JOURNAL OF REGIONAL SCIENCE, VOL. 47, NO. 1, 2007, pp. 109–123

ROAD INVESTMENTS, SPATIAL SPILLOVERS, AND DEFORESTATION IN THE BRAZILIAN AMAZON*

Alexander Pfaff and Juan Robalino

The Earth Institute, Columbia University, Hogan Hall, 2910 Broadway, New York, NY 10025. E-mails: ap196@columbia.edu; jar101@columbia.edu

Robert Walker, Steven Aldrich and Marcellus Caldas

Department of Geography, Michigan State University, 311 Natural Science, East Lansing, MI 48824-1115. E-mails: rwalker@msu.edu; aldric30@msu.edu; caldasma@msu.edu

Eustaquio Reis

Instituto de Pesquisa Economica Aplicada, Avenida Presidente Antonio Carlos 51 s/1703, Rio de Janeiro, RJ 20.020, Brazil. E-mail: ejreis@ipea.gov.br

Stephen Perz

Department of Sociology, University of Florida, 3113 Turlington Hall, P.O. Box 117330, Gainesville, FL 32611-7330. E-mail: sperz@mail.clas.ufl.edu

Claudio Bohrer

Departamento de Geografia, Universidade Federal Fluminense, Campos Praia Vermelha, Niteroi, RJ 24210-340, Brazil. E-mail: bohrer@vm.uff.br

Eugenio Arima

Department of Environmental Studies, Hobart and William Smith Colleges, 300 Pulteney Street, Geneva, NY 14456 and Instituto do Homem e Meio Ambiente da Amazonia, Brazil. E-mail: arima@hws.edu

William Laurance and Kathryn Kirby

Smithsonian Tropical Research Institute, Apartado 2072, Balboa, Republic of Panama. E-mail: laurance@si.edu **Q1**

ABSTRACT. Understanding the impact of road investments on deforestation is part of a complete evaluation of the expansion of infrastructure for development. We find evidence of spatial spillovers from roads in the Brazilian Amazon: deforestation *rises* in the census tracts that lack roads but are in the same county as and within 100 km of a tract with a new paved or unpaved road. At greater distances from the new roads the evidence is mixed, including negative coefficients of inconsistent significance between 100 and 300 km, and if anything, higher neighbor deforestation at distances over 300 km.

*We thank for helpful comments two anonymous reviewers as well as the participants at two LBA Science Meetings where we presented some of this analysis. Support was provided by the Tinker Foundation, Inc., NASA's LBA program (NCC5-694 and LC-05), and the National Science Foundation (BCS-0243102).

Received: XXX, XXXX; revised: XXX, XXXX; accepted: XXX, XXXX.

Blackwell Publishing, Inc., 350 Main Street, Malden, MA 02148, USA and 9600 Garsington Road, Oxford, OX4 2DQ, UK.

1. INTRODUCTION

Tropical forests continue to receive immense attention as locations where the dictates of economic development and ecosystem conservation regularly clash. The few major areas of tropical forest are prized for the species habitat and the carbon storage they provide. However, clearing is ongoing and some deforestation is likely to continue given not only the local importance of development but also the difficulty of regulation.[1] In this setting, road networks may significantly shape the amount and spatial pattern of remaining forest.

The Amazon region features exceptionally high levels of biodiversity and its large forested area may play important roles within the global carbon and hydrological cycles. In the early 1960s, the national government began to build roads linking the Amazon to other parts of Brazil. Over 16 percent of Brazil's original (pre-Columbian) Amazonian forest has disappeared and current rates of forest loss are on the order of 2 million hectare per year.[2]

Amazonia also currently features heated debate about new investments in roads. In 2000, a Brazilian-government initiative originally known as "Avança Brasil" (Advance Brazil) called for paving an additional 7,500 km of highways in Amazonia.[3] Some plans are widely debated, such as paving the 900 km BR-163 highway, which would facilitate soy exports.[4] How much forest would be lost from such investments for development? Solid empirical answers to this question, based upon decades of observed investment and forest clearing, are relevant for policy choices in Amazonia and forests around the globe.

Pfaff (1999), an econometric analysis of 1970s and 1980s Amazon deforestation, found that greater road density in a county is associated with higher deforestation in that county and higher deforestation in neighboring counties as well.[5] Along the lines of this now "conventional wisdom," Laurance et al. (2001) presented scenarios for the Amazon based upon mechanical assumptions about the impacts of infrastructure on forest cover. These suggested that, fully implemented, Avan ca Brasil would lead to the loss of 28 percent of the pre-Columbian forest by 2020 if "optimistic" and a loss of 42 percent if "nonoptimistic."[6] **Q2**

[1]Regulation may, in turn, permit contracts that would support forest, e.g., payments for carbon services.

[2]Laurance et al. (2004).

[3]Laurance et al. (2001); Fearnside (2001).

[4]Nepstad et al. (2002).

[5]While not often testing explicitly for roads' effects on neighboring locations, many find similar results for other places (see, e.g., Chomitz and Thomas, 2003; Pfaff and Sanchez, 2004). For the Brazilian Amazon, two such studies important to note here are Reis and Margulis (1991) as well as Reis and Guzman (1992).

[6]This assessment stimulated considerable debate, often focused upon the assumptions concerning impacts of new roads on forest cover. For debate, see e.g., do Amaral (2001), Goidanich (2001), Silveira (2001), and Weber (2001). For alternative derivations of parameters from observed deforestation see, e.g., Stavins and Jaffe (1990) and Nelson and Hellerstein (1995) plus, for the Amazon, Reis and Guzman (1992) and Pfaff (1999).

This contrasts with Andersen et al.'s (2002) econometric evaluation of census data for the Amazon. Concerning new road impacts on forest clearing, they give dramatically different conclusions.[7] They found not only that the impact of roads depends on the level of prior forest clearing but also, most dramatically, that given substantial prior clearing new roads *reduce* the rate of deforestation. This, they suggest, is because roads in these circumstances focus local development and draw it away from remaining forest tracts.

Pfaff et al. (2006) re-evaluated Andersen et al.'s roads findings with more precise data.[8] Andersen et al. employed Brazilian county (or município) combinations for which census data can be collected across decades, resulting in a sample of 257 units (large on average though varied in size). Pfaff et al. (2006) used census tracts to have over 20 times as many observations for a similar regression approach based on past deforestation with statistical controls for nonroad influences on land use. They find that lowering transport costs via paved highways or unpaved roads increases deforestation in the census tract in which the road investment was made. Furthermore, at no level of prior clearing did new roads decrease clearing; in fact, the largest effects are in census tracts deforested 50–75 percent.[9]

That Pfaff et al. (2006) used census tracts leaves open the possibility that new roads investments raise deforestation in census tracts receiving investments but lower it in other census tracts in that county. This paper addresses that possibility. Its robust finding is that road investments *increase* deforestation in nearby ($<$100 km) census tracts without roads.

To estimate the spatial spillovers from road investments in a given census tract, we join census-tract data on roads over time with satellite data on deforestation over time. We focus here on the earliest well-observed periods of Amazon deforestation and roads investment. This helps to avoid, as much as possible, the issue that the location of roads over time often followed upon past road investments. Thus we employ maps of the paved and unpaved roads networks (see paved in Fig 1) in 1968, 1975, and 1987 and use the 1968–1975 roads changes as explanatory variables for observed 1976–1987 deforestation, controlling for where roads already existed in 1968 and where they went in 1975–1987.

[7] We note that their book addresses a wide range of issues, including the economic benefits from clearing of the forest, and it is not focused on roads. Their work advanced methodologically the study of Amazon deforestation. For instance, their use of lagged road changes to explain deforestation is applied here and the census data permits not only analysis across multiple decades but also a set of useful explanatory variables. Furthermore, they put significant effort into application of an objective method for choosing among regressions.

[8] Pfaff et al. (2006) used the same approach as below but examined the census tracts where the roads are.

[9] The number of census tracts observations was sufficient to break up the sample into four categories based on the percent of forest cleared prior to the deforestation being explains: 0 percent; 0–50 percent; 50–75 percent; 75–100 percent.

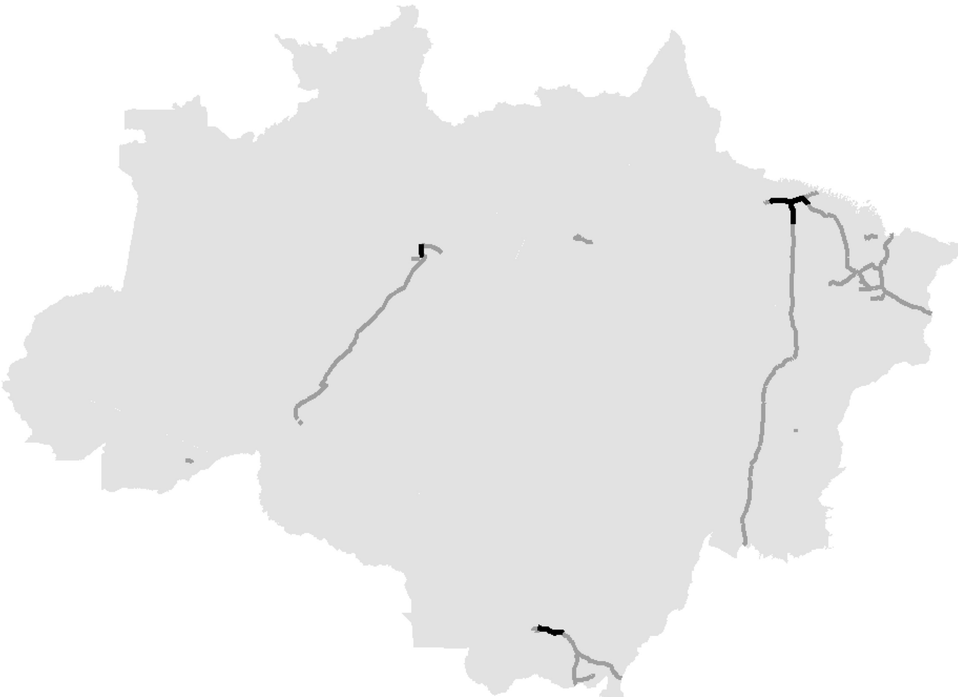

FIGURE 1: Paved Highways in Brazilian Amazonia in 1968 (Black) and 1975 (Add Dark Gray).

The roads-clearing relationship is estimated in regressions with control variables, i.e., other factors expected to affect the net benefits from deforestation and thus its pace. These include ecological characteristics (rain, soil quality, slope, and distances to rivers) as well as distances to cities. All of these controls have been shown in previous research to have significant effects on deforestation. We also confirm here those previous results.

The increase in observations made possible by moving from counties to census tracts, from under 300 to over 6,000, permits another set of important control variables. We clearly do not observe all of the factors that drive local clearing rates across the basin. However our data permit the inclusion of separate effects for each county unit,[10] a major gain in controlling for the effects of potential unobserved drivers while examining roads.

We find that when a census tract receives a new road investment, either paved or unpaved, the deforestation rates in the neighboring census tracts

[10]Note that we would also like to use population and output changes between 1970 and 1975 censuses as explanatory variables. Since they are observed at no lower than county level, though, this is not possible when we use the county spatial fixed effects. However, we can include them when using state fixed effects.

within that tract's county increase for census tracts within 100 km. Thus, new roads increase neighboring clearing.[11] This spatial-spillover result is robust to a broad set of specification checks (see Tables).

For census tracts at greater distances from the census tract receiving the new road, results are decidedly mixed. For tracts 100–300 km away we find some negative results, albeit of inconsistent significance, which raise the possibility that a road could lower the deforestation in its county. Beyond 300 km, if anything roads seem to increase clearing.

The rest of the paper is as follows. Section 2 provides a simple model of land clearing choice at the producer level, which respects the irreversibility of deforestation and can be aggregated to tract or county level. Section 3 describes the data. Section 4 provides our results, and finally Section 5 concludes with a summary and discussion.

2. MODEL

Like others (e.g., Stavins and Jaffe, 1990) we employ a dynamic theoretical model, though unlike others we emphasize irreversibility and spatial patterns in our empirical approach. Deforestation results from a decision by a risk-neutral land user on hectare j, selecting T, the time to clear, to maximize the expected present discounted value of returns from j:

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

where:

S_{jt} = expected return to forest uses of the land, affected by roads/transport cost;

R_{jt} = expected return to nonforest land uses, affected by roads/transport cost;

C_{jT} = cost of clearing net of obtainable timber value;

r = the interest rate.

Two conditions are necessary for clearing to occur at time t. Clearing must be profitable and, even if it is, (2) must hold since it could be more profitable to wait and clear at t+1:

$$\text{(2)} \qquad R_{jt} - S_{jt} - r_t C_{jt} + dC_{jt}/dt > 0$$

If a second-order condition holds, this necessary condition (2) is sufficient for clearing.[12]

[11] This echoes Pfaff (1999) but is within-county, a result that can be found only with our more precise data.

[12] It may be violated if environmental protection becomes more stringent and the returns to ecotourism rise. Our reduced form empirics can also be interpreted in terms of both (2) and the profit condition holding.

Consistent with this model, we assume irreversibilities, as trees take time to grow and development changes the marginal returns to land use. Thus empirically we examine clearing of the standing forest, not assuming reforestation on previously cleared land, in contrast with the many analyses of stock of forest common in the empirical literature.[13]

In the model deforestation occurs when condition (2) is satisfied for the first time. When this occurs differs across space due to differences in land quality, access to market and both exogenous and endogenous temporal shifts. As we observe forest loss in census tracts (i), aggregating the model predictions for these areas yields our empirical approach.

While our data for the drivers of deforestation are for census tracts, actual returns and changes in costs clearly vary across parcels in the census tract. We acknowledge that we do not measure the parcel net clearing benefits perfectly, such that clearing occurs if:

$$R_{ijt} - S_{ijt} - r_t C_{ijt} + \mathrm{d}C_{ijt}/\mathrm{d}t = X_{it}\beta - \varepsilon_{ijt} > 0 \tag{3}$$

where ε_{ijt} is a parcel-year-specific term for the unobserved relative returns to forest, so:

$$Probability\,[satisfy\,(3)\,so\,that\,clear\,if\,currently\,in\,forest] = Prob(\varepsilon_{ijt} < X_{it}\beta) \tag{4}$$

Since X_{it} are the same for each parcel in a census tract, predictions are effectively for the tract rates of deforestation during a given time interval. These predicted clearing rates depend on the X_{it} as well as on the assumed distribution of the ε_{ijt}. If the cumulative distribution of the errors ε_{ijt} is logistic, then we will have a logit model:

$$F(X_{it}\beta) = (1/(1 + \exp(X_{it}\beta)) \tag{5}$$

For our data, we estimate this model using the minimum logit chi-square method known as "grouped logit." If h_{it} is a tract's measured rate of forest loss, then we will estimate:

$$Log(h_{it}/(1 - h_{it})) = X_{it}\beta + \mu_{it} \tag{6}$$

The variance of the μ_{it} (referring to tracts) can be estimated by $(1\,/\,I_{it}\,h_{it}\,(1–h_{it}))$, where I_{it} is the number of forested parcels in tract i at the beginning of interval t. The estimator is consistent and asymptotically normal.[14] This is estimated by weighted least squares.

[13] Some papers have, of course, addressed the issue of deforestation per se, e.g., Ehui and Hertel (1989).

[14] Maddala (1983), p. 30. See also an explicit discussion of heteroskedasticity in, e.g., Green (1990).

3. DATA AND STRATEGY

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

Forest

Our analyses employ thousands of census-tract observations across space to study the drivers of deforestation for an eleven-year period, 1976 to 1987, following the first major era of highway expansion (see Fig 1 for the 1968–1975 paved road expansion). 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. Our dependent variable is the fraction of an area's 1976 forest area deforested by 1987.

Driving Factors

Roads and Neighboring Roads. We tracked the evolution of roads in each census tract. 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 use show the distribution of paved and unpaved roads for 1968, 1975, and 1987. For these years, we measured the length of paved and unpaved roads for each 1996 Legal Amazon census-tract polygon. Then we calculated the changes in paved and unpaved road density during 1968–1975 and 1975–1987. Thus we employ road investment variables, i.e., changes over time and not stocks, which means that each investment is evaluated by itself and not merged with past actions.

This is a "lagged" measure, i.e., investments during 1968–1975 to explain 1976–1987 deforestation. That helps to address concerns with endogeneity since the observed actual deforestation between 1976 and 1987 has no impact on past road construction.[15] Furthermore, in our core regressions we will also drop the census tracts in which there were roads in 1968 (our earliest information) and in which there were road investments made during 1975–1987. The latter simultaneous change could affect deforestation rates. We also rerun those regressions with those tracts and using those roads as control variables.

[15] Of course unobservables can still matter, but the fact that we are running these regressions not in levels but as changes in forest explained by changes in roads helps, as does the fact that we are including in these regressions county effects (below). For changes regressions, that is taking out all the county-specific trends.

From these measures of changes in road density within each of our census tracts, we calculate neighboring-road-density changes for each tract. Using centroid-to-centroid distances between census tracts, for 100 km-width rings out to 1,000 km around each tract we create neighboring-road variables.[16] For any given tract and a given neighboring ring, we sum the road investments occurring in tracts in that ring and divide by the tracts' area. To address others' county empirical results, we use only neighbors from the same county. We do this for all tracts for all rings for the 1968–1975 paved and unpaved investments.

Controls

We integrated maps of ecological conditions and distances to cities to control for their influences in testing our hypotheses about roads. The ecological variables are an index of soil quality, binary variables indicating slope categories (described in our Tables), and continuous rainfall data.[17] We also use the deforestation prior to 1976.

Our statistical approach exploits our numerous observations for implementation of "county-effect" controls for influences of unobserved factors shared by census tracts in the same county (e.g., soil qualities that are not within our data set and whose values are spatially correlated). This is a major advance in controls for potential nonroad factors, whose impact can be evaluated by comparing across the columns of our core Table 2.

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. We do robustness checks for these drops (e.g., first column of Table 3).

Samples

One important choice was to drop census tracts, which received road investments during 1968–1975 or had roads in 1968 or received road investments during 1975–1987. This is to focus upon the spillovers from neighboring roads, without any confusion due to roads within a census tract itself. However, we test robustness to using those observations (Table 3s second column, e.g., has all tracts except those with new roads in 1968–1975).

Another important choice concerns those census tracts, which have no neighboring census tracts within one or more of the rings we are using for

[16]One could also use "having a common border" to determine neighbors. However, since the tracts are so varied in size, that method would be less consistent across census tracts than our approach using distances.

[17]For discussion of the soil and rainfall data, see Laurance, et al. (2002).

TABLE 1: Descriptive Statistics

	Mean	Std. dev.	Min	Max
Changes in density from 1968 to 1975:				
Paved highways 000–100 km	7.5*e*–04	0.0024	0	0.0255
Unpaved roads 000–100 km	32.6*e*–04	0.0052	−0.0139	0.0270
Paved highways 100–200 km	4.9*e*–04	0.0011	0	0.0073
Unpaved roads 100–200 km	29.2*e*–04	0.0041	−0.0040	0.0218
Paved highways 200–300 km	2.8*e*–04	0.0007	0	0.0059
Unpaved roads 200–300 km	26.4*e*–04	0.0031	−0.0030	0.0174
Paved highways 300–400 km	2.2*e*–04	0.0006	0	0.0062
Unpaved roads 300–400 km	24.0*e*–04	0.0027	−0.0051	0.0143
Paved highways 400–500 km	2.2*e*–04	0.0009	0	0.0108
Unpaved roads 400–500 km	36.6*e*–04	0.0043	−0.0053	0.0259
Paved highways ≥500 km	5.3*e*–04	0.0028	0	0.0549
Unpaved roads ≥500 km	59.6*e*–04	0.0098	−0.0131	0.0678
Cleared percent in 1976	0.0370	0.1386	0	0.9996
Distance to city	502.87	268.01	20.53	1187.37
Next to big city	0.0024	0.4987	0	1
Distance to river	273.85	267.90	0.011	1001.39
Rain linear	2018.30	322.09	1200.00	3214.12
Slope—rock outcropping	0.0037	0.0379	0	1
Slope—steep	0.0030	0.0375	0	0.8481
Slope—mountainous	0.0437	0.1512	0	1
Slope—strongly hilly	0.0426	0.1429	0	1
Slope—hilly	0.0863	0.2134	0	1
Slope—gently hilly	0.3114	0.3426	0	1
Soil fertility	3.08	1.11	0	5.00

roads explanatory variables. Our core specification drops those observations. Since this leads us to drop over half our sample, we also include a robustness test (first column of Table 4) in which those census tracts are assigned a zero value for all the neighboring-road-rings variable(s) in question.

An alternative way of addressing the issue of missing values is to drop from our specification the neighboring-ring variables that have the most missing values. Those are the rings further out (not surprisingly, as we employ only neighbors in the same county). The second through sixth columns in Table 4 drop, one by one, all but the closest ring.

4. RESULTS

Core Results for Roads

As seen in Table 2, a robust result is that an investment in either a paved or an unpaved road increases the deforestation rate in the neighboring census tracts in the same county that lack roads but are within 100 km of the tract

TABLE 2: Core Results

	County effects	State effects	No spatial effects
Changes in density from 1968 to 1975			
Paved highways 000–100 km	189 (2.0)	179 (3.1)	210 (4.4)
Unpaved roads 000–100 km	86 (4.3)	83 (5.0)	81 (4.8)
Paved highways 100–200 km	−32 (0.2)	−55 (0.4)	−233 (1.8)
Unpaved roads 100–200 km	−35 (0.6)	−42 (0.8)	−5 (0.1)
Paved highways 200–300 km	84 (0.8)	71 (0.6)	69 (0.4)
Unpaved roads 200–300 km	−57 (1.4)	−68 (1.9)	−101 (2.4)
Paved highways 300–400 km	451 (3.7)	430 (5.5)	317 (1.9)
Unpaved roads 300–400 km	−67 (0.7)	−72 (0.9)	−104 (1.2)
Paved highways 400–500 km	−24 (0.2)	−17 (0.1)	30 (0.2)
Unpaved roads 400–500 km	50 (1.3)	46 (1.1)	8 (0.2)
Paved highways ≥ 500 km	−9 (0.3)	−16 (0.4)	−34 (0.9)
Unpaved roads ≥ 500 km	48 (5.8)	45 (7.0)	54 (3.3)
Cleared Percent in 1976	3 (3.7)	3 (3.8)	4 (3.6)
Distance to city	0006 (0.4)	−0.001 (1.1)	0.0004 (0.3)
Next to big city	3 (2.0)	3 (1.9)	5 (2.1)
Distance to river	0.003 (1.1)	0.002 (1.2)	−0.0006 (0.8)
Rain linear	−0.2 (2.5)	−0.2 (3.1)	−0.1 (2.1)
Rain quadratic	0.0002 (2.3)	0.0002 (2.8)	9*e*05 (1.9)
Rain cubic	−5*e*08 (2.2)	−5*e*08 (2.6)	−3*e*08 (1.7)
Rain quartic	6*e*12 (2.1)	−6*e*12 (2.5)	3*e*12 (1.6)
Slope—rock outcropping	−7 (8.0)	−7 (9.8)	−8 (3.9)
Slope—steep	0.2 (0.3)	0.01 (0.0)	−2 (2.1)
Slope—mountainous	−1 (1.7)	−1 (1.9)	−1 (2.7)
Slope—strongly hilly	−1 (4.3)	−1 (3.6)	−1 (3.0)
Slope—hilly	−1 (1.7)	−1 (1.9)	−1 (3.1)
Slope—gently hilly	−0.2 (0.5)	−0.2 (0.6)	−1 (3.2)
Soil fertility	0.4 (4.3)	0.4 (4.1)	0.4 (7.4)
Constant	116 (2.7)	118 (3.3)	66 (2.2)
County effects	Significant	–	–
State effects	–	Significant	–
R^2	0.49	0.49	0.39
Obs.	1642	1642	1642

receiving that investment. The new road appears to increase the demand for outputs produced on cleared land in these neighbors.

However, all three columns suggest the possibility that road investments lower the rate of deforestation in neighboring census tracts (with no roads) within 100 to 300 km. The significance of these variables is weak and inconsistent but still this is worth noting. Perhaps at this distance the local demand for outputs may mean less while opportunities for work created by the road investment might still migration from those census tracts.

Finally, for greater than 300 km there are some positive significant coefficients, i.e., some results suggest that neighboring roads are associated with higher deforestation in the census tracts that lack roads. These are relatively

TABLE 3: Drop or Add Locations

	Drop near medium cities	Add some roads locations
Changes in density from 1968 to 1975		
Paved highways 000–100 km	102 (1.8)	110 (2.0)
Unpaved roads 000–100 km	69 (4.7)	63 (3.7)
Paved highways 100–200 km	−153 (1.1)	−146 (1.6)
Unpaved roads 100–200 km	−66 (1.6)	−24 (0.6)
Paved highways 200–300 km	−68 (0.8)	189 (1.8)
Unpaved roads 200–300 km	−31 (0.9)	−75 (1.8)
Paved highways 300–400 km	320 (8.7)	443 (3.8)
Unpaved roads 300–400 km	−86 (1.1)	−49 (0.6)
Paved highways 400–500 km	−6 (0.0)	145 (1.9)
Unpaved roads 400–500 km	−16 (0.6)	52 (1.5)
Paved highways $\geq$ 500 km	−1 (0.0)	−31 (2.3)
Unpaved roads $\geq$ 500 km	43 (6.7)	32 (3.4)
Road investments in tract itself	–	–
1968 roads and 1975 to 1987 Changes	–	Included as controls
Cleared percent in 1976	2 (3.0)	3 (25)
Distance to city	−005 (3.1)	−0.001 (1.1)
Next to big city	3 (3.0)	−3 (4.9)
Distance to river	0.002 (1.0)	−001 (0.7)
Rain linear	−0.1 (2.1)	−0.2 (3.0)
Rain quadratic	0.0001 (1.9)	0.0001 (2.6)
Rain cubic	−3*e*08 (1.8)	−4*e*08 (2.3)
Rain quartic	4*e*12 (1.8)	4*e*12 (2.0)
Slope—rock outcropping	−5 (11)	−9 (4.6)
Slope—steep	0.005 (0.0)	−0.03 (0.1)
Slope—mountainous	−1 (2.4)	−1 (1.2)
Slope—strongly hilly	−2 (3.3)	−1 (4.1)
Slope—hilly	−0.3 (0.6)	−1 (1.9)
Slope—gently hilly	−0.2 (0.6)	0.1 (0.3)
Soil fertility	0.3 (3.3)	0.3 (5.1)
Constant	70 (31)	90 (3.4)
County effects	Significant	Significant
R^2	0.49	0.47
Obs.	1556	2494

consistent across the columns of Table 2, i.e., they are not dominated by the presence or lack of our county or state effects.

Drop or Add Locations

Table 3 redoes the first column of Table 2 with changed sample. The first column of Table 3 uses distances to cities to exclude urban areas further. More specifically, about 1,000 census tracts within 20 km of the largest 270 cities are

TABLE 4: Address Missing Values [a]

	Use a zero when no tracts in ring	Out to 500 km only	Out to 400 km only
Changes in density from 1968 to 1975:			
Paved highways 000–100 km	107 (2.5)	218 (2.4)	160 (2.0)
Unpaved roads 000–100 km	64 (2.9)	80 (4.3)	66 (2.4)
Paved highways 100–200 km	−56 (1.3)	−10 (0.1)	−99 (0.7)
Unpaved roads 100–200 km	7*e*06 (1.2)	−52 (1.1)	−15 (0.4)
Paved highways 200–300 km	−82 (1.3)	82 (0.7)	42 (0.3)
Unpaved roads 200–300 km	34 (2.2)	−66 (1.5)	−18 (0.5)
Paved highways 300–400 km	182 (1.7)	500 (2.9)	237 (1.5)
Unpaved roads 300–400 km	−48 (1.9)	−97 (1.2)	−15 (0.3)
Paved highways 400–500 km	−20 (0.2)	−52 (0.6)	–
Unpaved roads 400–500 km	64 (2.7)	47 (1.6)	–
Paved highways ≥ 500 km	−33 (1.4)	–	–
Unpaved roads ≥ 500 km	41 (9.5)	–	–
R^2	0.57	0.48	0.47
Obs.	4,529	2,068	2,470

	Out to 300 km Only	Out to 200 km Only	Out to 100 km Only
Changes in density from 1968 to 1975:			
Paved highways 000–100 km	112 (2.1)	127 (2.5)	166 (3.4)
Unpaved roads 000–100 km	70 (2.6)	49 (2.0)	44 (1.8)
Paved highways100–200 km	−208 (2.3)	−46 (1.0)	–
Unpaved roads 100–200 km	19 (0.7)	1*e*05 (1.4)	–
Paved highways 200–300 km	−58 (0.7)	–	–
Unpaved roads 200–300 km	17 (0.8)	–	–
Paved highways 300–400 km	–	–	–
Unpaved roads 300–400 km	–	–	–
Paved highways 400–500 km	–	–	–
Unpaved roads 400–500 km	–	–	–
Paved highways ≥ 500 km	–	–	–
Unpaved roads ≥ 500 km	–	–	–
R^2	0.48	0.54	0.54
Obs.	2,995	3,819	4,521

[a]All of the regressions above contained the same nonroad variables indicated in the first column of Table 2.

dropped, instead of about 100 near to the largest 19 cities.[18] The result of this shift to a more rural sample is that the first, 0–100 km neighboring-road-ring

[18]As discussed in Section 3.3., we are already dropping the census tracts that do not have neighboring tracts in each of our rings (we test this choice in Table 4). Thus the effect on our sample is not so dramatic for this run.

coefficients are significantly lower. Thus the effect of neighboring roads in 'bringing the economy/demand to those tracts without roads' seems to be lower when the setting being considered is further from existing cities.

The second column adds the census tracts that had roads already in 1968 as well as those which received investments in roads during 1975–1987 (Table 2 considered only tracts without any roads by 1987). Thus, the effect of neighbor roads seems to be lower if there are roads present before the neighbor investment or during the time of deforestation.

Address Missing Values

Table 4's first column examines our choice to drop tracts that lack neighboring tracts in one or more of the rings we have included. In previous tables, if a census tract had no neighboring census tracts with centroids within 400–500 km, e.g., that tract would have a missing value for that ring and be dropped. This motivated our use of "$\geq$500 km" since using each 100-km-wide ring up to 1,000 km would have further limited the sample.

As a robustness check, we redid our neighboring-road variables with zeros when no neighboring tracts exist within a ring. This close to triples our sample. However, it does not appear to affect a great deal the nature of the results, looking across the rings.

Table 4's other five columns drop, one by one, the further rings. As they are the variables with most missing values, by the last column again the sample almost triples. Of greatest note is that while the coefficients vary for the 0–100 km ring, they are in the range seen in the earlier tables and that they are significant does not vary. The same cannot be said for the negative coefficients, although there are still some significant results within the 100–400 km range. The rings at the greater distances are harder to look at here, obviously. Again, though, 0–100 km positive significance is seen to be relatively robust.

5. CONCLUSION

We exploited very detailed data on deforestation and roads investments over time to provide estimates of spatial spillovers from road investments in the Brazilian Amazon. We found that deforestation rises in census tracts with no roads when a road investment occurs in a census tract in the same county, which is within 100 km. We also find negative coefficients, albeit of inconsistent significance, suggesting possibly reduced deforestation in tracts 100–300 km from tracts with road investments. At distances greater than 300 km, again some positive coefficients are significant and are relatively robust to specification.

This paper was motivated in part by the desire to compare results from the use of census-tract data with results from county data. Greater evidence in census-tract results of increased deforestation due to roads could, in principle, arise from neglect in tract results of spillovers across the tracts within a county. Our results suggest that spillovers are not an explanation for those differences. This motivation, though, led us to look at spillovers only in the same county. A focus on spatial spillovers could include all of the neighbors.

Future work could benefit from measuring distances directly to roads in some fashion. While this paper already required laborious spatial calculations based upon an overlay of roads maps on census tract, county and state boundaries, using the distance between census-tract centroids to measure proximity of neighboring roads has certain limitations. Since the procedure used here has produced novel, significant, and policy relevant results, extending the measurement of proximity in this way may have value.

Another natural extension would be in time, e.g., the use of more periods for both deforestation and road investments to explore these spatial effects in a dynamic setting. For instance, another road spillover to explore could be increased future neighbor roads.

REFERENCES

Andersen, L. E. et al. 2002. *The Dynamics of Deforestation and Economic Growth in the Brazilian Amazon*, Cambridge Univ. Press.

Chomitz, K. M., and T. S. Thomas. 2003. "Determinants of Land Use in Amazonia: A Fine-Scale Spatial Analysis," *American Journal of Agricultural Economics*, 85, 1016.

do Amaral, S. S. 2001. Threat to the Amazon, *The Independent*, London, 26 January.

Ehui, S. K. and T. W. Hertel. 1989. "Deforestation and agricultural productivity in the Cote d'Ivoire," *American Agricultural Economics Association*, 703–711.

Fearnside, P. M. 2001. *Environmental Conservation*, 28, 23.

Goidanich, R. 2001. Science dEbates, 26 January (www.sciencemag.org/cgi/eletters/291/5503/438).

Green, W. H. 1990. *Econometric Analysis*, Macmillan.

Laurance, W. F. et al. 2004. "Deforestation in Amazonia," *Science*, 304, 1109.

——. 2001. "The Future of the Brazilian Amazon," *Science*, 291, 438.

——. 2002. "Predictors of Deforestation in the Brazilian Amazon," *Journal of Biogeography*, 29, 737.

Maddala, G. 1983. *Limited-Dependent and Qualitative Variables in Econometrics*. Cambridge: Cambridge University Press.

Nelson, G. C. and D. Hellerstein. 1995. "Do Roads Cause Deforestation? Using Satellite Images in Econometric Analysis of Land Use," *American Journal of Agricultural Economics*, 79, 80.

Nepstad, D. C. et al. 2002. *Science*, 295, 629.

Pfaff, A. 1999. "What Drives Deforestation in the Brazilian Amazon? Evidence from Satellite and Socioeconomic Data," *Journal of Environmental Economics and Management*, 37, 26.

Pfaff, A. and A. Sanchez. 2004. "Deforestation Pressure and Biological Reserve Planning: A Conceptual Approach & An Illustrative Application for Costa Rica," *Resource and Energy Economics*, 26, 237–254.

Pfaff, A., R. Walker, S. Perz, W. Laurance, C. Bohrer, J.A. Robalino, S. Aldrich, E. Arima, M. Caldas, K. Kirby. 2006. "Roads and Deforestation in the Brazilian Amazon." Submitted manuscript, available at www.columbia.edu/ãp196.

Reis, Eustaquio J., and Sergio Margulis. 1991. "Options for Slowing Amazon Jungle Clearing," in R. Dornbusch and J. Poterba (eds.), *Global Warming: The Economic Policy Responses*. MIT Press.

Reis, E. J. and R. Guzman. 1992. *An Econometric Model of Amazon Deforestation*, Rio de Janeiro, Brazil, IPEA.

Silveira, J. P. 2001. *Science*, 292, 1651.

Stavins, R. N. and A. Jaffe. 1990. "Unintended impacts of public investments on private decisions: the depletion of forested wetlands." *American Economic Review.* 80, 337.

Weber, D. 2001. *Ministério contesta estudo sobre devasta cão*. O Estado de S. Paulo, São Paulo, Brazil, 21 January.