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Impacts of conservation incentives in protected areas: The case of Bolsa Floresta, Brazil

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Impacts of conservation incentives in protected areas: the case of Bolsa Floresta, Brazil

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Abstract

Conditional incentives are a promising complementary approach to conserve tropical forests, for example, in multiple-use protected areas. In this paper we analyze the environmental impacts of *Bolsa Floresta*, a forest conservation program that combines direct conditional payments with livelihood-focused investments in 15 multiple-use reserves in the Brazilian state of Amazonas. We use grid-based data, nearest-neighbor matching, and panel data econometrics to compare three forest-related program outcomes – deforestation, degradation, and fires – of participating and non-participating reserve areas. Forest threats were low before and after treatment, because the program prioritized low-pressure sites. Thus, we find significant but small additional conservation effects from the implementation of the program. Notwithstanding, treatment effects are relatively larger in areas with higher deforestation pressure and higher potential agricultural income. Our findings add to the growing body of evidence showing that adverse spatial targeting of conservation incentives, i.e. disproportionately enrolling low-pressure sites, is a prime cause for the low additionality found in rigorous impact evaluations of many existing initiatives.

JEL classification codes: O13, Q15, Q56, Q57

Keywords: deforestation, protected areas, payments for environmental services (PES), spatial matching, Amazonas, Brazil

1 Introduction

Protected areas (PAs) play a key role in preserving biodiversity and natural landscapes, store forest carbon, and provide other environmental services. Worldwide, PAs cover 15% of land areas (UNEP-WCMC and IUCN, 2016). PAs were shown to be moderately effective in reducing deforestation within their boundaries (Joppa and Pfaff, 2010b; Nelson and Chomitz, 2011; Cuenca et al., 2016). Multiple-use PAs explicitly permit human presence and environmentally benign activities. Multiple-use protection is thus sometimes combined with incentives to promote win-win outcomes in terms of environmental conservation and local livelihoods. Some evidence suggests that multiple-use PAs have performed comparatively better than strict protection at global scale (Nelson and Chomitz, 2011; Porter-Bolland et al., 2012).

During the 2000s, Brazil’s environmental governance reform was upheld as a conservation success story as deforestation rates fell by 80% in a few years. This effect has been attributed primarily to innovations in monitoring and enforcement as well as a massive expansion of PAs. This paper contributes to disentangling which conservation instruments were most effective during Brazil’s “conservation era” by focusing on the success factors of PA-based conservation. Whether PAs are effective in reducing forest loss, their success allegedly depends on their exposure to anthropogenic pressure, park management, and forest governance at national level (Herrera et al., 2019; Geldmann et al., 2015).

This paper provides the first causal evidence on whether complementary conservation incentives can enhance the effectiveness of PAs. It examines an innovative large-scale effort to provide such incentives in multiple-use PAs: the *Bolsa Floresta Program (BFP)* in the Brazilian state of Amazonas. The BFP offers Payments for Environmental Services (PES) to households and communities in 15 of the state’s sustainable-use PAs to encourage the conservation and sustainable use of forests.

We investigate the BFP’s forest conservation impact using a 5-by-5 km grid-cell approach across 53 reserves, 265,000 km², and 12 years. We use spatial matching techniques to identify counterfactual sites and estimate treatment effects conditional on cell and year fixed effects. Heterogeneous impacts across our study region are analyzed using various forest pressure gradients. Our results document that yearly forest losses decrease on average by about 10% after the implementation of BFP’s conservation incentives within PA. Conservation effects gradually increase in post-treatment years (2008–2015) and are highest in locations close to the treated communities. At the same time conservation impacts tend to be higher in areas with relatively more pressure on forests. Nonetheless, robust counterfactual estimates translate to only 856 ha of avoided deforestation in absolute terms. Higher conservation impacts could potentially have been achieved if compliance were more effectively monitored and PES were targeted to areas with higher deforestation

pressure (i.e., spatial targeting).

The study contributes to the scientific literature on forest conservation and provides valuable insights to practitioners. It relates to the growing literature on the effectiveness of forest conservation policies using rigorous empirical evaluation techniques (Börner et al., 2020; Joppa and Pfaff, 2010a; Burivalova et al., 2019). More specifically, it speaks to both the literature on protected areas (Baylis et al., 2016; Geldmann et al., 2013; Oldekop et al., 2015) and PES (Lambin et al., 2014; Börner et al., 2017) by looking at the combination of a command-and-control instrument with conditional incentives. By investigating the heterogeneous conservation effects of this policy mix we add to the understanding of what drives variations of impact across instrument categories observed in the literature (Wunder et al., 2020). Theoretically, PES can come with the benefit of increasing the Pareto efficiency of regulation-based conservation, such as nature protection, via conditional compensation (Wegner, 2016). However, designing cost-effective PES schemes tends to be more demanding and knowledge-intensive than conservation that relies on conventional command-and-control measures (Engel, 2016). Correspondingly, the effectiveness of PES schemes implemented around the world is remarkably context-dependent and spans from the lowest to the highest end of the effectiveness range in the forest conservation toolbox (Wunder et al., 2020). Hence, the study's main contribution is to provide evidence for the theoretical claim that PES effectiveness hinges critically on a combination of spatial targeting criteria, such as high-value of ecosystem services, and substantial threat from non-forest land-use alternatives (Drucker and Ramirez, 2020). Our findings suggest that little to no additional conservation gains can be obtained from implementing PES in PAs that are located in areas under low deforestation pressure (Pfaff et al., 2014).

The remainder of the paper is structured as follows. Section 2 introduces the literature on conservation incentives for PAs. Section 3 presents the case study, while section 4 describes the data and the targeting process. Section 5 outlines the empirical approach. Section 6 presents the results, while section 7 discusses and concludes.

2 Conservation policy mixes: Brazilian Amazon and beyond

Annual deforestation in the Brazilian Amazon fell by some 80% from 2004–2012 (from 27,000 to 4,500 km²) and by 60% in Amazonas State (1200 to 520 km²; INPE, 2019). This is attributed to both economic and political factors (Canova and Hickey, 2012; Hargrave and Kis-Katos, 2013). The Plan to Combat Deforestation in the Amazon, launched in 2004, played an important role in establishing a regional forest-cover monitoring system, increasing the budget of the federal environmental enforcement agency (IBAMA), ex-

panding the PA system, and promoting the standardization of land registration cadasters (Arima et al., 2014; BenYishay et al., 2017; Assunção et al., 2012; Maia et al., 2011). The combined impacts of these measures have been documented in quasi-experimental evaluation studies (Canova and Hickey, 2012; Hargrave and Kis-Katos, 2013). After 2012, deforestation partially rebounded (INPE, 2019), mainly due to legal and political changes (Sengupta, 2018; Pereira et al., 2019).

Brazil's PA system, covered 2.2m km² (24% of Brazil's Legal Amazon) in 2012 — a 11%-point increase over the coverage in 2004. It has made an important and well-documented contribution to reducing deforestation (Soares-Filho et al., 2010; Pfaff et al., 2015a). PAs are particularly effective in areas with high deforestation pressure, in areas close to roads and cities (Nolte et al., 2013; Pfaff et al., 2015a), and in the period immediately following their establishment as PAs (Pfaff et al., 2015b). Unsurprisingly, leakage effects are larger in high-pressure areas (Amin et al., 2019). Outcomes appear to depend less on the quality of management (Nolte and Agrawal, 2013) than on PA type. Strictly conserved PAs were shown to be more effective than multiple-use ones (Nolte et al., 2013) - except in the state of Acre, where multiple-use reserves avoided comparatively more deforestation (Pfaff et al., 2014).¹

Although PAs effectively reduce deforestation, outcomes are subject to political pressure. Command-and-control policies, whether implemented through increased law enforcement or PA establishment, typically impose uncompensated economic costs on local actors, who thus tend to oppose them. Adding compensatory incentives to the pre-existing disincentive regime can curb local welfare losses and make conservation more politically acceptable (Börner et al., 2010; Santiago et al., 2018; Nepstad et al., 2014). However, the welfare and equity outcomes of such policy mixes are highly context-dependent (Börner et al., 2015).

PES are conservation incentives made conditional on land stewards adopting environmentally-friendly land-uses, while compensating forgone income from deforestation and degradation activities (Engel et al., 2008; Wunder, 2015). PES are widespread, especially in Latin America (Alix-Garcia and Wolff, 2014; Börner et al., 2017). Brazil has been a latecomer in PES development, but recently developed numerous programs, especially watershed schemes in the Atlantic Forest biome and carbon initiatives in the Amazon (Pagiola et al., 2013).

A key design challenge of PES is adverse self-selection. Landholders who were likely to protect forests even in the absence of payments may be most eager to participate, but their enrollment will not result in additional conservation (Persson and Alpizar, 2013). Samii

¹Brazil's National System of Protected Areas includes multiple-use reserves (áreas de uso sustentavel) and strictly protected areas (área de proteção integral). Multiple-use reserves allow traditional resident populations to pursue forest-benign livelihood strategies. Numerous sub-categories of reserves exist, such as Sustainable Development Reserves (RDS), Environmental Protection Areas (APA), Extractive Reserves (RESEX), and State Reserves (FE).

et al.'s (2014) early systematic review of rigorous forest-based PES evaluation studies found relatively low PES additionality, but their demanding methodological filters selected less than a dozen studies, all of which were from Costa Rica or Mexico. Wunder et al. (2020) reviewed a larger, less geographically biased sample, and found slightly more evidence of additionality. In the first-ever randomized evaluation of a PES scheme in Uganda, Jayachandran et al. (2017) found that payments temporarily reduced deforestation by more than half, followed by a rebound after the program ended (World Bank, 2018).

In some cases, PES has already been used within multiple-use PAs, for example in the national PES programs of Costa Rica and Mexico. Here, residents within PAs received PES explicitly to compensate for conservation opportunity costs (Pagiola, 2008). In Mexico, PES schemes and PAs both exhibited a complementary conservation effect within PA buffer regions (Sims and Alix-Garcia, 2017). Other PES interventions were designed specifically for PAs, as was the case in the Monarch Butterfly Reserve in Mexico (Honey-Rosés et al., 2011). Moving beyond land-cover protection, a Cambodian PES program paid local communities to protect the nests of threatened bird species (Clements and Milner-Gulland, 2014). PES inside multiple-use PAs may also motivate local people to report violations or environmental encroachment by external resource users, thus assisting in monitoring and control (Robinson et al., 2010).

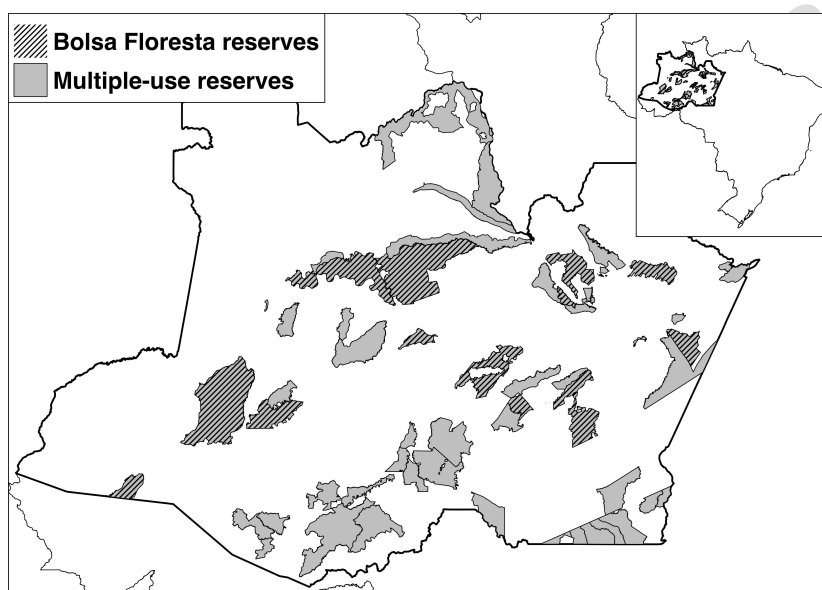
PA-cum-PES pilot impact evaluations have so far shown fairly encouraging results. Honey-Rosés et al. (2011) found large impacts in the Monarch Butterfly Reserve, but could not separate the effects of each component. Clements and Milner-Gulland (2014) found that establishing PAs in Cambodia had reduced deforestation by about half. Adding economic incentives (including PES) reduced it by another half, while increasing the well-being of participants. Montoya-Zumaeta et al. (2019) found that a watershed PES in Moyobamba (Peruvian Amazon), implemented within part of a simultaneously created multiple-use PA had significant conservation impacts.

3 The Bolsa Floresta program

The BFP started in 2008, with the dual aim of protecting multiple-use PAs in Amazonas State against deforestation pressures and in order to increase welfare among local residents (Börner et al., 2013; Viana et al., 2009). The Bolsa Floresta program is run by the Sustainable Amazonas Foundation (Fundação Amazonas Sustentável, FAS), a non-governmental organization co-financed by the state of Amazonas and the Amazon Fund and supported by multiple other domestic and foreign private donors over the years (e.g. Bradesco Bank, Coca-Cola Brazil, Marriott, Samsung, Petrobras, Lojas Americanas). The program was rolled out between 2008 and 2010, in 15 multiple-use reserve areas. Covering over 10 mil-

lion ha of forest area, BFP is one of Latin America's area-wise largest PES programs (Figure 1), with over 9,600 households currently enrolled and participation rates between 70 to 100%; (FAS, 2013; FAS, 2018; Newton et al., 2012).

Figure 1: Protected areas in Amazonas State and the Bolsa Floresta program



The main component of BFP, *BFP Family*, makes payments of BRL 50 monthly to households who have lived in a PA for at least two years and who sign a commitment to limit deforestation and adhere to additional sustainable land-use practices.² Land-use commitments are variable across reserves, but typically only marginally more restrictive than the PA rules (Börner et al., 2013).³ BFP also has three other components: *BFP Association* mostly subsidizes transport costs to strengthen community organization; *BFP Income* invests BRL350 per household/yr in alternative sustainable production;⁴ and *Bolsa Floresta Social* spends BRL350 per household/yr on education and health. Through its four main components the program invested BRL53 million (USD15.88 million) in payments alone in the years 2008 to 2015 (cf. Figure A1).⁵

In summary, BFP combines PES with additional support measures that are well-known from integrated conservation and development programs (ICDP).⁶ Participating house-

²BRL50 equals USD14.98, based on average 2015 exchange rates (Federal Reserve Bank of St. Louis, 2018). Average monthly income is estimated at BRL410 to BRL560 (Newton et al., 2012; Börner et al., 2013).

³E.g. BFP participants in the Juma and Uatumã reserves, must commit to follow PA rules, become a member of the reserve association, convert only secondary vegetation, implement defined forest protection measures, and enroll their children in schools (Börner et al., 2013).

⁴The most frequent *BFP Income* investments are in poultry, nuts, natural oils, agroforestry, fruits, and tourism (Newton et al., 2012).

⁵Deflated to the year 2015.

⁶Appendix A.1 describes in more detail the BFP components and A.2 lays out an impact framework of conservation.

holds thus receive immediate benefits from direct payments plus, theoretically, future gains from higher productivity and improved collective goods via community support. Additionally, the program stimulates peer-pressure by encouraging pro-active reporting PA rule violations and partially conditioning collective payments on individual compliance.

4 Data and selection

4.1 Data and descriptive trends

We use two spatial levels for our analysis: a reserve-level and a grid-cell-level. Reserve-level data on 33 state-administered multiple-use units in Amazonas help us to understand the BFP's selection process. To capture spatial heterogeneity in deforestation pressure and treatment effects we use grid-cells as units of analysis (cf. Andam et al., 2008; Nelson and Chomitz, 2011). Specifically, we intersect a grid of 0.045 degrees (5-by-5 km at the equator) with reserve boundaries.⁷ We exclude fractional units at the edge of reserves with areas below 1.25 km² (cf. Figure A2).⁸ Due to missing information and measurement error (e.g., cloud coverage) we exclude 10% of observations, resulting in 11,425 grid cells across 15 treated and 38 untreated reserves.

Our empirical analysis combines time-variant, remotely sensed data on forest loss and forest quality with administrative information on reserve areas, annual information on treatment status, and administrative data on districts. Spatial information is aggregated at the reserve-level and at the cell-level. Non-spatial attributes to our cells database are cross-linked via the spatial location of the cell-centroids within administrative entities (Appendix A.3 provides details on the data generating process).

Our main dependent variable is annual deforestation. Using the database of the Brazilian National Institute of Spatial Research (INPE), we aggregate forest losses, measured at a 30-meter resolution, to the reserve and grid-cell level. Forest losses are measured as recent clear-cut forest patches, with annual data from 2004 to 2015. The clear-cut forest measure may underreport forest degrading activities, which do not result in complete forest loss. We thus complement our main outcome with INPEs data products on forest degradation (2007–2013) and forest fires (2004–2014) (cf. Figure A3). It is important to note that deforestation rates in the study area are generally low, and the state-administered reserves only account for 0.07% to 0.5% of total forest losses in the Brazilian Amazon.

The BFP started its conditional payments scheme in 2007/08, when nine multiple-use

⁷This cell size is a compromise between spatial precision and spatial autocorrelation (Avelino et al., 2016). Using a lower spatial resolution would fail to capture spatial heterogeneity, whereas a higher spatial resolution risks creating redundant observations and spatial autocorrelation.

⁸Keeping the remaining fractional units avoids a potential bias from loss of information or misattribution of treatment at reserve borders.

reserves were enrolled. Another five joined in 2009, and one more in 2010 (cf. Table A1).⁹ BFP reserves had lower average annual deforestation rates (3 km²/yr) after the program start than non-BFP reserves (8 km²/yr), so a naïve comparison would have led to an overestimation of BFP impacts (cf. Figure A3). However, the BFP reserves already had lower annual average deforestation rates (2 km²/yr) before treatment than non-BFP reserves (4 km²/yr).

Due to the non-random nature of the program roll-out, we approximate treatment and deforestation probabilities using spatial and administrative data on forest conservation, economic, and political factors. Forest conservation characteristics are considered, including pre-treatment deforestation trends and fire incidents, initial forest cover, secondary vegetation, non-forest area (swamps and bush land areas), and water bodies (lakes and rivers). Further, we control for settlement projects and their spatial overlap with reserve areas. Settlement projects can have detrimental impacts on forests and conservation efforts, despite efforts to establish “sustainable settlements” in the Brazilian Amazon (Ludewigs et al., 2009). We measure the quality of PA management with an indicator equal to one if reserve areas adopted some form of forest management plan by 2007. We consider the conservation context measuring the distance to the nearest strictly protected reserve and indigenous territory as well as each cell’s distance to its own reserve border. Further, we control for reserve size and age.

The economic environment is considered at the cell-level by including distances to roads, rivers, and district capitals. Economic activities are controlled for at the cell level with remotely sensed land use classes - namely agricultural, mixed occupation, secondary vegetation, pasture, and urban land classes. Proxies for economic pressure on reserve areas are measured at district level, and include official statistics on population density, GDP and agricultural GDP per capita, the percentage share of farmland, the share of small farms, the average number of tractors per farm, and the average price of timber. Furthermore, we index land speculation potential at the cell-level, based on Bowman et al.’s (2012) spatial model of extensive cattle profitability. Since the BFP was co-founded by the state of Amazonas, programs roll-out may have been affected by political incentives, for example, to hand out benefits to potential voters or to support district administrations affiliated with the state governor. We use data on the party affiliation of each district’s mayor in 2007, and set a dummy for state-party affiliation equal to one if the district mayor is a member of the ‘Party of the Brazilian Democratic Movement (PMDB)’ — the party of the former state governor (Eduardo Braga). At the reserve area level, we set a dummy equal to one if at least one of the districts crossing the boundaries of the reserve area aligns with this ruling state-party political affiliation.

⁹In 2014, RDS Puranga Conquista was separated from APA Rio Negro, and FAS (2018) reports 16 reserves within their portfolio. We only consider the original 15 reserves in our analysis.

4.2 Bolsa Floresta’s targeting

Forest conservation initiatives often aim to fulfil multiple explicit and implicit goals beyond conservation, such as equity, poverty alleviation, or political patronage. Before we assess the overall program impacts across all reserves (section 6), we investigate the BFP’s selection process, considering potential economic and political motivations that could have influenced the selection of 15 out of 53 conservation units within Amazonas. We use the following linear probability model:

$$PES_r = \alpha + \mathbf{X}_r\beta + \varepsilon_r \quad (1)$$

PES_r indicates the BFP, with values of one indicating treated reserves and values of zero indicating control reserves. We restrict this sample to state-administered reserves, which were exclusively targeted by the program. \mathbf{X}_r comprises an array of covariates on forest conservation, economic, and political factors. To control for selection on outcomes, we regress participation on the remaining forest cover and the average deforestation growth rate prior to the program (2005–2007). To capture scale effects in the selection process, the logarithms of reserve size and years since creation are included.¹⁰ Well-administered PAs establish official forest management plans and thereby reduce the risk of excessive logging. As selection could have been sensitive to administrative quality, we control for the formal presence of management plans with a dummy equal to one if a plan existed prior to program start. Settlements are generally associated with in-migration and deforestation and BFP might have tried to countervail the negative conservation effect of unconditional support to settlers within reserves (Fearnside, 1987; Schneider and Peres, 2015). With geo-localized information on federal settlement projects, we are able to control for the share of reserves covered by federally supported settlements.

Economic prosperity can affect forest conversion to alternative land-uses and thus also motives for program placement. We control for the economic context using a weighted average of GDP growth from 2005 to 2007 in the surrounding districts. Furthermore, we index expected returns to forest conversion using Bowman et al.’s (2012) land speculation map, which indicates whether or not a particular forest area would be potentially profitable if converted to pasture. Program designers could have tried to balance travel costs against higher conservation values in remote areas. We control for this using the logarithmic travel time to the closest city from Schielein and Börner (2018). Political incentives are accounted for using a dummy for state-party affiliation for reserves residing in districts with mayors of the same party as the state governor.

Table 1 presents the results of estimating equation 1. All continuous variables are stan-

¹⁰Throughout the paper, we use the inverse hyperbolic sine, which is asymptotically equivalent to the logarithmic transformation.

standardized for comparability. Column 1 includes covariates on environmental characteristics, column 2 adds economic variables and column 3 adds political factors that might have played a role in the selection process. With only 33 state-administered reserves in Amazonas and more than six covariates we tend to overfit the data. Nonetheless, coefficients are fairly stable, and an analysis on the variation inflation factors shows a low level of multicollinearity (cf. Table A2). These results cannot be causally interpreted, but provide a general overview on the setting of the BFP.

Results indicate that BFP reserves were already on a different path before 2008. Selected reserves are larger and located in remote areas with rapidly declining deforestation rates (columns 1–3). A one standard deviation higher deforestation growth rate during the years 2005 to 2007 is associated with a 15%-points lower probability of treatment. Results show a negative correlation between management plans and treatment, suggesting that BFP did not systematically include reserves with better governance. Similarly reserve age is statistically unrelated to the BFP. Reserves with a high spatial coverage of federal settlement projects show a 25–28%-points increase in selection probability (column 1–3). Economic activities, measured by average GDP growth from 2005 to 2007, the travel distance to nearby cities, and the land profitability index have little association with treatment (column 2–3). Having a district mayor from the same political party as the state governor in 2007 is positively related with treatment but not significant (column 3).

Table 1: Bolsa Floresta's selection criteria

Dependent variable	Participation		
	(1)	(2)	(3)
Forest cover	0.002 (0.069)	0.007 (0.075)	-0.017 (0.076)
Av. deforestation growth rate	-0.155** (0.068)	-0.145* (0.074)	-0.159** (0.074)
Settlement project density	0.248*** (0.081)	0.257*** (0.089)	0.285*** (0.090)
Forest management plan	-0.169 (0.320)	-0.135 (0.373)	-0.335 (0.396)
Reserve age	0.014 (0.130)	-0.012 (0.183)	0.064 (0.188)
Reserve area	0.136* (0.073)	0.158* (0.089)	0.173* (0.088)
Av. GDP growth rate		0.022 (0.081)	0.065 (0.086)
Travel time		-0.043 (0.101)	-0.030 (0.100)
Land profitability		0.013 (0.118)	-0.044 (0.123)
State-party affiliation			0.304 (0.227)
Intercept	0.511*** (0.096)	0.503*** (0.108)	0.478*** (0.108)
Observations	33	33	33
Adj. R ²	0.391	0.322	0.345

Note: The sample comprises 33 state-administered reserves with 15 treated units. Average growth rates are calculated over the pre-treatment period, from 2005 to 2007. Cover variables run between zero and one. 'Forest management plan' and 'state-party-affiliation' are dummy indicators. Further covariates are standardized to a normal distribution $\mathcal{N}(0,1)$. Standard errors are reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

4.3 Construction of a counterfactual

BFP reserves already had systematically lower average deforestation rates than other reserves, and differed in key economic and political characteristics prior to the treatment (cf. section 4.2). We use matching techniques to address the potential selection bias from unobservables. Matching is a prominent quasi-experimental method to overcome selection biases in spatial environmental applications (Andam et al., 2008; Gaveau et al., 2009; Honey-Rosés et al., 2011; Pfaff et al., 2015b; Alix-Garcia et al., 2015). It reduces potential selection bias by finding the most similar untreated unit for each treated unit, considering observable pre-intervention characteristics. The matched control units thereby approximate the counterfactual of treated reserves - the unobservable state of what would have happened without treatment. We follow Ho et al. (2007) and use matching as a pre-processing technique before estimating treatment effects. It reduces model dependence by eliminating those control units which are dissimilar to the treated units.

To find control cells that address the selection bias and represent the deforestation pressure of treated units, we match on covariates of pre-treatment environmental, economic, and political characteristics. Working at the cell-level allows us to control for the spatial variation in the data. Treated cells are matched to control units from both state and federal reserves. Our main findings are robust to using only state-administered reserves, but suffer from larger covariate imbalances. Matching is based on a 1:1 nearest neighbor matching technique with replacement on the Mahalanobis distance metric.¹¹ Using other matching techniques and criteria does not change our main results. We set two additional restrictions to the matching procedure. First, as cell sizes are prone to be smaller in areas bordering reserves, we restrict the matching algorithm to only find pairs between similarly-sized cells, i.e. within a margin of 1.25 km². Second, since one reserve (RDS do Rio Negro) was established and treated in 2009, we restrict the algorithm to only find control units in reserves also established in 2009.

Table 2 shows the change in the covariate space from the full to the matched data sample. Columns 1-3 show the mean values of treated and control cells by matching covariates. Columns 3 and 4 present the standardized mean differences between the groups. On average standardized mean differences were reduced from 0.38 to 0.20. For almost all covariates the imbalance was reduced after matching, with differences below the standard threshold of 0.25 standard deviations (Rosenbaum and Rubin, 1985; Imbens and Wooldridge, 2009). The generally improved balance comes at a cost of small increases or non-constant differences in settlement area, GDP growth, and travel distance. More importantly, pre-treatment deforestation trends of the matched control group are similar to that of the treated from 2004 through 2007, i.e. control units and treated units in the matched data set now have fairly equal trends before the program was rolled out. A

¹¹We use Sekhon's (2011) 'Matching' R-package.

formal test of parallel pre-treatment trends is presented in the Appendix (cf. section A.4; Table A3). This suggests that the parallel trend assumption holds, and that the matching technique succeeds in finding unbiased estimates.

Table 2: Covariate balance before and after matching

	Treated (1)	Control (2)	Matched control (3)	Normalized difference (1) vs. (2) (4)	Normalized difference (1) vs. (3) (5)
Initial forest area	1991.16	1887.81	2030.68	0.14	-0.05
Deforestation in 2004	0.29	0.72	0.15	-0.12	0.04
Deforestation in 2005	0.12	0.33	0.04	-0.15	0.05
Deforestation in 2006	0.06	0.32	0.03	-0.25	0.02
Deforestation in 2007	0.07	0.61	0.04	-0.61	0.03
Settlement area	1281.01	46.12	12.64	1.07	1.10
Forest management plan	0.12	0.14	0.13	-0.06	-0.03
Reserve area [km ²]	13316.42	4228.36	4666.97	1.05	1.00
Average GDP growth (2004-07)	0.06	0.09	0.09	-0.33	-0.37
Travel time to major cities	1793.32	1218.19	1104.57	0.49	0.58
Land profitability index	766.44	225.95	344.42	0.54	0.42
State-party affiliation	0.02	0.04	0.02	-0.13	0.00
Fire in 2004	0.04	0.13	0.02	-0.28	0.08
Fire in 2005	0.04	0.09	0.02	-0.18	0.06
Fire in 2006	0.05	0.10	0.03	-0.14	0.07
Fire in 2007	0.04	0.21	0.02	-0.49	0.06
Degraded forest in 2007	0.06	0.20	0.03	-0.15	0.03
Av. cloud area (2004-07)	159.99	58.00	108.48	0.43	0.22
Non-forest area	33.70	19.98	22.72	0.07	0.06
Water bodies	65.43	32.12	30.86	0.17	0.18
Agricultural area use	17.00	40.52	8.55	-0.32	0.12
Urban area	0.01	0.30	0.00	-0.56	0.02
Dist. to indigenous territory	63.79	81.87	75.05	-0.41	-0.25
District farm coverage	0.02	0.04	0.02	-0.60	-0.08
District share of small farms	0.78	0.68	0.71	0.50	0.33
District tractors per farm	0.01	0.02	0.01	-0.58	-0.20
Mean				0.38	0.20
Observations	4747.00	1948.00	4747.00		

Note: A nearest neighbor 1:1 matching on the Mahalanobis distance metric is used to find pairs of cells (1.25–25 km²) between 15 participating Bolsa Floresta reserves, 38 control reserves and 33 matched control reserves. Statistics in columns 1-3 represent group mean values. Areas are measured in hectares if not differently indicated. Columns 4 and 5 show standardized differences in means between groups.

5 Empirical approach

We assess the effect of the BFP on environmental outcomes by using the panel structure of our grid cell database, and regressing newly deforested area transformed by the inverse hyperbolic sine ($asinh$), D_{irdt} , of cell i , in reserve r , district d and year t on the BFP

start:¹²

$$D_{irdt} = \gamma BFP_{rt} + \mathbf{X}_{irdt}\beta + \mu_i + \eta_t + \varepsilon_{irdt} \quad (2)$$

The intervention dummy, denoted as BFP_{rt} , takes on a value of one in year t and all subsequent years during which a reserve area r participates in the BFP. \mathbf{X}_{it} denotes a matrix of additional covariates, including yearly cloud cover and a dummy indicating if a grid-cell had 'protection' status since the beginning of its designation as a reserve area. Importantly, the panel data allows us to include fixed effects (FE), which capture unobserved time-invariant characteristics (μ_i). Year-fixed effects, η_t , control for common macroeconomic shocks or regional changes in environmental policies and enforcement. The idiosyncratic error term is denoted as ε_{irdt} . We use a similar approach to examine forest degradation and fire incidence. Information on annual degradation of forest areas is available from 2007 through 2015 (INPE, 2008). As a measure of fire incidence, we use the aggregate count of heat foci within a given year, obtained from satellite sources provided by INPE.

We use FE estimations on the unmatched and matched data set to assess the impact of BFP on our outcome variables. One particular challenge arises due to the relatively rapid three-year roll-out period of the program (2008–2010). Due to the small time-variation of the treatment, we rely on the assumption of parallel trends in outcomes in the pre-treatment period. Tests for non-parallel trends in pre-intervention years corroborate our estimation strategy (cf. section A.4).

Statistical inference remains the main challenge of our analysis. Although our unit of analysis is the grid cell, the number of treated groups (15 reserves areas) is relatively small and raises the question at which level errors should be clustered. Errors may be correlated within reserves, because selection into treatment was made at the reserve level, not the cell level. Angrist and Pischke (2008) suggest clustering standard errors at the group level when groups are treated. Ignoring the violation of the standard independence assumption ($E[\varepsilon_{irdt}, \varepsilon_{jrdt}] = 0$) usually results in overestimated and inaccurate significance levels (Cameron and Miller, 2015). Nonetheless, clustering at the level of reserves, which cover relatively large areas (on average, 4600 km²) is most likely too restrictive, and ignores that the BFP targets individual households and communities spread over diverse locations within reserves. We therefore choose to present estimation results with both a lower bound standard error and an upper bound standard error, clustered at the cell-level and the reserve-level, respectively. The true estimate most likely lies somewhere

¹²The inverse hyperbolic sine is asymptotically equal to the logarithmic transformation with the advantage of being defined at zero. With annual deforestation d_{irdt} , the inverse hyperbolic sine of deforestation is here defined as: $D_{irdt} = \ln(d_{irdt} + \sqrt{d_{irdt} + 1})$. The interpretation of coefficients as percentage changes remain equal to the logarithmic transformation.

inbetween.

6 Results

6.1 Average effects on forest loss and quality

The estimates of BFP’s average effect on deforested area degraded area, and fires are reported in Table 3 (Table A4 in the Appendix shows the full estimation results). The first column shows results using the unmatched data set, which indicate a 5% increase in annual deforestation rates due to BFP. Controlling for the endogenous process of BFP’s selection (column 2), the impact estimate changes its sign, pointing to a 10% decrease in deforestation. Likewise there is an 11% reduction in forest degradation. However, there is no impact on fires. Standard errors are reported for both, clustering at the cell-level (round brackets) and at the reserve-level (square brackets). The BFP impact after matching is significant at the 1% level when clustering at the cell-level. Reserve-level clustering increases the estimate uncertainty by a factor of five.¹³ A hypothetical tree-fold increase of the standard error at the cell-level would still render the BFP impact significant at the 10% level.¹⁴ Within these limits, we find evidence that the BFP reduced annual deforestation rates by 10%, which corresponds to 856 ha of avoided forest loss between 2008 and 2015 (590–1120 ha in a 90% confidence interval).

The estimates are robust to a variety of different tests presented in the Appendix section A.4. The results of the estimates hold using different matching techniques and criteria (cf. Tables A5 and A6). Further, we use different weighted regressions and sample restrictions (cf. Tables A7). Restricting the sample before matching to state-administered reserves only changes the results marginally (cf. Table A8).

Environmental conservation interventions often have a slow start, which can lead to underestimated effects in early evaluations. We test for delayed effects by regressing deforestation on year-wise intervention indicators. We test for delayed effects by using single-year treatment indicators for each year of the intervention (cf. Figure 2). We find no changes in forest conservation during the first year of implementation, but increasing effects on deforestation reductions for each year after the start of the BFP. After five to seven years, deforestation rates are 15–25% lower compared to the matched control units.

¹³Alternatively, we cluster at a lower spatial resolution: Matching with replacement leads to a repetition of control cells. Using the original cell-id to cluster standard errors at a finer scale is only slightly more restrictive than treating each matched unit as a new observation. Nonetheless, estimates of the BFP intervention remain insignificant even at this level.

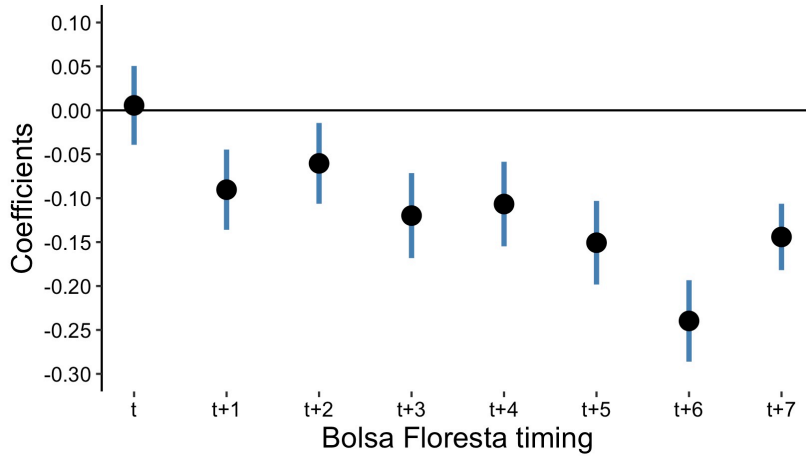
¹⁴ $(\hat{\gamma} - z^{\alpha=0.1})/sd(\hat{\gamma}) = (0.100 - 1.645)/0.020 = 0.060/0.020 = 2.98$

Table 3: The Bolsa Floresta effects on forest loss, degradation, and fire

Dependent	<i>asinh</i> Deforestation		<i>asinh</i> Degradation	<i>asinh</i> Fires
	Unmatched	Matched	Matched	Matched
	(1)	(2)	(3)	(4)
BFP	0.048 (0.020)** [0.040]	-0.100 (0.020)*** [0.098]	-0.108 (0.024)*** [0.108]	0.000 (0.003) [0.015]
Year FE	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes
Observations	136932	113928	66458	104434
No. cells	11411	9494	9494	9494
No. reserves	53	48	48	48
Adj. R ²	0.263	0.201	0.012	0.193

The dependent variable is the inverse hyperbolic sine of yearly newly deforested area, degraded forest area and fire incidences. Samples in Columns 2–4 are based on one-to-one nearest neighbour matching with replacement on the Mahalanobis distance. Further controls include yearly cloud coverage over remaining forest area and a dummy for protection status to control for the effect of reserve protection. Clustered standard errors at the matched cell level and the reserve level are reported in round and square brackets respectively. *, **, *** denote significance at the 10/5/1% level, respectively.

Figure 2: Bolsa Floresta effects across time



Bars indicate confidence intervals at a 90% level. Standard errors are clustered at the cell level. t denotes the year of the BFP start, $t + k$ denote the years after treatment.

6.2 Context-dependent heterogeneous impacts

Villages in the Amazon are often located alongside rivers and cover only a small share of a PA's area. BFP activities cover slightly over 600 registered communities. We expect treatment effects to be highest here. As distance from villages increases, forests are more difficult and costly to monitor. We test for a varying effect from differential exposure to the BFP by measuring the distance of each cell to the treated communities and analyze the impacts for the quintile-distance subsets. Results are presented in Table 4 (cf. Figure A4 for a non-parametric regression results). The highest forest conservation impacts are observed closest to treated communities. We find reductions in forest loss of 27% for directly treated cells located within 6 km of communities, compared to 13% for cells in the 6-12 km range, and no change for cells further than 12 km (or 60% of the treated sample) from communities.¹⁵

Table 4: Bolsa Floresta impacts by program exposure

Dependent Treated cells	<i>asinh</i> Deforestation				
	Distance to communities				
	very close (0–6 km) (1)	close (6–12 km) (2)	medium (12–23 km) (3)	far (23–46 km) (4)	very far (46–220 km) (5)
BFP	−0.308 (0.069)*** [0.178]*	−0.135 (0.050)*** [0.135]	0.024 (0.040) [0.127]	0.014 (0.021) [0.012]	0.004 (0.016) [0.024]
Year FE	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes
Observations	22800	22776	22776	22776	22800
No. matched cells	1900	1898	1898	1898	1900
No. reserves	44	41	44	36	26
Adj. R ²	0.182	0.213	0.278	0.177	0.140

The dependent variable is the inverse hyperbolic sine of newly deforested area in a year. Samples are based on one-to-one nearest neighbour matching with replacement on the Mahalanobis distance. Further controls include yearly cloud coverage over remaining forest area and a dummy for protection status to control for the effect of reserve protection. Treated units are subset into quintiles with respect to the distance to the the closest FAS community. For each quintile, treated cells are matched to untreated cells before estimating a fixed effect regression. Clustered standard errors at the matched cell level and the reserve level are reported in round and square brackets, respectively. *, **, *** denote significance at the 10/5/1% level, respectively.

In a situation with considerable heterogeneity, small estimated impacts of the BFP could be due to diverging positive and negative effects of the BFP offsetting each other. Where opportunity costs are low or zero, deforestation is likely to already be minimal, so payments may have no effect. Effects are expected to increase with opportunity costs when payments offset forgone profits from land-use change. However, where opportunity costs exceed payments, deforestation will continue (Persson and Alpizar, 2013). We expect opportunity costs to be directly related to the degree of market integration. To proxy

¹⁵ $e^{-0.308} - 1 = 0.265; e^{-0.135} - 1 = 0.126$

for deforestation pressure, we use the level of pre-treatment deforestation within a 20 km buffer around each cell, each cell's proximity to the nearest market, the average agricultural GDP within a district, as well as the distance to the reserve border.

The results, shown in Table 5, indicate that the BFP reduced deforestation at all pressure levels (low, medium, high). The BFP effect increases with the level of pre-treatment deforestation in nearby regions. At medium- and high-pressure levels, forest conservation increases from 7% to 10% (column 1). In areas with high levels of agricultural income, the BFP effect increases from a 6% to a 15% reduction in deforestation rates (column 2). Neither market proximity (column 3) nor distance to reserve borders (column 4) seem to affect the program's impact on deforestation.

Overall, conditional payments and conservation investments thus seemed to compensate for forgone income from forest-harming activities across the region. This is in line with earlier work that gauged average conservation opportunity costs in the study area (Börner et al., 2013). If opportunity costs were indeed covered for most BFP participants (Newton et al., 2012), our estimates suggest that conservation effects increase with the extensive margin of land demand. Alternatively, higher deforestation pressures and shorter distances to markets could also reflect differences in the implementation of the Program. Control visits by program staff as well as direct enforcement efforts are less costly close to markets, roads, and the BFP's head office. Conservation effects might thereby both increase with opportunity costs and the implementation quality across space.

7 Discussion and conclusion

A naive comparison of deforestation rates in reserve areas with and without the BFP suggests that the program has increased deforestation, while before and after comparisons point to reductions in forest loss. Both comparisons are misleading, however, because BFP-enrolled reserves already had lower deforestation rates prior to treatment and forest loss was declining in Amazonas state.

To arrive at more reliable estimates of program impact we control for this selection bias by matching treated areas with similar untreated areas, and combine this method with fixed effects regressions. Overall, we show that the BFP had a statistically significant effect on forest conservation, reducing deforestation by about 10% and forest degradation by about 11%. Conservation effects concentrate in areas near markets and at reserve boundaries, and where ex-ante deforestation pressure is relatively high. Near treated communities, forest losses declined by up to 26%. However, the absolute effect of the BFP is small, only avoiding about 850 hectares of forest loss. This compares to 0.2% of forest losses in Amazonas, and 3.8% of forest losses in reserves of the Amazonas state (from 2008 to 2015).

Table 5: Effects of the Bolsa Floresta program under heterogeneous market integration

Dependent Pressure index	<i>asinh</i> Deforestation			
	20 km buffer deforest. (1)	Agric. GDP (2)	Market- proximity (3)	Reserve border proximity (4)
BFP	-0.075 (0.015)*** [0.095]	-0.065 (0.028)** [0.104]	-0.091 (0.033)*** [0.104]	-0.091 (0.025)*** [0.092]
BFP × medium pressure	-0.033 (0.018)* [0.015]**	-0.038 (0.028) [0.049]	-0.025 (0.035) [0.062]	-0.032 (0.030) [0.035]
BFP × high pressure	-0.048 (0.037) [0.061]	-0.094 (0.039)** [0.072]	-0.002 (0.030) [0.049]	0.004 (0.029) [0.049]
Year FE	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes
Observations	113928	113928	113928	113928
No. matched cells	9494	9494	9494	9494
No. reserves	48	48	48	48
Adj. R ²	0.201	0.201	0.201	0.201

The dependent variable is the inverse hyperbolic sine of yearly newly deforested area. Samples are based on one-to-one nearest neighbour matching with replacement on the Mahalanobis distance. Further controls include yearly cloud coverage over remaining forest area and a dummy for protection status to control for the effect of reserve protection. Clustered standard errors at the matched cell level and the reserve level are reported in round and square brackets respectively. *, **, *** denote significance at the 10/5/1% level, respectively.

The usual caveats of quasi-experimental evaluation apply, including the potential influence of unobserved confounding variables (Rosenbaum, 2002). Research on PA effectiveness indicates that treatment selection biases typically lead to conservation impacts being overestimated. Our strategy of using appropriate matching techniques, combined with before–after comparisons in a panel setting and a systematic robustness analysis, makes us confident that we are capturing the actual impacts of the program on the measured outcome variables during the study period.

Our results resemble those found for various other conservation programs. Sims and Alix-Garcia (2017) show that a network of PES along with PAs can generate high conservation effects in Mexico. Although Mexico’s national forest PES program had statistically insignificant impacts at the national level, it significantly halved deforestation rates in high-pressure areas (Alix-Garcia et al., 2019; Alix-Garcia et al., 2015). Although enrolment decisions were partly based on deforestation risk, the small and variable weight given to this factor meant that many areas with negligible deforestation risk were enrolled. Similarly, Jayachandran et al. (2017) found that a PES program implemented in an area of high deforestation pressure in Uganda led to large, statistically significant reductions in forest loss. Conversely, enrolment of many areas with low deforestation pressure area led to low

additionality in Costa Rica's national PES program (Hanauer and Canavire-Bacarreza, 2015) as well as in Peru, where the national forest conservation program piloted PES in the Amazon region (Giudice et al., 2019). The estimated impacts of other conservation interventions have also been found to vary according to the spatial context: for example, in a sample of 136 conservation interventions, Börner et al. (2020) found that impacts were significantly larger in high—pressure contexts.

Other factors are also likely to have contributed to the relatively low BFP additionality in absolute terms. Chief among these are (1) pressure on PA resources by external resources users (who do not participate in BFP) and (2) non-compliance with PA-cum-BFP rules by reserve dwellers. Table A1 shows that external deforestation pressure often correlates with internal forest loss, but some reserves stand out as being exposed to higher external than internal pressure (e.g., RDS Juma and Uatumã), while others experience comparatively higher internal pressure on forest resources (e.g., RESEX do Rio Gregório and Catuá Ipixuna). External pressure is generally more common when reserves are located close to major roads and in the Southern part of Amazonas, where the agricultural frontier is expanding (Pfaff, 1999; Pfaff et al., 2007; Schielein and Börner, 2018). This type of pressure requires targeted collaboration with federal and state-level environmental law enforcement agencies and lies at the heart of the BFP's intervention strategy to strengthen local natural resource stewardship by the population living inside the reserves. However, internal pressure in reserves that are relatively well-connected to local and urban markets may also be the result of unenforced conditionality. The BFP uses a system of 'yellow-card' warnings, but at the time of the field visits for this study, had not yet resorted to strict sanctions, such as excluding or suspending payments to non-compliant participants. Participants who violate program rules thus often remain enrolled, leading to high participation rates even in reserves where deforestation is prevalent.

The low overall additionality would suggest high cost-ineffectiveness of the BFP in terms of forest conservation, even if administrative costs were excluded. Barely over 850 ha of forest were saved despite transfers totalling USD 15.9 million until 2015. It is important to remember, however, that the BFP includes several components that go beyond PES and enhance local livelihoods through productive investments and improved public services. The BFP was indeed found to have likely strengthened communal institutions and improved social and welfare outcomes by transferring significant resources to local households and communities (Hayes et al., 2017; Börner et al., 2013). In many ways, the BFP could thus be more accurately considered a rural development or social support program with environmental co-benefits, rather than a predominantly conservation-oriented initiative.

Our results imply potential to improve BFP's environmental impact and cost-effectiveness. As the program's additionality is higher in areas with higher deforestation pressure, focus-

ing conditional transfers to such areas (e.g., through differentiated payments or targeting high-pressured reserves) could result in improved conservation outcomes (cf. Wunder et al., 2018). To ensure that this happens, the BFP would need to enforce its conditionality more rigorously. Doing so would reduce costs as payments are reduced or suspended in areas where deforestation persists, thus making the program more cost-effective.

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Impacts of conservation incentives in protected areas: the case of Bolsa Floresta, Brazil

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Abstract

Conditional incentives are a promising complementary approach to conserve tropical forests, for example, in multiple-use protected areas. In this paper we analyze the environmental impacts of *Bolsa Floresta*, a forest conservation program that combines direct conditional payments with livelihood-focused investments in 15 multiple-use reserves in the Brazilian state of Amazonas. We use grid-based data, nearest-neighbor matching, and panel data econometrics to compare three forest-related program outcomes – deforestation, degradation, and fires – of participating and non-participating reserve areas. Forest threats were low before and after treatment, because the program prioritized low-pressure sites. Thus, we find significant but small additional conservation effects from the implementation of the program. Notwithstanding, treatment effects are relatively larger in areas with higher deforestation pressure and higher potential agricultural income. Our findings add to the growing body of evidence showing that adverse spatial targeting of conservation incentives, i.e. disproportionately enrolling low-pressure sites, is a prime cause for the low additionality found in rigorous impact evaluations of many existing initiatives.

JEL classification codes: O13, Q15, Q56, Q57

Keywords: deforestation, protected areas, payments for environmental services (PES), spatial matching, Amazonas, Brazil

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Declaration of interests

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The authors hereby declare no known interests related to their submitted manuscript.

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