THE GRADUATE INSTITUTE | GENEVA

INSTITUT DE HAUTES ETUDES
INTERNATIONALES ET DU DÉVELOPPEMENT

GRADUATE INSTITUTE OF INTERNATIONAL
AND DEVELOPMENT STUDIES

Graduate Institute of
International and Development Studies Working Paper
No: 05/2012

Down with diarrhea: Using fuzzy Regression Discontinuity Design to link communal water supply with health

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This paper contributes to the existing literature by demonstrating that the provision of communal water supply can be effective in improving child health if the targeted population shows adequate hygiene awareness and behavior. Until now, the fast growing body of literature on water development interventions could not establish a significant effect of communal water supply on health. The insignificant health effect regarding communal water supply (in contrast to other types of water interventions) found in meta-studies may be explained by re-contamination of the water between the source and the point of use; and by the lack of studies which address the mode of selection into treatment of water programs which may result in biased estimates. To identify the health effect of communal water supply, a fuzzy Regression Discontinuity Design set-up is applied using an eligibility criterion as source of exogenous variation. The paper also provides practical insights in a little explored extension of the fuzzy Regression Discontinuity Design which may have great relevance for applied research: As occurs often in practice, the forcing variable determining treatment could not be directly observed. For this reason, a slightly noisy measure was reconstructed. To convince the critical reader of the validity of this approach, a variety of robustness checks are carried out and the results are cross-validated through two additional identification strategies: a village fixed effects and an instrumental variable approach.

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March 26, 2012

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*I thank Jean-Louis Arcand and Eva Terberger for their supervision and support. Furthermore, I would like to thank Marlis Sieburger and the evaluations department of the KfW Entwicklungsbank, Rudolf Schumacher and Andreas Rach for making available the data on which this analysis is based. For their input and comments, I thank Ugo Panizza, Gunhild Berg, Lindsey Novak, Dany Jaimovich, Anja Bentlage, Jan Marcus, Anatol Sargin, Lister Chapeta, Ankai Xu, Jean-Daniel Champagnac, Tomas Hozik, Enrico Berkes, Abbad El-Rayyes, Cornelius Kuth, the participants of the Geneva Water Research Conference and the participants of the presentation at the KfW Entwicklungsbank. A special thanks to Tehani Pestalozzi, who read several versions of this paper, for her patience and numerous comments.

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1 Introduction

In much of the world, especially where public infrastructure is poor, disease resulting from inadequate water supply is a major public health issue and constraint to development. The most widespread health hazards linked to water are diarrheal diseases, which disproportionally affect young children. Diarrhea is "a leading cause of child mortality and morbidity" [WHO, 2009, p. 1]. and even if not fatal, may have long-lasting consequences for human health and development [Moore et al., 2001, Niehaus et al., 2002], hindering the economic and social potential of affected societies.

In 2000, the international community placed the issue of water and sanitation high on the development agenda by making it a Millennium Development Goal (MDG). Much progress has been made since then. In 2012, the Joint Monitoring Programme for Water Supply and Sanitation [JMP, 2012] announced that this MDG, "to halve, by 2015, the proportion of the population without sustainable access to safe drinking water", was met in 2010, five years ahead of schedule. Although impressive, the high global access rate of 89 per cent hides great heterogeneity in access and imperfect metrics. First, lack of access remains pronounced for rural and low income populations in Subsaharan Africa and Oceania with a high absolute number of people not having access to safe water. Second, the proxy (i.e. improved water supply infrastructure[1]) used to estimate MDG achievement, does not always translate into safe water and health benefits – as it does not take into account actual water quality, quantity, intermittent water supply and sustainability.

This paper contributes to the fast-growing water literature by demonstrating that communal water supply can be effective in decreasing child diarrhea. Meta-reviews of impact evaluations on water supply and sanitation [Waddington et al., 2009, White et al., 2008, Fewtrell and Colford Jr, 2004] show the effectiveness of some types of interventions (notably water quality treatment, hygiene, piped water (household connections)) but do not establish a significant health impact of communal water supply. In addition to the health impact, I find considerable and significant time savings vis-à-vis the control group. This finding has a strong gender implication since mainly women haul water and care for sick family members.

My analysis relies on household survey data from the rural water supply program "Fouta Djallon II" in Guinea. To address potential selection problems, an eligibility criterion is used in a fuzzy Regression Discontinuity Design (RDD) set-up to identify a local average treatment effect. The paper also provides practical insights in a little-explored extension of the fuzzy RDD, which may have great relevance for applied research. As occurs often in practice, the forcing variable determining treatment could not be directly observed. For this reason, a slightly noisy

[1] The infrastructure definition is according to the widely used typology of WHO and UNICEF [2006]. The categories are listed in 1. In this paper, I rely on the same definition of improved water supply.

measure was reconstructed. To convince the critical reader of the validity of this approach, a variety of robustness checks are carried out and the results are cross-validated through two additional identification strategies: a village fixed effects and an instrumental variable approach.

In all three approaches, I find a strong reduction in diarrhea prevalence in children, which can be attributed to the communal water supply program. The point estimates range between -0.29 and -0.31 and are robust to various specifications and tests. Although I find significantly increased access to improved water sources in program villages, I could not identify a significant change in hygiene awareness and behavior. The suggested ineffectiveness of the hygiene component may be explained by the already high level of appropriate behavior prior to the program in the region.

2 Empirical evidence from the literature on water

2.1 Access to water, disease prevalence and its consequences

The official MDG monitoring [JMP, 2012] shows the achievement of the aggregate water MDG in the year 2010, but also indicates that there is considerable heterogeneity between geographies and income strata (Table 2 and Figure 1). Although the proportion of those lacking access to improved water supply has been halved, safe water remains a challenge in absolute terms.

In 2010, an estimated 780 million people, most of which live in rural areas, still rely on unimproved sources of water. Sub-Sahara Africa is "not on track" to the regional achievement of the water MDG. The situation is worse when it comes to sanitation. The global target will not be met if current trends persist and in Sub-Saharan Africa, two-thirds of the population continue to lack improved sanitation [JMP, 2012].

Guinea is an example of country where considerable progress has been made in access to water supply. The access rate increased from 51 per cent in 1990 to 74 per cent in 2010 with rural areas (65 per cent coverage in 2010) lagging behind urban areas (90 per cent coverage in 2010). At the same time, more than 80 per cent of the population does not have access to improved sanitation infrastructure.

In the year 2000, one year before the start of the rural water supply program Fouta Djallon II, only 52 per cent of the population in rural Guinea could rely on improved water supply. The lack of access to water was not due to scarce water resources; Guinea is sometimes referred to as the *"château d'eau de l'Afrique de l'Ouest"* [France, 2004] and the mountainous area of Fouta Djallon gives source to numerous rivers, including the Gambia, the Niger and the Sénegal

[France, 2004]. This lack of access – despite the resource availability – is due to economic reasons and weak institutional capacity. [KfW, 2007].[2]

The insufficient provision of water supply and sanitation facilities is associated with a high prevalence of diarrheal diseases which varies greatly according to regions. According to the Demographic and Health Survey (Direction Nationale de la Statistique de Guinée and Macro International Inc [2000]) of 1999, two years before Fouta Djallon II started, the national diarrhea prevalence among children up to age five was at 21.2 per cent[3]. The rate was higher in rural areas at 22.4 per cent compared to 17.8 per cent in urban areas. The most pronounced rate of 24.8 per cent persisted in Central Guinea, where the program was to take place [Direction Nationale de la Statistique de Guinée and Macro International Inc, 2000]. In the program area, prior to Fouta Djallon II, the health survey data (upon which this paper is based) indicates an even higher diarrhea prevalence of 43 per cent among children below the age of six[4]. This elevated rate seems plausible, taking into account the complete lack of improved water supply. Although no empirical evidence is available, it can be expected that this strikingly high prevalence rate likely results in an elevated child mortality and negative long-term consequences for child development.

Out of the diseases caused by unsafe water, infectious diarrhea has the most widespread negative health consequences [Prüss et al., 2002]. Diarrhea[5] is a symptom of an intestinal infection which can be caused by various bacterial, viral and parasitic organisms. Three clinical types are distinguished: Acute watery diarrhea, which last several hours or days (including cholera), acute bloody diarrhea (dysentery) and persistent diarrhea which lasts 14 days or longer. Diarrheal disease is both treatable and preventable [WHO, 2009], but –despite this – still causes approximately 1.7 million deaths among children per year worldwide, of which 701 000 in Africa and 552 000 in South-East Asia (Figure 3).

In addition to causing high child mortality, diarrhea also has long-term consequences that hamper both the physical and cognitive development of the child. Moore et al. [2001] show that early childhood diarrhea can lead to substantially stunted growth that continues beyond the age of six. In their study of a Brazilian shantytown, Niehaus et al. [2002] find long-term cognitive deficiencies among six- to ten-year-olds, due to childhood diarrhea within their first two years. Considering the results from the evolving literature by Heckman and collaborators on human

[2]The Republic of Guinea is among the poorest countries in the world ranking 170 of 182 in UNDP's Human Development Index 2009 [UNDP, 2009].

[3]The diarrhea prevalence rate is based on a two week recall period [Direction Nationale de la Statistique de Guinée and Macro International Inc, 2000].

[4]The main diarrhea variable in the health data only reveals information about whether at least one child in the household experienced an episode of diarrhea in the previous two weeks. Information on the number of episodes of diarrhea in a household was only available for 200 untreated households. Using this information, I calculated a diarrhea rate within the households. In a subsequent step, I build an overall prevalence rate weighting the household rates by the number of children under five.

[5]Diarrhea is defined as "the passage of 3 or more loose or liquid stools per day (or more frequently than is normal for the individual)" [WHO, 2009]. The survey question on diarrhea incidence among children in the households until the five years of age is based on a two week recall period.

inequality [Heckman, 2007], which emphasizes the importance to invest early in human capital, childhood diarrhea may lead to millions lagging behind their potential later in adult life.

2.2 Are water interventions effective? Evidence from previous impact evaluations

To understand why a water supply and sanitation (WSS) program is effective or not in reducing water-related disease, it is essential to understand the pathways of diarrhea transmission. According to Schmidt and Cairncross [2009, p. 3], it is widely accepted that diarrhea can be transmitted through person-by-person contact, contact with contaminated soil and surfaces, food, and flies. Hence, drinking water is only one pathway of transmission (see Figure 2) and despite the fact that "there is no doubt that many water and excretory related pathogens can be transmitted by contaminated drinking water", the question of the importance of this channel of transmission vis-à-vis the others remains [Schmidt and Cairncross, 2009, p. 3].

In their meta-reviews, Fewtrell and Colford Jr [2004, p. 28] and Waddington et al. [2009, p. 26] find no significant reduction in diarrhea due to the provision of water supply. When further distinguishing between household connections and source water supply, Waddington et al. [2009, p. 29] find the former effective and the latter ineffective. Fewtrell and Colford Jr [2004, p. 28] also come to a similar conclusion: restricting the analysis only to high-quality studies, the authors find that household connections are more effective than the communal provision of water, whereby the latter has no significant effect.[6]

Gasana et al. [2002] find evidence of water contamination during transport and storage[7], which may explain the ineffectiveness of water provision at the community level in many instances. The authors investigate the effect of different types of water supply on diarrhea among children in Huye, Butare in rural Rwanda. Although the study is purely descriptive, it gives some striking insights to problems with communal water supply in rural settings. By performing chemical tests, Gasana et al. [2002] find that most of the water got contaminated during its transport to the households irrespective of the source of water. Once brought to the households, the water was stored in containers, where "the contamination of water by the storage utensils could be seen by the naked eye in half of the cases (the water appeared dirty)"[Gasana et al., 2002, p. 83]. Hence, it does not come as a surprise that there was no difference in the average episodes of diarrhea experienced by children between the group relying on the improved water source and the control group [Gasana et al., 2002, p. 86].

[6] Regarding other types of interventions, Waddington et al. [2009] find a significant reduction in child diarrhea morbidity. Water quality interventions reduce morbidity by 42 per cent, sanitation interventions by 37 per cent, hygiene interventions by 31 per cent and multiple interventions by 38 per cent on average. The estimates of sanitation and mixed interventions have high standard errors compared to the other interventions. The meta-review of the Independent Evaluation Group of the World Bank [White et al., 2008] also comes to similar conclusions.

[7] A forthcoming impact evaluation by Guenther et al. also finds evidence for contamination during transport and storage and was unable to identify a health impact of communal water supply.

In the discussion of the results from previous studies, I rely mainly on Waddington et al. [2009]. It is the most recent and also methodologically most rigorous meta-study[8] on water, sanitation and hygiene interventions. However, one warning is in order: the insignificant health impacts of water supply interventions suggested by Waddington et al. [2009, p. 26] should be interpreted with caution. Only one of the reviewed studies in this category (Jalan and Ravallion [2003]) was rated high-quality. Jalan and Ravallion [2003] analyze the effect of piped water in India and find a significant impact on health. Although in both Jalan and Ravallion [2003] and this paper, the analyzed intervention was rolled out in a rural area, the interventions differ with respect to the treatment level: in India, piped water was supplied to the households directly whereas in Guinea, water was supplied at the community level. Considering the evidence from Gasana et al. [2002], this difference may be decisive. The health impact of the provision of communal water supply may be hampered by potential intervening factors affecting the quality of water between the water well and its point of final consumption (e.g. transport and storage).

Taking together previous evidence regarding water supply interventions, a tendency in the literature can be noticed: piped water connections to households are effective while communal water supply seems to remain ineffective.[9] While rigorous econometric techniques were applied to the question of piped water connections (notably Jalan and Ravallion [2003] and Gamper-Rabindran et al. [2010]), the available studies regarding rural water supply at the community level reveal methodological weaknesses. This impact evaluation contributes to the literature by providing results on the health benefits of communal water supply based on a rigorous identification strategy.

3 The rural water supply program Fouta Djallon II

The program Fouta Djallon II is part of a greater effort of the German Development Cooperation to provide water supply to rural areas in Guinea. Since the 1980s, approximately EUR 50 million have been committed to the rural water sector. Germany is currently the most substantial donor in this area. The program was preceded by Fouta Djallon I and has been followed by the Fouta Djallon III-IV [KfW, 2007, p. 6]. This analysis is confined to Fouta Djallon II, for which survey data is available.

The program had two objectives: The first objective is the sustained provision of water throughout the year to adequately cover the needs of rural households in terms of quality and

[8] Waddington et al. [2009] apply the Cochrane/Campbell Collaboration (C2) standards of systematic review.

[9] The former result regarding piped water interventions is also in line with a recently published study. Gamper-Rabindran et al. [2010] find a significant effect of piped water provision on infant mortality in Brazil. Using quantile panel techniques, Gamper-Rabindran et al. [2010] show that the size of the effect varies with the infant mortality rate whereby a stronger effect was identified at higher quantiles. In contrast to this finding, a recent report by Klasen et al. [2011] did not identify a positive health impact of piped water in Yemen where the delivery of water is oftentimes interrupted.

quantity of water. The second objective is to impart adequate hygienic awareness and behavior change in the areas of water and waste disposal. Through these objectives, the program sought to reduce health risks due to quantitatively and qualitatively insufficient water supply as well as to improve the living conditions for the village inhabitants [KfW, 2000, p. 4]. The project document emphasizes the importance of this program for women: because women have the cumulative responsibilities of taking care of sick family members and hauling water, they are likely to benefit most from time savings and increased health [KfW, 2000, p. 14].

Six eligibility criteria (Table 4) defined which villages could take part in the open program. The intervention was implemented from April 2001 until August 2005 in the prefectures of Labé, Tougué, Faranah and Mamou, which are located in the mountainous Fouta Djallon region [KfW, 2007].

To reach the objectives, 533 water wells with hand and foot pumps were built in the program area (see Figure 3). Additionally, four solar-based systems of water supply were build and 18 water wells rehabilitated. To complement the provision of water supply, an awareness campaign on appropriate hygiene awareness and behavior was implemented. The accompanying measures also encouraged a participative approach to the planning process as well as the creation of water committees for the maintenance of the wells [KfW, 2007].

4 The data

The data includes information on 69 health-related questions from a total of 1770 households in 105 rural villages of the Fouta Djallon region.[10] The survey was carried out in the provinces of Labé and Mamou during the months of November and December in 2003 and 2005 [Schumacher and Albrecht, 2006]. This time of the year marks the end of the rainy season.

Treatment status varies between villages and over time. Information on 33 of the villages exists for both periods (also some untreated villages were surveyed in 2005 for the second time). However, the households in 2005 were newly selected in all villages. The data structure is cross-sectional. Due to the nature of the open program Fouta Djallon II, the participating villages were not randomly selected (seechapter 3). Partial randomization took place for the control group of 2003, although the universe and the selected sample coincide highly. The households were selected by dividing the respective village in four zones and spinning a bottle to select the first household. Subsequently, two to three neighboring households were also visited for interviews. If available, two women between the age of 15 and 45 were polled. [Schumacher and Albrecht,

[10] The surveys were directed by the medical consultant Rudolf Schumacher and his team. The consultant and his team conducted a previous descriptive analysis in which they compare differences in frequencies of characteristics between the treatment and control group [Schumacher and Albrecht, 2006].

2006]. In sum, the dataset shows a non-RCT[11] structure comprising a treatment and control group of similar size for both years.

The health data was complemented with administrative data from the project documentation (KfW [2000], KfW [2007], Beller [2006]). For the subsequent analysis, a subsample of the health data is used. Observations with missing values in one of the relevant covariates were excluded from the sample, resulting in a subsample of 1139 observations. All estimations (except the village fixed effects model) were performed on this sample to ensure comparability of the results. Table 5 reports summary statistics of the main variables of interest.

The water wells which were constructed by this program can be categorized as improved water supply, according to the WHO and UNICEF [2006] definition. The usage of the terms *improved and unimproved drinking water* relies on the assumption that "certain types of drinking-water sources are likely to deliver drinking-water of adequate quality for their basic health needs" [WHO and UNICEF, 2006, p. 6]. The types of water sources and their categorization according to this definition are listed in Table 1. The rationale behind the application of this typology instead of tests of water quality is pragmatism: It would be simply too costly and time-consuming to carry out quality tests of water in survey work [WHO and UNICEF, 2006, p. 6]. Hence, the availability of an improved water source cannot be equated with appropriate water quality, although it can be considered a good approximation for it.

Regarding sanitation, "facilities that are not shared between households and that hygienically separate human excreta from human contact are considered to be adequate", whereby those technologies which meet the outlined criterium are considered improved [WHO and UNICEF, 2006, p. 7]. When applying this strict definition of improved sanitation, only two households in the sample dispose of improved sanitation. Hence, in the subsequent analysis, more primitive types of sanitation are considered basic sanitation[12] to distinguish between households with basic sanitation and those without (i.e. defaecation in the bushes, open toilet and public toilets).

[11] Randomised-controlled trial.

[12] Specifically, covered toilets, ventilated fosses and toilets with a water flush are considered basic sanitation. The latter category, which would possibly qualify as improved sanitation, only appeared in two observations.

5 The identification strategy and results

In this section, I discuss the identification strategy and inherent endogeneity issues in the context of Fouta Djallon II and present the results of the estimations. The underlying model is depicted in equation 1. Y_{vht} is the dependent variable (i.e. the health status) in household h of village v at time t. The treatment status D_{vt} takes the value of one if the village received the treatment at time t and zero otherwise. The main coefficient of interest is β, which shows the effect of the program on the outcome variable (i.e. health status). To operationalize the health status, I choose diarrhea among children until five years of age[13], which is the dominant water-related health indicator in the literature (see Waddington et al. [2009], White et al. [2008] and Fewtrell and Colford Jr [2004]). This indicator seems to be an adequate operationalization of the health impact since children bear the highest burden from water-related disease of which diarrhea is the most important (see chapter 2.1).

$$Y_{vht} = D_{vt}\beta + X_{vht}\gamma + \psi_v + \eta_t + \epsilon_{vht} \tag{1}$$

X_{vht} signifies the exogenous controls (socio-economic household characteristics and basic sanitation[14]) which are included in the estimation. ψ_v is the village-specific time-invariant heterogeneity, η_t indicates the time-specific household-invariant unobserved heterogeneity and ϵ_{vht} is the error term. If the village specific hetereogeneity ψ_v, time-specific hetereogeneity η_t and possibly other unobserved hetereogeneity (e.g. household-specific) are not taken into account in the identification strategy, they are absorbed by the error term.

Potentially ψ_v, η_t and other unobserved hetereogeneity may be correlated with treatment status and diarrhea and thereby impose an endogeneity bias on all coefficients. In fact, it is likely that ψ_v is correlated with D_{vt} and Y_{vht} because the open program targeted villages which previously did not have adequate water supply in quality or quantity. Therefore, villages which systematically dispose of unfavorable observable and also most likely unobservable characteristics related to diarrhea were more likely to be selected. Apart from village-specific time-invariant unobservables, there may also be other unobserved heterogeneity (e.g. household-specific or time-variant) which could simultaneosly influence treatment and outcome.

In the case of the open program Fouta Djallon II, the treatment occurs at the village level. Therefore, it is likely that the selection into the treatment group is also mainly related to village-specific characteristics. For this reason, the main concern lies in the village-specific unobserved hetereogeneity ψ_v which may simultaneosly determine the treatment status and outcome (i.e.

[13]Households were asked to report whether one or more of the children under five years of age in the household experienced an episode of diarrhea within the last two weeks. Hence, the dependent variable is binary.

[14]More specifically: household size, number of children, children in schoool, basic sanitation, living situation, marital status, education, economic activity and year of survey.

diarrhea). Other unobservables are less likely to simultaneosly and systematically determine treatment status and diarrhea, however, this remains a possibility.

5.1 OLS and OLS with village fixed effects estimation

The Ordinary Least Squares (OLS) results from the estimations[15] (Table 6) indicate that the program had a highly significant (at the 1 per cent significance level) and negative impact on diarrhea among children. Children under six years of age who live in a village receiving the program had a 21 per cent lower chance of experiencing an episode of diarrhea in the two weeks prior to the survey vis-à-vis their peers in the control group.[16]

However, as discussed in the previous section, the OLS estimates are likely to be biased due to the unobserved hetereogeneity at the village level ψ_v. This unobserved hetereogeneity may simultaneosly determine treatment and outcome (i.e. it is likely that those villages with adverse unobservable characteristics regarding water are selected into the treatment group). A naive OLS estimator implies a comparison between the treated villages, which systematically dispose of unfavorable characteristics regarding water and diarrhea, with the control group. Hence, this comparison is likely to result in an underestimation of the program impact.

Since the largest concern pertains to village-specific unobservables, it is natural to apply a model with village fixed effects exploiting the structure of the dataset. 33 villages were visited in both survey years 2003 and 2005 (i.e. before and after they received the treatment). All villages which were not surveyed in both years are dropped from the sample[17] and the estimations are performed on a subsample of 33 villages. By estimating equation 1 by OLS with village and time fixed effects[18], I control for all village-specific time-invariant and time-specific household-invariant hetereogeneity. The latter is rather unimportant for the selection into the program because it has no differential effect on the households and is likely not to have a differential effect on the villages either. Under the assumption that only the time-invariant part of the village specific unobservables[19] drives the selection into the treatment group, OLS with fixed effects estimates the program impact consistently.

The results of the estimation of equation 1 by OLS with village and time fixed effects on the

[15] All estimations in this paper were implemented using R: a Language and Environment for Statistical Computing [R Development Core Team, 2010].

[16] $E(X) = \sum_{i=1}^{n} x_i * P(X = x_i)$. Hence, in the case of a binary independent and dependent variable, the coefficient β can be interpreted as the change in probability because $E(X) = P(X = 1) * 1 + P(X = 0) * 0 = P(X = 1)$.

[17] The treatment status and the village fixed effects would be perfectly collinear for those villages.

[18] Fixed effects models can be implemented in various ways. A conveniant way of implementing village and time fixed effects is by including a set of dummy variables (see Wooldridge [2002, chap. 10]). A dummy variable indicating one, if the particular observation is from this specific village and zero otherwise, is included in the specification for each village. Also, a time dummy variable is added to the controls indicating whether the observation is from 2003 or 2005.

[19] This assumption seems a good first approximation because it is unlikely that villages change their aggregate characteristics to a large extent within the rather short period of two years.

reduced sample are reported in Table 6. When controlling for fixed effects, the program impact appears considerably stronger: β is estimated -0.31. In other words, children living in a treated village have a 31 percent lower chance of experiencing an episode of diarrhea within the two weeks prior to the survey. The estimate is significant at the 1 per cent level. With this method, program impact is 48 per cent stronger compared to the OLS estimator. The fixed effects result is in line with the supposition that the OLS estimator is downward biased.

The fixed-effects estimation can serve as a robust workhorse model and provide a first indication of program impact. However, one caveat is in order for this approach. By applying a fixed effects model on a reduced sample, a large amount of information is lost. More concerningly, this loss of information is systematic. Only those villages that were already scheduled at the time of the first survey round to receive the treatment before the second survey appear twice in the sample.

5.2 Instrumental variables approach

Another strategy to identify program impact is an instrumental variables approach (see Wooldridge [2002, chap. 5] and Greene [2003, chap. 5]) which uses the distance to the next national road as an instrument for treatment. Distances have been used as a source of exogenous variation in various publications including Miguel and Roland [2006] and Arcand and Wouabe [2009].

The conditions outlined in Table 4 define the villages' eligibility to the open program but are not a sufficient condition for treatment. In practice, according to Beller [2006, volume 2, p. 4] additional criteria proved decisive in determining treatment status: accessibility to construction works, no cemeteries nearby, the construction area must not be easily flooded and should not be far from the village.

The distance to the next road seems a good proxy for accessibility. It is intuitively evident that accessibility was an important factor in the selection of the program. Hence, the relevance condition of the instrument seems to be met. But can we reasonably assume that there is no effect of the distance to the next national road, other than through the program, on diarrhea among children until the five years of age (exclusion restriction)? A priori, distance seems exogenous to diarrhea among children.

Critics may argue that the distance to the next national road is a proxy for remoteness and, hence, may be correlated with information (e.g. on hygiene awareness and behavior) or wealth (through better market access) which in turn may influence diarrhea. In the treatment and control group, there seem to be no strong differences as to the availability of information. According to Andreas Rach of the implementing consultant Beller, the villages interact intensively with other nearby villages. Table 19 shows indeed that levels of hygiene awareness in the control and treatment groups are similar. To counter potential critics and to remedy effects of remoteness on hygiene awareness and information and wealth, two distances (distance to the provincial capital and distance to the national capital Conakry) are included as controls. Conditional on remoteness (proxied by the distance to the provincial capital and the capital), I find no a priori reason why the distance to the next road should have a direct effect on diarrhea or through another mechanism than treatment. Because of the untestable nature of the exclusion restriction, this approach relies on a strong maintained assumption.

Program impact was estimated through a two-stage least squares procedure whereby treatment status in the structural equation (equation 2) is instrumentented by the distance[20] to the next road in the first stage (equation 3).

Second stage:

$$Y_{vht} = \alpha_1 + \widehat{D}_{vt}\beta + X_{vht}\gamma + \epsilon_{vht} \tag{2}$$

First stage:

$$\widehat{D}_{vt} = \alpha_2 + Z_v\pi + X_{vht}\delta + \mu_{vht} \tag{3}$$

A particular concern with this approach is weak instruments. When instruments are weakly correlated with the endogenous variable, this can lead to weak identification, making conventional inferences misleading [Stock et al., 2002]. The first stage results (Table 7) indicate that the instrument is indeed relevant. The distance to the next road has a negative effect on the probability of receiving the program, which is in line with expectations. Villages which are more difficult to access for the construction works have a lower probability of receiving the intervention. The effect is significant at the one per cent level. To further analyze instrument relevance, a partial R^2 is computed (see Shea [1997]). The partial R^2 of 10 percent indicates that the instrument is relevant but cannot be considered a strong instrument.

The program impact estimated by the instrumental variables approach is almost identical to the effect estimated by the fixed effects estimator (see second stage in Table 6). The program reduced diarrhea prevalence among children by 29 percent. The effect is significant at the 5 percent level. The almost identical result is an indication for the robustness of the effect size.

[20]The distances were constructed based on data of the technical reports from the implementing consultant Beller Consult. 92 of 105 villages could be precisely located from Beller [2006] with respect to their longitudes and latitudes. For the remaining 13 villages the geographical centers of their respective districts were used as an approximation. Guinea is composed of 8 administrative regions with 33 prefectures and 303 sub-prefectures. Sub-prefectures are then subdivided into districts (Information according to the Guinean government: http://www.guinee.gov.gn/adm_regionale.php.). Hence, the geographical center of a district is a relatively precise approximation of village location. Based on the geographical references, the distance from the respective village to the provincial capital, the national capital, Conakry, and the distance to the next national road were calculated. The distance to the provincial capital and capital was calculated using the Stata module *vincenty* which calculates the geodesic distance on the Earth's surface. The distance to the next road was measured using Google Earth.

5.3 Fuzzy Regression Discontinuity Design

A Fuzzy Regression Discontinuity Design (RDD) can be applied to "settings where treatment is determined by whether an observed forcing variable exceeds a cutoff point" [Lee and Lemieux, 2009, p. 1]. This situation is frequently found in social programs or development interventions where treatment is oftentimes determined by eligibility criteria; this is also the case for Fouta Djallon II (see eligibility criteria in Table 4). The basic idea is that individuals who barely do not fulfill one of the eligibility criteria are a good counterfactual to those who barely fulfill it. This statement is true under the assumption that indviduals do not have precise control over the forcing variable which determines whether an individual is above or below the threshold [Lee and Lemieux, 2009].

In Fouta Djallon, villages that were characterised by a specific investment cost of less than 100 Euro per inhabitant were eligible to receive the rural water supply program. The specific investment costs depend on the costs of the drill (i.e. geological characteristics), the costs of the pumps and other well components, engineering consulting and the number of inhabitants in the village. Hence, it can be taken as a given that the village inhabitants only have imprecise control of the specific investment costs at most. Thus, "there is a striking consequence: the variation in the treatment in a neighborhood of the threshold is 'as good as randomized'" [Lee and Lemieux, 2009, p. 13].

When treatment is only partly determined by whether the value of the forcing variable is below or above the threshold, a variant of Regression Discontinuity Design is applied: the fuzzy RDD (see Lee and Lemieux [2009, p. 20–22]). Treatment is then instrumented by the administrative rule in an instrumental variable set-up. The point estimate can be interpreted as a local average treatment effect, or "average treatment effect for the subpopulation affected by the instrument" [Lee and Lemieux, 2009, p. 22]. Non-perfect determination of treatment status by the rule may be due to different reasons. In the case of this impact evaluation, the original data on the specific investment costs from the planning and selection phase was not available. Instead, a slightly noisy measure was reconstructed based on technical reports by the implementing consultant [Beller, 2006][21] and applied in a fuzzy RDD framework.

Lee and Lemieux [2009, p. 73] suggest to extend the RDD framework to situations where one cannot directly observe the forcing variable but instead can make use of a slightly noisy measure of the variable. This departure from the classical RDD set-up is not as well explored as the case of non-perfect compliance [Lee and Lemieux, 2009, p. 73]. One can think of many

[21] The reports entail information on the average drilling costs per sub-prefecture, the costs of the components of a water well and the cost of the engineering consulting. Hence, the specific investment costs were reconstructed as the sum of the costs of the drills, the costs of the pumps and the costs of the engineering consulting divided by the number of inhabitants. Because the base year for the specific investment costs was set as 1994, the specific investment costs were adjusted for exchange rate depreciation on the basis of the actual payment stream of the program.

instances where researchers can approximate an unobserved forcing variable for an underlying exogenous rule. This paper demonstrates the practicality of the application of this extension and may provide insights for the application of this extension.

In the application of the fuzzy RDD approach, I follow Lee and Lemieux [2009]. Equations 4, 5 and 6 are estimated by two-stage least squares, whereby Y_{vht} relates to the dependent variable diarrhea, D_{vt} to the treatment status, T_v to the eligibility rule, $X_v - c$ is the forcing variable X_v deducted by the value of the threshold c, and $D_v(X_v - c)$ is an interaction term which allows different slopes for the relation between the forcing and outcome variables for treated and untreated individuals.[22] The coefficient of interest indicating the program impact is τ. In a strict RDD, the pooled estimation of equation 4 is identical to estimating an individual Ordinary Least Squares regression to the left and the right of the threshold, and then to evaluate the difference between the regression intercepts, which indicates the treatment effect [Lee and Lemieux, 2009, p. 38]. In the fuzzy RDD set-up, the treatment status D_{vt} is instrumented by the administrative rule T_v[23] [Lee and Lemieux, 2009, p. 47].

Second stage:

$$Y_{vht} = \alpha_l + \tau D_{vt} + \beta_l(X_v - c) + (\beta_r - \beta_l)D_{vt}(X_v - c) + \epsilon_{vht} \tag{4}$$

First stage 1:

$$D_{vt} = \gamma_l + \delta T_v + \phi(X_v - c) + (\psi_r - \psi_l)T_v(X_v - c) + \nu_{vt} \tag{5}$$

First stage 2:

$$D_{vt} \times (X_v - c) = l + \eta T_v + \omega(X_v - c) + (\chi_r - \chi_l)T_v(X_v - c) + \mu_{vt} \tag{6}$$

There are two critical choices in this identification strategy: The level of the threshold and the functional form of the forcing variable. Figure 4 shows the distribution of treated individuals aggregated by bins of specific investment costs.[24] When we inspect the relative frequency of

[22] Due to the local randomization, it is not necessary to include any covariates [Lee and Lemieux, 2009].

[23] The instrument is binary. In this paper, it takes on the value one if the specific investment costs of the respective village are above the threshold and zero otherwise.

[24] The bin size was chosen such that the obtained graph is visually informative and there is no overlap at the threshold. The bandwidth subsequently tested with two bin size tests which are suggested by Lee and Lemieux [2009]. The first test concerns oversmoothing. I test whether there is a significant difference in fit between a linear regression model with i bins and $2i$ bins when regressing the outcome variable diarrhea on bin dummies. If the null is not rejected, this is an indication that the bin specification is appropriate. The F of 0.12 is well below the critical value of 1.64. The second test gives an indication of whether the bins are narrow enough. The idea is to add interaction terms between the bin dummies and the forcing variable and to test whether the interaction terms are jointly significant (i.e. whether the value of the specific investment costs makes a difference regarding diarrhea conditional on a specific bin). The F statistic -0.37 does also not surpass the critical value of 1.55 in absolute terms. Hence, the bins seem to be narrow enough such that there is no differential effect of specific investment costs on diarrhea, conditional on the bins.

treatment plotted against the forcing variable, a considerable drop in treatment can be noticed close to 100 Euro, the value of the eligibility criterion. To be more precise, the threshold was identified at 97 Euro. Below the threshold, more than 60 per cent of the individuals were treated in any given bin. Above the threshold, the relative frequency falls considerably (by 33 per cent) and remains at a lower level afterwards before falling towards zero. The critical reader may have identified further kinks in the relative frequency of treatment around the values of 126, 164 and 194 which also could constitute a threshold (see Figure 7); those potential thresholds prove insignificant when tested through placebo regressions.[25]

The second critical choice is the adequate parametrization of the functional form of the forcing variable. There is no a priori reason to believe that the underlying true model is linear. A convenient way to allow for nonlinearity is through the inclusion of polynomials of the forcing variable and their interaction terms with treatment status and the eligibility rule respectively. Lee and Lemieux [2009] suggest to include bin dummies of the forcing variable in the regression and to test their joint significance to analyze "how well the polynomial model fits the unrestricted graph". Applied to this analysis, this procedure suggests that the polynomial of order one provides a good fit.[26] For this reason, I rely on the linear specification of the fuzzy RDD in the subsequent analysis.[27]

The fitted values of linear regressions above and below the threshold in Figure 5 give a first indication of a considerable jump at the threshold[28]. However, this graph can only provide an orientation: Due to the fuzzy character of the assignment of treatment, there are untreated villages to the left and treated villages to the right of the threshold; hence, the jump tends to underestimate the program impact. The visibility of the jump, despite the fuzziness of the treatment determination, is a strong indication for the effectiveness of the program in reducing diarrhea.

The estimation of the model in equations 4, 5 and 6 confirms the results from the eyeball

[25] Could those other drops in the relative frequency be the adequate threshold? To counter this potential critique, I run placebo fuzzy RDDs at those three thresholds to convince the critical reader otherwise. The results of the placebo regressions are reported in Table 17 for the first stage and Table 18 for the second stage. The first stages indicates a fall in the relative frequency of treatment - albeit not significant. When proceeding to the second stage estimation, the placebo treatment proves insignificant in all three cases.

[26] The Wald test statistic indicates a value of 3.07 while the critical value is 3.84. Hence, the null of joint insignificance of the bins (=good fit of the linear specification) cannot be rejected.

[27] To test the robustness of the linear specification, higher order polynomials were added in subsequent estimations. When second order polynomials and their respective interaction terms are included, those terms are not significant suggesting that they do not provide important additional information (see Tables 14 and 15). The LATE estimate changes slightly to -0.25 and is rendered insignificant. When higher order polynomials are added to the specification, the sign of the treatment coefficient remains negative but the estimates become unrealistically large. To further test for nonlinearities, I perform an OLS regression on the pooled second stage (equation 4) and inspect the residual plot (see Kohler and Kreuter [2006]). If the residuals show normal behavior, the relationship between diarrhea and the forcing variable and its interaction term is linear. When inspecting Figure 6, we can eyeball that the observations are evenly distributed above and below zero. The residuals seem well behaved and, hence, a linear specification for the fuzzy RDD estimations appropriate. In the subsequent estimations, I apply the linear specification following the results from the previous tests. The linear specification seems to do best and is also in line with the magnitude of effect size suggested by the other estimation methods used in this paper.

[28] First, the data was aggregated on the village level to obtain a continuous outcome variable instead of the dummy variable. In a second step, two OLS regression were run on the left and the right of the threshold separately. Their fitted values are plotted in the graph.

analysis: The first stage results indicate indeed a strong and significant drop by 33 per cent in the probability of treatment at the threshold (Table 8) and Table 6 shows a significant reduction by 31 percentage points in diarrhea which can be attributed to the program. The estimated effect size is in line with the instrumental variables as well as fixed effects estimates further underlining the robustness of the results.

To rule out lingering doubts about the fuzzy RDD specification based on a reconstructed measure of the forcing variable, I run various robustness tests. If the identification strategy is valid and results in a local randomization around the threshold, neither the inclusion of covariates nor limiting the sample to a subsample around the threshold should change the results. The point estimates in Tables 12, 13, 10 and 11 range between -0.34 and -0.28 and are in line with the previous findings.

RDD comes with the maintained assumption of imprecise control over the forcing variable (specific investment costs) by the individuals: How trustworthy is this? Although there is no direct way of testing this assumption because we can only observe one realization of the forcing variable per individual, an intuitive indirect test is available: If the observed pre-determined baseline characteristics are balanced on either side of the the cut-off point, this is an indication that the characteristics were indeed locally randomized [Lee and Lemieux, 2009, p. 48–49]. Following Lee and Lemieux [2009, p. 49], I test whether the characteristics are balanced on either side of the threshold by graphical inspection (Figure 8) of the covariates around the threshold and also by performing a formal estimation (Table 16) whereby I replace the dependent variable in the fuzzy RDD specification by the respective covariate. The results do not show systematic evidence for unbalanced characteristics around the threshold.

5.4 Summary and interpretation of the results

In the previous chapter, the health impact of the water supply program, Fouta Djallon II, was estimated by OLS, OLS with Village and Time Fixed Effects, Instrumental Variables and fuzzy Regression Discontinuity Design. The summary of results is reported in Table 6.

The Ordinary Least Squares estimates suggest a reduction in diarrhea by 21 percentage points. Due to the modus of selection, the estimate is likely to be biased towards zero. The open program Fouta Djallon II targeted villages which previously did not dispose of an improved water source. Hence, those villages are characterized by unfavorable observable and also most likely, unfavorable unobservable properties regarding water and water-related diseases. These unobservables may simultaneously determine treatment and diarrhea. When applying OLS to estimate the program impact, an implicit comparison of villages with unfavorable unobservables and the control group is made.

Different strategies to deal with this endogeneity problem were applied in this paper. The selection into treatment pertains the village level of aggregation. Hence, in a first step OLS with village fixed effects was applied. In a second step, the distance to the next national road was used as an instrument to create exogenous variation in treatment status. Third, a Fuzzy Regression Discontinuity Design was applied. The last approach has striking advantages: It relies only on mild assumptions and is a close cousin to randomised experiments [Lee and Lemieux, 2009]. For this reason, I rely mainly on the fuzzy RDD result in interpreting the health impact of the program.

The fuzzy RDD approach shows that the program reduced children's probability to suffer from diarrhea by 31 percentage points. This result is robust when confronted with extensive robustness and specification tests and is close to the estimates from the fixed effects and instrumental variables approaches, underlining further its credibility. The treatment effect is strikingly strong and shows that Fouta Djallon II was effective in achieving the intended health improvement.

This striking result opens a new question: Where does the impact come from? Was the water supply or the hygiene component effective, or both? Due to a lack of different treatment arms, the research design does not allow to econometrically disentangle the health impact of the water supply component and the accompanying hygiene and awareness measures. To investigate this question further, I take the analysis one step back and focus on the outcome dimension of the program.

5.5 Compliance with the program: Where does the impact come from?

The successful implementation of a program and its acceptance by the targeted population is a prerequisite for envisaged impacts to materialize. If a component of the program is not used, or was unsuccessful in the outcome dimension, one can exclude the possibility that the health impact was caused by this component. Regarding program outcome, the project document [KfW, 2000, p. 5] defines the following indicators of program achievement:

1. A minimum of 90 percent of the water wells are operational two years after initiation and guarantee water quality in line with WHO standards.
2. A minimum of 70 percent of the targeted population relies on the water wells two years after initiation.
3. A mean consumption of a minimum of 10 Liters of drinking water per person and day.
4. A sufficient knowledge about hygienic behavior and water-related diseases among at least 70 percent of the population.

An ex-post evaluation KfW [2007, p. 5], which was conducted in 2007 by the independent evaluation department of the KfW Entwicklungsbank, finds achievement in all four indicators.[29] Apart from the first criterion for which no data was available, the survey data supports the evidence from the field visit.[30]

When considering the descriptive statistics on hygiene awareness and behavior, a relatively high level of knowledge can be noticed among the treatment and the control group (Table 19). The interviewed persons could name on average 1.5 of the 4 most frequent water-related diseases. Only 22 percent of the interviewed could not name any sources of diarrhea: on average 1.6 of the 4 major reasons were identified by the surveyed population. Basic awareness regarding drinking water exists among 90 percent of the population. The large majority of village inhabitants reported appropriate behavior in terms of the covering of the drinking water, storing of the cup and safely disposing of leftover water. With respect to handwashing, the cleanliness of the domestic environment, and water disinfection, the rates were considerably lower but still at somewhat acceptable levels.

The descriptive evidence from the health survey confirms the results from the evaluation mission: all program achievement indicators on the outcome dimension can be regarded as fulfilled. However, can the achievement be attributed to the program? In a next step, I estimate the program effect on outcome indicators by OLS, IV and fuzzy RDD using the previous specification.

The estimates reported in Table 21 show a positive effect on the usage of an improved water source, both in the dry and rainy seasons. The RDD estimate suggests that the increase is stronger in the rainy season. Regarding water consumption, the evidence is mixed. While the RDD results identify no significant effect for those individuals close to the threshold, the instrumental variables estimates indicate an increase in the usage of water.[31]

Inspecting the descriptive statistics, the differences between the treatment and control group in hygiene knowledge and behavior seem negligible. Except for two of eleven hygiene and aware-

[29] With respect to the first and second indicators, the evaluation mission found that 92 percent of the visited facilities were operational and more than 90 percent of the targeted population were relying on them. Regarding the third indicator, KfW [2007, p. 5] indicates an average of approximately 10 liters per day. The hygiene behavior was found to be good and thus meeting the fourth target as well.

[30] Regarding the compliance with the water supply component, I also find achievement of the outcome indicator – although at a lower rate of compliance. More than 70 percent of the targeted population residing in the project area rely on an improved water source, indicating a high rate of compliance. A cross-tabulation (Table 20) shows that only 102 households (approximately 23 percent) in the dry season and 108 households (approximately 25 percent) in the rainy season are not using an improved water source, despite living in a treatment village. Regarding the third program achievement indicator, I find a somewhat higher average comsumption of water. On average, a person in a treated village consumes 14.42 Liters (Median: 13 Liters) of water per day which is above the required minimum of ten Liters. Water consumption in untreated villages is slightly higher at 17.93 Liters (Median: 14 Liters) per day and person.

[31] There is no clear theoretical expectation with regard to the program effect on water consumption. On the one hand, an increase in the availability of clean water may lead to increased consumption. On the other hand, water consumption may remain stable if its previous available quantity was already sufficient (substitution with higher quality water) or may even decrease due to the fact that the inhabitants face a water charge which they did not have to pay before.

ness indicators[32], no significant program impact can be identified (see Table 21) considering the estimates from the fuzzy Regression Discontinouity Design[33].

To sum up, the program increased the usage of improved water sources both in the dry and the rainy season. This result is robust with respect to different estimation methods. Almost no significant effect regarding hygiene awareness and behavior of the targeted population could be identified. The accompanying sensibilisation campaign appears largely ineffective in increasing awareness and changing behavior. Hygiene awareness is considerably more elevated than appropriate hygiene behavior. Strikingly, the levels of hygiene awareness and behavior were high even prior to the intervention in both control and treatment group.

5.6 Discussion of results

The previous estimates of program impact revealed a significant and strong reduction in the prevalence of diarrhea due to the program, whereby the fuzzy RDD point estimate suggests a reduction in the probability to suffer from diarrhea by 31 percent.

Although technically the impact of the water and hygiene components of the project cannot be disentangled due to their simultaneos implementation[34], careful reasoning can provide further insights. As shown in chapter 5.5, the hygiene awareness and behavior component of the project remained ineffective in improving the target populations' hygiene awareness and behavior. If hygiene awareness and behavior did not change due to the program, it can be reasoned that the health impact was caused by the other component: the provision of water supply.

The suggested strong health impact by a communal water supply program contributes new evidence to previous findings in the water literature (see Waddington et al. [2009], Fewtrell and Colford Jr [2004] and White et al. [2008]), which did not find a significant health impact of communal water supply. How can the effectiveness of Fouta Djallon II be explained in the light of previous studies? Two explanations seem possible. First, water supply at the community level can only be effective if the water is not contaminated during the transportation to the household and is stored appropriately afterwards (see Gasana et al. [2002]). In the case of Fouta Djallon II, the population demonstrated adequate hygiene awareness and behavior prior to the program (see chapter 5.5). It is intuitively compelling that the adequate hygiene behavior was

[32] There was a slight improvement in general awareness regarding drinking water and hand washing after toilet use.

[33] The program effect on hygiene awareness and behavior was not estimated using instrumental variables because the exclusion restriction, i.e. no correlation (conditional on remoteness) of distance to the next road and hygiene awareness and behavior, may be violated. Information is potentially correlated with remoteness and it remains unclear whether controlling for the distance to the provincial capital and the capital sufficiently controls for this fact. Hence, I take a conservative stance in this paper and only rely on the RDD estimate which requires weaker assumptions.

[34] In technical terms, there is no variance betweeen the two components of treatment in the sample which can be exploited to identify the impact of the subcomponents. The impact of individual subcomponents can only be rigourously identified using factorial designs.

important for the effectiveness of Fouta Djallon II. In other instances, poor hygiene awareness and behavior may have led to contamination of the water in transport and storage, undermining the health impact of communal water supply. The second explanation is that previous impact studies of communal water supply do not adequately take into account the endogeneity problem. The two studies of communal water supply (Gasana et al. [2002] and Tonglet et al. [1992]) included in Waddington et al. [2009]'s meta analysis do not attempt to address endogeneity and selection problems. Although evidence from rigorous econometric studies is available for household connections of piped water, this paper is the first one – to the best of my knowledge – to identify a significant health impact using a rigorous identification strategy in the context of rural water supply interventions provided at the community level. As indicated by the OLS estimates in this paper, not taking into account the endogeneity problem may result in the underestimation of the program impact. As a consequence, it remains a possibility that the prevailing insignificant effect reported in the literature is due to methodological shortfalls.

Moreover, while Fewtrell and Colford Jr [2004, p. 26] find it impossible to discuss the issue of water quality versus quantity in water supply projects due to a lack of data on water consumption, the analysis of this paper can rely on primary information of water consumption to answer this question. As discussed in chapter 5.5, water consumption in the treatment group is (insignificantly) lower than in the control group. Hence, the effect of the water supply intervention can be attributed to water quality rather than quantity conditional on the fact that the minimum target of supplied quantity was met.

In a nutshell, five conclusions can be drawn from this analysis. First, water supply interventions at the community level can be effective in reducing water-related diseases if the targeted population demonstrates appropriate hygiene awareness and behavior. Second, in the case of Fouta Djallon II, the reduction in diarrhea can be attributed to quality rather than quantity of available water. Third, in settings where a high hygiene awareness and behavior is already prevalent among the population, standard awareness campaigns may prove ineffective. In those instances, the design should be altered to focuses on issues for which knowledge and behavior is at intermediate levels (e.g. hand washing or water boiling) or to omit the component from the project design. Fourth, to rigorously answer the question of the interrelation between water supply and hygiene awareness and behavior, an impact study comparing different treatment arms should be undertaken. Fifth, the long-term impact of the project remains unclear. To answer the question whether the positive impact of the program will be sustained in the long run, another round of survey data would need to be collected and analyzed.

6 Extension: Health externalities

The likelihood for an individual person to get infected with diarrheal disease depends largely on the presence of pathogens in the environment. Hence, irrespective of the actual use of an improved water source, all households may benefit from the intervention through a lowered presence of pathogens. Miguel and Kremer [2004] showed that external health effects from intestinal worm treatment can be strong and it seems quite possible that they also play an important role in the context of water supply interventions.

In this section, I apply two methodologies to detect potential health externalities. The first approach relies on Miguel and Kremer [2004]'s methodology. The basic idea is that in the presence of externalities, there should be an effect of the fraction of individuals receiving treatment within a geographic area on diarrhea while controlling for the population density of the area. The estimates in Table 24 show a reduction in diarrhea which is associated with the spillover variable in both the OLS and RDD specifications. Even though the direction of the estimates seems robust and intuitive, the magnitude of the effect should be interpreted with caution because the effect size of the OLS estimate seems large and the RDD estimate is not significant.

The second methodology [Glaeser et al., 2002, Graham and Hahn, 2005, Günther and Fink, 2010] is based on the idea that treatment effects should be stronger in effect size at aggregate levels in the presence of spillover effects. Hence, the ratio of the estimated coefficients $\frac{treatment_{agg}}{treatment_{ind}}$ gives an indication whether externalities exist. To apply this method, I aggregate the household data to the village level. Table 24 shows inconclusive results as to whether the treatment effects are stronger in effect size at the village level of aggregation. The ratio is 1.10 and 0.9 for the OLS and RDD estimates respectively.

In conclusion, while the first method indicates health externalities associated with the water supply program, the second method shows mixed results.

7 Extension: Time savings and second round effects

Access to improved water may lead to second round effects which go beyond the direct impact. "[T]he final report of the UN Millenium Project Task [Force] [...] on Water and Sanitation states that better [water supply and sanitation] will contribute to reduced income-poverty, improved health and nutrition outcomes, higher educational attainment, and greater gender equality (2005:19)"[White et al., 2008, p.12].

Those effects work through two possible channels: health benefits and time savings. Better health implies less sick days for the concerned persons and those who would otherwise have taken care of them. Similar arguments can be made for educational attainment [White et al., 2008, p.12]. Due to a lack of indicators, I cannot directly test for second round effects. However, I can test the two channels. In the previous chapters, I already established a significant and positive effect on health (i.e. reduction of rate of diarrhea). In this extension, I will analyse the program effect on time saving.

The time spent to haul water per carriage per day is lower in the treatment group in both the dry and the rainy seasons (Table 22). While a person spends on average 24 minutes per water carriage in a treated village in the dry season, the water carriage takes on average 35 minutes in an untreated village. The elevated standard deviation of 15 and 17 respectively is remarkable and indicates that the situation differs considerably between households. The difference and the standard deviation appears greater during the dry season when less water is available, although, in the rainy season a person in a treated village still takes less time to haul water than in an untreated village.

The first impression from the descriptive statistics is supported by more rigorous estimations[35] (Table 23). In the dry season, the project decreases the time spent for hauling water. While the OLS estimate indicates an estimate of -11.16 minutes per water hauling, the RDD estimate points to a time saving of -27.79 minutes per haul. The latter translates into a time saving per day of 96.87 minutes per day per household. No significant effect in the time per carriage could be identified in the rainy season. The OLS estimate for the time saving per day finds a significant effect, which is not robust. The fuzzy RDD result suggests a small but insignificant time saving in water hauling time vis-à-vis the control group. The result does not suggest that those households which rely on an improved source of water take more time to haul water. In the rainy season, close-by – although unimproved – sources of water are available to individuals in the control group. The health data shows that mainly women and girls benefit from the time savings. In 63 per cent of the households, the interviewed woman hauls the water and in 33 percent of the households the woman and the children together are responsible of water hauling. While girls are responsible for water fetching in 4 percent of the households, boys are hauling water in only 0.5 percent of the households.

In sum, this paper identified considerable time savings in the dry season, but not in the rainy season. Mainly, women and girls benefit from the time savings. Taken together, the time savings in the dry season and the positive health impact, it seems possible that second round effects of the water supply intervention on education, gender equality and income materialize. However, those effects cannot be proven on the basis of the survey data due to a lack of appropriate indicators.

[35]The same specifications regarding OLS and fuzzy RDD are applied as outlined in chapter 5.

8 Concluding remarks

This paper contributes to the fast growing literature on water by providing evidence of the health benefits of communal water supply based on a rigorous identification strategy. I show that communal water supply can be effective in reducing diarrhea when the target population shows adequate hygiene awareness and behavior. Until now, meta-studies did not identify significant health impacts of communal water supply. This may be due to the recontamination of the drinking water between the source and point of use and a lack of high quality impact evaluations in this subfield (see Waddington et al. [2009, p. 25].

To identify the effect of communal water supply, a reconstructed measure of a forcing variable is used in a fuzzy RDD set-up to create exogenous variation in treatment. This identification strategy may have great relevance for applied research and may be adapted to other settings and problems. One can think of many situations in which treatment is determined by an underlying rule but the original information on which this selection was based is no longer available. In those instances, the forcing variable can be approximated by a reconstructed measure. To rule out lingering doubts on this identification strategy, I carry out various robustness tests and cross-validate the approach through two additional identification strategies: a within-village and an instrumental variable estimator. This paper shows the practicability of this approach and may provide interesting insights for further research taking a similar route to identification.

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A Graphs and diagrams

Figure 1: Hetereogeneity in drinking water coverage. Source: JMP [2012] and Sierra Leone DHS [2008] in JMP [2012, p. 27].

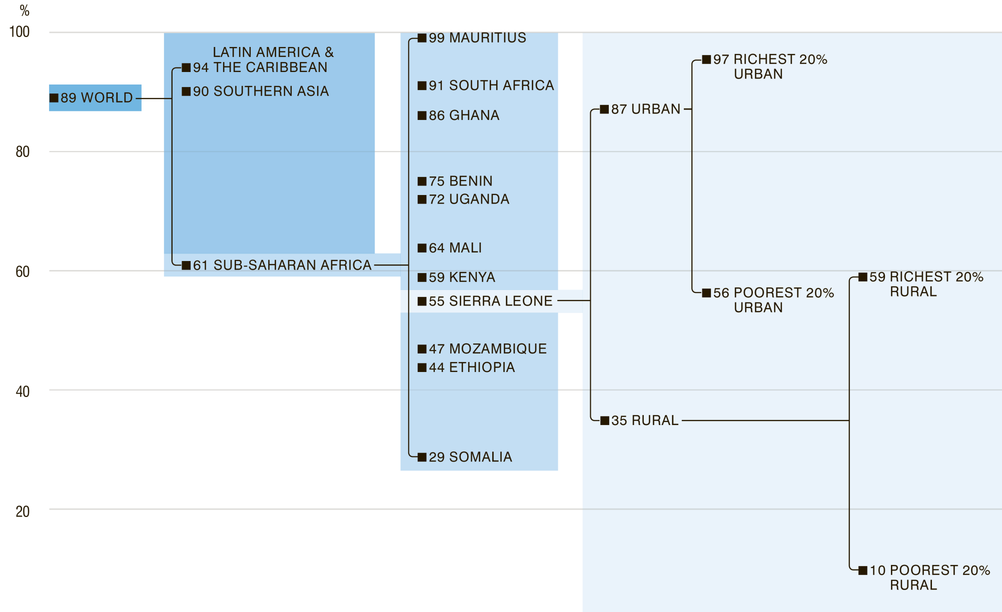


Figure 2: Transmission of faecal-oral diseases. Source: Waddington et al. [2009, p. 15] based on Prüss et al. [2002].

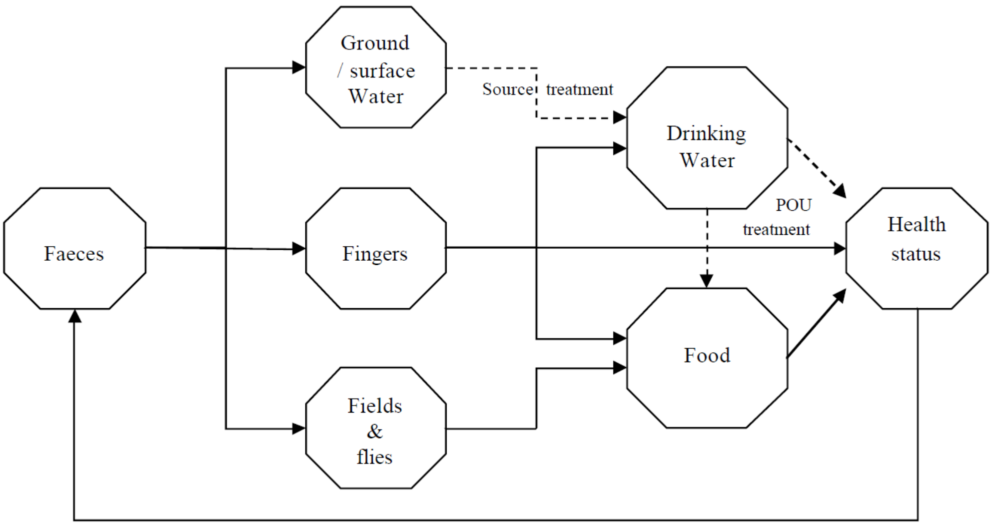
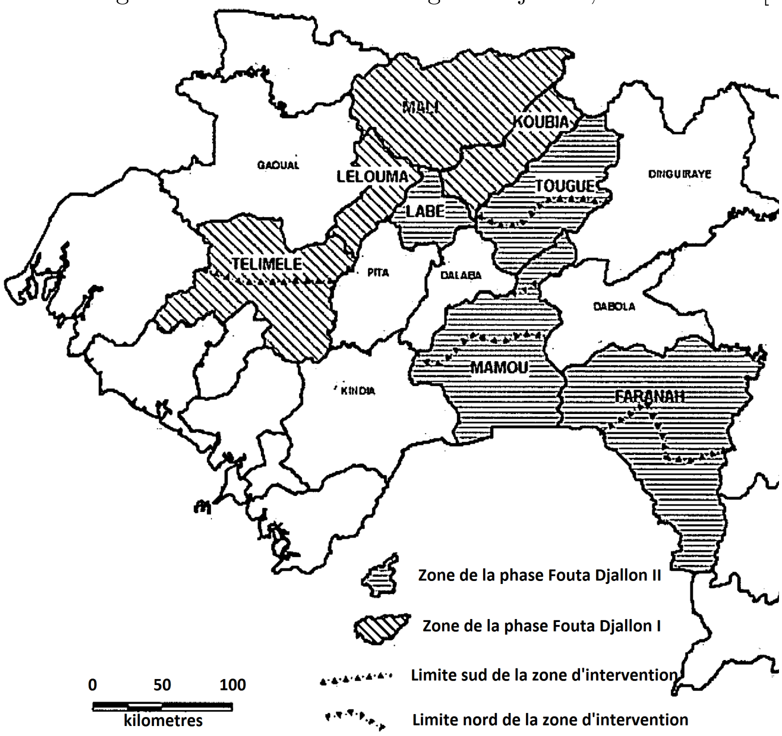


Figure 3: Program intervention area. Legend adjusted, source: KfW [2007]



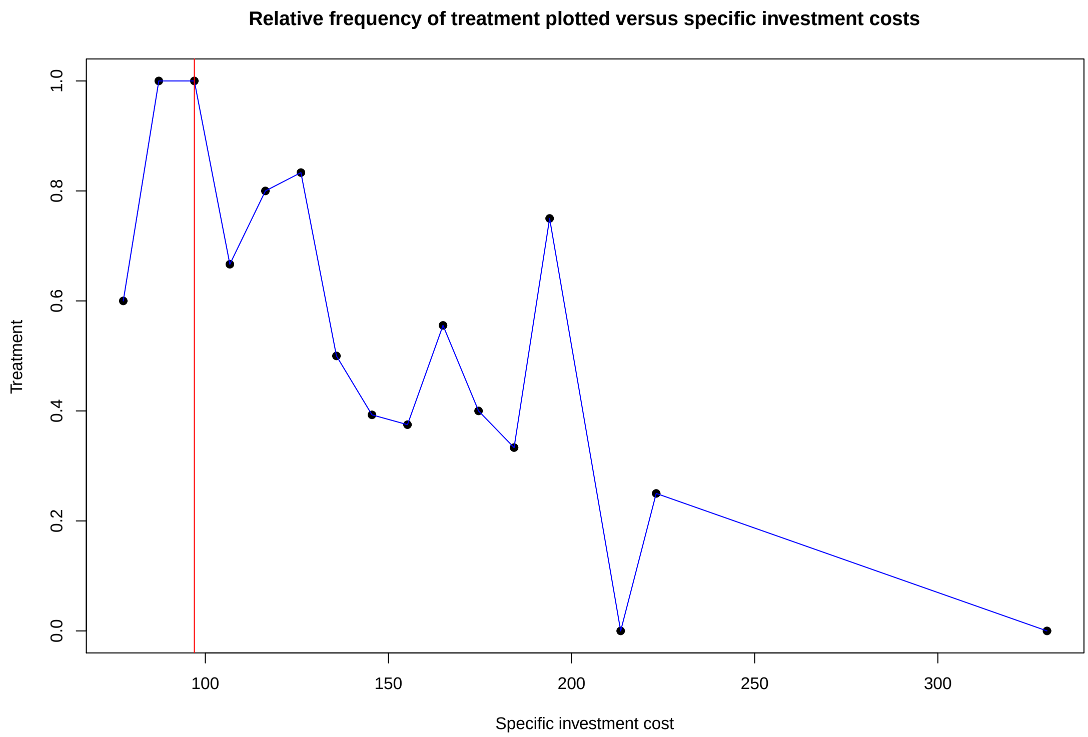


Figure 4: Administrative rule for treatment

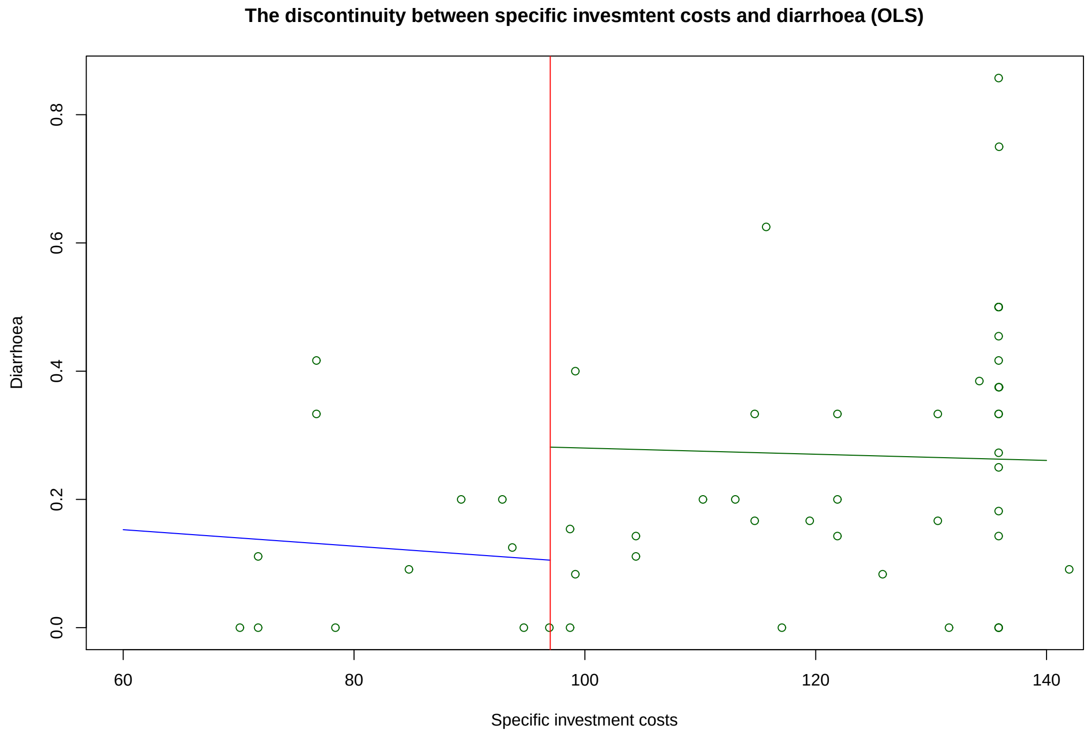


Figure 5: Graphical illustration of program impact. OLS regression of diarrhea on specific investment cost to the left and right of the threshold, data aggregated at the village level. Observations below the threshold in blue, above in black.

Figure 6: Residual plot for pooled regression

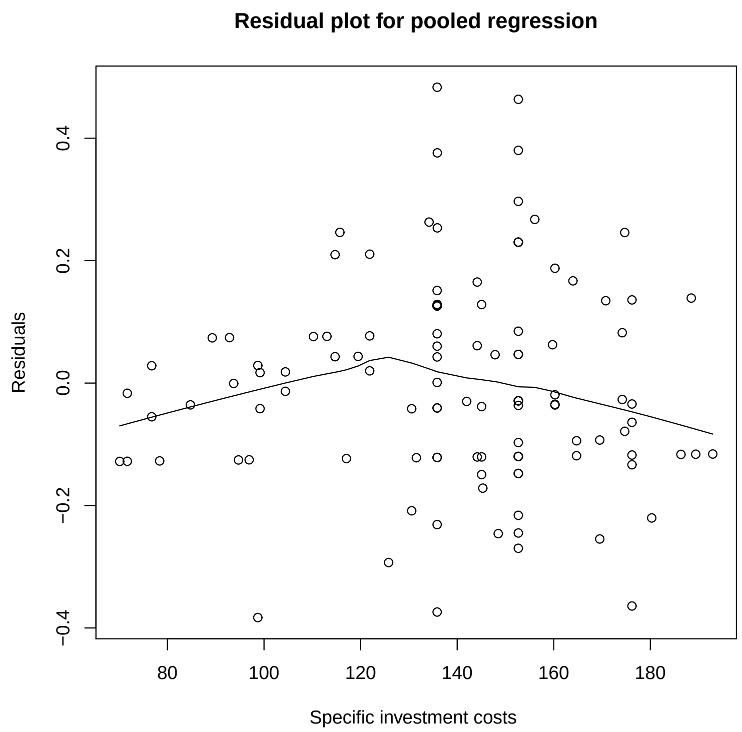


Figure 7: Testing other possible thresholds

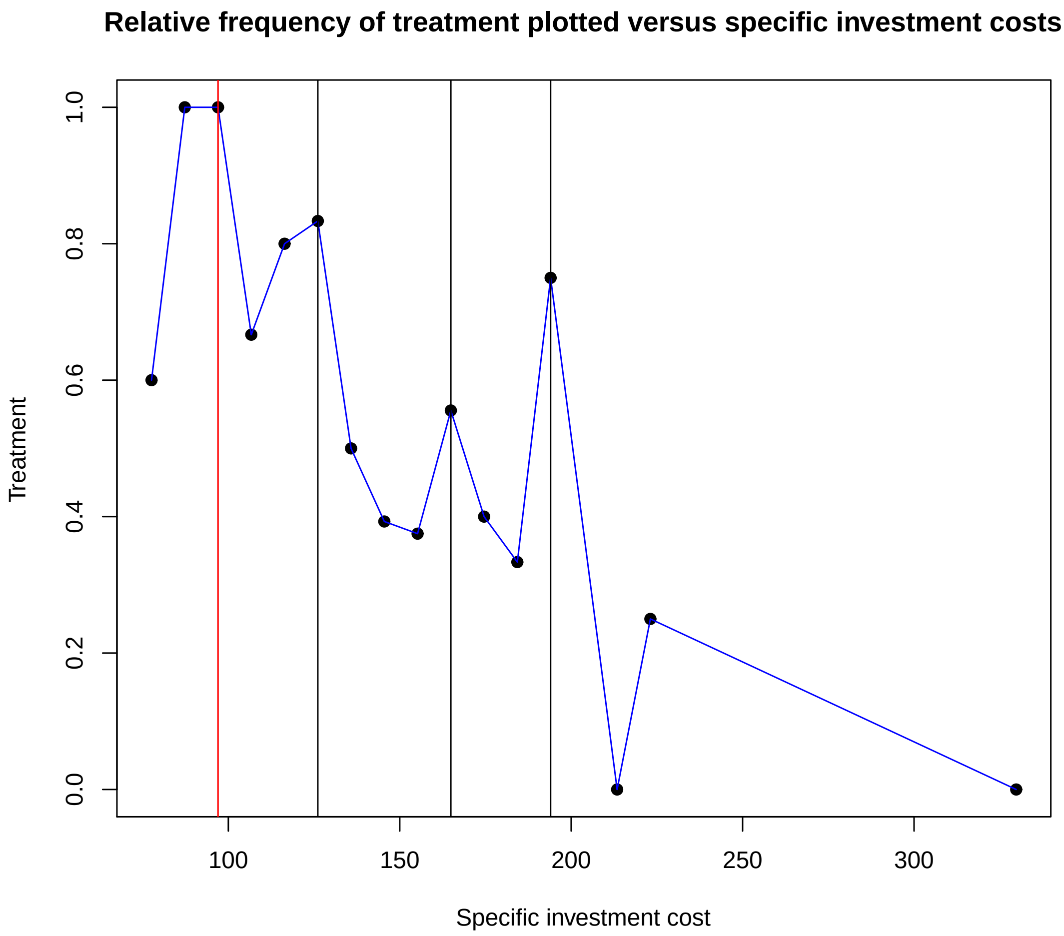
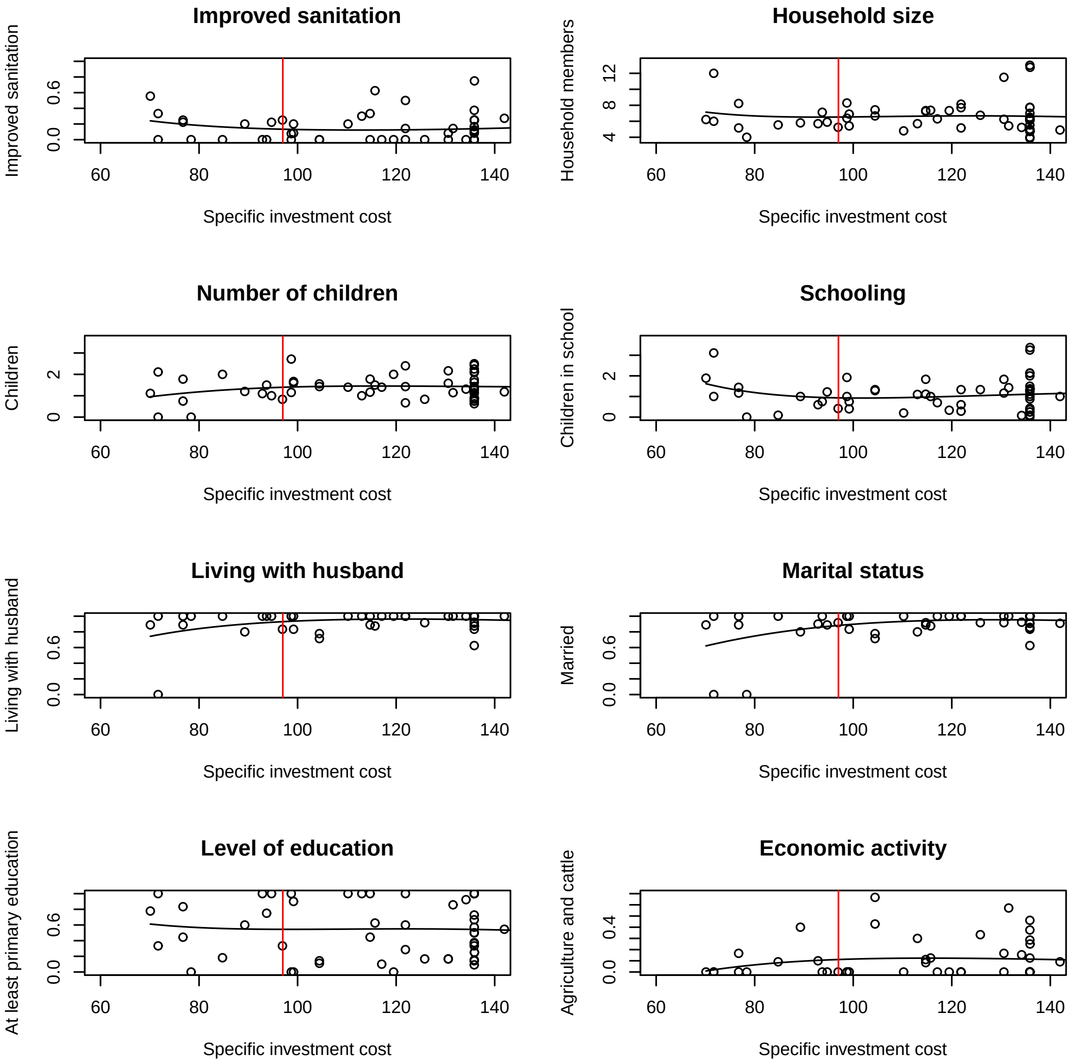


Figure 8: Inspecting the continuity of the covariates around the threshold.



B Tables

Table 1: Definition of improved / unimproved water sources and sanitation

Water supply		Sanitation	
Improved	Unimproved	Improved	Unimproved
piped water in dwelling	unprotected dug well	flush toilets	flush to elsewhere
piped water to yard/ plot	unprotected spring	piped sewer systems	pit latrine without slab
public tab or standpipe	a cart with a small tank/ drum	septic tanks	bucket
tubewell or borehole	surface water	pit latrines	hanging toilet or hanging latrine
protected dug well	tanker truck	ventilated improved pit latrines	no facilities or bush or field
protected spring		pit latrines with slab	
collected rain water		composting toilets	
bottled water*			

* if another improved water source is available for washing and other uses, bottled water qualifies as improved source of water. Source: WHO and UNICEF [2006].

Table 2: Water supply and sanitation coverage in per cent

	Water supply				Sanitation			
Year	1990	2000	2006	2010	1990	2000	2006	2010
Global								
Urban	95	96	96	96	76	77	78	79
Rural	62	72	77	81	27	38	44	46
Total	76	83	86	89	47	56	60	62
Sub-Saharan Africa								
Urban	83	82	83	83	43	43	43	43
Rural	36	42	46	49	19	21	22	23
Total	49	55	59	61	26	28	30	31
Guinea								
Urban	87	88	88	90	19	26	30	32
Rural	37	52	60	65	6	9	10	11
Total	51	63	70	74	10	14	17	18

Source: WHO/UNICEF Joint Monitoring Programme for Water Supply and Sanitation [JMP, 2012].

Table 3: Fatal cases of diarrhea among children per year and region

	Africa	Western Pacific	South-East Asia	Eastern Mediteranean	All Member States
Death from diarrheal disease	701 000	178 000	552 000	178 000	1 762 000
Fraction of total child death due to diarrhea	16%	18%	18%	17%	17%

Annual number of deaths for children under five years of age in WHO regions, estimates for 2000-2003. Source: WHO [2005].

Table 4: The eligibility criteria of Fouta Djallon II. Source: KfW [2007]

- A minimum of 200 inhabitants within a radius of 1km
- The availability of sufficient ground-water (10-20 liter per inhabitant per day)
- Population's consent to the construction of the necessary facilities of provision and disposal as well as willingness to contribute to it (pay a deposit)
- Willingness of the population to create committees for the operation and maintenance of the wells
- No other programs or assistance or operators of facilities for the provision and disposal of water present
- Specific investment costs of the water wells below EUR 100 per inhabitant (base year: 1994)

Table 5: Descriptive statistics

	Min	1st Qu.	Median	Mean	Sd	3rd Qu.	Max
Diarrhea	0	0	0	0.23	0.42	0	1
Treatment	0	0	0	0.48	0.5	1	1
Living situation*	1	3	3			3	4
Marital status*	1	2	2			2	4
Education*	1	1	3			5	6
Household size	1	4	6	6.2	3.5	7	39
Number of children	0	0	1	1.3	1.3	2	11
Children in school	0	0	1	1.1	1.4	2	16
Economic Activity*	1	3	3			6	6
Basic sanitation	0	0	0	0.16	0.37	0	1
Year	2003	2003	2005	2004	1	2005	2005
Water consumption (p.c.)	2	10	13	16	10	20	120
Investment costs (p.c.)	70.11	134.20	152.70	150.20	38.99	169.50	337.70
Distance in km							
Conakry (Capital)	200	210	220	220	21	230	270
Provincial capital	7.9	25	30	35	20	39	120
Next national road	0	2.9	6	8.5	7.1	14	25

Despriptive statistics. Obs.: 1139. *Living situation: 1 Owner, 2 Tenant, 3 Living with husband, 4 Other. Marital Status: 1 Single, 2 Married, 3 Divorced, 4 Widow. Education: 1 No schooling, 2 Primary, 3 Secondary, 4 Advanced, 5 Arabic alphabetisation, 6 Further training. Economic Activity: 1 Retired, 2 Employed, 3 Farming, 4 Cattle, 5 Both, 6 Other.

Table 6: Summary of results regarding program impact by method.

	OLS	IV	FE	RDD
Fouta Djallon II	-0.21 (0.03)	-0.29 (0.13)	-0.31 (0.07)	-0.31 (0.11)
Basic sanitation	0.01 (0.03)	0.01 (0.03)	-0.02 (0.03)	
Living situation				
Owner	reference category			
Tenant	-0.08 (0.09)	-0.07 (0.08)	-0.3 (0.17)	
Living with husband	0.01 (0.07)	0.10 (0.07)	0.07 (0.09)	
Other	0.13 (0.08)	0.12 (0.09)	0.04 (0.10)	
Marital status				
Single	reference category			
Married	0.09 (0.06)	0.08 (0.07)	0.17 (0.07)	
Divorced	-0.06 (0.06)	-0.08 (0.07)	0.01 (0.07)	
Widow	0.17 (0.08)	0.18 (0.08)	0.31 (0.08)	
Education				
No schooling	reference category			
Primary	-0.09 (0.05)	-0.1 (0.06)	-0.12 (0.10)	
Secondary	-0.07 (0.07)	-0.09 (0.08)	0.05 (0.13)	
Advanced	0.02 (0.03)	0.01 (0.03)	-0.01 (0.03)	
Further training	0.03 (0.07)	0.01 (0.08)	-0.09 (0.11)	
Household size	0.01 (0.01)	0.01 (0.01)	4e-3 (0.01)	
Number of children	0.08 (0.02)	0.08 (0.02)	0.09 (0.02)	
Children in school	-0.01 (0.01)	-0.01 (0.01)	1e-3 (0.02)	
Economic Activity				
Retired	reference category			
Employed	0.03 (0.21)	0.01 (0.24)	-0.43 (0.3)	
Farming	0 (0.17)	2e-3 (0.21)	-0.36 (0.3)	
Cattle	-0.2 (0.17)	-0.21 (0.21)	-0.65 (0.31)	
Both	-0.05 (0.17)	-0.08 (0.21)	-0.41 (0.3)	
Other	0.01 (0.17)	0.01 (0.2)	-0.36 (0.3)	
Year of survey	-0.01 (0.03)	-3e-3 (0.05)	0.01 (0.06)	
Distance to the capital		1e-3 (2e-3)		
Distance to the provincial capital		-1e-3 (1e-3)		
Specific investment cost				2e-3 (6e-4)
D × investment cost				-4e-3 (1e-3)
Intercept	-0.01 (0.19)	-0.19 (0.46)	0.49 (0.32)	0.39 (0.07)
Obs	1139	1139	875	1139

Dependent variable: Diarrhea among children until five years of age. Standard errors in parentheses Clustering applied at the village level using the Huber-White estimator.

Table 7: Instrumental variables estimates of program impact, first-stage

	Estimate	Std. Error	t value
Distance			
Next road	-0.33065	0.10325	-3.20247
Conakry (Capital)	0.01856	0.01396	1.32965
Provincial Capital	-0.02273	0.01391	-1.63441
Basic sanitation	0.03686	0.04725	0.78009
Living situation			
Tenant	0.40906	0.19852	2.06054
Living with husband	0.03678	0.10931	0.33643
Other	0.06862	0.13275	0.51689
Marital status			
Married	0.07759	0.12626	0.61449
Divorced	-0.07732	0.2012	-0.38428
Widow	0.27771	0.15117	1.83705
Education			
Primary	0.13155	0.09986	1.31741
Secondary	0.08267	0.12229	0.67604
Advanced	-0.08394	0.05995	-1.40026
Further training	-0.55899	0.07729	-7.23241
Household size	-0.00231	0.00676	-0.34112
Number of children	0.01906	0.01429	1.33367
Children in school	-0.00303	0.01668	-0.18194
Economic Activity			
Employed	-0.43983	0.22071	-1.99281
Farming	-0.36845	0.19543	-1.88538
Cattle	-0.21924	0.27695	-0.79163
Both	-0.40971	0.21258	-1.92735
Other	-0.24589	0.1962	-1.25324
Year of survey	0.09679	0.09929	0.97486
Intercept	1.22038	0.27976	4.36218

Obs: 1139. Dependent var.: Treatment. $adj.R^2 : 0.44$. Standard errors clustered at the village level using the Huber-White estimator.

Table 8: Fuzzy Regression Discontinuity Design estimates (base specification) of program impact, first stage 1

	Estimate	Std. Error	t-value
Rule	-0.33486	0.09541	-3.50975
Investment cost	0.01767	0.01439	1.22805
T x investment cost	-0.02144	0.01443	-1.48627
Intercept	1.00581	0.05964	16.86501

Obs: 1139. Dep. var. treatment. Standard errors clustered at the village level using the Huber-White estimator.

Table 9: Fuzzy Regression Discontinuity Design estimates (base specification) of program impact, first stage 2

	Estimate	Std. Error	t-value
Rule	16.73455	5.20402	3.2157
Investment cost	0.64204	0.29149	2.20261
T x investment cost	-0.55217	0.30919	-1.78583
Intercept	-0.11768	1.20816	-0.0974

Obs: 1139. Dep. var. D x investment cost. Standard errors clustered at the village level using the Huber-White estimator.

Table 10: Fuzzy Regression Discontinuity Design estimates (covariates included) of program impact, first stage 1

	Estimate	Std. Error	t value
Rule	-0.33065	0.10325	-3.20247
Investment cost	0.01856	0.01396	1.32965
T x investment cost	-0.02273	0.01391	-1.63441
Basic sanitation	0.03686	0.04725	0.78009
Living situation			
Tenant	0.40906	0.19852	2.06054
Living with husband	0.03678	0.10931	0.33643
Other	0.06862	0.13275	0.51689
Marital status			
Married	0.07759	0.12626	0.61449
Divorced	-0.07732	0.2012	-0.38428
Widow	0.27771	0.15117	1.83705
Education			
Primary	0.13155	0.09986	1.31741
Secondary	0.08267	0.12229	0.67604
Advanced	-0.08394	0.05995	-1.40026
Further training	-0.55899	0.07729	-7.23241
Household size	-0.00231	0.00676	-0.34112
Number of children	0.01906	0.01429	1.33367
Children in school	-0.00303	0.01668	-0.18194
Economic Activity			
Employed	-0.43983	0.22071	-1.99281
Farming	-0.36845	0.19543	-1.88538
Cattle	-0.21924	0.27695	-0.79163
Both	-0.40971	0.21258	-1.92735
Other	-0.24589	0.1962	-1.25324
Year of survey	0.09679	0.09929	0.97486
Intercept	1.22038	0.27976	4.36218

Obs: 1139. Dependent var.: Treatment. Standard errors clustered at the village level using the Huber-White estimator.

Table 11: Fuzzy Regression Discontinuity Design estimates of program impact (covariates included), second stage

	Estimate	Std. Error	t value
Water program	-0.28274	0.08815	-3.20764
Investment cost	-0.00108	0.00061	-1.77803
D x investment cost	0.00323	0.00156	2.06578
Basic sanitation	-0.00395	0.02924	-0.13516
Living situation			
Tenant	-0.12436	0.10815	-1.14988
Living with husband	0.11227	0.06618	1.6965
Other	0.13481	0.08592	1.56913
Marital status			
Married	0.06862	0.07111	0.96497
Divorced	-0.06425	0.06001	-1.0708
Widow	0.14263	0.09463	1.50712
Education			
Primary	-0.11182	0.05552	-2.01413
Secondary	-0.04003	0.07144	-0.56036
Advanced	0.03473	0.02467	1.40793
Further training	0.04579	0.07173	0.63837
Household size	0.00925	0.00577	1.60299
Number of children	0.08026	0.01649	4.86571
Children in school	-0.01302	0.01152	-1.13016
Economic Activity			
Employed	0.116	0.21135	0.54883
Farming	0.05899	0.17537	0.33638
Cattle	-0.13851	0.17564	-0.7886
Both	0.03021	0.18217	0.16585
Other	0.07434	0.17206	0.43209
Year of survey	-0.02652	0.03623	-0.73193
Intercept	0.02158	0.19909	-0.10539

Obs: 1139. Dep. var.: Diarrhea. Standard errors clustered at the village level using the Huber-White estimator.

Table 12: Fuzzy Regression Discontinuity Design estimates of program impact (sample restricted to 250 observations around the threshold), first stage 1

	Estimate	Std. Error	t value
Rule	-0.25152	0.18903	-1.33059
Investment cost	0.01767	0.01439	1.22805
T x investment cost	-0.0174	0.01925	-0.90391
Intercept	1.00581	0.05964	16.86501

Obs: 250. Dep. var.: Treatment.
Standard errors clustered at the village level using the Huber-White estimator.

Table 13: Fuzzy Regression Discontinuity Design estimates of program impact (sample restricted to 250 observations around the threshold), second stage

	Estimate	Std. Error	t value
Water program	-0.33576	0.22109	-1.51867
Investment cost	0.00012	0.02253	0.00541
D x investment cost	0.00175	0.03176	0.055
Intercept	0.42325	0.18512	2.28632

Obs: 250. Dep. var.: Diarrhea. Standard errors clustered at the village level using the Huber-White estimator.

Table 14: Fuzzy Regression Discontinuity Design estimates (second order polynomials) of program impact, first stage 1, dep. var. treatment

	Estimate	Std. Error	t-value
Rule	-0.4785	0.17855	-2.67989
Investment cost	0.08696	0.06257	1.38978
T x investment cost	-0.09274	0.06285	-1.47565
$Investmentcost^2$	0.00263	0.00215	1.22681
$Txinvestmentcost^2$	-0.00262	0.00214	1.22729
Intercept	1.21353	0.17105	7.09443

Table 15: Fuzzy Regression Discontinuity Design estimates (second order polynomials) of program impact, second stage, dep. var. diarrhea

	Estimate	Std. Error	t-value
Water program	-0.24478	0.26712	-0.91637
Investment cost	-0.00878	0.01521	-0.5773
D x investment cost	0.01535	0.02358	0.65089
$Investmentcost^2$	3e-05	7e-05	0.46858
$Dxinvestmentcost^2$	3e-05	0.00011	0.26403
Intercept	0.33823	0.23077	1.46564

Table 16: Detection of potentially unbalanced pre-determined characteristics on both sides of the threshold.

Covariates	Estimate	Std. Error
Improved sanitation	-0.35	0.22
Household size	-4	2.3
Number of children	-0.72	0.51
Children in school	-2.5	0.75
Living situation	0.027	0.13
Marital status	0.027	0.1
Education	0.31	0.42
Economic activity	-0.025	0.13

Obs: 1139. Fuzzy RDD applied on covariates as dep. var. Standard errors in parentheses. Clustering applied at the village level using the Huber -White estimator.

Table 17: Placebo fuzzy Regression Discontinuity Design, first stage 1

	Threshold: 126.1	Threshold: 164.9	Threshold: 194
Threshold	-0.33	-0.013	-0.12
	0.19	0.14	0.16
X	0.0019	-0.0045	-0.0039
	0.0072	0.0023	0.0018
DX	-0.0044	0.0016	0.0029
	0.0073	0.0029	0.002
Intercept	0.82	0.4	0.31
	0.18	0.077	0.1

Obs: 1139. Dep. var.: Treatment. Standard errors in parentheses. Clustering applied at the village level using the Huber-White estimator.

Table 18: Placebo fuzzy Regression Discontinuity Design, second stage

	Threshold: 126.1	Threshold: 164.9	Threshold: 194
Program	-0.14	2	-0.33
	0.23	4.1	0.42
X	-0.0016	0.0049	-0.0023
	0.00082	0.012	0.00089
DX	0.004	0.0071	0.0022
	0.0018	0.0083	0.0014
Intercept	0.32	-0.56	0.35
	0.13	1.6	0.13

Obs: 1139. Dep. var.: Diarrhea. Standard errors in parentheses. Clustering applied at the village level using the Huber-White estimator.

Table 19: Hygienic awareness and behavior in the treatment and control group

	treatment	control
Awareness		
Water related diseases	1.46	1.59
	(1.16)	(1.22)
Reasons of diarrhea	1.64	1.58
	(1.02)	(1.11)
Drinking water awareness	0.88	0.75
	(0.33)	(0.43)
Behavior		
Storage of drinking water covered	0.99	0.97
	(0.09)	(0.18)
Storage of cup	0.92	0.88
	(0.28)	(0.33)
Leftover water	0.86	0.8
	(0.35)	(0.4)
Handwashing before food preparation	0.63	0.57
	(0.48)	(0.5)
Handwashing after toilet use	0.86	0.78
	(0.34)	(0.41)
Handwashing in both situations	0.56	0.49
	(0.5)	(0.5)
Cleanliness of domestic environment	0.72	0.69
	(0.45)	(0.46)
Water boiling of drinking water	0.32	0.32
	(0.47)	(0.47)

Mean values of hygiene. Standard deviations reported in parentheses. The first two categories indicate a number count, the other values the fraction of the interviewed population with adequate knowledge or behavior.

Table 22: Mean time spent in minutes to haul water per carriage and total per day

	Treatment	Control
Dry season		
Time per carriage	23.77	34.65
	(15.17)	(17.48)
Time per day	68.19	109.92
	(55.91)	(65.36)
Rainy season		
Time per carriage	20.67	24.25
	(12.23)	(15.26)
Time per day	58.87	77.49
	(44.8)	(55.06)

Mean time to haul water. Standard deviations in parentheses.

	Water source	
	unimproved	improved
Rainy season		
untreated	572	25
treated	108	434
Dry season		
untreated	565	32
treated	102	440

Table 20: Crosstabulation of treatment and water source

Table 23: Program effect on time saving

	Season	
	Dry	Rainy
Time per carriage		
OLS	-11.16	-3.73
	(1.87)	(1.51)
RDD	-27.79	-8.68
	(4.43)	(4.56)
Time per day		
OLS	-40.57	-17.67
	(6.76)	(5.38)
RDD	-96.87	-41.02
	(24.93)	(22.46)

Obs. 1139. Dep.var.: Time to haul water. Standard errors in parentheses.

Table 24: Health externalities

Spillover effects between villlages		
	OLS	RDD
Program	-0.21	-0.37
	(0.04)	(0.18)
Population density (3km)	-1e-05	-3.5e-06
	(1.3e-05)	(1.9e-05)
Spillover (3 km)	-0.28	-0.59
	(0.21)	(0.37)
Health impacts at aggregate village level		
	OLS	RDD
	-0.23	-0.28
	(0.03)	(0.28)
Spillover (Ratio of village level and household level estimates)	1.10	0.90

Control variables omitted. Obs (between village spillovers): 868. Obs (aggregate village spillovers): 126 .

Table 21: Program effect on usage of improved water source and hygiene awareness and behavior

	OLS	IV	RDD
Water component			
Improved water source in dry season	0.77	0.9	1.24
	(0.05)	(0.27)	(0.18)
Improved water source in rainy season	0.77	0.82	1.16
	(0.05)	(0.27)	(0.2)
Water usage	-2.43	9.58	-0.67
	(0.88)	(3.24)	(6.33)
Awareness and behavior			
Awareness of water related diseases	-0.06		1.16
	(0.18)		(1.52)
Knowledge of the reasons of diarrhea	0.08		0.01
	(0.15)		(0.94)
Awareness regarding drinking water	0.15		0.32
	(0.03)		(0.13)
Storage of drinking water covered	0.03		0.07
	(0.01)		(0.05)
Storage of cup	0.04		0.06
	(0.02)		(0.13)
Reusage of leftover water	0.05		0.07
	(0.03)		(0.15)
Handwashing before food preparation and after toilet	0.08		0.55
	(0.06)		(0.47)
Handwashing before food preparation	0.08		0.39
	(0.06)		(0.43)
Handwashing after toilet	0.08		0.34
	(0.04)		(0.15)
Cleanliness of domestic environment	0.03		-0.11
	(0.05)		(0.19)
Water boiling of drinking water	-0.01		-0.76
	(0.06)		(0.42)

Impact of the program on the reported dependent variables. Controls used as in previous specification. Standard errors in parentheses. Obs. 1139.