Do Increases in Income Affect Averting Behavior?

A Study of Nicaragua's Social Protection Network Program

An honors thesis for the Department of Economics

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As people become richer, they do not continue with their prior spending habits. While change can sometimes be slow due to habit formation, regardless of income levels, changes in wealth will result in changes in consumption. Just as a college graduate with his first job finds himself with more disposable income and changes his habits, a poor Mexican villager raises his consumption of land-intensive goods, such as meat and animal products when he receives a positive income shock (Alix-Garcia et al. 2011). While consumption will not remain stagnant, it is not always clear how the new income will be spent.

With normal goods, we would expect to see an increase in demand as income rises. In developing countries, when incomes rise, demand for some goods must also rise. However, because poverty is higher and overall income levels are lower, consumers have to prioritize their spending. While a family may be able to make use of both food and a filtration device for drinking water, food may present a more immediate concern, so, while both would be useful in the long run, one may have to prioritize more pressing issues. Regardless of income level, everyone must make these types of consumption decisions, but due to the low level of income, goods which may be considered necessities in a country like the United States may be viewed as luxuries in other countries. In a developed country environmental quality is often considered a normal good (Lehmijoki and Palokangas 2009), but in a setting with more poverty, environmental quality may not follow this pattern.

One cannot infer spending behavior in developing countries from information on consumption behavior in developed countries. For example, while residents of high income countries generally have high access to clean water readily available in their home, in developing countries, available water can be untreated, leaving the household with the option of treating their water themselves. Hence, studying individual spending patterns in developing countries is

important for developing more specific and effective government policies, as well as helping aid be better used and distributed.

Environmental quality is often much lower in developing countries, due to factors such as clean water and sanitation being less available and air pollution and forest degradation often being higher (Arvin and Lew 2011). Factors such as poverty, low enforcement capabilities, lack of regulation, and lack of infrastructure compound one another, resulting in lower environmental quality. The poor often bear the brunt of environmental degradation, and environmental quality is often ignored when poverty reduction is the main focus of growth in developing countries (Liu 2012). While in developed countries people can go inside on a day with smog, go somewhere with air conditioning on a hot and humid day, and get piped water that has been treated for many potential contaminants, people in developing countries often do not have this luxury. They must live more exposed to the environment and must face and address the current conditions regardless of what they are.

Theory, such as the environmental Kuznets curve, implies that in a developing country, an increase in wealth is associated with degradation of the environment, until a certain point when the relationship reverses (McConnell 1997). This relationship is complex, and while there is a wide range of literature suggesting this theory holds for environmental bads such as pollution and deforestation (Alix-Garcia et al. 2011), it does not comment on an individual's efforts to protect himself from environmental degradation. As a country becomes wealthier, it can invest more in infrastructure to protect its citizens from environmental pollution and establish regulations to help protect the environment. With the low quality of the environment in developing countries and inability for government to address the concerns, it is up to the individual to seek protection for his family. The goal of my thesis is to investigate the impact of

an increase in household income on averting behavior to reduce health risks due to environmental pollution.

Many economists see poverty reduction as a means of spurring economic growth in developing countries. This, in turn, can lead to an increase in environmental quality (Grossman and Krueger 1995). Yet, there is no established relationship from the existing literature between positive income shocks and a household's efforts to protect itself from environmental harm in a developing country setting. Conditional cash transfer (CCT) programs have become an increasingly popular method of combating poverty in developing countries. Households receive a periodic monetary transfer as long as they meet the conditions of the program. The available cash transfers are exogenous, and therefore unrelated to other factors that may determine a household's level of income.[1] With effective data, evaluating CCT programs can provide insight into the spending patterns of the very poor in developing countries, and these results can in turn be used to more effectively support these nations and their people. In this study, I use data from Nicaragua's Social Protection Network CCT program to analyze the effects of increased income on environmental averting behaviors, such as drinking water treatment and cooking fuel type.

Nicaragua's Social Protection Network provides a unique situation because it included a pilot phase and can be treated as a randomized controlled trial (RCT). While the use of RCTs in policy evaluation has not been without controversy, they provide an effective mechanism for establishing causal impacts of a policy or program. They have sometimes been criticized for their financial unfeasibility and implementation issues (Barrett and Carter 2010), and Deaton [2010] raises questions about the external validity and applicability to policy of many randomized controlled trials. Because these experiments are performed in the real world and not

[1] The availability of the cash transfers is exogenous, but the actual amount received depends on compliance, which is endogenous to behavior. This paper uses the eligibility for the transfers as the source of exogenous variation.

a controlled laboratory, there is still room for error and bias, though less so than with other methods. Even if a participant is in the control group of an experiment and does not receive the treatment, he is still surveyed and simply being surveyed may cause him change his behavior (Kremer et al. 2011). This may lead the researcher to draw conclusions that are supported by the experimental data, but not completely representative of the real world, though this is a problem with all surveys. However, due to the ease and effective of determining causal relationships among variables, RCTs have become increasingly common in development economics. They are an effective and useful tool for studying the relationships between any number of variables and isolating causality.

This study will focus on the pilot phase of Nicaragua's Social Protection Network which took place from 2000 to 2002, with yearly surveys. The baseline survey in 2000 was completed before the treatment group began receiving transfers. Though the program showed positive results in the promotion of education and health of very poor Nicaraguans, the main initial goals, it was discontinued in 2005 for political reasons (Nigenda and González-Robledo 2005). In addition to exploring the general health and education effects of the program, this study will focus on the effects of a positive income shock on individual protection from environmental harm, specifically by using more environmentally healthy and effective cooking fuels and water purification methods. Taking advantage of the random nature of the policy's pilot design, I will use data collected for the initial policy evaluation, from the International Food Policy Research Institute (IFPRI) in conjunction with the Nicaraguan government and the Inter-American Development Bank (IADB).

After determining that the program actually results in an effective increase in income, by analyzing the effects of the program on expenditures for items such as food, I continue to

analyze the effects of the program on water treatment and fuel choice. Though I found no significant change in cooking fuel use, water treatment increased by about 25% in the treatment group in 2002, compared to the control group in the same year. There were also large increases in food expenditure in both 2001 and 2002, in addition to increases in school enrollment and overall health. These results add evidence supporting the theory that as incomes rise, at least some averting behavior increases, even among the very poor.

Section 2 provides further background information about conditional cash transfer programs in general, and Nicaragua's Social Protection Network program. Section 3 details the data used, while section 4 provides the empirical framework of the study. Section 5 provides the results, section 6 discusses the implications of these results, and section 7 concludes.

2. Background

a. Conditional Cash Transfer Programs:

Conditional cash transfer programs have been hailed as effective poverty reducing methods. One of the first, and most studied, of these programs is Mexico's Progresa, which later became Oportunidades. Oportunidades currently provides cash transfers to about one quarter of Mexico's population (Braine 2006). Families receive a monthly cash infusion, provided that they enroll children in school and verify attendance and receive regular health checkups and education (Nigenda and González-Robledo 2005). Mexico's Oportunidades and Progresa, which began in 1997, have inspired many similar conditional cash transfer programs, such as Brazil's Bolsa Familia, the world's largest CCT program, and Nicaragua's Social Protection Network, as well as dozens of programs in Central and South America and Africa. Common conditions for

the cash transfers include school attendance, medical checkups, and educational program participation.

Policy evaluations and other studies indicate that CCT programs have had a largely positive impact. For example, not only had poverty been reduced by 30% in Mexico, as of 2005, but maternal mortality had fallen by 11% and school enrollment had increased (Nigenda and González-Robledo 2005). Similar positive results have been observed in Brazil and in programs across the world (Fried 2012). Studies of CCT programs have covered a wide variety of topics—from their effects on general health and education, the focus of the programs, to effects on neonatal and infant mortality, and even the effects on voting behavior and deforestation (Barham 2009; Green 2006, Alix-Garcia et al. 2011). CCT programs often provide researchers with an effective method of exploring how impoverished people in developing countries react to increases in income.

b. Nicaragua's Social Protection Network:

Motivation:

In 2001, nearly 50% of Nicaraguans were living under the poverty line with an income of less than US$ 0.56 a day. Among rural inhabitants, statistics were even worse, with two-thirds of residents classified as poor and over a quarter as extremely poor.

Poverty is often associated with poor health, which is an argument that can be observed in the country. Within Nicaragua, there is a disconnect between rural areas, in which poverty is higher, and urban areas. For example, the global fertility rate for an educated urban-dwelling woman is 2.6, while for an uneducated woman living in a rural community, it is 4.4. Also, in rural areas adolescent fertility is higher and access to health clinics is lower. According to the 2001 Demography and Health Nicaraguan Survey (ENDESA), 20% of children under age five

are chronically under nourished, and rural peasants find it difficult to cover their needs in their economy based on subsistence agriculture (Nigenda and González-Robledo 2005).

Given the success of conditional cash transfer programs in other Latin American countries, and the poor health and economic situations of impoverished citizens, the Nicaraguan government promoted the Social Protection Network program. The overarching goal of the program was to use investments in human capital to promote a better quality of life for those living in poverty. The program was approved by the International Monetary Fund (IMF) and World Bank and financed by the Inter-American Development Bank (IDB).

Targeting:

Developed in two phases, the pilot phase of the program was designed as a randomized controlled trial, and the International Food Policy Research Institute (IFPRI) was responsible for the initial program evaluation. The pilot recipients included about 10,000 families in 21 localities in 6 municipalities, though the target sample for the survey was only 1,764 households and only 1,359 households responded in all three survey years. The pilot phase focused on only two of the seventeen departments in Nicaragua. The Nicaraguan Government chose Madriz and Matagalpa in the north Central Region as the first departments in which to implement the program. Eighty percent of the residents of these departments were poor and half were extremely poor. In addition, the departments were easily accessible from the capital and had strong local coordination and institutional capacity. Six out of 20 of the municipalities in these two departments were chosen. Forty-five to 90 percent of this population was either poor or extremely poor.

Next, using factors such as average family size, percentage of population without piped water, percentage without a latrine, and percentage of illiterate people over age 5, four priority

groups were established among the 59 census districts in these six municipalities. The 42 districts in the top two priority groups were deemed eligible for the pilot phase of the program. Of these 42 districts, 21 were randomly selected to be in the program group, and the other 21 were in the control group. The program was designed so that all households in the treatment districts would be eligible for the program (Mallucio and Flores 2005).

Transfer Eligibility:

All eligible households received the food security transfer (bono alimentario) of C $2,280 (US $224) per year. Additionally, households with children between ages 7 and 14 who had not yet completed fourth grade received a school attendance transfer (bono escolar) of C $1,140 (US $112) per year and the school supply transfer (mochila escolar) of C$275 (US $21) per child per year.

The food security and school attendance transfers were paid to the female head of the family every other month, while the school supply transfer was paid annually. C $80 (US $6) per child was also provided to the schools, so theoretically even if an eligible household declined to participate, if the child attended school, there would be spillover effects. In order to continue receiving transfers, the female head had to ensure that, every two months, she attended training sessions for various topics, such as sexual and reproductive health, nutrition, childcare, family hygiene, lactation, and environmental health. She also had to ensure that children between ages 7 and 14 were attending school regularly. Also, children under 5 years of age were required to be brought to health appointments and receive vaccinations.

Households could be expelled from the program if they failed to comply with the program requirements, failed to collect the transfer in two consecutive periods, had more than 27 unexcused absences per child during the school year, had a child who failed to be promoted to

the subsequent grade, or falsely reported information during data collection. Less than one percent of participants were expelled during the pilot phase of the program (Mallucio and Flores 2005).

3. Data

The data used in the current study come from IFPRI's evaluation data set for Nicaragua's Social Protection Network. The data come from a panel survey conducted jointly between IFPRI, the government administrators, and the Inter-American Development Bank. The survey was funded by the government of Nicaragua (IFPRI 2005).

A baseline survey was conducted in the fall of 2000, with follow-up surveys in the fall of 2001 and the fall of 2002. A community census was conducted in the summer of 2000 and used to determine the survey sample population. The household-level survey defined a household as a residence that was occupied at the time of the 2000 census, and it included all individuals that shared meals and had lived in the residence for at least three months of the previous year, regardless of whether or not they were related (Winters, Stecklov, and Todd 2009).

There were 1,359 households that completed the survey in all three rounds. This leads to a total of 4,077 observations. Variables in the analysis include dummy variables for years 2001 and 2002, a dummy variable indicating treatment district, water treatment status, and fuel type.

The two major averting behavior variables I measure are water treatment and cooking fuel type. I will also examine the general outcomes and effects of the conditional cash transfer program. The main focus of the program was promoting elementary school attendance and improving health of the rural Nicaraguans. Additional dependent variables I analyze include annual expenditures on food, school attendance for 7 to 14 year olds, general health data for both

children and the overall population, and various non-food expenditures. Table 1 presents the summary statistics for these general variables.

Table 2 contains information that compares the characteristics of the control group and intervention group in the year 2000, before the treatment had begun. One variable for which there was a significant difference between the control group and the intervention group was the percentage of household members described as having been healthy for the past month, rather than having an illness such as a cold, cough, measles, or diarrhea. It is not unexpected that out of a large number of comparisons, not all will be statistically equal among comparison and treatment baseline groups. For the majority of variables, however, there is no significant difference between the average values of the two groups, and due to the randomized nature of the study and relatively small sample size, any differences could likely be attributed to sampling error. This indicates that the two groups do not differ in observed characteristics and likely do not differ on unobserved characteristics, and that it would be valid to attribute any difference between the two groups in 2001 or 2002 to the treatment.

4. Empirical Model:

Together with the panel structure of the data, the randomized design of the pilot phase of the program and its evaluation allows us to draw causal inferences from the data. The changes in the economic, health, and educational status of survey participants in both the control and intervention groups were tracked for three years. Because a comparison of means indicates that the two groups were statistically the same for most of the measured characteristics in the baseline data, collected before the intervention began, it is logical to conclude that any changes observed in the 2001 or 2002 survey can be attributed to the intervention. Variation was on the district

and time levels, and to address any inherent differences between the districts, district-fixed effects were included in all regressions.

The average program effect can be calculated using the difference-in-difference technique. The difference-in-difference estimator can be written as: $(E_{T1} - E_{C1}) - (E_{T0} - E_{C0})$ where T is the treatment group, C is the control group, 0 and 1 indicate the baseline and treatment period, and E is the outcome variable. Though $(E_{T0} - E_{C0})$ should be close to zero, given that districts were randomly assigned, including this value in the estimator gives us a more robust measure of the program effect because it subtracts out possible preexisting differences between the two groups from the final differences (Mallucio and Flores 2005).

Water treatment and fuel choice are the main variables that I use to model averting behavior. The survey offered four options for water treatment: no treatment, boiling, using bleach, and other. Survey responders indicated that three fuel types were used: wood, kerosene, and butane. I first estimate models in which all water treatment options are grouped together and all non-wood fuel options are grouped together. I then look at the impacts on each treatment separately. Analyses of other program effects also use the same models.

In addition to looking specifically at water treatment and cooking fuel choice, I analyzed other program effects. The purpose of this was to establish that the program actually changed the incomes for the families. If all the income gained from the program was used to fulfill the health and education conditions, the effectual income of the households would not change, making it pointless to use the program to test whether increases in income promoted averting behavior. By looking at variables like food expenditure, it is possible to determine whether the program effectively increased income. It is also interesting to compare the magnitudes of the changes for the general program goals, compared to those of the averting behaviors.

The same basic difference-in-difference model is used to study each effect. I am looking at the difference across time between the treatment group and control group. Because districts were randomly assigned to the control or treatment group, from a simple difference-in-difference analysis, it is possible to infer a causal effect. Equation (1) shows the basic model:

$$E_{iDt} = \beta_0 + \beta_1 Y_1 + \beta_2 Y_2 + \beta_3 I_D + \delta_1 Y_1 I_D + \delta_2 Y_2 I_D + \varepsilon_{iDt} \qquad (1)$$

Where:
E_{Dit}= the outcome variable for household *i* at time *t* in district D.
Y_1=1 if the year is 2001
Y_2=1 if the year is 2002
I_D=1 if the intervention was in district D
ε_{iDt}= random standard error
δ_1= the average treatment effects for 2001
δ_2= the average treatment effects for 2002

The program effects are relative to the 2000 baseline. This same basic model was used to estimate the program effects for the goal outcomes of the program. Given the experimental nature of the pilot phase, the basic difference-in-difference model provides a simple method for determining causal impacts.

The treatment was not uniform among all recipients, however, as households with more children ages 7-14 received a larger transfer. There are multiple methods for addressing the heterogeneous treatment. It is important to note that number of children is endogenous to transfer income. While this equation can provide insight into the effects of each additional child on the outcome variables, to measure the exogenous impact of variations in income, transfers of different amounts would have to be randomly assigned to households or districts.

Equation (2) looks at the effect of each of the three different transfer types separately:

$$E_{iDt} = \beta_0 + \beta_1 Y_t + \sum_{p=1}^{3} \theta_p \, treat_{iD}^{p} + \sum_{p=1}^{3} \delta_p \, treat_{iD}^{p} * Y_t + \varepsilon_{iDt} \quad (2)$$

Where:
$treat^1$ = 1 if the household is eligible to receive the food security transfer
$treat^2$= 1 if the household is eligible to receive the school attendance transfer
$treat^3$=the number of school supply transfers a household is eligible to receive
δ_p= the average treatment effects for each respective transfer type and year

Every household in an intervention district is eligible to receive the food security transfer, but only households in an intervention district with at least one child ages 7 to 14 are eligible to receive school attendance transfers. The number of supply transfers the household is eligible to receive is the same as the number of children ages 7 to 14 in a treatment district household.

Equation (3) provides a method for determining the impact of each additional cordoba, Nicaragua's unit of currency, given to a treatment household.

$$E_{iDt} = \beta_0 + \beta_1 Y_1 + \beta_2 Y_2 + \beta_3 I_D + \delta_1 Y_1 T_t + \delta_2 Y_2 T_t + \varepsilon_{iDt} \quad (3)$$

Where:
T_t= the total transfer amount a household is eligible to receive
δ_1= the average effect of each additional cordoba in 2001
δ_2= the average effect of each additional cordoba in 2002

Each treatment household received a base amount, B, and households with children between the ages of 7 and 14 received an additional transfer per child, S_t and a school attendance transfer, A Then the treatment amount can be defined as $T_t = (B + K * S_t + I(K > 0) * A) * I_D$. If the household is not in an intervention district, this value is zero. Because the data for the amount of money actually received by each household, the values used in this model represent what a household was eligible to receive, assuming it fully complied with the conditions of the program. While it is interesting to examine the effects of each additional cordoba, because each transfer

was several thousand cordobas, it is also interesting to calculate the effects for each monetary level.

The same models are used to measure the effects of assignment into the intervention group of the conditional cash transfer program on the decision to treat water and the decision to use a "cleaner" fuel source, as well as several other outcome variables that are relevant to the goals of the conditional cash transfer program. The specific effects on each water treatment type—boiling, using bleach, and other—are also included in section 5.

The simple difference-in-difference model provides us with the average treatment effect, but this includes households that were eligible for the treatment but declined it. By including individuals that were eligible for the program but did not participate with those who did participate, the regression results are diluted. An interesting impact is the outcome effect for those who took up the treatment; the take-up group. This is referred to as the treatment on the treated (TOT). TOT can be estimated by replacing I_D with an indicator of the take-up group; I_T.

One complication is that the take-up group is no longer randomly assigned, since the individual households in the treatment districts have the option to opt out of the program, and they do not opt out randomly. Households that would opt out of the program are not the same as households that take up the treatment or households that are not eligible for treatment. Estimating the models by OLS will produce biased results. A consistent estimator in this case is instrumental variables where the treatment indicator, I_D, is a valid instrument for the take-up indicator, I_T. When instrumental variables is used in such a way, the situation mimics that of a natural experiment (Angrist and Krueger 2001). The results and comparison with model one are presented in section 5.

5. Results

The results from the basic OLS regression detailed in equation (1) of section 4 are displayed in Table 3. Many of the variables, including some of the averting behavior variables changed significantly for the treatment group throughout the program trial. For example, being in the treatment group increased the probability of a participant treating their water in 2002, the second round of the survey, two years into the program, by an average of 0.12, all else held constant. This effect is both statistically significant ($p<0.01$) and has a large practical effect. The partial elasticity of this variable is 0.25. This effect is at least as large, or larger, than many of the other general program effects. The experimental treatment did not have a significant effect on water treatment in treatment districts 2001, relative to control districts in 2001. Receiving cash transfers had no significant effect on fuel type choice in both 2001 and 2002.

It is not surprising that there was no significant effect on fuel type choice. As the summary statistics in Table 1 indicate, among baseline observations, only 0.66% of people used a cooking fuel type that was cleaner than wood. This represented nine out of 1,359 households surveyed. In 2002, after two years of treatment, 1.0% of households, or fourteen total households used a cleaner fuel source for cooking. The small number of households using a cooking fuel other than wood, such as butane or kerosene, was very small to begin with, and increased by only five households throughout the two year pilot.

Because there was a positive, significant treatment effect of treating drinking water, I ran several regressions looking at the effects on the individual treatment types that were asked about in the program evaluation survey—boiling, using bleach, and other. The results of these regressions are detailed in Table 4. The only significant effect was related to using bleach to treat drinking water. All other factors held constant, a survey participant who received the cash

transfers had a 0.108 higher probability of treating their water in 2002, compared to a survey participant in the control group in the same year. The semi-elasticity is 0.24. There were no significant results for 2001 or with the other two variables. This indicates that the significant effect on water treatment described in Table 3 was primarily driven by one type of treatment—bleach. Boiling and other treatment types did not play a role in the overall treatment result. These results are not surprising, as water treatment is the most income sensitive measure.

In order to explore the general effects of the program as they relate to the initial goals, I looked at general changes in expenditures and health characteristics. Table 5 explores the consumption habits of the study sample and the effects of the program on these variables. If each good behaved as a normal good, we would expect increases in expenditures among all goods. At the income level of the Nicaraguans who participated in the program, not all consumption responded positively to an increase in income.

The annual expenditures for all food items were significantly greater ($p<0.01$) for participants who received the treatment in both 2001 and 2002 compared to participants who did not receive transfers in both years. Specifically, in 2001, a household that was receiving the cash transfers spent an average of C$4,347 (US $337) more on food than a household in the control group, all other factors held constant. In 2002, a treatment household spent on average C$3,415 (US $247) more on food than a control household. Average food expenditure for households that were surveyed was C$7,389 in 2000. In 2001, the difference in food expenditures of treatment and control households was about 59% of this average, and in 2002, the difference was about 46% of the average annual household food expenditures. This is a large effect and indicates the program did effectively increase the available income of participants. This

observation then makes it valid to analyze the effects of this increased income on averting behavior.

There was no significant difference between the two groups' expenditures on many of the other variables. For example in both 2001 and 2002, there was no difference in expenditure for items such as toilet paper, napkins, or tissues, soap or shampoo, children's clothes, or sewing equipment. Some items only exhibited a significant difference in one of the follow up survey years.

In 2001, a household that was receiving the cash transfers spent C$31 less on clothes for adults ($p<0.01$), which is about 50% of the average baseline expenditure of C$62.4, but spent C$8 more on shoes for children ($p<0.1$), which is about 47% of the average baseline expenditure of C$17, in the six months prior to the survey than a household in the control group, all other factors held constant. In 2002, a treatment household spent on average C$1.1 more on cleaning equipment such as brooms and mops ($p<0.05$), C$2.5 more on toiletries like deodorant and lotion ($p<0.1$) in the month prior to the survey, and C$5.0 more on dishes in the six months prior to the survey than a control household. All of these are economically significant results, with semi-elasticities ranging from 0.25 to 0.8. Additionally, in both years, households receiving transfers spent less money on adult shoes than households not receiving transfers, all other factors held constant.

While some of these results seem logical, others are surprising. We would expect that when income increases, one would spend more money on food, whether it is through buying more of it or more expensive and nutritional food. However, we may not expect the expenditures for adult clothing to decrease. It is possible that the households did purchase some of these items, but the items are durable goods that they can use continuously for several months.

These expenditures would not show up in the survey, which only asks about the previous month or six months, but still could have been purchased. The expenditures most likely to come up in the survey are those for nondurable goods that have to continually be purchased.

The results displayed in Table 6 delve further into the health and school attendance related outcomes of the program. The major goals of the program were to increase primary school attendance and promote health. While results in these areas were somewhat mixed, there were, without a doubt, some positive results. For example, in both 2001 and 2002, the percent of children of primary school age enrolled in school was higher in districts that received the treatment compared to those that did not. In 2001, for a treatment household compared to a control household, a child was, on average, 8.1 percentage points more likely to be enrolled in primary school, in the same year, holding all other factors constant ($p<.01$). In 2002, being in a treatment household increased the percent of children enrolled in school by 5.5 percentage points ($p<.05$). The baseline mean is 81 percent. Compared to some other variables, these were not very big effects—the semi-elasticity for 2001 is only 0.10. There was no significant difference in the percentage of primary school age children working. And, in 2002, being in a treatment district actually lowered the percentage of children under five years enrolled in school by 15.9 percentage points ($p<.05$), holding all other factors constant. Given that overall only about 29% of children under five are enrolled in some type of school, this is a big effect. Pre-primary school enrollment was not a specific goal of the program, and households did not receive any additional incentive for enrolling their children in pre-primary school, but nevertheless, the result may initially seem surprising.

Table 6 also includes health related outcome variables. There was no significant difference between the two groups for factors such as reported diarrheal cases for children under

five in the prior month or overall general health of children ages 7 to 14 in the prior month. The overall percentage of all household members reported as being healthy for the prior month, and not suffering from a sickness such as a cough, cold, diarrhea or an eruptive illness, increased in treatment areas. Holding all other factors constant, the percentage of household reported as healthy was 7.5 percentage points higher in 2001 and 5.5 percentage points higher in 2002, in a treatment district compared to a control district in the respective years. While this is a positive result, it seems unexpected that, though the program emphasized child health and school factors, only adults reported significant health benefits. Given that averting measures with water increased in 2002, it is surprising that this did not in turn lead to an increase in child health.

Equations 2 and 3 describe two different methods of addressing the heterogeneous treatment. Because different monetary amounts were not randomly distributed and depended on the number of children, the heterogeneous treatment was not exogenous, limiting the interpretations of the results. Not every household received the same amount of money with each transfer—every household in the intervention group received C$2,280 per year through a food security transfer. Households with children between the ages of 7 and 14 received an additional C$1440 through a school attendance transfer and C$275 per child through a school supply transfer. So, a household with no children in this age range would receive C$2,880, while a household with one child in the age range would receive C$4,595 per year. Table 7, which provides the results of equation 3, also addresses the effects of each additional Nicaraguan cordoba. The transfer amounts for each household in each year were calculated based on what each was eligible to receive. For simplicity, I measured the transfer amount in thousands of cordobas. There were significant effects on both food expenditure and probability of treating water. In 2001, for each additional 1,000 Nicaraguan cordobas of income, households spent an

average of C$996 more on food compared to households in the control group, holding all other factors constant. In 2002, an additional 1,000 Nicaraguan cordobas of income led to a C$770 increase in annual food expenditures compared to control households. In 2002, there was also a positive effect of additional income on averting behavior—specifically probability of treating drinking water. A 1000 Nicaraguan cordobas increase in income led to, on average, a 0.024 increase in probability of treating water, compared to a household that did not receive a cash transfer in the same year, all other factors held constant. While this may seem like a small increase, it is important to note that each participant household received at least C$2,880. A household that had no children between 7 and 14 years old and received only the food security transfer had a 0.069 higher probability of treating their water, a household with one child had a 0.110 higher probability, and a household with two children had a 0.117 higher probability.

The other method I employ for analyzing the heterogeneous treatment effect looks at each of the treatment types as separate variables provides some interesting results. Results are displayed in Table 8. The water treatment, food expenditure, and percent total health variables are all mainly driven by the food security transfer. This result is expected, as it is the only transfer all treatment households receive, and, monetarily, it is the largest transfer. The attendance transfer, which everyone with at least one child receives, also has a significant effect on food expenditure and overall health in year two. This model was not valid for analyzing the percentage of children of a household enrolled in school and percentage of healthy children variables. Because the observations for these variables are limited to households that have children, all of these households receive both the food security transfer and school attendance transfer. For these two variables, rather than look at each transfer separately, I included a binary variable indicating the base amount all households with children were eligible to receive (the

food security transfer, school attendance transfer, and one school supply transfer), as well as a variable for the number of school supply transfers a household was eligible for, above one. The results of this regression are displayed in Table 9. While there was not a significant treatment effect for either variable for school enrollment, in 2001, for each additional school supply transfer a treatment household was eligible to receive, the percentage of children reported as healthy increased by 0.058 percentage points compared to a control household in the same year. This result suggests that while simple eligibility for program treatment did not increase child health, eligibility for an extra school supply transfer, which was based on the number of children ages 7 to 14 in a household, did significantly increase child health.

The results of the IV regressions are displayed in Tables 10 to 14. Table 10 shows the results of the basic regression. Table 11 shows the effects on expenditure and can be compared to Table 5. Table 12 shows the effects on health and education and can be compared to Table 6. And finally, Table 13 shows the impacts of each separate transfer type, and Table 14 shows the impacts of the varied transfers on child school enrollment and child health. The results of the instrumental variables regressions, which use experiment participation as an instrument for treatment take up, closely resemble those of the OLS regressions. While each of the variables is slightly different, the statistical and economic significance for the variables is the same as the variables in the OLS regressions. Because treatment take up was very high, the results of the two regression types are not very different.

Just as the effects of the conditional cash transfer program, as they relate to the initial health, education, and food security goals, are mixed, the effects of the program as they relate to the averting behavior variables are also mixed. The implications of these results are further discussed in section 6.

6. Discussion:

After seeing the success of Mexico's Progresa and Oportunidades conditional cash transfer programs, the Nicaragua's Social Protection Network program was developed (IFPRI 2005). The goals of the program were very similar to those of Progresa. In Nicaragua, a country with high rural poverty, the program aimed to promote health and increase educational attainment, while increasing the overall income level of rural households. Targeting the poorest Nicaraguans, the program showed many promising results, though not all goals were achieved.

Major positive results include increases in food expenditure, increases in overall reported household health, and increases in school enrollment. Overall food expenditure significantly rose among households that received transfers in the early 2000s. As the results of this study indicate, a large portion of the money received in the transfer went to buying food. Every participating household received a "food security" transfer, and while households could spend the money however they wanted, most of it went to buying food. The fact that there was such a large increase in food expenditures indicate that the program was effective in increasing income. Because the transfers did raise income, it was possible to see what the effect on this increase in income was on the averting behaviors.

Households with primary school-aged children were also eligible to receive an additional transfer if they could prove their children were attending school. Children from participating households were more likely to attend primary school than those from ineligible households, indicating that the program was successful at increasing primary school attendance rates for primary school-aged children. When asked if, in the prior month, a member of the household had any illness from a list of communicable and non-communicable sicknesses, households in the treatment area reported that a higher percentage of household members were healthy. But,

because there was no difference in the reported health of children, this effect is driven by an increase in adult health. While an increase in adult health is a positive result, it was not the primary objective of the program. This overall increase in health could possibly be a result of a few factors.

Overall health may have increased simply because the households were able to buy more food, so household members could eat an adequate and more nutritional diet. Alternatively, a condition of the program was that the female head of house attend bimonthly educational workshops. The head of house could have learned what the most nutritionally advantageous food to buy was, and they could have learned how illness could be prevented or alleviated. Having clean drinking water is also associated with overall health, and those with access to clean drinking water are at less of a risk for several illnesses including diarrhea and other communicable diseases. Households participating in the program were more likely to treat their drinking water than households in the control group.

Because all treatment households' heads of house were required to attend education, it is not possible to disentangle this effect from the effect of the increase in income. Meta-analyses of communication campaigns and similar educational programs have indicated that, while the average campaign has a positive impact on the intervention population, this average effect is only about five percentage points (Snyder 2007). In the current study, households in intervention districts were an average of 25% more likely to treat their water in 2002 compared to control districts in the same year, all else equal, so I argue that, while the educational programs may have positively affected this figure, the increase in income also had an important effect. Though, regardless of what drove the effect, the results provide support for the ability of similar CCT programs to promote water treatment.

Environmental quality in Nicaragua is not equal to the environmental quality in an industrialized nation. In a developing country, it is more difficult to enforce environmental laws and standards, even if they exist (Akpalu, Eggert, and Vondolia 2009). Sometimes, environmental standards are even irrelevant. For example, the World Bank estimates that, in 2000, only 62% of rural Nicaraguans had access to an "improved water source," which included a household connection, public standpipe, borehole, protected well, or rainwater collection (World Bank 2013). If such a small percentage of rural residents have access to piped water, even if the country had high water quality standards, they would be irrelevant for a large portion of the population. Even with many of the World Banks water sources that are classified as "improved," there is a real possibility for recontamination when the water is collected or stored by a household. With overall environmental quality being poor and few national environmental protection policies, it is often up to the individual household to protect itself from environmental harm.

Averting behavior is an important factor to consider when studying the development, environment, and health of developing nations. When faced with poor environmental conditions, an individual may be able to mitigate some of the negative effects through his personal behavior. Of course, depending on the location and situation, a household's options will be different, but there are many different actions someone in a developing country can take. For example, he can treat his drinking water, relatively easily. All averting behavior prevents some kind of tradeoff (Blomquist 2003). If there were not benefits from doing something, most people would not do it. Likewise, if there were not costs to doing something, one would think everyone would always do it. The costs are sometimes a monetary investment, or sometimes a time investment, the opportunity cost of someone's time. The benefits can be numerous and include improved health.

There is a tradeoff between the initial investment and the future benefits, which may not be immediate.

The two averting behaviors that I planned to analyze were a household's decision to treat water and a household's decision to use butane or kerosene for cooking fuel, rather than wood. It was expected that averting behavior was consistent among the program area before the intervention began, and any slight deviations could be attributed to sampling error. Some of the households received a previously unexpected, exogenous increase in income, which the other households in control districts did not receive. Households were not restricted in their use of the cash transfers and could spend the money in any number of ways. The study aimed to investigate how households spent this income, and whether increased averting behavior and protection from environmental harm were results of the transfers. While no effect was observed in the first year of the study, in the second year, there was a significant treatment effect. All else equal, households participating in the program were more likely to treat their water than households in the control group, and bleach-water treatment was the treatment type that drove this effect (Table 4). Although the "other" category in the survey is somewhat dubious, as respondents could be using an ineffective method of treatment, because so few households indicated this response, even putting these households in the "no treatment" category does not change the significance of results.

The same significant results were not observed for fuel choice. There were very few people using an alternative cooking fuel before the program began, and this number had barely risen by the end of the pilot. Given the initial costs and future benefits of these two types of averting behavior, these results do not seem surprising.

Treating drinking water is relatively cheap and easy. The program effect observed in Nicaragua can be attributed to bleach use, and chlorine bleach is the most easily used, widely used, and affordable method for treating drinking water in developing countries (Sobsey 2007). It is effective against almost all waterborne pathogens. Because diarrhea and other communicable disease caused by waterborne pathogens often very quickly result in sickness, it is not surprising that when the income of a household increases, and the household head attends health and environmental education workshops, the household will be more likely to treat its water. If the benefits to the averting behavior are timely, and the monetary costs are relatively low and implementation is easy, one would expect the averting behavior to increase.

Using wood as cooking fuel, rather than a natural gas that burns more efficiently and cleanly, presents health problems to residents of developing countries. Women and children are more severely affected by inefficient burning of biomass such as wood or agricultural waste, and it is estimated that worldwide, in developing countries, 2.5 billion people rely on biomass for cooking fuel. The World Health Organization estimates that, worldwide, about 1.3 million premature deaths can be directly attributable to indoor air pollution caused by burning biomass (IEA 2006). Using a cleaner burning fuel would improve the health and quality of life of Nicaraguans surveyed in this program evaluation, yet very few households use a source other than wood.

Most fuel wood was collected by a household member and could be obtained for no monetary price. Someone would have to spend time collecting it, but he would not have to pay for it. Any kind of gas, such as butane or kerosene, would have to be purchased, and given the very low number of users, it may not be readily available. If it is, it may be too expensive to purchase with the money households received in the program. There may be a longer history of

purchasing bleach for water treatment, but no such history for using natural gas to cook, so residents are more reluctant to begin spending money on something they never have before. Alternatively, residents may not be as educated about the benefits of using a non-biomass cooking fuel, or, if they, are the health benefits may not be as immediately visible. If one consumes a pathogen in compromised water, he can become sick very quickly, but a respiratory illness caused by indoor air pollution may take months or years to develop. The connection between the two actions may not be obvious. For a possible array of reasons, cleaner fuel use was not frequently used for cooking, and the increased income gained by the Social Protection Network conditional cash transfer recipients did not increase the use.

The focus of the conditional cash transfer program was to promote health and education, while reducing poverty. Receiving the cash transfers was conditional upon ensuring household educational attendance and health checkups. It is interesting to look at Nicaragua's Social Protection Network in the context of the Millennium Development Goals, which aim to promote development by economic and social conditions of the world's poor (United Nations 2013). The program showed direct progress towards goals such as ending poverty, universal primary education and access to safe drinking water, and indirect progress towards goals like relieving hunger, by significantly increasing food consumption. While this program was initially implemented on a small scale, it can still provide insight into and supporting evidence for what sorts of programs are effective in certain areas and what sorts of programs are not effective.

The program also indicates that averting behavior is a factor that is considered when making consumption decisions, which in turn indicates that demand for health does exist, though appears to be more focused on behaviors with more immediate results. Preferences and behavior

are more focused on the near future, more weight being put on behaviors with more immediate implications, than those with longer-term pay-offs.

Nicaragua's Social Protection Network adds evidence to the growing support of conditional cash transfer programs across the globe. After first being studied extensively in Mexico, CCT programs have expanded in Latin America, Africa, and Asia. Because Nicaragua's program was heavily targeted towards the very poor, the results may not be applicable to every income level. It does provide insight into the spending decisions of the very poor and how these patterns and behaviors change as income increases. There are still over one billion people in the world who live in extreme poverty (World Bank 2013), and this study provides insight into a possible and increasingly popular way to address the problem. With a conditional cash transfer program, such as the Social Protection Network program, not only are people given monetary transfers, which increase food expenditures and some averting behaviors, but people invest in human capital—education and health—which will hopefully continue to benefit recipients and ultimately lead to global development.

7. Conclusion:

After years of being mired in political unrest, Nicaragua emerged in the 1990s and has been able to grow consistently, though modestly, for the past two decades. The nation still suffers from inequality, however, and many rural Nicaraguans still live in poverty. The pilot phase of the Social Protection Network program showed many positive results, enough so that it was expanded after 2002 (IFPRI 2005). The results, while not successful in every aspect of the initial goals, were largely positive and provide hope for effectiveness of similar programs.

In developing countries, environmental quality is typically low because government either cannot or does not provide high standards. It is often up to the individual to do what he can to protect himself. Conditional cash transfer programs have been growing in popularity in recent years. While their primary goal is to provide monetary aid while investing in human capital, it is not always obvious how recipients will respond and exactly what they will spend their money on. Nicaragua's Social Protection Network was a conditional cash transfer program, active from 2001 to 2005, that, like most, aimed to promote health and education by requiring primary school attendance and health checkups and education as conditions for participation. The program was designed as a randomized control trial that made it simple to analyze and draw causal inferences. Depending on the behavior, results for averting behavior were mixed, though overall, the program achieved many of its health and educational goals. The program was deemed as successful and continued past the pilot phase. Programs like this provide insight into effective ways to reduce poverty, invest in human capital, and study expenditure patterns, even giving insight into topics like averting behavior. Due to their continued success, conditional cash transfer programs have been implemented in many nations all across the globe.

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Appendix 1: Tables

Table 1: Summary Statistics				
Variable	Mean	Std. Dev.	Minimum	Maximum
Intervention District	0.519	0.500	0	1
Number (of children 7-14)	1.079	0.979	0	5
Treated Drinking Water	0.475	0.500	0	1
Clean Fuel Choice	0.007	0.081	0	1
Food Expenditure	7389.08	5826.096	0	41798
Expenditure on Cleaning Equipment	1.200	4.587	0	38
Expenditure on Toiletries	9.350	20.402	0	240
Expenditure on Adult Clothes	62.397	157.951	0	1400
Expenditure on Child Clothes	33.680	97.732	0	1600
Percent of Children Under 5 with Diarrhea in past month	0 .463	0.447	0	1
Percent of Children (age 7-14) Enrolled in Primary School	0.806	0.328	0	1
Percent of Household Reported as Healthy	0.704	0.269	0	1
Percent of Children Healthy	0.796	0.331	0	1
Percent of Children Younger than 5 Enrolled in School	0.289	0.399	0	1

Values reported are the summary statistics from the baseline survey, conducted in 2000, before the treatment began. "Intervention District" is a binary variable that equals 1 if the household is in an intervention district and 0 otherwise. "Treated Drinking Water" and "Clean Fuel Choice" are also binary variables indicating whether a household performed the respective action.

Table 2: Comparison of Means				
Indicator	Intervention (n=810)	Control (n=771)	Difference (control-intervention)	Total (n=1581)
Number of children (7-14)	1.080 (0.0396)	1.077 (0.0424)	-0.0024 [0.967]	1.079 (0.0290)
Percentage of family members reported as "healthy"	0.668 (0.0104)	0.738 (0.0096)	0.070*** [0.000]	0.7027 (0.007)
Annual food expenditures (1,000s of cordobas)	7.119 (0.199)	7.417 (0.218)	0.298 [0.319]	7.264 (0.147)
Semi-annual expenditures for children's clothes	31.468 (2.853)	35.109 (4.063)	3.641 [0.4599]	33.244 (2.853)
Monthly expenditure for lighting	18.228 (0.925)	16.663 (0.792)	1.565 [0.1973]	17.426 (0.607)
Self-reported house value (1000s of cordobas)	11.249 (2.249) {n=639}	16.081 (3.157) {n=612}	4.832 [0.210]	13.613 (1.925) {n=1,251}
Percentage of children (7-14) enrolled in school	0.803 (0.016) {n=454}	0.791 (0.016) {n=453}	-0.011 [0.6201]	0.797 (0.011) {n=907}

Notes: Standard deviations are in parentheses and p-values for the test of equality of means across groups are in brackets. Data is from the baseline survey conducted in 2000. The intervention group has 810 observations and the control group has 771 observations except where noted. "***" indicates significance at the 1 percent level.

Table 3: Dependent Variables: Treated Water, Clean Fuel		
	(1)	(2)
Variables	Treated	Clean
2001	0.036	0.003
	(0.025)	(0.005)
2002	0.062**	0.000
	(0.025)	(0.005)
Number of Children	0.006	-0.001
	(0.006)	(0.001)
Average Treatment Effect 2001	-0.011	0.004
	(0.035)	(0.007)
Average Treatment Effect 2002	0.120***	0.007
	(0.035)	(0.007)
Constant	0.475***	0.007**
	(0.012)	(0.003)
Total Effect 2001	0.025	0.007
	(0.024)	(0.005)
Total Effect 2002	0.182***	0.007
	(0.024)	(0.005)
Observations	4,077	4,077
Number of districts	42	42
Adj R-squared	0.007	-0.011
SER	0.454	0.097
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Notes: All columns represent OLS regressions that include district fixed effects. The average treatment effect in 2001 is the interaction Treatment*2001, and the average treatment effect in 2002 is the interaction Treatment*2002, where "Treatment" is a binary variable equal to one if a household is in an intervention district. The total effect is the year effect plus the average treatment effect. "Number of Children" is an explanatory variable for the number of children ages7 to 14. "Treated" is a binary variable equal to 1 if a household treated their drinking water, and "Clean" is a binary variable equal to 1 if a household used a non-wood cooking fuel.

Table 4: Types of Water Treatment			
	(1)	(2)	(3)
Variables	Boiling	Bleach	Other
2001	0.006	0.026	0.005
	(0.009)	(0.025)	(0.003)
2002	0.002	0.063**	-0.003
	(0.009)	(0.025)	(0.003)
Number of Children	-0.001	0.008	-0.001
	(0.002)	(0.006)	(0.001)
Average Treatment Effect 2001	0.005	-0.015	-0.002
	(0.012)	(0.035)	(0.004)
Average Treatment Effect 2002	0.008	0.108***	0.003
	(0.012)	(0.035)	(0.004)
Constant	0.021***	0.444***	0.003*
	(0.004)	(0.012)	(0.001)
Total Effect 2001	0.011	0.011	0.003
	(0.008)	(0.024)	(0.003)
Total Effect 2002	0.010	0.172***	0.000
	(0.008)	(0.024)	(0.003)
Observations	4,077	4,077	4,077
Number of districts	42	42	42
Adj R-squared	-0.011	0.006	-0.009
SER	0.155	0.458	0.054
Standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

Notes: All columns represent OLS regressions that include district fixed effects. The average treatment effect in 2001 is the interaction Treatment*2001, and the average treatment effect in 2002 is the interaction Treatment*2002, where "Treatment" is a binary variable equal to one if a household is in an intervention district. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. The total effect is the year effect plus the average treatment effect. "Boiling," "Bleach," and "Other" are binary variables indicating method of water treatment.

Table 5 : Effect on Expenditures

Variables	(1) Treated Water	(2) Food expenditures	(3) Radio	(4) Toilet Paper	(5) Soap	(6) Toiletries	(7) Broom	(8) Adult Clothes	(9) Child Clothes	(10) Adult Shoes	(11) Child Shoes	(12) Sewing	(13) Dishes
2001	0.036	706.749**	0.060**	1.051*	0.866	0.685	0.595*	6.073	-1.408	-6.054	-7.245**	-0.349	0.052
	(0.025)	(329.823)	(0.027)	(0.619)	(0.846)	(1.014)	(0.310)	(8.143)	(5.519)	(5.056)	(3.156)	(0.522)	(1.843)
2002	0.062**	-213.625	0.047*	3.183***	1.936**	1.077	0.457	-4.029	5.666	-0.285	0.624	0.906*	-3.437*
	(0.025)	(329.930)	(0.027)	(0.619)	(0.847)	(1.014)	(0.311)	(8.145)	(5.521)	(5.058)	(3.158)	(0.522)	(1.844)
Number of Children	0.006	848.537***	0.020***	0.515***	0.384*	1.036***	0.135*	1.266	5.505***	3.157**	4.944***	0.178	0.944**
	(0.006)	(82.108)	(0.007)	(0.154)	(0.211)	(0.252)	(0.077)	(2.027)	(1.374)	(1.259)	(0.786)	(0.130)	(0.459)
Av.Treatment Effect 2001	-0.011	4,346.684***	0.028	-0.184	-1.870	-0.488	0.468	-31.065***	4.519	-15.832**	8.070*	0.179	1.827
	(0.035)	(457.549)	(0.037)	(0.858)	(1.174)	(1.406)	(0.431)	(11.296)	(7.657)	(7.014)	(4.379)	(0.724)	(2.557)
Av.Treatment Effect 2002	0.120***	3,414.907***	0.052	0.692	-0.263	2.532*	1.096**	-7.878	-1.578	-12.622*	1.755	0.674	4.977*
	(0.035)	(457.549)	(0.037)	(0.858)	(1.174)	(1.406)	(0.431)	(11.296)	(7.657)	(7.014)	(4.379)	(0.724)	(2.557)
Constant	0.469***	6,472.488***	0.474***	3.659***	6.919***	8.231***	1.053***	61.029***	27.734***	34.297***	11.748***	1.272***	5.088***
	(0.014)	(184.379)	(0.015)	(0.346)	(0.473)	(0.567)	(0.174)	(4.552)	(3.085)	(2.827)	(1.765)	(0.292)	(1.030)
Total Effect 2001	0.025	5,053.433***	0.088***	0.867	-1.000	0.197	1.063***	-24.993**	3.111	-21.89***	0.825	-0.170	1.879
	(0.024)	(317.214)	(0.026)	(0.595)	(0.814)	(0.975)	(0.299)	(7.831)	(5.308)	(4.863)	(3.036)	(0.502)	(1.773)
Total Effect 2002	0.183***	3,201.281***	0.100***	3.874***	1.672**	3.609***	1.553***	-11.907	4.087	-12.907**	2.379	1.580***	1.539
	(0.024)	(317.315)	(0.026)	(0.595)	(0.814)	(0.975)	(0.300)	(7.833)	(5.310)	(4.864)	(3.037)	(0.502)	(1.773)
Observations	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077
Number of districts	42	42	42	42	42	42	42	42	42	42	42	42	42
Adj R-squared	0.007	0.076	-0.003	0.010	-0.006	-0.002	-0.002	-0.008	-0.007	-0.004	0.001	-0.006	-0.009
SER	0.454	5958.979	0.485	11.178	15.294	18.312	5.610	147.113	99.720	91.350	57.029	9.431	33.300

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent OLS regressions that include district fixed effects. The average treatment effect in 2001 is the interaction Treatment*2001, and the average treatment effect in 2002 is the interaction Treatment*2002, where "Treatment" is a binary variable equal to one if a household is in an intervention district. The total effect is the year effect plus the average treatment effect. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. Variables "Toilet Paper," "Soap," "Toiletries," and "Broom" (cleaning equipment) represent the expenditures (in Nicaraguan cordobas) on these items in the month prior to the survey. "Food Expenditure" is the annual food expenditures. Other variables are the expenditures on the items in the prior six months.

Table 6: Effects on Health and Education

Variables	(1) Percent Diarrhea	(2) Pct Enrolled in Primary School	(3) Pct Working (of Children)	(4) Pct All Healthy	(5) Pct Children Healthy	(6) Pct Younger Enrolled
2001	-0.042	0.060***	-0.040	-0.050***	-0.014	0.025
	(0.039)	(0.018)	(0.027)	(0.014)	(0.023)	(0.032)
2002	-0.009	0.069***	-0.036	-0.005	0.013	0.436***
	(0.040)	(0.018)	(0.029)	(0.014)	(0.023)	(0.044)
Number of Children		0.034***	0.108***	0.012***	0.038***	0.028***
		(0.005)	(0.008)	(0.004)	(0.007)	(0.009)
Average Treatment Effect 2001	0.033	0.081***	-0.039	0.075***	0.012	-0.016
	(0.055)	(0.025)	(0.043)	(0.020)	(0.032)	(0.046)
Average Treatment Effect 2002	-0.068	0.055**	-0.008	0.055***	0.017	-0.159**
	(0.057)	(0.025)	(0.039)	(0.020)	(0.032)	(0.065)
Constant	0.462***	0.751***	0.118***	0.690***	0.732***	0.269***
	(0.019)	(0.012)	(0.017)	(0.008)	(0.016)	(0.019)
Total Effect 2001	-0.009	0.141***	-0.080***	0.025*	-0.002	0.009
	(0.039)	(0.017)	(0.030)	(0.014)	(0.023)	(0.032)
Total Effect 2002	-0.077*	0.123***	-0.044	0.049***	0.030	0.277***
	(0.040)	(0.017)	(0.029)	(0.014)	(0.022)	(0.048)
Observations	1,507	2,542	1,220	3,733	2,478	1,200
Number of districts	42	42	42	42	42	42
Adj R-squared	-0.027	0.036	0.103	-0.002	-0.003	0.085
SER	0.443	0.256	0.282	0.250	0.324	0.362

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent OLS regressions that include district fixed effects. The average treatment effect in 2001 is the interaction Treatment*2001, and the average treatment effect in 2002 is the interaction Treatment*2002, where "Treatment" is a binary variable equal to one if a household is in an intervention district. The total effect is the year effect plus the average treatment effect. "Number of Children" is an explanatory variable for the number of children ages7 to 14. "Percent Diarrhea" is the percentage of children under 5 years in a household reported to have had a case of diarrhea in the prior month. "Percent Enrolled in Primary School" is the percentage of children in a household ages 7-14 enrolled in primary school. "Percent Younger Enrolled" is the percentage of children under 5 years in a household that attend school. "Percent All Healthy" is the percentage of family members reported to have been healthy in the prior month, and "Percent Child Healthy" is the percentage of children of a household reported to have been healthy in the prior month.

Table 7: Effect of Each Additional Cordoba

Variables	(1) Treated Water	(2) Food Expenditure	(3) Total clothes Expenditure	(4) Total shoes Expenditure	(5) Pct Enrolled	(6) Pct Children Healthy	(7) Pct All Healthy
2001	0.034	870.985***	-2.982	-15.577**	0.071***	-0.027	-0.046***
	(0.024)	(313.091)	(10.918)	(6.426)	(0.018)	(0.023)	(0.014)
2002	0.072***	-59.815	-3.119	-1.268	0.079***	0.006	-0.008
	(0.024)	(312.815)	(10.908)	(6.420)	(0.018)	(0.022)	(0.014)
No. of Children	0.003	616.351***	7.163**	8.462***	0.032***	0.037***	0.008**
	(0.006)	(85.133)	(2.969)	(1.747)	(0.005)	(0.007)	(0.004)
Effect of 1,000 cordoba in 2001	-0.001	996.312***	-2.915	1.478	0.018***	0.013*	0.019***
	(0.008)	(101.751)	(3.548)	(2.038)	(0.005)	(0.007)	(0.004)
Effect of 1,000 cordoba in 2002	0.024***	770.092***	-0.095	-1.907	0.012**	0.011*	0.016***
	(0.008)	(100.876)	(3.518)	(2.021)	(0.005)	(0.007)	(0.004)
Constant	0.475***	7,389.083***	96.077***	54.795***	0.806***	0.794***	0.704***
	(0.012)	(162.507)	(5.634)	(3.323)	(0.009)	(0.011)	(0.007)
Observations	4,077	4,077	4,077	4,077	2,542	2,478	3,733
Number of districts	42	42	42	42	42	42	42
Adj R-squared	0.006	0.066	-0.011	-0.008	0.020	-0.015	-0.002
SER	0.454	5990.757	207.707	122.512	0.258	0.326	0.250

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent OLS regressions that include district fixed effects. The effect of each additional 1,000 cordoba in 2001 is the interaction Treatment Amount*2001, and the effect of each additional 1,000 cordoba in 2002 is the interaction Treatment Amount*2002, where "Treatment Amount" is the amount of money, measured in thousands of Nicaraguan cordoba, that each household was eligible to receive. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 5 and Table 6 for an explanation of the dependent variables.

Table 8: Effects by Treatment Type

Variables	(1) Treated Water	(2) Food Expenditure	(3) Total Clothes Expenditure	(4) Percent All Healthy
2001	0.037	712.630**	4.842	-0.050***
	(0.025)	(329.328)	(11.492)	(0.014)
2002	0.063**	-202.158	1.982	-0.005
	(0.025)	(329.548)	(11.500)	(0.014)
Number of Children	-0.004	752.539***	3.881	0.008
	(0.009)	(117.193)	(4.089)	(0.005)
Supply Transfer	0.015	-266.634	-3.993	-0.014
	(0.028)	(362.543)	(12.651)	(0.016)
Attendance Transfer	0.067	718.796	14.850	0.053
	(0.060)	(785.887)	(27.424)	(0.034)
Effect of Food Transfer in 2001	0.011	3,718.939***	-39.610*	0.088***
	(0.046)	(597.351)	(20.845)	(0.027)
Effect of Food Transfer in 2002	0.173***	2,780.287***	-12.812	0.030
	(0.046)	(603.819)	(21.071)	(0.027)
Effect of Attendance Transfer in 2001	-0.042	429.448	5.046	-0.064
	(0.085)	(1,111.760)	(38.795)	(0.049)
Effect of Attendance Transfer in 2002	-0.065	1,945.103*	-3.526	0.032
	(0.084)	(1,098.812)	(38.344)	(0.048)
Effect of Supply Transfer in 2001	0.000	316.715	8.560	0.023
	(0.037)	(480.864)	(16.780)	(0.021)
Effect of Supply Transfer in 2002	-0.014	-493.070	4.428	0.002
	(0.036)	(468.795)	(16.359)	(0.020)
Constant	0.452***	6,509.286***	89.647***	0.687***
	(0.017)	(220.672)	(7.700)	(0.010)
Observations	4,077	4,077	4,077	3,733
Number of districts	42	42	42	42
Adj R-squared	0.007	0.079	-0.010	-0.001
SER	0.454	5949.325	207.605	0.250

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent OLS regressions that include district fixed effects. The effect of each transfer type in each year is the interaction between the respective transfer type and respective year. "Food Transfer" is a binary variable that equals one if a household was eligible to receive the food security transfer, which every household in an intervention district was eligible to receive. "Attendance Transfer" is a binary variable which equals one if a household was eligible to receive the school attendance transfer, which every household with at least one child between the ages of 7 and 14 in an intervention district was eligible to receive. "Supply Transfer" is the number of school supply transfers a household was eligible to receive, which was equal to the number of children ages 7-14. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 5 and Table 6 for an explanation of the dependent variables.

Table 9: Effects of Different Treatments		
	(1)	(2)
Variables	Percent Enrolled	Pct Children Healthy
2001	0.087***	-0.027
	(0.017)	(0.022)
2002	0.093***	0.009
	(0.017)	(0.022)
Number of Children	0.044***	0.035***
	(0.007)	(0.009)
Base Transfer	0.034	0.061
	(0.032)	(0.041)
Per Child Transfer	-0.023	-0.026
	(0.017)	(0.022)
Effect of Base Transfer in 2001	0.048	-0.009
	(0.031)	(0.040)
Effect of Base Transfer in 2002	0.027	0.025
	(0.031)	(0.040)
Effect of Per Child Transfer in 2001	-0.019	0.058**
	(0.021)	(0.028)
Effect of Per Child Transfer in 2001	-0.020	0.005
	(0.021)	(0.027)
Constant	0.726***	0.719***
	(0.017)	(0.021)
Observations	2,542	2,478
Number of Districts	42	42
Adj R-squared	0.038	0.000
SER	0.256	0.324
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Notes: All columns represent OLS regressions that include district fixed effects. The effect of each transfer type in each year is the interaction between the respective transfer type and respective year. Observations in the regression are limited to households with children, so the variable "Base Transfer" is a binary variable indicating a household was eligible for the food security transfer, school attendance transfer, and one school supply transfer. "Per Child Transfer" represents the number of school supply transfers the household was eligible to receive, above the base transfer amount. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 6 for an explanation of the dependent variables.

Table 10: Basic IV Results

Variables	(1) Treated Water	(2) Food Expenditure	(3) Total clothes Expenditure	(4) Pct Enrolled	(5) Pct Children Healthy	(6) Pct All Healthy
2001	0.030	519.017	3.433	0.061***	-0.022	-0.053***
	(0.026)	(338.756)	(11.797)	(0.018)	(0.023)	(0.015)
2002	0.060**	-401.366	-3.680	0.069***	0.010	-0.006
	0.030	519.017	3.433	0.061***	-0.022	-0.053***
No. of Children	0.006	848.488***	6.772**	0.034***	0.038***	0.012***
	(0.006)	(82.117)	(2.860)	(0.005)	(0.007)	(0.004)
Treatment on Treated in 2001	0.001 (0.037)	4,708..061*** (481.086)	-24.175 (16.763)	0.080*** (0.026)	0.028 (0.033)	0.082*** (0.021)
Treatment on Treated in 2002	0.124*** (0.037)	3,776.305*** (481.085)	0.778 (16.763)	0.054** (0.026)	0.022 (0.033)	0.056*** (0.021)
Constant	0.469***	6,472.541***	88.762***	0.751***	0.732***	0.690***
	(0.014)	(184.398)	(6.422)	(0.012)	(0.016)	(0.008)
Observations	4,077	4,077	4,077	2,542	2,478	3,733
Number of districts	42	42	42	42	42	42

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent IV regressions that include district fixed effects. The treatment indicator is an instrument for the take-up indicator. The treatment on the treated effect in 2001 is the interaction Treatment*2001, and treatment on the treated effect in 2002 is the interaction Treatment*2002. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 5 and Table 6 for an explanation of the dependent variables.

Table 11 : IV Regression Effect on Expenditures

Variables	(1) Treated Water	(2) Food expenditures	(3) Radio	(4) Toilet Paper	(5) Soap	(6) Toiletries	(7) Broom	(8) Adult Clothes	(9) Child Clothes	(10) Adult Shoes	(11) Child Shoes	(12) Sewing	(13) Dishes
2001	0.030	519.017	0.071***	0.896	0.580	0.674	0.611*	6.567	-3.134	-5.575	-8.070**	-0.341	-0.697
	(0.026)	(338.756)	(0.028)	(0.635)	(0.869)	(1.041)	(0.319)	(8.362)	(5.669)	(5.193)	(3.242)	(0.536)	(1.893)
2002	0.060**	-401.366	0.049*	2.991***	1.230	0.807	0.336	-6.236	2.556	-1.745	-0.383	0.772	-3.690*
	(0.026)	(338.861)	(0.028)	(0.636)	(0.870)	(1.041)	(0.319)	(8.365)	(5.670)	(5.194)	(3.243)	(0.536)	(1.893)
Number of Children	0.006	848.488***	0.020***	0.515***	0.384*	1.036***	0.135*	1.267	5.505***	3.158**	4.944***	0.178	0.943**
	(0.006)	(82.117)	(0.007)	(0.154)	(0.211)	(0.252)	(0.077)	(2.027)	(1.374)	(1.259)	(0.786)	(0.130)	(0.459)
TOT Effect 2001	0.001	4,708.061***	0.006	0.115	-1.320	-0.467	0.438	-32.017***	7.842	-16.754**	9.659**	0.163	3.269
	(0.037)	(481.086)	(0.039)	(0.902)	(1.235)	(1.478)	(0.453)	(11.876)	(8.050)	(7.374)	(4.604)	(0.761)	(2.688)
TOT Effect 2002	0.124***	3,776.305***	0.047	1.061	1.096	3.052**	1.330***	-3.629	4.407	-9.812	3.694	0.933	5.463**
	(0.037)	(481.085)	(0.039)	(0.902)	(1.235)	(1.478)	(0.453)	(11.876)	(8.050)	(7.374)	(4.604)	(0.761)	(2.688)
Constant	0.469***	6,472.541***	0.474***	3.659***	6.918***	8.231***	1.053***	61.028***	27.734***	34.296***	11.748***	1.272***	5.089***
	(0.014)	(184.398)	(0.015)	(0.346)	(0.473)	(0.567)	(0.174)	(4.552)	(3.086)	(2.827)	(1.765)	(0.292)	(1.030)
Observations	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077	4,077
Number of districts	42	42	42	42	42	42	42	42	42	42	42	42	42

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

All columns represent IV regressions that include district fixed effects. The treatment indicator is an instrument for the take-up indicator. The treatment on the treated (TOT) effect in 2001 is the interaction Treatment*2001, and treatment on the treated (TOT) effect in 2002 is the interaction Treatment*2002. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 5 for an explanation of the dependent variables.

Table 12: IV Regression Effects on Health and Education

Variables	(1) Percent Diarrhea	(2) Pct Enrolled in Primary School	(3) Pct Working (of Children)	(4) Pct All Healthy	(5) Pct Children Healthy	(6) Pct Younger Enrolled
2001	-0.033	0.061***	-0.040	-0.053***	-0.022	0.034
	(0.040)	(0.018)	(0.028)	(0.015)	(0.023)	(0.033)
2002	0.002	0.069***	-0.038	-0.006	0.010	0.450***
	(0.041)	(0.018)	(0.028)	(0.015)	(0.023)	(0.044)
No. of Children	-0.020*	0.034***	0.108***	0.012***	0.038***	0.028***
	(0.010)	(0.005)	(0.008)	(0.004)	(0.007)	(0.009)
Treatment on Treated in 2001	0.017	0.080***	-0.040	0.082***	0.028	-0.034
	(0.058)	(0.026)	(0.044)	(0.021)	(0.033)	(0.047)
Treatment on Treated in 2002	-0.085	0.054**	-0.004	0.056***	0.020	-0.188***
	(0.059)	(0.026)	(0.041)	(0.021)	(0.033)	(0.066)
Constant	0.484***	0.751***	0.118***	0.690***	0.732***	0.269***
	(0.022)	(0.012)	(0.017)	(0.008)	(0.016)	(0.019)
Observations	1,507	2,542	1,220	3,733	2,478	1,200
Number of districts	42	42	42	42	42	42

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent IV regressions that include district fixed effects. The treatment indicator is an instrument for the take-up indicator. The treatment on the treated effect in 2001 is the interaction Treatment*2001, and treatment on the treated effect in 2002 is the interaction Treatment*2002. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 6 for an explanation of the dependent variables.

Table 13: IV Regression Effects by Treatment Type

Variables	(1) Treated Water	(2) Food Expenditure	(3) Total clothes Expenditure	(4) Pct All Healthy
2001	0.030	534.679	3.737	-0.054***
	(0.026)	(338.406)	(11.811)	(0.015)
2002	0.061**	-380.535	-3.362	-0.005
	(0.026)	(338.622)	(11.818)	(0.015)
Number of Children	-0.004	755.256***	3.962	0.008
	(0.009)	(117.224)	(4.091)	(0.005)
Supply Transfer	0.034	18.626	2.473	-0.008
	(0.029)	(382.246)	(13.341)	(0.017)
Attendance Transfer	0.018	171.365	1.239	0.040
	(0.064)	(842.726)	(29.412)	(0.037)
Effect of Food Transfer in 2001	0.016	4,142.423***	-31.154	0.101***
	(0.048)	(632.398)	(22.071)	(0.028)
Effect of Food Transfer in 2002	0.163***	3,105.716***	0.043	0.032
	(0.049)	(639.087)	(22.305)	(0.029)
Effect of Attendance Transfer in 2001	0.024	829.826	-20.806	-0.068
	(0.092)	(1,200.655)	(41.904)	(0.053)
Effect of Attendance Transfer in 2002	0.017	2,804.111**	9.309	0.033
	(0.090)	(1,185.056)	(41.359)	(0.052)
Effect of Supply Transfer in 2001	-0.028	31.025	16.645	0.020
	(0.039)	(517.017)	(18.044)	(0.023)
Effect of Supply Transfer in 2002	-0.045	-934.184*	-4.525	0.001
	(0.038)	(503.381)	(17.568)	(0.022)
Constant	0.455***	6,510.913***	90.027***	0.687***
	(0.017)	(223.121)	(7.787)	(0.010)
Observations	4,077	4,077	4,077	3,733
Number of districts	42	42	42	42

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All columns represent IV regressions that include district fixed effects. The treatment indicator is an instrument for the take-up indicator. "Food Transfer" is a binary variable representing the food security transfer, which every household could receive. "Attendance Transfer" is a binary variable representing the school attendance transfer, which every household with at least one child between the ages of 7 and 14 in an intervention district could receive. "Supply Transfer" represents the number of school supply transfers a household could receive, which was equal to the number of children ages 7-14. See the notes from Table 5 and Table 6 for an explanation of the dependent variables.

Table 14: IV Regression Effects of Different Treatments		
	(1)	(2)
Variables	Percent Enrolled	Pct Children Healthy
2001	0.086***	-0.033
	(0.018)	(0.022)
2002	0.089***	0.007
	(0.017)	(0.022)
Number of Children	0.048***	0.038***
	(0.008)	(0.009)
Base Transfer	0.011	0.037
	(0.035)	(0.045)
Per Child Transfer	-0.026	-0.030
	(0.017)	(0.022)
Effect of Base Transfer in 2001	0.052	0.010
	(0.032)	(0.041)
Effect of Base Transfer in 2002	0.036	0.026
	(0.032)	(0.041)
Effect of Per Child Transfer in 2001	-0.020	0.051*
	(0.022)	(0.028)
Effect of Per Child Transfer in 2001	-0.023	0.008
	(0.021)	(0.028)
Constant	0.086***	-0.033
	(0.018)	(0.022)
Observations	2,542	2,478
Number of Districts	42	42
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Notes: All columns represent IV regressions that include district fixed effects. The treatment indicator is an instrument for the take-up indicator. Observations in the regression are limited to households with children, so the variable "Base Transfer" is a binary variable indicating a household received the food security transfer, school attendance transfer, and one school supply transfer. "Per Child Transfer" represents the number of school supply transfers the household received, above the base transfer amount. "Number of Children" is an explanatory variable for the number of children ages 7 to 14. See the notes from Table 6 for an explanation of the dependent variables.