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Poverty, Growth and the Demand for Energy*

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Most future growth in energy use is forecast to come from the developing world. Understanding the likely pace and specific location of this growth is essential to inform decisions about energy infrastructure investments and to improve greenhouse gas emissions forecasts. We argue that countries with pro-poor economic growth will experience much larger increases in energy demand than countries where growth is more regressive. When poor households' incomes go up, their energy demand increases along the extensive margin as they buy energy-using assets for the first time. We also argue that the speed at which households come out of poverty affects their asset purchase decisions. We provide empirical support for these hypotheses by examining the causal impact of large increases in household income on asset accumulation and energy use in the context of Mexico's conditional cash transfer program. We find that transfers had a large effect on asset accumulation, and the effect is substantially greater when the cash is transferred over a shorter time period. Finally, we apply the lessons from the household analysis to aggregate energy forecast models using country-level panel data. We show that if a country's growth has been pro-poor, the responsiveness of energy use to income is nearly double that of a country with GDP growth that has been less favorable to the poor. These results suggest that not accounting for pro-poor growth could grossly underestimate future energy use.

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Today nearly 1.5 billion people live without electricity in their home, most of them in developing countries. This is likely to change in the near future as wide-scale poverty alleviation programs as well as continued economic growth lift the incomes of many of the world's poor. As incomes rise and electricity coverage expands, families formerly living in poverty will for the first time purchase refrigerators, water pumps, air conditioners, washing machines, and other household electrical appliances, as well light the rooms in their houses. In this paper, we argue that if the reduction in poverty is rapid, there could be a surge in the demand for energy. Such a large increase in energy consumption would have broad implications both for energy markets and for attempts to mitigate greenhouse gas emissions.

Understanding the likely growth in the demand for energy is critical for several reasons. First, investments in energy infrastructure require long lead times, and most governments and energy companies base their investment decisions on demand projections. Incorrect forecasts can lead to significant energy shortages that affect both productivity and welfare. On a global scale, larger than anticipated increases in the demand for energy can lead to significant increases in energy prices. Second, emissions from fossil-fuel use are a key contributor to climate change, so forecasting their likely path is important to understanding the range of possible effects of increased greenhouse gases in the atmosphere. Also, expected country-level emissions are critical inputs to any international climate agreement. Negotiations that aim to include developing countries can break down if the parties have different expectations about emissions paths.

Aggregate forecasts of energy use and greenhouse gas emissions suggest that the developing world will contribute to most of the growth over the next several decades (see, e.g., EIA 2010). These forecasts rely on assumptions on the form of the relationship between growth in GDP and energy use. In this paper, we argue that energy use may rise faster in response to growth in GDP when growth is pro-poor. Forecasts that ignore this may significantly underestimate the effect of GDP growth on energy use. [1]

Importantly, raising the income of the poor moves their demand for energy along the extensive margin as they buy energy-using assets for the first time. When a household acquires an energy-using asset, like a refrigerator or car, for the first time, its energy use increases substantially. While income growth will also affect energy consumption on the intensive margin

[1] For example, for its forecasts in the *International Energy Outlook 2010*, the U.S. Energy Information Administration uses elasticities of residential energy use to GDP that do not vary across countries in the developing world (EIA, 2010; personal communication).

(i.e., holding asset ownership fixed), the effect is small compared to the effect of accumulating more energy-using assets (i.e. the extensive margin). As households come out of poverty their demand moves mostly along the extensive margin leading to a large discrete jump in demand for energy.

This discrete jump is suggested by the well-documented S-shaped relationship between income and asset ownership (Letschert and McNeil, 2007; Dargay, Dermot and Sommer, 2007; Koptis and Cropper, 2005). Figure 1 uses household data from several Latin American countries to plot the share of households that own televisions, refrigerators and vehicles against household expenditures. While the cross-sectional relationships in Figure 1 are not necessarily causal, they do show that at a particular income level, which varies country by country, there is a large discrete jump in asset ownership and therefore in the demand for energy. Moreover, the greater the share of poor in a country and the more of them that come out of poverty together, the larger will be the jump in asset ownership and hence the bigger the associated bang in the demand for energy. Income growth beyond this point has less of an effect on energy demand as more of it occurs on the intensive as opposed to extensive margin.

We also show that the speed at which poor households come out of poverty affects their energy demand. In section 1, we present a simple two-period model of asset accumulation in the presence of liquidity constraints such as those faced by most families living in poverty. In this case, the intertemporal dynamics become important. We show that both income and savings accumulated from past income drive acquisition. As such, both income levels and income growth impact acquisition. In fact, with growing incomes, we show that it can be in a family's interest to reduce current consumption and save in order to acquire the asset faster in the future.

Our model has important implications for understanding the rate of lumpy asset acquisition in different countries. For example, it predicts that two countries that are at the same current level of income per capita may have different refrigerator ownership rates, with the country where recent growth was fast having a higher ownership rate than the country that grew more slowly. Our model also has implications for how poverty alleviation policies such as cash transfer programs affect asset accumulation. Specifically, we show that the rate of the payments should matter for asset acquisition rates. For instance, a program that distributes transfers on a quarterly basis may lead to more refrigerator acquisition than a program that distributes transfers bi-weekly.

In the empirical section of the paper, we examine the causal impact of different income streams on asset accumulation and energy use in the context of Mexico's conditional cash transfer program, Oportunidades. Oportunidades is one of the largest and most generous programs in the world, covering some 5 million Mexican families and providing benefits on the order of a 20 percent increase in income on average. The program provides a unique empirical setting to examine the relationship between income and asset acquisition both given the size of the cash transfers and given the fact that the program was rolled out randomly across villages.

Our results are consistent with the theoretical predictions of the model presented in Section 1. First, we find that the increase in income through the transfers had a large effect on asset accumulation. Specifically, we estimate that the median transfer amount led to a 7 percentage point increase in refrigerator ownership over a six-year period off a 4 percent base level of ownership. Second, we show that the effect on asset accumulation is substantially greater when the cash is transferred over a shorter timer period. Specifically, we estimate that the increase would have been nearly a 13 percentage points if the same level of benefits were delivered in 4 years instead of 6.

We also use data from Oportunidades to estimate a model of the demand for electricity, separately identifying the intensive and extensive margins. We find that most of the income effect is driven through asset ownership as opposed to the effect of income on the use of electricity conditional on asset ownership. This result is consistent with the our hypothesis that a fast reduction in poverty that leads to massive asset accumulation will have a greater effect on energy demand than a similar-sized increase in income for households that already own the assets.

Finally, we return to the aggregate energy forecast models that motivated this analysis, now incorporating the lessons learned from the causal, household-level data analysis. We use country-level panel data to describe the relationship between GDP and energy consumption. We show that if a country's growth has been pro-poor, the income elasticity of energy is nearly double that of a country with GDP growth that has been less favorable to the poor. These results suggest that not accounting for pro-poor growth would grossly underestimate future energy use.

The next section presents a simple two-period model of asset acquisition in the presence of borrowing constraints and varying rates of income growth. Section 2 describes the Oportunidades program, which we use to test the predictions of our model. Section 3 describes our data. We present results on asset acquisition by Oportunidades households in Section 4, and results on energy use, conditional on asset ownership in Section 5. Section 6 explores the implications of our model for cross-country estimates of the relationship between GDP and energy consumption. Finally, Section 7 concludes.

1. Conceptual Framework

Changes in income affect energy consumption through several channels. In their influential paper, Dubin and McFadden (1984) emphasized that energy consumption depends not only on the usual utility-maximization problem as a function of income and energy prices, but also on the household's current appliance holdings. A number of subsequent papers have analyzed appliance acquisitions, however, few researchers have analyzed the intertemporal dynamics that may influence these decisions. In fact, most researchers make assumptions that preclude intertemporal considerations, such as perfectly efficient capital markets.[2] While such assumptions may or may not be appropriate in the developed world, it is clear that capital constraints are significant among the poor in the developing world.[3]

To illustrate the impact of capital constraints and to motivate our empirical specification we develop a simple dynamic model of savings and durable good acquisition. We show that both current income as well as savings, accumulated from past income, drive acquisition. This implies that both current income and the speed at which income grows impact acquisition.

We present a simple dynamic, two-period model of durable good acquisition with credit constraints. Consumption is composed of two goods: a non-durable good, "food," that gives per-period utility $u_f(\cdot)$ with decreasing marginal utility, and a lumpy durable good, "refrigerator," that gives static per-period utility R if owned. A consumer has per period income Y, no access to credit, and the ability to save an amount $S \in [0, Y]$ from the first period to the second. For simplicity, there is no discounting, no interest, no complementarity between the two assets, and no on-going energy costs associated with owning the refrigerator.[4] We normalize the price of food to 1, and let the price of the refrigerator be P. In our context refrigerators are large purchases not easily made in one period. In fact, Gertler et al. (2011) show that low-income Mexican participants in the Oportunidades program allocate 76% of

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[2] For example, Dubin and McFadden (1984) and, more recently, Bento, Goulder, Jacobsen and von Haefen (2009) assume a perfectly competitive rental market for durables. This could exist in the presence of efficient capital markets and an efficient resale market. In recent work, Rapson (2011) and Schiraldi (2011) model dynamic considerations focusing on, respectively, consumer expectations about future energy (i.e., usage) prices and heterogeneous consumer transaction costs. No papers, of which we are aware, explicitly model credit constraints or analyze durable good acquisition in the developing world.

[3] Liquidity constraints and poverty has been explored in Banerjee and Newman (1993), Aghion and Bolton (1997), Lindh and Ohlsson (1998), Lloyd-Ellis and Bernhardt (2000), Banerjee (2004), and de Mel, McKenzie, and Woodruff (2008) amongst others.

[4] We show below that our main results are accentuated or unchanged if the two goods are complementary. Also, for simplicity, we abstract from the energy use by the appliance. One can consider R as the net benefit from using the asset including energy costs. Our main results are robust to this extension.

transfers towards time-specific consumption. Reflecting that, we assume that the refrigerator is too expensive to be purchased in one period: $Y < P$.[5]

Consumers vary in their valuations of the durable good R and their incomes Y. From decreasing marginal utility of food, it follows that for valuations of the durable good (income) below a threshold $\underline{R}$ ($\underline{Y}$) households do not purchase it. For valuations above that threshold, households save an amount $\frac{P}{2}$ in the first period and purchase the durable in the second period. Because of the credit constraints, households cannot borrow to purchase the durable in the first period. Under reasonable assumptions on the functional form of u_f and the distribution of R then the share of households with a given income who own a durable at the end of the second period is S-shaped.[6] Our assumptions thus far are consistent with Farrell (1954) and Bonus (1973) who assumed distributions of valuation parameters and income thresholds, respectively, and showed that these lead to the S-shaped logit or probit curves, for appliance ownership.

Figure 2 illustrates the threshold $\underline{R}$ ($\underline{Y}$) graphically. The figure plots a household's per-period marginal utility as a function of Y, so the area of the figure represents utility. As there is no discounting and no other changes to the household across periods, Figure 2 applies to both periods 1 and 2. The area under the rectangle with height $\frac{R}{P}$ and base $\frac{P}{2}$ will reflect the per-period utility the household receives if it saves $\frac{P}{2}$ in period 1 and purchases the refrigerator in period 2. The red shaded area reflects the lost utility of food from purchasing the refrigerator. As this is exactly equal to the green shaded area, which captures additional utility from the refrigerator, this household will be just indifferent between saving to acquire the refrigerator and consuming only food. Households with higher valuations of the refrigerator (higher R, i.e., taller rectangles) or higher incomes (higher Y, i.e. rectangles shifted to the right) and therefore lower marginal utility of food, will strictly prefer to purchase the refrigerator.

We next extend this framework to consider changes in income period-to-period. Since our empirical setting involves a conditional cash transfer program, we describe the model in terms of transfer payments, although those could equally be interpreted as expected changes in income. Consider an increase in household income by a per-period transfer, T, such that the refrigerator is still not affordable in one period ($Y + T < P$). This increase will cause more

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[5] This simplifies the analysis because no household can purchase the good in one period. Our results are robust to relaxing this assumption. Alternatively, we can impose restrictions on $u_f(\cdot)$ such that even if a consumer was able to purchase the refrigerator in one period he or she would choose not to (e.g., by imposing that the marginal utility from subsistence food levels are sufficiently high). Those restrictions yield similar results.

[6] For example, if u_f is log, and R is distributed log normally.

households to acquire the refrigerator. Specifically, all households with purchasing thresholds between $\underline{Y} - T$ and $\underline{Y}$ now purchase. This leads to our first empirical prediction:

Prediction 1: The larger the increase in income, the more likely the household is to purchase a durable.

We next consider differences in the rates of change in income. For example, in our empirical setting, not all households receive transfers at the same rate. Consider a household that receives the same transfers in total $(2T)$, but receives transfer $\mathrm{L} < T$ in the first period and $\mathrm{H} = 2\mathrm{T} - \mathrm{L}$ in the second period. Suppose that $\mathrm{H} - \mathrm{L} < P$. Observe that if the household saves and purchases the durable the path of consumption is exactly as it was for the household with even transfers (*T*), but savings are lower. However, the utility from not purchasing is reduced because consumption is uneven. The household would prefer to shift consumption from period 2 to period 1 but cannot because of the credit constraints.[7] Instead, additional households find it preferable to save and purchase the durable good. If $\mathrm{H} - \mathrm{L} \geq \mathrm{P}$, households do not save and the credit constraint prevents consumption smoothing. Some households who would prefer to consume more than $Y + L$ and not purchase the asset in the case of even transfers now find it optimal to purchase the durable in the second period.

This effect is represented in Figure 3. Figure 3 is identical to Figure 2, except that total income is equal to $Y + T$ instead of Y. Figure 3 establishes that for a low enough valuation of the refrigerator (R^L), the household will not purchase the refrigerator in the even transfer case. Compare this to the case where transfers are uneven, as depicted in Figure 4. In order to purchase the refrigerator, the household now must save only amount S in period 1. The lost utility from forgoing food consumption (net of the gain from the refrigerator in period 2) is indicated by the dark shaded area. In period 2, the household also forgoes the wedge under the marginal utility of food curve (dark shaded area plus dotted area), but gains the light shaded area. As drawn, these area are just equal, suggesting that the household with refrigerator valuation R^L will purchase the refrigerator in the increasing transfer case, whereas it would not have purchased the refrigerator in the even transfer case.

This result combines a "forced savings" and a "complementary savings" effect. A household whose transfers are delayed has the same income in each period as a household with even transfers that was forced to save $T - L$. Expecting that it will receive these forced savings in the second period, it may be willing to complement the forced savings and save an additional

[7] While optimal transfer design is not the focus of this simplified model, it is clear that front loading all transfers in the first period is first best in this simplified context. However, we restrict attention to higher second period transfers consistent with real world transfer programs and the effects of growth.

amount (S in Figure 4) in order to purchase the lumpy asset in period 2. These two effects cause some households who would not have purchased the refrigerator with evenly spaced transfers to buy the asset if transfers are delayed. This provides a second empirical prediction.

Prediction 2: Holding cumulative income fixed, households who gain more income in the second period (i.e., for whom income growth is faster) will be more likely to acquire assets.[8]

Finally, the model predicts that there will be an interaction effect between the size of cumulative transfers (T) and the timing of transfers. Specifically, hold the ratio of first to second period transfers, α, constant (the ratio, α, is 1 in the even-transfers case, and between 0 and 1 in the delay case). Consider an increase of total transfers from $2T$ to $2(T + T')$. As long as delayed households still save to purchase ($H + (1 - \alpha)2T' - L - \alpha 2T' < P$ is a sufficient condition) and the ratio of transfers is small enough, then the increase in total transfers by $2T'$ decreases the valuation threshold $\underline{R}$ for delayed households more than households with constant transfers.

To understand this mechanism, first note that if the household saves and purchases the durable good it has the same consumption pattern regardless of the pattern of transfers. So we only need to show that the delayed transfers receive a smaller increase in utility than the even transfers group from the increase in transfers. This follows from decreasing marginal utility if α is sufficiently small.[9] This leads our third empirical prediction:

Prediction 3: The effect of additional income on asset acquisition will be larger for households whose income is growing quickly.

While the model and these three predictions are described in terms of transfers expected by the households, it could easily be interpreted as additional income expected from economic

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[8] Complementarity between the two goods – food and refrigerators – will amplify the results of prediction 1 and leaves unchanged predictions 2 and 3. Prediction 1 is magnified because the benefit from the refrigerator then rises with income. Predictions 2 and 3 are unchanged because households who acquire and have the same total income have the same consumption path regardless of the path of their income. So complementarity changes the benefit of acquisition in the same way for all acquirers and does not affect the benefit of not purchasing the refrigerator.

[9] Formally, we prove this as follows: The increase in utility from not purchasing in the even case is $2\left(u_f(T + T') - ufT\right.$. For the uneven case it is $ufL{+}\alpha 2T'{-}ufL{+}ufH{+}1{-}\alpha 2T'{-}ufH$. As $\alpha \to 0$, $ufL{+}\alpha 2T'{-}ufL \to 0$ and, from decreasing marginal utility $2\left(u_f(T + T') - u_f(T)\right) > u_f(H + 2T') - u_f(H) > u_f(H + (1 - \alpha)2T') - u_f(H)$. So, by continuity, for a sufficiently small α: $2\left(u_f(T + T') - u_f(T)\right) > u_f(L + \alpha 2T') - u_f(L) + u_f(H + (1 - \alpha 2T'{-}ufH$.

growth.[10] The model predicts that holding constant total increases in income over some time period, asset acquisition will depend on the pattern of growth. For example, in Figure 5 at point t^*, the integral under the green line is equal to the integral under the red line, so the cumulative income of two sets of households facing these income trajectories would be the same. Our model suggests that households whose income followed the green line may be more likely to acquire a refrigerator in period t^* because of the forced and complementary savings results. While the incomes of households facing the green line are growing slowly, income levels may be so low that very few are willing to purchase a refrigerator. Fast income growth at the higher income levels may lead to more asset acquisition following prediction 2 above.

Also, while the model has two periods, the underlying mechanisms are quite general. Any multiple period model of asset acquisition with increasing income has three phases: a savings phase in which the asset is not owned and weakly positive amounts are saved, an endogenously determined purchase period when the asset is purchased, and a utilization phase in which the asset is enjoyed. Our model represents the first two phases, with the final phase held constant. With more periods, the comparative statics remain: The wealthier is the household, the more likely they are to purchase (and sooner). The more uneven is income in the savings and purchase period, the more likely the consumer is to purchase. Similarly, the higher future income is, the more likely the consumer is to delay purchasing. Finally, holding fixed the purchase period, the income increases lead to more acquisition the more the savings phase and purchase period are uneven.

A multi-period model also suggests interesting conclusions about the shape of the S-curve. For instance, if households are credit constrained and incomes are growing, the range of incomes where asset ownership is increasing narrows. Thus, the S-curve is steeper than it would be with either no income growth or no credit constraints. This is because expected income growth leads some poor households to delay asset purchases while richer households will not be constrained. As a result, in intermediate periods, there are poor households who are delaying their purchases because of growth in future incomes.

Our model has important implications for thinking about the rate of lumpy asset acquisition in different countries. Cross-sectionally, it predicts that two countries that are at the same current level of income per capita may have very different refrigerator ownership rates because of the timing and distribution of income growth. In terms of conditional cash transfer programs, our model also suggests that the rate of the payments may matter for asset acquisition rates. For

[10] All of the comparative statics hold if households are uncertain about second period income, but the predictions are in terms of FOSD distributions of future income instead of higher income.

instance, a program that distributes transfers on a quarterly basis may lead to more refrigerate acquisition than a program that distributes transfers bi-weekly.

2. Empirical Context

We analyze asset acquisition in the context of Oportunidades, a conditional cash transfer program in Mexico that was designed to break the intergenerational transmission of poverty. The program, originally called PROGRESA, aims to alleviate current and future poverty by giving parents financial incentives, in cash, to invest in the human capital of their children. Oportunidades was conceived as a temporary program that would become obsolete over three to four decades as soon as the initial generation of beneficiary children reached adulthood. The program, which started in 1997, is one of the largest conditional cash transfer programs in the world distributing approximately 4 billion US dollars annually to some 5 million beneficiary households.[11]

a. Program Benefits

Cash transfers from Oportunidades are given to the female head of the household, and are conditional on children attending school, family members obtaining preventive medical care and attending "pláticas" or education talks on health-related topics.[12] The cash transfers come bimonthly according to two criteria. First, all beneficiary households receive a fixed food stipend conditional on family members obtaining preventive medical care. Program designers expected families to spend this stipend on more and better nutrition.

Households also receive educational scholarships conditional on children attending school a minimum of 85 percent of the time and not repeating a grade more than twice. The educational stipend is provided for each child less than 18 years old enrolled in school between the third grade of primary school and the third grade of junior high (12^{th} grade) and varies by grade and gender. It rises substantially after graduation from primary school and is higher for girls than boys during high school. Beneficiary children also receive money for school supplies once or twice a year. Only children who were living in the household when the program started are eligible for the school transfers in order to prevent migration into the household. Total transfers for any given household are capped at a pre-determined upper limit.

[11] http://www.oportunidades.gob.mx/Portal/wb/Web/design_and_operation

[12] Compliance was verified through the clinics and schools, who certified whether households actually completed the required health care visits and whether kids attended schools. While full compliance varied, only about 1 percent of households were denied the cash transfer completely for non-compliance.

Table 1 describes the benefits to which beneficiary households were entitled in 2003. While the benefit levels and the grades covered have changed over the course of the program, its basic structure has not. In 2003, the basic (called ``alimentary'' or ``food'') support was 155 pesos per bi-month. The bi-monthly per child educational scholarship in 2003 ranged between 105 pesos for children in the third grade to 655 pesos for teenage girls in twelfth grade. Finally, Oportunidades also provides a yearly stipend to buy school supplies for children who do not get them at school.

As Table 1 documents, differently composed households are eligible to receive different transfer amounts. For example, households with more female children enrolled in higher grades are eligible for larger educational stipends than similar households with children enrolled in lower levels, or with more male children. We can compute the maximum potential bimonthly transfer for a family by applying the values from Table 1 to the following formula:

$$PT_{it} = \min\left(T_t^{max}, BT_t + \sum_s ST_{st} NK_{sit}\right) \quad (1)$$

where PT_{it} is the maximum potential transfer that could be received by household *i* in period *t*, T_t^{max} is the program cap on benefits, BT_t is the basic transfer amount that all households receive (the food support), ST_{st} is the transfer conditional on a child of type *s* (i.e. based on grade and sex) attending school, and NK_{sit} is the number of children of type *s* in household *i* at baseline aged forwarded to period *t*. Because of the cap on total benefits, potential transfers are a nonlinear function of the number of children at baseline who could attend the grades eligible for the educational scholarships in period *t*.

The actual transfers received by a household are less than the potential amount if some children do not attend school. Thus the actual bimonthly transfer amount received by household *i* at each time *t*, AT_{it}, is computed by applying the values from Table 1 to the following formula:

$$\boldsymbol{AT_{it} = min\left(T_t^{max}, BT_i + \sum_s ST_{it} K_{sit}\right)} \quad (2)$$

where K_{sit} is the number of children of type *s* in household *i actually* attending school in period *t.*

b. Eligibility, Enrollment and Duration of Benefits

When Oportunidades was first rolled out in rural areas in 1997, program eligibility was determined in two stages (Skoufias et al. 2001). First, the program identified underserved or marginalized communities and then identified low-income households within those communities. Selection criteria for marginalized communities were based on the proportion of households living in very poor conditions, identified by using data from the 1995 census (*Conteo de Población y Vivienda*).

To select eligible households within marginalized communities, Oportunidades conducted a socio-economic survey of all households, the *Encuesta de Características Socioeconómicas de los Hogares* (ENCASEH). The Mexican government used the ENCASEH to construct a proxy means index and classify households as eligible for treatment ("poor") or ineligible ("non-poor"). The original classification scheme designated approximately 52 percent of households as eligible ("poor") (Hoddinott and Skoufias 2004).

Eligible households were offered Oportunidades and a majority (90 percent) enrolled in the program (Gertler et al, 2011). Once enrolled, households received benefits for a three-year period conditional on meeting the program requirements. New households were not able to enroll until the next certification period, which prevented migration into treatment communities for Oportunidades benefits. Households in rural areas were "recertified" (re-assessed with a proxy means test) after three years on the program to determine future eligibility. If a household was recertified as eligible, it would continue receiving benefits. If not recertified, the household was guaranteed six more years of support before transitioning off the program. Thus, households could expect a minimum of nine years of benefits upon enrollment (Oportunidades 2003).

c. Experimental Design and Data

The data used in this study were generated for program evaluation. In order to evaluate the program, the government randomly chose 320 early intervention and 186 late intervention communities in seven states. Eligible households in the early intervention communities received benefits starting in April 1998, and those households in the late intervention communities did not receive benefits until October 1999. No sites were told in advance that they would be participating in the program, information about timing of program roll-out was not made publicly available, and there is no evidence of anticipatory behavior (Attanasio et al., 2011). Our analysis focuses on these 506 communities and the panel of approximately 10,000 households that were surveyed from 1997 through 2007.

Treatment and control households were similar on a wide array of measured characteristics. Table 2 and Appendix Table 2 summarize a number of different household-level attributes separately for control and treated households. For nearly all of the variables, the means are statistically indistinguishable across the treated and control groups, suggesting that the randomization successfully created comparable groups.

The data used in this study comes from the baseline ENCASEH, described above, and the Oportunidades Evaluation Survey (ENCEL), which is a panel data set that was gathered over six rounds. The first one was gathered a year after the program started, during the fall of 1998 and the second one in 1999. Similarly, during 2000 two different surveys were gathered, one in March 2000 and the other one in November 2000. The fifth one was gathered in 2003 and the last one was recently done in 2007.

The evaluation surveys gather information on a number of potential issues that the program may affect, including household and household members' characteristics, income and labor supply, expenditure, health and nutritional status, education, among others. Of particular importance for this study, the survey gathers information on energy-powered household durable asset possession, such as refrigerators, gas stoves, televisions, and washing machines. For 2007, the evaluation survey also included questions on electricity expenditures, which we analyze in Section 5.

3. Empirical Model and Identifying Assumptions

In this section, we describe an empirical model that allows us to test predictions of the conceptual framework in section 1. Specifically, we examine the causal relationship between the cash transfers, which is the exogenous shock to income in the Oportunidades context, and asset accumulation. Durable asset purchases are discrete events that occur very infrequently. Hence, we model the decision to purchase an asset such as a refrigerator as the probability of purchasing the asset in a particular period given that the household has not purchased the asset so far. Consistent with our conceptual framework, the cost of the assets we consider (e.g. refrigerators) is substantially higher than the monthly transfer amount.[13] To test predictions 1-3, we will examine the impact of cumulative transfers, the rate of change of transfer payments and their interaction on asset acquisition.

We estimate a linear discrete-time hazard specification that takes advantage of the panel structure of our data. Specifically, we estimate versions of the following equation:

$$h(a_{it}) = \Pr(a_{it} = 1 | a_{it-1} = 0) = \alpha_0 + \alpha_1 cumulative\ \tau_{it} + \alpha_2 treated_i + \alpha_3 treated_i \times cumulative\ \tau_{it} + \beta X_i + R_{rt} + v_{it} \quad (3)$$

[13] We examined the prices of a set of refrigerators in Mexico from PROFECO, the Mexican Federal Bureau of Consumer Interests. The price of the cheapest refrigerator in 2003 was nearly double the monthly maximum transfer amount.

where $h(a_{it})$ is the probability that household *i* adopts appliance a in period t, conditional on not having adopted it in period t-1. We specify this as a function of cumulative Oportunidades cash transfers for household i in period t, $cumulative\, \tau_{it}$, a dummy indicating that the household was in a treated community, $treated_i$, meaning that it began receiving transfers 18 months before the households in the control communities and the interaction between the treatment dummy and cumulative transfers. X_i is a vector of control variables, including household characteristics. In some specifications, we include a household fixed effect instead of the control variables. The R_{rt} is a vector of region-by-period dummies, separately estimated for seven regions for five periods. These help account for any region-specific changes in refrigerator or electricity prices.

The model in Section 1 predicts that α_1 will be positive (prediction 1) while α_2 and α_3 will be negative (predictions 2 and 3). Treated households began receiving their transfers eighteen months earlier than control households, so, conditional on having the same level of cumulative transfers as a control household, the growth in their cumulative transfers will have been slow and steady, akin to the red line in Figure 5. It is instructive to consider what variation in our data identifies α_1, α_2 and α_3, particularly as we are using both randomized and non-randomized variation to establish our counterfactual outcome.

First, variation in $treated_i$, is generated by the randomization that determined which households started receiving transfers early versus later. We see variation in $cumulative\, \tau_{it}$, both within a given household over time and across households. The variation in cumulative transfers at a point in time depends on when the family entered the program and the rate of accumulation since entry. The time the household was incorporated into the program was randomized, whereas the rates at which a household's cumulative transfers change over time is a nonlinear function of the household's family structure, where the nonlinearity is induced both by the variation in transfer rates by age and gender of the children, and by the maximum transfer rates. Rates of accumulation within a household vary with time as younger children age and receive more transfers and older children age out of the program. So long as the variation in the transfer amounts and hence the rate of change of cumulative transfers is not correlated with the propensity to buy an appliance, our specification will yield unbiased estimates.

As the actual cumulative transfers that a family receives are determined by choices about whether or not to keep children in school, it is conceivable that the decision to purchase an appliance would be correlated with household-level shocks that altered the parameters of these choices. For instance, if the household experienced a large negative income shock on its non-transfer income, it might decide to take children out of school so that they could work. The negative income shock could simultaneously make the household less likely to acquire an appliance. This would lead to a positive bias in the coefficient on actual cumulative transfers. Parker and Skoufias (2000, 2001) find that the program reduces child labor and increases enrollment in junior high (secondary) schools as the opportunity cost of these children being in the labor force is now higher. Schultz (2004) also finds positive effects for primary school and junior high school enrollment for boys and girls. These findings suggest that economic incentives influence schooling decisions, so concerns about potential endogeneity are real.

We address this problem by instrumenting for cumulative transfers with the potential cumulative transfers that a family could achieve if the maximum number of eligible children in the household attended school. At each time t, we compute a family's maximum potential transfer assuming that all eligible children that were enrolled at baseline have advanced one grade per year and met attendance thresholds. Potential transfers are a nonlinear function of the number of children at baseline who could be enrolled in school in period t. This is true because total benefits are capped, the transfer schedule is nonlinear (as in Table 1) and transfers are zero for the first 3 years of school.

Potential cumulative transfers are likely to be valid instruments for three reasons. First, they are a strong predictor of the actual transfers. Second, maximum potential transfers are unlikely to be correlated with asset accumulation via other pathways such as additional income sources. Indeed, they are uncorrelated with changes in children's labor supply due to the program as they are computed assuming that all eligible children enrolled at baseline are still in school and have advanced one school grade per school year. Nonetheless, the transfers could also affect leisure by reducing adult labor supply, which would reduce household income and therefore a household's propensity to purchase assets. Everything else held constant, this would imply a downward biased estimate of α_1. Parker and Skoufias (2000) show that there is no effect of the program on adult labor supply, so we can safely assume that the transfer variables are not correlated with other earned sources of income.

Because we are controlling for cumulative transfers, α_2 describes differences in refrigerator acquisitions between treated and control households who have had the same level of cumulative transfers. To have the same level of cumulative transfers as a treated household, a control household needs to have higher transfer rates, and thus more, older, or more female children. This difference in the age structures and gender of the children could influence the value of a particular appliance to the household. This could lead to a bias as the cumulative transfer is determined by the demographic structure of the household. However, maximum potential transfers are not strongly correlated with the number of children in the household because of the nonlinear allocation rule. Let's imagine the following extreme situations: a household with 3 girls in grade 2 of primary school, and a household with 3 girls in grade 2 of junior high school. Both households have 3 female children but while the first household will receive no school transfers, the latter household will receive a large monthly transfer. In addition, families with 4 or more children in junior high school would receive the same transfer amount as the latter household because the cap on total benefits would be binding. Thus, we are able to explicitly control for household size and the number of children in the household in the empirical specification and still identify the cumulative transfer variable.

Also, we will present results from placebo tests that suggest that the nonlinear function that translates family structure to cumulative transfers does not predict appliance ownership at baseline (i.e., before the program started). So, as long as changes in the propensity to buy a refrigerator were similar across households with different family structures, our specification will yield unbiased estimates of α_2.

To obtain valid estimates of α_2 and α_3, which examine the relationship between the timing of the transfers and appliance purchases, we would like to be able to observe households who have received the same level of cumulative transfers, some of whom started receiving the transfers early as part of the treatment group and some of whom started receiving the transfers later as part of the control group. While the specification above controls for cumulative transfers, we want to be sure that the distributions of cumulative transfer levels overlap between the treatment and control groups. Otherwise, α_2 and α_3 could simply be picking up nonlinearities in the relationship between cumulative transfers and appliance acquisition.

Table 3 reports cumulative transfer amounts over time for households in the treatment and control groups that are at different parts of the transfer distribution. We see that households at the 75th percentile of the control group had higher cumulative transfer amounts than households at the 25th percentile of the treatment group by late 2000 and higher amounts than households at the median of the treatment group by 2003. This suggests that we will have considerable overlap between the distributions of cumulative transfers by 2003.

Although there will be more overlap in the distributions in later years, we focus on observations in 2003, as in later years, differences between the treatment and control groups will be smaller relative to their accumulated transfers. For example, Table 3 shows that by 2007 the treated group's median actual transfers only exceeded the control group's median actual transfers by less than ten percent, while in 2003, the treated group's median was almost 25 percent higher. If we include later years in our estimates of equation (3), the coefficients on the treated dummy and the interaction term (α_2 and α_3) are negative but are attenuated to zero, as we would expect with more noise relative to systematic differences between the groups.

Finally, we note that previous related papers have examined the impact of income on appliance acquisitions (Dubin and McFadden, 1984) and ownership (Dargay, Dermot and Sommer, 2007). All of these papers have relied on cross-sectional variation in income and have limited controls for household demographics, meaning that unobserved differences may be correlated with income and taste for appliances. One substantial advantage of our empirical setting is that we can take advantage of the large shocks to income that households received via the transfers, and we use both within-household differences brought on by the nonlinear transfer schedule and cross-household difference driven, among other things, by randomization. We opt not to estimate an income elasticity for several reasons. First, we are primarily interested in the timing of income shocks and not the absolute level of income. Also, our data best measure transfers and not total income, as the Oportunidades households have substantial informal and non-monetary income sources. By examining household responses to transfers, however, we can identify the effects predicted by the model in Section 1.

4. Empirical Results on Asset Acquisition

To get a preliminary understanding of the variation in our data, Figure 6 plots cross-sectional refrigerator ownership as of 2003 on cumulative transfers through 2003. Following prediction 1 we expect upward-sloping ownership curves ($\alpha_1 > 0$). Prediction 2 suggests that the line for treated households is below the line for control households ($\alpha_2 < 0$), while Prediction 3 implies that the line for treated households is less steep than the line for control households ($\alpha_3 < 0$). We see all three of these relationships in the figure. By estimating the discrete-time hazards described in (3), however, we can include controls and, in some specifications, use within-household variation.

Table 4 presents several specifications of equation (3) for refrigerators, by far the largest and most expensive asset. We estimate (3) using a linear model and report robust standard errors clustered at the village level, i.e. the level of randomization. All specifications include state-by-round fixed effects. Columns (1) through (4) include a number of household controls (detailed in the footnotes to the table) while column (5) includes household fixed effects.

The first column includes only cumulative transfers. The coefficient (α_1) is positive as predicted, and highly statistically significant. The magnitude of the coefficient suggests that for every ten thousand pesos increase in a household's cumulative transfers, the probability that it acquires a refrigerator goes up by about 2 percent. At baseline, 3.8 percent of the households owned refrigerators.

When we include the treated dummy in column (2), the coefficient on cumulative transfers is virtually unchanged, and the coefficient on treated is negative, as predicted, and highly significant. The magnitude suggests that receiving transfers later as part of the control group is equivalent to an almost 6,000 pesos increase in cumulative transfers. When we include the interaction between treated and cumulative transfers in column (3), the interaction term is negative and statistically different from zero, while the coefficient on the treatment dummy drops in absolute value. As the coefficient on the treated dummy in column (3) reflects the treated effect at zero cumulative transfers, which is outside the range of our data, we also report the net treated effect at median 2003 transfers.

Column (4) instruments for both cumulative transfers and cumulative transfers x treated with a household's potential cumulative transfers in a given period and potential cumulative transfers x treated. The instruments are extremely strong, and the first-stage f-statistics, reported at the bottom of column (4) exceed one thousand. The coefficient estimates are very similar to the OLS estimates in column (3). If anything, the coefficient on cumulative transfers is slightly higher.

In column (5), we include household fixed effects, which allow us to control for any remaining differences across households not picked up by the household controls included in columns (1) through (4). For example, while the household controls include the number of children, we do not include precise variables measuring their exact gender and age makeup. For example, if across households with the same number of children, the households with older girls had higher valuations for refrigerators than the households with younger boys, the coefficient on cumulative transfers might be biased positive. This could in turn lead to a negative bias on the treated dummy as, for a given level of cumulative transfers, the treated households are more likely to be comprised of young boys.

With household fixed-effects, we can control for any time-invariant differences within a household. We have within-household variation in cumulative transfers because of the nonlinear increases in transfers depicted in Table 1 as well as because of children aging into or out of the program. We cannot, however, estimate the treatment dummy as this is a time-invariant household characteristic. The specification in column (5) uses instrumental variables estimation, and is therefore comparable to the results in column (4). The coefficient estimates on cumulative transfers and cumulative transfers x treated are remarkably similar across columns (4) and (5) suggesting that our household controls pick up most cross-household differences in tastes.

Table 5 presents results from specifications comparable to those reported in columns (4) and (5) of Table 4 for two additional appliances that require large upfront investments, as well as the number of rooms in the household's dwelling, which we use as an indicator for the amount of lighting. For comparability, we reproduce the results for refrigerators in the top two rows of the table. The net treated effect at the mean level of cumulative transfers is negative and statistically significant for both of the additional assets. For the number of rooms, the net effect is positive, though we only have data for 2003, so the specification is estimated using only cross-sectional variation. Also, it is less clear that a new room requires a large discrete payment, as low-income households in Mexico often accumulate the materials in small amounts over a long period of time. Treated x cumulative transfers is negative across both specifications for all the additional specifications in Table 5. It is statistically smaller than zero for stoves. While it is hard to draw strong inferences from only a few assets, the results in Table 5 are generally supportive of our model.

a. Alternative Explanations

The results presented so far are consistent with the model presented in Section 1. They suggest that households are more likely to acquire appliances the higher is their transfer income and the lumpier were their transfer payments. Also, the effects of the lumpy payments are stronger at higher cumulative transfer amounts.

The results may also be consistent with other explanations, however, the most obvious of which we address in this subsection. First, the treatment dummy is identified by considering households with the same cumulative income, some of whom received transfers steadily at low rates and some of whom received no transfers for eighteen months and then high transfers once they began the program. These households are by construction different from one another, so a natural question is whether the differences are systematically correlated with the household's value for appliances. The fact that the specifications estimated with household fixed effects are similar to the results that simply include household-level controls gives us some reassurance that the differences across households are not driving our results.

As an additional robustness check, we estimated cross-sectional specifications using data from the baseline survey that was conducted in 1997, before any of the households were receiving transfers. These specifications test whether the particular nonlinear relationship between family structure and transfers embodied in cumulative transfers through 2003 predicts appliance ownership. The results are presented in the left-hand column of Table 6. Each specification is estimated using instrumental variables and including household controls, comparable to the specification reported in column (4) of Table 4. The coefficients on cumulative transfers, treated and treated x cumulative transfers are insignificantly different from zero, except for cumulative transfers for stoves. This provides additional reassurance that the differential transfer rates experienced by households under Oportunidades are not systematically correlated with the propensity to acquire appliances.

Since the specifications at baseline in Table 6 are cross sectional while the results in Table 4 and Table 5 were estimated as discrete time hazards, we estimated similar specifications for 2003 by way of comparison. These are presented to the right of the baseline specifications for each appliance. These specifications confirm the results in Table 4 and Table 5. In all specifications, the coefficient on cumulative transfers is positive and usually significant, while treated and treated x cumulative transfers are generally negative.

A second concern is that treated status is an indicator for lower expected future transfers and thus the negative coefficient simply represents lower expected income. Specifically, among treated and control households with the same cumulative transfer levels at a given point in time, treated households might expect lower transfers in the future since their average transfer rate is lower than the control households.[14] For instance, control households may simply have more girls than treated households who are at the same level of cumulative transfers in 2003. Table 7 presents results from several specifications that include future transfers as additional explanatory variables. Column (1) of Table 7 reproduces column (4) of Table 4, and then columns (2) and (3) add information about the household's actual future transfers through 2007. With rational expectations, realized future transfers proxy for expected transfers. The alternative hypothesis put forward above would suggest that the coefficient on future transfers should be positive. In fact, we find that it is either undetectably different from zero or statistically significant and negative. A negative coefficient is consistent with the intertemporal optimization underlying our framework in section 1, as it suggests that households expecting higher transfers in the future are less likely to buy an asset now, presumably because they are waiting to buy it when their income is higher.

We also considered the possibility that the difference in acquisition is driven by a lack of self-control. Particularly, the logic of the intertemporal optimization in Section 1 suggests that treated households have the ability to replicate through saving the time path of transfers of the control households, but instead choose to allocate transfers differently. However, if these

[14] Because transfer rates vary over time within a household, increasing as younger children age and decreasing as older children age out, it is possible that a treated household will have the same cumulative transfers as a control household, but will have higher expected transfers. For example, the treated household could have begun with younger children, accumulating slowly, while the control household began with older children – accumulating quickly at first, but then slowly later when its children age out of the program.

households lack self-control or are otherwise myopic it is possible that the temporal effects we observe are the consequence of households spending the transfers as they receive them, rather than optimizing considering both current and future transfers. The negative coefficients on future transfers in Table 7, however, are not consistent with lack of self-control or other myopic behavior.

A final concern is that control households might have earned more non-transfer income than treated households during the period before they began receiving transfers, for instance from child labor, which is not reflected in the potential transfers instrument. To allow for this, we estimated the household fixed effect model of Table 4, column (5), excluding the rounds in which the control households did not receive transfers. The estimates are not statistically different from column (5).[15]

5. Empirical Results on Energy Consumption

We next examine the relationship between income and household energy use. Specifically, we examine whether higher household income, caused by Oportunidades transfers, leads to increased energy consumption conditional on appliance holdings. We compare the conditional income effect to estimates of the effect of an appliance acquisition on energy use. While previous research suggests that the response of energy use to income conditional on assets is small, those studies are of the developed world.[16] Our data allow us to obtain estimates from low-income households in Mexico.

Using cross-sectional data from the 2007 ENCEL, we estimate:

$$electricity\ use_i = \beta_1 + \beta_2 Current\ transfers_i + \beta_3 a_i + \beta_4 X_i + \delta_v + \epsilon_i \quad (4)$$

where *electricity use$_i$* is household i's bi-monthly expenditure for electricity and *current transfers$_i$* is the average Oportunidades bi-monthly cash transfer in 2007 for household i. a_i is a measure of assets – either a variable that takes a value of either 0 or 1 to indicate refrigerator

[15] The coefficient on Cumulative Transfers is 0.065*** [0.008] and the coefficient on Cumulative Transfers X Treated is -0.014* [0.008]. Note that this specification also addresses concerns about whether the control households would behave according to the framework presented in section 1 if they did not anticipate program transfers for the 18 months before they were enrolled. Without anticipating transfers, the model still predicts increased asset acquisitions via the forced savings effect, but households would not have complementary savings from the period in which they were not enrolled.

[16] See, e.g., Dubin and McFadden, 1984; Hsiao and Mountain, 1985, Reiss and White, 2008.

ownership by household i , or an energy-use-weighted sum of electricity appliances owned by household i (described in the appendix).[17] X_i is a vector of household covariates, δ_v captures village-level fixed effects and ε_i is the error term.[18]

Note that we observe only whether or not a household owns a particular type of appliance (e.g. a refrigerator or washing machine) and have no information on its purchase or usage price, nor on any of its other characteristics. We do estimate village-fixed effects, which control for much of the cross-household variation in energy prices, as electricity prices in Mexico are regulated at the regional level. We also observe electricity use only once, in 2007, so our analysis of energy use is purely cross-sectional.

As described in Section 3, transfers vary across households as a nonlinear function of family structure. So long as the variation in the current transfer amounts is not correlated with the propensity to use energy or own an appliance, conditional on household controls, our specification will yield unbiased estimates. On the other hand, unobservable household characteristics may be driving appliance use and acquisition decisions. For example, a negative health shock within a household may increase the utility from a gas stove, and may also make the household more likely to use it.

To address the endogeneity concerns, we instrument for appliance ownership using specifications analogous to those described in the above section. We instrument for asset ownership with Maximum Potential Cumulative transfers, Maximum Potential Cumulative transfers interacted with Treatment status, and asset ownership in 1997.[19] This means that our first stage is essentially a cross-sectional version of the asset acquisition specifications discussed in Section 4. As such, our specification is identified by variation in potential cumulative transfer amounts and randomized treatment. So long as that is not correlated with energy utilization, our specification yields unbiased estimates.

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[17] With some assets, such as air conditioning, fans, and lighting, the consumer has considerable latitude to adjust usage, suggesting that income might affect that decision. For other assets, like water heaters and refrigerators, the consumer has less control over how much energy it uses.

[18] Although this equation is similar to specifications used to estimate an income elasticity, we did not use a log-log specification since transfers represent a varying share of total income (transfer plus earned) across our households. Results are qualitatively similar when we use a log-log specification.

[19] We do not use Treatment status by itself as an instrument because it is collinear with the village fixed effects. We obtain similar results if we estimate state instead of village fixed effects and include Treatment status as an instrument.

It is conceivable, however, that there is additional endogeniety if the age structure and gender of children influences the value of using and/or owning assets. Because our data is only cross-sectional, we cannot employ the fixed-effects approach in the acquisition estimation. However, the similarity between the estimates of the asset acquisition models using household controls and those using fixed effects suggest that the included household controls capture the relevant variation. So, we include the same set of household controls as we did in Tables 4-7. In addition, because of the same endogeniety concern described with respect to asset acquisition regarding actual transfers, we instrument for Current Transfers using Potential Current Transfers.

Table 8 presents several specifications of equation (4) using a linear model. As above, we report robust standard errors clustered at the village level. Columns (1) and (2) do not control for asset ownership, and estimates marginal propensity to consume electricity out of transfers of about 1%. Columns (3) adds a control for asset ownership, and the coefficient suggests that for every additional aggregated 100-kWh per month of energy-using assets a household owns, bi-monthly energy expenditure increases by 43 pesos.[20] Once we control for assets, the marginal propensity to consume electricity is not significantly different than zero. When we instrument to allow for potential endogeniety, the estimated effects of asset ownership are larger and the marginal propensity to consume is even smaller. Columns (5) and (6) report the same specifications but replace the asset aggregate with a dummy for refrigerator ownership with similar results. Using the coefficients in Column (5) adding a refrigerator to a household has the equivalent energy expenditure effect of increasing their transfers by 7,900 pesos. These results are consistent with the hypothesis that the main pathway by which increases in income lead to energy use is through appliance acquisition, not through increased usage of existing appliances.

6. Aggregate Energy Use

We next consider the implications of our model for aggregate, country-level energy consumption. A number of papers have analyzed the relationship between aggregate energy consumption and GDP, showing that the "income elasticity" is higher when countries are at low income levels (Galli, 1998) and that the income elasticities at a particular income level have if anything increased over time, contrary to energy "leapfrogging" and possibly reflecting changes in consumption bundles (Van Benthem, 2010). We disagree with the interpretation of the estimates as income elasticities for reasons explained below, and consistent with concerns expressed in some of the previous literature, such as Van Benthem (2010). Forecasts from

[20] The implied retail cost of electricity suggested by this coefficient is lower than the rates faced by even low consuming Mexicans, though the coefficient could be biased downwards by measurement error.

models like these, however, have been influential in policy debates about how economic growth is likely to affect both the demand for energy and global warming. So, while we do not necessarily endorse the causal interpretation of the models, we believe that it is useful to ask how these forecasts might vary when accounting for possible differences across countries with more or less pro-poor growth.[21] This serves as an additional check on our model and provides preliminary evidence on the size of the effects we have identified relative to existing estimates.

Following the existing literature, we begin with a simple specification:

$$\ln(Energy_{it}) = \alpha \ln(Income_{it}) + \beta \ln(Price_{\mathrm{it}}) + \delta_i + \theta_t + \varepsilon_{it} \tag{5}$$

for country i in year t, where energy is total final per capita energy consumption, income is measured as GDP per capita and price is either oil price adjusted for exchange rates and the local CPI or a constructed price index, incorporating, for instance, local taxes.[22] δ_i captures country-level fixed effects, reflecting factors such as climate and natural resource endowment and θ_t captures worldwide trends, for instance in technology.

α is usually described as the income elasticity, although since there are no controls for supply-side factors and price is measured with considerable imprecision, it is best thought of as a descriptive parameter which captures the correlation between income and energy use. We drop the price term, as, consistent with previous estimates from the developing world, we find the coefficient on price to be either zero or positive, suggesting that the variable is picking up something other than a price response. Because we allow for a time trend and estimate country fixed effects, this coefficient could capture responses to country-specific price shocks, which are likely endogenous to local demand and regulation.

The first prediction of the model in section 1 is that households at higher income levels will be more likely to acquire energy-using assets. At a macro level, this suggests that asset ownership, and hence energy use, will be positively related to GDP per capita. This is a very straightforward prediction, and, at the most basic level, is confirmed by all the existing papers in the literature as they estimate positive coefficient on ln(income).

[21] Several recent papers explore the impact of electrification on various measures of economic development (Dinkelman, forthcoming and Lipscomb et al., 2011).

[22] Data were generously provided by Arthur Van Benthem and are described in Van Benthem (2010). "Final" energy consumption covers energy supplied to the final consumer for all energy uses. It does not include, for instance, coal burned to create electricity, but it does include electricity.

However, our model also suggests that the coefficient on ln(income) should be higher in countries that have experienced pro-poor growth than in countries that have lifted few households out of poverty. Our model suggests that countries where GDP growth mainly benefits the wealthy, who already own most energy-using assets, will not experience the same growth in energy use. Pro-rich growth that does not lead to substantial asset accumulation moves energy demand along the intensive margin, which is small. Indeed, using the Mexican data, our results in Section 5 indicate that the response of energy use to transfers, conditional on asset ownership, is very low. However, as pro-poor growth lifts households out of poverty to an income level where they begin to acquire energy-using assets like refrigerators, the demand for energy moves along the more explosive extensive margin.

To test whether prediction 1 is more relevant in countries with pro-poor growth, we estimate:

$$(6) \quad \ln(Energy_{it}) = \alpha \ln(Income_{it}) + \gamma_1 \ln(Income_{it}) \times ProPoorGrowth_i + \delta_i + \theta_t + \varepsilon_{it}$$

To estimate (6) we use data for 37 developing countries over 27 years, from 1980 to 2006. Our main results define the variable *ProPoorGrowth* at the country level as the decrease in the average Gini coefficient from the beginning of the sample (1980-1993) to the end of our sample (1994-2006). On average, inequality is increasing in the countries in our sample, as the mean of *ProPoorGrowth* is -1.25, but there is a wide range from -13 to 13.[23] Our prediction is that γ_1 will be positive.

Prediction 2 of our model is that faster growth will lead to more asset acquisition. To evaluate this prediction, we include a variable measuring the percent change in per capita GDP as well as its interaction with *ProPoorGrowth*:

$$(7) \quad \ln(Energy_{it}) = \alpha \ln(Income_{it}) + \mu\, IncomeGrowth_{it} + \gamma_2\, IncomeGrowth_{it} \times ProPoorGrowth_i + \delta_i + \theta_t + \varepsilon_{it}$$

Our model predicts that μ will be positive. Previous literature has included lagged GDP per capita on the hypothesis that energy use might adjust slowly to income. This would suggest a negative estimate of the parameter μ. In the end, the estimates will reflect the net of (at least) these two effects. We would expect our effect to be stronger in countries with *ProPoorGrowth*, suggesting that γ_2 will be positive.

[23] Data on Gini coefficients and other measures of ProPoorGrowth are from the World Bank Development Indicators. We include countries for which both energy use and multiple GINI measures are available.

Finally, prediction 3 of our model is that the effects of faster growth will be more pronounced at higher income levels. We evaluate this hypothesis by estimating:

$$\begin{aligned}\ln(Energy_{it}) = {} & \alpha \ln(Income_{it}) + \mu\, IncomeGrowth_{it} + \rho IncomeGrowth_{it} \\ & \times \ln(Income_{it}) + \gamma_1 \ln(Income_{it}) \times ProPoorGrowth_{it} \\ & + \gamma_2\, IncomeGrowth_{it} \times ProPoorGrowth_{it} + \gamma_3 \ln(Income) \\ & \times IncomeGrowth_{it} \times ProPoorGrowth_{it} + \delta_i + \theta_t + \varepsilon_{it}\end{aligned} \tag{8}$$

Our model predicts that γ_3 will be positive.

The results reported in Table 9 are consistent with our model. In the first column, we find that $\gamma_1 > 0$. In the second column, we find that $\gamma_2 > 0$ and in the third column, we find $\gamma_3 > 0$. The sizes of the coefficients suggest that the effect we have identified is quite large. Consider, for instance, the results in column (1). The income response for a country such as Brazil, which is at the 75th percentile of *ProPoor Growth* is over 1, while a country such as Argentina, which is at the 25th percentile of *Pro Poor Growth* has an income response almost half the size.

We have performed several robustness tests, including re-estimating the specifications in Table 9 on a balanced sample, measuring income growth over a longer time period, varying the cut-off date used to determine changes in inequality and replacing *ProPoorGrowth* with a dummy variable indicating that the country had above the median changes in inequality. The results are all very similar to those reported in Table 9.

We have also used alternative measures of *ProPoorGrowth*, including changes in the poverty gap, a measure of income of the population below the poverty line. On the one hand, this measure may be more consistent with our model since our model does not require increases in the incomes of the poor at the expense of the rich. On the other hand, our model probably best applies to households with incomes slightly higher than the poverty line (defined as income equivalent to \$2 per day), as Figure 1 shows, much of the long term acquisition appears to be above \$2 per day.

A final concern is that our results may simply reflect the fact, already established in the literature, that income elasticities vary with income. Considering the results in column (3), this is more likely to be the case in countries with pro-poor growth, which provides support for our model over other explanations offered for income elasticities that vary with income.

7. Conclusion

Accurate forecasts of energy demand are critical as investments in energy infrastructure require long lead times, and if the global demand for energy increases faster than anticipated, there could significant shortages and increases in energy prices. In addition, country-specific energy forecasts are critical inputs into international climate agreements. And, negotiations can break down if the parties have different expectations about emissions paths.

Much of the future increase in the demand for energy will come from low- and middle-income countries (EIA 2010). In this paper, we argue that the nature of economic growth in these countries is key to accurate forecasts of the demand. We show that there will likely be a surge in the demand for energy with pro-poor economic growth and explicit poverty alleviation interventions. The primary reason is that raising the income of the poor moves their demand for energy along the extensive margin as they buy energy-using assets for the first time. Acquiring an energy-using asset for the first time leads to a considerable increase in a household's energy use. While income growth also affects energy consumption on the intensive margin, the effect is trivial compared to the effect of accumulating more energy-using assets. As the poor come out of poverty their demand moves mostly along the extensive margin leading to a large discrete jump in demand for energy.

We also show that the speed at which the poor come out of poverty affects the size of this increase in energy demand, which has important implications for different countries. For example, we show that two countries that are at the same current level of income per capita may have different refrigerator ownership rates, with the country where recent growth was fast having a much higher ownership rate than the country that grew more slowly. Our model also has implications for how poverty alleviation policies such as cash transfer programs affect asset accumulation. Specifically, we show that the rate of the payments should matter for asset acquisition rates. For instance, a program that distributes transfers on a quarterly basis may lead to more refrigerator acquisition than a program that distributes transfers bi-weekly.

We provide empirical support for these conclusions from an investigation of the causal impact of an increase in the stream of income on asset accumulation and energy use in the context of Mexico's conditional cash transfer program, Oportunidades. We find that the increase in income through the transfers had a large effect on asset accumulation, and that effect on asset accumulation is substantially great when the cash is transferred over a shorter time period.

Finally, we applied the lessons learned from the household to the aggregate energy forecast models using country-level panel data. We show that if a country's growth has been pro-poor, the income elasticity of energy is nearly double that of a country with GDP growth that has been less favorable to the poor. These results suggest that not accounting for pro-poor growth would grossly underestimate future energy use.

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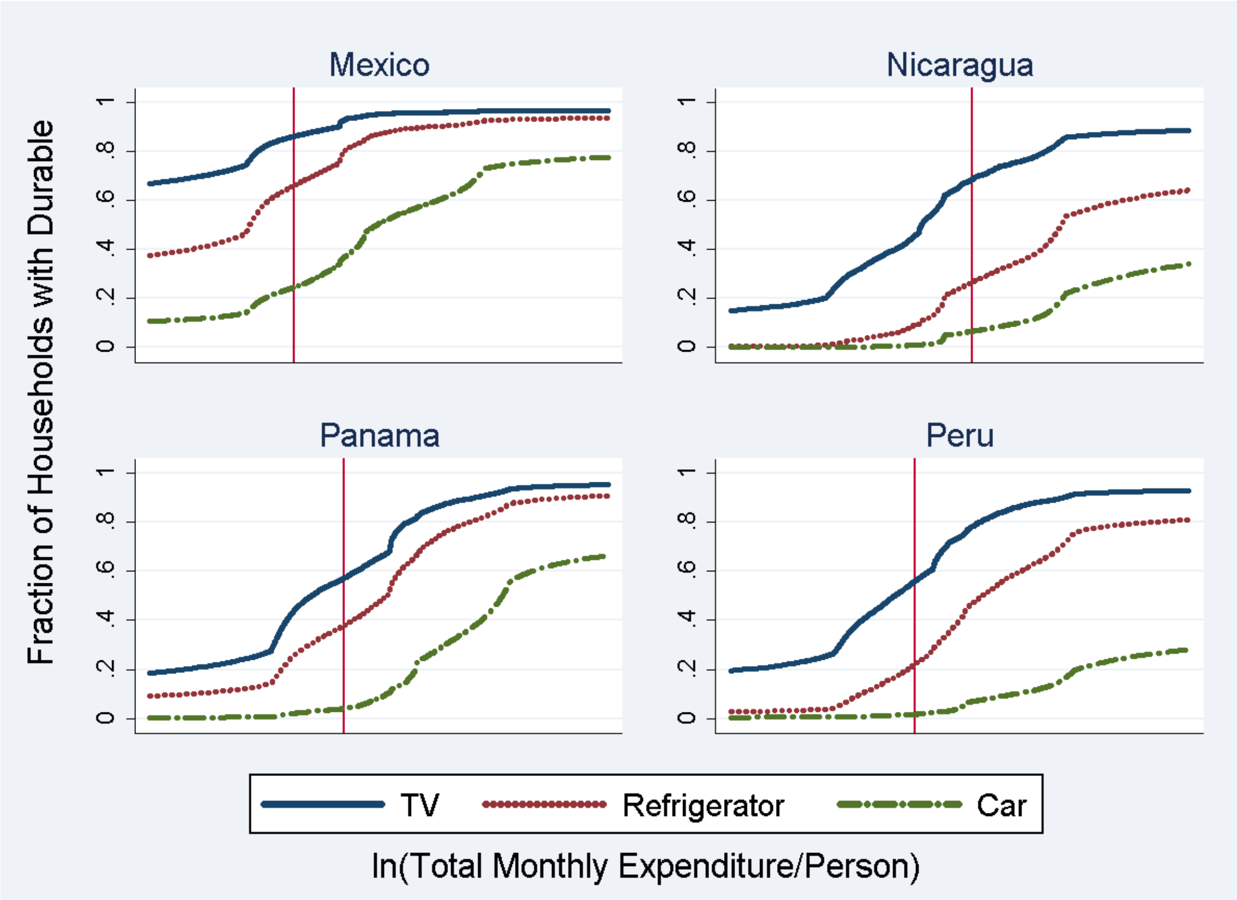


Figure 1

Vertical line at $2 per day.

Sources: Mexico, 2006, *Encuesta Nacional de Ingreso y Gasto de los Hogares.* Nicaragua, 2001, *Encuesta Nacional de Hogares sobre Medicion de Niveles de Vida.* Panama, 2003, *Encuesta de Niveles de Visa.* Peru, 1994, *Enquesta Nacional de Hogares sobre Medicion de Niveles de Vida.*

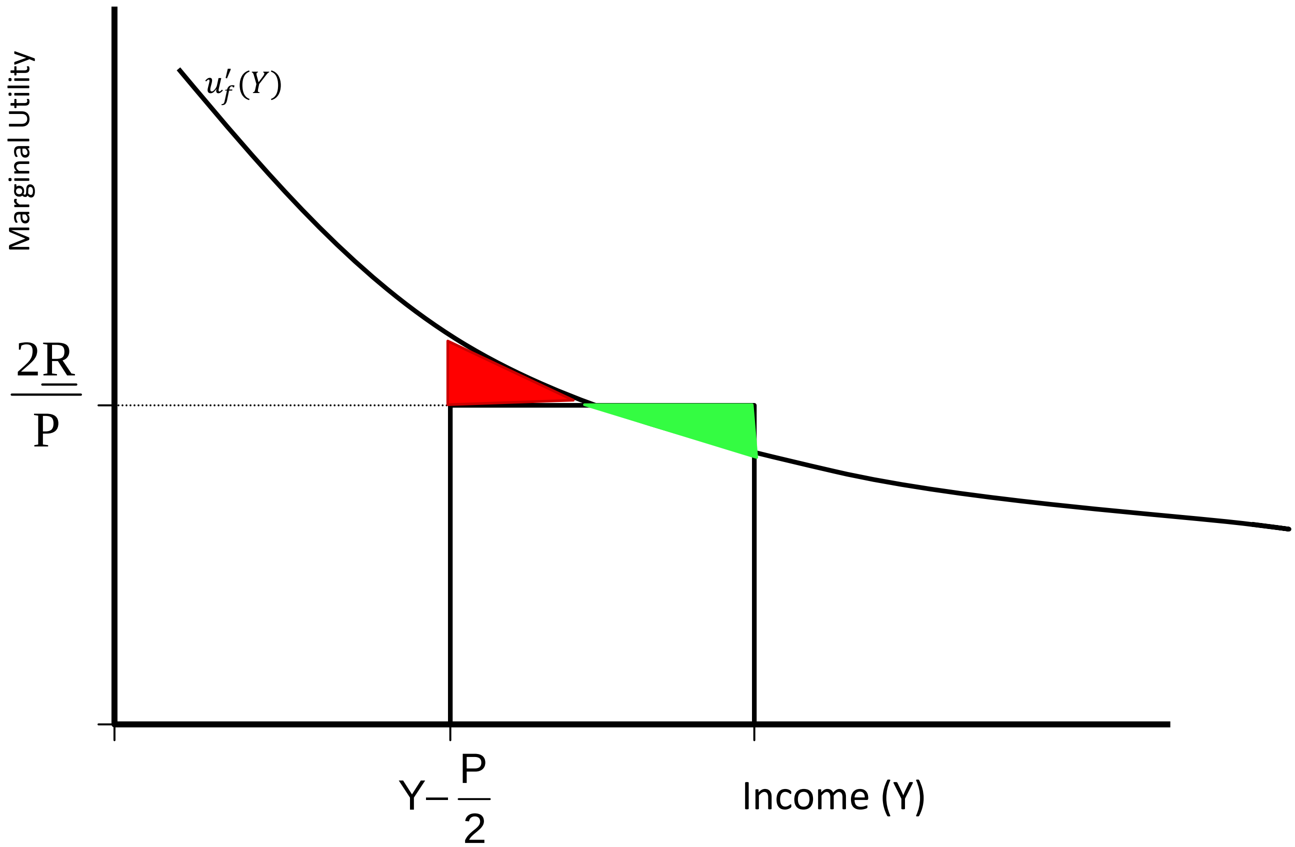


Figure 2

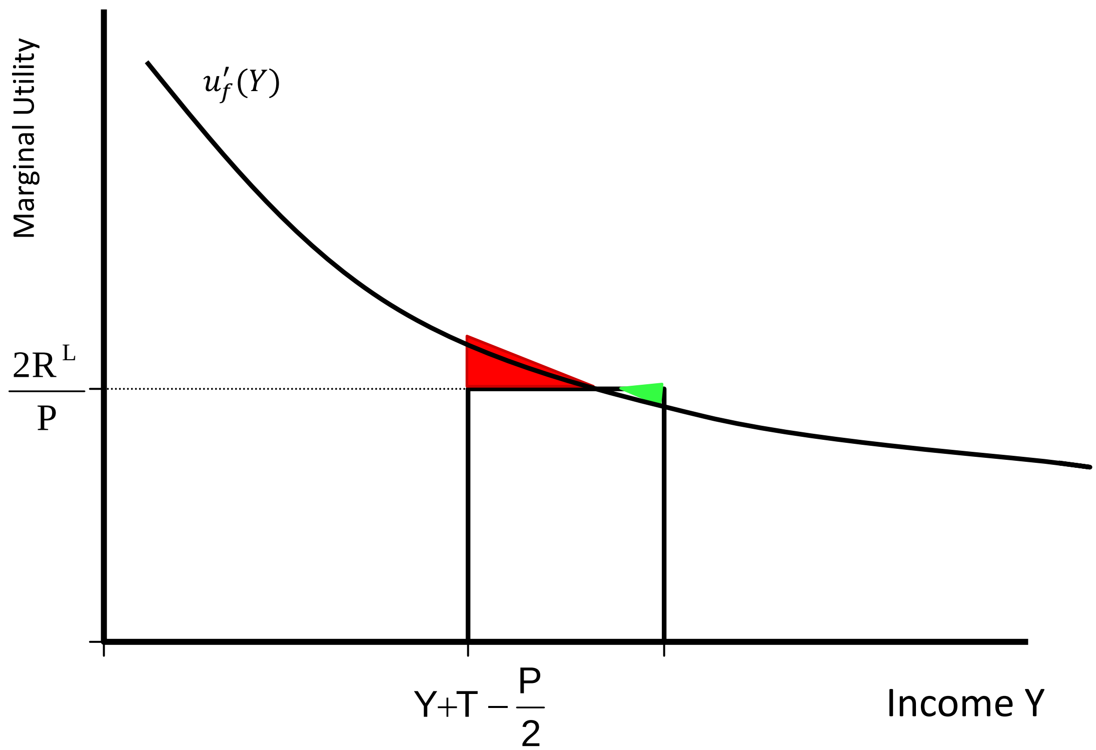


Figure 3

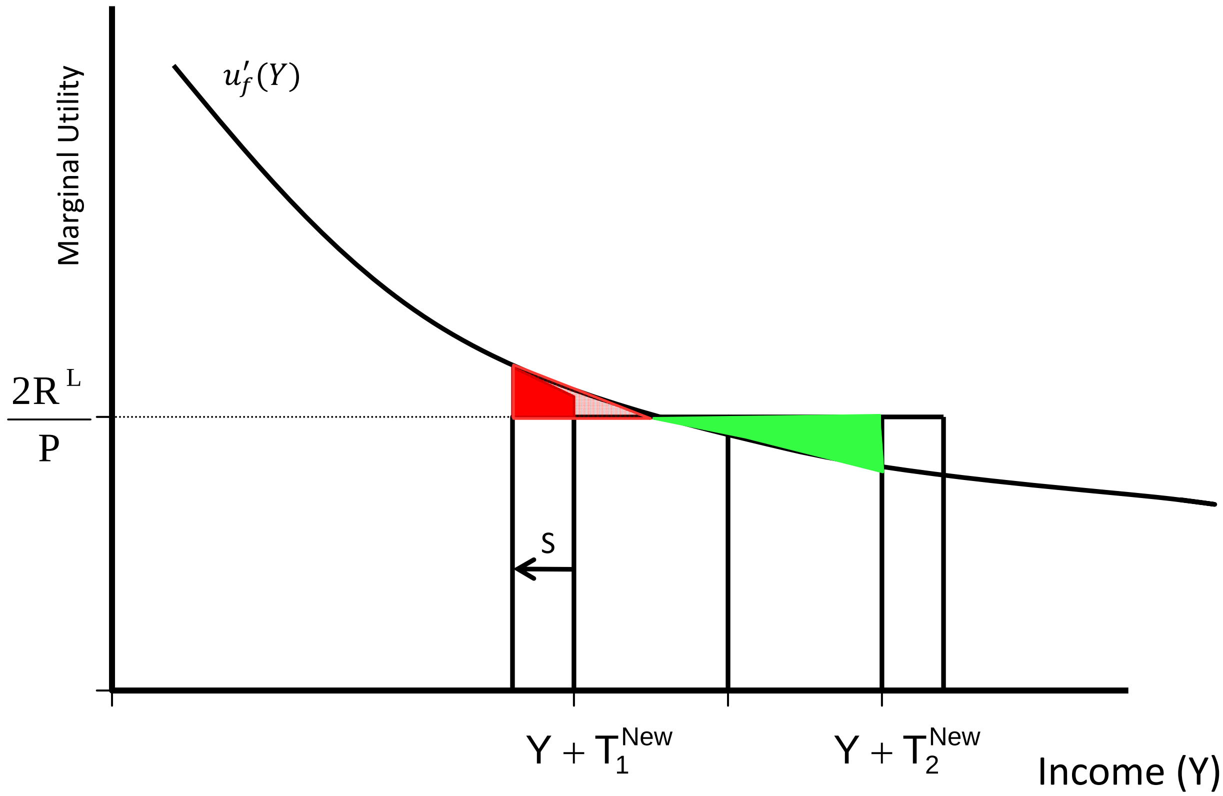


Figure 4

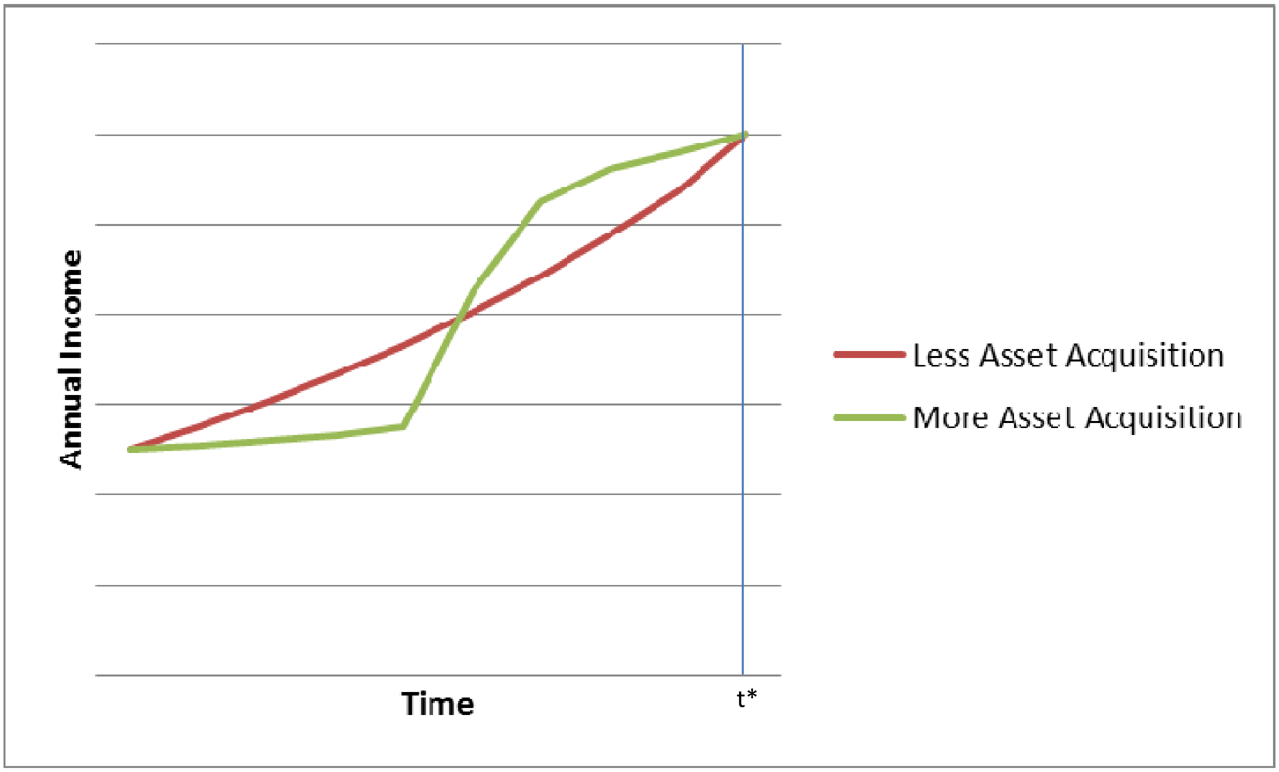

Annual Income
Less Asset Acquisition
More Asset Acquisition
Time
t*

Figure 5

Table 1: Oportunidades Bi-Monthly Support Levels in 2003 (pesos)

Basic Support:	155	
Educational Scholarship:		
Grade	Boys	Girls
Third	105	105
Fourth	120	120
Fifth	155	155
Sixth	205	205
Seventh	300	315
Eighth	315	350
Ninth	335	385
Tenth	505	580
Eleventh	545	620
Twelfth	575	655

A household can receive a maximum of 1,025 pesos with children through 6th grade or 1,715 pesos with children in 7th grade or higher.

An additional 200 pesos for children in 3rd-6th grades and 250 pesos for children in 7th grade or higher are provided once a year for school supplies.

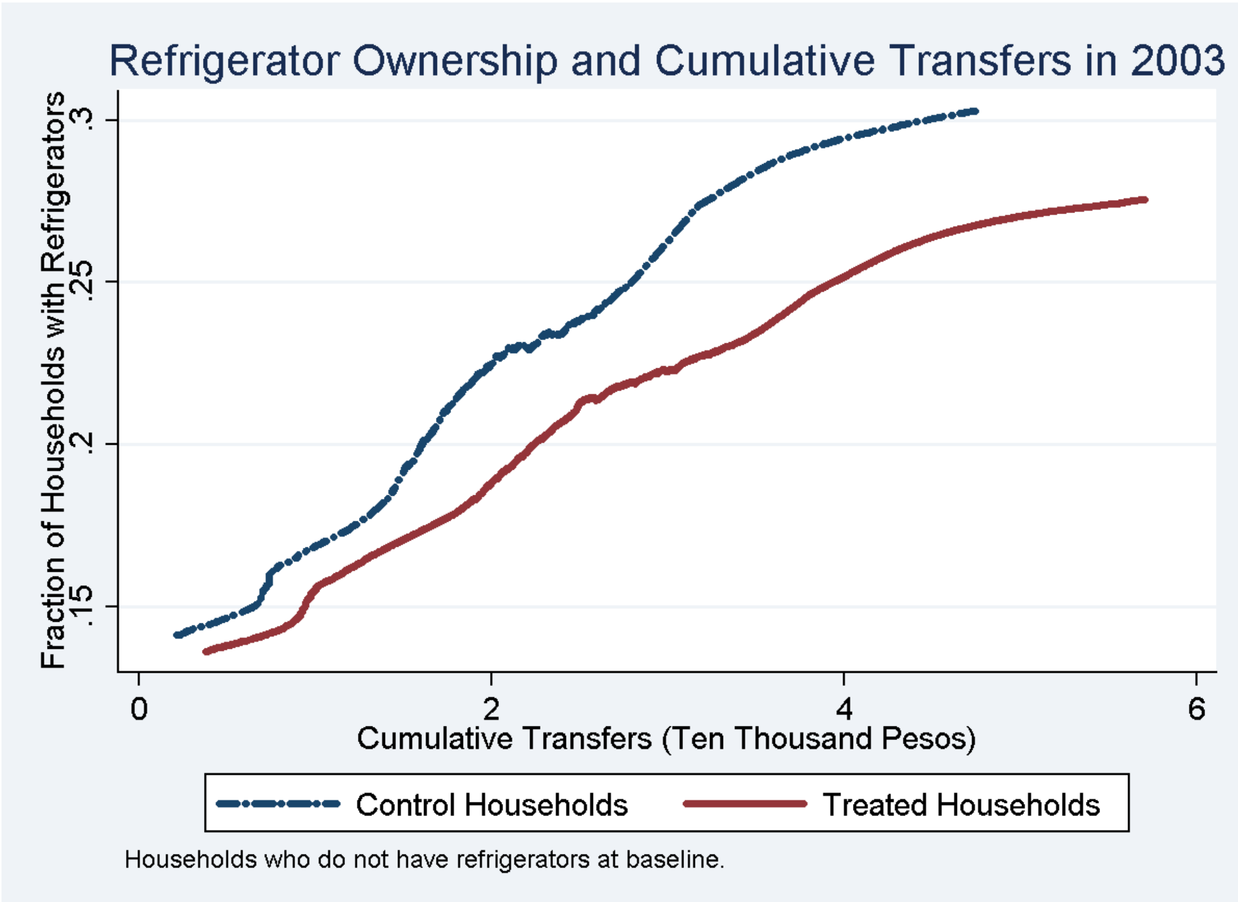
Table 2: Summary Statistics

	Control Households			Treated Households			Difference	
	Mean	SD	N	Mean	SD	N	Mean	P-Value
Assets - Dependent Variables At Baseline								
Refrigerator	0.038	0.191	3341	0.044	0.205	5185	-0.006	0.540
Washing Machine	0.012	0.109	3342	0.014	0.119	5184	-0.002	0.600
Asset Aggregate	36.909	29.049	3275	36.820	30.556	5151	0.089	0.957
Stove	0.165	0.371	3342	0.158	0.364	5186	0.007	0.777

Table 3: Summary Statistics -- Cumulative Transfers (Ten Thousands of Pesos (2003))

	Control Households			Treated Households		
	25%	Median	75%	25%	Median	75%
1998				0.09	0.13	0.21
1999				0.24	0.38	0.61
2000m	0.06	0.09	0.16	0.32	0.51	0.82
2000n	0.18	0.33	0.53	0.45	0.79	1.23
2003	0.93	1.69	2.63	1.24	2.19	3.36
2007	2.36	3.89	5.67	2.63	4.33	6.35

Figure 6: Refrigerator Ownership and Cumulative Transfers in 2003



Lowess regressions. Excludes the bottom and top 2% of cumulative transfers in each group.

Table 4: Basic Results - Refrigerators

	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV
		Discrete Time Hazard			Household FE
Cumulative Transfers	0.023***	0.028***	0.039***	0.056***	0.061***
	[0.004]	[0.004]	[0.007]	[0.007]	[0.007]
Treated		-0.016***	-0.007	-0.009*	
		[0.005]	[0.005]	[0.005]	
Cumulative Transfers X Treated			-0.015**	-0.021***	-0.018**
			[0.006]	[0.007]	[0.007]
Net Treated Effect at 2003 Median Cumulative Transfers			-0.025***	-0.033***	
			[0.008]	[0.008]	
N	30,414	30,414	30,414	30,414	30,258
R-squared	0.100	0.100	0.101		
F Stat on Excluded Variables – Potential Cumulative Transfers				1554	1226
F Stat on Excluded Variables – Potential Cumulative Transfers X Treated				1974	1889
Number of Households					6,655

Note: All specifications include state by round- fixed effects. All rounds through 2003 included. Specifications in columns (1) – (4) include household controls including number of children seven and younger, number of children 8 to 17, number of males 18 to 54, number of females 18 to 54, number of adults 55 and over, number of individuals with unreported ages, head of household's gender, head of household's and spouse's age, and education, and whether the household owns the house they live in, farm assets at baseline, number of other social programs the household is the beneficiary of, and village characteristics including migration intensity, marginalization and distance to nearest city. In columns (4) and (5), instruments include Potential Cumulative Transfers and Potential Cumulative Transfers X Treated. Column (5) including household fixed effects drops 156 singletons.
Robust standard errors clustered by village in brackets.
*** p<0.01, ** p<0.05, * p<0.10.

Table 5: Basic Results - Other Assets

	Cumulative Transfers		Cumulative Transfers X Treated		Treated		Net Treated Effect		N
Refrigerator									
DTH	0.056***	[0.007]	-0.021***	[0.007]	-0.009*	[0.005]	-0.033***	[0.008]	30,414
HH FE	0.061***	[0.007]	-0.018**	[0.007]					30,258
Washing Machine (ex 99)									
DTH	0.018***	[0.005]	-0.005	[0.004]	-0.004	[0.004]	-0.011**	[0.005]	26,166
HH FE	0.021***	[0.005]	-0.007	[0.005]					26,035
Number of Rooms (2003 only)									
Cross Section	0.151***	[0.031]	-0.035	[0.027]	0.079	[0.058]	0.038	[0.039]	6,952
Stove (LP Gas)									
DTH	0.029***	[0.008]	-0.017***	[0.006]	-0.008	[0.006]	-0.027***	[0.008]	26,007
HH FE	0.031***	[0.008]	-0.016**	[0.007]					25,798

Note: All specifications include state by round- fixed effects and household controls, as described in the notes to Table 4. Instruments include: Potential Cumulative Transfers and Potential Cumulative Transfers X Treated. All rounds through 2003 included except where noted. Washing machine not reported in 1999. Refrigerator entry repeats results from Table 4. Net treated effect is estimated at 2003 median cumulative transfers. DTH indicated Discrete Time Hazard. HHFE indicated Household Fixed Effects. Cross Section indicates that the regression is a cross section of all households with asset at baseline as a control.
Robust standard errors clustered by village in brackets.
*** p<0.01, ** p<0.05, * p<0.10
F-stat on excluded variables not reported. All exceed 200.

Table 6: Placebo Test: Do Cumulative Transfer Predict Asset Ownership at Baseline?

	Baseline		2003	
Refrigerator				
Cumulative Transfers	0.006	[0.006]	0.055***	[0.011]
Treated	0.005	[0.012]	0.028	[0.026]
Cumulative Transfers X Treated	-0.001	[0.005]	-0.030***	[0.011]
Net Treated Effect	0.004	[0.008]	-0.007	[0.018]
Washing Machine				
Cumulative Transfers	0.003	[0.004]	0.009	[0.007]
Treated	0.007	[0.007]	-0.025	[0.016]
Cumulative Transfers X Treated	-0.001	[0.004]	0.004	[0.014]
Net Treated Effect	0.005	[0.003]	-0.022**	[0.011]
Number of Rooms (Baseline and 2007)				
Cumulative Transfers	0.022	[0.015]	0.108***	[0.020]
Treated	0.088	[0.062]	0.143	[0.100]
Cumulative Transfers X Treated	-0.023	[0.014]	-0.038**	[0.019]
Net Treated Effect	0.033	[0.037]	0.049	[0.064]
Stove				
Cumulative Transfers	0.045***	[0.012]	0.041***	[0.012]
Treated	-0.005	[0.026]	0.013	[0.032]
Cumulative Transfers X Treated	-0.015	[0.011]	-0.029**	[0.012]
Net Treated Effect	-0.022	[0.018]	-0.021	[0.023]

Note: For each asset, we report results from two different specifications, one estimated using data at baseline and the other using data from 2003. All specifications include state fixed effects and household controls, as described in the notes to Table 4. All specifications estimated using IV with Potential Cumulative Transfers and Potential Cumulative Transfers X Treated as instruments. Net treated effect is estimated at 2003 median cumulative transfers.
Robust standard errors clustered by village in brackets.
*** p<0.01, ** p<0.05, * p<0.10
F-stat on excluded variables not reported. All exceed 400.

Table 7: Future Transfers and Refrigerator Acquisition

	(1)	(2)	(3)
	OLS	OLS	IV
Cumulative Transfers	0.039***	0.040***	0.084***
	[0.007]	[0.007]	[0.011]
Treated	-0.007	-0.008	-0.015***
	[0.005]	[0.005]	[0.005]
Cumulative Transfers X	-0.015**	-0.015**	-0.026***
Treated	[0.006]	[0.006]	[0.007]
Net Treated Effect at	-0.025***	-0.025***	-0.045***
2003 Median	[0.008]	[0.008]	[0.009]
Cumulative Transfers			
Future Cumulative		<0.001	-0.047***
Transfers X 03		[0.004]	[0.011]
Future Cumulative		-0.004***	-0.013***
Transfers X 00n		[0.001]	[0.003]
Future Cumulative		<0.001	-0.006**
Transfers X 00m		[0.001]	[0.002]
Future Cumulative		>-0.001	-0.003
Transfers X 99		[0.001]	[0.002]
Future Cumulative		<0.001	-0.001
Transfers X 98		[0.001]	[0.002]
N	30,414	30,414	30,414
R-squared	0.101	0.101	

Note: All specifications include state by round- fixed effects and household controls described in the notes to Table 4. All rounds through 2003 included. Instruments include Potential Cumulative Transfers, Potential Cumulative Transfers X Treated, and Potential Future Cumulative Transfers by round. Column (1) repeats results from Table 4. Robust standard errors clustered by village in brackets.
*** p<0.01, ** p<0.05, * p<0.10
F-stat on excluded variables not reported. All exceed 200.

Table 8: Short-Run Effect of Transfers on Energy Demand Oportunidades Households (2007)

	Dependent Variable: Bi-Monthly Electricity Expenditures					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Appliance Aggregate			432.1***	687.6***		
			[41.4]	[254.0]		
Refrigerator					54.2***	103.0**
					[5.4]	[44.4]
Current Transfers	94.6**	200.6	62.2	-37.0	68.8	-30.3
(Bi-Monthly, 10,000 2007 pesos)	[45.5]	[197.0]	[43.9]	[213.5]	[43.9]	[218.7]
N	3,960	3,960	3,960	3,960	3,960	3,960
R^2	0.256		0.507		0.261	
First-stage F-stat (Asset Index/Refrigerator)				36.97		22.55
First-stage F-stat (Current Transfers)		24.93		24.85		24.61

Note: All specifications include village fixed effects and household controls described in the notes to Table 4. IV instruments include: Potential Current Transfers, Potential Cumulative Transfers, Potential Cumulative Transfers X Treated, Asset Aggregate in 1997 (4 only), Refrigerator Ownership in 1997 (6 only). Includes only households with reported positive electricity expenditures. Asset Aggregate scaled to estimated MWhr/month.
Robust standard errors clustered by village in brackets.
*** p<0.01, ** p<0.05, * p<0.10.

Table 9: Aggregate Country-Level Energy Consumption

	(1)	(2)	(3)
Income (log)	0.925***	0.856***	0.909***
	[0.087]	[0.104]	[0.088]
Income Growth		-0.324	0.307
		[0.201]	[0.726]
Income (log) X Income Growth			0.097
			[0.116]
Income (log) X ProPoorGrowth	0.073***		0.057***
	[0.017]		[0.016]
Income Growth X ProPoorGrowth		0.122**	0.893***
		[0.053]	[0.233]
Income (log) X Income Growth X ProPoorGrowth			0.146***
			[0.037]
Country Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES
Observations	907	892	892
R-squared	0.981	0.981	0.983

Robust standard errors clustered by country in brackets.
*** p<0.01, ** p<0.05, * p<0.10.

APPENDIX

Appendix Table 1 describes the weights used to create the asset aggregate reflected in Table 8: Short-Run Effect of Transfers on Energy Demand Oportunidades Households (2007) above. The aggregate was constructed using data from two sources. The first is a document from the Mexican Ministry of Social Development based on data from the Federal Electricity Commission (CFE). The document estimates average monthly electricity consumption of a typical Mexican household through the use of appliances and the time spent in their use. They consider the average power needed for each appliance, as well as the average time a typical Mexican household (not necessarily a poor household) uses each appliance per month. We use the average monthly electricity usage to assign each appliance a KW-hour value in the aggregate and then use data from the ENCEL to select the appliances that the population of interest possesses. Thus, if a household owns all of the appliances in the table and lives in a 3 room house, the corresponding asset aggregate for that household would be equal to 199. However, if the household lives in a one room house and only owns a TV and a refrigerator, the corresponding aggregate would be equal to 158.

Appendix Table 1: Asset Aggregate Construction: Household Appliance Use and Average Monthly Kilowatt Consumption in Mexico

	Average Power in Watts	Time use per day	Time use per month	Kilowatts per month
Refrigerator	500 watts	8 hours/day	240 hours	120
Light bulbs (1 + 1 per room)	60 watts	5 hours/day	150 hours	9
Washing Machine[1/]	400 watts	1.3 hours/day	32 hours	13
TV	50 watts	6.3 hours/day	180 hours	10
Radio/ Stereo/CD Player	50 watts	5 hours/day	150 hours	8
Blender	400 watts	10 minutes/day	5 hours	2

1/ The use of the washing machine is 4 hours twice per week.

Source: "Tabla de consumo" (2010), http://www.cfe.gob.mx/sustentabilidad/ahorroenergia/Paginas/Tabladeconsumo.aspx, Federal Electricity Commission (Comisión Federal de Electricidad, CFE).

Note that these numbers were obtained assuming average electricity consumption for typical Mexican households, not necessarily for households living in poverty.

Appendix Table 2: Summary Statistics

	Control Households			Treated Households			Difference		
	Mean	SD	N	Mean	SD	N	Mean	P-Value	
Panel A: Household Socio-Economic Characteristics at Baseline									
Age of Head of Household	42.287	13.906	3336	41.575	13.337	5168	0.711	0.119	
Male Head of Household	0.929	0.257	3342	0.929	0.256	5187	<0.001	0.947	
Home Owner	0.929	0.256	3342	0.944	0.229	5187	-0.015	0.094	*
Age of Spouse	36.422	11.753	3017	36.244	11.793	4664	0.178	0.661	
Spouse Education - Incomplete Primary	0.609	0.488	3020	0.633	0.482	4676	-0.024	0.374	
Head of Household Education - Incomplete Primary	0.666	0.472	3342	0.668	0.471	5187	-0.002	0.902	
Spouse Education - Primary	0.028	0.165	3020	0.025	0.157	4676	0.003	0.573	
Head of Household Education - Primary	0.029	0.167	3342	0.035	0.183	5187	-0.006	0.259	
Spouse Education - More Than Primary	0.002	0.045	3020	0.003	0.058	4676	-0.001	0.272	
Head of Household Education - More Than Primary	0.006	0.075	3342	0.008	0.087	5187	-0.002	0.304	
Indigenous Spouse	0.315	0.465	3008	0.334	0.472	4651	-0.019	0.686	
Indigenous Head of Household	0.400	0.490	3330	0.384	0.486	5161	0.016	0.762	
Number of Other Social Programs	0.600	0.689	3253	0.468	0.591	5096	0.132	<0.001	***
Number of children 7 and under	1.744	1.276	3235	1.721	1.285	5055	0.024	0.571	
Number of children 8 to 17	1.905	1.561	3235	1.865	1.559	5055	0.040	0.396	
Number of Males 18-54	1.039	0.594	3235	1.042	0.606	5055	-0.003	0.852	
Number of Females 18-54	1.128	0.555	3235	1.123	0.570	5055	0.005	0.783	
Number of adults 55 plus	0.355	0.660	3235	0.337	0.637	5055	0.018	0.358	
Number of Age unknown	<0.001	<0.001	3235	<0.001	0.001	5055	>-0.001	0.317	
Electricity	0.652	0.476	3236	0.618	0.486	5062	0.034	0.453	
Horses	0.281	0.701	3232	0.283	0.692	5051	-0.002	0.947	
Mules	0.322	0.701	3229	0.332	0.712	5054	-0.010	0.808	
Oxen	0.053	0.412	3230	0.083	0.458	5055	-0.031	0.034	**
Goats	0.856	3.374	3233	1.085	3.962	5054	-0.229	0.214	
Cows	0.574	1.945	3230	0.607	1.857	5058	-0.032	0.708	
Chickens	6.476	6.337	3224	5.891	6.083	5051	0.584	0.073	*
Pigs	1.126	1.934	3226	0.971	1.777	5052	0.155	0.279	
Rabbits	0.175	1.690	3231	0.121	1.474	5061	0.055	0.317	
Hectares Irrigated	0.035	0.349	3236	0.037	0.340	5061	-0.002	0.902	
Hectares	1.778	2.715	3227	1.669	2.603	5047	0.109	0.427	
Hectares Grazing	0.121	1.149	3236	0.164	1.329	5062	-0.043	0.378	

Appendix Table 2: Summary Statistics (Continued)

	Control Households			Treated Households			Difference	
	Mean	SD	N	Mean	SD	N	Mean	P-Value
Panel B: Village Characteristics								
Migration Intensity	0.056	1.024	168	0.039	0.991	272	0.017	0.864
Degree of Marginalization Low or Moderate	0.077	0.267	168	0.091	0.288	274	-0.014	0.608
Degree of Marginalization High	0.756	0.430	168	0.719	0.450	274	0.037	0.389
Degree of Marginalization Very High	0.167	0.373	168	0.190	0.393	274	-0.023	0.536
KM to Nearest City	101.033	43.548	171	102.285	41.002	275	-1.252	0.763