Die approbierte Originalversion dieser Diplom-/Masterarbeit ist in der Hauptbibliothek der Technischen Universität Wien aufgestellt und zugänglich.
http://www.ub.tuwien.ac.at

The approved original version of this master thesis is available at the main library of the Vienna University of Technology.
http://www.ub.tuwien.ac.at/eng

INSTITUT FÜR HÖHERE STUDIEN
INSTITUTE FOR ADVANCED STUDIES
Vienna

TU WIEN CONTINUING EDUCATION CENTER

MSc Economics

Evaluating health interventions: Child Mortality and Insecticide-Treated-Net Distribution Programs in sub-Saharan Africa

A Master's Thesis submitted for the degree of "Master of Science"

supervised by

Derya Uysal

Casandra Hutchinson

1226490

Vienna, June 18, 2014

INSTITUT FÜR HÖHERE STUDIEN
INSTITUTE FOR ADVANCED STUDIES
Vienna

TU WIEN CONTINUING EDUCATION CENTER

MSc Economics

Affidavit

I, Casandra Hutchinson,

hereby declare

that I am the sole author of the present Master's Thesis,

Evaluating health interventions: Child Mortality and Insecticide-Treated-Net Distribution Programs in sub-Saharan Africa

66 pages, bound, and that I have not used any source or tool other than those referenced or any other illicit aid or tool, and that I have not prior to this date submitted this Master's Thesis as an examination paper in any form in Austria or abroad.

Vienna, June 18, 2014

Signature

Contents

List of Figures

List of Tables

List of Acronyms

ACT artemisinin-based combination therapy

ATE average treatment effect

ATET average treatment effect on the treated

DHS Demographic and Health Surveys

GIS Geographic Information System

IDP internally displaced person

IPTp intermittent preventive treatment for pregnancy

IPW inverse probability weighting

IRS indoor residual spraying

ITN insecticide-treated bed net

MERG Roll Back Malaria Monitoring and Evaluation Reference Group

NMCP National Malaria Control Program

PMI President's Malaria Initiative

RBM Roll Back Malaria

SES socio-economic status

SUTVA stable unit treatment value assumption

UNHRC United Nations Human Rights Council

USAID United States Agency for International Development

WHO World Health Organization

Abstract

Because of the wide-spread and devastating effects of malaria in sub-Saharan Africa, the U.S. launched a $1.2 billion initiative to rapidly scale-up malaria prevention and treatment interventions in high-burden countries in sub-Saharan Africa through the President's Malaria Initiative. One of the main mechanisms used to reduce child mortality due to malaria is the free and heavily subsidized distribution of insecticide-treated bed nets (ITNs). This paper evaluates the effect of such campaigns in Rwanda, Senegal, Uganda, and Zimbabwe using an estimator that exploits the fact that the intervention only affects mortality through a behaviour change from not using ITNs to using ITNs. I find that Rwanda sees a significant decrease in mortality for children under two years of age, but that no effect can be seen for Senegal, Uganda, and Zimbabwe. Through a series of plausibility and falsification tests, I find that the estimators for all countries except Zimbabwe fulfil the assumptions of the model. Therefore, no decrease in mortality for children under two can be seen from these ITN interventions in Uganda and Senegal. A decrease of 7.9 deaths in children under three per 1,000 live births per year is reported for Rwanda.

1 Introduction

According to the World Health Organization (WHO), in 2012 an estimated 3.4 billion people are at risk of malaria. In fact, an estimated 627,000 of the 207 million people infected with malaria in that year died as a result. Of these, roughly 482,00 were children under five years of age (World Health Organization, 2013). However, it would be short-sighted to consider the impact of malaria only in terms of morbidity and mortality. In fact, Malaney et al. (2004) estimate that malaria may account for a decrease in per capita GDP of nearly fifty percent in some countries, although they maintain that microeconomic estimates tend to assess this burden to be less than one percent of GDP. These statistics, while alarming, are not new to the region.

For this reason, in April of 2000 forty-four malaria-affected countries met in Abuja, Nigeria for the African Summit on Roll Back Malaria (RBM). Leaders from these African nations, as well as senior officials from organizations such as the WHO, the African Development Bank, and United States Agency for International Development (USAID), established a set of goals including:

- at least 60% of those suffering from malaria have prompt access to, and are able to correctly use, affordable and appropriate treatment within 24 hours of the onset of symptoms,
- at least 60% of those at risk of malaria, particularly children under five years of age and pregnant women, and benefit from the most suitable combination of personal and community protective measures such as insecticide treated mosquito nets and other interventions which are accessible and affordable to prevent infection and suffering, and
- at least 60% of all pregnant women who are at risk of malaria, especially those in their first pregnancies, have access to chemoprophylaxis or presumptive intermittent treatment.[1]

Additionally, development partners pledged to allocate substantial resources (at least US$ 1 billion per year) for RBM actions.

Unfortunately, the uptake of RBM was slow and initial gains were small. However, in 2005 the U.S. established the President's Malaria Initiative (PMI) which "strives to reduce the intolerable burden of malaria and help relieve poverty on the African continent." The interventions used by PMI include mass distribution of insecticide-treated bed nets (ITNs), spraying houses with insecticides

[1]Goals as reported in the Abuja Declaration: http://www.rollbackmalaria.org/docs/abuja_declaration_final.htm

(IRS), intermittent preventive treatment for pregnant women (IPTp), and prompt use of artemisinin-based combination therapies (ACTs) for those diagnosed with malaria. The original goal was to reduce malaria-related mortality by fifty percent after three years of full implementation in each country. Implementation in Uganda began in 2006 and in Rwanda and Senegal in 2007. Coincidentally, these years coincide quite well with the Demographic and Health Surveys (DHS) phases in these countries. The purpose of this paper is to use the data from these surveys to address the effect of the ITN distribution campaign on early childhood mortality.[2]

Measuring the efficacy of a nationwide campaign is notably difficult. In the case of developing countries, reliable data are very hard to encounter. Furthermore, when an entire population is exposed to an intervention, there is no contemporaneous comparison group against which to compare the treatment group. Nonetheless, several papers have attempted to evaluate ITN distribution programs. Some of these papers focus on intra-household distribution and propensity for ownership (Dupas, 2009, Njau et al., 2013, Oresanya et al., 2008, Mugisha and Arinaitwe, 2003); whereas others attempt to address the intra-household allocation dependent on the distribution mechanism—free versus purchased mosquito nets (Hoffmann, 2009, Cohen and Dupas, 2010). Recently, Deuchert and Wunsch (2014) proposed an estimator for evaluating nationwide health interventions—specifically for evaluating ITN campaigns in Africa.

Deuchert and Wunsch's estimator—the estimator used in this paper—addresses the major problem facing this type of estimation, which is that one cannot use a typical before-after estimator (as proposed by the WHO) because it requires that determinants of health between the two measurement periods to remain the same. As mentioned above, the RBM, as well as the PMI, are multi-faceted approaches. Each country launches several interventions all aimed at improving health and reducing malaria-related childhood mortality. Furthermore, there are several other initiatives unrelated to malaria aimed at decreasing overall childhood mortality. These programs include vaccination and vitamin A drives and increased family planning resources and access to antenatal care. Therefore, it is wholly infeasible to use a typical before-after estimator. Instead, Deuchert and Wunsch's estimator uses a measurement of behaviour change. After controlling for time-varying determinants of ITN usage, this estimator can evaluate the effect

[2]Infant mortality is defined as the number of deaths in children aged 1-11 months in a given year per 1,000 live births. Childhood mortality is defined as the number of deaths in children aged 1-59 months in a given year per 1,000 live births. This paper, however, considers only the number of deaths in children aged 0-35 months in a given year per 1,000 live births. For the purposes of this paper, early childhood mortality is, therefore, defined as number of deaths in children aged 0-35 months in a given year per 1,000 live births.

of the ITN campaign on child mortality via the change in behaviour from ITN non-usage to ITN-usage.

The rest of the paper is organized as follows. Section 2 describes the available data and the design of the experiment. Section 3 details the theoretical framework behind the Deuchert and Wunsch estimator. Section 4 presents the econometric methods employed. Section 5 presents the results. Section 6 assesses the sensitivity and robustness of the estimator, as well as addressing identifying assumptions. Section 7 concludes.

2 The Data

The paper is based on the following DHS data:[3]

- Senegal 2005—interviews from February through May
- Rwanda 2005—interviews from February through July
- Uganda 2006—interviews from May through October
- Zimbabwe 2005/2006—interviews from August 2005 through February 2006
- Rwanda 2010/2011—interviews from September 2010 through March 2011
- Senegal 2010/2011—interviews from October 2010 through April 2011
- Zimbabwe 2010/2011—interviews from September 2010 through March 2011
- Uganda 2011—interviews from June through December

The DHS surveys include three questionnaires: a household questionnaire, a women's questionnaire, and a men's questionnaire. The household questionnaire contains basic information on sex, age, number of occupants, education and household characteristics. Additionally, it contains information about the structure of the house, such as source of water, toilets, as well as other durable goods, such as bicycles and vehicles. The women's questionnaire includes detailed information on reproduction and sexual activity (such as number of children ever born and the age at which a child died), and maternal and child care. The women's questionnaire also includes questions about each surviving child born in the five years before the interview. The men's questionnaire elicits similar information to that of the women's questionnaire (MEASURE DHS/ICF International, 2012).

[3]Data publicly available at http://www.measuredhs.com.

Based on a stratified two-stage cluster design, the data are representative at a national, residence (urban/rural), and regional level. First, Enumeration Areas (EAs) are compiled from the census files from each country. A complete household listing is then created from each selected EA. From this complete list, a sample of households is selected. Each household completes the household questionnaire. The women's questionnaire is administered to all female household occupants aged 15-49 years. The men's questionnaire was performed at every third household of the women's sample. All men aged 15-54 of these households were surveyed (MEASURE DHS/ICF International, 2012). In this paper, I use both the responses from the women's questionnaire and the household questionnaire. It is important to note that this a repeated cross-sectional data, so the households surveyed in each period are not the same. Rather, the sample from each realization of the survey is chosen with the same method in each case, and the sample is representative in each case.

Because the PMI was launched in 2006 in Uganda and 2007 in Rwanda and Senegal, I consider the DHS surveys surrounding these dates. As mentioned in the introduction, the PMI invested heavily in ITN distribution schemes in these countries with the goal of decreasing malaria-related mortality by 50%. I also consider Zimbabwe because this country is not included in the PMI until 2011, which is after the DHS surveys under consideration time period; additionally, Zimbabwe accepted very little external aid during the time-line I consider. Although it is not the purpose of this paper to calculate the investment per life saved via ITN distribution programs, this would be an interesting future expansion.

An ITN is a type of mosquito net that has been treated with an insecticide. ITNs must be retreated every six to twelve months, whereas long-lasting insecticide-treated nets (LLINs) remain effective for at least three years and should afterwards be replaced. The mosquito net is hung above a bed, and then tucked under the mattress once all of the occupants are in the bed. The net prevents malaria infection by both providing a barrier between malarial mosquitoes and the people in the bed, and also by repelling mosquitoes from the bed area. The insecticide with which the nets are treated kills mosquitoes, as well as other insects that come into contact with the net. One goal of the PMI is to support the purchase and distribution of ITNs throughout each of the partner countries. It is these national scale ITN distribution campaigns that are evaluated in this paper.

In all of the countries in question, ITN distribution campaigns support using antenatal care visits, regional events, and local training programs to both distribute ITNs and encourage their use (especially among vulnerable populations).

Furthermore, each country attempts to increase ITN use by providing subsidies. Although the mechanisms differ slightly in each country, they all contain these key points.

All PMI Malaria Operation Plans can be found at http://www.pmi.gov; however, the plans are by and large the same across countries. Pregnant women and children receive free bed nets via national campaigns (including vaccination drives). Pregnant women also receive heavily subsidized ITNs at antenatal care visits at the local health posts. Moreover regional events and local training programs are held throughout each country to both distribute ITNs and encourage their use (especially among vulnerable populations). Additionally, subsidized nets are provided to local distributors to incentivize the market. The fact that some ITNs are free and some are not could pose a problem because whether a household purchases an ITN or receives it for free affects its use for children, and the data available do not include information about how an ITN is obtained (Hoffmann, 2009). However, since all women and children under three are exposed to the same program and price structure, I assume that this does not affect the estimation. Table 1 details how much PMI spent on the ITN intervention, separated into several categories. The first row for each country is the amount spent on procuring and distributing ITNs and LLINs. The second is money spend developing the infrastructure to transport the nets to the communities in need. The third is the budget used for training community members about proper ITN usage, and the fourth column is money spent developing and subsidizing the private ITN market. In all countries, PMI spend large sums of money procuring and distributing ITNs. In Uganda, there is a clear focus on developing the private sector, whereas in Rwanda the focus appears to be on training community members about ITN usage.

I have chosen to evaluate the impact of the campaigns on mortality for children under three years of age based on the timing of the PMI interventions in each country. Since the latest interventions began in 2007, and the corresponding DHS data for these countries was collected in 2010, the intervention could at maximum affect children who are three years old or younger. I consider only children who are strictly younger than three years old (or, in the case of children who died, who have a hypothetical age of strictly less than three were they still alive).

Section 4 describes the method of data cleaning and the treatment, outcome, and control variables. Appendix A includes tables of the summary statistics for each country at each time period.

Table 1: PMI Budgets in Thousands of U.S. Dollars

		2006	2007	2008	2009	2010	2011
Rwanda	ITN purchase	—	3,735	3,175	5,602	3,500	5,100
	ITN transport	—	150	—	—	—	—
	ITN training	—	150	700	350	499	250
	Private sector	—	—	—	—	—	—
Senegal	ITN purchase	—	5,500	5,270	5518	13,660	7,000
	ITN transport	—	500	1,034	—	—	2,327
	ITN training	—	—	205	200	425	300
	Private sector	—	200	—	—	—	—
Uganda	ITN purchase	4,035	4,950	4,920	5,686*	5,820*	5,200
	ITN transport	—	—	—	—	—	—
	ITN training	—	100	380	—	—	900
	Private sector	330	800	450	100	20	—

* Expenditure includes unspecified amount of behavioural change and communication training. Numbers represent thousands of U.S. dollars. Budgets obtained from the Malaria Operating Plan for each year on each country's profile page (www.pmi.gov).

3 Theoretical Framework

This paper examines the change in the mortality rate for children up to three years old in four sub-Saharan African countries due to nationwide ITN-distribution campaigns. Therefore, construction of a typical treatment effect structure is necessary (Greene, 2011, p. 889). Every individual has a potential outcome, Y, and can be exposed to the treatment, I. The treatment here is the nationwide ITN-distribution campaign. $Y = 1$ represents a child who passed who would be strictly younger than three had he or she lived, but who passed away. $Y = 0$ for living children.

$$I = \begin{cases} 0 & \text{if child born before campaign} \\ 1 & \text{if child born after the introduction of the campaign} \end{cases}$$

The potential outcomes are then Y_{i1}, the potential outcome for individual i subject to treatment, and Y_{i0}, the potential outcome for individual i not subject to treatment. The *average treatment effect* (ATE) is

$$\text{ATE} = \mathbb{E}[Y_{i1} - Y_{i0}].$$

Unfortunately, ATE has a fundamental drawback: it is impossible to measure both Y_{i1} and Y_{i0} since the individual can exist in only one of the two states.

Furthermore, in this case, every individual in the country is exposed to the treatment. Even if the ITN-distribution scheme fails to reach a specific individual, by the very definition of the campaign *every* individual in a nation is subject to a national intervention. Consequently, $T = I$, where T is the time of the interview ($T = 0$ before the intervention, and $T = 1$ after it), and the estimate of interest is the *average treatment on the treated* (ATET):

$$\text{ATET} = \mathbb{E}[Y_{i1} - Y_{i0}|T = 1].$$

Since these are nationwide campaigns, no individual can be in the Y_{i0} state at $T = 1$. One technique often employed is to assume that $\mathbb{E}[Y_{i0}|T = 0] = \mathbb{E}[Y_{i0}|T = 1]$, which is to say that an individual's outcome had he or she not subject to the treatment is the same as his or her pre-intervention outcome. However, health outcomes are affected by myriad factors. In this specific case, health outcomes are affected by climate patterns that affect the intensity of malaria, as well as the availability of food and resources; not to mention the fact that the countries under consideration experienced volatile domestic conflicts that also influence health outcomes. Moreover, all of the governments in question launched vaccination and vitamin A campaigns, as well as training programs for health improving lifestyle strategies, with the end goal of decreasing all-cause child mortality in their respective countries. For these reasons, it is impossible to use the typical before-after estimate in this case.

Instead, the estimator proposed by Deuchert and Wunsch (2014) is employed. This estimator is ideal for evaluating health interventions that accomplish the desired outcome only through behaviour change. Here, that behaviour change is ITN usage. An individual who receives an ITN only reduces his or her risk for contracting malaria if he or she uses the ITN. So, in fact, any drop in malaria mortality can be attributed to the behaviour change from not using an ITN to using an ITN. Deuchert and Wunsch's semi-parametric estimator, which is the product of 1) a standard inverse probability weighting (IPW) estimator for the average affect of ITN usage on childhood mortality; and 2) the change in behaviour induced by the ITN intervention, can be utilized to evaluate the overall effect of the ITN campaigns on early childhood mortality. The estimator is built on the following set of six assumptions.

Assumption 1 (Exclusion Restriction)**.** *Let Y_{TB} denote the potential outcome of behaviour B and treatment T. $B = 1$ indicates that an individual uses an ITN (otherwise $B = 0$), and T indicates whether the observation is before or after the*

intervention. (For simplicity, the running index i is omitted.) Then

$$\mathbb{E}[Y_{0B}] = \mathbb{E}[Y_{1B}], \quad B \in \{0, 1\}.$$

This assumption demands that the expected outcome between $T = 0$ and $T = 1$ remains the same if an individual does not change his or her behaviour—an individual who does not use an ITN in both the pre- and post-intervention periods faces the same expected outcome in both periods. The same must be true for individuals who use an ITN in both time periods. If this assumption holds, there should be no difference between the ATE calculated for the pre-intervention individual and the ATE calculated for the post-intervention individual. This assumption essentially places all of the motivation for a the change in outcome on the change in behaviour.

Assumption 2 (Monotonicity)**.** *Let B_1 denote the potential behaviour of an individual exposed to the intervention and B_0 that of an individual not exposed to the intervention. Then*

$$B_1 \geq B_0.$$

Here it is assumed that the intervention can only positively affect behaviour. That is to say that the intervention only motivates individuals to use ITNs; no individual stops using an ITN due to the intervention. In the previous assumption (Assumption 1), behaviour change is identified as the mechanism for change in outcome, and in this assumption behaviour is change is limited to a positive direction. Together, this means that any drop in mortality must come from an individual's change from non-ITN usage to ITN usage, since mortality is affected by ITN usage and ITN usage is affected by the campaign. The overall effect of the campaign is then captured by:

$$\mathbb{E}[Y_{11} - Y_{10} \mid T = 1, B_0 = 0, B_1 = 1]\mathbb{E}[B_1 - B_0 \mid T = 1]. \tag{1}$$

That is, the ATET of ITN usage on early childhood mortality for those who start to use ITNs *in response* to the campaign comprises two elements: 1) The average treatment effect of ITN usage on early childhood mortality and 2) the average treatment effect of the intervention on behaviour. Again, this latter treatment effect has the problem in which the no-intervention behaviour B_0 cannot be observed at time $T = 1$. This time, this requires that an individual's ITN usage had he or she not been subject to the campaign is the same as his or her pre-intervention ITN usage. This is to say that nothing other than the intervention may affect ITN usage, a claim which is expounded upon in the next assumption.

Assumption 3 (Conditional Before-After)**.** *Define X^B as all relevant determinants of behaviour, B. Then*

$$\mathbb{E}[B_0 \mid T = 1, X_B = x_B] = \mathbb{E}[B_0 \mid T = 0, X_B = x_B].$$

The behaviour of the pre-intervention population, conditional on a set of covariates (defined in Section 4.1), may be used as a proxy for no-intervention behaviour of the post-intervention population. Conditioning on all relevant time-varying determinants of ITN usage, the outcome for an individual with characteristics X_B at $T = 0$ is a very reasonable alternate for the counter-factual outcome at $T = 1$—the unobservable individual with characteristics X^B who has not been exposed to the intervention.

For example, I will condition on the education level of the child's mother under the assumption that her educational background influences whether or not her child sleeps under an ITN. Other important factors are income, mother's age, and geographical location of the household, to name a few. These factors are be delineated further in Section 4.1.

At this point, the ATE is measured as the product of 1) the effect of ITN usage on health, and 2) the change in behaviour induced by the ITN intervention. It remains to identify the first term. Recalling Equation 1, what remains is a standard inverse probability weighting estimator for the average affect of ITN usage on childhood mortality. The ideal situation in which to estimate the effect of ITN usage on childhood mortality would be to conduct a randomized controlled trial. However, this is not the type of data that I am using. Rather, I have observation data from a pre-intervention period and a post-intervention period. In each of these periods, there are both individuals who do and individuals who do not use ITNs. This means that ITN users and non-users face the same institutional and environmental conditions in each time period, which allows me to find a set of confounding variables that affect both ITN usage and health outcomes in each period.

Assumption 4 (Conditional Independence)**.** *Define X^Y as a set of confounding variables—a complete set of variables that affect both ITN usage and health outcomes. Denote Y_{TB} as the potential outcome at time T given behaviour B. Then*

$$Y_{11}, Y_{10} \perp B \mid X^Y$$

The crux here is identifying *all* of the variables that influence both ITN use and health outcomes, which requires quite a rich data set. If a confounding factor is omitted, the resulting estimate will be biased. Fortunately, the DHS surveys

include numerous questions covering many facets of health indicators. Section 4.1 describes each of the confounding variables, and a list can be found in the data appendix on page 43. Once the set of confounding factors has been constructed, it is necessary to ensure that for any given combination of these confounders, X^Y, the data set includes both ITN users and non-users.

Assumption 5 (Common Support)**.** *Define $p_1(x)$ as the post-intervention probability that an individual uses an ITN conditional on a set of X covariates. E.g. $\mathbb{P}(B = 1 \mid T = 1, X = x)$. Then*

$$0 < p_1(X) < 1.$$

The common support assumption guarantees that the probability of using an ITN (or not using one), given any specification of covariates is neither 1 nor 0. This means that for a covariate combination x, it is possible that the post-intervention individual uses an ITN, but also possible that the individual does not. If there were an instance in which $p_1(x) = 1$, then *every single individual* with characteristics x would be known to use an ITN with certainty. Similarly, $p_1(x) = 0$ would imply non-use with certainty. Were either of these cases to be true, it would be impossible to construct the counter-factual outcome of interest for this individual. Section 6 presents the results for several typical variations of the common support.

One final assumption must be imposed to derive the estimator of interest, the *stable unit treatment value assumption* (SUTVA), which is always needed if the ATE is to be unbiased.

Assumption 6 (SUTVA)**.** *The treatment of individual i affects only the outcome of that individual, and all individuals receive comparable treatment.*

It would seem obvious that one individual using a bed net has no effect on the health outcome of a different individual. However, ITNs work both by acting as a barrier between a mosquito and an individual as well as by repelling mosquitoes in near proximity and killing those that come in contact with the ITN. It is, therefore, possible that there would be fewer malaria carrying mosquitoes in a place where ITN usage and ownership is prevalent. Hawley et al. (2003) investigate these community-wide effects of ITNs to find that indeed ITNs have a protective effect for 300 meters. Additionally, statistically significant community-wide effects were found when village net coverage exceeded fifty percent. Although the DHS data do not include population data for the communities from which the sample was drawn, the percentage of households in a cluster that own ITNs can

reasonably approximate this percentage. Assumption violations are considered in Section 6.

Deuchert and Wunsch's semi-parametric estimator for the effect of an ITN distribution intervention on childhood mortality can now be derived.

Estimator. *Using Assumptions 1 through 6, the ATE for an intervention that affects health only through its effect on behaviour can be represented by* $\hat{\theta}$*, according to following definitions:*

- N_1*: post-intervention sample size.*
- y_{i1}*: outcome at time* $T = 1$ *for child* i*.* $y_{i1} = 1$ *indicates that the child has perished, whereas* $y_{i1} = 0$ *indicates that the child is still alive.*
- b_{i1}*: indicator for ITN use at time* $T = 1$ *for child* i*.* $b_{i1} = 1$ *indicates that the individual uses an ITN, whereas* $b_{i1} = 0$ *indicates that he or she does not.*
- $\hat{p}_1(x_{i1})$*: the predicted probability of ITN usage for individual* i *at time* $T = 1$ *as a function of characteristics* x_{i1}*. This estimate is obtained by performing a probit regression of* b_{i1} *onto the covariates and predicting the propensity scores for post-intervention individuals* ($\mathbb{P}(b_{i1} = 1 \mid T = 1, X_{i1} = x_{i1})$.
- $\hat{p}_0(x_{i1})$*: the predicted probability of ITN usage for individual* i *at time* $T = 0$ *as a function of characteristics* x_{i1}*. This estimate is obtained by performing a probit regression of* b_{i0} *onto the covariates and predicting the propensity scores for post-intervention individuals* ($\mathbb{P}(b_{i0} = 1 \mid T = 1, X_{i1} = x_{i1})$.

The estimator is then defined as:

$$\hat{\theta} = \frac{1}{N_1} \sum_{i=1}^{N_1} y_{i1} \left[\frac{b_{i1}}{\hat{p}_1(x_{i1})} + \frac{1 - b_{i1}}{1 - \hat{p}_1(x_{i1})} \right] [\hat{p}_1(x_{i1}) - \hat{p}_0(x_{i1})] \tag{2}$$

A proof for this estimator (as provided by Deuchert and Wunsch (2014)) is included in Appendix B.

This estimator is essentially the standard IPW estimator for the ATE of ITN use on child mortality in the post-intervention period (Wooldridge, 2001) multiplied by the probability of changing from non-ITN use to ITN use.

DHS data are an example of observational data, which is to say that individuals are not randomly assigned to the treatment. Under the unconfoundedness assumption (Assumption 4), determinants of ITN usage are assumed to be known. Therefore, IPW is used to overcome the selection bias (Rosenbaum and Rubin,

1983). The outcome for individuals who use an ITN is weighted by the inverse probability of ITN usage. Equivalently, the outcome for individuals who do not use an ITN is weighted by the inverse probability of not using one. Therefore, between the two terms, the first of which measures the effect of ITN use on child mortality and the second of which measures the change in behaviour induced by the intervention, there is an estimator for the effect of the intervention on early childhood mortality.

4 Methodology

Using the data described in Section 2, the following steps produce the estimator outlined in Section 3.

(i.) Predict $\hat{p}_0(x_{i1})$, the probability of ITN usage for individual i at time $T = 0$ as a function of characteristics x_{i1}. This is done by performing a probit regression of b_{i0} (ITN usage in the pre-intervention sample) onto the set of covariates and confounders and predicting the propensity scores for post-intervention individuals.

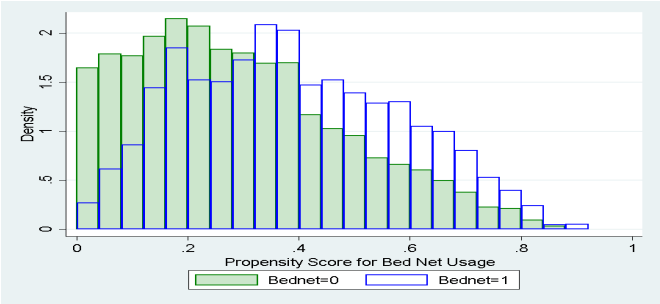
(ii.) Predict $\hat{p}_1(x_{i1})$, the probability of ITN usage for individual i at time $T = 1$ as a function of characteristics x_{i1}, in much the same way. This is done by performing a probit regression of b_{i1} (ITN usage in the post-intervention sample) onto the set of covariates and confounders and predicting the propensity scores for post-intervention individuals.

(iii.) Identify a common support so that the counter-factual outcome for individual i can be constructed. For the baseline approximation, I discard all post-intervention observations for which an ITN user (or non-user) does not have a comparable predicted propensity score, $\hat{p}_1(x_{i1})$, with a non-user (or user). For example, Figure 1 shows propensity scores of individuals at $T = 1$. The green bars represent the frequency of propensity scores for those who do not own bed nets; whereas the blue outlined bars represent those who do. The common support is then only the region in which these bars overlap. In this case, it is the interval $(0, 0.84)$. See Section 6 for additional implementations of the common support.

(iv.) Calculate the estimator using the remaining observations.

Unfortunately, data limitations prevent observation of death as a result of malaria. There is information about malaria-related symptoms, but as pointed out by Deuchert and Wunsch (2014), these variables are not suitable for measuring

Figure 1: Identifying a Common Support



The figure shows propensity scores at time $T = 1$ for individuals who do (and do not) own bed nets. Only the overlapping propensity scores may be used for the ATE estimation. All scores for which there is not both a treated and non-treated individual are dropped.

causal effects. They include things like whether the child had had a fever or received malaria treatment in the two weeks preceding the interview. This means that any infections occurring before this window, as well as any infection suffered by a child who died, would go unaccounted for. Indeed, the unavailability of information regarding a child who no longer exists complicates both the treatment and the outcome variables.

Regarding the outcome variable—malaria induced death—I opt instead to focus on all-cause mortality. However, based on the fact that an ITN reduces all-cause mortality only via its effect on preventing malaria, this should not introduce bias into my estimator (Phillips-Howard et al., 2003, Deuchert and Wunsch, 2014). Using data about births and deaths of all of a respondent's children, a measure for all-cause mortality for children strictly younger than three was created. That is, all children who died in the three years preceding the second interview. The maximum hypothetical age at death is limited to strictly less than three so that the children who died in the years preceding the second interview would have been fully exposed to the intervention. Table 2 presents the mortality rates for each country. The first three rows give the mortality rates for children younger than three in the pre-intervention period (measured in number of deaths per 1,000 live births per year); the last three rows do this for the post-intervention period. It is worth noting that in the pre-intervention period, the under-three mortality rate is higher for households that do not own an ITN in Rwanda, though the rates are generally the same across ITN ownership types. It is again true in the post-intervention sample, although the discrepancy in Rwanda is a bit more striking—the post-intervention mortality rate in Rwanda for non-ITN owners is more than twice that of ITN owners. It is also unsurprising to see a drop in

Table 2: Under 3 Mortality Rate by ITN ownership and Treatment Status

Treatment Status	ITN	Under 3 Mortality Rate			
		Rwanda	Senegal	Uganda	Zimbabwe
pre-intervention	No	29.5 (1.44)	21.0 (1.15)	25.9 (1.39)	23.3 (1.59)
	Yes	18.4 (2.2)	21.4 (1.87)	23.2 (2.74)	24.2 (4.58)
	Total	27.1 (1.23)	21.1 (0.98)	25.4 (1.24)	23.4 (1.50)
post-intervention	No	33.0 (5.22)	14.5 (1.68)	21.5 (2.13)	20.1 (1.61)
	Yes	15.3 (1.00)	16.4 (0.93)	18.2 (1.32)	20.9 (2.36)
	Total	16.5 (1.00)	16.0 (0.82)	19.3 (1.13)	20.4 (1.33)

Numbers represent deaths in children with a hypothetical strictly less than three years old that had died per 1,000 live births per year, according to intervention status and ITN ownership. Standard errors in parenthesis.

these rates in the post-intervention period since each country launched a series of interventions aimed at decreasing early childhood mortality. This paper strives to estimate just how much of this decrease is due to new ITN use.

With respect to the treatment variable—ITN usage—I elect to use ITN ownership as a proxy. The DHS data do not include information on whether a child typically sleeps under an ITN, nor does it include information on whether a child who had died had slept under an ITN. The only information on children's ITN usage is whether the child slept under a bed net the night before the survey. This is problematic for a couple of reasons:

(i.) an interview taking place during the dry season (when malaria risk is low) will not provide representative information for the child's behaviour during the rainy season (when malaria risk is higher), and

(ii.) any deceased child would not have slept under a bed net the night preceding the interview.

Deuchert and Wunsch (2014) argue that in lieu of ITN usage, bed net ownership can be used as the treatment variable. They found that the rate of infant mortality due to bed net ownership was comparable to randomized controlled trials on the efficacy of ITNs in decreasing all-cause infant mortality. The intervention programs in question, however, are designed to promote the use of ITN nets. Furthermore, in 2011, 96% of individuals who live in households that own an ITN actually use that ITN (World Health Organization, 2011). This suggests

Table 3: ITN Ownership Proportions

	ITN Ownership			
	Rwanda	Senegal	Uganda	Zimbabwe
pre-intervention	0.1604 (.0036)	0.2398 (.0050)	0.1652 (.0039)	0.1079 (.0051)
post-intervention	0.8192 (.0034)	0.7385 (.0050)	0.5998 (.0052)	0.3208 (.0073)

Proportion of sample population that owns at least on ITN in each time period. Standard errors for the mean in parenthesis.

that the largest barrier to ITN usage is simply ITN ownership. Moreover, as mentioned earlier, Hawley et al. (2003) found that proximity to ITNs also provides a strong protective effect; therefore, having an ITN in the household should provide some protective effect against malaria, regardless of whether the child in question typically sleeps under the ITN.

On the other hand, the intra-household gap between ITN ownership and usage (especially for children under five) is also well documented. In a cross-sectional study of fifteen African countries, Eisele et al. (2009) found that, after controlling for survey season and year, "access to ITNs is the strongest and most consistent determinant of use among children." Furthermore, they claim that gap between possession and usage will persist unless a household can achieve a ratio of no more than two members per ITN. In fact, the Roll Back Malaria Monitoring and Evaluation Reference Group (MERG) has since specified this measure as a malaria outcome indicator (MER, 2011). Kilian et al. (2013) performed a comprehensive analysis of universal coverage based on the new indicator. They concluded that the intra-household access gap (that is, the number of people who live in households that own an ITN, but who do not have access to its use) is considerable and consistent across all samples. Therefore, I use ITN ownership as a proxy for ITN usage for the motives listed above. However, it may very well be a poor proxy, and could lead to substantial underestimation of the treatment effect of the ITN campaign. Table 3 presents a quick glance of the proportion of ITN owners in the pre- and post-intervention sample populations. Table 4 provides some information about ownership versus usage of ITNs for living children under 5, and Section 6 also addresses this issue.

4.1 Control Variables

Table 4: Previous Night's Bed Net Usage by Under 5s in Households with ITNs

	Under 5s' Usage	Rwanda	Senegal	Uganda	Zimbabwe
pre-intervention	None	212 17.8%	963 53.9%	229 23.4%	221 62.7%
	All	697 58.7%	375 21.0%	520 53.2%	93 26.4%
	Some	279 23.5%	450 25.2%	228 23.3%	38 10.8%
	Total	1,188	1,788	976	352
post-intervention	None	852 16.8%	1643 31.4%	765 22.1%	725 61.7%
	All	3,640 71.7%	2,011 38.4%	2,024 58.4%	343 29.2%
	Some	588 11.6%	1,579 30.2%	678 19.6%	107 9.1%
	Total	5,080	5,233	3,466	1,175

Number and percentage of living children under 5 years old who slept under a bed net the night preceding the interview, given that the household owned an ITN. The survey did not specify whether the net the child slept under was an ITN, only that the household owns at least one ITN.

This section details the variables used to control for all relevant time-varying determinants of ITN usage, as well as variables that affect both ITN usage and health incomes, as required by Assumptions 3 and 4. Recall that Steps 1 and 2 of the methodology require performing a probit regression of ITN usage (actually its proxy, ownership) onto a set of covariates and confounders, X, in order to predict the propensity score for ITN ownership in each period. This section delineates this set of control variables, X.

Most explanatory variables are categorical variables which have been converted into a group of dummy variables. For example, the variable for the mother's ages was grouped into blocks: 15-19, 20-24, 25-29, 30-34, 35+. A dummy was then created for each group. In this example, this means that five dummy variables were created—one for each age block. I.e., MAge1 represents ages 15-19; MAge2 represents 20-24; and so on. When using categorical dummy variables in a regression, only $n-1$ dummies can be included. If all dummies were to be included, it would cause perfect multicollinearity (Greene, 2011, p. 152). This effect is referred to as *the dummy variable trap*. The dummy excluded from the regression in this paper is typically the one with the largest mean. (However, since I use the results only to calculate the ATE—that is, I do not interpret the coefficients of the probit regression–this is not of great importance.) For example, if I were considering mother's age as the only explanatory variable, the regression would look like the following:

$$\begin{aligned}&\mathbb{P}[\text{itn}_i = 1 \mid \text{MAge1}_i, \text{MAge2}_i, \text{MAge4}_i, \text{MAge5}_i] \\ &= \Phi(\text{MAge1}_i'\beta_1 + \text{MAge2}_i'\beta_2 + \text{MAge4}_i'\beta_3 + \text{MAge5}_i'\beta_4),\end{aligned} \tag{3}$$

where MAge3 has the largest mean and is, therefore, omitted.

In the event that an explanatory variable contains missing observations, a dummy variable for missing observations is also created. Again, using mother's age as an example, there would now be six dummy variables—one for each age block and an additional dummy for missing observations. I.e., MAge1 represents missing observations; MAge2 represents ages 15-19; and so on. For these cases, the dummy for missing observations is omitted from the baseline regression. In other words, the missing observations are grouped together with whichever dummy is left out of the regression to avoid the dummy trap. Analogous to the previous example, the regression would look like the following.

$$\begin{aligned}&\mathbb{P}[\text{itn}_i = 1 \mid \text{MAge2}_i, \text{MAge3}_i, \text{MAge5}_i, \text{MAge6}_i] \\ &= \Phi(\text{MAge2}_i'\beta_1 + \text{MAge3}_i'\beta_2 + \text{MAge5}_i'\beta_3 + \text{MAge6}_i'\beta_4)\end{aligned} \tag{4}$$

where MAge1 represents missing observations and Mage4 has the largest mean.

To assess the impact of the missing observations, I also perform a probit regression that includes the dummy variable. This would change Equation 4 to look like the following.

$$\begin{aligned}&\mathbb{P}[\text{itn}_i = 1 \mid \text{MAge2}_i, \text{MAge3}_i, \text{MAge5}_i, \text{MAge6}_i, \text{MAge1}_i] \\ &= \Phi(\text{MAge2}_i'\beta_1 + \text{MAge3}_i'\beta_2 + \text{MAge5}_i'\beta_3 + \text{MAge6}_i'\beta_4 + \text{MAge1}_i'\beta_5)\end{aligned} \tag{5}$$

I check the estimation results to see if the estimation coefficients are significantly different from zero. If they are not, I can conclude that the missing observations do not affect the ATE estimator. Section 6 investigates how much missing observations may have affected the estimation.

In addition to the categorical dummy variables, the few continuous variables (such as mother's age) are also included in the regressions to capture any non-linearities. Tables A through A.4 describe the control variables used for each country.

4.1.1 Socio-economic Status

The correlation between socio-economic status (SES) and health outcomes, as well as health seeking behaviour, is very robust and well established—so well established, in fact, that the term used to describe it is the *SES health gradient*. As one of the first to establish the link, Grossman (1972) developed a model which shows that individuals invest a commodity "good health" if the expected benefits outweigh the cost. This idea was further strengthened by the Whitehall Studies, a longitudinal investigation into the relationship between pay grade and coronary heart disease of 17,530 British civil servants from 1947-1972 (Marmon et al., 1978). The authors found pay grade to be the strongest indicator for mortality. Unfortunately, the Whitehall Study included only male British civil servants aged 40-64.

Marmon et al.'s finding, however, has also been extended to the relationship between SES and heath-seeking behaviour concerning malaria in sub-Saharan Africa. Oresanya et al. (2008), Dupas (2009) and Njau et al. (2013) all found a strong relationship between bed net usage and wealth. In fact, Njau et al. found that household wealth exhibited by far the strongest association with ownership of bed nets, and, furthermore, that it was the strongest predictor of children's bed net use. Oresanya et al. (2008) found that the wealth index was only important in combination with the education level of the caregiver. Mugisha and Arinaitwe (2003) found that bed net ownership is highly correlated with a household's wealth

index; however, they also find that this is not a good indicator for bed net usage for children under five. Therefore, the link between wealth and bed net usage is somewhat delicate. It is, nonetheless, included as a control variable.

Measuring wealth in developing countries proves to be a strikingly difficult task, not the least because of the lack of stable or measurable income. The data used in this paper (and also that of several papers cited above) are primarily concerned with gathering information about health. Therefore, the surveys provide no information about consumption expenditures, which are a good measure for current and long-run economic status (Deaton and Zaidi, 2002). Filmer and Pritchett (2001) introduced an innovative approach using principal components analysis to estimate long-run economic status. They established that principal components analysis provides plausible and defensible weights for an index of assets to serve as a proxy for wealth. Rutstein and Johnson (2004) applied this method to create the wealth index included in the DHS data. The index is calculated by selecting a series of shared and country-specific indicator variables. Nearly all household assets and utility services are included. Generally, any item that will reflect economic status is used (i.e. bicycle ownership, television ownership, drinking water source, etc.). Two additional items are also constructed for most surveys: whether there is a domestic servant and whether the household owns agricultural land.

Unfortunately, the DHS wealth index variables are not comparable across country or time. Therefore, Rutstein and Staveteig (2014) outlined a procedure for making the country-specific DHS wealth indexes comparable to one another through the use of a baseline survey and linking (or anchoring) items that are present in almost all DHS surveys carried out since the 1990s. Therefore, the wealth index used in this survey is the *comparative wealth index* provided by Rutstein and Staveteig (2014).

4.1.2 Household Characteristics

In addition to long-term socio-economic status, several other factors affect bed net usage and child health outcomes: age, sex, and education level of household head, number of children under five, and overall number of household members. Njau et al. found that the age, education level, and sex of the household head had an impact on children's bed net usage. They found that households headed by males were less likely to use a bed net for children, given that the household owned a bed net, and even less likely to own a bed net in the first place. Furthermore, younger household heads were also significantly more likely to both own bed nets and use them for children under five.

The number of children under five that live in a house increases the chance of receiving at least one ITN via the intervention. Moreover, the distribution of household resources to any one child depends on both the age of the child and the number of other children in the house (Oresanya et al., 2008, Yarnoff, 2011). The number of members in the household also serves to explain a child's resource allocation status.

4.1.3 Child's Characteristics

Child characteristics considered are whether the child is his or her mother's youngest child, age (hypothetical in the case of deceased children), size at birth, and place of delivery. Whether it is because youngest children are seen as more vulnerable or whether it is because youngest children are more likely to sleep with their mothers is unclear; what is clear, however, is that the youngest child in a household is significantly more likely to live in a household that owns a bed net and to sleep under a bed net (Mugisha and Arinaitwe, 2003). Size at birth and place of delivery are meant to capture how much exposure the child has had to health clinics—one critical distribution mechanism for ITNs. Furthermore, a child's size at birth is highly correlated with probability of death and would likely affect the intra-household distribution of resources, making a household more likely to own and use a bed net.

4.1.4 Mother's Characteristics

Maternal characteristics under consideration for influence on net ownership and usage include her age, education level and health literacy, work status, access to media, religion, whether she has final say on her health decisions, how many children she has had, and whether any of her previous children has died. A mother's education has an undisputed effect on a household's bed net ownership and usage for children under five. Oresanya et al. (2008) found that mother's education increased a household's likelihood to own a bed net by 30%. In fact, the link between a mother's education level and net ownership and usage is well documented (Dupas, 2009, Belay and Deressa, 2008, Pettifor et al., 2008, Deuchert and Wunsch, 2014). However, it is not just a mother's eduction that is important, but her health literacy—specifically literacy related to child morbidity and mortality (Kovsted et al., 2002). In fact, Kovsted et al. find that a mother's health literacy eclipses the importance of her education level when considering child mortality. Health literacy is captured by three variables: knowledge about dehydration and the usage hydration salts, whether any steps are taken to purify the household's

drinking, and whether a family planning worker has visited in the year preceding the interview.

Njau et al. (2013) found that access to media, defined as ownership and use of a television set or radio or reading newspapers at least once a week, had a significant effect on a child's bed net use in Tanzania. Furthermore, it is primarily via television and radio that households are exposed to information about malaria prevention and also about the presence of ITN campaigns in the area. Access to media in this paper is measured by household ownership of TV and radio.

As previously mentioned, the higher likelihood of households with female heads to own bed nets and use them for children under five speaks to the level of a mother's bargaining power (Yarnoff, 2011). A mother who earns money has much more bargaining power than one who does not, which is why the mother's working status is included as a control variable. To further capture a mother's bargaining power, I include whether she alone (also jointly with her partner) has the final say on her health decisions.

Although it may also be considered a household characteristic, a mother's religion is known to be an important factor in bed net ownership, perhaps due to the social influence of local religious leaders. However, when simultaneously considering a mother's education as a control variable, religion became less (or not at all) important (Oresanya et al., 2008).

Treating the household's drinking water is excluded as a control variable for Rwanda due to missing observations. Knowledge of hydration salts is excluded as a control variable for Zimbabwe for the same reason.

4.1.5 Regional Characteristics

Regional profiles are another important factor in both child mortality and bed net ownership. In fact, in part because of the distribution schemes, the region in which a household lives is a very significant factor in whether the household owns an ITN (Mugisha and Arinaitwe, 2003, Oresanya et al., 2008, Deuchert and Wunsch, 2014). Indeed, due to civil wars and regional conflicts, many of the regions under consideration have suffered debilitating infrastructure damage. This leaves these regions without health posts and without proper roads which make both access to health care and access to the bed net distribution programs very difficult (Njau et al., 2013). Additionally, there is a firmly established disparity in health outcomes between urban and rural areas (Stock, 1983, Auchincloss and Hadden, 2002). Moreover, natural phenomena such as droughts and floods occur on a regional basis throughout the region in question. Therefore, region as well as distance to the nearest city are included as control variables.

In addition to the regional and type of residence effects, it is important to take into account that malaria is passed by mosquitoes who thrive on standing freshwater. Kaufmann and Briegel (2004) determined that the mosquitoes which pass malaria can fly up to twelve kilometres, which is why distance to nearest lake (either in the country or a neighbouring country) is included as a control variable. Lastly, altitude is also included as a control variable since it is known to indirectly affect malaria via its impact on temperature.

The GPS coordinates for the sample clusters were provided by DHS. GPS coordinates for all cities and towns in Africa were obtained from Global Rural-Urban Mapping Project, Version 1 (GRUMPv1): Settlement Points provided by GRU (2011). And the GPS coordinates for inland water were acquired from the Digital Chart of the World. These files were loaded into a geographic information system (GIS) program which then calculated distance measurements. The distance between a cluster and the nearest city is the great circles distance between two points. The distance between a cluster and the nearest inland water body is calculated as the shortest great circles distance between the cluster point and the entire water body polygon.

5 Results

As described in the Section 4, two probit models are estimated for each country—one using pre-intervention data and one using post-intervention data. The results of these regressions can be found in Appendix A. These regression coefficients are then used, alongside the treatment and outcome variables (ITN ownership and death of a child under three) to calculate the estimator in Equation 2. Table 5 presents the ATE for varying common support enforcements. The baseline estimator reported in the first row of Table 5 uses the min-max rule employed by Dehejia and Wahba (2002). This support discards propensity scores that fall below the lowest propensity to own an ITN ($P[b_{i1} = 1|T = 1, X_{i1} = x_{i1}]$) or that lie above the greatest propensity to not own an ITN ($P[b_{i1} = 0|T = 1, X_{i1} = x_{i1}]$). Since the IPW estimate is sensitive to very large propensity scores, the estimator is also calculated by a) trimming all scores above 0.90, 0.95, and 0.99 to this respective value and trimming all scores below 0.10, 0.05, and 0.01 as well, and b) discarding all propensity scores that fall outside of these intervals.

In the case of Rwanda, the baseline estimate is highly positive, representing 7.9 all-cause deaths in children under three avoided due to the ITN intervention (per 1,000 live births per year). Discarding the propensity scores that fall outside the different common support structures only serves to strengthen the estimator.

Table 5: Estimated ATE of ITN Intervention for Varying Support Enforcements

Estimator	Rwanda	Senegal	Uganda	Zimbabwe
Baseline	**7.9**	**-1.0**	**1.5**	**0.3**
	(1.0, 18.8)	(-3.3, 2.7)	(-1.1, 5.2)	(-1.8, 2.5)
No support	7.5	-1.0	1.5	0.2
	(0.6, 17.6)	(-3.4, 2.4)	(-1.2, 1.9)	(-1.9, 2.4)
Discard to 10th largest/smallest	11.0	-0.4	1.8	-0.4
	(1.5, 19.2)	(-3.2, 3.0)	(-1.1, 5.1)	(-2.4, 2.1)
Discard to .10/.90	10.5	-0.4	1.5	0.3
	(0.3, 22.1)	(-3.4, 2.0)	(-1.5, 4.4)	(-2.1, 2.7)
Discard to .05/.95	8.8	0.1	1.7	0.2
	(0.9, 18.7)	(-3.1, 2.8)	(-1.1, 4.8)	(-2.1, 2.6)
Discard to .01/.99	9.2	-1.0	1.5	0.2
	(1.3, 18.5)	(-3.4, 2.5)	(-1.2, 5.0)	(-1.9, 2.4)
Trimmed to 10th largest/smallest	7.6	-1.0	1.5	0.0
	(0.6, 15.2)	(-3.4, 2.4)	(-1.2, 4.9)	(-2.0, 2.2)
Trimmed to .10/.90	1.0	-1.6	1.3	0.1
	(-2.6, 4.1)	(-3.7, 0.9)	(-1.4, 4.0)	(-1.9, 2.3)
Trimmed to .05/.95	4.1	-1.0	1.5	0.2
	(-2.6, 4.1)	(-3.7, 0.9)	(-1.4, 4.0)	(-1.9, 2.3)
Trimmed to .01/.99	7.5	-1.0	1.5	0.2
	(-2.6, 4.1)	(-3.7, 0.9)	(-1.4, 4.0)	(-1.9, 2.3)

95% confidence interval in parenthesis as obtained by performing 1999 bootstrap simulations. Values represent the number of avoided deaths per 1,000 for children strictly less than three years old per year due to the ITN intervention.

For all supports that discard the unmatched propensity scores, this estimate is significant. However, if the scores are instead trimmed to 0.9, 0.95, or 0.99 (and 0.1, 0.05, and 0.01, respectively) the estimate is no longer significant.

The results for all three other countries are quite small and are statistically insignificant with every type of common support enforcement. Therefore, I cannot say whether the ITN campaign had any effect on all-cause infant mortality.

The next set of checks addresses how successful the data are at controlling for all time-varying determinants of ITN ownership and early childhood mortality. Although, this assumption is not testable, Deuchert and Wunsch (2014) suggests omitting different blocks of explanatory variables to see how sensitive the estimator is to these changes. Table 6 details the response. The overall result is that the estimators are quite robust. Ignoring momentarily that the estimator is not significantly different from zero (at a 95% confidence level) for Senegal, Uganda, and Zimbabwe, the estimator for each set of covariates is quite similar within each country. In Rwanda, the largest positive difference is seen when omitting the household characteristics. However, when including only the non-wealth related household characteristics the efficacy of the campaign drops slightly. The

Table 6: Estimator Sensitivity to Covariates and Confounders

Estimator	Rwanda	Senegal	Uganda	Zimbabwe
Without survey weights	8.0	-1.4	1.0	-0.3
	(1.2, 20.0)	(-3.8, 1.6)	(-1.6, 4.1)	(-1.9, 1.7)
No child characteristics	8.7	-1.1	1.8	0.2
	(0.9, 21.6)	(-3.5, 2.5)	(-1.1, 6.2)	(-1.8, 2.5)
No mother characteristics	8.3	-0.6	2.0	0.1
	(1.6, 19.5)	(-3.0, 2.8)	(-0.8, 5.5)	(-1.6, 2.1)
No region characteristics	7.9	-2.3	-0.4	0.1
	(2.5, 18.4)	(-4.4, 0.4)	(-2.7, 2.1)	(-1.3, 1.6)
No household characteristics	8.9	-1.3	3.1	-0.2
	(1.0, 21.6)	(-3.6, 1.8)	(-0.1, 7.7)	(-2.0, 1.8)
No wealth characteristics	7.7	-1.2	1.8	-0.2
	(0.7, 19.4)	(-3.6, 2.5)	(-1.1, 6.2)	(-1.9, 2.0)

95% confidence interval in parenthesis as obtained by performing 1999 bootstrap simulations. Values represent the number of avoided deaths per 1,000 for children strictly less than three years old per year due to the ITN intervention.

strongest factors keeping the mortality rates from dropping in Senegal appear to be the regional characteristics. This could be a result of the execution of the campaign, or more likely due to internal natural and political factors. The strongest factor in keeping mortality rates from dropping in Uganda are the household characteristics. However, if you include only the wealth factors of the household characteristics, this is not so much a problem. Therefore, the thing keeping the rates from dropping must be related to the household head or the household's membership make-up. Again the estimates for Zimbabwe are very close to zero with rather tight confidence intervals, so one can comfortably reject the hypothesis that the ITN campaign had any effect on the all-cause early childhood mortality rate there.

6 Plausibility Checking

Section 4 details all of the joint factors of ITN usage and child mortality that allow the use of the estimator established in Deuchert and Wunsch (2014). In this section, I perform a few tests to check the validation of these assumptions.

Missing observations are always an obstacle when carrying out empirical evaluations. Insufficient observations prevent the model from being correctly specified. Table 7 describes the percentage of missing observations for the variables for that have any missings.

Table 7: Percentage of Missing Observations

	Rwanda		Senegal		Uganda		Zimbabwe	
	pre-	post-	pre-	post-	pre-	post-	pre-	post-
Size at Birth	0.4	0.4	0.8	0.8	0.9	2.5	1.6	2.4
Place of Delivery	0.2	0.0	0.3	—	0.1	0.1	0.2	0.0
Religion	0.7	0.2	0.1	—	0.1	—	—	—
Final Say	0.6	16.3	0.2	6.7	13.6	13.4	14.7	15.2
Drinking Water Treated	N/A	N/A	0.3	0.0	0.0	0.1	0.1	0.1
Barriers to Health Access	0.1	0.0	0.0	—	—	0.2	0.0	—
Mother Works	—	—	0.1	—	0.1	0.2	—	—
Hydration Salts	0.5	0.6	0.5	—	0.3	0.4	N/A	N/A
Family Planner Visit	0.9	—	0.7	—	0.1	0.1	—	—
Access to Media	0.1	0.0	0.1	—	—	0.1	—	—
ITN	—	—	0.1	—	—	—	—	—
Age of Household Head	—	—	0.2	0.5	—	0.0	—	—
Education of Household Head	0.4	0.3	1.6	2.5	0.7	0.8	0.3	1.3
Altitude	—	—	3.0	1.9	—	—	—	—
Distance to Lake	1.1	—	3.0	1.9	8.3	1.1	0.7	0.6
Distance to City	1.1	—	3.0	1.9	8.3	1.1	0.7	0.6

Percentage of missing observations per country per intervention period. Variables not included in the table have no missing observations. N/A corresponds to variables that were not available and therefore were not used in the probit regressions.

As described in Section 4, for each of the variables in Table 7 there exists a dummy variable that is 1 for missing observations and 0 otherwise. Additionally, a missing entries indicator variable was created. This variable takes the value 1 for an individual who is missing an observation for *any* explanatory variable. As can be seen in Table 8, when including the missing entries indicator in the regression, the treatment effect of the ITN campaign for all countries remain virtually unchanged. Furthermore, the coefficient for the missingness indicator variable in the probit regression is not significant. Therefore, the missing observations in the sample data do not affect the estimation.

Section 3 imposed some pretty strict assumptions on the estimator for the ATE of ITN ownership on child mortality due to a nationwide ITN distribution campaign. In what remains of this section, bounds for the estimate are derived, and several empirical assessments are performed to assess assumption violations.

To identify the effect of the ITN campaigns, it is necessary to observe all determinants ITN usage that may have changed between the pre- and post-intervention periods. For example, in mid-2006 the Lord's Resistance Army and the Ugandan government entered into peace talks after which displaced individuals made their way back to their original homes over the course of the next several years. (The United Nations Human Rights Council (UNHRC) closed its camps for internally

Table 8: Effect of ITN Intervention Including Missingness Indicator

	Missing Entries Indicator	
	Effect	95% C.I.
Rwanda	7.7 (0.113)	(1.0, 19.3)
Senegal	−1.1 (0.033)	(-3.4, 2.5)
Uganda	1.4 (0.053)	(-1.3, 5.3)
Zimbabwe	0.3 (0.036)	(-1.5, 2.8)

Standard errors in parenthesis and 95% confidence interval obtained by performing 1999 bootstrap simulations.

displaced persons (IDPs) in January 2012 citing that most displaced persons had returned home at this time.) This and many other factors resulting from conflict would have changed greatly between the 2006 and 2011 surveys. Another example would be the periodic, yet severe, droughts and floods that affect different parts of the continent over the years. For example, Rwanda experienced heavy (and very costly) flooding in 2007 and 2009. What is more is that the data do not observe certain important changes within a geographic location. For example, the data do not account for more (or better quality) health clinics. They do not account for infrastructure, such as the pavement of existing roads, or the creation of new roads.

For these reasons, bounds for the ATE of the campaign are derived. As explained in Deuchert and Wunsch (2014), exploiting the monotonicity assumption (Assumption 2) and the fact that the ITN ownership rate is bound between 1 and 0, it is clear that the lower bound on the effect of the campaign on ITN ownership (as a proxy for usage) is 0. That is, the least possible change in early childhood mortality is nothing. The most change is then defined as the ATE of ITN ownership on all-cause early childhood mortality multiplied by the maximum possible positive change in behaviour. The ATE of ITN ownership (based on Assumptions 4 and 5) is calculated according to Equation 6 (results shown in Table 10). The maximum possible positive change in behaviour is the difference between 1 and the ITN ownership rate at $T = 0$. (See Table 3 for ITN ownership rates at $T = 0$.) Multiplying these two numbers yields a bound on the effect of the campaign, as listed in Table 9. This upper bound on change is effectively the estimator from Equation 2 for the case in which every single household that did not own an ITN in period $T = 0$ obtained an ITN by period $T = 1$. Therefore, Assumption 3 (i.e. that conditioning on relevant time-varying determinants of

Table 9: Bounds on the Treatment Effect of the ITN Campaigns

	Effect	Bounds	Assumption Violated?
Rwanda	7.9	(0, 9.8)	No
Senegal	−1.0	(-1.5, 0)	No
Uganda	1.5	(0.0, 2.1)	No
Zimbabwe	0.3	(-1.1, 0)	Yes

ITN usage the behaviour of the pre-intervention population may be used as a proxy for no-intervention behaviour of the post-intervention population) cannot be rejected for Rwanda, Senegal, and Uganda. However, the average treatment effect for Zimbabwe falls slightly outside this boundary, indicating that the condition before-after assumption has likely been violated.

To check whether the estimates suffer from omitted variable bias, I calculate the ATE of ITN ownership on all-cause early childhood mortality using an IPW estimator on ITN ownership on all-cause early childhood mortality for each time period.

$$\hat{\Delta}_{\text{ATE}} = \frac{1}{N_T} \sum_{i=1}^{N_T} y_{Ti} \left(\frac{b_{Ti}}{\hat{p}_T(x_{Ti})} + \frac{1 - b_{Ti}}{1 - \hat{p}_T(x_{Ti})} \right) \quad T \in \{0, 1\}, \tag{6}$$

where N_T is the number of individuals at time T, y_{Ti} is the outcome for individual i at time T, b_{Ti} is the ITN ownership status for individual i at time T, and $\hat{p}_T(x_{Ti})$ is the propensity score for ITN ownership of individual i at time T, conditional on characteristics x_{Ti}. If there is no bias, this estimate should closely approximate the estimates for this value attained by Phillips-Howard et al. (2003) and the trials compared in Lengeler et al. (1998). Phillips-Howard et al. (2003) evaluated a controlled, randomized trial of ITNs conducted over two year periods in western Kenya. They investigated all-cause mortality in children aged 1-59 months. Lengeler et al. (1998) assesses the absolute decrease in all-cause mortality in child children 1-59 months using data compiled from four large-scale, randomized controlled trials in four African nations. If the numbers I calculate using the DHS data match the results from these trials, I can presume that my results do not suffer from bias due to non-random allocation into control and treatment groups. These estimators are presented in Table 10.

Unfortunately, the data from Phillips-Howard et al. and Lengeler et al. are reported for children 1-59 months; whereas the data in this paper are for children 0-35 months. However, the great majority of child deaths occur within the first

Table 10: Average Treatment Effect of ITNs

Country/Area	Intervention	ITN owner group death rate (/1000/year)	Non-ITN owner group death rate (/1000/year)	Rate difference[a] (deaths/1000 /year)
Gambia [b]	Treatment of existing nets	18.7	24.3	-5.6 (-9.2, -2.0)
Kenya [b]	Distribution of ITNs	9.4	13.2	-3.8 (-6.6, -1.1)
Ghana [b]	Distribution of ITNs	28.2	34.2	-6.0 (-9.5, -2.3)
Burkina Faso [b]	Distribution of ITNs	41.8	48.7	-6.9 (-11.8, -2.0)
Western Kenya [c]	Distribution of ITNs	43.9	51.9	-8.0 (-13.0, -3.0)
Rwanda (pre-intervention)	Distribution of ITNs	26.8	28.1	-1.4 (-11.4, 11.3)
Rwanda (post-intervention)	Distribution of ITNs	15.4	27.9	-12.4 (-31.1, -0.5)
Senegal (pre-intervention)	Distribution of ITNs	23.4	20.5	2.9 (-1.7, 8.6)
Senegal (post-intervention)	Distribution of ITNs	16.3	14.2	2.1 (-3.5, 6.5)
Uganda (pre-intervention)	Distribution of ITNs	16.1	25.3	-9.2 (-15.2, -3.4)
Uganda (post-intervention)	Distribution of ITNs	19.3	21.8	-2.6 (-10.9, 2.7)
Zimbabwe (pre-intervention)	Distribution of ITNs	25.4	23.3	2.1 (-13.0, 65.6)
Zimbabwe (post-intervention)	Distribution of ITNs	22.0	20.7	1.3 (-6.2, 10.5)

[a] 95% confidence interval in parenthesis
[b] Results from Lengeler et al. (1998) for children 1-59 months.
[c] Results from Phillips-Howard et al. (2003) for children 1-59 months.
Rate difference calculated using Equation 6. Intervention group deaths are calculated using only the first term in the parenthesis of Equation 6; control group deaths calculated using only the second. Results for children 0-35 months.

Table 11: Average Treatment Effect of ITNs Across Periods

Country	Pre-intervention ATE (deaths/1000/year)	Post-intervention ATE (deaths/1000/year)	Difference between periods
Rwanda	-1.4 (-11.4, 11.3)	-12.4 (-31.1, 0.5)	-11.1 (-33.3, 5.2)
Senegal	2.9 (-1.7, 8.6)	2.1 (-3.5, 6.5)	-1.5 (-9.1, 5.5)
Uganda	-9.2 (-15.3, -4.1)	-2.6 (-10.4, 3.3)	6.6 (-2.6, 15.5)
Zimbabwe	2.1 (-13.2, 62.7)	1.3 (-6.6, 11.2)	-0.8 (-61.8, 17.9)

95% confidence interval in parenthesis
ATE calculated using Equation 6. Results for children 0-35 months.

twelve months with rates very low after 35 months. For this reason, the rates for this paper should be slightly higher than those reported for comparison. The estimates for Rwanda and Uganda are, in fact, reasonable estimates, and the rate difference for post-intervention Rwanda, as well as the pre-intervention Uganda are statistically significant at a 95% confidence level. The ATE for ITN ownership on all-cause child mortality in all other cases is statistically insignificant, which means I cannot reject the hypothesis that ITN ownership has no effect on all-cause early childhood mortality.

Additionally, to ensure that all time-varying determinants of health and ITN ownership have been accounted for, I check whether the ATE of ITN ownership on early childhood mortality is not different between the two time periods. These results are presented in 11. It can be seen that although only the post-intervention ATE in Rwanda and only the pre-intervention ATE in Uganda are significant, the difference between ATEs of each period are insignificant in every case. This means that, at a 95% confidence level, I cannot reject the conclusion that the ATE did not change between the two time periods. This indicates that the assumption that all time-varying determinants of health and ITN ownership have been captured by the model.

Due to lack of data, this paper uses ITN ownership as a proxy for ITN usage. Therefore, the exclusion restriction (Assumption 1) requires the campaign to affect health only via ITN ownership, rather than ITN usage; it should not affect health by any other means. Several checks are preformed to assess whether the ITN intervention affects all-cause early childhood mortality only through ITN ownership. First is a comparison of the ATE of ITN ownership on early childhood mortality for each country at each time period. If all covariates and confounders

are accounted for, *and* if ITN ownership is a reasonable proxy for ITN usage, there should be no difference in the ATE in each period for each country. Unfortunately, it is clear (as reported in Table 10) that the difference between the efficacy of ITN ownership at preventing all-cause early child mortality is far from zero for Rwanda and Uganda. In Rwanda, the ATE at $T = 1$ is large and significant, whereas at $T = 0$ the ATE is not significant. Uganda also has an ATE that is significant in only one period. Therefore, the difference between the estimates at each period for these two countries is not zero. As implied above, although the covariates and confounders used in this paper are identical or very similar to those prevalently used in the literature, perhaps some omitted time-varying determinants of ITN are biasing the estimate.

Another plausible explanation is that due to increased awareness about the importance of ITNs, ITNs in the post-intervention sample are being used more efficiently. This would explain the increased rate difference in Rwanda. The direction of change for Uganda, however, is in the opposite direction. Which is to say that ITNs at $T = 1$ in Uganda are much less effective at preventing early childhood mortality than ITNs at $T = 0$.

Secondly mother's bed net usage is used to verify whether ITN ownership fulfils the exclusion restriction. Although there is no data about whether a child who died would have slept under a net the night before the survey, there is data about whether a mother slept under a net the night before the survey. If the exclusion restriction holds, a mother's bed net usage, conditional on owning an ITN, should not be different between the two intervention periods. This effect is again calculated using IPW, however, in this case the outcome is mother's bed net usage and the treatment is simply the time period.

$$\hat{\omega}_{\text{Mnet}} = \left(\frac{1}{N} \sum_{i:b_i=1} y_i \right) - \left(\sum_{i:b_i=0} \frac{\hat{p}(x_i)}{1-\hat{p}(x_i)} y_i \Big/ \sum_{i:b_i=0} \frac{\hat{p}(x_i)}{1-\hat{p}(x_i)} \right), \tag{7}$$

where N represents the number of bed net owners in the post-treatment sample; y_i represents the outcome, which is whether or not a mother slept under an ITN the night before the interview; and $\hat{p}_i$ is the predicted propensity of mother i to be part of the post-intervention sample given that she owns a bed net in the pre-intervention sample and conditional on characteristics x. All the same covariates and confounders are used as in the baseline estimation, though the entire calculation is conditioned on owning an ITN. This estimate measures the ATET of the campaign on mothers' usage given that the mother owns in ITN. In Uganda, the estimate suggests that it is much more common for the mother to use a bed net in the second period, although she owned an ITN in both time

Table 12: Effect of ITN Campaign on Other Outcomes

Country	Check	Effect	95% Confidence Interval
Rwanda	Mother's bed net usage	-23.2	(−52.8, 10.0)
	Received any vaccination	0.8	(−43.2, 12.4)
	Received Vitamin A	5.4	(−19.8, 39.2)
	Diarrhoea in last two weeks	6.7	(−3.8, 13.9)
	Cough in last two weeks	13.0	(−0.9, 22.2)
Senegal	Mother's bed net usage	12.8	(−201.3, 119.2)
	Received any vaccination	3.2	(−8.6, 6.9)
	Received Vitamin A	7.2	(−12.5, 7.4)
	Diarrhoea in last two weeks	-2.8	(−7.8, 6.9)
	Cough in last two weeks	-6.2	(−10.5, 0.3)
Uganda	Mother's bed net usage	-8.4	(−25.8, 25.4)
	Received any vaccination	0.9	(−9.7, 6.0)
	Received Vitamin A	2.9	(−10.0, 8.1)
	Diarrhoea in last two weeks	-1.1	(−9.9, 3.8)
	Cough in last two weeks	0.9	(−9.7, 6.0)
Zimbabwe	Mother's bed net usage	-27.1	(−163.2, 86.4)
	Received any vaccination	0.0	(−9.2, 3.8)
	Received Vitamin A	0.0	(−23.9, 6.7)
	Diarrhoea in last two weeks	0.0	(−7.6, 0.1)
	Cough in last two weeks	0.0	(−7.5, 0.3)

Numbers reported are the number of individuals per 1,000 per year whose outcome changed (direction indicated by sign) due to the ITN intervention.
The effect of mother's bed net usage is the ATET the intervention on mother's bed net usage, given that she owned an ITN in both periods (Equation 7).
All other effects are ATEs for the ITN campaigns.

periods. Unfortunately, as can be seen from the very wide confidence intervals, this test not very powerful. In Rwanda, Uganda, and Zimbabwe, the effect moves the other direction. I.e. Mother's are less likely to use a bed net in the post-intervention period, although they own an ITN in each period. Again, none of these statistics are significant. I therefore conclude that there is not compelling evidence to suggest that the exclusion restriction has not been violated. That is, if a household already owned an ITN—$b_i = 1$ at $T = 0$ and $T = 1$—then ITN campaign did not change the mother's bed net usage.

The next plausibility check tests whether the ITN campaign affected other household health investments. It is possible that the money a household saves by not having to purchase an ITN, as well as the change in preferences due to increased health literacy on account of the campaign, motivates the household to

increase the health investment for children under three. I estimate the effect of the campaign on whether the child receives a vaccination (for Tuberculosis; Diphtheria, Tetanus, and Pertussis; Measles; or Polio) and whether the child receives Vitamin A treatment, both of which decrease all-cause child mortality Sudfeld et al. (2010). If the ITN campaign has an effect on either of these variables, it means that the intervention affects early childhood mortality through more ways that just behaviour change from not using an ITN to using an ITN. In Rwanda, Senegal, and Uganda there appears to be a positive, though not statistically significant, effect of the campaign on other health investments. Zimbabwe shows no effect on vaccinations. However, in all cases the 95% confidence interval includes zero, and the hypothesis that the ITN campaign does not affect other health investments cannot be rejected, and the exclusion restriction is upheld.

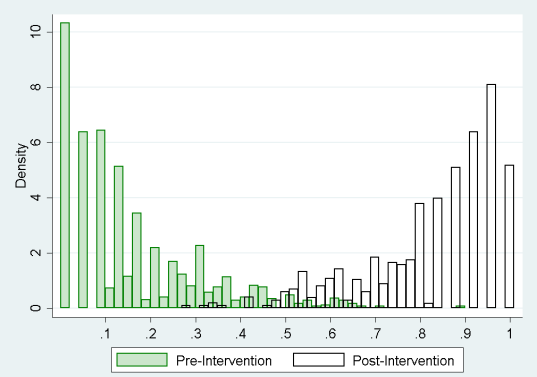
Finally, the placebo effect of the ITN campaign is considered, as proposed by Ashraf et al. (2014) and performed by Deuchert and Wunsch (2014). Diarrhoea and coughing are important health outcomes, but reductions in the prevalence of either one should in no way be related to ITN distribution since neither is a symptom of malaria. Therefore, the expected effect should be zero. The results are again found in Table 12. Rwanda shows a pretty strong increase in both symptoms, whereas Senegal shows a decrease in both. The results for Uganda are split, with the intervention causing a very slight increase in coughing and a very slight decrease in diarrhoea. The results from Zimbabwe perfect match the assumption, showing that the ITN intervention had no effect on diarrhoea or coughing. However, again the 95% confidence intervals all span zero, which means that I cannot conclude that the exclusion restriction has been violated.

As stated in Section 3, it has been assumed that ITN ownership affects child mortality only for the households who own them. However, Hawley et al. (2003) found that there is a protective community effect if the net coverage of a community exceeds 50%. Figure 2 plots the histograms for mean ITN ownership per cluster for each time period in each country. If the SUTVA assumption holds, the bars should appear only in the region $0 - 0.5$. It is assumed that the sample population from each cluster is representative of the cluster's community.

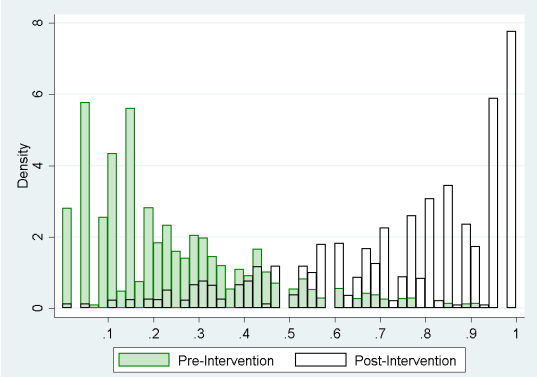
It is immediately clear that save for Zimbabwe, this assumption is handedly violated. In the pre-intervention time period, very few clusters have ITN coverage exceeding 50%. However, due to what could be considered the success of the ITN campaigns in Rwanda, Senegal, and Uganda, ITN coverage exceeds 50% for a great deal of the post-intervention communities. Presumably due to the lack of broad international investment in Zimbabwe, over 50% coverage is quite low for all Zimbabwean communities in both periods.

Figure 2: Mean ITN Coverage per Cluster

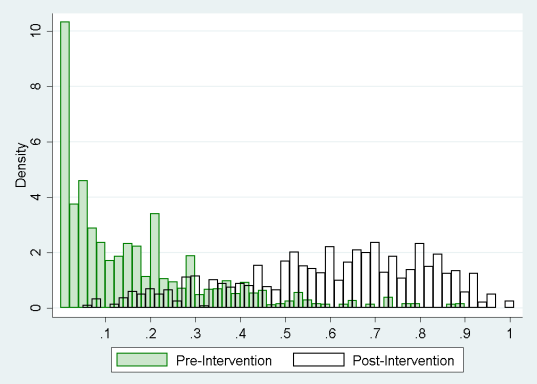
(a) Rwanda



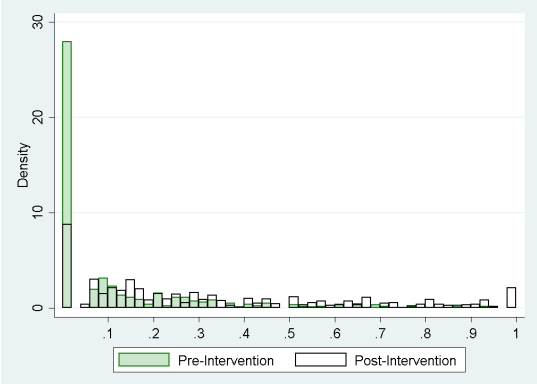
(b) Senegal



(c) Uganda



(d) Zimbabwe



In conclusion, the conditional before-after assumption, which states that the behaviour of the pre-intervention, conditional on a set of covariates, serves as a good proxy for the no-intervention behaviour in the post-intervention population (second column of Table 9), is violated only for Zimbabwe. The SUTVA assumption likely is violated for all countries, although this violation is unlikely to imply that there is not a causal effect of the ITN campaign on a reduction in early childhood mortality. Rather, it suggests that the true effect is stronger than the one reported, since any drop in mortality for non-ITN owners is not attributed to the ITN ownership. Based on three separate methods, the exclusion restriction, which specifies that outcome cannot change unless behaviour changes is fulfilled for all countries.

Therefore, I conclude that the assumptions upon which the estimator is built are fulfilled for the data from all Rwanda, Senegal, and Uganda. The conditional before-after assumption is likely violated for Zimbabwe, so the parameter for this country does not reflect the effect of the ITN campaign. The estimator reports that 7.9 all-cause deaths (per 1,000 live births per year) are avoided because of the ITN intervention, and no deaths are avoided in the other three countries. I would say that the estimates could be improved if there were data available surrounding ITN usage.

7 Conclusion

Using the framework proposed by Deuchert and Wunsch (2014), I assess the effectiveness of the large scale ITN distribution campaigns running in Rwanda, Senegal, Uganda, and Zimbabwe from 2006/2007 through 2010/2011. Through the PMI, Rwanda, Senegal, and Uganda received large sums of money and human resources to ramp up their National Malaria Control Programs (NMCPs) as part of the RBM initiative. Presuming that the ITN distribution portion of this plan reduces all-cause early childhood mortality only via its interaction with health-seeking behaviour, the total effect of the campaign can be measured by a two-component estimator: one part estimating the effect of ITN ownership on early child mortality and the other estimating the effect of the campaign on changing behaviour from no ITN ownership to ITN ownership.

I applied this estimator to the DHS data from Rwanda (2005, 2010), Senegal (2005, 2010), Uganda (2006, 2011), and Zimbabwe (2005, 2010). Only the estimator for Rwanda showed any significant sign of a treatment effect of the ITN distribution campaign on early childhood mortality. Furthermore, only the estimate for Zimbabwe shows signs of model misspecification. I, therefore, conclude

that the ITN campaign was only effective in reducing early childhood mortality in Rwanda. Looking at Table 1, one could conclude that the major strategy difference between Rwanda and the other countries was its focus on training community members about proper ITN usage. This would lead to the conclusion that future ITN interventions should also place a larger focus on training and socializing communities to use ITNs.

Also from Table 1, it is clear that in 2011 in Uganda the PMI invested heavily in training in Uganda in 2011. However, this is too close to the 2011 DHS survey for the results to be seen. It will be interesting to see how the numbers change between 2011 and the next survey period for Uganda. The table also gives the impression that there was a fundamental infrastructure problem in Senegal for which the PMI made a significant investment in 2011. I cannot say whether this was due to the costly flooding in 2009[4] or it if was due to an assessment that this was a flaw in the ITN intervention. In either case, perhaps the damage to infrastructure can account for the ineffectiveness of the ITN campaign in Senegal.

[4] http://www.preventionweb.net/english/countries/statistics/?cid=151 provides estimates on the human and monetary cost of disasters for numerous countries.

References

(2011): "Global Rural-Urban Mapping Project, Verson 1 (GRUMPv1): Settlement Points," Center for International Earth Science Information Network (CIESIN) Columbia University and Centro Internacional de Agricultura Tropical (CIAT).

(2011): "Roll Back Malaria Partnership: Changes to guidance for vector control indicators," Tech. rep., Roll Back Malaria Monitoring and Evaluation Reference Group, New York, USA.

Ashraf, N., G. Fink, and D. N. Weil (2014): "Evaluating the Effects of Large Scale Health Interventions in Developing Countries: The Zambian Malaria Initiative," in *African Successes: Health and Gender*, National Bureau of Economic Research, Inc.

Auchincloss, A. H. and W. Hadden (2002): "The Health Effects of Rural-Urban Residence and Concentrated Poverty," *The Journal of Rural Health*, 18, 319–336.

Belay, M. and W. Deressa (2008): "Use of insecticide treated nets by pregnant women and associated factors in a pre-dominantly rural population in northern Ethiopia," *Tropical Medicine and International Health*, 13, 13031313.

Cohen, J. and P. Dupas (2010): "Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment," *The Quarterly Journal of Economics*, 125, 1–45.

Deaton, A. and S. Zaidi (2002): *Guidelines for Constructing Consumption Aggregates for Welfare Analysis*, no. 14101 in World Bank Publications, The World Bank.

Dehejia, R. H. and S. Wahba (2002): "Propensity Score-Matching Methods For Nonexperimental Causal Studies," *The Review of Economics and Statistics*, 84, 151–161.

Deuchert, E. and C. Wunsch (2014): "Evaluating nationwide health interventions: Malawi's insecticide-treated-net distribution programme," *Journal of the Royal Statistical Society*, 177, pp. 523–552.

Dupas, P. (2009): "What Matters (and What Does Not) in Households' Decision to Invest in Malaria Prevention?" *American Economic Review*, 99, 224–30.

Eisele, T. P., J. Keating, M. Littrell, D. Larsen, and K. Macintyre (2009): "Assessment of Insecticide-Treated Bednet Use Among Children and Pregnant Women Across 15 Countries Using Standardized National Surveys," *The American Journal of Tropical Medicine and Hygiene*, 80, 209–214.

Filmer, D. and L. H. Pritchett (2001): "Estimating Wealth Effects without Expenditure Data-or Tears: An Application to Educational Enrollments in States of India," *Demography*, 38, pp. 115–132.

Greene, W. H. (2011): *Econometric Analysis*, Prentice Hall, 7 ed.

Grossman, M. (1972): "On the Concept of Health Capital and the Demand for Health," *Journal of Political Economy*, 80, 223–55.

Hawley, W. A., P. A. Phillips-Howard, F. O. Ter Kuile, D. J. Terlouw, J. M. Vulule, M. Ombok, B. L. Nahlen, J. E. Gimnig, S. K. Kariuki, M. S. Kolczak, and A. W. Hightower (2003): "Community-wide Effects of Permethrin-treated Bed Nets on Child Mortality and Malaria Morbidity in Western Kenya," *The American Society of TropicalMedicine and Hygiene*, 68, 121–127.

Hoffmann, V. (2009): "Intrahousehold Allocation of Free and Purchased Mosquito Nets," *American Economic Review*, 99, 236–41.

Kaufmann, C. and H. Briegel (2004): "Flight performance of the malaria vectors Anopheles gambiae and Anopheles atroparvus," *Journal of Vector Ecology*, 29, 140–153.

Kilian, A., H. Koenker, E. Baba, E. Onyefunafoa, R. Selby, K. Lokko, and M. Lynch (2013): "Universal coverage with insecticide-treated nets - applying the revised indicators for ownership and use to the Nigeria 2010 malaria indicator survey data," *Malaria Journal*, 12, 314.

Kovsted, J., C. C. Prtner, and F. Tarp (2002): "Child Health and Mortality: Does Health Knowledge Matter?" *Journal of African Economies*, 11, 542–560.

Lengeler, C., J. Armstrong-Schellenberg, U. D'Alessandro, F. Binka, and J. Cattani (1998): "Relative versus absolute risk of dying reduction after using insecticide-treated nets for malaria control in Africa," *Tropical Medicine and International Health*, 3, 286–290.

Malaney, P., A. Spielman, and J. Sachs (2004): "THE MALARIA GAP," *The American Journal of Tropical Medicine and Hygiene*, 71, 141–146.

Marmon, M., G. Rose, M. Shipley, and P. Hamilton (1978): "Employment grade and coronary heart disease in British civil servants," *Journal of Epidemiology and Community Health*, 32, 244–249.

MEASURE DHS/ICF International (2012): *Survey Organization Manual: Demographic and Health Surveys Methodology*, Calverton, Maryland.

Mugisha, F. and J. Arinaitwe (2003): "Sleeping arrangements and mosquito net use among under-fives: results from the Uganda Demographic and Health Survey," *Malaria Journal*, 2.

Njau, J. D., R. Stephenson, M. Menon, S. P. Kachur, and D. A. McFarland (2013): "Exploring the impact of targeted distribution of free bed nets on households bed net ownership, socio-economic disparities and childhood malaria infection rates: analysis of national malaria survey data from three sub-Saharan Africa countries," *Malaria Journal*, 12.

Oresanya, O. B., M. Hoshen, and O. T. Sofola (2008): "Utilization of insecticide-treated nets by under-five children in Nigeria: Assessing progress towards the Abuja targets," *Malaria Journal*, 7.

Pettifor, A., E. Taylor, D. Nku, S. Duvall, M. Tabala, S. Meshnick, and F. Behets (2008): "Bed net ownership, use and perceptions among women seeking antenatal care in Kinshasa, Democratic Republic of the Congo (DRC): Opportunities for improved maternal and child health," *BMC Public Health*, 8, 331.

Phillips-Howard, P. A., B. L. Nahlen, M. S. Kolczak, A. W. Hightower, F. O. Ter Kuile, J. A. Alaii, J. E. Gimnig, J. Arudo, J. M. Vulule, A. Odhacha, S. P. Kachur, E. Schoute, D. H. Rose, J. D. Sexton, A. J. Oloo, and W. A. Hawley (2003): "Efficacy of Permethrin-Treated Bed Nets in the Prevention of Mortality in Young Children in an Area of High Perennial Malaria Transmission in Western Kenya," *The American Journal of Tropical Medicine and Hygiene*, 68, 23–29.

Rosenbaum, P. R. and D. B. Rubin (1983): "The central role of the propensity score in observational studies for causal effects," *Biometrika*, 70, 41–55.

Rutstein, S. O. and K. Johnson (2004): "DHS Comparative Report 6: The DHS Wealth Index," .

Rutstein, S. O. and S. Staveteig (2014): "Making the Demographic and Health Surveys Wealth Index Comparable: DHS Methodological Reports 9," .

Stock, R. (1983): "Distance and the utilization of health facilities in rural Nigeria," *Social Science and Medicine*, 17, 563–573.

Sudfeld, C. R., A. M. Navar, and N. A. Halsey (2010): "Effectiveness of measles vaccination and vitamin A treatment," *International Journal of Epidemiology*, 39, i48–i55.

Wooldridge, J. M. (2001): *Econometric Analysis of Cross Section and Panel Data*, vol. 1, The MIT Press.

World Health Organization (2011): *World Malaria Report*, Geneva, Switzerland.

——— (2013): *World Malaria Report*, Geneva, Switzerland.

Yarnoff, B. (2011): "Household allocation decisions and child health: can behavioral responses to vitamin A supplementation programs explain heterogeneous effects?" *Journal of Population Economics*, 24, 657–680.

A Data Appendix

Table A.1: Descriptive Statistics for Rwanda Sample Data

Rwanda				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Mother's Characteristics				
Age*	30.17	6.70	29.52	6.51
15-19	0.02	0.12	0.03	0.16
20-24	0.22	0.41	0.22	0.41
25-29	0.28	0.45	0.32	0.47
30-34	0.22	0.42	0.21	0.41
35+	0.26	0.44	0.22	0.42
Education				
None	0.28	0.45	0.18	0.39
Primary	0.64	0.48	0.73	0.44
Secondary, higher	0.08	0.27	0.09	0.28
Religion (0.41% missing)				
Christian	0.96	0.20	0.97	0.18
Muslim	0.02	0.14	0.01	0.11
None	0.01	0.10	0.01	0.10
Other	0.01	0.09	0.01	0.09
Number of children born*	4.08	2.48	3.45	2.28
1-3	0.48	0.50	0.60	0.49
4-6	0.18	0.38	0.12	0.32
7+	0.34	0.47	0.28	0.45
Older child died	0.07	0.26	0.04	0.20
Access to media (0.08% missing)				
No	0.52	0.50	0.38	0.49
Yes	0.48	0.50	0.62	0.49
Married, living with partner	1.45	0.94	0.84	0.36
Barriers to healthcare (0.04% missing)				
No	0.20	0.40	0.40	0.49
Yes	0.80	0.40	0.59	0.49
Family planning visit (0.45% missing)				
No	0.93	0.25	0.67	0.47
Yes	0.06	0.23	0.33	0.47
Hydration salts (0.53% missing)				
Never heard of	0.13	0.34	0.08	0.28
Uses	0.03	0.16	0.06	0.24
Heard of	0.84	0.37	0.85	0.36

Continued on next page

Table A.1: Descriptive Statistics for Rwanda Sample Data (continued)

Variable	Rwanda			
	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Work				
Doesn't work	0.20	0.40	0.11	0.31
Works, worked in past year	0.80	0.40	0.89	0.31
Final say (8.20% missing)				
Respondent (and spouse)	0.60	0.49	0.61	0.49
Someone else	0.40	0.49	0.23	0.42
Child characteristics				
Place of delivery (0.10% missing				
Home	0.70	0.46	0.23	0.42
Public facility	0.28	0.45	0.76	0.43
Private facility	0.01	0.12	0.01	0.10
Child is youngest	0.82	0.38	0.88	0.32
Size at birth (0.37% missing)				
Larger than average	0.39	0.49	0.44	0.50
Average	0.47	0.50	0.39	0.49
Smaller than average	0.13	0.34	0.16	0.37
Age*	1.47	0.88	1.53	0.85
Household characteristics				
Number of members				
1-3	0.16	0.36	0.19	0.39
4-6	0.56	0.50	0.56	0.50
5+	0.28	0.45	0.25	0.44
Number of members ≤ 5*	1.83	0.78	1.72	0.72
0	0.03	0.17	0.02	0.15
1	0.31	0.46	0.37	0.48
2	0.46	0.50	0.47	0.50
3+	0.20	0.40	0.14	0.34
Sex of household head				
Male	0.83	0.37	0.81	0.39
Female	0.17	0.37	0.19	0.39
Age of household head*	36.82	10.91	36.31	11.61
15-29	0.28	0.45	0.34	0.47
30-39	0.38	0.48	0.35	0.48
40-49	0.23	0.42	0.18	0.39
50+	0.11	0.31	0.13	0.34
Education of household head (0.33% missing)				
None, preschool	0.28	0.45	0.22	0.42
Primary	0.61	0.49	0.67	0.47

Continued on next page

Table A.1: Descriptive Statistics for Rwanda Sample Data (continued)

Rwanda				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Secondary, higher	0.11	0.31	0.10	0.30
Region characteristics				
Region				
Kigali City	0.06	0.23	0.09	0.29
South	0.24	0.43	0.23	0.42
West	0.25	0.43	0.26	0.44
North	0.21	0.41	0.15	0.36
East	0.25	0.43	0.26	0.44
Altitude*	1741.62	326.79	1736.74	302.76
$<$ 1500m	0.24	0.43	0.27	0.44
$\geq$ 1500m	0.76	0.43	0.73	0.44
Comparable Wealth Index*	-0.99	0.40	-0.63	0.40
Poorest quintile	0.86	0.35	0.08	0.28
Poorer quintile	0.09	0.28	0.78	0.42
Middle quintile	0.02	0.15	0.07	0.25
Richer quintile	0.02	0.13	0.05	0.22
Richest quintile	0.01	0.11	0.02	0.14
Distance to lake* (0.57% missing)	17.40	12.51	17.56	12.38
$<$ 15km	0.48	0.50	0.48	0.50
$\geq$ 15km	0.51	0.50	0.52	0.50
Distance to city* (1.11% missing)	15.50	8.33	15.80	8.25
$<$ 15km	0.46	0.50	0.47	0.50
$\geq$ 15km	0.53	0.50	0.53	0.50
Number of observations	5497		5199	

With the exception of the continuous variables marked with (*), all variables are dummy variables. If the percentage of missing observations is not listed, the variable is not missing any observations.

Table A.2: Descriptive Statistics for Senegal Sample Data

Variable	Senegal			
	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Mother's Characteristics				
Age*	28.32	6.95	28.36	6.76
15-19	0.09	0.28	0.08	0.27
20-24	0.24	0.43	0.24	0.42
25-29	0.25	0.44	0.27	0.44
30-34	0.21	0.41	0.21	0.41
35+	0.21	0.41	0.21	0.41
Education				
None	0.71	0.45	0.71	0.46
Primary	0.22	0.41	0.21	0.41
Secondary, higher	0.07	0.26	0.09	0.28
Religion (0.04% missing)				
Muslim	0.97	0.18	0.96	0.19
Christian	0.03	0.17	0.03	0.17
Animist	0.00	0.03	0.01	0.08
None	0.00	0.01	0.00	0.01
Other	0.00	0.00	0.00	0.01
Number of children born*	3.89	2.53	3.72	2.40
1-3	0.53	0.50	0.55	0.50
4-6	0.16	0.37	0.14	0.35
7+	0.31	0.46	0.31	0.46
Older child died	0.09	0.29	0.06	0.24
Access to media				
No	0.10	0.30	0.16	0.37
Yes	0.90	0.30	0.84	0.37
Married, living with partner	0.96	0.20	0.94	0.24
Barriers to healthcare (0.06% missing)				
No	0.28	0.45	0.40	0.49
Yes	0.72	0.45	0.60	0.49
Family planning visit (0.33% missing)				
No	0.91	0.29	0.90	0.30
Yes	0.09	0.28	0.10	0.30
Hydration salts (0.27% missing)				
Never heard of	0.35	0.48	0.41	0.49
Uses	0.05	0.21	0.07	0.25
Heard of	0.60	0.49	0.52	0.50
Work (0.10% missing)				
Doesn't work	0.57	0.50	0.56	0.50

Continued on next page

Table A.2: Descriptive Statistics for Senegal Sample Data (continued)

Variable	Senegal			
	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Works, worked in past year	0.43	0.50	0.44	0.50
Final say (3.54% missing)				
Respondent (and spouse)	0.17	0.38	0.25	0.43
Someone else	0.82	0.38	0.68	0.46
Child characteristics				
Place of delivery (16.17% missing)				
Home	0.36	0.48	0.27	0.44
Public facility	0.59	0.49	0.69	0.46
Private facility	0.04	0.20	0.04	0.19
Child is youngest	0.63	0.48	0.64	0.48
Size at birth (7.54% missing)				
Larger than average	0.23	0.42	0.25	0.43
Average	0.46	0.50	0.45	0.50
Smaller than average	0.30	0.46	0.30	0.46
Age*	1.38	0.86	1.44	0.87
Household characteristics				
Number of members				
1-3	0.02	0.13	0.02	0.14
4-6	0.13	0.33	0.11	0.32
5+	0.85	0.35	0.87	0.34
Number of members ≤ 5*				
0	0.02	0.12	0.02	0.13
1	0.14	0.35	0.15	0.35
2	0.25	0.43	0.23	0.42
3+	0.59	0.49	0.61	0.49
Sex of household head				
Male	0.82	0.38	0.80	0.40
Female	0.18	0.38	0.20	0.40
Age of household head* (4.56% missing)	50.52	14.58	51.94	14.67
15-29	0.05	0.22	0.04	0.19
30-39	0.19	0.39	0.18	0.38
40-49	0.26	0.44	0.24	0.43
50+	0.49	0.50	0.54	0.50
Education of household head (2.55% missing)				
None, preschool	0.75	0.43	0.75	0.43
Primary	0.13	0.33	0.12	0.33
Secondary, higher	0.10	0.30	0.10	0.30

Continued on next page

Table A.2: Descriptive Statistics for Senegal Sample Data (continued)

Variable	Senegal			
	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Region characteristics				
Region				
Dakar	0.18	0.39	0.19	0.39
Ziguinchor	0.03	0.18	0.03	0.18
Diorbel	0.12	0.33	0.12	0.33
St. Louis	0.06	0.25	0.07	0.25
Tambacounda	0.07	0.26	0.07	0.25
Kaolack	0.13	0.33	0.14	0.34
Thies	0.13	0.34	0.12	0.33
Louga	0.07	0.25	0.07	0.25
Fatick	0.06	0.24	0.06	0.23
Kolda	0.09	0.29	0.10	0.29
Matam	0.04	0.20	0.04	0.20
Altitude* (2.39% missing)	29.93	34.65	27.08	20.82
Comparable Wealth Index*	-0.55	1.27	-0.44	0.67
Poorest quintile	0.50	0.50	0.35	0.48
Poorer quintile	0.10	0.30	0.18	0.38
Middle quintile	0.06	0.24	0.12	0.33
Richer quintile	0.14	0.35	0.32	0.47
Richest quintile	0.19	0.39	0.02	0.15
Distance to lake* (2.40% missing)	18.42	20.46	18.11	20.45
$<$ 15km	0.58	0.49	0.61	0.49
$\geq$ 15km	0.39	0.49	0.38	0.48
Distance to city* (2.39% missing)	16.23	16.08	15.34	15.43
$<$ 15km	0.65	0.48	0.69	0.46
$\geq$ 15km	0.32	0.47	0.29	0.45
Number of observations	6887		7617	

With the exception of the continuous variables marked with (*), all variables are dummy variables. If the percentage of missing observations is not listed, the variable is not missing any observations.

Table A.3: Descriptive Statistics for Uganda Sample Data

Uganda				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Mother's Characteristics				
Age*	28.32	6.95	28.36	6.76
15-19	0.08	0.27	0.08	0.27
20-24	0.29	0.45	0.28	0.45
25-29	0.25	0.43	0.29	0.45
30-34	0.20	0.40	0.17	0.38
35+	0.19	0.39	0.18	0.39
Education				
Education	0.93	0.59	1.09	0.59
None	0.21	0.41	0.13	0.34
Primary	0.64	0.48	0.64	0.48
Secondary, higher	0.14	0.35	0.23	0.42
Religion (0.03% missing)				
Christian	0.87	0.34	0.86	0.35
Muslim	0.11	0.31	0.13	0.33
Other	0.02	0.14	0.01	0.12
Number of children born*	4.51	2.77	4.30	2.67
1-3	0.43	0.50	0.46	0.50
4-6	0.23	0.42	0.21	0.41
7+	0.33	0.47	0.33	0.47
Older child died	0.07	0.26	0.05	0.21
Access to media (0.08% missing)				
No	0.37	0.48	0.32	0.47
Yes	0.63	0.48	0.68	0.47
Married, living with partner	0.86	0.35	0.87	0.34
Barriers to healthcare (0.09% missing)				
No	0.19	0.39	0.40	0.49
Yes	0.81	0.39	0.60	0.49
Family planning visit (0.14% missing)				
No	0.95	0.23	0.89	0.31
Yes	0.05	0.23	0.11	0.31
Hydration salts (0.43% missing)				
Never heard of	0.13	0.34	0.10	0.30
Uses	0.16	0.37	0.16	0.37
Heard of	0.70	0.46	0.73	0.44
Work (0.13% missing)				
Doesn't work	0.08	0.27	0.22	0.41
Works, worked in past year	0.92	0.28	0.78	0.41

Continued on next page

Table A.3: Descriptive Statistics for Uganda Sample Data (continued)

Variable	Uganda			
	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Final say (13.87% missing)				
Respondent (and spouse)	0.51	0.50	0.50	0.50
Someone else	0.35	0.48	0.37	0.48
Child characteristics				
Place of delivery (0.10% missing; not applicable				
Home	0.57	0.50	0.41	0.49
Public facility	0.30	0.46	0.46	0.50
Private facility	0.13	0.33	0.13	0.34
Child is youngest	0.80	0.40	0.81	0.40
Size at birth (1.65% missing)				
Larger than average	0.33	0.47	0.39	0.49
Average	0.46	0.50	0.37	0.48
Smaller than average	0.21	0.41	0.21	0.41
Age*	1.45	0.85	1.45	0.87
Household characteristics				
Number of members				
1-3	0.11	0.32	0.12	0.33
4-6	0.44	0.50	0.45	0.50
5+	0.44	0.50	0.43	0.49
Number of members ≤ 5*	1.95	0.83	1.97	0.82
0	0.04	0.20	0.03	0.18
1	0.24	0.43	0.26	0.44
2	0.44	0.50	0.42	0.49
3+	0.28	0.45	0.29	0.45
Sex of household head				
Male	0.78	0.41	0.79	0.41
Female	0.22	0.41	0.21	0.41
Age of household head* (0.007% missing)	36.70	11.95	36.88	12.09
15-29	0.29	0.45	0.30	0.46
30-39	0.40	0.49	0.38	0.49
40-49	0.18	0.38	0.18	0.39
50+	0.13	0.34	0.14	0.35
Education of household head (0.85% missing)				
None, preschool	0.14	0.34	0.13	0.34
Primary	0.64	0.48	0.57	0.50
Secondary, higher	0.22	0.41	0.29	0.45
Region characteristics				
Region				
Kampala	0.05	0.22	0.06	0.24

Continued on next page

Table A.3: Descriptive Statistics for Uganda Sample Data (continued)

Uganda				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Central 1	0.10	0.29	0.10	0.30
Central 2	0.08	0.28	0.11	0.31
East Central	0.11	0.31	0.11	0.31
Eastern	0.16	0.36	0.17	0.38
North	0.17	0.38	0.12	0.33
West-Nile	0.06	0.23	0.06	0.24
Western	0.16	0.36	0.14	0.35
Southwest	0.12	0.33	0.12	0.33
Altitude*	1223.11	256.74	1208.63	225.40
250-749m	0.01	0.08	0.02	0.14
750-999m	0.05	0.21	0.03	0.17
1000-1249m	0.66	0.47	0.68	0.47
1250-1499m	0.18	0.38	0.18	0.38
1500+m	0.10	0.30	0.09	0.29
Comparable Wealth Index*	-0.96	0.58	-0.57	0.58
Poorest quintile	0.68	0.47	0.32	0.47
Poorer quintile	0.19	0.39	0.42	0.49
Middle quintile	0.05	0.22	0.09	0.29
Richer quintile	0.06	0.23	0.13	0.33
Richest quintile	0.02	0.15	0.04	0.20
Distance to lake* (5.23% missing)	36.04	34.34	31.37	28.73
$<$ 15km	0.30	0.46	0.34	0.48
$\geq$ 15km	0.61	0.49	0.64	0.48
Distance to city* (5.23% missing)	21.74	15.46	21.90	15.62
$<$ 15km	0.34	0.47	0.39	0.49
$\geq$ 15km	0.57	0.50	0.59	0.49
Number of observations	5062		4761	

With the exception of the continuous variables marked with (*), all variables are dummy variables. If the percentage of missing observations is not listed, the variable is not missing any observations.

Table A.4: Descriptive Statistics for Zimbabwe Sample Data

Zimbabwe				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Mother's Characteristics				
Age*	26.61	6.40	26.81	6.32
15-19	0.11	0.31	0.10	0.30
20-24	0.34	0.48	0.31	0.46
25-29	0.25	0.43	0.28	0.45
30-34	0.17	0.38	0.17	0.38
35+	0.13	0.33	0.14	0.34
Education				
None	0.04	0.19	0.01	0.11
Primary	0.36	0.48	0.31	0.46
Secondary, higher	0.60	0.49	0.67	0.47
Religion				
Christian	0.89	0.31	0.92	0.26
Muslim	0.01	0.08	0.00	0.07
Traditional	0.10	0.30	0.07	0.26
Other	0.00	0.04	0.00	0.02
Number of children born*	2.79	1.92	2.68	1.72
1-3	0.73	0.44	0.76	0.43
4-6	0.06	0.23	0.04	0.19
7+	0.21	0.41	0.21	0.40
Older child died	0.03	0.18	0.03	0.16
Access to media				
No	0.51	0.50	0.46	0.50
Yes	0.49	0.50	0.54	0.50
Married, living with partner	0.83	0.37	0.87	0.34
Barriers to healthcare (0.01% missing)				
No	0.27	0.45	0.42	0.49
Yes	0.73	0.45	0.58	0.49
Family planning visit				
No	0.95	0.21	0.93	0.25
Yes	0.05	0.21	0.07	0.25
Work				
Doesn't work	0.58	0.49	0.61	0.49
Works, worked in past year	0.42	0.49	0.39	0.49
Final say (14.41% missing)				
Respondent (and spouse)	0.69	0.46	0.72	0.45
Someone else	0.15	0.36	0.15	0.36

Continued on next page

Table A.4: Descriptive Statistics for Zimbabwe Sample Data (continued)

Zimbabwe				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Child characteristics				
Place of delivery (0.07% missing; not applicable				
Home	0.33	0.47	0.36	0.48
Public facility	0.55	0.50	0.55	0.50
Private facility	0.12	0.33	0.09	0.29
Child is youngest	0.89	0.32	0.89	0.31
Size at birth (1.90% missing)				
Larger than average	0.38	0.48	0.38	0.48
Average	0.47	0.50	0.47	0.50
Smaller than average	0.14	0.35	0.13	0.34
Age*	1.43	0.86	1.36	0.87
Household characteristics				
Number of members				
1-3	0.17	0.37	0.22	0.41
4-6	0.47	0.50	0.50	0.50
5+	0.37	0.48	0.29	0.45
Number of members ≤ 5*	1.63	0.81	1.49	0.77
0	0.05	0.23	0.07	0.25
1	0.42	0.49	0.48	0.50
2	0.36	0.48	0.34	0.48
3+	0.16	0.37	0.11	0.31
Sex of household head	1.35	0.48	1.42	0.49
Male	0.65	0.48	0.58	0.49
Female	0.35	0.48	0.42	0.49
Age of household head*	39.47	14.52	38.21	14.47
15-29	0.28	0.45	0.33	0.47
30-39	0.34	0.47	0.33	0.47
40-49	0.14	0.35	0.14	0.35
50+	0.24	0.42	0.20	0.40
Education of household head (0.88% missing)				
None, preschool	0.10	0.30	0.06	0.24
Primary	0.37	0.48	0.33	0.47
Secondary, higher	0.53	0.50	0.60	0.49
Region characteristics				
Region				
Manicaland	0.13	0.33	0.15	0.36
Mashonaland Central	0.11	0.31	0.10	0.30
Mashonaland East	0.08	0.27	0.10	0.29
Mashonaland West	0.10	0.30	0.12	0.33

Continued on next page

Table A.4: Descriptive Statistics for Zimbabwe Sample Data (continued)

Zimbabwe				
Variable	Pre-intervention		Post-intervention	
	Mean	Std. Dev.	Mean	Std. Dev.
Matebeleland North	0.06	0.25	0.05	0.21
Matebeleland South	0.05	0.21	0.05	0.22
Midlands	0.15	0.36	0.13	0.33
Masvingo	0.16	0.36	0.11	0.32
Harare	0.12	0.33	0.15	0.36
Bulawayo	0.05	0.21	0.04	0.20
Altitude*	1063.02	328.81	1107.60	320.32
250-749m	0.18	0.39	0.15	0.35
750-999m	0.18	0.38	0.19	0.39
1000-1249m	0.32	0.47	0.29	0.45
1250-1499m	0.27	0.44	0.32	0.47
1500+m	0.05	0.22	0.06	0.23
Comparable Wealth Index*	-0.35	1.11	-0.19	0.88
Poorest quintile	0.44	0.50	0.27	0.44
Poorer quintile	0.13	0.33	0.19	0.39
Middle quintile	0.11	0.31	0.15	0.36
Richer quintile	0.07	0.26	0.21	0.41
Richest quintile	0.25	0.43	0.18	0.38
Distance to lake* (0.60% missing)	40.26	30.96	39.04	30.42
$<$ 15km	0.28	0.45	0.28	0.45
$\geq$ 15km	0.71	0.45	0.72	0.45
Distance to city* (0.60% missing)	45.68	38.92	42.70	36.45
$<$ 15km	0.32	0.47	0.36	0.48
$\geq$ 15km	0.67	0.47	0.63	0.48
Number of observations	3217		3601	

With the exception of the continuous variables marked with (*), all variables are dummy variables. If the percentage of missing observations is not listed, the variable is not missing any observations.

Table A.5: Results of Probit Regressions for Rwanda and Senegal

Variable	Rwanda		Senegal	
	pre-	post-	pre-	post-
Household Characteristics				
Number of household members				
1-3	-0.319**	-0.024	-0.070	-0.262
	(0.114)	(0.134)	(-0.186)	(0.189)
4-6	-0.350***	0.131	-0.110	0.001
	(0.078	(0.097)	(0.083)	-0.089
Education of household head				
Primary	0.259***	-0.060	0.110	0.016
	(0.068)	(0.079)	(0.099)	(0.101)
Secondary, higher	0.459***	0.043	0.157	0.124
	(0.111)	(0.176)	(0.110)	(0.150)
Female household head	-0.061	-0.093	-0.088	-0.246**
	(0.110)	(0.092)	(0.082)	(0.093)
Age of household head	-0.010	0.004	0.014***	0.001
	(0.008)	(0.006)	(0.004)	(0.004)
13-29	-0.042	-0.091	0.568**	0.001
	(0.099)	(0.099)	(0.214)	(0.214)
40-49	-0.047	-0.142	0.364**	0.222
	(0.108)	(0.128)	(0.137)	(0.147)
50+	-0.030	-0.469*	0.240*	0.110
	(0.229)	(0.216)	(0.097)	(0.106)
Wealth				
CWI	0.037	0.998**	0.064	-0.316
	(0.274)	(0.316)	(0.099)	(0.200)
Poorest quintile	-0.374**	-0.189	0.276*	0.365**
	(0.137)	(0.111)	(0.120)	(0.132)
Poorer quintile	0.361	-0.482*	0.358	0.375
	(0.216)	(0.219)	(0.187)	(0.211)
Richer quintile	0.090	-1.105**	0.226	0.371
	(0.294)	(0.366)	(0.219)	(0.289)
Richest quintile	0.112	-1.520*	0.073	0.070
	(0.579)	(0.718)	(0.316)	(0.409)
Child characteristics				
Youngest	-0.194**	0.061	-0.058	-0.044
	(0.074)	(0.098)	(0.062)	(0.060)
Place of delivery				
Home			-0.114	0.010
			(0.063)	(0.059)

Continued on next page

Table A.5: Results of Probit Regressions for Rwanda and Senegal (continued)

Variable	Rwanda		Senegal	
	pre-	post-	pre-	post-
Public institution	0.216***	0.185*		
	(0.059)	(0.073)		
Private institution	0.476*	-0.001	-0.056	-0.058
	(0.199)	(0.323)	(0.131)	(0.158)
Size at birth				
Larger than average	-0.088	-0.170*	-0.093	0.058
	(0.049)	(0.068)	(0.061)	(0.058)
Smaller than average	-0.155*	-0.130	-0.011	0.080
	(0.078)	(0.086)	(0.053)	(0.063)
Hypothetical age	-0.112***	0.085*	-0.052	0.007
	(0.033)	(0.038)	(0.030)	(0.031)
Mother characteristics				
Age	0.008	0.002	-0.002	0.031*
	(0.015)	(0.017)	(0.013)	(0.013)
15-19	-0.041	-0.133	-0.111	0.400**
	(0.241)	(0.212)	(0.150)	(0.142)
20-24	-0.085	-0.057	-0.114	0.226**
	(0.102)	(0.114)	(0.096)	(0.081)
30-34	-0.150	0.127	-0.024	-0.041
	(0.097)	(0.130)	(0.090)	(0.109)
35+	-0.109	-0.078	-0.114	-0.261
	(0.169)	(0.212)	(0.152)	(0.191)
Education				
None	-0.074	-0.198*	0.021	-0.172**
	(0.067)	(0.078)	(0.065)	(0.064)
Secondary,higher	0.253*	0.090	-0.059	-0.017
	(0.098)	(0.169)	(0.108)	(0.104)
Predominant religion	-0.041	0.201	0.035	-0.033
	(0.122)	(0.174)	(0.176)	(0.153)
Number of children born	-0.036	0.040	-0.038	0.022
	(0.034)	(0.048)	(0.026)	(0.032)
4-6	-0.017	0.042	0.253	-0.285
	(0.199)	(0.275)	(0.161)	(0.201)
7+	-0.021	-0.250	0.050	-0.059
	(0.098)	(0.140)	(0.085)	(0.096)
Treats drinking water	0.171	0.191*		
			(0.355)	(0.092)
Final say				
Respondent (and spouse)	-0.012	-0.116		
	(0.059)	(0.083)		

Continued on next page

Table A.5: Results of Probit Regressions for Rwanda and Senegal (continued)

Variable	Rwanda		Senegal	
	pre-	post-	pre-	post-
Someone else			0.028	-0.064
			(0.065)	(0.064)
Work				
Doesn't work	0.092	-0.149	-0.103	-0.045
	(0.069)	(0.111)	(0.054)	(0.068)
No barriers to health care	0.205**	0.217**	0.1945**	0.119*
	(0.068)	(0.070)	(0.059)	(0.056)
Married, living with partner	0.174	0.233	-0.132	-0.142
	(0.121)	(0.135)	(0.116)	(0.118)
Hydration salts				
Never heard of	-0.145	-0.214*	-0.239***	-0.036
	(0.081)	(0.099)	(0.051)	(0.053)
Used	0.053	0.061	0.056	-0.208*
	(0.159)	(0.137)	(0.115)	(0.094)
Family planning visit				
No	-0.158	-0.081		
	(0.093)	(0.066)		
Yes			0.218**	0.120
			(0.083)	(0.099)
Has no access to media	-0.516***	-0.215**	-0.340***	-0.125
	(0.061)	(0.075)	(0.091)	(0.078)
Older child died	0.004	-0.001		
	(0.116)	(0.160)		
Region Characteristics				
Kigali City	-0.328	-0.044	0.009	0.024
	(0.168)	(0.210)	(0.106)	(0.146)
South	0.077	0.289*		
	(0.143)	(0.147)		
North	-0.265*	-0.086		
	(0.125)	(0.104)		
East	-0.649***	-0.019		
	(0.107)	(0.138)		
Dakar			-0.171	-0.889***
			(0.148)	(0.196)
Ziguinchor			0.347*	-0.073
			(0.172)	(0.195)
Diorbel			-0.340*	-0.322
			(0.151)	(0.191)
St. Louis			0.233	0.063
			(0.198)	(0.191)

Continued on next page

Table A.5: Results of Probit Regressions for Rwanda and Senegal (continued)

Variable	Rwanda		Senegal	
	pre-	post-	pre-	post-
Tambacounda			0.288	0.144
			(0.172)	(0.179)
Thies			-0.057	-0.783***
			(0.144)	(0.161)
Louga			-0.023	-0.363*
			(0.156)	(0.182)
Fatick			0.181	-0.232
			(0.144)	(0.182)
Kolda			0.632***	0.552**
			(0.140)	(0.198)
Matam			0.679***	-0.110
			(0.187)	(0.196)
Distance to lake	-0.013*	-0.0121*	-0.003	-0.006*
	(0.006)	(0.006)	(0.003)	(0.003)
$< 15km$	-0.252	0.011	-0.187	-0.179
	(0.135)	(0.114)	(0.120)	(0.124)
Distance to city	0.023**	-0.001	0.008	0.000
	(0.009)	(0.008)	(0.004)	(0.005)
< 15 km			0.067	0.068
			(0.138)	(0.139)
≥ 15 km	-0.293*	-0.153		
	(0.137)	(0.121)		
Altitude	-0.001***	-0.001***	0.001	-0.001
	(0.000)	(0.000)	(0.001)	(0.002)
1500+	0.158	0.091		
	(0.116)	(0.133)		
Constant	2.587***	3.452***	-1.387*	0.045
	(0.692)	(0.723)	(0.639)	(0.601)

Standard errors are in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ in two-tailed t-test.

Table A.6: Results of Probit Regressions for Uganda and Zimbabwe

Variable	Uganda		Zimbabwe	
	pre-	post-	pre-	post-
Household Characteristics				
Number of household members				
1-3	-0.31*	-0.35***	-0.071	-0.034
	(0.12)	(0.10)	(0.16)	(0.10)
4-6	-0.14	-0.18*	-0.11	0.040
	(0.076)	(0.079)	(0.10)	(0.075)
Household head education level				
None			0.16	0.078
			(0.19)	(0.15)
Primary	0.044	0.10	-0.084	0.043
	(0.11)	(0.088)	(0.11)	(0.068)
Secondary, higher	0.27*	0.069		
	(0.14)	(0.11)		
Female household head	-0.040	-0.021	-0.30**	-0.016
	(0.081)	(0.080)	(0.093)	(0.065)
Household head Age	-0.0069	-0.0028	0.00025	-0.00015
	(0.0079)	(0.0063)	(0.0097)	(0.0052)
13-29			-0.042	0.089
		(0.13)	(0.079)	
30-39	-0.095	-0.040		
	(0.11)	(0.098)		
40-49	0.018	-0.039	-0.25	-0.075
	(0.18)	(0.15)	(0.14)	(0.11)
50-59	0.19	-0.048	-0.42	-0.038
	(0.30)	(0.25)	(0.26)	(0.17)
Wealth				
CWI	0.52*	0.23	0.63**	-0.016
	(0.21)	(0.21)	(0.19)	(0.11)
Poorest quintile	-0.020	-0.070		
	(0.14)	(0.092)		
Poorer quintile			-0.045	-0.14
			(0.19)	(0.10)
Middle quintile	-0.043	-0.14	-0.41	0.15
	(0.17)	(0.15)	(0.25)	(0.15)
Richer quintile	-0.0076	-0.26	-0.23	0.18
	(0.25)	(0.23)	(0.35)	(0.17)
Richest quintile	-0.23	-0.62	-0.71	0.33
	(0.41)	(0.37)	(0.51)	(0.25)

Continued on next page

Table A.6: Results of Probit Regressions for Uganda and Zimbabwe (continued)

Variable	Uganda		Zimbabwe	
	pre-	post-	pre-	post-
Child Characteristics				
Youngest	-0.043	-0.19**	-0.026	-0.024
	(0.078)	(0.066)	(0.11)	(0.079)
Place of delivery				
Public institution	0.14*	0.013	0.059	-0.025
	(0.064)	(0.061)	(0.11)	(0.061)
Private institution	0.079	0.19	0.10	-0.18
	(0.11)	(0.10)	(0.16)	(0.12)
Size at birth				
Larger than average	0.026	-0.030	0.13	-0.060
	(0.061)	(0.059)	(0.083)	(0.059)
Smaller than average	0.044	0.0081	-0.015	-0.013
	(0.069)	(0.069)	(0.11)	(0.081)
Hypothetical age	-0.032	-0.083**	-0.037	-0.021
	(0.037)	(0.031)	(0.044)	(0.031)
Mother Characteristics				
Age	0.014	0.024	-0.0088	-0.0016
	(0.015)	(0.014)	(0.022)	(0.016)
15-19	0.13	0.20	-0.24	-0.15
	(0.21)	(0.16)	(0.23)	(0.16)
20-24	0.14	0.072	-0.20	-0.11
	(0.12)	(0.097)	(0.13)	(0.099)
30-34	-0.11	0.097	0.011	-0.0087
	(0.12)	(0.10)	(0.20)	(0.12)
35+	-0.090	-0.047	0.095	0.065
	(0.20)	(0.17)	(0.30)	(0.21)
Education level				
None	-0.095	-0.070	0.20	-0.18 *
	(0.080)	(0.087)	(0.20)	(0.25)
Primary			0.014	0.071 *
			(0.11)	(0.070)
Secondary, higher	0.28*	0.21*		*
	(0.11)	(0.091)		
Predominant religion	0.14	0.085	0.30*	0.090
	(0.11)	(0.084)	(0.12)	(0.11)
Number of children born	0.025	-0.037	-0.0038	0.0022
	(0.032)	(0.032)	(0.053)	(0.045)
4-6	-0.017	-0.063	0.27	-0.15
	(0.12)	(0.11)	(0.15)	(0.12)

Continued on next page

Table A.6: Results of Probit Regressions for Uganda and Zimbabwe (continued)

Variable	Uganda		Zimbabwe	
	pre-	post-	pre-	post-
7+	-0.28	-0.030	0.10	0.23
	(0.20)	(0.20)	(0.31)	(0.26)
Treats drinking water	0.24*	0.014	0.15	0.11
	(0.10)	(0.075)	(0.12)	(0.072)
Someone else has final	-0.024	0.0027	0.098	0.055
say on health decisions	(0.078)	(0.069)	(0.092)	(0.086)
Employment				
Doesn't work			0.086	0.15*
			(0.088)	(0.065)
Has worked within	0.063	-0.098		
last year	(0.11)	(0.073)		
No barriers to health care	-0.050	0.15**	0.035	0.058
	(0.079)	(0.059)	(0.11)	(0.059)
Married, living with partner	0.22	0.079	0.097	0.30***
(0.11)	(0.095)	(0.12)	(0.089)	
Hydration salts				
Never heard of	-0.081	-0.34***		
	(0.12)	(0.092)		
Used	0.085	0.12		
	(0.081)	(0.069)		
No family planning visit	0.032	-0.097	0.22	-0.10
	(0.14)	(0.096)	(0.17)	(0.10)
No access to media	-0.11	-0.29***	-0.23*	-0.14*
	(0.077)	(0.072)	(0.10)	(0.068)
Older child died	-0.0023	0.032	0.048	-0.062
	(0.13)	(0.12)	(0.22)	(0.17)
Region Characteristics				
Kampala	-1.64***	-0.51*		
	(0.28)	(0.22)		
Central 1	-1.79***	-0.27		
	(0.24)	(0.20)		
Central 2	-1.43***	-0.40*		
	(0.23)	(0.17)		
East Central	-1.38***	-1.14***		
	(0.21)	(0.20)		
Eastern	-0.59**	-0.43*		
	(0.20)	(0.17)		
West Nile	-0.47	0.50**		
	(0.25)	(0.17)		

Continued on next page

Table A.6: Results of Probit Regressions for Uganda and Zimbabwe (continued)

Variable	Uganda		Zimbabwe	
	pre-	post-	pre-	post-
Western	-1.24***	0.072		
	(0.18)	(0.18)		
Southwest	-0.70**	-0.078		
	(0.22)	(0.25)		
Mashonaland Central			0.027	-0.49*
			(0.24)	(0.24)
Mashonaland East			-0.25	-0.031
			(0.23)	(0.20)
Mashonaland West			0.31	-0.65**
			(0.24)	(0.21)
Matebeleland North			0.080	-0.047
			(0.28)	(0.23)
Matebeleland South			-0.29	-1.72***
			(0.28)	(0.24)
Midlands			0.26	-0.17
			(0.21)	(0.22)
Masvingo			-0.52*	-0.91***
			(0.26)	(0.25)
Harare			0.20	-0.36
			(0.28)	(0.19)
Bulawayo			-0.17	0.15
			(0.30)	(0.21)
Distance to lake	0.0019	-0.0020	0.0013	-0.0032
	(0.0017)	(0.0019)	(0.0026)	(0.0024)
$<$ 15 km	0.22	-0.083	-0.095	-0.16
	(0.14)	(0.11)	(0.18)	(0.14)
Distance to city	0.0043	-0.000042	0.011***	0.00059
	(0.0049)	(0.0049)	(0.0026)	(0.0021)
$<$ 15 km			0.34	-0.066
			(0.22)	(0.16)
$\geq$ 15 km	0.012	0.014		
	(0.14)	(0.14)		
Altitude	-0.0016**	-0.00023	-0.00085	0.00095
	(0.00048)	(0.00049)	(0.00074)	(0.00071)
250-749	-0.42	0.082	0.20	1.31**
	(0.42)	(0.31)	(0.41)	(0.44)
750-999	0.10	0.18	0.23	0.74**
	(0.17)	(0.18)	(0.22)	(0.26)
1250-1499	-0.44**	-0.100	-0.028	-0.94***
	(0.16)	(0.16)	(0.26)	(0.22)

Continued on next page

Table A.6: Results of Probit Regressions for Uganda and Zimbabwe (continued)

Variable	Uganda		Zimbabwe	
	pre-	post-	pre-	post-
1500+	0.16	-0.19	0.14	-1.00**
	(0.32)	(0.41)	(0.41)	(0.36)
Constant	1.54	1.31	-0.85	-1.23
	(0.87)	(0.73)	(1.08)	(1.06)

Standard errors are in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ in two-tailed t-test.

B Proofs

This section presents the proof of the estimator provided by Deuchert and Wunsch (2014).

Estimator. *Using assumptions 1 through 6, the average treatment effect for an intervention that affects health only through its effect on behaviour change can be represented by $\hat{\theta}$, according to following definitions:*

- N_1*: post-intervention sample size.*
- y_{i1}*: outcome at time $T = 1$ for individual i. $y_{i1} = 1$ indicates that the child has perished, whereas $y_{i1} = 0$ indicates that the individual is still alive.*
- b_{i1}*: indicator for ITN use at time $T = 1$ for individual i. $b_{i1} = 1$ indicates that the individual uses an ITN, whereas $b_{i1} = 0$ indicates that the individual does not.*
- $\hat{p}_1(x_{i1})$*: the predicted probability of ITN usage for individual i at time $T = 1$ as a function of characteristics x_{i1}. This estimate is obtained by performing a probit regression of B^1 onto the covariates and predicting the propensity scores for post-intervention individuals.*
- $\hat{p}_0(x_{i1})$*: the predicted probability of ITN usage for individual i at time $T = 0$ as a function of characteristics x_{i1}. This estimate is obtained by performing a probit regression of B^0 onto the covariates and predicting the propensity scores for post-intervention individuals.*

$$\hat{\theta} = \frac{1}{N_1} \sum_{i=1}^{N_1} y_{i1} \left[\frac{b_{i1}}{\hat{p}_1(x_{i1})} + \frac{1 - b_{i1}}{1 - \hat{p}_1(x_{i1})} \right] [\hat{p}_1(x_{i1}) - \hat{p}_0(x_{i1})] \tag{8}$$

Proof. This parameter estimates the effect of a nationwide intervention, so it is defined as:

$$\text{ATET} = \mathbb{E}[Y^1 - Y^0 \mid T = 1],$$

where $T = 1$ represents the post invervention period. Because this is a nationwide intervention (i.e. every individual is subjected to the treatment), $I = T$, which is why T is used in the notation. Under assumptions 1 and 2, this can be written as the expected value of post-intervention $Y^1 - Y^0$ conditional on every possible combination of pre- and post-intervention behaviour times the probability of that behaviour combination.

$$\begin{aligned} \text{ATET} = {} & \mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 0, B^1 = 0]\mathbb{P}[B^0 = 0, B^1 = 0 \mid T = 1] \\ & + \mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 0, B^1 = 1]\mathbb{P}[B^0 = 0, B^1 = 1 \mid T = 1] \\ & + \mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 1, B^1 = 0]\mathbb{P}[B^0 = 1, B^1 = 0 \mid T = 1] \\ & + \mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 1, B^1 = 1]\mathbb{P}[B^0 = 1, B^1 = 1 \mid T = 1]. \end{aligned} \tag{9}$$

By Assumption 1, pre- and post-intervention behaviour is equivalent, $Y^1 = Y^0$, so the first and last lines above are 0. By Assumption 2, the probability that an individual changes from using an ITN to not using an ITN is 0. This leaves only

the second line, denoted with (9). Assumptions 2 and 3 allow the last term of the remaining expression to be calculated by conditioning on a set of observed objects, $\mathbb{P}[B^0 = 0, B^1 = 1 \mid T = 1, X = x]$ where the set $X \equiv X^B \bigcap X^Y$ is the set of all covariates and confounders. Adding and subtracting a term and subtracting a term totalling zero (by Assumption 2) converts this probability into a difference of expected values.

$$\begin{aligned}
&\mathbb{P}[B^0 = 0, B^1 = 1 \mid T = 1, X = x] \\
&= \big(\mathbb{P}[B^0 = 0, B^1 = 1 \mid T = 1, X = x] + \mathbb{P}[B^0 = 1, B^1 = 1 \mid T = 1, X = x]\big) \\
&\quad - \big(\mathbb{P}[B^0 = 1, B^1 = 1 \mid T = 1, X = x] + \mathbb{P}[B^0 = 1, B^1 = 0 \mid T = 1, X = x]\big) \\
&= \mathbb{P}[B^1 = 1 \mid T = 1, X = x] - \mathbb{P}[B^0 = 1 \mid T = 1, X = x]. \qquad (10)
\end{aligned}$$

ITN being a binary variable enables consideration of probabilities in equation (10) instead as expected values, since

$$\begin{aligned}
\mathbb{E}[B^1 \mid T = 1, X = x] &= 1 * \mathbb{P}[B^1 = 1 \mid T = 1, X = x] \\
&\quad + 0 * \mathbb{P}[B^1 = 1 \mid T = 1, X = x] \\
&= \mathbb{P}[B^1 = 1 \mid T = 1, X = x]
\end{aligned}$$

Substituting this into (10) and exploiting Assumption 3, a new expression for the second term of equation (9) emerges.

$$\begin{aligned}
&\mathbb{P}[B^0 = 0, B^1 = 1 \mid T = 1, X = x] \\
&\quad = \mathbb{E}[B^1 \mid T = 1, X = x] - \mathbb{E}[B^0 \mid T = 1, X = x] \\
&\quad = \mathbb{E}[B^1 \mid T = 1, X = x] - \mathbb{E}[B^0 \mid T = 0, X = x] \\
&\quad = \mathbb{E}[B \mid T = 1, X = x] - \mathbb{E}[B \mid T = 0, X = x]
\end{aligned}$$

The original ATET expression is now

$$\mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 0, B^1 = 1] \left(\mathbb{E}[B \mid T = 1, X = x] - \mathbb{E}[B \mid T = 0, X = x]\right).$$

As is typical in treatment effects literature, we can define observed B as a linear function of potential outcomes.

$$B \equiv TB^1 + (1 - T)B^0.$$

Similarly, we can define Y^I in terms of the potential outcomes. However, in this case, the behaviour-specific potential outcomes are the outcomes of interest,

$$\begin{aligned}
Y^1 &\equiv B^1 Y^{11} + (1 - B^1)Y^{10} \\
Y^0 &\equiv B^0 Y^{01} + (1 - B^0)Y^{00}
\end{aligned}$$

Substituting these definitions into the first term of expression (9),

$$\begin{aligned}
&\mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 0, B^1 = 1] \\
&\quad = \mathbb{E}\left[B^1 Y^{11} + (1 - B^1)Y^{10} - B^0 Y^{01} + (1 - B^0)Y^{00} \mid T = 1, B^0 = 0, B^1 = 1\right] \\
&\quad = \mathbb{E}\left[Y^{11} - Y^{00} \mid T = 1, B^0 = 0, B^1 = 1\right] \\
&\quad = \mathbb{E}\left[Y^{11} - Y^{10} \mid T = 1, B^0 = 0, B^1 = 1\right],
\end{aligned}$$

where the last equality comes from Assumption 1. Next, Bayes' Rule and the definition of expected value are utilized to obtain the parameter of interest.

$$\begin{aligned}
&\mathbb{E}[Y^1 - Y^0 \mid T = 1, B^0 = 0, B^1 = 1] \\
&\quad = \int \mathbb{E}\left[Y^{11} - Y^{10} \mid T = 1, B^0 = 0, B^1 = 1, X = x\right] f(x \mid T = 1, B^0 = 0, B^1 = 1)dx \\
&\quad = \int \mathbb{E}\left[Y^{11} - Y^{10} \mid T = 1, X = x\right] f(x \mid T = 1, B^0 = 0, B^1 = 1)dx \\
&\quad = \int \Big(\mathbb{E}\left[Y^{11} \mid T = 1, X = x\right] - \mathbb{E}\left[Y^{10} \mid T = 1, X = x\right] \\
&\qquad \times f(x \mid T = 1, B^0 = 0, B^1 = 1)dx \\
&\quad = \int \left(\mathbb{E}\left[Y \mid T = 1, X = x\right] - \mathbb{E}\left[Y \mid T = 1, X = x\right]\right. \\
&\qquad \times f(x \mid T = 1, B^0 = 0, B^1 = 1)dx \\
&\quad = \int \left(\mathbb{E}\left[Y \mid T = 1, X = x\right] - \mathbb{E}\left[Y \mid T = 1, X = x\right]\right. \\
&\qquad \times \mathbb{P}[B^0 = 0, B^1 = 1 \mid T = 1, X = x) f(x \mid T = 1)dx \\
&\quad = \int \left(\mathbb{E}\left[Y \mid T = 1, X = x\right] - \mathbb{E}\left[Y \mid T = 1, X = x\right]\right. \\
&\qquad \times (\mathbb{E}[B \mid T = 1, X = x] - \mathbb{E}[B \mid T = 0, X = x]) f(x \mid T = 1)dx.
\end{aligned}$$

Applying Bayes' rule to the last term, the estimator for this expected value is that found in Equation 2.

$$\hat{\theta} = \frac{1}{N_1} \sum_{i=1}^{N_1} y_{i1} \left[\frac{b_{i1}}{\hat{p}_1(x_{i1})} + \frac{1 - b_{i1}}{1 - \hat{p}_1(x_{i1})}\right] [\hat{p}_1(x_{i1}) - \hat{p}_0(x_{i1})]$$

□