RENOVATION OF HEALTH CENTERS AND OUTPATIENT DEPARTMENTS:

A Revision of the Evaluation Design, Hypothesis, Outcomes and Data Collection Needs

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I. Introduction

The implementation of the health sector activities under MCC compact has deviated significantly from the original plan. Construction and equipment of health centers (HCs) have experienced several delays that have compromised the experimental evaluation design for HCs. These delays have been driven by procurement problems, contractor learning curve, and climate constraints. In addition to delays in construction, HCs have to be equipped to become functional and this can delay an HC's operational date by at least three additional months.

NORC, MCC and MCA discussed several scenarios to preserve the original experimental design. After several deliberations, MCC/MCA made a project implementation decision that, as a result, requires a full revision of the evaluation design and methodology. This document assesses the impact of MCC/MCA's decision on NORC's evaluation designs for the health sector activities, revises its analysis plan for health centers and OPDs, and determines the additional information required for analysis. Toward this end, the document describes the updated plans for construction and equipping of HCs, the revised evaluation methodology, and data sources for HCs and OPDs in light of the agreed-upon evaluation hypotheses and outcomes.

II. Impact Evaluation Design

Health centers

For health centers, the MCA Lesotho intervention covers the rehabilitation of the buildings and provision of medical equipment. The original evaluation design was a randomized stepped wedge approach. HCs were stratified by eco-region (lowlands, foothills, river valley, and mountains) and infrastructure need (extreme, high, non-priority). Within each stratum, excluding those of extreme-need, HCs were randomly assigned to treatment and control groups. Additionally, matching was conducted to enhance comparison between treatment and control HCs. In this original design, construction in control HCs would start nine months after construction had been completed in their paired or matched treatment HC(s). The MCC/MCA's project implementation decision will reduce this time gap to six months, shortening the treatment period by one-third or three months. (NORC did not participate in this decision and was only informed of this change later in February 2012).

Taking into consideration delays in construction, NORC presented different scenarios that would maintain at least six months between completion of treatment HCs and the start of construction in the control HCs in an attempt to preserve the original experimental design. The scenarios required providing extensions to contractors. After evaluating the costs and completion risks to issuing extensions, MCC decided they would be too high. Therefore, MCC decided to leave (i) just three months between completion of a treatment HC (built and equipped) and start of construction at a control HC for centers located in Lot 1 and (ii) no time gap for HCs in Lot 2.

Although the experimental design may be preserved for HCs in Lot 1, NORC has several concerns. First, a three-month exposure to the intervention in treatment HCs may be too short to be able to detect an effect in most indicators; few hypotheses can be tested, as most of the outcomes require six to nine months of exposure. (A revision of hypothesis and outcomes is presented in the next section.) Second, the sample size now may be too low since the original design's sample size was estimated taking into consideration all HCs. Finally, because random assignment to treatment and control was conducted for each stratum, the subsample of HCs in Lot 1 now may be unbalanced, compromising the experimental design. If we find that the sample is no longer balanced, we propose to use propensity-score matching, a "quasi-experimental" evaluation design.

For HCs in Lot 2, although the scope for an uncontaminated experimental design is quite limited, we will built a model similar to Lot 1 trying to control for contamination (see analysis plan).

Given all the above-described limitations and risks from basing the evaluation on an experimental design approach, we propose to carry out in parallel a dose-response analysis, which is a model-based approach, for all HCs (see analysis plan). Other than requiring additional analysis time, retaining the experimental and quasi-experimental approaches carries no extra data collection costs.

Outpatient departments

For outpatient departments (OPD), the intervention includes rehabilitation of the building and integration of HIV and other health-related services into the same outpatient structure. In its original evaluation design NORC determined that the number of OPDs was too small to conduct an experimental design. Therefore, we propose to use a dose-response model for this evaluation as well.

III. Timeline for Construction, Equipment, and Data Collection

Table 1 presents the planned schedule for construction and equipment installation for HCs in Lots 1 and 2, as well as the timeline for data collection (as of April, 2012). For HCs, data collection is simultaneously carried out for Lot 1 and Lot 2 evaluation designs and is planned for the same period in which the baseline surveys were administered to avoid any seasonality bias in the analysis.

Deciding when to administer the endline for Lot 1 requires balancing two conflicting constraints. First, one would ideally like to delay the endline—and, certainly, beyond three months—to be certain that intervention impacts become manifest. However, an earlier endline would avoid contamination that could occur once construction begins in the control HCs three months after treatment health centers have been equipped. Contamination, in the form of changes in patients' behavior at theses control sites, could occur if the onset of construction requires them to go to a different temporary HC location or to experience other inconveniences.

Weighing the pros and cons of these contrary tendencies, we propose to collect household endline information (IEMS) six months after the treatment HCs in Lot 1 have been built and equipped. We recognize that waiting six months may lead to some contamination (from construction in control HCs having already started). However, given that few outcomes can be measured in three months and that we can try to measure the degree of contamination and thus control for it, this option is the best available approach for unbiased analysis. Data collection for Lot 2 will happen at the same time as for Lot 1, six months after HC have been built and equipped, although contamination in Lot 2 will be present during the entire period.

Data collection for the exit survey and the staff satisfaction survey, on the other hand, is planned October-November in 2012 because a three-month lag is sufficient to detect changes in patient and staff perceptions of infrastructure and service quality.

For OPDs, data collection for the Exit Survey and Staff Satisfaction Survey will occur at the same time as for HCs. However, assuming OPDs will be completed (constructed and equipped) by November 2012, we do not expect to see an effect on the quality of OPD services in the first follow-up of exit and staff surveys in October-November 2012. We do expect to see a longer term effect in October-November 2013, a year after completion. The data collection in 2012 will serve as baseline for those OPDs that came

into existence at the end of 2012 or a mid-term measurement for pre-existing OPDs that were part of the original baseline.

Data collection for the cohort study is planned for October-November 2013, but could be delayed till 2014 since we are collecting retrospective information and we just need to make sure that there is, at a minimum, a 12-month gap between completion of OPD improvements and data collection.

Table 1. Schedule for Construction and Equipping of HCs, and Data Collection

Activities for Lot 1	Treatment HCs equipped *(3 months)*			Time lag between treatment and control activities *(3 months)*			Construction of control HCs *(6 months)*						Control HCs equipped *(3 months)*				END COMPACT			
Activities for Lot 2	Construction of control HCs *(6 months)*						Control HCs equipped *(3 months)*													
	Treatment HCs equipped *(3 months)*																			
Month	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec/Jan
Year	**2012**									**2013**											
Data Collection							**Exit survey Staff survey**		**IEMS**									**Exit survey Staff survey**		**IEMS**	

Table 2. Schedule for Construction and Equipping of OPD, and Data Collection

OPD Activities	OPD Completed									End compact				
Month	**N**	**D**	**J**	**F**	**M**	**A**	**J**	**J**	**A**	**S**	**O**	**N**	**D**	**J**
Year	**2012**		**2013**											
Data collection	**Exit survey Staff survey**	**IEMS**									**Exit survey Staff survey Cohort study**		**IEMS**	

IV. Treatment, Outcomes and Data Sources

The hypotheses, treatments, outcomes, and sources of information are presented in Tables 3 and 5 for the evaluation of the HC activity and in Tables 4 and 6 for the OPD activity.

For HCs, the treatment is the construction or rehabilitation of the facilities (including construction and renovation of ancillary buildings such as staff houses and labor and delivery rooms) and some degree of exposure to two additional components: Health Systems Strengthening (HSS) and Health Waste Management (HWM). HSS and HWM include several trainings at the national level that started in 2010 and that will be completed by 2013. The main activities under these components are decentralization, human resource management, electronic medical records (outpatient) creation, and guidelines for HWM. These components were not included in the original evaluation design because their implementation plan was still being developed when NORC was defining its impact evaluation design. Therefore, HSS and HWM have been implemented in treatment and control facilities. NORC has been collecting information on these two components to account for coverage and specificities by HC. The evaluation analysis will attempt to control for HSS and HWM training influences and to test for differential effects of the intervention by intensity and coverage of trainings. For the dose response model, we will model duration of the treatment (from the date it became operational to the date of data collection).

Table 3. HC Treatment Variables and Data Sources

Treatment variable/indicator	Data source	Analytical method
HC renovated/equipped	Construction and equipment schedule (NORC template)	Difference in difference model
Level of training (HSS and HWM)	NORC template for HWM and HSS and HFS	
Duration of treatment	Construction and equipment schedule (NORC template)	Dose response
Level of training (HSS and HWM)	NORC template for HWM and HSS and HFS	

For OPD, the treatment is construction and rehabilitation of OPDs, integration of HIV coverage into other health services, and some amount of HSS and HWM training.

Table 4. OPD treatment variables and data sources

Treatment variable/indicator	Data source	Analytical method
Duration of treatment	Construction and equipment schedule (NORC template)	Dose response
Level of training (HSS and HWM)	NORC template for HWM and HSS and HFS	

For HCs, we identified for each outcome of interest the minimum time gap (between completion of treatments and start date of construction in controls) necessary to detect changes: three months for perception changes and either six or nine months for different illness types. For OPD and HCs in Lot 2, for which there is no lag period between treatment of the treated and construction at control HCs, the minimum time gap refers to the amount of lapsed time needed to observe the effect of renovation and equipping of the HC. In both cases, this minimum time also corresponds to the schedule for data collection.

For both HCs and OPDs the only outcomes that are likely to be detectable three months after completion are those related to perceptions of quality of services, assuming health facilities become fully functional after equipment is installed. The Exit Survey and the Staff Satisfaction Survey, which will provide the information to construct these outcome indicators, will be collected in October-November 2012 and 2013 for HCs (see Table 1) and October-November 2013 for OPDs (see Table 2).

For HCs, all non-HIV-related outcomes, except for income changes (which would require more time) should be detectable if data collection happens six months after the treatment HCs are completed (built and equipped).

Most of the HIV outcomes in HCs and OPDs, except for health-services utilization among HIV-positive individuals, require at least nine months before manifesting detectable effects. HIV mortality would require one year or more, depending on the survival curve.

Table 5. Mapping of HC treatments into outcomes to attribute, data sources, and minimum time for impact

Objective of treatment[a]	Outcome indicators (variables) to attribute	Data Sources *(Primary Source in bold)*	Minimum time to detect change
(1) Improve quality of services at health centers	• Probability that a patient states that the HC offered the services he/she needed • Probability that a patient positively rates the quality of the facility and services • Probability that a patient positively rated the time he/she had to wait to be seen?	**Patient Exit Survey** IEMS[b]	3 months
	• Probability that a health worker positively rates his/her working conditions • Probability that a health worker positively rates the overall conditions of his/her place of residence	**Staff Satisfaction Survey** IEMS[b]	3 months
(2) Increase number of individuals seeking essential health services	• Probability that an individual seeks care at a HC • Probability that a pregnant women seeks antenatal care at HC • Probability that a pregnant women gives birth at a HC	I**EMS** Administrative records[c]	6 months
(3) Increase number of HIV patients using health care services	• Probability that an HIV patient on ART stops treatment or is lost to follow-up • Probability that an individual reporting HIV/AIDS seeks care at a HC	**ART/Cohort study** IEMS	6 months
(4) Improve health outcomes	• Prevalence of diarrhea • Probability that an individual experiences any illness/symptom	**IEMS** Administrative records[c]	9 months
(5) Improve health status of HIV patients	• Probability that a patient on treatment is alive after 12 and 24 months of treatment • Probability that a patient reaches viral suppression	**ART/Cohort Study**	9+ months
(6) Increase time spent in economically productive activities	• Number of hours taken off from work/economic activity • Number of hours taken off from school	**IEMS**	6 months
(7) Increase HH income and/or productivity	• HH income generated through employment and other economic activity • Total HH income • Income per hour or day of economic activity (productivity)	**IEMS**	9 months

(a) Treatment entails rehabilitation of HCs, staff training, and improved health waste management; (b) IEMS includes questions on perceived quality of infrastructure and services that will be used in the analysis acknowledging that the recall bias is higher compare to patient exit interviews;. (c) This MOH time series of administrative records will not provide the same outcomes as those that can be constructed with the IEMS because only individuals who utilize the services are observed. With administrative records we can see trends in the number of visits by disease or service. We could see for example trends in the number of prenatal visits or number of deliveries comparing treatments and controls in Lot 1 and before and after in Lot 2.

Table 6. Mapping of OPD treatments into outcomes to attribute, data sources, and minimum time for impact

Objective of treatment[(a)]	Outcome indicators (variables) to attribute	Data Sources (Primary Source in bold)	Minimum time to detect change
(1) Improve quality of services at OPD	• Probability that a patient states that the HC offered services he/she needed • Probability that a patient positively rates the quality of the facility and services • Probability that a patient positively rated time he/she had to wait to be seen?	**Patient Exit Survey** IEMS[(b)]	3 months
	• Probability that a health worker positively rates his/her working conditions • Probability that a health worker positively rates the overall conditions of his/her place of residence	**Staff Satisfaction Survey** IEMS[(b)]	3 months
(2) Increase number of HIV patients using health care services	• Probability that an HIV patient on ART stops treatment or is lost to follow-up • Probability that an individual reporting HIV/AIDS seeks care at a HC	**ART/Cohort study** **IEMS**	6 months
(3) Improve health status of HIV patients	• Probability that a patient on treatment is alive after 12 and 24 months of treatment • Probability that a patient reaches viral suppression	**ART/Cohort Study**	9+ months

(a) Treatment entails construction or rehabilitation of the OPD, integration of HIV coverage into other health services, staff training, and improved health waste management; (b) IEMS includes questions on perceived quality of infrastructure and services that will be used in the analysis acknowledging that the recall bias is higher compare to patient exit interviews.

V. Analysis plan

Health centers

Health centers in Lot 1

For HCs in Lot 1, we will use a difference-in-differences (DID) model to estimate the effect of the intervention on the set of outcome indicators listed in Table 3. The DID model is defined as follows:

$$Y_{jt} = \beta_0 + \beta_1 R_{jt} + \beta_2 T_j + \beta_3 R_{jt} T_j + \beta_4 A_{jt} + \beta_5 T_j A_{jt} + \beta_6 R_{jt} A_{jt} + \beta_7 R_{jt} A_{jt} T_j + \vec{X}_{jt}\vec{\beta}'_X + \varepsilon_{jt},$$

where Y_{jt} is the outcome for the jth unit (individual or household) at time t; R is the round of the survey and equal to 1 if t=1 (the endline) and equal to zero if t=0 (the baseline); T is an indicator of exposure to renovation treatment (T=1 if j is in the treatment group, T=0 if j is in the control group), A_j is a measure of the amount of training assistance (HSS and/or HWM) the facility received that j went to (or the facility, itself, if the evaluation is at the level of HC), a vector $\vec{X}$ of covariates and ε is the normally distributed error term. The β_i are coefficients to estimate (with the prime symbol indicating the transpose of a vector, which in turn is denoted with the arrow above it). The interaction between the construction program and round is the difference-in-differences estimator of the average treatment effect, β_3 of facility rehabilitation.

As mentioned above, HSS and HWM are two additional components of the intervention that were not randomized. Coverage and intensity levels at each HC may vary across all facilities. To explore if the program had a differential effect by intensity of training, in the equation, above, we have included an indicator, A_j that measures the level of intensity of training assistance to the HCs. Intensity of trainings will be defined with information on number and/or type of trainings and number and/or type of staff that received the training, depending on the availability of information. By interacting A_j with the round, R we can estimate the difference-in-differences average treatment effect, β_6 of HSS/HWM. By interacting A_j and T_j with R we can estimate the difference-in-differences average treatment effect, β_7 of the synergy beyond their individual effects when all treatments are combined.

Another source of heterogeneity that we will attempt to address is the **quality and completion status of the construction/rehabilitation** of the health centers. Even within the same contractor, we may see differences in terms of quality/completion of the renovations that would affect the outcomes. The Health

Facility Survey (HFS) has information on the physical infrastructure, equipment and supplies, degree of operation, and service provision of 145 HCs in Lesotho. The HFS is collected bi-annually; the baseline survey was conducted in 2011 in 145 HCs although in 64 of them construction had started. We expect that the 2013 HFS will allow us to see the status of the HCs after construction, rehabilitation, and equipping. We will attempt to use the HFS to evaluate differential impact of construction quality, equipment installation and maintenance.

As mentioned earlier in the text, we plan to collect IEMS data six months after HC are built and equipped. According to the construction schedule, three months prior to data collection, control HCs would start construction resulting in a potential bias. We believe that health care utilization during construction could decrease due to inconveniences; therefore, we could overestimate the effect of the treatment. Though contamination would not be completely removed, it can be limited. First, in the analysis we will identify different groups of control HC based on the time they had been under construction. Second, we will add questions in the surveys to ask whether individuals had heard of the construction as well as whether it affected their use of service.

Finally, given our concern that our sample in Lot 1 may be unbalanced, we will check if there are **statistical differences between the treatment and control means** at baseline. If the sample is not balanced between treatments and controls, we will use propensity score matching to adjust for pre-treatment observable differences between treatments and controls. The difference-in-differences estimation would then be applied to the area of common support after propensity score matching.

Health centers in Lot 2

In Lot 2 there is no time lag between completion of treatments and start of controls. The six months lag between end of equipment in treatment HC and data collection will be contaminated by construction in the controls. As for Lot 1, we will try to measure the degree of inconvenience during construction to reduce the bias. We will apply the same model as in Lot 1.

Dose-response analysis

Given the potential bias related to contamination, in addition to the models described above we propose to build a dose-response model to test if facilities with a greater or longer exposure to the treatment had a greater effect. In this case, we would not need to compare treatments and controls and all HCs from Lot 1 and Lot 2 would be included. The independent variable would be the duration of the treatment (from the date it became operational to the date of data collection). The model can be laid out as follows:

$$Y_{jt} = \beta_0 + \beta_1 D_j + \beta_2 D_j^2 + \vec{X}_{jt}\vec{\beta}'_X + \varepsilon_j,$$

where Y_j would be the outcome of interest at follow-up or the change in the outcome between baseline and follow-up, D_j is treatment duration and $\vec{X}$ is a vector of covariates.

The effect of duration may not be linear. We may see a low effect at start if people are not yet aware of the new services offered, a high effect once the population learns the existence of the services but then a decreasing effect over time if people´s expectations are not met or if the quality of the infrastructure rapidly drops and infrastructure repairs and maintenance are poor. To account for such non-linearities, we will model different functional forms of the dose-response variable, for example, by including a quadratic term as shown above.

The effect of duration of the treatment on different outcomes may be different depending on the quality or degree of completion of the construction, equipment installation, and HSS and HWM trainings. Therefore, in this model we will also include interactions of duration with quality and degree of completion, and duration with intensity of training.

Duration of the treatment depends on the start and end date of construction. For HCs, the controls will start after the treatments are completed. Given that assignment to treatment or control was random, there is no reason to expect that the group that starts first and will have longer duration would be different from the controls that start later (shorter duration). However, delays in construction or equipment installation may not be at random and duration may capture other factors related to the outcomes of interest. For instance, if construction is delayed more often in more remote and poorer areas with difficult access and weather constraints for construction, the effect of duration may be biased. We will try to address the potential endogeneity of duration by including as an interaction term with duration a variable that measures poverty at the catchment area level or a measure of remoteness.

Outpatient departments (OPD)

For OPD, there was not a randomized experiment. Therefore, to estimate the association between the treatment and the quality of infrastructure and services as perceived from patients and staff, we will a dose response model (same model specified for HC).

For the HIV cohort study (in HCs and OPD), we will use a Cox Proportional Hazard Regression for survival analysis to assess retention rates, time to viral suppression, and mortality. Treatment will also be modeled as duration in a dose response analysis. The model is specified as follows:

$$h_j(t) = \mathrm{e}^{a+b_1 D_j + b_2 D_j^2 + \bar{X}_{jt} b' x},$$

where h is the hazard, t is the survival time, d is duration of treatment and $\vec{X}$ is a vector of covariates.

VI. Additional Information to Support the Analysis

In addition to the sources of data that will be used to estimate the effect of the program on different outcomes (Tables 3-6), NORC is collecting additional information to support the analysis.

Construction and equipment schedule

NORC is keeping track of the construction and equipment installation dates of completion by HC and OPD. This information will be used to monitor any deviation in the schedule that could affect the evaluation or the dates planned for data collection. The information will also be used in the analysis to account for different times of exposure if treatment or control HCs are completed in different periods and the length of time is significant.

Health Systems Strengthening (HSS) and Health Waste Management

NORC has been collecting information on these two components to account for coverage and specificities by HC. Information collected by health facility includes: number of trainings, dates, type of training, and staff who participated in the training and evaluations.

Other interventions and donor activities

Given that the original experimental design was not preserved due to delays in the construction and equipping of health facilities, the attribution of effect may be confounded by other programs or other donor activities in the areas where treatments and controls are located. If information on these activities is available, we could try to control for the presence of these other activities. Otherwise, our evaluation may attribute the combined impacts observed on HCs and OPDs all to the MCA intervention, exaggerating our estimations.

Validation of HIV patient data

The HIV cohort study relies on information retrieved from the ART register form that nurses have to complete. We learned on our recent trip to Lesotho that the form is not always filled out or not filled out correctly because nurses are usually overworked and do not have the time to enter the information for all patients. NORC proposes to assess the quality of the information from the ART register by comparing the data with clinical files in a subsample of HCs and patients in the cohort. This exercise assumes that the clinical files have all the information needed and are correct, which we will need to confirm before conducting any validity checks.

Additional questions in surveys

We will assess knowledge and perceptions of services before, during, and after construction of the population living in the catchment areas where the HCs and OPDs are located, of the patients utilizing the services, and of the HC and OPD staff. Towards this end, we will add questions to the IEMS, Exit Survey, and Staff Satisfaction Survey. In the IEMS we will add questions to assess how much the population knew about the intervention, whether they learned through community awareness talks, how much time it took for them to get exposed to information about the intervention (before, during or after construction), and what their sources were of information for developing perceptions related to the renovation of health facilities. For patients and staff we will add similar questions, as well as others to the Exit Survey and Staff Satisfaction Survey to evaluate knowledge and perceptions of quality of services during construction. For staff only, we will ask about trainings and staff rotation.

Focus groups

Focus groups in OPD are important for evaluating more in depth how patients and staff perceive the new or renovated buildings and the integrated services (HIV with non-HIV patients). Focus groups allow assessing perception of topics that are too sensitive and complex to ask through surveys.

The target population across different OPDs will be HIV patients, non-HIV/AIDS patients, and staff (nurses).

Topics covered (patients): Satisfaction and perceptions related to the rehabilitation of OPDs, integration of services, temporary services during the construction period, access to medications, and quality of services.

Topics covered (staff): Satisfaction and perceptions related to the rehabilitation and equipping of OPDs, integration of services, staff housing, trainings (under the Compact), temporary services during the construction period, drop-out HIV patients and outreach efforts, and changes in time sending samples for laboratory testing and receiving results.

Timing: 5 months after OPD completion (construction and equipment installation).

Size: For HIV patients: 6 to 8 (separated by gender); non-HIV patients: 8 and 10 (separated by gender); staff: 8 to 10.

Location: Ideally there would be one focus group per target population and eco-region (lowlands, foothills, river valley and mountains) or at least we will need to pick up urban and rural locations. Maseru

would be included. Ideally, focus groups would be conducted in the village or city where the OPD are located instead of moving them to Maseru.

Incentives: 30 to 50 Maloti to patients and at least 50 Maloti for nurses. For patients, options include condoms, money, grocery vouchers, and phone cards. For staff, we would consider grocery vouchers, and phone cards.