KALAHI-CIDSS impact evaluation: Detailed Evaluation Plan

This concept note outlines the background, motivation and design for IPA's evaluation of KALAHI-CIDSS in the Philippines, in partnership with the Millennium Challenge Corporation (MCC) and the Millennium Challenge Account Philippines (MCA-P).

1. Background on KALAHI-CIDSS

KALAHI-CIDSS: KKB[1] (also called KC) is the keystone poverty reduction program of the Government of the Republic of the Philippines (GRP), implemented by the Department of Social Welfare and Development (DSWD). The first phase of the project took place from 2003-2009 with the support of the International Bank for Reconstruction and Development – World Bank, providing roughly $100 million in lending, the GRP financing $31 million, and communities and local governments contributing $51 million, in training and grants to 4,583 barangay (villages) in 183 municipalities and the country's 42 poorest provinces (out of 81).[2] The project is moving into a second phase (called KC2) in 2011, providing grants and technical support to 362 municipalities and cities, within the original 42 provinces and six new provinces.[3] The number of municipalities to receive funding was determined by calculating 50 percent less one of the municipalities in each province.[4] KC2 is financed with renewed $59 million in loan funding from the World Bank, and a $120 million grant from the Millennium Challenge Corporation (MCC). In addition, local governments (region, municipality and/or barangay) are required to contribute 30 percent of project costs.[5]

Goals

The Kalahi-CIDSS project aims to improve welfare in rural areas by targeting communities with a poverty incidence greater than the national average, with small-scale, community driven development

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[1] The acronym/portmanteau stands for Kapit-bisig Laban sa Kahirapan (Kalahi) – Comprehensive and Integrated Delivery of Social Services (CIDSS): Kapangyarihan at Kaunlaran sa Bararangay (KKB). For more on the project, see http://kalahi.dswd.gov.ph/.

[2] To calculate poverty incidence for KC1, DSWD commissioned the Asian Pacific Policy Center (APPC) at the University of the Philippines (Diliman) to estimate poverty at the municipal level, using the Family Income and Expenditure Survey (FIES) 2000 and the Census 2000. The poorest one-fourth of municipalities received the project. To generate poverty incidence for KC2, the National Statistics Coordination Board (NCSB) used the Census 2000 and FIES 2003, taking household characteristics, socio-economic status and municipal characteristics from the census and FIES (ie, identical items from both surveys) to create a model to estimate poverty at municipal level. See NSCB publication by Albasea. IPA has these data from Dr Aniceto Orbeta in the file kc_univ0722.dta, which was used for the randomization (see more below). 873 municipalities and cities are eligible for KC2 funding.

[3] KC1 municipality recipients that still meet poverty criteria are guaranteed funding in KC2.

[4] This was originally 50 percent but later changed to 50 percent minus one (so 50 percent of municipalities minus one municipality) due to project funding constraints.

[5] Up to 40 percent of this 30 percent can be in-kind contributions.

subprojects.[6] (Projects at the barangay level are called subprojects because they are part of the overarching KC 'project.') Barangay develop proposals for infrastructure and services to meet poverty reduction goals. Proposal are evaluated by municipalities, which allocate approximately $10,000 per barangay. The process of proposing, implementing and sustaining subprojects is expected to first strengthen community participation and governance at municipal and barangay levels, and allow for communities to more adequately respond to their own needs. Second, the proposal process also may allow improved poverty targeting since communities work together to identify the poor, and propose projects that most appropriately address poverty in their areas. Finally, because the proposal process fosters cooperation and ownership, it is expected that the project will have lower operations and maintenance costs and hence improved sustainability over more traditional infrastructure projects. (Labonne and Chase, 2007). Overall, the project seeks to (a) empower communities to participate in development activities that address the needs they have identified and to manage assets sustainably, (b) build the capacity of communities to propose and manage local development projects, (c) improve the local government's response to community priorities, (d) promote accountability and use investments for community projects transparently, and (e) improve the welfare of community members by making sustainable investments in public goods.

Selection process and implementation

Eligible municipalities have on average 23.3 barangay and 30,000 residents.[7] All barangay within a beneficiary municipality receive roughly six months of training and facilitation (called 'social preparation') from DSWD facilitators in order to prepare a proposal.[8] This process includes establishing a village assembly, conducting a participatory situation analysis, and developing a village action plan, which outlines the types of poverty and challenges to be addressed by the proposed subproject. Once proposals are completed, representatives from each barangay assemble at the municipal level, at a Municipal Inter-Barangay Forum (MIBF), and prioritize subprojects for the year. Subprojects are ranked based on criteria established by the project and established by the MIBF, and financed based on the ranking and funding availability, eg, a large road project on one barangay may not receive funding if the MIBF decides that it would like to more equally distribute funds across barangay, supporting smaller projects in more barangay. Funding is allocated to priority subprojects until the annual funds are exhausted. The cost of each subproject may be more or less than $10,000, and in order to receive municipal funding, barangay must provide at least 30 percent of resources (cash and/or in kind).[9] Roughly one third of the barangay receive subprojects each year, although some barangay may receive multiple subprojects, others none.

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[6] The national average is 33 percent so municipalities with greater than or equal to 33 percent poverty incidence are eligible. Municipalities with 70 percent or greater poverty automatically receive the project, so the focus of this evaluation is municipalities between 33-69 percent poverty incidence.

[7] Population source is National Statistics Office 2007. Number of barangay per municipality ranges from 5 to 95 (Preliminary estimates).

[8] Thus barangay could see impacts, eg in the area of social capital, during the preparation phase, even if not selected to receive a grant.

[9] The formula that donors and GRP used to allocate funding to the municipalities is $10,000 per barangay, so each municipality receives $10,000 times the number of its barangay; but the MIBF is to allocate funding based on the quality of the proposals and no barangay is guaranteed funding.

On average a subproject takes six months to implement, thus the stages of preparation, funding and implementation generally take nine to twelve months and are called a cycle. The same process is repeated over three one-year cycles, with cycles two and three having a condensed social preparation phase since communities have already become familiar with the project and process.

The types of projects supported by KALAHI-CIDSS include road construction and rehabilitation, and construction of water, post-harvest, education and health infrastructure. This comprises 80 percent of projects. The remaining 20 percent are services or inputs, such as a rice drier.

MCC expects to see a 13 percent, Economic Rate of Return (ERR), generate more than $150 million in benefits, and have 5.2 million beneficiaries by 2030 (of which 39 percent earn under $2 a day in 2005 PPP dollars). These type of small infrastructure projects could generate income gains in several ways. First, and most importantly, the infrastructure itself can have economic returns that are dispersed throughout the communities benefitting from it. These returns could take the form of future earnings for better educated and/or healthier children, more immediate income gains from greater access to markets and inputs for farmers as a result of road improvements, or reduced costs for obtaining services such as water supplies. Second, poverty reduction could take place through improved targeting since projects will be tailored to community needs, and improved operation and maintenance due to community involvement. Finally, the small infrastructure projects could generate income as those employed could benefit from the income they receive while working on the project.

2. Evaluation purpose, research questions and survey content

This evaluation will serve to provide an independent assessment of the returns to MCC's investment, and to contribute to broader research about the impacts of community-driven development (CDD) projects, which to date have been mixed, and limited regarding the effects of income generation. Overall, the evaluation seeks to measure the causal impacts of the KC program on community institutions and relationships, residents' access to and use of key public goods and services, and their income and consumption levels. It also will assess whether benefits have indeed materialized (or whether the benefits that have accrued by year 5 are consistent with the *ex ante* ERR model over 20 years), and whether the projects are cost-effective.

Specifically, the evaluation will provide analysis in the following areas listed below.

Welfare Impacts
- Impacts on household income and expenditure

  - Proxied using changes in household expenditure
  - Used to calculate overall ERR and Beneficiary Analysis
  - Disaggregated by:
    - Individuals involved in construction and maintenance of KC subprojects (matched against a comparison group of individuals in control villages). This will

allow us to identify the magnitude of benefits accruing directly through the subproject construction and maintenance.
- Gender, including differential changes within households.
- Poverty, age, geography, and indigenous status and household receipt of remittance income
  - o Impacts on household poverty will be validated with subjective poverty measures
- Access, use, and impacts of key public infrastructure and services
  - o *Increased access to public infrastructure, services, and markets:* This effect can be decomposed into (a) upgraded roads and transport links to these services and markets and (b) proximity to newly constructed or upgraded services.
  - o Measured by: (i) Proximity, transport time and costs to markets, services and facilities; (ii) number of days without access to markets and facilities due to the rainy season; (iii) costs for use of public services such as health clinics, water user fees, etc.
- Agricultural impacts:
  - o Measured by total area under agriculture, cropping intensity, input use, yields and prices received for products
  - o Household surveys used as part of the KC1 evaluation captured total production by crop. We recommend recording plot-level data on the aforementioned variables, as well as who in the household is primarily responsible for managing and working on each plot.
- Health impacts:
  - o Measured by morbidity, mortality, corresponding time missed from work and medical expenditures
- Educational and early childhood impacts:
  - o Measured by day care and school attendance. IPA will also investigate the use of educational testing as part of the household questionnaire, omitting if prohibitively expensive.
- Labor force participation by men and women
  - o Measured by overall employment rates, hours per week employed, and diversity in employment
- Improved water consumption
  - o Measured by quantity of water, time and cost to obtain water, and potentially water quality.
  - o IPA will investigate the cost of testing water quality. If prohibitively expensive, the household questionnaire will include illness diary and health questions about water-related illnesses.
- Land and house prices, rents and assets

- These can reflect both increasing productivity of key assets and, potentially, an income effect that increases the price of scarce resources such as land
- social capital measures, such as

Implementation, Policy Coordination, and Governance

- Skill acquisition for management and maintenance of infrastructure
- Expansion of capacity for further community development

  - Measured by the likelihood of beneficiaries who participated in project selection to contribute in cash or in kind to operation and maintenance of facilities to KC as well as to other community-level projects. Teams will also collect data on KC and other public good user fees. This will also be captured by games-based measures discussed below.

- Whether KC is responsive to community needs

  - Measured by perceptions of what communities need/want vs what is implemented. Here we will attempt to look at barangay-level ranking or decisionmaking process used to select projects at the barangay level. The municipal surveys will include data on a ranking of proposed projects at the municipal level.[11] Does the type of subproject being proposed influence whether or not a proposal is funded?
  - Consider preferences of household respondents and local leaders/elites (The KC1 evaluation indicated that more unequal villages were more likely influenced by elites, and we will investigated whether this remains the case in KC2)

- Budget allocations at the municipal and barangay level, assessing the degree to which KC grants represent increases in total budget allocation
  - Measured through both community surveys and direct analysis of municipal and barangay budgets
  - Used to assess synergies and overlap with other community-based projects, including conditional cash transfer programs (CCTs).
- The role and efforts of facilitators
  - Considering the great influence that facilitators have on community level activities, especially the proposal, we also suggest surveying facilitators once the social preparation is well underway and barangay haven an idea about project priorities.
  - These surveys could be conducted during the April-June 2012 timeframe, potentially by phone, sms or email.
  - The surveys would capture (a) basic background details about facilitators (age, education, gender, hometown, previous experience), (b) time facilitators spend in each barangay (dates start and end), and (c) facilitators recommendations on projects they think are most useful in each barangay.

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[11] Evaluation team will also attempt to understand whether the KC project encourages provincial budget reallocation to non-beneficiary municipalities, and thus also generates benefits in control municipalities.
The team will assess the degree to which KC grants represent increases in total budget allocation at the municipal and barangay levels by examining government budgets and responses in community surveys.

Social Determinants and Impacts

In addition to these impacts on welfare that results from the subproject grants, IPA will also measure a variety of social interactions, with the goal of both observing whether there are changes in these dimensions and whether the baseline levels of these interactions determine the extent to which individuals and barangay experience these changes.

By facilitating inclusive and participatory community decision-making, the K-C project is expected to identify sub-grants that may lead to greater and broader benefits for each community than if sub-grants were determined via existing decision-making structures. It is also intended to expand these inclusive principles to intra-*barangay* relationships and collective action beyond the specific sub-grants. However, there remains considerable debate about the extent to which a CDD program can actually alter power dynamics within a community rather than reinforcing them. Gugerty and Kremer (2008), for example, find that aid projects in Kenya working with organized groups of disadvantaged members tend to see the membership of these groups expand to include individuals who are not in fact disadvantaged. Moreover, as Labonne and Chase (2010) find, the first phase of K-C increased participation in village assemblies but may have actually reduced group membership and other forms of collective action.

Given this, it will be crucial that the K-C evaluation assess two key issues: (1) The differential impacts (if any) from the program on disadvantaged populations based on pre-existing social positions, and (2) the extent to which social and power dynamics actually change as a result of the program. Studying the first issue has typically been done by comparing program impact by poor/non-poor household status, the gender of the household head, her age and her ethnic or language group. While these variables capture key dimensions along which a person might be marginalized in a society, there is considerable room for improvement here. Simply put, these measures do not always capture the degree of social isolation and exclusion that community residents may experience.

To better gauge these features, IPA's team will integrate geographical and social analysis into its survey and analytical work. IPA recommends that the survey firm utilize GPS as part of its household listing exercises in each *barangay*, enabling IPA to map households' relative proximity to community leaders' households, other households in the village, community centers and public services. Moreover, a social network module can be incorporated into the household survey to identify relatively isolated residents. While it will not be possible to construct a complete map of each village's social networks with only 30 households sampled in each village, this module will enable us to conduct relationship-level analysis.

This social network module will involve several sections:

- Perceptions of barangay leadership and participation, transparency, accountability, and inclusiveness in local decision-making and governance
- Description of collective action (*bayanihan*) experience: who initiates it, time spent, and nature of activity
- Engagement in the investment process and local governance /community participation (such as attendance at barangay assemblies or municipal development council, awareness of local government projects)

- Perceptions of power dynamics within the communities, including degree of marginalization and ability of people to participate
- Risk-sharing within the barangay (Individual's saving history; emergency borrowing sources, etc.)
- A section asking each respondent about her connections with a random subsample of other respondents along a number of key dimensions. Questions on these dimensions will cover:
  - Frequency of contact
  - Co-membership in community-based organizations and religious groups
  - Resource interactions (lending/borrowing, labor-sharing, informal insurance arrangements)
  - Trust and influence

In addition, such mapping can be combined with the qualitative work carried out in a subsample of communities, following Catley et al's (2008) techniques for eliciting how community members understand the boundaries of their "community" and who is excluded from it.

The social network module will also provide rich information to assess the extent to which social links actually change as a result of the project. For example, one set of questions will address whether a respondent trusts other village members. Typically, studies have been limited to assessing "generalized trust" of any community member at large, although a growing number are identifying trust of a list of different groups (including local government and village leaders, public service officials, neighbors, etc.). When one asks these generalized or anonymous questions, respondents are essentially aggregating their trust over all of the links in their networks and providing a summary measure. IPA suggests that, in addition to such questions, the evaluation incorporate questions on the degree to which a given respondent trusts the respondents in the other households sampled in their village, generating a bimodal variable of trust giver and trust recipient. We can then identify the change in trust across the full distribution of initial social positions, allowing us to learn, for example, whether trust in relatively disempowered households is growing, or whether these households increasingly trust more powerful members of the village. It is worth re-examining Labonne and Chase's (2010) result that generalized trust increases as a result of the K-C program. This could be because a large share of village members trusts each other more (consistent with CDD's inclusive principles), or it could be a much narrower set of members who trust each other much more intensely than they did previously (consistent with elite capture). Using a social network module that incorporates specific questions about other members of the sample can yield more precise and robust evidence on the changes in interactions within each village that K-C engenders.

Game-based measures

Self-reported social links, trust, and even public participation can serve as useful indicators of a village's ability to work cooperatively and support the provision of public goods. However, these survey responses can be subject to measurement error and bias, particularly when subjects have been informed of the goals of the intervention and may be anchored or cued toward over-reporting these

links. Using experimental games with real stakes may serve as useful validation of these survey responses, and may delineate between the potential roles of social capital in improving the returns from community grants. These channels include:

1. Improving the sustainability of investments by eliciting additional contributions toward maintenance of the K-C investments
2. Improving the quality / reducing the costs of investments by enhancing community monitoring
3. Improving the community's ability to generate contributions toward other, non-K-C public goods

To delineate these channels (particularly #2 and #3), we recommend exploring the use of the following games (please note that these are preliminary suggestions and will be refined in advance of the baseline survey):

- Variants of the "Public goods game" could be particularly useful to assess the extent of the 3rd channel. In this game, an individual decides whether to devote money they receive for participating in the game to a pot of money that shared and is typically incentivized by having the total amount devoted matched, doubled, etc.).
- Games that combine investment choice, trust, and monitoring, in which an individual decides how much the money they receive for participating in the game they would like to invest in a risky asset that generates positive expected returns but which is managed by another village member. Variants of this game could include:
    1. One in which the other (randomly selected) village member must decide how much effort to expend in generating this return, where this effort is not publicly known (simulating a scenario in which community monitoring is difficult)
    2. One in which the other village member's selected effort level is known (simulating a situation in which community monitoring is possible).

These games would be played only in a subsample of barangay and would complement and survey and qualitative evidence on the degree to which K-C leads to social linkages that foster further public investment.

3. Evaluation design, randomization, power and sample size

While KALAHI-CIDSS is a nationwide program, in order to accurately isolate the impacts of KC, this evaluation will focus on municipalities that did not receive KC1, ie, municipalities did not receive any intervention in the first phase. Recall that the Municipal Inter-Barangay Forums (MIBFs), allocate grant funding for subprojects to barangay based on barangay need and the quality of proposals. Even though the choice of barangay by the MIBFs is intentional, municipality selection is random, among eligible municipalities. Random selection was chosen in the interest of fairness and transparency, because there are generally a greater number of eligible municipalities than the province has funding for. Random selection among eligible municipalities was conducted by DSWD in eleven different public lotteries

throughout the country from 23 May to 30 June 2011.[13] (See details of selection events below, and a map of the events in an Appendix.)

The evaluation design team was tasked with deciding a) the number of municipalities needed for treatment and control groups, b) how the municipality randomization would take place, and c) number of households necessary for quantitative and qualitative baseline survey interviews and location of those households (ie, in which barangay).[14] Sections below address these questions.

Eligibility for lotteries/municipal selection events

As mentioned above, project eligibility is determined by poverty. At the provincial level, KC targets the 48 poorest provinces, 42 of which participated in KC1. At the municipal level, municipalities with 70 percent poverty incidence or above automatically receive the project, and municipalities with below 33 percent poverty incidence automatically do not receive the project; thus the evaluation will focus on municipalities with between 33-69 percent poverty incidence.

Recall that a province receives funding for half of the municipalities in the province minus one municipality, eg, if there are 25 municipalities, half would be 12 (rounding down) municipalities, and thus 11 municipalities would receive funding. So, for each province, the number of municipalities that enter a random draw to receive funding is the number determined by the 50 percent minus one rule, minus the number of municipalities with 70 percent poverty incidence or greater, which automatically receive the project. Thus, the probably of being selected for KC differs by province. Municipalities also do not enter the randomization if they received KC1 funding. Using the example above, if 11 municipalities are to receive funding, yet three of these municipalities have 70 percent or greater poverty incidence, only eight will enter the randomization. Given the combination of the 50 percent minus one and the poverty criteria, 26 provinces on Luzon, Visayas and Mindanao islands[15] were home to municipalities eligible for random selection. The other 22 provinces did not participate in the randomization because they were home to municipalities already part of KC1 and/or had sufficient poor municipalities such that all municipalities were guaranteed funding.

One other eligibility criterion is municipal mayors expressing interest, and the mayor or his/her representative being present at the municipal selection event for each province. Again using the example above, if 11 municipalities are to receive funding, and three are automatic recipients based on poverty, yet another two mayors do not attend the selection event, then only six municipalities would enter the randomization.

To recap eligibility criteria for randomization, a municipality must be/have

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[13] Locations were Kalibo, Guimaras, Dumaguete, Tacloban, Palawan, Abra, Masbate, Naga, Surigao, Pagadian, and Davao.

[14] Dr Aniceto Orbeta and MCC were responsible for task a and part of task b. Much of the section about power for the randomization and comes from Dr Orbeta's MCC-Report-Complete.pdf and discussions with Dr Orbeta. The IPA team is responsible for task c.

[15] The administrative hierarchy in the Philippines goes from island -> region -> province -> municipality -> barangay.

1. Between 33-69 percent poverty incidence
2. Mayor present at provincial selection event
3. Located in a province in which guaranteed municipalities have not been allocated all of KC funding

Power calculations/sample size

MCC's economic justification for KC was based on an eight percent change in income, and thus the design team wanted to ensure that at a minimum that level of income change was detectable at 95 percent significance and 80 percent power. This figure largely dominated the power calculations, but other variables, as mentioned below, were also considered. Dr Aniceto Orbeta, a senior research fellow at the Philippine Institute for Development Studies who was responsible for the power calculations, used a change in 0.2 standard deviations as the minimum detectable effect size for the other variables.

Data sources used for the power calculations included the baseline and midline data for KC1, [16] Family Income and Expenditures Survey (FIES), and Labor Force Survey (LFS).[17] The means and standard deviations of most household outcome variables were taken from FIES, LFS or survey data for KC1. When variables of interest were both available in the FIES or LFS and the survey data for KC1, the former were used because they have a larger sample and are representative at the national level.[18] Most of the trust and social capital indicators are only available from KC1 surveys. Understandably, the correlation coefficient between baseline and subsequent follow-up can only be generated from the baseline and midline surveys for KC1.

The outcome variables considered in the sample size computation include: From the FIES: total expenditure per capita, family income per capita, distance from main water source, proportion with source (safewater). From the LFS: proportion of children 6-17 years attending school and proportion of mothers in the labor force. From the KC surveys are household level and individual level outcome indicators. At the household level: proportion of households trusting others in the village, proportion of households trusting local officials, proportion attending village assembly, proportion joining barangay development planning, proportion having difficulty in fetching water and proportion within 30 minutes to post office. Individual-level indicators include, proportion who visited a health professional, and proportion of children within 30 minutes to school.

When determining the number of treatment and control municipalities, Dr Orbeta assumed a sample size of 30 households per municipality, ensuring an eight percent (positive) change in farmer income would be detectable at 95 percent significance and 80 percent power, as mentioned above.[19] Dr Orbeta

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[16] An endline survey for KC1 was recently completed, but data are not yet available.

[17] FIES, the LFS, and most surveys in the Philippines are only representative at the regional level. FIES generally takes place every three years and the LFS takes place every quarter.

[18] FIES has a sample of 40,000 households. The KC1 survey sampled 2400 households in 132 villages, 16 municipalities and four provinces.

[19] See files ssize_inc.do, prov_strat.slog, files06_power_onesided_clean.do. Dr Orbeta also experimented with 20 and 40 households per municipal cluster and determined that there was little difference in the amount of detectable income change with adding more households due to the high inter-cluster correlation among households.

considered the number of municipalities needed for a random draw among all municipalities and a scenario in which municipalities were paired on observables prior to randomization. Under both of these scenarios, he considered one and two-tailed tests. One tailed tests were justified since it was assumed that income would only increase and not decrease. Matched pairing was proposed because DSWD raised concerns that there would be insufficient municipalities meeting poverty eligibility criteria, and the fact that provinces would need to have an excess of eligible municipalities above the number they would receive funding for based on the 50 percent minus one rule, outlined above. The resulting number of required municipalities is 99 in each arm.

Several criteria have not yet been accounted for in the above design: (a) Stratification of municipal selection, likely reducing the needed sample size; (b) clustered survey sampling at the barangay level, likely increasing the sample size by introducing additional intra-cluster correlation; (c) the likelihood that not all barangay in a treatment municipality will receive subproject grants, thereby increasing the variability of returns within a treatment municipality. The net effect of these features on the sample size required per municipality is ambiguous, and IPA is presently revising these estimates. Below, we detail the stratified municipal selection process; in the subsequent section, we discuss the issues related to survey clustering and variance of effects at the barangay level.

There were 313 eligible municipalities, so if the randomization happened at the national level, there would be 156 (+1) eligible pairs. But because there is only funding for 110 municipalities, and DSWD decided to conduct the randomization at the provincial level, there are only 102 potential pairs due to 'singlets.' Singlets are the eight municipalities that are drawn from the controls because half the number of eligible municipalities was less than the number of municipalities allocated to province by KC. For example, if there were eight eligible municipalities, this would make four pairs. But if there were six beneficiary municipalities, then the random draw would happen among the four pairs, choosing four treatment and four control municipalities. In order to choose the fifth and sixth beneficiaries, DSWD would randomly draw two municipalities from the control group, sacrificing two pairs (see step g, iii below).

Randomization process: what happened at the municipal selection events

Here we detail step-by-step how the randomization took place in eleven locations from 23 May to 30 June 2011.

- IPA matched pairs of municipalities

As noted in the table above, there were 26 provinces with 313 municipalities that were candidates for KALAHI-CIDSS, and participated in the randomization. Randomization was done by province, with several provinces attending the one selection event at the same time (but not randomized together). The selection events happened in eleven different locations to minimize travel distance and time for municipal mayors or their representatives.

Municipalities within each province were matched on four variables: poverty Incidence, population, land area, number of barangay.[22] Municipal poverty incidence was naturally included as this is a key variable in project eligibility. The number of barangay was used to help balance the pairings since this is the unit of intervention, ie, grants are made at the barangay level. Population and municipality land area were included because they are factors in determining the Internal Revenue Allotment (IRA) of a municipality, which largely determines the financial resources available to the local government unit (LGU), and affects counterpart contributions (World Bank 2005).

Matching took place at the lottery sites and not beforehand because only municipalities that had mayors or authorized representatives present were eligible for the lottery. Thus, IPA conducted the matching only after all invited municipal mayors had been accounted for and deemed eligible by DSWD. For example, if there were 15 mayors eligible to receive funding but only 12 attended, then IPA would only match six pairs.

- DSWD randomly selected treatment and control municipalities by province, according to the following steps
  a. At the beginning of the randomization, DSWD revealed the spreadsheet generated by IPA to all participants, indicating the municipal pairings
  b. Mayors were called up to the front of the conference room by pair. (This was usually two mayors but in a number of cases, three mayors were called if there were an odd number of municipalities, see below.)
  c. Each mayor placed names of his/her municipality in plastic eggs, and put eggs to the side.
  d. Each mayor selected one lottery ping-pong ball marked A or B from an opaque container.
  e. There were two empty buckets. Each mayor put his/her egg in bucket marked A or B, corresponding to the ball s/he drew (see Figure below).
  f. Facilitators repeated steps above until all mayors had placed their eggs in buckets.
  g. Provincial governor chose one ball marked A or B, from the opaque container. (There were just two balls in the container.) The ball selected indicated the bucket the treatment municipalities were drawn from. The number of municipalities assigned to treatment and control status depended on the following scenarios
     i. If half the number of eligible municipalities was equal to the number of municipalities allocated to province by KC (based on the 50 percent minus 1), ie the number of pairs equaled the number of KC beneficiary municpalities, then all municipalities in the randomly-selected bucket became treatment municipalities, while matched counterparts in the other bucket were the comparison group.

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[22] See match_prov_data.do and matchprov.ado with data files for each municipality, prepared by Dr Orbeta. The program randomly assigned municipalities to two groups, and then found the closest matches across the two groups using mahalanobis distance matrix.

ii. If half the number of eligible municipalities > the number of municipalities allocated to province by KC, DSWD facilitators randomly selected plastic eggs from treatment bucket (after shaking bucket), until reaching the number of municipalities allocated to province by KC.

iii. If half the number of eligible municipalities < the number of municipalities allocated to province by KC, DSWD facilitators followed step g above, ie selected one bucket whose municipalities were allocated to the treatment group. However, since there was funding for more municipalities than those in the treatment group, DSWD or the provincial governor randomly drew plastic eggs from the control bucket until reaching the number of beneficiary municipalities. These municipalities were called 'singlets.'

iv. If the number of eligible municipalities was an odd number such that there were not an even number of pairs, then all pairs followed process above, and one group of three drew from a container with four ping-pong balls, two marked A and two marked B. (This odd-numbered strategy could be applied to all scenarios above.)[23] The matching program randomly chose one municipality to be paired with two others. In order to conduct the pairing, IPA calculated the average poverty incidence of each pair, and matched the odd municipality with the pair that had the most similar poverty incidence. Thus in the case of an odd number of municipalities, the last municipality was only matched on poverty incidence, and not the other three variables.[24]

Because the final selection event was only completed on June 30th, IPA has not yet obtained the final, verified data on the results of the selection events. Once these are available, IPA will provide tables detailing the attendance of eligible municipality mayors at the selection events, the pairing of these municipalities, and the number of pairs lost because beneficiary municipalities were drawn from controls. IPA will also provide tables comparing treatment and control municipalities in terms of their populations, poverty incidence, number of barangay and population densities.

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[23] This means that it is possible that two of the three triplets will be treated, one without a control. There was one municipality, Guimaras, which only had three eligible municipalities and one selected for funding, so all three eggs were placed in a bucket and the treatment was randomly selected from the bucket.

[24] This strategy was chosen because of the way the pairs were generated. As discussed above, the fact that the municipalities were randomly assigned to two groups and then matched made it impossible to match a third municipality on all four variables after all the other municipalities were matched, ie, changing the seed would generate totally different pairings.

Figure 1: Random selection

Survey sampling

Given that the sample will cover 204 municipalities across three islands, and that municipalities have on average 23 number of barangay and 30,000 people, there are three major considerations with drawing a random sample of households at the municipal level as originally envisioned in the design above.

First, and most importantly, not all barangay within a municipality receive grants because grants are made on a competitive basis and allocated by the MIBF, as discussed above. We know from the KC1 evaluation that by midline (three years after the baseline which took place in September 2003 and after two funding cycles) only one third of sample barangay had received subproject funding; by endline this figure was 65 percent (preliminary estimate, to be confirmed).[25] The baseline survey will need to take place before grants are allocated, expected by July 2012. (Ideally the baseline will take place prior to social preparation which happens municipality-wide and is expected to commence January 2012.) As a result, it is possible in the baseline that we unknowingly sample households in *barangay* that do not receive grants. One the one hand, this may be useful if there are spillover effects across *barangay* within treatment municipalities. On the other hand, this introduces an additional source of variability in the effects at the barangay level. For example, if barangay which receive two subproject grants experience twice the effects of those that receive only one, the mean effect would remain the same (e.g., 8% change in mean household income) but the variance of this effect across barangay would increase, worsening the design effect due to the clustered sample.

A random sample at the municipality level would avert this issue but raise other concerns, primarily practical in nature. First, a potentially very dispersed sample across the municipality could be very costly for enumeration. Second, in order to draw a municipal sample, one would need a household listing of all households in a municipality. Such a listing would be quite costly and time-consuming to obtain.

Given these considerations, IPA proposes stratified clustering at the barangay level that accounts for the differential likelihood of barangay subproject grants detected in the KC1 evaluation. Several barangay characteristics were correlated with the likelihood of barangay subprojects (we discuss these options below). IPA proposes ranking all barangay within each municipality by one or more of these variables (where the ranking is associated with probability that a barangay receives a subproject grant), and sampling one barangay from the top 50% of this distribution. In the largest ½ of the treatment municipalities, a second barangay would be sampled from the bottom 50% of this distribution.

Candidates for predicting subproject location include:
1. Poverty. We know from the KC1 evaluation results that poorer barangay are more likely to receive projects. This is a major challenge as barangay-level poverty data do not currently exist. Some options include:
    a. Household level targeting database from DSWD. These data are currently being used for the conditional cash transfer program. This data does not cover the whole country, and obtaining it would require aggressive pursuing with DSWD. We would likely need to get PMT household-level data and come up with poverty incidence for barangay. If data access and confidentiality is an issue, IPA could also conduct this work in DSWD's offices.

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[25] The KC1 evaluation took place in eight treatment and eight comparison municipalities across four provinces. For more details see World Bank 2005.

b. Conduct barangay poverty ranking with municipal leaders or mayors. This would be very subjective, and would involve working with all 204 municipalities. Nonetheless, the general poverty ranking of barangay will be sufficiently accurate to predict whether they are in the bottom/top 50% of the probability distribution.

c. Community-based monitoring survey (CBMS) from Department of Interior and Local Government (DILG). IPA's discussions indicate that these data are of relatively poor quality and may be difficult to obtain.

2. Availability of counterpart funding at the barangay level. During IPA's recent field visit to Minalabac, I asked the mayor if there was any possibility of ex ante making an educated guess about which barangay would have counterpart funding. He said no – he would have to look at barangay-level budgets. May want to ask DSWD further about this.

3. Indigenous persons (IP) status. Don't know if this is available at the barangay level, maybe only relevant to 20-30 percent of barangay and likely highly correlated with poverty.

4. Prior conflict. Don't know how many municipalities this would affect.

5. Distance from municipal center. This was done in the KC1 evaluation.[27]

To the extent possible, barangays and municipalities covered in the recent FIES (2003, 2006 and 2009) will be included in the sample. This will allow for (i) more refined analyses and (ii) cross-checks of data quality.

Household listing

In order to randomly sample households within a barangay or municipality, IPA will need a roster of all households in the administrative unit. These do not currently exist, to the best of our knowledge. Best option so far is asking survey firm to aquire household roster from barangay captain (if one exists) and asking survey firm to verify the listing, but this could add at least a month to survey work.

Alternately, MCA-P could request from NSO the barangay-level listing (there was a mid-decade census in 2007 that could be used for a household listing but list might not be reliable as this is now at least four years old).

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[27] Quoted from CDD and socal capital: designing a baseline report in the Philippines. The survey used two stage stratified probability-proportional-to-size sampling to draw respondent households. First stage units are the *villages*. The villages in each selected municipality were stratified into three groups according to proximity to the *población* (municipal center). The first 1/3 in the ranking comprise stratum 1 (the villages nearest the poblacion), the next 1/3 stratum 2 and the last 1/3 comprise stratum 3. One quarter of the total number of villages were randomly selected from each stratum using probabilities proportional to size with number of households in the village as measure of size. The poblacion village was excluded from the sample. Between 6 and 12 villages were selected per municipality, adding up to a total of 132 villages in the sample (see full list of the survey areas in tables A2.7-A2.10).

4. Qualitative methodology

Justification

While quantitative surveys and behavioral measures can provide important answers as to the size of impact (or average treatment effect), a combination of qualitative and quantitative measurement strategies at all stages of research is a particularly strong strategy for measuring the impacts of CDD interventions and especially the impacts on wellbeing and social capital (Jones & Woolcock, 2007; Shaffer, Kanbur, Hang, & Aryeetey, 2008). Qualitative work is also particularly well suited to poverty analysis, addressing sensitive subjects, and uncovering unexpected findings (Chambers 2008; Narayan et al. 2000).

Goals

The qualitative evaluation will serve six key purposes:

(1) identifying and refining survey questions and indicators,

(2) identifying gender-specific concerns and opportunities,

(3) promoting an understanding of poverty, welfare, community dynamics, and social capital (the evaluation's key questions) from the perspectives of the poor,

(4) interpreting and giving context to quantitative survey results,

(5) providing insight into the project process and mechanisms underlying impacts, and

(6) engaging communities as active partners in research.

Goals (2), (3) and (5) are also goals of the quantitative research and all six goals complement the quantitative evaluation.

Themes to investigate

As Catley et al. note, "Although it is tempting to try and capture as much information about a given project as possible, there is always a risk that in doing so, you will collect too much information to effectively manage and analyze. It is better to limit the assessment to a maximum of five key questions and answer these well" (2008, p.12). This is especially true of qualitative work where we encourage discussion and elaboration and do not want to stifle respondents by trying to "get through" too many questions. Reflecting the project goals and key evaluation questions, we have three themes for the qualitative work:

1. Poverty/wellbeing (access to markets, education, labor force participation, etc.)
2. Social capital (collective action, networks, trust, conflicts and conflict management, etc.)
3. Project-related issues (community priorities, experience of the project, mechanisms, etc.)

In designing questions to address all three themes, we will pay special attention to gendered effects.

Previous qualitative work in K-C Philippines evaluations[28]

In the previous evaluation of KC1, a team conducted qualitative research in two provinces: Albay and Agusan del Sur. The provinces were selected because both are poor with marginalized populations, but different in terms of accessibility to the center, resource endowment, and ethnic diversity. In each province, the research team conducted qualitative research in one treatment and one control municipality (total of 4 municipalities) and in each municipality, they worked in five barangay (total of 20 villages). The municipalities were selected from pairs that were already designated in the quantitative study. The barangay were purposively selected to meet the following criteria: one near the municipal centre of government; one far from the centre and difficult to access; one barangay between these two; plus two barangays that are either populated by indigenous people, mainly agricultural/coastal, and/or a barangay with "notable characteristics."

The research team conducted key informant interviews at the barangay and municipality levels and assembled diverse groups for focus groups discussions. They spent 15 days in each village in order to permit observation and also drew on project documents in their analysis. According to the timeline, qualitative fieldwork took approximately 2.5 months, plus 3 weeks for data processing and 3 weeks for report writing.

The key goal was for "the qualitative data [to] allow exploration of concepts that are difficult to quantify, a deeper investigation of specific KALAHI-CIDSS processes, and how, if at all, the presence of the KALAHI-CIDSS leads to results on the ground." While there was a baseline and follow-up round, there was no qualitative baseline at year zero.

Baseline

Sample:
Our goal is not to capture a statistically significant sample of a given area. Instead, we aim, through our qualitative work, to get at a cross-section of the different situations that characterize poor areas and to capture a diverse range of circumstances. This will best help us meet our six goals above.

As such, we suggest employing purposive sampling. This would involve conducting qualitative research on each of the three islands that will receive this round of the K-C project (Visayas, Mindanao and Luzon.) We first stratify communities that are eligible to receive the K-C project (now in the K-C treatment-eligible group) at the provincial and municipality levels, along key criteria: poverty incidence, agriculture/source of livelihood, conflict history, andethnic group/indigenous population. (We are also open to further suggestions). We would randomly select municipalities from each strata. We already have some of the information for this stratification and used it to help design the randomization strategy for the project. The qualitative firm would collect further information to assist IPA in stratifying and selecting the municipalities.

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[28] This section is based on Social Development Department (2005) and Anon., *Empowering Civic Participation in Governance* (2006).

The qualitative firm would then purposively choose barangay within selected municipalities based on criteria very similar to the K-C-1 evaluation: one near the municipal centre of government; one far from the centre and difficult to access; one barangay between these two; plus two barangays that are either populated by indigenous people, mainly agricultural/coastal, predominantly Muslim, and/or a barangay with "notable characteristics." All barangay selected for the qualitative evaluation will also have been selected for the quantitative evaluation. This is to address the issue that not all barangay within treatment-eligible municipalitities actually receive grants. Grants are made on a competitive basis and allocated by the MIBF. Since the baseline survey will need to take place before grants are allocated, it is possible in the baseline that we unknowingly sample households in barangay that do not receive grants. As such, the barangay selection for the qualitative work will also need to take into consideration the weighting and measures used in the quantitative survey to endeavor to oversample barangay most likely to receive subproject funding. The simplest way to do this is to choose a sample for the quantitative survey first, then select the qualitative sample from within it.

While the stratification focuses on communities eligible to receive the K-C project (potential treatment communities), we are also interested in conducting qualitative work in comparison communities. This will allow us to approximate the counterfactual of what would have taken place without the project. During the randomization process, pairs of municipalities have already been matched on poverty incidence, population, land area, number of barangay. When we select municipality along the above strata, we would also automatically include their match. The qualitative firm would then select matches for the qualitative research at the barangay level.

Numerically, we propose the following sample:

3 islands → 27 provinces -> select 4 municipalities per province (2 treatment and 2 matched control)→ select 4 barangay per municipality (2 treatment and 2 control = qualitative work in 48 barangay

We propose that the research team spend approximately 7 days in each barangay.

We welcome comments on feasibility from all stakeholders, as well as the qualitative firm. This is double the number of communities that participated in the K-C-1 qualitative evaluation, gaining greater representativity. At the same time, we suggest spending half the number of days in each community as the previous qualitative evaluation, netting out at approximately the same amount of work and time. This could mean an increase in travel costs, yet these costs will differ depending on whether or not the same firm does the qualitative and quantitative work.

Methodology:

Focus Groups: The information gathering will primarily take place through focus group discussion and exercises. Group-based exercises "encourage debate and which generate a range of opinions" (Narayan et al. 2000), especially when participants feel comfortable with each other. Focus groups will engage in conversations around open-ended, semi-structured questions and participatory impact assessment exercises, such as ranking (see preliminary ideas below). We anticipate that focus groups will comprise pre-existing groups (such as development committees, government, cooperatives, PTAs – full list to be

elaborated), such that respondents feel comfortable speaking in front of each other. We will aim to choose pre-existing groups that represent women, youth, and the especially poor. Investigators will also assemble some groups who are not yet organized or who do not yet know each other to ensure that we speak with potentially marginalized individuals as well. Key criteria for assembling these focus groups are: people from different households, age (perhaps half older than 40 such that participants have a long-range view of the community), and gender (approximately 50-50 gender split). In assembling these groups, accessing the "invisible poor" will be particularly challenging (Krishna 2005). Some focus groups – both pre-existing and newly-created – will be segregated by gender. We aim to have 5-15 participants per focus group. While we will keep individual comments anonymous, we will collect names and contact information on all participants with the aim of contacting the same participants at baseline and later stages, thereby approximating a panel data set.

Focus group logistics: Experience from previous research suggests that ideally, investigators should interview in pairs – one to conduct the focus group, and one to write down responses (Krishna 2005). We will create standardized reporting forms. Investigators will also audio record the sessions (with participants' permission) and photograph all work product (charts, maps, etc.) We anticipate approximately two hours for each focus group discussion. We suggest that each research pair could conduct two focus groups per day and have sufficient time for carefully documenting their findings. We will aim for approximately 8 focus groups in each barangay. As per convention on previous K-C research, participants will not be paid for their participation, but snacks will be provided. A pair of researchers could thus complete this portion in four days.

Focus group instruments: We will develop a comprehensive focus group guide to address the key goals and themes. All instruments will first be drafted in English, then translated and back-translated into local languages. A non-exhaustive list of examples/possibilities follows:

Theme 1: poverty wellbeing

Example A) As part of his "stages of progress" model, Krishna asks villagers "What does a household usually do…when it climbs upward gradually from a stage of acute poverty? Which expenditures are the very first ones to be made? Which ones follow immediately after? As more money flows in, what does this household do in the second stage, in the third stage, and so on?" We could also discuss with the community where to put the poverty line. (For an example of output, see Krishna 2005, p.18). This would help us interpret the survey results and understand poverty from the perspectives of the poor.

Example B) Catley et al. suggest that community indicators of impact can be solicited by asking participants "what changes in their lives they expect to occur as a direct result of the project." (At baseline stage, this could involve asking about a potential project, following Example E.) Catley proposes asking participants to write down, and rank about 5 changes/indicators. They would also identify their gender on their paper. Then, the exercise could be repeated as a facilitated discussion. By comparing the two formats, we could assess agreement and see whose suggestions get marginalized. In parallel, during the discussion, previous K-C evaluation researchers suggest probing to uncover the deeper changes that community members anticipate. For instance, if community members say that they expect to see a road

as a direct result of the project, facilitators would then ask about the changes that participants expect to see from the road (i.e. better access to markets, easier access to higher education, etc.). Co-PI King's previous research in Rwanda suggests that facilitators should also ask about a wide range of changes, including possibly negative ones. In a community that received a new road and a number of positive associated benefits, she found that parents now requested a daycare since children, who could previously mind themselves, could now run out and be hurt on the road.

Theme 2: Social Capital

Example C) Discussion questions on key themes of social capital, as per the World Bank's existing tools (see Jones & Woolcock 2007 for concrete examples of questions), previous K-C research, and other social capital research.

Example D) We can also use the focus group discussion themselves as a behavioral indication of community relations, dynamics, and social capital. As we discuss elsewhere, while participants may tell researchers what they think we want to hear, behavioral measures are often particularly strong measures of social relations. Practically, researchers would note information about who speaks and code the type of their participation (ie. offers opinion, questions, responds to question, disagrees and agrees). Levy-Paluck and Green (2009) have developed forms for this type of behavioral measurement in focus groups. This is another opportunity for a meaningful measure, without adding additional questions. It also documents the process of decision making, which we suggested doing in our proposal to MCC.

Theme 3: Project related-issues

Example E) At baseline, we are interested in eliciting people's preferences for grant investments. Previous K-C researchers, who did not build this into earlier qualitative evaluations, suggest that the best approach to doing so may be indirect: eliciting key issues/problems in the community, without prompting with a list. Rather than asking what communities want ("solutions" such as clean drinking water) this kind of an approach would get at ("problems" such as villagers getting sick) and ensure that solutions match up with the priorities (getting sick can be solved through clean drinking water) of various groups of barangay members. We could follow up with a question about "if your barangay were given X amount of money, what would you want your village council to do?" This type of questioning could also uncover strategic bidding for sub-grants. In previous rounds of K-C evaluation, researchers found that some communities thought that they were likelier to get funding for certain kinds of projects, and therefore proposed those, even if they would not have been at the top of their needs/wants list. We need to consider if and how this duplicates the social prep process that is part of the K-C intervention itself as well as the possibility that answers will be influenced by social prep. An alternate possibility would be to integrate a short game wherein people make contributions to various types of public goods.

Materials for focus groups: The material for each barangay includes flipchart paper, markers, tacks/tapes, and a sufficient number of forms (depending on questions chosen). We also propose that interviewers have digital cameras to take photos of flipcharts (for PI team analysis and eventual reporting) and audio recorders to record focus groups.

We will also draw on existing K-C qualitative research focus group guides. We will, however, narrow the current guide, to be more systematic, while still flexible, across barangay and provinces.

48 barangay x 8 focus groups per barangay = 384 total focus groups (estimated)

Key informant interviews: This qualitative evaluation will also include interviews with key informants to generate a comprehensive understanding of the communities, and eventually, project. Key informants may include: project staff, barangay captains, barangay councilors, sitio leaders, people's organization leaders, church-based group members, mayors, parish priests, NGO workers, civic groups, individuals who can offer special insight into life, poverty, and social capital in the community (see Anon. 2005, *Qualitative Baseline Survey for the Impact Evaluation of the KALAHI-CIDSS*). As in the focus groups, we will keep individual comments anonymous, but will collect names and contact information on all interviewees with the aim of contacting the same participants at later stages.

Key informant interview logistics: We again propose that investigators work in pairs – one to conduct the interview, and one to write down responses. We anticipate approximately one to 1.5 hours for each interview. At this rate, investigators could accomplish 3-4 interviews per day with time for accurate reporting. We anticipate that approximately 10 key informant interviews be conducted in each barangay. A pair of researchers could thus complete this portion in approximately 3 days.

Key informant interview instruments: Building on the previous K-C qualitative evaluation, we will develop a comprehensive focus group guide to address the key goals and themes. All instruments will first be drafted in English, then translated and back-translated into local languages.

Key informant interview materials: Sufficient questionnaires, audio recorders.

48 barangay x 10 key informant interviews per barangay = 480 key informant interviews (estimated)

Project documentation: The qualitative research team will gather all project documents possible from municipalities and communities on sub-grant proposals, selection processes, community monitoring, etc. Documents can be scanned and/or photographed.

Piloting: Before carrying out the full study, we will select two to three communities in which to pilot the focus group discussions, key informant interviews, and investigator reporting. Piloting the study involves all of the same steps as the full scale study, but will allow us to test the methods and instruments on site.

Hiring the Research Team: Roles[29]

Trainer: IPA will participate in training the research firm that is hired in relation to both this research and general research ethics. The firm itself should also have a *trainer* to further train the team with the instruments for this qualitative research.

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[29] This section is based on Krishna (2005) and Anon., *Empowering Civic Participation in Governance* (2006, p.14-5)

Team leader: The *team leader* will oversee all research. The team leader will also ensure that the research methodology is consistently followed. (IPA will also be involved with random quality checks.) In the field, s/he should introduce the study and research team to municipal and barangay officials, gain any required permissions for research, and set up the focus groups and key informant interviews. The team leader may need to identify a village representative, such as the barangay captain, to help make contacts in the local community. The team leader will also lead the drafting of all reports and be the main liaison with the administrative team. The team leader may also need to select *senior investigators* to help with these tasks.

Data Manager: A *data manager* should be the central recipient and organizer of all data collected. This includes the investigator's data from focus groups and interviews, as well as ongoing monitoring documentation. The data manager will also supervise data entry.

Research Coordinator: The firm should select a *research coordinator* to conduct supervision and arrange schedules and transportation.

Some of these roles may be filled by the same person.

Investigators: *Investigators* will be trained in the research methodology. They will facilitate focus group discussions and conduct key informant interviews. IPA will provide standardized forms for data collection. Investigators will be Filipino nationals, fluent in the appropriate local languages, with a minimum of high school education.

Data entry and reporting
Data entry: The hired qualitative research firm will be responsible for digitizing all data. They will also be trained on Atlas-ti qualitative software (which needs to be purchased) to code the data that they collected. All original data, including coding, will be submitted to IPA for review and for use in the final qualitative report.

Reporting: The research team will prepare monthly reports in the field and regular presentations of progress. They will also prepare a final baseline report, expected to be approximately 30 pages (as per last round). IPA will provide detailed outlines for the monthly reports and final report.

Ongoing
The ideas for the ongoing qualitative research are still in discussion and we look forward to integrating these ideas with the priorities of the implementing team on the ground. It might be most appropriate to make a decision during or after the baseline.

Sample: We propose selecting a subsample (which could be quite small) of the 48 barangay selected for the qualitative evaluation to participate in creative and participatory ongoing reporting and evaluation.

Project documentation: The qualitative research team will gather all project documents from municipalities and communities on sub-grant proposals, selection processes, community monitoring, etc. Documents can be scanned and/or photographed.

Reports from social preparation facilitators: In previous research on the K-C, researchers found that the preferences of facilitators sometimes influenced proposals. We will develop a short questionnaire for facilitators to elicit their preferences, their thoughts on the process, etc. One possibility is for this to take place by telephone. We will examine and coordinate with existing questionnaires for social preparation facilitators so as not to be too demanding on facilitators. This information will also be collected and used in the quantitative evaluation portion, so this would not constitute additional work.

Participatory reporting: Engaging communities as active partners in research is both a means and an end in itself. Ongoing engagement by the community will promote an in-depth understanding of poverty, welfare, community dynamics and social capital, from the perspectives of barangay members themselves. Ongoing participatory reporting will also provide particularly strong insight into the project process and allow us to get at mechanisms underlying process successes and failures. There could also be a good opportunity to prioritize women in this type of reporting and to gain their insight on the important events unfolding in their communities. Some concrete examples of how we might proceed follow.

Since we recognize that communities already carry a heavy burden of reporting, these suggestions build upon, and may be in lieu of, some existing reporting. By conducting this participatory ongoing research, using self-reporting, in just a sub-set of communities, and continuing with traditional monitoring in the others, we could also compare how well different methods of monitoring work and offer lessons for future projects. The type of data collected from these creative types of reporting is also particularly useful to communications and public relations staff of funding agencies, government, and implementers.

Example A) We could use cell phones to obtain high quality, verifiable, and real-time information about events and changes in barangay as the project unfolds. We would select community representatives, provide them with cell phones, and give them a detailed list of the type of events to report. Events would align with the three key themes (poverty/welfare, social capital, and project-related issues). Measures might include such things as school attendance, health clinic numbers (poverty/welfare), community meetings, communal conflicts (social capital), and sub-grant proposal timing, disbursal, other projects in the community, sub-grant maintenance (project-related issues). We would look forward to discussions with DSWD and MCA-P about their current indicators and monitoring and how we could build on them through this innovative technology. Event mapping has been very successfully done, for example, in the Democratic Republic of Congo (see Columbia Center for the Study of Development Strategies, no date).

Example B) We could alternatively use digital cameras to obtain high quality, verifiable, and real-time information about events and changes in barangay as the project unfolds. We would select community representatives, provide them with cameras, and give them a detailed list of the type of events to report (details available upon request from a project in Sri Lanka). Most of the same advantages of the cell phone idea apply here as well. We would also give participants the flexibility to take photos of other things they deem important, thereby allowing us to see the community through their eyes. There are

also a host of creative photography projects ongoing around the world that could be a fascinating way to gain insight into this CDD.

Example C: Ongoing participatory research should also include reporting back to communities themselves. As Narayan et al. write, "This is an important endorsement. If the people who contributed to the study and the people who have lived in these areas all their lives believe that the studies accurately capture the problems and priorities of the poor, then why should critics living elsewhere remain skeptical?" (2000, p.4; see also Catley et al. 2008 p. 58)

Midline and Endline:

The midline and endline qualitative evaluations will mirror the baseline in terms of sample and methodology. It will take place in the same treatment and comparison barangay as the baseline, with as many of the same focus groups and key informants as possible, in order to approximate a panel data set. Some of the instruments will be the same. Others will differ given the different goals of the baseline and endline surveys. The midline and baseline qualitative evaluations will also include gathering project documentation.

5. Implementation timeline

Timeline for Quantitative Evaluation

Below, we provide preliminary timeline estimates for the first year of the quantitative evaluation implementation as discussed with MCC and MCA-P.

Baseline household and community surveys – major milestones

Task	Timeframe optimistic	Timeframe realistic	Responsible entity	Notes
Prepare TOR and budget for data collection	June - July 2011	June - July 2011	MCA-P	Support from IPA. Possible delay due to budget example availability.
Procurement process	July – Sept 2011	Aug – Dec 2011	MCA-P	
Develop framework and survey instruments (including field tests)	July - August 2011	July – Sept 2011	IPA	With support from MCA-P and DSWD. Survey firm will also need to conduct a second pilot.
Select firm	Sept 2011	Dec 2011	MCA-P	IPA as observer

Task	Timeframe optimistic	Timeframe realistic	Responsible entity	Notes
				on technical evaluation panel
Piloting, develop data entry program, translation, training	Late September 2011	Jan – Feb 2012	Survey firm	
Data collection	October – November 2011	March – April 2012	Survey firm	May miss all or most of social prep
Data input and cleaning	November – March 2012	April –July 2012	Survey firm	
Delivery of the database	April 2012	Aug 2012	Survey firm	
Baseline report/presentation	July 2012	Nov 2012	Survey firm / IPA	

For *subsequent survey rounds*, IPA and MCC have agreed on the following timeline

- Baseline in 2012 (likely March-April 2012, as discussed above)
- Midline data collection in early 2015
- Endline survey to take place post-compact, timeline determined by MCC

Regarding *soliciting feedback and dissemination*, IPA proposes

- Half-day session with DSWD to discuss evaluation objectives and survey content (eg, what key research questions are most important to DSWD, what more do they want to explore beyond the KC1 evaluation, are the indicators proposed for the household and community surveys pertinent to the project). Proposed in late July/early August
- Survey pilot with MCA-P, MCC and DSWD to allow all organizations involved to familiarize themselves with the questionnaires. The survey firm would still need to conduct an additional pilot but this would serve in questionnaire development and in further engaging evaluation stakeholders
- Participation from MCA-P, MCC and DSWD during training and fieldwork (if these organizations want to participate)

Timeline for Qualitative Evaluation

We anticipate up to 5 stages of qualitative research, to take place over the course of the project. They are summarized in the table below, and detailed in the following sections. We focus especially on providing details for the upcoming qualitative baseline.

Timing	Goals	Practical
Pre-baseline (now)	Goals (1) and (2)	Speak to project staff, government, MCC, researchers from past evaluations, etc. about strengths and

		shortcomings.
		Unfortunately, given timing, we are unable to do this pre-baseline research in communities.
Baseline (Feb-April 2012, concurrently/just after quantitative survey)[30]	Goals (1), (2), (3) and (6)	Focus group discussions
		Key informant interviews
		To take place in barangay that represent key characteristics in which we are interested. This will be a sub-section of those selected for the survey and include both treatment and comparison communities.
Ongoing (to start with qualitative baseline, or shortly thereafter)	Goals (3), (4), (5), and (6).	Gather all project documents from municipalities and communities on sub-grant proposals, selection, unfolding, etc.
		Reports from social preparation facilitators
		Participatory ongoing reporting from treatment barangay (a sub-section of those selected for baseline, midline, and endline qualitative evaluation). We will continue to discuss the possibility of using a creative methodology for this monitoring.
Midline (early 2015)	Goals (3), (4), (5), and (6).	Focus group discussions
		Key informant interviews
		To take place in the same barangay as the baseline and to approximate a panel data set.
Endline (post compact)	Goals (3), (4), (5), and (6)	Focus group discussions
		Key informant interviews
		To take place in the same barangay as the baseline and to approximate a panel data set.

[30] This depends somewhat on whether or not the same firm is hired for the quantitative and qualitative portions of the evaluation. The qualitative work itself may have an "intervention effect" in that qualitative researchers would spend approximately seven days in each selected barangay. This may constitute an intervention itself and could even be more substantial than the social prep. If we do the qualitative before the quantitative research, we would have to control for this small sample that has the extra intervention.

6. Evaluation team

The team is composed of Jean-Louis Arcand, Amanda Beatty, Ariel BenYishay, Elisabeth King, Menno Pradhan, and a to-be-hired Research Coordinator (hired internationally, based in Manila), and a local Project Assistant working part time. This team will be supported by IPA's Manila office, led by Faith McCollister, IPA's Country Director, and Megan McGuire, IPA's Deputy Country Director.

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Appendix 1: Map of Randomized Selection Events in the Philippines