135477
$Post_t*ToughRegulator_c$	0.023	0.020	0.023	0.037	0.027	0.038
	(0.011)	(0.013)	(0.011)	(0.044)	(0.048)	(0.044)
	217142	162977	123627	137248	107318	81159
IRS dependent variable mean (any debt)	0.480	0.502	0.473	0.496	0.517	0.490
IRS affected observations	179343	132649	100199			
$Post_t*Big_c*ToughRegulator_c$, for p(bank loan) in SSBF	0.121			0.358		
	(0.156)			(0.572)		
	7466			4917		
SSBF p(has bank loan)	0.388			0.388		

Each cell of results presents the coefficient, standard error (in parentheses), and number of observations for the estimated effect of CRA incentives on the financial structure of potential borrowers. The "$Post_t*Big_c*Tough\ Regulator_c$" row presents results for the coefficient of interest from weighted linear probability estimation of equation (1) on IRS data; the "$Post_t*Big_c$" and "$Post_t*ToughRegulator_c$" rows present analogous estimates of CRA effects based on equation (2). The dependent variable in each IRS case is the probability of holding any debt. Columns present results based on the three different samples used in the IRS data for each of two different parameterizations of CRA incentives. The first three columns present estimates based on the fully discrete parameterization of CRA incentives (where Big_c=1 and $ToughRegulator_c$=1 if there are one or more CRA big banks and one more banks with tough regulators located in the firm's county, respectively), for each of the three samples. The last three columns do the same for deposit-weighted measures of CRA bank size and regulator toughness. Coefficients in the first three columns can thus be multiplied by 100 to obtain estimates in percentage point terms; for the final three columns, a one standard deviation (0.26) increase in $Post_t*Big_c*ToughRegulator_c$ produces increases of eight to ten percentage points in the IRS data. IRS standard errors are corrected for correlation within counties (the locus of CRA incentives), but do not change if they are left uncorrected or are corrected for correlation within firms. The final two rows concern estimates of equation (1) where the dependent variable is the probability of having a loan from a commercial bank in the 1993 and 1998 Surveys of Small Business Finances (SSBF). SSBF standard errors are corrected for correlation within counties. All dependent variable means are weighted and based on firms in affected counties and pre-reform years.

Table 4. Effect of CRA on Potential Borrower Debtholding: Incorporating the LMI Incentive

	measure of CRA incentives	**Full sample**	**Small shareholders sub-sample**	**Small assets sub-sample**
1	$Post_t*LMI_a$	0.005 (0.010) 231254	0.003 (0.011) 173625	0.004 (0.010) 130190
2	$Post_t*Big_c*LMI_a$	0.054 (0.052) 231254	0.008 (0.057) 173625	0.054 (0.052) 130190
3	$Post_t*ToughRegulator_c*LMI_a$	-0.018 (0.030) 208476	-0.032 (0.030) 156630	-0.019 (0.031) 118641
4	$Post_t*Big_c*ToughRegulator_c$ LMI firms only	-0.010 (0.093) 51401	-0.025 (0.097) 38968	-0.010 (0.094) 27979
5	$Post_t*Big_c*ToughRegulator_c$ non-LMI firms	0.085 (0.045) 157075	0.081 (0.051) 117662	0.088 (0.045) 90662

Each cell presents the coefficient, standard error, and number of observations in the estimation sample for the coefficient of interest from a weighted linear probability estimate of equation (2) (row 1), equation (1) with low- and moderate-income (LMI) status as one of the CRA incentive components (rows 2 and 3), or equation (1) with the sample split by LMI status (rows 4 and 5). LMI_a = 1 if the firm is located in a LMI census tract. The coefficients presented thus estimate the impact of CRA incentives, including some measure of CRA's LMI incentive, on the probability that a firm holds any debt in the IRS data. Coefficients can be multiplied by 100 to obtain estimates in percentage point terms. Results do not change if alternative, smooth measures of Big_c and $ToughRegulator_c$ are used.

Table 5. Effects of CRA on Bank Lending

	Full Sample			Regression Discontinuity Sample		
measure of CRA incentives	$Post_t*Big_b*ToughRegulator_r$	$Post_t*Big_b$	$Post_t*Tough\ Regulator_r$	$Post_t*Big_b*ToughRegulator_r$	$Post_t*Big_b$	$Post_t*Tough\ Regulator_r$
Loan type						
# small business	109 (331) 54421	437 (37) 55933	73 (50) 54421	220 (66) 5226	31 (18) 5412	43 (40) 5226
$ small business	21.6 (5.5) 54421	38.2 (1.2) 55933	6.6 (2.3) 54421	7.5 (3.3) 5226	1.8 (0.8) 5412	0.8 (1.9) 5226
Big business	64.9 (28.6) 54421	64.0 (3.7) 55933	4.8 (5.9) 54421	-0.4 (2.4) 5226	-0.5 (0.8) 5412	0.9 (1.1) 5226
Big business, no-LMI banks	0.02 (1.3) 17135	0.1 (0.2) 17375	0.7 (0.3) 17135	2.0 (3.1) 556	-2.5 (1.7) 578	-0.2 (1.5) 556
Home mortgage	81.6 (18.2) 54421	82.6 (3.3) 55933	13.2 (7.2) 54421	1.7 (5.6) 5226	-2.6 (1.4) 5412	3.8 (1.8) 5226
Non-CRA	-74.2 (37.5) 54380	-41.6 (5.5) 55891	-11.1 (11.2) 54380	-0.8 (6.0) 5219	6.7 (1.0) 5404	-0.2 (2.9) 5219
Total lending	97.8 (22.2) 54421	146 (8.6) 55933	14.2 (4.9) 54421	7.4 (7.7) 5226	5.4 (1.8) 5412	5.2 (5.1) 5226

Each cell presents the coefficient, standard error (in parentheses), and number of observations for the estimated effect of CRA incentives on bank lending. Read across a row for effects on the listed lending type. The first three columns present results based on the "full sample" of commercial banks from pooled 1993-1998 June 30th Call Reports; the last three columns limit the analysis to a "regression discontinuity sample" of banks with 1993 assets between $150 million and $350 million. The "$Post_t*Big_b*ToughRegulator_r$" columns present results for this variable from OLS estimation of equation (3); the other columns present analogous estimates of CRA effects based on equation (2). Coefficients and standard errors are in millions of dollars, except for small business loan counts ("#"). Standard errors are corrected for correlation within regulator-regions.

Table 6. Effects of CRA on Bank Performance and Consolidation

	Full Sample			Regression Discontinuity Sample		
Outcome	CRA effect	CRA effect	Dependent Variable	CRA effect	CRA effect	Dependent Variable
Profits		1.9	14.9		0.6	1.6
		(1.3)	(58.9)		(1.8)	(2.7)
Return on equity	0.010	0.012	0.06	0.006	0.023	0.03
	(0.009)	(0.009)	(0.22)	(0.011)	(0.030)	(0.43)
Return on assets	-0.00009	-0.0007	0.006	-0.0011	-0.0022	0.006
	(0.00090)	(0.0009)	(0.010)	(0.0014)	(0.0027)	(0.010)
Loans not accruing	-0.0025	-0.0026	0.01	-0.0044	-0.0051	0.01
	(0.0017)	(0.0017)	(0.03)	(0.0022)	(0.0026)	(0.02)
90-day late	0.0004	0.0003	0.003	0.0010	0.0009	0.003
	(0.0006)	(0.0006)	(0.005)	(0.0011)	(0.0011)	(0.006)
Loan loss provisions	0.0001	0.0001	0.002	-0.00063	-0.00091	0.003
	(0.0003)	(0.0004)	(0.009)	(0.00052)	(0.00061)	(0.007)
Loan loss allowances	0.0011	0.0012	0.02	-0.00086	-0.00020	0.02
	(0.0016)	(0.0016)	(0.02)	(0.00079)	(0.00012)	(0.02)
Charge-offs	-0.0003	-0.0004	0.003	-0.00110	-0.00180	0.004
	(0.0004)	(0.0004)	(0.005)	(0.00064)	(0.00085)	(0.007)
Merger or Acquisition	-0.074	-0.079	0.78	-0.043	-0.040	0.71
	(0.054)	(0.055)	(0.41)	(0.079)	(0.078)	(0.46)
Merger or Acquisition Survivor	-0.061	-0.071	0.55	-0.044	-0.045	0.49
	(0.055)	(0.056)	(0.50)	(0.077)	(0.077)	(0.50)
Asset controls?	N	Y		N	Y	
N profitability	52936	52936	52936	5002	5002	5002
N bad loans	52897	52897	52897	4998	4998	4998
N Merger or Acquisition	10722	10722	10722	1122	1122	1122

Each results cell presents an OLS estimate of CRA's effect on a measure of bank performance or behavior from equation (3). "Dependent variable" cells present the mean and standard deviation of the outcome for affected banks in the pre-reform period. "Profitability" outcomes are profits, return on equity, and return on assets. Profits are in millions of dollars. "Bad loan" measures include "loans not accruing" through "charge-offs". These are scaled by total lending. All profit and bad loan estimates are for $Post_t*Big_b*ToughRegulator_r$ and are based on a panel of pooled 1993-1999 Call Reports, dropping 1996 as a transition year (in terms of its effects on bank outcomes). The "Merger or Acquisition" rows present effects on the probability that a bank is involved in a merger or acquisition post-reform, or survives a merger or acquisition post-reform, respectively. These are estimated using a cross-section version of equation (3), with one observation per bank, where $Big_b*ToughRegulator_r$ estimates the effect of CRA. Results for all outcomes except profits are presented both with and without the X's ("asset controls") in equation (3), since the scaled nature of these outcomes may obviate the need for these heterogeneity controls.

Table 7. County Real Activity Summary Statistics, 1993-1998

	Full sample	LMI counties	Non-LMI counties	Affected counties	Unaffected counties	1993-1995 affected counties
Employment	32658 [6114] (122066)	51070 [10363] (156473)	6827 [3525] (17705)	47822 [10211] (149482)	21440 [4230] (95388)	45967 [9814] (144625)
Payroll	899 [119] (4103)	1430 [210] (5284)	153 [68] (562)	1352 [209] (5183)	563 [79] (3024)	1216 [191] (4614)
Business bankruptcies	17 [4] (72)	26 [6] (93)	4 [2] (7)	26 [6] (99)	10 [3] (41)	27 [6] (102)
Nonbusiness bankruptcies	334 [69] (1380)	523 [122] (1780)	70 [35] (121)	488 [115] (1777)	221 [47] (975)	373 [85] (1300)
Observations	18570	10842	7728	7896	10674	3948

Cells show mean, [median], and (standard deviation) for mid-March employment and annual payroll from County Business Patterns, and bankruptcy counts from Administrative Office of the United States Courts. Payroll is in millions of dollars. "Affected" counties are those for which $Big_c * ToughRegulator_c = 1$. "LMI counties" have one or more LMI census tracts within their boundaries; "no-LMI counties" have no LMI tracts (and are almost exclusively rural counties).

Table 8. Effects of CRA on Real Activity

Measure of CRA Incentives	$Post_t*Big_c*$ $ToughRegulator_c$			$Post_t*Big_c$			$Post_t*ToughRegulator_c$		
Sample	**Full sample**	**LMI counties**	**no-LMI counties**	**Full sample**	**LMI counties**	**no-LMI counties**	**Full sample**	**LMI counties**	**no-LMI counties**
Outcome									
Employment	0.40	-0.82	1.7	0.67	1.3	-0.09	0.15	0.46	-0.31
	(0.57)	(0.72)	(1.0)	(0.25)	(0.3)	(0.40)	(0.25)	(0.32)	(0.41)
	17838	10386	7452	18570	10842	7728	17838	10386	7452
Payroll	0.95	-0.54	2.7	0.90	1.3	0.48	-0.11	0.20	-0.51
	(0.65)	(0.83)	(1.1)	(0.29)	(0.4)	(0.47)	(0.29)	(0.36)	(0.48)
	17838	10386	7452	18570	10842	7728	17838	10386	7452
Business Bankruptcies	-5.2	-3.5	-5.7	-0.5	-5.2	6.1	-3.9	-5.3	-1.7
	(3.9)	(4.8)	(6.5)	(1.7)	(2.1)	(2.9)	(1.7)	(2.1)	(2.9)
	15327	9371	5956	15961	9779	6182	15327	9371	5956
Personal bankruptcies	-3.6	-5.6	-0.3	-2.1	-3.9	-0.1	-0.21	-1.2	0.9
	(1.8)	(2.2)	(3.2)	(0.8)	(1.0)	(1.4)	(0.80)	(1.0)	(1.4)
	17670	10309	7361	18394	10760	7634	17670	10309	7361

Each cell presents the coefficient, standard error, and number of observations for the estimated effect of CRA incentives on a measure of county-level real activity. All outcomes are in logs; coefficients and standard errors have been multiplied by 100 to obtain an estimate of the percentage change in the outcome in CRA-affected counties, relative to CRA-unaffected counties. Read across a row for effects on the listed outcome. The three "$Post_t*Big_c*ToughRegulator_c$" columns present results for this variable from OLS estimation of a real activity version of equation (1) on a 1993-98 panel of counties; the other columns present analogous estimates of CRA effects based on equation (2). "LMI counties" refers to the subset of counties that have one or more LMI census tracts within their boundaries; "no-LMI counties" have no LMI tracts (and are almost exclusively rural counties).

Appendix. Estimating Aggregate Borrowing and Lending Increases

A. Overview

One can use either the IRS results or bank lending results to estimate aggregate lending increases due to CRA, and each method has its advantages and disadvantages. The IRS models of net borrowing increases come closer to capturing the underlying structural relationship of interest, the effect of (access to) credit on real activity, than the bank estimates of gross lending increases. However, translating the IRS results (which capture borrowing participation) into dollars requires several additional assumptions about the nature of CRA borrowing (these are detailed below). Using the bank lending results entails fewer assumptions, but still requires critical decisions about which type(s) of lending to scale by (e.g., small business or total-- see the related discussion of private vs. social returns on p. 31) and which specification(s) to believe (e.g., full sample or regression discontinuity sample).

B. Using the IRS Results

The results in Table 3 imply that perhaps 8.5% of firms in affected counties obtain new loans where CRA binds. We know that affected counties account for approximately 62% of pre-treatment national employment, so assume that they account for 62% of firms (there are no publicly available firm counts at the county level, but at the MSA level the correlation between the proportion of national firms and the proportion of national employment is 0.987.) The 1998 SSBF's sampling frame provides an estimate of the relevant firm population, 5.3 million (the 1998 IRS universe of corporate returns is also 5.3 million). This implies that perhaps 0.085*0.62*5,300,000= 279,000 firms begin holding debt as the result of CRA. But how much do these marginal firms borrow? The median size of the most recent loan for 1998 SSBF

borrowers is $50,000. If 279,000 firms borrow $50,000, then CRA induces $14 billion in new loans. Of course firms don't actually finance the full loan amount— term loans are amortized and lines of credit revolve. The latter type seems more likely to be the marginal loan (since over half of bank business loans are lines of credit as it is, and this type of lending is typically uncollateralized and information-intensive), so adjusting for the fact the firms have drawn only 1/3 of their credit lines at any point in time (0.333 median, 0.39 mean in the 1998 SSBF) implies that new CRA borrowers finance perhaps $5 billion. Note that this estimate is biased downward to the extent that: a) CRA induces term loans as well; and b) CRA not only produces new borrowers but also increases borrowing by those that had some debt *ex-ante*.

C. Using the Bank Lending Results

Analogous calculations can be performed using the bank lending results by simply multiplying the estimated effect of CRA incentives on dollar volume outstanding, for the chosen lending type (Table 5), by the 533 affected banks operating in the post-reform period. This method produces estimated small business lending increases of $4.0 billion (using the regression discontinuity result) and $11.5 billion (using the full sample result).