Impacts of protected areas vary with the level of government: Comparing avoided deforestation across agencies in the Brazilian Amazon

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Protected areas (PAs) are the leading tools to conserve forests. However, given their mixed effectiveness, we want to know when they have impacts internally and, if they do, when they have spillovers. Political economy posits roles for the level of government. One hypothesis is that federal PAs avoid more internal deforestation than state PAs since federal agencies consider gains for other jurisdictions. Such political differences as well as economic mechanisms can cause PA spillovers to vary greatly, even from "leakage," more deforestation elsewhere, to "blockage," less deforestation elsewhere. We examine internal impacts and local spillovers for Brazilian Amazon federal and state agencies. Outside the region's "arc of deforestation," we confirm little internal impact and show no spillovers. In the "arc," we test impacts by state, as states are large and feature considerably different dynamics. For internal impacts, estimates for federal PAs and indigenous lands are higher than for state PAs. For local spillover impacts, estimates for most arc states either are not significant or are not robust; however, for Pará, federal PAs and indigenous lands feature both internal impacts and local spillovers. Yet, the spillovers in Pará go in opposite directions across agencies, leakage for indigenous lands but blockage for federal PAs, suggesting a stronger external signal from the environmental agency. Across all these tools, only federal PAs lower deforestation internally and nearby. Results suggest that agencies' objectives and capacities are critical parts of the contexts for conservation strategies.

deforestation | protected areas | spillovers | political economy | Brazil

Protected areas (PAs) long have been the dominant conservation policy, covering nearly 1/6 of global land (1). Further PA expansions have been widely advocated, as in the Aichi Biodiversity Targets (2). There is evidence that, to date and on average, PAs have reduced the rates of deforestation inside their boundaries. Impacts vary considerably, however, including effectively zero impacts in cases, with large variations in PA impact by PAs' locations, types, and enforcement (3, 4). Such variations are clearer because rigorous evidence on PAs has grown locally (5, 6), regionally (7, 8), and globally (9). The findings from such studies, however, have not addressed the possible roles of levels of government within the implementation and net impacts of public land-use restrictions.

Moreover, interactions between such public actions and private actors are central in conservation. PAs can be complemented by private conservation as induced by, for example, ecopayments (6, 10, 11), private rights for households (12–14) or firms (15–17), and adjustments in development investment in dams and transport (18–22). Alternatively, PAs' achievements can be undermined by shifts in private land use if, for example, deforestation blocked by PAs is substituted by "leakage" to areas nearby. Responses to PAs by private actors, and by public actors who, in turn, influence more private actors, can shift rates of deforestation outside PAs from immediately alongside a PA to across the globe. Thus, even rigorous evidence about internal PA impacts could misrepresent PAs' overall impacts if PAs influence land use outside their borders (i.e., PAs have significant spillovers to private land).

For the Brazilian Amazon, a critical region for forest, we study PA impacts by government agency, considering internal impacts (within boundary) and spillovers (nearby). After discussing political economy hypotheses about federal and state agencies, and describing a spillovers dynamic, we provide evidence concerning internal deforestation impacts and spillovers from those agencies' land-use restrictions. Thus, we also add to a limited empirical literature on PA spillovers (23, 24).

Different Government Agencies

The creation and operation of PAs are public choices, although other societal actors have influence. For the Brazilian Amazon, government agencies deciding on restrictions could be federal or state agencies. Among federal actors, the agency overseeing indigenous lands differs from that for federal PAs, including important differences in their objectives, links with local development, and capacities.

Levels of government are suggested to be important in a variety of theories about public choices. Some theories about environmental decentralization posit tradeoffs from the federal point of view. Local choices allow local actors to bring useful information to bear (25). However, decisions driven by local objectives could

Significance

As protected areas (PAs) are a leading conservation strategy, understanding what conditions affect their impacts is critical. We expect different government agencies to vary in their PA locations and management. We offer a test of whether PA impacts vary across agencies. For the Brazilian Amazon, we compare deforestation impacts for 3 types of agencies: federal indigenous lands, federal PAs, and state PAs. Across states within the "arc of deforestation," internal PA impacts are higher for indigenous lands and federal PAs than for state PAs. In Pará State, federal agencies also have spillover impacts: Indigenous lands raise deforestation nearby, and federal PAs lower deforestation nearby. Agencies' objectives and capacities affect impacts, and thereby are important considerations for implementing international environmental agreements.

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give less attention to the benefits for individuals outside the locality (26–28, all with environmental goals). Such concern clearly applies to rent maximization by local officials (29), even if local maximization includes, for example, local interests in common-property resources (30).

Local citizens do altruistically demand environmental improvements outside their localities (31). However, if locals put less weight on distant environmental gains than do agencies with federal mandates, then a more local level of government will be expected to put relatively more weight on local costs. Then, we expect agencies to differ in PA creation, location, and operations (including enforcement). That, in turn, predicts differences across agencies in the internal impact and forest spillovers of PAs. Variations on such hypotheses include the interests and relative strengths of political elites in any democracy (32) and spatial property-right variations that influence cross-jurisdictional effects (33).

Location and enforcement, i.e., pressure faced and confronted, affect internal impacts and spillovers. We hypothesize that government levels, which differ in their agencies' objectives and capacities, affect the magnitudes and even signs of PAs' deforestation spillovers by sending different "signals" [signaling may include choices of PA types that differ in local rights (ref. 34 and, for Acre State, ref. 5)].

Thus, actors who understand public objectives and capacities can respond according to the agency. For instance, PA creation by a federal agency may signal a broad decision to "keep the area green," which, for the federal government, can then potentially be backed up by varied other federal actions, such as adjusting development investments. States have fewer resources to back up such visions. Further, states often have different visions. Sometimes they respond to federal actions at least cost, sending no additional "green" signals [e.g., results for the Green Municipalities program (35)].

Indigenous lands historically focused solely on the land within their legal boundaries, not outside. For decades, their inhabitants have protected lands from invasions without sending further signals, given few resources for broad engagement on the landscape (36). That may leave the forest nearby open for leakage (of ranching, logging, or mining), even when boundaries are well defended (37). It would also eliminate expectations of lowered deforestation nearby. It is not clear there exists a capacity for influence nearby (38), even if there were a will to intervene outside indigenous lands.

Different Spillover Dynamics

When spillovers from land restrictions are considered, they are often assumed to yield "leakage." Indeed, there are multiple economic reasons to expect leakage to occur if land restrictions bind. For example, land-use restrictions in a particular location could lead labor and capital to shift location, lowering the cost of production nearby and thus raising deforestation nearby (39). Alternatively, significant restrictions upon land use could lower outputs enough to raise goods' prices [e.g., for timber (40)], increasing both profitability and deforestation elsewhere, even across the globe.

Understanding land-use dynamics helps to predict where and when forest spillovers might occur. That, in turn, can help to measure or test for them. For instance, Andam et al. (4) found forest land near PAs in Costa Rica did not suffer statistically significant leakage on average. That was confirmed by Robalino et al. (41); however, in addition, further theory about possible transport and tourism mechanisms suggested spatially specific tests that revealed leakage near roads far from PAs' entrances (tourism centers).

We use "blockage" to refer to spillovers in the other direction (i.e., lower deforestation elsewhere). That too can result from economic and political mechanisms underlying private responses to PAs [in addition, there is evidence of private–private spatial interaction "blocking" deforestation (42)]. As noted, tourism is an important economic mechanism for PAs. It can yield blockage by raising net private benefits of conservation, as tourism can raise local wages and reduce poverty (43–45).

Land-use dynamics in the Brazilian Amazon can include all those dynamics, as well as other ones. Brazil's Legal Amazon includes forest frontiers that are isolated [compared with Costa Rica; e.g., while differences are more stark, with intensely developed forests in China (46) and India (47, 48)]. Some areas are especially isolated. We separate those from the more active "arc of deforestation."

Further, even for relatively densely developed Amazon regions, there is little tourism around PAs. The building of early transport links led to many responses in this region. Migrants moved to new lands. There, they lobbied for public services as in health posts and schools. That led to more private investment and more migration. All of that, in turn, led to additional road investments. Restrictions by government agencies, in contrast, can interrupt such land-use dynamics. That, in turn, can signal that future public investments will differ, shifting local expectations. We emphasize such spatially path-dependent processes, on frontiers, within the economic development leading to deforestation.

They suggest a different model for local "blockage" in spillovers, which is fully independent of tourism. Public land-use restrictions (e.g., establishing PAs) could be signals that future public investments near the PAs will be less oriented toward economic development than would otherwise be the case. That can stop and even reverse path-dependent development processes, lowering local forest loss.

Internal Impacts and Local Spillovers on Deforestation, by Agency

We examine PAs' internal impacts on deforestation and local spillovers in the Brazilian Amazon. To start, very low pressure should eliminate the potential for PA impacts (as we confirm below). Further, we hypothesize that this would also suggest no spillovers. Without impacts inside a PA, spillovers would not be expected, given neither a spur for leakage nor any clear proforest signal.

Thus, for analyzing both internal impacts and local spillovers, we separated forests "in the arc of deforestation" (Rondônia, Mato Grosso, Pará, Maranhao, and Tocantins) from forests "not in the arc" (Acre, Amazonas, Roraima, and Amapá) (Fig. 1 shows the 2 regions). Outside of "the arc," on average, we do not expect significant internal impacts or, thereby, PA spillovers.

Even within the arc, however, there existed considerable variation in the pressures that PAs faced. One reason is considerable variation across states in which restrictions were adopted, and where. *SI Appendix*, Table S1*A* shows that Rondônia, for example, had most of the state PAs and meaningful shares of the federal restrictions, leaving only 6% of unprotected land. Mato Grosso had meaningful shares of both state PAs and indigenous lands, yet no federal PAs at all. Pará had the majority of both the federal PAs and the indigenous lands, yet no state PAs. Finally, both Maranhao and Tocantins are very small, in all categories. Thus, we do not include those states in our breakdown by state.

We split our data into 2 time periods because other Amazonian policies (49) generated a fall in deforestation around 2004. For our empirical tests, we separated out 2 deforestation "regimes": 2000 to 2004 and 2004 to 2008. Any restriction can differ in impacts and spillovers across periods.

Finally, there has often existed a bias toward low deforestation pressure in the siting of PAs (50), and the restrictions that we study do differ in their sites, on average, from unprotected forest land (*SI Appendix*, Table S1 *A–E*). That could bias our estimates of impacts of PAs (51, 52).

Thus, to reduce such potential bias, we apply matching methods for "observably apples-to-apples" comparisons. We implement and

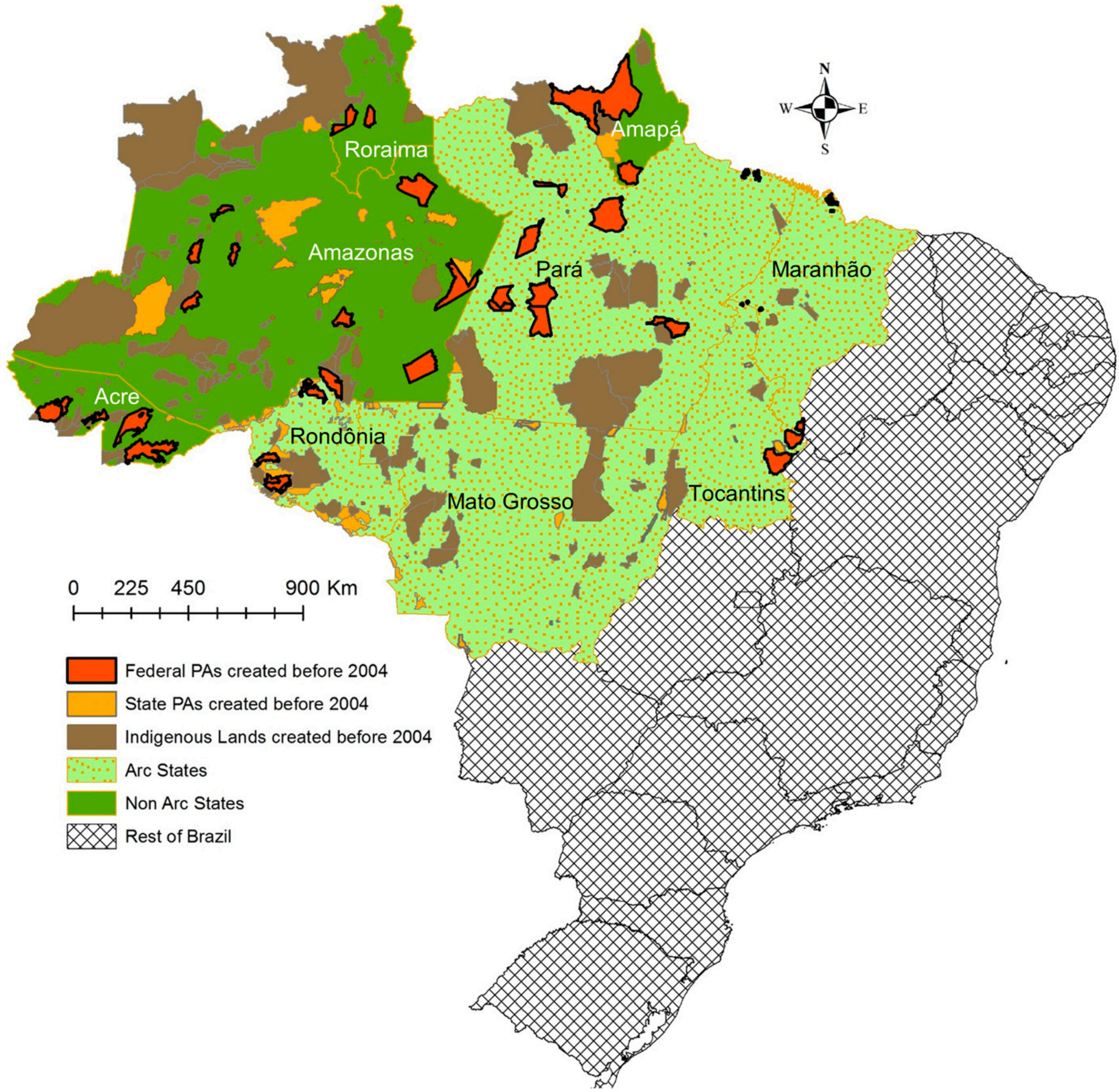

Fig. 1. PAs created before 2004 in Brazil's Legal Amazon region.

test consistency across propensity-score matching and covariate matching, plus postmatching regressions. As discussed further in *Methods and Materials*, we compare "treated" forests, inside PAs or for spillovers within 10 km, 20 km, or 30 km, with the untreated or unprotected forests more than 30 km away that are, observationally, most similar.

Results

To start, we look outside the "arc" and confirm essentially no internal impacts from any restrictions (Fig. 2; with a few very small impacts that are statistically significant, given many observations). We then show no local spillovers from restrictions: indigenous lands, federal PAs, or state PAs.

For the "arc of deforestation," we again start with the internal impacts from public land restrictions. First, we combine our 3 states of focus (i.e., Rondônia, Mato Grosso, Pará). In comparing across agencies, to start, we lump together the indigenous lands and federal PAs (i.e., the federal restrictions) to compare with state PAs. Next, we compare just federal PAs with state PAs, since the federal indigenous lands are a distinct restriction by a distinct agency. Fig. 3 (per *SI Appendix*, Table S5) shows significantly higher federal impacts, without or with clustering of SEs. This supports our hypothesis above. However, we also want to know whether the result holds up by state.

We then continue by examining the state of Rondônia, which contains all 3 agencies' policies and is the only state possessing all 3 of these types of public restrictions during these time periods. Comparing agencies, for each period, we focus here upon the internal impacts. For Rondônia, the local treated areas for spillovers within 0 to 30 km of PAs are a large share of nonprotected forests

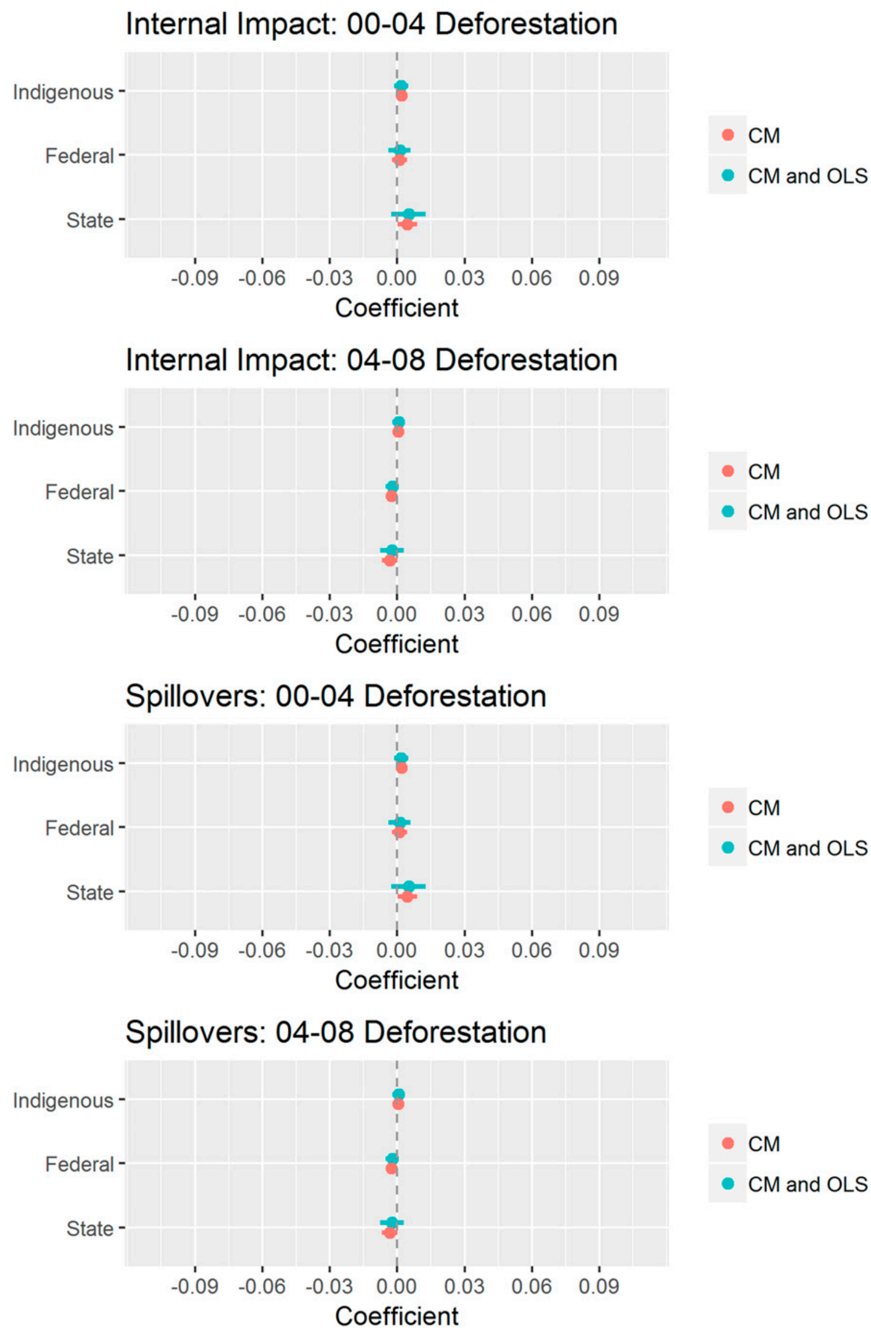

Fig. 2. Non-arc: pre-2000 PA internal and spillover (0 to 20 km) impacts on 2000 to 2004 deforestation and pre-2004 PA internal and spillover (0 to 20 km) impacts on 2004 to 2008 deforestation, both by agency. The coefficient estimates and 95% confidence intervals are for covariate matching (CM) without OLS (orange) and then postcovariate-matching OLS regressions within which we are clustering the SEs (green).

(per *SI Appendix*, Table S1*C*). Thus, for testing, we often have more treated than control points, with poor matching balance. Also, the 3 types of treated areas for local spillovers often overlap.

For internal impacts of restrictions in Rondônia, Fig. 4 (and *SI Appendix*, Tables S3*B* and S6) supports our hypothesis that state PAs have lower impacts (i.e., avoided deforestation) than federal restrictions. In fact, the state PAs have no impact, on average, with 3 of 4 estimated impacts being insignificant and the other estimate being low. In contrast, both federal PAs and indigenous lands have significant deforestation impacts for Rondônia (with estimated avoided deforestation effects of 3.7% and 6.5% for 2000 to 2004 deforestation, which is large relative to the baselines of 5% and 7%).

Thus, federal and state impacts are not the same, even in terms of statistical significance. However, that is not sufficient to claim that the impact estimates differ statistically across the different agencies. Given uncertainties in the impact estimates for each agency, we need to test whether impacts differ. The lowest rows in Fig. 4 provide a test of difference between impact estimates for each time period, specifically comparing government levels by combining the federal PAs and indigenous lands: Postmatching regressions show federal PAs' and indigenous lands' impacts are above those of state PAs. The difference between the impacts of the 2 types of federal restrictions and the single type of state restriction is not only statistically significant but also extremely similar across time periods.

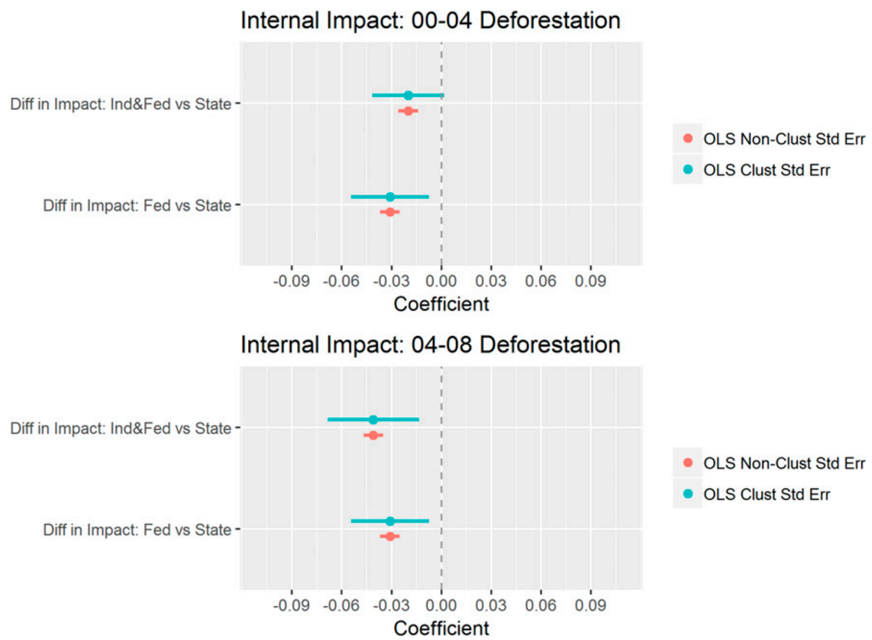

Fig. 3. Arc: pre-2000 PA internal impacts on 2000 to 2004 deforestation and re-2004 PA internal impacts on 2004 to 2008 deforestation, both by agency (indigenous lands, federal PAs, and state PAs) for Rondônia, Mato Grosso, and Pará combined. OLS tests of impact differences across agencies without clustering (red) and with clustering (green), using postcovariate-matching treated and control samples. (Carrying out these procedures using propensity score matching generates effectively the same results for our focus, the differences in impacts by agency type, even when impact estimates vary some across methods.) Clust, clustering; Diff, difference; Fed, federal; Ind, indigenous; Std Err, standard error.

Within the “arc of deforestation,” we continue with Mato Grosso. It faced high and rising pressure. For instance, Mato Grosso’s soy production and profits grew significantly across the time periods. Rising pressure was an anomaly in the region, as Amazon deforestation fell drastically after 2004 but, if anything, Mato Grosso rose in pressure and impact, given its location and its valued outputs. For Mato Grosso, we focus here upon the internal impact on deforestation, inside restrictions’ borders. For this state, the reason is that most of our estimates for spillovers simply were not significant, while the few statistically significant estimates were not consistent across the matching methods.

For the 2000 to 2004 period, Fig. 5 (and *SI Appendix*, Tables S3*C* and S6) supports the hypothesis that state PAs have less impact. The point estimate for impacts of indigenous lands is above that for impacts of state PAs. Given the uncertainty in each estimate, the third row of the 00-04 panel in Fig. 5 shows that a postmatching regression finds lower impact from state PAs relative to federal restrictions. In this case, the federal restrictions are the indigenous lands, given no federal PAs in Mato Grosso (the 2000 to 2004 indigenous avoided deforestation effect is 4.1% of a 5% baseline).

As Mato Grosso and Rondônia are the states with meaningful shares of state PAs, we next combine them to test differences in impact by level (i.e., state PAs versus both federal PAs and indigenous lands). The final row of the 00-04 panel in Fig. 5 confirms the 2000 to 2004 difference. Like the third row in the same panel in Fig. 5, this result holds with a clustering of errors by nearest PA that we believe is quite conservative (53).

In the second period, Mato Grosso’s results highlight that pressure levels are critical for impacts. Recall, very low pressure outside the “arc” implied no internal impact, or spillovers, for any agency. Here, we see that agencies also might not differ in impacts for very high pressure: Even state PAs have a 2004 to 2008 impact (not significantly different from indigenous lands). Despite this anomaly, however, our final row in the 04-08 panel in Fig. 5 clearly shows federal impacts above states’ for Mato Grosso plus Rondônia.

We conclude for the “arc of deforestation” with Pará State. It also has faced significant pressures. While it hosted both types of federal restrictions (PA and indigenous land), it had no state PAs at all. Fig. 6 (and *SI Appendix*, Tables S3*D* and S6) supports that both Pará’s federal PAs and its indigenous lands had statistically significant internal impacts (per magnitudes, 2000 to 2004 avoided deforestation effects were 5.5% and 2.2%, again large relative to the baselines of 6% and 2.6% here).

Novel for Pará is that spillovers are significant. Further, and fascinating, the spillovers vary even in their signs (Fig. 6 and *SI Appendix*, Table S4*B*). Indigenous lands generate leakage (increased 2000 to 2004 deforestation of 3% over a baseline of 6.4%), as often is assumed when such interventions restrict land uses, perhaps in particular if the agency’s focus is on defending the legal boundaries. However, the local spillovers from federal PAs differ greatly, as we find blockage from federal PAs (avoided 2000 to 2004 deforestation effect of 2.5% nearby, relative to a baseline forest loss of 6.8%).

Discussion

For Brazil’s Amazon, for 2000 to 2004 and somewhat lower 2004 to 2008 deforestation, we studied the deforestation impacts of PAs and indigenous lands: their internal impacts and local spillovers. For a single country, region, state, and period, we showed important variations across agencies.

These findings are relevant for any actor whose global commitment will be implemented locally. Such actors should consider which local actors have the best incentives to carry out agreed plans. Standard political economy suggests that the federal incentives align better with global interests. Our results supported that idea, for internal impacts, with a related variation for spillovers in Pará.

These results suggest political economic influences on choices that drove the restrictions’ impacts. They support our hypothesis for this time period and perhaps temporal averages. For the Amazon and other settings, however, it is important to allow that federal and state goals can shift over time, so that any predictions

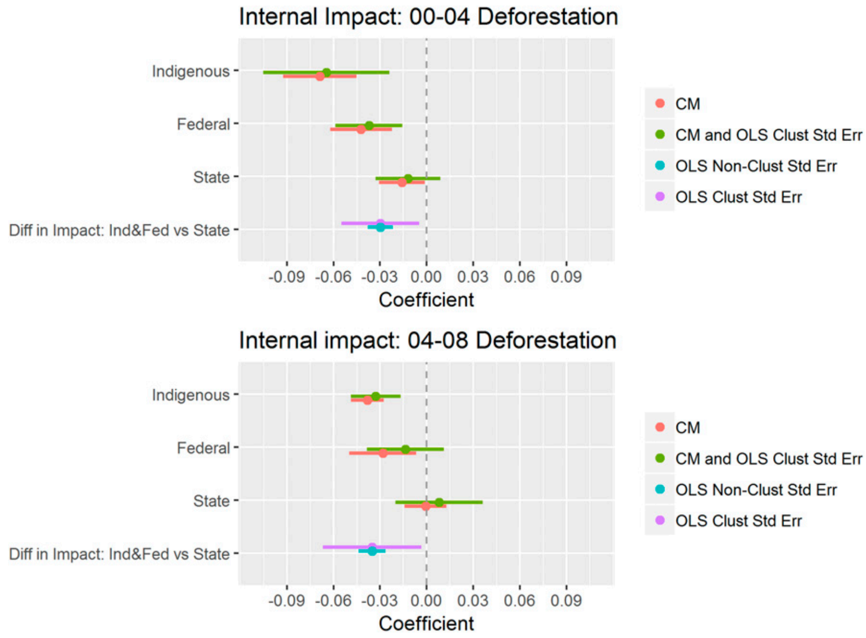

Fig. 4. Arc, Rondônia: pre-2000 PA internal impacts on 2000 to 2004 deforestation and pre-2004 PA internal impacts on 2004 to 2008 deforestation, by agency (indigenous lands, federal PAs, and state PAs). Added to covariate matching both without OLS (orange) and with OLS with clustered SEs (green), as in Fig. 2, are OLS tests of impact differences across agencies without clustering (blue) and with clustering (purple). (Carrying out these procedures using propensity score matching generates effectively the same results for our focus, the differences in impacts by agency type, even when impact estimates vary some across methods.) Clust, clustering; Diff, difference; Fed, federal; Ind, indigenous; Std Err, standard error.

based on these results must also take into account any such temporal shifts.

Most generally, we want to understand when PAs have internal impacts and spillovers, as forests provide large flows of ecoservices, locally to globally, while conservation resources are limited. Significant local leakage clearly diminishes the net benefits from protection, in terms of the forest. Significant local blockage, on the other hand, clearly enhances local net forest benefits from PAs. Of course, any avoided deforestation, internal or local, could simply be leaking to other regions. Even then, however, spillovers need to be taken into account in the evaluation of net PA impacts, including or in particular when different ecological values are assigned to different areas of forest.

Digging deeper on Pará's spillovers, they differ by federal policy (federal PAs and indigenous lands). The agency for indigenous lands is perceived to have little power to shift the local

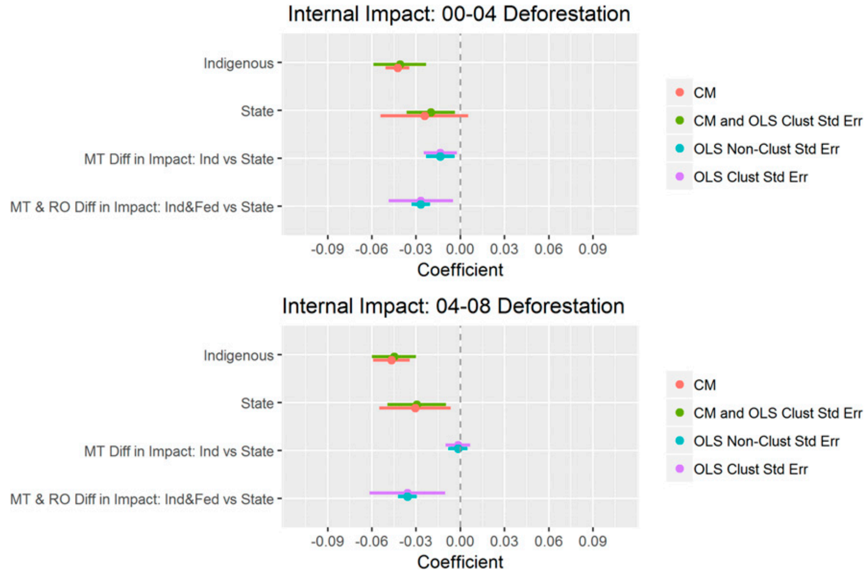

Fig. 5. Arc, Mato Grosso: pre-2000 PA internal impacts on 2000 to 2004 deforestation and pre-2004 PA internal impacts on 2004 to 2008 deforestation, by agency (indigenous lands and state PAs). As in Fig. 3, covariate matching and tests are without clustering (orange and blue) and with clustering (green and purple). (Carrying out these procedures using propensity score matching generates effectively the same results for our focus, the differences in impacts by agency type, even when impact estimates vary some across methods.) Clust, clustering; Diff, difference; Fed, federal; Ind, indigenous; MT, Mato Grosso; RO, Rondônia; Std Err, standard error.

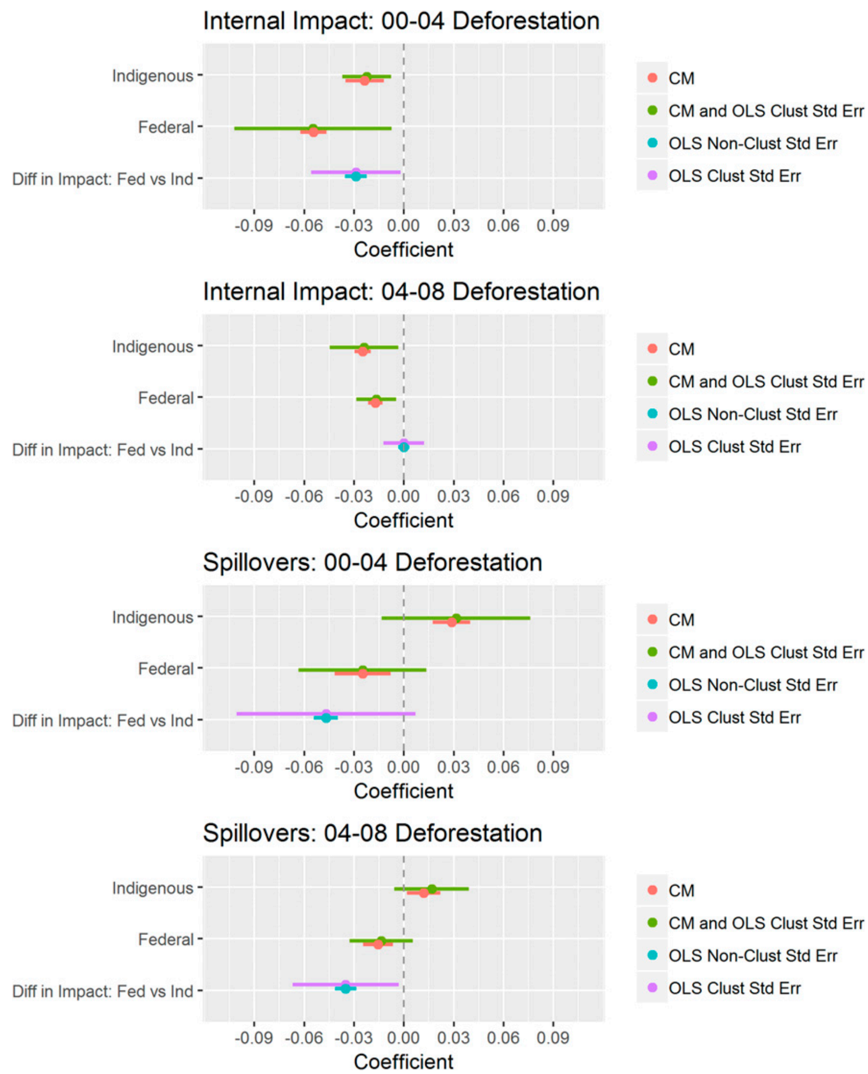

Fig. 6. Arc, Pará: pre-2000 PA internal and local spillover impacts on 2000 to 2004 deforestation and pre-2004 PA internal and spillover (0 to 20 km) impacts on 2004 to 2008 deforestation, both by agency. As in Fig. 3, covariate matching and tests are without clustering (orange and blue) and with clustering (green and purple). (Carrying out these procedures using propensity score matching generates effectively the same results for our focus, the differences in impacts by agency type, even when impact estimates vary some across methods.) Clust, clustering; Diff, difference; Fed, federal; Ind, indigenous; Std Err, standard error.

development. Its focus is on de jure rights (34), given borders, suggesting internal impacts of indigenous lands, yet not blockage, and perhaps leakage. Also, de facto political weaknesses could allow invasions, although there exist many anecdotes about fierce internal enforcement by these indigenous peoples. In contrast, however, the federal government could not only implement conservation policies but also adjust where and how development investments occur (e.g., creating and maintaining roads). State agencies have some related powers, yet their interests tend to be more about local economic development alone than are the interests of either the federal agencies or the indigenous peoples.

Concerning how best to take spillovers into account, more ideas on mechanisms could be useful. We showed that agencies certainly are part of the mechanisms. However, we would like to better understand the responses to such public land restrictions. For instance, while the blockage we show around federal PAs is consistent with our ideas (based on path-dependent development processes), making policy based on such spillover mechanisms may require further direct testing of the ideas. For instance, one could look at migration responses to new PAs, by government agency, and whether public agencies shift any other relevant actions, such as their investments in nearby roads.

Methods and Materials

Deforestation. We study 2000 to 2004 and 2004 to 2008 deforestation using *Programa de Cálculo do Desflorestamento da Amazônia* (PRODES) data for 2000, 2004, and 2008 of the Brazilian space agency, *Instituto Nacional de Pesquisas Espaciais* (INPE). Deforestation is measured as a change from forested to nonforested land cover. For each forest pixel in 2000, the deforestation outcome is binary (1 if forest in 2000 but not in 2004, 0 if forest in both years), and the deforestation outcome is the same for each forest pixel in 2004 (1 if forest in 2004 but not 2008, 0 if forest in both).

The PRODES dataset was downloaded from the INPE in raster format in the Geographic Coordinate System, South American Datum of 1969. The cell resolution of those raster data was 0.000808 decimal degrees, equivalent to 2.9088 s or 90 m around the equator once it is projected. INPE's own analyses, since 2001, have actually been conducted at a finer scale, but the results have been resampled to a resolution of 90 × 90 m to create the downloadable versions of these datasets.

PAs. The Brazilian Legal Amazon is a region of 521,742,300 ha (~5 million km^2, on the order of the size of the continental United States). By 2008, ~30% of the area was under one of the following forms of protection (within 1 of 532

PAs in total): 7% in federal conservation units (104 areas), 4% in state conservation units (114 areas), and 19% in indigenous lands (314 areas). In total, that is a very large fraction of an enormous area. Most of the PAs were created before 2000, although we include new PAs created during 2000 to 2004.

Land and Site Characteristics. Many factors affect the benefits and costs of clearing forests and, thereby, rates of deforestation. Profits from production on cleared lands can lead local stakeholders to resist the creation of PAs that limit production. Thus, all of the factors in such costs, as well as benefits, influence the siting of PAs. This implies that for comparing protected and unprotected locations to estimate impacts from PAs on deforestation, we need to control for such factors that affect both deforestation and PA siting. For example, if factors that lower profits and deforestation also raise the probability of a new PA, then any correlation of PAs with lower deforestation might not be due to causal PA impacts: PAs might not have lowered deforestation themselves, versus tending toward sites with low clearing.

Key factors include slopes, soils, and characteristics such as distance to the nearest road or city. Another relevant distance is to the forest's edge, as it is harder to access lands deep in the forest. To control for that, within our analyses of 2000 to 2004 deforestation, we use the distance to the forest edge in 2000, and for our analyses of 2004 to 2008 deforestation, we use the distance in 2004, in both cases employing the deforestation data just discussed. Digital road maps were from the Department of Geography at Michigan State University and from paper maps created by the *Departamento Nacional de Estradas de Rodagem*, an agency in the Transport Ministry in Brazil. The information about city locations comes from the 1991 Demographic Census, which also has information about urban populations. We also use metrics for biophysical conditions. We employ an index of soil quality, a continuous measure of rainfall (54), a slopes metric distinguishing "steeply sloped" from "rolling hills" (55), and an indicator of *cerrado*, a tropical savanna biome. *SI Appendix*, Table S1 *A–E* show values for subsets of our data.

Sample. To have sufficient observations to be able to break each treatment down to subtreatments by state, while also being able to use matching to select only controls that are similar to treatments, we start with a sample of 800,000 pixels. If the land-cover information available (16 categories) does not clearly indicate that the point was in forest cover, then we simply drop the observation (dropping the categories no data, nonforest, water, clouds, and residual). That leaves us with about 450,000 pixels in forest in 2000 (a bit lower in 2004) that can be examined for deforestation. That falls to about 200,000 pixels if we look only at points within the arc (*SI Appendix*, Table S1*A*). That sample of pixels is then broken down by states, plus treatments, to which we match controls.

Matching Approaches. If PAs in the Brazilian Amazon had been implemented randomly across all of the forested lands, then their impacts would be easy to estimate. We could look at the differences between rates of deforestation on treated versus untreated lands. The latter would provide unbiased estimates of the deforestation that would have occurred without PAs, since all of the other factors should cancel out.

Of course, PAs are not located randomly, and their sites do not appear to have been located across the landscape "as if by random processes" (*SI Appendix*, Table S1 *A–E*). Their locations are biased for the deforestation-relevant characteristics we measure, including distances to roads and cities. Thus, differences in deforestation between protected and unprotected lands reflect not only impacts of protection but also differences in characteristics. To reduce those differences, we used matching.

Its objective is to find an "acceptably similar" control group by matching each protected land unit with the most observationally similar unprotected units to get best "apples-to-apples" comparisons. For our analyses of local spillovers impacts, protected or treated units are not the pixels inside the boundaries of PAs but those in buffers of 10, 20, and 30 km around PAs, based on linear distances from the boundaries, while control units would be located more than 30 km away from boundaries.

For example, PAs and the forests near them may be in areas of relatively low profits in agriculture. If so, to test for spillovers, we should compare deforestation rates in and near PAs with those for low-profit areas farther away. Matching identifies such controls, using observed factors. To the extent observed factors affect both deforestation and PA siting, matching should reduce potential biases. However, this is based upon observed factors. We must also consider possible unobserved factors, as well as whether controlling for them using proxies, such as states, could improve similarities.

To define observed similarities, we used both propensity-score matching and covariate matching. Propensity-score matching links treated with untreated observations based on any unit's treatment propensity (probability to be treated). Treated observations are compared with untreated observations with similar treatment probabilities. Treatment probabilities are generated by a logit model, within which the regressors are factors expected to affect both treatment siting and the outcome (56, 57). We implement the estimator from a study by Abadie and Imbens (51), which adjusts for the presence of this initial step used for estimating propensities. Covariate matching, instead, defines its similarities using the Mahalanobis distance metric, in which the weights on factors are based on the inverse of the covariate variance-covariance matrix (58). For each approach, we report the average treatment effect on the treated observations.

With similarity defined, we choose how many untreated observations to compare with each treated observation. As that increases, the variance of the estimator will decrease because it will be based on more data. However, bias increases as we go beyond the most similar unprotected pixels to less similar units (56). The analyses we present use a single best untreated match for each treated unit (with replacement).

For our analyses, both types of matching (propensity-score and covariate) produce subsets of the untreated points that are observably more similar to the treated points, in terms of our key factors, versus differences before matching between the treated observations and the full set of untreated observations. *SI Appendix*, Table S2 *A–D* shows that each matching approach improved balance quite a lot, although never to perfect. *SI Appendix*, Table S2 *A–D* shows the standardized differences and variance ratios for the treated and untreated groups, both before and after matching. Standardized differences are differences in the treated versus untreated means, for each of the covariates, divided by the average of the variances of the treated and untreated groups for the factor in question. A perfectly balanced covariate has a standardized difference of 0 and variance ratio of 1. *SI Appendix*, Table S2 *A–D* shows all of these facts for covariate and propensity-score matching. While neither approach always has better balance, covariate matching has smaller deviations from balance, so we show covariate matching results in the previous sections.

In light of the nonuniformity of either approach possessing better balance (or even good balance), however, we also always consider the result of propensity-score matching. We note similar results, especially for our focus: the tests of differences in impacts across agencies. With regard to the issue of acceptably good balance (59, 60), we suggest that a rule of thumb for concerns about balance after matching is a standardized difference of 0.25 or higher (above which linear regressions tend to be sensitive to their specifications). Across *SI Appendix*, Table S2 *A–D*, anything over 0.15 is unusual for either of our matching approaches. Only for the indigenous lands in Rondônia are the standardized differences above 0.15 for more than 1 variable, and that is the case for each matching approach. That treatment state is the only one with any standardized difference above the 0.25 rule of thumb. Thus, we would again summarize that *SI Appendix*, Table S2 *A–D* shows significant progress, using the data for the observable factors we have utilized, in reducing differences between treated and untreated.

We must also highlight that in blending states to do this matching, over the whole Amazon region (or when blending the states for an average impact for only the entire "arc of deforestation" region), even if covariate and propensity-score matching both yielded acceptable similarity of observables, their estimates of PA impacts sometimes varied significantly. Our interpretation of this multistate result is that unobserved differences across states were particularly important, consistent with our belief that these large Amazon states vary considerably in economic and also political dynamics. Consequently, to proxy for state differences not measured in our data, we split our sample by state and repeated all of our analyses with both matching approaches (we extended this, for robustness, by also splitting each state into close to versus far from roads). With this form of "exact matching," when each matching approach generated acceptable similarity for observables, resulting estimates were quite similar. In particular, differences in agency impacts (i.e., our focus) were very similar.

Postmatching Approaches. As just highlighted, matching does not find comparisons that are exactly identical to treated pixels. As another robustness check, we ran ordinary least squares (OLS) using the matched samples from each approach to control for remaining differences in covariates, both without and with clustered SEs. When we did cluster the SEs, we did so by the nearest PA, based upon linear distances. One could instead use political boundaries. However, since municipalities are not creating these PAs, we do not think those would be appropriate units upon which to base clustering. The next largest units are the states, which are few. According to Abadie et al. (53), our clustering approach may be conservative. We found significant robustness of our results, for most but not all comparisons, to the clustering.

Our main focus in these postmatching OLS regressions is differences in impacts across agencies, for which we generally get consistent (in each method) and significant estimates of agency effects. To test differences in the estimated impacts, across agencies, we used the matching subsamples (including treated observations, controls, and respective weights). We merged these samples as a function of which agencies we want to compare (e.g., if comparing federal PAs with indigenous lands, we merged the matched datasets used for impact estimates), and then ran a deforestation regression on covariates, a treatment and its interaction with an agency. The latter can show different impacts.

However, as Ferraro et al. (23) noted, while that approach usefully compares each treatment with similar untreated observations, the resulting comparisons could focus upon different sorts of land. For instance, if state PAs were on low-pressure lands but federal PAs were on high-pressure lands, differences might be due not to state and federal PAs functioning differently on similar land but, instead, to the fact that these treatments have different sorts of locations. Thus, following the useful suggestion by Ferraro et al. 23, we also directly compare treatments, using covariate matching. We use the state PAs as the treatments and federal PAs as the controls for Rondônia and Mato Grosso. We use federal PAs as the treatments and indigenous lands as the controls for Pará. Matching in this way will identify where treatments are located on similar lands, which should remove the effects of the dissimilarities in locations to estimate whether these agencies have different outcomes on similar lands. In this fashion, our direct comparisons of treatments confirmed all of the above comparison results (*SI Appendix*, Table S7).

We used the software Stata for all matching computations and the commands "teffects psmatch," "teffects nnmatch," and "tebalance" to compare the balances between unmatched and matched samples.

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