CHAPTER 6

EFFECTS OF POVERTY ON DEFORESTATION

Distinguishing behaviour from location

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Abstract. We review many theoretical predictions that link poverty to deforestation and then examine poverty's net impact empirically using multiple observations of all of Costa Rica after 1960. Country-wide disaggregate (district-level) data facilitate analysis of both poverty's location and its impact on forest. If the characteristics of the places the poor live are not controlled for, then poverty's impact is confounded with differences between poorer and less poor areas and we find no significant effect of poverty. Using our data over space and time to control for effects of locations' differing characteristics, we find that the poorer are on land whose relative quality discourages forest clearing, such that with these controls the poorer areas are cleared more. The latter result suggests that poverty reduction aids the forest. For the poorest areas, this result is weaker but another effect is found: deforestation responds less to productivity, i.e., the poorest have less ability to expand or to reduce given land quality.
Keywords. deforestation; poverty; Costa Rica; development; land use

INTRODUCTION

Those concerned with the environment need to understand the role of poverty in land use and its impacts on species habitat, carbon storage and erosion. Those concerned solely with the fate of the poor may not care directly about such outcomes but may well be in favour of eco-payments to the poor. Their optimal targeting would depend upon the impacts of poverty. Finally, since much of the world's forest resides in poor areas, whatever one's motivation it is clear that policies that address rural poverty can affect a large forest area and many people.

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Theoretical predictions on how changing income affects forest clearing are ambiguous. Concerning macroeconomic growth and deforestation, Wunder's (2001) review of the evidence concludes that income levels have an ambiguous link to land degradation. In some countries, higher incomes are associated with higher deforestation while in others the opposite is true. Wunder states that as income growth occurs, forest outcomes will depend upon the strength of capital-endowment growth relative to incentives from the potential returns in other activities. The former change enables deforestation while the latter makes it less attractive. Their relative strengths, Wunder says, will depend on the resource endowment and the type of growth path.

Micro-theories linking incomes and deforestation also yield an ambiguous net impact. Increased wealth may relax capital constraints, raising the capacity to clear forest. However, a rising wage, which decreases poverty, will discourage forest clearing, as it is labour intensive. Such theoretical ambiguity highlights the value of empirical tests of poverty's impact. While this paper does not test each specific hypothesis above separately, as in principle all or many of them could apply we explore empirically the net effect of all of their actual impacts.

We use tropical-forest data for all of Costa Rica in 1963, 1979, 1986, 1997 and 2000, partitioned into over 400 districts. Our other data focus is a poverty index created from census district data for 1963, 1973, 1984 and 2000. These district data offer greater spatial detail than typical 'macro' data over time. Thus the locations of the poor can be distinguished. The location of the poor cannot be distinguished as household level but the census data exist over time, unlike typical 'micro' (e.g., household) data that could also be used to study poverty.

The data are particularly helpful in light of a challenge to estimating poverty's effect. While forest outcomes for poorer areas may differ from those in richer areas due to behaviour, i.e., the poor may use identical land differently, also the poor may have different-quality land. If 'marginalized', they have less profitable land. This can confound cross-sectional inference. However, with data over space and time we can control for the impacts of location differences when testing empirically for whether different decisions in poorer areas affect deforestation.

We analyse deforestation's relationship to poverty with and without spatial controls. Without location effects, we find no significant effect of poverty on the rate of deforestation. When controlling for the effects of the differing characteristics of poorer and less poor districts, we find evidence that the poor are on land whose relative quality (on observable and unobservable dimensions) discourages clearing. Controlling for this, poorer areas are cleared more rapidly than are richer areas.

Examining the very poorest tempers that conclusion, though, as the effect (including the controls for location) of being in the lowest quartile of the poverty index is less significant. However, another piece of evidence of poverty's impacts is found. Clearing in poorest areas responds less to land productivity, i.e., expands less on better and reduces less on worse land.

LAND USE, LOCATION AND POVERTY

Land use

We use a dynamic theoretical model (like Stavins and Jaffe 1990) but emphasize key irreversibilities as well as the dynamics of development[1]. We feel both are important for understanding deforestation within a developing country, including the effects of payments.

Each forested hectare *j* has a risk-neutral manager who selects *T*, the time when land is cleared of forest, in order to maximize the expected present discounted value of returns[2]:

$$\mathrm{Max}_T \int_0^T S_{jt}\, e^{-rt}\, dt + \int_T^{\infty} R_{jt}\, e^{-rt}\, dt - C_T\, e^{-rt} \tag{1}$$

where:

S_{jt} = expected return to forest uses of the land,
R_{jt} = expected return to non-forest land uses,
C_T = cost of clearing net of obtainable timber value and including lost option value,
r = the interest rate.

Two conditions are necessary for clearing to occur at *T*. First, clearing must be profitable. Second, even if that is so, it may be more profitable to wait and clear at *t+1*, so (2) must hold:

$$R_{jt} - S_{jt} - r_t\, C_t + \frac{dC_T}{dt} > 0 \tag{2}$$

and if a second-order condition holds this necessary condition is also sufficient for clearing[3].

Consistent with this, we assume that deforestation has irreversibilities, since trees take time to grow and incurring the costs of development changes marginal returns to land uses. We separate deforestation from reforestation and empirically examine deforestation, i.e., examine where forest present at the beginning of a period is cleared by the end of the period[4].

Deforestation occurs when (2) is satisfied for the first time. When that will occur differs across space due to variation in exogenous land quality, access to markets, and both exogenous and endogenous temporal shifts. The model's individual decisions are discrete, while we observe continuous rates of loss in districts. We aggregate the model's predictions.

Specifically, in our data set we do not perfectly observe the plot-level variables in (2), as deforestation and factors that explain it (X_{it}, i = district, t = time) are measured for districts. Thus X_{it} generates one estimated net clearing benefit per district, though returns and changes in costs vary across parcels. Thus we imperfectly measure net benefits, so clearing occurs if:

$$R_{ijt} - S_{ijt} - r_t C_t + \frac{dC_T}{dt} \;=\; X_{it}\beta \;-\; \varepsilon_{ijt} \;>\; 0 \qquad (3)$$

where again i is an area, j is a specific parcel, ij is a specific parcel j known to be in area i, and ε_{ijt} is a parcel-year-specific term for the unobserved relative returns to forested land uses, so:

$$\text{Prob (satisfying (3) so that cleared if currently in forest)} = \text{Prob } (\varepsilon_{ijt} < X_{it}\beta) \qquad (4)$$

Predicted district-level clearing rates depend upon X_{it} and on the distribution of the ε_{ijt}. If the cumulative distribution of the ε_{ijt} is logistic, then we have a logit model for each parcel:

$$\text{F}(X_{ijt}\beta) \;=\; 1 \,/\, (\, 1 + \exp\, (X_{ijt}\beta)\,) \qquad (5)$$

For our grouped data, we estimate this model using the minimum logit chi-square method also known as 'grouped logit' (Maddala 1983)[5]. If h_{it} is an area's measured rate of forest loss, then we estimate:

$$\log\,(\, h_{it} \;/\, (1\text{-}\, h_{it}\,)\,) = X_{it}\beta + \mu_{it} \qquad (6)$$

The variance of the μ_{it} (referring to areas, not parcels) can be estimated by (1 / I_{it} h_{it} (1- h_{it})). I_{it} is the number of forested parcels in area i at the beginning of interval t and the estimator is consistent and asymptotically normal (Maddala 1983). This is estimated by weighted least squares.

Poverty and location

Poverty may systematically cause land users to have higher or lower values of the X_{it} and to make different decisions because of different X_{it}. This impact may be misinterpreted as poverty that changes behaviour conditional on a given vector of non-poverty X_{it}.

Lacking assets and access to capital, the poor may not be on the most profitable land. Even if they could purchase it they might get lower returns due to lower skill and other inputs. Then poorer people might: have less productive land; migrate to frontiers far from markets; and if very poor, to 'squat' on land with low tenure security. Concerning productivity, Barbier (1996) claims that almost 75% of the poorest 20% in Latin America live on 'low-potential' marginal lands. In a model such as above, this could lower the rate of forest clearing.

Such marginalization could, though, have the opposite effect (Rudel and Roper 1997). In subsistence settings with all output consumed, low yields could raise clearing to meet the minimum consumption requirement. Also, if poor lands degrade faster, e.g., are sloped, again further clearing would be promoted. In the case of migration to frontiers far from markets, farmers could shift to transportable outputs such as cattle, which degrade extensive areas of poor quality land. Finally, farther from markets there may be fewer off-farm job opportunities.

Poverty and land use

Many argue that poverty is a driver of deforestation (i.e., poverty itself is in X_{it}, as it affects behaviour conditional on other X_{it}.). Rudel and Roper (1997) argue that poor households may be more likely to clear a given parcel due to: a) lower skills and lower off-farm economic opportunities; b) a need to insure given commodity and other shocks; and c) less preference on the margin for some environmental services. Others stress less productive capital (such as a tractor), less inputs (e.g., fertilizer) and less tenure security. Figure 1 summarizes many ideas.

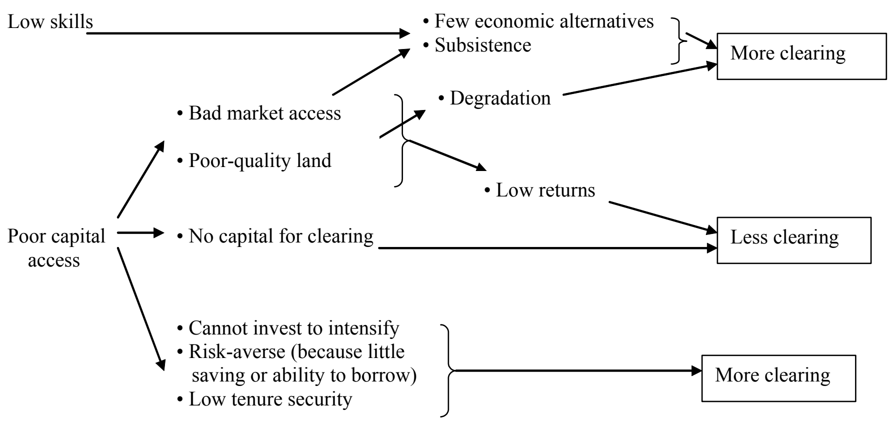

Figure 1. *Poverty and deforestation*

Income and asset levels

Poor households may not be able to invest to prevent soil degradation and lower harvests. Thus they may clear more if their goal is to maintain their level of output. Increased assets and access to capital for poor landowners could then reduce the need to clear forest.

Outside a subsistence setting, relaxing capital constraints could lead to more clearing. Zwane (2002) provides evidence, from a longitudinal household survey in Peru, that the poor use additional income for land clearing. Angelsen and Kaimowitz (2001) review farm-level and regional evidence from Latin America that links increased credit to greater deforestation rates.

Zwane's (2002) relationship between income and clearing is non-linear, however. At lower incomes more income does not increase purchases of fertilizers but at higher incomes it does. Thus farmers may initially clear more land as income rises but, above a certain income level, instead intensify production. Then lowering poverty could lower deforestation too but even then the prediction is not clear as intensification is also consistent with using more land.

Off-farm economic opportunities

In countries with small forests, peasant and shifting cultivator populations with few other economic opportunities may drive deforestation (Geist and Lambin 2001; Zwane 2002). Low skills or weak off-farm labour markets can lead poor households to undertake activities with low returns, such as exploitation of marginal lands. Then the poor may deforest more. Thus, Deininger and Minten (1996), with a focus on alternative income opportunities, find lower poverty to be associated with lower deforestation. Household analyses reviewed by Angelsen and Kaimowitz (2001) also suggest that greater off-farm employment opportunities reduce deforestation. Along these lines, policies that lower poverty could lower deforestation.

Security given income and price risk

Forest clearing for production can also provide income security, given shocks such as recessions, sickness and price changes in a setting of low savings and low ability to borrow. For instance, meeting one's minimal food requirements on one's own lowers effective risk. Rodríguez-Meza et al. (2002) note that this could mean that lowered poverty will yield greater forest clearing. Yet, as in Zwane (2002), this effect too can depend upon initial income. Further, if households can sell wood itself when income or prices shift disadvantageously, they might keep plots of land in forest as a store of natural capital to exploit in tough times. Eventually, though, rising income reduces such precautionary demand for clearing altogether.

DATA

Deforestation

We observe forest cover in Costa Rica at five points (1963, 1979, 1986, 1997, 2000). The country has 436 political districts. Our smallest unit of observation is a form of sub-district, distinguishing different 'lifezones'. The Holdridge Life Zone System (Holdridge 1967) assigns each location in Costa Rica to one of twelve lifezone categories. These reflect precipitation and temperature. On average there are about three lifezones present in a district so we can use up to 1229 observations per year. Yet as poverty is measured for districts, we focus on district (Table 2) while also providing results for sub-district observations (Table 3). In either case, our dependent variable is annual percentage loss of forest during an interval.

The 1963 data are from aerial photos digitized by University of Alberta to distinguish forest and non-forest. The 1979 data from Landsat satellite images come from the National Meteorological Institute of Costa Rica (IMN 1994). The 1986 and 1997 data from Landsat (FONAFIFO 1998) distinguish forest, non-forest and mangroves. The 2000 Landsat images were processed by the University of Alberta EOSL for consistency with 1986 and 1997 data.

For each district and interval, we calculate the area deforested. The 1986, 1997 and 2000 maps have clouds so we use the visible portions of each unit, i.e., images with consistent cloud masks. For intervals before 1986-1997 we cannot distinguish gross from net transitions and assume they are equal. If the measured gross deforestation is negative, we assign a zero.

Our dependent variable is the area deforested divided by the area of forest 'at risk'. We assume national parks and biological reserves are not at risk (they were not cleared[6]). We also drop areas for which we do not have poverty data (see below). Because our time intervals are of varying lengths, we use annualized rates of deforestation. If λ_{it} is the area deforested over a given interval divided by the area at risk and n is the number of years in that interval, then our annualized dependent variable (assumed constant during the interval) is calculated:

$$h_{it} = 1 - (1-\lambda_{it})^{1/n} \tag{7}$$

Explanatory variables

Poverty index

Lacking household data Cavatassi et al. (2002) employ principal-components analysis (PCA) using census data for districts, over four decades, to generate a district poverty index. Seventeen variables are common to the 1973, 1984 and 2000 census data, of which twelve are in 1963 too. The variables used include demographic, labour, education, housing, infrastructure and consumer durables measures (see Cavatassi et al. (2002)) concerning variables' meanings). They find that the variables expected *a priori* to be positively correlated with poverty have positive signs within the index, while the wage and education variables have negative signs.

They first create year-specific indices for 1963, 1973, 1984 and 2000. Those are not comparable as each is based on a scale relevant only to its year. Then they pool all years for a 1973-2000 index using the 17 common variables and a 1963-2000 index using the 12. For these pooled PCA estimations, changes over time arise only from changes in measured variables, not from changes in the weights. We use the pooled indices and, to focus on greater poverty, also their quartiles to allow for non-linearities within the poverty–deforestation relationship.

For the 1963-2000 index, to match the 1963-1979 deforestation interval we use 1963 index values. For 1979-1986 we use 1973 values, for 1986-1997 we use 1984 values and for 1997-2000 we use 2000. We also try 1984 values for 1997-2000 clearing as lagged option. For the 1973-2000 measure the difference is that for 1963-1979 we have only the 1973 values.

Returns proxies

Given the difficulty of perfectly measuring the agricultural returns in monetary units, we use proxies for the returns to clearing. Lacking a monetary measure of the transport costs, for instance, we use the minimum linear distance in kilometres to a major market, DISTCITY, i.e., the shortest of the three distances from an observation to San José, Puntarenas and Limon. For local markets, we include district-level population density POPDEN. The measure is from census data at district level, for 1950 and 1984, divided by the area of the district. As population is potentially endogenous to other factors, we can use lagged population densities.

Ecological variables proxy for agricultural productivity. We create dummies at sub-district level for groups of lifezones: GOODLZ includes humid (medium precipitation) areas, which have moderate temperatures; MEDLZ includes very humid areas (higher precipitation) in moderate to mountain elevations (and hence moderate temperature); and then BADLZ includes the very humid areas with high temperatures (tropical), very dry hot areas and rainy lifezones, all of which are less productive. District values are area-weighted averages of these. We also have data on seven different soil types outside national parks[7]. We create a BADSOIL measure, i.e., the proportion of a district-lifezone with low-productivity entisol soil.

We include a polynomial for total previous clearing in a district-lifezone (%CLEARED) as well as dummies for time periods. These variables proxy for unobservable changes in the net returns to clearing over time which resulted from exogenous improvements in infrastructure and development generally. Costa-Rican history suggests a trend of increasing returns as well as a shift in the trajectory over time (see Kerr et al. 2005). A polynomial for the previous forest clearing, e.g., our quadratic term (%CLEARED^2), is motivated by at least two types of priors. Selection, in which those parcels with the highest returns to clearing are the first to be cleared, would suggest a negative coefficient for the quadratic term. Endogenous local development, in which previous clearing raises future returns, suggests a positive one.

RESULTS

Table 1 provides statistics for the 25% poorest and the other districts. The first three rows do not change with time. The next two were pooled for 1963-2000. Deforestation is by period.

Poorer areas are further from markets and less densely populated. A lower proportion of their area has poor climatic conditions but a higher proportion has poor soil. In a crude first cut they seem, if anything, to have higher deforestation rates although not significantly so.

Table 2 presents results from regressions using districts, starting with poverty alone and focusing on poverty. In all columns, the poverty measure is the pooled 1963-2000 index. In columns I - III, (A) uses the continuous index while (B) uses a poorest-quartile dummy. In IV, to focus on interaction stories that may apply to the most poor, only the (B) version is run. Table 3 has the same format but it provides supporting results using sub-district observations.

Table 1. *Summary statistics for Costa-Rican districts* [a, b]

	Poorer districts	Richer districts
Bad climate [c]	0.47 (0.50)	0.63 (0.48)
Bad soil [d]	0.14 (0.26)	0.09 (0.19)
Distance to market (km)	87 (43)	56 (35)
Population density	0.16 (0.80)	0.79 (4.5)
Per-capita forest cover (ha)	4.5 (6.6)	3.7 (6.2)
Deforestation rate (%)		
1963 – 1979	0.033 (0.044)	0.025 (0.047)
1979 – 1986	0.046 (0.047)	0.018 (0.036)
1986 – 1997	0.0067 (0 .0083)	0.0091 (0.0096)
1997 – 2000	0.0015 (0.0041)	0.00062 (0.0016)

[a] For greatest relevance to the regressions, weights for these averages are the initial forest in each period.
[b] Standard deviations for these measures within these groups of districts are given in brackets.
[c] 'Bad climate' = fraction of district identified as a poor productivity or 'bad' lifezone.
[d] 'Bad soil' = fraction of district identified as a poor performing or 'bad' soil.

Poverty with and without spatial controls

In Table 2's column I, poverty is not significant in (A) or (B) (or in Table 3). While unobserved variation in poverty across sub-districts could complicate Table 3's analyses, given our district-level poverty index, at least for column I, in which there are no other factors, we believe that Table 3 supports Table 2's conclusion that column I's estimated effect is zero.

However, column II suggests that column I masks two significant but opposing effects. Table 2's column II uses district-level fixed effects to control for the fixed characteristics of each location. It also includes our only time-varying explanatory variable, the prior clearing. With the controls for areas' differences, (A) finds that poorer areas have higher deforestation.

Even with column II controls, the (B) result for the poorest quartile is not significant. Yet poverty is significant in Table 3's column II (A) and (B). Thus the poorest-quartile results are less significant but, overall, controlling for characteristics finds the poorer clearing more[8].

Table 2. *Deforestation, poverty and locations – district level* [i]

	I		II		III		IV
	A [ii]	B [ii]	A	B	A	B	B
Poverty [ii]	0.02	0.04	0.16	0.12	0.004	0.05	0.09
	(0.8)	(0.6)	(3.3)	(0.9)	(0.2)	(0.8)	(0.4)
Fixed Effects			F = 6.3 (P=0.00)	F = 6.1 (P=0.00)			
Constant	-2.8	-2.8	-3.6	-3.2	-3.9	-3.9	-3.7
	(30)	(46)	(16)	(17)	(21)	(23)	(18)
%Cleared			1.2	2.0	3.7	3.7	3.9
			(1.3)	(2.3)	(7.2)	(7.2)	(7.7)
%Cleared^2			-3.4	-4.1	-2.2	-2.1	-2.6
			(3.5)	(4.4)	(3.9)	(3.9)	(4.6)
BadSoil					-0.3	-0.4	-0.4
					(2.4)	(2.5)	(2.9)
BadLZ					-1.2	-1.2	-1.8
					(11)	(12)	(9.6)
Pov * BadLZ							0.4
							(1.5)
GoodLZ							0.08
							(0.4)
Pov * GoodLZ							-0.6
							(2.0)
DistCity					0.01	0.01	0.01
					(7.8)	(8.0)	(7.6)
Dist * 79-86					-0.00	-0.00	-0.00
					(0.9)	(1.0)	(1.1)
Dist * 86-97					-0.01	-0.01	-0.01
					(4.2)	(4.4)	(4.5)
Dist * 97-00					-0.00	-0.00	-0.01
					(1.1)	(1.2)	(1.4)
Time Dummies	[these are always significant as controls for time trends[iii]]						
Adjusted R^2	0.22	0.22	0.76	0.75	0.51	0.51	0.53
N	961	961	961	961	958	958	958

[i] All regressions are Grouped Logit explaining annualized deforestation probabilities, following expression (6), using district observations. Coefficient is reported, with t statistic below it, except for the fixed-effects component within II where F statistic is reported with P value below.

[ii] 1963-2000 pooled index in all columns. Column IV focuses solely on the poorest quartile as an interaction effect is motivated by the very poor. Within the other three columns (I – III) the A regression uses the continuous-poverty index while the B regression uses a poorest-quartile dummy.

[iii] Coefficients for time dummies not reported as not a focus here and would crowd the table (see Kerr et al. (2005) for discussion of time trends).

Table 3. *Deforestation, poverty and locations – subdistrict level* [i]

	I		II		III		IV
	A [ii]	B [ii]	A	B	A	B	B
POVERTY [ii]	0.01 (0.5)	0.01 (0.2)	0.12 (4.0)	0.21 (2.5)	-0.002 (0.1)	0.02 (0.5)	0.10 (1.4)
FIXED EFFECTS			F = 8.4 (P=0.00)	F = 8.3 (P=0.00)			
CONSTANT	-2.5 (41)	-2.5 (63)	-3.3 (26)	-3.0 (36)	-3.5 (31)	-3.6 (35)	-3.7 (34)
%CLEARED			0.5 (1.3)	0.9 (2.4)	1.8 (6.1)	1.8 (6.1)	1.9 (6.5)
%CLEARED^2			-1.5 (4.0)	-1.8 (5.0)	-0.3 (1.1)	-0.3 (1.0)	-0.5 (1.6)
BADSOIL					-0.1 (1.6)	-0.2 (1.6)	-0.2 (2.5)
BADLZ					-0.6 (11)	-0.6 (11)	-0.5 (6.5)
POV * BADLZ							0.1 (0.9)
GOODLZ							0.4 (5.2)
POV * GOODLZ							-0.3 (3.1)
DISTCITY					0.01 (10)	0.01 (10)	0.01 (11)
DIST * 79-86					-0.003 (2.0)	-0.003 (2.3)	-0.003 (2.7)
DIST * 86-97					-0.01 (5.9)	-0.01 (6.2)	-0.01 (6.5)
DIST * 97-00					-0.01 (2.6)	-0.01 (2.7)	-0.01 (2.9)
TIME DUMMIES	[these are always significant as controls for time trends[iii]]						
ADJUSTED R^2	0.20	0.20	0.79	0.79	0.37	0.37	0.38
N	2604	2604	2604	2604	2421	2421	2421

[i] All regressions are Grouped Logit explaining annualized deforestation probabilities, following expression (6), using subdistrict observations. Coefficient is reported, with t statistic below it, except for the fixed-effects component within II where F statistic is reported with P value below.

[ii] 1963-2000 pooled index in all columns. Column IV focuses solely on the poorest quartile as an interaction effect is motivated by the very poor. Within the other three columns (I – III) the A regression uses the continuous-poverty index while the B regression uses a poorest-quartile dummy.

[iii] Coefficients for time dummies not reported as not a focus here and would crowd the table (see Kerr et al. 2005, for discussion of time trends).

Is there evidence of the poorer being marginalized? We find no impact of poverty on clearing without controls and yet higher clearing in poorer areas with location controls. This suggests that the characteristics of land in the poorer districts are lowering or discouraging forest clearing. If this means that land's productivity or quality is lower, then these results do suggest that the poorer are marginalized.

Observable spatial controls sufficient?

Table 2's column III replaces the district (or sub-district in Table 3) fixed effects with the fixed locational characteristics that we can measure, retaining the prior clearing variable. Now poverty is again insignificant, in both the (A) and (B) regressions in both Tables 2 and 3. Thus our ability to observe the important differences across location seems somewhat limited.

That observables may not fully control for differences across locations finds additional support in column III and in Table 1. While bad soil and bad climate both reduce deforestation in column III, recall from Table 1 that poorer districts have more bad soil but less bad climate. Those districts are farther from markets on average. But while the prior on effects of distance is negative (and see Kerr et al. 2005) pooled regressions including pre-1963 deforestation, plus recent cross-sections), for 1963-1979 the opposite sign is found, i.e., distance raises clearing. Frontier development, perhaps linked to subsidies for cattle in areas far from cities, could well dominate that time interval. In any case, observed differences in Table 1 may not explain all.

Poverty and response to land productivity

Columns IV of Tables 2 and 3 use poorest-quartile dummies to study greatest poverty, specifically whether it limits adjustment. In a subsistence setting, for instance, one might not be able to reduce (and might even increase) clearing when land quality is low. And inability to invest might mean less clearing on good land. Both stories suggest interacting poverty with land productivity. They imply that productivity has less impact on the poorest's deforestation.

Column IV of Table 2 supports that the poor decrease clearing less if land is poor. The poverty–poor–quality interaction is positive. In Table 3, the poverty–poor–quality interaction is insignificant, but high productivity is positive and significant and its interaction with a dummy for poorest quartile is negative and significant. Thus, poorer areas appear to respond less.

DISCUSSION

This paper used a panel data set for tropical forest to control for differences between poorer and less poor areas in examining the effects of poverty itself on deforestation. The district poverty data have greater spatial detail than 'macro' (e.g., country) data, so that the location of the poor can be distinguished, but also have greater temporal coverage than many 'micro' or household-level data. The combination of spatial and temporal variation permits inclusion of spatial controls for locations' differences, which permits a cleaner test of the impact of poverty *per se* on deforestation.

Controlling for locations' differences, we find poorer areas to be cleared more rapidly. This suggests that, all else equal, poverty increases deforestation rates. Without controls for locations' characteristics, the impact of poverty on clearing is underestimated (in this case at zero) as overall the poorer appear to be on land whose relative quality discourages clearing. For the poorest areas, the impact of poverty is weaker, yet we find that there forest clearing responds less to the land's productivity.

An important caveat concerns the lack of parcel-level landownership data. With district-level poverty measures, these results shed light only on poorer *areas,* i.e., not necessarily on the poorer *landowners*. Where people are poorer on average, it still may be the case that much of the land is owned by the less poor or non-poor. This indicates the value of household-level data on both poverty and deforestation.

Finally, despite our results on poverty's impact it is not at all clear either that changing the incomes of the very poorest will affect deforestation greatly or that this would be the best way to affect deforestation. In addition, as noted in the literature, *how* incomes are raised (e.g., capital or off-farm wage) matters. Further, if raising the poorest households' incomes is the goal there may be better justifications, and approaches, than to focus upon and to pay for the forest.

Yet many are hopeful that 'win-win' options to lower both deforestation and poverty can be found. Some existing programs, for instance the PSA program of payments for environmental services in Costa Rica, are often viewed in this light. However as such programs are examined more thoroughly the hurdles to reducing both clearing and poverty, or even to achieving just one of those two goals, become clear even though we believe that there are circumstances where making payments to poor landholders to improve forest management could increase income and forest.

Consider for a moment the actual lowering of deforestation and of poverty by PSA, which did not explicitly target either land-use change or poverty reduction. Sánchez-Azofeifa et al. (in print) and Robalino et al. (2007) find little impact of pre-2000 or post-2000 PSA on clearing rates. This echoes and significantly extends Sierra and Russman (2006) and a World Bank panel evaluating the Ecomarkets Project, though others make claims to the contrary (Walker 2007). It is clear that the first decade of the program did not prioritize 'additionality' (i.e., impact above a baseline that would have occurred without PSA). It was not even a condition of the funding for the PSA.

Thus, payments had relatively little impact on land use and may essentially be transfers. They could reduce poverty if targeted to the poorest, yet such targeting was not central to the PSA effort (in part due to its requirements for participation) and clearly the program was not trying to reduce deforestation by reducing poverty.

This particular, pioneering program may have indirect impacts on forest and/or poverty (not to mention in catalysing others initiatives). Perhaps the 1997 law restricting deforestation would not be accepted without such payments to forested land. But in considering in general the 'win-win' concept that this kind of research raises, the evidence noted above indicates that targeting involving both information and political will would be needed. Even with them, it also seems worth comparing such an approach to programs that directly address either deforestation or poverty.

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NOTES

[1] For more discussion of the model and of structural change over time, see Pfaff (1999) and Kerr et al. (2005).

[2] In assuming full landownership by the manager, we are consciously not laying out a forest frontier model.

[3] Population and economic growth during development path may lead the second-order condition to hold. Yet the condition may be violated if environmental protection becomes more stringent, returns to ecotourism rise, and capital-intensive agriculture requiring less land expands. Should it be violated, our reduced-form empirical specification can also be interpreted in terms of the combination of expression (2) and the profitability condition.

[4] Unlike common regressions for how much forest is present now without regard for the previous deforestation.

[5] See also Greene (1990) for an explicit discussion of the heteroskedasticity.

[6] For discussion of the parks and their forest outcomes see Sánchez-Azofeifa et al. (2003).

[7] This comes from the Ministry of Agriculture of Costa Rica. It resulted from a joint project with the UN FAO.

[8] That the continuous-poverty-index result is stronger suggests that the differences in income above the poorest quartile matter for behaviour. This could be viewed, as was the case for the results from Zwane (2002) noted above, as evidence that marginal changes in income for the poorest simply do permit much behavioural response.

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