REDD Roads?

spatial frontier dynamics & spatial variation in causal impacts *

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Abstract

We examine whether new roads' impacts on deforestation vary with prior development given the likelihood of partial adjustment and endogenous development on the frontier. Our focus upon differences from average impact is motivated by the potential for REDD, or reduced emissions from deforestation and degradation that may earn carbon payments, if a country chooses to shift its spatial configuration of transport networks as it develops. Causal evidence results from the use of lagged (1968-1975) investments in new roads and application of matching methods to Brazilian Amazon deforestation during 1976 to 1987. Prior development, proxied by 1968 road distance, affects 'baseline' (without new roads) deforestation rates, consistent with stories of other investments responding to early access and then driving development. Failure to control for this history yields spurious results. Using exact matching to control for these baselines, prior roads affects new road impacts. That effect is non-monotonic (which helps to explain prior debate about roads' impacts). Specifically, new-road impacts were relatively low if a prior road was already close, such that prior access and endogenous development compete with the new road for influence, but also if a prior road was far, since first-decade adjustment in pristine areas is limited. In between these bounds, road investments immediately raise deforestation significantly.

Keywords

deforestation, roads, infrastructure, matching, Brazil, Amazon

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Θ we share lead authorship

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1. Introduction

Investment in new roads that cross forested frontiers have generated controversy over decades in part due to the apparent tradeoffs between development and environment and natural resources. Investing countries typically wish to expand employment and income. However, the migration and the production that result are often seen to have both local and global negative externalities.

At the moment, the greenhouse-gas emissions associated with deforestation in the tropics (in the temperate zones many forested areas are net sinks) are receiving great attention for a number of reasons. First, it is thought that these emissions could be reduced at relativly low cost. Second, should global payments for reduced emissions from deforestation and degradation (or 'REDD') be a mechanism for reducing those emissions, it could in principle bring net benefits to the host countries and more countries appear to be considering positively the potential for such transfers. Third, while any such contracts will be based on estimated impacts on climate change, frontier forest conservation is also thought to bring benefits for species and for particular stakeholders.

How much new roads drive tropical deforestation has long been discussed. On the one hand, the general assumption still seems to be that such investments in transport will raise deforestation. On the other hand, even after a second wave of empirical work within countries (the first wave involved cross-country regressions (more below)), the impacts of roads relative to other factors have not always clearly corresponded to the magnitudes that would appear to be assumed. There are even settings involving trade or analogous shifts in which new roads can lower deforestation in one location by shifting production to another. This can confound spatially bounded analyses.

One reason for the failure of an empirical literature to converge upon *the* average impact of roads could be that many or most locations do not display the average impact but instead impacts vary. Chomitz's Loggerheads volume, for instance, emphasizes different settings as a core framework.

Bringing this to empirics, with co-authors Weinhold and Pfaff each separately extends in careful empirical work for the Brazilian Amazon an early observation concerning Mexico by Nelson and Hellerstein, who found new roads to increase deforestation less in the places near to prior roads. In the recent Amazon work, prior deforestation is used as the indicator of past development that could affect the impact of new roads. That work is discussed further below but in summary it all suggests that impacts vary importantly with prior development and it supports the Mexico claim.

However, in all of that work data are real constraints, albeit to varying degrees. First, they may not permit the empirics to directly address endogeneity. In this paper, the data permit matching. Second, they tell only part of the story concerning varying impacts, in essence distinguishing only two categories in which impacts differ. Here, we distinguish three and demonstrate why three categories and prior data constraints can explain previous lack of clarity in the literature.

Specifically, this paper brings matching methods to spatially and temporally rich data on roads and deforestation in the Brazilian Amazon. The time period over which the evolution of roads can be tracked and linked to forests is rare, and that much moreso with the precision of pixels. This combination allows for the use of lagged roads investments and application of matching. These are significant steps forward in addressing endogeneity in the analysis of infrastructure and these methodological advances are applied to categories expected to show varied impacts.

We find that the impacts of new road investments upon local deforestation are very low where a road was already relatively close by in the past. In addition, we find that the impacts on clearing within the first 5-10 years after a new roads investment is also quite low in pristine areas far from prior roads. It is between these endpoints that new road investments significantly increase rates of deforestation in the next few years. This is where the changes in transport costs are relevant.

The rest of the paper is as follows. Section 2 summarizes prior empirical analysis of road impact as well as the typical von Thunen perspective on road impacts as well as some dynamic models which appear relevant. Section 3 presents the data and the matching methods that we will apply. Section 4 then presents our results and Section 5 summarizes then discusses some implications.

2. Empirical & Theoretical Background

2.1 Relevant Empirics

While many have previously analyzed tropical deforestation, including in the Brazilian Amazon, little empirical work of the sort presented in this paper has been done. A 'first wave' of empirics correlated factors of interest with national measures of deforestation.[1] The dominant results are about population density. In cross-country research, it is the most commonly collected variable.

A 'second wave' of within-country empirics controls for more variables, with more observations, in testing a given factor.[2] For the Brazilian Amazon, Almeida 1992 focuses on migration and agricultural colonization. Reis and Margulis 1991 and Reis and Guzman 1992 econometrically find population density, roads and crop area to be important. Pfaff 1999 extended this research, finding population's spatial distribution (and thus urbanization) to be critical and confirming that roads, not only within the county of interest but also in neighboring counties, are key factors.[3]

[1] Lugo et al. 1981, Allen & Barnes 1985, Palo et al.1987, Rudel 1989, Cropper & Griffiths 1994 and Deacon 1994.
[2] Panoyotou and Sungsuwan 1989 find Thai deforestation driven by population, wood price, income, and Bangkok. Southgate et al. 1991, for Ecuador's Amazon region, explain population with "prospect of agricultural rents" then explain deforestation with population and other factors. Harrison 1991, for Costa Rica, suggests differing effects of population by region. Kummer 1991 is one of few empirical studies to find only a small role for population growth.
[3] At about the same time, and using similar approaches, Chomitz & Gray 1996 analyzed deforestation in Belize and roads, Cropper and Griffiths 1996 examined Thailand, while Nelson and Hellerstein 1997 studied central Mexico and Geoghegan, Wainger and Bockstael 1997 considered the Patuxent Watershed in Baltimore –Washington area.

However, road impacts have not always been so large in such empirics, as noted in Chomitz and Thomas 2003's Amazon analysis. They focused on increasing the variables controlled for and on increasing observations by moving to the level of census tracts from counties. A general note is that biophysical factors such as rainfall were powerful controls, as is more apparent with better data. A road-specific note is that estimated road impacts can be influenced by the level of prior clearing, suggesting attention to prior development in such estimation. As noted above, Nelson and Hellerstein for central Mexico also find that existence of prior roads influences road impact.

Thus to address these questions it helps to go beyond Chomitz and Thomas 2003's cross-section to track the sequence of road investments over time and relate it to the sequence of deforestation. Pfaff et al. 2007, 2009a and 2009b, using census-tract data like Chomitz and Thomas over time (giving up on the use of census data not available at this level earlier), track this across decades.

These studies examine the tracts receiving road investments (Pfaff et al. 2009a,b) and, to look for spatial interactions, also at their neighbors not receiving any road investments (Pfaff et al. 2007). They find that deforestation rises not only in census tracts receiving roads but also in nearby tracts in the same county without roads investments. To the extent permitted by the data, this work takes care in its inference, for instance using lagged roads investments and dummies for counties to control for much unobserved variation over space. This is possible because data for census tracts has over twenty times as many observations as counties (roughly 6000 vs. 300).

That follows on Weinhold and Reis 2008 (extending Andersen et al. 2002) which also takes care in regressing deforestation on prior changes in roads. These works advanced the idea implicit in Nelson and Hellerstein 1997 by analyzing how road impact differs as a function of context, in particular prior clearing. Estimating an interaction between prior clearing and road investment suggests that prior clearing lowers road impact. Extrapolated, new roads could lower clearing.[4]

Yet the data constraint is critical here, as these works used county data, and in particular the polygons consistent with the county structure over time for which about 250 observations are available. This made it impossible to split the sample or to do anything other than a single interaction term which assumed a monotononic impact of prior development on road impact. While the basic idea of conditional impacts is critical, and supported by their findings of an interaction significantly different from zero, the detailed coefficients require re-examination.[5]

[4] Why this lowering of deforestation by new roads would occur is not clear in these works. One idea is that there could be spatial intensification in which all development in the region is drawn to the location of the roads, e.g. by migration, and thus clearing in other locations could decrease more than clearing rises where the new roads arise. However, Pfaff et al. 2007's finding with more precise data is that the census tracts in a county receving a new road increase in deforestation due to the investments in other census tracts. There could be stories concerning trade, such as in New England (see Pfaff 2000 and Pfaff and Walker 2009 for discussion of railroad connections to the Midwest and New England reforestation). However, Pfaff and Walker 2009 note this seems not to apply to the Amazon now.
[5] Another important issue is what the 'average' result is from such a finding. Weighting of observations matters, as using unweighted counties puts a very heavy weight near cities since numerous small counties are found there (this

Pfaff et al. 2009a and 2009b reevaluated these results by examining the much more numerous census tract observations in groups distinguished by level of prior clearing (for instance in 2009a the categories are 0% , 1-50%, 51-75%, >75%). The dominant first two categories show strong increases in deforestation from roads investments. And while the last category is insignificant (with fewer observations), for 50%-75% prior clearing the increase in deforestation resulting from new roads investments is higher, not lower, than it is for the more pristine areas. Thus at the level of census tract data, no deforestation lowering is found and prior development has a non-monotonic influence on the impact of roads, which is lower for very high and low prior clearing. Section 2.2 just below considers reasons why we might expect these empirical results to obtain.

However, despite appropriate care taken in each case above, conditional upon the data's limits, still we are concerned about causal inference given the potential for endogeneity of new roads. Moving to pixel-level data here helps in this by providing sufficient detail to permit matching. Pixel information on distance to the nearest road has other advantages as well, as census tracts are much smaller than counties but not tiny and a new road that crosses a census tract will not have the same impact on all parts of the census tract.[6] Here, though, we focus on endogeneity.

2.2 Relevant Theory

2.2.1 Transport Cost (von Thunen[7])

A dynamic theoretical model that includes transport cost can address issues of irreversibility and follows the land-use-and-deforestation literatures (see, e.g., Ehui and Hertel 1989, Stavins and Jaffe 1990, Kerr and Pfaff 2009). 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*:

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

where:

S_{jt} = expected return to forest uses of the land

appears to be a standard pattern of develoment; in the Amazon, the rural counties are simply enormous while those near the cities are orders of magnitude smaller). Weinhold gives 'average' claims that would not apply to the region on average in the sense that if you located a road by blindly tossing a dart one would expect those averages to hold. The average of counties (or census tracts for that matter) which applies there would spatially weight by their areas.

[6] And other supportive evidence concerning varied road impacts is now being generated using pixel data though not using matching to this point (while still using lagged road investments which are a considerable improvement upon contemporaneous investments). Delgado et al. 2009 (following Delgado's master's thesis) supports the idea that the impact of a new road in an already highly developed location may be quite low. Amor and Pfaff 2009 (following Amor's doctoral thesis) supports the idea that a new roads in a pristine area may have a low first-decade impact.

[7] As in Pfaff 1995, Chomitz and Gray 1996 and Nelson and Hellerstein 1997, this could also be done in a more static fashion but the critical lack remains, ie that after a shock land use adjusts then forest stays constant at that new level.

R_{jt} = expected return to non-forest land uses
C_T = cost of clearing net of obtainable timber value and including lost option value[8]
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:

$$\int_T^{\infty} (R_{jt} - S_{jt})\, e^{-rt}\, dt \; - C_T > 0$$

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:

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

Both conditions must hold for clearing to be preferred. However, if the second-order condition below holds[9], then either of the necessary conditions is also sufficient for clearing to be chosen.

$$\frac{dR_{jt}}{dt} - \frac{dS_{jt}}{dt} + \frac{d^2}{dt^2} C_t > 0$$

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.

2.2.2 Partial Adjustment

The model above assume instantaneous full adjustment on the frontier, i.e. that upon a new road making more locations worth deforesting for agricultural production, for instance, they will all be deforested in the next observed time period. However, adjustment takes time (as noted in various investment literatures) in particular on the frontier. When a road enters a previously less developed or pristine area, the labor and capital required to carry out all of the land-cover change

[8] 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.
[9] For land-use change in a developing country, population and economic growth along with improved infrastructure may lead this 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.

that may suddenly be economically worthwhile are not present. In particular, a sudden rise in the demand for labor, for instance, will change the marginal cost of hiring additional labor and then what seemed to be worth clearing instantaneously is not because of limited availability of inputs.

Two implications of partial adjustment on the frontier are worth emphasizing. First, given a past investment there will be ongoing clearing of forest in current periods even without more recent transport investments. As inputs become available at the originally envisioned marginal costs of inputs, for instance, the newly profitable locations will be cleared over time. Second, we would expect that the first-decade impact of a new road would be lower where inputs on scarce, e.g. in currently pristine forest areas, relative to where inputs are plentiful. This could even be the case if a quite considerable drop in transport costs has occurred at a location might remain remote.

2.2.3 Endogenous Development

The model above ignored the possiblity that future actions will be influenced by current actions. For instance, upon a road entering an area and connecting it to more developed areas outside of the frontier, other investments that the econometrician does not observe are are likely to occur. Settlers will agitate for additional roads investments, for schools, for agricultural subsidies, and more generally for any investment which would improve their quality of life. These are likely to alter the decisions made by other migrants and producers, yielding high spatial path dependence.

The implications of such endogenous development are in a way similar to but are distinct from the possibility of partial adjustment. The similarity is that given past investment such as an new road giving initial access to an area, without other observed new road investments deforestation can be expected to continue as other investments or changes shift the threshold for deforestation.

Endogenous development, though, has an additional implication for the expected impacts from new road investments. It would support the partial adjustment implication that on the far frontier where few observed road investments have occurred road impacts will be low, in this case due to the lack of other, unobserved complementary investments. Further, though, it may suggest that a new road investment in highly developed location could have low impact. The reason is that if other investments come to dominate the local spatial details of relative net benefits of agriculture then the new road may shift thresholds only inframarginally. Together, the two additional views on the frontier suggest that the transport cost shifts may matter most in an in-between level of prior development, where inputs are available but still other factors do not dominate land use.

3. Data & Methods

3.1 Data

We use highly detailed spatially explicit satellite data on forest overlaid on maps of roads and other factors that influence deforestation. Here, we describe data sources and characteristics.

Regardless of details, however, every variable below is sampled for 100,000 pixels across the Amazon. Considering that the Brazilian Legal Amazon encompases 5m km2, on average our data set has a pixel every 50 km2, i.e. a reasonable coverage without points on top of each other.

3.1.1 Deforestation

Deforestation maps were produced in 1997 by IBGE (Instituto Brasileiro de Geografia e Estatistica) for their "Diagnostico Ambiental da Amazonia Legal" data product. The pre-1976 clearing is from the RADAM Project vegetation maps, with classes of land cover. The clearing in 1987 is from IBAMA/INPE maps based upon Landsat imagery, as is the 1991 clearing. Our dependent variable for two of our three periods is the difference over time in these measures. For each pixel, we measure deforestation is the pixel was forested in 1976 and cleared in 1987, for our first period, and for our second period if it was forested in 1987 and deforested in 1991. Yet Figure 9a shows little deforestation in our second period, so we do not analyze it like the first.

Our third period is 2000-2004. We have a time gap (i.e., we skip 1991-2000) because we did not want to compare point-in-time forest measures from different sensors to infer pixel-level deforestation. For this period, both years of data are from the PRODES (available from INPE). We compute and discuss deforestation rates for this period but do not yet analyze them in the fashion applied to the 1976-1987 deforestation as we are waiting to acquire a 2000 road map. Figure 9b below shows important similarities between 200-2004 and 1976-1987 clearing rates.

3.1.2 Roads

We tracked the evolution of roads over time on maps, providing high spatial specificity. Digital road maps were developed in the Department of Geography at Michigan State University from paper maps by DNER (Departamento Nacional de Estradas de Rodagem), an agency within the Transport Ministry in Brazil. The digital maps that we employ for almost all of the analyses in this paper show the distribution of roads, paved and unpaved, for 1968 and 1975 (see Figure 1).

For these years, for each pixel in our sample and each year in which we have a road map, we measure the distance to the pixel from the nearest paved road and, separately, the distance to the nearest unpaved road. Then we calculated the change, if any, in these nearest-road distances during each period (1968-1975, 1975-1985, 1985-1993). In the measure we are currently using, we assign a zero if the nearest-road distance does not change. We call this "no investment" for that pixel though it really means "no relevant investment". Alternative measures could reflect the effect of road density, e.g. if another road slightly further away comes in density could matter.

We use these road investments (drops in nearest-road distances) as ‘lagged’ measures. Thus, we will use investment during 1968-1975 to explain 1976-1987 deforestation behaviors and, along the same lines, we could use road investment during 1975-1985 to explain 1987-1991 clearing (which as noted is seen in Figure 9a to be very small so we do not analyze it) and we could use investment during 1985-1993 to explain 2000-2004 clearing (but we are waiting for 2000 roads).

It is immediately clear that relationships between road-distance data timing and deforestation timing are not the same in each period. In the third time period, the roads had been in for seven years before our deforestation is measured. In the first and second time periods, the investment was just prior to the measured deforestation but the length of the deforestation period differs and thus the timings are not exactly the same. This is why we are waiting to acquire the 2000 roads.

3.1.3 Other Drivers

We integrated maps of ecological conditions and distances to cities to control for their influences in testing our hypotheses about roads. The ecological variables that we can employ here are an index of soil quality, continuous rainfall data[10], and binary variables indicating slope categories (for instance, whether on steeply sloped land or whether on rolling hillly land etc.).

Distance to the nearest city (in km) is computed using both a set of 19 large cities (density over 100 people/km^2) and a set of 270 medium and large cities (density over 11 people/km^2). These distance variables are used to represent transport costs, to indicate a tract’s proximity to a very large city, and to eliminate the census tracts closest to cities from the analysis.

3.2 Matching Methods

3.2.1 Defining ‘Apples to Apples’

The basic idea of matching to address endogeneity is that the observable differences between locations are central to the non-random allocation of the treatment, in this case the investments in new roads. If they are also central to deforestation, which is the case here, then the observable differences between the treated and untreated locations can bias the coefficient on the treatment. Comparing treated and untreated locations that are as similar as possible in terms of observable factors, i.e. ‘apples to apples’ comparisons, will then help to reduce the bias in treatment effect.

While those facilitating the application of such techniques agree on this point (Rubin 1980, Rosenbaum and Rubin 1983 and many others), similarity has been defined in different ways. To examine robustness, and to highlight the importance of this definition, we apply two rather different matching estimators. The first is a nearest-neighbors propensity score matching

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[10] For discussion of the soil and rainfall data, see Laurance, W.F. *et al.* (2002). *J. Biogeography* **29**, 737.

estimator (Rosenbaum and Rubin 1983, Hill et al. 2003*)*, using a fixed number of matched control observations for each treated observation and varying that number from 1 to 50.[11] Propensity score matching estimators define similarity based on estimated probabilities of being treated which are generated by a first-stage regression for whether observations are (not) treated.

The second matching approach we will apply is a nearest-neighbors covariate matching estimator (Abadie and Imbens 2006a) using an inverse weighting matrix to account for the difference in the scale of the covariantes. Here again, we employ a fixed number of best matches per treated observation; in Table 2, we apply matching approaches for six matches per treated observation. Covariate matching estimators define similarity without a first-stage regression but rather using the simple distances, in the space of the matching covariates, between the treated and matched.

The computation of standard errors is another difference in how those advancing such techniques have applied them. Abadie and Imbens 2006b show that the common practice of bootstrapping standard errors is invalid with non-smooth, nearest-neighbor estimators such as the propensity score matching estimator with a fixed number of matches that we have chosen to present here (contrasted with kernel versions that assign smoothly declining weights to progressively less-well-matched untreated observations). For propensity score matching, then, we do not bootstrap but follow Hill et al. 2003 in calculating weighted standard errors. For lack of certainty, though, we lean more on the covariate matching standard errors which follow Abadie and Imbens 2006a.

3.2.2 Match Quality

For all of the helpful analytics and important choices described above, the matching approaches control solely for selection on observable factors, unlike using instruments for where policies go. Our experience has been that upon recognizing this, many analysts ask why or if OLS is inferior. The basic reasoning is useful *per se* and it motivates further adjustments in matched estimations.

In short, the attempt to match treated with untreated observations explicitly examines whether in fact there exist untreated observations similar (by whatever criterion) to the treated observations. To the extent that the observed characteristics are not similar in these two groups, OLS uses the information at hand to control for differences but the burden on the specification is considerable:

> "Unless the regression equation holds in the region in which observations are lacking, covariance will not remove all the bias, and in practice may remove only a small part or it. Secondly, even if the regression is valid in the no man's land, the standard errors of the adjusted means become large, because the standard error formula in a covariance analysis takes account of the fact that extrapolation is being employed. Consequently the adjusted differences may

[11] A natural alternative to a fixed number of matched untreated points per treated observation is to fix the window for how good a match needs to be and let the number of matches per point be endogenous to the quality of matches.

become insignificant merely because the adjusted comparisons are of low precision. When the groups differ widely in x, these differences imply that the interpretation of an adjusted analysis is speculative rather than soundly based." (Cochran, in Rubin 1984).

This shortfall of OLS where the untreated are not similar to the treated, however, also relates to matching procedures when match quality is not good. That can motivate using a subset of the treated observations, and their matches, in particular dropping treated points with poor matches.

Crump et al. 2006 addresses the issue of a lack of covariate overlap, noting that many common estimators become sensitive to the choice of specification (much as Cochran had noted for OLS, and following also related prior work including Heckman, Ichimura and Todd 1997 and 1998). Crump et al. characterize optimal sub-samples for which treatment effects can be estimated most precisely, which under some conditions can be characterized by a rule based on propensity score.

4. Results

4.1 The Importance of Prior Development

Above the idea of endogenous development was put forward as potentially important for the analysis of impacts of roads invesments. Here we start by looking at deforestation rates without current roads invesments, recalling that by 'current' we mean the immediate lagged time period since we are trying (as have other recent analyses) to avoid problems with road endogeneity.

We consider in Figure 2, for instance, the deforestation rates for locations for which the distance to the nearest paved road did not change during the lagged period. This figure is for investments in roads during 1968 to 1975, the period we use to explain deforestation during 1976 to 1987. Along the diagonal are the locations with the same distance to nearest road in 1968 and 1975. Moving outwards on the diagonal those are locations which were farther from the nearest road.

Thinking in terms of perfect adjustment, we might expect no deforestation at all in these places. The investments were made in 1968 and during the years until 1975 all worthwhile clearing could have already occurred. If we observe all investments, e.g. if roads are the main shift and other factors like slope and rainfall remain constant over time, then forest level may stay fixed.

The perspective of endogenous development, however, would suggest something else entirely. In that view, observed prior road investments would be expected to have led to other investment, potentially unobserved, following upon the prior road investment. In that case, we would expect more of those deforestation-inducing investments where prior roads were relatively close, i.e. to the left within Figure 2. The measured deforestation rates support that this could be going on, as the deforestation rates without roads within 20km of roads is ten times that quite far from roads.

4.2 Ignoring Prior Development Confounds Results

The importance of controlling for implications of the past is demonstrated in Figures 3a and 3b. We consider what one might try based upon a belief in perfect adjustment to roads investments. Each 'row' contains the estimated impacts of a roads investment, defined as the 1975 distance being at least 5km less than the 1968 distance to nearest road, upon deforestation conditional upon the 1975 distance to nearest roads being in a given range. That range would be the critical one should it be the case that land use adjusts quickly to road shocks. In fact, if land use were to adjust perfectly we would expect that there would be no effect of investment all across that row because whether an investment occurred during 1968-1975 or not the forest should be the same.

We find several negative and significant impact estimates here, suggesting that having new roads lowers deforestation. This arises even though we are matching for many characteristics of pixels; plus, by working within a row, exclusively we are 'exact matching' on the 1975 distance range.

The explanation for these negatives is that we have ignored the past, the 1968 distance which as noted may represent not only past road investment but also followup complementary investment. In our matching, we did not match on the 1968 road distance, i.e. we intentionally ignored the past to implement the model one might typically apply to such data. That drives the negatives.

There are at least two ways to see this. In Figure 3a, the locations with investments are compared to section of the diagonal, which is the locations without investments, solely within that row. The comparison diagonal group is, then, by construction, for a lower 1968 distance than the locations in the rest of the row with the same 1975 distance but higher 1968 distance given the investment. This means that the investments are in places that per endogenous development are less likely to undergo deforestation (as in Figure 2 and 4.1 above), causing roads to appear to lower clearing.

Figure 3b changes the comparison group to the entire diagonal, i.e. full range of 1968 distances. In principle, this could address the spurious negative results (which need not arise and need not be significant; the same figure does not produce as many significant negatives in other periods). However, if the locations with road investments have larger 1968 distances on average than do the places without, again the higher investments are taking place in places with lower clearing. While the spurious negatives are smaller in magnitude, in this case they still signal that issue.

4.3 'Exact' Matching On Prior Development

Given the results in Figures 2 and 3, the analyses for Figures 4 and 5 aim to control for the past. Even a cursory skim of these figures suggests that this step alone eliminates spurious results. We believe that this should strip out the influence of endogenous development upon the baseline, i.e. on deforestation without investments, so that variation in impacts of roads investments is tested.

4.3.1 No Controls for the Other Factors

Each column of Figure 4a contains an estimate of the deforestation impact of a roads investment, defined as the 1975 distance being at least 5km less than the 1968 distance to nearest road, given that the 1968 distance to nearest roads is in a given range. That range is critical if, as in the view of endogenous development, past roads investments lead to relevant unobserved investments. Working within the columns is simply a form of 'exact' matching upon the prior investments.

Here we find positive impacts that are significant and no negative impacts. At greater distances from 1968 roads, i.e. lower prior development, impacts are lower. Without any controls for the other measured variables which may impact deforestation rates, with high prior development, i.e. to the left of Figure 4a, new roads' clearing impacts are suggested to be positive and significant.

4.3.2 Regression including the Other Factors

Figure 4b is analogous but now the '1968 distance exact matching' results are generated by regressions comparing the column diagonals, i.e. points with no roads investments, with the cells below the diagonal in the column using a regression that controls for our other measured drivers of deforestation. The positive impacts in the middle levels of prior development have remained. Also the lower impacts with lower prior development remain, although again as in Figure 4a the 300-400km prior distance has perhaps an unexpected positive effect for effects of new roads.

Of particular note, relative to Figure 4a, is the fall in estimated impact on deforestation for new roads' investments in locations where prior development was high, i.e. if distance to the nearest road was quite low already in 1968. Thus, these locations differ, for instance in distance to city, and controlling for differences in where roads go and where they don't affects estimated impact. Indeed for 20-40km from 1968 roads, the controls are meaningful enough to reverse the sign. It appears, then, that while regression controls for other drivers can matter and may indeed help to strip out the effects of some differences in characteristics, comparing apples to apples could help to reduce the burden on this regression to perfectly control for these differences in other factors.

4.3.3 Matching applied to the Other Factors

Figure 5 presents 'exact' matching on the 1968 nearest-road-distance range, which we are doing simply by splitting the sample according to the ranges, as well as standard propensity score (n=4) matching to get apples-to-apples comparisons based on other characteristics within each column.

Now the results look more straightforward. Far enough away from 1968 roads, i.e. when looking at locations with low prior development, even if there is investment in new roads there is not a significant impact on deforestation. That the first-decade impact on the forest in a pristine area is relatively low is consistent with partial adjustment +/or investments simply being inframarginal.

We might ask why matching reduces the impact for these lower prior development locations. Figures 6 and 7a suggest an answer. Figure 6 shows the deforestation rates predicted by levels of factors other than the roads distances, as a summary of differences across cells in other drivers of deforestation. Figure 7a looks at one difference underpinning such predictions, i.e. mean values of distances to the nearest medium-sized city, to see whether they vary for treated versus control Looking farther to the right of Figure 6, we see that the diagonal has predicted clearing different from a number of the cells below. This is also true for Figure 7a, i.e. factor differences do exist (noting lower distances in Figure 7a are consistent with the higher predicted clearing in Figure 6, while Figure 7b conveys that, not surprisingly, not all variables differ the same way across cells).

Close enough to 1968 roads, i.e. when looking at locations with high prior development, again an investment in a new road does not significantly raise deforestation. That result is quite consistent with endogenous development predictions that the initial prior access could have generate a set of development dynamics distinct from transport costs which could dominate relatively small additional changes in transport cost. Also, the size of the transport cost shift just may be smaller, i.e. the marginal impact of the investment is smaller where there is already road infrastructure.

In the middle columns, though, new road investments clearly raise deforestation significantly, meaning both significant statistically and of reasonable magnitude as an over 5% increase across one decade is important. Further, there are no negative results at all even though there are zeros.

4.4 <u>Resurrecting von Thunen</u>

As results in Figures 4 and 5 (and section 4.3) demonstrate controls for critical prior investments, a next natural question is whether, given these controls, greater investments cause more clearing. We know not to focus where prior distances to roads are either large or small, for reasons given. However, within the 80-300km 1968-distance range highlighted in Figure 5, we can look harder.

Figure 8 breaks down the columns, in the sense that the cells are compared to column diagonals, i.e. to the locations that do not receive new road investments and have the same 1968 distances. Thus, again controlling for the past, we compare larger and smaller reductions in transport costs and while some differences between the two post-matching estimates suggest imperfect balance, in each of the three columns in the 80-300km range the impacts of larger investments are larger.

5. Discussion

We find that the impacts of new road investments upon local deforestation are very low where a road was already relatively close by in the past. In addition, we find that the impacts on clearing within the first 5-10 years after a new roads investment is also quite low in pristine areas far from prior roads. It is between these endpoints that new road investments significantly increase rates of deforestation in the next few years. This is where the changes in transport costs are relevant.

Upon reflection this is not surprising, given partial adjustment and endogenous development on the frontier (discussed at length in the text). Yet the non-monotonicity has confounded summary. Depending on where new roads are sited, an average could easily find low impact of new roads, even when environmental advocates are right to insist that a given new road will devastate forest. More generally, with varied impacts the data demands are high to identify the impact by category and given the lower-higher-lower impacts as prior development increases any number of claims, including that roads lower deforestation, can erroneously arise using averages or extrapolations.

Non-monotonicity also leads one to consider both temporal dynamics and spatial nonlinearities. From first-decade impacts on deforestation one might claim that a road through pristine areas is less damaging than where prior development was neither high nor low. However, an endogenous development view again is relevant. When follow up investments drive ongoing clearing, e.g. if new roads follow old, long-run pristine impacts are far greater than the immediate forest impact.

Spatially, for instance thinking of species habitat as a 'co-benefit' of actions paid for by carbon, as demonstrated in Amor 2009 the habitat fragmentation by roads in pristine areas is enormous. Thus even in the short run, e.g. the first decade, roads through pristine areas look less positive.

The policy implication of the correct results is important. It seems clear from these results that a nation could choose to achieve its development goals with different shapes of transport networks and that which shape is chosen will affect deforestation. These choices could earn global carbon payments. Further, the same logic could in principle apply to pipelines as well (Finer et al. 2008).

The idea that variations in impact or deviations from the average impact are important for policy choice is generally relevant for forest. If protected area impacts on deforestation vary depending upon the context, then policy makers locating a new protected area can target based upon impact (see Andam et al. 2008 and Pfaff et al. 2009c). The same holds true for any type of payments for ecosystem services (see, e.g., Pfaff et al. 2009d and Robalino et al. 2009). Targeting matters.

For both development and conservation policies, further research in other locations would help to establish how general are these results. While the Brazilian Amazon is one critical specific area, and also one for which excellent data exist, to the extent possible replication would be of value.

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Figure 1

number of observations by cell (1968 x 1975 distances to nearest road)

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

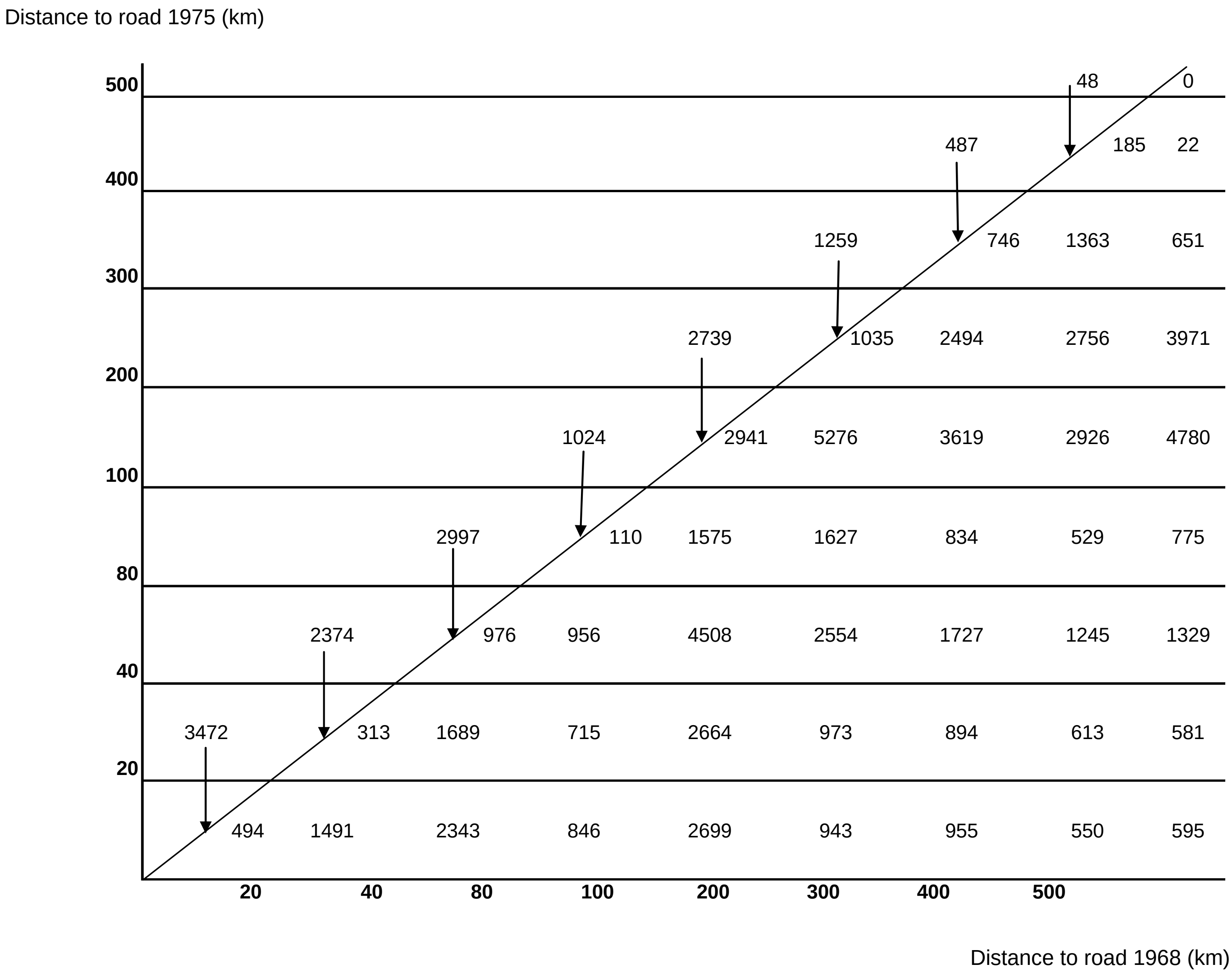
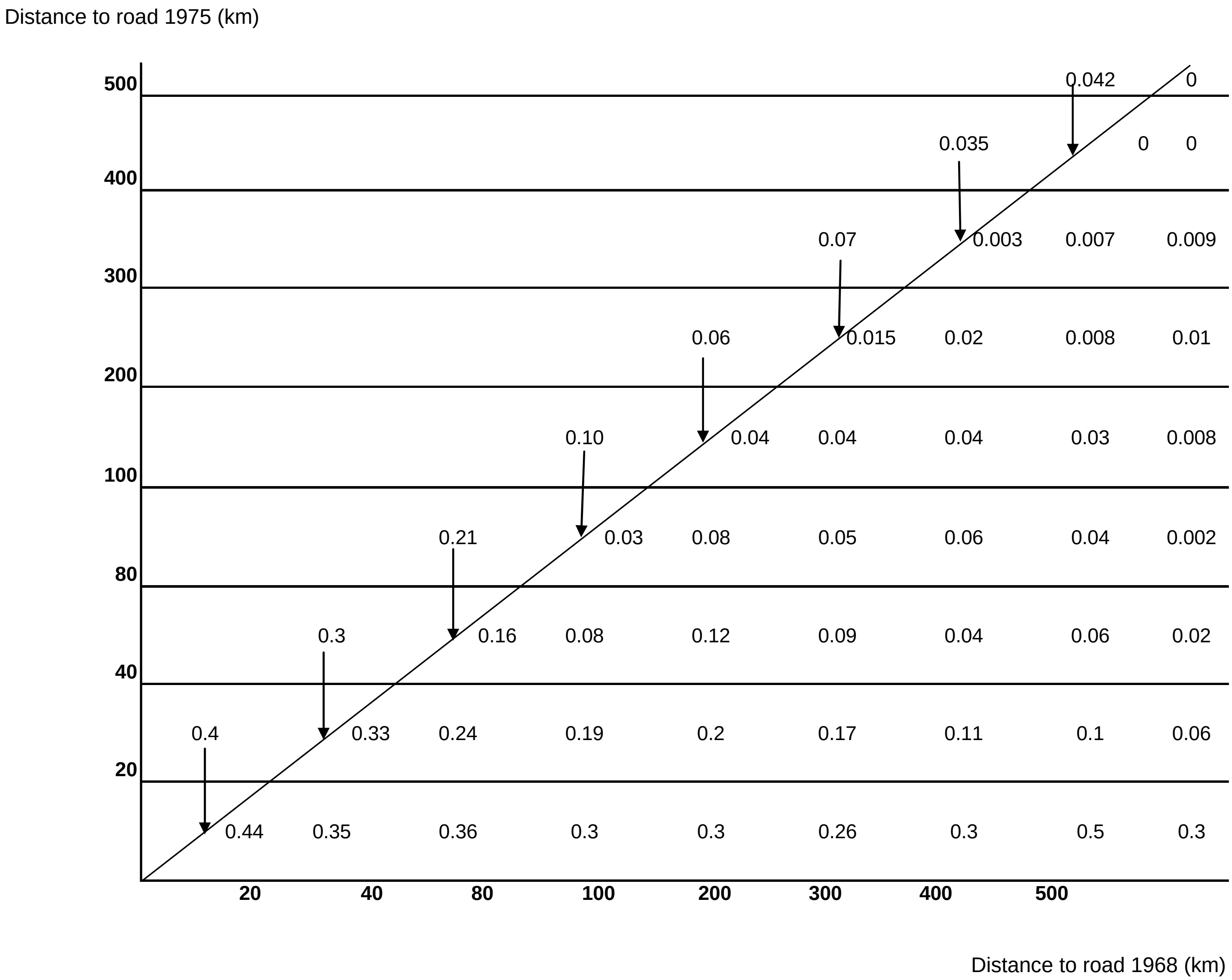


Figure 2

1976-1987 deforestation rates (fraction cleared) by 68x75 distance cell

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}



<u>Figure 3a</u>

matching impact estimates, exact match on 1975 distance,

explaining 1976-1987deforestation rates (fraction cleared)

[compare row cells to points on diagonal in the same row]

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

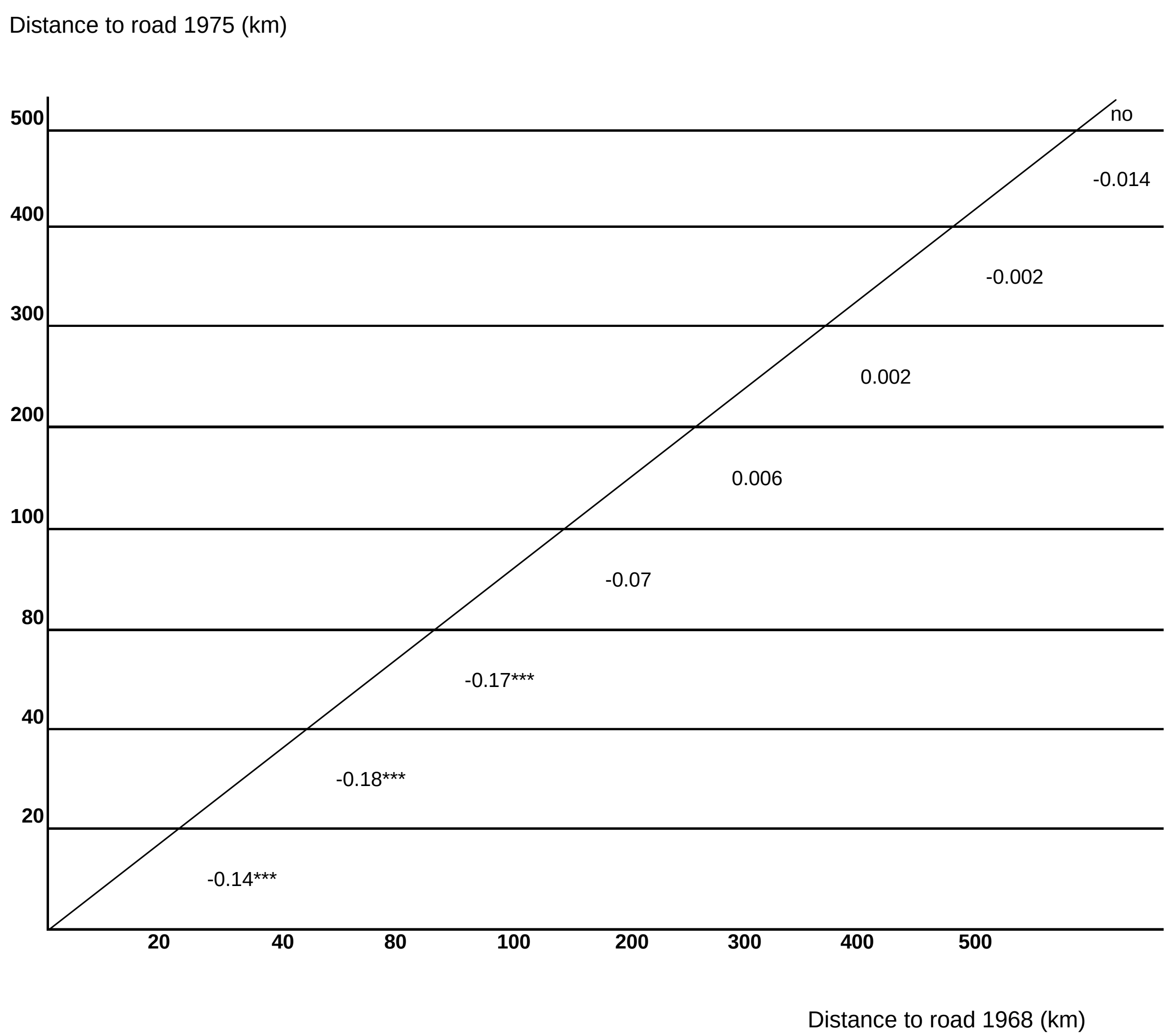


Figure 3b

matching impact estimates, exact match on 1975 distance,

explaining 1976-1987deforestation rates (fraction cleared)

[now compare row cells to points along the entire diagonal]

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

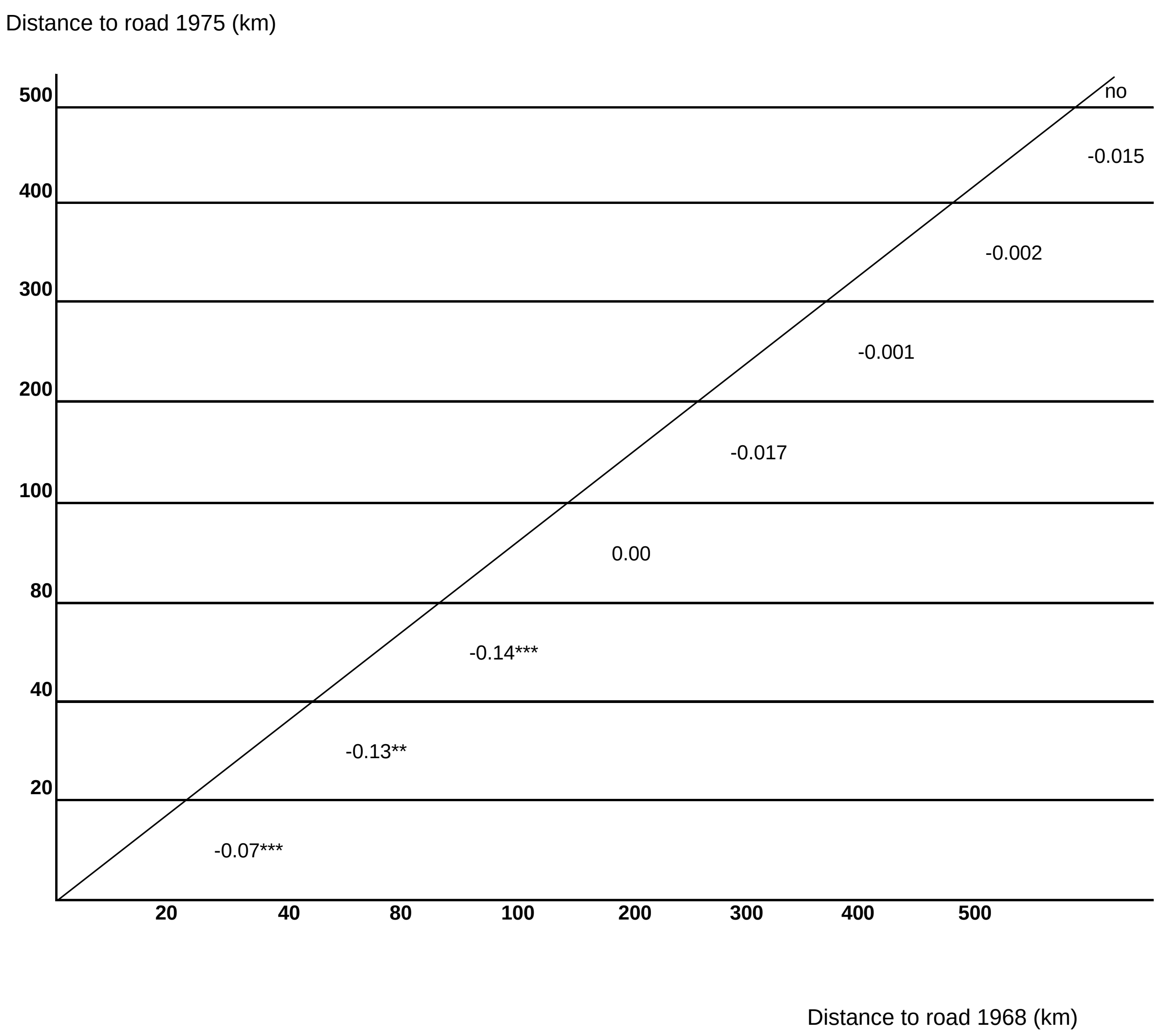


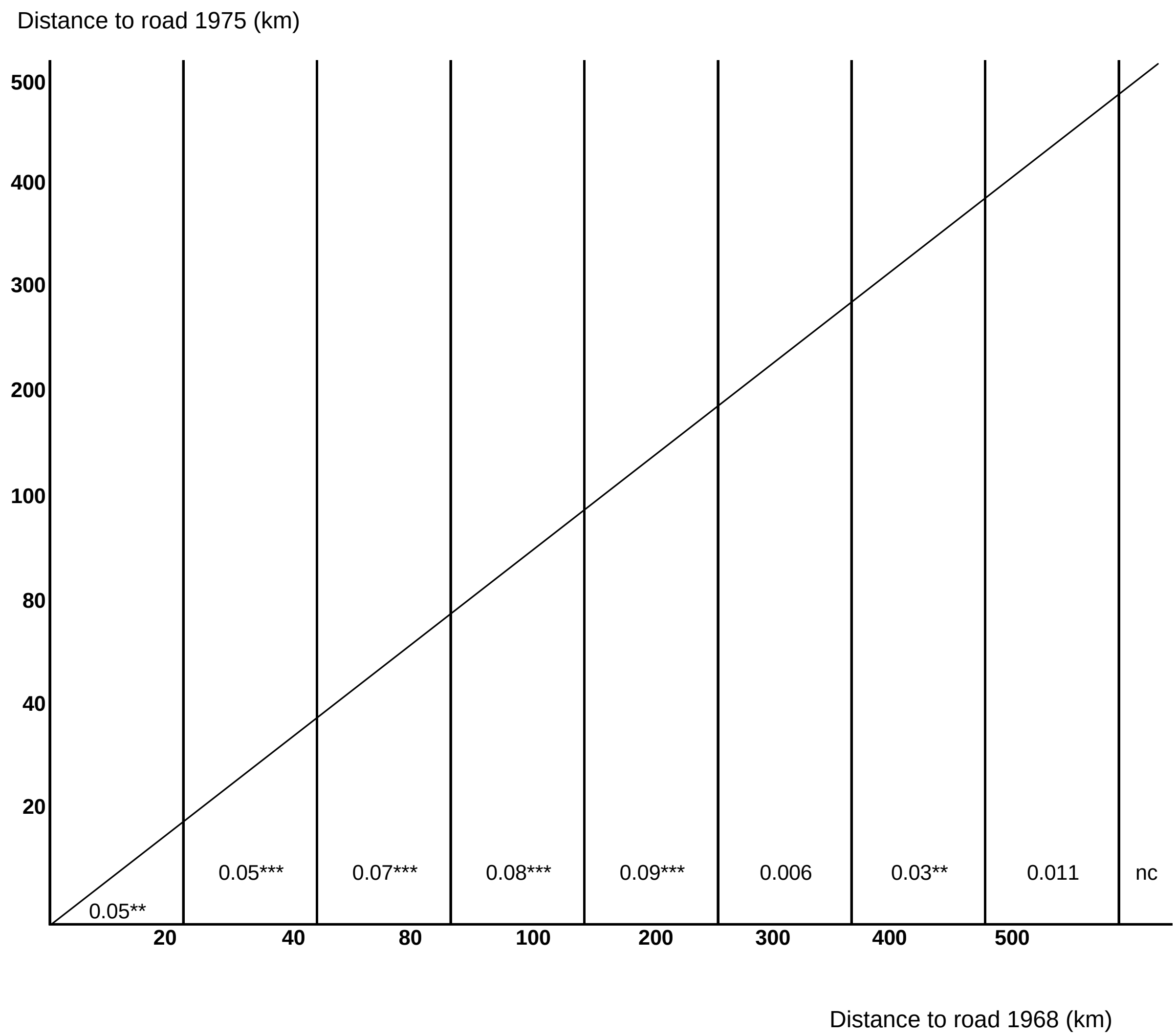
Figure 4a

no-controls impact estimates, exact match on 1968 distance,

explaining 1976-1987deforestation rates (fraction cleared)

[compare column cells to diagonal points in same column]

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}



<u>Figure 4b</u>

regression impact estimates, exact match on 1968 distance,

explaining 1976-1987deforestation rates (fraction cleared)

[compare column cells to diagonal points in same column]

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

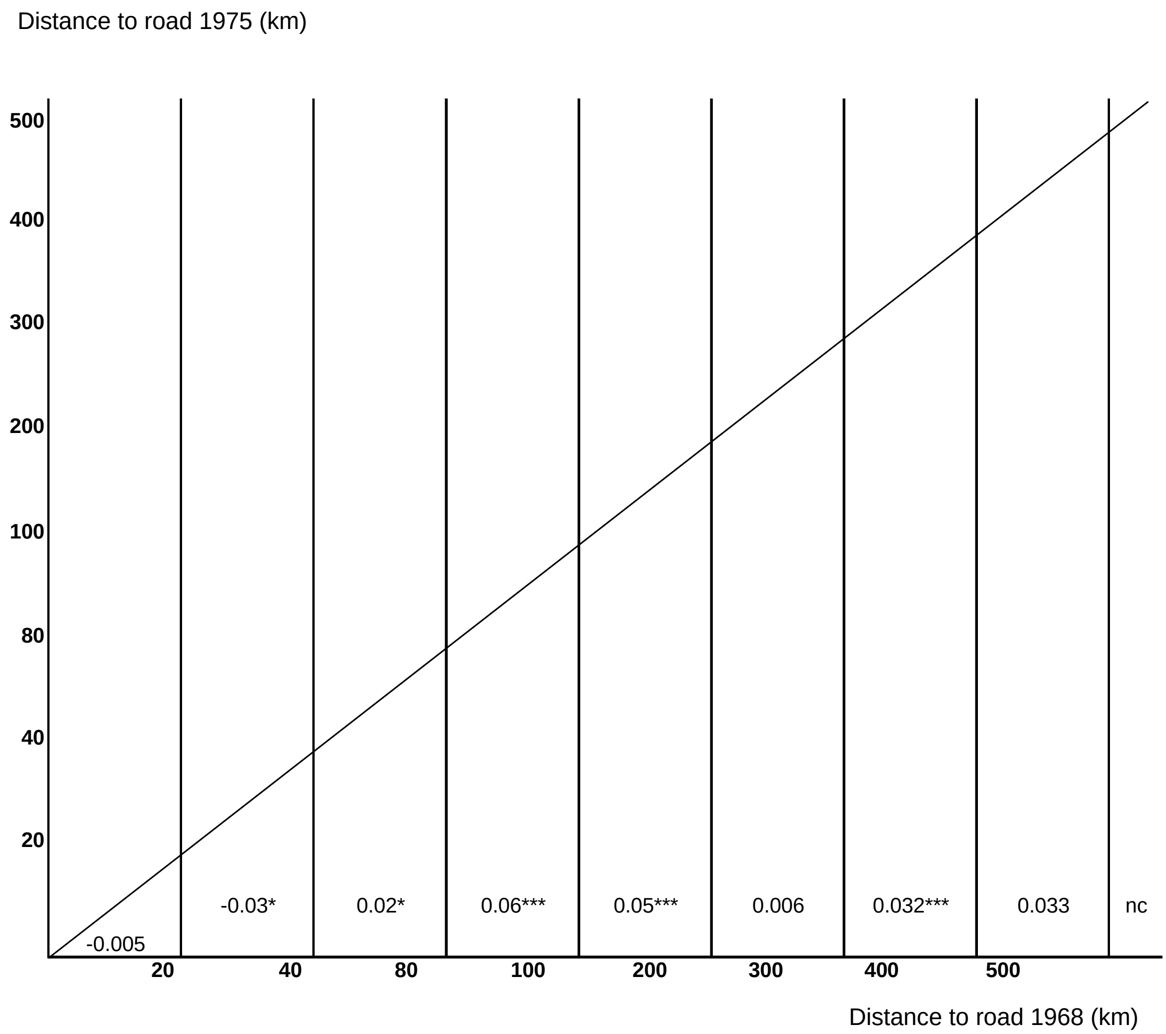


Figure 5

matching impact estimates, exact match on 1968 distance,

explaining 1976-1987deforestation rates (fraction cleared)

[compare column cells to diagonal points in same column]

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

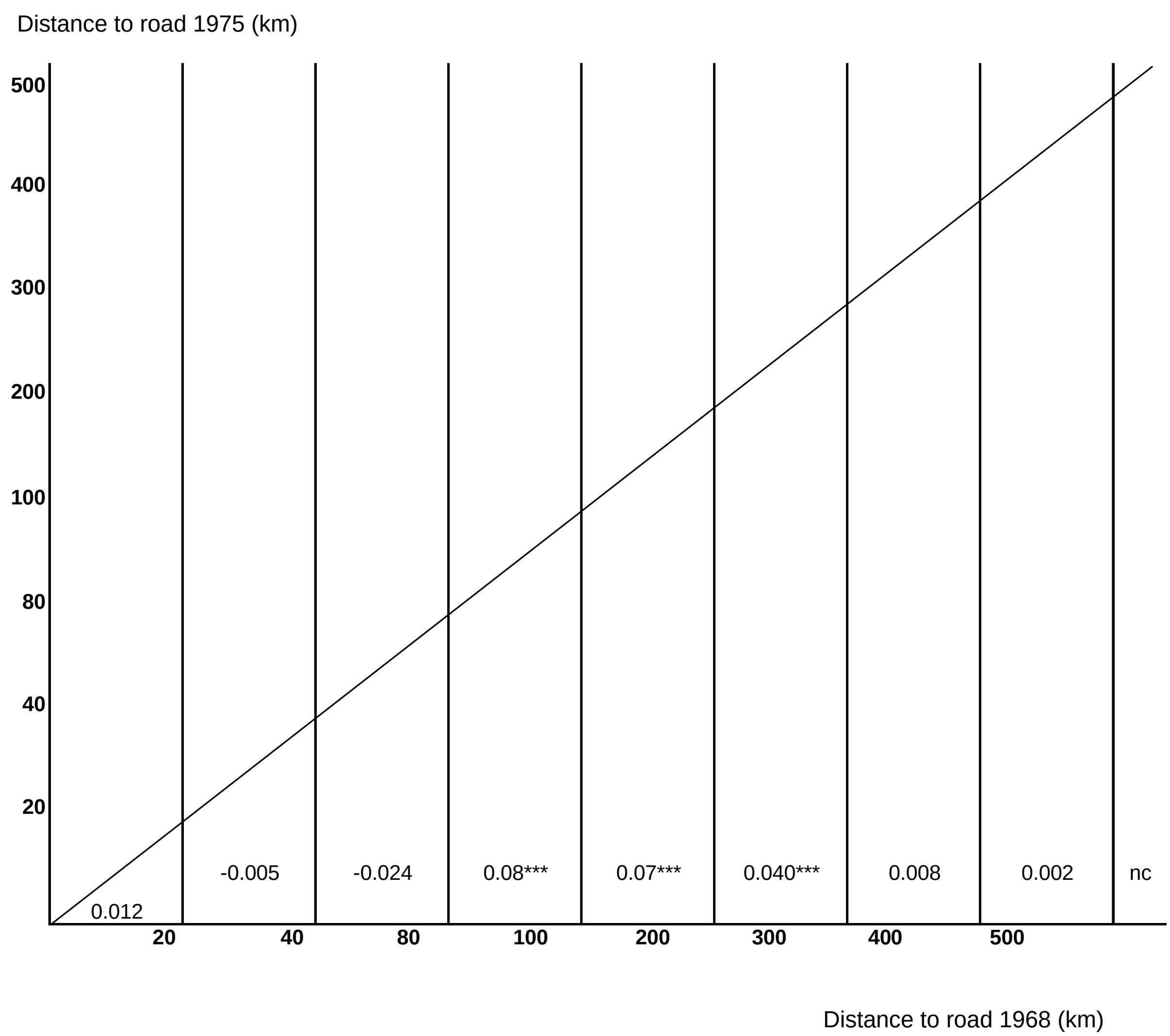


Figure 6

1968-1975 clearing predicted by other factors, by 1968 x 1975 distance cell

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

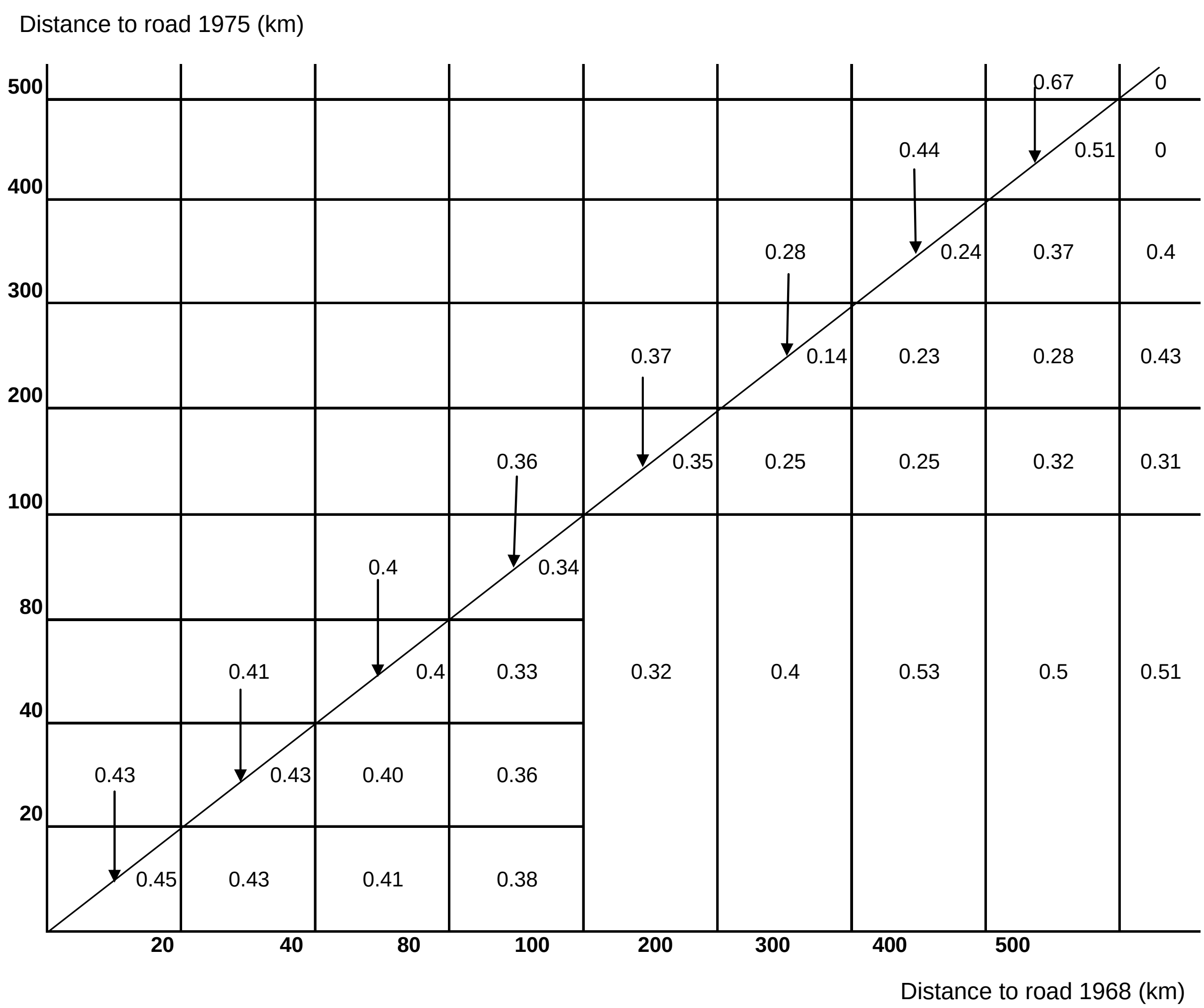


Figure 7a (distance to medium-sized cities)

mean values of other driving factors, by 1968 x 1975 distance cell

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

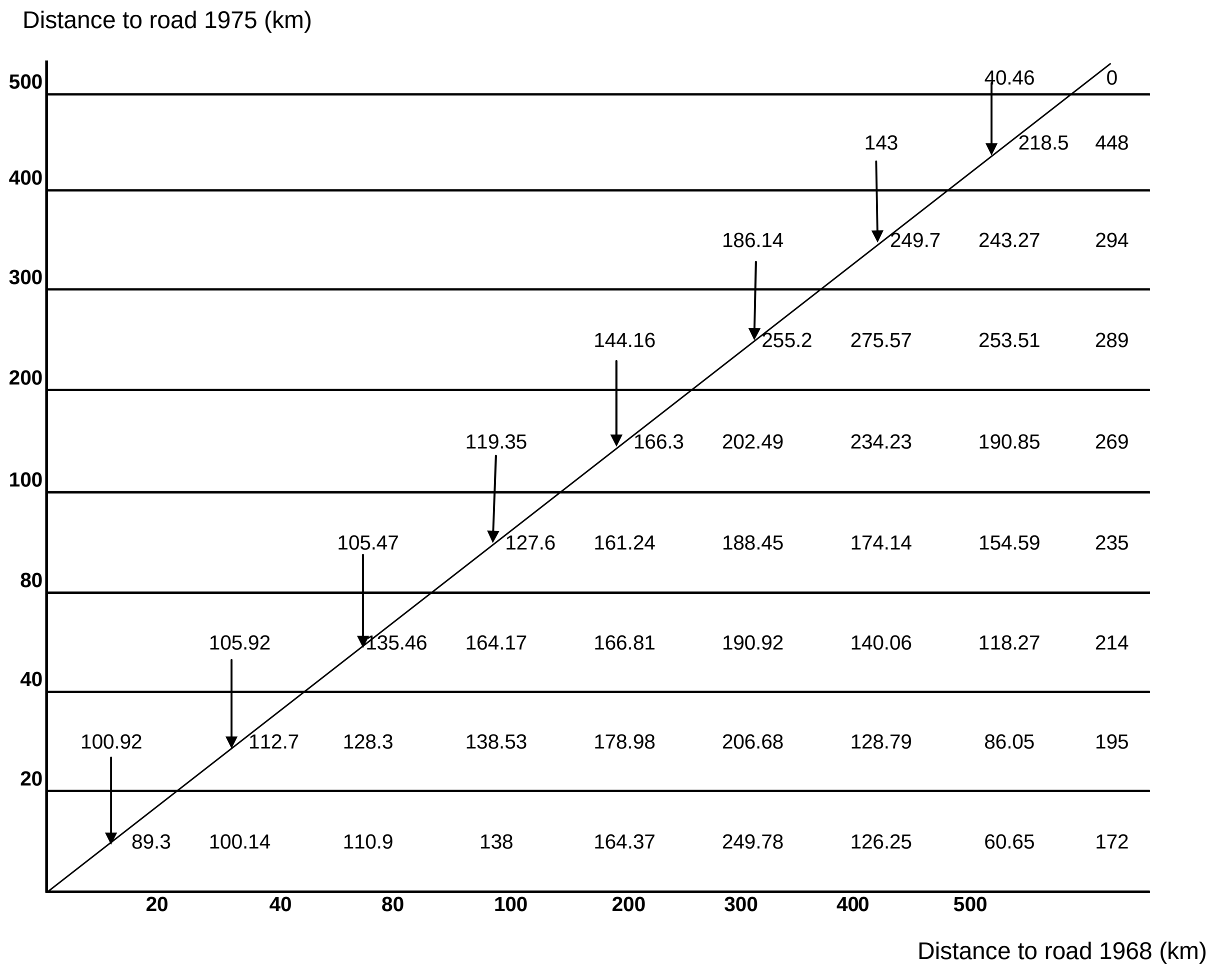


Figure 7b (mean annual precipitation (mm3))

mean values of other driving factors, by 1968 x 1975 distance cell

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

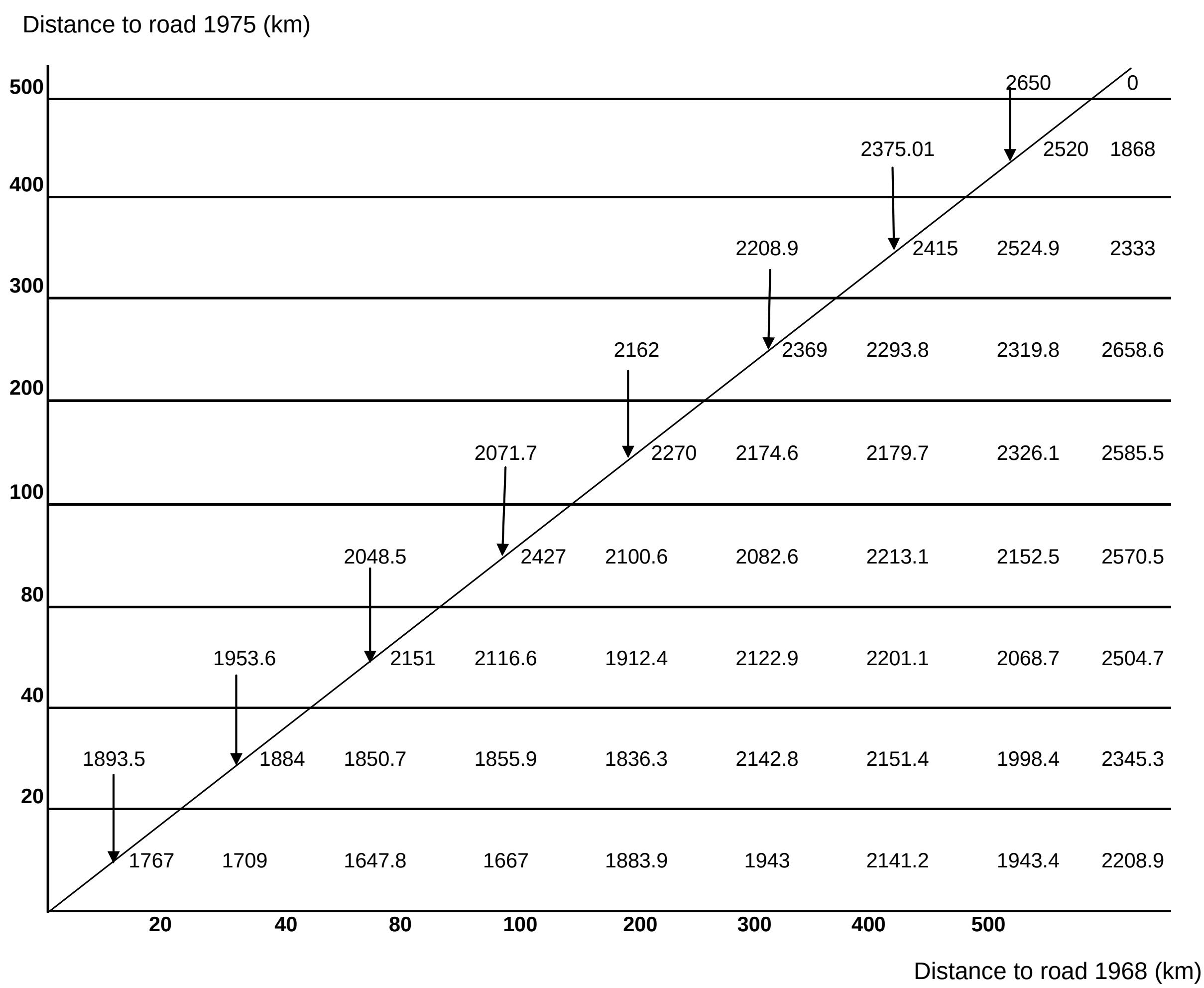


Figure 8

matching impact estimates, by 1968 x 1975 distance cell,

explaining 1976-1987deforestation rates (fraction cleared)

[compare any cell to diagonal points in the same column]

{drop 1975distance > 1968 distance & 0 < 1968distance – 1975distance < 5km}

Distance to road 1975 (km)

Distance to road 1975 (km) \ Distance to road 1968 (km)	20	40	80	100	200	300	400	500	
400								-0.00 -0.02	nc
300							0.00 0.00	0.02** 0.07*	nc
200						0.00 0.02**	0.01 0.02*	No Good Matches	nc
100					0.01 0.01	0.04 0.01**	0.00 -0.06	0.01* -0.08	nc
80				0.00 -0.00	0.06*** 0.04***	0.05 0.02	0.05 0.01	No Good Matches	nc
40			-0.04 -0.01	0.04* 0.06***	0.12*** 0.06***	0.10 0.06***	0.01 -0.03	No Good Matches	nc
20		-0.05 -0.03	0.00 -0.02	0.13** 0.07**	0.20*** 0.08***	0.18** 0.09***	0.09 -0.06	No Good Matches	nc
	0.01 0.01	0.03 0.01	0.009 -0.010	0.31*** 0.08**	0.35*** 0.15***	0.33*** 0.30***	0.06 -0.19**	No Good Matches	nc

Distance to road 1968 (km)

Note: Within each cell, the upper estimate results from post-matching comparisons of group means, while the lower estimate results from post-matching regressions that compare the two groups. The latter include the other covariates. They are rarely perfectly balanced between the groups.

Figure 9a

1987-1991 deforestation rates (fraction cleared) by 75x85 distance cell

{drop 1985distance > 1975 distance & 0 < 1975distance – 1985distance < 5km}

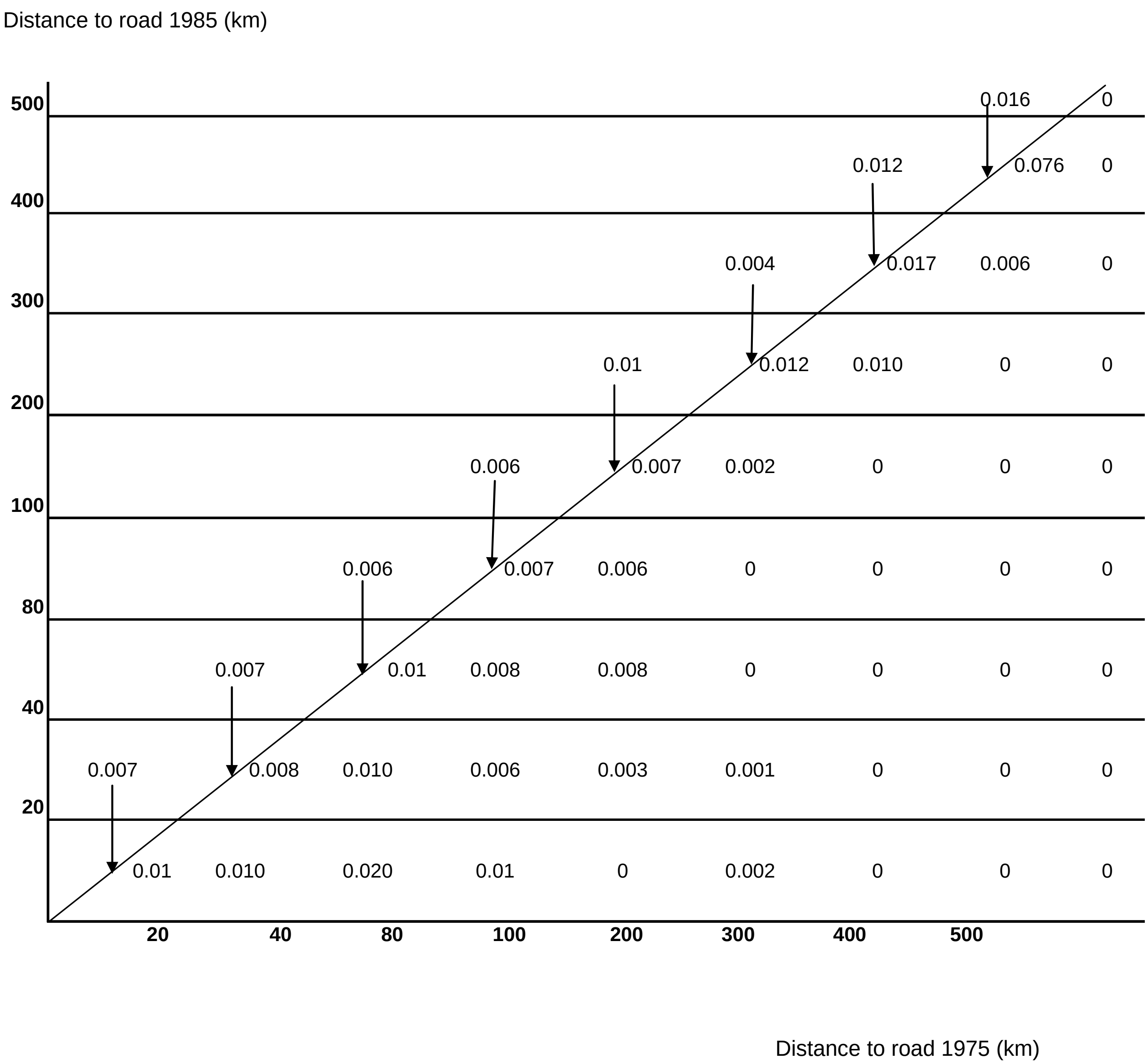


Figure 9b

2000-2004 deforestation rates (fraction cleared) by 85x93 distance cell

{drop 1993distance > 1985 distance & 0 < 1985distance – 1993distance < 5km}