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Reexamining the 'Role of the Community Reinvestment Act in Mortgage Supply and the U.S. Housing Boom'

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Abstract

Concerns have lingered since the 2007 subprime crisis that government housing policies promote risky mortgage lending. The first peer-reviewed evidence of a causal effect was published by the *Review of Financial Studies* in a paper (Saadi, 2020) linking the crisis to changes in the Community Reinvestment Act (CRA) in 1995. A review of that paper, however, shows that it misrepresents the policy changes as having taken effect in mid-1998, 2.5 years after they were implemented. When the correct timing is used, a similar analysis yields no evidence of a relationship between CRA and riskier mortgage lending. Instead, the results are shown to reflect an unrelated confounding event, the first collapse of the U.S. subprime mortgage market following Russia's debt default in August 1998.

Keywords: Community Reinvestment Act (CRA), house prices, mortgage lending, subprime crisis

JEL Classification: G21, G28, R38

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1. Introduction

Since the U.S. subprime mortgage crisis in 2007, concerns have lingered that government housing policies aimed at improving mortgage access for lower-income households lead lenders to originate riskier mortgages. Some have argued that the additional housing demand from borrowers who would not have qualified for mortgage credit without these government programs helped inflate the housing bubble during the mid-2000s and, when those borrowers were unable to repay their loans, triggered the subprime crisis (Wallison 2009; Hendershott, Henderschott, and Shilling 2010).

Two government programs, in particular, have been identified as potential causes of the crisis, the Community Reinvestment Act (CRA) of 1977 and the Affordable Housing Goals applied to Fannie Mae and Freddie Mac. Both programs promote lending in low- and moderate-income income (LMI) communities in similar ways. The CRA encourages commercial banks and thrifts (hereafter "banks") to make LMI loans and the GSE goals require Fannie Mae and Freddie Mac to devote a minimum portion of their purchases to LMI loans.

Despite the lingering concerns, the empirical evidence to support a causal relationship between these programs and riskier mortgage lending has largely involved general associations between neighborhood income levels and delinquency rates (Wallison 2009; Demyanyk and Van Hemert 2011). By themselves, such associations are insufficient to establish a causal relationship. Most studies that look for one fail to find evidence that either program leads to riskier lending or played a meaningful role in the subprime crisis (Bhutta and Canner 2009; Ghent, Hernández-Murillo, and Owyang 2015; Avery and Brevoort 2015; Ringo 2022). The few studies that have offered evidence have proved problematic upon closer examination.[1]

[1]Early work on this topic by Pinto (2010) and Wallison (2009) was carefully examined by the Financial Crisis Inquiry Commission (2011) who found little support for the idea that either program materially contributed to the crisis. Specifically, the Financial Crisis Inquiry Commission (2011, p. xxvii) finds that the GSE goals "only contributed marginally to Fannie's and Freddie's participation" in risky mortgages and that "the CRA was not a significant factor in subprime lending or the crisis." Similarly, an analysis by Agarwal et al. (2012) showing that banks make riskier loans around their CRA exams, was found by a group of researchers with substantial expertise on CRA issues to be based on a faulty understanding of the CRA examination process (Reid et al. 2013).

The first peer-reviewed evidence of a relationship between government housing policy and the subprime crisis was recently published in the *Review of Financial Studies*. This new paper, Saadi (2020), explores how the changes made to the CRA's implementing regulations in 1995 affected mortgage lending and house price appreciation. Using a difference-in-differences approach around the introduction of the new rules, the author finds that the CRA increased mortgage lending by banks in the LMI neighborhoods that are the focus of the CRA relative to a control group of credit unions and independent mortgage companies ("nonbanks") that were not covered by CRA. Based on this and subsequent analyses, the author concludes that the CRA is a "welfare-decreasing policy" that caused "significant shifts in the volume and riskiness of the mortgage market and a more severe crash in the housing market" (Saadi 2020, p. 5293).

A review of the analysis in Saadi (2020) reveals that it is based on a material error in which the 1995 revisions to the CRA's regulations are misrepresented as having been implemented 2.5 years later than they actually were. This error appears to be the result of the author relying on incorrect information from Agarwal et al. (2012) rather than original source material. Both the implementation timeline provided in the 1995 *Federal Register* notice that announced the new rules and contemporaneous CRA performance evaluations show that the new evaluation framework applied to mortgage originations starting January 1, 1996.

In this paper, I review the evidence presented by Saadi (2020) on the role of the CRA in inflating the U.S. house price bubble. After replicating the results in the earlier paper, I show that a similar difference-in-difference analysis centered around the correct effective date for the CRA revisions provides little evidence that the new CRA rules significantly increased bank lending in LMI neighborhoods. In the three years after the new rules went into effect, banks and nonbanks exhibited almost identical increases in LMI lending.

I also propose and evaluate an alternative explanation for the changes in mortgage lending that Saadi (2020) uses to link the CRA to the start of the U.S. house price bubble. In August 1998, when Russia defaulted on its debt and Long Term Capital Management nearly

collapsed, a flight to quality was triggered in financial markets that led to the first collapse of the U.S. subprime mortgage market (Sabry and Schopflocher 2007; Financial Crisis Inquiry Commission 2011). The failure or acquisition of subprime lenders during this period, who were disproportionately nonbanks, shifted LMI lending into the banking sector.

I show that the changes in mortgage lending after 1998 were more likely caused by this collapse of the subprime mortgage market than a delayed response to the new CRA regulations. Because CRA's lending test focuses exclusively on lending within designated *assessment areas*, banks have no incentive under CRA to increase their LMI lending outside of those areas. The collapse of the subprime market, on the other hand, would have provided opportunities for banks to increase their LMI lending both within and outside of their assessment areas. The results from difference-in-differences estimations suggest that, within their assessment areas, LMI lending by banks grew by small and statistically insignificant amounts. Outside of their assessment areas, however, the estimated increases in LMI lending were larger and statistically significant. These results are inconsistent with CRA having been the driving force behind increased bank LMI lending after 1998.

These results suggest that the findings reported in Saadi (2020) are incorrect. They rely on a material error regarding when the 1995 CRA regulations were implemented and ignore a confounding event that better explains the changes in LMI lending by banks after 1998. Ultimately, the evidence suggests that the CRA did not materially contribute to the U.S. house price bubble or its ensuing collapse.

The remainder of this paper reviews the evidence presented by Saadi (2020), documents the additional analyses undertaken, and explains how the conclusions were reached. The next two sections provide background information about CRA, including the implementation dates for the 1995 regulatory changes, and describe the data used, respectively. Section 4 replicates the difference-in-differences analysis from Saadi (2020) and shows how an identical analysis around the correct implementation dates alters the conclusions. Section 5.2 shows how the changes to mortgage lending after 1998 can better be described by the collapse of the U.S. subprime mortgage market. Section 6 concludes.

2. The Community Reinvestent Act

2.1. Background

The Community Reinvestment Act was passed in 1977 amid ongoing concerns about redlining, a practice in which lenders do not lend to creditworthy borrowers in particular neighborhoods (Barr 2005). Specifically, passage of the CRA was motivated by concerns that banks were collecting deposits in LMI neighborhoods that they were using to fund loans in higher-income areas and that this capital flight out of distressed communities was contributing to urban decay (Canner and Cleaver 1980).

Building upon earlier federal efforts to expand credit availability, including the Fair Housing Act and Equal Credit Opportunity Act, the CRA established that banks have an ongoing affirmative obligation to help meet the credit needs of their local communities. The CRA tasked the federal banking agencies with encouraging banks as part of regular examinations to satisfy these obligations in a manner consistent with their safe and sound operation. CRA ratings assigned during these exams are considered when approving applications for new branch locations or bank mergers.

In the early days of CRA, a uniform examination applied to all covered banks that evaluated performance on 12 assessment factors that generally focused on a bank's processes for meeting community credit needs.[2] In July 1993, President Clinton requested that the federal banking agencies streamline the regulations that implement the CRA to reduce compliance burdens and make the examinations more performance-based. The final rule implementing these changes was published in the *Federal Register* two years later on May 4, 1995.[3]

Among the changes instituted in 1995 was the creation of a *Retail Lending Test* that, among other things, evaluates each bank's record of originating or purchasing mortgages in

[2]For example, the first of the 12 assessment factors, Factor A, concerned the banks' efforts to ascertain its community's credit needs through community outreach or other means.

[3]The *Federal Register* is the official gazette of the United States Government. It is used by government agencies to provide legal notice of new regulations and for other purposes.

the low- or moderate-income neighborhoods within the *assessment areas* around its branch network. Each census tract's income level was categorized based on its median family income (MFI) relative to the MFI of the surrounding area, which is the metropolitan area for urban tracts and the non-metropolitan portion of the state for rural tract. Low-income neighborhoods are those tracts whose relative-income ratios, based on data from the most recently available Decennial Census, were below 50 percent and moderate-income tracts are those with relative incomes between 50 and 80 percent.

2.2. When Did the 1995 CRA Changes Become Effective?

Most provisions of the final rule became effective on July 1, 1995. However, because the newly instituted lending test required the collection of new data (mostly about small business loans, farm loans, and community development lending), industry commentators requested longer transition periods to help lenders develop the necessary procedures to comply with the new requirements. They also requested that data be collected on a calendar-year basis and that new collections not start mid-year. As a result, a separate set of effective dates applied to CRA's new lending test. These were summarized in the preamble to the final rule as follows (Community Reinvestment Act Regulations 1995, p. 22176).

> In light of these comments...the data collection requirements set forth in the final rule will become effective January 1, 1996. The reporting requirements will be effective January 1, 1997. Evaluations under the lending, investment, service, and community tests will begin July 1, 1997, in order to allow the agencies to use the new data. However, evaluations under the small bank performance standards, which do not utilize new data, will begin January 1, 1996.

Out of context, these different dates could cause confusion about when lending by large banks first became subject to the new lending test. The different effective dates, however, were all necessary to evaluate loans originated on January 1, 1996 or later to the new lending test.

The reason the different dates were necessary is that the lending test applied to large banks requires comparing a bank's lending within its assessment areas with lending of other

institutions in the same geographic areas. This required not only information from the bank being examined, but from all lenders in those areas. Data collection commenced on January 1, 1996, but because the data were being collected on a calendar year basis, loans originated in 1996 could not be reported until January 1, 1997 at the earliest, which is why that was given as the effective date for the reporting requirements. Reporting institutions were given a few months to supply the required data after which the federal banking agencies needed time to compile and validate the data submissions. To provide that time, the new evaluations were scheduled to begin July 1, 1997 expressly to allow the newly collected 1996 data to be examined. These later dates were all necessary to start evaluating lending on January 1, 1996.

In contrast, the lending test applied to small banks, those with assets of $250 million or less, did not involve comparing the bank's lending to that of other lenders. As a result, the new examinations could be conducted using only data provided by the bank being examined. There was no need to wait for the market-wide data that the yearly collections were providing. For these banks, the new examinations began on January 1, 1996.

Table 1: Lending Periods Examined Under CRA's Lending Test for the 10 Largest Banks Examined in the 12 Months After the Deadline for Large Bank Examinations to Begin

Table shows information selected from the CRA Performance Evaluations from the examinations of the largest banks whose examination dates were July 1, 1997 through June 30, 1998. 'Agency' refers to the federal banking agency that conducted the exam, FRB = Federal Reserve Board, OCC = Office of the Comptroller of the Currency. All information, except the lending test evaluation period, was compiled from the Federal Financial Institutions Examination Council's (FFIEC's) Interagency CRA Ratings Database, available online at http://ffiec.gov/craratings. Lending test evaluation periods were compiled from the CRA Performance Evaluations written following each of the exams. The table excludes Home Savings of American, Federal Savings Bank, because it was acquired by Washington Mutual before the exam process ended and, while a rating was assigned, no performance evaluation was written.

				Assets	Lending Test Eval. Period	
Name	Agency	ID	Exam Date	($billions)	Start	End
Bank of New York	FRB	541101	1997-08-18	57.5	1996-01-01	1997-06-30
Summit Bank	FRB	80703	1997-09-30	20.9	1996-04-01	1997-05-30
First National Bank of Chicago	OCC	8	1997-11-03	56.1	1996-01-01	1997-09-30
Mellon Bank, N.A.	OCC	6301	1997-11-26	37.8	1996-01-01	1997-06-30
South Trust Bank, N.A.	OCC	14569	1997-12-19	29.6	1996-01-01	1997-06-30
Union Bank of California, N.A.	OCC	21541	1997-12-31	30.4	1996-01-01	1997-12-31
Fleet Bank, N.A.	OCC	374	1998-02-17	24.6	1996-01-01	1997-12-31
Fleet National Bank	OCC	1338	1998-02-23	63.9	1996-01-01	1997-12-31
Wells Fargo Bank, N.A.	OCC	1741	1998-03-31	87.2	1996-01-01	1998-03-31
Crestar Bank	FRB	47920	1998-05-11	25.9	1996-05-20	1998-03-31

Corroboration that the new evaluations applied to loan originations beginning in 1996 can be found in the written performance evaluations that document CRA exams. Table 1 provides details from the evaluations of the 10 largest banks subject to the new large-bank lending test whose examinations commenced in the first 12 months after the new evaluation regime was scheduled to begin for large banks (July 1, 1997 to June 30, 1998). For each bank the exam date is listed, along with the evaluation period used to conduct the lending test. In all 10 cases, the evaluation period starts in 1996 and for 8 of the 10 banks it starts exactly on January 1, 1996.[4] Even though the exams may have started later, loans were subject to the new lending test starting January 1, 1996.

In contrast, Saadi (2020, p. 5297) asserts that CRA's "new regime became effective in July 1997 for small banks (less than $250 million in assets) and in July 1998 for large banks." As the source of these dates, the author cites a 2012 National Bureau of Economic Research (NBER) working paper (Agarwal et al. 2012). That this NBER working paper might not be a reliable source of information was pointed out by Reid et al. (2013, p. 5), who conclude it is based on a "deeply flawed" understanding of the CRA exam process. The authors of the NBER working paper do not provide a source for these dates, which are immaterial to their analysis. The dates, however, are clearly inconsistent with the timeline provided in the *Federal Register* notice announcing the final rule. Lending, both for small and large banks, became subject to the new evaluations starting on January 1, 1996. In fact, none of the provisions of the 1995 CRA rule changes became effective later than July 1, 1997.[5]

[4]The performance evaluations for the two banks where the evaluation period did not start on January 1st, Crestar Bank and Summit Bank, do not reveal why the evaluation period started a few months into the year.

[5]Indeed, a search of the *Federal Register* notice for the final rule for the term "1998" returns zero results. A search for "1997" returns 25 results and a search for "1996" returns 30.

3. Data and Empirical Approach

3.1. Replication Dataset

The analyses documented in this paper are based on two datasets. The first, the *replication dataset*, attempts to reproduce, to the extent possible, the data used by Saadi (2020). The replication dataset was constructed using data on home purchase lending collected pursuant to the Home Mortgage Disclosure Act (HMDA) for 1993 to 2002.[6] Loans were aggregated annually to the census tract level for banks and nonbanks.[7] The aggregated information includes the total dollar amount, number, and average size of originations in each tract made by banks and nonbanks.

Additional variables were created from other sources. The 2000 Census provided characteristics of each census tract including its median family income (MFI), population, total housing units, and housing vacancy rate. It also provided the MFI for the metropolitan area of the tract, specifically the Metropolitan Statistical Area (MSA) or, in larger areas, the Consolidated Metropolitan Statistical Area (CMSA).

Income and population growth rates were derived for each county from the 1990 and 2000 Decennial Censuses. Employment growth rates were calculated annually at the county level using the *County Business Patterns* data from the Census Bureau. Latent demand was calculated as the share of home purchase applications that were denied from 1993-1997. Finally, MSA-level housing supply elasticities were collected from Saiz (2010).

Observations are limited to those tracts in MSAs or CMSAs with housing supply elasticities from Saiz (2010). Additionally, census tracts with 11-digit tract codes that are not

[6]The HMDA data were collected from the publicly available files maintained by the National Archives. For each year of HMDA data, the National Archives retains two versions, unhelpfully named the *final* and *ultimate* versions. This study is based on the ultimate versions, which incorporate resubmissions made by lenders after the initial public release of the data.

[7]The definitions of "banks" and "nonbanks" are identical in all but name to the "regulated" and "unregulated" categories used by Saadi (2020). I prefer the bank/nonbank terminology because all lenders in HMDA are regulated to some extent; for example, they are all covered by the regulations implementing HMDA. Banks are identified as those HMDA reporters who are supervised by the Federal Reserve, Office of the Comptroller of the Currency (OCC), Office of Thrift Supervision (OTS), or the Federal Deposit Insurance Corporation (FDIC). Nonbanks are identified as the lenders who report HMDA data to the National Credit Union Administration (NCUA) or the Department of Housing and Urban Development (HUD).

Table 2: A Comparison of the Summary Statistics for Select Variables from the Replication and Corrected Datasets and those Reported by Saadi (2020)

Table shows summary statistics for the dependent variables and income variables used in the analysis. Summary statistics reported by Saadi (2020) are reproduced as the *Saadi Sample*. Statistics from the replication dataset are reported as the *Replicated Sample* and those from the corrected dataset are reported as the *Corrected Sample*. Census-tract-level median family income (MFI) variables are shown from the 1990 and 2000 Censuses, respectively.

Variable	Sample	N	Mean	SD	Min	Max
Total mortgages (millions)	Saadi	288,804	2.326	3.254	0.001	182.6
	Replicated	294,496	2.356	3.488	0.001	372.0
	Corrected	420,411	3.536	6.112	0.001	639.6
Average mortgage (millions)	Saadi	288,804	0.091	0.052	0.001	2.101
	Replicated	294,496	0.092	0.132	0.001	26.81
	Corrected	420,411	0.105	0.118	0.001	26.81
Number of mortgages	Saadi	288,804	25.30	28.51	1.000	1,483
	Replicated	294,496	25.34	28.28	1.000	1,483
	Corrected	420,411	31.61	41.27	1.000	4,029
Tract MFI, 2000 (thousands)	Saadi	288,804	40.51	9.747	20.83	57.06
	Replicated	294,496	40.83	9.764	20.81	57.06
	Corrected	420,361	48.76	15.94	2.499	200.0
Tract MFI, 1990 (thousands)	Saadi	NA	NA	NA	NA	NA
	Replicated	294,288	30.70	8.520	4.999	126.5
	Corrected	420,411	35.43	10.16	14.11	55.52

Table 3: A Comparison of the Summary Statistics for Control Variables from the Replication and Corrected Datasets and those Reported by Saadi (2020)

Table shows summary statistics for the control variables used in the difference-in-differences estimations in the paper, which constitute X_{it} in model specification in Equation (1). Summary statistics reported by Saadi (2020) are reproduced as the *Saadi Sample*. Statistics from the replication dataset are reported as the *Replicated Sample* and those from the corrected dataset are reported as the *Corrected Sample*. *Population* is the population of the tract from the 2000 Census.

Variable	Sample	N	Mean	SD	Min	Max
Population	Saadi	288,804	4,262.3	2,119.2	4.000	36,146
	Replicated	294,496	4,242.02	2,100.5	4.000	36,146
	Corrected	420,411	4,414.5	2,140.8	10.00	36,146
Latent demand	Saadi	288,804	15.56	8.174	0.000	50.00
	Replicated	294,496	15.50	8.415	0.000	73.00
	Corrected	420,411	13.17	7.719	0.000	60.71
Vacancy rate	Saadi	288,804	8.378	6.537	0.000	84.23
	Replicated	294,496	7.607	5.817	0.000	72.28
	Corrected	420,411	6.501	5.488	0.000	72.28
Housing units	Saadi	288,804	7.282	0.556	0.000	9.306
	Replicated	294,496	1.732	0.868	0.002	11.52
	Corrected	420,411	1.799	0.882	0.001	11.52
Employment growth	Saadi	288,804	1.660	3.461	-24.69	31.77
	Replicated	294,496	1.673	3.512	-35.13	148.6
	Corrected	420,411	1.748	3.425	-30.00	68.42
Income growth	Saadi	288,804	38.62	10.03	20.29	81.33
	Replicated	294,496	38.60	9.753	20.66	88.01
	Corrected	420,411	37.99	22.94	-87.77	596.0
Population growth	Saadi	288,804	11.37	13.27	-12.23	66.74
	Replicated	294,496	11.37	13.51	-12.23	123.2
	Corrected	420,411	10.22	43.84	-99.70	2,816
Elasticity	Saadi	288,804	1.670	0.934	0.630	5.450
	Replicated	294,496	1.675	0.943	0.630	5.450
	Corrected	420,411	1.584	0.908	0.600	5.450

in both the 1990 and 2000 Decennial Censuses are excluded. Finally, only tracts with MFIs from the 2000 Census that would have qualified as being LMI in at least one MSA and as middle- or upper-income ("non-LMI tracts") in another are retained. The resulting dataset, which is summarized in Tables 2 and 3, contains 294,496 observations, slightly more than the 288,804 observations reported by Saadi (2020). For most variables, the replication dataset produces sample means that are similar to those reported by Saadi (2020), though minimum or maximum values are often quite different.

3.2. Corrected Dataset

In constructing the replication dataset, it became apparent that some of the assumptions underlying its construction were inconsistent with the ways in which CRA defines LMI neighborhoods. There were two primary sources of inconsistency.

The first is that Saadi (2020) defines LMI neighborhoods (or what he calls "CRA-eligible tracts") based on tract and surrounding area MFIs from the 2000 Census. Those data only became available at the very end of the sample period and were not used to designated LMI neighborhoods under CRA until 2003, the year after the sample period ends. During the years of the sample, MFIs from the 1990 Census were used to designate LMI areas.

The second inconsistency concerns LMI neighborhood designations in CMSAs. CMSAs are large urban areas with populations over 1 million that are comprised of two or more Primary Metropolitan Statistical Areas (PMSAs). The Dallas-Fort Worth, TX CMSA, for example, was comprised of the Dallas, TX PMSA and the Fort Worth-Arlington, TX PMSA. At the time, the CRA used the PMSA as the surrounding area instead of the CMSA. This means that CRA uses different surrounding-area MFIs for tracts in Dallas and Fort Worth. By using the CMSA as the surrounding area, Saadi (2020) applies a uniform surrounding area MFI to both PMSAs that is correct in neither.

To resolve these inconsistencies, a second dataset, the *corrected dataset*, was constructed using the same data sources and methods as the replication dataset except when doing so was inconsistent with how LMI neighborhoods were designated under CRA. The corrected

dataset uses tract-level MFIs from the 1990 Decennial Census and the surrounding area MFIs at the PMSA level that are published annually by the Federal Financial Institutions Examination Council (FFIEC).[8]

The corrected dataset differs from the replication dataset in an additional way. In some analyses replicated in this paper, Saadi (2020) separately evaluates lending by banks inside and outside of their CRA assessment areas. The previous paper defines a bank's assessment area as the counties where it had offices according to the FDIC's Summary of Deposits. While this should be a reasonable approximation of bank assessment areas in most cases, relying on this approximations muddies the distinction between bank activities within and outside of their assessment areas. While the replication dataset uses these same approximations, the corrected dataset uses the actual bank assessment areas published by the FFIEC.[9]

The main difference between the corrected and replication datasets concerns the number of observations (Figure 2). One of the restrictions imposed on the data is that the sample is limited to census tracts with MFIs that would have qualified as LMI neighborhoods in at least one metropolitan area and as non-LMI neighborhoods in another. By using MFIs at the more granular PMSA level, a wider range of tract-level MFIs are admissible resulting in about 126,000 additional observations. Because of the larger sample size, the summary statistics for the corrected dataset are often significantly different from those in the replication dataset or those reported by Saadi (2020).

Despite the often large differences between the replication and corrected datasets, both generally produce similar results in this study, with one notable exception that will be discussed later. For that reason, most the analysis used here relies on the replication dataset. In cases where there are notable differences between the corrected and replication datasets results from both are provided.

[8]While the PMSA-level MFIs use income information from the 1990 Census, they can change from one year to the next when MSA or PMSA definitions change. To ensure that lending growth rates are not affected by the expanding geographic boundaries of metropolitan areas during the sample, I exclude from the corrected dataset census tracts that were not part of the same MSA or PMSA for the entire 10-year period or those whose LMI designation changed during the sample.

[9]Because CRA assessment areas were introduced by the 1995 changes, they are not available before 1996. For earlier years, I assign assessment areas based on the first reported assessment area for each bank after accounting for mergers.

4. Did the 1995 Changes to CRA Increase LMI Lending by Banks?

4.1. Replication

In this section, I replicate selected results from Table 3 of Saadi (2020) regarding mortgage growth rates at banks and nonbanks around 1998. The purpose of this exercise is to introduce the modelling approach used by Saadi (2020), to verify the replication dataset reasonably reproduces the reported results, and to gauge the extent to which the results are robust to using the corrected dataset.

Using i to denote each census tract, t to denote the year, and Y_{it} to denote the dependent variable, which in these replications will be the natural log of either the dollar amount of mortgage originations or the number of mortgage originations at the census tract level, the model is specified as:

$$Y_{it} = \alpha_s + \gamma_t + LMI_i\beta_1 + LMI_i \times Post_t\beta_2 + MFI_i\beta_3 + MFI_i \times Post_t\beta_4 + X_{it}\Gamma + \epsilon_{it} \quad (1)$$

where α_s and γ_t are state- and year-fixed effects respectively, $Post_t$ is an indicator variable that equals 1 for years 1998 or later, LMI_i is an indicator variable that equals 1 if census tract i is an LMI neighborhood, and MFI_i is the median family income of tract i. X_{it} is a vector of control variables, listed in Table 3. Each control variable in X_{it} appears by itself and as an interaction with $Post_t$. Finally, β_1 through β_4 and Γ are parameters to be estimated and ϵ_{it} is an independent and identically distributed error.

The estimation results produced by the replication dataset (Table 4(B)) are similar to those reported by Saadi (2020) (which are reproduced in Panel (A)). While the magnitudes of the coefficients produced by the replication datasets differ somewhat from the reported by Saadi (2020), the signs and statistical significance levels of each coefficient are identical. The results from estimations using the corrected datasets are less similar, primarily because all of the coefficients shown are statistically significant at the 1% level. However, the signs on

Table 4: Replication Results Using the Replicated and Corrected Datasets

Table shows results from selected estimations of Equation (1). Panel (A) reproduces the results reported by Saadi (2020). Panels (B) and (C) show results from the Replication and Corrected Datasets, respectively. The dependent variable is the log of the number of loans originated in Columns (1) and (3) and the log of the dollar amount of loan originations in Columns (2) and (4). All estimations include state and year fixed effects. Robust standard errors clustered at the county level are shown in parentheses. Stars denote statistical significance at the 1 (***), 5 (**), and 10 (*) percent levels.

	Banks		Nonbanks	
	ln(Loans)	ln(Amount)	ln(Loans)	ln(Amount)
	(1)	(2)	(3)	(4)
(A) Saadi Results				
LMI × Post	0.170***	0.103***	0.088**	0.034
LMI × Post	(0.033)	(0.025)	(0.035)	(0.027)
LMI	-0.010	-0.056**	0.018	-0.042
LMI	(0.030)	(0.026)	(0.038)	(0.038)
Controls	Yes	Yes	Yes	Yes
2000 MFI	Yes	Yes	Yes	Yes
1990 MFI				
N	146,023	146,023	142,781	142.781
Adj. R2	0.653	0.647	0.593	0.543
(B) Replicated Dataset				
LMI × Post	0.159***	0.109***	0.064**	0.022
LMI × Post	(0.033)	(0.028)	(0.031)	(0.028)
LMI	-0.018	-0.057**	0.029	-0.027
LMI	(0.031)	(0.029)	(0.035)	(0.041)
Controls	Yes	Yes	Yes	Yes
2000 MFI	Yes	Yes	Yes	Yes
1990 MFI				
N	148,771	148,771	145,725	145,725
Adj. R2	0.66	0.633	0.598	0.533
(C) Corrected Dataset				
LMI × Post	0.151***	0.117***	0.088***	0.074***
LMI × Post	(0.028)	(0.022)	(0.027)	(0.023)
LMI	-0.189***	-0.217***	-0.201***	-0.255***
LMI	(0.026)	(0.022)	(0.033)	(0.031)
Controls	Yes	Yes	Yes	Yes
2000 MFI				
1990 MFI	Yes	Yes	Yes	Yes
N	211,655	211,655	208,756	208,756
Adj. R2	0.723	0.674	0.63	0.547

the estimated difference-in-difference coefficient ($LMI \times Post$) are the same as those produced by the replicated dataset and are of similar magnitude. These results suggest that both the replicated dataset and the corrected dataset reproduce the central findings reported by Saadi (2020).

4.2. Using the Correct Implementation Date for the 1995 Changes

As discussed in Section 2.2, the new lending test applied to loans originated on January 1, 1996 or later, 2.5 years earlier than the date used by Saadi (2020). Figure 1 shows how the interpretation of the results changes when the correct effective dates are used. Panel (A) of the figure replicates the first two figures from Saadi (2020). Like the earlier paper, both graphs normalize lending relative to 1998. Saadi (2020) points to the sudden divergence between the LMI and non-LMI lending for banks after 1998 as evidence that CRA caused banks to increase their LMI lending, while their non-LMI lending continued its pre-1998 trend.

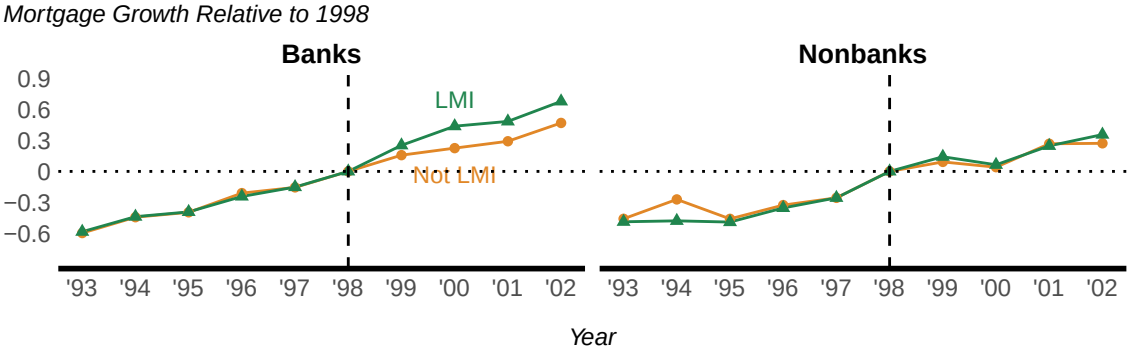
The connection between this divergence and CRA looks considerably weaker when the correct effective dates are used. Panel (B) of Figure 1 repeats the same analysis, but normalizes both series relative to 1995, the last year before CRA began collecting the data that would be evaluated under the new lending test. As these graphs show, lending in LMI and non-LMI neighborhoods appear to have grown at comparable rates in the first three years of the lending test. That growth accelerated only in LMI neighborhoods in 1999 and only for banks remains apparent; however, absent a rationale for why a regulation applying to loans starting on January 1, 1996 would only start affecting lending 3 years later, its link to CRA is unclear.

Similar results are obtained when the difference-in-differences analysis used by Saadi (2020) is conducted over the three years before (1993-1995) and after the introduction of the new CRA rules (1996-1998). Specifically, I estimate a modified version of Equation (1) where the $Post$ variable is replaced by $Post1995$, which takes on a value of 1 for years 1996-1998 and zero for 1993-1995 (Table 5). The estimated difference-in-differences coefficient

Figure 1: Using CRA's Correct Enforcement Dates Undermines the Argument that CRA Increased Mortgage Lending in LMI Neighbhorhoods

This figure shows the normalized rate of growth in the dollar volume of originations over time. Panel (A) reproduces Figures 1 and 2 from Saadi (2020). The y-axis on that panel shows the log of the dollar amount of loans in each year less the log of the dollar amount of loans in 1998. The y-axis in Panel (B) shows the log of the dollar amount of loans in each year less the dollar amount of loans in 1995.

(A) Replication of Figures 1 and 2 from Saadi (2020)



(B) Results with Correct Implementation Dates

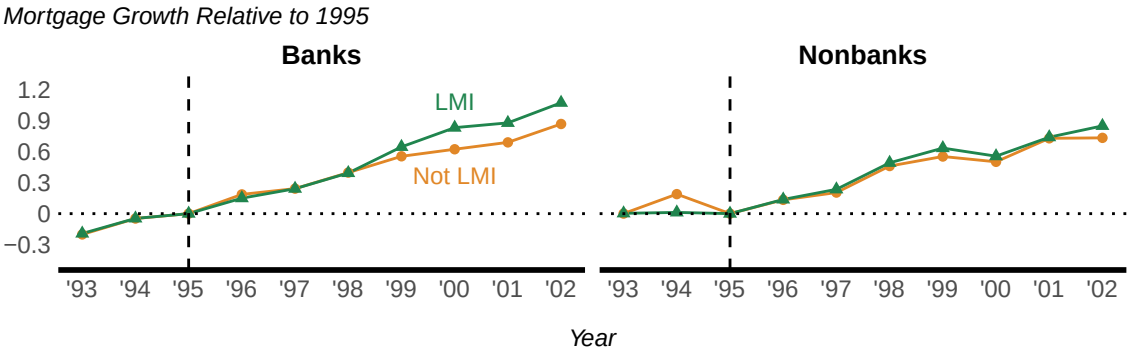


Table 5: Replication Results Using the Replicated and Corrected Datasets

Table shows results from selected estimations of Equation (1) conducted using data from 1993-1998. Panel (A) shows estimation results from the replication dataset. Panel (B) shows estimation results from the Corrected Dataset. The dependent variable is the log of the number of loans originated in Columns (1) and (3) and the log of the dollar amount of loan originations in Columns (2) and (4). All estimations include state and year fixed effects. Robust standard errors clustered at the county level are shown in parentheses. Stars denote statistical significance at the 1 (***), 5 (**), and 10 (*) percent levels respectively.

	Banks		Nonbanks	
	ln(Loans)	ln(Amount)	ln(Loans)	ln(Amount)
	(1)	(2)	(3)	(4)
(A) Replicated Dataset				
LMI × Post1995	0.037	0.036	0.051*	0.047*
LMI × Post1995	(0.027)	(0.022)	(0.029)	(0.027)
LMI	0.010	-0.044	0.027	-0.039
	(0.028)	(0.028)	(0.036)	(0.042)
LMI				
Controls	Yes	Yes	Yes	Yes
2000 MFI	Yes	Yes	Yes	Yes
1990 MFI				
N	88,889	88,889	86,701	86,701
Adj. R2	0.641	0.622	0.581	0.528
(B) Corrected Dataset				
LMI × Post1995	0.034	0.040**	0.034	0.045**
LMI × Post1995	(0.021)	(0.017)	(0.025)	(0.022)
LMI	-0.166***	-0.203***	-0.185***	-0.247***
	(0.026)	(0.023)	(0.033)	(0.031)
LMI				
Controls	Yes	Yes	Yes	Yes
2000 MFI				
	Yes	Yes	Yes	Yes
1990 MFI				
N	126,801	126,801	124,667	124,667
Adj. R2	0.714	0.671	0.62	0.55

($CRA \times Post1995$) is positive for each model using both the replication and corrected datasets, implying that bank lending in LMI neighborhoods grew more rapidly after 1995, though the coefficient is only significantly different from zero in a single specification using the corrected dataset.

Moreover, the coefficients on $CRA \times Post1995$ for banks are similar to, though slightly lower than, those for nonbanks. Given that nonbanks are not subject to the CRA, similar growth rates for banks and nonbanks are inconsistent with CRA having had an effect. An alternative explanation for the expansion of LMI lending after 1995 is the development of the subprime mortgage market during this period. Canner, Passmore, and Laderman (1999) reports that the number of subprime-specialist lenders and the number of loans they originated doubled between 1993 to 1995. Over the next three years (1996-1998), the number of subprime lenders increased by 300% and subprime loans by 400%. Because subprime loans were disproportionately made to LMI borrowers, lending at banks and nonbanks would have expanded more in LMI neighborhoods than in non-LMI neighborhoods with larger effects for nonbanks who originated more subprime loans.

While the similarity in growth rates provides no affirmative evidence that CRA increased LMI lending by banks, this absence of evidence should not be interpreted as evidence that CRA had no effect. Because nonbanks have a greater propensity to engage in subprime lending, one might have expected faster LMI growth at nonbanks absent the 1995 changes to CRA. CRA could have helped narrow the gap between LMI growth rates at banks and nonbanks. Without a way to measure the counterfactual there is no way to gauge the size of CRA's effect. The estimates generated here, however, suggest that it was not large enough to offset the difference in LMI loan growth between banks and nonbanks because of subprime lending.

5. The First Collapse of the U.S. Subprime Mortgage Market

5.1. Background

After the Russian government defaulted on its debt in August 1998 and Long Term Capital Management nearly collapsed, a "flight to quality" was triggered in financial markets (Sabry and Schopflocher 2007; Financial Crisis Inquiry Commission 2011). Because most subprime lenders were nonbanks, they were unable to fund their operations with federally insured deposits and had to rely on less stable funding sources. So when the flight to quality widened risk spreads, their operating costs increased. At the same time, there was less demand for private-label mortgage backed securities, which reduced the ability of subprime lenders to sell the loans they originated. Subprime lenders were thus placed in a financial vice in which they were being squeezed by higher costs and lower revenues. The results were catastrophic and immediate. The Financial Crisis Inquiry Commission (2011, pp. 74–75), which was established to investigate the origins of the 2007 subprime mortgage market collapse and its aftermath, provides the following description of how the flight to quality affected one subprime lender:

> Southern Pacific Funding (SFC), an Oregon-based subprime lender that securitized its loans, reported relatively positive second-quarter results in August 1998. Then, in September, SFC notified investors about "recent adverse market conditions" in the securities markets and expressed concern about "the continued viability of securitization in the foreseeable future." A week later, SFC filed for bankruptcy protection.

Southern Pacific Funding was not alone. In the two years following the Russian debt default, 8 of the 10 largest subprime lenders had declared bankruptcy, shut down, or been acquired by stronger firms (Financial Crisis Inquiry Commission 2011). The subprime market would sharply rebound years later, but in the late 1990s this was a major disruption.

Some of the effects of the collapse of the subprime market can be seen in the HMDA data by examining lending by the subprime lending specialists identified by the Department of

Figure 2: Lending by Subrprime Lenders Shifted Towards the Banking System After 1998

Figure shows the aggregate dollar volume of mortgage originations by lenders on the U.S. Department of Housing and Urban Development's subprime lender list. Loan volumes are calculated based on loans for home purchase included in the HMDA data for each year. Loan amounts Banks and Nonbanks are designated, as in the rest of the paper, based on the agency to whom each lender reports their HMDA data.

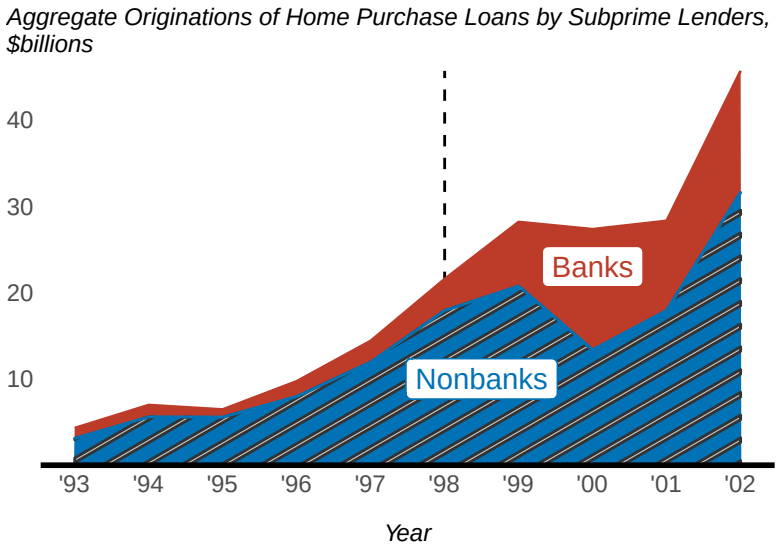
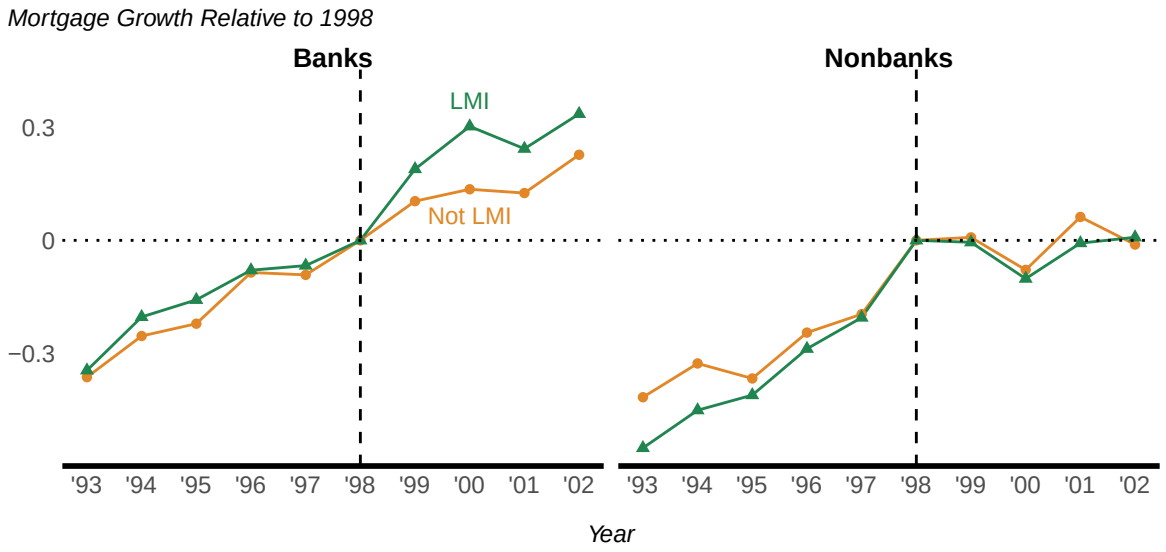


Figure 3: Loan Originations Plateaued at Nonbanks after 1998

This figure shows the normalized rate of growth in the number of home mortgage loans originated for home purchase over time. The y-axis shows the log of the number of home purchase loans originated each year less the log of the number of home purchase loans originated in 1998.



Housing and Urban Development (HUD).[10] There are two notable patterns in the aggregate lending volume of these lenders (Figure 2). First, prior to 1998, most subprime loans were made by nonbanks. Between 1993 and 1997, they originated 82% of subprime loans. Second, once the subprime market began its collapse, much of this lending shifted to banks, who went from originating 17% of subprime loans in 1997 to a majority of subprime loans (51%) in 2000.[11]

[10]Though HMDA did not collect sufficient information during this period to identify subprime loans, HUD produced an annual list of subprime lenders starting in 1993 (the "HUD list") that was widely used to study subprime lending (Calem, Gillen, and Wachter 2004; Ho and Pennington-Cross 2006). I follow these early studies and identify subprime loans in the data as those loans made by lenders on the HUD list. This method of identifying subprime loans is necessarily incomplete because not all subprime loans were made by lenders on the HUD list. Nevertheless, Mayer and Pence (2008) finds that identifying subprime loans using the HUD list generally captures important trends in subprime lending.

[11]This estimate of the shift away from nonbanks is likely an underestimate. Many of the borrowers who would have gotten loans from these subprime lenders had they continued to operate may have instead obtained loans from banks who were not subprime lending specialists. Such changes would not be reflected in estimates produced using the HUD list. Additionally, post-merger origination activity by subprime lending operations that were acquired by banks and absorbed in their operations, rather than continuing to operate as distinct subsidiaries, would not be reflected in these estimates.

While subprime lenders were likely hardest hit, the flight to quality would have adversely affected most nondepository institutions. As a result, lending by nonbanks plateaued after 1998. This plateau is not readily apparent in Figure 1, which shows aggregate dollar amounts lent. However, when that plot is changed to show the *number* of originated loans, the plateau in lending after 1998 becomes clear (Figure 3). Had this plateau only been observed for LMI lending by nonbanks, it could be dismissed as the result of nonbanks losing market share to banks who were originating more LMI loans because of CRA; however, the fact that non-LMI lending also plateaued makes it harder to attribute this change to CRA.

This shift in market share from nonbanks to banks offers an alternative explanation for the results reported in Saadi (2020). Nonbanks originate a larger share of their loans in LMI neighborhoods. As banks accumulated market share after the subprime market collapse, either by acquiring subprime lenders or the customers they otherwise would have served, LMI lending by banks should have grown more rapidly than non-LMI lending. This would cause the same divergence in growth rates used to support the hypothesis that the introduction of CRA's lending test caused more LMI lending.

5.2. Distinguishing the Effects of the CRA from the the First Subprime Collapse

The previous section discussed how the flight to quality in 1998 provides an alternative explanation for the increase in LMI lending by banks after 1998. In this section, I explore whether mortgage lending patterns around 1998 are more consistent with this event or with a delayed response to changes in the CRA.

CRA's lending test evaluates a bank's record of lending in the LMI neighborhoods within its assessment areas. Because LMI lending outside of these areas does not affect a bank's rating, CRA does not provide the same incentive to lend in LMI neighborhoods outside of a bank's assessment area. So, any effects of CRA on LMI lending by banks should only be observed within their assessment areas.

In contrast, the effects on bank lending in LMI neighborhoods resulting from a decline in lending by nonbanks would not have been limited to a bank's assessment areas. If a bank acquired a subprime lender during this period, its unlikely that the acquired firm's lending footprint would have substantially coincided with the acquiring bank's assessment area. Similarly, any newly available lending opportunities resulting from reduced nonbank lending would not have been limited to bank assessment areas.

This potentially offers a way to distinguish between these two competing explanations. If CRA caused the more rapid growth in LMI lending, that growth should have been limited to banks lending within their assessment areas. Significant increases in LMI lending outside of bank assessment areas would be harder to attribute to CRA.

The aggregate lending amounts from the replication dataset provide little evidence that banks increased their LMI lending within their assessment areas any more rapidly than they did their non-LMI lending (Panel (A) of Figure 4). After 1998, (normalized) LMI lending by banks was higher than their non-LMI lending, but the same was true most years before 1998. In contrast, outside of their assessment areas, bank LMI lending diverges from their non-LMI lending after 1998 to an extent not observed in prior years. These results suggest that banks increased their LMI lending more (relative to their non-LMI lending) outside of their assessment areas. Results from the corrected dataset (Panel B of Figure 4) are consistent with this result.[12]

Multivariate analyses generate similar results. Equation (1) was estimated using the log of the number of originations by banks inside of their assessment areas (Table 6) and outside of their assessment areas (Table 7) as the dependent variable. These estimations were conducted using both the replicated and corrected datasets.

[12]One of the most notable differences between the results produced by the replication and corrected datasets is the sharp increase in LMI and non-LMI lending by banks outside of their assessment areas in 1996. This increase is a result of the 1995 CRA reforms. Prior to 1996, banks were not required to report the census tracts for the mortgages they originated outside of their assessment areas, though many did so voluntarily. Reporting that information became mandatory starting in 1996. The fact that a similar increase is not as clearly observable in the replicated dataset reflects the degree of noise introduced by approximating assessment areas based on branch locations rather than using actual assessment areas.

Figure 4: LMI Lending by Banks Expanded More Outside of their Assessment Areas

This figure shows the normalized rate of growth in number of loans originated for home purchase over time. Results are shown from the replication dataset in Panel (A) and from the corrected dataset in Panel (B). The y-axes in all graphs shows the log of the number of home-purchase loans originated each year less the log of the number of home-purchase loans originated in 1998.

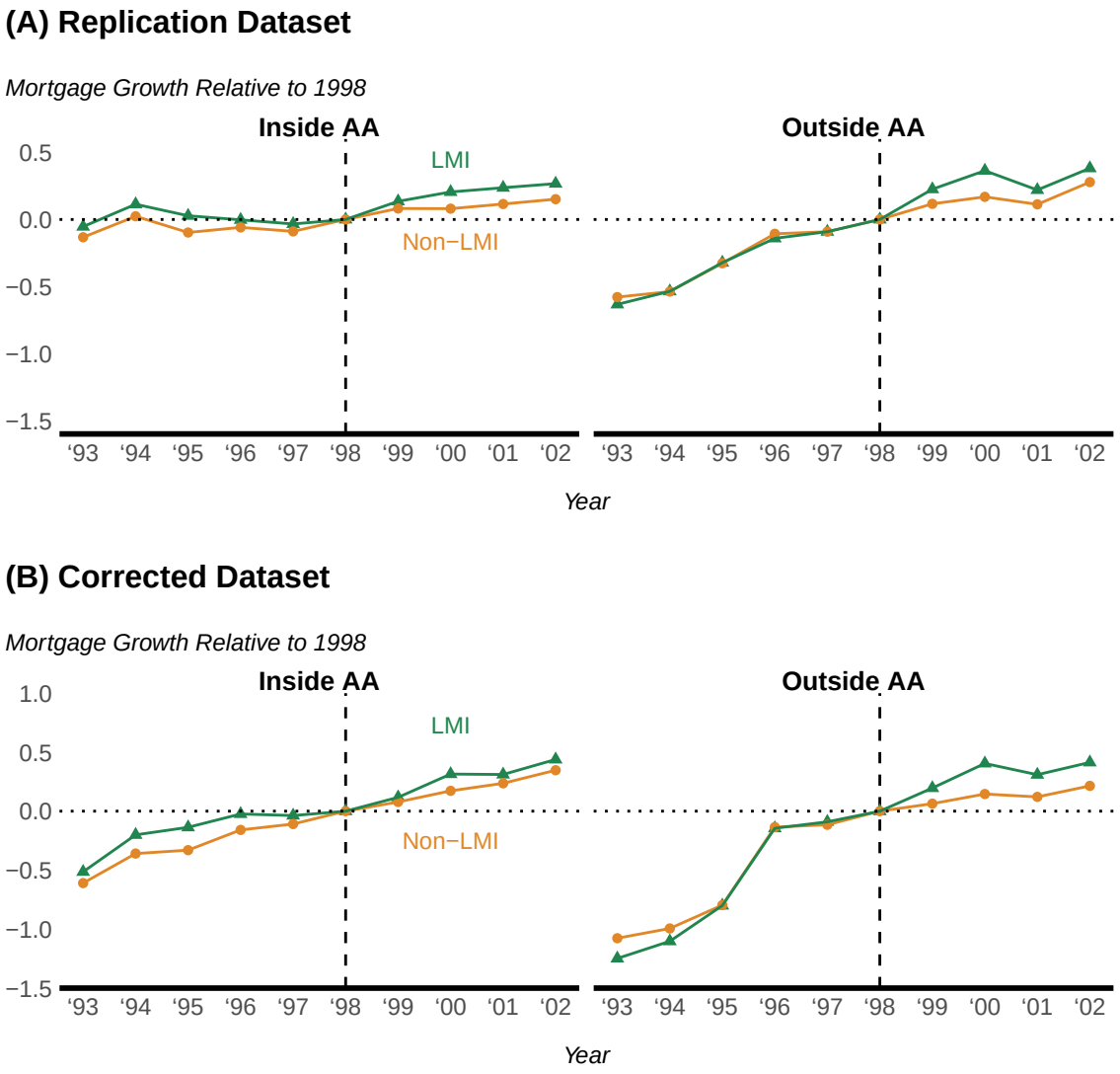


Table 6: Estimation Results for Banks within their Assessment Areas

Table shows results from different specifications of Equation (1) using data from 1993-2002. All columns use the log of home purchase originations by banks within their assessment areas as the dependent variable. Columns (1)-(4) show results from the replication dataset and Columns (5) and (6) show results from the corrected dataset. Panel (A) shows estimation results from the replication dataset. All estimations include state and year fixed effects. Robust standard errors clustered at the county level are shown in parentheses. Stars denote statistical significance at the 1 (***), 5 (**), and 10 (*) percent levels respectively.

	Replication Dataset				Corrected Dataset	
	ln(Loans)	ln(Loans)	ln(Loans)	ln(Loans)	ln(Loans)	ln(Loans)
	(1)	(2)	(3)	(4)	(5)	(6)
LMI × Post	-0.012	0.013	0.087**	0.037	-0.109***	0.015
LMI × Post	(0.022)	(0.022)	(0.034)	(0.027)	(0.020)	(0.036)
LMI	-0.637***	-0.397***	-0.082**	-0.253***	-0.729***	-0.094***
LMI	(0.031)	(0.030)	(0.041)	(0.029)	(0.034)	(0.030)
Controls	No	Yes	Yes	Yes	No	Yes
2000 MFI	No	No	Yes	No	No	No
1990 MFI	No	No	No	Yes	No	Yes
N	144,275	144,275	144,275	144,205	205,194	205,194
Adj. R2	0.219	0.453	0.476	0.469	0.267	0.529

Table 7: Estimation Results for Banks Outside of their Assessment Areas

Table shows results from different specifications of Equation (1) using data from 1993-2002. All columns use the log of home purchase originations by banks outside of their assessment areas as the dependent variable. Columns (1)-(4) show results from the replication dataset and Columns (5) and (6) show results from the corrected dataset. Panel (A) shows estimation results from the replication dataset. All estimations include state and year fixed effects. Robust standard errors clustered at the county level are shown in parentheses. Stars denote statistical significance at the 1 (***), 5 (**), and 10 (*) percent levels respectively.

	Replication Dataset				Corrected Dataset	
	(1)	(2)	(3)	(4)	(5)	(6)
LMI × Post	0.045*	0.048	0.064	0.038	0.047*	0.075**
LMI × Post	(0.027)	(0.035)	(0.047)	(0.038)	(0.025)	(0.031)
LMI	-0.740***	-0.444***	-0.026	-0.233***	-0.850***	-0.169***
LMI	(0.035)	(0.029)	(0.041)	(0.033)	(0.031)	(0.029)
Controls	No	Yes	Yes	Yes	No	Yes
2000 MFI	No	No	Yes	No	No	No
1990 MFI	No	No	No	Yes	No	Yes
N	141,992	141,992	141,992	141,910	198,648	198,648
Adj. R2	0.358	0.583	0.61	0.604	0.433	0.648

The results from the most parsimonious model produced by the replication dataset (shown in Column (1)), which excludes the control and MFI variables, are the most consistent with the patterns shown in the graphs. The coefficient on $LMI \times Post$ is small and insignificant for banks inside of their assessment areas. But outside of bank assessment areas, the coefficient on this variable is several times larger and significant at the 10% level. Adding the control variables (Column (2)) has little effect on these results, though the coefficient on $LMI \times Post$ is no longer significant outside of bank assessment areas.

It is only when a control is added for MFI from the 2000 Census that the story changes notably. In this case, the coefficient on $LMI \times Post$ increases and becomes statistically significant at the 5% level for banks inside of their assessment areas. Outside of their assessment areas, adding this MFI control also increases the size of the coefficient on $LMI \times Post$, though it remains statistically insignificant.

This specification (Column (3) of Tables 6 and 7) is particularly important. Among the robustness exercises included in Saadi (2020) is a similar analysis of lending by banks within and outside of their assessment areas. That analysis is limited to this specification and the coefficients he obtains on $LMI \times Post$ are similar to those shown here, both in term of magnitude and statistical significance level.[13] Despite finding a somewhat narrower difference between the two coefficients on $LMI \times Post$ than replication dataset produces, Saadi (2020, p. 5311) concludes that, because only the former coefficient is significantly different from zero, the overall increase in bank LMI lending can be attributed to CRA.

This result does not appear to be robust to resolving the inconsistencies with CRA, as shown by the results from estimations using the corrected dataset (Column (7) of Tables 6 and 7). Because this estimation uses a dataset that designates LMI neighborhoods using 1990 Census MFIs and includes a control for tract-level MFIs from the same source, it is more consistent with the hypothetical experiment Saadi (2020) uses to motivate his identification strategy in which outcomes are compared for two tracts with identical MFIs that are treated differently because of differences in their surrounding-area MFIs that cause one to be LMI and the other not. The resulting coefficient on $LMI \times Post$ for banks within their assessment areas (Table 6) is small and not significant at the 10% level. But outside of bank assessment areas (Table 7), the coefficient on $LMI \times Post$ is both notably larger and statistically significant at the 5% level.[14]

The estimation results produced by the corrected dataset are consistent both with results from the replication dataset that that do not control for tract-level MFIs and with the graphical evidence shown in Figure 4. Together they suggest that LMI lending by banks grew more rapidly after 1998, relative to their non-LMI lending, than it did within their assess-

[13] Specifically, the coefficients on $LMI \times Post$ reported by Saadi (2020) are 0.068 and 0.059 for banks inside and outside of their assessment areas respectively. The former of these coefficients is statistically significant at the 5% level while the latter is insignificant.

[14] Similarly, the results in Column (3) of Tables 6 and 7 are not robust to controlling for MFIs from the 1990 Census, instead of the 2000 Census, using the replication dataset (as in Column (4)). In that case, the size of the coefficients on $LMI \times Post$ drop so that they are almost identical for banks within and outside of their assessment areas, with neither being statistically different from zero. This particular specification is inconsistent because it includes controls for 1990 MFIs as independent variables while continuing to (incorrectly) determine each tract's LMI status based on MFIs from the 2000 Census.

ment areas. While these results are inconsistent with CRA having caused these trends, they are consistent with the lending changes that should have followed the decline in nonbank lending after 1998.

6. Conclusions

This review suggests that the conclusions reached by Saadi (2020) about the role the CRA played in triggering the subprime crisis are incorrect. The difference-in-differences estimations in that paper, which because of an error regarding when the 1995 CRA changes took effect are conducted over the wrong time period, yield results that are consistent with the first collapse of the subprime mortgage market but are inconsistent with CRA having played a role. Moreover, an analysis of lending around the correct implementation date yields no evidence that banks increased their LMI lending any more than nonbanks who are not covered by CRA.

The results discussed in this paper are limited to the evidence presented by Saadi (2020) about the relationship between CRA and LMI loan growth. I have not attempted to replicate his results concerning house price appreciation. As shown here, the estimates produced by his empirical approach around 1998 likely reflect expanded subprime lending by banks in LMI neighborhoods. Given that previous research shows that areas with more subprime lending experienced faster house price appreciation (Mian and Sufi 2009; Anundsen and Heebøll 2016; Griffin, Kruger, and Maturana 2021), it is not surprising that the same empirical approach would find a relationship with house price appreciation.[15] Without a link between CRA and increased LMI lending, it is hard to attribute house price appreciation to CRA.

Perhaps more importantly, however, an extensive literature already explores the causes of the house price appreciation that preceded the subprime crisis. These papers identify a

[15]While subprime lending is correlated with house price appreciation, the direction of causality is unclear and likely goes both ways. Subprime lending could increase housing demand and cause higher house prices. But at the same time, house price appreciation reduces the riskiness of mortgage lending and can make lenders more willing to make subprime loans (Coleman IV, LaCour-Little, and Vandell 2008; Goetzmann, Peng, and Yen 2012).

long list of contributing factors that include the rise in mortgage securitization (Purnanandam 2011), the role of investors (Haughwout et al. 2011), fraud (Griffin and Maturana 2016), mortgage innovation (Keys et al. 2013), and overly-optimistic expectations about future house price increases (Shiller 2007; Adelino, Schoar, and Severino 2018). None of these causes have been linked to the CRA.

Moreover, the literature examining regional differences in house price booms is inconsistent with CRA having been a material cause. Ferreira and Gyourko (2021) show that rather than sharing a common structural break, house price booms across metropolitan areas began at different points over a 10-year period. Sinai (2013) finds similar regional differences and highlights the strong overlap between the housing markets that boomed in the 1980s and those that boomed in the 1990s. As the authors of these studies note, it is hard to reconcile these facts with a single, national trigger.

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