Zero-deforestation commitments in Indonesia's palm oil sector achieve high compliance but no additionality

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In response to public pressure, companies have increasingly adopted voluntary sustainable sourcing commitments. Yet, it often remains unclear whether such commitments lead only to the narrow goal of supplier *compliance*, or also to the broader goal of *additionality*, benefits beyond what would have occurred without commitments. A company's ability to influence its suppliers' behavior partly depends on the complexity of supply chains, yet data on supply chain linkages is limited, inhibiting causal evaluations. Here, we evaluate the compliance and additionality of zero-deforestation commitments (ZDCs) in the Indonesian palm oil sector, a major driver of tropical deforestation. We compiled a detailed dataset that integrates information about corporate ZDC implementation, supply chain linkages, and maps of palm-driven deforestation. We use this dataset to evaluate the causal impact of ZDCs on deforestation from 2018-2020, using difference-in-differences econometric methods. We find that 12-17% of oil palm concessions in Indonesia supplied to or were owned by oil palm companies with ZDCs. These concessions had low deforestation (<1% per year from 2018-2020) and thus largely achieved *compliance*. Yet they experienced similar reductions in deforestation compared to concessions not covered by ZDCs, showing therefore no *additionality*. We attribute our finding of no additionality to broader changes in policy and economic conditions during the study period that appear to have reduced the remaining accessible forest as well as market and regulatory incentives to clear forests for palm oil. Should these conditions change in the future, continued compliance with ZDCs could yield additional reductions in deforestation.

Sustainability Standards | Causal Inference | Zero-Deforestation Commitments| Sustainable Supply Chains

Significance Statement

Despite public policy efforts, deforestation continues at alarming rates, threatening our ability to mitigate climate change. In response, companies have adopted zero-deforestation commitments (ZDCs) to eliminate deforestation from their supply chains. As ZDCs are transmitted to suppliers via complex supply chain linkages, understanding whether they really reduce deforestation requires a detailed understanding of such links.
We leverage a unique dataset of sourcing relationships in the Indonesian palm oil sector to evaluate ZDC effectiveness. Our findings reveal suppliers are complying, but there is no policy additionality, since reduced deforestation was observed in both ZDC-linked and non-ZDC oil palm concessions. We attribute this to broader economic and policy conditions which reduced growers' incentives to clear forests for oil palm during the study period.

1. Introduction

Tropical forests play a critical role for biodiversity, climate regulation, and carbon storage, yet they are disappearing at an alarming rate. Ninety percent or more of tropical deforestation is driven by agricultural commodities, notably beef, soy, and palm oil (Pendrill et al., 2019, 2022). Recognizing that the processing and trade of deforestation-linked commodities is concentrated among a few large companies, supply chain-driven zero-deforestation commitments (ZDCs) have emerged as a promising complement to producer countries' domestic policies to reduce deforestation. ZDCs are corporate pledges to refrain from sourcing products grown on deforested land, typically after a cut-off date (Brown and Zarin, 2013; Lister and Dauvergne, 2014). With the rise of due diligence regulations that ban deforestation-linked imports into the European Union and United Kingdom markets, zero-deforestation supply chains are rapidly becoming a legal, rather than voluntary, obligation for companies sourcing commodities such as palm oil, beef, or soy (Berning and Sotirov, 2023). Given the growing importance of supply chain regulations governing deforestation, evaluating the effectiveness of voluntary ZDCs at stemming forest loss is an important precondition for understanding whether existing efforts are meeting the scale of our global forest conservation challenge.

Evaluating the effectiveness of ZDCs requires assessing not just compliance– whether producers in ZDC supply chains adhere to companies' policies and avoid deforestation– but also additionality– whether ZDCs lead to lower rates of deforestation than would have occurred in their absence.

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Compliance can generally be interpreted as success from a corporate perspective, since the company achieved its goal of ensuring a deforestation-free supply chain. However, during periods of systematically low deforestation, all agricultural producers are likely to be deforestation-free. This would help supply chain actors comply with ZDCs but overstate claims of additionality by supply chain actors with ZDCs. Therefore, the mere presence of compliance does not establish the additionality of deforestation reductions attributable to ZDCs, calling for a separate analysis of additionality.

However, assessing the additionality of supply chain policies presents significant challenges. Impact evaluations aim to tease out the causal effect of a policy by identifying who has been treated by the policy and comparing the effects on this group to a counterfactual of what would have happened in the absence of the policy (Ferraro, 2009; Börner et al., 2020). While such analyses are well understood when there is a clearly-defined treatment group, they become more methodologically challenging for the study of supply chain policies for several reasons. First, supply chains are comprised of multiple layers of actors, starting with agricultural producers, passing through processors such as mills, refineries, and slaughterhouses, and finishing with traders, exporters, retailers, and consumers. Even when it is known which companies adopted and implemented a ZDC and are therefore *treated*, it is often unclear which producers are treated due to the multiple and often interwoven linkages between downstream companies and producers (e.g., in a given year, one producer sells to several companies and each of these companies buys from hundreds of producers). Second, different types of linkages exist: supply chain actors can be linked to each other either through legal ownership or commercial sourcing relationships. This implies that companies have different channels to implement their ZDCs, and therefore, that multiple types of treatment must be considered. Third, these ownership or sourcing relationships may change over time– a concept known as supply chain stickiness (Reis et al., 2020)– complicating further the identification of *treated* units. Thus, determining treatment status in supply chains requires temporally resolved data on ownership and sourcing linkages between all actors. Yet, despite multiple calls for transparency and traceability, such data are rarely available publicly (Gardner et al., 2019), rendering the analysis of the effectiveness of ZDCs very challenging.

Indeed, a lack of such data has limited evaluation of ZDCs in the palm oil sector, which has driven rapid deforestation in Southeast Asia (Austin et al., 2019; Carlson et al., 2013; Koh and Wilcove, 2008; Gaveau et al., 2022). The sector has comparatively high adoption of corporate ZDCs, whose implementation became effective in 2018 (Grabs and Garrett, 2023). By 2020, ZDCs covered roughly 83% of palm oil refining capacity in Indonesia and Malaysia (ten Kate et al., 2020). In Indonesia, the world-leading palm oil producer and country with one of the highest rates of primary forest loss in the tropics, oil palm expansion accounted for around 34% of all deforestation from 2001 to 2019 (Gaveau et al., 2022). Yet, studies evaluating palm oil ZDCs have remained limited to ex-ante assessments on biodiversity (Deere et al., 2020; Fleiss et al., 2022) and deforestation (Austin et al., 2017; Mosnier et al., 2017; Busch et al., 2022) from coarser-scale data.

Ex-post impact evaluation studies in the palm oil sector have primarily looked at the effects of certification under the Roundtable on Sustainable Palm Oil (RSPO). The RSPO is the main sustainability certification system in the sector and has required no clearance of High Conservation Value lands since 2005 but strong zero-deforestation criteria (i.e., identification and conservation of High Carbon Stock areas) only since 2018 (Carlson et al., 2018; Cattau et al., 2016; Gatti et al., 2019; Heilmayr et al., 2020a; Lee et al., 2020; Morgans et al., 2018; Noojipady et al., 2017; Santika et al., 2021; Garrett et al., 2019). While RSPO certification has reduced deforestation (Carlson et al., 2018; Heilmayr et al., 2020a; Lee et al., 2020), this impact occurred mainly on plantations with little remaining forest (a targeting challenge) (Carlson et al., 2018), and parts of this reduced deforestation were offset by deforestation leakage to land outside of Indonesia's state forest (a spillover challenge) (Heilmayr et al., 2020a). Furthermore, fires, forest loss, and degradation continued even on certified plantations (Cattau et al., 2016; Gatti et al., 2019; Noojipady et al., 2017). In the case of the RSPO, the supply-chain identification challenge is relatively easily addressed as certification provides a clear definition of treated (i.e., plantations supplying to certified mills) and non-treated (i.e., plantations supplying to non-certified mills) units. As such, RSPO studies primarily inform us about whether oil palm growing companies were able to reduce deforestation within the plantations that they owned and certified through third-party audits (Bishop and Carlson, 2022). They do not provide insight into whether oil palm growing and trading companies were able to reduce deforestation among *all* the plantations they own and/or source from through implementation of their ZDCs. Indeed, understanding whether a company can exert sufficient leverage as a buyer to influence its suppliers' practices, or if it needs instead to acquire direct ownership to ensure their compliance, is a crucial question regarding the (vertical) organization of sustainable supply chains, yet has been left unanswered. Given the targeting and spillover challenges of mill-level certification, as well as the fact that RSPO-certified palm oil only accounts for 19-20% of global palm oil production (RSPO, 2024), a wider look at whether companies are fulfilling their ZDC pledges, and if this reduces deforestation, both in plantations they own source from, is warranted.

To date, evaluation of ZDCs across all sourcing areas has primarily focused on cattle (Alix-Garcia and Gibbs, 2017; Gibbs et al., 2016; Pereira et al., 2020; Skidmore et al., 2021; Levy et al., 2023) and soy (Gollnow et al., 2022; Heilmayr et al., 2020b; Rausch and Gibbs, 2016) in Brazil and timber in Chile (Heilmayr and Lambin, 2016). In Brazil, the Soy Moratorium and cattle ZDCs have contributed to deforestation reductions (Levy et al., 2023; Heilmayr et al., 2020b), though their impact would have likely been even larger if more companies had participated (Levy et al., 2023; Heilmayr et al., 2020b), if the policies were fully implemented (Pereira et al., 2020; Gollnow et al., 2022), and if laundering and leakage opportunities were reduced (Alix-Garcia and Gibbs, 2017; Gibbs et al., 2016; Skidmore et al., 2021; Rausch and Gibbs, 2016; Lambin and Furumo, 2023). To link *treated* companies to their *treated* suppliers, these studies usually face a trade-off between the precision and number of linkages considered: they must either focus on well-defined company-supplier linkages but only cover a small number of actors or regions (Gibbs et al., 2016; Pereira et al., 2020; Skidmore et al., 2021), or consider a large number of suppliers but use only a proxy of their linkages to companies such as the distance to a slaughterhouse (Alix-Garcia and Gibbs, 2017) or the share of committed slaughterhouses in a given municipality (Levy et al., 2023; Gollnow et al., 2022).*

Here, we combine new data detailing the Indonesian palm oil supply chain with assessments of company-level ZDCs and remotely sensed measures of oil palm-driven deforestation inside Indonesian oil palm concessions during the 2001-2020 period. This unique dataset allows us to conduct causal evaluations of palm oil ZDCs in Indonesia on forest loss and make several contributions to the literature. First, compared to the previous literature focusing on RSPO certification in Indonesia, we investigate whether ZDC companies were able to reduce deforestation among *all* of the owned and supplying plantations linked to them rather than among only a (possibly selective) *subset* of their RSPO-certified plantations. Second, compared to the prior ZDC literature, we overcome the trade-off between the precision and number of linkages considered since we can analyze detailed supply-chain linkages while covering nearly the entire country of Indonesia. Finally, the availability of data on both ownership and sourcing links between companies and their suppliers allow us to compare the relative efficiency of the ownership versus sourcing pathways in influencing deforestation.

We aim to assess the effectiveness of corporate ZDCs in the oil palm sector on deforestation in Indonesia implemented through ownership and sourcing pathways. Specifically, we ask four questions: 1) How complex is the Indonesian palm oil supply chain and how does this impact the identification of ZDC-linked concessions? 2) How compliant (i.e., degree to which zero-deforestation was achieved) were companies with ZDCS? 3) How much additionality (i.e., reductions in deforestation beyond what would be expected without ZDCs) was achieved by companies' ZDCs? 4) Were ZDCs with sourcing-based linkages as compliant and additional as ownership-based ones?

2. Results

Supply chains linkages are complex, overlapping and show moderate persistence over time. Our dataset, which provides new information about supply-chain linkages in the palm oil sector between 2018 and 2020 (see *Data*), reveals the commonly described funnel-shaped structure of the palm oil supply chain (Ermgassen et al., 2022; Grabs et al., 2021). At each stage of the chain, we observe a decreasing number of actors (Figure 1): 2644 oil palm concessions, 1222 palm oil mills owned by 190 companies (hereafter *mill-owning companies*) and 75 palm oil refineries owned by 32 refining company groups (hereafter *sourcing companies*). Mill-owning companies manage an average of 4.3 mills annually, while sourcing companies transact with an average of 138 distinct mills per year. These linkages introduce cross-relations between owning and sourcing companies. On average, a sourcing company buys annually from 35.5 distinct mill-owning companies, whereas a mill-owning company sells annually to 3.1 sourcing companies. Conversely, an average mill sells to 2.2 sourcing companies each year. Calculating these statistics at the concession level is more complicated due to limited information linking concessions and mills. However, focusing on a subset of high-confidence linkages, we observe that, on average, a mill sources from about four concessions on average per year.

We find that the linkages between sourcing companies and mills changed rapidly over time between 2018 and 2020: the stickiness coefficient, a measure of the persistence of linkages over time (Reis et al., 2020) is 0.47. This indicates that, on average, there is only a 47% probability that an existing linkage between a sourcing company and a mill will persist over two years. Furthermore, only 54% of the observed linkages persisted over the three-year period covered by our data, whereas 26% of the linkages appeared only for one year. On the other hand, ownership linkages are much more persistent, with a stickiness coefficient of 0.96 and 91% of the owning-mill linkages persisting over three years.

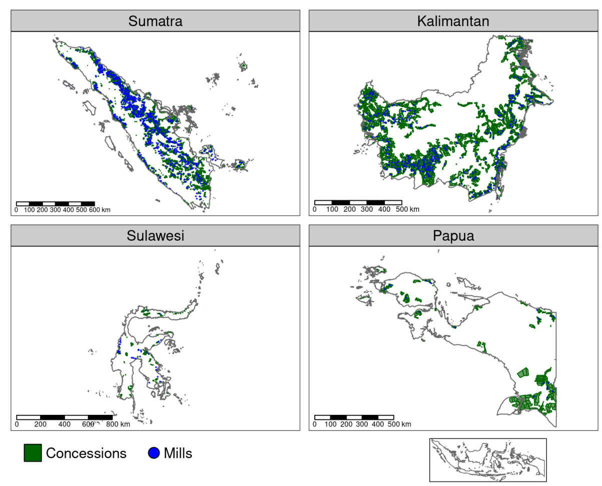

Fig. 1. Oil palm concessions (green) and palm oil mills (blue) in each island retained for the analysis.

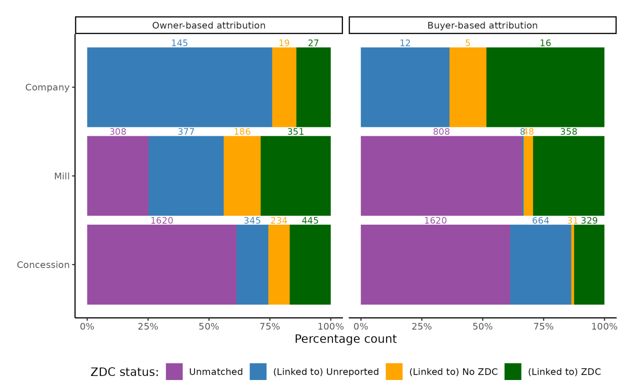

Fig. 2. ZDC status across supply chain levels by attribution method. We identified public ZDCs for 24% of mill-owning companies (top left) and 64% of sourcing companies (top right). Companies (top row) may influence their supply chain through ownership linkages (left column) or sourcing linkages (right column), potentially changing practices of both mills (middle row) and their supplying concessions (bottom row).

*A notable exception is Heilmayr et al. (2020b) who can analyse nearly all treated units across large areas in Brazil due to the near-universal adoption of the soy ZDC within its targeted region, the Amazon biome.

Between 12% and 17% of concessions are linked to ZDC companies. To identify the coverage of ZDCs across the supply chain and subsequently identify ZDC-linked concessions, we first classified companies as "No ZDC" or "ZDC" based on the strength of their public commitment reports (if any). We then attributed a ZDC status to every mill and subsequently to every concession using two approaches, based on 1) their ownership or 2) their most dominant sourcing linkages (or lack thereof) with the ZDC companies (see *Data*).

We were able to attribute a ZDC status to about a third to a quarter of mills and concessions (Figure 2). The remaining units could not be assigned a status either because no linkages could be established (*unmatched*), or because they were linked to companies without identifiable public reports (*linked to unreported*). We find differences in ZDC coverage across ownership versus sourcing linkages. Based on ownership linkages, we find that 14% of mill-owning companies have ZDCs, such that 29% of mills and 17% of concessions have ZDCs. When considering sourcing linkages, 48% of sourcing companies have ZDCs, while 29% of mills and 12% concessions have ZDCs. The ownership- and sourcing-based attribution approaches identify a different set of concessions as linked to ZDCs: only 48% of concessions have the same ZDC status according to both the ownership and sourcing linkages (see SI.T17).

High rates of deforestation prior to ZDC implementation were followed by significantly lower rates afterward. Turning to the average deforestation rates among the two main groups of interest, the *no-ZDC* and *ZDC* concessions, we find high annual rates of palm-driven deforestation peaking above 10% during the 2005-2012 period, followed by a rapid decline during 2013-2017 and then stabilization at low rates below 1% during 2018-2022 (Figure 3).

These time periods overlap with the introduction and implementation of ZDCs (see Figure SI.F1). In 2011-2012, at around the time deforestation rates peaked in concessions, the first companies began publishing initial no-deforestation pledges. Most companies adopted their commitments between 2013-2014. Implementation–the execution and enforcement of ZDCs– started in around 2014, and by 2018, when annualized deforestation was 1.4%, all the companies with ZDCs in our database were implementing their pledge (Grabs and Garrett, 2023; Lyons-White and Knight, 2018). We therefore refer to 2001-2012 as the "before ZDC implementation" period, 2013-2017 as "partial ZDC implementation", and 2018 onward as the "full ZDC implementation" period.

Contrasting forest cover and deforestation trends across ZDC categories, we observe two key patterns: 1) a reduction in the spread between ZDC and no-ZDC concessions over time, where initial differences in both forest cover and deforestation rates tend to converge towards very similar levels after 2018, and 2) a relatively small difference in 2001 forest cover in ZDC compared to non-ZDC concessions, with the relative ranking between them changing based on whether the definition is buyer- or owner-based. This initial difference is much smaller than compared to the *unmatched* and *linked to unreported* groups (see Table SI.T1 and Figure SI.F4) as well as compared to the 16% gap between RSPO-certified and non-certified groups reported in Carlson et al. (2018). This small difference indicates that the non-ZDC control group is relatively similar to the treated ZDC group in terms of average land cover and land cover change. It also suggests that ZDC companies did not necessarily choose which concession to own or source from based on initial forest cover, a key feature for our subsequent analysis using non-ZDC concessions as a control group.

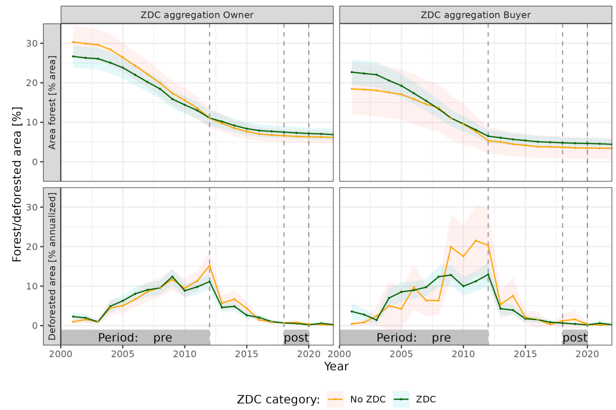

Fig. 3. Forest cover and deforestation rates within concessions averaged by ZDC status. Vertical dashed lines represent the periods used for the causal analysis: the "pre" period runs from 2001 to 2012 and the "post" from 2018 to 2020. Shaded area represent 95% confidence intervals of the averages.

ZDCs broadly achieved compliance. To investigate compliance, we examine deforestation rates in ZDC-linked concessions for the period after the policies were implemented when we have reliable supply chain data (2018-2020).[†] We find that average deforestation rates in both owner- and sourcing-linked ZDC concessions were low in 2018-2020, around 0.5%, in sharp contrast with rates of 7-8% before 2012 (Table 1). These low deforestation rates indicate notable rates of compliance with ZDCs, with the caveats that about 3% of ZDC concessions had individual deforestation rates with respect to 2000 forest cover above 1% in the 2018-2020 period, while total deforestation amounted to about 7,000 hectares across all ZDC concessions. If one were to consider compliance over the full period including the "partial ZDC implementation", we still find low rates of deforestation around 1.2%, although with a higher rate of non-compliance around 8.8%, suggesting companies eventually reached near compliance through a slow and gradual process.

Despite achieving compliance, ZDCs did not yield additionality compared to non-ZDC oil palm. To investigate additionality, i.e. the additional effect of ZDCs on deforestation rates beyond general trends, we use a difference-in-differences (DiD) causal inference approach comparing the deforestation means of treated (ZDC) versus control (*no ZDC*) concessions before (2001-2012) and during (2018-2020) full policy implementation.

When defining these groups based on the ownership linkages, we find that they experienced very similar reductions in deforestation of -6.50 (ZDC) and -6.63 (no ZDC) percentage points (see row *Diff post-pre* in Table 1). The difference-in-differences coefficient, equal to the difference (ZDC versus non-ZDC) between the two temporal differences (*pre* versus

[†] It should be noted, however, that some of the deforestation might occur on land that is neither peatland nor high carbon/conservation value forest and thus fall outside the scope of ZDCs (see *Data*).

Table 1. Results of the difference-in-differences analysis

ZDC group:	ZDC owner attribution: No ZDC	ZDC owner attribution: ZDC	ZDC buyer attribution: No ZDC	ZDC buyer attribution: ZDC
N	164	328	27	203
Mean pre:	7.12 ***	7.10 ***	9.56 ***	8.44 ***
Mean post:	0.62	0.48	1.06	0.42
Diff post-pre:	-6.50 ***	-6.63 ***	-8.50 ***	-8.02 ***
Diff-diff:	-0.12 (0.67)		0.48 (1.44)	
Wald stat:	0.49		<0.01	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\cdot}p < 0.1$ Standard errors clustered at the concession level. Outcome variable: Deforested area [% annualized]. *pre* denotes the 2001-2012 "before ZDC implementation" period and *post* the 2018-2020 "full ZDC implementation" period.

post), confirms this impression. ZDCs were associated with a small and not-significant decrease in annualized deforestation rates of -0.12 percentage points. The test of parallel pre-trends is not rejected, increasing confidence that the DiD model provides a causal estimate of the effect of ZDCs for the owner-based ZDC attribution.

Turning to the sourcing-based attribution, changes in deforestation rates are also similar across non-ZDC and ZDC concessions. The DiD coefficient is positive (0.48) yet not significant. However, the two groups did not exhibit parallel pre-trends, and the low stickiness of sourcing-based linkages raises the possibility of underestimation bias, as some partially treated units may have been considered as controls (see Methods). Together, these concerns warrant caution in drawing causal interpretations from this DiD model.

We interpret these DiD results, in particular those using the owner-based attribution, as evidence of a lack of additionality of ZDCs, since similar reductions in deforestation rates were observed in both the ZDC and non-ZDC groups. Given non-significant results for both the owner- and sourcing-based ZDC scores, we cannot compare whether ownership-based linkages are more effective at reducing deforestation than sourcing-based linkages.[‡]

A concern with the use of a DiD model is that companies might select the concessions they own or source from based on the concessions' previous deforestation patterns, which could potentially invalidate the parallel trend assumption. For example, Carlson et al. (2018) show that concessions certified by the RSPO had more oil palm and less forest in year 2000, compared to other concessions. Furthermore, as Garrett et al. (2016) argue, potential additionality from ZDC policies is higher when there are higher deforestation threats, which vary across frontier stages (Le Polain De Waroux et al., 2018), suggesting heterogeneity in the DiD coefficients across frontier stages. To address the first point, we investigate whether concessions' ZDC status can be explained by variables such as forest cover in 2000 and 2012, concession area, distance to mill, or average deforestation rate in the 2001-2012 period. The analysis reveals that only a few variables are significantly related to ZDC status, and that these variables have very low explanatory and predictive power (Table SI.T14). Furthermore, conducting our DiD analysis after matching on these variables still yielded the finding of no additionality (SI.F6). To address the second point, we cluster our concessions based on their forest cover in 2000 and 2010 (see Figure 4 and details in SI C.2) and conduct a DiD within each cluster. The clusters indicate that that most of the concessions included in our analysis (both ZDC and non-ZDC) are "old frontiers", with low to zero deforestation. Few concessions had high remaining forest area in 2010. The DiD coefficients are not significant in most clusters (see Table SI.T2), suggesting selection effects on forest cover and deforestation rate are not a reason for the null additionality results. We find only one exception with a significant coefficient yet a strong rejection of the parallel pre-trend test, suggesting caution in interpreting this result as causal.

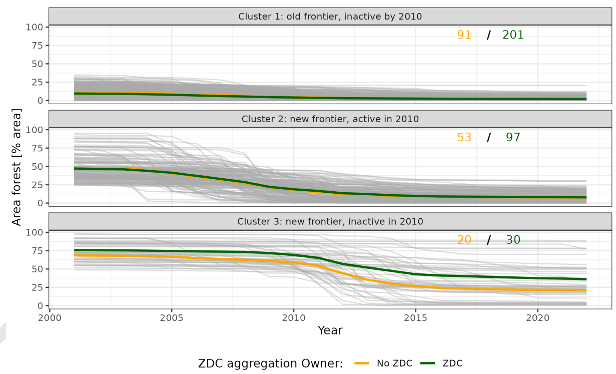

Fig. 4. Forest cover by cluster for each concession (grey lines), together with the average forest cover across ZDC and non-ZDC groups. For an interpretation of the clusters established using year 2000 and 2010 forest cover, see details in SI C.2.

The previous analysis was conducted using the *no-ZDC* concessions as control group for the *ZDC* concessions. This choice was motivated by the higher uncertainty in attributing a ZDC status to the *unmatched* or *linked to unreported* concessions, and because the *no-ZDC* group followed a trajectory more similar to the *ZDC* group during the pre-ZDC period (see Figure SI.F4 and Table SI.T1). However, because the higher uncertainty comes from either the absence of traceability reports mentioning the concession (*unmatched*) or from linkages to mills and companies without traceability reports (*linked to unreported*), there is still a high likelihood that *unmatched* and *linked to unreported* concessions were not subject to ZDCs. We therefore reran our DiD analysis using these alternate groups as controls. Results, shown in Table SI.T3, reveal a trade-off between strength of the results and credibility of the analysis: we find either weak (small and non-significant DiD coefficients) and more credible (lack of rejection of the parallel pre-trend test) results, or strong (large and significant coefficients) yet less credible (rejection of the parallel pre-trend test) results. In general, we believe these results reinforce our broader conclusion that ZDCs have not caused additional declines in deforestation.

Finally, we conduct a variety of robustness tests and re-estimate our DiD models 1) within individual islands (Table SI.T4), 2) changing the pre- or post-periods in various ways, in particular considering the full period 2013-2020 as treated (SI.T5), 3) further disaggregating the *ZDC* category into *low* and *high* ZDC commitments (SI.T6), 4) using alternate definitions of deforestation (SI.T7) or using

[‡] Formally, both coefficients could be non-significant yet still significantly different from each other. We tested this and found that we cannot reject the null hypothesis that both coefficients are equal.

forest cover (SI.T8), 5) comparing industrial-palm-driven deforestation to any deforestation in SI.T9 6) using an event-study analysis in SI.F5, 7) using a propensity-score matched DiD in SI.F6, 8) using alternate estimators such as matrix completion (Athey et al., 2021) or generalized synthetic control (Xu, 2017) in SI.T10, 9) looking at annual variations in treatment using de Chaisemartin and D'Haultfœuille (2024) in SI.F7, 10) using only concessions persistently linked to ZDC in SI.T12, 11) using as control concessions only those classified as control under both the owner- and sourcing-based ZDC scores in SI.T13, and 12) changing the sample inclusion rules (SI.T11). These additional tests largely confirm our main finding of no effect, except for a few cases that are not unexpected given the large number of hypothesis tests conducted.

3. Discussion & Conclusion

In media, corporate press releases, and civil society organization reports, company zero-deforestation commitments in the palm oil sector have been framed as both grand successes and abject failures (Zoological Society of London, 2020; Harvey, 2020; Mars, Incorporated, 2020). The truth behind these headlines depends on how one defines the goals and measures the impacts of such policies. Seen only through a before and after lens, Indonesian palm oil ZDCs appear to be highly successful at reducing deforestation (Purnomo et al., 2023). However, our analysis, using a counterfactual analysis, shows that ZDC supply chains did no better than non-ZDC supply chains in terms of deforestation. All concessions reduced their levels of deforestation substantially after 2012, especially compared to the peak in the late 2000s. During the *full ZDC implementation* period (2018-2020), annual deforestation rates in both the treated and control group were about 0.5%, much lower than the approximately 7% observed during the *pre* period 2001-2012. Thus, ZDC firms appear to be broadly complying with their own policies (a success from an internal corporate perspective), but these policies are not generating additional avoided deforestation (a failure from a broader policy perspective).

Why do we observe notable compliance without additionality? We posit that our result stems from a combination of low access to remaining forests, and decreasing market and regulatory incentives to clear forests. First, our analysis shows that companies had **low access to remaining forests to clear** within their concessions as most forests had already been cleared by 2012. On average, ZDC concessions lost 50% of their year 2000 forest during the 2001-2012 period, and by 2012 the remaining forest covered, on average, only 11% of the concessions' area. This result mirrors the findings from Carlson et al. (2018) who found that RSPO certification in the Indonesian palm oil sector was awarded to concessions largely free from forest. Interestingly, non-ZDC concessions displayed a similar pattern, with an average loss of 54% of 2000 forest and a remaining forest cover of 10% in 2012. Taken together, these two observations suggest possible explanations for why we see compliance (i.e., lack of remaining forest in ZDC concessions) and a lack of additionality (i.e., a similarly low forest area in non-ZDC concessions). However, ZDC implementation may have had impacts on forest outside of concessions in our dataset. There is indeed an increasing concern about off-concession deforestation, undertaken by industrial companies, mid-sized actors working without concession permits, and smallholder farmers (Grabs and Garrett, 2023). This is partly corroborated by Heilmayr et al. (2020a) who find that the proximity to RSPO-certified mills had both positive and negative effects on deforestation outside concessions depending on the type of land zoning considered.

A second contributing factor may be that, during the 2018-2020 treatment period, companies had decreasing and relatively **low market incentives to clear** remaining forested land on their plantations. Crude palm oil prices were significantly lower from 2018 to 2020 compared to 2001 to 2012 (see Figure SI.F3), reducing the likelihood of achieving positive returns on palm investments during 2017-2020 (Grabs and Garrett, 2023). Gaveau et al., in analyzing the rise and fall of forest loss in Borneo (Gaveau et al., 2019) and Indonesia as a whole (Gaveau et al., 2022), find that the price of crude palm oil is positively correlated with plantation expansion in the following year. Similarly, both Guye and Kraus (2022) and Cisneros et al. (2021) find a sizeable causal effect of prices on reducing deforestation, further supporting this argument. While palm oil prices started to increase in 2021, deforestation did not increase in the subsequent year (Gaveau et al., 2022), though there may be lags between price changes and oil palm development given the long pause in clearing and associated higher start-up costs.

Finally, the introduction and implementation of ZDCs coincided with multiple moratoria which decreased companies' **regulatory incentives to clear**. In 2011, the Indonesian State instituted a moratorium on the allocation of new forestry, agriculture, and mining concessions, including oil palm concessions, in primary forest and peatlands (Busch et al., 2015). This was followed by a nation-wide ban on clearing carbon-rich deep peatlands in 2016 (Jong, 2019) and a 2018 to 2021 moratorium on new oil palm plantation permits (Jong, 2018). These public policies decreased companies' ability to establish and exploit new concessions especially in lands with high forest cover, potentially explaining the low deforestation rates we observe across both ZDC and non-ZDC concessions. There are, however, two caveats to this argument. First, these policies have been criticized as ineffective by civil society and media observers due to the existence of loopholes, as well as a lack of transparency, monitoring, and enforcement (Jong, 2021, 2023). Scholarly work questions the effectiveness of the forest protection moratorium in significantly reducing forest and peatland loss compared to control areas (Busch et al., 2015; Groom et al., 2022). Second, the ban on new concessions pertains to concessions that are, by definition, not included in our sample of already-allocated concessions. While we expect that in the absence of this ban, hypothetical newly-allocated concessions would have had more forest and therefore experienced higher deforestation when developed for oil palm, providing a greater opportunity for ZDCs to conserve forest and achieve additionality, this counterfactual scenario is challenging to evaluate in practice.

In sum, our finding that ZDCs lacked additionality must be interpreted within the specific context of relatively low 2018-2020 deforestation rates across Indonesia. However, should access to forests, market incentives, and regulatory policies change and increase deforestation among some palm oil producers, ZDCs could achieve additionality provided that producers covered by ZDCs maintain compliance with these corporate policies. As such, we hypothesize that the additionality of ZDCs might become more significant during deforestation peaks than during the relatively calm period observed here (Garrett et al., 2016, 2019). This echoes the literature on the interactions between public and private conservation policies (Lambin et al., 2014; Lambin and Thorlakson, 2018; Furumo and Lambin, 2020), in particular the hypothesis made by Furumo and Lambin (2021) that "transnational actors provide a bulwark against changes in political winds". Only future developments will allow researchers to test this hypothesis.

Our study is one of the first to define ZDC treatment based on observed supply-chain linkages rather than proxies such as distance to processing facilities. In doing so, we were able to study whether companies, through ownership or sourcing channels, can transmit their sustainability pledges to their suppliers. Yet, our findings highlight how supply chain dynamics can complicate impact evaluations of supply-chain policies such as ZDCs. The complexity we documented, both in terms of interdependence and low stickiness of linkages, suggests that distinguishing between treated and non-treated units is and will remain a challenging task. This is exemplified by our owner- and sourcing-based attributions of ZDC status identifying different sets of concessions, with only 48% overlapping under both definitions. Furthermore, the fact that we do not observe supply-chain linkages during the *partial implementation* period raises the possibility that some concessions classified as controls in 2018-20 were in fact partially treated during 2013-17, potentially leading to an underestimation bias. The low stickiness of sourcing-based linkages makes this scenario more plausible, suggesting that results relying on these linkages should be interpreted with caution. The complexity of the supply chain also introduces the possibility that positive spillovers – i.e., unintended and indirect benefits that transfer from treated to non-treated units – occurred. Given the low stickiness and interdependence of trade linkages, an oil palm grower might perceive the risk of clearing land and thereby cutting itself off from potential buyers as too high to be worth it, even if the grower is currently not under any ZDC obligation. Furthermore, initiatives advocating for collective action across the entire sector (e.g. the Consumer Goods Forum) might also affect oil palm growing companies that are not directly linked to ZDCs (Lister and Dauvergne, 2014). If this is true, such positive spillovers may contribute to our finding of no additionality, as non-treated units might have mimicked treated units. This would imply that our analysis underestimates the effectiveness of ZDCs. While we cannot totally rule out this possibility, the analysis using alternate concession groups that are less likely to be affected by spillovers as controls either confirmed the finding of no additionality or proved inconclusive (SI.T3).

By shedding light on the additionality of supply chain policies in complex and dynamic supply chain structures and contexts, our results directly inform new deforestation and due diligence supply chain policies being set or discussed in Europe and the United States. In particular, our work informs ex-ante understanding of the potential impacts of the EU Deforestation Regulation, which aims to reduce the EU's carbon emissions footprint by prohibiting the import of deforestation-linked commodities including palm oil. Our results demonstrate that the vast majority of industrial palm oil production in Indonesian concessions is likely to meet the zero-deforestation requirement set by this legislation as the associated land was cleared much earlier than the EU's 2020 cut-off date. On the other hand, our work also shows that this state of affairs is based on large-scale clearance of forests prior to 2020, and may be due to favorable market and regulatory incentives that allowed for a substantial decline in industrial oil palm-driven deforestation. The low degree of purchasing stickiness and high number of overlapping linkages between sourcing companies documented here suggests that, at present, establishing completely differentiated ZDC supply channels may require substantial changes to sourcing strategies (e.g., reducing the number of suppliers) so that companies selling to the EU can avoid litigation risks related to the EU Deforestation Regulation. This outcome in turn runs the risk that companies consolidate their supply chains vertically, excluding smallholder producers (Chandra et al., 2024b; Eggen et al., 2024; Grabs et al., 2021; Cammelli et al., 2022; Verhaeghe and Ramcilovic-Suominen, 2024).

More broadly, our research adds to a growing body of evidence suggesting that tackling deforestation through supply chain policies often has limited direct effects on deforestation. A similar result was found for beef, the largest deforestation risk commodity in Brazil (Levy et al., 2023). In cases where voluntary supply chain policies have generated slightly higher additionality, such as the Soy Moratorium in Brazil (Gollnow et al., 2022) and RSPO-certified oil palm plantations in Indonesia (Carlson et al., 2018), these gains may have been accompanied by leakage to non-targeted areas (Villoria et al., 2022; Heilmayr et al., 2020a). Combining supply chain exclusion mechanisms like ZDCs with collaborative landscape approaches between companies and local government officials (Macdonald et al., 2023; von Essen and Lambin, 2021) may help to overcome both leakage and additionality concerns, at least at local to regional scales. Importantly, we emphasize that our result of no additionality should not lead companies in the oil palm sector to abandon their commitments, or civil society organizations to stop pressuring companies to eliminate deforestation from their supply chains. Our findings must be considered within the broader context of simultaneous ZDC implementation and recent deforestation reductions in Indonesia which may be driven by interactions between improved public forest governance and low economic incentives. If these policy and economic conditions change, as they did in Brazil to drive increases in deforestation circa 2015 after a decade of decline in forest loss, ZDCs may prove a critical safeguard for Indonesia's remaining carbon-rich and biodiverse forests.

4. Materials and methods

A. Data description. We integrated a large number of data sources to understand the coverage and impact of company-level zero-deforestation commitments on deforestation. These sources include data describing a) plantation concession boundaries, b) mill locations, c) forest cover, d) industrial oil palm plantations, e) ownership and sourcing links between mills and companies, f) sourcing links between mills and concessions, and g) companies' commitment quality.

The oil palm concession boundaries dataset (a) was assembled from several sources including manually digitized concessions from Roundtable on Sustainable Palm Oil (RSPO) audits and reports, Indonesian government data, NGO reports, and data provided by palm oil companies (see details in Chandra et al., 2024a). The data was cleaned, merged, and then duplicates and overlaps between concessions were removed following Chandra et al. (2024a).

The mill locations dataset (b) and the dataset on ownership relationships between the mills and companies (e) were identified using a comprehensive database of all palm oil mills in operation in Indonesia in 2022 (Benedict et al., 2023). Supply-chain linkages from mills to downstream refineries and exporting companies were defined using data from TRASE available for 2018-2020 (TRASE, 2022). These data are designed to describe trading relationships that are consistent with public traceability reports released by Indonesia's primary palm oil refiners and exporters. The data (f) on sourcing relationships between mills and upstream concessions were determined using data from TRASE, which seeks to match mills to concessions based on RSPO declarations and the names and locations of the concessions and mills (see TRASE, 2022, page 15).

We compiled comprehensive data about ZDC commitment quality (g) by computing scores for 51 companies–48 sourced from the SPOTT (Sustainable Palm Oil Transparency Toolkit) database (Oppenheimer et al., 2021) and 3 through manual assessments using the same SPOTT scoring method. We selected 15 indicators from the SPOTT database to evaluate companies' ZDC policy design and implementation, see Table SI.T20. Each indicator was evaluated according to a 3-level ZDC quality score, representing *high-quality*, *low-quality*, and *zero* ZDCs. Only companies committed to zero deforestation across all their suppliers were given either a *high*- or *low*-quality. Without this commitment, they were assigned a *zero* score. Companies that met a set of thresholds for all indicators were assigned a *high* score; otherwise, they received a *low* score, see Chandra et al. (2024b) for full details. For the main analysis, the *high* and *low* scores were merged together.

Having data on ZDC quality at the company level and supply-chain data linking companies to mills and then to concessions, we assigned a ZDC score to each concession. We first assigned each mill a score based on their linkages to companies, and then assigned each concession a score based on their linkages to the mills. The attribution method was slightly different for each step, as the TRASE data contains annual volumes and distinguishes between ownership or sourcing relationships for the company-mill links, whereas for the mill-concession links, we only had long-term linkages without volumetric information nor distinctions between ownership or sourcing. Figure 5 illustrates the aggregation problem we face for the attribution of a ZDC score at each step, noting that a concession can be linked to zero, one or multiple mills, which themselves can be linked to zero or multiple buyers or to a single owner, the latter not necessarily being matched to a SPOTT report. This raised the question of how to aggregate multiple ZDC quality scores into one (either from companies to mill or from mills to concessions), a question that, to the best of our knowledge, had not been addressed in the literature so far. We used a mode aggregation, attributing the most frequent ZDC score of downstream entities (either companies or mills) linked to a given upstream entity (either mill or concession). In the case of the companies-to-mill attribution step, we included the trade volume information indicated by TRASE by using a weighted mode. The procedure was simpler for ownership links, since ownership links involve only a one-to-one link between a mill and a company. While companies without public commitment reports likely did not have a ZDC, we kept a conservative approach and coded these companies separately as "Unreported" instead of "No ZDC", and coded subsequently-linked mills and concessions as "Linked to unreported". Finally, concessions without observed links to mills were classified as "Unmatched".

Deforestation data (c) for each concession were computed using the Hansen dataset (Hansen et al., 2013), setting a threshold of 90% tree canopy cover to define pixels as initial forest in 2000. We used Gunarso et al. (2013) and Gaveau et al. (2022) to exclude pixels that had already been planted with timber, rubber, mixed crop plantations, and oil palm in 2000. We then used Gaveau et al. (2022) to distinguish deforestation linked to industrial plantations from smallholder plantations or non-palm plantations. Ideally, one would consider only deforestation occurring on peatland or forest defined according to the High Carbon Stock (HCS) and High Conservation Value (HCV) frameworks to closely align with companies' ZDCs. However, such data is difficult to obtain, incomplete, and not consistently updated. Consequently, our measure of deforestation should be considered a proxy that may include some deforestation not infringing on companies' ZDCs. We construct deforestation rates as the annualized change in forest cover, that is, the change from year t to $t+1$ divided by the forest cover at year t. We report results with alternative definitions of the deforestation rate in Table SI.T7.

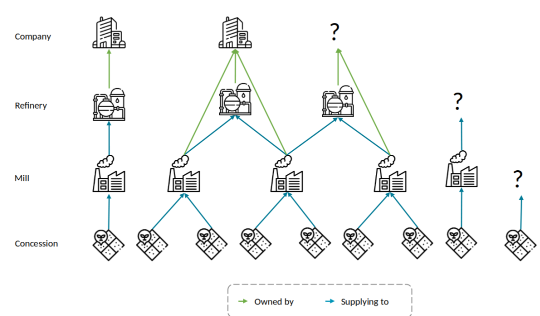

Fig. 5. Illustration of the possible configurations of linkages encountered in practice.

B. Methods.

B.1. Stickiness measures. To compute the stickiness of supply-chain linkages, we adapt equations (1) to (4) in Reis et al. (2020) modified for our two-layer mill-company network. Let $a_{ij}(t)$ be a 0/1 variable indicating whether there is a link between mill i and company j at time t. $C_i(t, t+1)$, the stickiness between year t and $t+1$ for mill i, is defined as:

$$C_i(t, t+1) \equiv \frac{\sum_j a_{ij}(t) a_{ij}(t+1)}{\sqrt{\left[\sum_j a_{ij}(t)\right]\left[\sum_j a_{ij}(t+1)\right]}} \qquad [1]$$

When a unit has no links at time t or $t+1$, this quantity is undefined, and we set it to 0 if unit i was at least connected once to any unit j, or discard it if it was fully disconnected at both times t and $t+1$. The unit-level and time-specific stickiness $C_i(t, t+1)$ is then aggregated over time and over units following equations (2) to (4) in Reis et al. (2020) to obtain the total mill-level stickiness coefficient. Likewise, we invert the j and i dimensions to obtain the total company-level stickiness coefficient.

B.2. Difference-in-differences model. To measure the additionality of ZDCs in reducing deforestation, we use a difference-in-differences (DiD) model. A DiD model compares treated and control groups before and after a policy, measuring whether the policy led to a change in the initial difference between control and treated. Our "policy" variable is the ZDC status, which is either "no ZDC" or "ZDC". The DiD coefficient can be given a causal interpretation as the average treatment effect under the "parallel trends" assumption, according to which the initial difference between treated and control units would have remained constant absent the policy.

To define the pre-intervention period for the DiD, we use 2012 as the last pre-intervention year, noting that the majority of companies announced their pledges between 2013 and 2016 (see Figure SI.F1), and refer to 2001-2012 as the "before ZDC implementation" period. Given the gradual nature of ZDC adoption and implementation, we divide the post-2012 period into two phase: "partial ZDC implementation" and "full ZDC implementation". The "partial ZDC implementation" phase (2013 to 2017) marks the rollout of pledge adoption and initial implementation efforts. This phase involved establishing supplier traceability, communicating commitments to suppliers, and monitoring compliance in order to take action against non-compliant suppliers. By 2018, the start of the "full ZDC implementation", pledges had been fully operationalized for nearly all companies (Grabs and Garrett, 2023; Lyons-White and Knight, 2018). According to Chandra (2024), the average target year for identified companies to achieve traceability to the mill was the end of 2017. Additionally, data collected by Palmoil.io also shows a sharp increase in the use of palm oil company grievance trackers during the 2018–2020 period compared to 2010–2017, see Figure SI.F8. Taken together, these elements suggest that by 2018, critical implementation milestones–including the establishment of monitoring systems, public disclosure of related information, and traceability efforts–had largely been achieved. In our main DiD analysis, we focus on the "full ZDC implementation" period as the (post) intervention period, while treating the "partial ZDC implementation" period as part of the (post) intervention period in robustness tests (see SI.T5). This modeling choice reflects the limited implementation observed between 2013 and 2017, as well as the availability of TRASE volumetric supply chain data only for the 2018–2020 period.

Our unit of analysis is an oil palm concession, with industrial-palm-driven deforestation as the primary outcome of interest. By focusing on concessions that could be clearly linked to mills, we target the areas where palm oil companies have the most direct influence in implementing their policies. A key limitation of this approach is that it assesses compliance and additionality within the supply chain but not at the broader landscape level. Expanding the analysis beyond concessions, though methodologically different, is a crucial direction for future research, as evidenced by Heilmayr et al. (2020a) regarding the RSPO mandate. We estimate our DiD model on a subset of the concessions that followed a set of criteria such as a positive level of forest cover in 2000 and the presence of industrial palm-oil driven deforestation, see SI C.1 and Table SI.T18 for details on the concession selection criteria as well as Tables SI.T3 and SI.T11 for robustness tests assessing the sensitivity of our results to these selection criteria.

Our outcome of interest is industrial-palm-driven deforestation, obtained by merging Hansen et al. (2013) with Gaveau et al. (2022). This focus is motivated by both the research question and data constraints. This type of deforestation is primarily controlled by large-scale palm oil corporations, making it the most relevant target for our analysis. Additionally, smallholder-palm-driven deforestation accounts for only 5% of total deforestation within the concession dataset and is predicted with lower accuracy by Gaveau et al. (2022).

The DiD is estimated using the following two-way fixed effect panel model:

$$y_{it} = \alpha_i + \alpha_t + \beta D_{it}^K + \varepsilon_{it} \qquad [2]$$

where the index i refers to the unit (concessions) while the index t corresponds to the time period. α_i and α_t are respectively the unit- and time fixed effects and y_{it} is the deforestation/forest cover variable of interest. D_{it}^K, with $K \in \{\text{owner}, \text{buyer}\}$, is a dummy variable taking a value of 0 during 2001-2012 and a value of 1 in 2018-2020 for the units which have a "ZDC" status and 0 for those that have a "no ZDC" status, where the definition of "ZDC" is based either on the owner or buyer attribution.

Because we observe supply-chain linkages only in 2018-2020, we cannot observe whether a concession classified as "linked to ZDC" during the *post* period was also "linked to ZDC" during the *pre* period, and vice-versa. Consequently, our DiD approach uses as treated units those concessions that were either *persistently linked to ZDC companies* or *newly linked to ZDC companies* (linked to ZDC companies during the *post* but not *pre* period). Likewise, our control group consists of concessions that were either *never linked to ZDC companies* or *previously linked to ZDC companies* (linked to ZDC companies during the *pre* but not *post* period). In the supplementary material SI B, we show that in such setting, the DiD estimates a (weighted) combination of three unobservable DiD estimators: the DiD on the *persistently-linked-to-ZDC*, the DiD on the *newly-linked-to-ZDC*, and the DiD on the *previously-linked-to-ZDC*, all evaluated using the *never-linked-to-ZDC* as control group. Assuming that each of these unobservable subgroups follows parallel trends with respect to the *never-linked-to-ZDC* subgroup in absence of treatment, we show that our DiD identifies a convex combination of the average treatment effect on the treated (ATT) on the *persistently-linked-to-ZDC* and the *newly-linked-to-ZDC* concessions. Furthermore, we demonstrate that under a relaxation of the parallel trends assumption, wherein ZDC companies maintain compliant concessions while excluding concessions with deforestation, our DiD estimator yields an upper bound of the weighted combination of ATTs. See supplementary material SI B for full details.

A further complication arises due to the fact that we do not observe treatment status during the *partial ZDC implementation* period. If, during that period, some concessions effectively reduce their deforestation under ZDC but are no longer linked to ZDC companies during the *full ZDC implementation* period, this could introduce under-estimation bias. The intuition behind this is that partially-treated units would act as control units; for a formal discussion see SI B. Such phenomenon is more likely to happen with the sourcing-based ZDC attribution, given the low stickiness of the linkages, so that the DiD estimates based on sourcing linkages should be interpreted with caution. As a robustness check, we restrict the sample to concessions that were permanently linked to ZDC companies versus those who were never linked to ZDC, and obtain qualitatively similar estimates (see Table SI.T12).

To examine whether the parallel trends assumption is likely to hold, we run a parallel pre-trends test. This is obtained by running an event study, normalizing the coefficient at time -1 (2011) to be 0 (see SI.F5), and then running a Wald test for the joint hypothesis that all normalized event-study coefficients before the intervention are 0. A low p-value would indicate that the null hypothesis of parallel trends is rejected, casting doubt on the interpretation of the DiD coefficient as causal.

In some robustness checks, we further disaggregated the ZDC score into "High" and "Low" (see section Data). This allowed us to run three DiD models: the two first comparing either "High ZDC" or "Low ZDC" to the control group of "No ZDC", the last one comparing "High ZDC" to the control group of "Low ZDC", see Table SI.T6.

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Supporting Information

Contents

SI A. Supplementary tables and figures.

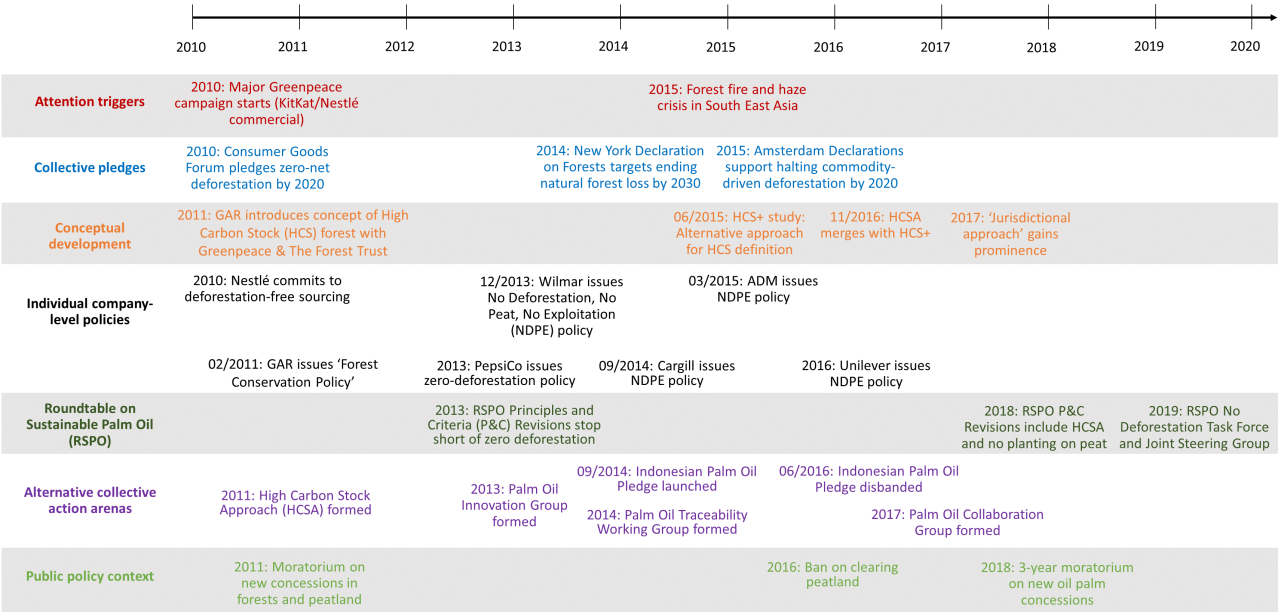

Fig. SI.F1. Timeline of ZDC implementation steps

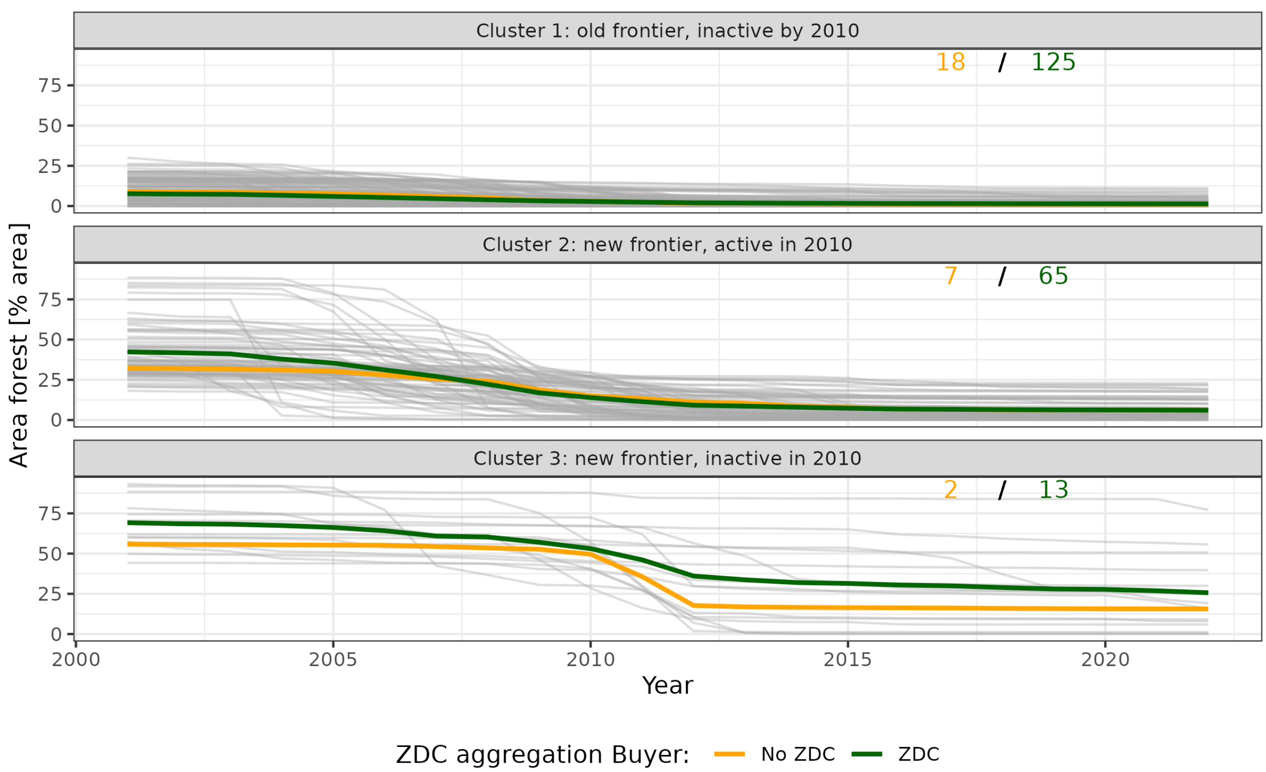

Fig. SI.F2. Forest cover for each concession, together with the average across ZDC and non-ZDC groups, buyer-based score

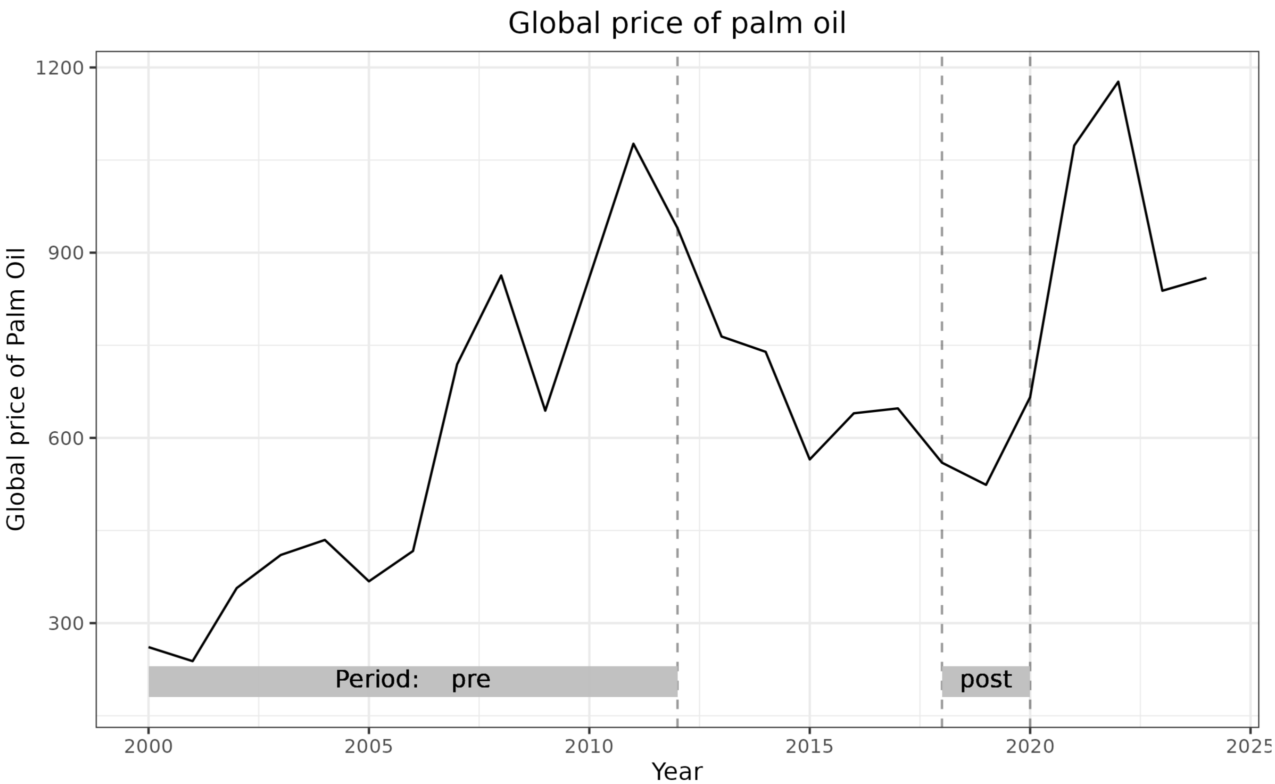

Fig. SI.F3. Evolution of global palm oil prices. Source: annualized values from International Monetary Fund, Global price of Palm Oil [PPOILUSDM], retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/PPOILUSDM, May 21, 2024.

Table SI.T1. Descriptive statistics of the sample, by ZDC score

Variable	Unmatched	Linked to Unreported	No ZDC	ZDC: low	ZDC: high	ZDC (low+high)
	ZDC score					
	ZDC score: owner					
N initial sample	1620	345	234	209	236	445
N remove: not in main island	145	1				
N remove: 0 forest 2000	122	22	14	7	22	29
N remove: 0 industrial-palm deforestation	1034	93	70	48	69	117
N final sample	584	252	164	161	167	328
Average distance to first mill [km]	14.66 (0.58)	7.05 (0.33)	8.87 (0.73)	7.70 (0.59)	7.64 (0.47)	7.67 (0.37)
Average concession area [km^2]	85.38 (3.67)	90.44 (4.7)	91.79 (5.28)	109.83 (6.87)	121.47 (6.23)	115.76 (4.63)
Oil palm suitability index (%)	6333.99 (59.23)	6333.58 (85.41)	6359.92 (100.94)	6519.06 (97.94)	6497.17 (89.23)	6507.92 (66.04)
Average forest cover in 2000	38.31 (2.44)	31.65 (2.77)	32.24 (3.5)	29.25 (3.3)	35.10 (3.66)	32.23 (2.47)
Average forest cover in 2012	24.07 (1.06)	12.08 (1.13)	11.11 (1.27)	10.87 (1.29)	11.29 (1.49)	11.08 (0.99)
Average deforestation (annualized)	3.36 (0.15)	5.66 (0.26)	5.33 (0.33)	4.94 (0.32)	5.24 (0.35)	5.09 (0.23)
Average deforestation 2001-12 (annualized)	4.05 (0.23)	7.58 (0.4)	7.12 (0.53)	6.72 (0.48)	7.46 (0.55)	7.10 (0.36)
Average deforestation 2018-20 (annualized)	1.28 (0.16)	1.16 (0.29)	0.62 (0.15)	0.51 (0.12)	0.44 (0.1)	0.48 (0.08)
	ZDC score: buyer					
N initial sample	1620	664	31	94	235	329
N remove: not in main island	145	1				
N remove: 0 forest 2000	122	26		2	37	39
N remove: 0 industrial-palm deforestation	1034	150	4	15	111	126
N final sample	584	514	27	79	124	203
Average distance to first mill [km]	14.66 (0.58)	8.25 (0.33)	7.19 (0.98)	5.73 (0.4)	6.92 (0.57)	6.46 (0.38)
Average concession area [km^2]	85.38 (3.67)	102.05 (3.69)	75.65 (7.52)	91.57 (6.13)	113.56 (6.57)	105.00 (4.72)
Oil palm suitability index (%)	6333.99 (59.23)	6288.45 (58.32)	7003.21 (210.44)	6594.50 (119.96)	6704.59 (102.24)	6661.75 (77.87)
Average forest cover in 2000	38.31 (2.44)	36.00 (2.18)	15.07 (2.92)	26.36 (2.85)	22.89 (2.9)	24.24 (2.09)
Average forest cover in 2012	24.07 (1.06)	13.70 (0.85)	5.35 (1.6)	9.52 (1.64)	4.57 (0.77)	6.49 (0.81)
Average deforestation (annualized)	3.36 (0.15)	5.10 (0.18)	6.73 (0.65)	5.63 (0.5)	5.82 (0.42)	5.75 (0.32)
Average deforestation 2001-12 (annualized)	4.05 (0.23)	6.68 (0.27)	9.56 (1.09)	8.28 (0.78)	8.55 (0.66)	8.44 (0.5)
Average deforestation 2018-20 (annualized)	1.28 (0.16)	0.85 (0.15)	1.06 (0.58)	0.43 (0.15)	0.42 (0.1)	0.42 (0.08)

The table gives various counts or averages, together with standard errors for the averages. Results are disaggregated by ZDC score. The last three columns indicate whether means are statistically significant comparing *L vs Z*: Low versus Zero, *H vs Z* High versus Zero and *H vs L* High versus Low.

Table SI.T2. DiD results by frontier-stage cluster

	ZDC owner attribution				ZDC buyer attribution			
	All	Cluster: 1	Cluster: 2	Cluster: 3	All	Cluster: 1	Cluster: 2	Cluster: 3
ZDC treatment	-0.12	-0.78	0.88	0.98	0.48	2.20	-3.75^{*}	1.91
	(0.67)	(0.89)	(1.18)	(1.03)	(1.44)	(1.92)	(1.85)	(4.46)
Num. obs.	7380	4380	2250	750	3450	2145	1080	225
Num. control:	164	91	53	20	27	18	7	2
Num. treated:	328	201	97	30	203	125	65	13
Parallel test: Wald stat	10.40	10.63	8.05	8.61	41.64	43.26	46.66	46913.89
Parallel test: p-val	0.49	0.47	0.71	0.66	0.00	0.00	0.00	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Column *All* refers to the benchmark regression including all clusters, and therefore corresponds to Table 1. Columns with *Cluster* refer to the regressions run for each frontier-stage cluster separately. We interpret the clusters as respectively: "Old frontier, inactive by 2010", "New frontier, active in 2010", and "New frontier, inactive in 2010" see SI C.2 for details on how the clusters are constructed and interpreted.

Table SI.T3. DiD results using alternative categories as control group

	ZDC owner attribution			ZDC buyer attribution		
Control group:	Unmatched	Linked to Unreported	No ZDC	Unmatched	Linked to Unreported	No ZDC
ZDC treatment	-3.85^{***}	-0.20	-0.12	-5.25^{***}	-2.18^{***}	0.48
	(0.47)	(0.64)	(0.67)	(0.58)	(0.61)	(1.44)
Num. obs.	13680	8700	7380	11805	10755	3450
Num. control:	584	252	164	584	514	27
Num. treated:	328	328	328	203	203	203
Parallel test: Wald stat	47.41	12.89	10.40	55.24	19.39	41.64
Parallel test: p-val	0.00	0.30	0.49	0.00	0.05	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Outcome variable: Deforested area [% annualized]

Table SI.T4. DiD results within each island

	Kalimantan		Papua	Sulawesi	Sumatra	
ZDC attribution:	Owner	Buyer	Owner	Owner	Owner	Buyer
ZDC treatment	0.36	-0.25	1.55	0.51	-1.79	3.28
	(0.82)	(1.62)	(1.25)	(1.30)	(1.29)	(1.94)
Num. obs.	4740	2115	300	90	2250	1305
Num. control:	114	24	8	1	41	3
Num. treated:	202	117	12	5	109	84
Parallel test: Wald stat	11.09	31.23	23.90	1445.92	6.80	263.91
Parallel test: p-val	0.44	0.00	0.01	0.00	0.82	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level. Outcome variable: Deforested area [% annualized].
Depending on the ZDC attribution method, some islands do not have enough units to run a difference-in-differences model.

Table SI.T5. DiD results changing the pre/post periods

	01-12 vs 18-20		01-12 vs 13-20		01-10 vs 18-20		01-12 vs 18-22		01-12 vs 13-17	
ZDC attribution:	Owner	Buyer	Owner	Buyer	Owner	Buyer	Owner	Buyer	Owner	Buyer
ZDC treatment	-0.12	0.48	-0.53	0.32	-0.68	-1.07	0.02	0.82	-0.77	0.22
	(0.67)	(1.44)	(0.76)	(1.48)	(0.66)	(1.44)	(0.65)	(1.31)	(0.89)	(1.67)
Num. obs.	7380	3450	9840	4600	6396	2990	8364	3910	8364	3910
Num. control:	164	27	164	27	164	27	164	27	164	27
Num. treated:	328	203	328	203	328	203	328	203	328	203
Num. periods:	15	15	20	20	13	13	17	17	17	17
Parallel test: Wald stat	10.40	41.64	10.40	41.64	6.16	36.88	10.40	41.64	10.40	41.64
Parallel test: p-val	0.49	0.00	0.49	0.00	0.72	0.00	0.49	0.00	0.49	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Outcome variable: Deforested area [% annualized]

Table SI.T6. DiD results with disaggregated ZDC score

	ZDC buyer attribution				ZDC owner attribution			
Control group:	Zero	Zero	Low	Zero	Zero	Zero	Low	Zero
Treat: Low	0.65				0.29			
	(1.57)				(0.75)			
Treat: High		0.36	−0.28			−0.53	−0.82	
		(1.50)	(1.05)			(0.79)	(0.76)	
Treat: Low-High				0.48				−0.12
				(1.44)				(0.67)
Num. obs.	1590	2265	3045	3450	4875	4965	4920	7380
Num. control:	27	27	79	27	164	164	161	164
Num. treated:	79	124	124	203	161	167	167	328
Parallel test: Wald stat	21.05	38.62	7.87	41.64	8.17	15.35	12.77	10.40
Parallel test: p-val	0.03	0.00	0.72	0.00	0.70	0.17	0.31	0.49

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level. Outcome variable: Deforested area [% annualized].
The rows starting with *Treat* indicate which treated group is considered, either *Low* ZDC, *High* ZDC, or both (*Low-High*). The columns indicate which control group is used.

Table SI.T7. DiD results using alternative definitions of deforestation

	Deforested area [hec]		Deforested area [% annualized]		Deforested area [% forest]		Deforested area [% area]	
ZDC attribution:	Owner	Buyer	Owner	Buyer	Owner	Buyer	Owner	Buyer
ZDC treatment	0.28	-0.59^{**}	−0.12	0.48	0.03	0.51	0.36^{*}	−0.28
	(0.20)	(0.18)	(0.67)	(1.44)	(0.28)	(0.62)	(0.16)	(0.24)
Num. obs.	7380	3450	7380	3450	7380	3450	7380	3450
Num. control:	164	27	164	27	164	27	164	27
Num. treated:	328	203	328	203	328	203	328	203
Parallel test: Wald stat	6.10	35.18	10.40	41.64	8.88	46.23	11.07	23.55
Parallel test: p-val	0.87	0.00	0.49	0.00	0.63	0.00	0.44	0.01

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Deforested area [hectare]: year-to-year deforestation
Deforested area [% annualized]: year-to-year deforestation divided by the concession's forest area the preceding year
Deforested area [% forest]: the year-to-year deforestation divided by the concession's forest area in 2000
Deforested area [% area]: the year-to-year deforestation divided by the concession's area

Table SI.T8. DiD results using forest area instead of deforestation

	Area forest [hec]		Area forest [% area]	
ZDC attribution:	Owner	Buyer	Owner	Buyer
ZDC treatment	1.96	-3.89^{*}	3.02^{*}	−0.60
	(2.13)	(1.77)	(1.51)	(2.35)
Num. obs.	7380	3450	7380	3450
Num. control:	164	27	164	27
Num. treated:	328	203	328	203
Parallel test: Wald stat	5.64	31.34	7.20	26.07
Parallel test: p-val	0.90	0.00	0.78	0.01

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Area forest [hec]: Forest area (in hectares) in each concession.
Area forest [% area]: Forest area divided by concession's total area.

Table SI.T9. DiD results comparing industrial-palm driven and any deforestation

	ZDC owner attribution		ZDC buyer attribution	
	Any deforestation	Industrial palm deforestation	Any deforestation	Industrial palm deforestation
ZDC treatment	-0.87	-0.12	-0.67	0.48
	(0.78)	(0.67)	(1.55)	(1.44)
Num. obs.	7380	7380	3450	3450
Num. control:	164	164	27	27
Num. treated:	328	328	203	203
Parallel test: Wald stat	12.02	10.40	48.15	41.64
Parallel test: p-val	0.36	0.49	0.00	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.

Table SI.T10. DiD results using the Generalized Synthetic Control and the Matrix Completion methods

	ZDC owner attribution			ZDC buyer attribution		
	Two-way FE	Generalized synthetic	Matrix Completion	Two-way FE	Generalized synthetic	Matrix Completion
ZDC treatment	-0.12	-0.38	-0.39	0.48	-0.60	0.41
	(0.67)	(0.59)	(0.53)	(1.44)	(2.20)	(1.44)
Num. obs.	7380	7380	7380	3450	3450	3450
Num. control:	164	164	164	27	27	27
Num. treated:	328	328	328	203	203	203
Hyperparameter:		2.00	0.01		1.00	0.05

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Two-way FE refers to the standard two-way fixed effect as used above.
Generalized synthetic refers to the Generalized Synthetic Control method of Xu (2017).
Matrix Completion refers to the matrix completion method of Athey et al. (2021).
The row *hyperparameter* indicates either the number of factors (Generalized synthetic) or the λ penalty (matrix completion) parameter selected by cross-validation.

Table SI.T11. DiD results including concessions that never had palm-driven deforestation

	ZDC owner attribution		ZDC buyer attribution	
Control sample:	Benchmark	Include 0 palm-deforest	Benchmark	Include 0 palm-deforest
ZDC treatment	-0.12	-0.38	0.48	1.79
	(0.67)	(0.56)	(1.44)	(1.34)
Num. obs.	7380	9540	3450	4815
Num. control:	164	220	27	31
Num. treated:	328	416	203	290
Parallel test: Wald stat	10.40	10.38	41.64	38.98
Parallel test: p-val	0.49	0.50	0.00	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Benchmark describes the standard sample used in the main text (see SI C.1) that excludes concessions that never had industrial palm-driven deforestation.
Include 0 palm-deforest includes concessions that never had industrial palm-driven deforestation.

Table SI.T12. DiD results using concessions that were persistently linked to ZDC

	ZDC owner attribution		ZDC buyer attribution	
Concessions:	All	Persistent-only	All	Persistent-only
ZDC treatment	-0.12	0.05	0.48	-1.23
	(0.67)	(0.76)	(1.44)	(2.57)
Num. obs.	7380	6255	3450	1725
Num. control:	164	135	27	7
Num. treated:	328	282	203	108
Parallel test: Wald stat	10.40	10.01	41.64	46.84
Parallel test: p-val	0.49	0.53	0.00	0.00

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Column *All* is the benchmark set of concessions used in the main analysis. Column *Persistent-only* uses only concessions that were persistently either ZDC or non-ZDC throughout the three years in which the linkages are observed.

Table SI.T13. DiD results using concessions that are neither linked to companies through ownership or sourcing linkages

	Benchmark		Control: neither owner- nor buyer-linked			
	Owner	Buyer	Owner and Buyer	Owner or Buyer	Owner only	Buyer only
ZDC treatment	-0.12	0.48	1.33	1.82	3.80	2.91
	(0.67)	(1.44)	(1.87)	(1.85)	(2.25)	(3.62)
Num. obs.	7380	3450	2190	2745	600	315
Num. control:	164	27	12	12	12	12
Num. treated:	328	203	134	171	28	9
Parallel test: Wald stat	10.40	41.64	69.14	74.02	27.65	14.55
Parallel test: p-val	0.49	0.00	0.00	0.00	0.00	0.20

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Standard errors clustered at the concession level.
Columns under *Benchmark* refers to the standard DiD using either owner- or sourcing-based ZDC scores separately.
Columns under *Control: neither owner- nor buyer-linked* refer to specifications where *control* concessions are those that are considered as control under both the owner- and sourcing-based ZDC scores. *Owner and Buyer* includes only concessions treated under both scores; *Owner or Buyer* includes those treated under at least one; and *Buyer only* and *Owner only* restrict treatment to those treated under a single score.

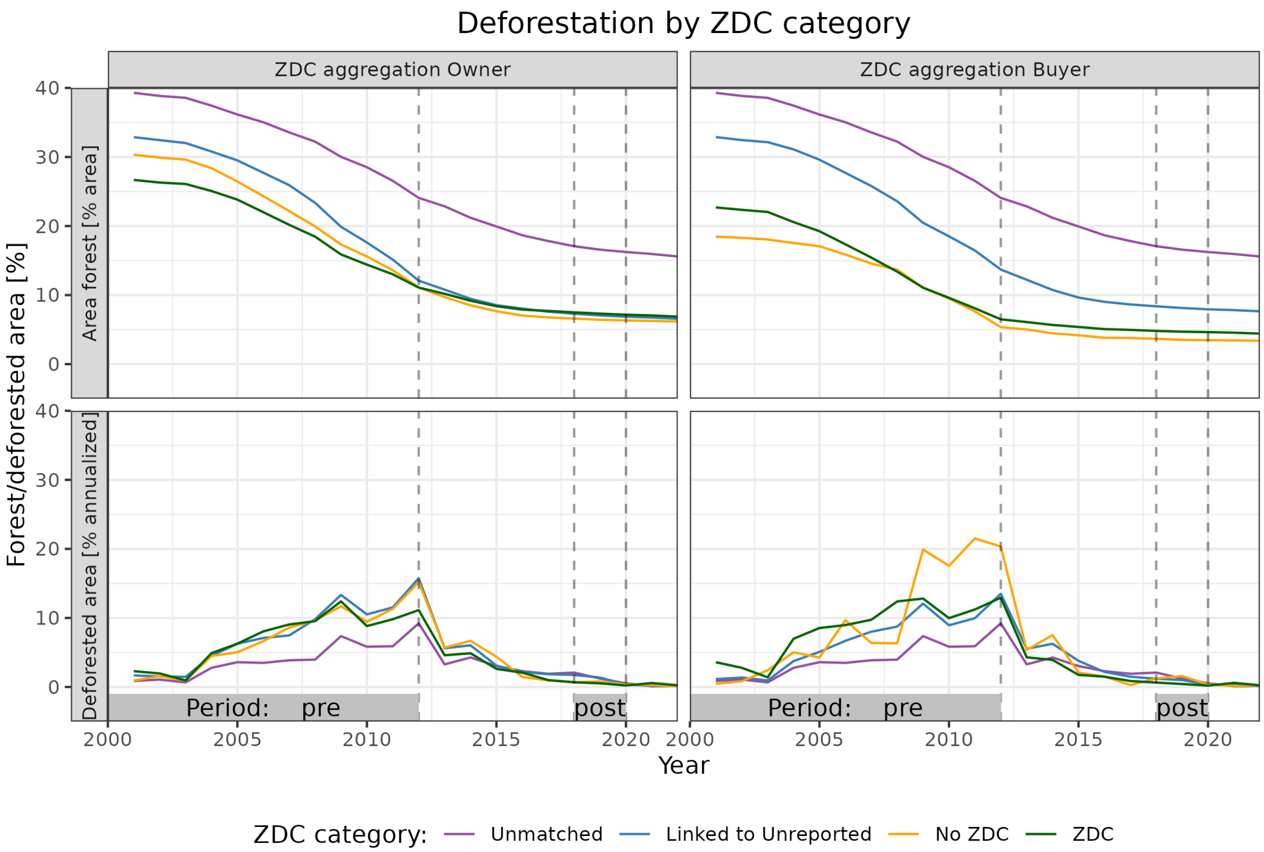

Fig. SI.F4. Forest cover and deforestation rates by ZDC status, all categories

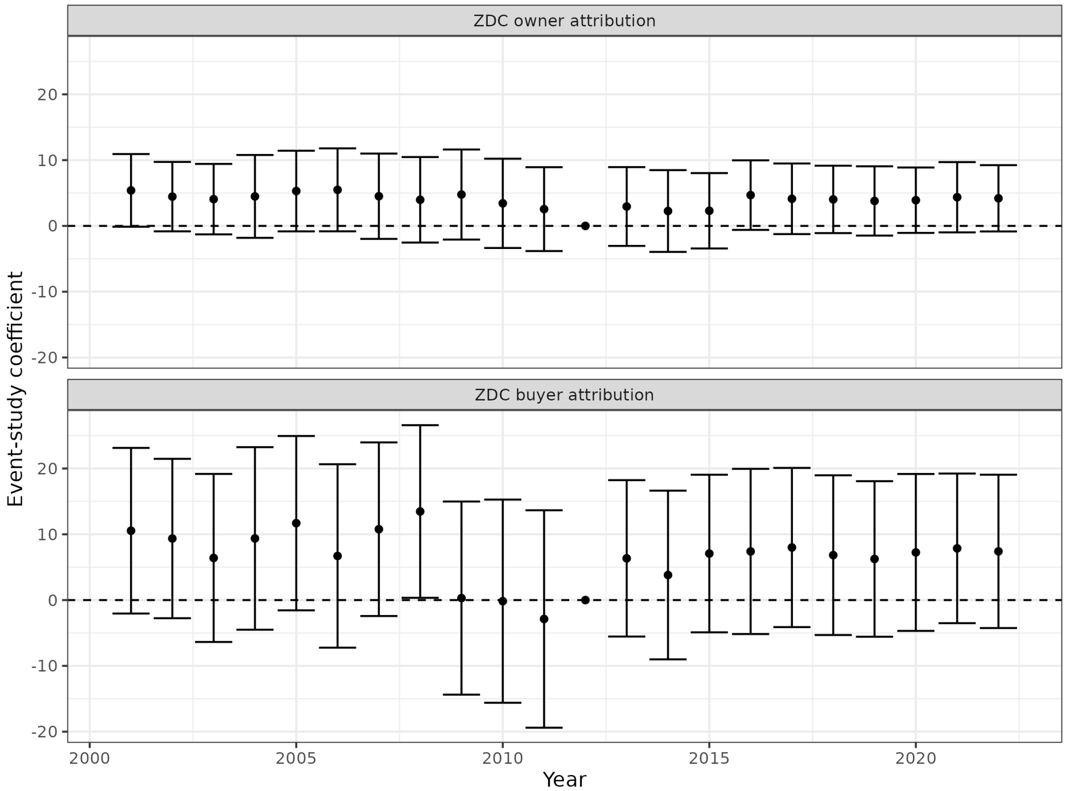

Fig. SI.F5. Event study plot

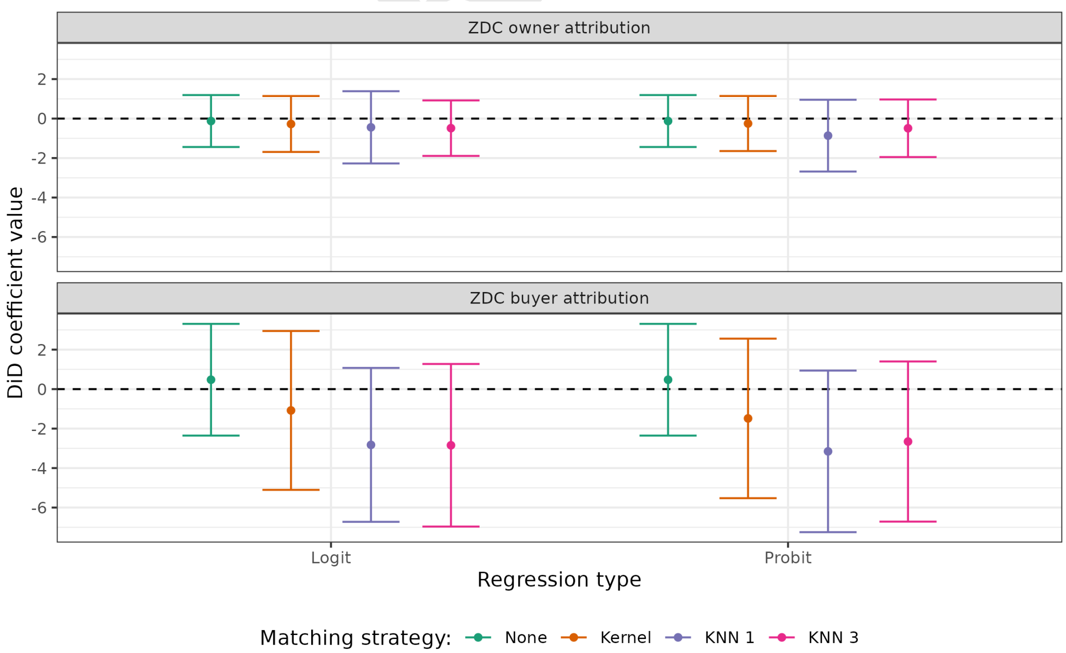

Fig. SI.F6. DiD coefficients using propensiy-score matching weights

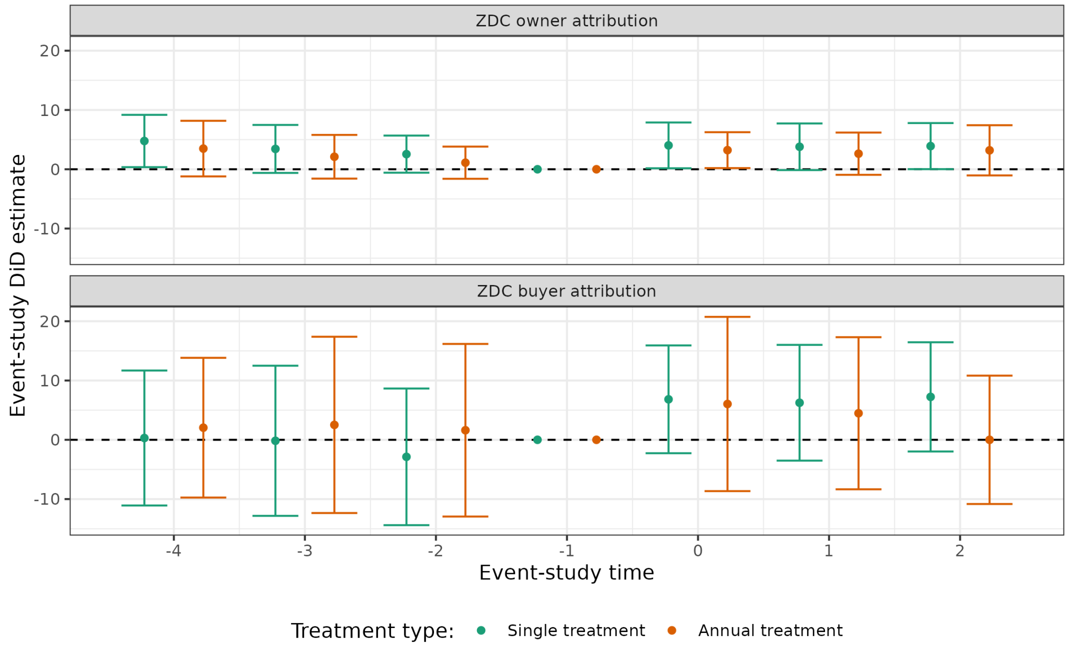

Fig. SI.F7. Event study coefficients using annual treatment with the de Chaisemartin and d'Haultfeuille (2024) estimator

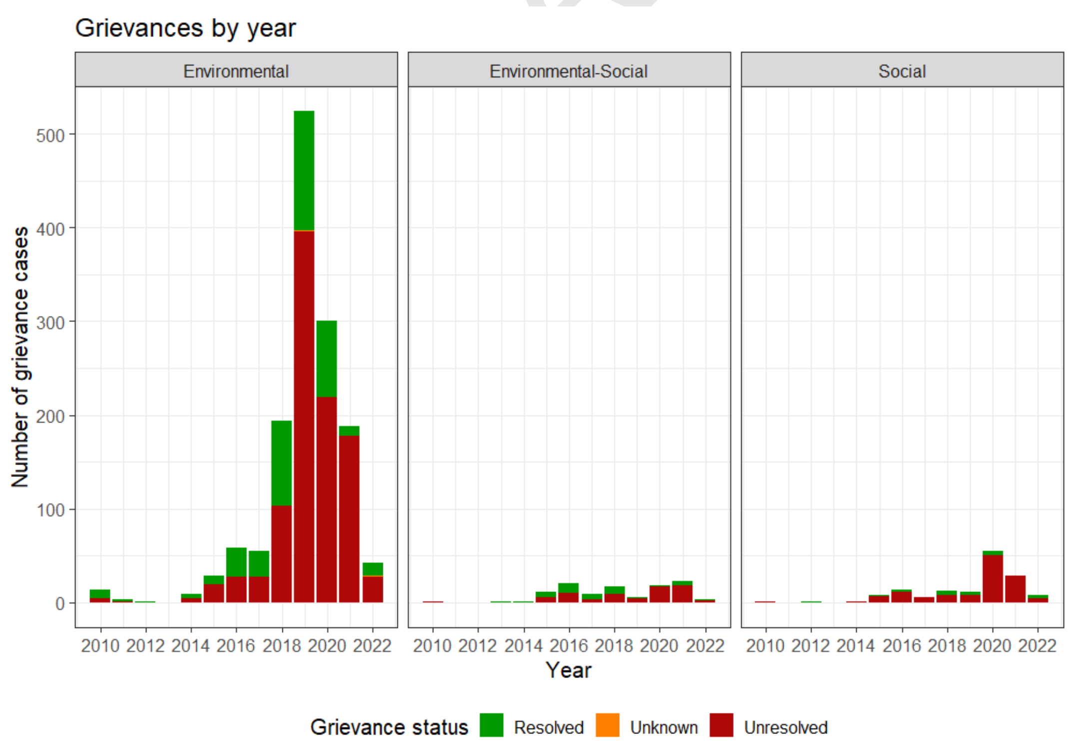

Fig. SI.F8. Grievances by year. Source: own illustration using data from https://www.palmoil.io/.

Table SI.T14. Estimates of ZDC status

	ZDC owner attribution			ZDC buyer attribution		
	Linear	Probit	Logit	Linear	Probit	Logit
Distance to first mill	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Concession size	0.001^{***}	0.002^{***}	0.002^{***}	0.000	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Concession forest % in 2000	-0.002^{*}	-0.003^{**}	-0.003^{**}	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Concession forest % in 2012	0.003	0.004	0.004	-0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Deforestation rate 2001-12	0.004	0.006	0.007	-0.013	-0.012	-0.012
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Palm oil suitability (GAEZ)	0.000	0.000	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
AIC	651.500	616.392	616.058	137.769	169.703	170.026
BIC	685.088	645.781	645.447	165.274	193.769	194.093
Num. obs.	492	492	492	230	230	230
Pseudo R^2	0.041	0.038	0.039	0.040	0.064	0.062
Kappa	0.012	0.051	0.070	0.000	0.000	0.000
F1	0.046	0.120	0.149			

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

The coefficients shown are average marginal effects estimated with R package *margins* (Leeper, 2024). Rows (McFadden's) *Pseudo* R^2, *Kappa*, and *F1* are the goodness of fit and of classification metrics.

Table SI.T15. Matching-only strategy: propensity-score estimates

	ZDC owner attribution				ZDC buyer attribution			
Matching method:	KNN 1	KNN 3	Kernel	None	KNN 1	KNN 3	Kernel	None
Constant	0.87^{**}	0.79^{***}	0.82^{***}	0.67^{***}	1.65	1.95	1.71	1.27^{*}
	(0.29)	(0.21)	(0.20)	(0.16)	(0.99)	(1.16)	(0.95)	(0.63)
ZDC treatment	-0.10	-0.08	-0.08	-0.05	-0.31	-0.38	-0.31	-0.21
	(0.07)	(0.06)	(0.05)	(0.04)	(0.25)	(0.29)	(0.24)	(0.16)
Num. obs.	1356	1470	1392	1476	687	690	399	690

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Heteroskedasticity-robust standard errors.
The regressions are weighted based on matching on propensity scores from a logit model. Three types of matching strategies are used: either first ("KNN 1") or three first ("KNN 3") nearest neighbor(s), or using a rectangular kernel ("Kernel"). The regressions are run using the years 2018-2020.

Table SI.T16. Sample means balance before/after matching

		Unweighted			Weighted		
ZDC attribution	Variable	No ZDC	ZDC	Diff	No ZDC	ZDC	Diff
ZDC owner attribution	Average distance to first mill [km]	8.9	7.7	-1.2	8.0	7.6	-0.5
	Average concession area	91.8	115.8	24.0	105.7	106.5	0.7
	Average forest cover in 2000	32.2	32.2	-0.0	32.4	32.7	0.2
	Average deforestation 2001-12 (annualized)	7.1	7.1	-0.0	7.1	7.1	0.0
	Oil palm suitability index (%)	6359.9	6507.9	148.0	6591.2	6492.2	-99.0
ZDC buyer attribution	Average distance to first mill [km]	7.2	6.5	-0.7	6.7	6.0	-0.7
	Average concession area	75.7	105.0	29.4	90.8	88.5	-2.3
	Average forest cover in 2000	15.1	24.2	9.2	19.4	18.0	-1.5
	Average deforestation 2001-12 (annualized)	9.6	8.4	-1.1	8.2	8.3	0.2
	Oil palm suitability index (%)	7003.2	6661.7	-341.5	6732.1	6742.6	10.5

Columns *unweighted* refer to the group means before matching, whereas columns *weighted* refer to the means after matching. Weighting was done by doing kernel matching on the propensity score estimated using a logit link.

Table SI.T17. Comparison of ZDC attribution based on owner/buyer score

Owner↓ / Buyer→	Linked to Unreported	No ZDC	ZDC	Total Owner
Linked to Unreported	272	8	65	345
No ZDC	159	14	61	234
ZDC	233	9	203	445
Total Buyer	664	31	329	1024

The numbers in the table indicate the number of concessions classified as *Linked to Unreported*, *No ZDC*, and *ZDC* based on both the owner-based or buyer-based attribution. The last row and last column indicate the total number of concessions based on each score, and therefore correspond to the numbers show in Figure 2.

SI B. Formalization of the DiD approach. In the main analysis, we use a difference-in-differences (DiD) approach comparing *post* and *pre*-intervention outcomes between concessions linked to ZDC companies (*treated*) against those not linked to ZDC companies (*control*). Unfortunately, we only observe the company-concession supply-chain linkages during the *post* period (2018-2020) and not during the *pre* period. Consequently, our DiD analysis defines the *treated* and *control* groups based on the *post* supply-chain linkages. However, some concessions that are part of ZDC in the *post* period might have been part of the non-ZDC supply chain in the *pre* period, or vice versa. Thus, we have four groups of concessions, the *persistently-linked-to-ZDC*, the *never-linked-to-ZDC*, the *newly-linked-to-ZDC* (linked to ZDC during the *post* but not *pre* period), and the *previously-linked-to-ZDC* (linked during the *pre* but not *post* period). Since we observe only the linkages to ZDC companies during the *post* treatment period, we cannot distinguish whether the treated units (concessions linked to ZDC in the *post* period) are *persistently-linked-to-ZDC* or *newly-linked-to-ZDC*, nor whether control units (concessions linked to no ZDC in the *post* period) are *never-linked-to-ZDC* or *previously-linked-to-ZDC*. In the following, we show that in this setting, the DiD estimator can be decomposed into three unobservable DiD estimators, corresponding to the DiD on *persistently-linked-to-ZDC*, on *newly-linked-to-ZDC*, and on *previously-linked-to-ZDC* concessions. We then show that under a "groupwise parallel trends" assumption, our DiD estimator identifies a weighted average of the causal effect on *persistently-linked-to-ZDC* and *newly-linked-to-ZDC* concessions. We also show how relaxing the groupwise parallel trends assumption towards an "adverse selection" assumption results in an upper bound on the causal effect on *persistently-linked-to-ZDC* and *newly-linked-to-ZDC* concessions.

To investigate this formally, we first introduce some notation to describe the four possible scenarios for a concession based on its linkage to the ZDC or non-ZDC supply chain during the *pre* and *post* periods. Concessions groups are denoted by the pairs YY, YN, NY, NN. The first letter (Y or N) indicates whether they were linked to a ZDC company during the *pre* period (Y for linked to ZDC, N for linked to non-ZDC), while the second letter represents their linkage to ZDC companies during the *post* period. The four groups of concessions are:

		Post period	
	ZDC:	Not linked to ZDC	Linked to ZDC
Pre period	Not linked to ZDC	**NN**: Never linked to ZDC	**NY**: Newly linked to ZDC
	Linked to ZDC	**YN**: Previously linked to ZDC	**YY**: Persistently linked to ZDC

- YY: **"Persistently linked to current ZDC companies"**: concessions that were linked both during the *pre* and *post* periods to companies that took ZDCs.
- NY: **"Newly linked to current ZDC companies"**: concessions that were not linked during the *pre* period, but were during the *post* period, to companies that took ZDCs.
- YN: **"Previously linked to current ZDC companies"**: concessions that were linked during the *pre* period, but not during the *post* period, to companies that took ZDCs.
- NN: **"Never linked to current ZDC companies"**: concessions that were never linked to companies that took ZDCs.

In our main analysis, we use what we call the **Post-supply-chain DiD (PSC-DiD)**, where we consider as treated/control the group of concessions that were linked/not-linked to ZDC companies during the *post* period. This means that we are considering as treated both *persistently-linked-to-ZDC* (YY) and *newly-linked-to-ZDC* (NY) concessions, and as control *previously-linked-to-ZDC* (YN) and *never* concessions (NN).

Definition 1 (Post-supply-chain DiD) *Let $DiD(\{A\};\{B\})$ denote the difference-in-differences estimator that uses units $\in \{A\}$ as treated, and units $\in \{B\}$ as control. We call* ***Post-supply-chain DiD****, and write $DiD^{PSC}(\{YY,NY\};\{NN,YN\})$, the DiD that uses post-treatment supply-chain status to define treated/control groups:*

$$DiD^{PSC}(\{YY,NY\};\{NN,YN\}) \equiv (\overline{Y}_{i\in\{YY,NY\},t\in post} - \overline{Y}_{i\in\{YY,NY\},t\in pre}) - (\overline{Y}_{i\in\{NN,YN\},t\in post} - \overline{Y}_{i\in\{NN,YN\},t\in pre})$$

Where Y is the outcome variable of interest (deforestation or forest cover) and $\overline{Y}$ denotes the sample average of Y.

We have the following decomposition result:

Theorem 1 (Decomposition of DiD^{PSC}) *The DiD^{PSC} estimates a combination of three unobservable DiDs:*

$$\begin{aligned} DiD^{PSC} \equiv & DiD(\{YY,NY\};\{NN,YN\}) \\ = & \pi_{YY} DiD(\{YY\};\{NN\}) + \pi_{NY} DiD(\{NY\};\{NN\}) - \pi_{YN} DiD(\{YN\};\{NN\}) \end{aligned} \quad [3]$$

Where $\pi_{YY} \equiv \frac{N_{YY}}{N_{YY}+N_{NY}}$ is the share of YY units among the units considered as treated ($\{YY,NY\}$). Likewise, $\pi_{NY} \equiv N_{NY}/(N_{YY}+N_{NY})$, and $\pi_{YN} \equiv N_{YN}/(N_{NN}+N_{YN})$.

Moreover, DiD^{PSC} can be decomposed into the following causal parameters:

$$\begin{aligned} DiD^{PSC} = & \pi_{YY} ATT(\{YY\}) + \pi_{NY} ATT(\{NY\}) \\ & + \pi_{YY}\Delta_0(\{YY\};\{NN\}) + \pi_{NY}\Delta_0(\{NY\};\{NN\}) - \pi_{YN}\Delta_0(\{YN\};\{NN\}) \end{aligned} \quad [4]$$

Where:

- *$ATT(\{A\})$ represents the average treatment effect on the treated group $\{A\}$.*
- *$\Delta_0(\{A\};\{B\})$ denotes the "deviation from trends absent treatment", i.e. the difference in trends between group $\{A\}$ and group $\{B\}$ assuming that group $\{A\}$ is not treated during the* post *period, i.e. $\Delta_0(\{A\};\{B\}) \equiv \mathbb{E}_{i\in\{A\}}(Y(0)_{t\in post} - Y(0)_{t\in pre}) - \mathbb{E}_{i\in\{B\}}(Y(0)_{t\in post} - Y(0)_{t\in pre})$, where $Y(0)_{i,t}$ represents the potential outcome of unit i at time t if it is not treated.*

Equation [3] shows that the post-supply-chain DiD^{PSC} estimates a combination of three different unobservable DiDs:

- The DiD of **"persistently-linked-to-ZDC"** (YY) versus **"never-linked-to-ZDC"** (NN) concessions, $DiD(\{YY\};\{NN\})$
- The DiD of **"newly-linked-to-ZDC"** (NY) versus **"never-linked-to-ZDC"** (NN) concessions, $DiD(\{NY\};\{NN\})$
- The DiD of **"previously-linked-to-ZDC"** (YN) versus **"never-linked-to-ZDC"** (NN) concessions, $DiD(\{YN\};\{NN\})$.

Equation [4] shows how the three underlying DiD can be written in terms of causal effects ATT and deviations from trends Δ_0. Importantly, there is not causal effect attached to the last DiD on *previously-linked-to-ZDC* ($DiD(\{YN\};\{NN\})$) since there is no treated unit among *previously-linked-to-ZDC* concessions (units were either linked to ZDC but before treatment, or not linked to ZDC after treatment). We show here two different sets of assumptions under which the PSC-DiD has a clear causal interpretation.

Assumption 1 (Groupwise parallel trends) *Assume that every subgroup* $\{YY, NY, NY\}$ *is parallel with respect to the* never-linked-to-ZDC *(*$\{NN\}$*) subgroup absent treatment. That is:* $\Delta_0(\{YY\};\{NN\}) = \Delta_0(\{NY\};\{NN\}) = \Delta_0(\{YN\};\{NN\}) = 0$*. Then:*

$$DiD^{PSC} = \pi_{YY} ATT(\{YY\}) + \pi_{NY} ATT(\{NY\})$$

Assumption 2 (Selection on trends) *Assume that the ZDC companies selectively kept or included those concessions that were anyway doing better than the reference group* $\{NN\}$*, and selectively discarded those concessions that were doing worse than the reference group* $\{NN\}$*. That is:* $\Delta_0(\{YY\};\{NN\}) \geq 0$*,* $\Delta_0(\{NY\};\{NN\}) \geq 0$ *and* $\Delta_0(\{YN\};\{NN\}) \leq 0$*. Then:*

$$DiD^{PSC} \geq \pi_{YY} ATT(\{YY\}) + \pi_{NY} ATT(\{NY\})$$

Under the "Groupwise parallel trends" assumption 1, every subgroup is assumed to be parallel, absent treatment, to the "never-linked-to-ZDC" subgroup (NN), i.e. $\Delta_0(A, \{NN\}) = 0 \; \forall A \in \{YY, NY, YN\}$. Under this condition, the post-supply-chain DiD^{PSC} identifies a composite effect $\pi_{YY} ATT(\{YY\}) + \pi_{NY} ATT(\{NY\})$. This is a well-defined parameter that is a weighted mean of the average treatment effect on the treated (ATT) of the "persistently-linked-to-ZDC" (YY) and "newly-linked-to-ZDC" (NY) groups, where the weights π_{YY} and π_{NY} represent the share of each subgroup, with $\pi_{YY} + \pi_{NY} = 1$. Under the ownership-based ZDC linkages, where ownership changes slowly over time and therefore π_{NY} is likely small, DiD^{PSC} likely identifies a parameter close to the ATT for the "persistently-linked-to-ZDC" (YY). On the other hand, for the sourcing-based ZDC linkages, where changes happen more often, DiD^{PSC} likely identifies an effect representative of the ATT on both the "persistently-linked-to-ZDC" (YY) and the "newly-linked-to-ZDC" (YN).
The "Groupwise parallel trends" assumption 1, is stronger than the usual parallel trends assumption as it requires not only that the whole *treated* group be parallel to the *control* group, absent treatment, but that this holds for (unobserved) subgroups. It also implies that "persistently-linked-to-ZDC", "newly-linked-to-ZDC" and "previously-linked-to-ZDC" would also be parallel to each other, absent treatment. This basically assumes that ZDC companies do not selectively choose concessions based on differential trends.
Assumption 2 shows that the "Groupwise parallel trends" can be relaxed by allowing selection into the treatment. We assume here an *adverse selection* scenario under which companies select the concessions they own/source from in order to clean their supply-chain, that is : 1) concessions that would be doing better than the *never-linked-to-ZDC* are either included (NY) or kept (YY) into the ZDC supply-chain, 2) ZDC concessions that would be doing worse than the *never-linked-to-ZDC* are excluded from ZDC (YN). Under this scenario, assumption 2 shows that the DiD^{PSC} provides an upper bound on the composite effect. To illustrate it, assume that the outcome variable is forest cover. Adverse selection implies that concessions that were conserving more forest become ZDC, $\Delta_0(\{NY\};\{NN\}) > 0$, and that those that were conserving less become non-ZDC, $\Delta_0(\{YN\};\{NN\}) < 0$. Then we have $DiD^{PSC} \geq \pi_{YY} ATT(\{YY\}) + \pi_{NY} ATT(\{NY\})$. Conversely, when the outcome variable is deforestation, we have $\Delta_0(\{NY\};\{NN\}) < 0$ and $\Delta_0(\{YN\};\{NN\}) > 0$. This implies that $DiD^{PSC} \leq \pi_{YY} ATT(\{YY\}) + \pi_{NY} ATT(\{NY\})$, and thus that the DiD is over-stating deforestation reductions.

A further complication arises from the fact that the treatment status is not observed during the "partial ZDC implementation" period. The same reasoning as above can be applied, using now three letters to represent the pre-, partial-, and full-implementation statuses. For simplicity, suppose there are only four groups: YYY, YYN, NNN, and NNY. The notation follows the same logic as before: NNY, for instance, refers to a concession that was not linked during the *pre* and *partial* periods but became linked to ZDCs in the *full* period. In that case, Equation (3) can be rewritten as following under the groupwise parallel trend assumption: $DiD(\{YYY, NNY\};\{NNN, YYN\}) = \pi_{YYY} ATT(\{YYY\}) + \pi_{NNY} ATT(\{NNY\}) - \pi_{YYN} ATT(\{YYN\})$. Unlike the previous case, where $DiD(\{YN\}, \{NN\})$ was zero due to absence of any treatment for YN units, the term $DiD(\{YYN\}, \{NNN\})$ now remains in the equation due to the presence of partially-treated units YYN. As a consequence, $DiD(\{YYY, NNY\};\{NNN, YYN\})$ will be larger than the parameters of interest, the effect of ZDC treatment during the *full* implementation period, $\pi_{YYY} ATT(\{YYY\}) + \pi_{NNY} ATT(\{NNY\})$.

Proof of Theorem (1):
Let us denote by $\Delta(\{A\})$ the time difference of average post-pre outcomes Y for group $\{A\}$:

$$\Delta(\{A\}) \equiv (\overline{Y}_{i \in \{A\}, t \in post} - \overline{Y}_{i \in \{A\}, t \in pre})$$

Note that, if group A consists into two subgroups, A' and A'', then:

$$\Delta(\{A', A''\}) = \pi_{A'}\Delta(\{A'\}) + \pi_{A''}\Delta(\{A''\})$$

where $\pi_{A'}$ is the share of A' among A' and A'', i.e. $\pi_{A'} = N_{A'}/(N_{A'} + N_{A''})$. This comes from the simple fact that the mean of a group is equal to the weighted mean of the subgroups, i.e. $\overline{Y}_{i\in\{A',A''\}} = \pi_{A'}\overline{Y}_{i\in\{A'\}} + \pi_{A''}\overline{Y}_{i\in\{A''\}}$.

$$\begin{aligned}
DiD^{PSC}(\{YY, NY\}; \{NN, YN\}) \equiv& \Delta(\{YY, NY\}) - \Delta(\{NN, YN\}) \\
=& \pi_{YY}\Delta(YY) + \pi_{NY}\Delta(NY) - \pi_{YN}\Delta(YN) - \pi_{NN}\Delta(NN) \\
=& \pi_{YY}\left(\Delta(YY) - \Delta(NN)\right) + \pi_{NY}\left(\Delta(NY) - \Delta(NN)\right) - \pi_{YN}\left(\Delta(YN) - \Delta(NN)\right) \\
& + (\pi_{YY} + \pi_{NY} - \pi_{NN} - \pi_{YN})\Delta(NN) \\
=& \pi_{YY}DiD(\{YY\}; \{NN\}) + \pi_{NY}DiD(\{NY\}; \{NN\}) - \pi_{YN}DiD(\{YN\}; \{NN\})
\end{aligned}$$

SI C. Supplementary details on data assembly.

SI C.1. Data selection rules. Concessions were selected based on the following sequential criteria for the econometric analysis:

1. We only kept concessions that were in the (groups of) islands of Kalimantan, Papua, Sulawesi and Sumatra, excluding therefore concessions in Java (126) and Maluku (20). For Java, this was motivated by the fact that concessions in Java are likely to be relatively old with little remaining forests. For Maluku, this was motivated by the fact that there are very few concessions and furthermore only a single mill on that group of islands, so that there would not be any variation in the ZDC status of the concessions.

2. We then only kept concessions that had some initial forest cover in 2000, given that no further deforestation is possible on these concessions and that annualized deforestation rates are not well defined in this case (division by zero).

3. We then only kept concessions that had experienced some oil-palm driven deforestation according to Gaveau et al. (2022). This was motivated by the fact that concessions where no deforestation occurred from 2001 to 2020 might not represent actual concessions. We test the sensitivity of our results to this decision in Table SI.T11.

4. We then only kept concessions that were matched to a mill and hence received a status of *no ZDC* or *ZDC*, using the *Unmatched* and *Linked to unreported* as control groups in robustness tests (see Table SI.T3). The main reasons to focus on the *no ZDC* as control group in the main analysis were due to the increased confidence in the ZDC status of this group (given that *Unmatched* and *Linked to unreported* cannot be given a ZDC score due to absent linkage information) as well as the closer behavior of this group compared to the treatment group (see Table SI.T1 and Figure SI.F4).

Table SI.T18 indicates the number of units removed (*N remove*) at each step. Note that this number is highly dependent on the ordering of the sequential rules, which is mainly arbitrary. For the final econometric analysis, we kept the concessions that were given a ZDC status of *ZDC* or *no-ZDC*, discarding those that had either a status of *Unmatched* or *Linked to unreported*. We did this either based on the ownership or sourcing linkages.

Table SI.T18. Selection rules for concessions.

Rule type	Selection Rule	N Initial	N Remove	N Final
Sequential rule	Is concession in a main island?	2644	146	2498
	Does concession have initial forest >0?	2498	178	2320
	Does concession contain industrial palm oil?	2320	992	1328
	Is concession matched to a mill?	1328	584	744
Alternative rule	Is parent mill matched to known company (owner linkages)?	744	252	492
	Is parent mill matched to known company (buyer linkages)?	744	514	230

The table indicates the number of concessions before the selection rule is applied (*N initial*), the number of units affected by the selection rule (*N Remove*), and the remaining number of units once the rule has been applied (*N Final*).

SI C.2. Clustering procedure. Our goal here is to categorize concessions according to their stage of frontier development, defined by the temporal evolution in terms of initial forest cover and subsequent deforestation during the pre-ZDC period. To cluster the concessions according to their frontier stage, we applied a k-means algorithm using forest cover in 2000 and 2010 as clustering variables. Both variables were scaled to have unit variance. After careful graphical examination, we opted to keep three clusters. This decision was made to facilitate interpretation and to ensure that each cluster contained a sufficient number of observations. Table SI.T19 shows the means for each cluster. We interpreted the three clusters as following:

1. "Old frontier, inactive by 2010", noting that the forest cover is low both in 2000 and 2010.

Table SI.T20. Details on the components retained for assessing corporate ZDC quality and criteria adapted from SPOTT indicators

Principle categories	Included SPOTT criteria
Conservation commitment	
ZDCs should be stringent with clear targets, geographical scope, and commitment on conserving remaining forest cover, preventing forest conversion, and restoration commitments.	1. Conservation commitment includes a pledge on deforestation- and forest conversion-free supply chain 2. Commitment to not planting on peatland.
Traceability commitment	
ZDCs should have a time-bound commitment to achieve traceability to both mill and plantation levels for their own and supplier's plantations to enhance transparency and prevent spillover.	1. Timebound commitment to achieve full traceability to mill 2. Timebound commitment to achieve full traceability to plantation 3. *Traceability implementation to mills* 4. *Traceability implementation, from own mills to plantation.*
Smallholder inclusion	
ZDC should provide program aimed to support smallholders, for example via capacity building, that are differentiated for different types of smallholders with a clear informed target.	1. Design program to support scheme/plasma smallholders 2. Design program to support independent smallholders 3. *Participation of independent or plasma smallholder involvement in the programme*
Compliance commitment	
ZDCs should provide clear and functional assessment and engagement mechanisms.	1. Program to support high-risk mills (at high risk of contributing to deforestation) to become compliant. 2. *Regularly engages with high-risk mills*
Monitoring commitment	
ZDCs should provide reliable and frequent monitoring system.	1. Available and open grievance or complaint system 2. *Self-reported evidence of monitoring deforestation*
Transparency	
ZDCs should allow for transparency in their enforcement approach.	1. Disclosing the details of complaints and grievances, which includes the action and enforcement status.

Criteria in *italics* pertain to the progress of commitment implementation by companies, which will be included in analyzing the quality of companies' ZDC.

2. "New frontier, active in 2010", noting that there is a relatively high forest cover in 2000 and a large decline in forest cover by 2010.

3. "New frontier, inactive in 2010", noting that there are high levels of initial forest cover in 2000 and a small decline by 2010.

Table SI.T19. Cluster means.

ZDC agegation method	Cluster number	ZDC score	N units	Forest cover 2000	Forest cover 2010	2000-2010 forest cover change
ZDC owner attribution	1	No ZDC	91	11.9	4.7	7.2
	1	ZDC	201	9.8	4.1	5.7
	2	No ZDC	53	48.3	17.8	30.4
	2	ZDC	97	47.7	18.8	29.0
	3	No ZDC	20	69.0	59.3	9.7
	3	ZDC	30	75.9	69.1	6.9
ZDC buyer attribution	1	No ZDC	18	9.0	2.9	6.1
	1	ZDC	125	7.9	2.8	5.1
	2	No ZDC	7	32.4	15.0	17.4
	2	ZDC	65	43.3	13.9	29.3
	3	No ZDC	2	56.0	49.6	6.4
	3	ZDC	13	69.9	53.1	16.8

The table indicates various statistics for each cluster as defined above. *N units* indicates the number of concessions in each cluster, *Forest cover 2000* and *2010* indicate the mean forest cover (in %, with respect to the concession total area) in each cluster, whereas *2000-2010 forest cover change* indicates the difference in forest cover between 2000 and 2010.