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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 increases demand for resource-intensive goods. The corresponding production response and deforestation increase is more detectable in communities with poor road infrastructure. These results are consistent with the idea that better access to markets disperses environmental harm and the full effects of treatment can only be observed where poor infrastructure localizes them.

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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. Grossmand & 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 raise demand for environmental resources, inducing households to invest in those resources, or raise the opportunity cost of extractive activities. To date, empirical work on the environmental effects of poverty alleviation has been significantly limited by the possible endogeneity of household income changes (as noted in a recent review report (World Bank 2008)). 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 on environmental degradation.

Previous work has also not adequately considered the estimation of 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. How impacts are mediated through markets is a fundamental causal inference issue and is usually difficult to disentangle because markets are relatively homogenous. Here we take advantage of large variation in transportation infrastructure to investigate whether 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 150,000 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 support 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 in 1998-2000 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 2008, 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%, suggesting increases in an already high likelihood of deforestation. Among communities who do deforest, the results indicate an increase in the *rate* of deforestation ranging from 17 to 30 percent. To understand the micro-behavior that underlies this increase in deforestation, we turn to household data from the randomized pilot phase of the program: the Progresa evaluation sample. The experimental data show that the additional household income significantly increases consumption, and recipient households shift strongly into resource-intensive goods such as beef and milk.

We also find that although consumption increases appear constant across localities, the corresponding production increases and thus deforestation patterns are not. Consistent with the idea that transportation infrastructure is a significant determinant of the spatial profile of market-mediated production impacts, we find larger deforestation effects in treated localities that have poor road infrastructure and thus are more isolated from outside markets. We also find corroborating evidence at the household-level of a production response only in treated localities which are more isolated. Finally, we investigate spatial spillovers of treatment using a new method for calculating spatial auto-correlation 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. Overall, our results are consistent with the idea that broader markets may simply disperse environmental harm.

Our results suggest that there are significant environmental impacts of poverty alleviation. The increase in deforestation may be caused indirectly 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, households invest in forest resources and deforestation rates slow. It implies instead that 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 likely to fail 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

[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.

the Oportunidades program in more detail, and presents the estimation strategy and results of the 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 careful 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. Much theoretical work has examined the first two issues, but remains largely inconclusive. Reliable predictions regarding the sign of any causal link rely on a thorough understanding of the relevant household decision process. Unfortunately, many of the channels through which household decisions might affect the environment lead to ambiguous predictions, rendering the question almost exclusively an empirical one. Empirical analysis of the relationship, however, introduces the third issue. When the household behavior change that drives any potential environmental impact passes first through local markets, detection of any such impact relies heavily on the extent to which local markets are connected to outside national and global markets. We focus here on forests as an environmental outcome of interest. For the remainder of this section we discuss in more detail the theoretical and empirical work that has examined the relationship between poverty and deforestation, the relevant household decisions, and our contribution to this literature, concluding with a discussion of the detection of the effects of income changes on local environmental outcomes when impacts can be dispersed.

2.1 Does alleviating poverty increase or decrease forest cover?

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).

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)). 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 – the magnitudes may not be large enough to correspond to realistic poverty-reduction goals. 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 affect deforestation. Many of these could apply in the case of programs designed to alleviate poverty by improving incomes. Foster & Rosenzweig (2003) propose that higher incomes will increase demand for forest products which will induce a supply response by households or communities where there is clear ownership of forest resources. In this case we would expect a conditional cash transfer program to result in less deforestation. 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). If poverty alleviation programs also reduce credit constraints, this mechanism would be relevant. Zwane (2007)'s model proposes different deforestation effects of income increases at high and low initial levels in part because of borrowing constraints. Across two periods of decision-making, an exogenous increase in income decreases borrowing in the first period, thereby increasing the money available to purchase agricultural inputs in the second period, and hence the value of cleared land. 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.

An advantage of using Oportunidades as a case study is that it was preceded by a randomized pilot program, Progresa. This experimental design, along with the rich household surveys that were conducted as a part of the program, allow us to unpack the household decisions corresponding to observed aggregate deforestation impacts. The evidence in the Mexican case leads us to propose a new indirect mechanism by which higher incomes increase deforestation. It is similar to Foster & Rosenzweig (2003) in that higher incomes increase demand for consumption goods. However, here increased demand increases deforestation because the production of the relevant goods requires more cleared land rather than more forested land. Notably, over the same period that India experienced net gains in forest, Mexico experienced net losses. We assume that food goods produced by low-income households are inferior relative to other goods. As household income increases, households substitute away from these inferior goods (e.g. beans) to normal goods (e.g. beef). If the normal good (beef) is more resource intensive than the inferior good (beans) then households will increase their "ecological footprint" as they become richer, resulting in additional deforestation.

2.3 The ecological footprint of market-mediated shocks

In order to test our hypothesis that 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. The initial 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 villages were first selected according to their level of marginality, and then surveys were conducted within villages to determine who would receive payments. 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.

3.2 Data description

To conduct the analysis we merge information on localized deforestation with the program evaluation sample of Progresa, the full national eligibility and rollout data for Oportunidades, and a variety of other sources. Our unit of analysis is the locality. Locality-level eligibility for the program is based upon marginality indices calculated by CONAPO, which were created for 105,749 of the approximately 200,000 localities[2].

The spatial coordinates of each 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 Oportunidades office. We have information on enrollment by village through 2003[3].

To measure deforestation at the locality level we rely on data from the Mexican National Forestry

[2]Ninety-three percent of the villages for which there is no marginality index 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).

[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 examine the subsample of villages enrolled over 1998-1999, when the bulk of the enrollment took place.

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 across the whole country (Monitoreo Nacional Forestal). CONAFOR's data pieces together a large number of 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[4]. In addition, because CONAFOR was primarily concerned with identifying areas of new deforestation, the 2000-2003 analysis does not include information on afforestation. We correct for this potential censoring problem in the data analysis. Practically speaking, we do not believe that afforestation is a major concern. Mexico was not a net afforester during this period. 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 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[5]. Figure 1 shows the distribution of forest across Mexico in 2000.

Program eligibility was defined at the locality level. We have point data on their locations, however data on the boundaries of the localities do not exist. In order to assign each part of the landscape to a unique locality, we use the method of Thiessen polygons. 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.

[4] 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.

[5] The NFI data are based on a combination of remote sensing using Landsat images and field sampling to verify the classification system.

3.3 Empirical strategy

We observe a cross-sectional relationship between enrollment in Oportunidades by the year 2003, and suspected 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 higher 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, understanding 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, I, 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[6]. We address the second problem by using a fuzzy discontinuity strategy, following approaches developed by Green (2005), Jacob & Lefgren (2004) and Hahn et al. (2001) . Nonlinear combinations of the eligibility

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

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. The vector X_i generally includes 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, and regional dummy variables.

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 generally assume that this relationship is approximately linear over the small range of marginality around the discontinuity which is relevant. However, we also experiment by including higher-order terms (up to a fourth-order polynomial) to control for potential non-linear effects.

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 relationship between enrollment, deforestation, and marginality for the full sample of localities. 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. 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[7].

[7]The proportion enrolled remains high for intermediate values of the marginality index and then is lower at high

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[8].

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). 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.) 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 .0004 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 .0008 percent.

We have demonstrated visually that there appears to be a significant discontinuity in both treatment and outcome variables. The validity of regression discontinuity relies 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 that could be driving the results. 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[9].

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.

[8]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.

[9]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.

3.4 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. There are also significant differences in covariates. Two points are important to note relative to these differences. First, forest and polygon area are both larger in the non-eligible group than the eligible group, while slope is lower. Basic resource economics would suggest then that we should observe higher deforestation amongst the non-eligible population, given that they have better land and more resources to exploit. This is not what we observe. Second, while many of the differences are significant using standard tests of difference (t- and chi-squared tests), these tests depend upon sample size. If we consider normalized differences of means, which are independent of the number of observations, we observe no covariate between which the difference in means even approaches half a standard deviation.

3.4.1 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, using Tobit estimation. The first two columns show results from the full sample and the last column from 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 (Road density is calculated as the kilometers of roads within a ten kilometer buffer around each locality.) Column 2 adds up to a fourth order polynomial of the marginality index[10]. The third column shows results from the restricted sample, which includes the marginality index linearly.

We see that the coefficients on eligibility are positive and significant (10% level) in all specifi-

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

cations, 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. To interpret these magnitudes, recall that among non-eligible localities, the probability of deforestation is 4.9%. The calculated marginal effect on the probability of deforestation is to increase this probability by approximately 0.1-1.3 percentage points, or 2-22 percent of the mean. This suggests a considerable potential increase in the likelihood of deforestation. At a minimum, the estimated marginal effect of intention to treat on deforestation among the deforesters is .0005. Given the baseline rate of deforestation amongst ineligible deforesters (.006), this change constitutes around a 8.3% increase.

Relying on this simple methodology, we also conduct a simple 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 (-1.22). 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[11].

3.4.2 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 2-4 test the power of the set of fuzzy discontinuity instruments on the full sample. The last column shows results for the restricted sample. 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 2 – 4 include nonlinear terms of the marginality index. F-tests of the set of excluded instruments show that the instruments have excellent power.

[11] Results available upon request.

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. Table 5 shows the estimated impact of the program on deforestation using the fuzzy discontinuity approach. The estimates are nearly identical to the simple approach. The marginal effects suggest an increase in the probability of deforestation of 1.8 to 3.7 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 area deforested among deforesters in the non-eligible population is .006, which means that the marginal effects implied by the estimation amount to a 17-33% increase in the area 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. 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. A large and careful literature exists on the consumption impacts of Progresa, and the program is found to have increased the intake of meat and animal products (Hoddinott & Skoufias

2004)[12]. Of primary interest in our analysis is not this previously established intention to treat effect, but whether we observe heterogeneity in this effect across the degree 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. We examine changes in consumption of beef and milk products, and how these changes are affected by local road density. We might also 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.

As mentioned in Section 2, there is no one-to-one relationship between the location of consumption changes and the corresponding production adjustment. Establishing a pattern of consumption changes is only part of the story; when the relevant goods are purchased in the market, observing local production responses (and therefore local environmental consequences) depends on the extent to which local markets are connected. Evidence exists to show that Mexican households substitute into the consumption of home produced goods during periods of economic recession, and hence we may expect that reverse would be true in response to increases in income (Hicks 2008). Therefore, we wish to not only examine changes in production behavior of treated households, but also that of their neighbors who did not participate in the program. We assess changes 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 used. We also consider the impact of the program on child labor.

Since the program was randomized among households in this dataset, we consider the simple difference-in-differences specification, restricting the sample to eligible households (treatment effects) or ineligible households (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 land use, 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

[12] 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

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. The intention to treat effect among the eligible is given by γ_3. In order to demonstrate consistency with previously established program impacts, we show the results of this specification before adding interaction terms to test for heterogeneity of this effect across the quality of local transportation infrastructure. To test this 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. 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.

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 and has a few very large outliers 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[13]. Progresa

[13]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

does not appear to provoke a substantial increase in the intensive or extensive margin of agricultural production among beneficiary households, regardless of the level of isolation. 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 is driven entirely by landless and non-agricultural households in their sample, indicating that the increase is unlikely to be due to large animals. Our data confirm that the result is driven by increased ownership of chickens and turkeys. They also demonstrate increases in land use in some subsamples (increased probability of land use among the previously landless, and an increase in hectares in production among large farms), though like us they find no significant effect in the full sample.

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 experiment, 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 this increased consumption demand. In Table 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 5% and 8% increase in the number of cows owned when evaluated at the 90th percentile and the median respectively, and a 21% 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 quite homogeneous, 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. We now return to our discontinuity analysis to examine the extent to which this issue affects the observable impact on deforestation.

5 Heterogeneity in the Impact of Oportunidades

5.1 Road Density and Treatment Effects

If the most plausible mechanism underlying a decrease in deforestation is increasing demand for land-intensive goods, we should expect heterogeneity in estimated treatment impacts across localities consistent with this mechanism. To this end, we test for variation in estimated effects by expected deforestation probability and by the quality of local transportation infrastructure. We expect that the estimated impact of the program should be greater where land is more suitable for livestock or agricultural production and therefore the supply response is greater. We also expect to see apparently larger impacts where the supply response is more localized by poor infrastructure.

Table 9 examines how the estimated impact varies with the suitability of land for production by looking for variation in a locality's propensity to be deforested. We first predict the amount of deforestation as a function of marginality, baseline forest, population, area, slope and road density, using only the ineligible localities of the restricted sample (Column 1). This estimation shows that deforestation increases with polygon area, percentage of the polygon in forest, and population, though most of the relationships are not statistically significant – the bulk of the variation in the data is explained by the ecosystem controls. We then use this estimation to predict the probability of deforestation for each locality and interact this predicted value with the eligibility cutoff indicator for the program. These results are shown in columns 2 and 3, and support our hypothesis: having a higher risk of deforestation (higher quality land) increases the likelihood of deforestation significantly more in localities which are eligible for the program. This result is robust to including non-linear terms of the marginality index (Columns 3) and to using only the restricted sample (not shown).

Next, we consider the possibility that transport infrastructure might be related to the estimated deforestation impact of the program. The problem of estimating responses when shocks can be dispersed through spillovers as described in Section 2 suggests that we will be more likely to detect impacts where road networks are poor. Table 10 shows the apparent differential impact of eligibility at different categories of road density. The three columns are the entire sample divided into three equal sized groups according to road density. Here we observe that the program only has a significant positive local impact on deforestation where road densities are low. In addition, the marginal effects both on the probability of deforestation and on rates if deforestation occurs are lower in magnitude for the higher road density classes than for the lowest one. The results are nearly identical for the restricted sample (not shown). It is worth noting that an interaction term between the road density classes and the eligibility variable does not generate significant results – the fact that the marginal effects of the program are only significant in the lowest road density category could imply that the other parameters in the regressions are quite different in isolated regions relative to more connected areas.

In summary, the results from both the household and locality data discussed above are consistent with the framework introduced in Section 2. Oportunidades induces 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, the large increase in demand for animal protein (as well as the diversification of fruit and vegetable consumption found in Hoddinott & Skoufias (2004)) will be met through a marketplace that serves up more consumer variety sourced from a greater variety of locations. In such circumstances, a large component of the treatment effect spills over into other places and hence the observed difference between treatment and control localities is small. Importantly, under homogenous treatment effects the *true* total treatment effect is the one observed in the most isolated locations. The environmental damage from consumption is not necessarily lower in places with better infrastructure, it is simply undetectable.

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 auto-correlation 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 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 subsample that we used for the discontinuity analysis, i.e., one which is located between -1.6 and -0.4 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 40 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. We then drop all cells with no baseline forest cover and estimate:

$$d_i = \alpha + \sum_{k=0,10,20,30,40,50} [\beta_k s_{ik} + \theta_k s_{ik} c_i] + \Gamma X_i + \epsilon_i, \tag{6}$$

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 and 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 11. The table contains only partial results – in all cases, the mean poverty level in each buffer is included, along with the variables indicating zero observations in a buffer. The last column also includes fixed effects at latitude, which capture spatial variation in ecosystem, as well as cultural heterogeneity, to the extent that it varies geographically in Mexico. The variable capturing infrastucture quality is a dummy variable equal to one in the case where there are less than 150 kilometers of road within a 30 kilometer buffer around the locality. In the simplest specification, which does not include interactions of saturations with road density, having low road density significantly decreases 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. In particular, in very 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 (3) by distance, for the subsample with high road density and that with density less than 150 kilometers. 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 completely flat with respect to the location of treatment for well-connected cells, whereas in isolated cells the deforestation effect is more localized. This corresponds with our hypothesis that good infrastructure may help spread the impacts of the program to the point where they are non-detectable locally.

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 resource use.

Average household income increases by one-third as a result of the transfers, which leads the probability of deforestation to increase by 30 percent and the rate of deforestation among deforesters to increase by 17 to 30 percent. These increases are significant in the entire sample, but are strongest in two subgroups: places that were already at high risk of deforestation, and 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.

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 impacts seen in isolated parts of Mexico when treated with Oportunidades is deeply troubling, because it is precisely in these environments that we are closest to capturing the full impact of treatment. We 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

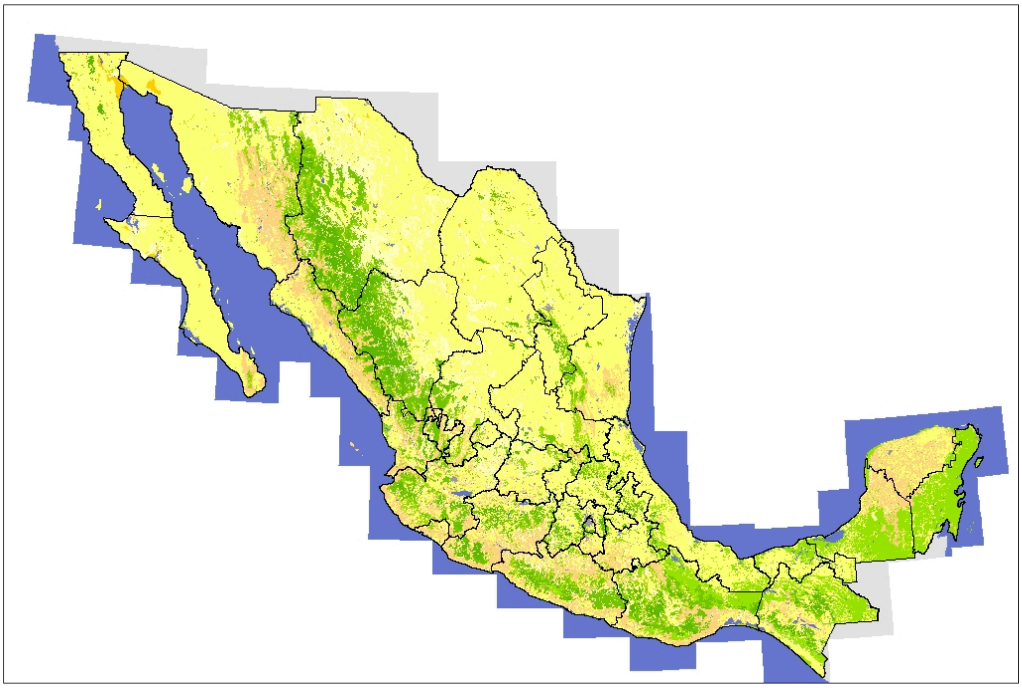

Figure 1: Forest Cover in Mexico, 2000

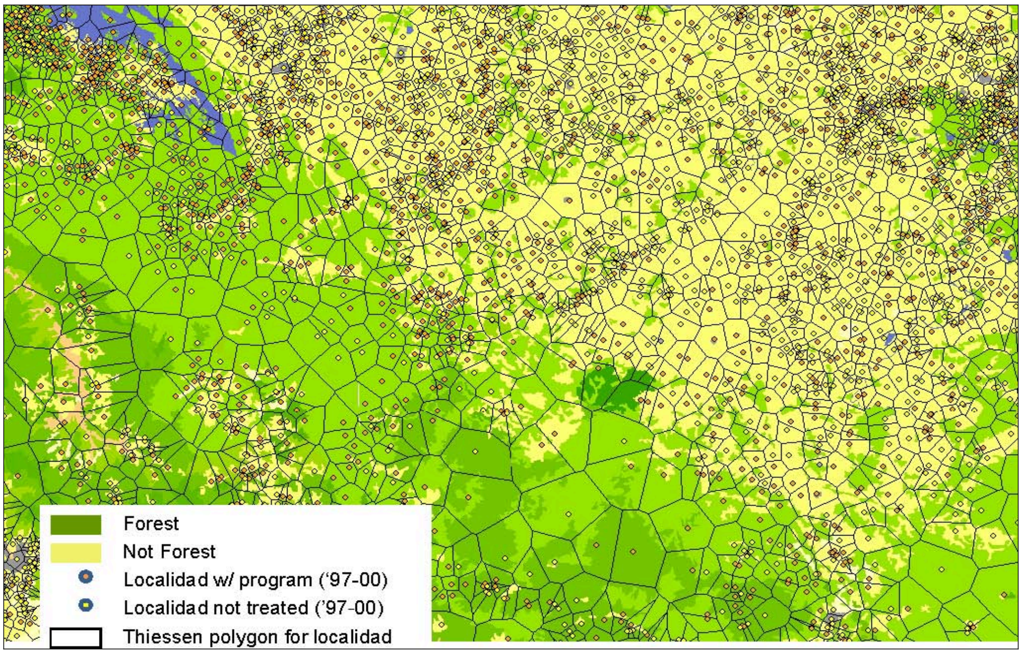

Figure 2: Thiessen Polygons

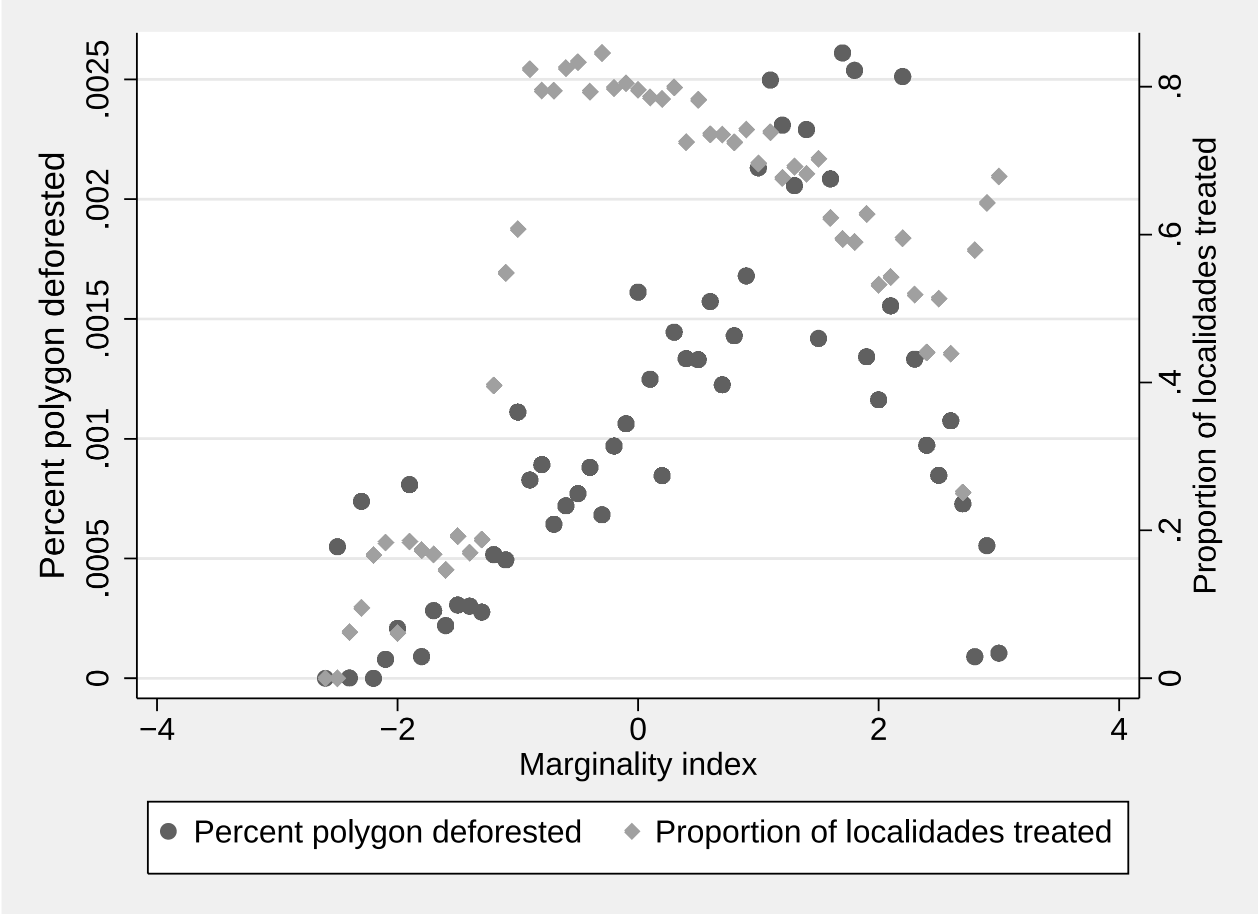

Figure 3: Entire sample minus observations with index > 3 (51 observations missing)

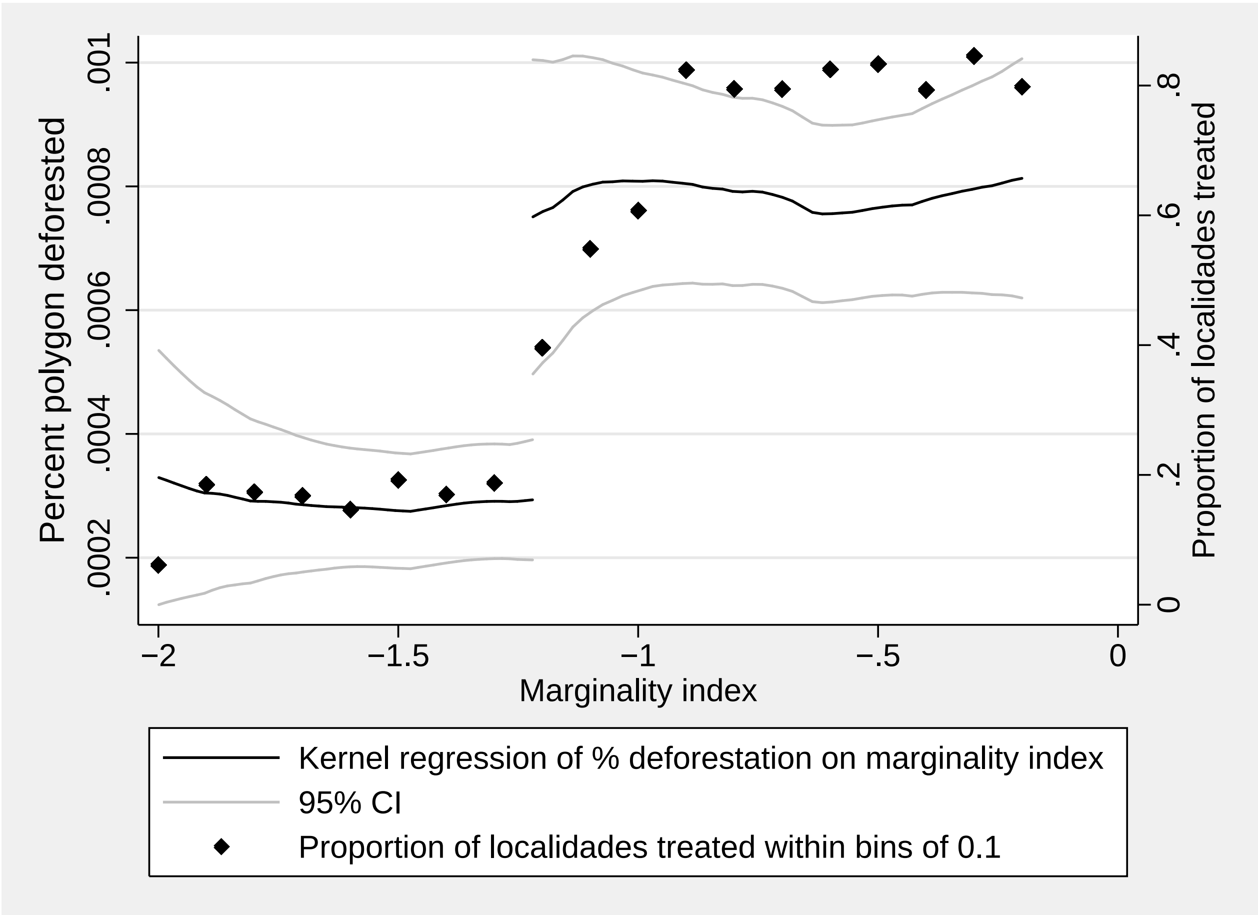

Figure 4: Kernel estimation of deforestation on marginality index – restricted sample

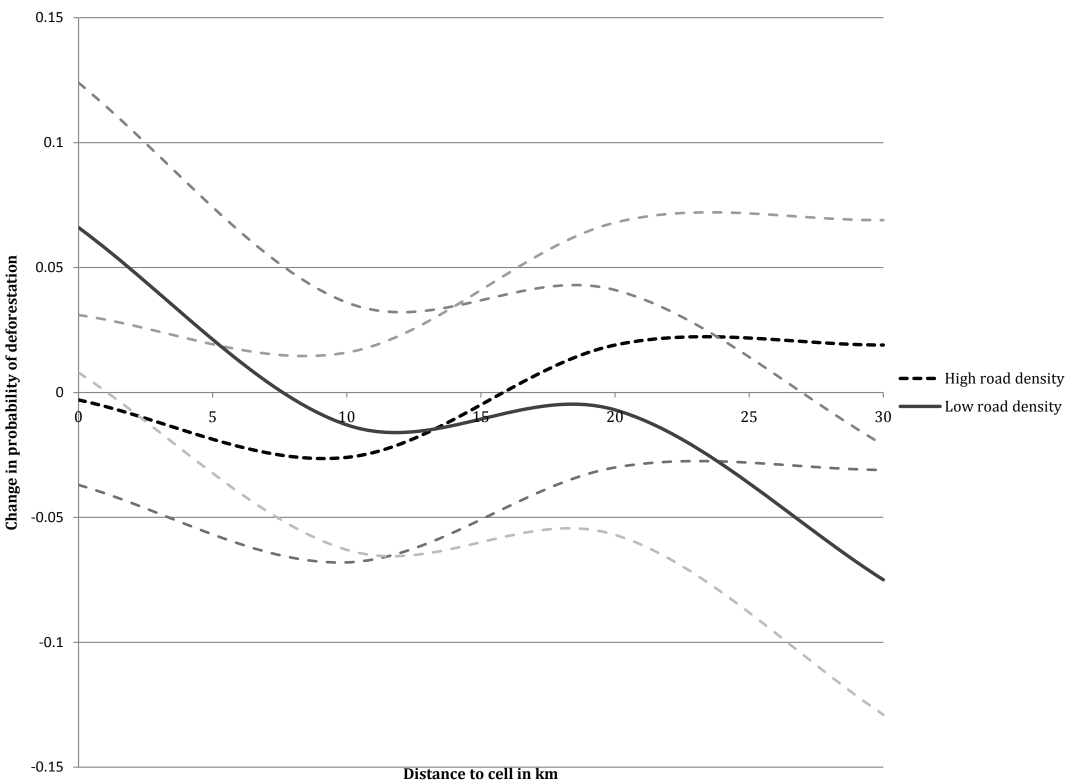

Figure 5: Own deforestation probability as a function of treatment within distance bands

8 Tables

Table 1: Summary statistics across eligibility

	Non-eligible <-1.2	Eligible >= −1.2	Test of difference	Normalized difference
Full sample				
Polygon area	37.9	18.9	18.17	-.163
Average slope in polygon	5.63	9.63	34.4	.482
Percent forested in 2000	12.1	10.5	3.24	0.035
Km roads in 10 km buffer	47.0	32.7	32.7	-.36
Percent polygon deforested	.0003	.0014	6.78	.11
Proportion with deforestation	.048	.098	9.64	
Observations	3510	55077		
Restricted sample				
Polygon area	37.9	25.6	7.43	-.095
Average slope in polygon	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
Percent polygon deforested	.0003	.0008	4.14	.139
Proportion with deforestation	.049	.072	4.89	
Observations	3350	12408		

Table 2: Simple approach – eligibility as proxy
Dependent variable: percent polygon deforested

	Whole sample		Restricted sample
	(1)	(2)	(3)
Eligible	.004	.006	.004
	(.002)**	(.003)*	(.002)*
Marginality index	.005	.007	.002
	(.0004)***	(.0008)***	(.002)
Index^2		.0007	
		(.0007)	
Index^3		-.0009	
		(.0004)**	
Index^4		-.00003	
		(.0002)	
Baseline area in forest, 2000	-3.58e-06	-1.00e-05	.00004
	(9.72e-06)	(9.79e-06)	(.00002)**
Ln(polygon area)	.009	.011	.007
	(.0004)***	(.0004)***	(.0007)***
Ln(total population in 1995)	.001	.001	.0004
	(.0002)***	(.0002)***	(.0003)
Ln(average slope)	-.0005		-.00009
	(.00005)***		(.0001)
Ln(road density)	-.0006	-.00008	.0003
	(.0003)**	(.0003)	(.0005)
Obs.	58587	58587	15758
Log-likelihood	251.624	212.297	74.211
F statistic			
Ecoregion controls	yes	yes	yes
Marginal effects			
$\Pr(y > 0)$	.011	.001	.004
	(.005)**	(.0004)**	(.002)*
$y > 0$	.0006	.004	.0005
	(.0003)**	(.002)*	(.0003)*

Tobit estimation. Standard errors in parentheses. * significant at 10%; ** significant at 5%.

Table 3: First stage regressions

Dependent variable = 1 if locality received Oportunidades before 2004

	Full sample			Restricted sample
	(1)	(2)	(3)	(4)
Eligible	.676 (.008)***	.466 (.037)***	.843 (.097)***	.676 (.046)***
Marginal		1.031 (.099)***	1.041 (.089)***	1.077 (.087)***
Marginal x index		1.248 (.095)***	1.156 (.088)***	1.194 (.085)***
Eligible x index		-.184 (.023)***	.222 (.081)***	.078 (.034)**
Marginality index	-.049 (.002)***	.105 (.023)***	-.189 (.083)**	.021 (.029)
Index^2			-.046 (.005)***	
Index^3			.006 (.005)	
Index^4			-.001 (.001)	
Baseline area in forest, 2000	.00008 (.00009)	.00007 (.00009)	.0003 (.00008)***	.0006 (.0001)***
Ln(polygon area)	-.021 (.002)***	-.020 (.002)***	-.029 (.002)***	-.027 (.004)***
Ln(total population in 1995)			.159 (.001)***	.129 (.002)***
Ln(average slope)			.003 (.0003)***	.003 (.0005)***
Ln(road density)	.050 (.002)***	.050 (.002)***	.028 (.001)***	.009 (.003)***
Obs.	58587	58587	58587	15758
Log-likelihood	-33926.8	-33383.9	-25860.08	-5884.458
Adjusted R-squared	.123	.139	.334	.462
Ecosystem controls	yes	yes	yes	yes
F-test of instruments		2018	425	350

Linear probability model estimation. Robust standard errors in parentheses. * significant at 10%;** significant at 5%; *** significant at 1%

Table 4: Simple discontinuity approach – instrumentation with eligibility
Dependent variable: percent polygon deforested

	Full estimation sample		Restricted sample
	(1)	(2)	(3)
Treated	.006	.013	.010
	(.003)**	(.007)*	(.006)*
Marginality index	.005	.006	-.0007
	(.0004)***	(.001)***	(.003)
Index^2		.002	
		(.001)	
Index^3		-.0009	
		(.0003)***	
Index^4		-.0001	
		(.0002)	
Baseline area in forest, 2000	-5.11e-06	-7.95e-06	.00003
	(9.75e-06)	(9.96e-06)	(.00002)**
Ln(polygon area)	.010	.010	.008
	(.0004)***	(.0005)***	(.0007)***
Ln(total population in 1995)	.0005	-.0006	-.001
	(.0005)	(.001)	(.0009)
Ln(average slope)	-.0005	-.0006	-.0001
	(.00005)***	(.00006)***	(.0001)
Ln(road density)	-.0008	-.0009	.0002
	(.0003)***	(.0003)***	(.0005)
Obs.	58587	58587	15758
Log-likelihood	-26468.95	-25855.74	-6047.682
Ecoregion controls	yes	yes	yes
Marginal effects			
$\Pr(y > 0)$	.018	.038	.030
	(.008)**	(.019)**	(.017)*
$y > 0$	.0009	.002	.001
	(.0004)**	(.001)*	(.0008)*

IV Tobit estimation. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Fuzzy discontinuity estimates
Dependent variable: percent polygon deforested

	Full sample		Restricted sample
	(1)	(2)	(3)
Treated	.006	.012	.010
	(.002)**	(.006)**	(.005)**
Marginality index	.005	.007	-.0007
	(.0004)***	(.0009)***	(.003)
Index^2		.002	
		(.001)	
Index^3		-.0009	
		(.0003)***	
Index^4		-.00008	
		(.0002)	
Baseline area in forest, 2000	-5.01e-06	-7.74e-06	.00004
	(9.73e-06)	(9.89e-06)	(.00002)**
Ln(polygon area)	.010	.010	.008
	(.0004)***	(.0005)***	(.0007)***
Ln(total population in 1995)		-.0005	-.001
		(.0009)	(.0007)
Ln(average slope)	-.0005	-.0006	-.0001
	(.00005)***	(.00005)***	(.0001)
Ln(road density)	-.0007	-.0009	.0002
	(.0003)**	(.0003)***	(.0005)
Obs.	58587	58587	15758
Log-likelihood	-33026.33	-25575.68	-5809.478
Ecoregion controls	yes	yes	yes
Marginal effects			
Pr($y > 0$)	.018	.037	.030
	(.007)***	(.016)**	(.013)**
$y > 0$	.0009	.002	.001
	(.0004)**	(.0008)**	(.0006)**

IV Tobit estimation. 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	1.557		0.388		1.440	
variable in baseline	(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	.031	-.014	-.015	.092	.036
	(.039)	(.040)	(.038)	(.039)	(.057)	(.057)
Treatment x inverse road density		-.107		.142		.936
		(.210)		(.223)		(.522)*
Village chosen to receive Progresa	.014	.037	-.004	.017	-.004	.058
	(.056)	(.057)	(.040)	(.040)	(.087)	(.085)
Post treatment year	-.094	-.077	.312	.317	-.239	-.180
	(.032)***	(.033)**	(.033)***	(.033)***	(.046)***	(.046)***
Inverse of road density		.833		.820		2.122
		(.161)***		(.227)***		(.799)***
Village x inverse road density		-.263		-.217		-.760
		(.317)		(.258)		(.872)
Post treatment x inverse road density		-.275		-.235		-.982
		(.149)*		(.128)*		(.402)**
Obs.	45087	45087	32631	32631	34248	34248
Mean dependent	0.824		1.724		0.604	
variable in baseline	(0.955)		(3.535)		(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	1.031		2.844		1.577	
variable in baseline	(1.667)		(5.322)		(4.675)	

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

Table 9: Predicted deforestation risk and impact of Oportunidades
Dependent variable: percent polygon deforested

	(1)	(2)	(3)
Eligible		.0002	.0005
		(.003)	(.004)
Predicted risk		.262	.260
		(.031)***	(.031)***
Eligible x risk		.094	.094
		(.032)***	(.032)***
Marginality index	-.100	.008	.011
	(.151)	(.0004)***	(.0008)***
Index^2			.0009
			(.0007)
Index^3			-.001
			(.0004)***
Index^4			-.0002
			(.0002)
Baseline area in forest, 2000	.001		
	(.0009)		
Ln(total population in 1995)	.025		
	(.024)		
Ln(polygon area)	.237		
	(.045)***		
Ln(average slope)	.004		
	(.009)		
Ln(road density)	-.030		
	(.042)		
Obs.	3510	58587	58587
Log-likelihood	-630.484	-414.326	-384.168
Ecoregion controls	yes	no	no

Tobit estimation. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Deforestation and infrastructure
Dependent variable: percent polygon deforested

	Low density	Medium density	High density
	(1)	(2)	(3)
Treated	.017	.006	.018
	(.008)**	(.008)	(.015)
Marginality index	.006	.008	.005
	(.001)***	(.001)***	(.003)**
Index^2	.001	.0007	.003
	(.001)	(.002)	(.003)
Index^3	-.002	-.0008	-.0004
	(.0006)***	(.0005)	(.0008)
Index^4	.0003	9.13e-06	-.0003
	(.0002)	(.0002)	(.0005)
Baseline area in forest, 2000	-1.00e-05	.00003	.0003
	(9.04e-06)	(.00002)	(.0001)***
Ln(total population in 1995)	-.0008	.001	-.004
	(.002)	(.001)	(.002)**
Ln(polygon area)	.006	.007	.017
	(.0006)***	(.0007)***	(.002)***
Ln(average slope)	-.0003	-.0005	-.001
	(.00006)***	(.00009)***	(.0002)***
Obs.	19529	19529	19529
Log-likelihood	-8873.69	-7910.647	-7716.973
Ecoregion controls	yes	yes	yes
Marginal effects			
Pr($y > 0$)	.080	.020	.026
	(.040)**	(.030)	(.019)
$y > 0$	.002	.001	.002
	(.001)**	(.001)	(.002)

IV Tobit estimation. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Spatial regressions – dummy variable for low density
Dependent variable = 1 if deforestation

	(1)	(2)	(3)
Own saturation	.012	.004	-.003
	(.018)	(.018)	(.017)
Within 10-20 km	.012	.0009	-.026
	(.022)	(.022)	(.021)
Within 20-30 km	.090	.105	.019
	(.023)***	(.025)***	(.025)
Within 30-40 km	.104	.137	.019
	(.021)***	(.027)***	(.026)
Density < 150 km	-.128	.011	-.011
	(.011)***	(.029)	(.029)
Baseline forest	.0005	.0005	.0008
	(.0001)***	(.0001)***	(.0001)***
Density x own saturation		.071	.078
		(.032)**	(.034)**
Density x 10-20 km		.008	-.003
		(.026)	(.024)
Density x 20-30 km		-.094	-.024
		(.031)***	(.029)
Density x 30-40 km		-.127	-.076
		(.041)***	(.036)**
Obs.	10977	10977	10977
R^2	.059	.062	.188
Lat-long fixed effects	no	no	yes

OLS with bootstrapped standard errors. ** significant at 5%; *** significant at 1%