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Dear Professors Breza, Viviano, and Chandrasekhar,

I am writing to apply to the *NBER Workshop on Development Economics and Networks for Early-Career Researchers*. I am an Assistant Professor at the O'Neill School of Public and Environmental Affairs at Indiana University Bloomington. My research is in applied and environmental economics, with a focus on how institutions shape land and water use in Brazil, India and the US.

For the workshop, I am submitting my single-authored work in progress, "Land Titling and Deforestation Spillovers: Evidence from the Brazilian Amazon." The paper asks whether incentives created by land titling spread across neighboring landholdings. I construct a network of more than one million parcels in the Brazilian Amazon using shared property boundaries and link these parcels to annual satellite measures of forest loss. The paper also raises broader methodological questions about how exposure should be defined and measured in empirical network settings.

I am particularly interested in the workshop because these network questions have become an important part of my research agenda. My previous work has mainly studied how land and environmental institutions affect individual parcels, households, and municipalities. This project instead studies how responses to these institutions may spread across connected landholders. I would value the opportunity to receive feedback on the empirical design and to learn from researchers working on network formation, diffusion, interference, and measurement of network exposure.

My application includes this cover letter, my curriculum vitae, a statement of research interests, and an extended abstract with key results from the paper I propose to present.

If I am shortlisted, either of the following faculty members could provide a recommendation:

David A. Keiser
Professor of Water Policy and Management
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Thank you for considering my application. I would be grateful for the opportunity to participate in the workshop and to receive feedback on this work while the project is still developing.

Sincerely,

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Land Titling and Deforestation Spillovers: Evidence from the Brazilian Amazon*

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Land titling can create incentives to clear forest when land use helps establish a property claim. This paper asks whether these incentives spill over through a network of neighboring landholdings. I construct a parcel-level network of the Brazilian Amazon by linking administrative land records to annual satellite measures of forest loss. A standard design, in which a parcel becomes treated when its first neighbor receives title, produces a striking result: estimated spillovers increase with the parcel's degree. I show that this gradient need not reflect stronger behavioral responses among more connected parcels. Because treatment is defined by the first treated neighbor, degree mechanically affects the probability and timing of treatment and the number of treated neighbors subsequently encountered. Consistent with this explanation, the gradient is substantially attenuated when exposure is held fixed and is reproduced by a placebo titling program that provides no contemporaneous incentive to clear. I then measure exposure directly using the share of a parcel's boundary adjoining titled land and find little evidence of deforestation spillovers. The results highlight a general problem for empirical work on networks: when treatment is defined as exposure to any treated neighbor, network position can generate apparent treatment-effect heterogeneity even when responses do not vary with network position.

1 — Question

Under Brazilian land law, an occupant of federal land earns a title by demonstrating productive use. On the forest frontier, the most visible form of productive use is cleared land. Prior work has documented that this rule changes behavior on the parcel being titled: under SIGEF, where eligibility is assessed at inspection, clearing rises in the years before a title is granted and peaks in the year of titling; under Terra Legal, where eligibility depends on occupation before a fixed 2004 cutoff, there is no such rise (Baragwanath and Shinde, 2026). Probst et al. (2020) find a related increase in clearing among small holders titled under Terra Legal.

This paper asks a different question: whether the incentive to clear spreads from a titled parcel to the parcels that adjoin it. When a neighbor receives title, does the untitled parcel next door clear more, and if so, what determines the size of the response?

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Two mechanisms could produce a spillover. The first is information: a neighbor's title reveals that formalization is attainable in this place, at this time, through this procedure. The second is strategic: a title next door removes one competing claimant and creates a boundary that the state will now enforce, which may raise the value of marking one's own claim before the remaining land is allocated. Both mechanisms predict that an untitled neighbor clears more after a title arrives. They differ in which neighbors should respond most.

The question has direct policy relevance. Titling programs are implemented in geographic batches. If the incentive to clear propagates across boundaries, the environmental cost of a titling campaign exceeds the sum of effects on the titled parcels, and the size of the multiplier depends on the network structure of the parcels the campaign reaches.

Related work. Neighbor interactions in deforestation have been estimated at the municipality level using spatial econometric methods, and at the plot level by Robalino and Pfaff (2012), who instrument neighbors' clearing in Costa Rica with the slopes of neighboring plots. Satake et al. (2007) provide a theoretical model in which learning from neighbors synchronizes clearing across parcels. The property-rights literature on the Amazon frontier treats clearing as a means of establishing a claim (Angelsen, 1999; Alston, Libecap and Mueller, 2000). My earlier work with coauthors uses the same registries to study collective titling (Baragwanath, Bayi and Shinde, 2023) and the sensitivity of measured deforestation to satellite detection thresholds (Baragwanath and Shinde, 2025). None of this work observes a parcel-level network, uses a neighbor's title as the treatment, or has a placebo program in the same setting.

The closest methodological antecedents are in the literature on diffusion through social networks in developing countries, which studies how an injection at one node—a microfinance offer, a piece of information—spreads to others (Banerjee et al., 2013; Breza et al., 2019). Our setting differs in that the network is physical rather than social, the injection is a legal act rather than information, and the response is land use. It shares that literature's central difficulty: the estimated spillover depends on how the network is measured and how exposure to a treated neighbor is defined.

2 — Contribution

A parcel-level network of an entire frontier. We construct the first contiguity graph of the Brazilian Legal Amazon: 1,076,250 parcels and 3,418,006 shared boundaries, harmonized across four registries. Nodes are landholdings; edges are shared boundaries within a 30 m tolerance.

A network in which exposure is measured rather than assumed. Most spillover designs define a neighborhood by distance. Here the neighborhood is an administrative fact: two parcels either share a surveyed boundary or they do not. This makes it possible to ask not only whether titling propagates, but what the appropriate measure of exposure to a neighbor's title is. Section 5 shows that this question is not incidental.

A placebo from the same setting. Terra Legal shares geography, administering agency (INCRA) and decade with SIGEF, but by statute it removes the forward-looking clearing incentive. It provides a falsification test both for the pooled effect and, as Section 5 shows, for the interpretation of the degree gradient.

3 — Data

We harmonize four registries to a common schema: SIGEF (151,461 parcels), SNCI (13,910), Terra Legal (176,694) and the self-declared CAR (760,688). Appendix A describes each registry, and Figure A1 maps where each one is: SIGEF and CAR trace the Arc of Deforestation, SNCI is concentrated in Mato Grosso, and Terra Legal follows the

Amazonas corridor. SNCI and SIGEF are one certification instrument administered under two procedures: SIGEF replaced the paper-based process under IN 77/2013, and earlier certifications remained valid. We therefore define "verified title" as their union.

The outcome is annual primary-forest loss as a percentage of each parcel's 1985 primary-forest stock, from MapBiomas Collection 10 at 30 m resolution, extracted with geodesic areas. Without any calibration, mean annual loss reproduces the PRODES series: a peak of 2.4–2.6 in 2003–04, the decline following PPCDAm (Assunção, Gandour and Rocha, 2015; Baragwanath and Shinde, 2025), a trough of 0.82 in 2011–12, and the increase in 2019–20.

Constructing the graph. All polygons are projected to an equal-area coordinate system and repaired. Two parcels are joined by an edge when their boundaries lie within 30 m of each other over at least 60 m of length; the tolerance absorbs the drawing error inherent in self-declared CAR polygons. Polygon pairs that describe the same holding in two registries are merged rather than joined (Section 4).

The graph is measured with error, and the error is not random. Self-declared polygons are drawn by hand on satellite images; the 30 m tolerance recovers boundaries that were drawn slightly apart and also creates edges between parcels that do not in fact touch. Chandrasekhar et al. (2024) show that estimates of diffusion on a network can be highly sensitive to this kind of edge error. We report results under alternative tolerances and under a graph restricted to unambiguous boundaries, and we treat sensitivity to graph construction as a finding rather than a nuisance (Section 6).

Properties of the graph. Degree is right-skewed with a log-log tail slope of -1.74: median 5, mean 6.4, maximum 1,892. The graph is not connected. There are 40,709 components, and the largest contains 46.5% of parcels, so more than half of all parcels cannot be reached from any titled parcel through a chain of neighbors.

The panel covers 2014–2020, the period over which the CAR node set is complete: registration opened in May 2014 and closed on 30 June 2020. Figure A2 maps how verified title and self-declared registration accumulate across that window. CAR expands roughly tenfold, most of it between 2014 and 2016, which is why the panel cannot begin earlier.

4 — Design

Focal units are CAR parcels: self-declared registrations that never receive a title of their own. This restriction ensures that a neighbor's title is not confounded with the focal parcel's own titling event, which is known to affect clearing independently (Baragwanath and Shinde, 2026). Treatment is the first year in which a parcel sharing a boundary receives verified title. This binary definition is conventional. Section 5 shows that it is also consequential: because it is a minimum over a parcel's neighbors, it interacts with degree in ways that have nothing to do with behavior. Controls are not-yet-treated parcels. We estimate Sun and Abraham (2021) interaction-weighted event studies with parcel and municipality $\times$ year fixed effects, clustering standard errors by municipality. In plain terms, the estimator compares each parcel's clearing before and after its first neighbor is titled with the clearing of parcels in the same municipality whose first neighbor has not yet been titled, and it does this separately for each titling year so that parcels treated at different times are never used as controls for one another.

The estimating equation is

$$y_{it} \;=\; \alpha_i + \gamma_{m(i)t} + \sum_{k \neq -1} \beta_k \mathbb{1}\{t - E_i = k\} + \varepsilon_{it},$$

where y_{it} is primary-forest loss on parcel i in year t as a share of its 1985 stock, E_i is the first year a neighbor of

i receives verified title, α_i is a parcel fixed effect and $\gamma_{m(i)t}$ is a municipality $\times$ year fixed effect. The identifying assumption is that, within a municipality and year, the timing of a neighbor's title is unrelated to the focal parcel's own clearing shocks. INCRA titles in batches, and batches follow demand—the regional clustering is visible in Figure A2; what is required is that, once the municipality-year shock is absorbed, the order in which parcels within a batch are titled does not track which neighbor was about to clear. The pre-treatment leads test this directly, and the Terra Legal placebo tests it a second way.

Three construction choices matter, and each reduces the sample.

Same-property pairs are not edges. 187,955 polygon pairs have intersection-over-union above 0.5. These are single holdings recorded in two registries, 54,940 of them in both CAR and SIGEF. Before their removal, 10.7% of treated parcels were "treated" by their own duplicate.

Settlements are not parcels. The 2,221 agrarian-reform settlements in the Legal Amazon contain 350,558 beneficiary families, a median of 85 per polygon. A single polygon with a single date cannot represent 85 independent holdings, and settlements are excluded.

Already-treated status is assessed at the parcel's own entry. 63.1% of treated parcels had a titled neighbor before or in the year they registered. These parcels have no pre-period and are excluded. The exclusion is not neutral with respect to degree: it removes 1.5% of parcels with at most one neighbor and 68% of those with twenty-six or more.

5 — The degree gradient and what it measures

A conventional any-neighbor event study produces a striking pattern. The pooled effect is close to zero, while the estimated effect rises steadily with degree (Table 1). Read at face value, this resembles the comparative static that network games with strategic complements predict (Galeotti et al., 2010). We show below that it has a different origin.

Table 1 — Effect of a titled neighbor, by degree quartile

Degree quartile	Mean degree	ATT	(s.e.)
Q1	2.0	−0.0388	(0.0538)
Q2	4.5	−0.0137	(0.0296)
Q3	6.8	+0.0288	(0.0314)
Q4	13.9	+0.0961	(0.0505)
Pooled	6.4	+0.0199	*(0.0176)*

Notes. Sun–Abraham interaction-weighted event study, parcel and municipality $\times$ year fixed effects, standard errors clustered by municipality. Panel 2014–2020; 580,085 CAR parcels, of which 144,134 treated. The interaction of treatment with log degree is 0.0333 (0.0167).

Treatment is the first year in which any bordering parcel receives verified title—a minimum over a parcel's neighbors. Degree therefore enters the design in several ways before any behavioral response is involved. A parcel with more neighbors is more likely to be treated at all, is treated earlier, is more likely to have been treated before it enters the panel, and accumulates more titled neighbors afterwards (Table 2; Figure 1, Panel B).

Three restrictions separate network position from the exposure it carries.

Holding the dose constant. Adding the number of titled neighbors as a second interaction reduces the degree interaction to 0.0233 (0.0201).

Table 2 — Degree governs the exposure process

Degree	Parcels	P(ever treated)	Median first title	Excluded, already treated
0–1	69,371	0.03	2018	1.5%
3–4	200,685	0.25	2017	13.1%
7–9	128,605	0.57	2015	36.7%
15–25	21,803	0.80	2014	65.0%
26+	6,693	0.84	2013	68.4%

Notes. CAR parcels; five of eight degree bins shown. Cumulative titled neighbors by 2020 rise from 1.08 in the lowest degree quartile to 2.90 in the highest. High-degree parcels that survive the already-treated exclusion are an unusual group: parcels with many neighbors, none of which was titled before the focal parcel registered.

Using a predetermined graph. A third of a parcel's neighbors register after it does. Recomputing degree from neighbors registered on or before the focal parcel's own entry—mean degree falls from 6.05 to 4.05, and 46% of parcels change quartile—gives an interaction of 0.0226 (0.0143).

Fixing exposure exactly. Among the 79,604 treated parcels that ever have exactly one titled neighbor, where degree still ranges from 1 to 474, the interaction is 0.0045 (0.0257). The quartile pattern is no longer monotone: -0.019, -0.063, -0.052, $+0.096$ (Figure 1, Panel C).

The placebo reproduces the gradient. Terra Legal awards title on the basis of occupation before a fixed 2004 cutoff, so contemporaneous clearing cannot strengthen a claim. Its pooled spillover is null, as the statute predicts: -0.0335 (0.0440), with no significant leads. Its degree interaction, however, is $+0.0264$ (0.0232), against $+0.0333$ (0.0167) for verified title—most of the original coefficient, and not statistically distinguishable from it (Figure 1, Panel D). Whatever produces the gradient is common to both programs.

Taken together, these results locate the gradient in the exposure process rather than in behavior. One structural fact remains and is worth stating separately: titled parcels border other titled parcels on 7.31% of edges, against 3.88% under a degree-preserving null (Newman $r = +0.26$). We read this as evidence that INCRA titles in regional batches, as Figure A2 shows directly, which is also why a never-treated control group is unsuitable here—parcels with no titled neighbor are located in regions where nothing has been titled.

6 — Titled-boundary exposure

If interaction operates across property boundaries, the economically interpretable measure of exposure is not a count of titled neighbors but the share of a parcel's own boundary that adjoins titled land. A count inherits an arbitrary unit: a neighboring holding subdivided into three polygons counts three times although the boundary is unchanged. For focal parcel i in year t, define

$$E_{it}^{V} \;=\; \frac{\text{length}\big(\partial i \,\cap\, \bigcup_{j:\,\text{titled by } t} B(j, 30\,\text{m})\big)}{\text{perimeter}(i)},$$

and E_{it}^{TL} analogously for Terra Legal. The denominator is the focal parcel's own geometry, fixed at CAR registration; it is predetermined without reference to any neighbor's dates and is invariant to later subdivision of the surrounding mosaic, which is the source of endogeneity that made degree unusable. The numerator is a geometric union rather than a sum, because two neighbors can lie along the same stretch of boundary. The measure is bounded in $[0, 1]$ by construction.

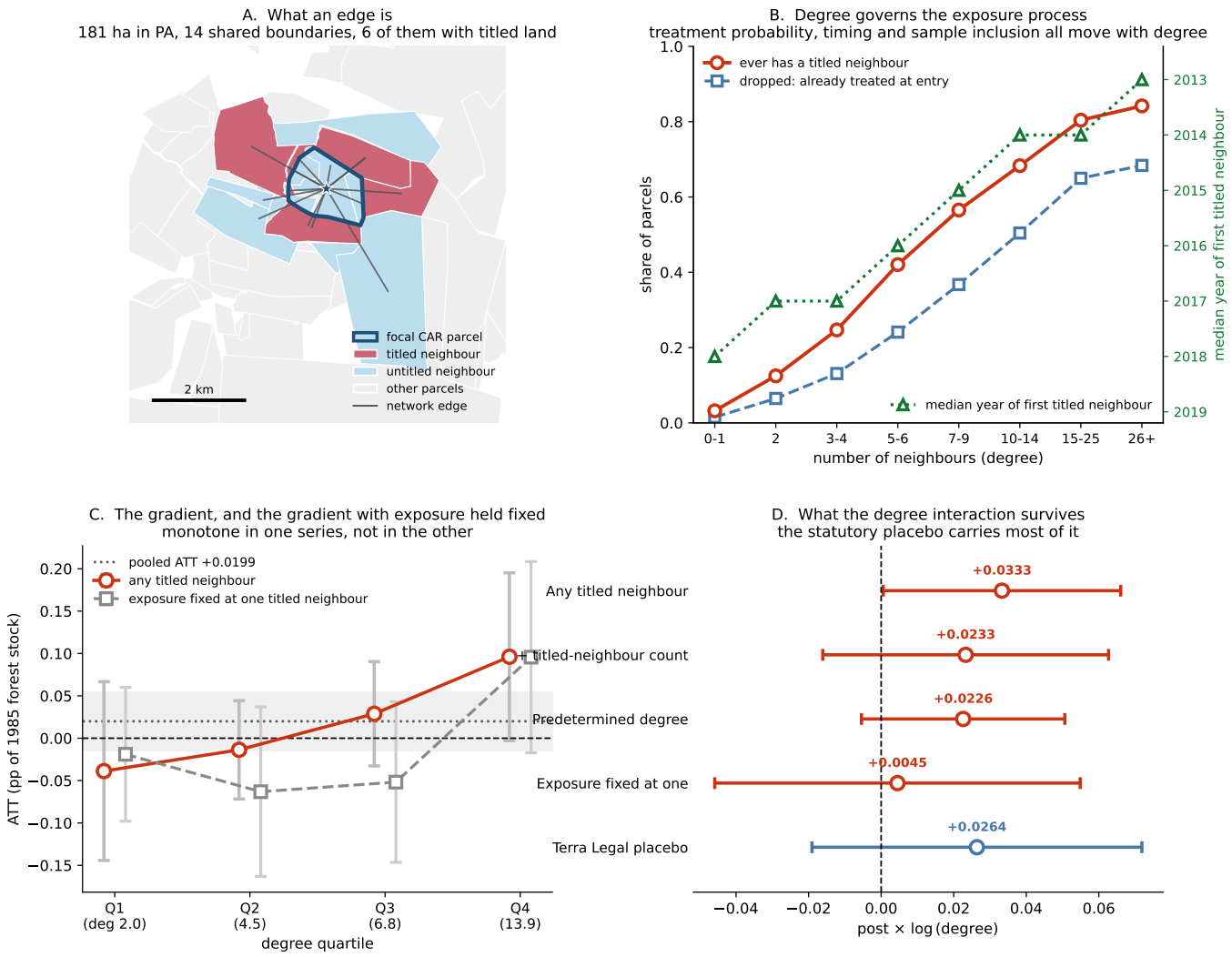


Figure 1 — The degree gradient and the exposure process

Notes. Panel A shows one neighborhood in Pará: the focal CAR parcel is outlined in dark blue, parcels sharing a boundary with it are shaded by whether they hold verified title, and grey lines are the network edges. Panel B shows that degree governs the exposure process—the probability of ever having a titled neighbor, the year the first title arrives, and whether the parcel survives the already-treated exclusion. Panel C compares the quartile ATTs of Table 1 with the same estimates when exposure is held at exactly one titled neighbor; the first series is monotone in degree and the second is not. Panel D reports the degree interaction across specifications, with the Terra Legal placebo in the same frame. Bars are 95% confidence intervals.

This quantity had to be computed rather than read from the existing graph. The stored adjacency length is the length of the lower-indexed parcel's boundary within tolerance of the other, and because node identifiers are assigned in registry blocks, all 695,779 CAR × titled edges store the neighbor's boundary rather than the focal parcel's. We recompute the focal direction for all 4,460,574 directed CAR-focal edges.

Exposure is well identified. Parcel and municipality × year fixed effects absorb 85.9% of the variation in E^V, leaving 173,472 parcels whose exposure changes within the panel and 4,910 municipality-years with within-cell variation (Appendix Table A3). This is the diagnostic that the Trase supply-zone network fails (Section 7).

Table 3 — Continuous titled-boundary exposure

	Verified title	Terra Legal	$\beta_V - \beta_{TL}$	p
Own perimeter, full graph	+0.0272	+0.0078	+0.0193	0.588
	(0.0231)	(0.0281)	(0.0357)	
Own perimeter, clean boundary	+0.0175	+0.0781	−0.0610	0.211
	(0.0317)	(0.0394)	(0.0488)	
Neighbour denominator	+0.0236	+0.0038	+0.0196	0.605
	(0.0221)	(0.0300)	(0.0378)	

Notes. Outcome is annual primary-forest loss as a percentage of the parcel's 1985 stock; parcel and municipality × year fixed effects; standard errors in parentheses clustered by municipality. 4,815,713 parcel-years, 687,959 parcels, 771 municipalities. The coefficient is the effect of moving from zero to complete titled-boundary exposure. The last two columns test $\beta_V = \beta_{TL}$ from the combined specification, using the estimated covariance rather than comparing two p-values.

Verified-title exposure gives +0.0272 (0.0231). Moving a parcel from no titled boundary to a fully titled boundary raises annual loss by 2.9% of the mean, and the confidence interval excludes effects above 7.8% of the mean. This is a reasonably precise null rather than an uninformative one. To put the magnitude in context: the average CAR parcel loses about 0.9% of its 1985 forest stock per year; the point estimate implies that a parcel whose entire boundary became titled would lose 0.93% instead, and the upper bound of the confidence interval implies 0.97%.

Table 4 — Dose response, bins fixed in advance

Titled share of own boundary	Verified title	Terra Legal
(0, 0.10]	+0.0165	−0.0587
	(0.0158)	(0.0314)
(0.10,0.25]	+0.0008	+0.0401
	(0.0136)	(0.0323)
(0.25,0.50]	+0.0262	+0.0496
	(0.0171)	(0.0274)
>0.50	+0.0227	−0.0073
	(0.0194)	(0.0243)
Joint test of all bins, p	0.466	0.038

Notes. Zero exposure is the omitted category; cutpoints were set before estimation and not revisited. Support is ample in every bin—for verified title, 200,051 / 255,928 / 325,356 / 427,948 parcel-years against 3,606,430 at zero. Fixed effects and clustering as in Table 3.

The dose response is flat and jointly insignificant, so the null does not depend on imposing linearity (Table 4; Figure 2). Two features of the comparison matter more than either coefficient. First, the placebo is not different: $\beta_V - \beta_{TL}$ is never significant, and under the clean-boundary graph Terra Legal is the larger and nominally significant

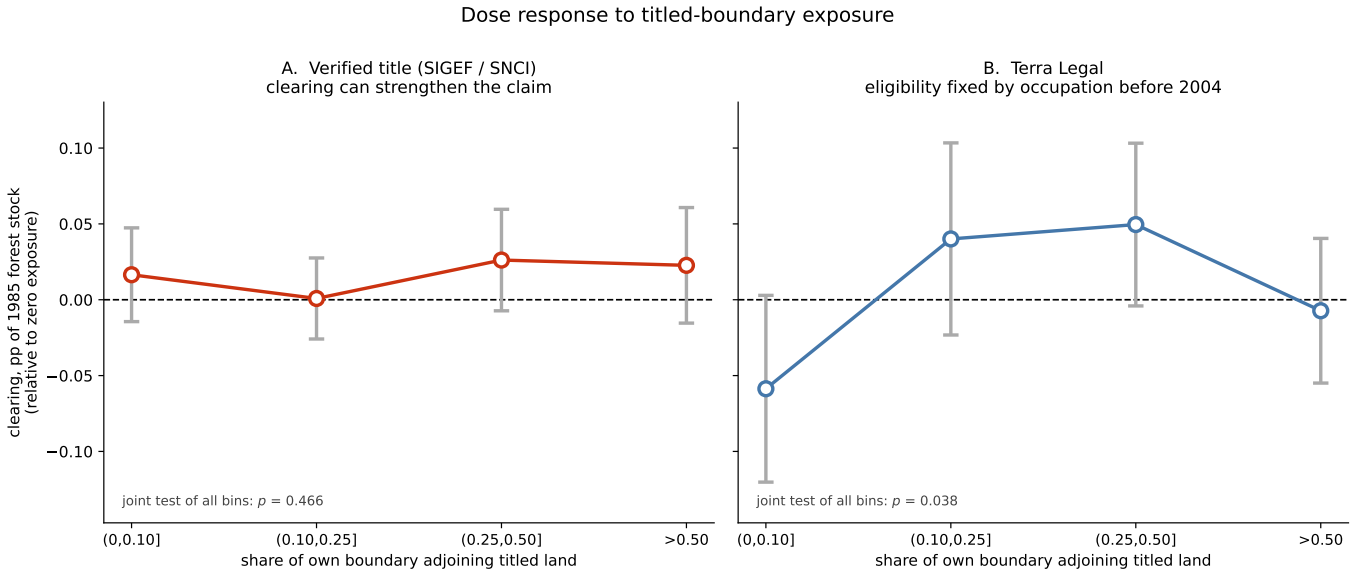


Figure 2 — Dose response to titled-boundary exposure

Notes. Identical bins and identical vertical scales, so that the two programs can be read against one another. Bars are 95% confidence intervals. Panel A is the program under which clearing can strengthen a claim; Panel B is the program under which eligibility was fixed by occupation before 2004.

estimate while verified title is not. Second, the placebo's joint dose test rejects ($p = 0.038$) while the treatment's does not ($p = 0.466$), and the placebo's profile is non-monotone—negative at low exposure, positive in the middle, zero at high. That is not a dose response. It is structure the design picks up from something other than the titling incentive.

Overlapping claims are a different relation from adjoining ones. Restricting the graph to unambiguous boundary adjacency—dropping the 29.4% of edges with non-zero polygon overlap—leaves verified title roughly where it was, $+0.0175$ (0.0317), and moves Terra Legal by an order of magnitude, from $+0.0078$ (0.0281) to $+0.0781$ (0.0394). For an overlapping pair, "boundary within tolerance of j" is not a boundary at all: if j covers i, the measure approaches i's entire perimeter, and partly captures encirclement by competing claims. Pooling the two edge types is not a data-cleaning choice but a substantive assumption, and the results indicate it is the wrong one. In the language of the network literature, the frontier is a multiplex network: two parcels can be linked by a shared boundary, by a competing claim to the same ground, or by both, and these relations carry different things. Chandrasekhar et al. (2026) show that collapsing layers of a multiplex network into one graph can misstate how anything diffuses across it. Our placebo result is an empirical instance of that warning.

In sum, boundary-intensity exposure does not isolate the productive-use mechanism. Common spatial and institutional processes remain the more plausible account of the little that the estimates show.

7 — Design diagnostics

The pooled null is stable across estimators, two of which do not rely on parallel trends (Table 5).

Two alternative networks built from Trase supply-chain data test whether formalization travels along economic rather than physical links. They differ in one respect that matters more than the estimates.

Facility catchment. Assigning each parcel to its nearest of 27,498 silos and slaughterhouses produces exposure that genuinely varies within a municipality: parcel and municipality × year fixed effects absorb 85.9% of it, leaving

Table 5 — The pooled null across estimators

Estimator	ATT	(s.e.)
Callaway–Sant'Anna, not-yet-treated	-0.0010	(0.0107)
Sun–Abraham + municipality trends	-0.0298	(0.0224)
Synthetic DiD, municipality level	-0.0011	(0.0281)
Synthetic DiD, parcel level	$+0.0593$	(0.0336)
Giant component only	$+0.0318$	(0.0293)
Honest DiD at $\bar{M} = 1$	CI $[-0.209,\ +0.196]$	

Notes. Synthetic DiD follows Arkhangelsky et al. (2021); at parcel level the donor pool is capped at 8,000 and results averaged over 40 draws, because the optimizer does not converge on the full pool. Honest DiD follows Rambachan and Roth (2023); the Callaway–Sant'Anna estimator is Callaway and Sant'Anna (2021) and the continuous-treatment extension Callaway, Goodman-Bacon and Sant'Anna (2024).

identifying variation. The estimate is $+0.0039$ (0.0037), $t = 1.06$. This is an informative null.

Supply zone. Linking municipalities that ship through a common logistics hub or sell to a common exporter group produces exposure that is fundamentally municipality $\times$ year-level. Parcel and municipality $\times$ year fixed effects absorb essentially all of its variation, so the parameter is not identified at that level; even state $\times$ year fixed effects absorb 98.8%, and the parcel-level residual that remains is generated largely by the rule excluding municipalities that contain a geographic neighbor. The supply-zone coefficient of $+0.0572$ (0.0268) under state $\times$ year fixed effects should therefore not be read as evidence of supply-chain spillovers.

The distinction deserves to be stated plainly, because the two cases look alike in a table and are not alike. The facility channel is an estimated null. The supply-zone channel is a parameter that is not identified under these fixed effects. Only the first is evidence about the world.

A final caution on Section 5. The original degree interaction is $t = 2.00$. Under randomization inference that permutes degree among treated parcels within municipality and cohort, holding the panel and the degree distribution fixed, its p-value is 0.065, and the permutation null is not centered at zero. The highest-degree quartile, which carries the positive estimate, has two of five significant pre-treatment leads.

8 — What we take from this

Exposure and network position. The gradient in Section 5 is informative, but not about strategic interaction. Because treatment is first-neighbor titling, degree simultaneously changes the probability of treatment, its timing, the post-treatment horizon over which effects are averaged, and the number of titles that subsequently arrive. Holding the number of titled neighbors at exactly one reduces the interaction from 0.0333 (0.0167) to 0.0045 (0.0257) and removes its monotonicity; a predetermined graph gives 0.0226 (0.0143); and a statutory placebo with no clearing incentive reproduces $+0.0264$ (0.0232) of it. We therefore do not interpret the raw degree gradient as evidence of strategic complementarity. The finding we do take from it is more general: in a staggered design on a network, a binary any-neighbor treatment is an exposure mapping whose meaning varies systematically with degree, so heterogeneity in the estimated effect across degree need not reflect heterogeneity in response. This also clarifies what the network-games literature does and does not deliver here. Galeotti et al. (2010) provide a comparative static on equilibrium action levels in degree; that is a different object from the marginal effect of one neighbor's title, and the two should not be equated. Adjudicating complements against substitutes requires a design that identifies the response of a parcel's clearing to its

neighbors' clearing rather than to their titling, for which the lowest eigenvalue of the adjacency matrix, not degree, is the relevant statistic (Bramoullé, Kranton and D'Amours, 2014), and for which the intransitive triads in this graph (Bramoullé, Djebbari and Fortin, 2009) are the natural instrument.

What the exposure measure settled, and what it did not. Section 6 replaces the binary treatment with the estimand this diagnosis calls for and returns a precise null that the statutory placebo does not separate from. Two questions survive it. The first is which network relation is the appropriate one: adjoining and overlapping claims move the placebo by an order of magnitude, so "sharing a boundary" and "contesting the same ground" are not one relation and should not be pooled. The second is the exposure mapping itself (Aronow and Samii, 2017). With contiguity, treated units sit in one another's comparison groups, and the question is not whether SUTVA holds but which mapping makes the estimand well defined. Butts (2023) provides an estimator for spatial spillovers in staggered designs that we have not yet implemented. The experimental literature on networks has thought carefully about exposure mappings and about what can be learned when interference is present but the network is only partly observed (Chandrasekhar and Jackson, 2025; Viviano, 2025); we would like to know which of those tools transfer to an observational setting with an administrative network.

Endogenous network formation. Degree is a property of the cadastral mosaic, which is itself the outcome of subdivision decisions. The econometrics of network formation (Chandrasekhar, 2016; Chandrasekhar and Jackson, 2025) offers ways to model how the graph came to be; we have not applied them, and we do not yet know whether a formation model would change the exposure estimates or only their interpretation.

From clearing spillovers to claim diffusion. The registry crosswalk built for this project identifies CAR holdings that subsequently appear in SIGEF or SNCI—54,940 matched pairs, currently removed from the graph as duplicates. Those matches make a parcel's own formalization an outcome rather than a nuisance. Does a neighbor's title accelerate the focal holder's own move from self-declaration to verified title? This asks directly whether claims spread, using a behavioral response far less noisy than a percentage of forest stock, and it is closer to the question the network was built to answer. A natural mechanism test accompanies it: with 30 m annual rasters and parcel geometry, clearing can be located within the parcel relative to a newly titled boundary, distinguishing a race to mark a contested edge from diffuse learning about the titling process. A second mechanism test uses soil: if the spillover operates through learning, it should be weaker across parcels with dissimilar soils (Munshi, 2004; Conley and Udry, 2010), whereas a claiming race should be indifferent to soil. These are the questions on which we would find workshop feedback most valuable.

What the null means for policy. A titling campaign's environmental cost is often discussed as if it included a contagion term. Our estimates put a bound on that term for boundary-based contagion: whatever titling does to the titled parcel, we find no evidence that it adds more than about 8% of mean clearing on the parcels next door, and the central estimate is under 3%. The result does not rule out spillovers through other channels—prices, labor markets, or the diffusion of formalization itself—and the last of these is what we intend to study next.

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A — The five land registries

Brazil records rural land in several overlapping systems. They differ in who creates the record, whether boundaries are verified, and whether the record confers a property right. These differences drive every design choice in this paper.

SIGEF (*Sistema de Gestão Fundiária*). INCRA's georeferenced certification system, in operation since November 2013 under IN 77/2013. Boundaries are surveyed by an accredited technician and checked for overlap. Eligibility for title depends on *productive use demonstrated at inspection*, which creates the forward-looking incentive to clear. 151,050 parcels in our Legal Amazon extract.

SNCI. The same certification under the earlier paper-based procedure (IN 13/03, IN 25/05). SIGEF replaced the procedure without revoking prior certifications, so SNCI and SIGEF constitute *one instrument across a change of procedure*: a parcel certified under SNCI in 2010 is a titled parcel in 2014. Treating them separately was the largest single error in an earlier version of this analysis. 13,842 parcels, median event year 2011.

Terra Legal. Law 11.952/2009, administered by INCRA over the same federal lands. Eligibility depends on *occupation before 9 November 2004*, documented from historical imagery and testimony. Current land use plays no role in the claim, which is why the program serves as the placebo. 174,678 parcels.

CAR (*Cadastro Ambiental Rural*). A self-declaration registry created by the 2012 Forest Code. The holder draws a polygon on a satellite image and submits it, with no ownership documentation, no field inspection, and no cross-check against the land cadastre. It *confers no property right*, but it is a prerequisite for SIGEF titling and provides de facto standing on undesignated public land. It is by far the largest registry, 736,680 parcels, and is the source of our focal units.

Settlements (*projetos de assentamento*). Agrarian-reform projects. Each polygon is a *collective* holding: the 2,966 in our extract contain 350,558 beneficiary families, a median of 85 per polygon. A settlement is therefore not a parcel, and admitting one as a network node would let a single node with a single date represent roughly 85 independent holdings. They are excluded from the graph and reported here only for completeness.

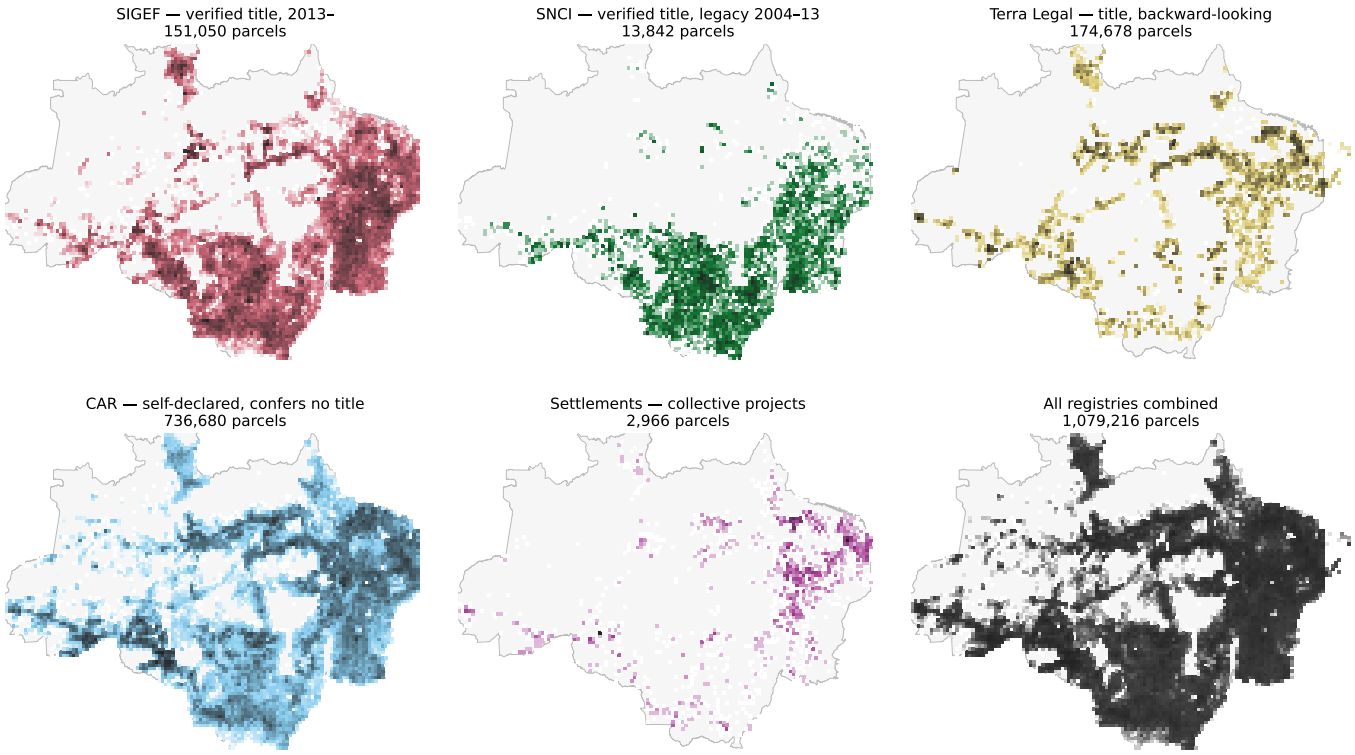


Figure A1 — Where each land registry is

Notes. Parcel counts on a common 25 km grid with a log color scale, so that registries differing a hundredfold in count can be compared. The outline is the Legal Amazon. SIGEF and CAR trace the Arc of Deforestation along the southern and eastern frontier; SNCI is concentrated in Mato Grosso and reflects the pre-2013 certification era; Terra Legal follows the Amazonas river corridor and the arc; settlements cluster in Pará and Maranhão.

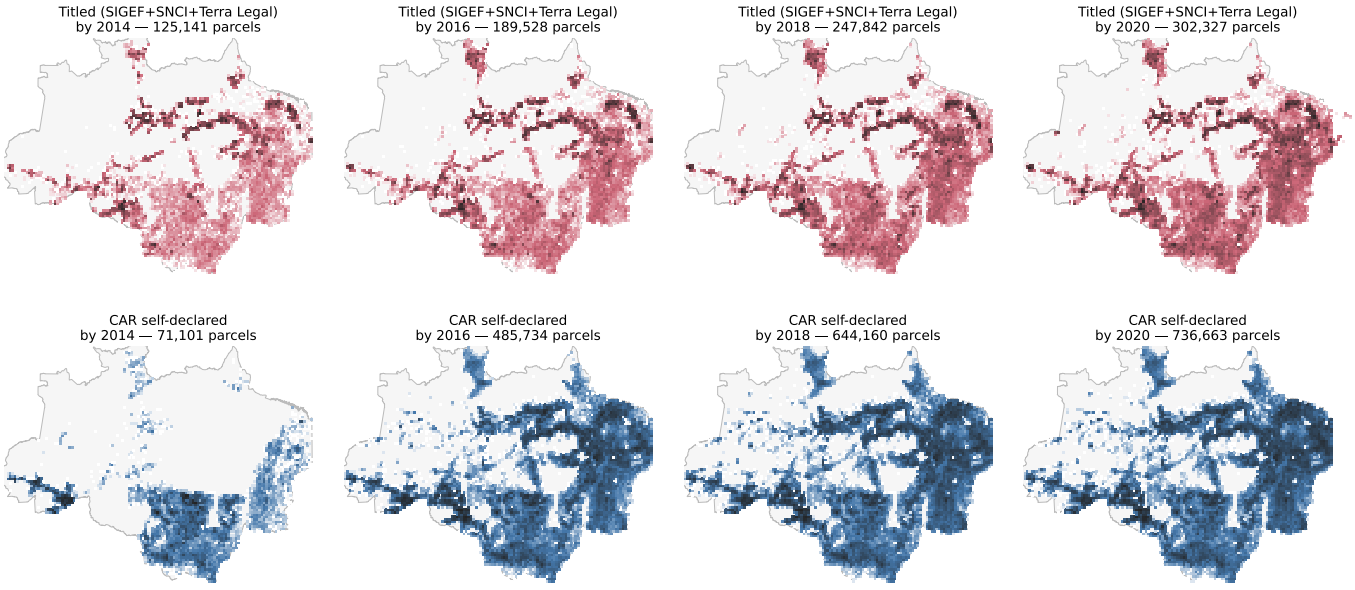


Figure A2 — The staggered rollout of titling and registration

Notes. Cumulative parcels on the same grid. Top row: verified title (SIGEF + SNCI + Terra Legal), 125,141 parcels by 2014 rising to 302,327 by 2020. Bottom row: self-declared CAR, 71,101 by 2014 rising to 736,663 by 2020—a tenfold expansion, most of it between 2014 and 2016, following the 2012 Forest Code. This staggering across space and time is the variation the event studies use.

Summary statistics

Table A1 — Parcel characteristics by registry

	SIGEF	SNCI	Terra Legal	CAR	Settlements
Parcels	151,050	13,842	174,678	736,680	2,966
Area (ha)	471.4 (1,684.9)	2,388.0 (4,709.0)	79.6 (715.4)	316.0 (1,824.7)	8,228.3 (13,018.3)
Fiscal modules	6.0 (20.6)	30.4 (69.7)	1.1 (10.6)	4.2 (27.7)	114.1 (191.5)
Neighbours (degree)	7.2 (4.9)	12.2 (12.1)	6.4 (6.9)	6.1 (12.7)	–
Neighbours, predetermined	5.6 (4.1)	1.4 (2.4)	4.3 (5.9)	4.1 (8.6)	–
1985 forest (km^2)	2.9 (21.3)	14.3 (89.9)	0.9 (33.6)	3.0 (77.4)	0.3 (0.6)
Annual loss (% of 1985)	0.5 (6.6)	0.3 (1.1)	1.4 (115.1)	0.9 (2.0)	0.3 (0.9)
Event year	2017.6 (2.5)	2011.4 (2.5)	2015.6 (2.5)	2016.2 (1.6)	2001.1 (6.5)

Notes. Mean (standard deviation) unless noted. Area and fiscal modules are from the harmonized registry database; degree is the number of parcels sharing a boundary in the contiguity graph, and predetermined degree counts only those neighbors registered on or before the focal parcel's own entry year; 1985 forest is the parcel's primary-forest stock in that year; annual loss is the mean over 2014–2020 of primary-forest loss as a percentage of that stock. "Event year" is the titling year for the titled registries, the registration year for CAR, and the project creation year for settlements. Settlements have no degree because they are excluded from the graph (Appendix A). The large standard deviations on area and loss reflect genuine right skew: the median CAR parcel is 55 ha against a mean of 316 ha.

Table A2 — The contiguity graph

Statistic	Value
Nodes (parcels)	1,076,250
Edges (shared boundaries)	3,418,006
Mean degree	6.35
Median degree	5
Maximum degree	1,892
Mean degree, predetermined	4.28
Connected components	40,709
Giant component share	46.5%
Edges with non-zero polygon overlap	29.4%
Edges with overlap above 0.1	5.0%
Same-property pairs removed	187,955
Titled–titled edge share, observed	7.31%
Titled–titled edge share, degree-preserving null	3.88%
Newman assortativity on title status	+0.260

Notes. Nodes are parcels from the four private registries; settlements and same-property pairs are excluded. An edge is a shared boundary of at least 60 m within a 30 m tolerance. Overlap is intersection-over-union between the two polygons: pairs above 0.5 are treated as the same holding registered twice and removed, but a long tail of partial overlaps remains and is counted as adjacency, so part of what degree measures is competing claims rather than boundaries. The titled–titled edge share is compared with a null that permutes title status within degree deciles, preserving the degree sequence; the observed share is nearly twice the null, which we read as evidence that INCRA titles in regional batches rather than as behavior.

Table A3 — Titled-boundary exposure: identifying variation

Exposure measure	Parcels whose exposure moves	Absorbed by parcel + muni×year FE	Residual variation
Verified title, own perimeter	173,472	0.859	0.141
Terra Legal, own perimeter	60,614	0.923	0.077
Verified title, clean boundary	142,338	0.862	0.138
Terra Legal, clean boundary	49,484	0.929	0.071
Verified title, neighbour denominator	176,250	0.854	0.146
Terra Legal, neighbour denominator	61,339	0.925	0.075

Notes. Share of each exposure measure's variance absorbed by parcel and municipality × year fixed effects, and the residual variation left to identify the coefficients in Table 3. Contrast the Trase supply zone, where the same fixed effects absorb essentially all variation and no coefficient is identified (Section 7).

Research Interests

Nilesh Shinde — Indiana University Bloomington

I study how institutions shape the use of forests, land, and water in developing countries, with a focus on the Brazilian Amazon and India. I combine administrative records, satellite data, and quasi-experimental methods to study how property rights and environmental policies change the way people use natural resources.

My previous work has studied several parts of this question. I have examined how collective property rights affect forest recovery (*PNAS*, 2023), how land regularization creates incentives to clear and subdivide land (RCI Working Paper), and how environmental registration of landholdings affects land conflict (revise and resubmit, *JEEM*). Other work studies whether conservation on private land persists after compliance is achieved. My methodological work shows that the threshold at which satellites detect forest loss can change conclusions about the effects of environmental policy (*JEEM*, 2025). In India, I study groundwater depletion and the effects of drought on farmers and courts.

Most of this work treats the parcel, household, or municipality as the unit of analysis. My current work asks what changes when land-use decisions are connected across these units. Landholders observe their neighbors, compete over land and boundaries, and respond to policies that are implemented across space. For this workshop, I am submitting a 'work in progress' paper that studies these interactions directly by constructing a network of neighboring landholdings in the Brazilian Amazon.

My paper, "Land Titling and Deforestation Spillovers: Evidence from the Brazilian Amazon," asks whether incentives created by land titling spread to neighboring landholders. Under some titling procedures, clearing can help an occupant establish a claim to land. A title granted to one parcel may therefore change the incentives of nearby claimants. I construct a parcel-level network of the Brazilian Legal Amazon that links more than one million landholdings through shared boundaries, and I match each parcel to annual satellite measures of forest loss.

The paper begins with a result that appears to be a network effect. When a parcel is defined as treated after its first neighbor receives title, estimated deforestation spillovers increase with the number of neighbors the parcel has. I show, however, that this pattern need not reflect differences in behavior. Parcels with more neighbors are more likely to have a titled neighbor, tend to receive this exposure earlier, and accumulate more titled neighbors over time. When I hold exposure fixed, the degree gradient is substantially reduced. A placebo titling program, in which current clearing cannot strengthen a land claim, produces a similar gradient.

This finding motivates a more direct measure of exposure: the share of a parcel's boundary that adjoins titled land. Unlike a count of titled neighbors, this measure does not change simply because a neighboring holding is divided into several polygons. Using this measure, I find little evidence that the clearing incentive created by titling spreads across property boundaries. The broader lesson is methodological. In a staggered network setting, defining treatment as exposure to "any treated neighbor" can make treatment depend systematically on network position. As a result, differences in estimated effects across degree can appear even when behavioral responses do not differ with degree.

This project has raised several questions that I have not yet resolved, and these questions motivate my interest in the workshop. The cadastral frontier is not a single, fixed network. Two parcels may share a boundary, overlap because of competing claims, or both. The links are measured with error, and the network itself reflects decisions about subdivision and registration. I am also extending the project from deforestation to the diffusion of formalization: does a neighbor's title make an informal claimant more likely to seek formal title?

I would especially value feedback on how to measure exposure when network position affects treatment intensity, how to distinguish diffusion from common spatial and institutional shocks, and how to study networks whose links are both endogenous and imperfectly observed. These questions are central to the next stage of the project and to my broader research agenda. The workshop would be particularly useful now, while the network design and the next set of outcomes are still being developed and can change in response to feedback.

Nilesh Shinde

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Academic Appointments

Assistant Professor, Indiana University Bloomington 2025–
Postdoctoral Research Associate, University of Massachusetts Amherst 2022–2025

Education

Ph.D., Economics and Informatics, Virginia Tech, USA
M.Sc., Development Economics, University of Campinas, Brazil
B.E., Mechanical Engineering, Savitribai Phule Pune University, India

Fields

Applied Economics; Environmental and Natural Resource Economics; Environmental Informatics

Publications

1. Primary Care, Weather, and the Missed Opportunity for Savings. *Health Affairs Scholar*, 2026, qxag225 (with Maranna Yoder, Guangqing Chi, Seth Freedman, Kosali Simon, and Sonia Angell).
2. The Cold, Hard Truth: Heterogeneous Snowpack Insulation Effects on Winter Wheat Yields in China. *Environment and Development Economics*, 2025, 1–24 (with Zhenyu Yao).
3. Beyond the Canopy: How Satellite Data Resolution Influences Policy Evaluation and Deforestation Behavior. *Journal of Environmental Economics and Management*, 134, 103219, 2025 (with Kathryn Baragwanath).
4. The Human Well-Being Potential of Tree Plantations in Sub-Saharan Africa. *Forest Policy and Economics*, 160, 103141, 2024 (with Edouard Mensah, Ange Kakpo, and Ida Djenontin).
5. Collective Property Rights Lead to Secondary Forest Growth in the Brazilian Amazon. *Proceedings of the National Academy of Sciences*, 120(22), e2221346120, 2023 (with Kathryn Baragwanath and Ella Bayi).
6. Incentives for Rural Households to Establish Tree Cover on Agricultural Land in Andhra Pradesh, India. In *Environmental Economics in Developing Countries*, Routledge, 2022 (with Stella Schons, Gregory Amacher, Kelly Cobourn, and Haripriya Gundimeda).

Working Papers

7. Unintended Consequences of a Large Irrigation Project for Groundwater Recharge (with Prashikdivya Gajbhiye and Matt Woerman). *Revise and resubmit, Journal of Development Economics.*
8. Self-Declared and Mandatory Environmental Registration of Landholdings Reduce Land Conflicts in the Brazilian Amazon (with Stella Schons, Alexandre Gori Maia, and Gregory Amacher). *Revise and resubmit, Journal of Environmental Economics and Management.*
9. Addressing the Threat of Groundwater Depletion: Does a Command-and-Control Approach Work? (with

Jayash Paudel and Sakshi Upadhyay). *Revise and resubmit, World Development.*

10. Gross External Damages of Water Pollution in the United States (with David A. Keiser, Daniel J. Phaneuf, Catherine L. Kling, Yongjie Ji, Tihitina Andarge, Mohammad Mainul Hoque, and Yau-Huo (Jimmy) Shr). *NBER Working Paper; under review.*
11. Divide and Deforest: The Perverse Incentives of Land Regularization in the Brazilian Amazon (with Kathryn Baragwanath). *Ronald Coase Institute Working Paper; under review.*
12. Thirteenth Month of a Drought Year: Agricultural Shocks and Judicial Outcomes in India (with Sampriti Sarkar). *Invited, Journal of Development Economics special issue, 2027.*
13. Forest Disease, Wildfire Risk, and Recreational Welfare: Evidence from the USA (with Calvin Bryan). *Under review.*
14. Improving Environmental Outcomes Through Investments in Local Governance: Evidence from Brazil (with Stella Schons, Kathryn Baragwanath, Alexandre Gori Maia, and Gregory Amacher). *Under review.*
15. Compliance Persistence and Conservation Permanence on Private Land in the Brazilian Amazon (with Stella Schons, Alexandre Gori Maia, and Gregory Amacher). *Under review.*
16. Disease at the Root, Deforestation at the Edge: Cassava Epidemic and Deforestation in Uganda (with Neha Deopa). *Under review.*
17. Drought-Induced Deforestation in the Brazilian Amazon (with Tengda Gong, Alankrita Goswami, and Mahmoud Osman). *Under review.*
18. Hazardous Waste Sites Alter Regional Soil Chemistry: Evidence from 80,000 Soil Tests in Tennessee (with Sampriti Sarkar). *Under review.*
19. The Agrarian Cost of Groundwater Depletion in India (with Prashikdivya Gajbhiye). *Under review.*

Work in Progress

20. Realization versus Selection: Evidence from Africa's Land Rush.
21. Land Titling and Deforestation Spillovers: Evidence from the Brazilian Amazon

Grants, Fellowships, and Awards

NIH R01 (submitted), Local Weather and Home Health Care; shared	2026
NIH R21 (submitted), Heat, Humidity, and Health; shared, $403,810	2026
NIH R21 (submitted), Sanitation, Extreme Weather, and Health; shared, $426,775	2026
USDA–NIFA–AFRI (declined), Biodiversity and Recreation Valuation; shared, $800,000	2026
USDA–NIFA–AFRI (declined), Forest Configuration and Health; shared, $649,129	2026
CLACS Faculty Research Grant, Rural Conflict Records in Brazil; $3,500	2026
FADS Grant, SiCAR Land Records in Brazil; $5,500	2026–2027
Ronald Coase Institute Research Grant, Divide and Deforest; $20,000	2025
USDA Travel Grant; $5,000	2022–2024
MapBiomas Award, 5th Edition	2023
ICTAS Doctoral Research Grant; $136,000	2018–2022

Robert S. Burruss Fellowship; $2,000 2022

William J. Dann Fellowship; $2,000 2019

NASA–LCLUC Fieldwork Grant; $2,200 2018

Central Única dos Trabalhadores M.Sc. Fellowship; $7,000 2016–2017

Presentations

Stanford–Hoover Institution; Heartland Workshop (Planned) 2026

NBER Environment and Energy Economics Spring Meeting; Social Cost of Water Pollution Workshop; TWEEDS; UMass Amherst; IU Bloomington 2025

Kansas State; IU Bloomington; AAEA Annual Meeting; Camp Resources XXX; Social Cost of Water Pollution Workshop 2025

UW–Madison; UConn; AAEA Annual Meeting; LEEP–Exeter; ASSA; UMass Amherst 2024

NEUDC; AERE; CES; Social Cost of Water Pollution Workshop; UMass Amherst 2023

AERE; AAEA; Social Cost of Water Pollution Workshop; Virginia Tech GIS Symposium 2022

Virginia Tech GIS Symposium; ICTAS Doctoral Scholar Meet 2021

Teaching

Instructor, *Environmental Economics and Finance*, IU Bloomington Fall 2025, Spring 2026

Guest Lecturer, *LEAP Executive Program*, IU Bloomington Fall 2025

Guest Lecturer, *Economics of Water Policy*, UMass Amherst Fall 2023, Fall 2024

Teaching Assistant, *Water Policy and Economics*, Virginia Tech Fall 2019

Teaching Assistant, *Natural Resource Economics*, Virginia Tech Fall 2018

Advising and Mentoring

Ph.D. students, IU Bloomington: Junying Tong (ESSI Award, 2027); Dennis Vera 2026–2029

Research interns, IU Bloomington: Farah Vianda (Public Affairs); Vinay Kamath (CS/Data Science); Rishikesh Kumar Stalin (CS/Data Science) 2026

Ph.D. student, UMass Amherst: Prashikdivya Gajbhiye (Economics) 2023–2025

Research intern, UMass Amherst: Shreyas Mishra (Economics) 2023–2024

Professional Service

Co-organizer, Workshop on Integrated Assessment Models and the Social Cost of Water Pollution 2023–2025

Refereeing

Journals: *Land Economics*; *Ecological Economics*; *Journal of Institutional Economics*; *Journal of Agricultural and Resource Economics*; *Journal of Public Economics*; *Journal of the Association of Environmental and Resource Economists*; *Science*; *PNAS*

Grants: The British Academy

Conferences: AAEA (2023–2025); AERE (2022–2025); Eastern Economic Association (2024); Social Cost of Water Pollution Workshop (2023–2025)

Professional Memberships

American Economic Association; Agricultural and Applied Economics Association; Association of Environmental and Resource Economists

Computational Skills

Programming	Python, R, Julia, C++, JavaScript, Fortran, Bash, SQL
Econometrics / ML	Python (Causal-learn, DoWhy, EconML), R, Stata, Julia
Databases	SQL (BigQuery, MySQL, MS-SQL)
High-performance	Julia, Python (Dask, Numba)
Version control	Git, GitHub, GitLab
Remote sensing / GIS	Google Earth Engine, ArcGIS Pro, QGIS, ENVI, SAGA, GRASS

Languages

English (fluent); Marathi (native); Brazilian Portuguese (basic)

References

Available upon request.