Living with Disability:
Evidence from a Medical Intervention in Uganda

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Abstract

This study evaluates the effect of a medical intervention that provided orthotic equipment to individuals with lower-limbs disability in Kampala, Uganda. The assignment to treatment rule implied by the intervention causes a discontinuity in the probability of treatment according to the day subjects turned up. We exploit this feature to estimate the causal effect of the intervention on a number of market and non-market outcomes. Based on a regression discontinuity design approach, we find evidence that the intervention only had marginal effects on labour market outcomes. One year after the intervention, treated women are 25 percent more likely to be part of the labour force than their untreated counterpart. We do find, however, important effects of the intervention on the time subjects allocate to various non-market activities. The results show that, on average, treated women increased the amount of time spent in the household chores by roughly 2.4 hours. Moreover, children of treated women increased the amount of time spent in school-related activities by more than 6 hours per week. These findings suggest that the provision of orthotic equipment may generate benefits for the whole household in low-income countries.

Keywords: disability, medical intervention, regression discontinuity design.

JEL Classification: D13; I12; I15; J71; J13; J24.

1. Introduction

Only recently the literature of developing countries has focused on a hidden part of the population that has been too often ignored: the disabled. The UN (2006) reports there are at least 60 million people with disability in Africa. For the specific case of Uganda, the Census (2006) one in six of the whole population being disabled. The interest in this group of the population stems from the fact that disability is closely linked to poverty. Not only are individuals with disability the poorest of the poor, but they are also often excluded from schools, political institutions and labour market, which translates into their marginalisation from society.

This study evaluates the effect of a medical intervention that provided orthotic equipment to individuals with lower-limbs disability in Kampala, Uganda. The intervention took place in Mulago hospital, Kampala, and lasted for 15 days. During the first 10 days of the intervention, together with a team of medical doctors and orthosists, we treated the majority of subjects that came to visit us in Mulago hospital. In the final 5 days of the intervention none of the subjects was treated. These final days were dedicated to the collection of detailed information on (treatable) subjects who would then provide a control group for our analysis. Study participants were interviewed and tested before and after the intervention for the evaluation of treatment impacts.

Given the non-random assignment of the treatment, a naïve comparison between treated and untreated is unlikely to provide reliable estimates of intervention impacts. For example, subjects who turned up in the first 10 days of the intervention may be better connected and with larger social networks than those who turned up in the final days. This would imply an upward bias in the estimated treatment effect. Vice-versa, we could also report a downward bias if, for example, subjects who turned up at the final days of the intervention were already in more advantageous positions and did not need additional support.

In this paper, we use a regression discontinuity design (RDD) to cope with this issue and estimate the causal effect of the intervention on a number of market and non-market outcomes. Specifically, we exploit the discontinuity in the probability of being treated offered by the day of intervention subjects turned up in Mulago hospital. For subjects who turned up after day 10, the probability of receiving the treatment jumped from 40 percent to zero. The key identifying assumption of this design is that individuals who turned up relatively before and relatively after day 10 of the intervention, the discontinuity point, are comparable both in observables and unobservables.

When estimating treatment effects, we are interested in three types of outcomes that may be affected by the intervention. Firstly, we are interested in whether the intervention improved the subjects' medical condition. For this, we tested patients on a series of medical evaluations to capture their level of body mobility, their walking speed and their cardiovascular fitness level. Secondly, we collected extensive information on a number of information, such as employment status, labour force participation and monthly income, to estimate whether the intervention had an effect on labour market outcomes. Lastly, given the chronic nature of disability, patients are likely to require assistance from their family members, e.g. from their spouses and children. An improvement in the subjects' medical condition, therefore, is likely benefit the whole household by freeing up time and efforts of other family members. In order to capture these potential benefits, we collected a detailed time allocation survey for the patients and all the household members.

We find evidence of improved medical conditions. One year after the intervention, men reported an improvement in their level of body mobility and walking speed. The improved medical conditions, and their productivity, did not lead however to gains in labour market outcomes. Women, on the other hand, showed an improvement in their fitness level. The results suggest that, one year after the intervention, treated women are 25 percent more likely to be part of the labour force than their untreated counterpart. Moreover, we find evidence that, on average, treated women increased the amount of time spent in the household chores by roughly 2.4 hours. Finally, children of treated women increased the amount of time spent in school-related activities by more than 6 hours per week. This finding is in line with the literature that mothers tend to invest more in children's human capital (see Hill and King, 1995). Overall, our results suggest that the provision of orthotic equipment to PWD in low-income countries, may generate both direct benefits for subjects treated and also indirect benefits for the whole household.

The remainder of the paper is structured as follows. Section 2 defines the concept of disability and presents the relevant literature. In section 3, the design of the intervention and the measurements are described. Section 4 explains the identification strategy used in the paper. In section 5, we present the summary statistics. Section 6 reports the intervention impacts and section 7 concludes.

2. Conceptual Framework

Disability is complex and controversial concept to define. We can identify two main models of disability: the medical and the social model. Not only are these two models different in the way disability is defined, but also they contrast in the way disability should be cured.

In the medical model, disability is defined as the result of a person-specific health condition which causes a disadvantage to the individual's quality of life. According to this model, curing or managing a certain type of disability entails identifying, understating and curing disability in a purely clinical manner. Hence, this model believes in medically curing persons with disability, improving their functioning and extending their functionality so that individuals can live a life according to the normally accepted population standards.

On the other hand, the social model holds that disability is created by the society. According to this model, individuals are disabled because of the barriers and negative attitudes that are inherent in the society. Person with disabilities are then individuals who are excluded from the society because of this barriers. It follows, that for the social model it is the society that should drop these barriers and allow for all individuals to be included. In 2001, the Word Health Organisation (WHO) defines disability as an umbrella term that combines medical and social models: limits in functioning, activities and participation.

The literature of developing countries, so far, has focussed on studying the relationship between disability and poverty. Filmer (2008) uses data from 8 developing countries and one country in transition (Romania) to analyse the relationship between disability and poverty. He considers the income distribution of these countries and measures the proportion of disabled among different quintiles of the income distribution. He finds evidence that the poorest quintile has a higher proportion of disabled than the richest quintile. Moreover, the paper shows that while the relationship between disability and poverty is not always positive and significant, the relationship between disability and school participation is strongly significant and, in magnitude, much powerful than other demographics, such as age, gender and location. This study, however, does not consider that, for the case of low-income countries, not always income data are reliable.

By combining Demographic Health Survey (DHS) and Census data in Uganda, Hoogeven (2005) performs a similar comparison using data on per capita household consumption data. He finds evidence that per capita consumption in disabled headed household is much lower than the non-disabled counterpart. Specifically, the proportion of households whose consumption per capita falls below the poverty line is 43 percent when the head is disabled headed vs 27 percent for non-disabled headed.

The literature presented above presents two main issues. Firstly, the above studies suffer from a reverse-causality problem: does disability causes poverty or, vice-versa, poverty cause disability? These studies only can tell about the overall correlation between the two variables, but cannot offer any information with respect to cause-effect. In this paper, we deal with this problem by means of a regression discontinuity design. A second potential problem of the above literature is that it relies on self-reported information on health status. We cope with this issue by having a team of doctors from the UK who assessed the individuals in the sample.

3. Intervention Design and Data

a. The Uganda Disability Project

During the spring of 2011, the Uganda Disability Project (UDP) was established by one of the author of the paper[1] and staff members from Nottingham Hospitals University (NHU), with the target of taking used and unwanted orthotic equipment from the UK to treat people with lower-limbs disability in Kampala, Uganda. Uganda is chosen due to its commitment to helping persons with disability and a strong link with Makarere University. The realisation of the project was planned in two steps. Firstly, used and unwanted equipment would be collected from various hospitals and National Health Service (NHS) trusts in the UK. In the final step, the collected equipment would be shipped in Kampala, Uganda, and then allocated to people with lower-limbs disability. A fundamental feature of the project is that a team of UK-trained orthosists and medical doctors was envisaged to go to Kampala in order to oversee the allocation and the fitting of the equipment.

Collection and sorting of the equipment occurred in the fall of the year 2011. In the spring of 2012, the equipment was shipped and delivered to the Mulago National Referral Hospital in Kampala, the largest hospital in Uganda. The intervention took place in June 2012 in the Mulago hospitals. In the first stage of the intervention, patients were *triaged* by the team of medical doctors and eligible subjects for the intervention were selected. Eligibility was determined according to the underlying patient's medical condition and on the availability of the equipment the patient required. A total number of 346 subjects were selected to be part of

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the intervention. In the second and final stage, subjects were attended by the team of orthosists and were fitted with the equipment.

The intervention ran for a total of 15 days. During the first 10 days of the intervention all the subjects who turned up in Mulago hospital were to receive treatment. Depending on the underlying medical condition of the subjects and the equipment required, however, the fitting could potentially take a full day. For this reason, not all individuals who turned up during the first 10 days of the intervention received the treatment. From day 11 to 15, instead, none of the subjects who turned up in Mulago hospital was allocated with the equipment. During these final days, information were collected for a sample of treatable subjects (patients that would have been treated had they turned up during day 1 to 10) that provide a control group in our analysis.

The sample was recruited in two distinct ways. First, we recruited a sample of subjects with lower-limbs disability through two NGOs based in Kampala: the National Union of Disabled Persons of Uganda (NUDIPU) and the Uganda National Action on Physical Disability (UNAPD). Only a small number of subjects recruited from the NGOs, however, showed up. To increase sample size, we adopted snowball sampling and included in the intervention any treatable individual with lower-limbs disability who would turn up in Mulago Hospital. In table A.1, we compare baseline characteristics between subjects recruited and non-recruited through the NGOs and show that the two groups only appear to differ marginally.

Study participants are very heterogeneous: they differ in their underlying medical condition and, consequently, in the equipment required. In other words, the treatment is not common to all subjects, but it varies according to the specific needs of each patient. In particular, subjects could have received a combination of the following 5 types of equipment:

(i). Knee-Ankle-Foot Orthosis (KAFO): This is a long-leg orthosis that spans from the knee to the ankle of the patient in order to stabilise the joints and assist the leg's muscles.

(ii). Ankle-Foot Orthosis (AFO): It intends to control the position and motion of the ankle, compensate weakness, or correct deformities such as foot drop.

(iii). Knee Brace: it is a brace which extends above and below the knee joint to support and align the knee.

(iv). Crutches and Leg Stick: They aid mobility of the patients by transferring the body weight from the legs to the upper body.

(v). Shoe Raise: It is footwear that raises the foot of the wearer and compensate for leg length discrepancy.

In this study, we define the treatment as complete medical care offered to the patients. This implies that treatment effects should be interpreted as the overall effects of the intervention package rather than the effect of a specific type of treatment.

In order to comply with ethical requirements, the treatment was phased-in: subjects in the control group received medical assistance and were fitted with equipment required in June 2013. Between June 2012 and 2013 we report a sample attrition of 23 percent. In table A.2, we show attrition was balanced between treatment and control group members. In what follows, we focus on the 233 subjects for which we have pre and post intervention data.

b. Data and Measurements

The questionnaire was piloted a week prior to the intervention in Kampala. Enumerators, and their supervisors, received a 2-day training workshop on the questionnaire. Interviews were carried out face-to-face and conducted in local language, Luganda. The questionnaire consists of three main parts: (a) a medical survey; (2) an individual level survey and (3) a time allocation survey.

The medical survey was administered to the patients at baseline and follow-up, for both treatment and control groups. This survey contains detailed information on the medical condition of the subjects, a score for their muscles strength, and the results of three medical tests which capture the patient's level of body mobility. The medical tests performed are:

(1) Time Up and Go (TUG) test: In this test, subjects start from sitting in a standard chair, stand up and walk three meters, then turn and walk back to the chair and sit down. An explanation of the test is shown in figure A.1. This test assesses the level of patients' mobility, as it requires both static and dynamic balance. A score smaller or equal than 10 seconds identifies an average individual. A score that lies between 11 and 20 seconds, instead, identifies elderly and disabled patients. Finally, a score greater than 20 seconds implies the subject may be prone to falls and needs assistance.

(2) Ten Meters Walk (TMW) test: The test requires subjects to walk ten meters at their fastest speed. As figure A.2 shows, the total length walked by the patients is more than 10 meters to allow for an acceleration and deceleration, but time is only measured for the intermediate 10 meters. The aim of this test is assess the level of walking speed of the patients.

(3) Resting Heart Rate: This is the number of heart beats per minute (BPM) measured when the body is not engaged in any physical activity. This measure captures the fitness and health level of the patients. A normal resting heart rate can vary as low as 40 BPM to as high as 100 BPM. 70 BPM is usually the average for men, and 75 BPM is average for women. A smaller number of BPM implies stronger lungs, stronger heart and, therefore, a better fitness level.

To avoid potential measurement errors, medical tests were collected for two trials and the best score was taken into consideration. Due to differences in the experimental conditions, follow up measures for the medical tests of the control group cannot be used for the analysis.

Further, an individual-level survey was collected at baseline and follow up, including information on demographic and socioeconomic characteristics of the patients. Given that the intervention is supposed to improve the medical condition of the subjects and, consequently, their productivity, we also collected information on employment, labour force participation and monthly income, to estimate whether we observe effects of the intervention on labour market outcomes.

Lastly, a time allocation survey was collected for all household members. In the survey, subjects were asked to report the amount of hours they, and their household members, spent in a set of market and non-market activities the week prior to the intervention. Data were collected at baseline and follow up. The set of activities are then gathered in the following three main groups: (a) income-generating activities: working in for wage, working for own business and working in the family land; (b) household chores: cleaning, cooking, washing and ironing and water and wood collection; (c) for the case of children, school-related activities: school participation and homework.

4. Identification Strategy

In this section, we describe the basic framework of potential outcomes presented by Rubin (1974), and present the econometric strategy we rely on when estimating intervention effects. Suppose there are two states of the world, a treated and an untreated state. Let D_i be an indicator function that equals one for subjects who received the treatment and zero otherwise. Each state is associated with a potential outcome. Let the pair (Y_i^1, Y_i^0) denote the outcomes individual i would obtain in the treated and untreated state, respectively. The observed outcome, Y_i, can then be written as follows:

$$Y_i = Y_i^0 + (Y_i^1 - Y_i^0)D_i, \tag{4.1}$$

where $Y_i^1 - Y_i^0$, the difference between the two potential outcomes, is the causal effect of the treatment. This amount corresponds to the difference between the outcome that the subject obtained in case of treatment and the counterfactual outcome in case of no treatment. Because any individual can be at most in one of the two potential states, we do not observe the above difference, and therefore cannot observe the treatment effect for any individual. This is known in the literature as the problem of causal inference, and can be seen as a missing data problem (Heckman, 1997), i.e. we do not observe what would have happened to treated units in the absence of treatment. In what follows, we describe the strategies adopted to address this problem.

a. Before-After Estimator

As mention in section 3, due to differences in the experimental conditions, we can only rely on pre and post medical tests measures for the treatment group. This constraint naturally leads to the before-after estimator, which simply consists in a comparison of the medical outcomes of treated individuals before and after the intervention occurred. The before-after estimator solves the missing data problem explained above by using pre-intervention data of treated units as counterfactuals for what would have happened in the absence of treatment. Assuming common treatment effects, the before-after estimator in a regression framework is the least-squares solution to the following problem:

$$\Delta Y_i = Y_{it} - Y_{it'} = \mu(X_{it}) + \beta + \Delta u_i, \tag{4.2}$$

where the subscript t and t' idenfy time after and before the intervention, respectively. The term X_{it} is a vector of pre-treatment variables that may or may not vary across time, but are not affected by the treatment. In our case, this vector includes a set of dummy variables that identifies patients' medical condition and the lower-limbs muscles strength score. The coefficient β is the average effect of the treatment on the treated. Finally, the term $\Delta u_i = u_{it} - u_{it'}$ is a random disturbance.

We can identify two main limitations of the before-after estimator which are known in the literature as history and maturation (Shadish, Cook and Cambell, 2001). The former refers to the fact that events other than the treatment might have happened between baseline and follow up, and might have affected the observe differences between outcomes before and after the intervention. The latter, instead, refers to the fact that the sample under consideration became older. This implies that observed differences between baseline and follow up data may be affected by the aging of the sample. Given that aging and maturation are likely to have a negative effect on the medical tests we performed, we believe the estimated coefficients we present in this study to be, at the very least, a lower bound of the true population parameters.

b. Difference-in-Differences Estimator

In case of non-random assignment, the difference-in-differences (DiD) estimator is now widely used in applied research to estimate program impacts. This estimator measures the effect of the intervention by comparing before and after differences in outcomes, between treated and untreated. In a regression framework, the DiD estimator is the least-square solution to the following problem:

$$\Delta Y_i = \mu(X_{it}) + \beta D_i + \Delta u_i \tag{4.3}$$

A consistent estimation of β requires $E[\Delta u_i|D_i, X_{it}] = 0$. This assumption entails that time-varying unobservables that affect the outcome of interest should not be correlated with the assignment to treatment mechanisms. In our case, this assumption is unlikely to be satisfied. For example, subjects who turned up in the first 10 days of the intervention may be better connected and with larger social networks than those who turned up in the final days. This would imply an upward bias in the estimated treatment effect. Vice-versa, we could also report a downward bias if, for example, subjects who turned up at the final days of the intervention were already in more advantageous positions and did not need additional support. For all the reasons explained above, the DiD estimator is not ideal for our purposes.

c. Regression Discontinuity Design

In this study, we exploit our complete knowledge of the assignment to treatment mechanism implied by the intervention and implement a regression discontinuity design (RDD) to estimate the causal effect of the intervention on a number of market and non-market outcomes. The defying feature of this class of models is that the probability of treatment changes discontinuously as a function of an assignment variable, which we denote Z_i, being above or below a certain cut-off point, denoted z_0. They underlying idea of the

RDD is that individuals just above and below the pre-identified cut-off point are counterfactuals.[2] There are two types of RD design: the sharp and the fuzzy design. In the sharp design, treatment status, D_i, depends deterministically on the assignment variable, Z_i, being above or below the cut-off z_0. Contrarily, in the fuzzy design the probability of treatment it is not a deterministic function of Z_i, but it is known to be discontinuous in the cut-off point z_0.

In our case, given that the probability of treatment is a discontinuous function on the day of the intervention subjects turned up, it is more appropriate to use the fuzzy type of design.[3] Formally, let $Z_i \in \{1, 2, 3, .., 14, 15\}$ be a discrete variable which identifies in which day of the intervention subjects turned up. Let the cut-off point of interest be $z_0 = 10$, since subjects who turned up in the first 10 days were likely to receive treatment and those who turned up after day 10 did not receive treatment. Then, we can write:

$$Pr(D_i = 1|X_{it}) = \begin{cases} f_1(Z_i), & if \;\; Z_i < z_0 \\ f_0(Z_i), & if \;\; Z_i \geq z_0 \end{cases}; \tag{5.1}$$

where, thanks to the cut-off point, $f_1(Z_i) \neq f_0(Z_i)$. Figure 1 shows the proportion of subjects treated as a function of the day of the intervention they turned up. The figure shows an important discontinuity in the probability of being treated at day 10.

[Figure 1]

Figure 1, moreover, is very useful in that it shows that none of the subjects who turned up in after day 10 received treatment. This feature of the intervention enables us to implement a special case of the fuzzy design that Battistin and Rettore (2001) call *partially* fuzzy. This particular type of design is advantageous because, even though we are in the realm of a fuzzy design, we can identify and estimate the average treatment effect on the treated under a minimal set of assumptions, as in a sharp design. The main intuition behind this result is that all individuals who receive the treatment are compliers, as in an experimental framework (see Bloom, 1984).

[2] In table A.3, in the appendix, we report the results of an informal test for this assumption and compare baseline characteristics for subjects just above and below the cut-off point.

[3] We would have used the sharp design if all subjects who turned up during days 1 to 10 of the intervention had received the treatment.

In the spirit of Hahn et al. (2001), as Z_i affects the probability of being treated, a natural way to implement a RDD is by means of an instrumental variable framework. Define the indicator function $I_i = 1(Z_i < z_0)$, which takes value of unity for individuals who turned up days 1 to 10 of the intervention. The equation we estimate is then:

$$\Delta Y_i = g(Z_i) + \beta D_i + \Delta u_i, \tag{5.2}$$

where D_i is instrumented by I_i. The function $g(.)$ is a polynomial in the days of the week of the intervention. Notice, in our case we prefer not to impose a functional form to the day of the arrival of the subjects and we include, instead, a dummy variable for each day. The identifying assumption for this strategy is that unobservable variables of subjects just above and just below the point of the cut-off vary continuously. A limitation of the RDD approach is that treatment effects are identified locally, that is, in a neighbourhood of the cut-off.

5. Descriptive Statistics

a. Baseline Characteristics

In table 1, we compare baseline characteristics of treatment and control groups in columns (1) and (2), respectively, and in column (3) we report the p-value of a two sided test which compares the means of the two groups. Even in the absence of randomisation, the table suggests that treatment and control groups are very similar in terms of baseline observable characteristics.

[Insert Table 1]

With respect to the medical tests, at the baseline subjects both in treatment and control groups performed the TUG test in around 12 seconds. This is in line with existing studies in the medical literature, which suggest a score between 11 and 20 seconds for elderly and disabled patients. Likewise, the TMW test ranges between 12-13 seconds with no statistically significant differences between treated and untreated. The heart rate of study participants ranges between 75 and 76 beats per minute. Further, the table shows no evidence of statistical difference between treated and untreated in the proportion of individuals with polio, the age of onset of the underlying medical condition and the lower-body muscles strength score.

If we consider the demographics, we see no significant differences between treatment and control groups in the proportion of females in the sample. The average age of the sample is above 40 years. About 40 percent of the sample reports to be married and, on average, the household size is around 5 members per household. This large number highlights that persons with disability need to stay in big households and need the assistance of their members.

When we consider the background variables, again, we notice treatment and control groups to be very similar. On average, subjects in the sample completed over 8 years of formal education which, according to the current educational system in Uganda, corresponds to the beginning of secondary school. Around 80 percent of the baseline sample has lived with their natural parents. This is a relative large number, considering that members with disability tend to be sent away. Further, we do not observe statistical differences between the levels of parental education of treated and untreated. In the questionnaire, we also included cognitive tests score, the Raven and the Wechsler tests, and we observe marginal differences between the two groups.

Lastly, we compare treatment and control groups according to pre-intervention labour market outcomes. Unexpectedly, more than two thirds of the sample is employed at the time of the baseline survey. The table shows that individuals in the control group were more likely to be have a job and to be self-employed than the treated subjects. On average, members of the treatment group earn 260 thousand Ugandan shilling (the equivalent of almost 10 US dollars). Through a detailed collection of the employment history we computed the total number of work experience, which ranges from 14 to 17 years for treatment and control groups, respectively.

b. Take up

Orthosists were instructed to fit subjects with the equipment required during day 1 to 10 of the intervention. Given the nature of the project, aim was to provide the subjects with long lasting treatment that would not need further revision for at least one year. Figure 2 shows that after the intervention not all treated patients used the equipment regularly. The great majority, 70 percent, used the new fitted equipment at least once a week. An important part of the treated patients, over 20 percent, never used the treatment, and this was largely due to pain caused by the treatment and because, at some point, the treatment broke. For this reason, we prefer to interpret the estimated impacts of the intervention that follow in the next section as Intention to Treat (ITT) effects, rather than treatment effects on the treated.

6. Program Impacts

a. Before-after Estimates

Table 2 report before-after estimates of the effect of the intervention on the medical tests of the patients. Each entry in the table is entry in the table represent a coefficient estimate of a separate regression. In column (1) we report estimated coefficients for the full-sample and in columns (2) and (3) we distinguish between men and women.

[Insert Table 2]

When we consider the full-sample we see a negative effect of the intervention in the TUG test. One year after the intervention, treated subjects performed the TUG test 3.2 seconds faster. This implies that, on average, the new orthopaedic equipment did increase the level of mobility of the subjects. When we distinguish between men and women, we observe that this improvement is driven by the sample of men: one year after the intervention, treated men improved their score in the TUG test by more than 5 seconds, on average. The estimated coefficients imply no intervention effects on the level of body mobility of women. A similar patter can be found when we take into consideration the TMW test: one year after the intervention treated men performed the TMW test more than 4 seconds faster. Again, we find no evidence of intervention impacts for the sample of women. Lastly, we do find evidence of intervention impacts on the level of cardiovascular fitness for the sample of women. The estimated coefficient implies that one year after the intervention the heart rate at rest of treated women decreased by almost 8 beats per minute.

To sum up, we do find evidence of positive program impacts on medical outcomes. We find evidence of increased body mobility and waking speed for the sample of men, while we find evidence of improved cardiovascular fitness for women. As we mentioned in section 4, it is worth noticing that, because individuals got older, the presented before-after estimates should be interpreted as a lower bound of the true effect of the intervention.

b. Difference-in-Differences Estimates

While the Difference-in-Differences (DiD) estimator is not ideal for assessing the impact of our medical intervention, in this subsection we briefly report DiD estimates on the effect of the intervention as a benchmark. Table 3 shows DiD estimates of the impact of the intervention on labour market outcomes. The table reports that the intervention had no significant effect on employment status of the individuals. It does show, however, that one year after treatment, treated subjects were 10 percent more likely to be in the labour force than their untreated counterpart. This result is driven by women. Treated women, on average, are 17 percent more likely to be in the labour force. Further, the table reports no statistically significant effect of the intervention neither on the amount of hours worked per week nor on the monthly income of the subjects.

[Insert Table 3]

In table 4, we report DiD estimates of the intervention impacts on time allocation. In particular we focus our attention on the effect the intervention had on the time allocation of (1) patients (Panel A), (2) their respective spouses (Panel B), and (3) their children (Panel C). Panel A suggests that treated subjects spend, on average, an additional 1.2 hour on household chores than untreated ones. The table shows that women are driving the results. Treated women spend 2.5 hours per week more on household chores than their untreated counterpart. Panel B reports no effect of the intervention on the time allocation of the patient's spouse. To conclude, Panel C suggests that, one year after treatment, children of treated subjects are more likely to spend time in school-related activities. Women, once again, are driving the results. The estimated coefficients imply that children of treated women spend almost 8 hours per week more on school-related activities than children of untreated women.

[Insert Table 4]

c. RDD Estimates

As mentioned in section 4, the DiD estimator provides unreliable estimates of the treatment effects if time-varying unobservables, such as connections and socials networks, are correlated both with the treatment status and the outcome of interest. To address this issue, we next implement the RDD approach explained in section 4. First stage regressions are presented in table 5, and report that subjects who turned up in the first 10 days of the intervention were over 70 percent more likely to be treated than their counterpart. The table shows that this does not change when we include in the regression a vector of covariates including medical conditions and lower-body muscles strength score, confirming that most of the variation in treatment status is determined by the day of the intervention subjects turned up. In table 6, we report RDD estimates of the intervention impacts on labour market outcomes. The table suggest no statistically significant effect of the intervention on the probability of being employed. One year after the being fitted with the equipment, treated individuals do not significantly differ with their untreated counterpart, in the probability of being employed. We do find, evidence, however, of increased labour force participation. Treated individuals appear to be almost 15 more likely to be in the labour force than their untreated counterpart. When we distinguish between men and women, we see that women are driving this result. Again, when we focus on the sample of workers, we find no evidence of treatment effects on the amount of hours worked per week and the level of monthly income.

[Insert Table 6]

Lastly, we in table 7 we report RDD estimates of intervention impact on the time allocation. Consistently with what we found in the previous section, Panel A shows no effect of the intervention on the amount of time individuals spend in income-generating activities, for both men and women. The table, however, suggests that one year after the intervention, treated women increased the amount of time spent in performing household chores with respect to their untreated counterpart, by almost 2.5 hours per week. Panel B reports no effect of the intervention on the time allocation of the patients' spouse. Finally, in Panel C, we report the effect of the intervention on the time allocation of children of study participants. The estimated coefficients suggest that, on average, one year after the intervention children of treated patients spend additional 5.3 hours per week on school-related activities than their untreated counterpart. Also in this instance, it is the sample of women driving the results. The estimated coefficient in column (3) suggest that children of treated women spend 6.4 hours per week more on school related activities.

[Insert Table 7]

7. Conclusions

This study evaluates the effect of a medical intervention that provided orthotic equipment to individuals with lower-limbs disability in Kampala, Uganda. The assignment to treatment rule implied by the intervention causes a discontinuity in the probability of treatment according to the day subjects turned up. We exploit this feature to estimate the cause effect of the intervention on a number of market and non-market outcomes.

We first compare treated individuals before and after the intervention, and find that one year after receiving the equipment, treated men improved their level of body mobility and walking speed. Women, on the other hand, reported an enhanced cardiovascular fitness level. Based on a regression discontinuity design approach, we find evidence that the intervention only had marginal effects on labour market outcomes. We find that study participants and women in particular, one year after the intervention are 25 percent more likely to be part of the labour force than their untreated counterpart.

Given the chronic nature of disability, patients may become unable to perform simple tasks and activities, and may require the assistance of other household members. An improvement in the subjects' medical condition, therefore, not only is likely to improve the well-being of the patients, but is likely to benefit the whole household by freeing up time and efforts that other family members can allocate now in different activities. We find evidence that children of treated women increased the amount of time spent in school-related activities by more than 6 hours per week. This finding is in line with the literature that mothers tend to invest more in children's human capital (see Hill and King, 1995).

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Tables & Figures

Alessio Gaggero

31st October 2014

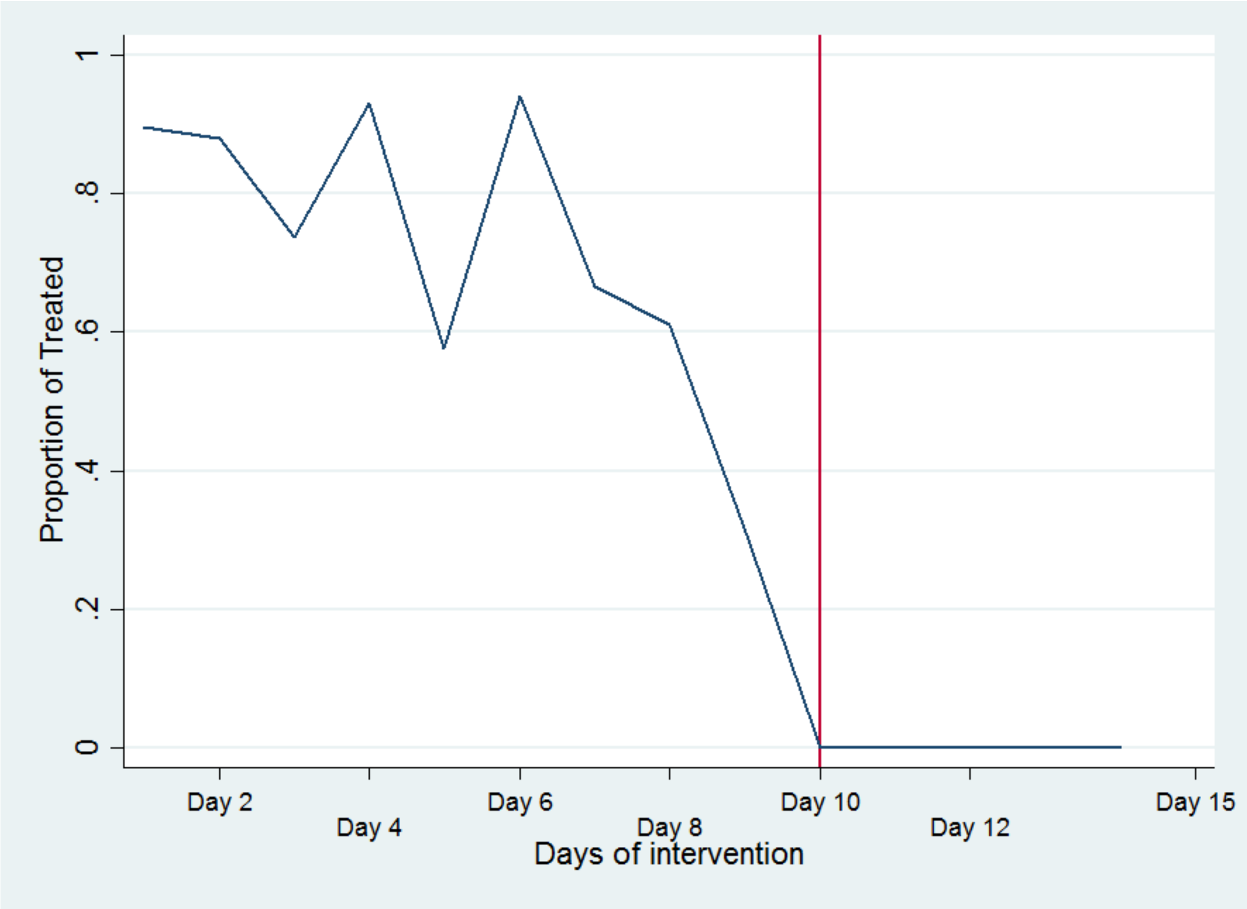
Table 1: Baseline Differences between Treatment and Control Groups

	(1) Treated	(2) Control	(3) p-value
Medical			
Time Up and Go	12.90	12.58	0.721
Ten Meters	11.99	13.21	0.139
Resting Heart BPM	76.46	75.64	0.642
Polio	0.59	0.65	0.395
Age of Onset	11.06	7.88	0.125
Muscle Strength Score	0.14	-0.35	0.163
Demographics			
Female	0.39	0.49	0.147
Age	40.31	42.89	0.145
Married	0.45	0.40	0.480
HH Size	4.85	4.92	0.822
Background			
Schooling	8.42	8.81	0.509
Natural Parents	0.80	0.84	0.508
Father's Years of Schooling	5.50	5.79	0.699
Mother's Years of Schooling	4.19	4.69	0.441
Raven Test Score	8.76	9.39	0.076
Wechsler Test Score	14.59	15.34	0.258
Labour			
Employed	0.74	0.85	0.054
Self-Employed	0.56	0.68	0.090
Monthly Income	263.89	211.88	0.391
Work Experience	14.42	17.14	0.145
Observations	158	75	233

Source: Uganda Disability Survey

Notes: The table shows characteristics of treated and control subjects at the baseline. The proportion of self-employed, the monthly income and the work experience are for the sample of workers.

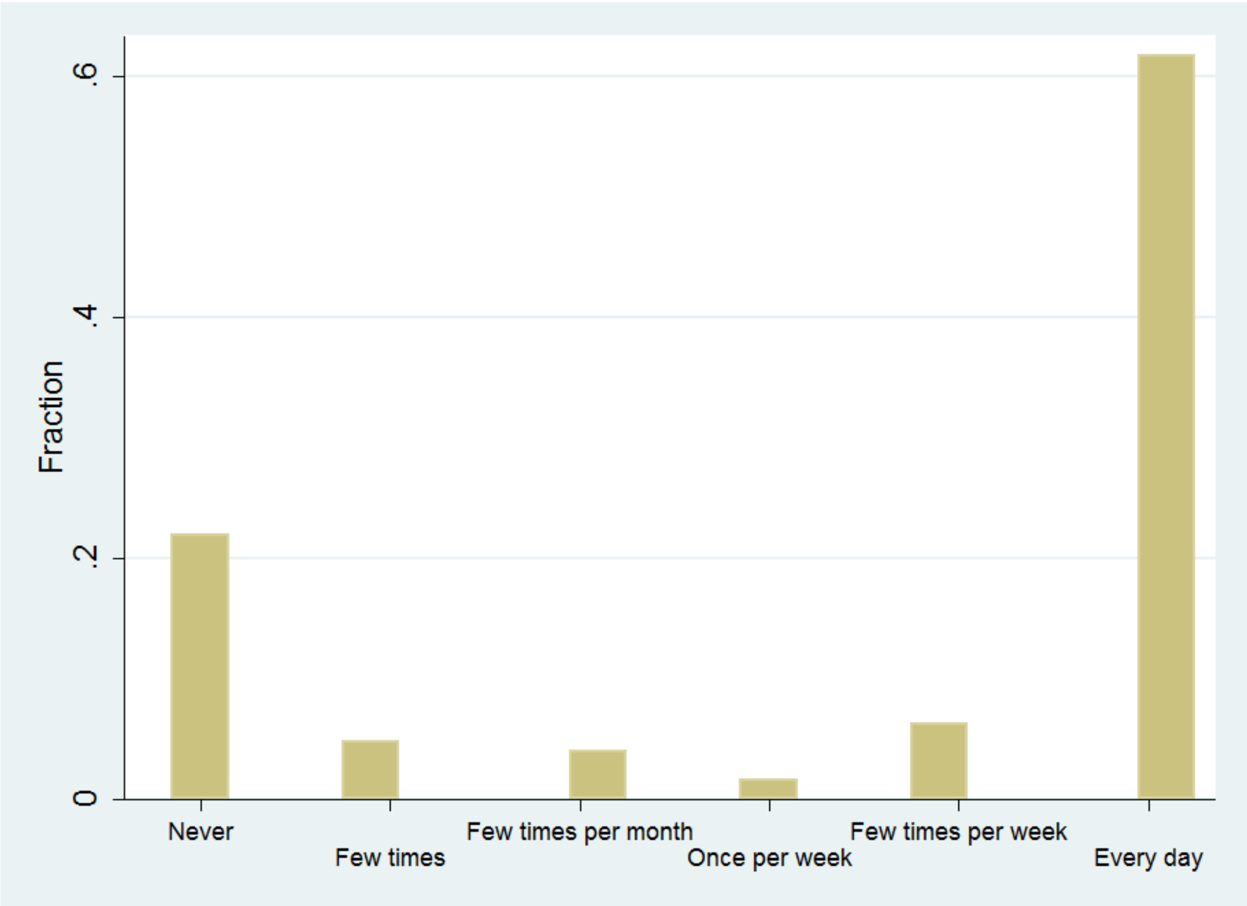
Figure 1: PROBABILITY OF BEING TREATED



Source: Uganda Disability Survey

Notes: The figure shows the probability of being treated as a function of the day of the intervention individuals turned up.

Figure 2: Take up Rate



Source: Uganda Disability Survey

Notes: The figure shows the frequency at which subjects used the treatment.

Table 2: BEFORE-AFTER ESTIMATES OF THE TREATMENT EFFECTS ON MEDICAL OUTCOMES

	(1) **Full-Sample**	(2) **Men**	(3) **Women**
Time Up and Go	-3.223***	-5.499***	0.570
	(1.074)	(1.217)	(1.822)
	113	68	45
10 Meters Walk	-2.617***	-4.218***	0.228
	(0.832)	(1.027)	(1.220)
	115	71	44
Resting Heart Rate	-4.929**	-3.278	-7.900*
	(2.172)	(2.554)	(4.042)
	117	71	46
Excercise Heart Rate	-2.000	-7.000	6.333
	(4.416)	(5.340)	(7.780)
	70	42	28
Difference between Heart Rate	3.867	-0.800	13.200**
	(3.588)	(4.353)	(6.181)
	69	41	28

Source: Uganda Disability Survey

Notes: Each coefficient in the table is from a separate regression. The table presents before-after estimates of the intervention impact on medical outcome. Results are conditional on the medical condition and the muscle strength score of the patients. Robust standard errors in parentheses ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.* Sample sizes for each regression in brackets

Table 3: DiD estimates of the Treatment Effects on Labour Outcomes

	(1) **Full-Sample**	(2) **Men**	(3) **Women**
Employment	-0.041	-0.037	-0.048
	(0.056)	(0.070)	(0.093)
	[223]	[127]	[96]
Labour Force Participation	0.099**	0.048	0.169**
	(0.044)	(0.062)	(0.070)
	[223]	[127]	[96]
Workers:			
Monthly Income	85.539	25.682	163.064
	(87.889)	(136.627)	(101.238)
	[133]	[77]	[56]
Hours Worked p/w	-2.195	-9.296	5.683
	(4.986)	(6.440)	(7.516)
	[219]	[126]	[93]

Source: Uganda Disability Survey

Notes: Each coefficient in the table is from a separate regression. The table presents before-after estimates of the intervention impact on medical outcome. Robust standard errors in parentheses ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.* Sample sizes for each regression in brackets

Table 4: DiD estimates of the Treatment Effects on Time Allocation

	(1) **Full-Sample**	(2) **Men**	(3) **Women**
Panel A: Patient			
Income Generating Activities	0.025	-0.095	0.057
	(0.748)	(0.999)	(1.106)
	[223]	[127]	[96]
Household Chores	1.201*	0.066	2.507***
	(0.637)	(0.798)	(0.909)
	[223]	[127]	[96]
Panel B: Spouse			
Income Generating Activities	0.022	0.055	0.000
	(0.024)	(0.042)	(0.019)
	[223]	[96]	[127]
Household Chores	0.094	0.000	0.123
	(0.218)	(0.000)	(0.377)
	[223]	[96]	[127]
Panel C: Kids			
School-Related Activities	6.305***	4.811	7.970***
	(2.050)	(2.966)	(2.702)
	[223]	[127]	[96]
Household Chores	0.707	0.550	0.863
	(0.641)	(0.774)	(1.048)
	[223]	[127]	[96]

Source: Uganda Disability Survey

Notes: Each coefficient in the table is from a separate regression. The table presents before-after estimates of the intervention impact on medical outcome. Robust standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sample sizes for each regression in brackets

Table 5: First Stage

	Without Covariates			With Covariates		
	(1) **Full-Sample**	(2) **Men**	(3) **Women**	(4) **Full-Sample**	(5) **Men**	(6) **Women**
Treatment Weeks	0.747***	0.745***	0.750***	0.746***	0.724***	0.770***
	(0.064)	(0.096)	(0.086)	(0.066)	(0.100)	(0.091)
Observations	225	127	98	225	127	98
R^2	0.382	0.326	0.443	0.393	0.348	0.457

Source: Uganda Disability Survey

Notes: The vector of covariates includes medical condition and the muscles strength score of the patients.Robust standard errors in parentheses

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Table 6: RDD estimates of the Treatment Effects on Labour Outcomes

	(1) **Full-Sample**	(2) **Men**	(3) **Women**
Employment	0.028	-0.045	0.105
	(0.068)	(0.087)	(0.109)
	[223]	[127	[96]
Labour Force Participation	0.147***	0.056	0.247***
	(0.056)	(0.077)	(0.091)
	[223]	[127]	[96]
Workers:			
Monthly Income Over all Activities	144.746	28.246	205.380
	(104.462)	(173.936)	(126.592)
	[133]	[77]	[56]
Hours Worked p/w	2.433	-1.432	3.886
	(7.858)	(9.696)	(11.110)
	[152]	[93]	[59]

Source: Uganda Disability Survey

Notes: Each coefficient in the table is from a separate regression. The table presents estimates from a partially fuzzy regression discontinuity design. Cut-off point is day 10 of the intervention. Robust standard errors in parentheses ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.* Sample sizes for each regression in brackets

Table 7: RDD estimates of the Treatment Effects on Time Allocation

	(1) **Full-Sample**	(2) **Men**	(3) **Women**
Panel A: Patient			
Income Generating Activities	-0.494	-0.513	-0.011
	(0.940)	(1.297)	(1.296)
	[223]	[127]	[96]
Household Chores	1.055	-0.123	2.475**
	(0.765)	(0.977)	(1.132)
	[223]	[127]	[96]
Panel B: Spouse			
Income Generating Activities	-0.024	-0.097	0.042
	(0.049)	(0.077)	(0.027)
	[223]	[96]	[127]
Household Chores	-0.234	0.000	-0.518
	(0.267)	(0.000)	(0.456)
	[223]	[96]	[127]
Panel C: Kids			
School-Related Activities	5.301*	5.187	6.404*
	(2.723)	(3.911)	(3.428)
	[223]	[127]	[96]
Household Chores	1.061	0.893	1.194
	(0.822)	(0.957)	(1.261)
	[223]	[127]	[96]

Source: Uganda Disability Survey

Notes: Each coefficient in the table is from a separate regression. The table presents estimates from a partially fuzzy regression discontinuity design. Cut-off point is day 10 of the intervention. Robust standard errors in parentheses ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.* Sample sizes for each regression in brackets

1 Appendix

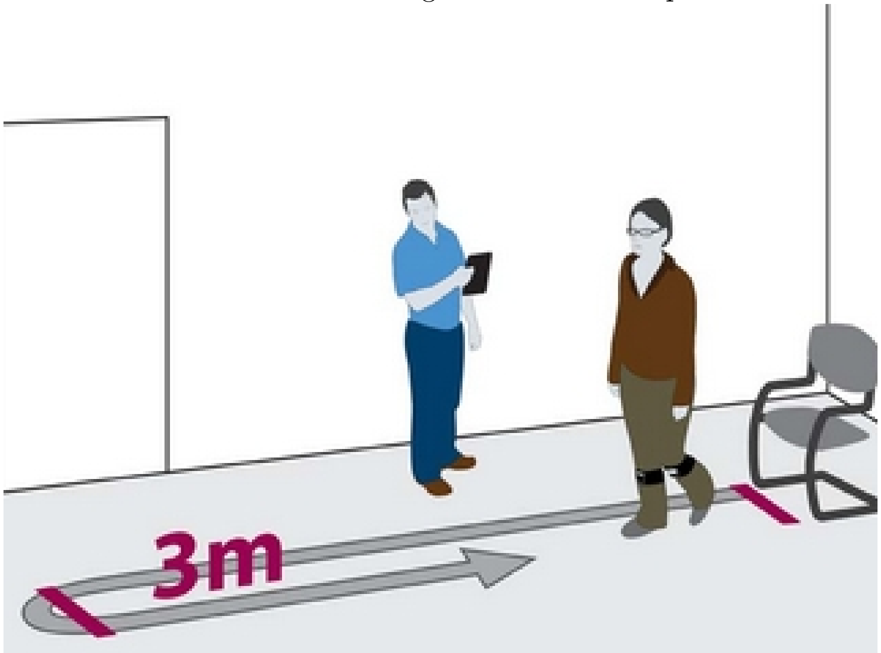
Table A.1: Pre-treatment Differences Between Recruited and Non-Recruited

	(1) Recruited	(2) Non-Recruited	(3) *p*-value
Treatment	0.65	0.69	0.582
Medical			
Time Up and Go	12.58	12.90	0.717
Ten Meters	12.03	12.57	0.518
Resting Heart BPM	75.73	76.42	0.694
Polio	0.69	0.58	0.091
Age of Onset	6.55	11.80	0.009
Muscle Strength Score	-0.29	0.11	0.262
Demographics			
Female	0.44	0.42	0.688
Age	39.71	41.80	0.241
Married	0.51	0.40	0.098
HH Size	5.46	4.61	0.008
Background			
Schooling	8.61	8.52	0.881
Natural Parents	0.79	0.83	0.533
Father's Years of Schooling	5.76	5.52	0.749
Mother's Years of Schooling	5.21	3.97	0.054
Raven Test Score	8.75	9.08	0.356
Wechsler Test Score	14.84	14.84	1.000
Labour			
Employed	0.81	0.76	0.483
Self-Employed	0.65	0.58	0.281
Monthly Income	219.29	258.19	0.528
Work Experience	14.52	15.58	0.573
Observations	72	161	233

Source: Uganda Disability Survey

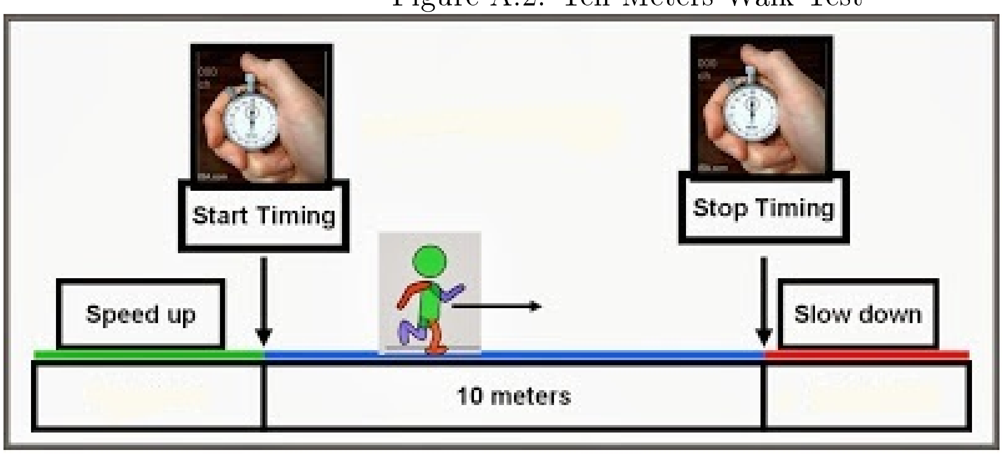
Notes: The table compares baseline characteristics of subjects recruited and non-recruited form the NGOs.

Figure A.1: Time Up and Go Test



Source: Uganda Disability Survey
Notes: The figure offers an explanation of the time up and go test.

Figure A.2: Ten Meters Walk Test



Source: Uganda Disability Survey
Notes: The figure offers an explanation of the timed ten meters walk test.

Table A.2: Characteristics of Attrited

	(1) Treated	(2) Control	(3) *p*-value
Medical			
Time Up and Go	12.71	12.33	0.799
Ten Meters	12.36	12.56	0.884
Resting Heart BPM	78.78	77.79	0.724
Polio	0.00	0.00	.
Age of Onset	10.55	10.87	0.919
Muscle Strength Score	0.38	-0.41	0.088
Demographics			
Female	0.41	0.37	0.718
Age	39.25	38.63	0.820
Married	0.50	0.41	0.349
HH Size	5.36	5.46	0.835
Background			
Schooling	8.64	8.27	0.691
Natural Parents	0.78	0.78	0.929
Father's Years of Schooling	5.72	6.61	0.429
Mother's Years of Schooling	4.03	4.69	0.504
Raven Test Score	8.78	8.90	0.802
Wechsler Test Score	14.25	13.10	0.249
Labour			
Employed	0.71	0.70	0.932
Self-Employed	0.49	0.57	0.425
Monthly Income	375.24	180.29	0.170
Work Experience	11.72	9.09	0.243
Observations	76	37	113

Source: Uganda Disability Survey

Notes: The table compares baseline characteristics of treated and controls from the sample of 2 attrited subjects.

Table A.3: Pre-treatment Differences at the Cut-off point - Day 10 of Intervention

	(1) **Below Cut-off**	(2) **Above Cut-off**	(3) ***p*-value**
Medical			
Time Up and Go	12.18	11.48	0.608
Ten Meters	14.15	10.84	0.014
Resting Heart BPM	74.90	74.41	0.878
Polio	0.53	0.64	0.406
Age of Onset	8.67	7.29	0.664
Muscle Strength Score	-0.29	0.09	0.573
Demographics			
Female	0.51	0.44	0.576
Age	43.45	42.44	0.754
Married	0.42	0.44	0.866
HH Size	5.14	4.48	0.260
Background			
Schooling	8.19	9.40	0.276
Natural Parents	0.81	0.88	0.483
Father's Years of Schooling	6.54	4.88	0.226
Mother's Years of Schooling	5.35	4.72	0.607
Raven Test Score	9.24	9.96	0.163
Wechsler Test Score	15.76	14.88	0.450
Labour			
Employed	0.88	0.84	0.614
Self-Employed	0.77	0.56	0.076
Monthly Income	195.24	263.95	0.530
Work Experience	18.49	18.42	0.985
Observations	43	25	68

Source: Uganda Disability Survey

Notes: The table compares baseline characteristics of subjects just below and above the cut-off.