Better to be indirect? Testing the accuracy and cost-savings of indirect surveys for poverty targeting

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Abstract

We test the validity of indirect surveying as a method to collect household data. We compare household and informant reports of assets, develop poverty indices from both, and examine errors in reporting and targeting resulting from using indirectly reported variables. Informant-based targeting indices are highly correlated with household measures, do not vary systematically across informant characteristics, and can be used to assign a simulated anti-poverty program with similar error rates to related methods. Informant indices can be reasonable substitutes for self-reported indices in simple regressions. In our setting, eliminating direct household surveys would have reduced survey costs by 50%.

JEL codes: O12; I32; I38; C83
Keywords: targeting; poverty measurement; indirect survey

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1 Introduction

Governments and organizations managing anti-poverty programs face an important challenge in identifying the communities and households best qualified to receive program benefits. Determining eligibility requires individual household data, often collected using face to face enumeration. The costs of such data collection are high and reduce funds available for the anti-poverty programs themselves. For example, during the first four years of Mexico's PROGRESA educational subsidy program, the largest cost item was beneficiary identification, which constituted 34 percent of total program costs (Caldés, Coady and Maluccio, 2006). Kenya's Household Safety Net Program, another cash transfer mechanism, cites targeting costs of 7 percent of the operating budget (Bahri and Merttens, 2018). Although an average survey cost per household is difficult to calculate, available information suggests a wide range – from \$18 per household surveyed in an agricultural household survey in Mozambique (Kondylis, Mueller and Zhu, 2015) to an implied cost of around \$300 per household surveyed in the average LSMS (Sustainable Development Solutions Network, 2015).

Important progress has been made towards lower cost methods for locating the poor. These include poverty mapping (Davis, 2003; Elbers et al., 2004), geographic targeting (Bigman and Fofack, 2000), and community-based methods of beneficiary selection (Alderman, 2002; Galasso and Ravallion, 2005; Alatas et al., 2012; Karlan and Thuysbaert, 2016; Stoeffler, Mills and Del Ninno, 2016; Basurto, Dupas and Robinson, 2017). In this paper we test the validity of an additional method, "indirect surveying", which provides a possible low-cost way to collect information about household poverty levels. In indirect surveying, a key informant reports on characteristics of other households. An assets index based on these responses may be less susceptible to bias than community-based methods because it relies on the dispassionate collection of observable information, rather than the direct identification of poor households. Like community-based targeting, indirect surveying has the potential to lower survey costs by leveraging the information available to community leaders rather than needing to reach each household.

We report the results of a comparison between household and leader-reported characteristics from a survey in rural Mexico. To test the validity of indirect surveying, we assess (1) whether leaders report household characteristics accurately; (2) how program targeting accuracy differs using direct versus indirect survey measures; (3) whether errors in reporting or targeting vary systematically across community size, leader education, measures of elite capture, or social capital; and (4) what are the cost savings of indirect surveying. Furthermore, cognizant of the large number of surveys implemented in pilot projects or for research purposes only, we also examine the viability of using informant information in regression frameworks.

We find that the leader and household reports of the same characteristic are highly correlated, including when conditioned on community fixed effects. Using the set of variables that are reported by both leaders and households, we construct binary, proportional weighting, and principal components asset indices. In all cases, the indices constructed using the leader reports are highly correlated with the household indices, with correlations near 0.70 across communities and 0.54 within communities.

To assess how indirect surveying would compare with direct surveying as a means to target anti-poverty programs, we consider a hypothetical program where households are identified as poor (eligible for the program) if they fall below the median index value in our sample. We find that if we were to use the leader-constructed indices to identify the poor and non-poor, the households would be correctly identified between 69 and 77 percent of the time, with highest accuracy for the simplest index. The error from classifying households as poor who are not (error of inclusion), ranges from 9 to 22 percent, while the misidentification of households as non-poor when in fact they are poor (error of exclusion) is from 26 to 46 percent. These error rates are similar or lower than those reported in recent examinations of community-based targeting (Alatas et al., 2012; Karlan and Thuysbaert, 2016; Stoeffler, Mills and Del Ninno, 2016) and for proxy means methods (Sabates-Wheeler, Hurrell and Devereux, 2015; Brown, Ravallion and Walle, 2016). We also examine whether or not network size, capture of leadership positions, social capital, or education levels affect reporting accuracy. We find only small associations between social capital and inclusion error and between leader education and age and exclusion error. We also show that leader and household indices produce similar results in simple regressions common to research applications.

Finally, we construct estimates of the potential cost-savings of indirect surveying using information on staff and travel costs. We find that in the context in which one leader and ten households were interviewed per village, interviewing the leader alone could save nearly 50% of the budget. Back of the envelope calculations using actual conditional cash transfer amounts to proxy for benefits suggest that these saved costs would outweigh possible lost benefits from targeting errors due to misreporting. Cost-savings of indirect surveying would likely be highest when populations are sparsely distributed and the transport costs of reaching households within communities are high.

Our central contribution is to assess the tradeoff between misreporting due to indirect surveying and the potential cost savings of this method. Here we show that in the case where policymakers agree that targeting can be done using an asset index approach (Filmer and Pritchett, 2001; McKenzie, 2005; Filmer and Scott, 2008) – i.e., using relatively observable indicators of poverty – the saved costs from indirect surveying outweigh the benefits from mis-allocation.[1] As mentioned above, there is a growing body of work examining lower cost methods for improving targeting of social programs. Some of these analyses focus on leveraging existing data for future targeting, such as poverty mapping (Davis, 2003; Elbers et al., 2004) or geographic targeting (Bigman and Fofack, 2000). Others have examined the possibility of lowering costs of program implementation either via self-targeting (Alatas et al., 2016; Olken, 2016), or through the community-driven methods for locating relevant recipients of anti-poverty programs mentioned above. Indirect surveying is most similar to this last group in that it has the potential to increase the cost-effectiveness of the data collection process itself. However, it differs in that rather than asking leaders to assess poverty directly, informants supply specific characteristics which can then be used to facilitate targeting.

This paper also contributes to the extensive literature on survey methodology related to key informant surveys and rapid rural assessments. Our results are consistent with prior work on key informant reporting (Kumar, 1989; Finan and Schechter, 2012) and other work that finds that the quality of secondary reporting varies with the observability of the characteristic (Macours, 2003; Frenk et al., 2010). Prior literature suggests that covariates like gender, race, age (Frenk et al., 2010), productive capital (Takasaki, Barham and Coomes, 2000), and community characteristics (Young and Young, 1961) tend to be more accurately reported than characteristics like small consumer durables, food consumption, or changes in religious values. Perhaps unsurprisingly, some key informants have been found to report inaccurately, particularly when they do not have much outside contact (Macours, 2003), if they believe that their answers could influence the distribution of programs (Rigol, Hussam and Roth, 2017), if they do not trust interviewers (Chambers, 1994), if enumerators exert undue influence on respondents (Bergeron, Morris and Banegas, 1998), or if informants are not very well-informed (Phillips, 1981; Garrett and Downen, 2001; Romney, Weller and Batchelder, 1986). In our case, the leaders, who are elected officials within relatively small communities, appear to fulfill the criteria to avoid these biases and our results indicate they do. Our work is similar to the prior literature on key

[1] We also recognize that all approximate measures of poverty suffer from a wide array of challenges (Brown, Ravallion and Walle, 2016). Our purpose here is not to interrogate this particular method, as we do not have consumption data with which to compare our indices.

informant surveys and rapid rural assessment, in that it tests the degree of misreporting of individual assets and whether or not this misreporting is systematic. However, it contributes to our existing understanding by using a considerably larger and more nationally representative sample than prior tests. To our knowledge, this is also the first study to examine whether reporting is related to community social capital.

2 Data and empirical approach

2.1 Data description

The test of indirect surveying was added on as a short module to a survey collected for the purposes of evaluating the social and environmental impacts of a payments for ecosystem services (PES) program in Mexico (Alix-Garcia et al., 2018*b*). The design of the evaluation took advantage of a threshold in the enrollment criterion for the program, where beneficiaries were selected based upon a point system assigned by the implementing agency (National Forestry Commission of Mexico – CONAFOR). As a result, the sampling strategy selected treatment and control communities with assignment scores around the acceptance threshold, moving farther away from the enrollment threshold as properties nearby were exhausted. In total, 862 communities and 8,413 households were surveyed in 12 Mexican states in 2016. All participants had applied to the PES program between 2011 and 2014.

Figure A1 highlights the states in which interviews took place. Table A1 includes the means and normalized differences of means between the sampled applicants and the universe for the survey (column 2) and universe of all common properties (column 5). The differences between the sampled and not sampled communities within states are generally quite small in magnitude – the main differences are that sampled villages tend to be in municipalities with greater proportions of indigenous people, have slightly higher forest canopy cover, and be somewhat farther from major cities. The fourth column of the table lists a small set of covariates from all of the agrarian communities or "ejidos" and "comunidades") in the entire country.[2] Compared to this group, the communities in our sample tend to be larger in area, but similar in population size (within .25 standard deviations). This makes sense, given that a requirement for being in our survey was

[2]These are collectively managed communities, many of which have origins before the Spanish conquest (Knight, 1986). Current titles to the rights in these communities occurred as part of Mexico's Agrarian Reform between 1917 and 1992, and additional "regularization" of holdings occurring to the present. Today over half of the country's land area and half its rural population is held in agrarian communities (De Janvry, Gordillo and Sadoulet, 1997; Fox, 2007).

to have submitted an application to a land cover conservation program, and communities with larger area tend to also have more vegetation to conserve. In other respects, these communities are likely to be broadly representative of rural agrarian communities.

CONAFOR helped enumerators from Chapingo University in Mexico gain access to communities. Chapingo staff implemented the survey, but no CONAFOR staff were present for interviews. Enumerators identified themselves as coming from Chapingo and conducting academic research. Leaders and individual households were interviewed separately. Leaders were individuals holding one of the main elected community positions of president, secretary, treasurer, or oversight committee. Surveyors asked first to speak with the president, but if the president was absent, they would speak with the designated representative. For the household survey, ten households were randomly selected to be interviewed, based on lists of members obtained in advance, with responses given by the household head.

The data used here are the questions related to household indicators, participation in community work, and meeting attendance. These questions appeared at the end of the leader survey, and the interviewed leader was only questioned about three out of the ten households selected for household surveys. Leaders were also asked about the functioning of the payments for ecosystem services program, community characteristics (infrastructure, productive activities, size, etc.), and social capital (Alix-Garcia et al., 2018*a*).

Households responded to a set of questions about the PES program and their participation in community work, standard household demographic characteristics, income sources, housing characteristics, and small assets. Housing characteristics included type of walls, roofing material, floor material, number of rooms, as well as access to public services, including existence of water and sewage connections, and amount of water and electricity availability. In order to construct a broad assets index, questions were asked about the presence or absence of the following items: television set, refrigerator, blender, microwave oven, laptop, car, truck, motorbike, bicycle, gas or electrical cooker, telephone landline, and mobile phone.

Leaders were asked to report on a smaller set of questions about the three households. The indirect survey questions included: whether the household head had secondary education, type of walls of the household's home, ownership of a television, car, or mobile phone, the number of household members, how many assemblies the household attended in the past year, if someone from the household participated in forest maintenance in the past year, and how many heads of large livestock the household owned. Although limited, these have the following useful characteristics. First, they include several assets that are used to help construct Mexico's marginality index (CONAPO, 2019), which is used for targeting welfare programs throughout the country. Second, they include goods which are easier (a car) and more difficult (a mobile phone) to observe. There are both binary and continuous measures. And finally, these characteristics encompass a range of possible indicators from common to uncommon, which is useful in capturing the variation in wealth in the population.

2.2 Empirical approach and accuracy of reporting

The empirical strategy to assess how well leaders report household characteristics is straightforward. We examine simple correlations and variation in correlations within villages using fixed effects. The assumption here is that the preferred approach is to interview all households individually, so although we are well aware that survey respondents may misreport for a variety of reasons, our interest is in assessing the accuracy of the leader reports relative to the households' reports.

Table 1, panel (A) examines the correspondence between individual characteristics reported directly by households and indirectly by leaders, as measured by normalized differences, correlation coefficients, and point estimates from regressions with community fixed effects. The normalized differences should be interpreted as standard deviations of difference between the two listed means.

The table suggests that the normalized differences tend to be small – in all cases, less than one-quarter of a standard deviation. In keeping with prior studies, more observable items – housing walls, cars, and number of livestock – have the smallest differences. The largest differences are found in the number of assembly meetings, whether or not the household head has secondary education, and if the household has a cell phone. It is interesting that in eight out of the nine variables, leaders seem to overestimate – i.e., they are more likely to report positively for the binary variables, and also report slightly larger household and cattle herd sizes. Similarly, we find strong correlations between leader and household reports of assets. These range from 0.36 and 0.79 across the entire dataset, and from 0.19 to 0.68 with community fixed effects. For the responses that might be integrated into a wealth measure, the lowest overall correlation is with household head's schooling (0.36). We conclude from this table that reporting accuracy is generally high, but again tends to be higher for more observable characteristics.[3]

[3]The fixed effects estimation on number of assembly meetings is not estimated due to the fact that leaders generally report the same number of assembly meetings attended by everyone in the subsample of households surveyed. While

3 Predicting poverty using leader versus household reports

To understand how indirect reporting could be used to save program cost, we create parsimonious poverty indices from the leader and household reports. We include only the variables that are in the indirect survey and are also included in Mexico's national "marginality index" used for targeting poverty programs. Such asset indices have been shown to be useful measures for measuring wealth inequality in Mexico (McKenzie, 2005). We then test the magnitude of the error that occurs if the leader indices are used for targeting rather than the household reports. We aggregate variables using three approaches.

1. "Simple": We calculate a simple average of indicator variables for: a household head with secondary education, high quality walls on the home, ownership of a television, car, or mobile phone.

2. "Inverse proportion weighted" (IPW): We use a summation that weights each asset by the inverse proportion of its presence in the sample (Decancq and Lugo, 2013).

3. "Principle components" (PCA): We use principal components to aggregate these same binary responses (Filmer and Pritchett, 2001).

Table 1, panel (B) reports the mean values of the indices calculated with each methodology using both the leaders' and the households' responses to the survey questions. There is a strong and similar correlation for all indices, with values of the correlation coefficient close to 0.70 and the coefficients from the fixed effects estimation greater than 0.50 in all cases. The normalized difference of the mean reports is the largest for the IPW index, and smallest for the PCA index. Appendix Table A2 shows these same values for the indices aggregated up to village averages. The aggregation in all cases slightly increases the correlation between household and leader reports. The variance of the raw indices also differs across aggregation methods – the simple index has the lowest standard deviation and the principle components index the highest. For subsequent analysis, we normalize each index to facilitate comparability.

Figure 1 visualizes the correlations between the leader reports (on the y-axis) and the household reports (on the x-axis) of each index. The 45-degree line shows hypothetical perfect correlation. In both cases, the correlations between the leader and household indices are strongly positive. However, they are clearly

attendance rates are generally high, it may be the case that reporting for outcomes that might have social desirability associated with them (e.g., attending assemblies) is likely to be more problematic. Recalling attendance precisely may also be particularly difficult.

imperfect – leaders tend to report households as wealthier than households self-report, while the opposite is true for wealthier households (this pattern may also indicate some simple reversion to the mean). The problem of over-estimating household wealth appears worst in the IPW index.

Table 2 examines the level of error that would be introduced in a program that used the leader indices for targeting of an anti-poverty intervention. The targeting criterion is that a household is identified as "poor" if it falls below the median value of the poverty index calculated over the entire sample of household responses[4]. This is meant to proxy for what might occur if an index were used for targeting – although the actual cutoff value might be established using a different set of data, such as the census. As shown in Table 2, in our sample using the simple index of household responses, 41.7% were non-poor and 58.3% were classified as poor (The numbers are not exactly 50-50 due to lumpiness in the indices). Leaders then correctly identified households as poor or non-poor 77% of the time (34.7%+42.4%). Conditional on not being poor, leaders correctly identified households as non-poor 83% of the time (34.7%/41.7%) and conditional on being poor, leaders correctly identified households as poor 73% of the time (42.4%/58.3%). Under standard error classifications, this means that for the simple index, inclusion error – where the non-poor were classified as poor – was 17% (7%/41.7%). Exclusion error – where the poor were classified as non-poor – was 27% (15.9%/58.3%). Exclusion error was largest for the IPW index (46.5%) and lowest for the PCA index (26%), with the magnitudes for inclusion error reversed. The overall targeting error rates range from 23% to 31%.

These rates are similar to those reported in recent work. Alatas et al. (2012) report that proxy means testing results in an overall targeting error of 30 percent in an experiment in Indonesia, with an inclusion error of 20% and an exclusion error of 53%, based on a poverty classification using detailed consumption information. The overall error increased by 10 percent (3 percentage points) using ranking by community leaders as a targeting method, with the increase coming from errors of inclusion. Other work on hybrid household and community targeting also suggests comparable error rates (Karlan and Thuysbaert, 2016; Stoeffler, Mills and Del Ninno, 2016).

[4]Table A3 shows how leader and household indices rank individuals from poorest to richest in a community. For the simple index, leader indices yield give the same rank as households 55% of the time. Correspondence in ranking tends to be higher for poorer than for wealthier households. Confounding occurs most commonly between the two poorest households, rather than between the wealthiest and the second ranked.

4 Are errors systematic?

This section examines whether or not village or leader characteristics influence the magnitude of misclassification. There are a variety of reasons why this might occur. First, leaders might be misreporting if they believe that such behavior might result in more resources being sent into the community or to particular households in the community. One might also think that reporting accurately might be easier where there is more community coherence and stronger networks; conversely, it might be more difficult in settings where populations are larger – as larger networks are well-known to create complications in the dissemination of information (Genicot and Ray, 2003). We also examine whether or not leaders who have been in power longer tend to report differentially. Individuals in community leadership positions are only allowed to hold a post for 3 years before there is a new election, and consecutive re-elections to the same position are not allowed. However, individuals may move into other positions within the community, return to a leadership position after a respite of 3 years, or simply ignore the rule. To try to capture this behavior, we take the average number of years within the past ten that the secretary or president has held a community leadership position, and create an indicator for when this average exceeds three years. Our intention is that this variable proxy for capture of the leadership, although it might also simply capture good leaders who are frequently elected.

Because community cohesion may affect the ability of a leader to report accurately on the characteristics of other households, we also test the relationships between error and an index measuring social capital. This index is based on participation in community assemblies, self-reported measures of trust, the presence of local public goods (a village vehicle, community house, etc.), the institutional structures that support social capital, such as the range of decisions made in assemblies, and the ability of non-community members to participate in them. The index comes from Alix-Garcia et al. (2018*a*), which tests the impact of payments for ecosystem services on social capital using a regression discontinuity approach. More detail can be found in Appendix C. Finally, we examine if the education or age of leaders affects their ability to report accurately on the characteristics in which we are interested. These are examined individually, and then together in the same regression.

Table 3 shows regressions of binary indicators of exclusion or inclusion error for each of the indices. We do not find that community size or whether leaders were in power for more than three years is associated with error. Higher social capital is correlated with greater inclusion error for the binary and PCA indices, however, the magnitudes of this effect are small. In both cases, one standard deviation higher social capital results in an increase in inclusion error of 4 percentage points. Secondary education and greater age of leaders are significantly associated with greater exclusion error. A one standard deviation increase in the probability of having an educated leader leads to an increase in the probability of exclusion error of 7 percentage points, and an increase of parallel magnitude in leader age leads to a 5 percentage point increase in exclusion error. Table A4 shows these correlations in separate regressions for each covariate. The point estimates in these separate regressions are nearly equivalent to those in the estimation including all covariates simultaneously.

When we examine the drivers of erroneous reporting of specific characteristics (Appendix Table B1), the only asset misreporting that we observe associated with social capital is television – a higher social capital index is associated with marginally statistically significant underreporting of the presence of televisions, which may explain the inclusion error associated with social capital. More educated leaders and older leaders are also significantly more likely to report that household heads have secondary education, which may partially explain increased exclusion error associated with these characteristics.

Finally, there are a significant number of households for which leaders state that they "do not know" at least one of the characteristics that they are requested to recall on the indirect survey. 68% of households have indirect observations on all five characteristics used in the poverty index. However, 89% are missing only one (usually secondary education). Appendix C examines patterns of missing observations and examines two possible corrections. We show that poorer communities are likely to have more missing information but that there is no correlation between poverty and misreporting within villages. Furthermore, simple corrections for "don't know" responses do not substantially increase targeting error.

5 Cost-benefit

The cost of directly surveying households rather than using key informants is the extra time spent visiting households once enumerators have arrived at a community and spoken to the leader. This cost is a function of the number of enumerators needed and the travel costs to get additional team members to each community, as well as the time that households spend taking the surveys. The survey implemented here occurred over an 11 week period, with survey teams of three per community, with one of these enumerators acting as a "manager" and earning a slightly higher salary. The assumptions were that each team could visit 1 to 2 communities per day, and transportation generally occurred via rented trucks. In total, 30 enumerators and 15 managers were hired for this project. The approximate cost of the entire survey was $551,000 USD to reach 862 communities and survey 10+ households in each one. Transportation costs were a substantial portion of this budget.

By eliminating the household surveys (or only interviewing one or two in order to incentivize truth-telling by leaders), we estimate that the number of team members could be reduced to 10 managers and 10 enumerators[5]. This would also increase the probability of getting to more communities in a given day, reducing transportation and time costs. Under conservative assumptions regarding how many communities could be reached, the approximate cost of surveying only leaders is $274,000 USD – a decrease of nearly 50% in the survey budget. We approximate the time cost of the full household survey to include both the time spent by households and the time spent by leaders responding to the survey. This amounts to a total of $3,332. By surveying only leaders and slightly increasing the amount of time allocated to respond, we arrive at a time cost of $1,379.[6]. The differences in these figures are likely to underestimate the cost-savings, as they do not account for the value of the time taken by the households to travel to the central site, or for leaders to assist enumerators in locating households.

It is difficult to establish a comparable change in value for the benefits of more accurate targeting. One rather stark assumption that one might make is that the value of a correctly allocated transfer is the monetary amount of that transfer, while the value of a mis-allocated transfer is zero, so the gain from allocation is exactly the value of the correctly assigned transfers. The annual transfers for a family with three children ages 6 months, 6 years and 9 years from Mexico's Prospera program (formerly Oportunidades/Progresa) is currently around $700 per year, depending on the selection of health and education supports (Dávila Lárraga, 2016). Under our binary, household-reported measure of poverty, 58.3 percent of the 1741 households should be classified as poor. If all households defined as poor by the household responses were to receive payments, then the Prospera budget for our sample would be $710,502 for this group. Again, assuming that

[5]We arrived at this number of field staff by assuming people must always travel accompanied and in vehicles with 4x4 traction; this is for safety reasons and due to the poor state of rural roads and remoteness of communities. The low number of surveys means that teams of two people suffice. We examined different budget scenarios by varying the number of teams until arriving at the cost-minimizing number of 10.

[6]The average agricultural day wage reported in communities in our survey was 137 pesos (7.61 USD/day). If we assume six hours of work per day, this is approximately $1.28 per hour. We assume that the survey time was equal to one hour per household or leader, and that the survey time would increase to 1.25 hours for surveying just leaders.

benefits to society only occur in the event that resources are properly transferred according to the household definition of poverty and that there are no multiplier or crowding-out effects of the transfers, then the ratio of benefits properly transferred to survey costs for the household survey is $1.28 ($710,501/$554,331). If, instead, we engaged in targeting based upon the leader binary index, the required budget would be $602,037 (49.4*$700*1741), but 7/49.4 = 14 percent of this budget would go to those mistakenly classified as poor by the leader index, so total benefits transferred would be $516,729 ($602,037-0.14*602,037). This means that the ratio of benefits properly transferred to survey costs using the indirect survey would be $1.88 (516,729/275,379). The benefit-cost ratio is clearly higher in this setting for the indirect survey than for the direct approach.

6 Using noisy measures in regressions

Because data is very frequently collected to conduct empirical academic studies, we use this final section to examine whether using the leader versus household indices makes a worrisome difference in a simple OLS or fixed effects regression. We are cognizant that the results here could vary substantially depending upon the outcome analyzed, and may not be generalizable. However, in the interest of examining at least two cases, we run a series of predictive regressions of school enrollment and migration to understand the relationship between these outcomes and the poverty indices. In particular, we generate variables measuring the proportion of children between 12 and 18 enrolled in school in the households, and if the household has at least one migrant in another country. We run simple regressions limiting our sample to households that have children in these age ranges in the first case, and on all the households in the second case. We include only households that have full data for leader and household wealth indices, as well as the outcomes. We also test whether results are sensitive to the type of index.

Table 4 shows correlations calculated using both leader (odd columns) and household (even columns) indices, and with (columns (5)-(8)) and without (columns (1)-(4)) fixed effects. The point estimates for the relationship between household wealth and outcomes using simple OLS are very close in magnitude and significance, with slightly larger differences for the migration outcome. With fixed effects, the results are similar, although there is a loss of statistical significance in the migration regressions for the IPW index. Chi-squared tests of difference across leader and household reported indices for each outcome all fail to reject equivalence of coefficients at the 5 percent level.

This table also confirms two standard results from the literature on measurement error. First, classical measurement error attenuates coefficients in a manner proportional to the signal to noise ratio. This explains why the coefficients in column (1) are slightly smaller than those in column (2). The differences are quite similar across indices, which is consistent with the very similar correlations between leader and household reported indices across aggregation methods. However, the difference in magnitude in the fixed effects estimation is larger than that of the simple regressions, confirming that a panel type structure can exacerbate this problem, particularly where there is high correlation within the panel. These results suggest that the indirect method of surveying would increase measurement error, but would stil be useful for research purposes based on a realistic survey collection and application.

7 Conclusion

Indirect surveying provides a simple methodology that can lower survey costs for both program targeting and basic research. This methodology exploits knowledge from leaders (key informants) to help collect information on the basic assets of other households that can be used to generate asset indices. Using the leader-generated indices results in targeting error that is within the usual range for other related targeting methodologies to assess poverty using community knowledge. These key informant-generated indices also appear to work well as regressors in fairly standard estimation frameworks. The survey cost savings in our application is around 50% of the budget.

The utility of indirect surveying is likely to depend upon the configuration of households and key informants, as well as the roles those informants play. The setting in which we assess indirect surveying – villages in rural Mexico – has features that would likely facilitate the implementation of this methodology and lead to cost-savings. First, communities are rural and have an average number of members of 170, so most are small enough for households to know each other well. While we do not observe significant differences in reporting bias as our sample approaches its maximum number of community members (over 3,000), this may be because we have only small numbers of villages with very high membership.

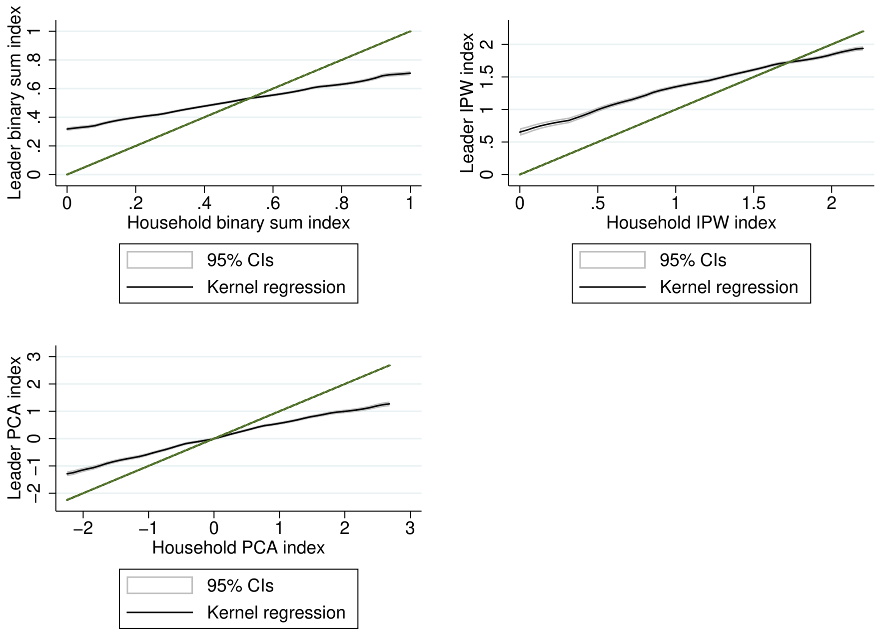
Second, leaders in this situation are elected and have no direct incentive to misreport. Those responding to our survey also knew that we would also be asking similar questions to the same households. This may have created incentives for more accurate reporting. One could replicate this incentive by simply surveying one household at random (rather than many), and still benefit from significant cost reductions.

These features are not unique to Mexico, but are indeed likely to be present in many parts of the globe where there are sparsely dispersed populations. Our findings are likely most relevant for rural areas, particularly those with poor road quality or rugged terrain, where transport costs will be high. However, given the known relationships between remoteness and poverty itself, these are likely the types of areas where poverty alleviation programs themselves may matter most in a range of countries.

Finally, we acknowledge that our study provides an important preliminary test of a method, but also raises additional questions that should be answered before trying to scale up this methodology. In particular, it would be useful to probe the limits of indirect surveying, including the number of households and covariates about which informants can accurately respond. It would also be useful to compare this method more directly with other lower-cost methods of targeting, including geographic approaches or subjective leader measures. We hope that future research will illuminate more information about these parameters in the interest of lowering survey costs to make more funds available for programming or for other research endeavors.

8 Tables and figures

Figure 1: Asset indices calculated using leader versus household reports



Figures show kernel regressions of leader reported indices on household reported indices. The y-axis are leader reports and the x-axis household reports. The green line follows the 45 degree path from the origin.

Table 1: Summary of data and indices

	Leaders	Households	Norm Diff	ρ	FE
A: Household characteristics					
Cell Phone	0.518	0.423	-0.134	0.485	0.358
	(0.500)	(0.494)			(0.027)***
Head secondary education	0.191	0.114	-0.152	0.370	0.417
	(0.393)	(0.318)			(0.033)***
Ln(HH size)	1.985	1.943	-0.065	0.712	0.660
	(0.506)	(0.507)			(0.023)***
Wall type	0.489	0.478	-0.016	0.699	0.499
	(0.500)	(0.500)			(0.025)***
Television	0.823	0.783	-0.071	0.589	0.376
	(0.382)	(0.412)			(0.023)***
Car	0.446	0.430	-0.023	0.720	0.614
	(0.497)	(0.495)			(0.024)***
# of assembly meetings	2.323	2.550	0.234	0.729	n/a
	(0.729)	(0.637)			
IHS(# of large livestock)	1.209	1.094	-0.075	0.789	0.682
	(1.557)	(1.547)			(0.025)***
Participated forest care	0.788	0.730	-0.096	0.363	0.187
	(0.409)	(0.444)			(0.024)***
B: Household poverty indices					
Simple index	0.493	0.446	-0.130	0.698	0.559
	(0.270)	(0.248)			(0.024)***
Inverse proportion weighted index	1.415	1.211	-0.224	0.700	0.537
	(0.688)	(0.605)			(0.024)***
Principle components index	0.006	0.032	0.014	0.695	0.548
	(1.334)	(1.246)			(0.024)***
Observations	1741	1741			1741

The first two columns report means and standard deviations. Norm diff is the normalized difference in means between the first two columns. ρ is the correlation coefficient, which is equivalent to the coefficient on an OLS regression without a constant. Fixed effects estimates use community-level fixed effects. Index regressions (panel B) use the continuous version of the asset indices calculated using leader responses on those calculated using household responses. Standard errors are clustered at the level of the community. Indices are normalized before being used in the regression. * p <.10, ** p< .05, *** p<.01.

Table 2: Classification of poverty using household versus leader reporting

	Household responses		
	Non-poor	Poor	Total
Leader responses			
Simple index			
Non-poor	34.6	15.9	50.5
Poor	6.9	42.6	49.5
Total	41.5	58.5	100.0
	Non-poor	Poor	Total
Inverse proportion weighted index			
Non-poor	37.5	27.4	64.9
Poor	3.6	31.5	35.1
Total	41.1	58.9	100.0
	Non-poor	Poor	Total
Principle components index			
Non-poor	36.3	13.9	50.2
Poor	10.4	39.4	49.8
Total	46.7	53.3	100.0

Cells show percentages in each category. In all cases, the poor are those with less than the median level of the index calculated across the entire sample using the index developed from household reports.

Table 3: Correlates of errors of inclusion and exclusion

	Exclusion error			Inclusion error		
	Binary	IPW	PCA	Binary	IPW	PCA
IHS(Community members w/ full rights)	-0.001	0.003	0.001	-0.006	-0.005	0.006
	(0.015)	(0.018)	(0.016)	(0.016)	(0.012)	(0.017)
President/secretary in office > 3 yrs	0.077*	0.053	0.065	-0.008	-0.006	-0.003
	(0.046)	(0.052)	(0.048)	(0.046)	(0.036)	(0.050)
Social capital index	0.136	0.077	0.149	0.327**	0.150	0.322**
	(0.146)	(0.158)	(0.153)	(0.139)	(0.109)	(0.143)
Leader completed secondary sch.	0.094***	0.136***	0.105***	-0.025	0.030	-0.056
	(0.036)	(0.040)	(0.038)	(0.033)	(0.024)	(0.034)
Leader age	0.003	0.005***	0.003**	-0.000	0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	1032	1039	941	732	725	823
Mn dep. var.	0.271	0.465	0.261	0.167	0.088	0.222

Regressors are binary variables indicating errors of inclusion or exclusion. Standard errors are clustered at the community level and regressions are simple OLS with a constant. Regressions are run on the sub-sample of households identified as poor (exclusion error) or non-poor (inclusion error) according to the household data. All panels contain only the covariates reported and a constant. * p <.10, ** p< .05, *** p<.01.

Table 4: Sample regressions using constructed poverty indices

	Secondary		Migration		Secondary		Migration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Simple index								
Simple index, leader responses	0.081***		0.014**		0.062*		0.015*	
	(0.016)		(0.006)		(0.037)		(0.009)	
Simple index, hh responses		0.095***		0.022***		0.092**		0.028***
		(0.016)		(0.006)		(0.037)		(0.009)
P-value diff.		0.207		0.081		0.314		0.150
B: IPW index								
IPW index, leader responses	0.073***		0.013**		0.054		0.011	
	(0.016)		(0.006)		(0.037)		(0.009)	
IPW index, hh responses		0.087***		0.021***		0.079*		0.028***
		(0.016)		(0.006)		(0.042)		(0.010)
P-value diff.		0.205		0.099		0.448		0.056
C: PCA index								
PCA index, leader responses	0.077***		0.014**		0.057		0.015*	
	(0.016)		(0.006)		(0.035)		(0.009)	
PCA index, hh responses		0.093***		0.021***		0.092**		0.028***
		(0.016)		(0.006)		(0.037)		(0.009)
Ejido FE	no	no	no	no	yes	yes	yes	yes
Observations	614	614	1764	1764	614	614	1764	1764
Mean DV	0.703	0.703	0.071	0.071	0.703	0.703	0.071	0.071
P-value diff.		0.165		0.135		0.235		0.147

Estimate shown are coefficients from OLS and FE OLS regressions of the proportions of children in a household attending secondary school or higher, or on having an international migrant on the binary wealth indices. School age is defined as 12-18 years old. Regressions only use households who have children in one or both of these age categories. Standard errors are robust and clustered at the ejidal level. Only households with complete information for both leader and household indices are used in the regressions. Indices are normalized before using in regression. The p-value in the last row of each panel is a chi-squared test for the difference in coefficients between the leader and the household reported measure. * p <.10, ** p< .05, *** p<.01.

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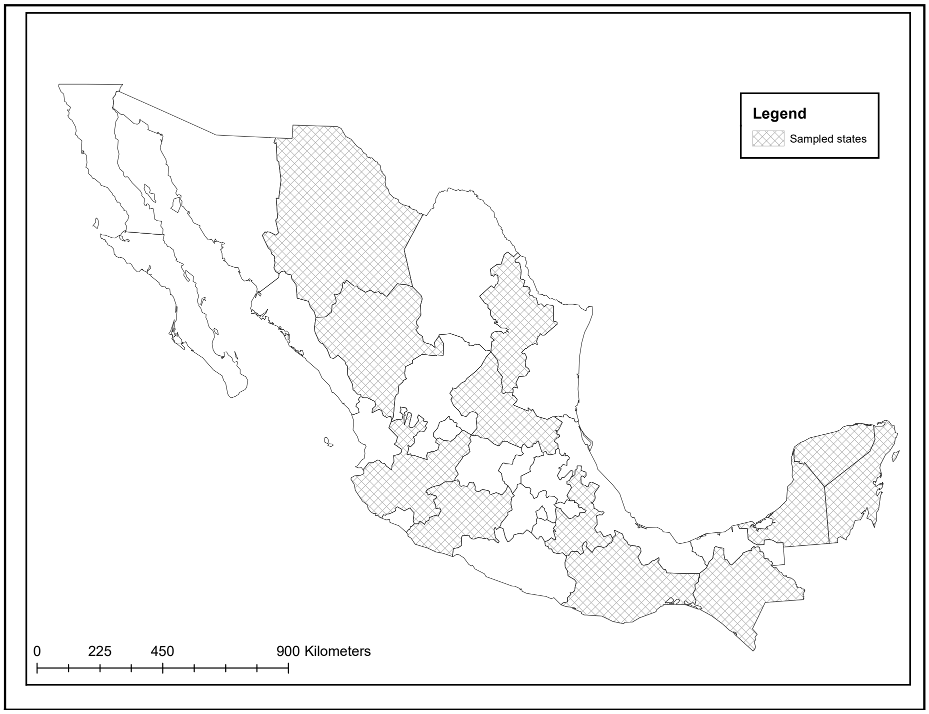
Appendix A Supplementary tables and figures

Figure A1: Sampled states



States within which surveys occurred are cross-hatched.

Table A1: Sample versus universe of PSA applicants and universe of ejidos

	Sampled	Not sampled		Not in universe	
	(1)	(2)	(3)	(4)	(5)
			(1) v (2)		(1) v (4)
Ln(ejido area, ha)	8.21	8.27	-0.04	6.84	0.79
Ln(common property area, ha)	7.81	7.76	0.02	4.08	1.02
Ln(ejidatarios)	4.60	4.60	-0.00	4.30	0.25
Ln(non-ejidatarios)	1.35	1.42	-0.03	1.73	-0.17
Ln(area submitted, ha)	6.70	6.68	0.01		
Average slope (deg)	11.00	12.28	-0.13		
Average elevation (m)	1309.40	1388.84	-0.06		
Ln(km to any road)	7.64	7.77	-0.09		
Ln(km to major city)	4.60	4.43	0.20		
Ln(km to city > 5,000)	3.23	3.23	-0.00		
Mean canopy cover	50.91	42.43	0.20		
% indigenous in municipality	0.33	0.16	0.29		
Deforestation risk (INE)	0.06	0.06	-0.02		
Obs	862	1977		27497	

Columns (1) and (2) show mean values of variables for communities that were sampled versus those that were not sampled but did apply for PSA. Column (3) presents the normalized difference between these two. The fourth column shows means for variables available in Mexico's PHINA database, which gives information on all agrarian communities existing as of November 2016. The last column presents the normalized differences in means between the sampled communities and nationwide PHINA database.

Table A2: Summary of aggregated community indices

	(1)	(2)	(3)	(4)
	Leaders	Households	Norm diff	ρ
Simple index	0.489	0.442	-0.152	0.733
Inverse proportion weighted index	1.405	1.204	-0.259	0.741
Principle components index	-0.021	0.012	0.022	0.735
Observations	735	735	735	735

Columns (1) and (2) report mean reported values for indices calculated using assets reported by leaders and households, respectively, aggregated to the level of the whole ejido. Column (3) lists the normalized difference of the two means. ρ is the correlation coefficient between the indices constructed with household and leader reports. The total number of communities included is less than 862 due to missing data.

Table A3: Classification by rank within community, leaders versus households

	Household responses			
	1	2	3	Total
Leader responses				
Simple index				
1	27.2	12.0	2.7	41.8
2	11.1	16.0	8.1	35.2
3	3.5	7.3	12.2	23.0
Total	41.8	35.2	23.0	100.0
	1	2	3	Total
Inverse proportion weighted index				
1	28.1	10.6	3.1	41.8
2	10.1	18.1	7.0	35.2
3	3.5	6.5	13.0	23.0
Total	41.8	35.2	23.0	100.0
	1	2	3	Total
Principle components index				
1	27.5	11.1	3.2	41.8
2	10.7	17.5	7.0	35.2
3	3.6	6.6	12.8	23.0
Total	41.8	35.2	23.0	100.0

Cells show percentages in each category. Rankings are unique, with the lowest value going to the poorest household. In the event that there are ties – which occurs for about 30 percent of the households – the ties are broken arbitrarily.

Table A4: Correlates of errors of inclusion and exclusion

	Exclusion error			Inclusion error		
	Binary	IPW	PCA	Binary	IPW	PCA
A: Community size						
IHS(Community members w/ full rights)	-0.001	0.004	0.002	-0.005	-0.002	0.005
	(0.015)	(0.018)	(0.016)	(0.015)	(0.012)	(0.016)
B: Leadership capture						
President/secretary in office > 3 yrs	0.069	0.053	0.054	-0.020	-0.008	-0.021
	(0.045)	(0.051)	(0.047)	(0.044)	(0.035)	(0.048)
C: Social capital						
Social capital index	0.041	-0.040	0.050	0.334**	0.126	0.334**
	(0.138)	(0.152)	(0.145)	(0.140)	(0.115)	(0.143)
D: Education/age						
Leader completed secondary sch.	0.092**	0.135***	0.103***	-0.034	0.025	-0.062*
	(0.036)	(0.039)	(0.038)	(0.033)	(0.025)	(0.034)
Leader age	0.002	0.005***	0.003*	-0.001	0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	1032	1039	941	732	725	823
Mn dep. var.						

Regressors are binary variables indicating errors of inclusion or exclusion. Standard errors are clustered at the community level and regressions are simple OLS with a constant. Regressions are run on the sub-sample of households identified as poor (exclusion error) or non-poor (inclusion error) according to the household data. All panels contain only the covariates reported and a constant. * p <.10, ** p< .05, *** p<.01.

Appendix B Correlates of inaccurate reporting

This section examines whether or not community characteristics influence the magnitude of misreporting, using the same type of analysis as is contained in the main text of the paper.

Table B1 reports regressions of the difference between leader and household reports of the variables of interest. The table shows only those households and leaders where there were no missing values for any of the items. In these regressions we do not examine number of meetings attended, since the results above suggest that leaders largely report the same value for all households within their community.

We find that accurate reporting of individual characteristics can be influenced by community size – having more potential members upon whom to report decreases accuracy for reporting on the presence of televisions and mobile phones, household size, participation in collective forest work and household size. Higher social capital (panel (c)) results in lower reporting of television sets and household size, which explains the correlation with exclusion error of this covariate. Leaders with secondary education tend to be more likely to misreport that other household heads have secondary education, and that they have larger households. Older leaders over-report secondary schooling of other household heads and participation in forest work. The over-reporting of secondary education increases the asset indices and illuminates the source of the exclusion error associated with these characteristics.

Table B1: Difference between leader and household report

	Sec. sch. (0/1)	Walls (0/1)	Television (0/1)	Car (0/1)	Mobile (0/1)	Forest wk. (0/1)	HH size (num.)	Cattle (num.)
A: Community membership size								
IHS(Community members)	0.013	-0.002	0.019*	-0.011	-0.029**	0.027*	0.023**	0.028
	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)	(0.010)	(0.018)
B: Leadership capture								
President/secretary in office > 3 yrs	-0.021	-0.013	0.016	0.023	0.062	0.037	0.006	-0.061
	(0.036)	(0.027)	(0.021)	(0.027)	(0.041)	(0.042)	(0.024)	(0.049)
C: Social capital								
Social capital index	0.026	0.132	-0.136*	-0.022	-0.191	0.115	-0.113*	0.131
	(0.100)	(0.084)	(0.081)	(0.078)	(0.120)	(0.110)	(0.067)	(0.161)
D: Education/age								
Leader completed secondary sch.	0.085***	-0.020	0.028	0.014	-0.002	0.005	0.049**	-0.059
	(0.023)	(0.024)	(0.019)	(0.021)	(0.031)	(0.028)	(0.020)	(0.039)
Leader age	0.002*	-0.001	0.000	0.000	0.001	0.002*	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
E: All								
IHS(Community members)	0.009	-0.003	0.019*	-0.010	-0.027*	0.029**	0.022**	0.029
	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)	(0.010)	(0.019)
President/secretary in office > 3 yrs	-0.018	-0.005	0.014	0.018	0.040	0.058	0.006	-0.043
	(0.035)	(0.028)	(0.022)	(0.029)	(0.041)	(0.043)	(0.025)	(0.051)
Social capital index	0.049	0.113	-0.131	-0.004	-0.149	0.176	-0.094	0.090
	(0.100)	(0.085)	(0.083)	(0.083)	(0.125)	(0.117)	(0.072)	(0.167)
Leader completed secondary sch.	0.085***	-0.017	0.021	0.016	0.000	0.002	0.042**	-0.062
	(0.023)	(0.024)	(0.020)	(0.022)	(0.031)	(0.029)	(0.020)	(0.040)
Leader age	0.002*	-0.001	0.000	0.000	0.001	0.003*	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	1741	1741	1741	1741	1741	1734	1690	1646
Mean dep. var.	0.077	0.011	0.040	0.016	0.094	0.058	0.042	0.122

Notes: Regressors are the value of the difference between the leader and household report of the same measure. Positive values indicate leader values greater than household values. Standard errors are clustered at the community level and regressions are simple OLS with a constant. Only observations with all variables non-missing are included.* p <.10, ** p< .05, *** p<.01.

Appendix C Missing data

Leaders rather frequently responded that they were unsure of their responses to the indirect survey questions (Table C1). The most common problem was being able to identify if the household head had completed secondary schooling (Table C2).

Table C1: Frequency of "don't know" responses in indirect survey

Number missing	Frequency	Percent
0	1764	68.29
1	537	20.79
2	216	8.36
3	55	2.13
4	8	0.31
5	3	0.12
Total	2583	100.00

Cells show frequency and percentages for each number of missing responses.

Table C2: Proportion "don't know" responses by question

	Mean	SD	Obs
Car, don't know	0.017	0.131	2583
Wall type, don't know	0.012	0.107	2583
Television, don't know	0.128	0.334	2583
Mobile phone, don't know	0.111	0.314	2583
Secondary education, don't know	0.189	0.392	2583

Table shows proportions of missing responses for each variable in the indirect survey.

"Don't know" responses are, of course, ubiquitous in survey settings. For the majority of this paper, we used only the set of households for which leaders responded to all indirect survey questions. However, we are concerned about two issues. The first is that those about whom leaders know less may be influenced by the household's poverty level, and the second is the potential error generated by using other index constructions that allow for missing data. Regarding the first issue, Table C3 shows the correlations between having at least one missing observation, household level poverty as measured by a full household wealth index, and leader characteristics.

Table C3: Correlates of "don't know" responses, community level

	OLS	Ejido FE
Full assets index, hh survey	-0.113**	0.051
	(0.048)	(0.060)
Leader age	0.001	
	(0.001)	
Leader completed secondary sch.	-0.039	
	(0.024)	
Social capital index	-0.184*	
	(0.107)	
IHS(Community members)	0.033***	
	(0.012)	

Notes: Regressor is an indicator equal to one if there is at least one ' response in the indirect survey. The asset index is calculated from all the household data reported at the household level. Standard errors are clustered at the ejidal level and regressions are simple OLS with a constant. The OLS estimations include state fixed effects.

We note that across villages (column (1)), wealthier households are less likely to have missing information. Communities with larger numbers of members are also more likely to have "don't know" responses, while higher levels of social capital are weakly associated with fewer missing responses. Inclusion of fixed effects eliminates the correlation between misreporting and the assets index. This suggests that it is more likely that poorer villages will have more misreporting, not that poorer households within a particular village are more likely to suffer from missing responses.

Next we examine targeting error in the event that we (1) replace "don't know" responses with the average of the variable from the village level and (2) use a modified index that is weighted by the inverse of the covariances between variables, as suggested in Anderson (2008).

Simply replacing the "don't know" responses with the community and then the state average of the covariates does not substantially change the percentage of households that are correctly classified. For the simple index, the correct poverty classification decreases from 77.2% of households to to 73.2%, for the inverse proportion weighted index it increases from 68.2 to 68.9%, and for the principle components index it decreases from 75.7 to 71.4%. Using the index proposed by Anderson (2008) yields correct classifications

Table C4: Classification of poverty using household versus leader reporting

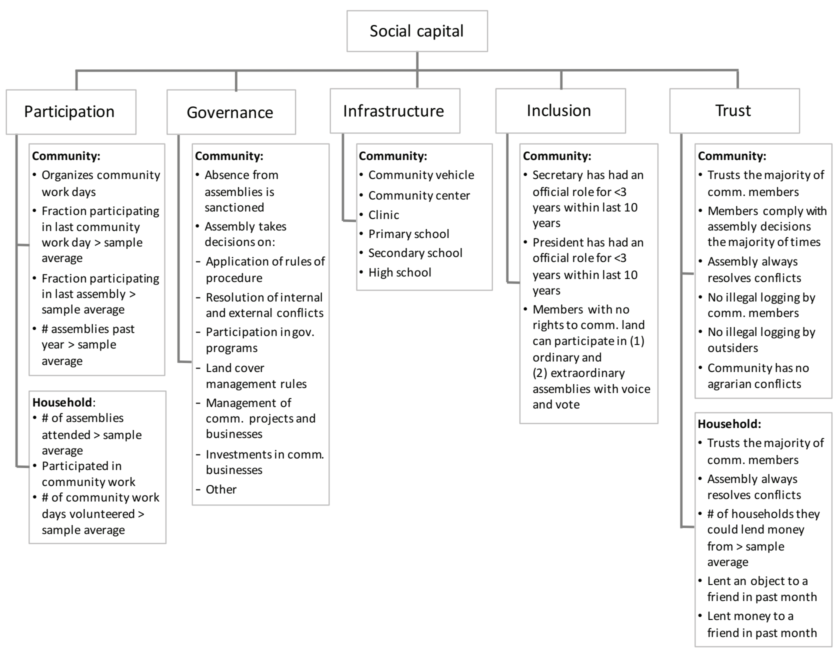
	Household responses		
	Non-poor	Poor	Total
Leader responses			
Simple index			
Non-poor	34.8	21.0	55.7
Poor	5.8	38.4	44.3
Total	40.6	59.4	100.0
	Non-poor	Poor	Total
Inverse proportion weighted index			
Non-poor	36.2	27.1	63.3
Poor	4.0	32.7	36.7
Total	40.2	59.8	100.0
	Non-poor	Poor	Total
Principle components index			
Non-poor	35.1	14.6	49.7
Poor	10.6	39.6	50.3
Total	45.7	54.3	100.0
	Non-poor	Poor	Total
Anderson index			
Non-poor	33.9	19.8	53.7
Poor	8.8	37.5	46.3
Total	42.7	57.3	100.0

Cells show percentages in each category. In all cases, the poor are those with less than the median level of the index calculated across the entire sample using the index developed from household reports. Missing values in leader observations for binary, IPW and PCA indices replaced with community mean value for that variable. The Anderson index is calculated as described in Anderson (2008) in 71.4% of cases.

Appendix D Social capital index

This paper uses the aggregate social capital index described in Alix-Garcia et al. (2018*a*). The measurement of social capital is challenging, as definitions of its content can vary depending upon discipline and context. We built upon a social capital index developed for Mexican forest communities by Merino and Martinez (2014) and modified it so that it would also be relevant for areas without a long history of forest management and reflecting a broader definition of social capital. Furthermore, we included questions about trust and infrastructure from the World Values Survey (Inglehart et al., 2014). A full list of the variables can be seen in figure D1.

Figure D1: Variables included in social capital index



This figure originally appeared in Alix-Garcia et al. (2018*a*).

In preliminary analysis we used a variety of methods common in the measurement literature to aggregate these values, including simple summation of the presence or absence of particular characteristics, principal components, polychoric principal components (which takes into account categorical variables), and inverse proportion weighting (which applies higher weight to very scarce items). Because results vary little depending upon these aggregations, we present here the simple summation approach. For this approach, we created sub-indices for each of the branches of the figure (participation, infrastructure, etc.). We calculated the proportion of positive indicators over the total possible. We then created a total social capital index by summing up the sub-indices and dividing by five. For survey questions that were not originally binary, we computed a variable equal to one if the original value was above the mean.