

Evaluating Borrower Counseling: Review of Recent Studies¹

Overview

Resources for counseling and education targeted to borrowers in the mortgage and other credit markets have expanded dramatically over the last decade and a half. In the early 1990s, less than \$10 million in federal funds were allocated to this purpose, and very few states provided significant funds. By 2005, over \$40 million in federal funds were appropriated for mortgage counseling and education, with millions more provided by financial institutions, states and local governments. The belief that technical information and advice can help consumers make better financial decisions and reduce loan defaults is widely-held, as evidenced by the counseling requirements built into the recent bankruptcy reform law (Bankruptcy Abuse Prevention and Consumer Protection Act of 2005), as well as proposed mortgage lending regulations (H.R. 1295 Ney-Kanjorski). The effectiveness of counseling, however, is largely unproven.

This paper reviews the existing literature evaluating credit and mortgage counseling programs, focusing primarily on three studies completed in this decade. The focus is on studies that use empirical methods to test the causal effect of counseling services, rather than papers that summarize programmatic data or discuss issues in counseling research.

Mortgage and Credit Counseling Programs

Federal support for mortgage counseling began in the early 1970s (Quercia and Wachter, 1996). Initially policymakers created programs as an effort to encourage first-time homeowners to buy homes in specific areas, or to avoid buying homes with severe problems. Counseling was also viewed as a tool to prevent foreclosures, especially in urban neighborhoods. Policy makers' intent was to provide support for government and nonprofit agencies to offer information to borrowers, which would result in fewer costly loan defaults. Services were often tied to government subsidy programs developed by the then newly-established U.S. Department of Housing and Urban Development (HUD). The predominant theory driving these programs was that first-time low-income homeowners face knowledge and financial barriers when buying their first home. Related to job training and anti-poverty policies at the time, as well as policies seeking to mitigate racial segregation, counseling was assumed to help improve work force participation, financial budgeting behavior and consumption choices (Mallach, 2000). Counseling programs were largely implemented at a local level, with a certification granted by HUD for independent nonprofit agencies providing services. Individual sites were encouraged to develop strategies tailored to each community.

In the late 1970s, the Community Reinvestment Act (CRA) created incentives for private lenders to provide credit to low-income and minority households or for properties located in underserved neighborhoods. Private lenders became interested in counseling as a screening device to sort loan applicants by their commitment to repay their loan, as well as a mechanism to generate loan applications, which satisfied regulatory requirements (Mallach, 2000). In 1992,

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changes to the Home Mortgage Disclosure Act (HMDA) and an influential study by the Boston Federal Reserve Bank led to an increased emphasis on expanding mortgage markets to low-income and minority borrowers. As a result, federal support for counseling and education grew dramatically, as did special programs developed by private lenders designed to boost mortgage applications.

Credit counseling programs began in the 1950s, as private sector lenders with portfolios of consumer loans began to support the development of local nonprofit agencies to provide consumer credit counseling. These agencies established debt management plans for borrowers unable to maintain their credit accounts, in exchange for retaining a portion of payments as a fee. The National Foundation for Consumer Credit (NFCC) supported the development of consumer credit counseling service agencies (CCCS) in over 1,000 communities across the United States. These agencies traditionally have focused on consumer debt management, but many provide mortgage delinquency counseling as well. Other than tax-exempt status and a small portion of HUD mortgage counseling funding, these agencies operate predominately on fees. While not a creation of policymakers, per se, credit counseling agencies are increasingly included in state and local efforts to address high foreclosure rates in communities (Hornburg, 2004).

Brief Background on Evaluation Approaches

Given the great interest in the usefulness of education and counseling, there have been several significant attempts to study its actual utility. Researchers have selected from a number of prevailing research methodologies.

The so called “gold standard” in the evaluation of any social policy or program is a randomized experiment. In these studies clients are randomly assigned to a “treatment” group, which receives a service, and a “control” group, which is prevented from receiving a service. Despite preferences for such experiments by many policy analysts, there are problems with random experiments. First, and foremost, it is very difficult to truly randomly assign clients to services while denying others, regardless of need or circumstance. Second, random experiments often are biased due to the randomization process where administrators manipulate the assignment process, artificially enlarge their pool of clients or otherwise alter their intake and service delivery processes. Third, in experiments, highly motivated clients in the control group will find alternative avenues to receive services. This unobservable crossover of control clients to receive services creates substitution bias.

There are several alternatives to random experiments. One close variation is a natural experiment—anything that creates something like random assignment. One example are clients on a waiting list where the only factor used to ration intake is the date the client applied to the program. Another example are requirements for services based on a strict qualification standard, such as income level. Clients very close to being just above and below that income standard are likely to be similar in many ways and useful as a comparison.

Quest for a Valid Comparison

A critical question for any impact evaluation is “compared to what?” There are many factors that can explain any particular outcome. Studies must take great pains to avoid crediting the impact of a program to a particular service when in fact the impact is due to something else entirely. Researchers are searching for a so-called “counterfactual” – what would the exact same client look like if they had not taken part in a program? Of course it is impossible to simultaneously take part in a program and not take part. This is what makes evaluation studies so complicated.

Other approaches do not have any random assignment mechanism. These studies rely on statistical techniques developed in econometrics. These approaches attempt to predict which clients will choose to participate in a program, and control for other factors which might also explain the causal effect of a service. Some approaches, called quasi-experiments, take advantage of matched pairs of clients, track clients over time, or use other methods to approximate something close to an experimental design, except without truly random assignment. Finally, using statistical techniques involving multiple variables, impacts can be extensively modeled. Each factor which may cause an outcome can be estimated, assuming there is a way to measure it. In these models the relative effect of a program or service can often be demonstrated, although concerns about bias usually remain unresolved.

Early Evaluation Studies

The literature on mortgage counseling dates back to the late 1960s, during which time the Federal Housing Administration (FHA) mortgage insurance program struggled to manage its troubled Section 235 program (Quercia and Wachter, 1996).² Initially research focused on the effectiveness of counseling for changing financial behavior, such as budgeting to buy a home. An early study now out of print, but discussed by Strauss and Phillips (1997), involved an experimental design published in 1970. A total of 58 low-income families receiving subsidies to buy a home in San Francisco were randomly assigned to counseling as a treatment group, and 67 served as a control. The study concluded low-income families with counseling experienced fewer delinquent loan payments. The U.S. Department of Housing and Urban Development (HUD) reviewed the study in a 1975 report to Congress, noting that the sample was too small, factors correlated with the outcome were unobserved, and the treatment itself was highly heterogeneous. The study also suffered because it is likely that clients in the control group found similar sources of information to guide their home purchase and borrowing choices (so called “substitution bias”).

Later studies focused on the decision to buy a home. A series of non-experimental studies conducted by HUD in the early 1970s analyzed the impact of counseling on the likelihood that renter families became homeowners. These studies examined questions related to the home purchase transaction, describing how the counseled consumer pursues the process, the

Selection Bias

Any program that includes clients who voluntarily select to participate will likely introduce selection bias.

In mortgage counseling, this takes the form of the most motivated and financially savvy borrowers seeking counseling. Motivation is not a characteristic that can be measured. Therefore when counseled clients are compared to non-counseled clients, the differences could be caused by motivation, not the counseling. Counseled borrowers may simply be more motivated rather than acting as a result of the counseling received.

Experiments using random assignment are the most common technique used to remedy selection bias, although various statistical techniques can be used with non-experimental data. Problems arise when unobservable factors affect behavior—researchers can develop theories and models to approximate such factors, but they are obviously difficult to test.

² The Section 235 Homeownership Assistance Program was enacted in 1968 to provide mortgage insurance and reduce interest rates to as low as 1% for low-income borrowers. Over half a million mortgages were made from 1969 to 1979, 90,000 of which foreclosed.

speed of their progress and the outcome of their decision-making compared to similar populations using the same FHA programs. These studies used crude before and after measures with no matching or randomized control group. None of the findings were conclusive (Mallach, 2000).

A 1974 study funded by HUD and conducted by the Organization for Social and Technical Innovation, Inc. (OSTI) employed a mixed-methods design to evaluate counseling provided to families in the process of buying a home, as well as families already in their home but in default. It used a *before* and *after* estimator, and compared the results at 19 study sites to the average rates of default at non-study sites. The study did not collect socioeconomic covariates and there were problems with assignment—there was no oversight of how clients were referred to programs (Phillips and Strauss, 1997).

HUD conducted two follow-up studies to try to correct the problems in the OSTI study. The follow up studies were the first in the literature to use a matched pair analysis based on socioeconomic factors, and the first to analyze the intent to receive counseling instead of the effect of actually receiving counseling (also called the “intent to treat” versus the effect of treatment on those selected for treatment). HUD’s 1975 and 1977 studies (Counseling for Delinquent Mortgagors and Counseling for Delinquent Mortgagors II) also included a cost-effectiveness calculation from the perspective of HUD’s program costs and expenses. These studies were conducted only in selected cities, however, and the results are not generalized. Also, the type of counseling was heterogeneous but unmeasured, which results in ambiguity over the actual treatment being evaluated. Participation rates among the treated groups were generally low. Thus, the effectiveness of counseling remained unproven (Phillips and Straus, 1997).

HUD published the last of the counseling studies of this era in 1980. Authored by Feins, Bain, and Kirlin of Abt Associates, participation in the pre-purchase borrower counseling program was voluntary and only five percent of eligible purchasers enrolled. One-third of these households received written information, one-third received group counseling, and the remainder received one-on-one counseling. No control group without counseling was established. The study took place over a year and a half period in Philadelphia, Atlanta, and Phoenix, sites that were not representative of counseling programs nationally. Due to the study design and low-participation rate, the results of the study were inconclusive (Shroder, 2000).

Table 1 provides a summary of mortgage counseling evaluations conducted during this period. This series of studies, many of which are highlighted in Greenberg and Shroder’s *Digest of Social Experiments* (2004), are important illustrations for designing and implementing evaluations, in large part due to the failure of these costly studies to provide valuable insights. HUD’s support for mortgage counseling programs continued throughout the 1980s and 1990s, but it no longer funded large-scale program evaluations. Over the 1990s, however, homeownership and credit counseling programs expanded dramatically, many supported not by HUD but by financial institutions and credit providers. By the late 1990s, interest in evaluations re-emerged.

| Table 1: Pre 1990 Evaluations of Counseling | | | | |
|--|---|---|--|---|
| Evaluation | Evaluation Design | Analytical Techniques | Data Used | Finding |
| San Francisco (1970) A Move to Homeownership | Experimental Treatment: various types of counseling, job support & financial assistance | Before and after estimator. Matched pairs, by race, to program participants who were not assigned to training | 104 households; default and tenure tracked after 2 years; racial characteristics. Only one site. | Pre-purchase counseling had no significant effect on default rates But... Small sample, poor matching. |
| OSTI (1974) Study on the Effectiveness of Voluntary Counseling Programs | Mixed-methods (included interviews and focus groups); Nonrandom assignment | Cost effectiveness analysis. Default rates at 6 study sites compared to the average rates at non-study sites. | Tracked mortgage default for 1 year. No socioeconomic variables. No data on reason for referral or treatment provided. | 11% reduction in "mortgage failures." But... Selection bias, poor matching |
| HUD (1975) Counseling for Delinquent Mortgagors I | Randomized referrals to counseling. Estimate intent to treat. | Cost effectiveness analysis. Default rates at 4 sites with available data. | Tracked mortgage default for 1 year. Matched pairs based on socioeconomic data. | 16% reduction in foreclosure rates. But... Small sample; treatment not uniform. |
| HUD (1977) Counseling for Delinquent Mortgagors II | Experimental design Counseling offered to every other borrower on an alphabetical list- 25% of treated actually participated | Cost effectiveness analysis. Assumed single treatment over multiple sites. Default rates at 10 sites in 5 cities. | 1,500 borrowers tracked for 1 year. Included demographic variables, type of counseling and services provided | Huge variation across sites; mixed results. No conclusive findings; but counseling was cost-effective in some sites. |
| Feins, Bain, and Kirlin (1980), Abt Associates Prepurchase Homeownership Counseling Demonstration | Experimental design. Assignment to one of 3 treatments as measure of program intensity | Multiple site, multiple treatments. Default rates in 3 sites | Take up rate for treatment group too low (5%) to generate a large enough sample in 15 month period. | Counseled less likely to buy a home at all. No findings regarding default. |

Despite the long history of consumer credit counseling service programs, there have been no empirical evaluations of the effectiveness of consumer credit counseling until recently. In part this is due to the funding structure of counseling which has been rooted in fees to consumers and lenders, rather than in public subsidies. More recently, regulatory attempts to reduce predatory lending in mortgage markets have mandated counseling for certain borrowers. For example, consumers filing for Chapter 7 bankruptcy after 2005 will be required to first complete credit counseling. Credit counseling is assumed to have either a rehabilitative or preventive role in how consumers manage their borrowing and repayment. Yet even the Federal Reserve Board's staff economists have noted evaluations of credit counseling have "not kept pace" with the growth of providers (Staten, Elliehausen and Lundquist 2003).

A Review of Recent Studies

After a twenty year hiatus, three new studies were conducted in the late 1990s and several more have been proposed.³ Evaluating the impact of counseling remains challenging due to variation in the timing, mode and type of counseling provided. As programs have shifted focus from FHA insurance to private market lending, mortgage lenders rather than government agencies hold most data. Each of these three studies was conducted in partnership with credit providers to monitor borrower behavior over time, rather than a federal agency. None of these studies uses an experimental design.

Hirad and Zorn

Economists working for Freddie Mac, a government-chartered, but stockholder owned corporation which stimulates a secondary market for mortgages, completed the first of this new wave of studies. The authors, Hirad and Zorn (2002), had access to data from Freddie Mac's portfolio of millions of loans. In the early 1990s, the company required counseling for low-income borrowers (earning less than the area median income) in its Affordable Gold loan program, and carefully tracked what form of counseling was provided.

Study Design:

The study was not an experimental design, but rather used the fact that about 3 percent of borrowers in its Affordable Gold loan program were exempted from counseling by virtue of owning a home before or having a larger downpayment. The study sample included 39,318 mortgages purchased by Freddie Mac from 1993 through 1998. Because Freddie Mac has extensive data on origination characteristics and monthly performance, many of the longer-term data problems that plagued past studies were not an issue in this study. The authors identified four different types counseling: individual, classroom, telephone, and self-study. They also identified four types of counseling providers: government agencies, lenders, mortgage insurers and non-profit agencies.

The study set out to answer the following questions: (1) Does counseling prior to loan closing reduce 90-day delinquency rates? (2) Is there variation in the effectiveness of each type of counseling? (3) Is there variation in the effectiveness of each provider of counseling?

³ HUD is expected to issue a call for proposals in FY 2006 for an experimental design to test pre-purchase home counseling and education's effect on loan delinquency and default.

Approach:

This study confronted the problem of unobservable factors using three methods. First, each loan was matched to a loan with similar observable risk characteristics from non-Affordable Gold purchases. This methodology was used for an original version of the study, but was not used for the final version. Although matching was conducted using a generous set of variables, unobservable borrower characteristics remained a concern. Borrowers in the Affordable Gold program often obtained their loan through special circumstances such as customized loan programs or from lenders undertaking intensive outreach. The manner in which borrowers entered the loan program might amplify issues such as motivation and attitude which also will effect loan performance. Another problem stemmed from a lack of enough borrowers to provide a valid comparison (called “common support” in evaluation research).

The authors recognized a second approach was possible—using borrowers in the Affordable Gold program who were exempted from counseling as a comparison group. This too created issues with comparability. Borrowers were exempted either because they had past experience with a mortgage loan or had sufficient savings to make a downpayment larger than the required 5 percent. As a result, the authors tried a third variation.

This third approach also used the exempted borrowers, but tried to assign each borrower to counseling by predicting which program and counseling type they received, instead of using the actual service they reported in the data. The first stage of the assignment model predicted the probability of the borrower receiving each type of counseling from each counseling provider. For example, some borrowers were predicted to be most likely to receive counseling from a nonprofit, as opposed to a lender or other source. Then that borrower would be predicted to have received individual counseling, group counseling, phone counseling or self-study. These predictions, which are based on existing data in Freddie Mac’s database, serve as a proxy for treatment. In theory, this addresses the issue of selection bias.

Using these predictions of counseling, the authors then analyzed if each borrower ever experienced a 90-day delinquency, controlling for variables such as characteristics of the mortgage and property, demographic variables of the borrower, and variables to account for the time period in which the loan was originated. The authors also used risk scores from Freddie Mac’s Loan Prospector database, a system used by lenders to predict the foreclosure probability of mortgages. Given this rich set of data, the effects of selection biased would ideally be mitigated using this assignment approach.

Findings:

The study concluded that the effects of counseling are positive. On average, counseling provided a 19 percent reduction that the homeowner would ever experience a 90-day delinquency on a Freddie Mac mortgage. This overall result remained robust even under the “assignment” approach. Different types of counseling vary in their effectiveness at reducing delinquency rates, however. Clients receiving individual counseling experienced a 34 percent reduction in delinquency. Those receiving classroom counseling resulted in a 26 percent reduction, and self-study had a 21 percent reduction. Telephone counseling did not have a significant effect. Borrowers counseled by nonprofit organizations and lenders also had a lower probability of ever experiencing a 90-day delinquency than borrowers counseled by government or mortgage insurers. The authors note that this finding “primarily appears to reflect the effective

mix of counseling these groups provide.” In other words, lenders and nonprofits tended to use classroom counseling, which proved to have the most powerful effects.

However, the results from the assignment model were less impressive. The model was not particularly robust in estimating which borrowers received counseling.⁴ Predicting what service each borrower received in theory would have controlled for the selection bias in the data—hence the issue of a borrower who seeks out counseling also being a better loan risk regardless of whether they receive counseling is less of a problem. But because the process of modeling which borrower received each service was imprecise, its results were not statistically significant. The assignment model returned significant results only for classroom counseling, and did not find a statistically significant result for individual or home study counseling. Also, there were no significant differences in the effect of counseling between providers using the assignment model. Because the general direction of the assignment model supported the findings of the simpler comparison of exempted and counseled borrowers, the authors concluded their results remain valid, however.

In many ways this is a groundbreaking study. It overcomes the data issues, which hampered past studies. While it does not use an experimental design, which is the ‘gold standard’ of program evaluation, it does exploit access to Freddie Mac’s portfolio of loans to create a plausible quasi-experimental design.

Issues and Constraints:

There are several important issues regarding this study which are often overlooked by proponents of counseling. First, the issue of selection bias could be a problem. The fact that the assignment model did not produce significant results may be an artifact of the data and the complicated systems used to estimate the probability of borrowers receiving counseling. It is encouraging that the findings supported the general direction of the non-assignment model, but this still suggests borrower motivation and self-screening effects that are highly correlated with counseling.

Second, this study is specific to pre-purchase counseling and loans originated 1993-1998. During this period counseling methods were still developing, particularly telephone counseling which has matured dramatically in the last ten years. This study’s sample is large enough that the authors could have explored more interactive effects, particularly between year and counseling type. In the early 1990s, loans with low-downpayments were not frequently originated; by the late 1990s loans with zero downpayments became common. Today’s first-time, low-income mortgage borrower may not be similar enough to borrowers in this study to conclude counseling would have the same effect today.

A third complication is related to borrowers in the Affordable Gold program receiving counseling as well as other special services not recorded in the loan documents. For example, many programs couple a private sector first mortgage with a second mortgage originated by the nonprofit with a below market interest rate. Likewise, banks and local governments occasionally

⁴ An alternative analytical model regarding the assignment problem would have been a regression discontinuity design using the key factor that resulted in Affordable Gold borrowers being exempted from counseling—downpayment level. The distribution of borrower downpayments around the 5% threshold for being exempted from counseling permits comparing borrowers making 5.01% downpayments to those making 4.99% downpayments, for example. Assuming borrowers did not alter their downpayment amount specifically to avoid counseling, comparing borrowers on either side of the distribution could provide another approach for dealing with unobservable factors.

will provide downpayment subsidies. These complementary services might also result in reduced probabilities of ever being 90-days delinquent, and would tend to bias the results upward.

Another complication with this study, like all studies of credit and borrower counseling, are substitution effects. Borrowers not identified as receiving counseling may have found alternative means of gathering information, whether from associates, industry professionals or self-study (there are over 500 books on “how to buy a home” at Amazon.com). The fact that this study finds statistically significant effects only reinforces the strength of the effect of counseling, however. If borrowers not labeled as receiving counseling did receive some weaker form of counseling, then the effects of formal counseling must be that much larger to be statistically significant.

Hirad and Zorn also did not report the effects of counseling by borrower demographic group or loan characteristics. A tabulation of these effects could provide a sense of the relative impact of counseling for borrowers in the current market. These results would be helpful in determining which borrowers are most likely to benefit from counseling, as well as those borrowers who do not need counseling to be successful.

A final issue with this study is the outcome measure of a borrower having experienced a 90-day delinquency at any one point while the loan was in Freddie Mac’s database. Loans made at the beginning of the study period have years of opportunities to trigger this criteria. Loans made in the later years have a shorter period. The authors included variables for years, but the coefficients of these variables are not provided in their paper. Moreover, some loans may become 90-days delinquent more than once. A more precise measure of loan defaults would be preferable, potentially including the probability of actual foreclosure and severity of loan losses from counseled and non-counseled loans.

Staten, Elliehausen and Lundquist

In 2003 Staten, Elliehausen and Lundquist released a report evaluating credit counseling services. This study estimated the effectiveness of credit counseling on the subsequent credit usage and payment behavior of clients. The authors carefully screened out clients who also received debt management plans, thus measuring the impact of counseling in isolation. The study’s driving question was how the counseled group’s credit performance improved in a three year period following an initial counseling session, relative to a non-counseled comparison group.

Study Design:

This study used a quasi-experimental design. The counseled group included all clients for whom an initial counseling session was conducted during a five-month period of April to August 1997 at one of five sites. The sites were Atlanta, suburban Detroit, San Francisco, Phoenix and Dallas, although many of these programs serve clients throughout their state or region. About 80 percent of counseled clients received in-person counseling, while the remainder received telephone counseling. Unfortunately while data on the form of counseling each client received is available in aggregate, it was not recorded in each client’s record. Clients were excluded from the study if they set up a debt management plan after their counseling session, or they did not have matching credit reports in June 1997 and June 2000 with a credit score for both years.

The comparison group was created from a random sample of records provided by a major credit bureau. These records were matched by geographic location (3-digit zip code) and credit score to the counseled group. Like the treatment group, clients in the control group lacking credit scores for both 1997 and 2000 were dropped from the analysis. Due to confidentiality concerns, the comparison group's data was scrubbed for any identifying information prior to release from the credit bureau. In substitute for the censored data the authors obtained Census bureau data on the mean tract income, home value, race and other characteristics as proxies for individual data. The authors confirmed the control group members did not receive counseling from one of the study sites in 1997. But they are unable to confirm whether control group clients received counseling at some other time. They suggest this could only result in a negative bias to the results, however, reinforcing any positive findings.

Approach:

The final sample included 11,487 borrowers (5,973 counseled and 5,514 in the control group). Borrowers with very high or very low debt levels were dropped from both control and treatment groups, based on the assumption these were outliers. The authors analyze changes in different credit behaviors, measured through nine separate variables over a three year period. The authors recognize counseling was a choice rather than randomly assigned, and that choice may be a signal of borrower motivation to change behavior. In order to address this selection bias, instead of using a variable for counseling, the authors first use a model to predict the receipt of counseling based on the client's level of credit use, debt repayment history, and transaction costs (as measured by proximity to credit counseling offices). This model correctly assigned two-thirds of clients to counseled or comparison groups, which the authors viewed as reasonably accurate.

Because the authors hypothesized that borrowers with lower initial credit scores would benefit most from counseling, the model includes an interaction between receiving counseling and credit score. Because credit records were collected in June 1997, they expected clients who came in for counseling during April and May to have experienced credit problems which would be reflected in their reports. If clients receiving counseling in June, July and August were motivated to attend counseling due to an immediate credit crisis, that would not be reflected in the records. Thus, the authors includes controls in their analysis for the time the client entered into counseling, expecting the April-May initial credit reports to be lower and June-August reports to be higher. This way the authors could account for clients with depressed credit measures at the time of the June report.

Study Findings:

Overall the study found that holding other factors constant, credit counseling had a significant and positive impact on summary measures of borrower creditworthiness. Table 2 shows some of the results. Overall, three years after counseling credit scores went up for the counseled group, although not as much as for the un-counseled comparison group. However, scores did go up substantially more for the counseled group with low initial credit scores. More impressively, counseled clients reduced their debt by over \$6,700 on average, while the comparison group *increased* its debt by over \$8,800. Counseled clients reduced the number of credit cards they owned and demonstrated a reduction in 60 day delinquencies, both of which fared better for counseled clients than the comparison group. The effect of each of these factors tended to be greatest for clients who had lower credit scores at the outset.

Table 2

| Three-year changes | Counseled | Comparison |
|--|------------------|-------------------|
| Credit score | 11 | 25 |
| Total debt | -\$6,752 | \$8,844 |
| Non-mortgage debt | -\$8,813 | \$171 |
| Number of active bank cards | -1.19 | -0.13 |
| Revolving debt | -\$7,378 | -\$1,045 |
| Percent with any 60+ day delinquency in last 12 months | -0.94 | -0.76 |

Issues and Constraints:

This is a well-designed study using a unique data source. The authors employ appropriate selection models and a large number of variables to measure credit behavior. Unfortunately this study is largely a “black-box” approach. Before and after credit characteristics are explained only by the *receipt* of counseling. Counseling type, duration and delivery mechanisms for each counseled client is unknown. Only two points in time are measured with a three-year intervening period. Each client’s credit behavior in that period is unknown.

Another problem with this analysis is the comparison group is artificially constructed using credit bureau files. Ideally, this analysis could be performed with clients who are similar but do not receive credit counseling, either through random assignment or by using program drop-outs or other groups from home counseling agencies have data. Even if credit bureau data could be shown to be a valid comparison group, the restrictions on disclosing information about the borrower’s income and other characteristics are a major limitation. The use of census data as a proxy for these data is reasonable, but obviously much of the variation in client characteristics is lost by relying on neighborhood averages.

Finally, because the data lack information on individual statistics, the dimensions of impact cannot be analyzed by income or other characteristics. Ideally the impact of counseling could be estimated for each demographic group. Counseling may have a strong impact on younger clients, or clients with specific educational backgrounds. Analysis of these data would help better target services to those families who could most benefit from credit counseling.

Hartarska and Gonzalez-Vega

The most recently published study on counseling is by Hartarska and Gonzalez-Vega (2005). The authors studied the Community Mortgage Loan Program operated by a large Midwestern bank from 1992 to 2000. Initially, all low-income borrowers were referred for counseling, but due to differential access to services, the receipt of counseling varied by participant. According to anecdotal accounts by the bank involved in the program, counseling followed a fairly random pattern. In 1996, however, counseling became mandatory. The amount of counseling was determined on a case-by- case basis by a consumer credit counseling agency. Depending on the client, counseling could last up to two years. Clients also received financial assistance in many cases. The pre-1996 sample includes 919 borrowing households, 509 of

whom did not receive counseling. Another 387 borrowers in the program from 1996 to 2000 by definition all received counseling.

Study Design:

The authors use a “competing risks” approach. They assert from a lender’s perspective, default is one important measure of loan performance, the other being early loan re-payment. Using an options model, the authors estimate when borrowers would optimally default on or prepay their mortgage, using a framework developed in finance literature. Based on the type of trigger event a borrower experiences, he or she will place a value on the put and call options of default (walking away) or prepayment (refinancing the loan or selling the home outright). The authors separately predict the value of the prepayment and default options for each borrower. The authors determine the pre-1996 sample is more useful than the combined sample, in large part due to spurious comparison of one control group of borrowers from 1993-1996 to counseled borrowers from 1996-2000.

Findings:

Overall, the study suggests counseled borrowers are more likely to prepay their loans, but the results are insignificant for reducing default. Using the full sample (including the spurious comparisons across time periods) prepayment probabilities are increased, and default probabilities are decreased. The paper finds limited evidence of the effect of counseling. The authors conclude counseling decreases defaults but makes default more costly for lenders. Meanwhile counseling increases borrower prepayments, but reduced the cost of prepayment to the lender. They conclude overall that counseling will affect lender profits and suggest lenders consider both prepayment and default as factors when deciding to require counseling.

Issues and Constraints:

The introduction of the competing risks model is a genuine contribution to the counseling evaluation literature. Prepayment is clearly a borrower behavior of interest, not only for lenders, but also for those concerned with how consumers make choices, which result in long-term financial security. There are many problems with this paper, however. The data itself are not representative of borrowers or counseling programs. Counseling is not offered in isolation, yet other services received are unobserved. The sample itself is small and highly varied. The treatment received by each household is unique, and again unobserved. There are also issues of selection bias; the evidence that counseling was truly randomly received is largely based on conjecture.

Lessons Learned and Suggestions for Improvements

Evaluations of the effect of credit and mortgage counseling on consumer behavior are an interesting case study of how evaluations evolve over time. Initially, federally-funded counseling programs included evaluation components and were analyzed with the goal of guiding HUD and FHA policies. While counseling flourished in the 1990s, motivated in large part due to federal regulations levied upon the financial sector, evaluation was rare. Counseling became more varied in delivery and approach, and data on outcomes of interest were entirely proprietary. In many ways evaluating counseling in 2005 is more difficult than in 1975. Yet, recent evaluations have proven far more successful, although are few in number.

Two standard problems in evaluations remain: selection bias and substitution bias. Added to these problems is the need for large samples with detailed information. Because default is a rare event (less than 3 percent even among low-income, first-time borrowers), samples of 10,000 or more are needed (Wachter and Quercia, 1998). The timing of studies is also important, a lesson learned in the 1970's when interest rates were high. Studies also need sufficient follow-up to track performance consistently for both control and treatment groups over a multi-year period. Counseling providers generally have poor data tracking capacity and lack the ability to track former clients.

The ideal counseling evaluation would have an experimental design, be prospective, and use random selection and random assignment. Proportional hazard modeling would be used to account for prepayment and default. Income, assets, household characteristics, previous experiences with homeownership, employment status and history, and credit records would all serve as controlling factors. Random assignment is difficult, however. Most counseling providers are not willing to deny clients services. There are feasible mechanisms to force loan applicants to complete counseling, but exceptions would likely remain. As a result a selection model would need to be used, based on an extensive array of borrower and loan characteristics.

Assessment of Counseling Based on Studies to Date

Table 3 provides a summary of the three most recent studies discussed. The theory of credit and mortgage counseling is that technical information and advice helps consumers make better financial decisions. While early studies provided some support for this theory, the results were hampered by poor evaluation designs and data problems. Two studies conducted in the 2000s, one by Hiram and Zorn, the other by Staten, Elliehausen and Lundquist, are carefully conducted using unique datasets. While neither is a true experiment, each attempts to control for selection bias. In both cases the impact of counseling is positive.

Unfortunately, the term “counseling” covers a highly diverse set of services. The Hiram and Zorn study suggests the type and provider of counseling matter. This suggests counseling ought to be more optimally prescribed for borrowers. An update of this study could be highly valuable, particularly focusing on interactions between the type of counseling and borrower demographics. Given advances in telephone counseling, a new study might find this method is equally, or even more, effective as individual or classroom counseling provided effective triage is implemented.

Staten et al provide another example of an excellent data set, which is carefully used to adjust for selection bias. By focusing on credit counseling in isolation, this study provides a valid measure of “pure” counseling. Again, a revised version of this paper could produce insights.

Ideally credit report data would be collected at regular intervals to create a multi-period panel. This would allow measurement of marginal changes over the course of the study. More variables in the control group also would produce more robust modeling. Finally, tracking the type and duration of counseling would help to distill the relative effects of various counseling strategies.

Even in their current form both of these studies suggest counseling has a positive effect on borrower behavior. The third study by Hartarska and Claudio Gonzalez-Vega also suggests borrower behavior is impacted by counseling, although the implications of this paper are that counseling may be a poor business proposition for lenders. The data used in this study is more suspect however. More research is needed before accepting their conclusions.

Table 3: Evaluations of Counseling 2000-2005

| Evaluation | Evaluation Design | Analytical Techniques | Data Used | Finding |
|--|--|--|---|--|
| Hirad and Zorn (2002) | Quasi Experiment Compare counseled to exempted borrowers | Nested logit assignment | 39,000 mortgages from 1993-2000 nationally. Follow up period varies. | 19% reduction in 90 delinquencies; method of delivery matters: Individual counseling: 34% classroom counseling: 26% home study: 21% Phone counseling had no effect. (results not adjusted for endogeneity) But...cannot generalize to all borrowers. Selection model lacked precision. |
| Staten, Elliehausen and Lundquist (2003) | Quasi Experiment Compare to randomly drawn individuals matching criteria. | Logit assignment | Borrower credit report, census tract, income and race characteristics as proxy for individual data suppressed for privacy. 5 sites for 3 year follow up period. | Counseling increased credit worthiness reduced delinquencies and reduced total debt utilization. But...mode of delivery not measured. Two time period black box approach. |
| Hartarska and Gonzalez-Vega (2005) | Quasi Experiment Access to counseling random among participants (participants also received financial and other services) | Options-based theory of mortgage termination: prepayment and default as competing risks. | 919 loans from 1992-1996 with 'random' counseling. 387 post 1996 with required counseling. Follow up period varies. | Counseling lowers default but increases loan prepayments But... Small sample; cannot be generalized. Selection bias. |

Defining Outcomes

There are many outcomes associated with homebuyer counseling programs. A lot of anecdotal evidence of counseling and education points to success. Stories of a family realizing homeownership is actually within reach, or a buyer saved from signing a deal they learned was a potential mistake are common. Practitioners and policymakers believe counseled individuals who buy homes are more likely to maintain their home well, keep up with their mortgage payments and become engaged in the neighborhood. But documenting these impacts, and then showing the causality of a nonprofit counseling program for making them happen, is challenging.

Table 4 (see page 17) illustrates a typology of questions in four categories, as well as potential measures for each that might be used in a research design to support or refute these impressions. The first set of questions is related to the effectiveness of counseling and education to actually changing consumer knowledge levels. This is associated with the consumer learning specific ideas and concepts, as well as skills. This may also be related to changing financial behavior, such as savings and credit management.

The second set of questions is related to the home purchase transaction itself. This describes how the educated consumer pursues the process, the speed of their progress and the quality of their decision-making. This includes financial and non-financial measures.

The third set of questions is related to how an educated consumer behaves over time in regards to their home maintenance and community involvement. In addition to managing routine issues of homeownership, this category also includes how homebuyers manage extraordinary events, such as a job loss, divorce, or a health care or other crisis.

The last set of questions is related to long-term financial behavior and decision-making. This is not just the performance of the loan, but also refinance and other borrowing choices, as well as decisions related to selling the home and purchasing another (trade-up) home. It is unlikely many these impact measures can become regularly collected metrics. But periodic evaluation studies can examine one or more aspects of these impacts.

Conclusion and Recommendations

More answers are required regarding which types of counseling and providers are most effective for various types of borrowers. But it is clear the information and advice provided through credit and mortgage counseling do indeed have an impact and are worthy of policy makers' attention. As funders, practitioners and financial regulators consider the impact of counseling, the following issues remain important:

- Although the findings are plagued by an esoteric data source, Hartarska and Claudio Gonzalez-Vega's suggestion of using a competing risks model is significant. Borrower default and prepayment are both worthy of analysis.
- Other outcomes besides loan performance are also worthy of analysis, including the borrower's choice of loan product and home, as well as their overall satisfaction with their purchase.
- Finding a suitable comparison group is difficult. Selection bias will always hamper non-experimental data. A large scale study using truly random assignment could provide valuable insights.
- Experimental data will have drawbacks, including its effects on program administration, ethical concerns regarding service delivery and the costs of designing the research.
- Counseling is highly heterogeneous. Studies should consider more precisely the marginal impacts of an additional hour of counseling, as well as standardized methods of delivery and content. Like all education, the effects will likely vary by type of client, location and environment.
- Longer-term data may be needed to analyze how borrowers manage events over the course of their loan. Counseled borrowers may demonstrate positive behaviors for subsequent loans and home purchases, as well, which are generally not captured in existing studies.

Much of economic theory, and current public policy, is based on the idea that informed consumers make rational decisions. Counseling holds great promise to help consumers make better financial decisions, especially regarding borrowing and credit. But more refined, longer-term studies are needed to best determine the best forms of counseling, and for whom that counseling should be targeted. The public and private sector, as well as consumers themselves, all have a vested interest in the results.

Table 4: Categories of Questions and Potential Measures

| Questions | Measure |
|--|---|
| 1 Consumer Knowledge | |
| Do consumers know more if they get counseling? | Pre- and post tests with control group; structured qualitative interviews |
| Does counseling help clean up credit problems? | Pre- and post credit scores with control group |
| Do consumers' intentions to buy, what they plan to buy and how they plan to finance their purchase, change? | Pre- and post attitude/perception surveys with control group; structured qualitative interviews |
| 2. Home Purchase Transaction | |
| Do educated consumers contribute to speedier transactions that reduce costs for lenders, real estate brokers, etc? | Measure overall purchase cycle time for counseled and control group. |
| Does it facilitate shopping around? | Number of loan applications and range of offers received |
| Do borrowers get a better deal/price/terms if they have counseling? | Mortgage contract records |
| Do consumers save money and time during the transaction? | Expenditure surveys for counseled and control group |
| Do some consumers decide not to buy? Do the right buyers buy? | Pre- and post attitude/perception surveys with control group; structured qualitative interviews |
| 3. Maintenance and Crisis Management | |
| Do educated consumers spend more time and money on maintenance? | Expenditure and time surveys for counseled and control group |
| Do educated consumers have higher levels of community involvement? | Expenditure surveys for counseled and control group |
| Are consumers with counseling better prepared for trigger events? | Surveys for counseled and control group |
| 4. Long Term Financial Behavior | |
| Do educated borrowers watch interest rates more regularly than non-counseled buyers? | Surveys for counseled and control group |
| Are educated buyers more likely to refinance and/or borrow against home equity optimally? | Surveys for counseled and control group |
| Do educated buyers make better decisions related to selling the home and purchasing another (trade-up) home? | Surveys for counseled and control group |
| Is the delinquency rate for counseled buyers better than non-counseled buyers? | Loan performance records for counseled and control group. |
| Are default losses better for counseled buyers than non-counseled buyers? | Loan performance records for counseled and control group. |

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