THE ECOLOGICAL FOOTPRINT OF POVERTY ALLEVIATION: EVIDENCE FROM MEXICO'S OPORTUNIDADES PROGRAM

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Abstract

We study the consequences of poverty alleviation programs for environmental degradation. We exploit the community-level eligibility discontinuity for a conditional cash transfer program in Mexico to identify the impacts of income increases on deforestation, and use the program's initial randomized rollout to explore household responses. We find that additional income raises consumption of land-intensive goods and increases deforestation. The observed production response and deforestation increase are larger in communities with poor road infrastructure. This suggests that better access to markets disperses environmental harm and that the full effects of poverty alleviation can be observed only where poor infrastructure localizes them.

JEL classifications: O12, O13, Q01, Q56

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1 Introduction

Environmental quality and natural resource stocks are key components of welfare for the world's poor but are being degraded at an alarming rate (MEA 2005). Are efforts to alleviate poverty likely to mitigate or exacerbate this degradation? This is a crucial question for policymakers pursuing sustainable development goals and has been a perennial debate in the economics literature (e.g. Grossman & Krueger (1995), Dasgupta, Laplante, Wang & Wheeler (2002), Harbaugh, Levinson & Wilson (2002), Foster & Rosenzweig (2003)). Poverty alleviation may raise demand for goods which are resource-intensive in production, increasing degradation. However, increased wealth may also augment demand for environmental resources, inducing households to invest in those resources, or may raise the opportunity cost of extractive activities, reducing degradation. As noted in a recent review (World Bank 2008), empirical work on the environmental effects of poverty alleviation has been significantly limited by the possible endogeneity of household income changes. In this paper, we exploit the discontinuity in the community-level eligibility rule for a conditional cash transfer program in Mexico, as well as random variation in the pilot phase of the program, to study the consequences of poverty alleviation programs for environmental degradation.

Previous work has also not adequately considered problems in estimating the response to income changes when impacts are market-mediated and therefore can be spatially dispersed. Recent work on the effects of local rainfall shocks (Keller & Shiue 2008, Donaldson 2009) shows that as infrastructure improves, price changes become less correlated with local shocks. Similarly, we show that even if the true impact of a wealth increase on production is constant, we will detect apparently heterogeneous impacts. Stronger effects will be found where infrastructure is poor and thus the source of environmental resources for production is more geographically constrained. The market-mediation of impacts is a fundamental causal inference issue but is often difficult to disentangle because markets are relatively homogenous. Here we take advantage of large variation in transportation infrastructure to investigate whether observed heterogeneity in impacts is consistent with these theoretical predictions.

Our analysis focuses on deforestation as a measure of environmental quality. Deforestation is locally and globally important and in our dataset can be consistently measured for the more than 105,000 localities in Mexico. Locally, forests contribute to welfare through fuel wood, fodder, timber, watershed protection and wildlife habitat. Globally, forest loss is a major environmental concern. Net forest cover is estimated to have fallen by 9.4 million hectares (just under one percent) per year during the 1990s (FAO 2005). Carbon emissions from deforestation are estimated at approximately 20% of the global total (IPCC 2007) and have been an important focus of recent international climate negotiations. We link spatial data on deforestation in Mexico from the period 2000-2003 to the location and eligibility of every locality in Mexico, and exploit this data structure to examine whether deforestation rates are affected by the program.

Oportunidades represents an ambitious attempt to increase consumption among the poor in Mexico by building human capital. The program funnels large cash payments to households conditional upon their children's school attendance and receipt of regular health checkups. The program has an annual budget of $2.6 billion, or half a percent of GDP, and treats 40% of rural households, increasing per-capita income among recipients by an average of one-third. The program's rollout featured centralized eligibility thresholds at both the locality and the household level, with eligibility defined according to a marginality index. It therefore introduced a large income shock which is discontinuous where localities are defined as just "poor enough" to participate in the program. While a relatively large literature exists using the household-level discontinuity in Oportunidades (Bobonis & Finan 2009, Angelucci & de Giorgi 2009), few previous analyses use the community-level discontinuity (exceptions are Barham (2009)'s paper on the impact of Oportunidades on child health and Green (2005)'s study of political impact). This structure provides us with an unusual ability to study economy-wide effects from the nation-wide introduction of a conditional cash transfer program in a large and diverse country.

We find that exposure to Oportunidades increases deforestation. The results imply roughly a doubling in the probability that any deforestation occurs in a locality. The probability that any deforestation occurs in a locality not eligible for the program is 4.9%, so this represents an increase in an already high likelihood of deforestation. Among communities who do deforest, the results indicate an increase in the rate of deforestation ranging from 15 to 33 percent. To understand the micro-behavior that might explain this increase in deforestation, we turn to household data from the randomized pilot phase of the program. These experimental data show that additional household income significantly increases consumption, and recipient households shift strongly into land-intensive goods such as beef and milk. Consumption increases appear to be constant across localities, but the corresponding production increases and deforestation patterns are not. We observe significant household-level production responses only in treated localities which are more isolated. We also find larger deforestation effects in treated localities that have poor road infrastructure and thus are more isolated from outside markets. Finally, we investigate spatial spillovers of treatment using a new method for calculating spatial lag functions in a regression discontinuity context. This analysis shows the spatial contour of impacts to be flat where roads are good, and to be concentrated around the location of treatment where roads are bad. These results are consistent with the hypothesis that transportation infrastructure is a significant determinant of the spatial profile of market-mediated production impacts.

Our results suggest that there are significant environmental impacts of poverty alleviation. There is an increase in deforestation as households shift demand from less land-intensive goods to more land-intensive goods, increasing their "ecological footprint" (Wackernagel & Rees 1996). This contrasts with Foster & Rosenzweig (2003)'s finding that as incomes rise, household demand for forest products increases, strengthening incentives to conserve forests. It implies that in cases where the demand for agricultural products is likely to rise faster than the demand for forest products in response to higher incomes, poverty alleviation programs should be accompanied by environmental regulations that correctly price externalities or clearly establish property rights to environmental goods (i.e. carbon markets). The results also indicate that policymakers should be cautious in interpreting the magnitude of apparent impact estimates without taking into account how these are mediated through markets. Given a set of localized demand shocks, better-integrated local markets will allow demand to be sourced from a broader set of producers. To the extent that new demand is satisfied by national or global markets, we will not observe a clear link between local consumption increases and local environmental degradation. Therefore where local infrastructure is good, impact studies are unlikely to capture the full magnitude of the "ecological footprint" effect[1].

The paper is organized as follows: we begin in the next section by discussing the literature on poverty and deforestation and the empirical problem introduced by the study of micro-interventions when agents may participate in market transactions on a broader spatial scale. Section 3 describes the Oportunidades program in more detail, and presents the estimation strategy and results of the

[1]It is possible that by sourcing production more broadly, goods will be produced more efficiently and thus the true impacts might actually be smaller in better-integrated markets rather than constant. Caution is still warranted because environmental goods may not be efficiently priced and therefore not efficiently sourced.

discontinuity analysis. Section 4 seeks to disentangle the mechanisms through which this impact occurs by using household data from the randomized evaluation phase of the program. Section 5 presents results on the heterogeneity and spatial distribution of observed impacts, and the final section concludes with a discussion of the policy implications of our findings.

2 Poverty, Deforestation, and Spatial Impact Analysis

Conditional cash transfer programs that seek to alleviate household poverty and improve access to education or health are increasingly popular in developing countries, but may have unintended secondary effects. One possibility that has not received adequate attention is the potential for environmental consequences. It is not clear, *ex ante*, whether we should expect income increases to exacerbate or reduce environmental degradation: a large previous literature on the Environmental Kuznets Curve suggests the relationship is complex and non-linear (Stern 2004, Dasgupta, Laplante, Wang & Wheeler 2002, Panayotou 1997). Disentangling this relationship requires examination of three distinct yet interrelated issues: the existence of a correlation or causal link; the micro-foundations of the relevant household production and consumption decisions; and the role of local markets in mediating the relationship.

2.1 Does alleviating poverty increase or decrease forest cover?

We focus on forests as an environmental outcome of interest. Forests are a key local resource and global public good. Understanding how to prevent further deforestation would significantly contribute to efforts to limit greenhouse-gas emissions (Kaimowitz 2008, Stern 2008). However, even if we limit the scope to the relationship between income and deforestation, previous empirical results and theory are ambiguous (Pfaff, Kerr, Cavatassi, Davis, Lipper, Sanchez & Timmins 2008, Chomitz 2006).

Whether higher household incomes increase or decrease pressure on forest resources depends on multiple factors (Barbier & Burgess 1996, Wunder 2001, Pfaff, Kerr, Cavatassi, Davis, Lipper, Sanchez & Timmins 2008) including prices of agricultural and pastoral goods (Pfaff 1999), demand for forest products (Baland, Bardhan, Das, Mookherjee & Sarkar 2007, Fisher, Shively & Buccola 2005, Foster & Rosenzweig 2003), credit constraints (Zwane 2007), returns to alternative household activities (Deininger & Minten 1999, 2002), agricultural intensification and extensification (Shortle & Abler 1999, World Bank 1992), and demand for environmental amenities (Cropper & Griffiths 1994). The complexity of the relationship between household incomes and deforestation means that research has generated few unambiguous theoretical predictions, and the search for sufficiently large, plausibly exogenous sources of income variation for empirical analysis has been a challenging one.

Initial work on the development-deforestation link focused primarily on the presence and shape of an Environmental Kuznets Curve (Cropper & Griffiths 1994, Pfaff 2000), positing that forest cover initially decreases as income rises but then recovers as income increases beyond some turning point. Subsequent work has shown both increases and decreases in forest cover as income increases. Foster & Rosenzweig (2003) use a general equilibrium framework to show that devotion of land to the production of forest products should rise as demand rises. They confirm this relationship using long-term changes in income and forest cover across Indian states. Deininger & Minten (1999, 2002) suggest that as countries grow richer, relative returns to off-farm labor would increase and reduce pressure on forests. They illustrate such a relationship in data from Mexico. Zwane (2007) finds that the relationship between income and deforestation in Peru is positive at low levels of income but may be negative at higher levels. Baland, Bardhan, Das, Mookherjee & Sarkar (2007) assesses the impacts of income growth on firewood collection in Nepal and find a net negative but very small effect.

The empirical literature on the relationship between income and deforestation has been hampered by concerns about the endogeneity of income growth. Rates of deforestation are clearly influenced by multiple factors which could be correlated with income shocks. These include population growth, agricultural returns, forest product prices, capital availability, technology, accessibility and institutional variables (see reviews by Angelsen & Kaimowitz (1999), Barbier & Burgess (2001), Chomitz (2006)). The endogeneity problem may be particularly severe for studies using cross-sectional variation to identify impacts. Conversely, in studies using panel variation in income (Zwane 2007, Baland, Bardhan, Das, Mookherjee & Sarkar 2007), the relatively small income changes observed in a short-term panel may not reflect true economic development. Also, these short-term fluctuations are different in nature than permanent income changes. Households are likely to respond differently to income changes that are perceived to be substantial and permanent versus small and temporary.

Exploiting Mexico's rollout of Oportunidades allows us to make two contributions to the existing empirical literature. First, the implementation of the Oportunidades program creates an exogenous source of variation in income, allowing for clean identification of causal effects. Second, the magnitude and duration of the program represents a substantial and durable increase in income for a large share of the households in poor communities. We are thus able to estimate impacts using a positive shock to income that is as large as is likely to be achievable by any actual poverty alleviation program.

2.2 The household response to income shocks

In the set of empirical studies discussed above, several potential mechanisms are proposed to explain how changes in household income might affect deforestation. On the production side, Deininger & Minten (1999, 2002) suggest that income increases which occur through increased returns to off-farm labor would reduce agricultural land use and ease pressure on land, also reducing deforestation. Although a conditional cash transfer program might not directly raise off-farm wages, it could raise the opportunity cost of leisure, and therefore discourage on-farm production through a similar mechanism. Other researchers have suggested that income increases could spur capital improvements or technological adoption, which would facilitate agricultural intensification and reduce pressure on forests (Shortle & Abler 1999, World Bank 1992). Zwane (2007), in contrast, suggests that the expected effect of relaxing a credit constraint depends on initial income. At low incomes, relaxing the credit constraint increases deforestation while at higher incomes there is an offsetting increase in the marginal utility of leisure which may result in less deforestation.

On the consumption side, Foster & Rosenzweig (2003) propose that higher incomes will decrease deforestation through increased demand for forest products and a corresponding supply response by households where there is clear ownership of forest resources. However, their results depend on the demand for forest products rising faster than the demand for agricultural products in response to an income increase. If instead households rapidly increase demand for land-intensive agricultural goods, we would expect to see increased deforestation. This pattern might be particularly pronounced if inferior goods are relatively more land-efficient than normal goods. As incomes increase, households would substitute consumption away from these land-efficient inferior goods (e.g. beans) to land-intensive normal goods (e.g. beef), thus expanding their "ecological footprint".

2.3 The ecological footprint of market-mediated shocks

If income changes lead to consumption-driven impacts on deforestation, we must address an issue that is fundamental to the estimation of all market-mediated impacts: there is by no means a one-to-one mapping between the location of the consumption change and the location of the corresponding adjustment in production. Particularly when the treatment unit (and therefore the source of variation in demand) is small relative to the geographic coverage of the program, the extent to which production impacts spill over will determine what is measured by comparing treated and untreated units. In trying to understand how these local shocks alter market demand and supply of forest-intensive resources, we can draw an analogy with the literature estimating the effect of localized rainfall shocks on prices. A well-established result from this literature is that as infrastructure improves, prices become less correlated with localized rainfall shocks and more correlated with the rainfall shocks of adjacent areas (Keller & Shiue 2008, Donaldson 2009). This effect occurs because demand within a given area is sourced from more distant producers when infrastructure is improved, and hence shocks are spread over a greater area.

When we measure market-mediated treatment effects from localized experiments (even randomized ones), this same phenomenon will generate observed heterogeneity in the measured treatment effect across infrastructure quality. This heterogeneity will be present even if the true, total treatment effect is constant. To see this, we can think of a market as a grouping of a set of units into a single price-setting mechanism, so that shocks to one unit within a market are transmitted to the other units. Let the number of units per market be given by η, which proxies for infrastructure quality. A treatment induces a constant increase in demand equal to τ per unit, and this increase in demand is sourced on average from itself and the $\eta - 1$ other members of the market.

The increase in outcomes within a unit as a function of its own treatment is the part of the effect that does not spill over, namely $\frac{\tau}{\eta}$. In addition to the direct effect of treatment, each unit will receive an expected spillover effect equal to the indirect treatment effect from the number of individuals within the market who were treated. Writing the share treated as σ, then $\sigma\eta$ units per market will be treated and the expected spillover effect will be $\sigma\eta\frac{\tau}{\eta} = \sigma\tau$. The average treatment effect is given by the difference between treated and untreated units, or

$$E(Y \mid T) - E(Y \mid C) = (\frac{\tau}{\eta} + \sigma\tau) - \sigma\tau = \frac{\tau}{\eta}.$$

This says that the experiment measures not the total effect of treatment but only the component of it that does not spill over to other members of the same market. Now if we think of infrastructure (in our case roads) as being an intermediating variable that determines the size of the market, it can be thought of as determining the number of units on to which the treatment effect τ spills. In environments where the road network is excellent, η moves towards infinity and we have a single national market where the measured difference between treatment and control units is zero. With poor road infrastructure, consumption is localized to the spatial unit of treatment, η goes to one and the estimated difference between treatment and control converges on the true total treatment effect, τ. If what we set out to do with our experiment was to measure the total environmental impact of the treatment, then the error, meaning the difference between the true total treatment effect and the result of the micro-experiment is given by $\tau(\frac{\eta-1}{\eta})$, which vanishes as markets become completely autarkic.

In a sample with variability over the quality of local infrastructure, we will observe heterogeneity in impacts even when the actual treatment effect is constant. The reason for this differential is that spatial arbitrage removes the difference between treated and control units when the pixel size of treatment is small and transport costs are low. Under the assumption of homogenous treatment effects, such an argument implies that we only get the correct estimated treatment effect when spatial arbitrage is shut off. This argument is consistent with the results of Foster & Rosenzweig (2003), who observe a positive feedback effect of higher income on forest reserves only in closed economies, but not in open ones. Presumably the reason for this heterogeneity is that closed economies do not arbitrage their increased demand for forest products across global markets, and hence they manifest the full treatment effect on internal markets. In what follows we investigate the heterogeneity in impacts across infrastructural quality and confirm that our largest observed treatment effects occur precisely where they are the most localized.

3 Oportunidades and Deforestation: Overall Impact

3.1 Program description

The intention of Oportunidades is to increase school attendance and health care among poor families in Mexico. The financial scope of Oportunidades is large. The annual budget is approximately $2.6 billion a year, about half of Mexico's anti-poverty budget. It treats some four million households, providing cash transfers conditional on health care provision and school attendance. On average the transfers are about one-third of total income in these poor households, clearly meaningful income changes.

The program has been widely studied and lauded for its success in achieving these objectives (Schultz 2004, Fernald, Gertler & Neufeld 2008, Skoufias & McClafferty 2001). The transparent nature of its enrollment criteria and benefits has contributed to the attractiveness of the program, and it is currently being replicated in various other countries. The program was implemented in stages. A pilot implementation of the program (beginning in 1997) was randomized, and combined with detailed household-level data collection. The full rural roll-out of the program occurred mainly in 1998-2000, but new communities continued to enroll at a slower rate after this. This phase was not randomized, but was targeted to localities based on a marginality index; this created the discontinuity in treatment which we use. Eligible rural villages were first selected according to their level of marginality, and then surveys were conducted within villages to determine who would receive payments.

3.2 Data description

Our analysis of the national rollout focuses on rural localities[2]. We combine information on locality eligibility and program rollout with national deforestation data.

The spatial coordinates of each locality (village) in Mexico, along with the population and marginality index numbers for 1995, are from the National Institute of Geography and Statistics in Mexico (INEGI), and the data describing the roll-out of Oportunidades come from the Oportu-

[2]We exclude villages with more than 2,500 inhabitants as these are defined as "urban" communities in Mexico and were not eligible for the program until after 2000. Focusing only on rural localities means that we are likely to underestimate the total environmental impacts of Oportunidades because we are not taking into account possible consumption increases resulting from the urban roll-out.

nidades office. We have information on enrollment by village through 2003[3]. Locality-level eligibility for the program is based upon marginality indices calculated by CONAPO for 105,749 localities[4].

To measure deforestation at the locality level we rely on data from the Mexican National Forestry Commission (CONAFOR). The data are based on mosaics of Landsat satellite images from 2000 and 2003 (30 m resolution) and were created by CONAFOR under a mandate to accurately measure and monitor deforestation (Monitoreo Nacional Forestal). CONAFOR's data pieces together a large number of Landsat scenes in order to achieve wall-to-wall coverage for the entire country. This is in contrast to the method used by Foster and Rosenzweig (2003) which looks at forest cover for a representative sample of villages. Here we are measuring deforestation for all of the more than 105,000 localities with a marginality index in 1995[5]. We restrict the analysis to localities which had at least 10 hectares of land classified as forest in the 2000 National Forest Inventory, focusing on localities in which deforestation is possible[6]. Figure 1 shows the distribution of forest across Mexico in 2000. In order to assign each part of the landscape to a unique locality, we use the method of Thiessen polygons. (INEGI gives point data on the locations of each locality, but data on the detailed boundaries of the localities does not exist.) This method assigns land to localities based on the closest locality point and has the advantage of avoiding the problem of double counting caused by other shapes such as circles around each locality. Figure 2 shows a zoomed in picture of land use in 2000 along with the locality boundaries assigned by the Thiessen polygons method. Finally, because CONAFOR was primarily concerned with identifying areas of new deforestation, we do not have data on afforestation. We correct for this potential censoring problem in the data analysis by

[3] Although the bulk of enrollment in rural areas occurred before 2000, some villages were enrolled after this date. We include these villages although the presence of these villages, which were not enrolled according to the eligibility cutoff, potentially biases the results towards zero and against finding any impact of the program. Leaving them in the dataset therefore generates the most conservative estimates. Our results hold and are in fact stronger if we exclude villages enrolled in and after 2000 or before 1998

[4] By 2000, points were available for approximately 200,000 localities; the missing points in 1995 are very small localities: ninety-three percent of the villages for which there is no marginality index in 1995 had fewer than 25 inhabitants in 2000. The index is a continuous measure and was created using a principal components analysis based on seven variables from the 1995 Conteo (short census) and 1990 census, including illiteracy rates, dwelling characteristics, and proportion of the population working in the primary sector (Skoufias et al. 1999).

[5] The correct georeferencing and interpretation of Landsat data is a specialized and labor intensive process. After putting images together from several Landsat "scenes," the classification of deforestation is based on changes in the Normalized Difference Vegetation Index (NDVI) values across time. Comparisons are made using images from the dry season. NDVI is an indicator of vegetation cover and is used worldwide to measure changes in forest cover. Although NDVI change is the best available indicator of changes in forest cover, we note that the measure can have some errors due to weather shocks such as unusually high rainfall or drought conditions. These errors are in the dependent variable but are unlikely to be correlated with variation in treatment.

[6] The NFI data are based on a combination of remote sensing using Landsat images and field sampling to verify the classification system. The results are not sensitive to using lower thresholds.

using Tobit estimations. Practically speaking, we believe our measure picks up the key land use change dynamic of the study period because Mexico was a net deforester across this time. In fact, FAO's 2005 Global Forest Resources Assessment places Mexico in 13th place in the world in terms of net forest loss over the period 2000-2005 (FAO 2005). We present results using the percent of each locality deforested as the dependent variable, but all results in the paper are robust to alternative specifications of the dependent variable, including ln(total deforestation) and percent of baseline forest area deforested.

3.3 Illustrating the discontinuity

Figures 3 and 4 illustrate the variation in program enrollment and deforestation across the marginality index. The marginality index, which is continuous, is divided into bins with width = .1 for these illustrations. In each of these figures the left axis measures the percent of each locality deforested and the right the proportion of localities treated.

Figure 3 shows the relationship between enrollment, deforestation, and marginality for the full sample of localities[7]. As expected by program rules, we see a sharp increase in enrollment to the right of values of -1.2 on the marginality index. The discontinuity is not perfect – there is a small jump in enrollment before the eligibility criteria. This jump is due almost entirely to the enrollment of villages post-2000, when the program became more demand-driven[8].

Figure 3 also shows that deforestation rates vary with poverty in a roughly inverse-U relationship. This is an interesting confirmation of the empirical environmental Kuznets curve relationship: we see lower rates of deforestation for very poor communities (high marginality index), higher rates of deforestation for poor communities, and lower deforestation rates among less poor communities[9].

[7]It is important to note that the number of observations in each bin varies considerably across bins because the marginality index itself has frequencies which are roughly normally distributed. Therefore there are few observations per bin in the extreme bins and many more per bin towards the middle. This means that outliers have more influence on the points at either end of the marginality distribution.

[8]The proportion enrolled remains high for intermediate values of the marginality index and then is lower at high levels of marginality; we suspect that the decreases in enrollment at very high levels of marginality may be related to the fact that the very poorest villages may not have been eligible as a result of their lack of infrastructure.

[9]Note that because income is decreasing as we move to the right, a treatment that increases income is effectively pushing households to the left on this figure. The implication is that while the cross-sectional data are supportive of a Kuznets-style relationship (deforestation highest in the middle part of the distribution) the eligibility discontinuity lies above this value, and so if we took the Kuznets relationship to be causal, we would have expected an income increase in this part of the poverty distribution to decrease deforestation. This would appear to provide another piece in the already substantial body of evidence suggesting that cross-sectional Kuznets relationships do not depict a causal link between income and environmental changes.

Figure 4 zooms in on the range of the marginality index around the eligibility cutoff, showing the discontinuity more clearly. The figure uses a kernel regression to estimate the relationship between deforestation and the marginality index (the results are robust to larger and smaller windows). The data range in Figure 4 includes marginality levels from -2 to -.2, which constitutes 27% of the total sample with baseline forest and populations less than 2,500. This is referred to as the "restricted sample" in the sections that follow. We can see the clear increase in the proportion of localities to the right of -1.2. We also see the increase in deforestation rates around the discontinuity. Deforestation rates average around .03 percent on the richer end of the discontinuity, but once a locality becomes just poor enough to qualify for Oportunidades, average deforestation jumps to nearly .08 percent.

3.4 Empirical strategy

We observe a cross-sectional relationship between enrollment in Oportunidades by the year 2003, and deforestation between 2000 and 2003. One way to estimate the effect would be to apply OLS to the equation:

$$\Delta f_i = \alpha + \delta T_i + \beta' X_i + \varepsilon_i \tag{1}$$

where Δf_i represents the percent deforestation in polygon i over the period 2000-2003, T_i is equal to one if the locality associated with the polygon was enrolled in the program by 2003, X_i represents a vector of locality-level characteristics which might also affect deforestation, including poverty, and ε_i are unobserved factors affecting deforestation. If the program had been randomly assigned, then this would be an appropriate way to measure its effect on environmental outcomes. However, it is not randomly assigned; it is offered to those who are poor, and who may be likely to have different rates of deforestation even in the absence of the program. In addition, since enrollment in the program is voluntary, it is possible that those communities where enrollment is very high are systematically different than those where enrollment is very low – i.e., that selection problems could bias the estimates of the parameters in equation 1.

If the discontinuity is sharp, meaning that the rule for eligibility perfectly predicts treatment, then one can simply include the eligibility cutoff as a proxy for the treatment itself. In our case, this is a dummy variable (E_i) equal to one if the locality's marginality index exceeds -1.22. This corresponds to the boundary between "medium" and "low" levels of poverty, as classified by the marginality index. We use this simple approach in several specifications, noting that it captures the intention to treat effect, rather than the treatment effect on the treated.

Our situation differs from a sharp discontinuity in two ways. First, enrollment is not one hundred percent beyond any threshold. Second, the probability of enrollment increases rapidly over a range of the marginality index between -1.2 to -0.9. The first problem can be dealt with in the standard way by using the eligibility cutoff to instrument for the probability of enrollment[10]. We address the second problem following approaches developed by Hahn et al. (2001), Green (2005) and Jacob & Lefgren (2004). Nonlinear combinations of the eligibility rule and the marginality index (equation (3)) are used to instrument for treatment in the main regression. The two equations are given as:

$$\Delta f_i = \alpha + \delta T_i + \gamma I_i + \beta' X_i + \varepsilon_i \tag{2}$$

$$T_i = \omega + \tau_1 E_i + \tau_2 E_i I_i + \tau_3 M_i + \tau_4 M_i I_i + \mu I_i + \Gamma' X_i + \nu_i \tag{3}$$

where T_i represents treatment, E_i is the eligibility cutoff dummy, I_i is the marginality index and M_i is a dummy equal to one over the zone where enrollment increases rapidly and zero otherwise. Other variables are as defined above. Note that all specifications include a control for the marginality index, I_i, in order to control for the underlying relationship between deforestation and poverty. We also estimate results both for the full sample and a narrow window around the discontinuity. Within a narrow window around the discontinuity, it is reasonable to assume that the relationship between poverty and deforestation is linear. When we use a wider window, we include higher-order terms of the index (up to a fourth-order polynomial, following the example of (Lee, Moretti & Butler 2004). We also include additional controls, represented above by the vector X_i and including the size of the polygon in kilometers squared, the population in 1995, the percentage of the polygon that was forested in 2000, kilometers of roads in a 10 kilometer buffer around the locality ("road density"), and regional ecosystem dummy variables. Finally, in order to address issues surrounding the appropriateness of the IV Tobit estimator when the endogenous variable is binary (Wooldridge 2002, p. 546), we also estimate the equation substituting the continuous proportion of households treated in lieu of the binary treatment variable.

[10]For a review of regression discontinuity approaches, see Imbens & Lemieux (2008).

Valid estimates based on a regression discontinuity design rely on the assumption that the discontinuity in the outcome can be attributed to the discontinuity in treatment; i.e. there is not another unobservable variable which also changes discontinuously over the relevant marginality range which could be driving the results. To test this assumption, we analyzed all covariates using the kernel regression specification applied in Figure 4. No variables showed a significant jump at the discontinuity, with the exception of slope, which is slightly higher among the eligible population. Given that deforestation generally decreases with increases in slope, we feel that this strengthens our results. In addition, we control for slope in all specifications.

As a falsification test, we check for a discontinuity in forest cover around the eligibility cutoff prior to the start of the program, using data on 1994 land use. We find no difference in 1994 forest levels (measured in percent of polygon in forest) at the point of the discontinuity either visually or statistically[11].

3.5 Results

Table 1 presents some simple summary statistics from the two samples comparing average deforestation levels and other covariates across the eligibility criteria for the program. In both the full and restricted samples, there are significant differences in both the probability of deforestation and in the level. These simple comparisons of means across the running variable seem to indicate the presence of a jump in deforestation around the discontinuity. They do not, however, control for the underlying relationship between poverty and deforestation, nor do they control for any other covariates which might be correlated with both of these.

3.5.1 Simple approach

We first present results from the simplest approach of regressing deforestation outcomes on the eligibility cutoff as a proxy for treatment (i.e. intention to treat; which replaces T_i in equation 1 with E_i). Table 2 shows the results of this approach. The first three columns are estimated using a Tobit. Columns (1) and (2) show results from the full sample and the last column from

[11] Unfortunately, the data on 1994 forest areas is missing large tracts of land in northwest Mexico and in parts of the state of Guerrero; but at least 30,000 relevant observations remain. We also note that the classification of this data into land uses is not directly comparable with the 2000 Forest Inventory so we must use forest cover rather than changes in forest cover for this test.

the restricted sample (marginality index between -2 and .2). Column 1 includes in addition to the eligibility cutoff: the marginality index, the area of each locality, the baseline percentage of the locality in forest, locality population, road density, slope, and ecoregion controls. Column 2 shows results with a fourth order polynomial of the marginality index[12]. The third column shows results from the restricted sample and includes the marginality index linearly.

We see that the coefficients on eligibility are positive and significant (10% level) in all specifications, suggesting that the program increased deforestation. Marginal effects of eligibility on the probability of deforestation and on the rate of deforestation among deforesters calculated at the mean of the covariates are given at the bottom of Table 2. As a robustness check, we also consider OLS estimates of the probability of deforestation in a given polygon (column (4)), and on the percent deforested in those polygons with positive deforestation (column (5)). Note that the estimates from the linear probability model are nearly identical to the marginal impact of eligibility on the probability of deforestation estimated using the Tobit. The impact on percent deforestation among the deforesters is larger in the linear model that in the marginal effect estimated with the Tobit, but it is also not adjusted for the probability of deforestation in the sample.

Relying on this simple methodology, we also conduct a basic falsification test of the results using pseudo eligibility rules. We chose the eligibility cutoff based on the defined boundary between "medium" and "low" levels of poverty (-1.2). Using other cutoffs should not indicate deforestation effects. We re-run the specification in Column 2 of Table 2 on subsamples both to to the left and to the right of the discontinuity, but re-define eligibility at each tenth of the marginality index. We do not find any significant results using these placebo eligibility thresholds[13].

3.5.2 Instrumental variables approach

Results from the instrumental variables discontinuity approach are presented next. We begin by examining the predictive power of the instruments and then show the impact estimation results. Table 3 shows the results of the first stage OLS regressions (corresponding to equation (3)) of a dependent variable equal to one if the locality was treated by 2003. The first column tests the significance of the simple instrument of eligibility using the full sample, and columns 2-3 test the

[12]Results are robust to including just second and third order polynomials of the index as well.

[13]Results available upon request.

power of the set of fuzzy discontinuity instruments on the full sample. Column (4) shows results for the restricted sample. Column (5) shows an estimation of the fuzzy discontinuity variables on the proportion of households receiving Oportunidades in a locality between 1997 and 2003. The variables have the expected signs – being eligible for the program (in the zone above -1.2) increases the probability of enrollment, as does being in the marginal zone. The slope of the increase in probability of enrollment in the marginal zone is given by the interaction of the marginality index with the marginal zone, and is positive and significant as predicted. Estimations 3 and 5 include nonlinear terms of the marginality index. F-tests of the set of excluded instruments show that the instruments have excellent power.

Table 4 shows the estimated impact of the program on deforestation using the eligibility as the sole instrument. The results are consistent with those of the simplest approach, showing participation in the program increasing the probability and amount of deforestation. Two robustness checks in Table 4 warrant discussion. First, IV OLS is used in columns (5) and (6), and yields a nearly equivalent marginal effect of treatment on the probability of deforestation, and, as in the simplest approach, a slightly larger impact on percent deforestation. Column (3) uses a continuous variable to measure impact – the average proportion of the locality treated – and the marginal impact on the probability of deforestation is substantially larger than using the binary treatment. It is important to note, however, that the binary treatment variable should pick up the treatment for the average locality, which in terms of proportion treated is .42. Multiplying .120 by .42 yields a marginal effect estimate nearly identical to the marginal effect estimated using the binary treatment in column (2).

Table 5 shows the estimated impact of the program on deforestation using the fuzzy discontinuity approach. The estimates are similar to the simple approach. The marginal effects for the binary treatment indicate an increase in the probability of deforestation of 1.8 to 3.8 percentage points. Given the baseline probability of deforestation among the non-eligible population of 4.9%, this suggests nearly a doubling of deforestation probability around the discontinuity. The baseline percent deforested among deforesters in the non-eligible population is .6, which means that the marginal effects implied by the estimation amount to a 15-33% increase in the percentagearea deforested among deforesters.

The discontinuity results indicate that Oportunidades is associated with an acceleration of deforestation. Localities that received treatment show greater deforestation than localities with very similar poverty levels that did not receive treatment. In order to try to understand the household-level changes that might underlie these broader impacts, we turn to the evaluation data from the randomized pilot of the program.

4 Understanding Household Channels using a Randomized Trial

4.1 The Progresa data

The initial, experimental phase of Oportunidades was known as Progresa. The pilot phase featured a three-year period during which the intervention was directly randomized at the locality level. This evaluation design provides a unique opportunity to study the micro-foundations of the household production and consumption decisions that underlie the observed deforestation impacts. Of the pool initially identified for participation in the program, 506 localities were randomized into 320 "treatment" (initial intervention) and 186 "control" (delayed intervention) groups. Within each locality, households were assigned eligibility status for the program depending on their degree of poverty; eligible households within the treatment localities received the program. The experiment included several baseline and evaluation surveys that have been used in previous studies (see Skoufias (2005), Section 3 for a description of the evaluation design). For our analysis, we combine the 1997-98 baseline surveys with the 2000 follow-up survey which occurred at the end of the experimental phase.

Since the program was randomized among households in this dataset, we apply a difference in difference specification. We use the sample of eligible (poor) households to estimate direct treatment effects and the sample of non-eligible (non-poor) to estimate spillover effects:

$$y_{it} = \gamma_0 + \gamma_1 T_i + \gamma_2 P_t + \gamma_3 T_i P_t + \upsilon_{it} \tag{4}$$

where y_{it} is the household-level outcome variable related to consumption or production, T_i equals 1 if the household is in a treated locality, P_t is equal to one in the post-treatment period, $T_i P_t$ is the interaction of T_i and P_t, and υ_{it} is the household specific error. Because randomization was at the locality level we cluster standard errors at the locality level.

We test first for relevant consumption impacts of the program. Given the previous results by

Foster and Rosenzweig (2003), we might suspect that there would be an increase in demand for forest products. Since the survey does not contain direct measures of timber demand, we use measures of new housing construction (number of rooms) as a proxy for timber demand. Previous literature on the consumption impacts of Progresa has indicated that the program increased the intake of meat and animal products (Hoddinott & Skoufias 2004). Given the well-documented significant increase in the resources required to supply an animal-intensive diet (White (2000), Gerbens-Leenes & Nonhebel (2002), Bouma et al. (1998)) and the intense competition between cattle-rearing and forest resources in Mexico (Barbier & Burgess (1996), Kaimowitz (1995)) this seems a natural place to look for a demand-driven increase in pressure on forest cover. We therefore examine changes in consumption of beef and milk products.

As mentioned in Section 2, there is not necessarily a one-to-one relationship between the location of consumption changes and the corresponding production adjustment, but we might expect that some increased production could come directly from the treated households. We therefore assess changes by treated households in the number of cattle owned, number of plots of land that households report using for livestock grazing or agricultural purposes, and total area of all plots. Since these goods are also traded in markets, increased production could come from neighboring non-recipient households. Therefore, we also examine changes in production behavior by neighboring households were in treated localities but were not eligible for the program.

We would expect that the degree to which we should observe local production responses (and therefore local environmental consequences) depends on the extent to which local markets are connected. To this end, we will use road density (as measured by total kilometers of roads within a 10km buffer of each locality) as a proxy for market-connectedness. To test for heterogeneity, we include a second specification for each outcome variable which examines the interaction between treatment effect and the inverse road density in the locality (R_i):

$$y_{it} = \beta_0 + \beta_1 T_i + \beta_2 P_t + \beta_3 T_i P_t + \beta_4 R_i + \beta_5 R_i T_i + \beta_6 R_i P_t + \beta_7 R_i T_i P_i + \varepsilon_{it} \tag{5}$$

The coefficient β_7 measures the variation in the intention to treat effect according to infrastructure quantity.

4.2 Progresa results

The experimental household data confirm the findings in previous literature that Oportunidades strongly increased consumption of land-intensive resources (Hoddinott & Skoufias 2004). Table 6 shows regression results for demand-side outcome variables. We see no increase in the direct demand for timber products in the context of the home improvements proxy, but we do see increases in beef and milk consumption. The estimated treatment effects represent increases relative to the baseline mean of 29% and 23%, respectively. The interactions with road density however show that these demand-side impacts do not vary significantly with the quality of local road networks–it appears as though the treatment effect on consumption of these resource-intensive goods is homogeneous across infrastructure quality.

Table 7 presents production-side results on number of cows, total hectares of land in production and number of plots in production. The baseline distribution of total hectares in production is highly skewed so we use the natural logarithm of this variable in both specifications. We do not see significant increases in the number of cows owned, plots used, or the total area cultivated by recipient households, nor do these effects vary with road density[14]. Progresa does not appear to provoke a substantial increase in agricultural production among beneficiary households, regardless of the level of isolation[15].

The discussion in Section 2 motivates the analysis of market-mediated spillovers which may vary with the depth of local markets despite the very constant increases in consumption observed so far. In order to address this question using the Progresa data, we examine the extent to which non-recipient households (households that reside in eligible localities but who do not themselves qualify as poor) adjust their production behavior in response to the arrival of program transfers. In Table

[14] The results indicate that we can rule out increases in land use and cow ownership greater than 9% and 18% respectively, with 95% confidence. Given the 29% and 23% increase in beef and milk consumption, it seems unlikely that recipient households are supplying their entire increase in demand. Skoufias (2005) documents a significant decrease in child labor (not surprising given the conditionality of the program). Since this type of labor is disproportionately used on the family farm, this provides a possible reason for why households eligible for Progresa/Oportunidades may produce less on their own household farms and consume more goods produced elsewhere

[15] This result would seem to contradict the findings of Gertler, Martinez & Rubio-Codina (2006). In that study the authors show that recipient households do invest a small portion of Oportunidades transfers in livestock and land. However, they aggregate all animals into two categories: "production" animals which include cows, pigs, chickens, turkeys; and "draft" animals (horses, oxen). While they do find a significant increase in ownership of production animals, this appears to be driven by landless and non-agricultural households in their sample, indicating that the increase is unlikely to be due to large animals. Our data confirm this. We concentrate on the demand for animal protein but previous studies also suggested a diversification of fruit and vegetable consumption in response to the program Hoddinott & Skoufias (2004) which could also increase deforestation.

8 we observe that while the program does not have significant effects on production in this group overall, in road-poor areas there is a significantly stronger increase in the number of hectares under cultivation and in the number of cows owned by non-recipient households. The estimate of the coefficients on the interaction of inverse road density with the spillover effect in Column 4 indicates less than a one-percent increase in hectares in production at the 90th percentile of road density, and a 1.2% and 3.2% increase at the median and 10th percentile, respectively. The estimate of the same interaction effect on the number of cows owned (Column 6) indicates a 3% and 5% increase in the number of cows owned when evaluated at the 90th percentile and the median respectively, and a 12% increase when evaluated at the 10th percentile.

The micro-data from the randomized pilot phase of the program therefore provide evidence that the consumption increases caused by Progresa were similar across localities with different connection to markets, but the corresponding production increases among nearby wealthier households were not. Specifically, in localities with good road infrastructure there is no production-side response among local ineligibles, but where poor infrastructure localizes economic activity the increased consumption engendered by the program is met by an increase in output. This is in accordance with our hypothesis that even homogenous treatment effects will appear heterogeneous when they are mediated by markets of different sizes.

Given these estimated consumption increases by households, are the deforestation impacts previously estimated of a reasonable magnitude? To explore this question we conducted a back-of-the-envelope calculation using the marginal effects on milk and beef consumption combined with estimates of consumption and the resource intensivity of cattle-raising to estimate the additional land required[16]. Our simulation indicates that the average locality would require maintenance of eight additional cows, more than twice the number that Table 8 shows were being provided by ineligibles in local villages. This would suggest that even in isolated places more than half of demand

[16]Our simulation assumes each household consumes a quarter gallon of milk and a pound of beef each day they consume it, that a beef cow produces 400 pounds of beef and a dairy cow 1500 gallons of milk per year – these numbers in the US are 500-650 and 2400, respectively (Iowa State Extension Services 1994, United States Department of Agriculture 2009). Given the Progresa treatment effects, this gives us a number of beef cattle slaughtered over the 3-year period, and the incremental size of the dairy herd needed. We assume that 9 acres is needed to support a cow (midpoint of the estimates from Peel, Johnson & Mathews (2010)), and that the resource intensity of the counterfactual vegetable-based diet is 1/5th of the animal-based diet (Science Daily 2007), and this gives us the additional number of square kilometers needed for the dietary change: just under a quarter of a square kilometer per locality. The simulation of observed average deforestation per locality multiplies locality size times the fraction of localities in the treatment group with any deforestation and the marginal effect where deforestation occurs. The estimated deforestation is roughly a hundredth of a square kilometer per locality.

was being satisfied from production outside of the locality. If we then estimate the land required to support these cows, we come to a figure roughly 20 times the observed deforestation estimated in column 3 of Table 5. This demonstrates that the measured consumption increases are more than large enough to account for the observed deforestation. That the predicted amount of land needed is larger than the observed effects is not surprising, both because much of the marginal land is likely not to be forested and because the market-mediated spillovers cause us to underestimate total treatment effects[17].

5 Heterogeneity in the Impact of Oportunidades

5.1 Road Density and Treatment Effects

If the most plausible mechanism underlying an increase in deforestation is increasing demand for land-intensive goods, we should expect to observe heterogeneity in estimated treatment impacts across localities consistent with this mechanism. To this end, we test for variation in estimated effects by the quality of local transportation infrastructure. We expect that the estimated impact of the program should be greater where the supply response is more localized by poor infrastructure.

The problem of estimating responses when shocks can be dispersed through market transactions suggests that we will be more likely to detect impacts where road networks are poor. Table 9 shows the apparent differential impact of treatment at different categories of road density. The first six columns divide the entire sample into three equal sized groups according to road density. Results are shown for both IV OLS and IV Tobit specifications. Here we observe that the program only has a significant positive local impact on deforestation where road densities are low. We also see much larger point estimates for the marginal effects on the probability of deforestation for the low road density class. The results are nearly identical for the restricted sample (not shown). Columns 7 and 8 interact treatment with low road density for the full sample. We find the percent deforested difference to be marginally significant in the Tobit estimation (although the marginal effects in the low-density areas are several times those in the other groups), but isolated localities have a significantly higher probability of seeing some deforestation. The coefficient estimates for the sum

[17] This market demand mechanism between treated and ineligible households within treatment villages provides an alternative channel for the well-documented spillover effects of Progresa. Rather than working through peer effects (Bobonis & Finan 2009) or insurance and credit markets (Angelucci & de Giorgi 2009), ineligible households may have realized benefits by increasing output to satisfy local demand.

of the interaction with treatment in both the tobit and OLS estimates are almost identical to the estimates from the low road density sample.

5.2 Spatial ACFs in a RD framework

An alternate test of our hypothesis that production is sourced from surrounding markets is to examine the spatial contours of program effects directly. Since treatment is potentially endogenous, we cannot calculate spatial lag functions in the standard way. Instead, we adapt techniques introduced by Conley & Topa (2002) to the regression discontinuity framework. This mirrors the logic of the discontinuity analysis in that while the distribution of outcomes may be endogenous across the broader distribution of the eligibility score, it is plausibly exogenous within a window around the discontinuity.

The underlying information used here is the same as that used in the discontinuity analysis, but the structure of the data is slightly different. Here we divide the country in a grid of equally-sized cells of 10x10 km. For each cell we calculate deforestation and a "saturation" of treatment, which is composed of a ratio where the numerator is the number of villages out of the "study" localities that receive Oportunidades and the denominator is the number of "study" localities in the cell. We define a study village as one which is in the restricted subsample that we used for the discontinuity analysis, i.e., one which is located between -2 and -0.2 on the poverty index. This provides a conservative way of using "as if random" saturation in the intensity of treatment in the window around the discontinuity to measure spillover effects.

s_{i0} represents this saturation ratio in each cell, which we refer to as "own" saturation. For each cell, we then calculate saturation for all of the neighboring cells, excluding the own cell (saturation at 10 kilometers, s_{i10}). We proceed outwards in a similar fashion, calculating saturation in successive rings around a given cell up to 40 kilometers. We also calculate the density of road networks in the 50 kilometers surrounding each cell. We call this variable c_i and interact it with each of the saturation variables to help us understand how road access might affect the probability of deforestation. For areas which have no "study" localities in them, we include a dummy variable equal to one when there are no localities, and for these observations include zeros in the saturation observations[18]. We

[18]This follows Foster & Rosenzweig (2003)'s approach for dealing with missing data.

then drop all cells with no baseline forest cover and estimate:

$$d_i = \alpha + \sum_{k=0,10,20,30,40} [\beta_k s_{ik} + \theta_k s_{ik} c_i] + \Gamma X_i + \epsilon_i, \tag{6}$$

where $d_i = 1$ if there is deforestation in the cell, s_{ik} is the saturation at each distance, c_i is road density, X_i are control variables including average poverty level, road density within 0-50 kilometers around cell, latitude and longitude fixed effects, and baseline forest. ϵ_i is the error term. We calculate standard errors using bootstrapping in order to avoid the problem of spatial autocorrelation of error terms (for a discussion of spatial autocorrelation in the probit, tests, and estimation strategies, see Pinkse & Slade (1998)). Our theory tells us that deforestation should be most strongly correlated with nearby treatment intensity where infrastructure is poorest.

5.3 Spatial analysis: Results

The results from the spatial regression are shown in Table 10. The table contains only partial results – in all cases, 10 latitude/longitude fixed effects and the mean poverty level in each buffer is included, along with the variables indicating zero observations in a buffer. The fixed effects capture spatial variation in ecosystem, as well as cultural heterogeneity, to the extent that it varies geographically in Mexico. We use two variables capturing infrastructure quality: the natural log of total road density (measured as total length of roads in all the cells around a sample cell), and a dummy variable equal to 1 if the density is less than the median[19]. In the simplest specification, which does not include interactions of saturations with road density, saturations have no significant effect on the probability of deforestation. In the two versions where interactions are included, however, we observe that road density is very important in determining the effect of program concentration on deforestation, but that the key determinant is the interaction of saturation with infrastructure. In both cases, in more remote areas (those with low road density), the probability of deforestation as a result of Oportunidades recipients nearby increases.

Figure 5 graphs out the reported coefficients from column (2) by distance, calculating the interaction effects at 90% road density ("high") and at 10% road density ("low")[20]. The horizontal axis indicates the distance to the baseline cell in kilometers, and the plots include dotted lines in-

[19]Results are robust to various cutoff points less than the median as well.
[20]The graph looks nearly identical using coefficients from column (3).

dicating 95% confidence intervals. At each cell distance, the marginal effect is calculated for a one standard deviation change in saturation. This provides a visual image of the effect of the program on deforestation according to distance, and shows that the spatial contour of deforestation is not significantly different from zero with respect to the location of treatment for well-connected cells, whereas in isolated cells the deforestation effect is more localized – increases in saturation increase the probability of deforestation, but at a decreasing rate. The impact of increases in saturation goes to zero at the 20-30 kilometer band. This confirms our hypothesis that good infrastructure may help spread the impacts of the program to the point where they are non-detectable locally.

In summary, the results discussed above are consistent with the framework introduced in Section 2. Oportunidades appears to induce greater consumption of resource-intensive goods everywhere, and hence increases pressure on resources regardless of network quality. However, since treatment does not increase output among recipient households, this additional demand is mediated through market networks. With poor transportation infrastructure, demand must be met locally and so we see greater production responses. Where infrastructure is better, increases in demand will be sourced from a greater variety of locations.

6 Conclusions

This paper conducts an analysis of the impact of large income transfers on deforestation, taking advantage of the discontinuity created by the eligibility rule for Oportunidades. We find that the income transfer increases deforestation, at least in the population that is just below the marginality level required to be able to receive payments. We then use household data to test for a plausible mechanism consistent with this increase in forest loss. Here we observe that households increase their consumption of two relatively land-intensive goods – beef and milk. We do not detect a corresponding increase in consumption of a good that might increase forest cover through increasing demand for forest products– housing construction. Nor do we detect consistent changes on the production side triggered by exposure to Progresa, suggesting that the observed deforestation effects of the program arise from consumption changes, in other words through an expansion of each household's "ecological footprint" of land use.

Average household income increases by one-third as a result of the transfers, which leads the probability of deforestation to nearly double and the rate of deforestation among deforesters to increase by 15 to 33 percent. These increases are significant in the entire sample, but are strongest in places with poor infrastructure. These results underline the importance of considering spatial spillovers in the analysis of micro-experiments, and provide no support for the argument that increasing incomes will translate into improved environmental outcomes. Although we demonstrate that there were potential negative secondary environmental effects of the Oportunidades program, we cannot draw firm overall welfare conclusions. Welfare losses due to deforestation may have been outweighed by the health and education benefits of the Oportunidades program. In addition, a full welfare analysis of the program would take into account how long-term changes in income might affect environmental quality. Income growth may improve education or institutional quality, potentially leading to better environmental outcomes in the long term (e.g. Bhattarai & Hammig (2001)).

In recent years the use of local average treatment effects in the analysis of development program impacts has come under fire for answering small questions using a non-representative sample, and for obfuscating important sources of heterogeneity in outcomes (Deaton 2009). Although we estimate local average treatment effects in this paper, our use of the national rollout means that we have a very large and heterogeneous sample at the discontinuity. Therefore we are able to exploit the jump in program participation to cleanly identify impacts of poverty reduction but also to investigate a critical source of heterogeneity. Furthermore, the eligibility cutoff that we use for identification in this paper is close to the extensive margin of the actual program, and hence measures plausibly the impact of expanding the current program, as in Karlan & Zinman (2009). Hence we submit that the treatment effect estimated in this paper is both policy relevant and has substantial richness in terms of the analysis of heterogeneity.

In terms of the generalizability of these results, it is important to recognize the dimensions in which impacts of a CCT program may not reproduce the dynamics of a more endogenous long-term increase in income. Most obvious is the conditionality; it explicitly seeks to alter the prices faced by households in the use of one input to production, child labor. The program also features conditionality on regular health checkups for beneficiary children, and this increase in focus on their health may lead to dietary changes that would not be replicated with a simple increase in income. Further, Oportunidades payments are made monthly and hence provide a cash flow that may be more suited to consumption than investment. It is quite possible, for example, that an alternative program delivering the same total amount of cash to beneficiary households in one lump sum would have seen more investment and less consumption, particularly if credit markets are imperfect. Finally, no particular household receives Oportunidades payments for longer than they have children of eligible age, and so the program features a rolling beneficiary pool and is not likely to generate the real wealth effects that would be seen if permanent income had increased. Despite these caveats, CCT programs have emerged as a major policy tool in the fight against global poverty, and so to the extent that they present one of the most obvious policy levers for decreasing poverty our results are relevant even if we interpret impacts as limited to these programs.

Our findings, particularly the spatial contours of estimated treatment effects, motivate the idea that transportation infrastructure plays a critical role in determining the location of environmental impacts–i.e. where the "ecological footprint" lands. This underlines the empirical issues generated by spatial spillover effects when we examine the production response to market-mediated increases in local demand. A well-established result in the literature on rainfall shocks and on famines is the idea that infrastructure decreases the correlation between localized shocks and local market prices (Keller & Shiue 2008, Donaldson 2009). Extended to a program evaluation context, this logic suggests that when treatment is administered at small spatial units, market-driven spillovers cause an underestimation of the true harm from treatment. By this logic, the strong deforestation impact seen in isolated parts of Mexico when treated with Oportunidades is troubling, because it is precisely in these environments that we are closest to capturing the full impact of treatment. We therefore see these results not as a criticism of poverty-alleviation programs but rather as a cautionary tale. Should we wish to achieve increases in wealth simultaneously with improvements in environmental quality, our study suggests that carefully designed environmental management schemes should accompany poverty alleviation programs.

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7 Figures

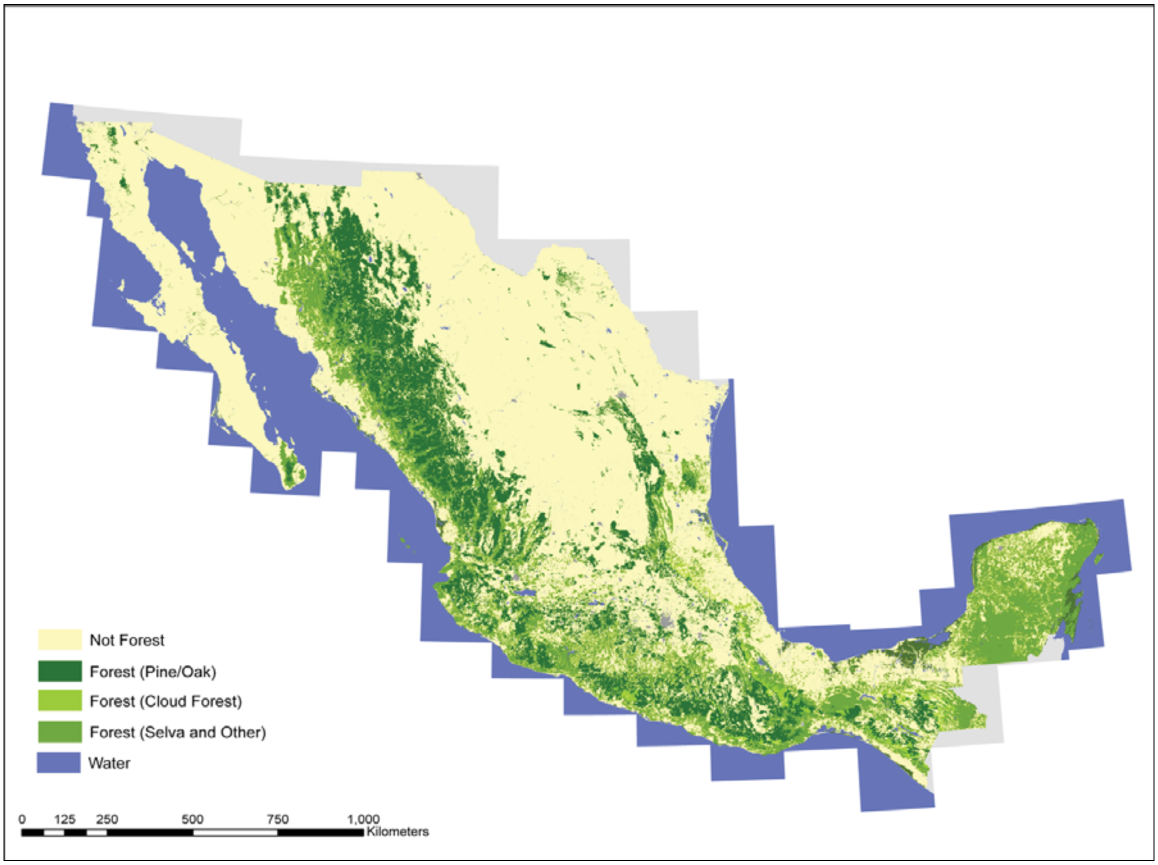

Figure 1: Forest Cover in Mexico, 2000

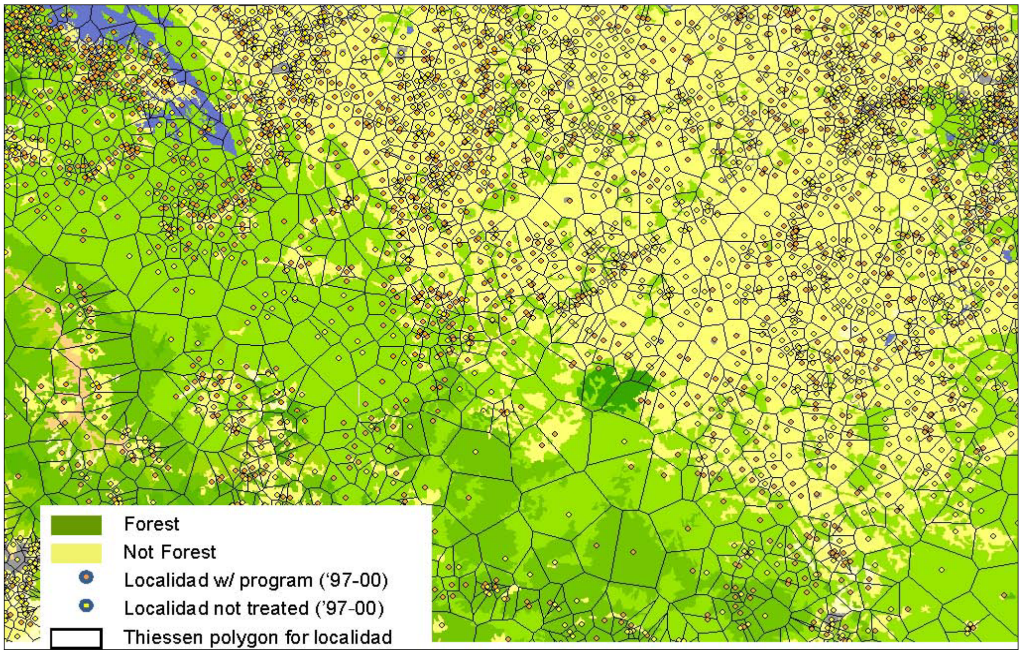

Figure 2: Thiessen Polygons

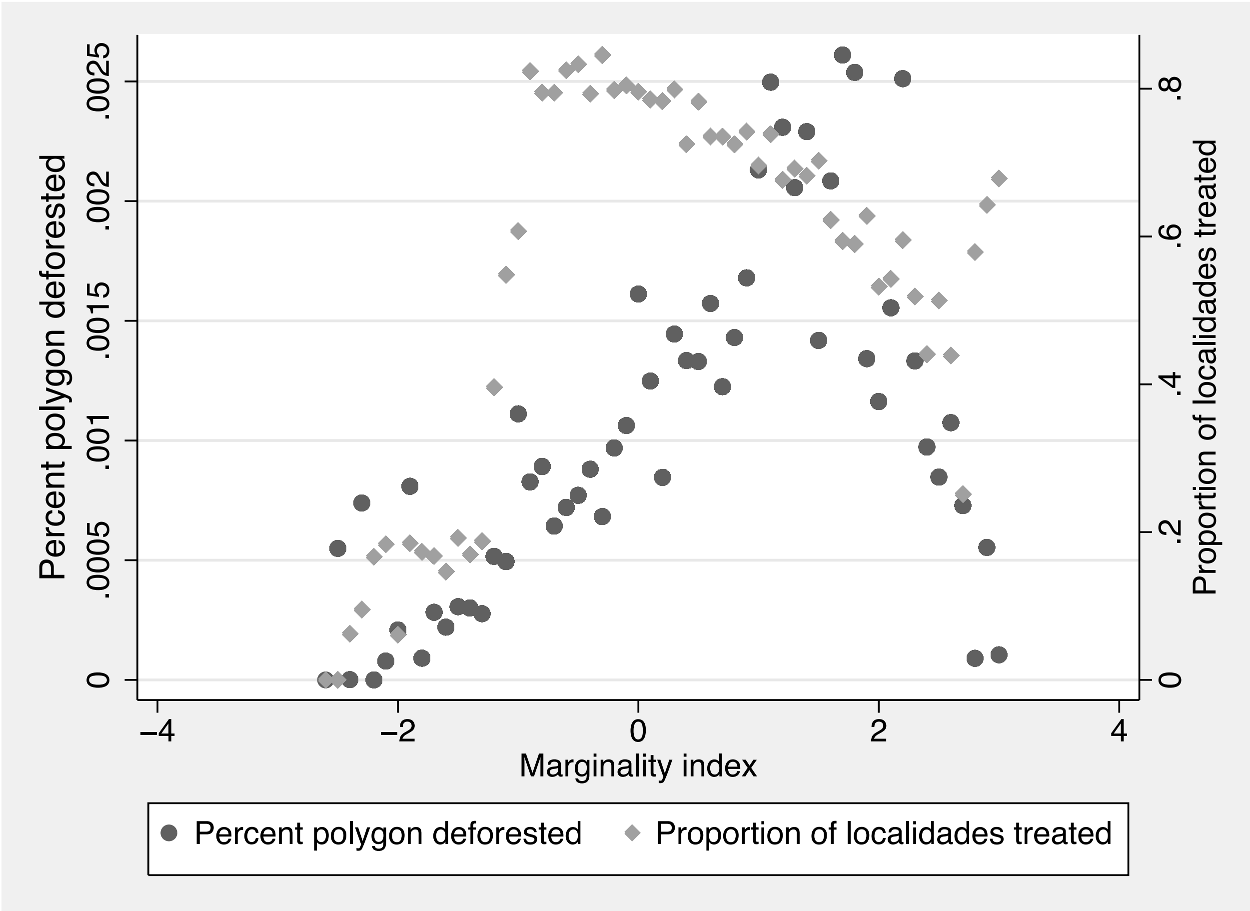

Figure 3: Entire sample minus observations with index > 3 (51 observations missing)

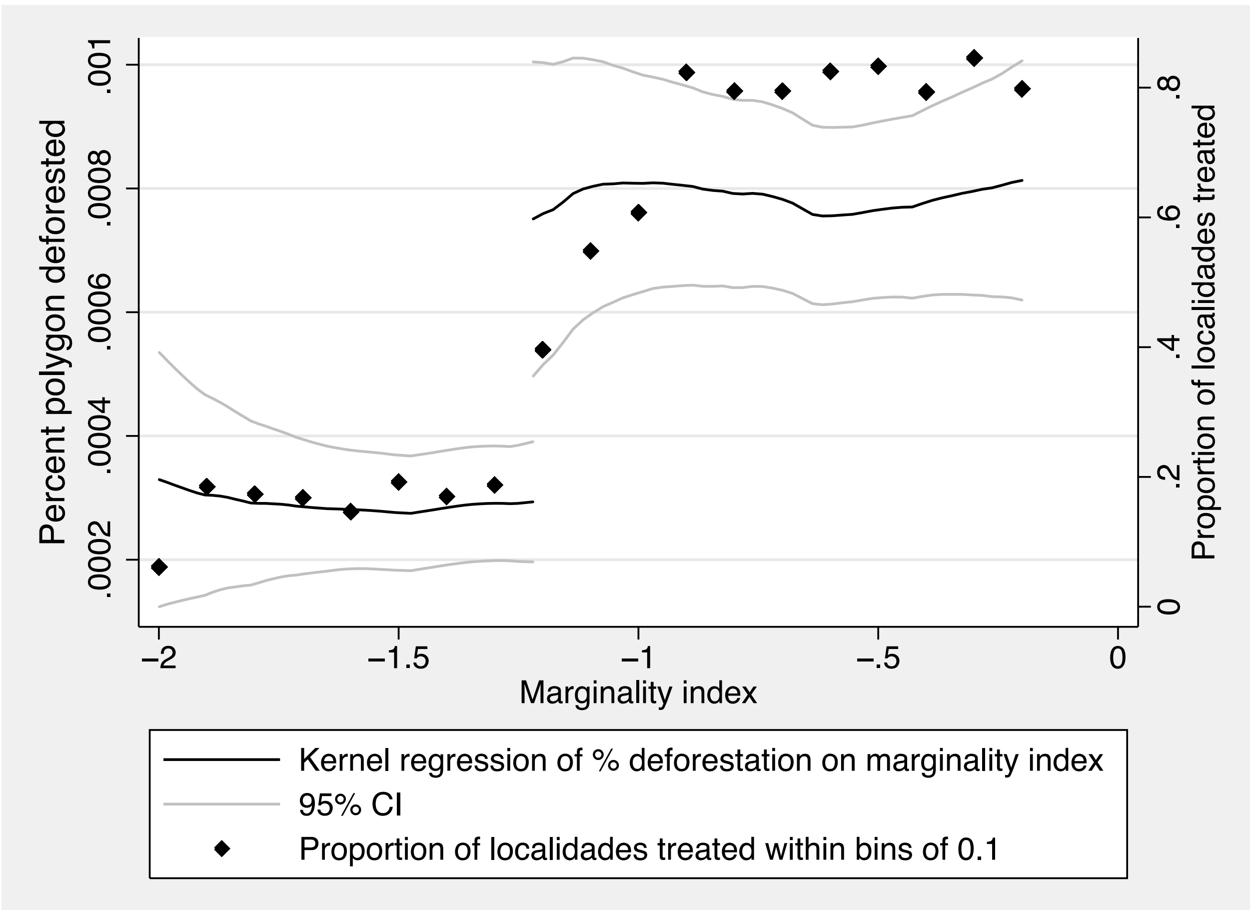

Figure 4: Kernel estimation of deforestation on marginality index – restricted sample

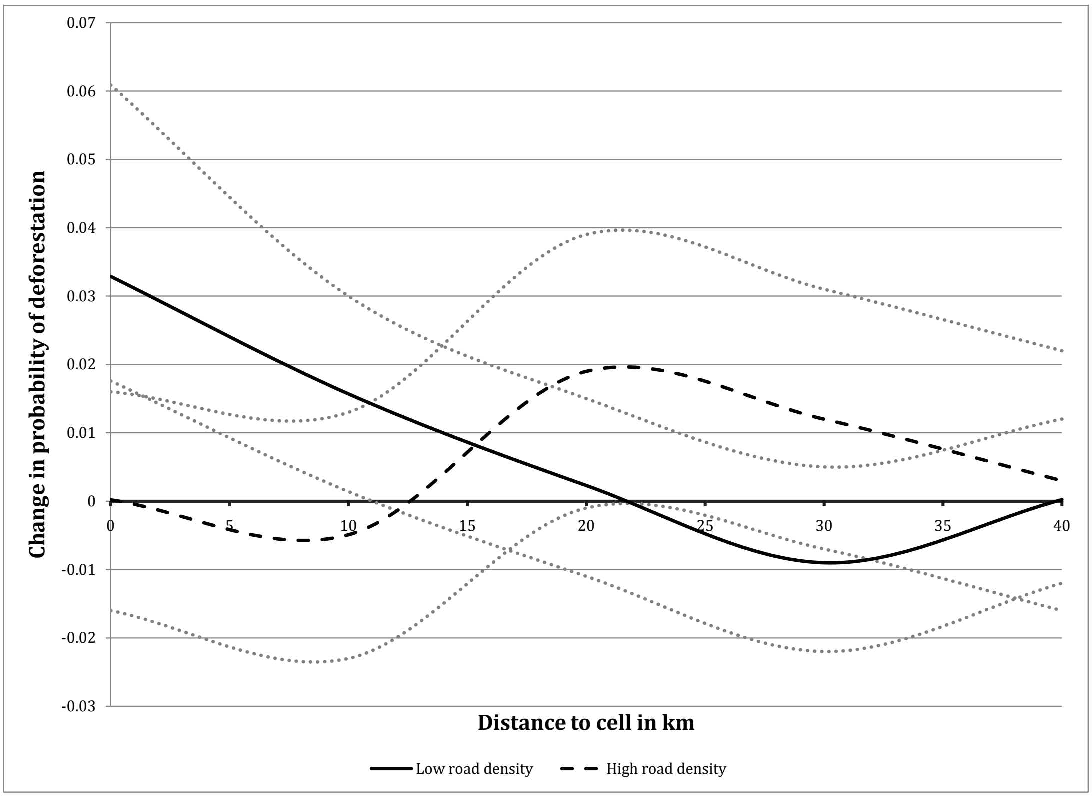

Figure 5: Own deforestation probability as a function of treatment within distance bands

8 Tables

Table 1: Summary statistics across eligibility

	Non-eligible <-1.2	Eligible $>= -1.2$	Test of difference	Normalized difference
Full sample				
Polygon area (km^2)	37.9	18.9	18.17	-.163
Average slope in polygon (degrees)	5.63	9.63	34.4	.482
% polygon forested in 2000	12.1	10.5	3.24	0.035
Km roads in 10 km buffer	47.0	32.7	32.7	-.36
% polygon polygon deforested	.0003	.0014	6.78	.11
Proportion with deforestation	.048	.098	9.64	
Observations	3510	55077		
Restricted sample				
Polygon area (km^2)	37.9	25.6	7.43	-.095
Average slope in polygon (degrees)	5.61	6.95	12.5	.18
Percent forested in 2000	12.2	10.4	3.37	-.042
Km roads in 10 km buffer	46.4	41.2	9.88	-.129
Proportion polygon deforested	.0003	.0008	4.14	.139
Proportion with deforestation	.049	.072	4.89	
Observations	3350	12408		

% polygon deforested measured as decimal.

Table 2: Simple approach – eligibility as proxy

	Tobit			OLS	
	% polygon deforested			Deforestation (0/1)	% deforested if 1
	(1)	(2)	(3)	(4)	(5)
Eligible	.004	.005	.004	.013	.004
	(.002)**	(.003)*	(.002)*	(.008)*	(.002)*
Marginality index	.005	.008	.002	.031	.0008
	(.0004)***	(.0008)***	(.002)	(.003)***	(.0008)
Index^2		.0006		.002	.0005
		(.0007)		(.003)	(.0008)
Index^3		-.001		-.004	-.0002
		(.0004)***		(.001)***	(.0003)
Index^4		-.00002		-.0001	-.0001
		(.0002)		(.0005)	(.0001)
Baseline area in forest, 2000	-3.72e-06	-4.78e-06	.00004	.0006	.00005
	(9.77e-06)	(9.78e-06)	(.00002)**	(.0001)***	(1.00e-05)***
Ln(polygon area)	.010	.010	.007	.046	-.010
	(.0004)***	(.0004)***	(.0007)***	(.002)***	(.0006)***
Ln(total population in 1995)	.001	.001	.0004	.010	-.0004
	(.0002)***	(.0002)***	(.0003)	(.001)***	(.0003)*
Ln(slope)	-.0005	-.0005	-.00009	-.003	-.0003
	(.00005)***	(.00005)***	(.0001)	(.0002)***	(.00006)***
Ln(road density)	-.0006	-.0006	.0003	-.004	-.0001
	(.0003)**	(.0003)**	(.0005)	(.001)***	(.0003)
Obs.	58587	58587	15758	58587	5551
Ecoregion controls	yes	yes	yes	yes	yes
Marginal effects of eligibility					
Pr($y > 0$)	.011	.015	.011	.013	
	(.005)**	(.021)**	(.007)*	(.008)*	
$y > 0$	.0006	.0008	.0005		.004
	(.0003)**	(.00042)**	(.0003)*		(.002)*

In column (4) the dependent variable is an indicator for any deforestation, and in column (5) is percent polygon deforested, but only for those polygons experiencing positive deforestation. Standard errors in parentheses. * significant at 10%; ** significant at 5%.

Table 3: First stage regressions

	Full sample			Restricted sample	Proportion treated
	(1)	(2)	(3)	(4)	(5)
Eligible	.645	.621	.843	.676	.751
	(.008)***	(.040)***	(.097)***	(.046)***	(.060)***
Marginal		1.117	1.041	1.077	.351
		(.087)***	(.089)***	(.087)***	(.052)***
Marginal x index		1.331	1.156	1.194	.416
		(.084)***	(.088)***	(.085)***	(.051)***
Eligible x index		-.063	.222	.078	.427
		(.025)**	(.081)***	(.034)**	(.051)***
Marginality index	.007	.041	-.189	.021	-.329
	(.002)***	(.025)*	(.083)**	(.029)	(.052)***
Index^2			-.046		-.073
			(.005)***		(.004)***
Index^3			.006		.010
			(.005)		(.003)***
Index^2			-.001		.0003
			(.001)		(.0009)
Baseline area in	.0003	.0003	.0003	.0006	.0002
forest, 2000	(.00009)***	(.00009)***	(.00008)***	(.0001)***	(.00007)***
Ln(polygon area)	-.030	-.029	-.029	-.027	-.034
	(.002)***	(.002)***	(.002)***	(.004)***	(.002)***
Ln(total population in 1995)	.158	.158	.159	.129	.119
	(.001)***	(.001)***	(.001)***	(.002)***	(.0008)***
Ln(slope)	.004	.004	.003	.003	.003
	(.0003)***	(.0003)***	(.0003)***	(.0005)***	(.0002)***
Ln(road density)	.028	.028	.028	.009	.016
	(.001)***	(.001)***	(.001)***	(.003)***	(.001)***
Obs.	58587	58587	58587	15758	58587
Adjusted R-squared	.314	.330	.334	.462	.336
Ecosystem controls	yes	yes	yes	yes	yes
F-test of instruments		2017	425	349	239

The dependent variable in columns (1)-(4) is equal to 1 if the locality received Oportunidades before 2004, and 0 otherwise. In column (5), the dependent variable is the average proportion of households receiving Oportunidades from 1997 to 2003, inclusive. All estimates are completed in OLS. Robust standard errors in parentheses. * significant at 10%;** significant at 5%; *** significant at 1%

Table 4: Simple discontinuity approach – instrumentation with eligibility

	IV Tobit				IV OLS	
	Full estimation sample			Restricted sample	Deforestation (0/1)	% deforested if 1
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	.006	.013		.010	.031	.013
	(.003)**	(.007)*		(.006)*	(.019)*	(.007)*
Proportion treated			.038			
			(.021)*			
Marginality index	.005	.006	.002	-.0007	.028	-.00005
	(.0004)***	(.001)***	(.003)	(.003)	(.003)***	(.001)
Index^2		.002	.004		.004	.002
		(.001)	(.002)*		(.004)	(.001)
Index^3		-.0009	-.0005		-.003	-.0004
		(.0003)***	(.0003)		(.001)***	(.0003)
Index^4		-.0001	-.0004		-.0003	-.0002
		(.0002)	(.0002)*		(.0005)	(.0001)
Baseline area in forest, 2000	-5.11e-06	-7.95e-06	-1.00e-05	.00003	.0006	.00004
	(9.75e-06)	(9.96e-06)	(1.00e-05)	(.00002)**	(.0001)***	(1.00e-05)***
Ln(polygon area)	.010	.010	.011	.008	.047	-.009
	(.0004)***	(.0005)***	(.0008)***	(.0007)***	(.002)***	(.0007)***
Ln(total population in 1995)	.0005	-.0006	-.003	-.001	.005	-.003
	(.0005)	(.001)	(.002)	(.0009)	(.003)	(.001)**
Ln(slope)	-.0005	-.0006	-.0006	-.0001	-.003	-.0003
	(.00005)***	(.00006)***	(.00008)***	(.0001)	(.0002)***	(.00007)***
Ln(road density)	-.0008	-.0009	-.001	.0002	-.005	-.0005
	(.0003)***	(.0003)***	(.0004)***	(.0005)	(.001)***	(.0004)
Obs.	58587	58587	58587	15758	58587	5545
Ecoregion controls	yes	yes	yes	yes	yes	yes
Marginal effects of treatment						
$\Pr(y > 0)$	.018	.038	.12	.030	.031	
	(.008)**	(.019)**	(.067)*	(.017)*	(.019)*	
$y > 0$	.0009	.002	.005	.001		.013
	(.0004)**	(.001)*	(.003)*	(.0008)*		(.007)*

Columns (1)-(4) are estimated using IV Tobit, and use a dependent variable of % polygon deforested. Columns (5)-(6) are estimated using OLS, with column (5) using a binary dependent variable indicating deforestation, and (6) % deforestation in polygons with positive deforestation. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Fuzzy discontinuity estimates

	IV Tobit				IV OLS	
	Full estimation sample			Restricted sample	Deforestation (0/1)	% deforested if 1
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	.006 (.003)**	.012 (.006)**		.010 (.005)**	.035 (.015)**	.009 (.005)*
Proportion treated			.022 (.012)*			
Marginality index	.005 (.0004)***	.007 (.0009)***	.005 (.002)**	-.0007 (.003)	.028 (.003)***	.0002 (.0009)
Index^2		.002 (.001)	.002 (.001)		.005 (.003)	.001 (.001)
Index^3		-.0009 (.0003)***	-.0006 (.0003)**		-.003 (.001)***	-.0002 (.0003)
Index^4		-.00008 (.0002)	-.0002 (.0002)		-.0003 (.0005)	-.0002 (.0001)
Baseline area in forest, 2000	-5.06e-06 (9.75e-06)	-7.74e-06 (9.89e-06)	-9.81e-06 (1.00e-05)	.00004 (.00002)**	.0006 (.0001)***	.00005 (1.00e-05)***
Ln(polygon area)	.010 (.0004)***	.010 (.0005)***	.010 (.0006)***	.008 (.0007)***	.047 (.002)***	-.010 (.0006)***
Ln(total population in 1995)	.0006 (.0005)	-.0005 (.0009)	-.001 (.001)	-.001 (.0007)	.004 (.003)	-.002 (.0008)**
Ln(average slope)	-.0005 (.00005)***	-.0006 (.00005)***	-.0006 (.00007)***	-.0001 (.0001)	-.003 (.0002)***	-.0003 (.00006)***
Ln(road density)	-.0007 (.0003)***	-.0009 (.0003)***	-.0009 (.0003)***	.0002 (.0005)	-.005 (.001)***	-.0004 (.0003)
Obs.	58587	58587	58587	15758	58587	5545
Ecoregion controls	yes	yes	yes	yes	yes	yes
Marginal effects of treatment						
Pr($y > 0$)	.018 (.007)***	.037 (.016)**	.070 (.039)*	.030 (.013)**	.035 (.015)**	
$y > 0$	.0009 (.0004)**	.002 (.0008)**	.003 (.0019)*	.001 (.0006)**		.009 (.005)*

Columns (1)-(4) are estimated using IV Tobit, and use a dependent variable of % polygon deforested. Columns (5)-(6) are estimated using OLS, with column (5) using a binary dependent variable indicating deforestation, and (6) % deforestation in polygons with positive deforestation. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Household-level Consumption Impacts, Progresa

	Rooms in home		Days Ate Beef		Days Drank Milk	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	.014	.017	.114	.118	.337	.331
	(.033)	(.035)	(.030)***	(.031)***	(.081)***	(.087)***
Treatment x inverse road density		-.034		-.070		.183
		(.148)		(.097)		(.669)
Village chosen to receive Progresa	.0001	.002	-.025	-.031	-.133	-.143
	(.037)	(.038)	(.029)	(.030)	(.111)	(.118)
Post treatment year	.053	.049	-.137	-.138	-.655	-.664
	(.028)*	(.029)*	(.024)***	(.025)***	(.061)***	(.065)***
Inverse of road density		.266		-.156		.051
		(.169)		(.069)**		(.499)
Village x inverse road density		.043		.102		.232
		(.236)		(.140)		(.682)
Post treatment x inverse road density		.067		.016		.155
		(.140)		(.068)		(.252)
Obs.	23318	23318	33128	33128	33128	33128
Mean dependent variable in baseline	1.557		0.388		1.440	
	(0.930)		(0.661)		(2.367)	

* significant at 10% ** significant at 5%; *** significant at 1%

Table 7: Household-level Production Impacts, Progresa

	No. of Plots		Log 1+ Total Hectares		No. of Cows	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	.030 (.039)	.031 (.040)	-.014 (.038)	-.015 (.039)	.092 (.057)	.036 (.057)
Treatment x inverse road density		-.107 (.210)		.142 (.223)		.936 (.522)*
Village chosen to receive Progresa	.014 (.056)	.037 (.057)	-.004 (.040)	.017 (.040)	-.004 (.087)	.058 (.085)
Post treatment year	-.094 (.032)***	-.077 (.033)**	.312 (.033)***	.317 (.033)***	-.239 (.046)***	-.180 (.046)***
Inverse of road density		.833 (.161)***		.820 (.227)***		2.122 (.799)***
Village x inverse road density		-.263 (.317)		-.217 (.258)		-.760 (.872)
Post treatment x inverse road density		-.275 (.149)*		-.235 (.128)*		-.982 (.402)**
Obs.	45087	45087	32631	32631	34248	34248
Mean dependent variable in baseline	0.824 (0.955)		1.724 (3.535)		0.604 (2.304)	

* significant at 10% ** significant at 5%; *** significant at 1%

Table 8: Local Spillover Impacts of Progresa
Impacts on Ineligible Households in Treatment Villages

	No. of Plots		Log 1+ Total Hectares		No. of Cows	
	(1)	(2)	(3)	(4)	(5)	(6)
Spillover effect	-.001	-.017	-.037	-.052	.153	-.021
	(.038)	(.040)	(.041)	(.042)	(.125)	(.125)
Spillover x inverse road density		.372		.535		3.605
		(.240)		(.167)***		(1.243)***
Village chosen to receive Progresa	.042	.094	-.015	.022	-.121	.052
	(.055)	(.056)*	(.047)	(.047)	(.219)	(.215)
Post treatment year	-.208	-.202	.254	.256	-.702	-.551
	(.028)***	(.028)***	(.034)***	(.034)***	(.108)***	(.106)***
Inverse of road density		1.009		1.207		6.036
		(.196)***		(.387)***		(2.021)***
Village x inverse road density		-1.051		-.620		-2.846
		(.249)***		(.430)		(2.395)
Post treatment x inverse road density		-.119		-.257		-3.060
		(.159)		(.122)**		(1.180)***
Obs.	40569	40569	30068	30068	31184	31184
Mean dependent variable in baseline	1.031		2.844		1.577	
	(1.667)		(5.322)		(4.675)	

* significant at 10% ** significant at 5%; *** significant at 1%

Table 9: Deforestation and infrastructure

	Low density		Medium density		High density		Interactions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	%	0/1	%	0/1	%	0/1	%	0/1
Treated	.016	.075	.006	.019	.018	.023	.008	.008
	(.009)*	(.037)**	(.008)	(.030)	(.015)	(.021)	(.006)	(.015)
Treated x low road density							.010	.059
							(.005)*	(.017)***
Low road density							-.004	-.028
							(.004)	(.012)**
Obs.	19529	19529	19529	19529	19529	19529	58587	58587
Ecoregion controls	yes	yes	yes	yes	yes	yes	yes	yes
Marginal effects of treatment								
$\Pr(y > 0)$	.080		.020		.026			
	(.040)**		(.030)		(.019)			
$y > 0$	.002		.001		.002			
	(.001)**		(.001)		(.002)			
							.018+	.067+
							(.007)***	(.020)***

Partial results. All estimations contain full set of covariates. % represents percent polygon deforested, 0/1 = 1 if any deforestation. IV Tobit used for columns with % deforested as dependent variable, IV OLS used for columns with 0/1 dependent variable. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. + indicates the sum of the treatment coefficient with the treatment and low road density interaction.

Table 10: Spatial regressions
Dependent variable = 1 if deforestation

	(1)	(2)	(3)
Own saturation	.017	.192	-.006
	(.018)	(.044)***	(.019)
Within 10-20 km	.025	.120	-.027
	(.019)	(.047)**	(.033)
Within 20-30 km	.038	-.070	.067
	(.023)*	(.067)	(.056)
Within 30-40 km	-.006	-.154	.066
	(.028)	(.079)*	(.076)
Within 40-50 km	.012	-.014	-.068
	(.031)	(.080)	(.075)
Ln(road density, 0-50km)	-.027	-.081	
	(.016)*	(.039)**	
Baseline forest	.001	.001	.001
	(.0002)***	(.0002)***	(.0002)***
Density x own saturation		-.090	
		(.020)***	
Density x 10-20 km		-.063	
		(.029)**	
Density x 20-30 km		.068	
		(.045)	
Density x 30-40 km		.099	
		(.051)*	
Density x 40-50 km		.013	
		(.054)	
Density < median			-.061
			(.044)
Density < median x own saturation			.058
			(.020)***
Density < median x 10-20 km			.069
			(.032)**
Density < median x 20-30 km			-.043
			(.058)
Density < median x 30-40 km			-.093
			(.079)
Density < median x 40-50 km			.095
			(.078)
Obs.	11007	11007	11007
R^2	.195	.198	.196
Lat-long fixed effects	yes	yes	yes

OLS with bootstrapped standard errors. ** significant at 5%; *** significant at 1%. These are partial results. Regressions also contain the average poverty index from 0-50 kilometers, ln(total population) in each band, and band level dummy variables indicating zero observations within "study" sample in the band.