# Development and Deforestation: evidence from Costa Rica [?]

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**Abstract**

We estimate a deforestation equation for Costa Rica during the 20th century. Following the land-use literature our model of clearing for agriculture is dynamic, with irreversibilities, but we are not focused on the testing of individual dynamics. Our econometric approach addresses irreversibilities in deforestation by focusing solely upon the area that is still in forest at the beginning of a given time interval. Further, our empirical approach and data over time facilitate a novel effort to test for 'dynamics of development and deforestation' including: national trends; local endogenous development following early clearing; adjustment costs given shocks to profitability; and early use of the best lands. Our results confirm the impacts of agricultural productivity and market access (although upon rates of deforestation, not upon forest stocks as in the literature). The trends results suggest that within a development setting agricultural/forest land use is out of equilibrium as defined solely by local observable drivers. Given trends controls, we also find significant 'dynamic' effects. These results help to understand agricultural clearing patterns and could provide a basis for baseline carbon projections within global regulation.

Keywords:
land use, deforestation, climate change, transitions, development, Costa Rica

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## 1. Introduction

Deforestation leads to changed habitat, release of carbon to the atmosphere, soil degradation, and flooding. However, it allows agricultural development and the expansion of human settlement. While we have considerable understanding of land-use patterns in developed countries, and even of land use at points in time within some developing countries, we have little understanding of the nature of deforestation over time along a development path. Early clearing can lead to rapid, ongoing, and massive losses of forest or to movements toward the protection of remaining forest. Thus, we find it difficult to predict future deforestation patterns in the developing world.

One motivation for predicting forest change is the focus on net carbon emissions within global climate-change regulation. If we are to reward developing countries for protecting forests to sequester carbon, we want a way to predict what countries would have done with their forests in the absence of rewards. Without one, we may give rewards for forest that would have been standing anyway or, to avoid such complications, may choose not to reward the protection of forest even though incentives might have been effective in lowering global net carbon emissions.

To extend forest prediction, we estimate a deforestation equation for Costa Rica using data on forest for five points in time and a partition of the country into over one thousand units of observation (subdistricts). Costa Rica is an excellent country for initiating such analysis of development and deforestation. Not only has it passed through many different development and clearing stages but also its perhaps unsurpassed forest data over time cover a number of decades. Also, Costa Rica is small enough that it faces clear exogenous shocks from international markets, and it features immense exogenous natural ecological variation that greatly affects productivity.

Our work builds on the rapidly growing literature which uses past land-use behaviors to reveal the drivers of land-use change (Stavins and Jaffe 1990 is an early example, Irwin and

Bockstael 2002 a more recent one). In the developing country context Chomitz and Gray 1996, Nelson and Hellerstein 1997, Pfaff 1999, Cropper et al. 2001 and Geoghegan et al. 2001 offer a range of analyses. Kaimowitz and Angelson 1998 offer a wide-ranging survey of approaches to economic modeling of tropical deforestation. Several studies of deforestation have analyzed Costa Rica specifically, notably including regression analysis by Rosero-Bixby and Palloni 1996. Most of these studies, though, have taken approaches somewhat different from our model-based regressions (see, e.g., Sader and Joyce 1988 on rates of deforestation, Harrison 1991's focus on the effects of population, and the rule-based extrapolations in Pontius, Cornell and Hall 2001).

While our dynamic model of individuals' land-use choices follows from the literature, our empirical implementation differs in that it addresses irreversibilities in deforestation. Most land-use-and-deforestation literature analyzes what share of an area is forested at a given time, regardless of history. This assumes areas can be fully reforested between any two points in time. In contrast, to be consistent with the dynamic model we drop this assumption and analyze the fraction deforested during an interval of the forest at the beginning of the interval. The latter has often fallen over time, although some areas in Costa Rica have experienced partial reforestation.

This transitions-based approach and our data over time facilitate a novel effort to test for 'dynamics of development and deforestation' (see also the effort by Cropper and Griffiths 1994). This includes using proxies to control for unobservable national shifts that affect ongoing rates of deforestation. Their significance suggests that agricultural/forest land use is out of equilibrium as defined solely by observed local drivers, i.e. such unobserved national changes matter. As a comparison to the literature, note that if unobserved changes over time interact with explanatory factors that vary only over space they can cause such fixed factors to have ongoing effects upon clearing, which can not be the case in the typical 'static' model of the share of an area in forest.

Along these lines, we provide confirmation for the significant effects within the literature of ecological factors that affect productivity and vary only spatially as well as of fixed distances to markets (a proxy for market access) on forests. However, our productivity and access proxies' results differ in that we find significant effects on ongoing deforestation rates, not forest stocks.

We also make a particular effort to capture other changes over time that are suggested by theories of dynamics within development: endogenous changes in local returns and thus in local clearing rates following earlier agricultural production and forest clearing; adjustment costs that lead to partial adjustment and thus to clearing over time following shocks to profitability; and early clearing of the best lands. We use different measures of past clearing behavior to try to capture these effects and find significant effects, although multiple interpretations are possible.

The rest of the paper proceeds as follows. Section 2 presents the Costa Rican setting and then a dynamic model of individual land-use choices and forest clearing in Costa Rica. Section 3 derives our econometric specification, drawing on other literature analyzing transitions as well as theories suggesting dynamics of development and deforestation. Section 4 discusses the land-use data and explanatory variables that we use, while Section 5 presents and discusses our results.

## 2. A Dynamic Model Of Deforestation For Costa Rica

### 2.1 The Costa Rican Setting

Costa Rica has an area of about five million hectares, about 1% of the Brazilian Amazon. Its deforestation has arisen primarily through transformation of forest into agricultural land rather than through logging. Thus we focus on agricultural extensification rather than logging returns.

The major export crops produced in Costa Rica are coffee, bananas, sugar cane and beef. The predominant crops have varied over both space and time. Spatial variation is affected by the dispersion of "lifezones", or bands of precipitation and temperature, as certain crops grow better in certain lifezones. The crops have also varied over the nearly century and a half since the only clearing was in the Central Plateau (site of the capital, San José, and a good location for growing coffee) and around Puntarenas, a Pacific port. The economy has opened to trade since 1960 and export prices have shifted dramatically over time, altering both returns and crop mix. Additional variation in returns has resulted from changes in the technologies of production and thus yields.

While both income per capita and population grew dramatically from 1900 through 2000, clearing was slow until a considerable rise from 1950 through the early 1980s, fueled in part by population growth including immigration from Nicaragua. Cattle production and coffee exports expanded greatly. Both coffee and beef prices rose sharply, peaked in the late 1970s, and then fell somewhat in the mid 1980s. Deforestation dropped dramatically in the late 1980s and 1990s. In addition to prices, key factors included the creation of national parks, other forest policies, and transformation of the economy from an agricultural base towards manufacturing and services.

2.2 <u>A Dynamic Model</u>

Our use of a dynamic theoretical model addresses the issue of irreversibilities and follows the land-use-and-deforestation literature (e.g., Ehui and Hertel 1989, Stavins and Jaffe 1990, and Parks and Hardie 1995). The manager of each hectare *j* faces a dynamic optimization problem. Risk neutral by assumption, the land manager selects *T*, the time when land is cleared, in order to maximize the expected present discounted value of returns from the use of hectare *j*:

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

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[1]

r = the interest rate

Two conditions are necessary for clearing to occur at time *T*. First, clearing must be profitable. For clearing to occur, the present discounted rents from non-forest uses will have to more than compensate the manager for the lost returns from forestry uses and the net cost of land clearing:

$$\underset{T}{\overset{?}{?}} \, (R_{jt} - S_{jt})\, e^{-rt}\, dt \ - C_T > 0 \tag{2}$$

However, even if clearing is profitable at time *t*, it may be more profitable to wait and clear at *t+1*. For example, clearing costs may fall. Thus, the following 'arbitrage' condition must hold:

$$R_{jt} - S_{jt} - r_t\, C_t + \frac{dC_T}{dt} > 0 \tag{3}$$

Both conditions must hold for clearing to be preferred. However, if the second-order condition (4) holds[2], then either of these necessary conditions is also sufficient for clearing to be chosen.

$$\frac{dR_{jt}}{dt} ? \frac{dS_{jt}}{dt} ? \frac{d^2}{dt^2} C_t \ ? \ 0 \tag{4}$$

The forest status for each parcel at each point in time will be determined by whether or not these conditions hold or have held previously. Land parcels will have different outcomes across space due to different returns, e.g. from varying land quality and as well as varying access to markets. Returns and costs will also change over time, for both exogenous and endogenous reasons. These all feed through individual decisions to determine aggregate patterns of deforestation over time.

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[1] A more comprehensive model would also include uncertainty, risk aversion, and forward-looking knowledge of the ability to shift back and forth optimally between cleared and uncleared states. Uncertainty combined with the irreversibilities in deforestation and the ability to learn over time implies an option value to waiting to clear, though as noted neither in the related deforestation literature nor in this paper are individual dynamics the empirical focus.

[2] For land-use change in a developing country, population and economic growth along with improved infrastructure may lead (4) to hold, although at a certain stage of development this may be reversed. As development proceeds, environmental protection may become more stringent, returns to ecotourism may well rise, and agriculture can be more capital intensive and require less land. Agricultural returns could fall relative to forest returns on some land.

## 3. Econometric Specification

### 3.1 Deriving A Regression Equation

To derive an econometric specification from the model, we assert (4) and work from (3). Our reduced form regression equation, however, can also be interpreted in light of (2), the profit condition. We are not attempting to determine whether the profit or arbitrage the condition is the dominant constraint and the best structural model. Rather, we test whether our measured factors have theoretically predicted effects on observed transitions, given that any measured factor could affect more than one variable in the arbitrage condition and variables within the profit condition.

Following the model, we examine parcels' transitions from forest to non-forest. The theoretical reason to separate deforestation and reforestation transitions is that reforestation does not result automatically from reduced deforestation pressure. Deforestation has irreversibilities, since trees take time to grow and incurring the costs of development changes marginal returns. After an export boom causes clearing, even if prices fall to a level insufficient to induce clearing it may not induce abandonment and reforestation of recently cleared land. Thus, we work with transitions, treating deforestation and reforestation differently and focusing upon deforestation. That is ruled out by the forest-share approach, which focuses upon how much forest is present, as a function of determinants, regardless of whether an area was previously all forested or cleared.

The model predicts time of clearing, i.e. the timing of the deforestation transition. Our intuitions draw upon econometric analysis of 'duration' (see Kiefer 1988 or Lancaster 1990), for instance of the probability that a firm adopts a given technology or an individual ends a spell of unemployment by finding a job. Along these lines, applying (3) to all parcels and years predicts the clearing of individual forest parcels: if (3) has ever been satisfied since the last transition to forest, then the parcel is cleared; if not, then the parcel is forested and a candidate for clearing in the next interval, such that if (3) is satisfied during that interval then the parcel will be cleared.

However, our data on deforestation are not for parcels but for larger areas. We observe not discrete clearing of parcels during intervals but continuous rates of loss in these larger areas. The model's predictions can also be aggregated. For any larger area *i* with parcels $j = 1 \ldots I_i$, the clearing or deforestation rate is predicted to be the number of parcels that satisfy (3) during this interval but not before, and so are cleared during the interval, divided by the number that had not previously satisfied (3), i.e. were not previously cleared. Thus, summing parcel-level predictions within area *i* yields predicted forest at the start of the interval and forest loss during the interval.

These predictions are deterministic for any parcel *j* and thus also for any larger area *i*. Because the net benefits of clearing are exhaustively described by the variables in (3), literally applying (3) to our measures of each of the returns and the clearing costs yields deterministic forest/non-forest predictions. However, we do not perfectly measure the parcels' returns and costs, let alone the expected future values. This is particularly clear for aggregated data, in which not only forest outcomes but also the explanatory variables are measured not for parcels but for larger areas (subdistricts). Since each parcel in an area has the same measured net benefits, (3) predicts that any area should be entirely forested or entirely cleared. Often neither is the case.

Actual returns and changes in costs will vary across parcels within an area, though the observable factors in benefits and costs ($X_{it}$, $i$ = larger areas) yield a single estimated net benefit. Thus the $X_{it}$ imperfectly measure net benefits from clearing for the parcels within a larger area. Instead of applying (3) directly to all land parcels using only the larger-area measures $X_{it}$, then, we explicitly acknowledge that we do not measure returns perfectly, such that clearing occurs if:

$$R_{ijt} - S_{ijt} - r_t C_t + \frac{dC_T}{dt} = X_{it}ß - ?_{ijt} > 0 \qquad (5)$$

where again *i* refers to an area, *j* to a specific parcel, *ij* to a specific parcel *j* known to be in area *i*, and $?_{ijt}$ is a parcel-year-specific term for the unobserved relative returns to forested land uses.

The unobservable factors in returns represented by the $?_{ijt}$ could include productivity of the land, market access, availability of family labor, access to credit, and the slope and aspect of a parcel. They could also include national trends, although we will include explanatory proxies for trends. The presence of this term means that parcels' net benefits of clearing are uncertain, given the $X_{it}$, which makes the model's predictions probabilistic and shaped by the assumed distribution of $?_{ijt}$:

$$\textit{Probability (satisfying (5) so that cleared if currently in forest)} = \textit{Prob}\ (?_{ijt} < X_{it}ß) \quad (6)$$

However, recall that the parcels cleared during an interval not only satisfy (5) during that interval but also have not previously satisfied (5), or they would no longer be forested parcels. Thus, our predicted deforestation rate within larger areas is analogous to a 'hazard rate' defined in the transitions literature. Following (6), we can define this loosely as $h_{ijt} = f(X_{it}ß)/(1-F(X_{it}ß))$. The key intuition is seen in the denominator's use of the cumulative distribution function for $?_{ijt}$: parcels that are not yet cleared may have higher unobserved relative returns to forested land uses. For instance, they might be poor land, for unobservable reasons, for earning profit in agriculture. Because for an interval we only examine the clearing of the still-forested parcels within an area, we will include the extent of previous forest clearing within an area as an explanatory variable to test whether the still-forested parcels are high-$?_{ijt}$ parcels less likely to be cleared for a given $X_{it}$.

Given that, for examining the clearing of the still-forested land we proceed, following (6), with probabilistic predictions that a parcel satisfies (5) given the observed $X_{it}$. Since the $X_{it}$ are the same for each parcel in an area, the predictions are effectively for areas' deforestation rates. The predicted clearing rates depend upon the $X_{it}$ as well as on the assumed distribution of the $?_{ijt}$. If the cumulative distribution of $?_{ijt}$ is logistic, then we have a logit model for each parcel:

$$F(X_{it}ß) = \frac{1}{1 ? \exp(X_{it} ?)} \quad (7)$$

For our grouped data, we estimate this model using the minimum logit chi-square method also known as "grouped logit".[3] If $\hat{h}_{it}$ is an area's measured rate of forest loss, then we estimate:

$$\log \frac{\hat{h}_{it}}{1 ? \hat{h}_{it}} ? X_{it} ? ? ?_{it} \tag{8}$$

The variance of the $?_{it}$ (referring to areas, not parcels) can be estimated by $\frac{1}{I_{it}\hat{h}_{it}(1 ? \hat{h}_{it})}$, where $I_{it}$ represents the number of forested parcels within area $i$ at the beginning of interval $t$, and the estimator is consistent and asymptotically normal.[4] This is estimated by weighted least squares.

3.2 Proxies For Dynamics Of Development

Here we consider some changes in net benefits during development that are not directly observable but for which we might use proxies in our regressions to capture effects on clearing. We have noted that, for given $X_{it}$, lower-$?_{ijt}$ parcels may be cleared first since local land users are likely to know more about local land quality than we do. We include past clearing as a proxy for this 'selection dynamic', with the prior that past clearing has a negative effect upon clearing. As is the case for all variables, its precise impact depends upon the assumed distribution of $?_{ijt}$. A given shift in relative returns when in the tails of the distribution affects fewer parcels than does the same shift when a greater number of parcels are marginal, near the modes of the distribution.

We also noted that the $?_{ijt}$ may include national trends, exogenous to local land use, that are not well measured. Improving legal and economic institutions increase tenure security, credit, and insurance, yielding increased investment. Improved infrastructure, e.g. distribution networks, lowers costs and raises output prices. Improving technologies lower costs and improve quality. Thus one may observe that deforestation occurs without shifts in the observed $X_{it}$ over time, and

[3] Berkson 1953, cited in Maddala 1996. See also Green 1990 for explicit discussion of heteroskedasticity.
[4] Maddala 1996, p. 30.

we include time as a proxy for these trends. Without reason to believe that such effects of development are constant over time, either in general or for Costa Rica (Section 2.1), to allow for a non-linear relationship between development and clearing we include a polynomial for time.

Such changes may interact with spatially varying $X_i$, e.g. better parcels may benefit more from higher farm-gate prices because of their higher yields. This means that $X_i$ that are fixed over time but vary over space could have ongoing effects upon the rate of deforestation. Our tests of the effects of market access and agricultural productivity address clearing rates, not forest stocks.

Other aspects of economic and institutional development may be locally endogenous. One process exogenous to individual land users but endogenous to regions is that as the forest is cleared and human activity increases, this stimulates further investment in infrastructure such as in credit agencies, transport networks and services, raising returns in unobserved ways. Thus past clearing may stimulate current clearing. This suggests the opposite prior for past clearing from that associated with the 'selection dynamic' above. Which dominates is an empirical question, and to allow for tradeoffs between these two effects we include a polynomial for past clearing.

Finally, a process endogenous even to individual land users is that whenever agricultural returns shift, adjustment to the new equilibrium level of cleared land may not be instantaneous. Costs may rise with adjustment due to limited local labor, labor mobility and access to credit. If partial adjustment results, past changes in returns can have ongoing clearing effects, suggesting persistence in rates of deforestation. We include lagged clearing as a proxy for such effects, but only in later cross-sectional regressions since for early intervals this is the same as past clearing.

### 3.3 Spatial Autocorrelation

The $?_{ijt}$ in (5) and $?_{it}$ in (8) may not be independent over space. Contiguous land parcels may share productive soil qualities that are unobserved, or demand or supply conditions that are unobserved may be correlated across our larger areas but within a region. If the $?_{it}$ are spatially autocorrelated, regressions for (8) will provide consistent but inefficient coefficient estimates.[5]

Using the distances among the subdistricts' centroids, we test for spatial autocorrelation at each point in time. First we use a spatial-weight matrix to compute the Moran I statistic.[6] This requires a judgement about which observations may be related to others and how. We assume that within a cutoff distance (we try 5, 10, 30 and 50 km) of a given subdistrict, each subdistrict 'nearby' (within the cutoff distance) exerts an equal potential influence on the given subdistrict.[7] As the number of such influential observations varies over sub-districts, we row-standardize the matrix so that the influence weights always sum to one, i.e. we divide a fixed total influence upon each subdistrict equally among those observations assumed to potentially have influence.

With this approach, for each cutoff distance (5-50km) we reject spatial independence. Further, in order to go beyond magnitudes of the Moran I statistics in considering what distance might be the best cutoff to use, without assuming a particular spatial-weight matrix we also apply Conley and Topa 2002's non-parametric estimate of spatial covariance as a function of distance. Estimated covariance is above the range for accepting spatial independence until about 40km.

Given these results, we also correct for spatial autocorrelation in two ways, first using the spatial-weight matrix following Anselin 1988's EGLS approach to more efficient estimates.[8] For each cutoff distance, we find significant coefficients for the spatial-weight matrix, and the basic patterns of the results are unchanged. However, estimating the $X_{it}$ coefficients more efficiently

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[5] If the $?_{ijt}$ are spatially correlated across parcels, subdistricts may differ on average in unobservable relative returns. This suggests a positive prior for past clearing other than the endogenous development prior (and contrary to the negative selection-dynamic prior) since subdistricts better for clearing in the past remain that way, *ceteris paribus*. Because this implies a $?_i$ along with $?_{it}$, below we do fixed-effect estimation to better test other past-clearing stories.
[6] Anselin 1988, and 1995 with Florax (eds.), discuss testing for and correction of autocorrelation with such matrices.
[7] We have done some testing for robustness in which influence declines with distance but have found little effect.

does yield some changes, such that the choice of the correct cutoff distance becomes a concern.[9] Thus we retreat from such re-estimation to computing robust standard errors. Specifically, for the coefficients from the weighted OLS we apply, for a 40km cutoff, a covariance matrix estimator from Conley 1999 shown to be consistent when effective distances are not precisely observed.[10] This approach increases the estimated standard errors, on the order of at least doubling them on average, so below we note which significance results vanish as a result of using these estimates.

## 4. Data

Table 1 gives the means and standard deviations of the variables used, weighted by the forested area per observation. The following sections describe the data sources and variable definitions.

### 4.1 <u>Forest Cover & Dependent Variable</u>

We observe forest cover at five points in time: 1963, 1979, 1986, 1997 and 2000. We use the separation of the country into 436 political districts, although as noted our unit of observation in space is a form of subdistrict. Specifically, in each district we have one observation for each 'lifezone' that is present (the Holdridge Life Zone System (Holdridge 1967) divides Costa Rica into twelve ecological 'lifezones' reflecting levels of precipitation and temperature). On average there are about three lifezones present in a district and we can have 1229 observations per year. Our dependent variable (more below) is the annual percentage loss during a given time interval from the area of forest present within a given district-lifezone at the beginning of the interval. As some district-lifezones become fully deforested, over time the observations per interval fall.

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[8] OLS is run, then the residuals are used for a maximum likelihood estimate of the coefficient for the assumed spatial-weight matrix, and then based on this weight matrix and coefficient we compute the desired Estimated GLS coefficients for the $X_{it}$. This process is repeated using the resulting residuals until a convergence criterion is satisfied.
[9] See, e.g., Anselin 1988 p.109: "In finite samples, the distribution of the estimates is not well defined, nor is EGLS necessarily superior to OLS in a mean square error sense. In addition, the properties of the EGLS estimate are sensitive to a correct specification of [the covariance matrix, primarily determined by the spatial weight matrix]".
[10] We applied this approach for the other cutoff distances as well. It always increases the estimated standard errors.

The data used to create the dependent variable come from several different sources. The 1963 data are from aerial photos (translated into maps) digitized by the University of Alberta to distinguish forest and non-forest.[11] For calculating 1900-1963 transitions, we assume that before 1900 all the area that could potentially be forested (not rocks, water or mangroves) is in forest.[12] The 1979 data were produced from Landsat satellite images by the National Meteorological Institute of Costa Rica (IMN 1994), with final products printed at a 1:200,000 scale.[13] Within potentially forested areas, they distinguish 'forest' and other land-uses we group as 'non-forest'. The 1986 and 1997 data were also derived from Landsat satellite images (see FONAFIFO 1998) and distinguish forest, non-forest, and mangroves, while also indicating secondary forest (land classified as forest in 1997 but not 1986) with final maps at 1:250,000 scale. The 2000 Landsat images were processed by the University of Alberta EOSL to be consistent with 1986 and 1997.

The 1986 and 1997 data are thought to mis-classify forest in areas where there is deciduous tropical forest, i.e. primarily in the Guanacaste region, depending on when the data were collected. Satellite images are generally collected during the dry season, when there are few clouds, but at those times often there are also no leaves in the deciduous forest, which can be mistaken for bare soil or pasture. This introduces measurement error in the dependent variable.[14]

For each district-lifezone for each time interval, we calculate the area deforested. The 1986, 1997 and 2000 maps all have clouds so we calculate these areas deforested (and thus also rates of loss) from the visible portions of each observation, using pairs of images with consistent

[11] In fact, the maps appear to represent a range of years centered around 1963, one source of measurement error.
[12] Some areas had been cleared by that time, but we find little difference using 1850 as the "earliest clearing date".
[13] They had support from the Ministry of Environment and Energy (MINAE) and the Agriculture Ministry, and the data were improved by the University of Alberta. They used remote sensing from two different sensors: Landsat 4 (Multispectral Scanner, 80x80m of spatial resolution and 4 spectral bands) and Landsat 5 (Thematic mapper, 25x25 m of spatial resolution and 7 spectral bands). In general, they visually interpreted data from black and white photographic products, and manually extracted fractal boundaries between classes (i.e., did no image processing).
[14] We reran our regressions for the subset that does not include this area. The results do not change significantly.

clouds. For intervals before 1986-1997 we cannot distinguish the gross from net transitions, and assume gross deforestation equals net.[15] If the measured gross deforestation is negative, since we are analyzing deforestation we use a value of zero. After 1986, we know the gross deforestation.

Our dependent variable, the deforestation rate, is the area deforested during an interval divided by the area within the district-lifezone of the forest "at risk" at the start of the interval. Areas with no forest at the start of an interval are dropped, as there is no risk of deforestation. We assume that forest in national parks and biological reserves is not at risk of deforestation (it was not in fact cleared[16]). This yields over 4300 observations for pooled regressions.[17] Finally, because our time intervals are of varying lengths, for comparison we use the annual rate of deforestation. If $?_{it}$ is the deforestation rate (area deforested over area at risk) for a given interval and $n$ is the number of years in that interval, our annual deforestation rate dependent variable is:

$$\tilde{h}_{it} ? 1 ? (1 ? ?_{it})^{\frac{1}{n}} \tag{9}$$

Thus we implicitly assume that this annual deforestation rate was constant during each interval.

## 4.2 Explanatory Variables

### *4.2.1 Direct Measure of Returns*

The annual return $r_{jkt}$ to a given hectare $j$ in crop $k$ at time $t$ is the crop price $p_{kt}$ times the annual yield per hectare $y_{jkt}$ minus the costs of production $cost_{jkt}$ minus the transport cost $t_{jkt}$. For each year, we estimate the returns for the four major export crops: coffee, bananas, sugar and beef. We have data from 1950 onward although its quality improves significantly in later years. For each interval, returns are averaged across the years for an average return (in 1997 US$) to crop $k$ on one hectare of cleared land during that interval (see Appendix on data and techniques).

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[15] Anecdotes suggest reforestation was not widespread before 1986, so that this is probably not a major problem.
[16] For discussion of the parks and their forest outcomes see Sanchez et al. (2003).

Any parcel is used for one crop at a time. We define $s_{jk}$ as the probability of a crop being chosen as the use of newly cleared land. For larger areas, these probabilities imply expected shares of the area in each crop, to be used as weights in our measure of expected annual return:

$$R_{it} = E(r_{ijt}) = \sum_{k} s_{jk} r_{jkt} \tag{10}$$

We calculate the $s_{jk}$ using data on production patterns in the 1970s and 1980s and information on the suitability of different lifezones for different crops. For example, in a humid, lower-montane area we represent the land-manager's choices by assuming that cleared land will be used for coffee or something with a similar return. The resulting $R_{it}$ is our returns measure, AGRETURN. While this is not our strongest variable, still we expect higher returns to predict higher clearing.

*4.2.2 Returns Proxies*

Given the difficulty of perfectly measuring returns, we also consider proxies for returns to clearing. Lacking a dollar measure of transport costs, we use the minimum linear distance in kilometers to a major market, DISTCITY, i.e. the shortest of the distances from an observation's center to San José, Puntarenas and Limon. We also interact this with time since we expect this effect to diminish with time due to improved roads and vehicles. In cross-sectional regressions for recent years (1986-2000), we also use road density (ROADSDEN), i.e. total length of roads in a district in the mid-1980s divided by the district's area, as an additional proxy for market access.

To control for local market size, we include population density POPDEN and its square POPDEN$^2$. Population is census district data, for 1950 and 1984, and is divided by district area. Because population is potentially endogenous to other factors that lead to economic activity and deforestation, we use lagged population densities (i.e., 1984 only for the last two time intervals).

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[17] To avoid dropping zero-clearing observations in (8), we add one hectare of clearing to all measured deforestation. This addition is not a significant error. It is smaller than the minimum mapping unit and well within the error range.

Our ecological variables proxy for agricultural productivity. More productive land should have higher clearing rates. We create dummies for three groups of lifezones: GOODLZ includes all humid (medium precipitation) areas, which have moderate temperatures; MEDLZ includes very humid areas (higher precipitation) in moderate to mountain elevations (and hence moderate temperature); BADLZ includes very humid areas with high temperatures (tropical), very dry hot areas, and rainy lifezones, all of which are less productive. We also have data on seven different soil types for land outside national parks, another proxy for agricultural productivity.[18] We create a BADSOIL measure, i.e. the proportion of a district-lifezone with low-productivity entisol soil.

Finally, as discussed in section 3.2, we also include a polynomial for the total previous clearing in a district-lifezone (%CLEARED), a polynomial for time (TIME), and for cross-sectional regressions for more recent years also the clearing rate in the previous interval (PREVCLEAR).[19] The latter proxies for the effects of costs of adjustment in the previous interval, which may delay clearing until this interval. Thus it is expected to have a positive sign. TIME and its square $\text{TIME}^2$ proxy for unobservable changes in net returns over time resulting from exogenous improvements in infrastructure and the general development process.[20] The discussions of Costa Rican history above suggest a trend of increasing returns but also shifting trajectories over time, motivating the polynomial. The polynomial for %CLEARED is motivated by the existence of at least two priors for this proxy. The selection dynamic, in which the parcels with the highest unobservable returns to clearing are the first to be cleared, suggests a negative effect. Endogenous local development, in which clearing spurs human choices that raise returns and clearing, suggests a positive effect.

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

[19] Recall from section 3.2 that PREVCLEAR is equivalent to %CLEARED for the second interval and highly correlated for early intervals. To measure TIME we use the number of years from 1900 to the middle of the interval in question.

[20] The creation and protection of parks could be part of and also an observable proxy for development trends leading deforestation to fall in the 1990s. Park or reserve locations are likely to be endogenous to agricultural productivity, though, and we do not have instruments. However, recall that we examine deforestation only outside of park areas.

## 5. Results and Discussion

Figure 1 shows that the unconditional, national deforestation rates start relatively low, rise to a peak in 1979-1986, and fall to almost zero by 2000. Our regressions help to separate the roles of factors we directly measure, certain unobserved processes, and the distribution of land quality.

Tables 2 and 3 report uncorrected results from grouped logit weighted OLS regressions. As noted, a non-parametric test found significant spatial autocorrelation up to 40km, and for 5 - 50km parametric tests reject the null of spatial independence. Given spatial autocorrelation our weighted OLS is consistent, while using Anselin 1988's EGLS estimator to increase estimation efficiency yields small changes in coefficients that depend upon the spatial-weight matrix. Thus we computed robust standard errors, for a cutoff of 40km, and discuss their implications below.

### 5.1 Returns Measure & Standard Proxies

*Returns*

Table 2 presents results from the grouped logit with all five time intervals pooled. Our direct measure of returns contributes little when we control for returns using our proxy measures. The returns coefficient is insignificant and the magnitude of returns' marginal effect is small.[21] If we drop the other variables except time (as a quadratic or dummies), however, in regressions that control for district-lifezone fixed effects the impacts of returns are significant and positive. Yet as suggested by Table 3's cross-sectional regressions, we do not feel that we have strong data on returns for the earliest interval (1900-1963). For 1979-1986 a surprisingly significant negative effect is found though for the later years, with the best data, returns' effects are positive and further their effects' statistical significance remains if using robust estimated standard errors.

[21] Our model suggests that expected future returns matter too, but next-interval returns are unreliable in both sign and significance in a short pool and in earlier cross-sections. In the 1986-1997 cross-section, i.e. the best returns data we have to test this, future returns are significant but are multicollinear with current returns, which lose significance.

*Access To Markets*

Transport costs and access to markets appear to play an important role in deforestation. This is often noted within the literature but recall that our testing of this effect concerns ongoing influence upon the rates of deforestation, versus a permanent influence upon the stock of forest. In Table 2, the further land is from a market center the less likely it is to be cleared in the earlier intervals. However, as hypothesized concerning the effects of development (including improved infrastructure), the negative effect upon deforestation of a given distance to key markets falls in magnitude over time, as evidenced by the significant positive coefficient on DISTCITY*TIME. Both coefficients remain significant when using standard errors robust to spatial autocorrelation.

In Table 3, again increased distance initially significantly reduces the probability of deforestation significantly. Between 1963 and 1979, though, more isolated places appear to face more deforestation pressure, and for 1979 to 1986 this effect is suggested as well, although the spatially robust standard errors make these effects insignificant. Still this shift in coefficients is worth noting. It may reflect the 'frontier' nature of deforestation as new agricultural areas were opening rapidly, in part as a result of policies. In the later periods, distance is a slight deterrent to deforestation, though insignificant in the last period with robust standard errors. This shift over time is consistent with Table 2's overall marginal effect (for a 1986 medium lifezone, good soil and otherwise (forest-weighted) average characteristics), although that at this point in time the positive interaction term dominates may suggest limitations of this simple interactive term.

Finally, from Table 3 the additional proxy for market access, the density of roads (not in the pooled regression because it is measured only in the mid-1980s) is significant only in the period 1997-2000, where it has the hypothesized positive effect. More roads reduce travel costs. Our other measure related to markets is local population density. Before 1986, higher population density leads to more deforestation, although a negative quadratic term suggests that it is the initial populating of an area rather than increasing the density that is most significant.[22] In recent years, the effect becomes insignificant and then the signs reverse. Possibly cities protect their few remaining forests or maybe such areas have already exhausted the available productive land, although using robust standard errors makes the latest-interval effects marginally significant.[23]

*Ecological Factors in Productivity*

Our ecological proxies for agricultural productivity are generally significant and have the expected signs in Tables 2 and 3. Good and medium lifezones are significantly more likely to be cleared than are the bad lifezones (including with robust standard errors). These are large effects, as indicated by the marginal calculations in Table 2. In Table 2 though not always in Table 3 the good category is significantly different from the omitted medium category. Areas with bad soil are less likely to be cleared in both tables, although these coefficients are not robust (for instance, using the robust standard errors in the pooled regression makes the effect insignificant). These results provide support for findings within the related literature, although again our testing of the effects of the spatially varying factors examines their effects upon ongoing rates of deforestation.

### 5.2 Proxies For Dynamics Of Development

*Trends*

We find that time has an initially positive but concave and hence ultimately negative effect on deforestation rates. This significant concave shape remains in a fixed-effect regression, and when using the standard errors that are robust to the presence of spatial autocorrelation. Further, the unobserved changes that this proxy represents are important trends within relative

[22] Pfaff 1999 also found such non-linearity. It suggests that the spatial distribution of people affects total clearing.

[23] Note that roads and population are somewhat collinear, and that without roads population is insignificant in this last cross section as well as 1986-1997. Without population, roads appear more significant in the 1986-1997 interval.

returns to clearing, as shown by the marginal effect in Table 2. Estimation employing time dummies as well as other variables (unlike in Figure 1) confirms large significant time effects, specifically a relatively constant baseline deforestation rate until a significant drop after 1986. These estimated drops may reflect political and regulatory change as well as economic shifts. Clearly time can be an important control for estimating the effects of other explanatory factors.

*Previous Clearing*

Starting with Table 3 and the clearing immediately previous to intervals being analyzed, in the cross-sectional regressions for recent years higher previous-interval clearing significantly increases current clearing (even controlling for the total past clearing, which is discussed below). These effects remain significant when using the robust standard errors, especially for 1997-2000. This suggests partial adjustment, i.e. that the adjustment to land-use equilibrium may not always be reached during an interval, such that shocks to profitability have impacts across time intervals.

For total past clearing, Table 2 presents a significant positive linear effect but a negative significant effect for the squared term, suggesting potential importance of both the endogenous-local-development and selection-dynamic phenomena that give rise to the two conflicting priors. Concavity is robust to fixed effects but only the positive effect is significant with robust standard errors. Much of the same is found in Table 3, including greater robustness of the positive linear effect. The marginal effect in Table 2 is on net positive, and even on net this is a sizeable impact.

The fixed-effect result is notable given the evidence for spatial autocorrelation of errors across the district-lifezones. This suggests that there may well be spatial autocorrelation across parcels within district-lifezones. One area may have a cluster of high-$?_{ijt}$ parcels and another a cluster of low-$?_{ijt}$ parcels. More generally, larger areas *i* may have different average unobserved returns. That could yield a spurious positive estimated effect of past clearing on current rates of clearing due to some of the district-lifezones simply being better places to clear. Controlling for fixed effects removes that correlation, yielding clearer evidence concerning the two phenomena.

Given that a concave result and in particular the positive effect remain with fixed effects, the evidence appears to support endogenous development. A positive effect in the early intervals could be simply partial adjustment (as lagged and past clearing are the same in early intervals), but significant effects are found even controlling for lagged clearing in the later intervals. The negative squared term could indicate that the selection dynamic matters when more of the best land has already been cleared. However, the positive effects are consistent and are dominant in both magnitudes and robustness, suggesting an important dynamic within local development.

*Structural Change & Summary*

Overall, our results conform well with theory and we can explain a significant percentage of the variation in deforestation, including in the cross-sectional regressions. This is the case despite what appears to be significant change over time as development occurs. For both a five-interval common specification involving only the variables in the first column of Table 3 as well as a four-interval common specification involving the variables in the second column of Table 3 other than population density, we find rejections of the null hypothesis of no structural change. These and the changing coefficients in Table 3 are themselves interesting development results.

In sum, consistent with previous literature we find that spatially varying market access and natural productivity have explanatory power, although we find this for deforestation rates. Also, the long interval covered by our data allows us to examine development and deforestation. We find significant trends and that proxies for dynamics of development have significant effects. This suggests a dynamic process in which early forest clearing affects later deforestation patterns and may even help to trigger the processes causing the effects of explanatory factors to change.

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## Appendix – direct measure of returns from beef, coffee, sugar and bananas

Units: crop price is in $/kg; yield is in kg/ha; production cost is in $/ha; transport cost is in $/ha.

Observations: 436 districts in Costa Rica. From 1900-1997 in principle, but 1950-1997 in fact. Limitations on historical data mean that we do not have good measures for years before 1950. More generally, even within 1950-1997 the quality of the data is higher for later time intervals.

Prices: though some production is sold domestically, Costa Rica is a small country and we use exogenous export prices (in 1997 US$). Price data are taken from two sources, the Costa Rican Ministry of Planning (Vargas and Saenz 1994) and the Central Bank of Costa Rica website.

Yields: crop yields vary over time because of technological change, and across space because of differences in general productivity and in suitability for particular crops. While lifezones and soils proxy for this variability, here we estimate yield. For instance, in some areas the yield for a particular crop is effectively zero since it would never be grown there. Our data is of two types.

For some crops we have data on yield per hectare: for bananas, county level for 1977-1997, and given no obvious trend we assume this to be constant before 1977; for sugar, province level for several years between 1950 and 1977 and for county level in 1998, and we apply the province-level trends to extrapolate the yields for all counties within a province before 1998.

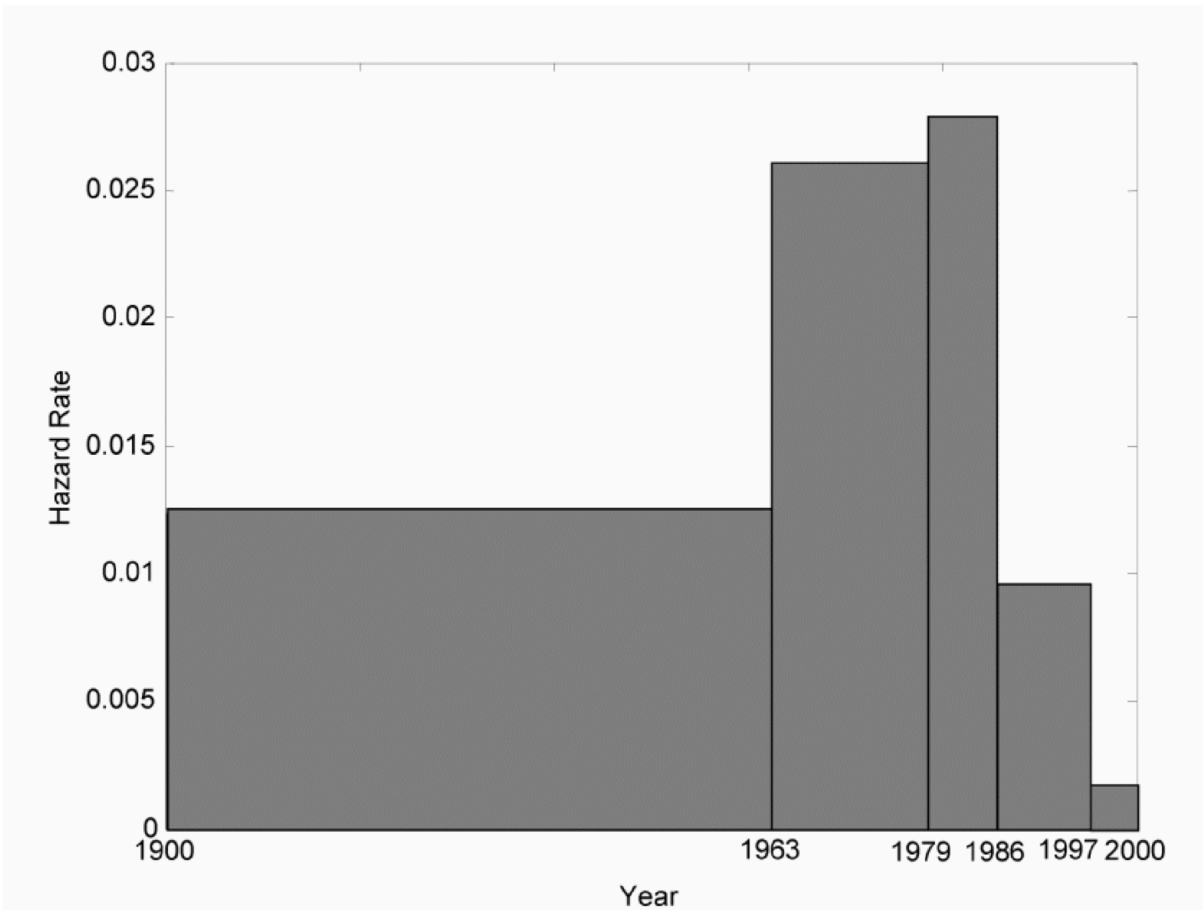
Else we observe production (kg) and area in production (ha) and divide to get the yields. For coffee we have production from 1974-1992 and 1996 at county level and area at county level from the census for 1950, 1955, 1963, 1973, and 1986. We assume production is fixed pre-1974 and area is fixed post-1986, and then interpolate the coffee areas before calculating yield ratios. For pasture we use national production from 1950 to 1995 and divide by census estimates of area for a national yield estimate. We create county-level variation by utilizing the ratio of number of cattle to pasture in the census data, assuming this variation is related to productivity. In locations where the yields for particular crops are undefined within our data, we assume that they are zero.

Costs: we estimate operating costs on an annual basis, although the data are sparse. For coffee, we observe costs only in 1979 and 1981 by coffee zone. For beef we have a single reliable estimate from 1974 at the national level. For sugar, data is better although still at national level, with estimates from Barboza, Aguilar, and León 1982 and Chaves-Solera 1994 for 1963, 1966, 1972, 1977, 1979 and 1994–96. For bananas we have a technical estimate from Hengsdijk (personal communication, Wageningen Agricultural University) for 1997, but no previous data. These are assumed constant outside the period within which they are observed and interpolated. For transport costs, lacking direct measures, currently we rely upon the proxy described above.

Crop Shares: to predict how likely each of the four crops is to be chosen, we use a combination of census and satellite land-use data to estimate the share of each crop in each district. While the satellite data are more precise, they distinguish not crops but simply land uses (permanent crops, pasture, and forest). The data is from 1973 and 1984, and our shares do not change over time.

We combine the district shares with the crop returns for expected annual return per district-year, following (10) above. Then we average returns across intervals to generate the mean returns to which we assume our estimated constant annual interval deforestation rate will have responded.

**<u>Figure 1</u> -- Unconditional National Deforestation Rates By Time Interval**[?]




[?] See Section 4.1 for discussion of data sources and calculations. For each district-lifezone for each time interval, we calculate the area deforested using pairs of images with consistent clouds. Deforestation rate is the area deforested during an interval divided by the area within the district-lifezone of the forest at risk of clearing. As time intervals are of varying lengths, we use annual rates of deforestation within time intervals, implicitly assuming that this rate was constant during each interval. For this figure, we then do forest-weighted averaging for the country as a whole.

**Table 1** -- **Variable Definitions and Descriptive Statistics (weighted by forest area)**

| Variable | Name | Mean | Std. Dev. | Minimum | Maximum |
|---|---|---|---|---|---|
| Annual Deforestation Rate | | 0.016 | 0.048 | 0.00001 | 1 |
| Returns & Returns Proxies | | | | | |
| Agric.Returns/ha (US$ 1997) | AGRETURN | 659 | 1154 | 0 | 5047 |
| Distance to major markets (km) | DISTCITY | 73 | 38 | 0 | 186 |
| Roads density (km/ha) | ROADSDEN | 0.0026 | 0.0022 | 0 | 0.049 |
| Population density (#/ha) | POPDEN | 0.089 | 0.38 | 0 | 107 |
| Ecology (for productivity) | | | | | |
| Dummy for humid lifezones | GOODLZ | 0.24 | 0.43 | 0 | 1 |
| Dummy for very humid (pre montane, lower montane) and montane lifezones | MEDLZ | 0.23 | 0.42 | 0 | 1 |
| Dummy for very humid (tropical), dry (tropical), and rainy lifezones | BADLZ | 0.54 | 0.50 | 0 | 1 |
| Proportion of area in soil type entisol | BADSOIL | 0.11 | 0.23 | 0 | 1 |
| Dynamics | | | | | |
| Time at midpoint of interval | TIME | 66 | 26 | 33 | 100 |
| Proportion of forest cleared | % CLEARED | 0.21 | 0.26 | 0 | 0.99996 |
| Lagged deforestation rate (for 1979 onward) | PREVCLEAR | 0.0080 | 0.017 | 0 | 0.46 |

## Table 2 -- Pooled Regression Results

| | Grouped Logit[a] (uncorrected[b]) | | | |
|---|---|---|---|---|
| **Years** | 1900 – 2000, pooled transitions | | | |
| **Dep.Variable** | annualized deforestation probability[c] | | | |
| **Explanatory Variables** | **Coefficients** (t statistics) | **Defaults &** | **Changes to compute marginal effects** | **Marginal Effects 1986**[d] ('86 default defor.rate = 0.048) |
| AGRETURN | 4.7 E-06 (0.26) | 1232 | 1685 | 0.00036 (< 1% of default) |
| DISTCITY | -0.020 (-16) | 71km | 38 km | 0.011 (23%) |
| DISTCITY*TIME | 3.0 E-04 (16) | (implied | by the above) | (joint with above) |
| GOODLZ | 0.21 (6.0) | 0 | 1 | 0.011 (23%) |
| BADLZ | -0.46 (-11) | 0 | 1 | -0.017 (35%) |
| BADSOIL | -0.13 (-1.9) | 0 | 1 | -0.0057 (12%) |
| TIME | 0.13 (21) | 86 | 10 | -0.026 (54%) |
| $TIME^2$ | -0.0012 (-25) | (implied | by the above) | (joint with above) |
| % CLEARED | 1.8 (7.4) | 37% | 28% | 0.019 (40%) |
| % $CLEARED^2$ | -0.71 (-2.6) | (implied | by the above) | (joint with above) |
| _CONS | -6.1 (-36) | | | |
| $R^2$ | 0.36 | | | |
| N | 4343 | | | |

[a] As discussed in section 3.1, can be run as weighted OLS, addressing heteroskedasticity from aggregating parcels.
[b] See section 3.3 on spatial error correction. Robust standard errors make BADSOIL and %$CLEARED^2$ insignificant.
[c] Area deforested during an interval divided by the area at risk of clearing, then rate is annualized (see section 4.1).
[d] Default before changes is 1986, medium lifezone, good soil, and otherwise forest-weighted average characteristics.

## Table 3 -- Cross-section Regression Results

| | Grouped Logit (uncorrected) | | | | |
|---|---|---|---|---|---|
| **Years** | 1900-1963 | 1963-1979 | 1979-1986 | 1986-1997 | 1997-2000 |
| **Dependent Variable** | annualized def. prob'y | annualized def. prob'y | annualized def. prob'y | annualized def. prob'y | annualized def. prob'y |
| **Explanatory Variables** | | | **Coefficients** (t statistics) | | |
| AgReturn | | 2.8 E-04 (2.4) | -1.3 E-04 (-4.3) | 2.7 E-04 (13) | 2.5 E-04 (5.6) |
| DistCity | -0.011 (-17) | 0.0034 (3.6) | 0.0018 (1.4) | -0.0043 (-4.5) | -0.0016 (-1.1) |
| PopDen | | 0.46 (2.7) | 1.0 (3.5) | -0.068 (-0.41) | -0.11 (-1.5) |
| PopDen$^2$ | | -0.030 (-2.1) | -0.068 (-2.8) | 0.0065 (0.36) | 0.0038 (1.8) |
| RoadsDen | | | | 18 (0.79) | 80 (3.3) |
| GoodLZ | 0.22 (4.2) | 0.080 (0.93) | 0.20 (2.2) | 0.15 (1.7) | 0.035 (0.23) |
| BadLZ | -0.68 (-8.5) | -0.41 (-4.4) | -0.60 (-5.8) | -0.60 (-8.1) | -0.65 (-4.6) |
| BadSoil | -0.055 (-0.52) | -0.37 (-2.2) | -0.17 (-0.97) | -0.91 (-5.3) | -0.48 (-1.7) |
| % Cleared | | 2.4 (5.0) | 2.0 (3.7) | 3.0 (7.4) | 0.41 (0.58) |
| % Cleared$^2$ | | -0.61 (-1.1) | -1.8 (-3.0) | -1.1 (-2.6) | 1.26 (1.8) |
| PrevClear | | | | 2.4 (2.6) | 14 (7.1) |
| _cons | -3.0 (-57) | -3.8 (29) | -2.9 (-16) | -5.1 (-33) | -5.9 (-25) |
| $R^2$ | 0.31 | 0.29 | 0.29 | 0.51 | 0.39 |
| N | 1128 | 799 | 638 | 649 | 782 |