Oxford Policy Management

CGP IMPACT EVALUATION

Sampling Design and Targeting Evaluation Research

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1 Overview

The quantitative analysis of Programme impact will be based on a comparison of changes ('difference in differences') in a range of indicators between households selected by the Programme's targeting process in treatment community (Group A in Table 1.1 below – the **treatment group households**) with comparable households in 'control' communities (Group B in Table 1.1 – the **control group households**). By comparing the changes in welfare indicators between control and treatment households the impact of the Programme can be assessed.

Moreover, including some non-selected households from both treatment communities (Group C in Table 1.1) and control communities (Group D in Table 1.1) in the panel sample allows analysis of how the wider community benefits from the Programme (i.e. spill-over effects) and also of how recipients' welfare compares over time relative to those households that were not selected during the targeting process (i.e. targeting analysis).

Table 1.1 Categorisation breakdown of the study population, by control/treatment and beneficiary status

Treatment / control: / **Beneficiary status:**	**Treatment EDs**	**Control EDs**
Selected	A TREATMENT GROUP *(Beneficiaries)*	B CONTROL GROUP *(Pseudo-beneficiaries)*
Not selected	C *(Non-beneficiaries)*	D *(Pseudo-non-beneficiaries)*

The following population groups can be identified:

- **Group A** – Households selected to be enrolled in the Programme in areas (EDs) selected for inclusion in the Programme. These are households with children, belonging to NISSA levels 1 and 2, and validated by the community in treatment EDs.
- **Group B** – Households in control areas (EDs) that should (in theory) have been selected to be enrolled in the Programme had the Programme operated there. These are households with children, belonging to NISSA levels 1 and 2, and validated by the community in control EDs.
- **Group C** – Households in Programme areas (EDs) that were not called to enrolment in the Programme. These include household without children, households NISSA levels 1 or 2 but non validated by the community, households NISSA levels 3, 4 or 5, in treatment EDs
- **Group D** – Households in control areas (EDs) that would not (in theory) have been called to enrolment had the Programme operated there. These include household without children,

households NISSA levels 1 or 2 but non validated by the community, households NISSA levels 3, 4 or 5, in treatment EDs

The comparison of trends in groups A and B over time provides the basis for the analysis of Programme impact. The sample included units from groups C and D to provide contextual information on the entire population in these areas, in order to assess the extent to which the Programme had selected the poorest households and examine potential spill over effects.

2 Impact Evaluation Design

In combination with community randomisation, this controlled design will enable very robust impact analysis based on difference-in-difference estimates and econometric impact analysis techniques. **The random allocation of the CGP** to a sufficient number of evaluation communities (Electoral Divisions) means by design there will be no systematic differences between treatment and control households observable and non observable characteristics, and therefore the difference-in-difference and other impact estimates will not suffer from systematic selection bias.

The opportunity to assign the Programme randomly across Electoral Divisions (EDs) arises as a consequence of the programme not having enough resources to cover the whole eligible population in all 10 community councils in the next expansion round. It is suggested that the control EDs should eventually be covered by the Programme. However this cannot happen before sufficient time has passed for there to be observable impacts amongst the beneficiary households as a result of the Programme; at least two years is recommended.

It is important to recognise that the exact interpretation of the impact results is determined by the research questions that the survey design has been set up to address, and, more specifically, to the precise variation in 'treatment' experienced between treatment and control households. For example, if it is decided to bring control group households into the Programme after two years, then the specific impact being measured is that of the effect of receiving regular cash transfers over a *two year* period versus not receiving such transfers over this period. It is also important to note that there will inevitably be issues of external validity, i.e. the extent to which the impact results can be generalised (e.g. to a fully scaled-up programme). In fact the gradually phasing of the programme implementation, and the fact that the districts to be covered by the evaluation were not chosen randomly means that the impact results are representative of the programme, as implemented in the evaluation communities, but not representative of Lesotho as a whole.

It has been agreed in the inception mission that **the units of randomisation are the Electoral Divisions (EDs)**. In this way, within the 10 evaluation CCs, half of all the EDs will be randomly selected to be covered by the programme, with the other half not benefiting.

In half the EDs the Programme will implement the targeting process and selected recipients will receive the transfer (i.e. be enrolled on the programme) and proceed to enrolment – these are referred to as the **treatment EDs**. In the other half the Programme will implement the targeting process and selected recipients who should receive the transfer but not proceed to enrolment – these are referred to as the **control EDs**.

It is important to note that the manner in which the control households (Group B) are identified has significant implications for the robustness of the impact analysis. In this case it was agreed during the inception mission that the programme would **implement the targeting process in control communities in an identical fashion to treatment communities**, which is sometimes referred to as the "perfect mimicking" approach. This process of perfect mimicking of the targeting process in control EDs provides an opportunity to compare actual beneficiaries in treatment EDs with a similarly identified group of "would-be" beneficiaries in control EDs.

The programme will be allocated to EDs on the basis of a procedure for **random assignment**, while the evaluation will be conducted on a sample of EDs and households selected through a **random sampling** process.

3 Targeting Evaluation Design

An essential component of the impact evaluation of the CGP is a review of the effectiveness of the targeting. This will aim to answer questions such as those shown in Box 1.

Box 1. Key questions—evaluating targeting performance

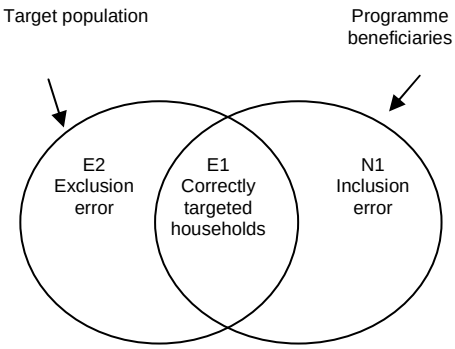
- Do the programme's targeting criteria and application process effectively target the poorest households?
- Are the poverty and other criteria being appropriately applied in the selection process?
  - What proportion of households that meet the eligibility criteria are benefiting from the programme?
  - What proportion of recipient households do not in fact meet the eligibility criteria?
- Is the net effect that the programme is successful in selecting the poorest households?

The targeting analysis will be based on comparing those households that were selected for the programme (Group A and B), with those that were not selected (Group C and D). Note that since the targeting process is being implemented in both treatment and control areas, Group B and Group D households can be included in the targeting analysis.

Leakage and coverage

The evaluation of targeting performance will aim to measure errors of inclusion in the programme (leakage) and errors of exclusion (undercoverage) (Figure 3.1). Errors of inclusion occur when cash transfers are received by households that are not in the target population. A standard measure of programme leakage is the proportion of recipient households that are not part of the target population. Conversely, errors of exclusion are generally measured as the proportion of the target population that are eligible to receive transfers but do not receive them (low coverage implies high errors of exclusion). Both types of error can occur at the design stage or during the implementation of the eligibility criteria.

Figure 3.1 Inclusion and exclusion errors



E1 = eligible beneficiaries
E2 = eligible non-beneficiaries
N1 = non-eligible beneficiaries

Inclusion error *(leakage)* is defined as the proportion of beneficiary households that are not in the target population

- **Inclusion error = N1/(E1+N1)**

Exclusion error *(under-coverage)* is defined as the proportion of the target population that are not benefiting from the programme

- **Exclusion error = E2/(E1+E2)**

Source: OPM.

Design and implementation impacts

In principle one could decompose targeting problems into design and implementation errors. Well designed targeting criteria (e.g. PMT model) are reflected in high potential coverage of poor households and low design leakage. Implementation issues relate to how successfully the eligibility

criteria are applied in practice. Targeting errors in implementation will occur if households are accepted onto the programme in error, households remain in the programme in error, or households are not part of the programme even though they are eligible.

As previously mentioned the targeting process in use for the CGP is complex and entails a number of subsequent validation steps, combining a PMT approach and community targeting . Depending of the final distribution of groups Cs and Ds across categories, the targeting analysis will allow to disentangle the effectiveness of each of the targeting steps in focussing to the most needy households and the magnitude of inclusion and exclusion errors occurring at each stage.

A few issues are worth mentioning in the specific context of the CGP:

- The programme is already aware of the inclusion and exclusion errors that depend from the design of the PMT. This is due to methodological challenges experienced in the development of the PMT model and in turn to the bad quality and timeframe of the data (HBS 2002) used to construct the model. For this reason it will be more important to address a) the relevance of the community validation stage in mitigating the PMT targeting bias. Because of the way the two methods are sequenced, community validation can only contribute to reducing inclusion errors.
- Based on the interest for understanding the interaction between proxy means test targeting approach and community validation, and based on the presumption that receiving a household list that was categorised, i.e. each household was assigned to one of the five NISSA categories, might influence the community selection, during the inception mission it was proposed that across all the EDs in the 10 evaluation CCs, half will have Community Validation using a categorised list (anchored to NISSA levels), whereas the other half will have Community Validation based on a list with no categories specified and with households ordered randomly (not anchored).
- As the sample will be drawn from the NISSA and community validation lists produced by the programme implementation team during the targeting stage, it will not possible to estimate the size of implementation errors, as compared to design errors. The analysis targeting analysis will be drawn on the targeting process as it is implemented, and will assess the quality of its design and implementation jointly.
- One contributing factor for exclusion of eligible households is that at least one household member must be listed as part of the NISSA data collection exercise**.** This may lead to exclusion of marginalised groups who may be less likely to become aware of the programme. As the sampling framework for the study is constituted by NISSA lists it will not be possible ot assess quantitatively the size of exclusion errors at this first stage.

If the targeting process is effective at targeting the poorest households, then poverty rates should be higher amongst selected households. The analysis will also seek to disentangle the effect of each targeting stage in terms of poverty targeting. Such an analysis could take the form of the following tables (poverty estimates are hypothetical, and are for illustrative purposes only).

Table 3.2 Dummy Table - Proportion of households below the national poverty line

Population group	HHs with children	All HHs	Data source	Comments
Lesotho overall			National data	Targeting effect of focussing on children – in whole country
Five CGP districts			National data	Targeting effect of focussing on children – in study districts
Study population (10 IE CCs):				
All households				CANNOT ESTIMATE – investigate in qual study
All NISSA households			OPM data & predicted cons exp	Assuming not many households missed from NISSA lists, then comparing this with national poverty rates tells you the geographical breakdown.
All households missed from the NISSA lists				CANNOT ESTIMATE – investigate in qual study

Table 3.3 Dummy Table - Proportion of households below the relative (within sample) poverty line

Population group	HHs with children	All HHs	Comments
All NISSA households			Benchmark
All NISSA1 & NISSA2 households that pass filters			Effect of PMT and filters
All NISSA1 & NISSA2 households (that pass filters) confirmed by community as poor			Effect of community validation
All NISSA1 & NISSA2 households (that pass filters) rejected by community as being poor			Effect of community validation
All NISSA1 & NISSA2 households (that pass filters) confirmed by community as poor and prioritised			Effect of prioritisation
All selected households (Y,Y)			Effect of quotas & prioritisation
All eligible beneficiary households			This tells you whether leakage to non-eligibles is improving or worsening targeting effectiveness
All households confirmed by community as poor			

4 Sample Design

The core of the quantitative survey will be a household panel survey. This survey will be used to assess both the programme's targeting performance and its impact. This would require at least two rounds of the survey: (1) the baseline (implemented just after targeting and before the recipients receive their first payment); (2) the follow-up (interviewing exactly the same households as at baseline).

The survey for the evaluation will be collected in a **sub-sample of treatment and control EDs**. Those EDs that are covered by the evaluation are referred to as the **evaluation EDs**. The households in the treatment communities that are selected for the programme are referred to as the **treatment group**. These households are beneficiaries of the programme. In control communities a set of households that are comparable to the treatment group will be identified. These are referred to as the **control group**. These households are *exactly* the ones who would have been selected by the programme had it been operating in the control community because the programme will implement the targeting process in control communities (but not actually enrol and provide transfers to the selected households).

Not all households in the treatment communities who are selected for the programme (i.e. the **treatment group**) will be interviewed as part of the quantitative survey – those interviewed are referred to as the **treatment sample**. Similarly, not all households in the control communities who are identified as being comparable to the treatment group (i.e. the **control group**) will be interviewed as part of the quantitative survey – those interviewed are referred to as the **control sample**.

Table 4.4 Sampling Framework. Distribution of EDs, Villages and Households.

District	Community Council	Number of EDs	Number of Villages	Number of Households	Number of Households Called to Enrolment	Proportion called to Enrolment
Maseru	Quiloane	8	55	2,949	614	20.8%
	Rapoleboea	9	38	791	316	39.9%
Leribe	Malaoaneng	9	38	1,318	248	18.8%
	Litjotjela	11	70	3,316	550	16.6%
Berea	Tebe-Tebe	10	57	2,940	873	29.7%
	Kanana	11	55	3,433	518	15.1%
Mafeteng	Metsi-Maholo	11	90	3,513	708	20.2%
	Malakeng	9	62	1,347	477	35.4%
Qacha's Nek	White-Hill	9	32	529	79	14.9%
	Mosenekeng	9	11	469	192	40.9%
Total		**96**	**508**	**20,605**	**4,575**	**21.8%**

Source: CGP MIS Data – NISSA dataset

The sample will be drawn from the list of households that has been collected in early 2011 by the Programme in the 10 community councils as a first step of the targeting process for the calculation of the PMT scores (NISSA dataset). It represents a census of all households living in the 10 community councils of interest for the study and contains 20,605 households living in 508 villages across 96 EDs.

Ayala (2011) report that, according to the latest census run by the Lesotho Bureau of Statistics, the expected population living in the 10 community councils was 30,603, hence indicating that the MIS (i.e. our sampling frame) covered on average 67% of the target population. There are several explications for this inconsistency. First, the boundaries of some Community Councils have been redesigned since the latest census, leading to a smaller population actually living in the 10 community councils. This is especially the case in Maseru, where the MIS covers just slightly more than 50% of the number of households registered in the latest Census. Second, some households may have actually relocated, moved or extinguished. Third, some households whose dwelling was found in the field, were not available for an interview at the time the MIS-NISSA census was collected (11%).

Moreover, the MIS-NISSA census may not be fully comprehensive, as some households may have been only temporarily unavailable at the time of the NISSA data collection, may have refused the interview, or parts of villages/EDs may have been missed by enumerators. This may constitute an original source of exclusion error in the CGP targeting, as well as limit the representativeness of the evaluation sample overall (as the MIS represents our sampling framework).

The sample drawn for the evaluation will only be representative of the population included in the MIS-NISSA dataset, hence in any case of all households called to enrolment, as GCP beneficiaries are selected from the MIS dataset. The magnitude of potential targeting exclusion errors due to non-comprehensiveness of the sampling framework will be assessed as part of the qualitative targeting analysis (plus an attempt could be done to use information collected on networks in the household questionnaire for this purpose).

It must also be noted that, while the CGP targeting process was originally designed with the expectation of getting about 10,000 eligible households (NISSA 1 or 2 and validated) across the 10 Community Councils (half of which – 5,000 – would be called to enrolment in treatment EDs), the final number of potential beneficiaries (identified in the dataset after administering the PMT and recording the outcome of the community validation process) was roughly half of what planned: 4,575 households across the 10 community councils, meaning an expected 2,300 in the EDs that will be randomly allocated to treatment. This low coverage, coupled with the fact that some of the EDs and Villages have a remarkably low number of households to start with, creates some challenges in finding beneficiary households.

A multi-stage stratified random cluster sample design will be adopted. The processes of random assignment and random sampling are distinct and independent, though interlinked in practice. The steps are described below:

1. Firstly **all** EDs (Primary Sampling Unit – PSU) were paired based on a range of characteristics such that each ED is paired with another ED (possibly in the same CC) which is similar across a range of characteristics. Since there are 96 EDs in total, 48 pairs pairings were constructed. (Random Assignment, Step 1)
2. Once all pairs have been constructed, **40 pairs** have been randomly selected to be covered by the evaluation survey.
3. Within each selected ED, **2 villages (or clusters or villages)** have been selected Secondary Sampling Units (SSU) (villages or clusters of villages)
4. In every cluster a random sample of **20 households** (10 potentially called to enrolment and 10 potentially non-called to enrolment) will be randomly selected and interviewed. (Random Sampling, Step 3)
5. After the survey data has been collected in all evaluation EDs, **public meetings** will be organized (possibly at the community council level) where a **lottery** will be held to assign the elements of each pairs (both sampled and non-sampled) to either treatment or control. Only at

this stage it will be known which EDs are going to be covered first (treatment group) and which are going to be delayed (control group). (Random Assignment, Step 2)

The original sampling strategy is summarised in Table 4.5 below.

Table 4.5 Original Sampling strategy - summary

	Treatment	Control	Total
Districts	5	5	5
Community councils per district	2	2	2
Total community councils	10	10	10
Total EDs	48	48	96 (48 pairs)
Selected EDs	40	40	80 (40 pairs)
Selected SSUs (villages or clusters of villages)	80	80	160
HHs per ED			
Selected households	20	20	
Non-selected	20	20	
Total	40	40	
HHs per Cluster			
Selected households	10	10	
Non-selected	10	10	
Total	20	20	
Theoretical target sample size (1)			
Selected households	800	800	1,600
Non-selected	800	800	1.600
Total	1,600	1,600	3,200 (1)

Notes: (1) In practice, because of the small number of households called to enrolment overall, the expected sample size is smaller than what indicated here, as shown below.

We now proceed to provide further detail of how each of the sampling stages described before has been undertaken.

Step 1. Pairing Electoral Divisions

The pairing was undertaken on the basis of a multidimensional measure of distance[1] constructed on the basis of ED aggregate level information that was obtained from the NISSA dataset. The matching criteria included a series of characteristics regarding population, household demographics, assets and main socioeconomic traits.[2] Each pair is composed of two EDs, the

[1] The Mahalanobis distance was calculated using the Stata routine mahascores

[2] ED level characteristics: number of households, number of households called to enrolment, number of villages. Household level characteristics, averaged at ED level: household size, number of children 012, number of disabled household members, self-reported food security, number of meals, quality of heating, quality of roof, availability of toilet, number of rooms per capita, number of TVs, number of cell phones, Tropical Livestock Units, number of poultry, access to ARV treatment.

most similar on the basis of available information. This is to ensure balance in covariates across treatment and control EDs.[3]

First EDs were paired with each other whiten the same Community Council. This was done to facilitate the implementation of public lotteries in which the random assignment would take place. As most electoral divisions contained an odd number of elements, the remaining unpaired EDs were paired with each other across Community Council and District boundaries.

Step 2. Selection of Pairs of Electoral Divisions

Out of the 48 pairs constructed, 40 were selected randomly with probability proportional to size (PPS) of the total population (number of households) of the two elements (EDs) of the pair. In order to ensure that a fixed number of EDs (80) is selected in the end, 30 pairs whose probability of being selected was higher than a certain threshold were selected with certainty (self-selected). Out of the remaining 18 pairs, 10 were selected with PPS.

The outcome of this first selection stage is reported in the table below.

Table 4.6 Sample of Electoral Divisions (PSU)

District	Community Council	**Number of EDs**	**Selected EDs**
Maseru	Quiloane	8	8
	Rapoleboea	9	7
Leribe	Malaoaneng	9	7
	Litjotjela	11	11
Berea	Tebe-Tebe	10	10
	Kanana	11	11
Mafeteng	Metsi-Maholo	11	11
	Malakeng	9	9
Qacha's Nek	White-Hill	9	4
	Mosenekeng	9	2
Total		**96**	**80**

Note that in 22 out of the 96 EDs (12 of which in the Qacha's Nek District) there are less than 20 household who could be called to enrolment; 10 of them were randomly selected for the evaluation sample, leading to some losses with respect to the original intended sample size.

Step 3. Construction an Selection of Clusters of Villages

[3] At every step of the matching algorithm all possible pairs were formed from all (remaining) EDs, and the pair with the minimum multidimensional distance was selected and extracted from the universe before the next iteration.

Based on the information in the NISSA dataset, each ED is composed on average by 5 to 6 villages, but there is significant variation, as in some cases all households from one ED are registered in the same village, while at its maximum an ED can contain as many as 20 villages. The size of villages also varies significantly across community councils. The population is expected to be highly dispersed in the rural areas where fieldwork is going to take place. For this reason, and in order to facilitate the logistic implementation of fieldwork, it has been decided to include an additional sampling stage in the design, by randomly sampling secondary sampling units (SSUs) within each ED, before drawing a random sample of households.

SSUs are defined as villages or clusters of villages on the basis of geographical proximity. Clusters of villages are constructed using GPS coordinates[4]. The algorithm used to construct clusters of villages works as follows:

- Villages in which the number of potential beneficiaries is 0 are excluded from the evaluation sample. While this means that overall the sample is not representative of all the population living in the 10 community council, this does not constitute a threat to the external validity of the evaluation sample, as all potential beneficiary households are maintained in the sampling framework. As for the analysis of spill over effects, the sample is representative of all households living in villages where there is at least one potential beneficiary: i.e. all households who are potentially subject to within village spill over effects.

- In each ED the remaining villages are first sorted according to their size, from small to big.

- As soon as a village is found whose population of potential beneficiaries and non beneficiaries is respectively smaller than 12[5], the village is clustered with its nearest neighbouring village in an iterative way until the threshold is hit. Villages in newly formed clusters are excluded from the initial sorted list

- The same process is repeated, proceeding along the sorted list, until the total population of potential beneficiaries or non beneficiaries in the residual group of villages is smaller than the threshold.

- If there is a residual group of villages, with total population of potential beneficiaries or non beneficiaries smaller than the threshold, each of them is separately added to the cluster where the nearest neighbouring village is contained

Once clusters have been constructed in the way described above, 2 clusters are selected in each electoral division, with probability proportional to size (number of households in the cluster). The result is that some clusters with a large population are randomly selected twice, so the total number of clusters included in the evaluation is 127 rather than 160 (see table below).

[4] GPS coordinates were collected for each household as part of the NISSA data collection effort. Average village level coordinates have been calculated, which should represent the midpoint around which most of the villagers' houses gravitate.

[5] While 10 potential beneficiaries and 10 potential non beneficiaries will be selected in each cluster of villages, clusters are designed in such a way to allow for a minimum buffer of replacements.

Table 4.7 Sample of Cluster of Villages (SSU)

District	Community Council	Number of Villages	Number of Excluded Villages	Number of Clusters	Selected EDs	Selected SSUs	Selected Clusters
Maseru	Quiloane	55	2	25	8	16	14
	Rapoleboea	38	7	14	7	14	10
Leribe	Malaoaneng	38	4	14	7	14	11
	Litjotjela	70	9	24	11	22	17
Berea	Tebe-Tebe	57	1	29	10	20	17
	Kanana	55	3	25	11	22	18
Mafeteng	Metsi-Maholo	90	8	38	11	22	19
	Malakeng	62	4	22	9	18	15
Qacha's Nek	White-Hill	32	7	9	4	8	4
	Mosenekeng	11	1	9	2	4	2
Total		**508**	**46**	**209**	**80**	**160**	**127**

Step 4. Selection of Housheolds

In each selected cluster, a stratified sample of potential beneficiaries and non beneficiaries was drawn. A fixed number of potential beneficiaries and non beneficiaries was randomly selected from the household list contained in the NISSA census. The fixed target was defined as follows: 10 and 10 when the cluster is selected once, and 20 and 20 when the cluster is selected twice. There wasn't any further stratification criteria for the group of non-beneficiaries.

Because of the small size of some of the EDs and cluster selected, in 10 clusters it was not possible to sample the number of potential beneficiaries and non beneficiaries that would be required by design. This leads to a total reduction in sample size from the original target of 3,200 to the achievable target of 3,102.

The intended evaluation survey sample sizes are presented in Table 4.8 below (with the letters in the cells matching groups A–D as listed).

Table 4.8 Intended sample size, by population group

Selected to be a recipient/control household	Area		Total
	Programme	Control	
Called to Enrolment	766 [A]	765 [B]	1,531
Not Called to Enrolment	786 [C]	785 [D]	1,571
Total	**1,532**	**1,570**	**3,102**

Notes: Originally the intended total sample size agreed with the Programme was 3,200, broken down as follows: A – 800; B – 800; C – 800; D – 800. However, due to the small size of some of the Primary and Secondary sampling units, some observation were lost while drawing the sample. See more below on this point.

The breakdown between group A and B and between group C and D is only indicative at this stage, as it is yet not know which EDs will be randomly allocated to treatment and which to control.

Table 4.9 Intended sample size, by community council

District	Community Council	Called to Enrolment	Non-Called to Enrolment	Total
		Group A/B	Group C/D	
Maseru	Quiloane	160	160	320
	Rapoleboea	132	132	264
Leribe	Malaoaneng	133	140	273
	Litjotjela	212	220	432
Berea	Tebe-Tebe	200	200	400
	Kanana	220	220	440
Mafeteng	Metsi-Maholo	218	220	438
	Malakeng	177	180	357
Qacha's Neck	White-Hill	39	71	110
	Mosenekeng	40	28	68
	Total	**1531**	**1571**	**3102**

As some of the basic characteristics of the households are available in the NISSA/MIS, including household demographics, the NISSA level and the outcome of the community validation process, it is possible to know in advance the expected distribution of the sample across groups that will be relevant for the targeting analysis. The Table below presents this information.

Table 4.10 Intended sample size, by type of household

		Treatment and Control Areas
Beneficiaries	Selected - Called	1,531
Non Beneficiaries, with children	NISSA 12 - Non validated	530
Non Beneficiaries, with children	NISSA 345	590
Non Beneficiaries	Without Children	437
Non Beneficiaries	Missing Data for NISSA PMT	14
Total		**3,102**

Replacements

Inevitably, not all sampled households will be identified and/or interviewed. Some households will not be found, whilst others will refuse to be interviewed. These households will be replaced from a randomly selected replacement list in each stratum and village (when possible) or at least within the same cluster or EDs. Compounded by the fact that some replacement households themselves will have to be replaced, the final sample sizes will therefore be slightly lower than intended.