Draft – March 6, 2006

What Might We Know?
Research Design Issues for Measuring CDFI Subsector Impacts

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March 6, 2006

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Acknowledgements

I would like to thank Valerie Chang, Deborah Schwartz, and Michael Stegman, all of the John D. and Catherine T. MacArthur Foundation, for commissioning this paper and for their feedback on earlier thoughts and a first draft. I would like to thank Julia Sass Rubin for continual exchanges we have had about topics related to this paper. Finally, I would also like to thank Carla Dickstein, George Galster, and Robinson Hollister for their comments on the first draft of this paper. I remain responsible for any errors, omissions, and opinions contained in the paper

Introduction

The purpose of this paper is to consider the following questions:

1) What types of research might be conducted to gain a better understanding of the outcomes that are associated with different CDFI activities? What types of data exist to place these outcomes in some appropriate context – either in terms of non-CDFI, market activity or in terms of some measures of demand or need?

2) What is the appropriate level at which to examine the outcomes and impacts of CDFIs? Should we measure outcomes or impacts at the level of the entire subsector, a product line, or a specific product? Can we identify product lines that are relatively better candidates for impact evaluation?

3) What are some of the methodological approaches that might be used to conduct summative impact evaluation for specific CDFI product lines, either in particular places or across many places?

4) What types of research might be conducted to estimate the impact of CDFI product lines on key, cross-cutting outcomes? By cross-cutting outcomes, I mean those that cut across many if not most CDFIs involved in the same product line. Cross-cutting outcomes for CDFIs financing the rehabilitation of single-family homes, for example, might include increased or stabilized property values. Related to this question is whether there are relatively accessible indicators that serve as good proxies for a wider variety of outcomes that may be difficult to measure directly.

In order to address these questions, some basic foundations need to be established. First, I distinguish between evaluations of individual CDFIs, on the one hand, and outcome measures or impact evaluations for CDFI activity aggregated by product line. The goal here is not to suggest methods aimed at the evaluation of single institutions—although I borrow liberally from such methods—but to aim for approaches that could gauge the activity or impact of CDFIs in some *aggregations*. Of course, a complete aggregation of CDFI activity might prove quite unhelpful from an analytical perspective. The CDFI field is one tied together quite loosely by some broad notions of community development and by the common denominator of providing credit, capital, or basic account services as an instrument towards such goals.

This paper is intended to be helpful to those who are seeking a reasonably sound response to the question "in this area, what difference has the CDFI sector made?" where "area" might, for example, include single family mortgage finance, multifamily lending, or small business lending.

One challenge is to specify what is meant by "area" more clearly. How broad or narrow should we look when we aim to measure outcomes or impacts? I will argue that the appropriate level of aggregation is at the level of product line, which is in turn defined by

borrower characteristics, characteristics of the financing (e.g., debt or equity), and purpose of the financing (e.g. to rehabilitate single family homes). For example, one might look at the effect of all single-family home purchase lending done by CDFIs, either within a particular place (e.g., city) or across the country.

Certainly, in some cases, it may be appropriate to aggregate two or more product lines together when it appears that the product lines are aimed toward similar objectives. But such aggregation should be done carefully and be justified based on some notion of similar targets and outcomes.

Second, I discuss the difference between "outcomes" and "impacts." This is important because the latter term has been used in many different ways, and there is a need to be clear on the difference, because it is a very real one. An outcome is an event or condition that is *purported* to have resulted from CDFI activity, while impact implies demonstrated causation, i.e., establishing the portion of the outcome that would not have happened, *but for* the intervention of CDFIs.

Third, I some effort is spent distinguishing between evaluations of place-based CDFI strategies and evaluations of those targeting households or firms regardless of place. Pure place-based approaches are distinguished from "placed-based people" approaches. Place-based programs are often considered particularly difficult to evaluate (Gambone, 1998; Hollister and Hill, 1995; Rossi, 1999). This is often true, especially when compared to those people-based social programs that have access to sets of rich, individual-level data on participants (and sometimes even control groups) due to the ability of program administrators to compel data from program participants.

At the same time, I will argue that, given the challenges of measuring impact in the CDFI field, placed-based approaches – especially those that reach some scale at the neighborhood level – might actually be easier to evaluate in the context of CDFI efforts. This is because the *relative density* of place-based CDFI activity is likely to be greater than that of CDFIs that do not target their activity at a small area level. Depending upon the outcome of interest, relative density might be operationalized in different ways. It might be the level of CDFI activity compared to other providers of credit or capital in the same submarket. Alternatively it might be CDFI activity compared to potential demand or need in the same submarket. This does not mean that CDFIs that target their efforts geographically necessarily have more impact. That may or may not be the case and may vary depending on subsector and product line. However, it may be that small area-targeted CDFI activity is easier to evaluate for impact.

There are other factors that may make some types of CDFI activity better candidates for impact evaluation. In particular, those programs that are expected to affect the quality of life for residents at the neighborhood level and that suffer less from problems of geographic dispersion or leakage of benefits, may be easier to evaluate (e.g., housing impacts). Employment impacts are notoriously difficult to evaluate, especially at small area levels. Adding jobs in a neighborhood may or may not benefit residents of a

neighborhood. The distribution of employment benefits depends on commuting patterns, job-ladder chains, and multiplier effects, among other factors.

Fourth, I examine the challenge of choosing cross-cutting indicators that can be used to measure product-line-wide outcomes and impacts of CDFIs. This is a real challenge, because different CDFIs, even in the same product line, may work in quite different directions. Goals and objectives may not always align and might even conflict. There is also a need to deal with the common challenges of collective preferences that plague so much of policy analysis and program evaluation, whereby different stakeholders can hold quite different views of even a single CDFI's goals and objectives.

In this regard, I see no alternative but to fail to satisfy all parties completely. Some reduction of the goals and objectives of different programs into some common measures will typically be required to address questions of impact that extend beyond an individual, particular CDFI. The trick is to recognize that such measures will not be comprehensive. No evaluation will capture all the benefits of a program. And an effort to measure the impacts of all CDFIs active in a product line, in a given community, is likely to be even less comprehensive in its scope of outcomes.

Fifth, I consider potential research designs for assessing impacts of CDFI product lines. Here, I include a selective review of some particularly relevant literature on the evaluation of community development programs, economic development programs, comprehensive community initiatives, and other efforts. Much of this literature concerns place-based outcomes, in part because that is where I see more promise. I also consider more descriptive studies that might at least identify patterns of correlation between CDFI activity and other sorts of activity, patterns that could be at least suggestive of positive impact.

For example, one might look to whether measures of CDFI activity seem to be correlated with measures of conventional bank activity in the same general product lines. Through cross sectional multivariate statistics, it may be possible to identify whether CDFI activity seems to have a positive or negative effect on bank activity in the same product line, independent of other factors. Such analysis would also prove helpful to informing competing models of how CDFI intervention affects retail financial markets.

CDFI Borrower-Investee Types, Product Groups, and Product Lines

Before considering issues of outcomes and impacts, I first develop a typological breakdown of CDFI products and services. This is critical for understanding the context in which we might measure outcomes and to prevent an over-aggregation of different products. It will also provide a framework for which we can analyze available data sources both on CDFIs and on market providers of similar (but not identical) products.

A good deal of descriptive literature on CDFIs has developed over the last two decades. Much of this work has focused on distinguishing types of CDFIs first and then, within

each CDFI class, examining the types of projects (business, housing, nonprofit facilities, e.g.) financed by that category of CDFI (Tholin, 1994; CDFI Data Project, 2004). While the literature sometimes focuses on specific product types, there has been little effort to disaggregate the predominant product types within the CDFI sector.

When one approaches questions of CDFI outcomes by first thinking about the different types of institutions (e.g., community development bank vs. community development loan fund), it encourages a focus on financial performance measures, measures of financial health, etc. This may not be a bad thing to do, especially if financial sustainability is of central interest. If the key questions concern the mission-oriented outcomes of the CDFI sector, however, it may be more productive to begin with the nature of the outcomes, not with the nature of the delivery systems and the financial strength of those systems.

Some researchers have begun by thinking about the types of borrowers/investors that CDFIs target (Benjamin, Rubin and Zielenbach, 2004). This is closer to the approach we adopt here. We first consider the sorts of borrowers or investees that CDFIs target directly. Then we examine the sorts of products they provide to these different customer groups.

Table 1 provides a basic typology of CDFI product lines by type of borrower/investee ("customer") and financial product group. Customer types include households, micro-entrepreneurs, low-to-moderate growth firms, emerging or high-growth small firms, nonprofits (excluding housing development organizations), and real estate developers (both for-profit and nonprofit). The table is not intended to include every type of product or service provided by CDFIs and explicitly does not include the technical assistance or educational services they provide. It also does not consider any broader community development or community building activities or services that do not involve financing. These services certainly can be added to such a typology, but they were omitted for purposes of simplicity and focus on financial products and services. In many cases, the provision of CDFI financial products is closely and, sometimes, necessarily linked to the provision of technical assistance. However, given the data challenges we describe below, the difficulty of separating out financing provided with technical assistance from that provided without technical assistance could easily prove too daunting.

Financial product groups include bank accounts and related products, unsecured consumer loans and car loans, single-family mortgages, multifamily real estate loans, commercial real estate loans, business equity financing, and business term loans and lines of credit. These groups are essentially defined by the structural characteristics of the products (e.g., mortgages versus term loans, etc.). Some CDFI product groups are generally focused on one customer type (e.g., low-cost bank accounts) while others serve multiple customer types (e.g., commercial real estate loans).

Table 1. A Typology of Major CDFI Financial Product Lines, Excluding Technical Assistance and Education

Type of Borrower/Investee	Financial Product Groups						
	Bank Accounts	Unsecured Loans & Auto Loans	Single-Family Mortgages	Multifamily Real Estate Loans	Commercial Real Estate Loans	Business Equity/ Quasi-Equity	Business Term Loans & Lines of Credit
Households	Low-cost savings and checking accts *CU, CB*	Payday alternatives Overdraft products Student loans Auto Loans *CU, CB*	Purchase -senior/junior Improvement -senior/junior Refinances -rate/term -antipredatory *LF, CB, CU*				
Micro-enterpreneurs							↑
Low-to-Moderate-Growth Small Firms					↑		Term loans -senior/junior Lines of credit *LF, CB, CU* *ML for micro-ents*
Emerging, High-Growth Small Firms					Construction Loans Land financing Purchase - senior/junior Refinance -senior/junior Pre-development grants/recoverables *LF, CB, CU*	Equity Unsecured Debt Royalty financing *VC*	
Nonprofits/Charter Schools/Public Facilities (excluding housing developers)							↓
Real Estate Developers (for-profit and nonprofit)				Construction loans Purchase - senior/junior Refinance -senior/junior Pre-development grants/recoverables *LF, CB, CU*	↓		

Key to institution types: LF – loan funds; CB – community development banks; ML – microlenders; CU – CDCUs; VC – CDVCs

Within the various cells of the table are listed the more specific product line categories. For example, for the household customer category, the single-family mortgage group is disaggregated to include home purchase loans, home improvement loans, and refinance loans. Even within these categories we can disaggregate further. For example, home purchase loans might include junior as well as senior mortgages.

The versatility of some product groups across multiple customer types is indicated by the vertical arrows, which signify that a group covers more than one customer type. For example, CDFIs may extend term loans to microenterprises, small firms (from low- to high-growth), or nonprofit organizations. While the customers, rates and terms of these loans may vary by customer type, the basic loan structures are likely to be relatively similar.

This table represents an assessment of major product lines currently being offered by CDFIs. Of course, different observers of the CDFI sector would construct such a typology differently and many would include other products that they view as major product lines for CDFIs. However, the basic typological approach used here is quite robust, so that most additional product lines can be placed somewhere in this table.

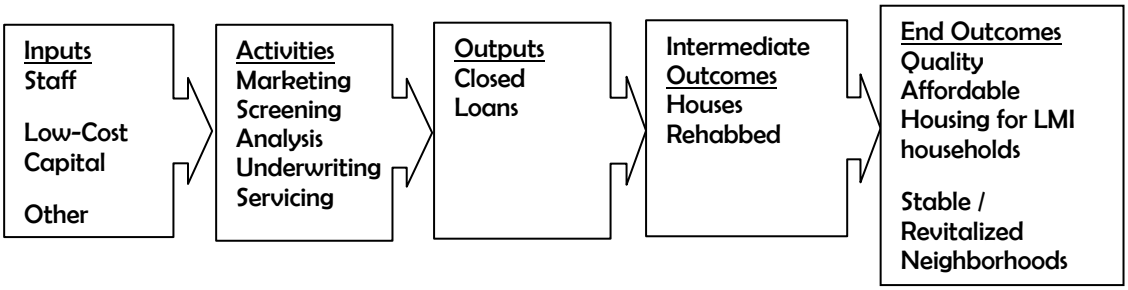
Another important component of Table 1 is the set of acronyms (bold, italicized) at the bottom of the cells. These identify the types of CDFIs that tend to offer the various product groups. The identification of CDFI product lines and their organization into customer and product groups will assist with thinking about measuring outcomes and gathering data on comparable non-CDFI products.

Performance Measurement versus Summative Impact Evaluation in the CDFI Arena

When people talk about CDFI "impacts," what do they mean? By impacts, some may mean what are called "outcomes" in the world of performance measurement (PM) (Hatry, 1999). Outcomes are the *purported* effects of completed CDFI activities (which in turn are called "outputs"). They are the concrete, desirable conditions that a program hopes to bring about for individuals and communities. PM distinguishes between end outcomes, intermediate outcomes, and outputs, with the former being defined by the basic mission and objectives of the CDFI. End outcomes might include things like access to quality, affordable housing for lower-income households, decreased unemployment or underemployment among a target population, higher wages, etc. Intermediate outcomes are outcomes that are generally recognized as good in and of themselves, but are generally most important in their critical role in bringing about desired end outcomes. An example might be the expansion of firms in a region, which is expected to lead to reduced unemployment or underemployment in a community. Outputs are basically completed program activities, which in and of themselves, have little intrinsic value. They are desirable only in that they lead to intermediate or end outcomes.

In PM, outputs and outcomes are closely tied to the use of logic models for describing and analyzing the theory, operation and performance of programs. In the case of a hypothetical CDFI with a home improvement loan product line, a logic model might look something like the following:

Figure 1. A Simple Logic Model for a CDFI Home-Improvement Loan Product Line



Those adopting the PM perspective tend to focus on challenges in defining and distinguishing outputs, intermediate outcomes, and end-outcomes, and in obtaining the data needed to constitute sound, quantitative indicators for each of these. Distinguishing between outputs and intermediate outcomes, for example, can be a key task of a performance measurement exercise.

In discussions of CDFI outcomes, the term impact (or "social" or "community" impact) frequently appears to be used when what is really meant is end-outcome. This is because an outcome—in PM language—is indeed a *purported* effect. That is, PM does not require that evidence be presented that supports the notion that the program *caused* the outcome.

Strictly speaking, the term impact is best left to the domain of summative impact evaluation (SIE). SIE attaches a meaning to the term impact that is critically linked to causality. The key question becomes "did the CDFI make a difference?" Therefore, measuring impact requires measuring the difference between CDFI outcomes and whatever would have occurred without CDFI intervention. Evaluators call the latter the "counterfactual." SIE seeks to identify causality between the program and the outcome and, in fact, typically involves measuring the extent of that causality, or how much of the measured outcome can be attributed to the program.

To SIE practitioners, the primary challenge typically becomes identifying the counterfactual, or developing a reasonable estimate of it. Although the evaluator would prefer measuring impacts on end outcomes, she may be willing to settle for measures of impact on intermediate outcomes or outputs if she can be relatively confident of establishing an accurate counterfactual for them. This is especially true if there is a strong, well established link between these more proximate effects (outputs or intermediate outcomes) and the end outcomes. (This is also a perspective frequently adopted by proponents of "theory of change" evaluation.) For example, if available evidence strongly demonstrates that access to capital is a key driver of a desired outcome, an evaluator's concerns might lie chiefly in whether, in the absence of CDFI activity, borrowers would have access to capital from somewhere else and, if so, at what costs and conditions compared to the CDFI products that they received.

Some SIE practitioners may look upon PM with a degree of skepticism. They may view any effort to measure outcomes without identifying the extent to which such outcomes can truly be attributed to CDFI intervention as a hollow task. Some might even argue that such efforts are misleading because they imply or, in some cases, assert causation.

PM practitioners are frequently more pragmatic. They will argue that, together with qualitative and interpretive knowledge of an initiative and its context, PM can, at least in some cases, allow us to construct a reasonable argument that impact is likely or unlikely and, perhaps, tell us something about the scale of any likely impact. PM can be used as a sort of heuristic tool that, when combined with other, often less formally or quantitatively acquired knowledge, can contribute to assessing impact.

Regardless of whether an analytical exercise provides us with a strong measure of impact of CDFIs in a given product line area, comparing CDFI outcomes to contextual data regarding needs or similar market activity can be useful. For one thing, it can give us some sense of how much aggregate difference a program *might* be making at its current scale.

For example, assume we know that all of the CDFI microenterprise lenders in Big City A made a total of 10 microloans in a recent year and the CDFI microenterprise lenders in Big City B made a total of 200 microloans in the same year. Let us also assume that the cities were similar in size and economic demography, and that we have a measure of bank lending to very small firms that indicated that such lending was also similar across the two cities. That is, there was no apparent difference in the appetite for lending to very small firms between banks in the two cities. This information would be useful information in comparing CDFI microlending activity across cities. Moreover, it seems fair to say that CDFI microlenders in Big City B were, at this point in time, *likely* to be having a greater overall impact on access to capital for very small firms than those in Big City A. Of course, the SIE practitioner might question whether CDFI microlenders in either city were actually having any impact, and that is a reasonable question. However, the point here is simply to consider outcomes and their context and not to quantify or prove impact. It seems that, in many cases, even this limited information would provide us with a good deal more information than we currently have.

Of course, it is critically important not to draw definitive conclusions from such analyses that microlending programs in Big City A "don't work." We have not been presented sufficient information to determine impact in any thorough, systematic way. This is precisely what CDFI practitioners are likely to worry about—that findings about current (and perhaps nascent) operations and outcomes will be used to draw such conclusions, perhaps not by researchers, but by some people who really matter: the media, policy-makers, funders, and the general public.

Depending on the intermediate and end outcomes of the CDFIs in a particular subsector, we might look for data to place these outcomes in an appropriate context. If the microlenders, for example, are looking to influence the behavior of conventional banks towards microenterprises (e.g., encourage them to make more loans to very small firms)—what might be called an *institutional* or *structural* impact—it is important to understand the scale of outcomes relative to the activity of the industry that is the target of influence. Is it likely that a subsector making just a few loans in a large city is having a significant influence on the behavior of banks in the city? Probably not.

Relative scale is just one form of context. Other aspects of context include the relative challenges of working in different places. It may be that, in some places, a certain outcome would be quite impressive, while in another, it would not be so remarkable. To complement quantitative

measures of performance or outcomes, qualitative data, including information from detailed key informant interviews could prove helpful.

At the same time, in the realm of institutional impacts, scale matters for reasons beyond just the subsector's ability to deliver more capital directly to the target firms or households. Scale matters because the subsector becomes substantial enough to affect lender behavior in different ways. Mainstream lenders (e.g., banks, thrifts, finance companies) actions are affected by a complex set of forces that are derived not only from traditional competitive pressures but also from regulatory obligations, reputational concerns, policy debates and developments, and their involvements in the corporate and civic life of a community. An active, productive microlender is likely to draw the attention of the banking community—especially small business lenders. Bankers may begin to view microenterprise as a more important activity, may see value in becoming more involved with smaller firms, and may be interested in developing ways of partnering with a CDFI that they see as possibly bringing reputational or Community Reinvestment Act benefits. Banks tend also to be interested in developing strong relationships with those whom they perceive as the community development leadership in an area.

One problem is that, even before considering methods for measuring impact, we do not always have a firm grasp on outcomes for many CDFI activities at a level necessary for even the crudest attempts at impact assessment. When we do have data on outcomes, the data are frequently not placed in any sort of context regarding potential need or demand.

The intent of this paper is not to take sides on the PM vs. SIE perspective, but rather to assess what may be achievable from both perspectives. Certainly, PM is generally less ambitious and much more feasible than SIE for most CDFIs and for most applications. It will often be sufficient for purposes of gaining some confidence that a particular CDFI is headed in the right direction and is *likely—or not—*to be having some sort of meaningful impact on its community. Systematic, quantitative evidence of impact is not always required or appropriate.

Again, PM can provide some important information which, together with a broad and perhaps less than systematic set of other, often less quantitative set of knowledge, gives decision-makers a high degree of *comfort* that a program is making a difference. PM provides just one heuristic tool that, as part of what Schorr (2003) calls "multiple ways of knowing," can contribute to a much less systematic, but more pragmatic and perhaps achievable, way to discern—if perhaps not precisely measure—impact.

Though SIE studies may promise more systematic and quantified measures of impact, their greater ambition brings with them greater risks for error. Many studies that do purport to measure impact will be done so poorly that they will be seriously misleading. As in any type of social science research, individual studies should be rigorously reviewed and should never be considered conclusive in and of themselves.

At the same time, to dismiss the potential for research that may give us better information than we currently have on CDFI impacts may condemn the field to a sort of purgatory of "we can never convince skeptics that we are making a real difference." In the end, in a world of many different programs and policies competing for scarce resources, resigning to an inability to

measure the field's impact is unlikely to be good enough to sustain the field. It is important to recognize the limits of impact research, especially as it exists today, but it is also important to invest in improved data and methods so that the CDFI field has an opportunity to document the differences it makes in lower-income and disadvantaged communities.

Unit of Analysis: Household- or Firm-Level Versus Geographic

In considering issues of outcome measurement and impact evaluation, we might consider measuring outputs or outcomes at the level of households, firms, or place. In conventional social program measurement or evaluation, data are generally desired at the individual or household level. However, there are a variety of reasons why households may be either an infeasible or undesirable unit of analysis when it comes to the evaluation of CDFI activity.

For some CDFI product lines and outcomes, we may want to measure outcomes at the level of the household or, in the case of business-oriented activities, at the level of the firm. The difficulty often lies in identifying appropriate control or comparison groups. In the absence of establishing experiments, quasiexperimental methods demand some sort of comparison group. As Hollister (2004) argues, however, identifying appropriate comparison groups can be very challenging. Beyond the problems of comparison groups, even obtaining data from members of treatment groups—either firms or individuals—can be daunting. In the case of employment-focused business development programs, for example, attempting to gather data on workers, and not just the firms directly receiving assistance, can be difficult.

For other product lines and outcomes—particularly those with high degrees of relative spatial density—we may expect neighborhood or geographic effects. This may be out of accident (the programs just happen to be clustered spatially) or out of design. Some CDFI activities are tied—to a lesser or greater extent—to some notion of place. Place-based impact is in fact a part of their mission.

As a field, the perspective that CDFIs take towards spatial targeting covers a broad spectrum. Although she was not considering CDFIs per se, Ladd (1994) provides a useful typology of three approaches to alleviating the problems of lower-income people and places that is relevant here. First, people-based strategies focus on helping people but do not pay any significant attention to the places where they live. Examples of CDFI activity falling into this category might include minority business financing programs that make loans to firms regardless of their location throughout some large metropolitan area or state.

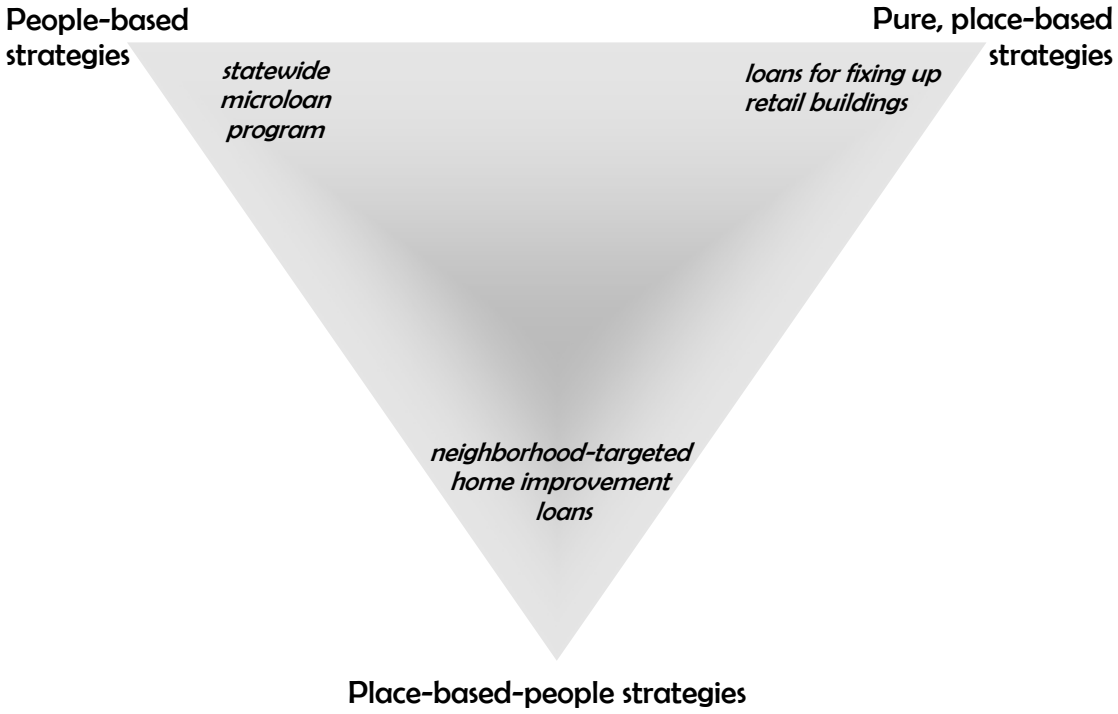
Second, place-based-people strategies focus on places—perhaps certain neighborhoods or types of neighborhoods—to increase the well-being of people living in such areas at the time of intervention. The focus is on improving the lives of lower-income or disadvantaged people in such areas. But there is no primacy given to changing the quality of the place. Place is a tool for helping local residents. These sorts of approaches pay special attention to any positive spillover effects of financing activity on the *neighbors* of customers. For example, it may be important to consider the impact of a home improvement loan program on the neighbors of loan recipients as well as on the recipients themselves. Also underlying many such strategies – especially those

targeting the neighborhoods—is the notion that the relative density of lending or investing activity becomes important.

Third, pure place-based strategies aim to improve the physical and economic vitality of a geographical place or set of places with little attention given to the impact on the current residents of the place. The condition of residents is not seen as the end, so much as a means to improve the geographic community. Differentiating, for example, between a neighborhood that "improves" via incumbent upgrading and one that improves via in-migration and out-migration is not of great concern. Such a difference would be of significant concern to those pursuing a place-based-people approach.

In reality, these three strategic types are only models, and CDFIs can and do adopt mixed strategies or move from one to another. Figure 2 provides a conceptual map of this strategic space. Particular product lines can be positioned within this triangular space. Some may be more likely to be employed as a part of people-based strategies while others (e.g., real estate-related programs) are likely to be part of pure-place-based to place-based-people strategies. An individual CDFI may offer a variety of products that vary in their strategic direction. Some of its products could be part of a pure place-based strategy, while others might follow a place-based-people approach. Moreover, some product lines are quite flexible, strategically. A small business loan product might be employed in a targeted, place-based strategy to revitalize a retail strip, or might be used to encourage employment over the CDFI's entire service area.

Figure 2. Strategic Space: Targeting Places, People or Both



Distinguishing among these strategic approaches is critical to developing appropriate outcome measures. Product lines that are employed as a part of a place-based strategy are better candidates for geographic analysis and evaluation, while products that are used as a part of a pure people-based strategy are less likely to be appropriate for such techniques.

Related to the issue of the spatial targeting of CDFI activity are the issues of market size, the mobility of capital and labor, and the capitalization of benefits in residential property values. The size of labor markets, for example, makes targeting of employment effects at the neighborhood level frequently problematic. Firms can hire from broad distances and there is some evidence that firms in minority neighborhoods tend not to draw their workforces from near their facilities as much as firms in other locations (Ihlanfeldt, 1999).[1] The relatively large geographic scale of labor markets as compared to housing markets means that the benefits of employment-oriented CDFI interventions will be more diffuse spatially as compared to housing market investments.

Moreover, because business lending or equity investment frequently focuses on increasing a firm's stock of relatively mobile capital (equipment, inventory) or human resources, less goes into permanent improvements in spatially fixed real estate. Housing investment is much more fixed spatially. Of course, some of the benefits of housing investment may diffuse spatially, especially over time, as beneficiaries move on to different residential locations, perhaps partly as a result of earlier CDFI interventions (e.g., as new homeowners become established homeowners and move into a larger house). Overall, however, a larger portion of the benefits from housing investment is likely to accrue locally as compared to investments in job creation or retention.

When considering measuring effects spatially versus individually, it is important to keep in mind the general phenomenon of the capitalization of neighborhood amenities into residential property values. As various aspects of the "quality of life" in a neighborhood improve, residential property values are expected to rise. In essence, the future value of those improvements are at least partially capitalized into property values. If schools improve in a community, crime drops, or sufficient numbers of residents fix up their homes, we expect to see an increase in property values (Ding, Simons and Baku, 2000; Judd and Watts, 1981; Lynch and Rasmussen, 2001).

Of course, property values should be used with some caution as a measure of neighborhood quality of life. If values appreciate very quickly, displacement of existing residents or speculative price bubbles are a concern. Moreover, many nonmarket neighborhood qualities, including various forms of social capital, may not be fully capitalized into property values.

Notwithstanding these limitations, moderate levels of appreciation (and certainly the avoidance of falling values) are generally seen as a positive neighborhood indicator. It is important to note that this signal of neighborhood quality of life is not just relevant to homeowners. Because property values reflect all sorts of improvements in the quality of life for residents, it is expected that the quality of life for renters would also improve, assuming that increases in rents or taxes are not excessive.

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[1] It is true that the evidence shows that minority-owned firms employ minority workers at higher rates than similar white-owned firms (Bates, 1993). However, on average, most employment in minority neighborhoods is not based at minority-owned firms.

Necessary Reduction: The Problem of Dealing with Collective Preferences and Diverse Outcomes

Identifying the outcomes of a single product line of a single CDFI can be difficult enough. Different stakeholders—program managers, funders, clients, and other—frequently have different views of desired intermediate and end outcomes. This is essentially a problem of collective preferences or collective rationality. The problem gets even more complicated when dealing with multiple CDFIs, even if they are offering similar products in the same overall marketplace.

For example, in the arena of small business lending, different CDFIs may have similar loan programs. However, some CDFIs may focus their lending on firms that promise to create high-paying, high-skill jobs while others may focus on creating entry-level, lower-skilled jobs. The former are likely to identify quite different end outcomes than the latter. In the arena of home purchase lending, one CDFI may focus its product on enabling very low-income households to purchase homes, while others may focus on helping moderate-income people to do so. Again, the end outcomes identified by different CDFIs can vary substantially.

A complete reconciliation of such diverse outcomes in an effort to measure the impact of CDFI activity within an entire product line is impossible. However, it may be possible to identify dominant outcomes or indicators that many if not most of the CDFIs offering such products would agree upon as reasonable—if not comprehensive—measures of success for a product line. Also, we might be able to identify measures that are strong proxies for these dominant outcomes. The work of CDFI trade associations on performance measures would be a key source for identifying such indicators.

An example of such work is that of Galster, Hayes and Johnson (2005), who developed what they call "robust, parsimonious" indicators, which provide strong proxies for a number of key neighborhood social and economic dimensions. Galster, Hayes and Johnson used multivariate factor analysis to reduce a broad list of neighborhood conditions into a set of factors that do not suffer from the sort of statistical problems that arbitrarily chosen multi-attribute indexes often do.[2] They identified algorithms that combine variables from a large set of census and other data to describe six key neighborhood dimensions: Social Disadvantage, Housing Tenure and Type, Prestige, Business and Employment, Crime, and Housing Vacancy. Each dimension was determined from a different combination of underlying variables. Then, from a set of annually updated, regularly available (at reasonable costs) variables, they identified subsets of variables which would serve as strong proxies for each of the different dimensions. Among these indicators were median home purchase loan size, available from Home Mortgage Disclosure Act data, and Dun and Bradstreet business count data.

What this technique provides is a way to develop proxy measures for a complex array of neighborhood conditions. The proxies utilize regularly updated, affordable data. Because many

[2] Some researchers have developed neighborhood "quality of life" type indices that use some sort of simple additive summation (frequently a weighted sum) of a number of highly interrelated variables. Such methods are highly problematic and suffer essentially from what statisticians call multicollinearity, in which two or more variables are highly correlated, so that the index ends up, in effect, merely giving a great deal of weight to one or two of the underlying components.

data sources are infrequently updated, they are not directly useful for methods of impact evaluation that require measuring outcomes at many intervals. As we will discuss later, the ability to have frequent measures of outcomes (or strong proxies), improves our ability to develop counterfactuals and provide stronger quasiexperimental evidence of impact.

Experimental Methods for Summative Impact Evaluation of CDFI Product Lines

In considering potential methods for evaluating the impact of CDFI product lines, we first consider experimental methods, which are generally considered the first-best option in program evaluation. In experimental evaluations, "treatment" and "control" groups are randomly selected from some broader population. While experimental research designs are more often associated with household- or firm-level data, it is possible to conduct experiments at the neighborhood or community level, and we will discuss these possibilities.

When applied to CDFI activity, experimental impact evaluation, traditionally considered the gold standard of summative impact evaluation, might look something like this: A randomly chosen subset of applicants would be provided with CDFI products or services, while the remaining applicants would not. Thus, the CDFI services are actually withheld from a control group. The experimental group cannot consist of all households or firms that express a desire for CDFI products. Otherwise a selection bias problem would exist, because applicants are likely to possess some traits (e.g., motivational characteristics), which make them more likely to do better (or worse) on the chosen outcome measures.

The traditional experimental approach to program evaluation is to randomly assign applicants to treatment and control groups. The control group is then denied program services. Given sufficient numbers of applicants in each group, randomization will insure that the two groups are "mean equivalent" on both measured and *unmeasured* characteristics. Then, outcome data are collected on each group and differences in outcomes are attributed to the program.

Quantitatively, the impact is then simply measured by the difference in mean outcome between those receiving the CDFI product (treatment group) and those that do not (control group).

$$\text{Impact} = \text{MEAN}_{\text{treatment}} - \text{MEAN}_{\text{control}}$$

There are a number of practical challenges to implementing experimental studies in the case of CDFIs. First, gathering the necessary data on outcomes from members of the experimental and, especially, the control groups can be quite difficult. The process of acquiring data on the firms or individuals receiving assistance is at least conceptually straightforward, albeit not without substantial practical challenges.[3] We know who participated in a program and who received loans or investments or obtained a bank account from the administrative data of the CDFIs.

[3] Beyond the challenges of how to collect data, either from control or treatment groups, there remain the problems of data privacy and confidentiality. It is particularly important to obtain the necessary permissions from firms or individuals when any potential disclosure of participants may occur.

Ideally, we also have information on attributes of participants that concern key outcomes, and not just outputs. In the case of firms receiving loans, we would hopefully have employment information at different points in time, although obtaining this information can be difficult, especially as time goes on. It is even more challenging to gather data on firms or households that do not receive loans or assistance. What incentives do they have to provide data and comply with research requirements? If some control group members drop out of the dataset over time, this can introduce potential bias into the results.

A second problem with experimental studies involving CDFI programs is the difficulty of denying services to control group firms. Some CDFIs may view such denial as unfair.[4] For others, denying services to otherwise eligible firms may be politically difficult.

A third problem, and one that is converse to the problem of service denial, is that, for some programs at certain points in time, the deal flow may be so weak that there are insufficient firms or households to constitute a control group.

Bartik (2002) has suggested an alternative experimental technique for evaluating economic development programs at the firm level which avoids the need to deny service to firms. From a list of potentially eligible firms, a control group is randomly selected for intensive marketing. Another randomly selected group, which does not receive any special marketing, is tracked as the control group. Then, the outcome of interest is tracked for both groups—including those firms in both groups that did not receive assistance. Not all firms in the experimental group will apply for or receive assistance, and some firms in the control group will still apply for and receive assistance, even without being aggressively marketed to.[5]

The treatment here is essentially the focused, special marketing applied to the randomly selected treatment group. The method is essentially measuring program impact on the sorts of firms that respond to the targeted marketing. If the program has different effects on those firms that use the program without any need for targeted marketing, then this method will not capture those differences. This could prove a serious shortcoming of the approach. This method also presumes access to outcome data not only on firms not receiving assistance from a CDFI, but also on those that do not even seek assistance. Overall, it is difficult to see where the approach would prove feasible.

[4] Justification for social experiments (including potential CDFI experiments) is based on random assignment to the control group and on a need to demonstrate program impact in a context of allocating scarce resources. Of course, care should be taken to follow ethical research protocols.

[5] To measure the effect of the program—including the marketing of the program— researchers would begin in a similar way as the classical experiment above, albeit with treatment and control groups defined differently. They would measure the gains in outcomes among firms in the treatment group and subtract from that figure the gains in the control group. The net gain should then be normalized by the usage rate in the treatment group.

Experiments Using Geographic Areas

In addition to experiments at the individual level, researchers can conduct experiments structured by geographic clusters such as neighborhoods. The classical experiment can be conducted by identifying the universe of eligible geographies (based on programmatic criteria) and then randomly assigning areas to treatment and control groups. As in the case of the individual-level experiment, services would be withheld from the control group areas and differences in outcomes between the two groups would be tracked. Bloom (2005) suggests five contexts in which using randomized geographies might be the preferred method for program evaluation:

1. The effects of the program have a potential to spill over to a substantial degree among participants or from participants to nonparticipants.
2. The program's services are delivered most efficiently when targeted spatially.
3. The program is designed to address a spatially concentrated situation.
4. Maintaining the integrity of the experiment requires the physical separation of treatment group members from control group members.
5. Using such a method will reduce political opposition to randomization.

Several of these reasons may apply to some CDFI programs. The fifth reason, for example, is one of political feasibility and appears to be relevant to many CDFIs. It may be easier to withhold the offering of a product line from some set of otherwise eligible geographic areas rather than attempting to withhold services from different households or firms. A CDFI—or set of CDFIs—could randomly choose, from a larger set of eligible neighborhoods, a group of neighborhoods in which its products or services would be made available. This might not be done for all products, but for a specific product line. Politically, such an approach might prove more feasible in the case of new products that are not already offered in a region.

One advantage of geographic analyses is that outcome data, while perhaps not plentiful, generally do not have to be directly from individual participants. Rather, census or other, more frequently collected small area data may be sufficient to provide quantitative indicators for key outcomes. This is particularly advantageous in the case of control groups. In individual level experiments, data from control group members may be particularly hard to gather.

One challenge that immediately arises with group or geographic randomization is the requirement to choose large enough numbers of geographies in both experimental and control groups to be able to ensure that sufficient randomization is ensured. Bartik (2002) suggests a rule of thumb of at least 20 areas in each of the treatment and control groups, though he provides no justification for this rule. Bloom (2005) discusses some of the detailed statistical issues in designing randomized experiments based on geographic clusters. Bloom's analysis suggests that, depending on the statistical power desired, groups of 20 geographies may be less than sufficient for randomization.

Quasiexperimental Methods for Measuring Impact

When experimental methods are not feasible, researchers typically attempt to apply what are called quasi-experimental methods to identify programmatic impacts. These same sorts of methods could also be used to identify the impact of aggregated CDFI activity in specific product lines. While quasi-experimental methods of different sorts are typically not immune from validity threats (Hollister, 2004), some methods may provide a sufficient degree of rigor – or at least provide us with information that is expected to, on average, give us substantially more accurate information than we had without the use of such methods.

There are a variety of quasiexperimental impact evaluation methods that may be appropriate for assessing CDFI impact. Not all of the potential methods and their faults are reviewed here. Much of that has been done elsewhere (Hollister and Hill, 1995; Hollister 2004; Dickstein and Thomas, 2005). Some of these alternatives can get quite complicated methodologically, and an extensive discussion of all of these is not appropriate here. Rather, the focus here is on a few approaches that have been given the most serious attention in the relevant literature.

These methods fall into three groups. First is econometric simulation, in which multivariate methods are used to control for differences between nonrandom treatment and comparison groups, including the difference in the likelihood of applying to or being recruited into a program. This latter difference is key. If we merely attempt to identify differences in groups that we believe will affect the outcome variable, we may not adequately control for selection bias. Conventional econometric methods are either used to predict some raw level of outcome indicator, or are used to explain the "difference in differences" between the treatment and comparison group. That is, multivariate estimation is used to explain differences in gain (or loss) in a key outcome measure following program intervention (e.g., receiving CDFI loans of some kind) between the treatment group and a comparison group.

The second nonexperimental category of methods used to measure impact is commonly referred to as propensity score matching. This approach is related to econometric methods that explicitly control for selection bias. A propensity score is basically the probability that, given certain features, a household, firm, or geography will receive "treatment" (e.g., receive a loan or investment). In this method, households, firms or geographies are grouped according to propensity scores. Within each of these groups, outcomes for those receiving treatment are then compared to outcomes for those not receiving treatment.

Propensity-score-based matching studies have been used in the arena of economic development evaluation. Greenbaum and Engberg (2000) used this method to evaluate the impact of enterprise zones on housing markets in six states by comparing changes in prices for zip codes that contain enterprise zones to those that do not. They first measure the probability that a zip code contained an enterprise zone and then compared only those zip codes that fell within the middle third of this probability distribution. O'Keefe (2003) used propensity score matching to measure the impact of the California Enterprise Zone program on census tracts. Each enterprise zone tract was matched to the non-enterprise-zone tract with the closest propensity score and that lie within the same county. Differences in growth between the zone tract and the nonzone tract were then used as estimates of program impact.

Generally, however, propensity scoring techniques are not yet well accepted as strong quasiexperimental methods. Hollister (2004) argues that propensity score matching techniques have not provided estimates of impact that are "consistently close" to those obtained from experimental methods.[6]

The last general category of approaches reviewed here is that of geographically-based adjusted interrupted time series analysis (AITS), which is a special subset of the econometric simulation approaches (Galster, Temkin, Walker, and Sawyer, 2004). In this approach, outcome data for geographies receiving treatment are compared to those not receiving treatment. However, in AITS, researchers utilize time series data over a relatively substantial period of time (e.g., several years or longer) that are frequently collected (e.g., annually or more frequently) and that provide us with many observations over the study period—the more the better. This approach allows for the measurement not only of pre-intervention levels of the outcome indicator for treatment and comparison geographies, but also the trends of the indicators in both groups before and after intervention. By being able to control for the differences in both the *levels* and *trajectories* of the treatment and comparison groups before and after the intervention, researchers can control for any omitted characteristics that might influence the outcome indicator.

Predecessors of AITS approaches include methods that seek to match geographies using more limited historical data, and so do a poorer job of controlling for selection bias. Instead, selection bias is discounted as a problem as long as the comparison group is found to be in no better a position in terms of outcome indicators before the intervention, as compared to the treatment group. A relatively well known example of this approach is Isserman and Rephann's (1995) study of the Appalachian Regional Commission (ARC). In this study, the authors measured the impact of the ARC on county population growth by identifying a matched "twin" county for each ARC county. There are hundreds of ARC counties, which makes this approach feasible. Matched twins were identified by identifying a variety of variables that are expected to be predictive of population growth and developing an index that measured the different between two counties based on these variables.[7] Matching twins could come from anywhere within the U.S., but not within 60 miles of the ARC area. Matching was based on both level and trajectory data from 1959 and 1950-1959 before the beginning of the ARC in 1965.

The accuracy of the match between the treatment and control group twins was then compared by identifying how well each pair was matched on 1959-1965 growth—again before the ARC was established. Once it was established that the comparison group twins, on average, grew as much or more than the ARC counties, then the comparison counties were established as a conservative basis for estimating impact following ARC initiation. Because this method does not require the frequency of data that the adjusted interrupted time series does, it may prove more feasible in some cases. However, the approach is more vulnerable to selection bias.

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[6] To be fair to those who used propensity score matching in analyses of geographic, rather than individual, data, there appears to have been relatively little testing of these methods for geographic applications.

[7] Isserman and Rephann (1995) used Mahalanobis distance as their measure of difference between counties (or, conversely, similarity between counties).

Measuring Structural Impacts via Financial Output Variables

Many of the analytical approaches that have been discussed so far might be aimed at determining whether CDFIs are having impacts on key, crosscutting social and economic outcomes, including housing conditions, property values (which may be indicating broader conditions), employment, income or other phenomena.

However, there are at least three reasons why it may be useful to look at the impact of CDFI activity on more intermediate outcomes—or even outputs—rather than these broader socioeconomic end outcomes. First, as we move further down the logic model continuum from output to end outcome, we generally will have greater difficulty attributing outcomes to CDFI activity. This is because, as we move down this continuum, a greater number of other factors become involved in producing the outcome of interest. For example, even if we can ascertain that CDFI activity was responsible for spurring flows of capital to firms that otherwise had poor access to capital, we will have greater difficulty identifying how many jobs should be attributed to such access and not to some other factors. While capital access may be critical to producing the desired outcomes, to the extent other factors are involved, measuring the impact on employment outcomes will be more challenging.

Second, it may be that we have better data on the financial outputs of CDFIs and related financial activity than we do on neighborhood or firm level socioeconomic outcomes. This may be because CDFIs are more likely to report output or intermediate outcome data than end outcome data. Moreover, if we are placing CDFI data in the context of data on other mainstream financial providers, it is much less likely that we will have data on end outcomes associated with such providers. Yet we may have information on financial outputs or outcomes for such providers.

The third reason why it may be useful to examine CDFI impacts on outputs and intermediate outcomes is because CDFI activity may have institutional or structural impacts on some segment of financial markets. CDFIs may encourage conventional lenders to behave differently in some way. Banks may learn different techniques by partnering with CDFIs. They may discover that the types of borrowers or projects that they believed to be too risky are not so risky after all. Even if banks do not partner with CDFIs directly, just witnessing CDFI activity in the same general market area may change how a bank or conventional lender views or approaches a certain market segment.

Some argue that CDFIs are simply "gap fillers," making loans that banks are unlikely to make otherwise. As banks reach farther into less traditional market segments, such a model may imply a substitution from CDFI activity toward bank activity. Such a model would imply that bank activity and CDFI activity might be negatively correlated. Other models for the sector suggest that CDFIs influence the behavior of conventional financial institutions in ways that encourage these institutions to become more involved in the market segments targeted by CDFIs. Under such models, we might expect CDFI and bank activity to be positively correlated.

Anecdotally, this is the sort of story that has often been associated with South Shore Bank (now Shorebank) in Chicago. Prior to South Shore's targeted lending to multifamily apartment rehabbers in the South Shore neighborhood in the 1980s, few conventional lenders appeared interested in funding such projects in Chicago's low- and moderate-income neighborhoods (Goldwater and Bush, 1995). After South Shore fostered this market and the scale of rehab activity grew, larger, conventional lenders became more interested in this sort of lending. Then, most of the larger banks in the city established regular multifamily lending operations and the market for such loans grew much stronger.

Again, to have institutional or structural impacts, CDFIs may need to reach some minimum scale in a product line area. Scale may matter for reasons beyond just the ability of CDFIs to deliver more capital directly to the target firms or households. Scale matters because the CDFIs become substantial enough to affect lender behavior in different ways. Mainstream lenders are affected by a complex set of forces that are derived, among other things, from regulatory obligations, reputational concerns, policy debates, and their involvements in the corporate and civic life of a community.

Structural impacts might best be detected at the level of financial market areas, e.g., the level of metropolitan areas or counties. These market areas are similar to the geographical areas that the Federal Reserve and other agencies use for evaluating competition in considering mergers and acquisitions. The general approach to such a study would be to measure CDFI activity in a particular product line across a large number of metropolitan areas. Because CDFIs may be more likely to operate on a more substantial scale in central cities, however, it may make more sense to use central cities (each within its unique metropolitan market area) as the geographic unit of analysis. Mainstream financial institution (banks, finance companies, etc.) activity in closely related product lines would be measured for each city. Models would be developed to explain the level of desirable financial end outcomes that might be encouraged by CDFI activity. For example, if CDFI loans to fund multifamily rehab are expected to spur a more robust market in conventional loans for multifamily rehab, then we might construct a model as follows:

$$MF_{it} = \alpha + \beta_1 MF_{it-1} + \beta_2 CDMF_{it} + \beta_3 CDMF_{it-1} + \boldsymbol{\beta}_4 \mathbf{X}_{it} + \boldsymbol{\beta}_5 (\mathbf{X}_{it+1} - \mathbf{X}_{it}) + \varepsilon \qquad (1)$$

where:

- MF_{it} equals the number of multifamily rehab loans by conventional lenders in period t for city i
- MF_{it-1} equals the number of multifamily rehab loans by conventional lenders in period t-1 for city i
- $CDMF_{it}$ equals the number of CDFI multifamily rehab loans by CDFIs in period t for city i
- $CDMF_{it-1}$ equals the number of CDFI multifamily rehab loans by CDFIs in period t-1 for city i
- $\mathbf{X}_{it}$ is a set of local economic and demographic characteristics that are expected to affect the demand or supply of multifamily loans made by conventional lenders. These variables might include indicators of economic growth, employment, and population as well as data on financial services providers and market competition.
- $\mathbf{X}_{it} - \mathbf{X}_{it-1}$ is a set of variables that measure the changes over the t-1 to t period in the economic and demographic characteristics for each city.

The above model measures the change in city-level multifamily lending by conventional lenders as a function of both the initial level of CDFI multifamily lending and the change in CDFI

multifamily lending, while controlling for a variety of other economic and demographic conditions and changes in those conditions. If CDFIs have no impact on the market for multifamily loans, then β_2 and β_3 are expected to be zero. If CDFI activity reduces the level of conventional multifamily rehab loans in the same region (suggesting a substitution effect between CDFI and conventional lender activity), then β_2 and/or β_3 are expected to be less than zero. If CDFI activity increases conventional activity, the β_2 and/or β_3 are expected to be greater than zero.

A lagged value of CDFI lending ($CDMF_{it-1}$) is included on the right hand side of equation 1) because the impact of CDFI lending on mainstream lenders is not expected to be immediate. Moreover, this model might be enhanced by including more than one lag period. For example, if the initial lag period is a year, data for earlier (two or three) years of both conventional and CDFI lending might be included as independent variables.

One important issue that should be addressed in a model such as 1) is the potential for nonlinear or threshold effects. Nonlinear affects might occur, for example, if there are diminishing marginal returns to CDFI lending. That is, if increased CDFI lending has a positive effect on mainstream lending, but that this effect saturates as CDFI lending increases. Second, a threshold effect would be one where a minimum amount of CDFI lending is required to have any impact on mainstream lending. This can be tested for by converting the continuous CDFI lending variables into categorical variables which represent different thresholds of lending volumes. Iterative specifications of such categorical variables may be required to detect any thresholds.

Another method for potentially testing for the influence of CDFI activity on mainstream financial institutions—and for addressing concerns about the direction of causality of any relationship found in estimating equation 1—would be to develop measures of financial product innovation among CDFIs and banks or other mainstream lenders. Retrospective historical analysis of innovations in product design, pricing, or terms could identify whether new products and terms introduced by mainstream lenders were preceded by similar products or terms developed by CDFIs.[8]

Data Availability for Product Line Impact Evaluation

In order to identify the prospects for research that would develop some reasonable measures of impacts of CDFI activity within product-line areas, I evaluate the types and quality of the data that are available, or might be made available at reasonable cost, for the financial product groups identified in Table 1. Categories are ordered from "most promising" to "least promising," with most promising meaning that sufficient data may exist for measuring *some aspect* of CDFI impact. Less promising product groups are those which the data appear to be far from sufficient to provide for any meaningful evaluative analysis.

[8] Thanks to Michael Stegman for making this suggestion.

Single Family Mortgages

Within this CDFI product group there are a number of different sorts of product lines, including senior and junior mortgages for home purchase, senior and junior mortgages for home improvement, and refinance loans. Some CDFIs, especially those with a major homeownership focus, will offer programs in more than one of these product lines. Perhaps the best known examples are the NeighborWorks affiliates (e.g., local Neighborhood Housing Services) around the country. Because many CDFI programs focusing on single family lending may include community development outcomes among their primary objectives, and because there is significant evidence that home improvement (Ding, Simons, and Baku, 2000) and homeownership (Haurin, Dietz, and Weinberg, 2003) result in positive neighborhood spillover effects, they may lend themselves well to geographic impact analysis.

Before considering quasiexperimental methods, we should consider the potential for experimental approaches. One approach would be to use geographically based group random assignment. In such an approach, after identifying potential census tracts that meet the criteria for a new CDFI single family lending program researchers would randomly divide tracts into treatment and control groups. It would be critical for the selection process to be truly random and not corrupted by political or other criteria.

Of course, such a method requires the designation of randomized groups before a program is initiated and requires maintaining the "purity" of these groups during the study period. Such an approach may not be feasible in many cases. Therefore, quasiexperimental methods may be required. A substantial amount of quasiexperimental research has been conducted to attempt to evaluate the impact of concentrated community development or housing investment programs. Research efforts have ranged from the somewhat casual to the quite highly sophisticated. Examples of more casual studies include an analysis of the programs of community development impacts in specific Chicago neighborhoods (Metropolitan Chicago Information Center, 2003) and of community-based homeownership programs for the Local Initiatives Support Corporation (Higgins, 2001).

An example of a more sophisticated approach to analyzing housing interventions is that from Schill, Ellen, Schwartz and Voicu (2002). In analyzing the impact of New York City investments in housing (single and multifamily) on property values, they employed a model that controls for both neighborhood location and time of property sale to implicitly control for the fact that properties in different neighborhoods will likely be subject to different trajectories in property values. In this way, they attempt to control for selection bias problems. They then control for aggregate recent housing investment in the within 500 feet of each transaction and, separately, whether the transaction is located within 500 feet of a City-funded investment.

Galster, Tatian, and Accordino (2005) used property sales data in the city of Richmond to determine if efforts of the city and the area's Local Initiatives Support Corporation to concentrate community development investments strategically in certain neighborhoods had any positive affect on neighborhood property values. They utilized an adjusted interrupted time series (AITS) method that measures the impact of nearby community development investments on the level

and trajectory of property values. This is essentially a hedonic model that measures prices of different properties at different points in time.

The Galster approach addresses a primary problem of efforts to estimate the effect of community development interventions at the neighborhood level. It measures the effect of the intervention on both the level of the outcome variable and on its trajectory over time. It requires data that is collected over a substantial period of time and at relatively frequent intervals.

Galster, Tatian and Accordino (2005) found that Richmond's "Neighborhoods in Bloom" program produced significant revitalization in the targeted areas and in nearby areas. They also found the impacts to be substantially larger when investments exceeded a threshold amount per block.

Methods utilizing individual property sale transactions to estimate effects of loan or investment flows on property values are an approach that can be used for product lines in which neighborhood effects are expected. Thus, they should be especially valid for housing rehabilitation programs and single-family programs generally. In cases where multifamily lending is expected to have neighborhood effects (especially rehab-oriented programs), similar techniques are appropriate.

However, it should be acknowledged that the data requirements for such property value impact models, especially ones that do a good job addressing the issue of pre-intervention trajectories and selection bias, can be quite demanding. Not only are sales transaction data over time generally required, researchers will need information on property attributes (typically from a property tax assessor or a multiple listing service when available). These data are not always readily available at an affordable price. At the same time, electronic versions of such datasets are becoming increasingly available from third party vendors such as First American Real Estate Solutions and prices may decline over time.

Generally, the property value models that are used in such approaches fall into variants of what are called either hedonic regressions or repeat-sales models. The classic hedonic model is a cross-sectional model that looks at the impacts of different characteristics of a property on its selling price. One of those characteristics may be the amount of CDFI lending in close proximity to it. A wide variety of other property and neighborhood attributes are also controlled for. Variations on the basic hedonic model allow for some consideration of time in determining property values, allowing one to control, perhaps imperfectly, for value trajectories.

The repeat sales model utilizes data on properties for which at least two sales have occurred in some reasonable period of time (less than 10 years, e.g.). Then, price trajectories are compared for properties in close proximity to the intervention of interest (in this case, CDFI lending) and those that are farther from such intervention. Unfortunately, repeat sales tend to occur more often in more affluent communities and so researchers are likely to run into a "thinness" of repeat sales in areas that are of most interest to CDFIs. Thus, this approach will rarely be of use to the applications of interest here.

Combining insights from Galster, Tatian, Santiago, Pettit and Smith (2003) and Galster, Hayes and Johnson (2005), I suggest a second-best alternative that uses more readily available data that proxies for property value as a dependent variable and that is based on a census tract unit of analysis. In such a model, neighborhood median home purchase loan amount would be the key outcome, or dependent, variable:

$$V_{ct} = \alpha + \beta_1 V_{ct-1} + \beta_2 CD_{ct} + \beta_3 CD_{ct-1} + \beta_4 Z_{ct} + \beta_5 Z_{ct-1} + \boldsymbol{\beta}_6 \mathbf{X}_{c1990} + \boldsymbol{\beta}_7 (\mathbf{X}_{c2000} - \mathbf{X}_{c1990}) + \rho S_{ct} + \varepsilon \qquad (2)$$

where

- V_{ct} = the median value of home purchase loans in period t in census tract c
- V_{ct-1} = the median value of home purchase loans in period t-1 in census tract c
- CD_{ct} = the number of CDFI loans in the product line of interest in period t in tract c
- CD_{ct-1} = the number of CDFI loans in the product line of interest in period t-1 in tract c
- Z_{ct} = the number of conventional loans in the product line of interest in period t in tract c
- Z_{ct-1} = the number of conventional loans in the product line of interest in period t-1 in tract c
- $\mathbf{X}_{c1990}$ = a set of characteristics of census tract c in the year 1990
- $\mathbf{X}_{c2000}$ = a set of characteristics of census tract c in the year 2000
- S_{ct} = a spatial lag measure of median purchase loan values in tracts adjacent to c in period t

In this model, median home purchase loan amount is used as a proxy for single family housing value. Tract level housing value is expected to depend on the value in a previous period as well as the flow of credit into the tract in the previous period, including both CDFI flows and flows from conventional lenders.[9] The model would also control for demographic changes in the tract that are expected to affect property values. Since this model does control for factors that may, themselves, be affected by CDFI activity, the model would provide a conservative estimate of CDFI impact.

Multifamily Real Estate Loans

Estimating the impact of CDFI multifamily lending activity on local outcomes is conceptually feasible. However, one critical issue is determining what, if any, cross-cutting outcomes might be identified in the case of multifamily lending activity. Some CDFIs may view community development outcomes (perhaps measured by neighborhood stability indicators such as property values) as a desired product of multifamily lending. This would especially be the case for programs focusing on the repair and improvement of dilapidated multifamily properties. When small-area community development goals are the focus, analyses that look at local property value effects, such as the single-family lending model described by 2) above, may be feasible.

[9] Due to the potential noise in a single-year measure of v (annual sales may be sparse in many tracts), the period here should probably be a minimum of two years.

However, many CDFIs making multifamily loans are likely to focus much more on the aggregate production of affordable rental housing, with less regard to neighborhood impacts. Moreover, because little good data (other than the decennial census) are routinely available on vacancy rates or affordability measures at the small area level, analyses at the neighborhood level will be generally quite difficult.

At the same time, a city-level analysis that attempts to identify the impact of CDFI multifamily activity on indicators of housing affordability might prove feasible. For example, one might attempt to estimate the impact of CDFI lending on a measure of local rental affordability or, conversely, of rent-burdened households. Assuming that increased multifamily lending by conventional (non-CDFI) lenders may also influence rent levels (by impacting the supply of various sorts of rental housing), our model would control for such flows as well. The hypothesis would be that, other things equal, additional multifamily CDFI lending will increase the number of affordable rental units over time, leading to more affordable rental housing. A model for testing this hypothesis might look something like the following:

$$A_{it} = \alpha + \beta_1 A_{it-1} + \beta_2(CD_{it}) + \beta_3(CD_{it-1}) + \beta_4(Z_{it}) + \beta_4(Z_{it-1}) + \boldsymbol{\beta}_5\mathbf{X}_{it} + \boldsymbol{\beta}_6(\mathbf{X}_{it+1} - \mathbf{X}_{it}) + \varepsilon \quad (3)$$

where

- A_{it} = rental housing affordability index for lower-income households in city i in period t
- CD_{it} = the dollar value of CDFI multifamily loans made in city i in period t
- Z_{it}= the dollar value of conventional multifamily loans made in city i in period t
- $\mathbf{X}_{it}$ is a set of local economic and demographic characteristics that are expected to affect the demand or supply of rental housing. These variables might include indicators of economic growth, employment, and population as well as data on residential construction and operating costs, and the profitability of supplying housing for middle and upper-income groups.

Assuming that data on the CDFI loans could be compiled, data on the conventional activity could be gathered from HMDA. One shortcoming of the HMDA data on multifamily loans, however, is that until this year (2004 data), the HMDA data on multifamily loans do not indicate whether the loans were for purchase, rehabilitation or refinance of the property. This problem does not exist for single family data, where purchase, home improvement and refinance loans have been disaggregated historically.

Small Business Term Loans and Lines of Credit

CDFI lending to small businesses appears to be a weaker candidate for geographic impact evaluation. Many CDFIs that do small business lending do not target their programs at small area levels; nor do they generally seek small area impacts. At larger geographies, the lack of relative density is unlikely to allow for the discernment of geographic impacts. Hollister (2004) and Caskey and Hollister (2001) reviewed some attempts to evaluate the impact of what they call "business development financial institutions." They find existing efforts generally lacking and are less than optimistic in suggestions for additional research in this area.

It is conceivable that experimental methods might be used to evaluate the impact of specific CDFIs or small groups of CDFIs. Because withholding products or services from some firms and not others may prove problematic in some instances, Bartik's usage-normalized method, which was discussed earlier, might at first seem appealing (Bartik, 2002). However, as discussed earlier, this approach requires collecting data from all firms in both treatment and control groups, and both of these groups would include firms that did not apply for any CDFI assistance. Experiments utilizing geographic areas instead of individual clients are more promising. However, scale and density of activity remains an issue.

In terms of quasiexperimental approaches, there at least two possible routes, both of which appear quite challenging given existing data sources. First is a model similar to 1), which would attempt to measure the structural effects on small business lending markets on an intermetropolitan level. The goal would be determine whether increased CDFI small business lending is related to increased small business lending by conventional lenders. While Community Reinvestment Act small business lending data may prove sufficient for this task, the data are not without problems. Until recently, the data did not include loans made by banks with less than $250 million in assets or by finance companies. With recent changes to CRA regulations, even more institutions—those with assets below $1 billion—will not have to report these data. Unfortunately, the distribution of reporting lenders is not expected to be uniform. Small business lending in larger cities (with larger banks) and in cities with less competitive banking markets are likely to be better covered by the data.

The second approach would be to attempt to estimate neighborhood-level impacts of small business lending by CDFIs on neighborhood outcomes, similar to the approach described by equation 2) for single-family lending. Again, part of the problem here is the relative weakness of small business lending data, as compared to mortgage data. Another problem is that many of the more active CDFI small business lenders do not target their loans spatially, so that these programs are likely to achieve fairly low levels of relative density at the neighborhood level. In fact, many CDFI small business lenders avoid targeting programs at small geographic levels in order to maintain their ability to specialize by industry and product (as opposed to geography) and diverse portfolios.

Robinson Hollister has suggested a potential research method for determining whether CDFIs making loans to small firms are indeed making loans that otherwise would not have been made by conventional lenders and whether CDFI intervention appears to be improving the loan performance of firms.[10] The small business credit scores of CDFI borrowers could be analyzed to see whether their actual loan performance appears to exceed that predicted by the scoring model. If it does, then an argument can be made that, assuming the credit score data are generally accurate, CDFI intervention is adding value to the firms and enabling them to repay their debt at higher rates. Ideally, similar data would be gathered from banks in the same area to compare both approval practices and loan performance to that of bank borrowers. Of course, credit scores are not the sole basis for loan approvals that most banks use for many small business customers, but the basic research approach is still quite promising and might be operationalized at relatively modest costs.

[10] Per email and telephone correspondence with Robinson Hollister, December 20, 2005 through January 5, 2006.

Bank Accounts

In the arena of CDFIs offering consumer bank accounts (e.g., CD banks and credit unions), a good deal of performance measurement work is feasible, and some has been done. CDFIs can track outputs and intermediate outcomes of their affordable savings and checking accounts, alternatives to payday loans, and the like. Gathering data on end outcomes (e.g., aggregate savings of deposit holders) is much more challenging. Another major, and perhaps insurmountable, challenge in this arena is obtaining any sort of strong comparison group data. The branch deposit data collected by federal regulators from banks is too highly aggregated (e.g., commercial deposit accounts are not distinguished from consumer deposit accounts) to prove of much use to attempt even a rough correlational analysis. FDIC deposit data do not give any details regarding the incomes or other characteristics of account holders. FDIC data can be broken out by branch, although the attribution of deposits to branches may not be entirely consistent across financial institutions. In general, regulators encourage institutions to report deposits for branches where the account is opened, where transactions are most commonly made, or in nearest proximity to customer address. However, the bank has discretion on which method to use. Data from the Survey of Consumer Finances cannot be broken out geographically at any reasonable scale.

The best that might be achieved in this arena is to develop some estimates of market share held by CDCUs and CD banks in different cities. Data from the Survey of Consumer Finances might be analyzed to approximate what percentage of a typical branch's checking or savings accounts are held by different types of customers, although these would be hard to apply at anything below the county or metropolitan level. Banking industry market research (Sheshunoff, e.g.) firms have models that build off FDIC data to estimate deposits at the neighborhood level. Claritas, for example, regularly markets consumer financial data that it collects from a rolling survey of 100,000 households over 3 years. They do create census tract estimates for much of these data. However the accuracy of such data at this low level of geography is questionable given the size of the survey. Estimates at the county or MSA level may be somewhat more reliable.

Insufficient Data: Unsecured Consumer and Auto Loans, Commercial Real Estate Finance, Nonprofit Finance, and Business Equity

In these product groups, there are generally insufficient data to conduct any meaningful level of impact evaluation. Very little data exist to describe the level of mainstream financial institution activity in these product lines, especially with any geographic specificity. In the case of venture capital, while some data exist, they are proprietary and can be quite expensive. Significant research in these areas would require substantial, specialized survey work. Nonprofit finance, because it is substantially smaller than the other arenas, would probably be the arena for which such work would be most affordable, especially if it were restricted to just certain nonprofit submarkets, such as education or child care.

In the case of commercial real estate and business equity, CDFIs are less established and so are less likely to have reached a scale at which they expected to influence conventional market

activity in measurable ways. Moreover, apparently relatively few CD venture capital firms have reported data to the CDFI data project in a form that would allow for geographically specific analysis. Moreover, because CDVC firms rarely target their efforts at small geographic levels, evaluation efforts would most likely have to be at level of the firm. Other than experimental techniques, we see few options here.[11]

It may be that, in particular regions, CDFIs have made a substantial thrust in the area of commercial (nonresidential) real estate finance. In such cases, then geographic impact analyses such as those described for single family lending programs might be feasible. One might be able to examine the impact of such programs on property values or median purchase loan values. However, it would be difficult to control for commercial real estate flows from conventional lenders, which may be closely associated with CDFI activity.

The CDFI Data Project

One potential source of data for impact evaluations or program measurements of CDFI subsectors is the CDFI data project. This dataset represents the results of an ambitious and valuable effort to collect data from the CDFI sector at the institutional level. As of the latest round of data, 2004, the CDP dataset includes information on 517 CDFIs, including CDLFs, CD banks, CDVC funds, and a relatively small number of microenterprise funds who report data to the Aspen Institute. Of these CDFIs, all but 21 chose to have their data publicly disclosed. Thus, the public dataset for 2004 contains data from 496 CDFIs. 501 of the responding CDFIs provided permission to release their identities to CDP partners (a group of CDFI trade associations and funders), and 406 agreed to the release of their identities to the general public.

The CDP dataset provides the definitive source for descriptive information on the CDFI field. It is less valuable in describing the microenterprise field due to the lack of participation of that organization's trade association. However, for the other subsectors, the data set appears to provide a fairly robust and large sample of the activity in each subsector.

The CDP data fields are described in the Appendix. There are a total of 170 fields. (Some of these are combined in the Appendix). The fields do an excellent job of capturing the flow of funds out of the individual CDFIs and their individual stocks of assets. Of course, not all CDFIs report for all fields, and in many cases, a field may be irrelevant or not collected for an entire subsector.

The CDP dataset was not designed for the types of more rigorous product-level impact evaluation that has been the focus of much of the second part of this paper. The chief problems with using this dataset for such a purpose include:

1) CDP is an institution-level dataset, not a transaction-level dataset. Thus, it is difficult to identify the locations or attributes of a particular financial transaction. Thus, attempting

[11] Again, Caskey and Hollister (2001) and Hollister (2004) discuss some of the difficulties of firm level studies of the sort that would be required to measure impact for venture capital firms.

to employ one of the geographic analyses above at the neighborhood level, for example, would be impossible.

2) The disaggregation of different product or customer types is quite limited. Again, the dataset was not designed for many of the more specific purposes described in this paper. Therefore, trying to focus on a particular product line is generally not feasible. For example, housing loans are not disaggregated from business loans, specifically. Total financing outstanding for housing is disaggregated from total financing outstanding for businesses and other customer groups, but it is impossible to distinguish whether this financing is for multifamily rental versus single-family purchase or rehab, for example. The data also do not include flow measures (e.g., loans closed), broken out by customer type. It may be possible to develop some reasonable estimates of loan flows by customer type, however, especially if the number of observations remains large.

3) The CDP dataset does not include data that can be used for establishing counterfactuals. There are no baseline data on customers or geographies that can be used in some sort of pre-post, quasiexperimental research design. Such data would have to come from other sources.

4) There is little-to-no information on the geographic details of CDFI service areas. Theoretically, this might be collected separately for CDFIs that have agreed to reveal their identity and data to CDP partners. However, almost half (246) of the responding CDFIs did not provide their geographic scale of operation (e.g., neighborhood, city or town, county, metro area, state) to the CDP data collecting participants. Most of the CDFIs that do respond to this question operate at fairly large geographic scales. Only 23 report a neighborhood service area, and 21 of these are credit unions. Only 28 report serving a city or a town, and 24 of these are credit unions. At the same time, 83 CDFIs report serving a metropolitan area or multi-county area, and another 29 report serving a single county.

Conclusions and Implications for Foundation and Public Policy

This paper probes the possibilities for new research on impacts among CDFI subsectors and product lines. In particular, it focuses on potential research designs that might be used to measure impact, especially in the context of moving beyond evaluations of single CDFIs. I have attempted to recognize the many formidable challenges involved in measuring impact, either through experimental or quasiexperimental methods. Moreover, I have argued that, in many situations, well-implemented performance measurement, which does not attempt to identify a convincing counterfactual, is preferable to impact evaluation, which is more ambitious and more vulnerable to serious error.

At the same time, recognizing that philanthropic and government funders and investors are under increasing pressure to allocate resources based on evidence of impact, this paper attempts to move beyond the current state of affairs by proposing some potential research designs and methods that may provide us with more evidence than we currently have about the impacts of the

CDFI field. Unfortunately, in some product line areas, there appears to be very few prospects for feasible research in the near-to-mid-term. In some other areas, however, especially single-family housing lending in which spatial, neighborhood-level impacts are a substantial focus, there may be feasible methods for reasonably accurate impact evaluation.

Granted, these techniques are fairly sophisticated and have sizeable data requirements. At the same time, the sorts of data required are available in many places, sometimes at a sizable but usually not insurmountable cost.[12] Moreover, in some places, county governments may provide the necessary data at a much lower cost, particularly to nonprofit or public-sector users.

Some may come away from reading this paper with a significant level of pessimism regarding the ability to demonstrate the impact of CDFI activities. This is understandable. However, the intent is not to promote such discouragement but to provide a generally sober view of the challenges involved. Just as it takes substantial resources to develop the CDFI field, it will take a significant, albeit smaller, investments to develop the data and methods needed to measure impacts of the field. In my view, it makes sense to pursue some of the more feasible methods in the near term, while exploring the possibility for improved data systems and availability for those areas and product lines where such data currently do not exist.

Improved impact research and the application of existing, state-of-the art methods to CDFI programs will require affordable access to more data. Moreover, access to existing types of data should not be taken for granted. Increasingly, local governments are viewing such data as source for generating revenue. Unfortunately, by increasing the cost of obtaining such data, they may be providing a serious barrier to public-interest researchers working on a wide variety of issues. Therefore, access to property and other publicly held data at affordable prices is a key cross-cutting policy issue that affects such research. [13]

In different product-line areas, there are related data issues. For example, in recent years, community reinvestment advocates have been urging bank regulators to collect data from banks on consumer deposit accounts that could be disaggregated spatially and by income. These sorts of data could prove tremendously useful in gauging both outcomes and impacts of CDFIs providing basic financial services. Moreover, continued access to HMDA data that are of sufficient quality for research and the improvement of small business lending data are both federal policy issues that tend to get much less attention than they deserve. Such data, of course, are useful for more than impact research projects. They are important to the regulatory process and are the life-blood of community reinvestment advocacy. But, their utility for research is quite

[12] For example, data on single family property sales, including property attributes, when available from a private vendor, are likely to be priced on an order of magnitude of $0.20-0.40 per record. A study of 20,000 sales in a city or metropolitan area, for example, might then require data costing $4,000-$8,000. Of course, this is just one element of research costs.

[13] As an example of the on-the-ground policy work involved in accessing data, more than nine months after receiving a request for tax assessor property data from a nonprofit researcher, last year Cook County, Illinois, passed an ordinance permitting two-tier pricing of its data, so that nonprofit, public interest users could obtain the data for much lower prices than for-profit purchasers.

sensitive to any reductions in their scope, scale or quality. And as regulated industries gain power, they often seek to weaken the collection and quality of such data.[14]

In considering the potential for research on CDFI impacts, one certainly needs a long-term view. There is a small, but fairly steady stream of innovative research being conducted in the community development arena—some of it described above—that should continue to evolve. More applications of such methods could be applied to the CDFI sector. There is also a potential for more applications of true experiments, including geographically based experiments. Finally, improvements in data, especially in small area data, will hopefully continue to develop. Efforts such as the National Neighborhood Indicators Project and its members and the Urban Markets Initiative project at the Brookings Institution are two examples of research clusters that bode well for the provision of data that could serve impact research well.

[14] The elimination of the requirement for the new "intermediate" small banks in the recent CRA regulatory changes to report small business lending data is a perfect example here.

Appendix

CDFI Data Project Fields

(170 fields total)

Delinquencies for DIFFERENT PERIODS	Venture Capital Fund Y/N
Debt with Equity Closed # and $	Fiscal Year End Date
Equity Closed # and $	For-profit Y/N
Guarantees Closed, # and $	Government Y/N
Loans Closed # and $	Nonprofit Y/N
Loans Purchased # and $	Nonprofit Cooperative Y/N
Financing Committed $	Quasi-Government Y/N
Loan Loss Total $	FTE Total FEMALE/MALE, MINORITY Staff #
Debt with Equity, Total Reported # and $	FTE Total STAFF BY FUNCTION #
Equity Outstanding # and $	FTE Total Volunteer Staff #
Guarantees, Total Reported # and $	FTE Total Paid Americorps/Vista #
Loans Outstanding, # and $	FTE Total Paid Welfare to Work #
Direct Financing Outstanding for Business # and $	Number of community service organizations (CSOs) financed in FY #
Direct Financing Outstanding for Housing # and $	In CSOs financed in FY, number of existing child care slots at time of financing #
Direct Financing Outstanding for Microenterprise # and $	In CSOs financed in FY, number of existing educational slots at time of financing #
Direct Financing Outstanding for Other # and $	In CSOs financed in FY, number of existing health care slots at time of financing #
Direct Financing Outstanding for Personal Development # and $	As a result of financing in FY, number of projected new/expanded child care slots #
Direct Financing Outstanding for Community Facilities # and $	As a result of financing in FY,number of projected new/expanded educational slots #
Direct Financing Outstanding, Total Reported # and $	As a result of financing in FY, number of projected new/expanded health care slots #
Loan Loss Reserve Total $	Number of First Accounts opened at FYE #
Vehicle breakout of Loans Outstanding # and $	Number of Pay Day Loans made in FY #
Equity Total $	Dollar value of all Pay Day Loans made in FY $
Liabilities Total $	Client Services Alternate Pay Day Loan Y/N
Total Assets $	CDFI Offers Bill Payment Y/N
Name of Organization Text	CDFI Offers Check cashing Y/N
Charter number (for banks and credit unions) Text	CDFI Offers Direct deposit Y/N
City Text	Electronic Funds Transfer (EFT) accounts (both ETAs and direct deposit) Y/N
Organization FDIC Certification Number Text	CDFI Offers Money Order Y/N
State Text	CDFI Offers Wire Transfers Y/N
Taxpayer Identification Number Text	Loans to People with no credit history Y/N
Year that CDFI started Financing Activities Yr	Geographic area served by CDFI (State, Multi-state, County Multi-Count etc) Text
Funds Borrowed by CDFI, Total Reported $	Number of deals closed that were mortgages #
CDFI Borrowings, Total Reported $	Housing Units created within FY (Projects developed by CDFI or organizations financed #)
Credit Union Shareholders/Members, Total Reported #	Housing Units renovated in FY (Projects developed by CDFI or organization financed #)
Credit Union Shares, Total Reported $	IDA Holders (People) in FY #
Deposits, Total Reported $	Total dollar value of IDA contributions by IDA holders in FY $
Equity Capital $	Percent of FY Clients located in Major/Minor Urban areas % %
Total Debt Capital to CDFI from DIFFERENT SOURCE TYPES	Percent of FY Clients located in rural areas % %

Equity Equivalent, Total Reported $	Percent of female FY Clients % %
Non-Member Deposits, Total Reported $	Percent of clients served with low to moderate household income % %
Secondary Capital, Total Reported $	Percent of FY Clients identified as a racial/ethnic minority % %
Capital, Total Reported $	Number of businesses (other than microenterprises) financed in FY #
Certified CDFI	Number of microenterprises financed in FY #
Equity Capital CU Net Worth $	Total Reported of businesses and microenterprises financed in FY #
Capital BK Tier One $	Jobs (FTEs) Created by Microenterprise/Business units with financing outstanding to the CDFI in the FY. Negative number indicates Job Loss. #
Capital Weighted Cost Borrowed Funds %	Jobs (FTEs) Maintained by Microenterprise and Business units that closed financing with the CDFI in the FY. #
Capital Weighted Cost Shares %	Jobs (FTEs) assisted by Microenterprise and Business units that closed financing with the CDFI in the FY. #
Capital Weighted Cost EQ2 %	Number of Individuals who received group based training from CDFI #
Capital Weighted Cost CU Non-member Depositor: %	Number of individuals receiving one-on-one technical assistance from CDFI #
Capital Weighted Cost Secondary Capital %	Number of Organizations that received training from CDFI #
Capital Weighted Term EQ2 # Mos	Change in Unrestricted Net Assets
Capital Weighted Term Borrowed Funds # Mos	Confidentiality - Share name with CDP Partners (Y/N)
Capital Weighted Term Secondary Capital # Mos	Confidentiality - Share name with funders/public (Y/N)
Operating Expense Total Reported $	Confidentiality - Do NOT publish in funder/public dataset (Y/N)
Change in Net Income (Change in Net Assets) $	Loans Sold in FY #
Total Contributed Operating Revenue $	Loans Sold in FY $
Earned Revenue Total Reported $	Zipcode
Gains/Losses Total $	Off-balance sheet loans underwritten during FY 2004 $ and #
Operating Revenue After Gains/Losses Total $	Off balance-sheet loans serviced during FY 2004 $ and #
FTE Total Female/Male Board Members #	Org Website
Thrift, Bank, Bank Holding Company Y/N	Contact Person
Credit Union Y/N	Phone number
Loan Fund (including microenterprise loan funds) Y/N	E-mail
	Street Address Memo

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