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Forest concessions and eco-certifications in the Peruvian Amazon: Deforestation impacts of logging rights and logging restrictions☆

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ABSTRACT

Concessions that grant logging rights to firms support economic development based on forest resources. Eco-certifications put sustainability restrictions on the operations of those concessions. For spatially detailed data, including many pre-treatment years, we use new difference-in-differences estimators to estimate 2002–2018 impacts upon Peruvian Amazon forests from both logging concessions and their eco-certifications. We find that the concessions – which in theory could raise or reduce forest loss – did not raise loss, if anything reducing it slightly by warding off spikes in deforestation pressure. Eco-certifications could reduce or raise forest loss, yet we find no significant impacts.

1. Introduction

Forest losses due to productive activities are a challenge to sustainable development, especially in developing countries with many poor citizens on forest frontiers. In the tropics, losses of local and global forest public goods result from ranching, agriculture, mining, and logging (Laurance et al., 2001; DeFries et al., 2010). Unlike in temperate zones, where forest regrew on net in 1990–2015, for tropical forests 129 million hectares were lost in that time (FAO 2016). Deforestation contributed about 15% of global carbon emissions and consequences included erosion, extinctions, and water-quality degradation (Wright and Muller-Landau 2006; van der Werf et al., 2009). Protected areas (PAs), which restrict some or all economic activities, have been the leading conservation response.

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PAs have reduced forest loss on average. However, their impacts have varied considerably across locations, and PA types, and in some cases PAs have had no short-run effects (Andam et al., 2008; Joppa and Pfaff, 2011; Pfaff et al., 2015; Herrera et al. 2019; Börner et al., 2020).

Since governments often promote economic activity, which PAs restrict, here we focus on forest impacts from the promotion of regulated timber extraction, in logging concessions. Firms granted medium-to long-term logging rights can earn revenues from timber harvests, while states raise revenue through extraction fees. In principle, rights can balance economic development with forest conservation, and such regulated timber activities have risen in popularity. Concessions now cover over 123 million hectares within the tropics – an area around the size of South Africa (Hensbergen 2018). Yet behaviors under logging rights within these concessions are not always well regulated. Weak monitoring and enforcement have led to forest-management certifications run by non-state actors (Auld et al. 2008). The Forest Stewardship Council (FSC) offers a well-known third-party certification option, with 231 million hectares certified in 89 countries (FSC 2022b). The FSC audits concessions' practices to ensure that certified concessions comply with all applicable laws, in addition to FSC's own ecological and social standards (FSC 2022a).

We study impacts of logging concessions and FSC certifications on forest loss. We note that even the direction of concessions' impacts is theoretically ambiguous. Rights holders may defend their forest assets and reduce forest losses by preventing others from harvesting. Yet, instead, they may raise forest loss by violating concessions' terms. At the concession level, the sign of certifications' impacts is not ambiguous, since meeting sustainable-management standards does not harm forests. However, FSC can fail to improve forest outcomes if standards are not binding, such as when most of the firms that adopt certification already were meeting the FSC's standards. Certification could raise forest loss at the firm level if advertising existing sustainable practices achieves firms' goals of increasing demand and output – which can occur even if the FSC prevents deceptive advertising that lets firms sell uncertified timber that consumers perceive to be certified, i.e., 'greenwashing'. The importance of logging concessions and certifications in forest management around the world, as well as these ambiguities regarding the direction of impacts, motivate our empirical assessment.

To estimate the impacts upon loss of forests due to logging concessions, as well as the addition of FSC certifications, we compile spatially detailed panel data. Given newly available long data sets, we can study 1986–2018 annual forest loss both inside and outside of logging concessions in Peru's Amazonian region. Peru's Amazon is the world's fourth-largest area of tropical forest, a hotspot for biodiversity, watersheds, and carbon storage (Ministerio del Ambiente, 2015), as well as Peru's dominant timber region. The state auctioned off most logging concessions between 2002 and 2004, for relatively isolated and sparsely populated regions with few private rights before concessions. Concession enforcement was limited, however, which helps to motivate third-party certification. For instance, starting in 2006, the FSC certified practices in some of these logging concessions.

We employ improvements not only in forest data but also in econometric models using panel data. A longer time series for forests provides more pre-treatment years. This supports our applications of new difference-in-differences (DID) estimators that solve key problems identified in two-way fixed effects (TWFE) estimators, which are widely utilized for policy evaluations with panel data.

It is worth highlighting three benefits from these advances. First, this new DID literature describes TWFE estimators as weighted sums of group-period average treatment effects (ATE), finding that some group-period 'impact mini-estimates' can receive negative weights which, in turn, can result in biased estimates if those ATEs are not all the same (de Chaisemartin and D'Haultfœuille, 2020). New DID estimators, in contrast, do not rely on homogeneous treatment effects for identification. Second, if TWFE regressions include multiple treatments – concessions and FSC for us – a TWFE estimator for the impact of one treatment can be 'contaminated' by other treatments' impacts (de Chaisemartin and D'Haultfœuille, 2022b). We employ de Chaisemartin and D'Haultfœuille (2021; 2022b) estimators robust to having multiple treatments. Third, given the importance of the core identification assumption when using panel data – parallel trends – new DID estimators offer useful options for conducting formal tests of this condition. Along these lines, following advances in synthetic-cohort literatures (e.g. Arkhangelsky et al., 2021; Ben-Michael et al. 2021) we pre-match the treated units using pre-treatment forest loss. We also employ a continuous forest-loss outcome to avoid biases from using binary deforestation outcomes in panel data models (Garcia and Heilmayr 2022). Given all of those helpful advances by others, this empirical strategy could guide various evaluations of forest policy. To the best of our knowledge, we are the first to estimate forest impacts of uncertified concessions and FSC certification using: (i) DID estimators robust to heterogeneity and contamination issues for TWFE (de Chaisemartin and D'Haultfœuille, 2020; 2022b); as well as (ii) a considerably longer panel, for better tests concerning parallel trends.

We find no rise in forest loss due to the granting of logging rights within these forest concessions. If anything, concessions might ward off upward spikes in external deforestation pressure to slightly reduce forest loss – much like stricter PAs, though achieving a bit less than less strict multiple-use PAs which allow limited smallholder activities (we compare with analyses using the same data in Rico-Straffon, Wang, and Pfaff, 2022). We also find that FSC certification of logging-concessions' practices provided no additional reductions of forest loss, relative to these uncertified concessions.

Our quasi-experimental results concerning Peru's logging concessions[1] are consistent with limited prior studies, which find either forest-loss reductions or no effects upon forests due to concessions and community-managed forests (Fortmann et al. 2017; Panlasigui et al., 2018; Tritsch et al., 2020; Blackman and Villalobos 2021). Peru's concessions stand out in terms of their context, one in which logging is informal and hard to regulate (Sears and Pinedo-Vasquez 2011). We also add to a sparse quasi-experimental literature on certifications of supply chains – a growing issue in climate change, since huge shares of greenhouse emissions arise within value chains. Our finding of insignificant reductions for FSC is consistent with evidence ranging from null impacts to loss reductions (Miteva et al. 2015; Heilmayr and Lambin 2016; Blackman et al. 2018; Komives et al., 2018; Panlasigui et al., 2018; Rana and Sills 2018;

[1] Other analyses of varied types concerning Peru's forests include Swenson et al. (2011); Raschio et al. (2014); Miranda et al. (2016); Campos-Cerqueira et al. (2020); and Anderson et al. (2019).

Villalobos et al. 2018; Anderson et al. 2019; Tritsch et al., 2020).

The rest of the paper is as follows. Section 2 discusses both concessions and certifications in Peru, as well as their potential forest-loss impacts. Section 3 describes our data. Section 4 presents our empirical strategy, along with descriptive statistics, and then Section 5 provides all of our results. In Section 6, we summarize our results and then suggest some possible future research directions.

2. Background

2.1. Forest interventions

2.1.1. Forests & logging concessions

Across their 72 million hectares (MapBiomas Amazon Project, 2022), Peru's tropical forests have supported both ecological and social outcomes. They host: 97% of Peru's freshwater supply; high-value timber, such as from cedar and mahogany; non-timber forest products; immense ecological biodiversity; and further over one thousand indigenous communities (Ministerio del Ambiente and Ministerio de Agricultura, 2011). Over fifty ethnic groups inhabit the Peruvian Amazon, described as "the poorest and most disenfranchised segment of the […] population" (Urrunaga et al., 2012).

Peru's constitution states that all of the country's forests belong to the nation, such that the state is in charge of all forest management (República del Perú, 2018). In 2000, Peru enacted a Forestry & Wildlife Law No. 27308 to reform this sector (República del Perú, 2000; Sears and Pinedo-Vasquez 2011). It categorized forests and established logging concessions in Permanent Production Forests[2] designated for timber harvest or non-timber forest products. Concessions are 40-year contracts for 5–40,000 ha that allow private actors to extract timber under rules (República del Perú, 2000). Private actors applied and the state auctioned most areas between 2002 and 2004, with all granted by 2006 (Fig. 1, Panel A). We examine all those starting after 2000 as those starting before 2000 had short-term timber-harvest permits prior to this concession system.[3] Our study area includes 525 logging concessions covering 7.1 million hectares in Madre de Dios, Loreto, and Ucayali − which includes 10% of all forests and around 90% of the total area in logging concessions in Peru.

To extract timber legally, concession holders present forest-management plans every five years and annual operating plans indicating the sub-sections and volumes to be harvested (República del Perú, 2000). All harvested woods need a Forest Transport Permit, describing "species and volume of the material and its place of origin" (Urrunaga et al., 2012). Yet evidence suggests illegal actions all along chains (Sears and Pinedo-Vasquez 2011; Urrunaga et al., 2012; Finer et al., 2014). Some firms underreport the timber harvested inside the concession, fail to report extraction from outside concessions, or directly falsify key approval documents (Urrunaga et al., 2012; Chavez Solis, 2017).

Peru's Supervisory Body of Forest Resources & Wildlife (OSINFOR) monitors concessions using field visits (República del Perú, 2013a). Most visits occurred in 2009, after OSINFOR established its independence from the forest authority (Finer et al., 2014; Chavez Solis, 2017). OSINFOR may start an administrative process to investigate the irregularities that are revealed during field visits. If a firm violates a contract, OSINFOR may issue monetary sanctions and even cancel concessions, depending upon the degree of a violation (República del Perú, 2013b; Finer et al., 2014). By 2013, OSINFOR had visited 64% of concessions, at least once, and its field supervisors had detected irregularities in most of them (Finer et al., 2014). About 60% of the timber inspected by OSINFOR was found to be illegal, in fact (Global Witness 2019), and 45% of the logging concessions in our study area were cancelled, or went inactive, due to such administrative processes initiated between 2006 and 2013 (Fig. 1, Panel B). Like field visits, most of those cancellations were during 2009. Concessions can be cancelled for various reasons, which include: presenting false information in forest-management plans; unauthorized changes in land uses; causing risks for the environment; and not paying fees within established times (Kometter 2019).[4] Most of these logging concessions lasted at least four years and about half of them were still active in 2014. Below, we discuss this 'unstaggered treatment' in terms of our choices of DID estimator and within our robustness checks.

2.1.2. FSC certification

The Forest Stewardship Council (FSC), which offers one of the world's best-known certifications, started in 1993 to aid in "environmentally appropriate, socially beneficial and economically viable management of the world's forests" (FSC 2022a). It audits logging concessions' compliance with applicable laws and FSC standards (FSC 2022a). Certifications are called: "rigorous, transparent, and participatory" (Hale and Held 2011). Yet their impacts upon forests have been evaluated only rarely and even less often with rigor (though see some good examples, and a review, cited above).

During 2006–2013, 34 out of the 525 logging concessions within our study area (Fig. 2) received FSC's forest-management

[2] The National Institute of Natural Resources mapped Permanent Production Forests via participatory processes with stakeholders using criteria such as: i) belong to state and lack other use (native communities, agricultural lands); ii) access to transport (rivers, roads); iii) good quality of forest cover (Salo and Toivonen 2009; Romero and Marco, 2005).

[3] Prior logging was informal and unregulated. Private actors could ask the government for permission to extract timber from "Free Availability Forests" via 1000-ha short-term contracts with little state control as it was hard to monitor and enforce for a high number of small logging permit holders (Urrunaga et al., 2012). This was a different treatment.

[4] If a concession is cancelled, it is returned to the state. This can take years, with procedures before any declaration. During this period, the area is not exploited by the concessionaire – who with no control also no longer has monitoring responsibilities. After all this, the area can be assigned again, through the 'Abbreviated Procedure' (Kometter 2019).

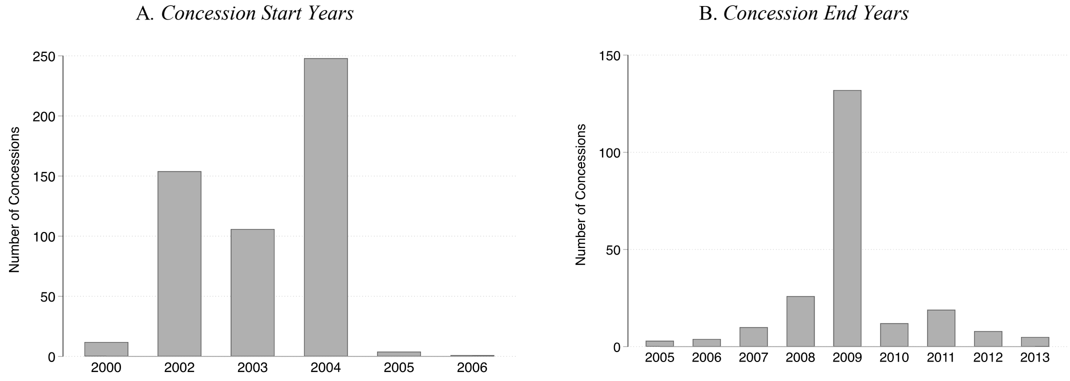


Fig. 1. Logging concessions' start and end years.
Notes: This figure shows the number of logging concessions granted in Madre de Dios, Ucayali, and Loreto by their start years (Panel A) and their end years (Panel B), if they were cancelled during our study period.

certification (FSC Peru, 2022b) – the largest set in 2011 and the second largest in 2007 (Fig. 3, Panel A). By October 2022, in total 1.18 million hectares in the Peruvian Amazon were under FSC's forest-management certifications (FSC Peru, 2022b). FSC's certificates last for five years, although FSC can suspend (or even cancel) certificates if their auditors uncover violations. Five FSC certificates were suspended between 2009 and 2014, while other certificates simply ended before the end of our study period 2018 (Fig. 3, Panel B). Thereby, FSC also has an 'unstaggered' design, with some concessions that got certified switching back to uncertified.

2.2. *Impacts framework*

2.2.1. *Uncertified concessions*

Concessions grant logging rights to private actors for 40 years, imposing forest-management rules. We assume concession holders maximize profits, though with unclear weights on short-run versus long-run. Maximizing their long-run profits may imply avoiding overextraction, including because concession rights can be renewed for those who followed the rules. Profits also imply an incentive to take actions – at the very least low-cost ones – to exclude other actors from taking one's timber. Maximizing short-run profits, though, could instead imply not complying with concession terms, such as by extracting more than agreed with a state. Thus, impacts depend upon public monitoring and enforcing terms. Weak public monitoring and enforcement, due to the difficulty of inspecting remote forest areas, has led to violations of contracts and illegal logging, likely raising forest loss.

Forest-management techniques also affect forest outcomes. In this region, firms often do selective logging, with smaller losses of forest than from, for instance, clearcutting (Urrunaga et al., 2012). This choice is also driven by profit maximization since both timber extraction and transport costs vary with management technique, especially in forests with low shares of merchantable species.

Concessions' locations also matter for deforestation. Being nearer to cities and markets may lower deforestation, since monitoring and enforcement costs are lower near agencies (Sims 2010; Chavez Solis, 2017). Yet lower transport costs for more accessible concessions raise profits and thereby pressures from economic activities. In sum, forest impact from these logging rights is ambiguous.

2.2.2. *FSC certification*

Certified firms can gain from: i) key export markets that prohibit illegally-sourced timber; ii) price premia, if consumers are willing to pay; iii) government incentives; iv) operational efficiencies; and v) NGO funding (Breukink et al. 2015; Blackman et al. 2017). The legality of imported timber sources is increasingly an issue – e.g., the US′ Lacey Act and European Union's FLEGT Action Plan require verification of origin for timber imports (Urrunaga et al., 2012; European Forest Institute 2022). Thus, a firm could gain from getting certified by FSC.

Yet certification is costly. FSC requires compliance with the law, plus its more stringent social and environmental standards. Firms must invest in monitoring, including auditing of their inventories, and pay to hire consultants. Complete compliance with FSC may also raise firms' labor costs since employees should be on payroll and should receive health benefits. That is uncommon in Peruvian uncertified concessions (FSC Peru, 2022a; Urrunaga et al., 2012). Thus, there exist reasons for firms not to seek certification at all and/or to not comply fully with FSC (Heilmayr and Lambin 2016).

If all such requirements are well enforced, as certifications are well audited, then FSC can improve forest outcomes. At the level of any single certified concession, it seems unlikely that certifications would raise forest losses, since meeting sustainable-management standards does not harm a forest. However, FSC can fail to improve forest outcomes if standards are not binding, such as when most of the firms that adopt certification already were meeting the FSC's standards. Certification could raise forest loss at the firm level if advertising existing sustainable practices achieves firms' goals of increasing demand and output – which can occur even if the FSC prevents deceptive advertising that lets firms sell uncertified timber that consumers perceive to be certified, i.e., 'greenwashing'.

Finally, third-party certifications might interact with concession enforcement. We hypothesize that gains are more likely when there exist public and private capacities to defend forests, as excluding others seems a necessary condition. Thus, certification impacts

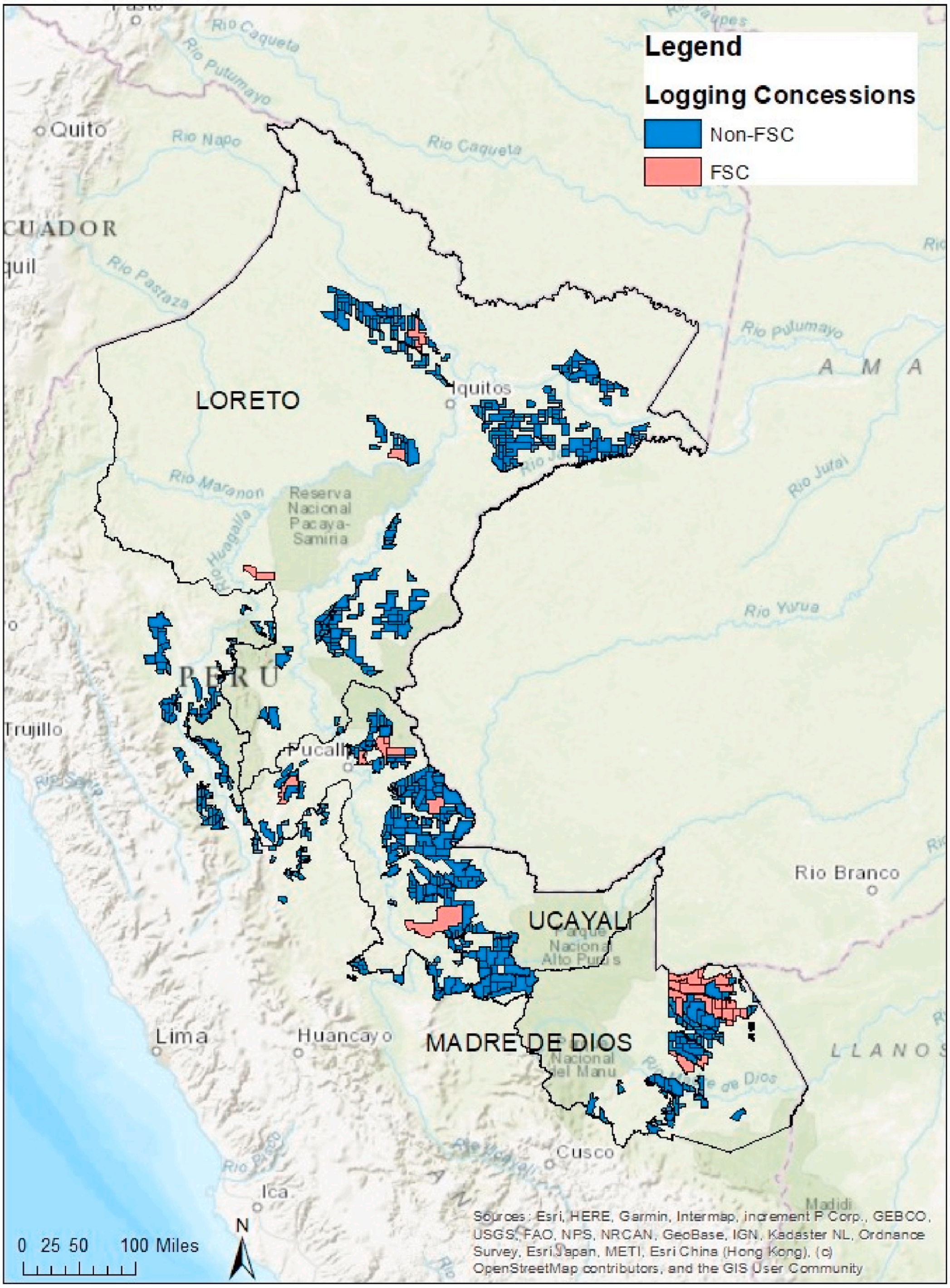


Fig. 2. Logging Concessions in the Peruvian Amazon by FSC certification status

Notes: The map shows the 525 logging concessions in our study area: Loreto, Madre de Dios, and Ucayali. We exclude from our analyses the 91 concessions which fall outside our study area and represent around 10% of the total concession area in Peru. Uncertified concessions are shown in blue, while 34 ever FSC-certified concessions are shown in red. (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

Source: We produced this map with data gathered by WWF Peru from OSIFNOR, MINAM, MINAG, and FSC Peru.

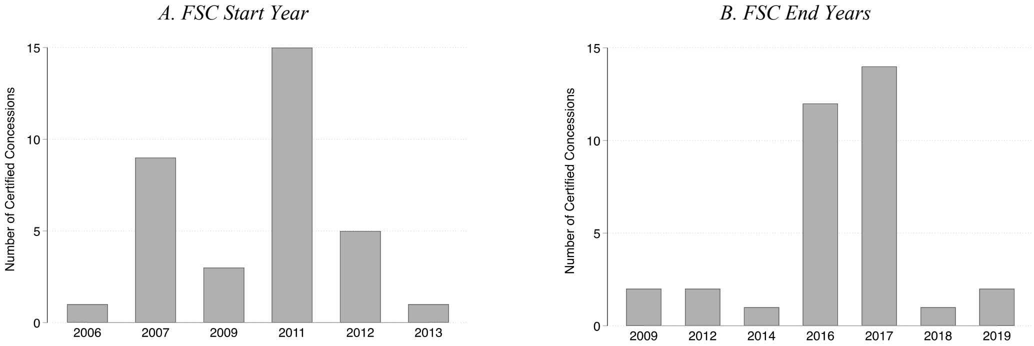


Fig. 3. FSC certificates.
Notes: This figure shows the number of logging concessions that received FSC forest-management certificates in Madre de Dios, Loreto, and Ucayali by their start (Panel A) and end years (Panel B). FSC certificates were granted for five years and could be renewed, but five certificates were suspended in 2009–2014 due to violations of FSC's standards.

may require concession impacts.

3. Data

3.1. Treatments

3.1.1. Logging concessions

We have geospatial data on logging concessions from WWF Peru who had gathered, revised, and updated OSINFOR's boundaries. We got administrative data from the Ministry of the Environment (MINAM) and Ministry of Agriculture (MINAG) on concession characteristics: contract number; concessionaire's name; concession's legal status; creation date; cancellation year (if applicable); harvesting volume; logging cycle; planned investments; and harvesting technology and methods. Other than the first five, for many variables the data were incomplete for most of these concessions. Fortunately, our empirical strategy does not rely upon them within identification (see Section 4).

3.1.2. FSC certification

We also obtained the information on FSC certification of logging concessions from WWF Peru, who had gathered data from FSC Peru and from MINAG. That includes: FSC license; FSC code; type of FSC certificate; and certification status. We added from publicly available data on the FSC certificates' start dates, suspension dates (if applicable), and expiration dates (FSC Peru, 2022b).

3.1.3. Control forests

To estimate the effects of FSC certification, we consider only concession forests, since only those forests have FSC certificates (the treatment "FSC" only occurs after the treatment "concession"). Thus, the best comparisons for forests in FSC-certified concessions are forests within uncertified concessions, while the forests outside of all concessions are not relevant for evaluations of FSC.

To estimate the impacts of uncertified concessions, we constructed a control group of "untreated" forests, outside of concessions, by excluding all public and private PAs, as well as all indigenous communities, from the pool of potential control forest. Other initiatives surely exist in our controls, yet we lack their polygons' boundaries. While the National Forest Conservation Program paid for ecosystem services – apparently with small conservation benefits (Börner et al. 2016; Giudice et al., 2019) − it focuses on indigenous lands, which we dropped from the controls. The state also granted oil, rubber, and Brazil nut concessions (Anderson et al. 2019) − with the potential to slow deforestation (Nunes et al., 2012) − that suffer from weak governance (Willem et al. 2019). REDD + projects existed, although their forest impacts tended to be minimal (Montoya-Zumaeta et al. 2021). Finally, a Local Forests scheme allows regulated extraction of forest products by rural populations, yet there is evidence of unauthorized timber extraction within these areas due to a lack of oversight (OSINFOR 2017).

Any loss reductions resulting from any such programs implemented within our "untreated" forests, would imply that our estimates of uncertified concessions' impacts in reducing losses are actually lower bounds. Any such initiatives strengthen our claim, since we emphasize only that logging concessions did not raise forest loss (see Results) – while, if anything, maybe lowering it.

3.2. Outcomes

We construct forest outcomes using detailed satellite data on Peruvian Amazon forests at a 30-m resolution from publicly available sources: MapBiomas Amazon Project (2021); and Hansen et al. (2013). We calculate 1986–2018 annual forest-loss rates for the Peruvian Amazon region (all of Madre de Dios, Loreto, Ucayali) using MapBiomas data. We obtain 2000–2021 annual tree-cover-loss rates from Hansen et al. (2013).[5] As our concessions started in 2002, and as FSC certifications started in 2006, our main analyses are based upon the MapBiomas data, since it provides far more pre-treatment years to test for the core assumption of parallel trends. We check robustness for our results using Hansen et al.'s (2013) tree-cover data, which have been widely used in the literature.

3.3. Units of analysis and panel data

We compiled a balanced panel data set, including annual forest loss for each "aggregated pixel" (3 × 3km) inside or outside concessions in our study area, for 1986–2018. All pixels are forested in 1985. Both MapBiomas Amazon Project (2021) and Hansen et al. (2013) provide a 30 × 30m pixel with a binary forest outcome: 0 if not deforested; 1 in the year it is deforested; and missing afterward. Most literature on forest impacts uses binary 30 × 30m pixels as units, yet TWFE and new DID estimators are then biased (Garcia and Heilmayr 2022). Thus, we employ aggregated forest pixels with continuous forest-loss percentages to avoid such bias and reduce any concerns about spatial autocorrelation and false precision. For our main uncertified-concession specification, we use 100-pixel x 100-pixel aggregates (3 × 3km). We check robustness to pixel size with 300-pixel x 300-pixel aggregates (9 × 9km). For the smaller FSC treatment, we also check the robustness to a smaller 30-pixel x 30-pixel aggregates (900 × 900m, 1/100th the size of the 9 × 9km).

As some aggregated pixels are partially inside a concession, we utilized the following definitions: "ever treated" if 100% of a pixel's area is inside concessions; "untreated" if none of a pixel's area intersects concessions; and dropped if a pixel-concession intersection is between 0% and 100%.[6] We also dropped all the pixels in PAs and indigenous reserves from "untreated" (as described in Section 3.1.3). We exclude pixels inside concessions which were created before 2000 from the untreated group as they had logging permits before the logging concession regime was established.

3.3.1. Pixel-level panel for evaluating uncertified concessions

To estimate the impacts of uncertified concessions, we use the panel of aggregated pixels (3 × 3km), dropping pixels inside concessions ever certified by FSC to isolate uncertified concessions' effects. Our final 3 × 3km sample includes a balanced panel of 1,128,006 pixel-year observations, of which 16% (n = 180,906) were defined as ever being within an uncertified concession (Figure A1).

3.3.2. Concession-level panel for evaluating FSC certification

To estimate the impact from adding FSC certification, we only analyze forests inside concessions. We use a concession as the unit of analysis, instead of the aggregated pixel, since decisions to get certified are at the concession level. This unit makes use of all the data we have for the concessions, whose boundaries are well defined. We observe forest losses for 33 years (1986–2018), inside each of the 513 concessions created after 2000. We can then estimate the impacts on rates of forest loss from adding FSC to uncertified concessions, using 16,929 concession-year observations (noting that 6.24%, or n = 1,056, were ever FSC certified, i.e., 32 concessions established post-2000).[7]

4. Empirical strategy

4.1. New DID estimators (departing from TWFE)

4.1.1. Identifying assumptions and issues with TWFE

To estimate the effects of uncertified logging concessions as well as their eco-certification, we use new difference-in-differences (DID) estimators. They overcome limitations recently identified for two-way fixed effects (TWFE) estimators widely utilized for policy evaluations with panel data. For identification, DID and TWFE rely on "parallel trends": the treated and untreated units' mean post-treatment outcomes would have proceeded along parallel paths, had there been no treatment.

Recent DID literature has shown that TWFE also relies on a homogenous treatment assumption. TWFE estimators are described as weighted sums of group-period average treatment effects (ATE) in which some group-periods' contributions to the impact estimation can receive negative weights (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna 2021; Sun and Abraham 2021). That, in turn, can imply biased estimates when the group-periods' ATEs are not homogeneous. Thus, unbiased TWFE would require ATEs to be the same for all spatial units − aggregated pixels or concessions – and time periods, which is unlikely. Even if all TWFE estimation weights are non-negative, TWFE does not generally estimate the average treatment effect on the treated (ATT), while recent new DID estimators do (de Chaisemartin and D'Haultfœuille, 2020). Even assuming parallel trends hold, the TWFE only estimates the ATT for: i) staggered designs; ii) with a binary treatment; and iii) with no variation in the timing of treatment (de Chaisemartin and D'Haultfœuille, 2022a). Since our context does not satisfy the first or the third condition, we rely on new DID estimators.

[5] We define 'forest' as at least 50% tree cover and check robustness to a 30% threshold used in the literature.

[6] We check the robustness of our results to shifting this definition of a "treated" concession to employ a cutoff of 80%.

[7] Our sample includes all 32 concessions that ever received FSC forest management certification in our study area, during our study period, except for the two concessions we have previously dropped as they were created before 2000.

Further, de Chaisemartin and D'Haultfœuille (2022b)'s recent DID analyses found that if TWFE regressions include multiple treatments – for us concessions and FSC certifications – estimators for each treatment can be biased by the effects of any other treatment. For example, FSC's TWFE estimator is a sum of two terms: i) a weighted sum of FSC's effects, in each concession-year; and ii) a weighted sum of the effects of the concessions (de Chaisemartin and D'Haultfœuille, 2022b). Thus, FSC certification's TWFE estimator can be 'contaminated' by the concession effect through the second term.

4.1.2. The DID_L estimator

Our main results use de Chaisemartin and D'Haultfœuille (2021; 2022b) DID estimators (DID_L), which are robust to heterogeneous and 'dynamic'[8] treatment effects, as well as 'contamination'. Among the new DID estimators, DID_L is also the most appropriate for our empirical setting, where some logging concessions as well as some FSC certifications get cancelled or expire. DID_L applies to 'unstaggered' designs, in which treatments not only 'switch on' but also can 'switch off' again.

For each treatment, their DID_L estimator produces an "event-study" graph, where the event at $t = 0$ is the first time that the treatment status switches on. The graph shows the instantaneous effect (at $t = 0$) plus dynamic effects which are "reduced-form estimates of the effect of having been exposed to a weakly higher amount of treatment for l periods" (de Chaisemartin and D'Haultfœuille, 2021). Dynamic effect l compares the evolutions of outcomes between the units (pixels or concessions) that switched into treatment l periods ago and the units that have not yet switched into treatment. If we take into account the proportion of those units to which each dynamic effect applies, then the DID_L estimator's average total effect has a policy interpretation and is an estimator of an ATT, unlike the interpretation of the TWFE estimator (de Chaisemartin and D'Haultfœuille, 2020; 2021). The DID_L's average effect is just a weighted sum of the DID_L instantaneous and dynamic effects.

The DID_L estimator also allows for formal testing of the parallel-trends assumption, with "event-study" graphs that also show the DID_L placebo estimators for each year before the treatment 'switches on' for the first time. We run a joint significance test of such placebo estimators, where the null hypothesis is that all placebo estimators are zero (no differences in pre-treatment trends).

4.1.3. Treatment cohorts

For either treatment, when there are cohorts – i.e., multiple treatment-start years, as in our case – DID_L's estimated 'dynamic effects' blend those cohorts by combining them using common years after the treatment (e.g., the second dynamic effect combines effects in 2005 for a 2003 concession and in 2006 for a 2004 concession). This presumes impact is a function of years after treatment: discouraging illegal invasion can work better over time if word gets out about strong enforcement.

Yet not all impact mechanisms have such a 'years after treatment' property. Thus, we also estimate impacts separately for each cohort. In that approach, any given number of years after a treatment corresponds to a specific calendar year. That can test if, e.g., price shocks driven by global trends affect deforestation rates. Should a price shock drive more extraction of forest across all of a region in a given calendar year, and should concession impacts arise from repelling pressure to follow all agreed concession plans, then impact will be a function of a specific point in time, not years after treatment. For each treatment, we run DID_L for each cohort separately to explore such mechanisms.

When we do blend the cohorts, DID_L allows cohort trends – linear or non-parametric – to account for differential trends across treatment cohorts (e.g., if later concessions were operated by distinct political regimes). Using a categorical variable to define the cohorts – equal to the concession start year if the unit was ever inside a concession, and zero otherwise – DID_L fits forest-loss trends for each cohort, using the pre-treatment forest losses for that cohort (all years for never-treated forest).

To estimate FSC's impact, we follow de Chaisemartin and D'Haultfœuille (2022b) to isolate it from any effects of the concessions. First, we restrict the sample to those concession-year observations during which a concession is active. Second, we include concession cohort trends. This holds the initial concession year constant for comparing the evolution of forest losses between concessions that do versus do not adopt FSC – ensuring that FSC and non-FSC concessions are exposed to the concession treatment for the same number of years (de Chaisemartin and D'Haultfœuille, 2022b).

4.2. Comparing with Two-Way Fixed Effects (TWFE)

4.2.1. Uncertified concessions

Our TWFE specification comparing uncertified concessions with forests outside is:

$$L_{it} = \beta_0 + \beta_1 concession_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where i refers to a forest pixel, L_{it} the share of pixel i deforested in year t; and $concession_{it}$ equals one if an uncertified concession for site i was active in year t, zero otherwise. We add pixel and year fixed effects, α_i and λ_t. The TWFE estimator of uncertified concessions' effect on forest loss is β_1. We clustered standard errors using: the concession ID, when a pixel is completely within a concession; or the pixel ID, when the pixel is completely outside concessions (per our treatment definition). We check for negative weights in order to assess the potential for heterogeneity biases.

[8] The assumption made for 'dynamic effects' is that the impact of a treatment is a function of the time since treatment (like an event study). All past treatments are assumed to drive outcomes (de Chaisemartin and D'Haultfœuille, 2021).

4.2.2. FSC certifications

Our TWFE regression for a concession-level panel comparing the evolution of forest stocks within FSC-certified concessions with the deforestation over time within the uncertified concessions is:

$$L_{jt} = \gamma_0 + \gamma_1 concession_{jt} + \gamma_2 FSC_{jt} + \sigma_j + \lambda_t + \mu_{jt} \tag{2}$$

like (1) above but with an indicator (FSC_{jt}) for whether concession *j* had an active FSC certificate in year *t*. The TWFE estimator of certification's effect on forest loss is γ_2. TWFE is often estimated using all concession-year observations, without restricting to observations for which a concession is active. We follow the literature in this sense to help assess how large is the contamination bias, again calculating the prevalence of negative weights and contamination weights in the TWFE.

4.3. Pre-estimation matching

4.3.1. Uncertified concessions

Forests ever in uncertified concessions had lower average rates of forest loss than untreated forests, for 1986–2018 in our study area (Fig. 4, Panel A). After the concession system started in 2002, we observe a spike up in average loss in the untreated forests, around 2005, and then a trend down. In contrast, these uncertified concessions had a relatively flat if upward trend (Fig. 4, Panel A). Before 2002, without any pre-estimation matching (Fig. 4, Panel A), we can observe that pre-treatment loss trends might not pass as "parallel", which is a threat to our identification strategy.

Thus, we match uncertified concessions with untreated forested pixels using: (i) fixed observable characteristics such as access to roads, rivers, and cities[9]; and (ii) pre-treatment forest-loss levels. Fixed characteristics in (i) are used widely in evaluation of forest policies (Alix-Garcia et al. 2015; Anderson et al., 2019; Blackman et al. 2018; Panlasigui et al., 2018), while (ii) follows the synthetic-cohort literature (Arkhangelsky et al., 2021; Ben-Michael et al. 2021). In both cases, we used 1:1 nearest-neighbor matching with Mahalanobis distances and without replacement. Matching using fixed characteristics did little to find better-fitting controls (Fig. 4, Panel B). However, matching on pre-treatment loss levels shows real improvement in achieving parallel trends (Fig. 4, Panel C). Thus, we employ the matched sample of pixels based upon pre-treatment loss levels to run DID_L and TWFE models.

4.3.2. FSC certification

Average forest losses were similar, for both levels and trends, if comparing ever–FSC–certified to uncertified concessions. That is true both before and after 2006, the year when the first certificate was granted (Fig. 5). Thus, we do not need to pre-match the sample for estimating FSC's impact. We do see 'noisier' 1986–2018 loss levels in ever-certified than in never-certified concessions. However, the rate of forest loss trended upward after 2006 within all concession types (Fig. 5).

5. Results

5.1. Parallel trends & dynamic effects over time

5.1.1. Uncertified concessions

We employ DID_L with concession-cohort linear trends to estimate the effects of uncertified logging concessions on forest loss using a sample of pixels (3 × 3km) matched on pre-2002 loss (Fig. 4, Panel C) for those concessions which were granted in the early 2000s, starting in 2002 (Fig. 1, Panel A). We find that all of the placebo estimators are near to zero, and all are statistically insignificant (Fig. 6, to left of t = 0), with a joint test of all of these placebos' significance failing to reject the null hypothesis with a p-value of 0.40. Thus, the parallel-trends assumption is not violated.

The instantaneous effect (at t = 0), which estimates an impact on forest loss at the start of treatment, suggests no immediate effects of uncertified concessions. Then the dynamic effects, to the right of t = 0, are all slightly negative (Fig. 6). This graph suggests that uncertified concessions clearly did not raise forest loss, and they may have reduced it slightly in the three initial years (statistically significant at the 5% level). This timing is consistent with the spike upward in average forest losses for untreated pixels around 2005 (Fig. 4, Panel C). That suggests perhaps these uncertified concessions were blocking those spikes in forest loss − a mechanism we examine by separating the cohorts to compare the timing of impacts (Section 5.3.1 and Figure A2).

5.1.2. FSC certification

We also run DID_L to estimate the effects of adding FSC certifications to uncertified concessions, seeing no significant violations of pre-treatment parallel trends (Fig. 7, joint test p-value = 0.22). The instantaneous and dynamic effects of FSC on forest loss are all slightly negative: the second and third are statistically significant; while the instantaneous and first, fourth, and fifth dynamic effects are not significant (Fig. 7). Such inconsistency in significance is also present when we varied the specification to check on the robustness of these FSC impact estimates (see Section 5.3).

[9] We calculate distances to the nearest roads, rivers, and cities using data from MINAM. We collect biophysical characteristics (precipitation, elevation, hill shade, slope) from WorldClim (Hijmans et al., 2005) and CGIAR-CSI (Jarvis et al., 2008) global data. We also have included region indicators.

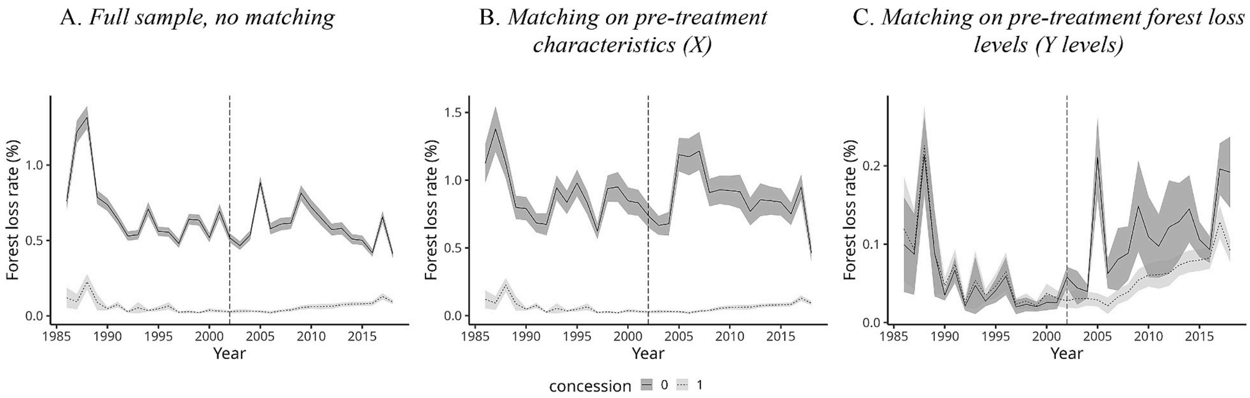


Fig. 4. Mean forest loss trends inside and outside ever uncertified concessions in Peru.
Notes: Mean annual forest loss trends (1986–2018) and 95% confidence intervals by ever concession status (1 = concession, 0 otherwise) for different samples of forested pixels. The vertical line is 2002, the first year concessions were granted. We calculate forest-loss rates from Collection 5 of the MapBiomas Amazon Project (2021). Panel A uses the full sample of pixels (n = 1,228,006). Panel B uses a matched sample based on observable pre-treatment: biophysical characteristics (precipitation, elevation, hill shade, and slope); region; and access to the nearest roads, rivers, and cities. Panel C uses a matched sample based on pixels' pre-treatment forest-loss levels. For panels B and C, we used 1:1 nearest neighbor matching with Mahalanobis distances and without replacement. So the sample size is the same (n = 361,812). Note that the scale of the y axis changes in each panel.

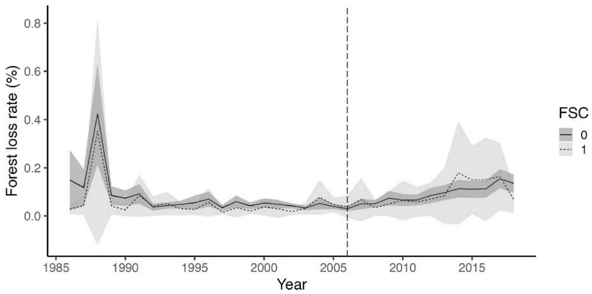


Fig. 5. Mean forest loss trends in ever FSC-certified and Non–FSC–certified concessions in Peru
Notes: Mean forest loss trends (1986–2018) and 95% confidence intervals by ever FSC status (1 = FSC, 0 otherwise) using an annual concession-level panel of all forests inside the 513 post-2000 logging concessions in our study area (n = 16,929 concession-years). The vertical line indicates 2006, the first year an FSC certificate was granted. Loss rates were calculated from Collection 5 MapBiomas Amazon Project (2021).

5.2. Average forest-loss impacts

5.2.1. Uncertified concessions

We estimate uncertified concessions' average DID_L effect with the same (pre-matched) sample and same number of placebo- and dynamic-effect estimators (four) as in Fig. 6 (which supports parallel trends across five years). DID_L's average effect is a weighted sum of the instantaneous and dynamic effects. Here the average total effect of changing a pixel from untreated (no concession) to being treated within an uncertified concession for five years is −0.0451% (Table 1, column 2). That is statistically significant at the 5% level. However, this is a tiny effect, equal to 1/100 of a standard deviation of the sample's 1986–2018 annual forest-loss rates (mean = 0.76, S.D. = 3.76).

We run the regression for equation (1) and find that the TWFE estimator for uncertified concessions' impact on forest loss is also negative and statistically significant, with a magnitude around 10% above the average DID_L estimator (Table 1, column 1). We check the prevalence of negative weights and find none for any pixel-year ATE. Our preferred specification is DID_L as noted above, however, because it is an unbiased estimator of the ATT (see the discussion within Section 4.1.1).

We note that the sample sizes (N) provided by the TWFE and the average DID_L estimators differ. TWFE's N is the number of unit-year observations, a product of the numbers of units and years. N from the average DID_L depends on the number of dynamic effects computed, as it is a weighted sum of instantaneous and dynamic effects. Each of those effects has its own small control group to avoid "forbidden comparisons"[10] (de Chaisemartin and D'Haultfœuille (2022a) – and also see Tables A1 and A2 in Appendix D showing

[10] DID_L's comparison between treated and controls "forbids" comparison with control units that are already treated.

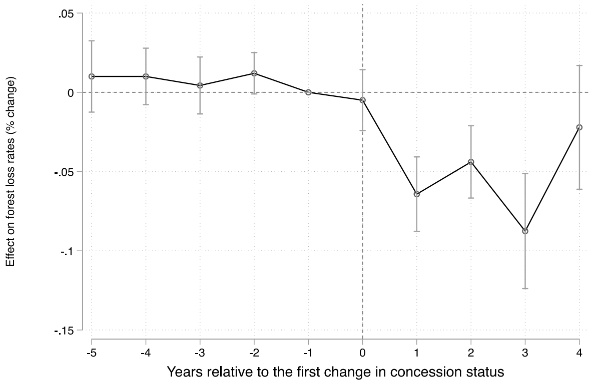


Fig. 6. Forest loss impacts of uncertified concessions in Peru
Notes: This figure shows the results of running de Chaisemartin and D'Haultfœuille (2021) DID_L estimator of forest-loss effects of uncertified concessions relative to the first switch of treatment status. We use a matched pixel-level panel of all forests inside and outside ever uncertified post-2000 concessions in our study area during 1986–2018. The y-axis shows the effects of uncertified concessions, where negative values are reductions in forest loss rates. The p-value of the joint significance test for the placebo estimators is 0.40, which fails to reject the null hypothesis of no pre-treatment trends for 5 years. Thus, we include the instantaneous effect and 4 dynamic effects. Standard errors are clustered at the concession level for pixels inside concessions and at the pixel level for pixels in never treated forests. The average effect of uncertified concessions on forest loss rates with DID_L was −0.0451% (SE = 0.0112, n = 172,853). We calculated forest loss rates from Collection 5 of MapBiomas Amazon Project (2021).

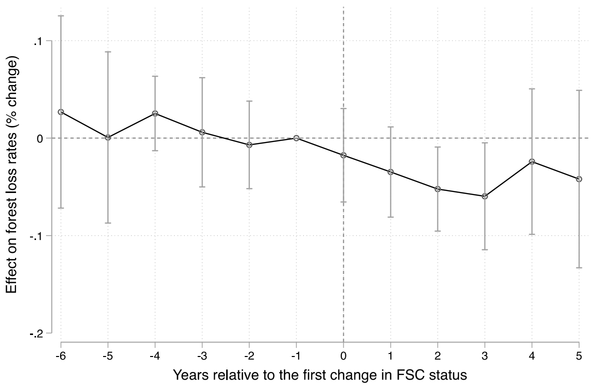


Fig. 7. Forest loss impact of ever FSC-certified concessions in Peru
Notes: This figure shows the results of running de Chaisemartin and D'Haultfœuille (2022b) DID_L estimator of the forest-loss effects due to FSC certification, relative to the first of treatment status, using a concession-level panel and methods described in Section 4.1.3 to isolate the effect of FSC. The y-axis shows the effects of FSC certification, where negative values are reductions in forest loss rates. The p-value of the joint significance test for the placebo estimators is 0.22, which fails to reject the null hypothesis of no pre-treatment trends. The average effect with DID_L was −0.048 (SE = 0 0.026, n = 11,159), insignificant at the 5% level. We calculated forest loss rates from Collection 5 of MapBiomas Amazon Project (2021).

did_multiplegt output with the Ns for our main results).

5.2.2. FSC certification

We use DID_L to estimate an average effect for FSC, using the same panel and specification as in Fig. 7 with five placebo estimators and five dynamic effects, as parallel trends hold for six years. We eliminated inactive concession years and included concession-cohort non-parametric trends to isolate FSC's impacts from effects of concessions (de Chaisemartin and D'Haultfœuille, 2022b). We find a negative but statistically insignificant average effect of FSC certification on forest loss (Table 1, column 4). That is consistent with average loss trends in ever-certified and never-certified concessions looking similar (Fig. 5) – and with some insignificant dynamic effects (Fig. 7). FSC's TWFE estimator is also negative and statistically insignificant, with one fifth the magnitude of the average FSC effect from the DID_L. The difference in magnitude can be explained by the TWFE estimator of FSC impact being heavily contaminated, though, with 71% of ATEs associated with the effects due to concessions getting negative weights (Table 1, column 3; see Section 4.1.1).

Table 1
TWFE and DID_L estimators of forest-loss impacts of uncertified concessions and FSC.

	Uncertified Concessions		FSC Certification	
	TWFE (1)	*DID_L (2)*	*TWFE (3)*	*DID_L (4)*
Average Effect	−0.0500	−0.0451	−0.0080	−0.0479
S.E.	(0.0142)	(0.0112)	(0.0219)	(0.0260)
N	361,812	172,853	16,929	11,159
Spatial Unit	3 × 3 km pixel	3 × 3 km pixel	concession	concession
P-value Placebo Joint Test	–	0.4029	–	0.2234
# Negative Weights/Total ATEs	0/61,479	–	4202/5937	–
Sum of Negative Weights	0	–	−1.1007	–

Notes: We present the TWFE estimator (columns 1 and 3) and de Chaisemartin and D'Haultfœuille (2021; 2022b) average DID_L estimator (columns 2 and 4) for both treatments in the Peruvian Amazon during 1986–2018. The sample size provided by these estimators differs since they use different subsets of the panel data set as explained in Section 5.2.1. Column 1 presents the TWFE estimator of the concession effect on forest loss rates (equation (1)), where none of the ATEs receive negative weights. Column 2 presents the average DID_L estimator of the concession effect with five placebo estimators and dynamic effects. The joint significance test of the placebo estimators rejects the null hypothesis (p-value = 0.40). We clustered standard errors at the pixel level for untreated pixels but at the concession level for pixels inside concessions, when estimating uncertified concessions' effects (Columns 1 and 2). Column 3 presents the FSC coefficient of running a TWFE regression in equation (2). In this case, FSC's TWFE estimator is a weighted sum of two terms. The first term is a weighted sum of FSC's ATEs in each concession and year, while the second term is a weighted sum of the concession effects (de Chaisemartin and D'Haultfœuille, 2022a). Column 3 shows that 4202/5937 ATEs in the second term receive negative weights summing −1.1. Thus, the TWFE estimator of the FSC effect is heavily contaminated by the effect of the concession treatment. Column 4 presents the average DID_L estimator of the FSC effect, which is robust to heterogeneous effects and contamination biases. The joint significance test of the placebo estimators rejects the null hypothesis (p-value = 0.22). We clustered standard errors at concession level for FSC's impacts (Columns 3 and 4). We calculate forest loss from Collection 5 of MapBiomas Amazon Project (2021).

5.3. *Robustness checks*

5.3.1. *Uncertified concessions*

Further DID_L specifications help to assess the robustness of our estimated effects due to uncertified concessions (Table 2). All these suggest that uncertified concessions led to a tiny loss reduction – one that is statistically significant at the 5% level for all but two specifications. Using concession cohort non-parametric trends or no trends at all yields very similar results, though a placebo joint significance test is rejected so we ran a weaker test using first-difference placebos (de Chaisemartin and D'Haultfœuille, 2021) which suggests that the parallel-trends assumption holds for up to seven periods (Figure A3). Shifting the definition of "ever treated" pixels from 100% of pixel area being inside concessions to 80% raises our share of total area covered by concessions, as we drop fewer pixels. However this leads to almost the same results, with a slightly higher magnitude of the effect.

Some analyses suggest deforestation leakages around Peruvian Amazon concessions (Oliveira et al., 2007; Finer et al., 2014; Chavez Solis, 2017). If sites with leakage are in controls – e.g., nearby untreated forest – it can falsely suggest concession impact. We address leakage by dropping from the controls a 10-km buffer around the concessions. This drop has no effect on our estimates. While such a

Table 2
Robustness checks of the effect of uncertified concessions using the DID_L estimator.

	DID_L	Observations	P-value placebo joint test
	(1)	(2)	(3)
Without cohort trends	−0.0499 (0.0081)	172,853	0.0031
With non-parametric cohort trends	−0.0499 (0.0081)	172,853	0.0031
Changing concession threshold to 80%	−0.0454 (0.0180)	201,739	0.5445
Matching pixels further than 10 km from concession boundary	−0.0483 (0.0112)	142,928	0.2035
Without changing treatment to zero after concessions go inactive	−0.0446 (0.0111)	172,853	0.4029
Eliminating concessions that ever expired	−0.0452 (0.0203)	90,621	0.3682
Using Hansen et al.'s (2013) tree-cover loss rates as an outcome (≥50%)	−0.0095 (0.0076)	172,853	0.1528
Using Hansen et al.'s (2013) tree-cover loss rates as an outcome (≥30%)	−0.0098 (0.0075)	172,853	0.1532
Using 9 × 9 km pixels	−0.0335 (0.0094)	22,906	0.1131

Notes: We present the robustness checks of the uncertified concession effect on forest loss rates in Peru during 1986–2018. For the baseline specification, we matched uncertified concession pixels (3 × 3 km) with untreated pixels using 1:1 nearest neighbor matching without replacement and Mahalanobis distance based on pre-treatment forest loss. We calculated annual forest loss rates from Collection 5 of MapBiomas Amazon Project (2021) and created a panel with this sub-sample of pixels. We then ran de Chaisemartin & d'Haultfoeuille's (2021) average DID_L estimator. We clustered standard errors at the pixel level for uncertified pixels and at the concession level for pixels inside concessions. This table shows the average DID_L estimator and standard errors in parentheses (Column 1) for different robustness checks, as well as their sample size (Column 2) and the p-value of the placebo estimators' joint significance test (Column 3). For a couple of robustness checks we used an alternative outcome of tree-cover loss from Hansen et al. (2013), defining forest as at least 50% or 30% tree cover. This outcome is available for 2001–2021, and since the first concession was granted in 2002, we only included two placebo estimators. But we ran it with the pre-matched sample based on pre-treatment MapBiomas outcome. Figure A4 graphs the placebo and dynamic effects using this alternative outcome and a threshold of 50% tree cover.

test does not rule out all the possibilities for leakage, it addresses the most common claims.

Given the share of logging concessions which switched back to inactive during our study period, we run our main specification without ever-cancelled concessions and see almost identical results. We find the same for a robustness check in which the concession treatment indicator is equal to 1 for all concessions throughout the panel once a concession started (this is then a staggered treatment, where no pixel switches back out of a concession treatment). For this context, that estimates the effect of the establishment of a logging concession whether or not that concession remains active.

We also re-check using Hansen et al.'s (2013) measure of tree-cover loss for the forest outcomes, defining forest as at least 50% tree cover or 30% tree cover, respectively. We find a smaller and insignificant effect for both of those forest thresholds, on the order of one fifth of the original effect (dynamic effects in Figure A4, Panel B). Some difference is not surprising, since forest-loss trends differed by source (Fig. 4, Panel A vs. Figure A4, Panel A). Even spikes are in different years.

Our final data robustness check increases the size of the aggregated pixel to 9x9 km. We again see a negative and statistically significant effect, about three fourths the size of the main effect (Figure A5). This reduces sample size by a factor of nine and we can check if adjusting standard errors for pre-matched data shifts conclusions (Figure A6). All of the dynamic effects that were statistically significant still are, although the average effect is no longer significant with this adjustment. Nonetheless, that supports our main conclusion about logging concessions not raising forest loss.

To shed light on the mechanism underlying these small concession impacts in reducing forest loss, we also ran our main DID_L specification for each concession cohort (i.e., 2002, 2003, and 2004). This could find that spikes in deforestation in particular calendar years drive our estimated impacts and, indeed, that is what our results suggest. The third dynamic effect is negative and statistically significant for the 2002 cohort (Panel A, Figure A2), while the most negative dynamic effect for the 2003 cohort is the second (Panel B, Figure A2) and, in turn, the first effect for the 2004 cohort is negative and statistically significant (Panel C, Figure A2). The timing of each cohort's impacts suggests our average effect is driven by the concessions blocking a 2005 spike up in deforestation. This argues against leakage, as impacts do not seem to be due to a drop in concession deforestation.

5.3.2. FSC certification

We check robustness for FSC's estimated effect on forest loss, using the average DID_L estimator (Table 3). Overall, our negative but statistically insignificant effect of FSC on forest loss is robust. Adding linear instead of non-parametric trends reduces the magnitude of the effect by around 40%, while eliminating the cohort trends attenuates impact by 70%. If we keep inactive concession-year observations in the sample instead of dropping them, we find a negative and statistically significant effect of almost the same magnitude as when we employ our preferred sample and specification.

We also check the robustness of this result to the spatial unit of analysis by using different pixel sizes (3 × 3km and 900 × 900m), instead of using the whole concession as the spatial unit. For both of those pixel sizes, we find negative but statistically insignificant estimates of FSC impacts which are smaller in magnitude than the effects estimated using the full concession polygons (Figure A7).

We must mention statistical power, given that only 32 post-2000 concessions were ever certified, while 481 were never certified. On the one hand, we confirm insignificance with pixels that are 10 and 100 times higher in frequency. Yet, on the other hand, we consider it is most sensible to cluster our standard errors at the concession level (such that breaking up a concession polygon in smaller pixels does not actually raise power). We compute a Minimum Detectable Error (MDE = 0.0728, significance level = 5%, power = 80%) post-estimation, and we find that the MDE is larger than the absolute value of our coefficient estimate. Thus, we might not be able to observe the true impact of FSC simply for lack of power. Still, if an effect exists, that threshold indicates a low magnitude.

When we restrict the sample by matching ever-certified concessions just with the never-certified concessions on the basis of the pre-2002 annual rates of forest loss, again we find a small negative and statistically insignificant estimate for the effect upon forest losses

Table 3
Robustness checks of the effect of FSC certification using the DID_L estimator.

	DID_L (1)	Observations (2)	P-value placebo joint test (3)
Adding linear cohort trends	−0.0283 (0.0183)	11,159	0.2409
Without cohort trends	−0.0147 (0.0179)	11,159	0.3455
With all concession-year observations	−0.0482 (0.0232)	17,781	0.5759
Using 3 × 3 km pixels	−0.0217 (0.0283)	131,734	0.6289
Using 900 × 900 m pixels	−0.0299 (0.0333)	1,819,278	0.2974
Pre-matching the data	−0.0111 (0.0247)	2160	0.3867
Using Hansen et al.'s (2013) tree-cover loss as an outcome (≥50%)	−0.0298 (0.0792)	11,159	0.3035

Notes: We present the robustness checks of the FSC certification effect on forest loss rates. We calculated annual forest loss rates from Collection 5 of MapBiomas Amazon Project (2021). The main specification runs the average DID_L estimator with concession cohort non-parametric trends using a concession-level panel and eliminating concession-year observations if concessions were inactive as recommended by de Chaisemartin and D'Haultfœuille (2022b). We cluster standard errors at the concession level. Column 1 shows the average DID_L estimator and standard errors in parentheses for different robustness checks. Column 2 shows their sample size and Column 3 presents the p-value of the placebo estimators' joint significance test. We ran two robustness checks that change the spatial unit of analysis from polygon to aggregated pixels (either 3 × 3 km or 900 × 900 m). Figure A7 plots their instantaneous and dynamic effects. For another check, we matched the concessions based on pre-2002 loss levels (i.e. the first year that concessions were granted) using Propensity Score Matching with three neighbors and replacement. Finally, we used an alternative outcome of tree-cover loss from Hansen et al. (2013), available for 2001–2021. Since the first concession was granted in 2002 and the first certificate started in 2006, we only included four placebo estimators. Figure A4 graphs the placebo and dynamic effects using this outcome.

due to FSC certifications. And when using Hansen et al.'s (2013) tree-cover loss rates as the outcome, instead, the average reduction of FSC certification is 30% smaller and insignificant (see Figure A4, panels C and D).

6. Conclusion

We estimate forest-loss impacts of logging concessions themselves, as well as additional impacts from FSC certification of their forest-management practices, all for the Peruvian Amazon − which is the fourth-largest area of tropical forest in the world (Ministerio del Ambiente, 2015). We find no significant rise in rates of forest loss due to the granting of logging rights within uncertified concessions. If anything, concessions ward off temporary spikes in external deforestation pressure, slightly reducing loss. That supports hypotheses based on firms' incentives to defend forest assets, and timber profits, by excluding other actors who might deforest. As one comparison for this effect, this small forest benefit of uncertified concessions is much like that generated by strict protected areas in this region – while lower than that from multiple-use PAs which allowed limited activities (Rico-Straffon et al., 2022; Figure A8 for detail). As this suggests some capacity of the concessionaires to defend forests, we would think it possible that adding FSC certifications could further reduce forest losses. Yet we found that FSC certifications generated no significant impacts.

Our results shed empirical light on allowing extractive economic activities in forests (concessions) and adding FSC's conservation restrictions on activities to balance sustainable development goals. We identify several avenues for future research along these lines. First, adding analyses of impacts from these interventions upon economic and social outcomes could further inform policymakers concerning the details of sustainable-development tradeoffs across, or complementarities between, different forest-based objectives. Second, due to incomplete data on firm characteristics, we could not investigate if these impacts were heterogeneous by firm type. Concessions and certifications might have impacts for some firms, if different types of companies utilize these logging rights or respond to these restrictions differently. For instance, firms which export from both certified and uncertified concessions may raise demand for uncertified timber too, if an FSC label helps them to market some practices as sustainable. Thus, higher forest output, added to no impact from FSC, could hurt forests on net. Yet small firms that were not already meeting FSC's standards in their few concessions may instead reduce forest losses. Third, more precise data about where and when logging takes place within concessions (e.g., detailed management plans) could allow answers to questions about concession compliance. Fourth, the fact that firms self-selected and the location of concessions was not random is a threat to identification. While we did the best we could with the data in hand, future studies could try to identify exogenous variation to address this limitation. Finally, one could analyze other forest indicators, such as forest fires and degradation. Our lack of data on forest degradation is a clear limitation for the present study. Given the strong trend toward improvements in forest data, though, we can expect ongoing improvements in policy evaluations.

Declaration of competing interest

The authors declare that the World Wildlife Fund (WWF) was the source for the geospatial data on concession boundaries which got this research project started. They do not have paid or unpaid positions as relevant officer, director or board member that create conflict with being an author of the analyses. In 2015, Rico-Straffon and Panlasigui received WWF's financial support to extend research done for their master's projects at Duke University.

Appendix

Table A1
DID_L Estimators of Forest-Loss Impacts of Uncertified Concessions. Main Specification

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect 0	−0.0049	0.0098	−0.0242	0.0143	38,739	5482
Effect 1	−0.0642	0.0120	−0.0877	−0.0408	35,363	5482
Effect 2	−0.0439	0.0116	−0.0667	−0.0211	32,953	5482
Effect 3	−0.0876	0.0185	−0.1239	−0.0514	32,906	5482
Effect 4	−0.0221	0.0199	−0.0612	0.0170	32,892	5482
Average Effect	−0.0451	0.0112	−0.0670	−0.0231	172,853	27,410
Placebo 1	0.0120	0.0067	−0.0010	0.0251	38,739	5482
Placebo 2	0.0043	0.0092	−0.0136	0.0223	35,363	5482
Placebo 3	0.0100	0.0091	−0.0078	0.0278	32,953	5482
Placebo 4	0.0100	0.0115	−0.0125	0.0325	32,906	5482

Notes: This table corresponds to the results shown in Fig. 6. This is the output of running the *did_multiplegt* command in Stata to obtain de Chaisemartin & d'Haultfoeuille's (2021) DID_L estimators. See more details in Sections 4 and 5. This table shows the estimator, standard errors, the lower bound and the upper bound of the 95% confidence intervals, as well as the sample size and the number of switchers.

Table A2
DID_L Estimators of Forest-Loss Impacts of FSC Certification. Main Specification

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect 0	−0.0176	0.0245	−0.0655	0.0304	2160	32
Effect 1	−0.0348	0.0236	−0.0810	0.0115	2084	32
Effect 2	−0.0522	0.0220	−0.0954	−0.0091	1885	32
Effect 3	−0.0597	0.0280	−0.1145	−0.0048	1727	32
Effect 4	−0.0241	0.0381	−0.0987	0.0505	1675	32
Effect 5	−0.0420	0.0464	−0.1330	0.0490	1628	32
Average Effect	−0.0479	0.0260	−0.0988	0.0031	11,159	192
Placebo 1	−0.0069	0.0229	−0.0518	0.0380	2155	32
Placebo 2	0.0060	0.0286	−0.0500	0.0621	1831	32
Placebo 3	0.0253	0.0195	−0.0129	0.0635	1388	32
Placebo 4	0.0007	0.0448	−0.0871	0.0885	1174	31
Placebo 5	0.0268	0.0503	−0.0718	0.1254	931	22

Notes: This table corresponds to the results shown in Fig. 7. This is the output of running the *did_multiplegt* command in Stata to obtain de Chaisemartin & d'Haultfoeuille's (2022b) DID_L estimators. See more details in Sections 4 and 5. This table shows the estimator, standard errors, the lower bound and the upper bound of the 95% confidence intervals, as well as the sample size and the number of switchers.

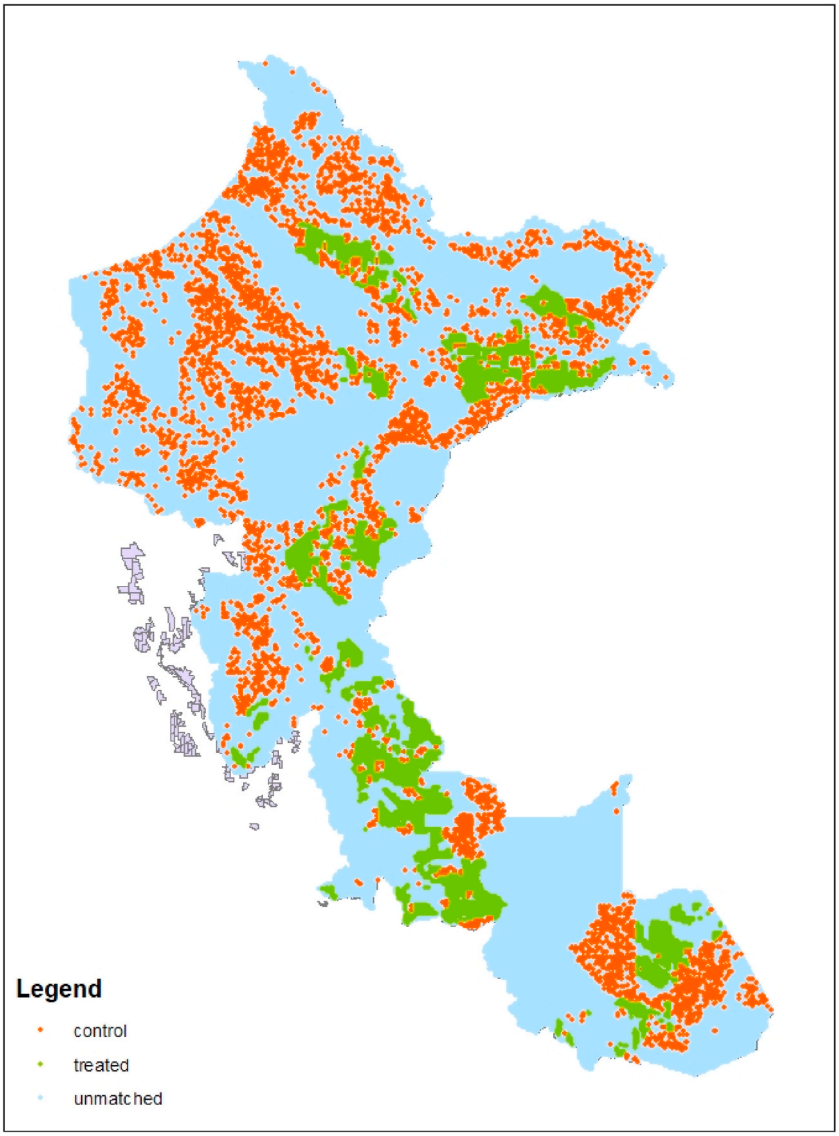


Fig. A1. Matched sample of pixels used to estimate Uncertified concessions' effects
Notes: In green, the treated pixels (3 × 3 km) that fall within logging concessions' boundaries in our study area: Loreto, Madre de Dios, and Ucayali.

We excluded the 91 concessions which fall outside those regions and represent about 10% of the total concession area in Peru. We matched treated concession pixels to untreated pixels in our study area, as described in the Empirical Strategy. Matched control pixels are shown in red, with the unmatched shown in blue.

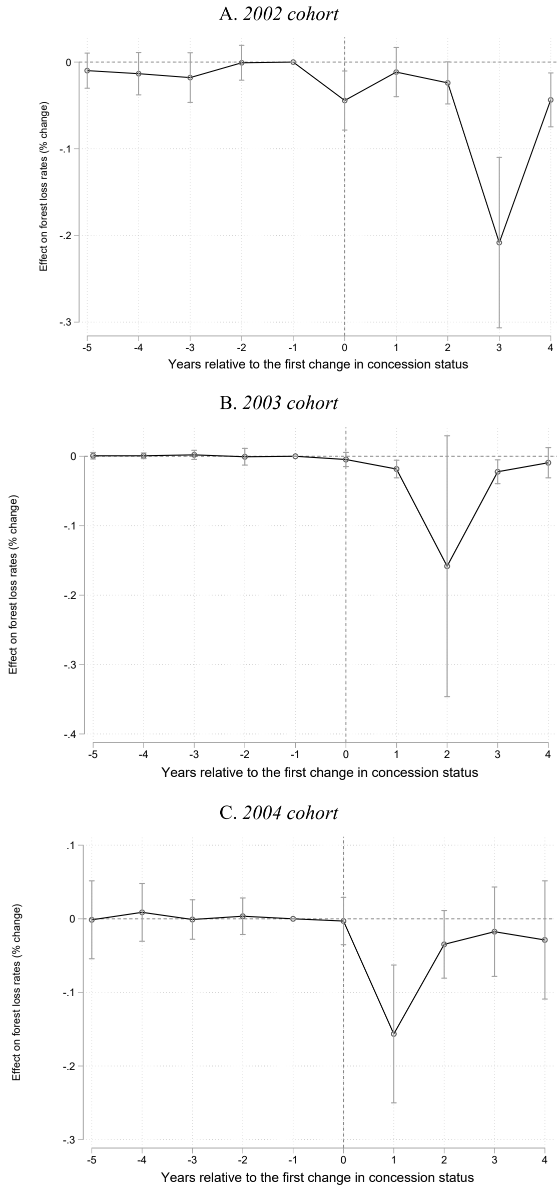


Fig. A2. Uncertified concessions' forest loss impacts by cohort
Notes: We constructed an annual pixel-level panel (3 × 3 km) of all forests inside and outside ever uncertified concessions during 1986–2018 in Madre de Dios, Loreto, and Ucayali. We calculated forest loss rates from Collection 5 of MapBiomas Amazon Project (2021). This figure shows the results of running de Chaisemartin and D'Haultfœuille (2021) DID_L estimator of the forest loss effects of uncertified concessions separately for each concession cohort (we excluded 2005 and 2006 because they had too few observations). For each cohort we matched pixels based on pre-treatment forest loss levels up to one year before treatment. We clustered standard errors at the pixel level for uncertified pixels and at the concession level for pixels inside concessions. Note that the vertical line at t = 0 is 2002 for Panel A, 2003 for Panel B, and 2004 for Panel C. The y-axis shows the effects of uncertified concessions, where negative values are reductions in forest loss rates. We ran a joint significance test for the placebo estimators (to the left of t = 0), which failed to reject the null hypothesis of no pre-treatment trends for all cohorts. The average effect of uncertified concessions on forest loss rates with DID_L was −0.067% (SE = 0.014, n = 21,060, p-value joint test = 0.560) for the 2002 cohort, −0.043% (SE = 0.020, n = 9,660, p-value joint test = 0.967) for the 2003 cohort, and −0.049% (SE = 0.024, n = 23,630, p-value joint test = 0.881) for the 2004 cohort.

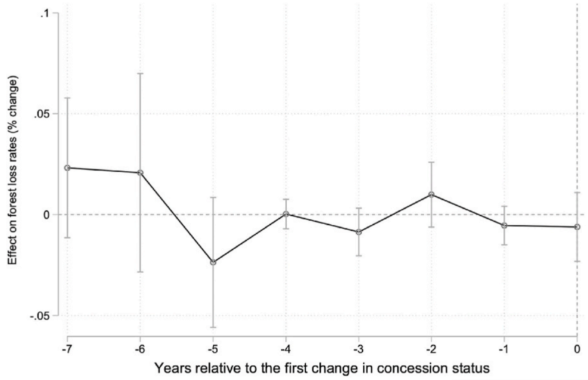


Fig. A3. First-difference placebos of uncertified concessions' effects
Notes: This figure shows the first-difference placebos of the uncertified concession effects with no cohort trends. See more details in de Chaisemartin and D'Haultfœuille (2021). The p-value of the placebo joint significance test is 0.25.

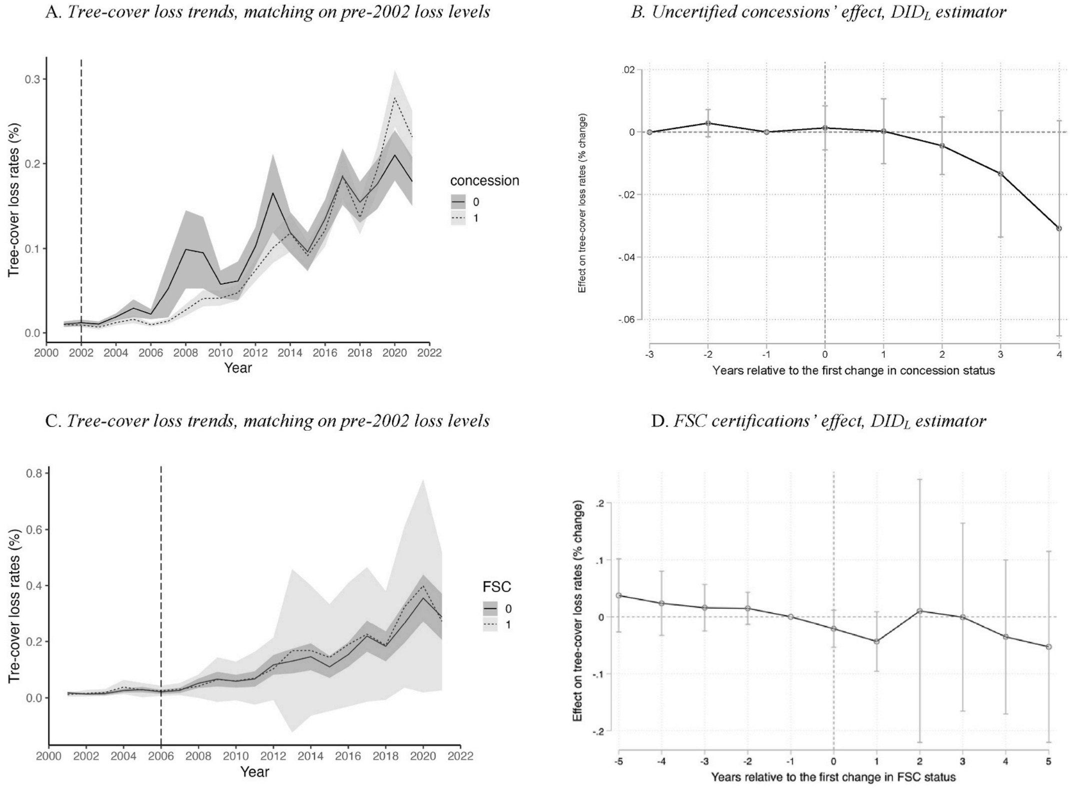


Fig. A4. Tree-cover loss trends and effects with Hansen et al. (2013) data
Notes: The outcome variable is tree-cover loss from Hansen et al. (2013) available for 2001–2021. We define 'forest' as at least 50% tree cover. To evaluate uncertified concessions, we constructed an annual pixel-level panel (each pixel is 3 × 3 km) of all forests inside and outside ever uncertified concessions in Madre de Dios, Loreto, and Ucayali. Panel A shows mean loss trends and 95% confidence intervals by ever concession status in a matched sample of pixels based on each pixel's pre-treatment forest loss levels. Pre-treatment loss levels come from MapBiomas data which started in 1986 while Hansen et al. (2013) started in 2000. Panel B shows de Chaisemartin and D'Haultfœuille (2021) DID_L estimators of the forest loss effects of uncertified concessions relative to the first switch of treatment (n = 172,853). The y-axis shows the effects of uncertified concessions. The p-value of the placebo joint significance test is 0.15. The average DID_L effect of uncertified concessions is −0.0095% (SE = 0.0076). We clustered standard errors at the pixel level for uncertified pixels and at the concession level for pixels inside concessions. To evaluate the additional effects of FSC certification, we constructed a yearly concession-level panel of all forests ever inside logging concessions in our study area. Panel C shows concessions' mean loss trends and 95% confidence intervals by ever FSC status, where the vertical line indicates 2006, the year when the first certificate was granted. Panel D shows the DID_L estimators of the forest loss effects of FSC certification (n = 11,159) using the strategy outlined in de Chaisemartin and D'Haultfœuille (2022b). The y-axis shows the effects of FSC certification. The p-value of the placebo joint test is 0.30. The average DID_L effect of FSC certification is −0.0298% (SE = 0.0792). We clustered standard errors at the concession level. .

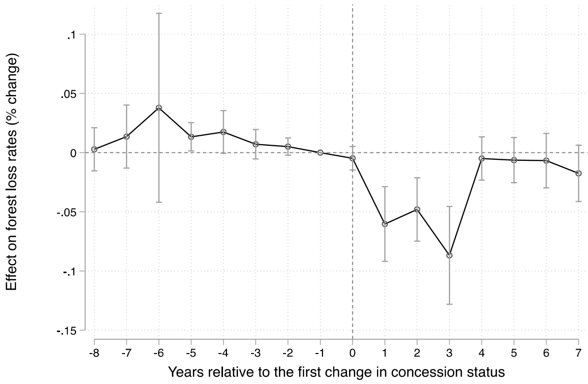


Fig. A5. Uncertified concessions' forest loss impacts with 9 × 9 km pixels
Notes: This figure shows the results of running de Chaisemartin and D'Haultfœuille (2021) DID_L estimator of forest-loss effects of uncertified concessions relative to the start of treatment for an alternative spatial unit: 9 × 9 km pixels. We use the same procedure described in Section 4 to construct a matched panel of pixels of forests inside and outside ever uncertified concessions in our study area during 1986–2018. We matched pixels based on pre-treatment loss using Propensity Score Matching with replacement and 3 neighbors. To define treated pixels, we used a threshold of 95% instead of 100% to have better coverage of concession area. We calculated forest loss rates from Collection 5 of MapBiomas Amazon Project (2021). The y-axis shows the effects of uncertified concessions, where negative values are reductions in forest loss rates. The p-value of the placebo joint significance is 0.11, which fails to reject the null hypothesis of no pre-treatment trends. The average DID_L effect of uncertified concessions on forest loss rates is −0.033% (SE = 0.009, n = 22,906). Standard errors are clustered at the concession level for pixels inside concessions and at the pixel level for pixels in never treated forests.

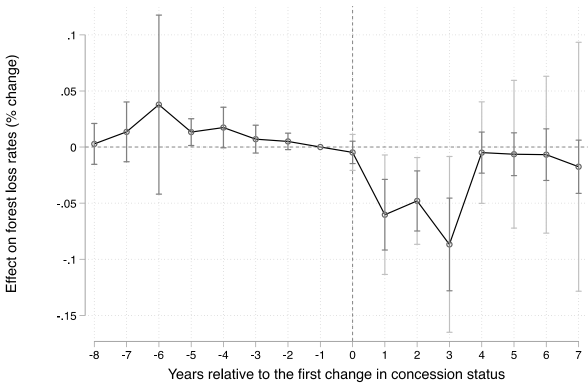
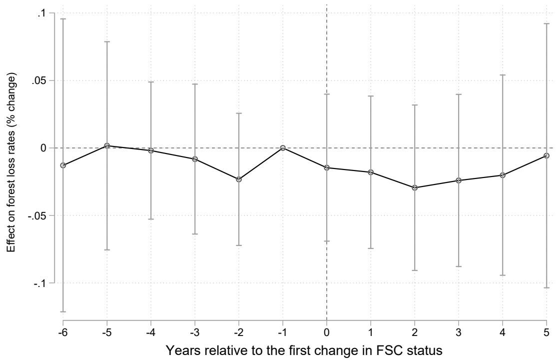


Fig. A6. Uncertified concessions' forest loss impacts with 9 × 9 km pixels, adjusting standard errors for matching
Notes: This figure shows the same results presented in Figure A5 above, showing the original confidence intervals (CI) in dark gray and adjusted CIs of the instantaneous and dynamic effects in light gray. The adjustment to standard errors (SE) was done to account for the fact that we matched uncertified concession pixels with untreated pixels using Propensity Score Matching with replacement and three nearest neighbors prior to running DID_L. We used 100 bootstrapped samples of pixels to run the matching, create the panel, and run DID_L. The adjusted SE for effect $k \in \{0,..7\}$ is the standard deviation of the 100 bootstrapped estimates of effect k. The average DID_L effect of uncertified concessions on forest loss rates is −0.033% (SE = 0.009, adjusted SE = 0.023, n = 22,906). We calculated forest loss rates from Collection 5 of MapBiomas Amazon Project (2021). .

A. Pixels, 3 x 3 km



B. Pixels, 900 x 900 m

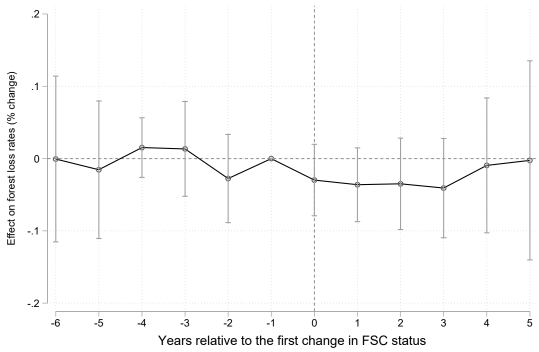


Fig. A7. FSC certification's forest loss impacts with different spatial units

Notes: We constructed two annual pixel-level panels (3 × 3km and 900 × 900m) of all forests inside and outside ever uncertified concessions during 1986–2018 in Madre de Dios, Loreto, and Ucayali. We calculated forest loss rates from Collection 5 of MapBiomas Amazon Project (2021). This figure shows the results of running de Chaisemartin and D'Haultfœuille (2021) DID_L estimator of the forest loss effects of FSC certification with two different pixel sizes instead of concession polygons. We defined treated pixels as those with 100% of their area inside a concession, untreated as 0% of their area intersecting with a concession, and we dropped the pixels with a partial intersection with a concession as described in Section 3.3. We ran the same specification as in Fig. 7.

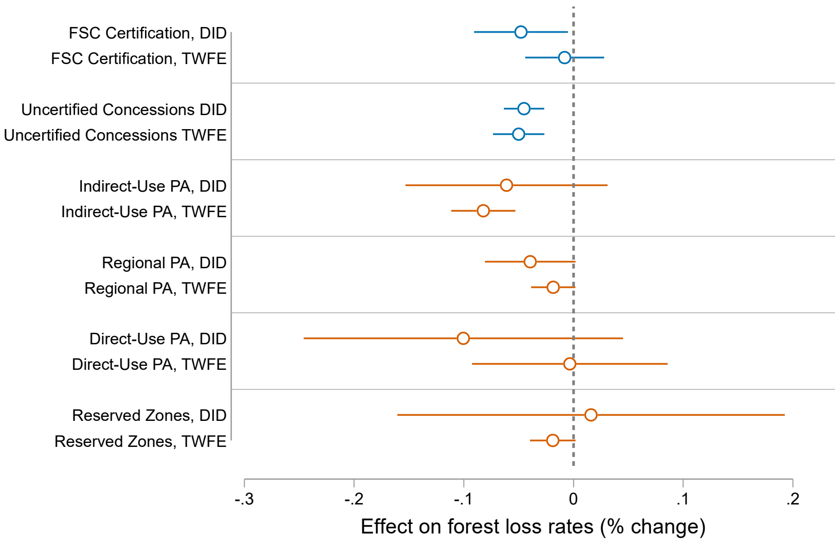


Fig. A8. Comparison of forest loss impacts from different forest policies in the Peruvian Amazon

Notes: This figure compares the forest loss impacts of FSC certification and uncertified concessions estimated in this paper with the impacts of three different types of protected areas (PAs), as well as reserved zones in the Peruvian Amazon estimated in Rico-Straffon et al. (2022). Indirect-Use PAs are the strictest PA type. Direct Use and Regional PAs allow some resource extraction by local communities. Reserved Zones are transitory areas that are in the process of becoming a PA, so they are the least strict since they allow more economic activities. All impacts were estimated using the same data, methods, study area, and period. The spatial unit is 9 × 9 km for PAs and reserved zones, 3 × 3 km for uncertified concessions, and the concession polygon for FSC certification. For each forest intervention, we show de Chaisemartin and D'Haultfœuille (2021; 2022b) average DID_L estimators of the forest loss impacts clustering standard errors at the forest policy level for pixels within the boundaries of a concession or a PA, and at the pixel level for pixels outside concessions or PAs. We also present the TWFE estimator of the forest loss impacts using the same clustering as in DID_L. Note that we ran different regressions for each treatment. For each estimator, the graph shows the point estimate in a circle, as well as the 90% and 95% confidence intervals, in dark and lighter color respectively.

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