The Real Effects of Liquidity on Behavior: Evidence from Regulation and Deregulation of Credit Markets

by

Jonathan Zinman
Massachusetts Institute of Technology

for

SBA
Office of Advocacy

under contract number SBAHQ-01-M-0152

Release Date: November 8, 2002

The Real Effects of Liquidity on Behavior: Evidence from Regulation and Deregulation of Credit Markets

by

Jonathan Zinman
B.A. Government
Harvard College, 1993

SUBMITTED TO THE DEPARTMENT OF ECONOMICS IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY IN ECONOMICS
AT THE
MASSACHUSETTS INSTITUTE OF TECHNOLOGY

ACCEPTED SEPTEMBER 2002

Signature of Author: ____

Department of Economics
July 31, 2002

Certified by: ____

Jonathan Gruber
Professor of Economics
Thesis Supervisor

Accepted by: ____

Peter Temin
Elisha Gray II Professor of Economics
Chairman, Departmental Committee on Graduate Studies

The Real Effects of Liquidity on Behavior: Evidence from Regulation and Deregulation of Credit Markets

by

Jonathan Zinman

Submitted to the Department of Economics in partial fulfillment of the requirements for the Degree of Doctor of Philosophy in Economics

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. Chapters 1 and 2 develop 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, Chapter 1 develops evidence that CRA does increase credit to small businesses as intended. Chapter 2 then exploits 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, although important questions remain.

Despite a growing number of studies concluding that a substantial proportion of US households are liquidity constrained, there remains little consensus as to the quantitative importance or nature of these constraints. This paper develops a new type of evidence on the impacts of consumer credit markets on behavior by examining household-level responses to an exogenous liquidity shock. A United States Supreme Court decision effectively deregulated bank credit card interest rates in December 1978, and I develop evidence that consumers from states with binding usury ceilings before the decision became more likely to hold bank cards after the decision, relative to their counterparts in unaffected states. The marginal cardholders appear to have characteristics widely associated with credit constraints, and to borrow frequently on their new cards. Yet there is little evidence that these cardholders exploit their newfound liquidity by shifting into higher-yielding, less liquid, or riskier assets. This finding is at odds with most models of liquidity constraints, and motivates consideration of alternative explanations for the widely observed sensitivity of consumers to liquidity.

Thesis Supervisor: Jonathan Gruber
Title: Professor of Economics

General Acknowledgements

I thank my parents for instilling a passion for learning and work, my advisors Jon Gruber and Daron Acemoglu for teaching me how to channel that passion, other MIT faculty members to numerous to mention for their accessibility and guidance, my classmates for their ides, acumen, and friendship, the Department of Economics support staff for amiable and professional service, friends and family who gracefully accepted my prolonged absences, my son Elias for inspiration and joy, and my wife Barbara for tolerating this very lumpy investment.

Chapters 1 and 2

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

Jonathan Zinman*
Massachusetts Institute of Technology
July 19, 2002

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. Chapters 1 and 2 develop 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, Chapter 1 develops evidence that CRA does increase credit to small businesses as intended. Chapter 2 then exploits 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, although important questions remain.

* Jonathan.Zinman@ny.frb.org; tel: 212-720-1204. I am particularly grateful to Daron Acemoglu and Jon Gruber for their guidance. The author also thanks Josh Angrist, Adam Ashcraft, Abhijit Banerjee, Olivier Blanchard, Ricardo Caballero, Susan Dynarski, Amy Finkelstein, Jon Guryan, Raghu Rajan, Jim Poterba, Melissa Schettini Kearney, participants in the Development, Macro, and Public Finance Lunches at MIT, and seminar participants at MIT and the Board of Governors of the Federal Reserve for helpful comments. Thanks also to David Seif for assistance with compiling the bankruptcy data, Nancy Andrews of the Chicago Fed for help accessing and interpreting banking data, Kenneth Thomas for his work and counsel on CRA enforcement practices, and George Plesko for his extraordinary efforts to facilitate access to restricted IRS data. The Social Science Research Council's Program in Applied Economics (Pre-Dissertation Fellowship), National Bureau of Economic Research (Nonprofit Dissertation Fellowship), Small Business Administration Office of Advocacy (Dissertation Grant), Harvard Joint Center for Housing Studies, and MIT's George and O'bie Schultz Fund have provided generous and timely financial support for this research.

Chapter 1. The Efficacy of Credit Market Interventions: Evidence from the Community Reinvestment Act

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 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 (see Chapter 2).

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.

---

[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).

[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.

These first two chapters exploit 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, Chapter 1 presents evidence that regulation can allocate credit; namely, it appears that CRA does increase lending to small businesses as intended. Chapter 2 then exploits these CRA-induced supply shocks to identify evidence on 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, although important questions remain.

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).

Chapter 1 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 reduced form 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. Tellingly, non-CRA lending does not appear to increase. Section V summarizes the results on the efficacy of CRA.

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;[6] 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.[7] 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.[8] 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

---

[6] 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.

[7] 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.

[8] 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.[9] 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 "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

[9] 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.

development"-- and a concern that consolidation in the banking industry was tightening credit availability to small businesses generally.[10] The new lending tests made clear reference to the importance of small business lending within and outside of LMI areas, and political will backed 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).[11] 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.

---

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

[11] 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.

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.[12] 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 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.

[12] 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.

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.

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.[13] This motivates the following model:

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

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 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-

[13] 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.

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).[14] 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 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.

---

[14] 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.

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.[15] 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.[16] A regulator-region is labeled "tough" if it 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 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

---

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

[16] 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 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.

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.[17]

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 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.[18] 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.

[17] 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.

[18] 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.

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.

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, or more particularly the CRA reforms, successfully targets firms in LMI areas.

IV. The Effects of CRA on Bank Lending

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.[19]

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

[19] 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).

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.

V. Conclusion

This chapter establishes that 1995 reforms to the Community Reinvestment Act (CRA) provided a source of plausibly exogenous variation in bank credit supply, and then estimates whether CRA has had its intended effects on credit markets using data on both banks and potential borrowers. The data sources are complementary. The data on banks permits estimation of bank responses to CRA incentives, and is sufficiently rich to construct falsification tests (and tests for distortions, as in Chapter 2). Data on potential borrowers provides an estimate of the net effect on small business balance sheets, and is relatively free of the functional form issues that plague bank data.

Reduced-form estimates from both data sources suggest that the CRA reforms delivered their intended effect of increasing credit to small businesses. The results on banks provide some evidence that banks affected most by the CRA reforms did increase their small business lending, on the order of 12 to 15 percent. 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. The results on potential borrowers allay concerns that the bank findings might be driven by functional form assumptions and moreover suggest that any CRA-induced lending increases did not (completely) crowd-out other sources of financing, as the number of firms holding *any* debt increased by seven to ten percentage points in areas served by affected banks. This suggests that CRA increased access to credit for approximately five percent of firms overall. The next chapter explores whether this increase in credit had macroeconomic implications by estimating the effects of CRA and its associated credit flows on county-level economic activity.

References

Banerjee, Abhijit; Duflo, Esther (2001). "The Nature of Credit Constraints: Evidence from an Indian Bank", mimeo, MIT.

Barefoot, Jo Ann S. (1998). "Caught Between Two Worlds", *ABA Banking Journal*, 90, no. 3 (March), 32-37.

Belsky, Eric S.; Lambert, Matthew; von Hoffman, Alexander (2000). "Insights into the Practice of Community Reinvestment Lending", Joint Center for Housing Studies, Harvard University.

Berger, Allen N.; Udell, Gregory F. (1992). "Some Evidence on the Empirical Significance of Credit Rationing", *Journal of Political Economy,* 100(5), 1047-1077.

Black, Fischer (1975). "Bank Funds Management in an Efficient Market", *Journal of Financial Economics*, v2, 323-39.

Blanchflower, David G; Levine, Phillip B.; Zimmerman, David J. (1998). "Discrimination in the Small Business Credit Market", NBER Working Paper No. W6840

Blanchflower, David G.; Oswald, Andrew J. (1998). "What Makes an Entrepreneur?", *Journal of Labor Economics* v16, n1 (January 1998): 26-60

Broome, J Tol, Jr. (1996). "Less Paperwork, More Bank Loans", *Nations Business*, 84, n9, 38.

Cocheo, Steve (1996). "Performance-Based CRA, Round One", *ABA Banking Journal,* Vol. 88, No. 6 (June), 41-44.

Cole, R., Wolken J.D., Woodburn, R.L., (1996). "Bank and nonbank competition for small business credit: Evidence from the 1987 and 1993 National Survey of Small Business Finances", *Federal Reserve Bulletin* 82 (11), 983-995.

Evans, David S.; Jovanavic, Boyan (1989). "An Estimated Model of Entrepreneurial Choice under Liquidity Constraints", *Journal of Political Economy*, 97(4), 808-827.

Fama, Eugene F. (1985). "What's Different About Banks?", *Journal of Monetary Economics*, 15, 29-40.

Fazzari, Steven M.; Hubbard, R. Glenn; Petersen, Bruce C. (2000). "Investment Cash Flow Sensitivities Are Useful: A Comment on Kaplan and Zingales", Quarterly Journal of Economics, May, 695-705.

Gale, William (1991). "Economic Effects of Federal Credit Programs", *American Economic Review*, Vol. 81, no. 1, 133-152.

General Accounting Office (1995). "Community Reinvestment Act: Challenges Remain to Successfully Implement CRA", November, GAO/GGD-96-23.

Gramlich, Edward M. (1999). "A Policy in Lampman's Tradition: The Community Reinvestment Act", *Focus*, (University of Wisconsin-Madison Institute for Research on Poverty), 20(3), Fall 1999, 11-14.

Haag, Susan White (2000). "Community Reinvestment and the Cities: A Literature Review of CRA's Impact and Future", Brookings Institution Center on Urban and Metropolitan Policy Discussion Paper, March 2000.

Hart, Oliver; Moore, John (1994). "A Theory of Debt Based on the Inalienability of Human Capital", *Quarterly Journal of Economics*, v109 (4), 841-79.

Holtz-Eakin, Douglas; Joulfaian, David; Rosen, Harvey S. (1994). "Sticking It Out: Entrepreneurial Survival and Liquidity Constraints." *Journal of Political Economy.* Vol. 102 (1). p 53-75.

Hubbard, R Glenn (1998). "Capital-Market Imperfections and Investment." *Journal of Economic Literature*. Vol. 36 (1). p 193-225.

Immergluck, Dan (1997). "New Small Business Data Show Loans Going to Higher-Income Neighborhoods in Chicago Area", *Woodstock Institute Reinvestment Alert*, no. 11, November.

Jayaratne, Jith; Strahan, Philip E. (1996). "The Finance-Growth Nexus: Evidence from Bank Branch Deregulation." *The Quarterly Journal of Economics*, Vol. 111 (3), 639-70.

Johnson, Shane A.; Sarkar, Salil K. (1996). "The Valuation Effects of the 1977 Community Reinvestment Act and its Enforcement", *Journal of Banking & Finance*, 20, 783-803.

Kaplan, Steven N.; Zingales, Luigi (1997). "Do Financing Constraints Explain Why Investment is Correlated with Cash Flow?", *Quarterly Journal of Economics*, February, 169-215.

Kaplan, Steven N.; Zingales, Luigi (2000). "Investment-Cash Flow Sensitivities Are Not Valid Measures of Financing Constraints", *Quarterly Journal of Economics*, May, 707-712.

Lamont, Owen (1997). "Cash Flow and Investment: Evidence from Internal Capital Markets", *Journal of Finance*, Vol. 52, No. 1, pp. 57-82.

Lindsey, Lawrence (1995). "Statement before the Subcommittee on Financial Institutions and Consumer Credit of the Committee on Banking and Financial Services, U.S. House of Representatives, March 8, 1995", *Federal Reserve Bulletin*, May, 424-430.

Litan E. Robert; Retsinas Nicolas P.; Belsky, Eric S.; Haag, Susan White (2000). "The Community Reinvestment Act After Financial Modernization: A Baseline Report.", Brookings Institution.

Matasar, Ann B., and Pavelka, Deborah D (1998). "Federal Banking Regulators' Competition in Laxity: Evidence from CRA Audits", *International Advances in Economic Research*, 4(1), 56-69.

Mester, Loretta J.; Nakamura, Leonard I.; Renault, Micheline (2001). "Checking Accounts and Bank Monitoring", Federal Reserve Bank of Philadelphia Research Working Paper: 01-3.

Modigliani, Franco and Miller, Merton H. (1958). "The Cost of Capital, Corporation Finance, and the Theory of Investment", *American Economic Review*, 48(3), 261-97.

Munnell, Alicia H.; Tootell Geoffrey M. B.; Browne, Lynn E.; McEaney, James (1996). "Mortgage Lending in Boston: Interpreting the HMDA Data", *American Economic Review*, LXXXVI, 25-53.

National Community Reinvestment Coalition, *CRA Commitments: 1977-1999*. (Washington: NCRC), 2000.

Petersen, Mitchell A.; Rajan Raghuram G. (1994). "The Benefits of Lending Relationships: Evidence from Small Business Data", *Journal of Finance*, vol. XLIX, no. 1, 3-37.

Petersen, Mitchell A.; Rajan Raghuram G. (1995), "The Effect of Credit Market Competition on Lending Relationships", *Quarterly Journal of Economics*, vol. 110, No. 2., pp. 407-443.

Rajan, Raghuram G; Zingales, Luigi (1998). "Financial Dependence and Growth." *American Economic Review*. Vol. 88 (3). p 559-86.

Robinson, Joan. *The Rate of Interest and Other Essays* (London: Macmillan, 1952).

Schumpeter, Joseph. *The Theory of Economic Development* (Oxford: Oxford University Press, 1969).

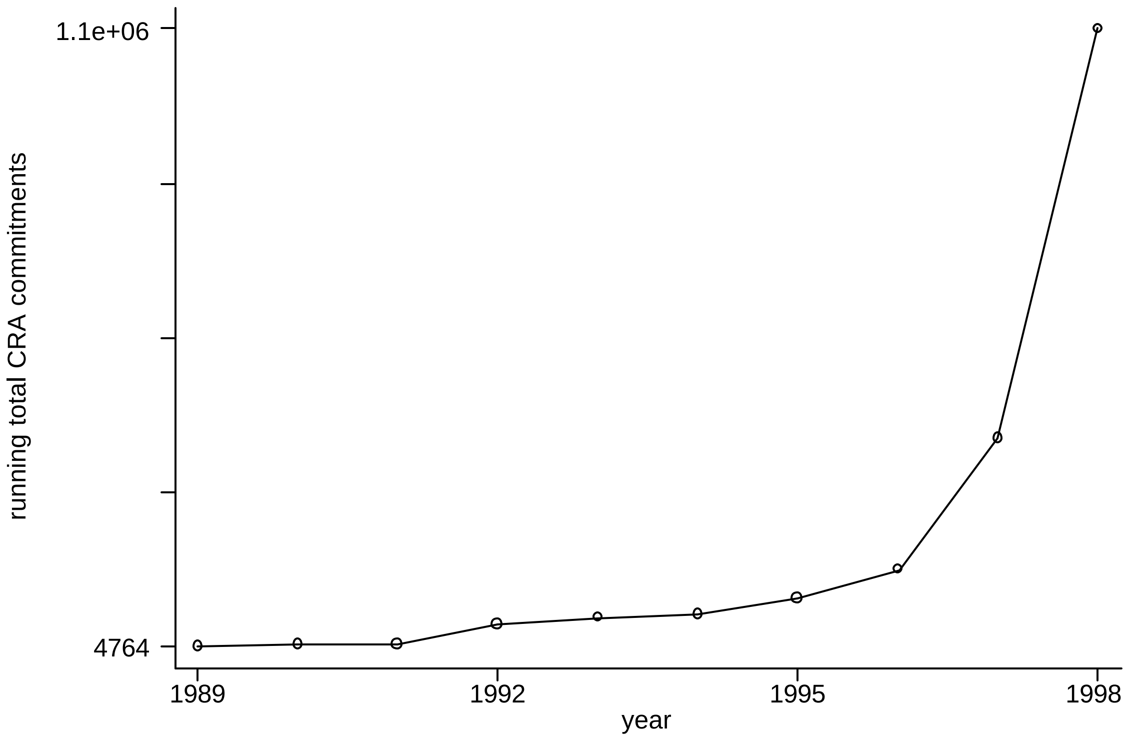
Stiglitz, Joseph E.; Weiss, Andrew (1981). "Credit Rationing in Markets with Imperfect Information", *American Economic Review* 71(3), June, 393-410.

Strahan, Phillip E.; Weston, James P. (1998). "Small Business Lending and the Changing Structure of the Banking Industry", *Journal of Banking and Finance*, 22, 821-845.

Thomas, Kenneth, *The CRA Handbook* (McGraw-Hill), 1998.

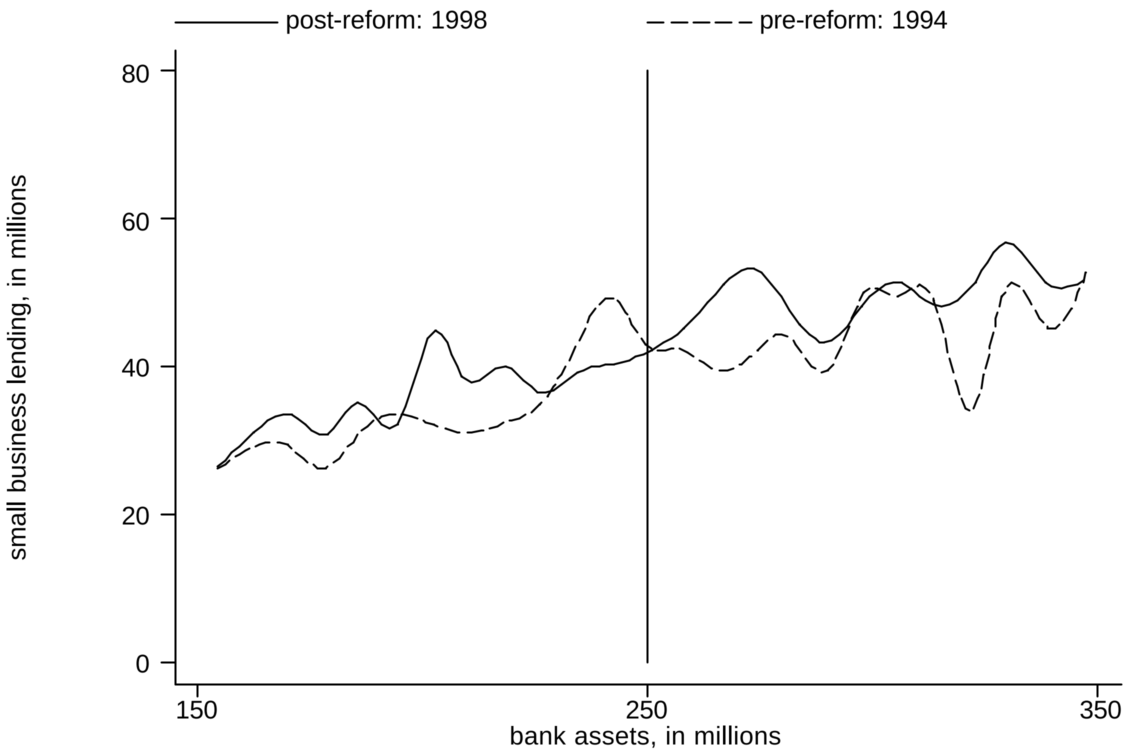
Tootell, Geoffery M. B. (1996). "Redlining in Boston: Do Mortgage Lenders Discriminate Against Neighborhoods?", *Quarterly Journal of Economics*, November, 1049-1079.

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

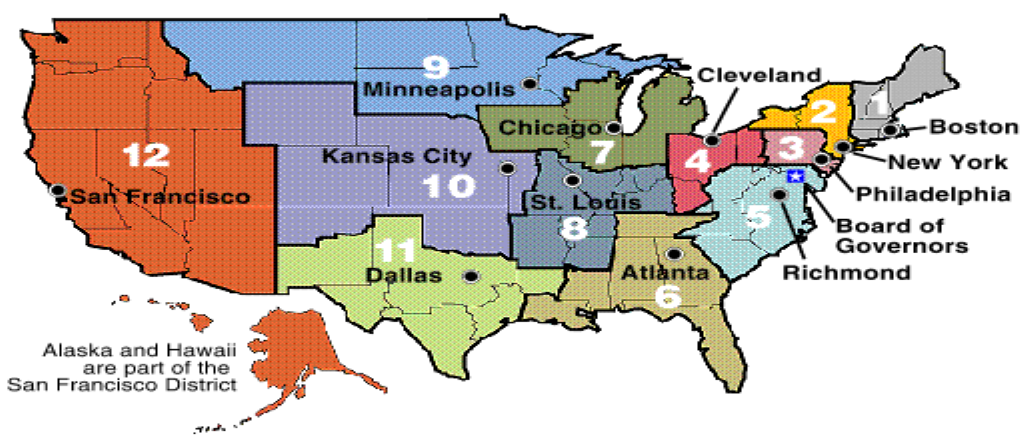
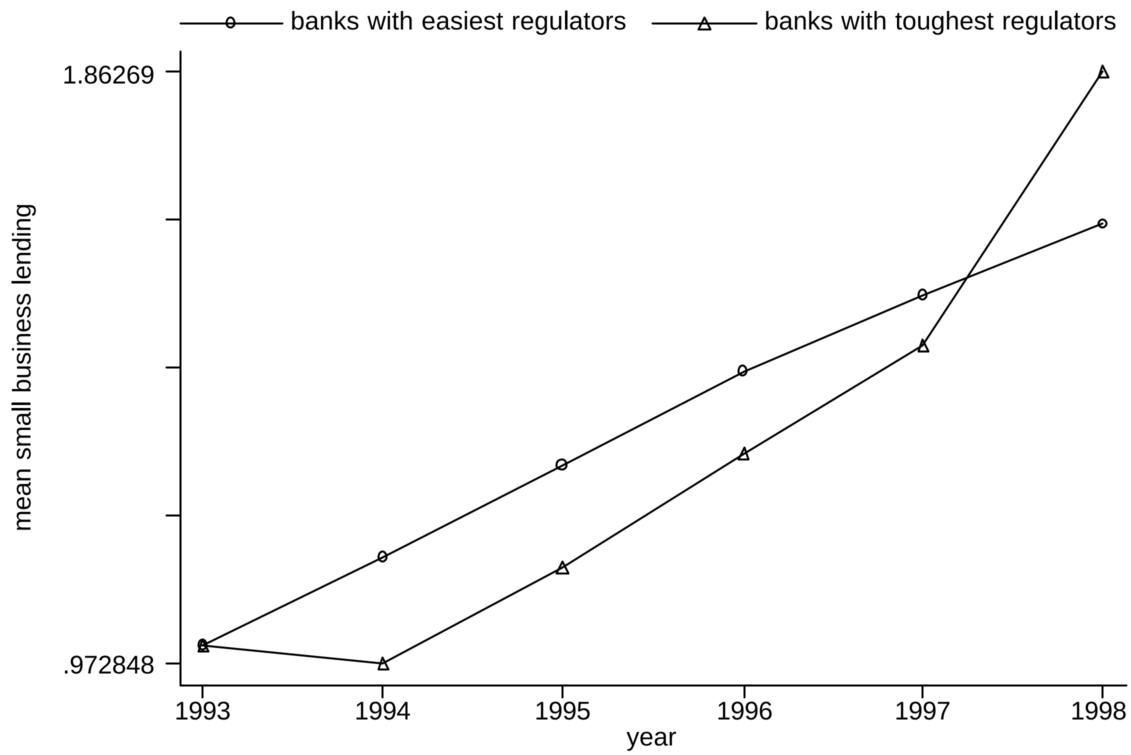


Figure 4. Small Business Lending:
Banks with Tough CRA Regulators vs. Banks with Easy CRA Regulators



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	3086	392	726	755	720
	(3582)	(12297)	(1738)	(588)	(684)	(568)
Small business $	32.4	193	21.6	47.1	52.1	46.1
	(156)	(508)	(82.0)	(31.1)	(35.4)	(30.2)
Home mortgage	62.7	553	29.6	58.1	77.6	54.4
	(650)	(2404)	(206)	(62.5)	(83.1)	(57.0)
Big business lending	49.4	524	17.4	17.2	23.5	16.0
	(615)	(2194)	(250)	(26.8)	(31.7)	(25.6)
Big business lending, no-LMI banks	1.0	6.5	0.9	9.7	14.1	9.2
	(6.4)	(12.3)	(6.2)	(18.9)	(14.1)	(19.3)
	17135	258	16877	556	49	507
Non-CRA lending	87.6	930	30.7	43.5	56.8	41.0
	(1554)	(6023)	(278)	(45.6)	(65.5)	(40.2)
	54380	3442	50938	5219	837	4382
Total lending	236	2212	103	170	213	161
	(2648)	(9900)	(762)	(123)	(165)	(112)
Bank <u>characteristic</u>						
"Big" bank	0.14	1.0	0.09	0.40	1.0	0.28
	(0.35)	(0.0)	(28)	(0.49)	(0.0)	(0.45)
Tough regulator	0.30	1.0	0.25	0.43	1.0	0.32
	(0.46)	(0.0)	(0.43)	(0.50)	(0.0)	(0.47)
FDIC	0.63	0.35	0.64	0.50	0.36	0.53
	(0.48)	(0.48)	(0.48)	(0.50)	(0.48)	(0.50)
FED	0.07	0.17	0.07	0.08	0.15	0.06
	(0.26)	(0.37)	(0.25)	(0.27)	(0.36)	(0.24)
OCC	0.30	0.49	0.29	0.42	0.49	0.42
	(0.46)	(0.50)	(0.45)	(0.49)	(0.50)	(0.49)
p(M&A) 1996-99	0.42	0.77	0.39	0.58	0.69	0.56
	(0.49)	(0.42)	(0.49)	(0.49)	(0.46)	(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 (0.50) 240608	0.51 (0.50) 180439	0.48 (0.50) 135477
Big_c*$ToughRegulator_c = 1$	0.48 (0.50) 179343	0.50 (0.50) 132649	0.47 (0.50) 100199
Big_c*$ToughRegulator_c = 0$	0.58 (0.49) 37799	0.59 (0.49) 30328	0.57 (0.49) 23428
$Big_c = 1$	0.49 (0.50) 232749	0.51 (0.50) 174052	0.49 (0.50) 129674
$Big_c = 0$	0.60 (0.49) 7859	0.62 (0.48) 6387	0.60 (0.49) 5803
$ToughRegulator_c = 1$	0.48 (0.50) 182306	0.50 (0.50) 135016	0.48 (0.50) 102352
$ToughRegulator_c = 0$	0.57 (0.49) 34836	0.59 (0.49) 27961	0.57 (0.50) 21275
$LMI_a = 1$	0.53 (0.50) 57758	0.56 (0.50) 43711	0.52 (0.50) 31127
$LMI_a = 0$	0.49 (0.50) 173496	0.51 (0.50) 129914	0.48 (0.50) 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			
$Post_t*Big_c*ToughRegulator_c$,	0.121			0.358		
	(0.156)			(0.572)		
for p(bank loan) in SSBF	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	0.003	0.004
		(0.010)	(0.011)	(0.010)
		231254	173625	130190
2	$Post_t$*Big_c* LMI_a	0.054	0.008	0.054
		(0.052)	(0.057)	(0.052)
		231254	173625	130190
3	$Post_t$*$ToughRegulator_c$*LMI_a	-0.018	-0.032	-0.019
		(0.030)	(0.030)	(0.031)
		208476	156630	118641
4	$Post_t$*Big_c*$ToughRegulator_c$ LMI firms only	-0.010	-0.025	-0.010
		(0.093)	(0.097)	(0.094)
		51401	38968	27979
5	$Post_t$*Big_c*$ToughRegulator_c$ non-LMI firms	0.085	0.081	0.088
		(0.045)	(0.051)	(0.045)
		157075	117662	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	$Post_t*Big_b*ToughRegulator_r$	$Post_t*Big_b$	$Post_t*ToughRegulator_r$	$Post_t*Big_b*ToughRegulator_r$	$Post_t*Big_b$	$Post_t*ToughRegulator_r$
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.

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).

Chapter 2. The Real Effects of Credit Market Interventions: Evidence from the Community Reinvestment Act

I. Introduction

This section tests whether the CRA-induced increases in credit observed in Chapter 1 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.

This chapter is similar in spirit to Peek and Rosengren's (2000) work on the international transmission of Japanese banking sector shocks to U.S. borrowers, and to the "credit crunch" literature on bank responses to events surrounding the recession of the late 1980s and early 1990s (e.g., Sharpe 1995, Bernanke and Lown 1991). Both cases find some evidence that regulation-induced *negative* supply shocks to bank lending *decrease* output. There may be an important asymmetry, however. Forcing banks to *increase* lending-- as CRA apparently does-- will not *increase* output if banks produce a (constrained) Pareto optimum when left to their own devices (see footnote 5, Chapter 1).

The findings in this chapter (and the previous one) also provide evidence on the plausibility of the lending channel as a transmission mechanism for monetary policy. The existence of a such a channel— whereby effects of monetary policy on bank lending supply impact bank-dependent borrowers-- remains controversial.[20] Three conditions must hold for a lending channel to exist. First, of course, monetary policy must induce changes in bank credit supply (see, e.g., Ashcraft 2001). Second, bank and nonbank finance must be imperfect substitutes for some borrowers (see, e.g., Hadlock and James, 2002). Chapter 1 provides a new type of evidence that this condition does in fact hold, as it finds that policy-induced changes in bank lending do indeed produce equilibrium changes in net access to financing. Third, changes in financial market equilibria must have real effects. Whether this final condition holds is the subject of this chapter.

The chapter proceeds as follows. Section II describes the data and model used to estimate the reduced form effects of CRA-induced lending and borrowing on county-level real activity. Section III presents the results. It 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. Section IV develops evidence suggesting that any real benefits do not appear to be offset by costs associated with distorting bank lending decisions. The findings in Section III do not appear to be driven by redistribution across counties, and there is little evidence of adverse effects on bank profits or loan quality. Section V scales the reduced form estimates of Section III by estimates of the total credit increase implied by the results from Chapter 1 to roughly calculate the effects of bank lending on real activity.

[20] In contrast, the existence of a balance sheet (demand-side) channel is relatively well-established (Bernanke and Gertler 1995; Hubbard 1994).

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 summarizes the findings, and their implications for monetary policy and credit market interventions generally. It then 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. Data and Model

The impacts of CRA on county-level measures of real activity are estimated using a county-level analog of equation (1) in the previous chapter:

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

As before, $Post_t$ captures whether year t is 1996 or later (i.e., after the CRA reforms), and Big_c and $ToughRegulator_c$ are continuous or discrete measures of the presence of banks facing plausibly binding CRA incentives (with counties that have one or more banks that are both "big" for CRA purposes and facing tough regulators again defined as the "affected" counties in the discrete parameterization). Again, β will capture the casual, reduced-form effect of CRA on the outcome of interest if there is no unobserved shock that hits in 1996 or later and affects the realization of that outcome differentially across counties that are more or less affected by CRA; i.e., across counties with a greater or lesser prevalence of banks that are plausibly affected by the CRA reforms.

The outcomes of interest (Y_{ct}'s), 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.[21] This setup will identify any reduced-form effects of CRA on real activity; I explain below (in Section V) 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 1.

Dropping the 43 counties with CBP disclosure issues and 122 counties with uncertain regulator toughness produces a "full sample" of 2,973 counties and 17,838 county-year observations from 1993-98.

[21] 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).

III. The Real Effects of CRA

Estimating (3) on the full sample, with a fully discrete parameterization of CRA incentives, produces the results in the first "$Post_t$*Big_c*$ToughRegulator_c$" column of Table 2.[22] 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.[23] 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 finding a positive effect on business bankruptcies.[24] 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*.[25] 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 (3) (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 Chapter 1, Section III).

---

[22] Using the smoother parameterizations of CRA incentives (as in Chapter 1, Section III) does not materially alter the results.

[23] 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).

[24] 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.

[25] 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.

IV. Does CRA Impose Efficiency Costs? Tests for Distortions

A. Are the Real Effects Simply Redistribution?

There is little evidence suggesting that the observed changes in real activity are driven by redistribution across counties rather than changes in the affected counties alone. 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 total lending by less than affected banks with offices in only a single county. 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.[26] 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 a 1 percent change in payroll in affected counties, $11.6 million, would represent a 2.3% change in payroll in unaffected counties.

B. Effects on Bank Performance and Consolidation

Table 3 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.[27] 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.

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.

[26] I have conducted these tests only on the subset of counties located within Metropolitan Statistical Areas (MSAs)— 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).

[27] 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.

C. Summary

Varied approaches uncover no glaring evidence that CRA creates efficiency costs by distorting bank lending decisions. However, precision issues in estimating impacts on bank performance preclude drawing firm conclusions.

V. Estimating the Effect of Bank Lending on Real Activity

The models used thus far have estimated reduced form effects of CRA on borrowing, lending, and real activity. The underlying relationship of greatest interest, however, is the impact of (access to) credit on real activity. This section develops estimates of this parameter.

A. Combining Reduced-Form Results from Chapters 1 and 2

The point estimates in Table 2 suggest the possibility of economically meaningful effects of CRA on real activity.[28] If one scales these estimates 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 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.[29] Analogous aggregation using the small business lending or IRS results generates estimated borrowing increases ranging from $4.0 billion to $11.5 billion (see the Appendix to Chapter 1). 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.

B. 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

[28] 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.

[29] 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.

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 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 input 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.[30] Conversely, 20 to 58 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. (Future work should examine 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.

[30] 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 here that CRA affects mortgage lending (Chapter 1, Table 5), and little evidence that CRA in general is distortionary (Section IV). Furthermore, Canner, Laderman, Lehnert, and Passmore (2002) find that CRA does *not* cause banks to subsidize mortgage loans.

VI. Conclusion

These first two chapters present 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. The 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 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. In all, the results suggest that two of the three necessary conditions for the existence of a bank lending channel do in fact hold; i.e., that changes in the supply of bank credit produce changes in the equilibrium financial position of a nontrivial number of firms, and that these financial changes have aggregate effects on economic activity.

The efficiency implications of these results are not entirely clear. On one hand, there is little direct evidence that the apparent credit increases produced by CRA are accompanied by 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 might 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.

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.

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.

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.

References

Ashcraft, Adam (2001). "New Evidence on the Lending Channel", Federal Reserve Bank of New York Staff Report No. 136, September 2001.

Bernanke, Ben S.; Lown, Cara S. (1991). "The Credit Crunch", *Brookings Papers on Economic Activity* v0, n2, 204-39.

Bernanke, Ben S; Gertler, Mark. (1995). "Inside the Black Box: The Credit Channel of Monetary Policy Transmission", *Journal of Economic Perspectives*, 9(4), 27-48.

Canner, Glenn B.; Laderman, Elizabeth; Lehnert, Andreas; Passmore, Wayne (2002). "Does the Community Reinvestment Act Cause Banks to Provide a Subsidy to Some Mortgage Borrowers?", mimeo.

Fay, Scott; Hurst, Erik; White, Michelle J (2001). "The Household Bankruptcy Decision", forthcoming, *American Economic Review*.

Hadlock, Charles J., and James, Christopher M. (2002). "Do Banks Provide Financial Slack?", *Journal of Finance*, forthcoming.

Hubbard, R. Glenn (1994). "Is There a 'Credit Channel' for Monetary Policy", National Bureau of Economic Research, Working Paper No. 4977.

Lang, William W.; Nakamura, Leonard I. (1993), "A Model of Relining", *Journal of Urban Economics* v33, n2: 223-34

Ordover, Janusz; Weiss, Andrew (1981). "Information and the Law: Evaluating Legal Restrictions on Competitive Contracts", *American Economic Review Papers and Proceedings*, 71(2), May, 399-404.

Peek, Joe; Rosengren, Eric S. (2000), "Collateral Damage: Effects of the Japanese Bank Crisis On Real Activity in the United States", *American Economic Review*, 30-45.

Petersen, Mitchell A.; Rajan Raghuram G. (1995), "The Effect of Credit Market Competition on Lending Relationships", *Quarterly Journal of Economics*, vol. 110, No. 2., pp. 407-443.

Sharpe, Steven A. (1995). "Bank Capitalization, Regulation, and the Credit Crunch: A Critical Review of the Research Findings", Board of Governors of the Federal Reserve System, Finance and Economics Discussion Series: 95/20.

Table 1. 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 2. 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).

Table 3. 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 (1.3)	14.9 (58.9)		0.6 (1.8)	1.6 (2.7)
Return on equity	0.010 (0.009)	0.012 (0.009)	0.06 (0.22)	0.006 (0.011)	0.023 (0.030)	0.03 (0.43)
Return on assets	-0.00009 (0.00090)	-0.0007 (0.0009)	0.006 (0.010)	-0.0011 (0.0014)	-0.0022 (0.0027)	0.006 (0.010)
Loans not accruing	-0.0025 (0.0017)	-0.0026 (0.0017)	0.01 (0.03)	-0.0044 (0.0022)	-0.0051 (0.0026)	0.01 (0.02)
90-day late	0.0004 (0.0006)	0.0003 (0.0006)	0.003 (0.005)	0.0010 (0.0011)	0.0009 (0.0011)	0.003 (0.006)
Loan loss provisions	0.0001 (0.0003)	0.0001 (0.0004)	0.002 (0.009)	-0.00063 (0.00052)	-0.00091 (0.00061)	0.003 (0.007)
Loan loss allowances	0.0011 (0.0016)	0.0012 (0.0016)	0.02 (0.02)	-0.00086 (0.00079)	-0.00020 (0.00012)	0.02 (0.02)
Charge-offs	-0.0003 (0.0004)	-0.0004 (0.0004)	0.003 (0.005)	-0.00110 (0.00064)	-0.00180 (0.00085)	0.004 (0.007)
Merger or Acquisition	-0.074 (0.054)	-0.079 (0.055)	0.78 (0.41)	-0.043 (0.079)	-0.040 (0.078)	0.71 (0.46)
Merger or Acquisition Survivor	-0.061 (0.055)	-0.071 (0.056)	0.55 (0.50)	-0.044 (0.077)	-0.045 (0.077)	0.49 (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.

Chapter 3

Liquidity and Consumer Behavior:
Some Evidence from the Deregulation of Credit Card Interest Rate Ceilings

Jonathan Zinman*
Massachusetts Institute of Technology
July 19, 2002

ABSTRACT

Despite a growing number of studies concluding that a substantial proportion of US households are liquidity constrained, there remains little consensus as to the quantitative importance or nature of these constraints. This paper develops a new type of evidence on the impacts of consumer credit markets on behavior by examining household-level responses to an exogenous liquidity shock. A United States Supreme Court decision effectively deregulated bank credit card interest rates in December 1978, and I develop evidence that consumers from states with binding usury ceilings before the decision became more likely to hold bank cards after the decision, relative to their counterparts in unaffected states. The marginal cardholders appear to have characteristics widely associated with credit constraints, and to borrow frequently on their new cards. Yet there is little evidence that these cardholders exploit their newfound liquidity by shifting into higher-yielding, less liquid, or riskier assets. This finding is at odds with most models of liquidity constraints, and motivates consideration of alternative explanations for the widely observed sensitivity of consumers to liquidity.

* Jonathan.Zinman@ny.frb.org; tel: 212-720-1204. . I am particularly grateful to Daron Acemoglu and Jon Gruber for their guidance. The author also thanks Daniel Bennett and Kurt Johnson for excellent research assistance, and Ricardo Caballero, Jim Poterba, and participants in the MIT macro and public finance lunches for helpful comments. The views expressed do not necessarily represent those of the Federal Reserve Bank of New York or of the Federal Reserve System.

I. Introduction

Consumer credit markets have grown dramatically over the last 30 years. Figure 1 shows that revolving consumer credit outstanding in the United States has increased sixty-fold in real terms since 1968. The development of bank credit cards has been a primary driver of this growth. The proportion of households using these cards has risen from 0.07 in 1968 to 0.68 in 1998 (Kennickell, et. al. 2001) with year 2001 aggregate outstanding credit card balances totaling about $600 billion (Board of Governors of the Federal Reserve System, 2002). This equates to over $5500 per household and 6% of GDP.

Various literatures are concerned with the implications of this growth in consumer credit, but fundamental puzzles remain. Growth could be caused by secular changes in demand for liquidity, and/or by technological innovations that relax liquidity constraints. Disentangling the roles of supply and demand in consumer credit markets has been a difficult task empirically, as Gross and Souleles (2002) note. Not surprisingly, then, there is little consensus on the quantitative importance of liquidity constraints and precautionary motives (Browning and Lusardi 1996).

Of course the lack of obvious exogenous variation in access to liquidity has also frustrated attempts to identify the effects of the growth of consumer credit. Meanwhile, interest in estimating these effects has grown along with recent theoretical work showing that consumer credit constraints have important implications. On the macro side, they can amplify business cycles (Hubbard and Judd 1986) and retard growth (Jappelli and Pagano 1999). More surprisingly, various works have shown that the welfare implications of expanded consumer credit markets are ambiguous, given various types of incomplete markets or nonstandard preferences.[31] Interactions between easy credit and incomplete contracts can lead to social welfare losses, as Athreya (2001) demonstrates for the case of bankruptcy law. Relaxing interest rate ceilings to increase credit supply might reduce welfare if usury laws mitigate insurance market failures (Glaeser and Scheinkman 1998), or combat lender market power (Blitz and Long, 1965). Expanding consumer credit markets may provide "too much liquidity" if consumers have self-control problems (Laibson 1997), leading consumers to undersave (Laibson, Repetto, and Tobacman 1998), to underestimate their credit card borrowing (Ausubel 1991), or to overestimate their probability of paying down high-interest balances. Any of these phenomena could produce optimization failures and result in welfare losses.

This paper develops new evidence on the causes and effects of liquidity growth by using the deregulation of interest rate ceilings to help identify increases in bank card use. These increases are arguably exogenous to credit demand and other unobservable determinants of household behavior. I identify states that had a binding usury ceiling ("affected" states) prior to the 1978 *Marquette* Supreme Court case that deregulated bank card interest rates, and show that following deregulation the proportion of households using bank cards increased in those states, relative to "unaffected" states that did not have binding usury ceilings prior to the case. Consideration of various potential confounds suggests that this result is not driven by unobservable differences between affected and unaffected states (or across households therein). Changes in usury law then can be used to identify the effects of shifts in access to credit on other margins of consumer behavior, including portfolio and occupational choice. This paper is thus the first study to

[31] With complete markets, consumer credit will expand if demand increases and/or lender costs decrease. In either case the expansion will be efficient.

exploit a plausibly exogenous shock to directly estimate effects of access to consumer credit on these types of outcomes; i.e., on outcomes in addition to borrowing.[32]

Specifically, having found that households in affected states become more likely to use a bank card, I then estimate whether households in these states become more likely to hold illiquid and/or risky assets. Both buffer stock and precautionary savings models predict that credit constraints will force consumers to be more liquid and conservative than optimal in their asset holdings. The joint test of whether consumers increase card use *and* shift into illiquid, riskier (and presumably higher-yielding) assets following usury deregulation offers a more complete test of the existence and impacts of liquidity constraints on consumer behavior than previous studies.

The rest of the paper proceeds as follows. The next section describes the framework used here for testing for the existence and impact of liquidity constraints. Section III describes the data on usury laws and the 1977 and 1983 Surveys of Consumer Finances (SCFs), the sources of household-level data on credit card use and other financial decisions used in this paper. Section IV details the econometric methodology, discussing threats to identification and presenting some preliminary evidence on the validity of the exclusion restriction. Section V presents results on the response of bank credit supply to interest rate deregulation, showing that bank card interest rates and consumer bank cardholding did appear to rise in states affected by *Marquette*. Section VI estimates a basic reduced-form model of the response of bank card borrowing to the liquidity shock provided by *Marquette*. The results suggest that the marginal cardholders borrowed frequently, and that they responded differently than inframarginal cardholders. The results for interest rates, card possession and borrowing all appear to be robust to various controls for household characteristics and demographic shifts. Falsification tests developed in Section VII further buttress the conclusion that these findings are driven by an exogenous shock to credit supply rather than unobserved shifts in demand. Section VIII tests for hetereogeneity in bank card use following deregulation in at attempt to parse out countervailing structural effects obscured by the basic reduced form estimation. Importantly, the marginal cardholder appeared to be young, poor, and minimally educated— all characteristics commonly associated with facing liquidity constraints. Section IX tests whether households, and the marginal cardholders in particular, appeared to adjust their portfolios "appropriately" in the face of increased access to liquidity. It finds little evidence that they did in fact increase illiquid or risky asset holdings, or decrease stocks of liquid assets. Section X concludes that the findings in this paper affirm the growing consensus that a great number of U.S. households have nontrivial (and very possibly substantial) marginal propensities to consume (MPCs) out of liquidity, but cast fresh doubt on the common conclusion that liquidity constraints drive these MPCs. This motivates several natural offshoots of this paper, which are sketched briefly.

[32] There is a small literature that does look specifically at the impact of credit card usury ceilings on card use (Dunkelberg, et. al. 1981, and Goldberg 1975). But these studies utilize small samples, lack critical control variables (such as state fixed effects— see Section IV), and do not examine any impacts of credit card use itself on consumer behavior. Gross and Souleles (2002) use proprietary account-level data from credit card companies and arguably exogenous features of firm credit-granting rules to identify marginal propensities to borrow out of liquidity and interest rate elasticities of borrowing. The nature of their data does not permit observation of other important margins of household behavior, however, nor does the data include households without a credit card.

II. Framework

A. Credit Constraints and Consumer Behavior

This paper addresses the questions of whether liquidity constraints exist, and whether they have empirically important impacts. It proceeds in two steps, first using the deregulation of usury laws to identify plausibly exogenous variation in bank credit card use, and then using this variation to estimate the impact of bankcard use on household portfolio choice. The first step serves more than the aforementioned instrumental purpose, as it will also shed light on the pervasiveness of liquidity constraints and the impact of interest rate regulation (see also Canner and Fergus 1987).[33] The second step is designed to develop evidence on the welfare effects of expanded consumer credit. Under most theories of liquidity constraints, a key source of welfare loss is that consumers are forced to be overly liquid and conservative in their asset holdings in order to smooth wealth shocks. I therefore test whether, when liquidity constraints are relaxed, consumers shift out of liquid and/or safe assets and into illiquid and/or risky assets.

B. Credit Cards and Consumer Behavior

Bank credit cards are a natural focal point for studying the growth and impacts of consumer credit. Empirically, bankcards have grown to dominate the other consumer credit products that preceded them: store-specific credit cards, lines of credit, and installment loans; and "traditional" consumer loans from banks and finance companies (Evans and Schmalensee, 1999). Conceptually, the very features that make bank credit cards dominant— e.g., their widespread acceptance, relatively high credit lines, and the ability to obtain cash advances— suggest that the growth of this type of consumer credit is more likely than any other to have reduced any pre-existing liquidity constraints, whether for good or for ill.

The "for good" scenario is relatively obvious-- under traditional (time-consistent) preferences, relaxing liquidity constraints will be efficient, as supply-side innovations in consumer credit add to the space of Arrow-Debreu markets. The "for bad" scenario is more controversial, but gaining currency as models which incorporate self-control problems using quasi-hyperbolic preferences are formalized. Under such a model, relaxing credit constraints may create "too much liquidity" (Laibson 1997) by permitting time-inconsistent consumers to indulge their current (time t) selves by splurging, at the expense of later consumption (and their time t+n selves). This can create welfare losses relative to a benchmark where the t=0 self is able to commit his future selves to implement his optimal consumption plan.[34]

Importantly, consumers with self-control problems most likely face greater difficulties with bankcards— which can used virtually anywhere to make purchases or obtain cash advances from ATMs[35]-- than with a store card. In the latter case, the sophisticated consumer need only avoid a

[33] The results also bear on consumer interest rate elasticities of borrowing, and the shape of the credit supply curve (please see Section V).

[34] Welfare losses need not result if consumers are (partly) sophisticated about their self-control problems (see, e.g., O'Donoghue and Rabin 2001; DellaVigna and Malmendier 2001) and possess commitment devices that effectively constrain future selves (e.g., Laibson, et. al. 1998). I broach possible impacts of "commitment constraints" in the concluding section.

[35] An extreme example is the prevalence of ATMs in casinos.

particular establishment to control his consumption, whereas controlling bankcard spending might require more costly commitment devices.[36]

C. Usury Law and Bank Card Use

More instrumentally (pun intended), idiosyncratic variation in the regulation of bank card interest rates can be used to help identify arguably exogenous shocks to the supply of credit. Specifically, the confluence of state usury laws and a United States Supreme Court ruling, in *Marquette National Bank v. First of Omaha Service Corporation*, 439 US 299 (1978), created a quasi-experiment where banks suddenly were granted the authority to charge discretely higher interest rates to consumers in several states in December 1978. These were states that maintained binding interest rate (usury) ceilings on bank credit cards as of that date.[37] (I define "binding" as less than 18%, since this has historically been both the modal rate charged, and the modal ceiling where ceilings existed.[38]) The *Marquette* decision gave banks the authority to "export" the bankcard interest rates permitted by their home state to customers in other states. Banks located in a state with a high or no ceiling could then charge high rates to consumers residing in other states. This opened the door to mass interstate marketing, and within two years leading bankcard issuers such as Citibank and MBNA had relocated to high interest states South Dakota and Delaware, respectively (Athreya 2001). *Marquette* thus quickly functionally deregulated bankcard interest rates by enabling out-of-state banks to circumvent the remaining strict state-level usury ceilings (and by putting banks chartered in those states at a competitive disadvantage, prompting actual state-level deregulation in most cases).[39]

Accordingly, it seems plausible that *Marquette* increased the supply of bankcards to households residing in those states that had binding usury ceilings at the time of the decision. The specific exclusion restriction that must hold for this shock to identify increases in bankcard use that are exogenous to other behaviors of interest is discussed and defended in Section IV.

[36] Ausubel (1991) and others have popularized the anecdote where consumers entomb their cards in ice and store them in the freezer to prevent impulsive purchases. Bertaut and Haliassos (2002) consider how credit limits might be used by consumers as a commitment device against overspending on bank cards.

[37] Penalties for violating these usury ceilings were typically severe, and violators faced potentially massive exposure to civil judgements (Illig, 1978).

[38] 18% was the modal rate charged on (bank) credit card balances in states that had ceilings of 18% or higher in both years considered in this study (1977 and 1983), and it remains the modal rate in the most recently published (1998) Survey of Consumer Finances. (Ausubel 1991 examines the stickiness of bankcard interest rates.) 36 of 38 states represented in the 1977 Survey of Consumer Finances placed some restriction on bankcard interest rates at the time of the survey, and 22 of these states had 18% as their ceiling.

[39] This view of *Marquette*'s impact is widely held by both legal scholars and economists. See, e.g., Ausubel (1991), Evans and Schmalensee (1999), and [legal cite]

III. Data

A. Survey of Consumer Finances

I draw microdata on household credit card use, assets, and demographics from the 1977 and 1983 Surveys of Consumer Finances (SCF), primarily.[40] The SCF provides the best available nationally representative data on credit card use (and on household balance sheets in general), but has increasingly well-documented limitations. The samples are small (2,563 households in 1977, and 3,665 in 1983 if one excludes the high-income oversample). Credit card use is underreported-- Blanchflower, Evans, and Oswald estimate that 1983 SCF respondents understated their bankcard balances by a factor of 2, and their number of credit card accounts by a factor of 1.5. Nevertheless the SCF provides some important advantages over the issuer-based data used in Gross and Souleles (2002) and Ausubel (1999). Most obviously, the SCF is publicly available (although geographic identifiers are not, after 1983), contains more comprehensive data on household characteristics, and permits direct examination of the impact of credit card use on margins of consumer behavior other than credit card borrowing. More subtly, perhaps, SCFs contain data on households without credit cards, avoiding the concerns about selection due to entry and attrition that are inherent to the use of Gross and Souleles' account-level data.

I also use data from the 1968 and 1970 Surveys of Consumer Finances to examine pre-treatment trends (see Section IV) and conduct falsification tests (see Section VII). These surveys are comparable in size (they contain 2,677 and 2,576 observations, respectively) and content to the 1977 and 1983 surveys, although both earlier surveys lack data on credit card interest rates and the 1968 survey contains relatively few details on credit card use.

B. Bankcard Usury Laws

I determined whether each state that appears in the SCF had a binding bankcard usury law as of the 1977 survey date (July 1977) by referring to the appropriate superceded state statutes. I then confirmed that my reading of the statutes was correct (e.g., that there were no legal loopholes or enforcement practices that might effectively raise *de jure* low ceilings, or lower *de jure* high ceilings), by consulting secondary sources (including Gushee, various years; American Bankers' Association, various years; and dozens of law review articles). Oregon's bankcard usury law was both unique in construction and in a state of flux in 1976-77, so I drop the 47 Oregon households in the 1977 SCF from my estimation sample.

IV. Econometric Methodology

A. Reduced form model

The basic reduced form model is estimated using Ordinary Least Squares (OLS) or probit as follows:

(1) $Y_{ist} = a + \beta X_{st} + \chi W_{ist} + \delta_s + \phi_t + \varepsilon_{ist}$

Y is a measure of bankcard use or asset holding from the SCF by household i, living in state s, at time t (where t is either 1977 or 1983). X is an indicator variable taking the value of 1 if banks had clear authority to charge 18% or higher on bankcard balances to residents of state s at time t. X_{st} therefore takes the value of zero only for 1977 households in states with binding

[40] The 1977 survey was originally entitled the "Consumer Credit Survey", and was sponsored by various bank regulating agencies (including the Federal Reserve Board). It was designed to provide some continuity with the earlier, annual SCFs from 1947-1970 that had been sponsored by the Federal Reserve Board. The 1983 survey was more comprehensive, and updated most of the variables collected in 1977.

usury ceilings in 1977, since the *Marquette* decision of 1978 effectively deregulated bankcard interest rates by 1983 (see Section II). W is a vector of control variables, and includes household and state-level characteristics. δ_s and ϕ_t condition on state- and year-specific means, respectively, of the dependent variable. Standard errors are adjusted for the fact that the variation of interest occurs at the state-year level by allowing for clustering within state-year cells.

I also will use (1) to verify that usury deregulation did in fact permit higher interest rates, by setting Y_{ist} equal to the (bank) credit card interest rate.

The coefficient β will capture the causal effect of X_{st}, the usury law (or "deregulation") variable, on Y if there are no unobserved, differential *trends* in Y across households in the two groups of states X_{st} classifies—those that had binding usury ceilings in 1977 (and therefore were plausibly affected by *Marquette*) and those that did not (and therefore were not plausibly affected by *Marquette*).[41] Note the emphasis on unobservable *trends*; any persistent differences across states (e.g., some are debt-loving, others are debt-hating) are captured by the state fixed effects δ_s. In other words, (1) will capture the within-state variation in Y due to X, for households in states that had binding usury ceilings in 1977 relative to households in states that did not, if the identifying assumption holds. The raw data suggests that it does. Figure 2 reveals little evidence of differential trends in bank card use across affected and unaffected states *before* 1977, and suggests breaks from trend in affected states (relative to unaffected states) only *after* 1977 (presumably due to *Marquette*).[42] Figure 3 indicates that variables that should *not* have been affected by *Marquette* (these are used in the falsification tests of Section VII) do *not* in fact appear to break from trend after 1977. Table 1 shows few observable differences in demographic characteristics or economic conditions between affected and unaffected states in 1977, but a stark difference in bank card interest rates (which presumably were depressed in affected states by binding usury ceilings).

The other particularly notable feature of Table 1 is the evident lingering effects of the 1981-82 recession. Credit card growth, which had surged throughout the 1970s, slowed dramatically in the late 1970s and early 1980s (see also Figure 1), and unemployment remained high in 1983. These macroeconomic effects arguably stack the deck against finding effects of usury deregulation in a statistical sense, since there is probably less variation in card use than there would have been counterfactually. But the recession should not otherwise contaminate the results, since the year effects capture time series conditions common to the entire sample, and household- and state-specific control variables capture local conditions.

Of course, the deeper question to consider regarding the identification issue is why several states maintained binding usury ceilings as late as the *Marquette* case while others did not. The political economy of usury regulation is poorly understood (and the drivers of usury *deregulation* even less so), but the fact that the deregulation considered in this paper occurred as the result of a federal intervention, and a court case at that, mitigates concerns that subsequent behavior in affected states might be driven by unobserved changes in consumer demand rather than bank supply. Nevertheless I condition on various household and state characteristics that could be correlated with both usury law status as of 1977 and changes in demand for credit and various

[41] No states made material changes to their usury ceilings between the 1977 SCF survey date and the 1978 *Marquette* decision.
[42] The aggregate slowdown in bank card growth evident after 1977 has been attributed to a combination of the early 1980s recession and industry growing pains (Mandell 1991).

assets. This strategy is detailed in Section V. Section VII then presents several falsification tests designed to detect any spurious correlation between deregulation and demand shocks.

B. Structural Models

Although (1) captures the structural relationship of interest quite well in the case of interest rates and card possession, many of the other outcomes (Y_{ist}'s) considered in this paper have additional structural parameters of interest. For example, the structural equation of interest for bankcard borrowing is:

(2) $B_{ist} = a + B_1 H_{ist} + B_2 r_{ist} + \chi W_{ist} + \delta_s + \phi_t + \varepsilon_{ist}$

Where H and r are the endogenous regressors of interest, with H measuring whether household i has a bankcard, and r measuring the interest rate i faces if it borrows on its bankcard.[43]

(2) reveals that estimating (1), the reduced form, for B_{ist} masks important heterogeneity, since (1) pools two very different types of cardholders— the marginal ones, for whom new access to card represents a decrease in the cost of borrowing, and the inframarginal ones, who likely experience no change (if they live in an unaffected state) or an *increase* (if they live in an affected state) in the cost of borrowing. Put differently, to estimate a true interest rate elasticity of bankcard borrowing, one needs to control for selection into bankcard holding, and therefore one needs to instrument for both H and r.

This presents a problem. The results in Section V suggest that X_{st} (the deregulation variable) can serve as one instrument, but another is required. Ongoing work seeks to develop well-identified structural models of bankcard borrowing and portfolio choice using additional instruments.

For now, I rely on an alternative approach to put a bit more structure on the reduced-form results; namely, adding interactions of household characteristics with the deregulation variable to (1). With reference to existing evidence on which types of consumers are likely to be liquidity constrained (e.g., Jappelli 1990), these results will test for heterogeneity in responses across different types of consumers and help identify the marginal card user (see Section VIII).

V. Impacts of Interest Rate Deregulation on Credit Card Interest Rates & Possession

The next two sections discuss estimates of equation (1) for various outcomes related to credit card use. The results suggest that deregulation increased both bankcard holding and borrowing. Robustness and falsification tests in Section VII will generally support the interpretation that deregulation caused these increases by increasing the supply of bankcards.

[43] Note that the interest rate should not have an independent effect on demand for card possession (as opposed to borrowing) under standard preferences, since consumers can choose whether to finance balances, and there are several other reasons to hold cards (including: the option to borrow, the free float on balances paid in full after one billing cycle, payment services). If consumers have self-control problems, however, all bets are off. Sophisticated consumers with self-control problems might well exhibit an interest rate elasticity of cardholding, since they may forgo cardholding in order to commit not to borrow at high rates.

A. Interest Rates

Table 2 presents estimates of the effect of deregulation on credit card interest rates. The variables of interest are constructed from SCF questions asking respondents for the interest rate they pay on bank or store card balances that are not paid in full (i.e., that are carried beyond the free float period). The SCF does not ask about bankcard rates in particular, so I construct a crude approximation by limiting the sample to those who report an interest rate, but have only a bankcard (not a store card). Each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different regression. As such each column presents results for a different specification, as follows:

- column 1 regressions include only the deregulation variable, state and year effects
- column 2 adds variables capturing the race and age of the household head
- column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator
- column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate.
- column 5 adds interactions of the year dummy and individual covariates, to capture any time-varying influence of a household's characteristics on its financial decisions.

The general approach here is to ensure that any observed relationship between deregulation and the outcomes of interest is not driven somehow by demographic shifts in affected states, either due to coincidence, or to a political economy story whereby increasing demand in affected states set in motion the legal process that culminated in the *Marquette* decision.[44] The state aggregate variables are motivated in part by Glaeser and Scheinkman's (1998) findings that the likelihood of a usury ceiling may be increasing with equality and decreasing in income growth, and in part by the importance of network externalities in bankcard supply (Evans and Schmalensee, 1999). These factors suggest that card issuer (mass) marketing strategy might depend on state-level characteristics not entirely captured by the micro data.

The results in Table 2 suggest that deregulation did in fact increase bankcard interest rates. The approximated annual bankcard rate rises by about 130 basis points in affected states relative to unaffected states (this would imply a 9% increase over the 1977 mean of 14.4% in affected states). Taking the log of the dependent variable yields similar magnitudes, with implied increases of about 9%. Both the level and log results are consistent across specifications, and all are statistically significant by a comfortable margin (with t-statistics of 3 or greater). The combined bank and store card rate increases less, not surprisingly, but still substantially, by about 90 basis points (implying a 6.4% increase over the 1977 base mean of 14.1% in affected states). The logged results are insignificant, with slightly larger standard errors than their logged bankcard rate counterparts, and much smaller point estimates clustered around 0.04. In all, it seems plausible, at least, that *Marquette* did in fact increase the interest rate banks charged on credit card balances held by households in affected states. This finding provides support for Ausubel's (1991) "upward-quick" model of credit card interest rates.

---

44 *Marquette*-related proceedings were in fact initiated by a bank in a state with a binding usury ceiling (Minnesota)— in 1976. If a potentially confounding secular increase in demand were driving this proceeding, we might then expect to see a break from trend in card use before the 1977 SCF, in affected states relative to unaffected states. This does not appear to be the case (Figure 2).

B. Card possession

Table 3 presents analogous results for bankcard possession from linear probability estimates of (1).[45] Possession is arguably the bankcard behavior of greatest interest, since it is measured with less error than bankcard borrowing (Blanchflower, Evans, and Oswald), and provides a better summary of the benefits available to cardholders than actual borrowing. This is because a bankcard provides services that plausibly decrease liquidity constraints *regardless of whether the cardholder actually borrows*. These services include payments, free float, and the option to borrow**.** The results suggest that the proportion of households using a bankcard rose between 3.5 and 5.5 percentage points in affected states, relative to unaffected states, between 1977 and 1983. These are large changes— they would imply a 10 to 15 percent increase over the base period mean of 0.36 in affected states, and account for 49 to 77 percent of the time series growth in bank card possession in affected states during this period (Table 1). The estimates are marginally significant, with t-statistics of about 2 in every case. Similar inferences are obtained for the number of bankcards held by a household (this can be thought of as a proxy for available credit). Row 2 presents results with the dependent variable topcoded at 2 to reduce the influence of outliers (this censors 2.3% of the estimation sample), and Row 3 presents results with the number of bankcards topcoded at 3 (censoring 0.7% of the estimation sample). These point estimates imply increases of 0.07 to 0.12 bankcards per household in affected states, or about a 15% increase over the base period means. This percentage increase is similar to that obtained for card possession, suggesting the much of the increase in the number of cards may actually be driven by the extensive margin.

The analogous results for all credit cards (where the count does not include gas cards, and therefore is comprised primarily of bank and store cards) are sensitive to the censoring rule, with significant increases found when topcoding at 10 cards, but not at 5. The increases of 0.21 to 0.31 of a card found in Row 6 would imply a 9 to 13 percent increase over the base period mean of 2.4 cards in affected states. There is no evidence of an effect on the extensive margin of holding any card (Row 4), nor is there a significant effect on holding a store card in particular (Table 5, Row 1).

The relationship between household characteristics and bank card possession (not reported) confirm most of the findings of Blanchflower, Evans, and Oswald's analysis of pooled 1977-1995 SCF data (which did not include state fixed effects or the deregulation variable). Conditional on the other observables, white, rich, educated, married, middle-aged, female-headed, homeowning households with fewer members appear most likely to hold bank cards.

C. Summary

In all, the results presented thus far suggest that deregulation did in fact increase both the number of households holding bank cards, and the number of bank cards in circulation. Deregulation does not appear to have had a significant effect, however, on overall cardholding. These results raise the questions of whether bankcards were substitutes for store cards, and/or if the marginal cardholder produced by deregulation was in fact liquidity constrained. I explore these questions in the next three sections.

[45] Probits produced nearly identical results.

VI. The Impact of Interest Rate Deregulation on Credit Card Borrowing

A. Unconditional Borrowing

I continue by estimating the reduced form effect of interest rate deregulation on credit card borrowing. Table 4, Row 1 presents results for whether a household reported having a bankcard balance that was incurring finance charges as of the month prior to the SCF survey date. (This binary parameterization of borrowing is motivated by concerns about outliers and measurement error.) The estimates suggest a marginally significant, 3 percentage point increase in the number of households carrying bankcard balances. Taking the point estimates literally, this is a large increase compared to the estimated 3.5 to 5.5 percentage point possession increases reported in Table 3. It suggests that *at least* 55% of the marginal cardholders were borrowing on their bankcards (the full sample mean in the 1983 SCF is 52%). Of course the true proportion is likely somewhat higher, since, as Section IV outlines, the reduced form for unconditional borrowing captures the net effect of marginal cardholders borrowing more (since one can't borrow without a card), and inframarginal cardholders borrowing less (since they now face a higher interest rate). Row 2 estimates the reduced form effect on credit card balances generally (this measure is dominated by store and bank cards, and does not include gasoline cards). The results are virtually identical to those obtained for bank card balances only. This is reassuring-- to the extent there are changes in overall card borrowing, they should be driven by bank cards (i.e., if the increase in overall credit card borrowing was significantly larger than for bank card borrowing, this would arouse suspicion). Row 3 reports results for whether the household reports that it typically borrows on its bank or store card. The results are generally insignificant, with somewhat larger standard errors and smaller coefficients than the other measures of unconditional borrowing (this could well be due to a relatively big underreporting problem with this variable).

Unfortunately, the 1977 SCF lacks balance data on most other types of borrowing, so it is not possible to directly calculate the extent to which, if any, new bank card borrowing crowds out other sources of debt. However, preliminary estimates provide little evidence of crowd-out on the extensive margin of likely substitutes (revolving credit and installment debt from stores, consumer loans from banks).

B. Conditional Borrowing

Rows 4 and 5 present estimates for the logarithims of bank and credit card balances, respectively, and thus condition on borrowing. Although one should not attribute a causal effect to deregulation in this context, since we know that deregulation also effects that probability that one borrows, it may be worth noting that the results here appear consistent with a substantial decrease in borrowing by inframarginal cardholders; i.e., with a nontrivial interest rate elasticity of borrowing. Alternately, marginal cardholders may borrow less on average than their predecessors.

C. Interpretation

In all, the reduced form results on credit card borrowing raise two important possibilities. One is that the marginal cardholder was quite credit constrained; the results on unconditional borrowing are consistent with very high probabilities of borrowing for marginal bank cardholders. The second hints that responses to deregulation were heterogeneous, as the results on unconditional borrowing point to potentially important differences between marginal and inframarginal cardholders. This further motivates richer models that can distinguish the mechanisms underlying responses by marginal and inframarginal cardholders. Ongoing work seeks to develop structural models of the sort described in Section IV. Another approach is to enhance the reduced form model. This is undertaken in section VIII.

VII. Falsification Tests

Before exploring heterogeneity in the responses of card use to interest rate deregulation, I first conduct several falsification tests in an attempt to rule out demand-driven explanations for the observed correlations between interest rate deregulation and bank card use. Table 5 presents the results of falsification tests where I simply replace the dependent variables of equation 1 with variables that should *not* be affected by bankcard interest rate deregulation. Finding a "false positive" here would raise concerns that the "shock" provided by *Marquette* is actually correlated with unobserved determinants of demand; i.e., that households in states affected by *Marquette* actually had a secular uptick in demand between 1977 and 1983 relative to unaffected states.

The probability of holding a *store* card should be uncorrelated with bank card deregulation, since *Marquette* did not apply to store cards, and since none of the states affected by *Marquette* deregulated store card rates between 1977 and 1983. The results in Table 5, row 1 provide no evidence to the contrary. This is hardly airtight evidence in support of a causal effect of the deregulation variable on bankcard use, since bank and store cards could be substitutes (or less likely, complements). Nevertheless one would be concerned if store cards increased along with the deregulation variable. Row 2 shows no significant effect on first mortgage interest rates. Rows 3 and 4 display correlations between the deregulation variable and beliefs about the benefits of consumer credit that are *negative*, if anything; i.e., the proportion of households responding that consumer credit is a "good thing", or that it is "all right to borrow" to finance discretionary purposes, appears to *drop* following deregulation.[46] It is not yet clear how we should interpret these results (see the previous footnote), or whether we should take attitudinal questions seriously at all. But if nothing else these results further alleviate concern that deregulation might be correlated with unobserved increases in demand.

Table 6 presents estimates for a timing falsification model where (1) is estimated on 1970 and 1977 SCF data for the card possession and lending variables of interest (credit card interest rates are not available in 1970), with deregulation falsely coded as taking effect in 1977 for households in states that still had binding usury ceilings at that time. This test creates a selection problem, unfortunately, since many states changed their usury ceilings between 1970 and 1977, and I am forced to limit the sample to those states that had stable interest rate regulation during this period. The results therefore should be interpreted with caution. Estimates are ultimately based on a sample of households from 27 states (as opposed to the 37 states represented in every other sample used in this paper), of which only 5 states are labeled as affected (as opposed to 7

[46] These results are intriguing, particularly since the dominant reasons households offer for consumer credit or credit cards being a "bad thing" evoke self-control problems ("encourages impulse buying…. too easy to buy now, pay later….. buy things don't want or need", "buy more than you can pay for"). The results hint that there may have been some (social) learning about the self-control problems posed by bankcard possession, and that households updated their beliefs about bankcards' costs and benefits as cards became more prevalent. The results do *not* appear to be driven by inframarginal cardholders responding to higher interest rate, since "costs too much" is a separate, mutually exclusive response. (Other alternative explanations must be explored, of course; e.g., what if consumer groups targeted consumer credit "awareness" campaigns to affected states in the wake of *Marquette*?) Future work on the nature of consumption responses to increased bankcard use (smooth, or splurge?) will examine these results more closely.

elsewhere). The deregulation variable should be uncorrelated with the outcomes of interest in these tests if deregulation was in fact an exogenous shock to bank card supply. Indeed, there is virtually no suggestion of a statistically significant relationship in this table. It should be noted, however, that the standard errors are large enough in most cases to admit the possibility of correlations that would raise concerns about mean reversion.

VIII. Heterogeneity: In Search of the Marginal Bank Card User

The reduced form models presented thus far may obscure important heterogeneity in responses between marginal cardholders (for whom borrowing on bank cards became less expensive, or at least feasible) and inframarginal cardholders (for whom borrowing might have become more expensive) in states affected by usury deregulation. In this section I attempt to identify any important sources of heterogeneity by adding interactions between the deregulation variable (X) and each of the household control variables (in matrix W) to equation (1). This increases the main effect on the deregulation variable approximately twofold for the case of bankcard possession (compare to Table 3, row 1, column 4); however, the standard errors jump as well (with a p-value of 0.155 for any bank card, and .081 for the number of bank cards). The interaction terms suggest that marginal cardholders are much more likely to be young (a whopping 26 percentage point increase in deregulated states relative to the oldest households), have only a high school education (a 9 point increase relative to heads without a high school education), poor (9 to 13 point increases for the poorest group relative to richer groups), and a mortgagee.[47] Each of these characteristics except for the latter have been widely associated with liquidity constraints (e.g., Jappelli 1990).

The main effect of deregulation on the proxy for bank card interest rates jumps dramatically when the interactions are added (coefficient=3.14, standard error=1.49; compare to Table 2, row 1, column 4). Working households with children, male heads, and very little education appear more likely to face higher rates after deregulation.

Adding the interactions eliminates the main effect of deregulation on binary bankcard borrowing (coefficent=0.006, standard error=0.045; compare to Table 4, row 1, column 4). Only young households (compared to the oldest households), married households, and those with some college education (compared to the least-educated) appear to borrow more, and the effects are only marginally significant. Conditional borrowing falls dramatically but is imprecisely estimated (coefficient= -0.89, standard error= 0.70), with the self-employed and those with some college appearing more likely to borrow.

In all, the large increases in bank cardholding (and, to a lesser extent, on borrowing) among classes borrowers widely thought to be liquidity constrained would seem to bode well for identifying the effects of liquidity on portfolio choice in the next section.

[47] Income categories are based on four approximately equal-sized groups in the 1977 data (this approach is necessitated by the categorical nature of the data). I then deflate the 1983 income variable (which is continuous) to 1976 dollars and use the 1977 break points to define the 1983 categories as well.

IX. Interest Rate Deregulation and Asset Choice

The results in previous sections suggest that interest rate deregulation delivered bank cards to a nontrivial number of households on the margin. Many of these households borrowed actively on their cards, and many of the marginal cardholders were plausibly credit constrained *ex ante*. Most models of buffer stocks and precautionary saving have the implication that liquidity constraints force agents to be sub-optimally liquid and conservative in their asset holdings. Accordingly, in this section I estimate reduced form models of whether households in states affected by the deregulation of interest rate ceilings, and in particular those households that appear to be the marginal cardholders in Section VII, do in fact shift into higher-yielding illiquid and risky assets.

This exercise is hampered somewhat by data limitations in the 1977 SCF, which reports only categorical values for most asset types. Although the natural variables of interest here are ratios of asset types to total assets, it is impossible to construct these proportions with any precision in the 1977 SCF. Consequently I am forced to parameterize asset holdings as binary variables, based either on the extensive margin or arbitrary cutoffs. The combination of these measurement problems and the small net increase in bank cardholding in affected states (3.5 to 5.5 percentage points) dims the prospect of finding significant effects in the basic reduced form (equation 1).

I begin by estimating whether deregulation appears to induce households to reduce liquid asset holdings. There has been a longstanding particular interest in whether credit card use reduces liquid asset holdings (dating at least to White 1976), motivated primarily by the question of whether the payments feature of credit cards reduces the transactions demand for money (see also Duca and Whitsell 1995, and Blanchflower, Evans, and Oswald). Of course credit cards might reduce liquid asset holdings through another channel as well— if they relax liquidity constraints and thereby reduce the need to hold buffer stocks (which must be kept relatively liquid for emergencies). Table 7, Rows 1-3 present the reduced form estimates for checking account balances (row 1), savings account balances (row 2), and a binary variable for savings account ownership (row 3). The model delivers the expected signs for the savings account variables but not for checking balances, and the standard errors are too large to identify the plausibly small effects one would expect on these binary outcomes. I encounter a similar problem with illiquid assets (certificates of deposit and government savings bonds) and a risky asset (stocks). Results are presented for the extensive margin in each case, but binary variables based on the various categorical cutoffs posed by the 1977 survey fared no better.

The basic reduced form model accordingly provides little leverage for testing the prediction that interest rate deregulation should induce portfolio shifts (via relaxation of liquidity constraints). Part of the power problem stems from the fact that, as in the case of borrowing, the reduced form model may obscure countervailing effects on marginal and inframarginal cardholders in affected states. In contrast, the large effects on cardholding and borrowing for certain, plausibly credit-constrained groups (Section VII) suggest that the enhanced reduced form model with interactions holds some promise for identifying effects of deregulation (and bank card use) on asset choice.[48]

Indeed, adding the interaction terms to the equations for asset holdings yields some significant results— and little support for the prediction that marginal, liquidity-constrained

[48] The limitations of the reduced form again motivate structural approaches. Ongoing work suggests that certain structural parameters should appear in asset choice models that are absent from the borrowing model. For example, it would be useful to distinguish the impact of the option value of borrowing on portfolio choice from the impact of actual borrowing (or convenience use).

households should adjust their portfolios in favor of more illiquid, risky assets upon accessing credit. The youngest households in affected states do appear to become much more likely to hold a government savings bond and less likely to have savings account (relative to the oldest households) after deregulation, which jibes with the canonical predictions. However they seem far less likely to hold stock or be self-employed, findings that are at odds with the canonical predictions. The poorest households appear to become more likely to be self-employed or to hold a government savings bond, certificate of deposit, or stock than their richer counterparts, but they also seem to increase their liquid asset holdings (this is a case where it would be particularly useful to have data on the ratios of each of these different types of assets to total assets). There does not appear to be significant variation in portfolio choice by education level in deregulated states, in contrast to bank card usage results. Males seem to begin holding more assets in their savings accounts, and are less likely to be self-employed (these could be inframarginal effects of males facing the higher interest rates observed in Section VII). Working households become more likely to hold both stock and liquid assets. Married households become more likely to hold a CD and substantial amounts of stock, but also more likely to hold high checking account balances after deregulation. Blacks appear to enter self-employment and hold larger savings account balances, despite no observable changes in card use.

Although measurement issues discourage drawing firm conclusions from the results in this section, it nevertheless seems fair to say that the findings here do not jibe easily with standard models of liquidity constraints.

X. Conclusion

This paper develops a new type of evidence on the impacts of liquidity on consumer behavior. The results suggest that binding interest rate ceilings in several states during the late 1970s kept perhaps 4 or 5 out of every 100 households in these states from obtaining bank credit cards, and that at least 55% of these households borrowed on bank credit cards when given the opportunity. Moreover these marginal households were probably even *less* likely to consume out of liquidity than the 57% of households that remained without bank credit cards in the 1983 SCF (Jappelli, Pischke, and Souleles 1998). The results here are thus consistent with a clear majority of U.S. households having a nonzero marginal propensity to consume (MPC) out of liquidity. The fact that it is possible to identify borrowing increases at all in data reported by consumers (who are notorious for understating their credit card borrowing) suggests that these MPCs could be substantial, as in Gross and Souleles (2002).

But an MPC to consume out of liquidity is not a sufficient condition for establishing the existence or importance of liquidity constraints. A stricter test for the presence of liquidity constraints is whether consumers respond to increased liquidity by shifting into illiquid, riskier assets. The results in this paper offer no evidence that they do, despite the fact that the marginal cardholder appears to be young, poor, and relatively poorly educated— all characteristics commonly associated with facing binding liquidity constraints. In other words, a substantial number of households that appeared to be credit constrained suddenly obtained access to liquidity as the result of interest rate deregulation, and they did not adjust their portfolios as most models of liquidity constraints would predict. Data limitations prevent drawing firm conclusions, but the results are suggestive and motivate consideration of alternative explanations.

One type of alternative (or complementary) model postulates that consumers face what might be termed "commitment constraints". The possibility that some consumers are prone to splurge, with potentially adverse consequences, in the absence of effective commitment devices is gaining currency. Recent simulations, for example, have shown that preferences which allow for self-control problems explain household balance sheets much better than standard preferences (Angeletos, et. al. 2001; Laibson, Repetto, and Tobacman forthcoming).

Far more work can be done to study the impact of liquidity, and or any lack thereof, in consumer credit markets. One offshoot of this paper is developing more structural models that should yield new evidence on the interest rate elasticity of borrowing, and shed some light on the importance of option value in consumer borrowing. Structural models may also offer greater traction in examining the impact of liquidity on the portfolio choices of marginal, credit-constrained borrowers than the reduced form models estimated in this paper. Future work will study the impact of liquidity on the composition and time-path of consumption— do households smooth or splurge when they obtain a credit card?— and proceed to estimate any long-run impacts on savings rates and savings adequacy. This new work may shed some light on the preferences underlying consumer behavior in consumer credit markets as well as on the nature of the markets themselves.

References

American Bankers' Association, "Summary of State Banking Legislation", various years.

Athreya, Kartik, "The Growth of Unsecured Credit: Are We Better Off?", *Federal Reserve Bank of Richmond Economic Quarterly*, 87/3 (2001), 11.

Angeletos, George-Marios, Laibson, David I., Repetto, Andrea, Tobacman, Jeremy, and Weinberg, Stephen, "The Hyperbolic Buffer Stock Model: Calibration, Simulation, and Empirical Evaluation", *Journal of Economic Perspectives*, 15/3(Summer 2001), 47-68.

Ausubel, Lawrence M., "The Failure of Competition in the Credit Card Market", *American Economic Review*, 81(1991), 50-81.

Ausubel, Lawrence M., "Adverse Selection in the Credit Card Market", mimeo, June 17, 1999.

Bertaut, Carol C., and Haliassos, Michael, "Debt Revolvers for Self-Control", mimeo, June 10, 2002.

Blanchflower, David G., Evans, David S., Oswald, Andrew J., "Credit Cards and Consumers", National Economic Research Associates Working Paper (undated).

Blitz, Rudolph C., and Long, Millard F., "The Economics of Usury Regulation", *Journal of Political Economy*, 73/6(1965), 608.

Board of Governors of the Federal Reserve System, *Statistical Release G.19*, July 2002.

Browning, Martin, and Lusardi, Annamaria, "Household Saving: Micro Theories and Macro Facts", *Journal of Economic Literature*, 34(1996), 1797-1855.

Canner, Glenn B., and Fergus, James T., "The Effects on Consumers and Creditors of Proposed Ceilings on Credit Card Interest Rates", *Federal Reserve Bulletin*, 73/10(October 1987), 783.

DellaVigna, Stefano, and Malmendier, Ulrike, "Contract Design and Self-Control: Theory and Evidence", mimeo, December 3, 2001.

Duca, J. V., and Whitsell, W. C., "Credit Cards and Money Demand: A Cross-Sectional Study", *Journal of Money, Credit, and Banking*, 27(1995), 604-623.

Dunkelberg, William, et. al. "CRC 1979 Consumer Financial Survey", Monograph 22 (Purdue University, Krannert Graduate School of Management, Credit Research Center), 1981

Evans, David S., and Leder, Matthew R., "The Role of Credit Cards in Providing Financing for Small Businesses", National Economic Research Associates Working Paper (undated).

Evans, David S., and Schmalensee, Richard, *Paying with Plastic: The Digital Revolution in Buying and Borrowing* (MIT Press: Cambridge, MA), 1999.

Glaeser, Edward L., and Scheinkman, Jose, “Neither a Borrower Nor a Lender Be: An Economic Analysis of Interest Restrictions and Usury Laws”, *Journal of Law and Economics*, XLI(1998), 1.

Goldberg, Lawrence G., “The Effect of State Banking Regulations on Bank Credit Card Use: Comment”, *Journal of Money, Credit, and Banking*, 7/1(1975), 105.

Gross, David B., and Souleles Nicholas S., “Do Liquidity Constraints and Interest Rates Matter for Consumer Behavior? Evidence from Credit Card Data”, *Quarterly Journal of Economics*, 117/1(2002), 149-177.

Gushee, Charles, ed., *The Cost of Personal Borrowing* (Basic Books: Boston), various years.

Hubbard, R. Glenn, and Judd, Kenneth, “Liquidity Constraints, Fiscal Policy, and Consumption”, *Brookings Papers on Economic Activity*, 1(1986), 1-59.

Hurst, Erik, and Lusardi, Annamaria, “Liquidity Constraints, Wealth Accumulation, and Entrepreneurship”, mimeo, March 2002.

Jappelli, Tulio, “Who is Credit Constrained in the U.S. Economy”,

Jappelli, Tulio, and Pagano, Marco, “The Welfare Effects of Liquidity Constraints”, *Oxford Economic Papers*, 51(1999), 410-430.

Jappelli, Tulio, Pischke, Stephen, and Souleles, Nicholas, “Testing for Liquidity Constraints in Euler Equations with Complementary Data Sources”, *Review of Economics and Statistics*, LXXX(1998), 251-262.

Kennickell, Arthur B., Starr-McCluer, Martha, and Surette, Brian J., “Recent Changes in U.S. Family Finances: Results from the 1998 Survey of Consumer Finances”**,** *Federal Reserve Bulletin*, vol. 86 (January 2000), pp. 1-29.

Laibson, David I., “Golden Eggs and Hyperbolic Discounting”, *Quarterly Journal of Economics*, 112/2(1997), 443.

Laibson, David I., Repetto, Andrea, and Tobacman, Jeremy, “Self-Control and Saving for Retirement”, *Brookings Papers on Economic Activity*, Vol. 1998/1 (1998), 91.

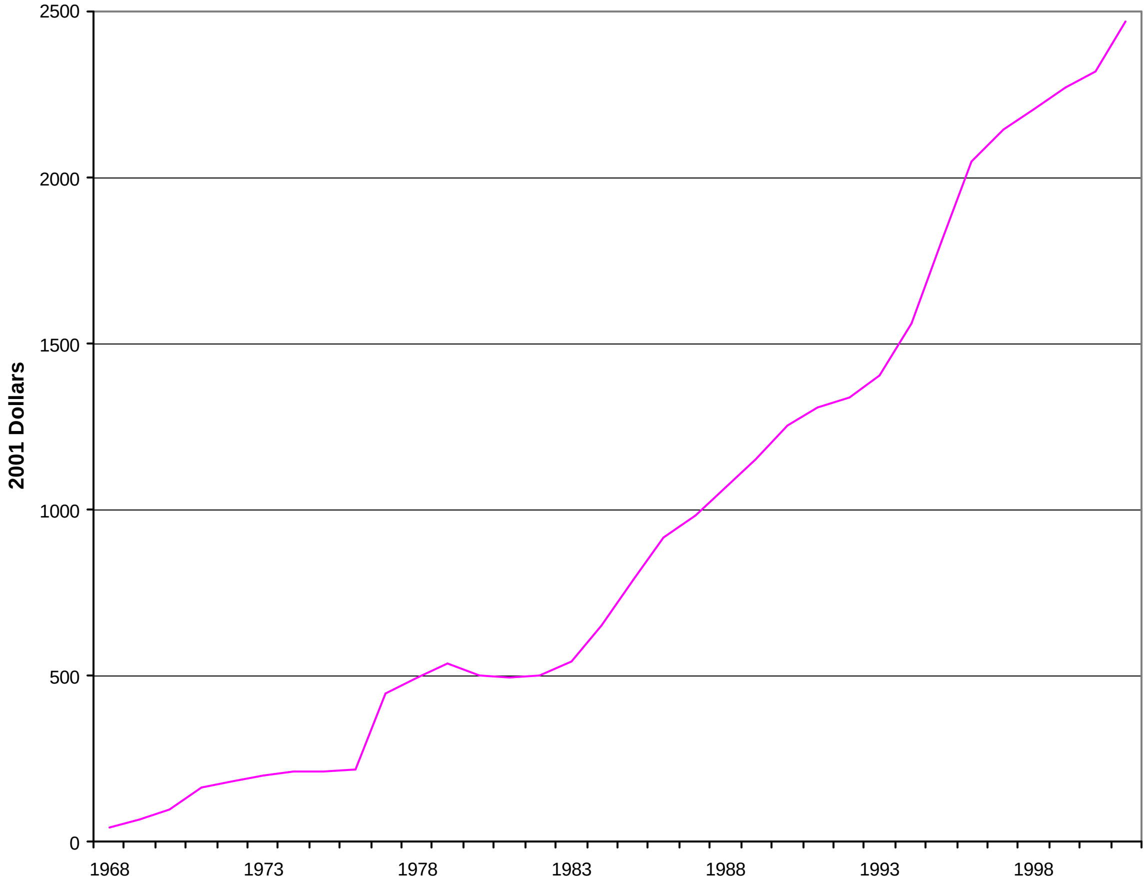
Laibson, David I., Repetto, Andrea, and Tobacman, Jeremy, “A Debt Puzzle”, in eds. Aghion, Phillipe, Frydman, Roman, Stiglitz, Joseph, and Woodford, Michael, *Knowledge, Information, and Expectations in Modern Economics: In Honor of Edmund S. Phelps*, forthcoming.

Mandell, Lewis, *The Credit Card Industry: A History* (Twayne: Boston, MA), 1990.

O’Donoghue, Ted D., and Rabin, Matthew, “Choice and Procrastination”, *Quarterly Journal of Economics*, 116/1(February 2001), 121-160.

White, K. J., “The Effect of Bank Credit Cards on the Household Transactions Demand for Money”, *Journal of Money, Credit, and Banking*, 8(1976), 51.

Figure 1. Revolving Credit Per Capita (2001 Dollars)



Source: Series G-19, Board of Governors of the Federal Reserve System

Figure 2. Trends in Bank Card Use, Affected vs. Unaffected States

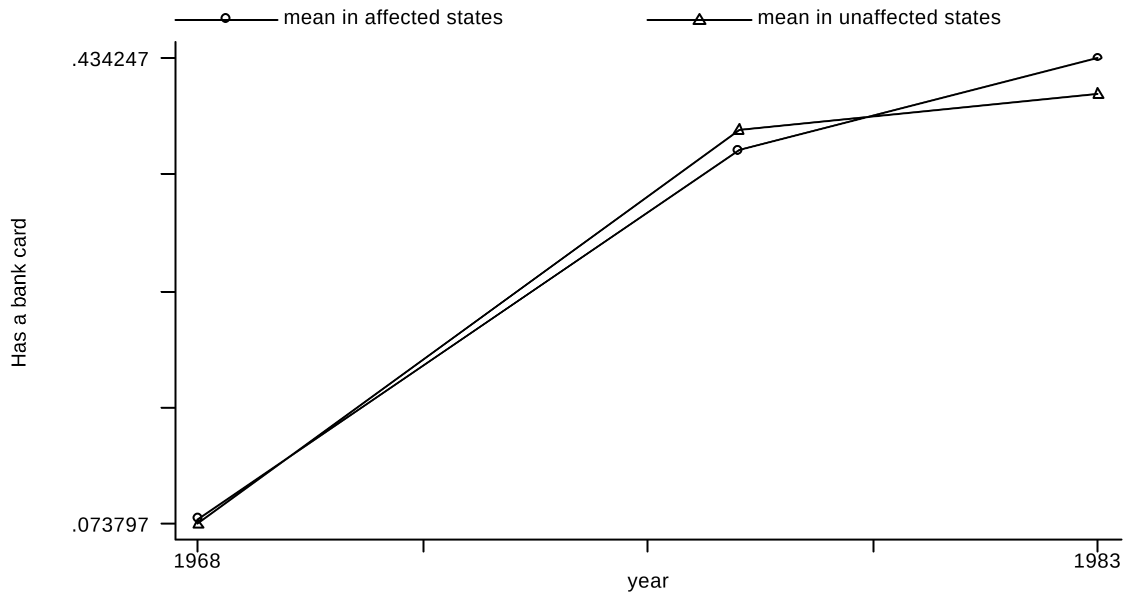
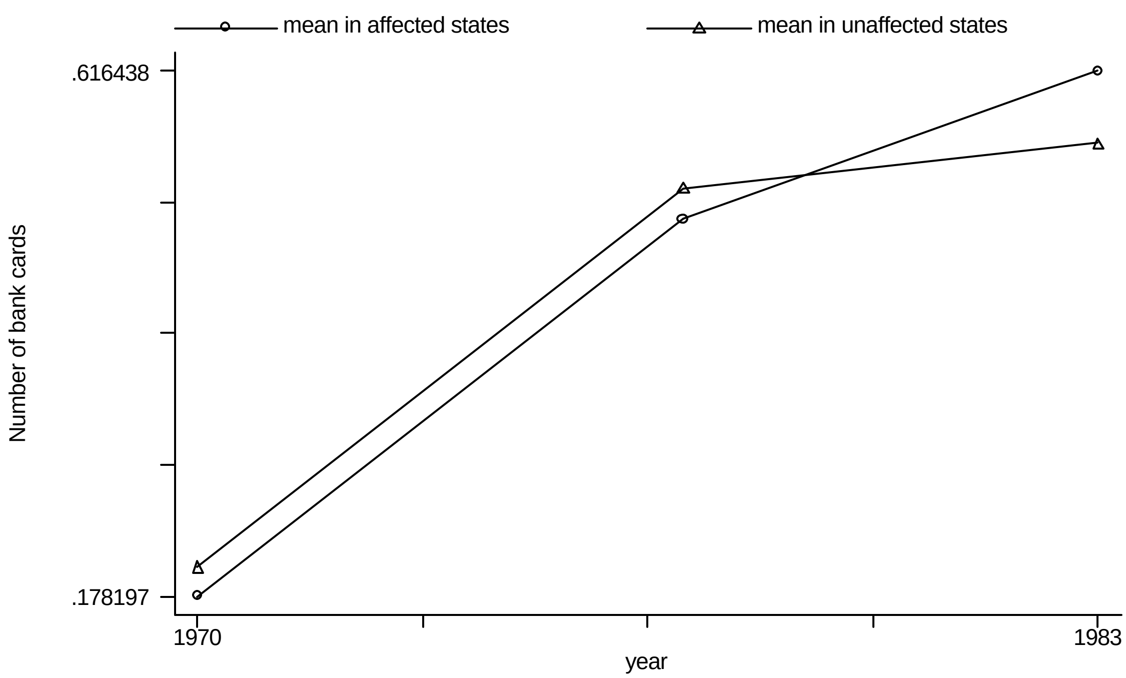


Figure 2 compares pre-deregulation trends and post-*Marquette* breaks in bank card use across affected vs. unaffected states. All data points are group-year means from the given year's Survey of Consumer Finances, where group membership is based on whether the household lived in a state that had a binding usury ceiling on bank cards prior to deregulation ("affected" states), or in an "unaffected" state that did not. The top figure presents the proportion of households holding a bank card, the middle figure the mean number of bank cards held by households, and the bottom figure the proportion of households that reported carrying a balance (paying interest) on a bank card in the month prior to the survey date.



.

Figure 2, continued

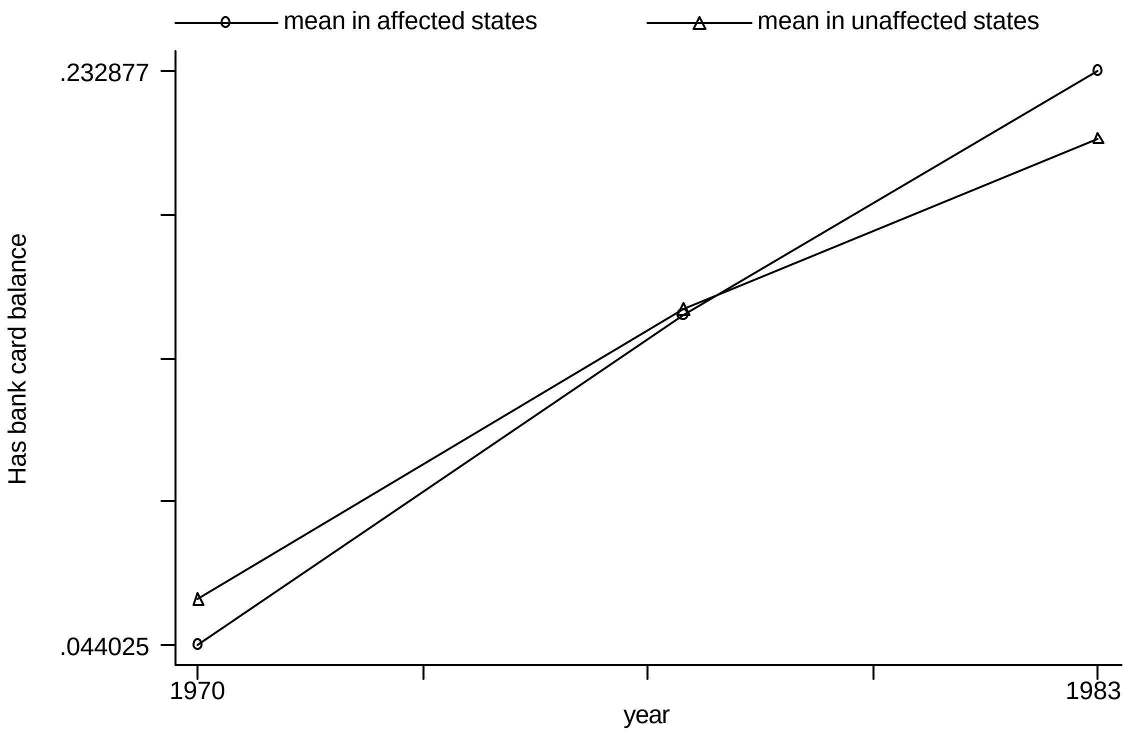


Figure 3. Trends in Falsification Variables, Affected vs. Unaffected States

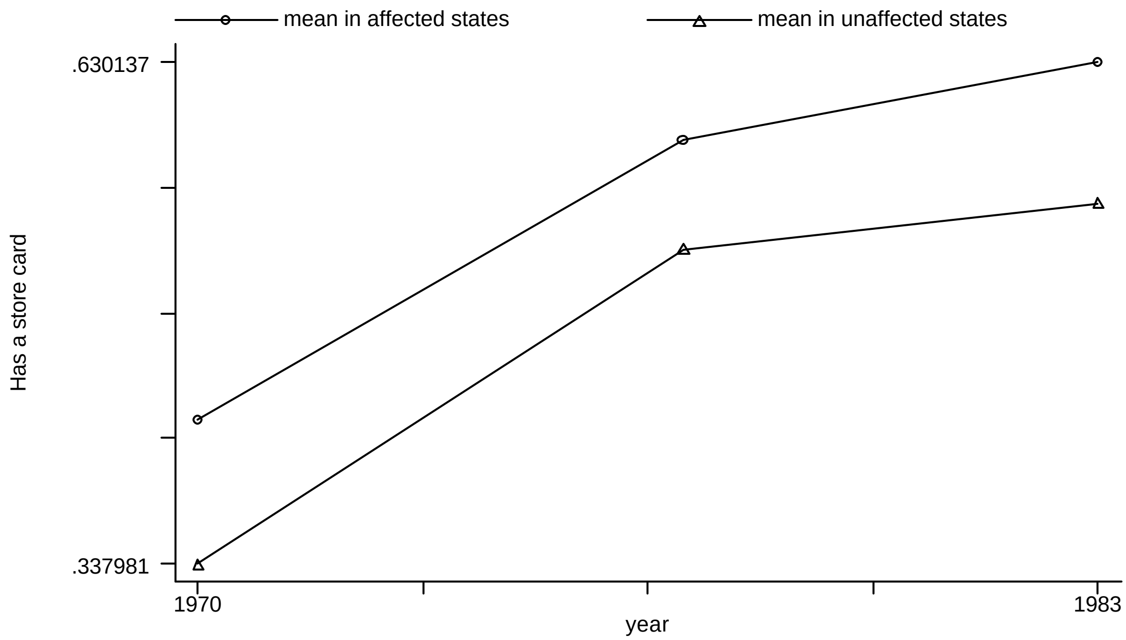


Figure 3 compares pre-deregulation trends and post-*Marquette* breaks (or lack thereof) in two falsification variables across affected vs. unaffected states. We expect the relative trends in these variables to be unperturbed by *Marquette*. As in Figure 2, all data points are group-year means from the given year's Survey of Consumer Finances, where group membership is based on whether the household lived in a state that had a binding usury ceiling on bank cards prior to deregulation ("affected" states), or in an "unaffected" state that did not. The top figure presents the proportion of households holding a store card, and the bottom figure the mean interest rate reported by households on first home mortgages.

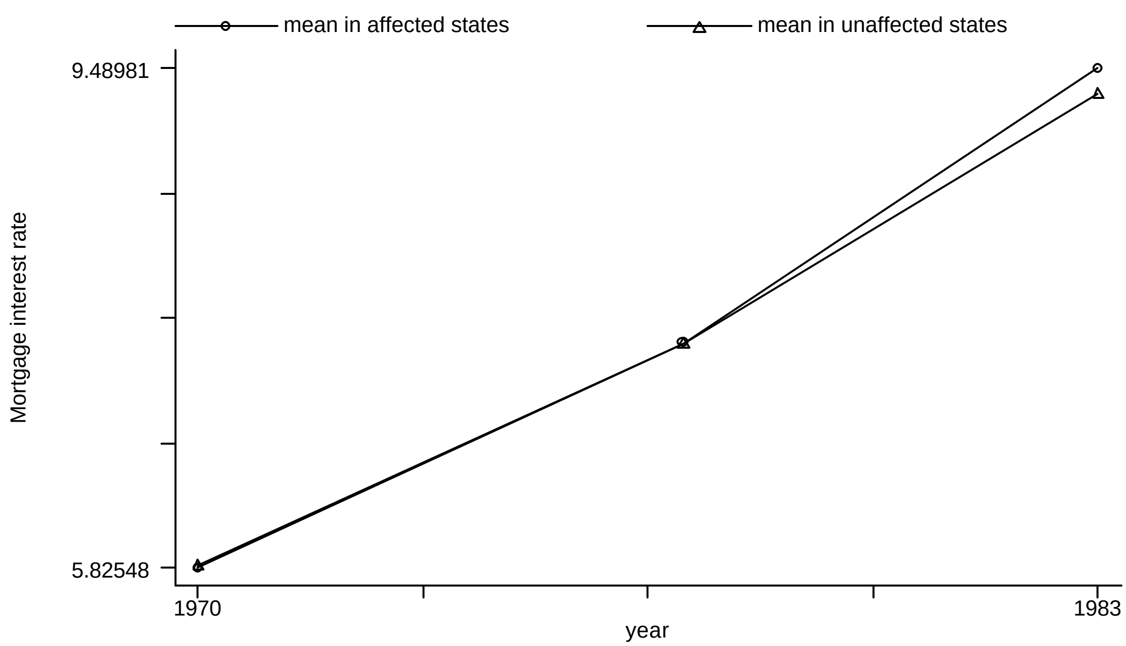


Table 1. Summary Statistics, Selected Variables from the 1977 & 1983 SCFs

Variable	1977	1983	1977 affected	1977 unaffected	1983 affected	1983 unaffected
Any bank card	.376	.412	.363	.380	.434	.407
	(.485)	(.492)	(.481)	(.486)	(.496)	(.491)
Number of bank cards	.515	.568	.493	.521	.616	.556
	(.726)	(.746)	(.715)	(.729)	(.776)	(.738)
Paying interest on bank card	.155	.215	.153	.155	.233	.211
	(.361)	(.411)	(.360)	(.362)	(.422)	(.408)
Interest rate on bank card	16.4	17.9	14.4	17.0	17.0	18.1
	(3.4)	(3.0)	(3.8)	(3.1)	(3.2)	(2.9)
Interest rate on credit card	16.2	17.6	14.1	16.8	16.2	17.9
	(3.6)	(3.5)	(3.7)	(3.4)	(4.1)	(3.3)
Any credit card	.597	..627	.643	.585	.682	.613
	(.491)	(.484)	(.479)	(.493)	(.466)	(.487)
Any savings account	.784	.611	.826	.773	.633	.606
	(.411)	(.487)	(.380)	(.419)	(.482)	(.489)
Any CD	.138	.197	.145	.137	.226	.190
	(.346)	(.398)	(.353)	(.343)	(.419)	(.393)
Any stock	.255	.286	.279	.248	.301	.282
	(.436)	(.452)	(.449)	(.432)	(.459)	(.450)
Male head	.775	.738	.762	.779	.742	.737
	(.418)	(.440)	(.426)	(.415)	(.438)	(.440)
High school educated or less	.323	.291	.298	.338	.273	.295
	(.470)	(.454)	(.458)	(.473)	(.446)	(.456)
Unemployed head	.029	.075	.038	.027	.060	.079
	(.168)	(.263)	(.191)	(.161)	(.238)	(.269)
Wife works	.327	.321	.355	.320	.335	.317
	(.469)	(.467)	(.479)	(.467)	(.472)	(.465)
Age of head	46.9	46.6	47.0	46.9	46.8	46.5
	(17.3)	(17.3)	(17.5)	(17.2)	(17.1)	(17.4)
Less than $7500 income ($1976)	.223	.325	.208	.227	.316	.327
	(.416)	(.469)	(.407)	(.419)	(.465)	(.469)
Gini coefficient on income, state	.399	.409	.401	.399	.403	.410
	(.019)	(.017)	(.022)	(.018)	(.015)	(.017)
Employment rate, state	.463	.487	.454	.465	.480	.489
	(.026)	(.036)	(.023)	(.026)	(.034)	(.036)
N	2417	3665	504	1913	730	2935

Notes to Table 1.

Cells present (sub-)sample means, with standard deviations in parentheses. Selected cells include the number of nonmissing observations; 1983 variables have few if any missing observations due to imputations by Federal Reserve Board staff. Bank card interest rates are observed only for those households with a bank card but not store card. "Affected" households are those living in states that had binding usury ceilings on bank credit card interest rates as of the 1977 SCF. "Unaffected" households are those living in states that did not have binding usury ceilings as of the 1977 SCF. The last row gives the number of observations in the full (sub-)sample (I exclude the high income oversample from the 1983 data, and observations from Oregon or with unknown state of residence from the 1977 data).

Table 2. The Effect of Interest Rate Deregulation on Credit Card Interest Rates

	1	2	3	4	5
Bank card interest rate	1.39 (0.37) 1936	1.34 (0.39) 1936	1.36 (0.40) 1933	1.25 (0.35) 1933	1.18 (0.36) 1933
Log of bank card interest rate	0.099 (0.027) 1936	0.093 (0.029) 1936	0.094 (0.03) 1933	0.084 (0.028) 1933	0.077 (0.028) 1933
Bank or store card interest rate	0.969 (0.451) 2755	0.884 (0.454) 2755	0.944 (0.452) 2750	0.893 (0.5) 2750	0.845 (0.485) 2750
Log of bank or store card interest rate	0.042 (0.037) 2755	0.034 (0.037) 2755	0.041 (0.037) 2750	0.038 (0.043) 2750	0.034 (0.042) 2750
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

Each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate; column 5 adds interactions between the year indicator and each of the household covariates.

Table 3. The Effect of Interest Rate Deregulation on Credit Card Possession

	1	2	3	4	5
Has bank card	0.055 (0.025) 6059	0.047 (0.025) 6059	0.035 (0.02) 6037	0.039 (0.018) 6037	0.038 (0.02) 6037
Number of bank cards (topcoded at 2)	0.103 (0.043) 6059	0.092 (0.043) 6059	0.071 (0.039) 6037	0.070 (0.031) 6037	0.068 (0.031) 6037
Number of bank cards (topcoded at 3)	0.118 (0.047) 6059	0.107 (0.046) 6059	0.083 (0.040) 6037	0.080 (0.033) 6037	0.077 (0.033) 6037
Has credit card	0.011 (0.021) 6063	0.002 (0.022) 6063	-0.002 (0.015) 6041	0.004 (0.013) 6041	0.004 (0.013) 6041
Number of credit cards (topcoded at 5)	0.133 (0.085) 6033	0.099 (0.089) 6033	0.044 (0.062) 6012	0.054 (0.062) 6012	0.066 (0.060) 6012
Number of credit cards (topcoded at 10)	0.350 (0.133) 6033	0.306 (0.139) 6033	0.209 (0.104) 6012	0.232 (0.088) 6012	0.252 (0.084) 6012
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

Each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate; column 5 adds interactions between the year indicator and each of the household covariates.

Table 4. Reduced Form Effects of Interest Rate Deregulation on Credit Card Borrowing

	1	2	3	4	5
Has bank card balance	0.028 (0.016) 6082	0.029 (0.016) 6082	0.028 (0.015) 6057	0.030 (0.013) 6057	0.029 (0.012) 6057
Has credit card balance	0.026 (0.021) 6082	0.027 (0.020) 6082	0.033 (0.019) 6057	0.031 (0.015) 6057	0.026 (0.016) 6057
Reports typically carrying a balance	0.012 (0.022) 6020	0.012 (0.021) 6020	0.022 (0.020) 5997	0.029 (0.015) 5997	0.028 (0.016) 5997
Log of bank card balance	-0.185 (0.084) 1162	-0.250 (0.080) 1162	-0.249 (0.072) 1161	-0.327 (0.088) 1161	-0.351 (0.107) 1161
Log of credit card balance	-0.054 (0.116) 1885	-0.065 (0.112) 1885	-0.079 (0.090) 1882	-0.077 (0.089) 1882	-0.058 (0.096) 1882
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

As in Tables 2 and 3, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate; column 5 adds interactions between the year indicator and each of the household covariates.

Table 5. Falsification: Testing for "False Positives"

	1	2	3	4	Mean of Dependent Variable
Has store card	0.015	0.007	0.003	0.012	.585
	(0.022)	(0.023)	(0.016)	(0.011)	(.493)
	6062	6062	6040	6040	501
First mortgage interest rate	0.234	0.183	0.144	0.142	7.47
	(0.173)	(0.169)	(0.154)	(0.144)	(1.77)
	2190	2190	2187	2187	
Log of mortgage interest rate	0.026	0.019	0.013	0.016	
	(0.022)	(0.023)	(0.021)	(0.019)	
	2190	2190	2187	2187	
Consumer credit is a "good thing"	-0.015	-0.017	-0.022	-0.045	.407
	(0.036)	(0.022)	(0.039)	(0.026)	(.491)
	6018	6018	5995	5995	
"All right to borrow" to finance discretionary items	-0.041	-0.041	-0.046	-0.067	.627
	(0.025)	(0.026)	(0.026)	(0.02)	(.484)
	6008	6008	5984	5984	
State and year effects?	Y	Y	Y	Y	
Race and age of household head?	N	Y	Y	Y	
Complete set of household covariates	N	N	Y	Y	
State-level covariates	N	Y	Y	Y	

As in previous tables, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate. The key difference in this table, of course, is that we expect the deregulation variable to be uncorrelated with the dependent variables tested here. Dependent variable means are calculated for observation in affected states only, in 1977, to provide a sense of the "base period" value.

Table 6. Falsification: Timing Tests

	1	2	3	4	5
Has bank card	-.039 (.026)	-.024 (.028)	-.005 (.028)	.020 (.021)	-.014 (.019)
Number of bank cards (topcoded at 2)	-.045 (.042)	-.023 (.046)	.010 (.046)	.052 (.036)	-.003 (.032)
Number of bank cards (topcoded at 3)	-.051 (.043)	-.029 (.046)	.005 (.046)	.049 (.037)	-.009 (.032)
Has credit card	-.023 (.033)	-.001 (.035)	.019 (.028)	.019 (.030)	.009 (.028)
Number of credit cards (topcoded at 5)	-.107 (.112)	-.030 (.115)	.099 (.099)	.103 (.104)	-.002 (.105)
Number of credit cards (topcoded at 10)	-.228 (.122)	-.123 (.121)	.069 (.119)	.100 (.129)	-.081 (.136)
Has bank card balance	-.03 (.025)	-.029 (.024)	-.024 (.024)	-.008 (.023)	-.018 (.021)
Has credit card balance	-.013 (.029)	-.014 (.027)	-.012 (.027)	-.019 (.028)	-.016 (.028)
Observations	4084	4084	4063	4063	4063
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

Notes to Table 6 overleaf.

Notes to Table 6.

Results are presented for estimates of equation (1), the basic reduced form model, using 1970 and 1977 data, with households in states that still faced binding usury ceilings in 1977 falsely labeled as having deregulated. We therefore expect the deregulation variable here to be uncorrelated with card usage. Sample sizes are lower than for previous estimates due to the elimination of 10 states that made material changes to their bank card interest rate usury laws between 1970 and 1977. As in previous tables, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate.

Table 7. Reduced Form Effects of Interest Rate Deregulation on Asset Choice

	1	2	3	4
Checking account balances above 1977 median	0.032	0.023	0.009	0.014
	(0.028)	(0.031)	(0.026)	(0.025)
	5775	5775	5764	5764
Savings account balances above 1977 median	-0.033	-0.037	-0.041	-0.053
	(0.026)	(0.025)	(0.024)	(0.022)
	5683	5683	5672	5672
Has savings account	-0.019	-0.025	-0.021	-0.007
	(0.024)	(0.025)	(0.021)	(0.019)
	6027	6027	6012	6012
Has government savings bond	0.025	0.019	0.016	-0.005
	(0.02)	(0.02)	(0.021)	(0.019)
	6025	6025	6007	6007
Has certificate of deposit	0.024	0.026	0.019	0.020
	(0.017)	(0.017)	(0.017)	(0.017)
	6017	6017	5999	5999
State and year effects?	Y	Y	Y	Y
Race and age of household head?	N	Y	Y	Y
Complete set of household covariates	N	N	Y	Y
State-level covariates	N	N	N	Y

As in Tables 2, 3, 5, and 6, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate.