Seeds of Growth Conference
Public Policy: CRA
Comments
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Overview

Given the entreaty of the conference organizers to be provocative in our comments, let me begin by considering the big picture of academic research on CRA for a moment. My overall impression is that we are asking the right questions and studying the right markets. Research on CRA and on access to credit and the workings of credit markets generally has relatively high marginal value. I say this because the questions at hand have implications for the study of markets and decision-making generally, the markets and policies themselves may directly impact the well-being of millions if not billions of people, and the state of knowledge is thin.

The reasons for this state of affairs are manifold. To start with, credit markets are extremely complex. They are rife with potential market imperfections, yet we still lack convincing evidence on whether and what flaws need to (and can be) fixed. Put another way, we have far more theory demonstrating the potential for credit market failures than we have convincing evidence showing that any particular market failure matters in practice.

Unfortunately, the social sciences are not yet especially well equipped to handle this complexity. Our disciplines are still relatively young, and social scientists are just beginning to develop, refine, and apply the methodologies needed to identify the causal effects of market and policy variables. The upshot is that one must read academic works with caution and apply their lessons only after paying careful attention to whether their findings actually reveal a causal relationship of interest.

The good news is that we have made considerable progress in recent years. The papers here by **Bostic and Robinson,** and by **Avery, *et al***, are indicative of the potential of research methodologies that seek to, and increasingly succeed in, isolating the causal effects of credit market interventions on the outcomes we care about.

In my comments on the two papers I will focus on their models — on their methodologies — rather than on any specific findings. This is not motivated purely by time constraints. It also speaks to the general value of getting the methodology right before we venture into the interpretation and dissemination of particular findings.

Comments on Bostic and Robinson, "What Makes CRA Agreements Work? A Study of Lender Responses to CRA Agreements"

This paper documents, with admirable precision, an association between CRA agreements and increased CRA lending, for a subset of such agreements. It does not, however, establish a causal link between CRA and lending. My comments will focus on agreements generally, as opposed to best practices, and on refinements to the model that could produce more plausibly causal estimates.

The difficulty in measuring the causal link, if any, between CRA agreements and lending behavior is that agreements are evidently endogenous; i.e., agreements do not materialize randomly, but rather come about because of market and policy incentives that may be correlated with other aspects of lending decisions that we don't observe (and hence can't capture directly in our statistical models). An important example of worrisome endogeneity in this context would be if banks make commitments only when it is "cheap" for them to do so: an extreme example of this would be if banks made commitments only for the amount of CRA lending they would do anyway, in the absence of a commitment.

The challenge, therefore, is to develop a convincing test of whether a commitment produces counterfactual lending — lending that would not have happened in the absence of the commitment. How to do this? **Bostic and Robinson** appear to have endogeneity concerns in mind when they include random agreement effects, but this strategy unfortunately does not address the potentially important link between unobserved determinants of bank lending behavior and observed CRA commitments.[1]

[1] One might think to include bank fixed effects instead (i.e., to control for average lending for each bank observed during the sample period). However, this approach does not solve the problem that savvy banks that are about to increase lending growth anyway (i.e., about to break from trend for reasons unrelated to CRA) might try to get CRA credit or public relations mileage out of this secular increase.

Rather, I would pursue alternative strategies for identifying the causal effect of CRA agreements on lending behavior. One approach would be to try to identify exogenous variation in the likelihood of an agreement. One implementation of this approach is being explored in other research that seeks to use measures of community capacity to predict the likelihood of an agreement. This approach has its pitfalls, but it certainly has potential.

Another approach focuses on controlling for (counterfactual) trends in lending. Examples of this would be to condition on lagged lending[2] or to use matching techniques to effectively limit statistical comparisons to sets of banks exhibiting plausibly homogenous lending behavior, "but for" the CRA agreement.[3]

The paper also would benefit from some refinements and extensions. On the causality front, adding more controls for bank characteristics (principally size and holding company fixed effects) would help. The sample also easily could be expanded to include commitments that are not geographically targeted, by taking lending measures from the Call Report data. These and other data, moreover, could be used to include banks that don't make commitments in the analysis (this would be useful to do with a matching approach).

The authors should also explore readily available alternative measures of their key variables. Why not consider the size of the commitment as well?[4] Another alternative measure of a commitment (the authors use only years-in-force) would be a simple 1/0 (dummy) variable capturing whether an agreement was in force at all. On the lending side, care always should be taken with simple regression estimates on dependent variables with skewed distributions (lending measured in levels fits this description), as OLS is vulnerable to outliers. As such lending should be specified in growth terms, logs, or quintiles as well.

The authors also should add to the set of outcomes they consider. One question is what happens in real (purchasing-power) terms. Some simple calculations here could shed some insight into whether bank monitors suffer from nominal illusion (e.g., do commitments even promise real loan growth?) A second question is what happens to small-business lending, which (arguably) has taken greater prominence under CRA since the regulatory reforms of the mid-90s. Small-business lending data are available since 1993 at the bank level (Call Reports), and since 1996 with geocodes for large banks as part of CRA reporting requirements. Two potentially revealing tests that could be done with these additional data sets are: 1. What happens to small-business lending following a specific commitment to increase it? 2. What happens to small-business lending (and noncovered lending more generally) by banks that make mortgage commitments but not small-business lending commitments?

In sum, there is still work to be done here to identify the causal relationships of interest, but I am optimistic that the authors can make considerable progress in this direction and add to an already fine contribution. A final note of caution: In the interest of erring on the side of provocation, unless we can convincingly control for counterfactual trends in lending, we can't be sure that the true effect of CRA agreements is not in fact to *reduce* lending. This is especially relevant if in fact lenders get CRA "credit" for making agreements *per se* (as the authors appear to suggest on p. 3). If this holds, lenders who might otherwise increase lending by $X *for CRA reasons* might be able to use agreements as a smokescreen and increase lending by only $Z<$X.

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[2] One might also consider controlling for lags and leads in merger and acquisition activity, given the prominent role of M&A in "activating" CRA incentives, and the possibility that M&A itself has an independent causal effect on lending behavior.
[3] Matching typically would be done based on ex-ante lending behavior and possibly other bank characteristics such as assets, profitability, and lending volume.
[4] This raises the question of how to allocate the commitment size over time. One possibility is pro-rating; another is using long differences.

Comments on Avery, Calem, and Canner, "The Effects of the Community Reinvestment Act on Local Communities"

This paper provides a cogent review of the literature on CRA incentives and institutions and develops two types of tests designed to identify the causal impact of CRA on neighborhood outcomes. The tests exploit the fact that the neighborhood targeting aspect of CRA operates with a discrete cutoff — only census tracts with median incomes below 80 percent of their MSA average are targeted. The methodology of the paper, in essence, compares the outcomes of tracts just below the 80 percent cutoff (which are targeted for increased CRA lending) with those just above the cutoff (which are not geographically targeted for CRA lending). The argument is that the two groups of tracts are essentially identical along unobservable characteristics that might be correlated with CRA targeting status (i.e., with being below the 80 percent cutoff) and trends in outcomes. The authors then use this approach to estimate the impact of CRA on several outcomes of interest (homeownership rates, median home value, crime rate, vacancy rate) over the medium run (1990 to 2000) and long run.

The methodology is valid if the tracts above and below the cutoffs would have followed identical trends in the absence of the CRA. The authors could bring more evidence to bear on this question. Graphical examination of the pre-1990 trends is essential (inspection of pre-period trends helps to rule out mean reversion in the sample period), and more specific evidence ruling out confounding impacts from other initiatives targeting low-income communities would also help. I have in mind a list of targeting criteria for other significant programs during this period, everything from the GSEs to Block Grants to LISC. More analysis that seeks to tie observed outcome changes to changes in *CRA* lending will also help address this concern (see below).

Overall, though, the medium-run model takes a very promising approach. The paper implements it by first estimating regression models for changes in outcomes using the subsample of tracts just *above* the 80 percent threshold as of 1990, then using the coefficients from this model to predict changes in tracts just *below* the threshold, calculate residuals from the predicted models, and determine whether the residuals are statistically significant. If there are significant differences, this suggests a CRA effect. A simple, more flexible way of executing the same comparison would be:

(1z) $Y_j = f(LMI_j, Income_j, X_j)$

Where Y is a change in the outcome of interest for tract j from 1990 to 2000, LMI is a 1/0 variable for whether j fell below the 80 percent threshold in 1990, Income is j's percent of median MSA income in 1990 and functions thereof, and X includes control variables capturing j's observable 1990 and fixed characteristics.

The key implications of these simple modifications are: 1) the result on the LMI variable would now (plausibly) identify the discrete effect of CRA geographic targeting, conditional on smooth income and other ex-ante characteristics; 2) one maintains the flexibility of allowing the relationships between right-hand-side variables and outcomes to vary across the LMI threshold; 3) this is all accomplished in a single regression.

One then can easily extend the model to develop evidence on whether CRA itself appears likely to have driven any LMI effects estimated in (1z), by adding proxies for CRA activity to the model. Key variables here would capture the prevalence of lenders, including each tract in their assessment area, and lending to LMI *borrowers* in the tract. Note: it is not evident that we should rely solely on market shares to parameterize these measures of CRA presence — levels plausibly matter instead (or as well). Another concern is that a bank's assessment area is potentially endogenous (e.g., if banks try to limit their assessment areas to areas on the upswing). As such, it would help to condition on some measure(s) of generic bank presence (e.g., total assets of banks or BHCs with a given tract in their assessment area, per capita; ex-ante bank branches in j, per capita) in some specifications.

This framework generates another testable implication: CRA presence (proxied by assessment area prevalence) should matter only for LMI tracts, conditional on lending to LMI borrowers. More formally, LMI*A_j should be positive, where A_j is a measure of the prevalence of lenders, including j in their assessment area. (Alternatively, worries about endogenous regressors might motivate splitting the sample by LMI status. In this case, the testable prediction is that A_j should be statistically significant only in the LMI sample.)

Some other relatively straightforward extensions would shed light on the existence and nature of any causal relationship: 1) a falsification test: 2000 LMI status (conditional on 2000 smooth income) should not matter, if CRA is the driver of any observed changes; 2) a test of whether any observed CRA lending increases are in fact net increases; e.g., by regressing (changes in) total lending in j on lending by assessment area banks; 3) some simple calculations of how, mechanically, banks increase CRA lending, if and when they do — how much of the increase is due to increased applications vs. increases in the probability of approval or loan size?

All of my specific comments thus far pertain to refining the medium-run model. I am less sanguine about the potential of the long-run model for identifying any causal effects of CRA because of data limitations. The correct test here, I believe, is comparing long differences over 1970 to 2000 for tracts just above and below the LMI threshold ex-ante (i.e., in 1970). Currently, the authors compare outcome levels in 2000, using data on 1980, 1990, and 2000 characteristics. Perhaps the desired 1970 data do not exist. This data-availability issue should be addressed, given that CRA was enacted in 1978. Either setup faces additional data hurdles. One problem, as the authors note, is that tract definitions change over time. A second problem is that it will be difficult (perhaps impossible) to tie any observed outcome changes to changes in lending behavior before 1990. The good news is that the medium-run model quite plausibly covers the era during which CRA had the most teeth.

Concluding Provocation: Where Do We Go from Here?

In the face of mounting (although still inconclusive) evidence that CRA does indeed change bank behavior, my hope is that we will soon be able to move beyond establishing the responses to CRA incentives, to analyzing market microstructure and the welfare (cost-benefit) effects of the policy. These issues must be understood in order to design optimal regulation (or lack thereof). Some of the key questions are:

1. Do changes in bank behavior produce changes in *equilibrium* access to credit?

- How much do nonbanks matter in these markets?
- Inframarginal vs. marginal changes (reshuffling vs. increased access)
- How special are banks?
- Helpful to have data on (potential) borrowers, not just banks

2. Are any CRA-induced changes for good or for ill? Is there empirically important incompleteness in CRA's target markets, and does CRA itself mitigate it?

- Are responses to CRA incentives consistent with theoretical models of credit market failures? Behavior among lenders and borrowers, and aggregate outcomes, could be used to identify the empirical importance, if any, of credit rationing, market power, discrimination, spillovers (particularly strong testable implications)

There is no shortage of variation in CRA that can be brought to bear on these questions. In theory, at least, CRA's "bite" has varied across time, individual banks, and targeted loan types and beneficiaries. Both anecdotal and statistical evidence suggest that much of this variation has occurred in practice as well. The following table summarizes:

Variation That Could Be Used to Estimate the Causal Effects of CRA

Across Time	*Across Banks*	*Across targeted loan types and beneficiaries*
late 70s enactment and implementation	Size	Home mortgages
late 80s/early 90s modifications (include HMDA)	Local examiner rating propensities	Small business
mid 90s rewrite of regulations	M&A propensities	in LMI areas
	Ex-ante presence in LMI areas	to LMI borrowers
		Community capacity • Monitoring • loan production

I conclude with a more general entreaty to link to study of CRA to specific models of credit markets and market frictions. Neglecting the underlying economics can lead to erroneous inferences. Two examples from today's papers:

Mitigating market failures does *not* necessarily imply that bank profits will increase (contra **Bostic and Robinson,** p. 7) *even if overall efficiency improves*.

- Banks can lose some, and borrowers can gain more, under various types of models (even credit rationing, more obviously if market power was a problem).

Lending behavior and outcomes can change even if the bank loan production function doesn't (contra **Avery, Bostic, and Calem** p.12). Say:

1) *CRA solves a coordination problem* (e.g., internalizes some spillovers in information or property values)

2) *Banks make loans based on the following rule*:

Interest rate$_{bih}$ = f(p_b, $V_{h,}$ other stuff), where b is an individual bank, i is a potential borrower, h is the property i would like to buy, p = perceived probability that other banks will lend in the neighborhood, and V is the market value of h (ceteris paribus, banks prefer to lend against high V).

3) Then the mere *existence* of an effective CRA could increase p and V immediately (based on expectations), and therefore, interest rates would fall (and rationing decrease) without banks having to change a thing.

Overall, the outlook for developing useful research on CRA and related credit markets is bright. The path to progress is relatively clear, and we are already on the right road, as these two papers indicate.