ENTERPRISE AND COOPERATIVE
DEVELOPMENT DEPARTMENT
— SOCIAL FINANCE UNIT —

# *The use of control groups in impact assessments for microfinance*

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# Foreword

The Microcredit Summit Campaign aims at reaching 100 millions of the world's poorest with credit by the year 2005. As microcredit is increasingly claimed to be a powerful tool for alleviating poverty, it is naturally interesting to find out whether it actually has this effect. While there is substantial first-hand evidence of the empowering effect of microfinance on the poor, the effects, ways and causes are less known — a call for more thorough impact assessments in microfinance. For the ILO in particular, impact assessments are essentially intertwined with its objective of social justice, because they help understand how some people can be better off than others, as a result of the same intervention.

This paper by Paul Mosley was prepared as a contribution of the ILO to the CGAP working group on Microfinance Impact Assessment Methodologies. CGAP members decided in April 1996 to establish a working group on this issue, recognizing that assessments of the impact of microfinance services are complex and entail several methodological challenges. A virtual meeting was held April 7-19th 1997, reviewing several background papers[1] and a synthesis report by David Hulme[2].

Paul Mosley is professor at the University of Reading, and the head of its International Development Centre. He recently published with David Hulme *Finance Against Poverty* (London: Routledge, 1996) that sparked off a lively discussion among microfinance specialists on the trade-off between financial sustainability of microfinance institutions (MFIs) and outreach to the poor.

As Paul Mosley states in his introduction, the evaluation of any project or activity, including microfinance, aims ideally at knowing the effects and who is affected, but also the causes. Social sciences have developed a number of methodological approaches that attempt to simulate the situation which would have prevailed if there had been no project.

One of them is the control group method, which compares a population that had benefitted from a microcredit scheme to another group which had not. While being increasingly used in microfinance impact assessments, not least due to its limited data requirements, this method is beset by a number of methodological challenges — three of which are dealt with in more detail in this paper, i.e. *sample selection bias*, *misspecification of causal relationships* and *motivational problems*.

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1 Feinstein, Osvaldo (1997): A Transaction Costs Approach for Microfinance Project Assessment; Little, Peter (1997): Assessing the Impact of Microfinance Programs on Incomes and Assets; Mayoux, Linda (1997): Impact Assessment and Women's Empowerment in Micro-Finance Programmes: Issues for a Participatory action and Learning Approach; Beuringen, Coven (1997): Outline for Impact Assessment in the Credit Line; Wiig, Anne (1997): Credit Expansion in Microcredit Programmes: Dilemmas and Feasible Methods for Studying Them; Chao-Beroff, Renee (1997): Impact Assessment Seen From an NGO Practitioner's Scope.

2 Hulme, David (1997): Impact Assessment Methodologies for Microfinance: A Review.

The preparation of this manuscript by Paul Mosley was supervised by Haje Schütte of the Social Finance Unit of the International Labour Office.

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# 1. Why use control groups in impact assessments of microfinance projects?

The evaluation of any project or activity, including microfinance, aims ideally at knowing not only what happened to whom in the process of that activity, but also why it happened. The scientist's approach to assessing cause and effect is generally experimental: the application of a particular stimulus to a particular substance in a controlled environment which eliminates extraneous influences.

This experimental approach, because of the nature of the subject matter, finds limited application in the social sciences. Although the experimental method is increasingly being used to deal with problems such as choice under uncertainty, it is impossible to conduct experiments which recreate the imaginary situation which would have prevailed if, say, the Grameen Bank had never existed. Without the possibility to make and unmake projects, it is not possible to assess the impact of such projects by experimental means.

Confronted with this basic difficulty, evaluators have resorted to a range of methods for impact assessment, some of them specious and some less so:

- *before versus after comparisons* demonstrating "progress" or the lack of it in the time trend of specified indicators; this method is flawed by the impossibility to separate project and non-project influences. A microfinance project may claim to have achieved poverty reduction, which may be actually the result of many other factors, price fluctuations, changes in government policy, improved infrastructure, or simply better weather. To put it differently, a project in which the target group's income declines may still be a success if, without it, the outcome would have been worse;

- *comparisons of plan versus realization*; the validity of this method depends on how meaningful the planned target is; setting the target in a meaningful way presupposes substantial prior information about the extent and nature of poverty and what microfinance can at best do; most often, target setting is relatively arbitrary; a target such as "moving 10,000 people across the poverty line this year" may be over- or under-fulfilled; this can be a reflection of the project's performance, but also of the significance of the target; in the absence of supplementary analysis, this is difficult to tell;

- *beneficiary and staff appreciation: subjective judgments by individuals involved in or affected by the project:* target group individuals may have an axe to grind; similarly project staff want the project to be seen as a success and their professional competence confirmed;

C closest to the experimental methods of the natural sciences are *quasi-experiments* (Casley and Lury,1982), i.e. operations which attempt to simulate the situation which would have prevailed if there had been no project;

C one of these is *multiple regression analysis*: the assessment of the changes in a specified dependent variable (say poverty) produced by changes in a specified independent variable (say a microfinance project) holding other specified influences (say weather, input availability, output prices, political conditions….) constant;

C another quasi-experimental approach, less intensive in its data requirements and already widely used is the *control-group method*. A population which benefitted from, say, microcredit is compared to a population which did not. This method calls for a baseline survey as well as ex-post assessments.

***Figure 1: The control-group approach to impact assessment***

| | ***BEFORE*** | Treatment (i.e. project) | ***AFTER*** |
|---|---|---|---|
| Treatment group (e.g. borrowers) | Target variables | ------> | Target variables |
| Control group (e.g. non-borrowers) | Target variables | ------> | Target variables |

This control-group approach is used, for example, to assess the effects of specified fertilizer treatments on a particular crop, or of specified drugs on a particular type of patient. Likewise it can be used in microfinance to show an "impact" — i.e. the difference in target variables between borrowers and non-borrowers. It can further be used, subject to the same provisos, to determine the impact of alternative lending techniques, for example a sequence of loans, or loans given with training and marketing advice compared to a minimalist (i.e. loans only) approach.

The drawback of the control-group approach is that, unlike regression analysis, it cannot tell us the quantitative impact of project - in relation to non-project influences; on the other hand, it is free of biases associated with regression analysis in those cases where the standard assumptions of the normal linear regression model (normally distributed disturbances, constant variance of the error term, etc.) do not hold.

The objective of this paper is to define ways of applying the control-group methodology in micro-finance which take us as close as possible to the ideal of estimating the pure impact of the project, with all non-project influences distilled out of the assessment; alternatively put, we are trying to find ways of steering clear of the elephant-traps which most easily befall the user of the control-group technique, which we describe as *sample selection bias, misspecification of causal relationships* and *motivational problems.* We shall try to determine the costs and benefits that occur if one avoids these errors. In framing recommendations, we shall attempt to keep the costs and benefits of different variations on the control-group theme clearly in mind.

# 2. Biases of the control-group method and how to avoid them

A range of difficulties confront the practical implementation of impact evaluations based on control groups. In decreasing order of complexity these are:

- *Sample selection bias:* the control sample may turn out not to be completely comparable, i.e. it fails to satisfy the criterion of "being like the treatment (credit recipient) group in all respects save that of not being a credit recipient".

- *Misspecification of underlying relationships:* simple trend comparisons of target group and control group may obscure the causal processes at work; for example there may be hidden relationships between the selected independent and dependent variable, or externalities, or variations over time in the structure of response of the target group to the treatment ("structural breaks").

- *Motivational problems:* the target group and, especially, the control group may either refuse to reply or sabotage the impact assessment by giving intentionally false replies.

## 2.1 Sample selection bias

The essential problem here is simply the risk that the control group, or more precisely the comparison between it and the target group, may be contaminated by factors which inhibit the control group from effectively simulating the without-project situation. There are several factors in play here, namely:

i) If the target group (i.e. borrowers) have a tendency to possess an attribute which is not usually controlled for (such as "entrepreneurial ability", or even the ability to remember) then the comparison between target and control group will be biased, since it will ascribe to the project (provision of microfinance) achievements which are in fact due to pre-existing attributes of the treatment group. The case is similar to that of an agronomic experiment which reports a favorable pay-off to using fertilizer when the higher productivity of the fertilized plot was in fact due to the higher productivity of its soil.

ii) Borrowers, by virtue of having the priviledge of being targeted by a microfinance project, may develop a sanguine attitude which subjects their estimates of change in income and assets to an upward bias. Here, the analogy is with the 'Hawthorne effects' (Roethlisberger and Dickson), in which factory workers who knew themselves to be the subject of an experiment exhibited systematically higher productivity than otherwise identical workers who were not singled out in this way.

iii) Microfinance project benefits are fungible, hence a loan aimed at a particular target group or activity may not be used by that group or for that activity. Relevant examples are the demonstration by Goetz and Gupta (1995) that the proceeds of some loans in rural Bangladesh awarded to women are in fact productively used by men; another example (in Hulme and Mosley 1995: Chapter 16) is that fertilizer intended for use on the plots of members of the Malawi Smallholder Agricultural Credit Administration farmers' clubs was sold by them to estates. Here it is the *target group,* and the comparison between it and the control group, which is contaminated, not the control group proper.

iv) Control groups may become contaminated through contact with the target group: borrowers may have passed to members of the control group seeds and fertilizers bought with the proceeds of a loan. This influence increases over time, and is particularly serious in the case of what are sometimes called "downstream evaluations", i.e. evaluations carried out some time after the end of project disbursement.

v) Most obviously, bias may arise because of aspects of the location of the target group which are not controlled for, such as cost of inputs supplied to borrowers and demand for the goods and services which they provide.

To avoid this sample selection bias it is useful to select the control group more carefully so as to hold constant factors which may vary between the target and control groups. For example, it should be possible to eliminate most locational biases (v) by holding access to infrastructure, constant across the target and control groups, which is a key determinant of user costs of inputs and producer prices of outputs. By the same token, it ought to be possible also to eliminate bias through contact between target and control group (iv) by locating the control group far enough away from the target group. Naturally, this will make the survey more expensive.

Biases (i) and (ii) (Hawthorne effects and propensity to be entrepreneurial) are possible most easily countervailed by using *accepted borrowers-to-be, who have not yet received a loan,* as the control group (Hulme and Mosley 1996, chapter 4). Accepted borrowers who have not yet borrowed are presumably just as entrepreneurial, and feel just as much a sense of belonging to the microcredit experiment as those who already are using loans. Possible differences between control and target group in the subjective ability to recall can be compensated for by using internal cross-checks, e.g. income can be cross-checked against expenditure and assets to identify several cases of inaccuracy due to poor recall.

This leaves the hardest nut to crack, loan fungibility issue (iii). Gaile and Foster (1996:24) write that 'no study has successfully controlled for the fungibility of resources between the household and the assisted enterprise'. However, this does not mean that it is impossible to insure against this bias. The most promising approach would once again appear to be that of *using case-study material to cross-check the formal loan use recorded by the lender.* Supposing, for example, that a loan was recorded by a microfinance institution as having been for the purpose of acquiring draught oxen, or a water pump, or a sewing machine; it should be possible then to check in a random sample of (say) 10% of borrowers whether there was any evidence of the asset in question having been acquired and retained. This has usefully been done in the evaluations of IRDP in India surveyed by Pulley(1989). The retention rate emerging from such a sub-survey can then be grossed up to derive an estimate of “leakage” of loan money into non-productive activities.

However, the significance of any such estimate should not be overestimated. Donors have long sought to prevent “leakage into consumption” of aid money destined for “productive investment”, but several micro-level studies of households reveal that a great deal of such investment is unproductive, and a great deal of consumption productive, not only when spent on human capital items such as school fees but also when spent on “true” consumption items such as food.

This is particularly relevant to microfinance projects, where sequences of the type small savings deposit > emergency cash reserve > decrease in risk aversion > willingness to borrow for working capital > small loan > increase in income and productivity > willingness to borrow for human capital and even technical progress, are common. It is often a “leakage” into consumption of a loan intended for investment which starts off a sequence of this type. Again, this is something which can be revealed by diachronic case studies of individuals, and not by classical impact evaluation designs using control groups of the type summarized in Figure 1.

## 2.2 Misspecification of underlying causal relationships

The standard methodology of impact assessment portrays a project intervention as a *one-way, homogeneous process*: i.e. the project exerts a steady influence, hopefully beneficial, on a target variable such as repayment, income or poverty. In practice, it may be more complicated. The causation may run from impact to project effort as well as the other way around: for example, perceived poor performance as seen by project staff may demotivate them, thus reducing project inputs in the following period; or, to take a more complex chain of cause and effect, efforts by project staff to improve repayment performance may in the short term succeed, but induce the borrower to sell assets (machinery, trees or land) which reduce the borrower’s capacity to repay in the subsequent period.

The simultaneous interaction between project input and target output, as well as the dynamic process by which a microfinance intervention may aggravate rather than reduce poverty, are concealed by the simple control-group procedure portrayed in Figure 1. This is especially the case, if the “after-project” measurements are taken early in a project’s life, and if borrowers are not replaced, who drop out during the survey period because of pressures to ensure repayment.

The major disadvantage of the control-group method in relation to econometrics is its simplicity or crudeness: it contains no other assumption than inputs > outputs > impact, and in particular it does not allow for a lag-structure linking the independent variables, or causes, to the dependent variables, or effects.

The antidotes to these shortcomings are relatively simple and inexpensive: replace dropouts, avoid judgments on impact until programmes are “mature” and monitor interim impact regularly so that not only is there a regular feedback to management, but also information on whether the underlying relationship between microcredit and the various performance measures listed in Table 2 below is a simple one way relationship (credit> performance) or more complex. A slightly more expensive antidote is the use of retrospective depth interviews which will expose processes of interaction over time and between target variables, such as those described in the previous paragraph, which a simple with-versus-without-project comparison will conceal.

However, none of these techniques will deal with the problem of simultaneous causation between project inputs and project impact. The antidote for this problem, two-stage least squares, requires the construction of a model of simultaneous cause and effect between project inputs and project impact, specification of a group of “instrumental variables” correlated with the dependent variable, and collection of data on all of the variables listed in Table 2. The “instrumental variables” should be causally related to the right-hand side dependent variable and serve as proxies for it in the regression. An example to illustrate this point: the standard of infrastructure determines the cost and availability of project inputs and can thus be used as an “instrument” for project inputs, as in Figure 2. This procedure can be used (see for example Khandkar and Pitt (1995,1996)) primarily for research projects attached to large showpiece microfinance institutions. Sophistication of this sort is expensive, perhaps prohibitively so for routine monitoring and evaluation.

***Figure 2: Simultaneous causation between project effort and impact***

| | | | |
|---|---|---|---|
| Project impact | --------><br><br><-------- | | Project inputs, other factors (e.g. weather, input prices) |
| | | “Instrumental variables” (e.g. project budget, infrastructure, political conditions) | |

## 2.3 Motivational problems

Other problems are associated with dropouts within either target or control group. If, between the time of the baseline survey being carried out and the time that impact is measured, individuals drop out of the survey because of reluctance to respond the second time around, emigration, death, etc., the results may be biased if the dropouts are in any way unrepresentative of the sample as a whole.

The solution to this problem is to replace dropouts with individuals sampled at random from the original population, and from the same stratum if the sample is stratified. This is made easier if the original sample for the baseline is made larger than necessary; substitute interviewees can then be brought from the original list in case of dropout without the need to go back to the original sample frame.

More challenging is the problem of motivating the entire control group to respond: if (as is necessary for any control group) they have no connection of any kind to the activity evaluated and if, in particular, they do not borrow, then they will have no incentive to cooperate with the survey. They will either refuse to respond or give biased, incomplete or misleading answers.

Experience suggests that in most cases this obstacle can be overcome by a skilled and sympathetic enumerator. Alternatively, two other solutions may be considered:

i) *Bribery.* It seems entirely legitimate to treat the willingness to give an interview as a service (to research) which should be rewarded by a fee for the time sacrificed. In developing countries this fee is frequently paid in kind (e.g. beer) but there is no reason why it should not be paid in cash. In the evaluation of the Gatsby Foundation project in Cameroon (Mosley 1997) the control group were paid for their participation at the same per-hour rate as enumerators; the response rate has been better than in our previous surveys using no payment to respondents (see (ii) below).

ii) *Prospective borrowers*. Let the control group be individuals who have been approved for loans but have not yet borrowed, as experimentally done by Hulme and Mosley (1996:Volume 2 case-studies). This gets around the motivational problem, as the control group, by hypothesis, are eager to co-operate as a consequence of having been admitted to the 'club' of borrowers, indeed possibly the more eager to cooperate as a consequence of believing that their willingness to respond may favorably influence the extent of their access to credit.

The use of approach (ii), of course, also limits the problem of sample selection bias mentioned above: there is no reason to suppose that those more recently accepted for loans have any less entrepreneurial disposition than those accepted earlier.

In Table 1 below we summarize the types of control-group problems and the methods suggested to mitigate them

C *design modifications* to control more effectively for extraneous influences; and

C *complementary analytical methods* to provide a cross-check on whether the results emerging from control-group evaluations are correct.

These complementary analytical methods can themselves be divided into informal (and therefore cheap) and rigorous but expensive tests. Considerations of cost become important in this context.

***Table 1:  Possible corrective measures for improving the accuracy of quasi-experimental (control-group) evaluation methodologies***

| Problems | Possible corrective actions | Supplementary data collection / design modifications | Complementary analytical methods: informal but cheap | Complementary analytical methods: rigorous but expensive |
|---|---|---|---|---|
| 1. Sample selection bias | (a) Initial selection | Control for a larger number of determinants of welfare change (suggested list: initial income and assets, access to infrastructure, weather, land quality, gender, ethnicity).<br>Randomisation of control location. | RRA comparing asset changes etc. among borrower and non-borrower groups. | Within sample regressions using quantity of credit (or N/ of loans) as independent variable. |
| | (b) Recall methods | Cross-check components of income; check declared income against declared expenditure.<br><br>Also measurements of “willingness to accept loan” in non-borrower sample, if the exercise described at right is wanted. | | Regress “willingness to accept loan” on income within *non-borrower sample*, then use results of this regression to correct for “greater willingness to be entrepreneurial” among borrowers. |
| | (c) Fungibility of capital (i.e. attribution) | | | Use other information (e.g. did borrower’s stock of physical and human capital increase?) |
| 2. Inaccurate specification of causal mechanisms | (a) Endogeneity | Instruments for propensity to accept a loan (e.g. capitalisation, gender) | | 2 SLS regressions on borrwer sample. |
| | (b) Failure to pick up structural changes operating through time | Agricultural projects: collection of data over at least two cropping seasons.<br><br>Replacement of dropouts in both borrower and control groups (choose a larger sample than was intended initially). | Case studies within borrower group, in particular to assess interactions between borrower and control group. | |

| | | | | |
|---|---|---|---|---|
| 3. Motivational problems | (a) Persuading chosen control group to be interviewed | Cash or kind payments.<br><br>Use of borrowers-to-be as control group (*can only be used for periods before interviewee became a borrower*). | | Multiple regression within borrower group. |
| | (b) Survey fatigue | Replacement of dropouts in borrower and control groups. | | |
| 4. Resource costs | | Keep borrower and control groups close together (*but risk of contamination of control group through interaction with borrower see 2(b) above*). | | |

# 3. Conclusions and recommendations

What can be done to make the control-group method as accurate as possible? Partly it is a matter of cost, and partly a matter of who is using the method. Evaluators of microfinance projects have the tendency to assess progress on a bewilderingly large range of indicators. For example, *programme managers,* who need immediate information on what works and what does not, will be more concerned with operational performance, delivery and the like. *Evaluators in head office*, on the other hand, are more interested in measures of social impact, direct or indirect, and especially with poverty reduction (outputs 2(b) through 2(d),and 3).

*Table 2: Information required from impact evaluations of microfinance, in relation to categories of user*

| Information category | Examples of target variables in this category | Users |
|---|---|---|
| **1. Inputs**<br>a) Policy | Macro-stability<br>Economic growth<br>Prudential regulation<br>Interest rate controls | Mainly policy makers<br>Central Bank staff |
| b) Financial | Loan collection method<br>Savings/insurance arrangements<br>Lender's pricing policy<br>Incentives to repay<br>Progressivity of loans | Mainly programme managers |
| c) Non-financial | Social intermediation<br>Training<br>Market information | Programme managers and donors |
| **2. Outputs**<br>a) Financial | Profitability<br>Arrears rate<br>Subsidy dependence index | Programme managers and sponsors |
| b) Non-financial, direct beneficiaries | Borrower income<br>Employment<br>Technology<br>Income uncertainty/vulnerability<br>Education indicators<br>Health indicators, incl. contraceptive use, benefits to women and children, empowerment/social relations | Mainly donors and researchers |
| c) Non-financial indirect beneficiaries | As 2 b) but for beneficiaries not borrowing from the institution being evaluated | Donors and (mainly) researchers |
| d) Downstream | As 2 a) and 2 b) but measured for benefits accruing after end of project. | |

It all boils down to what *sort* of information the evaluator wants, in terms of the inputs/outputs/goals stratification; then appropriate *indicators* are selected in relation to the information required. A *control group* is then determined, appropriate to that type of information; the *vulnerability* of the control group to possible bias (Table 1) needs to be looked into, as well; and finally *corrective measures* (modifications of the originally selected control group, or cross-checks using an alternative analytical method) need to be taken to eliminate or reduce these biases.

The complexity and costs of these corrections vary, obviously: they will be more expensive in relation to impact, poverty reduction, than in relation to input, like loan collection. Whether more frequent loan collection improves the repayment rate is a fairly straightforward question that can be assessed by comparing the repayment rate of the target group and a control group who have loans collected less frequently.

The impact of a particular microfinance project on poverty is more complex and requires:

i) assessments of the downstream effects of the project, i.e. effects materializing after the project ends;

ii) assessments of poverty impacts materializing by indirect routes, e.g, through a lower cost of credit offered by informal moneylenders competing with the institution being studied;

iii) case studies to assess project impacts on dimensions of poverty not easily captured by questionnaire methods, e.g. empowerment and vulnerability (see Mosley and Hulme, *Finance Against Poverty*); and possibly also

iv) regressions to check against control-group bias itself (item 1a in Table 1).

Which particular corrective measure is implemented depends on the evaluation budget, but at the minimum a revisit to a subset of the original sample should always be carried out.

***Figure 3: Decision trees for evaluators and programme managers***

| | Donors and evaluators | | Programme managers | |
|---|---|---|---|---|
| Stage 1: Nature of information requirement | Feedback wanted (outputs and goals) | | Feedback wanted (inputs and direct outputs) | |
| Stage 2: Indicators | Direct beneficiaries only (outputs 2a, 2b) | Indirect beneficiaries/downstream effects (outputs 2c through 3) | Project as a whole | Particular design features (e.g. loan collection methods, savings instruments, credit + or -) |
| | | | | Compare across alternative projects with different design features |
| Stage 3: Control group | Choose control group | | | |
| Stage 4: Methodology assessment | Assess vulnerability to problems (1) to (4) | | | |
| Stage 5: Refinements and cross-checks | Implement “corrective measures” specified in table 2, starting in left-hand column and moving as far to the right as the budget allows. | | | |

## General recommendations

1. Where possible, microfinance programmes should be assessed by means of a quasi-experimental method which, as far as possible, holds constant all causes of variation in the chosen target variable apart from microfinance itself. For practical purposes, this means either the use of control groups or regression analysis. If data are available over a number of years, time-series multiple regression analysis is another possibility.

2. Various sources of potential distortion arise with the use of control groups, in microfinance as in any other field of application. These and possible countervailing measures are summarized in Table 1. How far it is appropriate to go with these countervailing measures will depend on the budget and the information needs of the analyst (Table 3). Evaluations of the impact of microfinance on poverty need more elaborate corrective measures than evaluations of the right method of borrower group organization. Generally, evaluations of ultimate impact will need more complex cross-checks than assessments of input.

## Recommendations for evaluators and donors

3. Evaluations of the overall impact of microfinance programmes should be conducted sufficiently long after the end of disbursement to take note of downstream effects, occurring over time, and should also take account of indirect effects, in particular the influence of microfinance on non-borrowers via increased competition and, as a consequence, the increased availability and reduced cost of credit from traditional moneylenders.

4. Evaluations of poverty impact using control groups present particular problems because of the multi-dimensionality of poverty. For these, a range of poverty indicators listed in Table 2 should be used.

## Recommendations for programme managers

5. Programme managers will typically be interested to know whether a microfinance programme works better if specific features (e.g. variations in loan collection method, insurance or savings arrangements) are added to or subtracted from it. For the evaluation of such features, the appropriate method of evaluation will be the “pilot project” in which the participants in the pilot project are the target group.

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