Learning lessons from over-crediting to ensure additionality in forest carbon credits

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Abstract

Independent evaluations have shown substantial over-issuance of REDD+ (Reducing Emissions from Deforestation and Degradation) credits traded on the voluntary carbon market. To improve credit integrity we need to synthesise these evaluations to understand the additional forest conservation achieved by first-generation REDD+ projects, and explore the mechanisms by which over-crediting occurred. We bring together six independent ex-post evaluations of avoided deforestation by 44 REDD+ projects. These consistently show that most projects successfully reduced deforestation, but they also claimed an aggregate of 10.7 times more avoided deforestation than independent evaluations indicate is justified. This discrepancy is not an artefact of which forest cover layer is used but is linked to selection bias in projects'

choices of control areas and modelling approaches. Given the need to close the forest finance gap, there is unwillingness to abandon the potential of the voluntary carbon market because of past failures. Current initiatives which transfer assessment to unconflicted parties and eliminate methodological flexibility is critical but insufficient. Ex post certification against a credible counterfactual is also necessary to ensure sufficient integrity in carbon markets for them to contribute to the vital task of slowing deforestation.

Introduction

Halting tropical deforestation is essential to limit global temperature rise to below 2°C[1] and prevent mass extinction[2,3]. Yet tropical forests continue to be lost[4,5] and forest conservation is severely underfunded, with an estimated annual finance gap of US $216 billion[6]. There is substantial global interest in the potential for both compliance and voluntary markets to fund forest conservation despite widespread evidence that many first-generation REDD+ (Reducing Emissions from Deforestation and forest Degradation) projects have issued more credits than were justified by their impact on deforestation[7–11]. The resulting scandal[12] contributed to more than US $1.1Bn being wiped from the value of the voluntary carbon market in 2023 and a further contraction in 2024[13]. Understanding what these early projects did and did not achieve, and the mechanisms that resulted in over-crediting, is key to informing efforts to improve the integrity of forest carbon credits, which is an essential step if they are to contribute to closing the forest finance gap.

A major challenge in measuring the impacts of forest conservation projects is estimating counterfactual outcomes (what would have happened to deforestation in the absence of a project)[14–18]. The first-generation of REDD+ projects could choose from several certification methodologies (Table 1a), each of which used measurements of historic deforestation in expert-selected ‘reference’ areas in conjunction with *ex ante* modelling approaches to predict ‘baseline’ deforestation[11]. Credits were issued by comparing the *ex ante* baseline with observed deforestation in project areas. In parallel, a rich scientific literature has developed methods for estimating counterfactual outcomes from observational data[19–22] and for applying them to conservation[14,15,17,18]. These quasi-experimental methods account for confounders in selecting control areas and measure outcomes *ex post* in both project and control areas (Table 1b).

The prominence of REDD+ has drawn scrutiny from several independent research groups that have applied these quasi-experimental methods to evaluate the impact of first-generation REDD+ projects on deforestation[7–9,23–25]. Consistently, these studies have found evidence of over-crediting. Proposed explanations include the use of inappropriate reference areas, unrealistic *ex ante* modelling that exaggerated expected deforestation, and selective use of certification methodologies that inflated credit issuance[7,9,11,23]. However, these hypothesised mechanisms have not been systematically tested[10]. Some industry insiders have rejected the evidence for over-crediting altogether[26,27], arguing that (1) quasi-experimental estimates of avoided deforestation are inconsistent across studies due to methodological differences[28], and (2) over-crediting is an artefact of using of publicly available global forest cover datasets that detect less deforestation than the bespoke remote sensing classifications used by projects[29].

Carbon markets are, of course, not the only way tropical forest conservation can be funded[30,31]. There are well-known problems associated with carbon offsetting[32] and with the integrity of forest carbon credits beyond the issues of over-crediting and additionality[33,34]. Nevertheless, carbon markets are likely to play a role in bridging the forest funding gap, at least in the short to medium term[6]. It is therefore critical to learn as much as possible from the first-generation of REDD+ projects, both their successes and failures, to inform the future development of methods for certifying credits. Given how harmful over-crediting is to the overall objective of carbon markets (reducing carbon in the atmosphere), understanding the mechanisms through which over-crediting occurred is vital if future approaches are to avoid repeating the failures of the past.

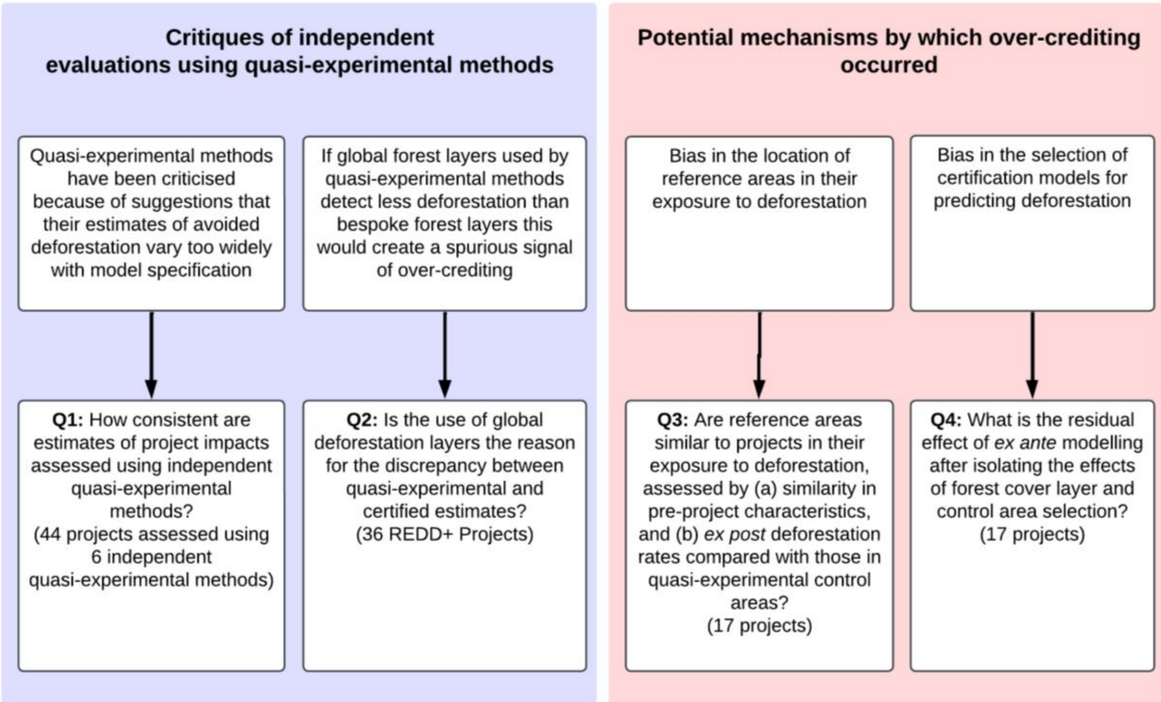

Figure 1: Research questions exploring the mechanisms behind over-crediting. The analyses test critiques of independent quasi-experimental evaluations of REDD+ projects and investigates mechanisms that may have led to over-crediting. Specific research questions (Q1-4), and the number of REDD+ projects with data available to answer each question are shown. The list of projects included in each analysis is given in Supplementary Table S2.

In this study, we synthesise results from six quasi-experimental evaluations to provide a comprehensive assessment of the impact of 44 first-generation REDD+ projects (representing 45% of the projects producing credits by 2020). We compare these results with certified assessments from the projects themselves. We address critiques of independent quasi-experimental evaluations (questions 1 and 2 in figure 1) and investigate two hypothesised mechanisms by which over-crediting could have occurred (questions 3 and 4).

Our results show that most projects slowed deforestation consistently across independent evaluations, yet over-crediting remained substantial. The discrepancy between quasi-experimental and certified estimates cannot be explained by differences in the remote sensing layers used, rather, it stems from bias in reference-area selection and unrealistic modelling of future deforestation. These findings point to changes needed in crediting methodologies to ensure forest carbon credits deliver their intended climate benefits.

Table 1. Description of the methods / studies used to evaluate REDD+ projects.

Method / study	**Method for estimating the counterfactual rate of deforestation**	**Unit size**	**Forest cover layer**[Φ]	**Number of projects**[Ω]
*a. Certification methodologies used by first-generation REDD+ projects **				
VM0006[†]	Reference area selected with exposure to drivers of deforestation within ±10% of the range observed in project area. Counterfactual deforestation rates are predicted *ex ante* from beta regression or historical average of deforestation observed in reference area during reference period.	Whole project	Bespoke	11
VM0007[‡]	Reference area selected with exposure to drivers of deforestation within ±20% of the range observed in project area to measure historic deforestation. Counterfactual deforestation rates are predicted *ex ante* from linear or non-linear regression, or historical average of deforestation or population growth observed in the control rea during reference period. These are allocated to the project using a spatial risk model produced for the area surrounding the project which should have exposure to drivers of deforestation within ±30% of the range observed in project.	Whole project	Bespoke	15
VM0009	Reference areas are selected with no defined requirement for their similarity to the project in exposure to drivers of deforestation. Counterfactual deforestation rates are predicted *ex ante* from logistic regression of deforestation observed in the reference area during the reference period.	Whole project	Bespoke	3
VM0015[‡]	Reference areas selected with exposure to drivers of deforestation within ±10% of the range observed in project area. Counterfactual deforestation rates are predicted *ex ante* from linear or non-linear regression or historical average or	Whole project	Bespoke	15

	simulation approaches of deforestation observed in the reference area during the reference period.			
b. Quasi-experimental methods				
West et al., 2020	Synthetic control produced by weighting untreated polygons of land titles by their similarity to projects in terms of deforestation trajectories and observable confounders. Deforestation in project and control areas observed ex *post*.	Whole project	MapBiomas (reprocessed)	11
West et al., 2023	Synthetic control developed from randomly placed circular plots weighted by similarity to projects in terms of deforestation trajectory and observable confounders. Deforestation in project and control areas observed ex *post*.	Whole project	GFC	18
Guizar-Coutiño et al., 2022	1:1 matching with replacement selected from untreated control units based on similarity in observable confounders. Deforestation in project and control areas observed ex *post*.	0.09 ha pixel	ACC	35
Guizar-Coutiño et al., 2025	1:1 Propensity score matching with replacement via random forests, followed by propensity score sub-classification on the matched dataset to ensure covariate balance. The average treatment effect was estimated using multiple model specifications to assess robustness and sensitivity to unobserved confounding evaluated using the *sensemakr* framework[35]. Deforestation in project and control areas observed ex *post*.	7.10 ha plot	ACC	32
Tang et al., 2025	Penalized synthetic control developed from randomly placed circular plots weighted by similarity to projects in terms of deforestation trajectory and observable confounders. Deforestation in project and control areas observed ex *post*.	Whole project	GFCI	37
PACT	1:1 matching without replacement selected from untreated control units based on similarity in observable confounders. Bootstrapped 100 times using random samples from candidate controls. Deforestation in project and control areas observed ex *post*.	0.09 ha pixel	ACC	35

[Φ] Forest cover layers were either locally trained 'bespoke' classifications used in certified estimates by projects, or region or global peer-reviewed data products, specifically: MapBiomas, Global Forest Change (GFC) or the Tropical Moist Forest Annual Change Collection (ACC)[5].

[Ω] The projects included in our analysis were those for which we were able to obtain certified estimates of avoided deforestation (S2) and were assessed by at least one quasi-experimental approach.

* West, Bomfim and Haya applied all Verra methodologies to assess the avoided deforestation impacts of four REDD+ projects.

[†] For VM0006 the forest scarcity factor was applied at two separate levels, resulting in two separate estimates.

[‡] For VM0007 and VM0015 two different algorithms were used to develop spatial deforestation risk maps, resulting in two separate estimates for each.

Results

Q1. How consistent are estimates of project impacts assessed using independent quasi-experimental methods?

The majority of projects (36 of 44) reduced deforestation according to assessments by the quasi-experimental methods (Figure 2). Among projects with multiple quasi-experimental estimates, 12 (29% of the 42 with calculable confidence intervals) showed statistically significant reductions in deforestation at the 95% confidence level. Eight projects (shown in red in Figure 2) experienced more deforestation than their quasi-experimental controls, although only in one case was this significant.

Despite most projects reducing deforestation, over-crediting was widespread with certified estimates of avoided deforestation significantly exceeding mean quasi-experimental estimates (one-tailed paired Wilcoxon signed-rank test; $V = 973$, $p < 0.001$). Over-crediting was statistically significant at the 95% confidence level for 32 projects (76% of the 42 with calculable confidence intervals; Fig. 2a). Consequently, the mean over-crediting ratio (the certified estimate divided by the mean quasi-experimental estimate) was 4.1 (95% bootstrapped CI: 2.7 to 7.0), indicating the typical project achieved about a quarter of the avoided deforestation reported by certified estimates. Across all the credits assessed, the global over-crediting ratio was 10.7 (95% bootstrapped CI: 5.4 to 26.5), indicating that a credit bought at random would offer only an eleventh of the avoided deforestation claimed (Fig. 2b). Over-crediting was not universal: two projects from Madagascar and one from Peru had a mean over-crediting ratio less than 1 meaning these credits achieved more avoided deforestation than claimed (S8).

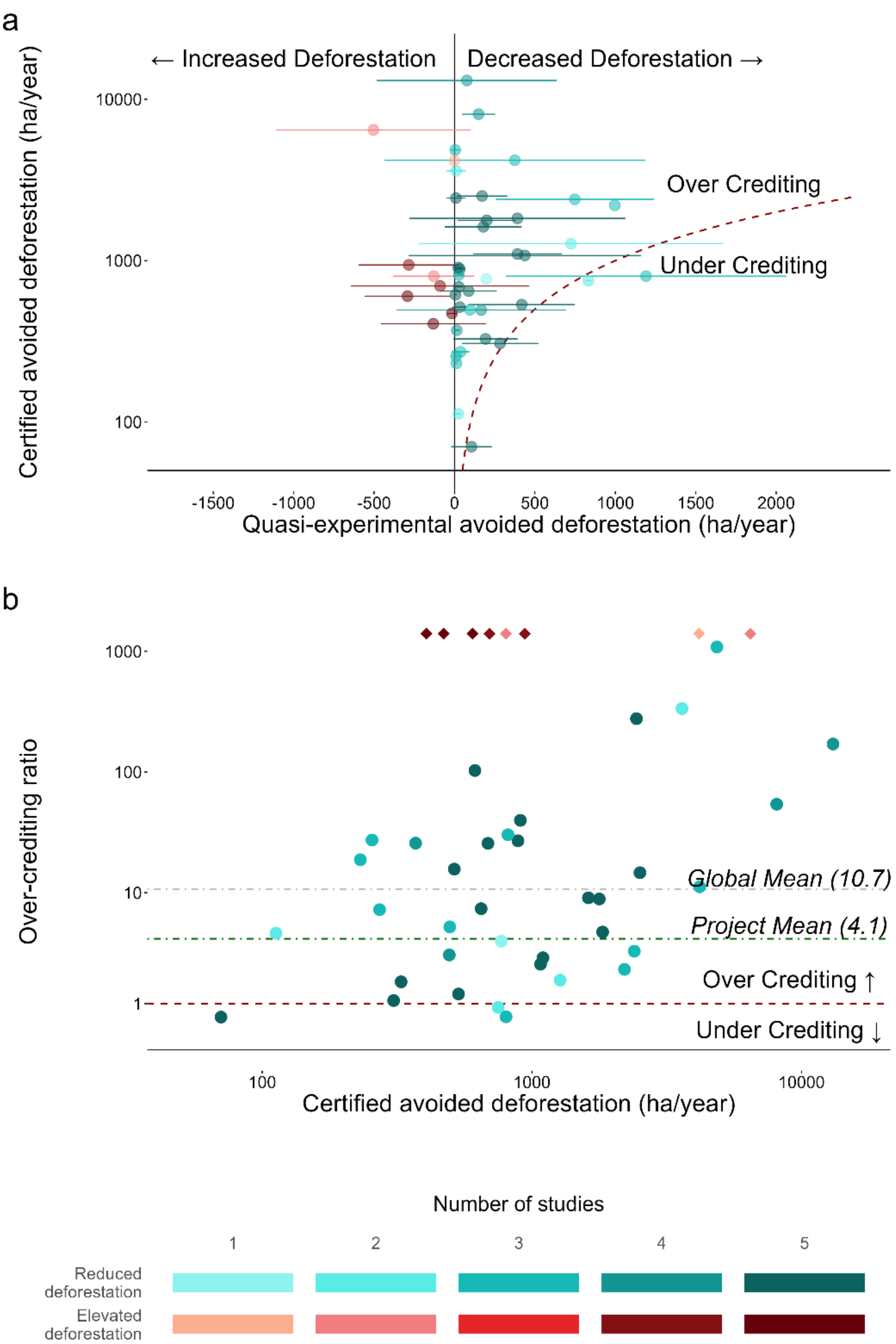

Figure. 2: Comparison of quasi-experimental and certified estimates of project performance. (a) Quasi-experimental evaluations show consistent evidence that REDD+ projects slowed deforestation, but certified estimates were higher, indicating widespread over-crediting. Points represent mean estimates from the six quasi-experimental studies, with error bars show 95% confidence intervals. (b) The over-crediting ratio (the certified estimate divided by the mean quasi-experimental estimate) plotted against certified avoided deforestation estimates. The dashed grey line indicates the global mean over-crediting ratio and the dashed red lines mark parity between certified and quasi-experimental estimates. Points above and below the red line represent over- and under-crediting respectively. Darker

colours denote projects evaluated by a greater number of quasi-experimental approaches (up to five as there was no overlap in the projects assessed by the two West et al. studies). The over-crediting ratio is undefined for the eight red points that experienced more deforestation than predicted by their quasi-experimental controls. In both panels use log-scaled y-axes.

Q2. Is the use of global deforestation layers the reason for the discrepancy between quasi-experimental and certified estimates?

A high-profile critique of independent studies which demonstrated over-crediting by first-generation REDD+ projects has suggested that global layers used in quasi-experimental analyses detect less deforestation than bespoke layers used by projects and that this, rather than over-crediting, explains the observed discrepancies[29]. We tested this critique by comparing estimates of deforestation (loss of undisturbed forest) in project areas derived from global deforestation layers with those reported by projects using their bespoke forest cover classifications. We were unable to perform the comparison for reference areas because deforestation rates there were not reported in project certification documents. For project areas, we found that, rather than being lower, deforestation rates measured independently using the European Union's tropical moist forest annual change collection (ACC[5]) were actually greater than certified measurements based on bespoke forest cover layers. Among the 36 projects that reported certified measurements, the median rate measured using the ACC was 0.26%/year, compared with a median certified deforestation rate of 0.17%/year (Fig. 3a). The median pairwise discrepancy between ACC and certified rates was 0.13%/year (Fig. 3b), which was significantly different from zero (*Wilcoxon paired signed-ranks test*, V = 520, d.f. = 35, $p = 0.003$). When we ran the analysis to include degraded forest in the ACC-measured forest area at project start, so that deforestation was only measured as long-term (>2.5 years) transitions to non-forest, deforestation rates were lower and not significantly different from certified values (S4). Together these results indicate that deforestation products (such as the ACC) used in quasi-experimental analyses have not detected less deforestation than the bespoke layers used in certification. Under-estimation of deforestation in control areas therefore cannot explain the observed over-crediting.

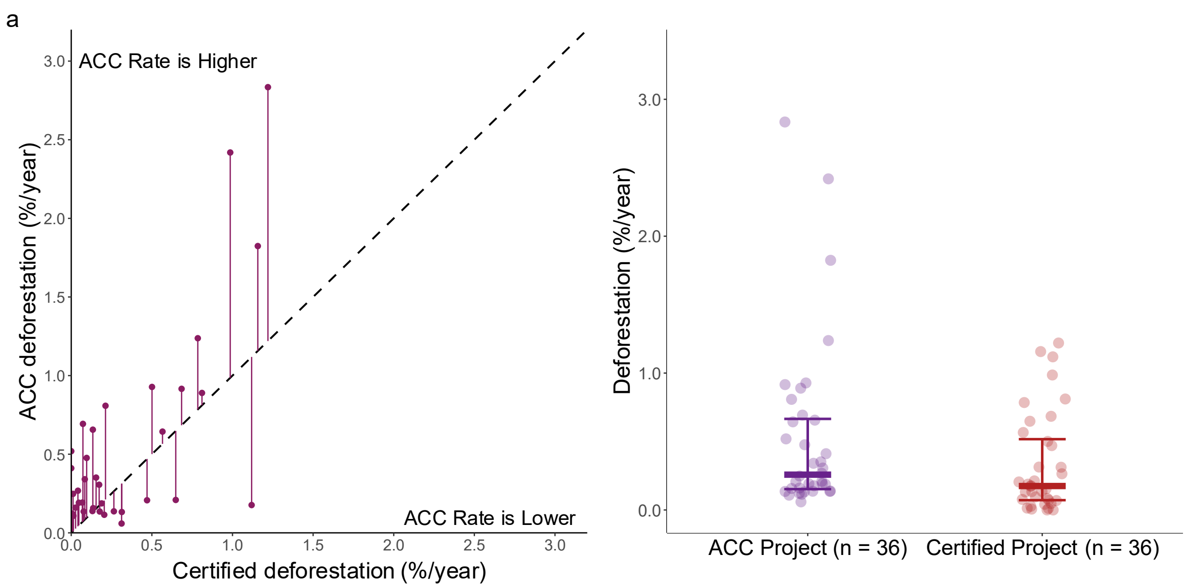

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Figure 3. Comparison of deforestation measured by the ACC and bespoke remote sensing. (a) *Ex post* annual deforestation rates in project areas measured using the European Unions' Annual Change Collection (ACC) versus the bespoke forest cover layers used in certification. (b) Median deforestation rates are significantly higher when measured by the ACC (*Wilcoxon paired signed-ranks*, $p < 0.01$). Error bars show the interquartile range. In both panels points represent individual REDD+ projects.

Q3. Were reference areas similar to projects in their exposure to deforestation?

Having established that reports of lower additionality were consistent across quasi-experimental assessments and were not driven by forest-change layers that underestimate deforestation, we turn next examine why certification methodologies may have overestimated project additionality. Specifically, we ask whether the reference areas used were representative of the deforestation pressures to which projects were exposed.

Reference areas were systematically different from project areas, exhibiting lower exposure to deforestation before project implementation. To illustrate this, we focus on project 958 in Peru (equivalent figures are shown for all projects in S5). Project 958's reference area was located immediately outside the project (Fig 4a). Despite this proximity, the reference area was less inaccessible (a mean of 13.8 hours compared to 25.5 hours), and less forested at the start of the reference period in 2001 than the project was at its start in 2010 (78% compared to 95%) (Fig. 4b). Standardised mean differences of -1.02 and -0.58 respectively indicate that mean values for the reference area were significantly different from those for the project area. By contrast, the control units selected by the quasi-experimental matching approach were distributed much more widely across Peru (Fig. 4a) and were nearly identical to the project area in the distributions of observable confounders (Fig. 4b).

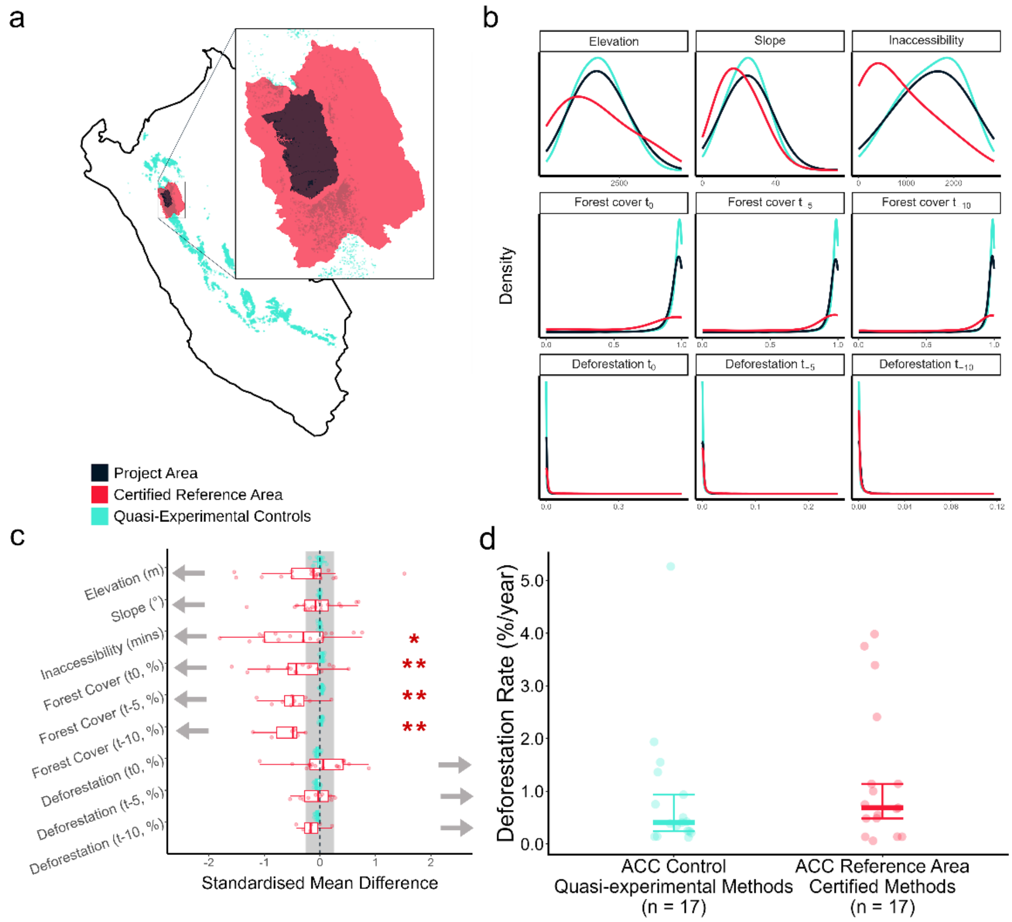

Figure. 4: Differences in exposure to deforestation drivers and resulting deforestation rates across project, reference and control areas. (a-b) Example from Project 958 in Peru. (a) Geographic locations of the project (black), reference area (red) and quasi-experimental controls (green). (b) Univariate frequency distributions of observable confounders which may influence deforestation, showing discrepancies between the reference area and the project in elevation, inaccessibility and forest cover. (c-d) Summary for the 17 projects with mapped reference areas. (c) Standardised mean differences from the mean project values for each observable confounder. Arrows indicate the direction of change associated with increased deforestation. Project-level significance is indicated by values beyond the grey band (-0.25 to 0.25), and significance across projects by * $p < 0.05$ and ** $p < 0.01$. (d) Median and interquartile ranges of deforestation rates measured by the ACC in quasi-experimental control and reference areas during the project period. In (b-c) the timing of measurements is expressed relative to the project start year (t_0) for quasi-experimental control areas, or the start of the reference period for reference areas

The same pattern was observed across all 17 projects that provided mapped reference areas (Fig. 4c, S3). Compared with project areas, mean inaccessibility in reference areas was lower (median standardised mean difference = -0.30, t = -2.41, d.f. = 16, p = 0.028), as was forest cover at the start of the project (median standardised mean difference = -0.42, t = -3.10, d.f. = 16, p = 0.007) as well as five and ten years beforehand (median standardised mean difference = -0.48, t = -4.04, d.f. = 16, p = 0.003; and -0.48, t = -4.28, d.f. = 5, p = 0.008, respectively). Historic deforestation rates were not significantly different. In contrast, none of the quasi-experimental control areas differed from the project areas in any of the observed confounders (Fig. 4c).

These differences in exposure to observable confounders between reference and quasi-experimental control areas were associated with a directional difference in their deforestation rates during the project period when assessed using the ACC layer. Across the 17 projects, median deforestation rates in reference areas were 0.69%/year compared with 0.41%/year in the quasi-experimental control areas (Fig. 4d). Although this was 1.69 times more deforestation the increase was not statistically significant (*one-tailed Wilcoxon paired signed-ranks test,* V = 103, p = 0.1123). The same pattern was found when including the loss of either undisturbed or degraded forest in the ACC deforestation measurements (S7).

Q4. After isolating the effects of forest cover layer and reference area selection, how much over-crediting can be explained by ex ante modelling?

To dissect the relative contributions of the mechanisms identified in preceding analyses, we examined their effects in the subset of projects (n = 17) for which all required data were available. The smaller sample size meant that most effects within this subset were not statistically significant, but they were of a similar magnitude to those observed in the larger analyses of each mechanism. Certified assessments reported substantially more avoided deforestation than quasi-experimental assessments (0.95 %/year shown in pink compared to 0.18%/year shown in green in Fig. 5; *Wilcoxon paired signed-ranks test*, V = 134, p = 0.002). We then sequentially re-estimated avoided deforestation using different combinations of the deforestation rates drawn from the certified and quasi-experimental assessments to isolate the effect of each mechanism, with any remaining over-crediting assumed to be explained by *ex ante* modelling.

First, to account for differences between the remote sensing layers in project areas, we substituted the ACC-measured deforestation rates with the certified rates. Because certified rates were lower, this substation increased median avoided deforestation slightly to 0.20 %/year (orange in Fig. 5), indicated that the choice of deforestation rate for the project area contributed only about 2% of the observed over-crediting.

Second, to isolate the effect of reference area selection, we substituted the quasi-experimental control areas with the certified reference areas and measured deforestation in both using the ACC. Median avoided deforestation then increased to 0.64 %/year (blue in Fig. 5), intermediate between quasi-experimental and certified assessments, suggesting that reference area choice probably accounted for 57% of the over-crediting.

Next, we estimated the effect of remote sensing differences on reference area deforestation rates. Because project documents did not report deforestation rates in reference areas, we inferred them by multiplying the ACC-derived control area deforestation rates by 0.58, which was the median ratio of bespoke to ACC rates across the 17 projects. Substituting these inferred values reduced avoided deforestation to 0.35 %/year (grey in Fig 5), suggesting that lower detection of deforestation by bespoke remote sensing (rather than the ACC) was not a cause of over-crediting.

Finally, having adjusted for reference area selection and differences in deforestation rate estimation, we attributed the remaining ~78% of over-crediting to the various *ex ante* modelling approaches used in certified assessments, recognising the considerable uncertainty surrounding this allocation.

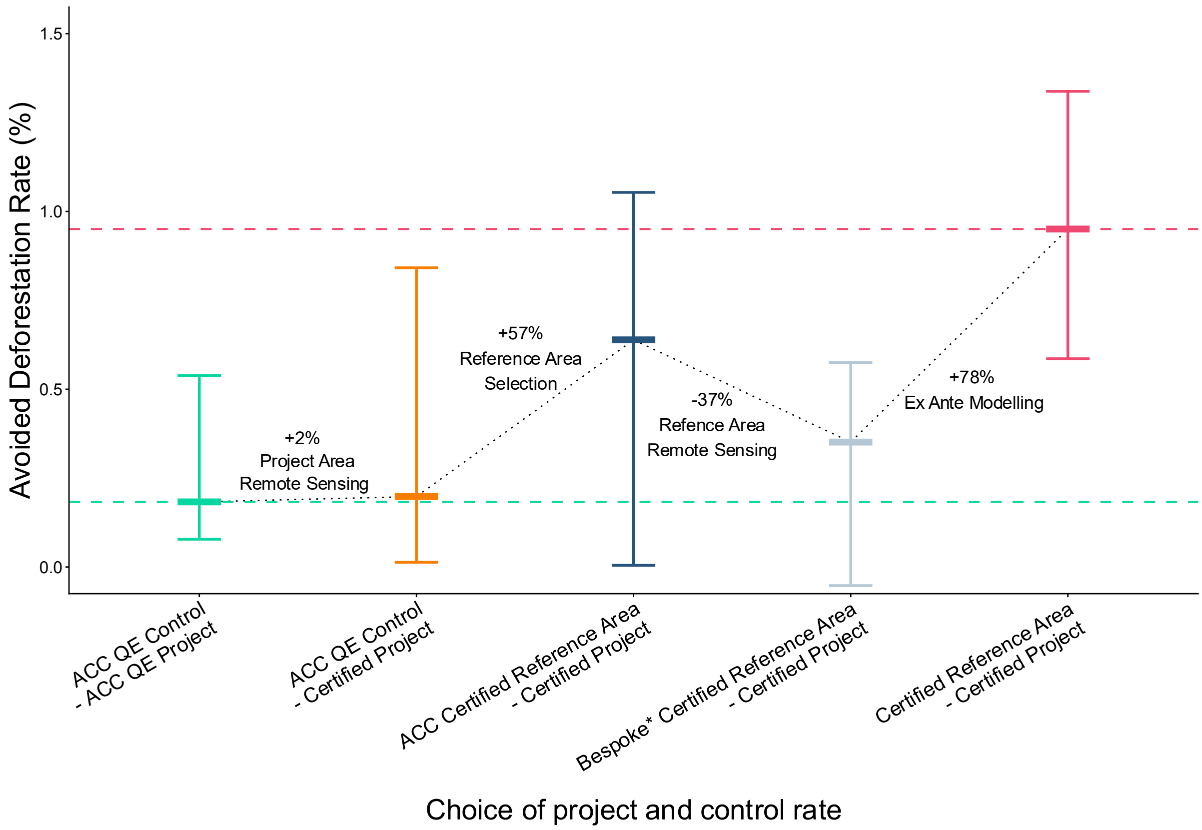

Figure 5. Contributions of different mechanisms to over-crediting. Quasi-experimental (QE) estimates of avoided deforestation (green) are significantly lower than certified assessments (pink; *Wilcoxon paired signed-ranks test*, $p < 0.01$) across a subset of 17 projects for which all necessary data are available. Substituting certified deforestation rates for project areas into the QE assessments produces a small increase in avoided deforestation (orange). A much larger increase occurs when certified reference areas are used instead of QE control areas, both measured by the European Union's tropical moist forest annual change collection (ACC; dark blue). Avoided deforestation is lower when deforestation rates inferred from bespoke remote sensing products are used instead of ACC values (grey). The remaining discrepancy is attributable to *ex ante* modelling. These comparisons reveal the relative contributions of each mechanism to overall over-crediting. Error bars represent medians and interquartile ranges.

Discussion

Researchers in this space, including our co-authors, differ in how strongly they view carbon markets are essential to closing the forest finance gap and tackling climate change[12,32–34,36,37]. Regardless of this, if forest conservation carbon credits are being traded, then it is

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crucial that one credit reflects the amount of additional deforestation avoided that is claimed. We interpret our findings with that challenge in mind.

By synthesising the results of multiple independent quasi-experimental analyses, we find that most REDD+ projects did reduce deforestation compared to credible counterfactual estimates, demonstrating a tangible contribution to forest conservation. However, the combined evidence from the six studies also shows that these projects issued far more credits than were justified. These were bad credits in that they certainly did not reduce emissions as much as claimed, and because many were used to offset emissions elsewhere, this will have had a negative impact on progress towards climate goals[32]. Nonetheless, they were not necessarily bad projects: many achieved measurable on-the-ground conservation benefits.

While some industry-linked commentators continue to deny that over-crediting in first-generation REDD+ projects was a problem[26,27,29], the scientific evidence is now clear. Our synthesis shows that across the portfolio of projects examined, almost 11 times more credits were issued than was justified. However, most of this excess was driven by a small number of projects that generated the largest volumes of credits (top right in Fig. 2b). After excluding the 9 biggest issuers, over-crediting among the remaining projects was much lower, around 4.0 times, though still substantial. This pattern highlights the need for methodological advances that prevent egregious cases of over-crediting that disproportionately shape the overall integrity of REDD+.

One counterargument made by some commentators is that over-crediting is an artifact of lower detection of deforestation by the globally available datasets used in independent quasi-experimental evaluations compared with the bespoke layers used by REDD+ projects[29]. Several studies have found that locally trained remote sensing products can be more accurate in measuring forest cover and deforestation than global layers, such as the ACC[38–40]. However, we found no evidence that the widely used ACC layer systematically detected less deforestation. In fact, it detected similar or higher rates in project areas than certified estimates using bespoke layers. This may have occurred because the ACC is a multi-temporal approach that considers all disturbance events occurring since the start of the time series[5] whereas bespoke approaches typically detect non-forest pixels at two points in time, which may miss short-term disturbances and regrowth events[41]. Expanding the forest cover definition to include degraded forest and counting only long-term transitions to non-forest produced lower deforestation rates (see S4 for comparisons including different forest cover definitions). We could not examine these differences in reference areas, because project documents rarely report deforestation rates there[42], but classification errors are unlikely to differ systematically between project and reference areas unless they differ markedly in terms of species composition or fragmentation.

So why might over-crediting have occurred? One possible mechanism is that projects were situated in areas at low risk of deforestation, whereas reference areas faced greater threats[7,43]. Indeed, there is good evidence that site-based and policy interventions tend to target areas of disproportionately low risk[44,45]. Our results confirm that reference areas were more exposed to known drivers of deforestation than project areas and experienced about 1.7 times more deforestation than quasi-experimental controls.

A second, potentially complementary mechanism arises from the flexibility in model choices available to projects for making *ex ante* predictions of counterfactual deforestation. Such prediction is inherently uncertain, especially when involving spatial or non-linear components[46,47]. Projects have been shown to select methodologies that produce higher estimates of avoided deforestation from among those approved for certification[11]. We extended this analysis by contrasting multiple certified and quasi-experimental estimates (S6). For most projects, the methodology used for credit issuance significantly overestimated avoided deforestation, but alternative or differently parameterised certification methodologies were available which could have produced more credible estimates. For example, across four projects, VM0006 produced estimates not significantly different from quasi-experimental results (S6). We conclude that methodological flexibility for project proponents and certification bodies, both of whom have financial incentives to produce greater numbers of credits[48,49], provided an additional mechanism contributing to over-crediting in first generation REDD+ projects.

Our study has several limitations. Firstly, quasi-experimental methods for project impacts on deforestation are difficult to verify because the counterfactual, how much deforestation would have occurred without the project, is unobservable. Quasi-experimental methods are advancing all the time and new methods exist which combine features of difference-in-differences and synthetic control, such as interactive fixed-effects, augmented synthetic controls, and synthetic difference-in-differences [35,50,51]. However, simulated landscapes where deforestation rates are known have been used to demonstrate the reliability of the approaches used in the studies we synthesize[52,53].

Secondly, the credibility of quasi-experimental designs requires that all significant confounders are controlled for[15] which requires an understanding of the reasons some areas became REDD+ projects and others did not. Guizar-Coutiño *et al*. (2025)[25] explore the potential influence of hidden confounders and reveal they would need to have an effect size multiple times larger than the most impactful observed covariates to undermine the evidence of over-crediting. A potentially important hidden confounder in our analysis is the economic value of land, which we proxied using accessibility (travel time to healthcare in 2019[54]). This covariate has weaknesses that could result in the selection of inappropriate controls. While healthcare is often synonymous with certain size population centres, these places are not necessarily the same as processing or transportation hubs for timber or agriculture. Equally, most projects pre-date 2019, meaning transportation patterns are partly determined by project activities.

Finally, REDD+ projects can cause spillover effects (leakage) that might influence deforestation rates in control units. While some studies have attempted to overcome spillovers driven by localised changes in markets by only matching to areas beyond a certain distance from project boundaries[23,24], this strategy may be insufficient because of the complex and unpredictable manner in which spillovers can occur[55].

The first-generation REDD+ methodologies evaluated here are now being replaced by new jurisdictional approaches (JREDD+) for voluntary and compliance markets. JREDD+ methodologies (specifically VM0048 and ART TREES) still make *ex ante* counterfactual predictions but these are now based on the mean annual deforestation rate across the jurisdiction during a 5-6 year reference period[56,57]. By eliminating project-selected reference areas, a major source of bias has been removed. Crucially, the analysis is performed by independent data providers with no direct financial stake in the number of credits issued, which is also a substantial improvement. However, reliance on *ex ante* predictions still carries risk: it assumes deforestation drivers remain constant between the historic and project periods, which is rarely true. Factors such as forest conservation or trade policies, major infrastructure projects or extreme climatic events will vary through time with significant effects on deforestation. *Ex post* measurements of deforestation in untreated jurisdictions differ by as much as 100% from *ex ante* predictions[58,59]. Random enrolment, without regard for future deforestation trends, would not result in systematic bias. However, if jurisdictional enrolment is more likely when there is the expectation that historical rates are about to fall, or if the anticipation of enrolment stimulates temporarily increased deforestation, systematic over-crediting is again a risk[60,61]. One final consideration is that VM0048 permits nested projects with baselines produced using *ex ante* spatial modelling, which we have shown to be problematic.

A potential solution is to only issue credits *ex post* based on assessments which integrate new developments in quasi-experimental methods[34,37]. Ideally, different quasi-experimental results from methods such as the Permanent Additional Carbon Tonne (PACT) method used in our study or advances in synthetic controls that require fewer assumptions should be considered[35,50,62]. Of course, there continue to be significant uncertainties in project-level estimates, but these can be ameliorated by pooling credits at the scheme-level or reducing the number of credits issued to safeguard any claims being made[36,63]. Quasi-experimental crediting would not alter the timing of credit issuance, as even methods that use *ex ante* counterfactuals still rely on *ex post* measurements within projects to quantify additionality. These approaches are now being adopted by newer certification methodologies for afforestation, restoration and revegetation (ARR)[64] and improved forest management (IFM)[65]. Uncertainties in credit yields due to the unpredictable nature of the counterfactual, along with the implications of over-crediting for existing projects would seem to be reasons such *ex post* approaches have not yet been applied but have a key role to play in ensuring the future integrity of forest carbon credits[37].

While the first-generation of REDD+ projects achieved far less than claimed, many nonetheless reduced deforestation. Given the modest progress towards deploying engineered carbon capture and storage, there remains a role for forest-derived carbon credits on the path to net zero. The challenge is to ensure that claimed impacts reflect real outcomes. Removing methodological flexibility, particularly the use of project-selected reference areas and *ex ante* modelling, is a crucial first step, but embedding quasi-experimental evaluations would ensure issued credits represent truly additional reductions in deforestation. Either way, far fewer credits would be issued to projects using these approaches, meaning prices will need to rise to pay for the genuine cost of equitable projects with real climate benefits[66]. Bridging the forest-finance gap will require abandoning the illusion of cheap carbon and paying the true cost of credible mitigation.

Methods

Certified estimates of avoided deforestation

Certified rates of avoided deforestation, used to generate carbon credits, were sourced from the design and monitoring documents of 44 REDD+ projects accessed through the Verra registry between April and June 2023. These include the loss of all areas that had been forest for at least 10 years prior to the establishment of the project. Forest cover was classified using cloud-free satellite images and bespoke remote sensing approaches (see S1 for details). Projects included in the study were those that had sufficient publicly available data, including geospatial polygons of project areas, and had reported the area of avoided deforestation in their most recent monitoring reports (S2).

Certified project documents did not report deforestation consistently; however, the *ex post* observed deforestation within the project and the *ex ante* modelled deforestation under the counterfactual scenario were available. Verra projects refer to the counterfactual scenario as the 'baseline'. Importantly, deforestation was provided as a total amount in hectares over the whole evaluation period or broken down into annual amounts. Where avoided deforestation was not reported, it was calculated by subtracting project deforestation from counterfactual deforestation.

For Q1, the mean certified annual avoided deforestation a year in hectares was used. For question Q2-Q4, for each project, the total amount of avoided deforestation claimed was disaggregated across evaluation period (which had a mean length of 6.4 years) to produce annualised compound avoided deforestation percentage rates (see below). Taking this approach ensured that the avoided deforestation used was the same as that used to issue credits.

Projects issued credits using four different Verra methodologies (VM0006, VM0007, VM0009, VM0015) to estimate counterfactual outcomes. The different methodologies are summarised briefly in Table 1 and the process common across these methodologies is described in S1. The extracted project and counterfactual deforestation rates as well as the certified avoided deforestation are presented in S9.

Quasi-experimental estimates of avoided deforestation

We compiled data from six *quasi-experimental* analyses which estimated *ex post* counterfactual and project outcomes for REDD+ projects (Table 1). These included work published by West et al.[7,9] (two analyses), Guizar-Coutiño et al.[8] and Tang et al.[23].

We also included results from a study under review by Guizar-Coutiño et al.[25], which implemented a novel framework to reanalyse the set of projects examined in Guizar-Coutiño et al.[8] study. In this new study, 7.1-ha circular sample regions were used instead of 0.09 ha pixels to characterise forest loss observations. Guizar-Coutiño et al.[25] assess the causal

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impact of REDD+ projects on deforestation by adjusting for observed confounders and pre-treatment deforestation trends, using a two-stage matching approach, commencing with propensity score matching via random forests, followed by propensity score subclassification on the matched datasets[67]. They assess the average treatment effect of the treated using multiple model specifications with different algorithms, parameters and covariates. The avoided deforestation estimates used in our study correspond to the doubly robust model specification reported in this new study.

Finally, we used the Permanent Additional Carbon Tonne (PACT) v2 method[24] (described below) to produce additional estimates and to explore the mechanisms underlying over-crediting. In each case, we included projects for which certified estimates were also available.

Avoided deforestation estimates from *quasi-experimental* methods were received from the study authors as project level estimates of cumulative avoided deforestation over a defined study period. Annual avoided deforestation rates were calculated by dividing cumulative avoided deforestation by the total number of years of the evaluation, to compare estimates produced over different time periods. Evaluation periods were 5 years for both Guizar-Coutiño et al. studies[8,25], and a mean of 6.0, 7.6, 9.0, and 8.1 years for the West et al. (2020)[7], West et al. (2023)[9], Tang et al. (2025)[23], and PACT[24] studies respectively.

Deforestation was assessed using publicly available remote sensing products for tracking changes in forest cover, specifically the corrected MapBiomas[68] dataset used by West et al. (2020)[7], the Global Forest Change (GFC)[69] dataset used by West et al. (2023)[9] and Tang et al, (2025)[23], and the Tropical Moist Forest Annual Change Collection (ACC)[5] used by Guizar-Coutiño et al.[8,25] and PACT[24]. GFC reports deforestation as the year of gross forest loss. The ACC dataset reports degradation and deforestation separately. The Guizar-Coutiño and PACT studies considered deforestation to be degradation (disturbances lasting <2.5 years) or deforestation (disturbances lasting >2.5 years) of the undisturbed class, even if regrowth subsequently occurred. Degradation was included because it has been shown to result in biomass reduction of >50% within 12 months. Our analysis of above ground biomass densities for different ACC cover classes shows that both degraded and regrowth classes had <50% of the above ground biomass of the undisturbed class (see S11). Therefore, for the mechanistic component of our study, we followed the PACT methods by defining forest cover solely as the undisturbed class within the ACC dataset. We explore the differences that arise using different definitions of forest cover in S4 and S7. We chose to focus our assessment on avoided deforestation because it lies at the centre of the REDD+ controversy, while recognising that estimating carbon fluxes is an additional, important and complex topic.

The PACT method

The PACT method used a bootstrapped one-to-one pixel (0.09 ha) matching approach to produce paired project and control units from which to estimate mean project and counterfactual outcomes[70,71]. Project areas, start dates, and the date of the most recent monitoring period were obtained from the VCS registry (accessed between April and June 2023). Projects were then assessed following four main steps: (1) compile layers for characterising project units; (2) determine the set of suitable untreated units; (3) form the control set by matching untreated and project units; and (4) measure and average outcomes across project and control sets (S1).

First, geospatial raster layers were compiled to track changes in forest cover. The ACC dataset[5] was used for this purpose. We used the 2021 version, a 30 m resolution (0.09 ha) time series spanning December 1990 to December 2021, classifying pixels as one of six classes: undisturbed, degraded, deforested, restored, water and other.

Secondly, layers were compiled to select control units analogous to project units using a method informed by a causal model of land use change. This model incorporated covariates that capture the effects of local policies and regulations, economic pressures, historical changes in land cover, and environmental conditions on deforestation rates and conservation efforts. We used the following layers: International country borders (OpenStreetMap[72]) to encompass national regulatory limits; ecoregions (Resolve[73]) to ensure biotic equivalence; and elevation (NASA's Shuttle Radar Topography Mission[74]) as a proxy for abiotic conditions. From the elevation layer, we calculated slope using GDALDEM v3.8.4 (default settings), an important proxy for accessibility and suitability for economic activities. This produced a slope variable consisting of integer values representing the mean slope (0-90°) in each raster cell.

Motorised accessibility to healthcare in 2019 (Malaria Atlas Project[54]) was used as a proxy for market exposure/economic pressure. This accessibility layer integrates all known travel routes, including rivers, along with terrain, land cover, and road quality to assess the travel time to population centres large enough to have a healthcare facility. Finally, we computed the proportional cover of undisturbed and deforested classes from each year of the ACC time-series (1990-2021) to assess land-use change trajectories in the surrounding area through time. Proportional cover was calculated by counting the number of pixels in each class and dividing by the total number of pixels within the 1 km radius neighbourhood around each pixel.

It is important to note that accurate data regarding land value and accessibility to markets is not universally available, particular in frontier landscapes. As such we use spatial proxies, specifically proportional cover and motorised accessibility to healthcare, recognising that these leave space for unobserved confounders.

Finally, to minimise interference arising from local leakage, we excluded control units within the vicinity of any REDD+ project. This is important because, under the assumption that leakage arises through the reorganisation of local supply chains or markets, there would be concentrated interference at short distances from projects. To do this we produced a binary raster indicating the presence or absence of a REDD+ project within 5km. REDD+ polygons were accessed from the VERRA registry and supplemented by any REDD+ project polygons shared directly with the Cambridge Centre for Carbon Credits. All layers were reprojected to the same coordinate reference system as the ACC.

Projects were sampled using a spatial grid with a density of 0.25 points per ha for projects smaller than 250,000 ha and 0.05 points per ha for larger projects (to reduce processing time). The characteristics of these sample points were extracted from the layers described above. Time-varying characteristics, including the ACC pixel class and proportional cover were extracted at the start of the project (t_0) and at five (t_{-5}) and ten (t_{-10}) years prior.

The domain of untreated units was defined as all pixels within the same countries and ecoregions as each project, located at least 5 km from the project boundary, but no further than 2,000 km away. Leakage effects caused by the displacement of production could be present but were assumed to be small, given the large domain of the untreated units and the relatively limited effects of individual projects. To reduce computation time, untreated units were filtered to include only those within the range of values observed in the project area for each known driver of deforestation with an added tolerance of ±200 m for elevation, ±2.5° for slope, ±10 minutes accessibility, and ±10% points for proportional cover.

Matching proceeded by taking a random sample of 10% of the project units, which were then processed sequentially to identify control units with the smallest Mahalanobis distance across the continuous characteristics, and identical values for country, ecoregion and ACC land cover class at t_0, t_{-5} and t_{-10}.[75]. This matching approach is referred to as 'greedy' because the algorithm sequentially finds the best pairs, which are then removed from the pool for subsequent matching.

The standardised mean difference (SMD) between the control and project sets for each of the continuous characteristics was calculated as follows:

$$SMD = \frac{\mu_c - \mu_p}{\bar{\sigma}} \quad \text{Eq. 1}$$

Where μ_c is the mean of the control set, μ_p is the mean of the project set and $\bar{\sigma}$ is the square root of the mean of the variances. SMD values outside the range [-0.25, 0.25] for any continuous characteristic were considered statistically different and excluded from further analyses[14,75]. This process was repeated 100 times to produce matched project-control sets, each comprising 10% of the total points sampled from within the projects.

Ex post deforestation was measured for the project and control units by calculating changes in the undisturbed class since the start of the project. To calculate the area of undisturbed forest at each time point, we computed the proportion of all units in the undisturbed class and multiplied it by the project area. This process was repeated across all 100 matched sets to produce estimates of project and counterfactual forest cover.

Averaging across these sets, we produced annualised mean forest cover (in hectares). By subtracting counterfactual forest cover from the project forest cover, we derived annual cumulative avoided deforestation values. Total avoided deforestation during the project period was measured at the final year of the project's most recent monitoring period, as additionality figures are cumulative.

Q1. How consistent are estimates of project impacts assessed using independent quasi-experimental methods?

For each of the 44 projects, we compiled the certified estimates of annualised avoided deforestation (in hectares) along with all available quasi-experimental estimates. For the 42 projects with more than one quasi-experimental estimate, we calculated the mean, standard deviation, coefficient of variation, standard error, and 95% confidence intervals. For the remaining two projects, which had only a single quasi-experimental estimate, this value was taken to be the mean.

We determined how many projects had mean quasi-experimental estimates below their certified estimate, as well as how many had upper 95% confidence intervals below their certified estimates. To test whether certified estimates were significantly greater than mean quasi-experimental estimates, we applied a one-tailed Wilcoxon signed-ranks test, due to the non-normality of the data. We also evaluated how many projects had avoided deforestation estimates significantly greater than zero, using the lower bound of the 95% confidence intervals across quasi-experimental studies.

We next assessed the over-crediting as the ratio of the certified to the mean quasi-experimental estimate of avoided deforestation. We refer to this as the over-crediting ratio, which expresses how many times more credits were issued through certification than suggested by quasi-experimental estimates. The mean over-crediting ratio was calculated for all projects that had issued credits and also had positive mean quasi-experimental estimates; projects with negative mean quasi-experimental estimates were excluded.

The global over-crediting ratio, reflecting how many certified avoided deforestation units likely correspond to one quasi-experimentally validated unit across the REDD+ portfolio, was calculated by dividing total certified avoided deforestation by the total of the mean quasi-experimental estimates (including negative values). Projects were randomly sampled with replacement 10,000 times, with the results then used to derive mean values and 95% confidence intervals.

Determining comparable deforestation rates between Certified and ACC sources

We calculated annual deforestation rates to make the quantities of deforestation measured by the certified assessments and the ACC layer (e.g. those produced by PACT) directly comparable. The future forest cover (F_t) resulting from a constant annual deforestation rate (*r)* after an interval (*t)*, given the starting forest cover (F_0), was calculated as follows:

$$F_t = F_0(1 - r)^t \qquad \text{Eq. 2.1}$$

Because we had extracted the forest cover at the beginning and end of the evaluation period, we calculated the rate by rearranging the formula to:

$$r = 1 - \left(\frac{F_t}{F_0}\right)^{\frac{1}{t}} \qquad \text{Eq. 2.2}$$

To determine F_0 we took the proportion of the ACC pixels classified as undisturbed class in the yearly layer closest to the project start date and multiplied this by the total project area derived from the project polygon. Thus F_0 was the total area of undisturbed forest (in hectares) at the start of the project. F_t was calculated by subtracting the area of deforestation in the certified or ACC measurements from F_0. The evaluation interval *t* was the number of years between the start and end assessments. For the certified assessments, this was calculated from the number of total days between the start and end dates, whereas for the ACC data, this was the number of whole years between the ACC layers used. The calculations for each project are presented in section S10 of the supplementary information.

We produced certified and ACC mean annual percentage deforestation rates for 36 project areas and 17 counterfactual estimates. Between these two sets there was an intersection of 17 projects, for which we could produce mean annual percentage avoided deforestation rates using all combinations of certified and quasi-experimental estimates for project, reference and control areas, sufficient for inclusion in Q3 and Q4.

Q2. Is the use of global deforestation layers the reason for the discrepancy between quasi-experimental and certified estimates?

We used the annual deforestation rates to test if there was a difference in the amount of *ex post* deforestation measured in project areas. Because the data were non-normal, we tested whether the paired differences were significantly greater than zero using a one-tailed Wilcoxon signed-ranks test. In the main analysis, we focussed exclusively on the deforestation or degradation of undisturbed forest in the ACC measurements. In S4 we broaden the forest cover definition to include undisturbed and degraded forest and measure the deforestation of either.

Q3. Were reference areas similar to projects in their exposure to deforestation?

We compared the exposure to deforestation risks across project, reference and control areas to assess the extent to which the selection of reference areas explained differences between certified and quasi-experimental approaches. For reference areas, we focused on the *Reference Regions for Deforestation* (RDD) used by certified methods as these were used to measure the rate of deforestation, rather than the *Reference Regions for Location*, which were used to model how much of the deforestation was expected to occur within project areas.

Reference area polygons were not publicly available as shapefiles and were therefore digitised by georeferencing maps available in project design documents from the Verra registry. This was done either by tracing the polygons by hand in QGIS (v3.26.3) or through colour thresholding and automated polygonisation procedure in R, equivalent to the process described by the Environmental Systems Research Institute[76]. Colour thresholding was applied when reference areas presented in project design documents were complex shapes represented by colour coded systems within the georeferenced maps. The colours representing the reference areas were identified as the most frequent pixel colours in the maps. Binary maps were then produced from these pixels using a Boolean comparison to the reference area colours, and their fidelity was checked against the original maps. Finally, the binary maps were converted to geospatial polygons using the *polygonise* function in the *Terra* package in R.

We successfully digitised reference areas for 17 projects (see supplementary information S5 for the projects included). We then sampled the characteristics of reference areas using the same PACT method for sampling project units (described above). Time-varying characteristics were sampled from the beginning of the historical reference period (usually 5-10 years before the start of the project), to ensure comparability with project characteristics.

To test the similarity of control and project areas in terms of exposure to deforestation risks, we examined the distributions of key characteristics prior to any measurement of forest loss. For the project areas, these covered the pre-project period up to a maximum of ten years prior. This was the same for the quasi-experimental controls. For reference areas, the period extended up to ten years prior to the start of the documented reference period.

We tested for differences in the univariate distributions of pre-project characteristics using the SMDs between the reference or control areas and the project areas. Values outside the range [-0.25, 0.25] indicated a significant project-level difference[14,75]. Across the set of projects, we tested whether the distribution of the SMDs for each characteristic differed significantly from zero using t-tests.

We also compared the observed annual deforestation rates between reference and quasi-experimental control areas using the ACC dataset. For reference areas, this was measured across the reference period; for quasi-experimental controls, it was measured across the project evaluation period. Because the data were non-normal, we applied a one-tailed Wilcoxon paired signed-ranks test to assess whether reference areas were exposed to significantly more deforestation than the quasi-experimental control areas.

Q4: What is the residual effect of ex ante modelling after isolating the effects of forest cover layer and reference area selection?

To assess the effect of *ex ante* modelling, we used a process of isolating all possible explanations for over-crediting for the same 17 projects used in Q3. This required determining the annual avoided deforestation rate produced by five different combinations of reference or control area and project area estimates.

First, we quantified over-crediting as the overall difference between avoided deforestation from quasi-experimental estimates (PACT in combination with ACC) and certified estimates. To isolate any effect of project area remote sensing within this overall difference, we examined the change in avoided deforestation resulting from substituting quasi-experimental project area deforestation estimates with their certified equivalents.

From this new combination, we then isolated the effect of reference area selection by substituting quasi-experimental control area estimates with ACC-derived estimates for the reference areas (covering the reference period) made in Q3. This third combination therefore represents the combined effect of the two mechanisms explored in Q2 and Q3. Th residual difference between this combination and the avoided deforestation rates produced by purely certified estimates captures the influence of two remaining factors: (1) the bespoke remote sensing measurements made in reference areas; and (2) the *ex ante* modelling of reference area deforestation to predict counterfactual outcomes for project areas.

Although we could not disentangle these explanations, we examined whether differences between remotely sensed forest cover layers affected the magnitude of the reference area effect. To do this, we inferred the deforestation rates that would have been observed if bespoke measurements in control areas occurred at the same relative proportion of ACC-measured deforestation rates observed in project areas. We divided certified estimates by quasi-experimental estimates in project areas and took the median of these ratios, to produce a correction coefficient. We then multiplied the ACC measurements in reference areas by the coefficient to generate corresponding "bespoke" estimates, from which we produced our final possible avoided deforestation rate. This yielded two possible residuals covering a range of impact attributable to *ex ante* modelling.

Statistical differences between ranges of avoided deforestation were tested using Wilcoxon paired signed-ranks tests because paired differences were not normally distributed.

Data availability

All data necessary to reproduce the analyses in this paper are made publicly available (zenodo.org/records/14895067).

Code availability

All analyses were undertaken in R (v4.2.1) using Terra (v1.7.65), Simple Features (v1.0.15) and Raster (v3.6.26) for geospatial processing; Vegan (v2.6.4) for ordination analysis; and the Tidyverse (v2.0.0) for data manipulation.

In an effort to contribute to improved transparency, we have made the code necessary to run the PACT evaluations (github.com/quantifyearth/tmf-implementation) and our analysis (github.com/quantifyearth/REDD-Over-Credit-Reasons).

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Competing interests

The Cambridge Centre for Carbon Credits (4C) has no commercial interest in carbon credits. TS has an advisory position with Symbiosis, an initiative to finance carbon storage in nature.

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