A Review of Healthcare at the Beginning of Life and Child Survival: Evidence from a Cash Transfer Experiment in Nigeria

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1. Project Overview

Despite the emergence of Nigeria as one of the largest economy in Africa, its child mortality rate far surpasses other countries in the region with considerably less resources. Nigeria's high under five child mortality rate of 128 per 1000 live births has been attributed to the inadequate use of maternal healthcare services. At the start of this study, it was estimated that 34% of women in Nigeria did not use any prenatal care, and only 36% of the women delivered at a formal healthcare facility.

Many studies in the literature suggest that conditional cash transfer (CCT) is effective in increasing healthcare uptake (to be further confirmed by this study). On the other hand, there is limited evidence on the effectiveness of maternal healthcare services in Nigeria. For this reason, the main research question of this study is as follows:

What is the effect of increasing healthcare consumption through CCT on the child survival rate in Nigeria?

2. Research Design and Sampling Procedure

A two-group, pre-test post-test randomized experiment was used in this longitudinal study. The program was implemented in 180 primary healthcare service areas (HSA) across five states in Nigeria. HSAs are the catchment areas of healthcare facilities. Sample selection was performed with the assistance of government officials and therefore it did not involve random selection (rather, expert sampling was used). The selected states were amongst the lowest in terms of institutional delivery rates in Nigeria, and the sampled HSAs were mostly located in rural areas. Thus, the women in the study were most in need of the intervention. There was also a second stage of sampling—a total of 2383 clustered areas (a cluster of households) in the sampled HSAs were randomly selected as part of the study. The sampled clustered areas were then randomly assigned to treatment or control. During the enrollment phase of the study, the field agent would visit eligible households (with woman in her 1st/2nd trimester) in the sampled clustered area and attempt to enroll women to the study. The field agents would sequentially visit each clustered area in the randomized list of clustered areas until the enrollment target of 60 women per HSA is reached. The sampling process is summarized in Figure 1.

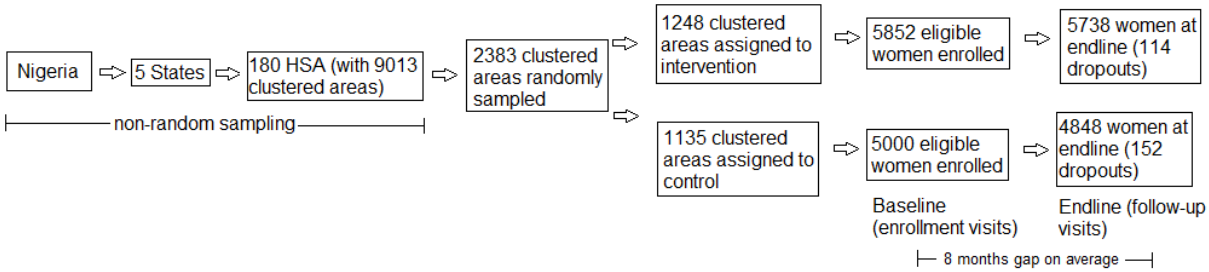

Figure 1: Random sampling and assignment

The sampling frame for this study is a list of states, HSAs, and clustered areas (clustered areas are census tracts in each HSA that are defined by the National Population Commission of Nigeria). The authors used the list of clustered areas to perform area random sampling. At the household level, the field agents performed purposive (non-random) sampling to quickly obtain a sample of pregnant women. There was

differential drop-out across the treatment and control groups that occurred between baseline and endline (see Figure 1) ; however, the overall attrition rate is small (more on this in Section 4).

3. Measurement Issues

The outcome variables in this study are child survival (whether a child that was in-utero at baseline was alive at endline), fetal loss and stillbirth. Since the outcome variables are unambiguous (as oppose to an abstract variable like "self-esteem"), they were measured (with very little operationalization concerns) with end-line surveys and could be verified by medical records. Similarly, the CCT intervention is relatively simple to operationalize. Women in the intervention group were informed about the program during enrollment. They were paid a lump sum of $14 during the follow-up visit if they used maternal healthcare services during pregnancy, at birth, and post-birth. Healthcare usage can be verified by receipts and registries from healthcare facilities. The authors did not describe whether a pilot study was conducted and the type of training that field agents received. They mentioned that a supervisor was assigned to 3 to 4 field agents, but no details were given about the type of supervision that was provided and whether there was quality control on the collected data. Therefore, there might be non-sampling errors in the survey responses which would adversely impact the internal and construct validity of this study (more on this in Section 4). Unfortunately, the survey items were not provided by the authors; therefore, the length and quality of the surveys cannot be assessed. However, I assume that the surveys are short since only 20 demographic/mother characteristics variables were collected at baseline and only a few outcome variables were collected at endline; thus, respondents probably did not experience survey fatigue. However, it is possible that the wording of the survey questions is ambiguous (since it was not pre-tested in a pilot study). In addition, there might also be variations in the quality of the translations (from English to presumably multiple Nigerian regional languages). For these reasons, different respondents might interpret the questions differently and this reduces the reliability of the responses.

4. Threats to Validity and Recommendations

This study provided convincing evidence that there is correlation between CCT assignment and child survival. This is mainly because of the large sample size (close to 5000 samples even after drop-outs) and the ease of measuring the outcome variables. As mentioned previously, the author did not provide details about field agents training and quality control on the collected data. However, I think this will mostly lead to high measurement errors in the mother characteristics/demographics variables collected at baseline rather than the outcome variables. The child survival (the dependent variable) is relatively easy to measure and verified and the CCT is assigned by the researchers (the independent variable). Thus, the intent-to-treat effects in the study are all significant at the 1% level. For this reason, I believe that this study has strong conclusion validity.

As for internal validity, this study faces multiple selection and social threats. First, there was imbalance between treatment and control (in age and number of 3rd trimester women). However, the imbalance was driven only by data in one state, Gombe, where two-thirds of the participants were in the intervention group. Clearly, the field agents in Gombe deviated from the research protocol and over-enrolled women to the intervention group. Although the control and treatment groups were shown to be balanced after Gombe was excluded, the possibility of measurement errors in the demographic variables collected at baseline made this claim less convincing. It is possible that due to the inadequate training of field agents, many of the women were interviewed in the presence of their husbands. As the

result, they would give similar answers to sensitive questions such as whether the husband has multiple wives, or the outcomes of prior births. Therefore, the baseline covariate might only *appeared* to be balanced due to non-sampling error. Second, several healthcare facilities in the sample took part in another study where they receive an additional health worker (I call these "enhanced facilities"), thereby increasing their quality of care. For this reason, women might be attracted to maternal care because of the increase in quality of care. If the number of enhanced facilities between the treatment and control groups is different, then there will be selection-history threat. However, since the authors controlled for HSA-fixed effect in all of their regression analyses, I think that this threat has no impact on the main results. Third, this study faces selection-mortality threat as more women in the control group dropped out. Furthermore, the drop-out was non-random since women with more birth experience, less education, and no prior history of fetal loss were more likely to drop out. However, I agree with the authors that since the overall attrition rate was small (2.5%), this issue will likely have minimal impact on the main results. Fourth, since some women in the control and intervention groups lived in the same clustered area, social interaction threats were possible. Women in the treatment group might transfer part of the cash reward to their friends in the control group and this might encourage/enable them to use maternal care. Imitation of treatment is also possible if women in the control group were motivated to use maternal care because of peer effects. In sum, I believe that if the authors show that there was adequate training and a pilot study was conducted, then I would find the covariate balance at baseline more credible and the causal relationship more convincing.

The construct validity of this study can potentially be weak due to social threats. Measurement error in the responses to sensitive questions can occur due to evaluation apprehension if field agents (due to inadequate training) failed to engender trust and rapport with the respondents, or if the survey was taken in the presence of the husband. Finally, this study has very little external validity because the sample was not representative of Nigeria as a whole (only the rural areas with high child mortality rate was selected).

With better training, supervision and quality control, the problematic data from Gombe would have been detected earlier during the enrollment visits, thus allowing the data to be recollected. Moreover, the covariate balance at baseline would be more convincing if I know that the baseline data was collected by well-trained and supervised field agents with a pre-tested survey. In sum, with better measurements, the selection threat to internal validity and the social threat to construct validity can be reduced and the casual inference of this study would be more creditable.

Ideally, the authors should have replaced the HSAs that participated in another study with a similar/neighbouring HSA in the state. Alternatively, the timeline of the study could have been postponed until the other study is completed. Understandably, this might not be possible due to research logistic or scheduling issues with the local partner. The differential attrition between control and treatment groups is difficult to avoid. One recommendation would be to keep in touch with the women in the control group via a phone call or a visit between the enrollment and follow-up visits. However, the field agent must ensure that this additional contact will not affect the women's uptake decisions. Spillover effects might be reduced if the study expands to more than five states in Nigeria and random assignment is performed at the HSA level. Thus, social contact between women in the two groups would be minimized. In addition, the external validity of this study would be strengthened if it was conducted across Nigeria. However, this might be difficult due to time and budgetary constraints.

References

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