# Price incentives and unregulated deforestation: Evidence from Indonesian palm oil mills *

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**Abstract**

Global demand shifts and supply chain interventions have the potential to reduce palm oil's environmental footprint, especially in otherwise unregulated plantations. This ultimately depends on deforestation reacting to prices in upstream, complex plantation-mill systems. We produce the first microeconomic panel of geolocalized palm oil mills, and we model their influence on palm plantations across Indonesia where the issue is most critical. We leverage our data granularity and the nature of the value chain to isolate downstream mill-gate price shocks that are exogenous to deforestation upstream. We find a positive elasticity to the mill-gate price of crude palm oil, in general and in two specific cases of unregulated deforestation — for smallholder plantations and for illegal industrial plantations. However, smallholder deforestation decelerates as palm fruit prices increase. These results inform the design of fair and effective conservation policy.

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# 1 Introduction

In this century's first decade, the conversion of Indonesian forests to oil palm plantations has released 1% to 4% of anthropogenic greenhouse gas emissions (Busch et al. 2015). Consequences in other environmental and socio-economic dimensions are substantial too (Qaim et al. 2020). Oil palm plantation expansion and associated deforestation rates have declined since 2013, coincidentally with lower palm oil prices (Gaveau et al. 2022) and with new conservation interventions. The latter — a moratorium on permitted forest conversion, and private conservation initiatives — have not proven highly effective because they do not reach smallholders and illegal industrial plantations, both de facto unregulated by weak institutions (Heilmayr et al. 2020; Drost et al. 2021; Groom et al. 2022). On the other hand, price incentives are identified as a major driver of agricultural deforestation in the tropics (Busch and Ferretti-Gallon 2017; Berman et al. 2023). Ramping up palm oil prices thus threaten to trigger a new conversion crisis in Indonesia, driven by unregulated deforestation. Meanwhile, theoretically effective ways to address unregulated deforestation have been proposed, using price instruments (Heine et al. 2020). Hence, the extents to which prices represent both a threat and a potential solution depend on whether prices effectively incentivize unregulated deforestation.

In this paper, we investigate whether and how price signals cause deforestation in the complex landscapes upstream the Indonesian palm oil supply chains. We primarily seek to identify whether crude palm oil price signals affect deforestation, for all plantations and for specific unregulated ones — namely illegal industrial plantations and independent smallholder plantations. We conceptualize heterogeneous plantation agents to hypothesize the mechanisms underlying the deforestation price elasticity. We test these by estimating elasticities to prices at different stages upstream the palm oil supply chain.

Previous national-scale studies have estimated a relationship between international palm oil prices and deforestation for all kinds of oil palm plantations together (Wheeler et al. 2013; Busch et al. 2015; Cisneros et al. 2021; Gaveau et al. 2022; Busch et al. 2022; Hsiao 2022). Wheeler et al. (2013) first established a positive correlation between time series of palm oil futures prices and forest loss alerts at a monthly rate. Gaveau et al. (2022) found a similar relationship at annual rates, although less precise in the distinct case of smallholder plantations. Subsequent studies advanced identification of the causal price effect by adding spatial variation. They proxied local incentives by interacting international prices with agro-climatic measures of local suitability for palm plantations (Busch et al. 2015; Cisneros et al. 2021; Hsiao 2022). Although powerful, this approach is subject to three causal inference challenges: reverse causality due to deforestation in most suitable areas affecting the international price; correlation patterns with other crops in terms of suitability and prices; and systematic measurement errors if deforestation rates correlate with information on suitability or international price pass-throughs. While these studies addressed these challenges to some extent, they did not focus primarily on the link between palm oil price and deforestation and thus, they did not provide in depth analyses of this relationship. In particular, they did not study smallholder or illegal industrial deforestation distinctly, letting unknown whether this could overtake regulatory conservation interventions as prices rise again freely.

We produce new country-wide data that allows us to study in new depth the influence of price incentives on deforestation, notably unregulated, in the upstream stages of the supply chain. Data on the Indonesian palm oil supply chain available to researchers is limited. Previously, only the location and total capacity of most palm oil mills was known, together with the establishment date for a subset of them. For about half of all known Indonesian palm oil mills, we extend the geo-localized information to the full set of attributes in the Indonesian manufacturing census (IBS).[1] We notably observe annual mill-level input (fresh fruit bunches) and output (crude palm oil) volumes and monetary values, as well as public, private and foreign ownership shares, and export shares. We use data based on satellite imagery to measure deforestation as 30 m-pixel events of primary forest loss eventually converted to oil palm plantations. These secondary data distinguish industrial from smallholder plantations based on scale and shape features that match economically and politically relevant differences. We further detect illegal deforestation by associating knowledge of the legal requirements with the best available data on palm concessions and on land use zoning. To measure price signals, we leverage the agronomic constraint on the distance between fresh fruit bunches harvest, in plantations, and processing into crude palm oil, in mills. For every plantation site that can reach at least one mill, we average the crude palm oil prices received at reachable mills. By assigning higher weights to closer mills, we model the relative influences of reachable mills in a way that is consistent with unobserved vertical integration and transport cost structures. By averaging annual price signals over the four last years, we capture the information that we assume relevant and avail-

[1] In the economics literature, this dataset has also been referred to as *Statistik Industri*; see, for instance, Amiti and Konings (2007).

able to prospective plantation agents forming expectations of the profitability of the perennial oil palm crop.

Our identification strategy relates to a reduced-form shift-share instrument where exogenous variation is in the shifts. Here, the shifts are the yearly departures of mill-gate crude palm oil prices from local markets. Such departures occur because the off-take agreements between mills and their buyers follow idiosyncratic schedules within a year. Over the year, mills thus receive a price that aggregates different combinations of intra-annual downstream and macroeconomic pricing dynamics. Since deals' schedule and pricing conditions are imposed on mills by the oligopoly of buyers (international traders and refineries), the variation in mill-gate prices that stem from their combinations are exogenous to upstream deforestation. We document the concentration of buyers (Pirard et al. 2020), their power to set the purchasing conditions (Purnomo et al. 2018; Wiggs et al. 2020) and the frequency of typical intra-annual off-take agreements (Wiggs et al. 2020; EBRD 2024). We argue that reverse causality is not a threat in this context, because of the concomitant lack of mills' bargaining power and necessity to turn upstream productivity gains into more competitive prices during the study period (Byerlee et al. 2016; Pirard et al. 2020). We corroborate this empirically by showing the robustness of the results to controlling for past deforestation; and with a falsification test finding no correlation between mill input volumes and output prices. The granularity in our primary data allows us to isolate the exogenous variation in prices from endogenous macro and local dynamics by the means of contextually highly resolved fixed effects. Specifically, we allow for price formation to be endogenous with the political economy in a district-year and with the level of development of the local market for fresh fruit bunches.

This paper makes three contributions with respect to the aforementioned studies of price incentives to deforest in the Indonesian palm oil context. First, this paper estimates a comparable parameter with internal validity grounding on an alternative and no less robust method, and with external validity stemming from country-wide, 14-year representative data. Second, our unique mill-gate price signal data allows to attribute this parameter to the pivotal plantation-mill segments of the supply chain and to narrow down the analysis to policy-relevant types of deforestation – namely illegal deforestation for either industrial or smallholder plantations. Third, we further analyze supply chain actors' interests in and agency on deforestation for smallholder plantations, and we document the decision rules of deforesting agents.

Our main results are threefold. First, we find that a 1% increase in crude palm oil price signals increases the average conversion of primary forest to oil palms by 1.5%. This estimate is robust in a range of alternative settings, including mill catchment radius assumptions, price signal dynamics, control sets, fixed effect and standard error clustering levels. Second, looking at unregulated deforestation, we find that deforestation for smallholder plantations, and for illegal industrial plantations, are price responsive. In contrast, we find no evidence that deforestation for legal industrial plantations be price elastic. Thirdly, while deforestation for smallholder plantations increases with crude palm oil price signals, it decreases with fresh fruit bunches price signals. We keep finding such negative elasticity after controlling for an eviction effect from industrial plantations.

Together, these results have several implications. First, deforestation left unregulated will follow prices. Our results suggest that last decade's price contraction helped deforestation to slow down. The recent surge in prices could hence push unregulated deforestation to overtake the conservation effects of regulations and corporate initiatives that tackle only the most institutionalized parts of the supply chain. We estimate that a positive standard deviation in price signals leads 10 thousand hectares of primary forest to be converted the next year. Our results can feed into more dedicated efforts to model deforestation impacts of e.g. trade or biofuel policies. Notably, our 1.5 crude palm oil price elasticity estimate is one order of magnitude higher than that used by Busch et al. (2022) to conclude that export bans have little effectiveness in curbing deforestation.

However, such consumer-side restrictions — like, most recently, the European Union Deforestation Regulation (EUDR) — face coordination (Hsiao 2022; Bastos Lima et al. 2024) and traceability challenges (Lyons-White and Knight 2018). Hence, price interventions at upstream choke points, like mills, represent promising complementary policies, especially to steer hardly regulated deforestation (Heine et al. 2020). Our finding that deforestation for smallholder and for illegal plantations respond to prices implies that such interventions could work. This matters because the relative footprint of smallholder expansion is predicted to grow (Schoneveld et al. 2019) and illegal deforestation is documented to prevail in active frontiers.[2] Our back-of-the-envelope estimation indicates that a 19% tax levied uniformly on palm oil mills could curb deforestation 29% under average and earn Indonesia about USD 120 million a year for avoided emissions (at a USD 5/ton $CO_2$ price). Refunding against proof of sustainable production would further improve effectiveness, at low monitoring costs (Heine et al. 2020).

[2] Notably in the island of Papua: https://news.mongabay.com/2018/11/the-secret-deal-to-destroy-paradise/

Finally, our results indicate that protecting smallholder revenues further increases the effectiveness of a conservatory tax on crude palm oil, as smallholders otherwise compensate depressed prices by expanding plantations into the forest. The revealed agency of mills on smallholder expansion is informative to supply chain management for conservation.

The results and their implications relate this paper to the literature on the economic drivers of land use change[3] and smallholder agriculture in particular (Krishna et al. 2017; Dalheimer et al. 2022); and to the literature on the interdependent roles of regulatory and incentive-based tropical supply chain governance (Godar et al. 2014; Busch et al. 2015; Harding et al. 2021; Lambin and Furumo 2023). More broadly, we connect to the literature on the detrimental effects of commodity prices, especially in weak institution contexts (van der Ploeg 2011; Blair et al. 2021; Gehring et al. 2023).

Next, we provide background and a conceptual framework in Section 2; data and measurements in Section 3; estimation and identification strategies in Section 4; results in Section 5; and concluding discussions, including of the policy implications, in Section 6.

## 2 Background and conceptual framework

### 2.1 Background: unregulated upstream plantations and pivotal mills in the palm oil supply chain

**The Indonesian palm oil supply chain.** The global demand for vegetable oil has led to an increase in the cultivation of oil palm, the most productive oil plant (Corley and Tinker 2015). In Indonesia, where around 50 Mha of land are suitable for oil palms (Austin et al. 2017), a complex supply chain has developed to currently account for more than half of the global production. Across the country, around 15 Mha of oil palm tree plantations provide fresh fruit bunches (FFB). The majority of the planted area and production comes from large, grid-shaped landscapes, ranging from a hundred hectares to hundreds of thousands of hectares, called industrial plantations (Gaveau et al. 2016; Austin et al. 2017). FFB is processed locally into crude palm oil (CPO) by nearly 1100 mills, owned by 178 corporate groups (Pirard et al. 2020). Foreign and domestic companies, as well as local governments invest in mills and industrial plantations, and operate their development. Some industrial plantations are vertically integrated with mills, but this

[3]We point in particular to Busch and Ferretti-Gallon (2017) for a review; Leblois et al. (2017) and Berman et al. (2023) for global analyses; Souza Rodrigues (2019) in the Amazon context.

seems limited (Pirard et al. 2020).[4] At the next stage, between 55 and 80 corporate groups take custody of mills' CPO in one of 61 ports, to export it as such or to refine it in one of 400 refineries. At this stage, just three corporate groups buy more than half of the CPO (Pirard et al. 2020). Crude palm oil is a standardized, fungible commodity that, unlike fresh fruit bunches, can be transported across long distance without degrading (Byerlee et al. 2016). Part of the milling and refining capacity is vertically integrated, but this is not the dominant model (Pirard et al. 2020). Refined palm oil is sold to a myriad of manufacturers for further processing into final products including food, biodiesel and cosmetics. The palm oil supply chain is hence hourglass shaped and characterized by limited vertical integration and an opaque ownership structure (Glenday et al. 2015; Pirard et al. 2020). This hampers the traceability of physical and financial flows with upstream plantations, thus hindering supply chain governance from downstream interventions (Lyons-White and Knight 2018; Pirard et al. 2020; zu Ermgassen et al. 2022). It also gives actors at the most concentrated part of the supply chain, the traders and refiners that buy CPO from mills, the power to steer business conditions (Purnomo et al. 2018; Wiggs et al. 2020; EBRD 2024).

**The unregulated oil palm plantations.** Governing oil palm plantations through upstream, place-based interventions is challenging as well in Indonesia, in particular because of the prevalence of de facto unregulated plantations (Heilmayr et al. 2020; Drost et al. 2021; Groom et al. 2022). These plantations are unaffected by weakly enforced voluntary or compulsory institutions, and they are in two main kinds: independent smallholder and illegal industrial plantations.

Independent smallholder plantations can be defined in opposition to large-scale industrial plantations managed by a company and to smallholder plantations developed contractually with a company in the industrial plantation (in a "plasma" scheme). This distinction has three implications. First, independent smallholder plantations are recognizable by their relatively small sizes and irregular shapes. Second, they tend to be de facto unregulated, as they rarely hold the required permit (Craw 2019).[5] Moreover, as the permit required below 25 ha exempts

[4]This is also suggested, in particular until 2013, by Agriculture Regulation No. 98/2013 which stipulates that plantations larger than 1000 hectares must integrate with mill operations, and that mills must source at least 20% FFB from their own plantations (Glenday et al. 2015).

[5]Plantations below 25 ha require a Plantation Registration Certificate (STD-B), and a Plantation Business License (IUP-B) above 25 ha.

them from most legal processes (Paoli et al. 2013; Jelsma et al. 2017), we consider all independent smallholders as de facto unregulated, rather than distinguishing between legal and illegal ones. Third, they are not bound to a single mill exclusively, but rather sell their fruits on the spot to different mills through intermediaries called 'middle-men' (Cramb and McCarthy 2016; Baudoin et al. 2017). However, independent smallholders are not completely independent regarding the location and timing of their expansion, which partially depends on more powerful actors of the local political economy like the companies operating mills and industrial plantations. These companies have the potential to set enabling conditions for the expansion of independent smallholders via infrastructure deployment, support to the palm oil economy (e.g. providing loans, seedlings or extension services) and their political influence — notably in the land bargaining process that has been especially acute in the context of transmigration schemes (McCarthy et al. 2012b,a; Potter 2012; Paoli et al. 2013; Li 2015; Euler et al. 2016b; Jelsma et al. 2017). Independent smallholders have driven the smallholder expansion since the early 2000's (Byerlee et al. 2016; Euler et al. 2016b), until accounting for more than a third of the total oil palm acreage (Gaveau et al. 2022) and a tenth of oil palm deforestation (Lee et al. 2014a). In this paper we focus on these independent smallholders and simply call them 'smallholder plantations'.

Illegal industrial plantations are developed in forested areas that are not legally designated for oil palm cultivation, and/or without all the required authorizations (see Paoli et al. (2013) for a comprehensive regulatory overview). The complexity of land institutions implies that illegality can take multiple forms. It can be summarized as a problem of weak central law enforcement, which is commonly attributed to the decentralization process that started in 2001.[6]

**The pivotal situation of palm oil mills.** Governing the expansion of oil palm plantations on forest is important, because it has significant adverse consequences on biodiversity, climate change and forest-reliant livelihoods in particular (Petrenko et al. 2016). However, the hourglass-shaped supply chain and unregulated expansion hamper the effectiveness of upstream and downstream interventions. In this perspective, mills have a pivotal position in the palm oil supply chain.

They are sufficiently upstream to have the ability to trace sourcing to the plantation and they

[6] Mechanisms include corruption being fostered by competition for resource rents between local authorities (Burgess et al. 2012; McCarthy et al. 2012b), and law bypassing being facilitated by ambiguous and overlapping authority between institutions (Setiawan et al. 2016).

can have enough local power to influence expansion decisions for their own plantations and for independent smallholders (Purnomo et al. 2018). Moreover, they have bargaining power over FFB purchases, thanks to their locally oligopsonic positions (Maryadi et al. 2004; Masliani et al. 2014). This reportedly enables mills to apply discretionary pricing, especially on independent smallholders and their intermediaries (Jelsma et al. 2017; Alamsyah et al. 2021), in spite of regulations[7] providing that FFB prices be set as a monthly agreement between representative stakeholders at provincial level, according to CPO prices and different cost components (Mawardati (2018) and Bachtiar et al. (2020) and Appendix C.3.1).

On the other hand, mills are sufficiently downstream to be governable (Purnomo et al. 2018). Hence, palm oil mills represent a strategic choke point in the supply chain, from where it is possible to steer the expansion of plantations, and especially of unregulated ones, with a price instrument (Heine et al. 2020). However, the effect of any such intervention — and of any downstream shock or demand shift more generally — depends on whether, upstream the supply chain, forest conversions to the different kinds of plantations react to crude palm oil prices on mills' downstream market. In light of the context outlined above, we now clarify how we conceptualize this reaction.

### 2.2 Conceptual framework

A standard conceptual framework posits that a deforestation event results from the decision of an optimizing micro-economic agent. The typical decision rule is the comparison of the expected discounted present utilities (or profits) from alternative inter-temporal scenarios, defined by the kind, the timing and the amount of deforestation.[8] To form such expectations, every agent grounds on privately observed informational elements (Stavins 1999). Here, we are interested in those of such elements that are prices, and we call them price signals. Price signals can inform about future prices and profits, and can also reflect accrued revenues available for new investments. Hence, the price elasticity of deforestation essentially refers to the observation of a price signal and the act of deforesting accordingly, by what we call a plantation agent.

[7] During the study period, these are Permentan No. 627 of 1998, Permentan No. 395 of 2005, Permentan No. 17 of 2010 and Permentan No. 14 of 2013.

[8] The counterfactual scenario includes both conservation and deforestation to other land uses. Conservation includes both expansion outside forest or no expansion (i.e., intensification or not entering the fresh fruit bunches market). We do not distinguish between these alternative scenarios in our analysis.

In the present context, plantation agents are not homogeneous in their decision rules. Industrial plantation agents owning large plantations and/or milling capacity are sufficiently powerful in the local political economy to influence the timing and location of smallholder plantations, even if they do not own them. Moreover, plantation agents who own milling capacities are more attentive to price signals for crude palm oil, while others consider primarily the price signals for fresh fruit bunches. Hence, we conceptualize two types of plantations agents to reflect this essentially heterogeneous context. The first type of plantation agents, $A$, has only agency on its own expansion and expect to sell only FFB. This typically corresponds to smallholder plantations. The second type, $B$, corresponding to industrial plantations, has agency on the expansion of smallholder plantations they do not own, and can sell FFB or CPO depending whether they own milling capacity. Given the supply chain structure, we assume that mills can have market power in their local FFB markets, but not in the globalized CPO market downstream, such that local price shocks are passed-through upwards only. Hence, focusing on CPO, which market is most exposed to national and international policy and demand shocks, we decompose the CPO price elasticity of deforestation for the plantations of agent type $j$ ($\alpha^{j}_{CPO}$), into theoretical mechanisms as follows.

$$\alpha^{j}_{CPO} = \mathbb{1}[j = A][\varepsilon^{FFB}_{CPO} \times (\iota^{A}_{FFB} + \phi^{B}_{FFB}) + \phi^{B}_{CPO}] \quad + \quad \mathbb{1}[j = B][\varepsilon^{FFB}_{CPO} \times \iota^{B}_{FFB} + \iota^{C}_{CPO}] \tag{1}$$

$\varepsilon^{FFB}_{CPO}$ is the elasticity of FFB price to CPO price in the local FFB market, or price pass-through (common to all agents in this market). $\iota$ parameters represent plantation agents' own elasticities, i.e., price-driven decisions to deforest for their own plantations. These coefficients may be positive or negative, depending whether price signals encourage investments at the extensive or intensive margins respectively, which depends on the relative cost of land, capital and labor for each agent type. $\iota^{B}_{FFB}$ represents both the output price for industrial plantations without milling capacity, and an opportunity cost of not reselling FFB for industrial plantations with milling capacity. $\phi$ parameters represent the propensity of industrial plantation agents to deforest for smallholder plantations they do not own, in response to prices. We hypothesize that $\phi^{B}_{FFB}$ is negative, as industrial plantations compete for land with smallholder plantations, and tend to evict them when price signals are high, or release land when they are low. $\phi^{B}_{CPO}$ captures the influence of large-scale plantation and milling capacity owners over deforestation for smallholder plantations, in reaction to a CPO price change. It may be driven by an eviction mechanism similar to $\phi^B_{FFB}$, but it may also be driven by the need for a larger supply base when signals of future profit from CPO sales increase. Moreover, agents with milling capacity have interest in maximizing fruit production from smallholder plantations in their supply shed as a way to depress local FFB prices and access cheap adjustment supply. Thus, we do not hypothesize the sign of $\phi^B_{CPO}$.

# 3 Data sample and measurements

## 3.1 Data sample

To estimate these parameters, we sample data in an annual panel of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014, where and when most oil palm deforestation occurred (Austin et al. 2017). On average, deforestation in a grid cell and year is less than 1% (5.2 ha) of the cell area (Table 1). Yet, our analysis sample covers one fifth of all oil palm deforestation in the two islands (Table A.1).[9] We sample observations based on proximity to a mill and mill data availability (Appendix C.1 and Table A.2 for attrition implications). Figure B.1 shows the covered area (note that deforestation for industrial plantations can occur in the same grid cell and year as deforestation for smallholder plantations). Breaking down descriptive statistics by deforestation types, Table 1 shows illegal industrial deforestation operations in a grid cell and year are twice lower than legal ones on average. Regarding price signals, they are slightly higher for crude palm oil in times and areas of unregulated deforestation, notably for smallholder plantations (USD 706/ton CPO on average), and rather homogeneous for fresh fruit bunches (around USD 128/ton FFB). In terms of spatial coverage, it is notable that deforestation for smallholder plantations during the study period occurred mostly in Sumatra (Figure B.2). Annual deforestation rates followed generally similar patterns across plantations types, with important year-on-year changes (Figure B.3).

## 3.2 Measurements of deforestation for oil palm plantations

**Plantation sites.** We approximate the true (unknown) boundaries of the areas affected by the decisions of individual plantation agents as arbitrarily delineated *plantation sites*. We delineate plantation sites as $3 \times 3\,\mathrm{km}$ grid cells (900 ha), as in Busch et al. (2015), to balance the trade-

[9] And deforestation for oil palms represents one third of territorial deforestation (Gaveau et al. 2022).

Table 1: Estimation samples - descriptive statistics

| Deforestation for: | Industrial plantations | | | | | | Smallholder plantations | | Unregulated plantations | | All | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Legal | | Illegal | | All | | | | | | | |
| | # grid cells = 3431<br># grid cell-year = 20532 | | # grid cells = 3189<br># grid cell-year = 17091 | | # grid cells = 11782<br># grid cell-year = 65368 | | # grid cells = 3211<br># grid cell-year = 20721 | | # grid cells = 4266<br># grid cell-year = 24596 | | # grid cells = 12687<br># grid cell-year = 71926 | |
| | mean (SD) | med. [min;max] | mean (SD) | med. [min;max] | mean (SD) | med. [min;max] | mean (SD) | med. [min;max] | mean (SD) | med. [min;max] | mean (SD) | med. [min;max] |
| Deforestation (ha) | 6.8 (35.3) | 0 [0; 847.5] | 3.2 (24.2) | 0 [0; 763.2] | 4.4 (28.5) | 0 [0; 847.5] | 4.2 (21.1) | 0 [0; 653] | 5.1 (26.7) | 0 [0; 763.2] | 5.2 (29.6) | 0 [0; 847.5] |
| CPO price signal (USD/ton) | 663 (89) | 657 [395; 926] | 669 (95) | 665 [350; 921] | 668 (93) | 665 [350; 926] | 706 (87) | 722 [350; 905] | 685 (93) | 697 [350; 926] | 673 (92) | 672 [350; 926] |
| FFB price signal (USD/ton) | 129 (19) | 130 [59; 178] | 128 (20) | 129 [60; 175] | 128 (20) | 129 [59; 180] | 130 (20) | 134 [59; 179] | 129 (20) | 130 [60; 179] | 128 (20) | 129 [59; 180] |
| # reachable UML mills | 8.5 (5.9) | 7 [1; 37] | 7.1 (4.7) | 6 [1; 35] | 7.7 (5.2) | 7 [1; 37] | 8 (4.1) | 7 [1; 27] | 7.7 (4.7) | 7 [1; 35] | 7.8 (5.1) | 7 [1; 37] |
| Primary forest cover 2000 (ha) | 326.1 (303.7) | 229 [0.2; 903.2] | 527.9 (327.4) | 582.4 [0.2; 903.2] | 366.7 (324.5) | 271.9 [0.1; 903.2] | 332.7 (326.2) | 204.5 [0.2; 903.2] | 464.2 (336.8) | 443.7 [0.2; 903.2] | 359.2 (324.5) | 261.2 [0.1; 903.2] |
| Mill public ownership (%) | 13.3 (24.1) | 0 [0; 100] | 18.6 (29.1) | 0 [0; 100] | 14.8 (25.8) | 0 [0; 100] | 16.7 (26.2) | 0 [0; 100] | 17.2 (26.7) | 0 [0; 100] | 15 (25.4) | 0 [0; 100] |
| Mill domestic private ownership (%) | 68.6 (32.3) | 77.8 [0; 100] | 65.8 (33.1) | 72 [0; 100] | 68.1 (32.2) | 75.6 [0; 100] | 71.2 (28.9) | 76.2 [0; 100] | 68.4 (30.9) | 74.2 [0; 100] | 68.4 (31.6) | 75.3 [0; 100] |
| Mill foreign ownership (%) | 18.1 (27.6) | 0 [0; 100] | 15.6 (26.7) | 0 [0; 100] | 17.1 (26.8) | 0 [0; 100] | 12.2 (18.7) | 0 [0; 100] | 14.3 (24) | 0 [0; 100] | 16.7 (26.1) | 0 [0; 100] |
| Mill CPO exports (%) | 12.7 (22.6) | 0 [0; 100] | 13.4 (22.6) | 0 [0; 100] | 14.2 (23.5) | 0 [0; 100] | 21.2 (24.8) | 12 [0; 100] | 16.8 (23.7) | 3 [0; 100] | 14.8 (23.5) | 0 [0; 100] |

NOTE. This table shows descriptive statistics in the samples used to estimate the price elasticity of deforestation for different types of plantations. # means "number of". Price signals, mill ownership and mill CPO exports are calculated as inverse-distance weighted averages of these variables at reachable mills.

off between statistical power reduction and inflation. This approximation requires assuming that the agents of a given type ($A$ or $B$) operating in a plantation site are homogeneous.[10]

**Deforestation.** We overlay remote sensing maps of primary forest in 2000 (Margono et al. 2014),[11], annual forest loss (Hansen et al. 2013), and subsequent oil palm plantations (Petersen et al. 2016; Austin et al. 2017). In every plantation site and year, we measure *deforestation* as the count of the primary forest loss events occurring that year in the eventual plantation extent (Appendix C.2). This captures decisions by plantations agents to clear forest for palm oil, productively or speculatively, because where oil palm can be grown, it is the most lucrative land use and thus the prime motivation to clear forest (Byerlee et al. 2016).

**Plantation types.** We observe where oil palms are grown according to remote sensing. We use data from Austin et al. (2017) to detect industrial plantations as large rectangular grid landscapes. We use data from Petersen et al. (2016) to detect smallholders as small- and mid-sized plantation mosaic landscapes (palm tree patches up to 100 ha, comprising at least 50% of a mosaic landscape wider than 100 ha). The latter captures the independent smallholder plantations characterized above, as it excludes the "plasma" plantations, jointly developed near and alike industrial plantations wider than 250 ha (Paoli et al. 2013; Byerlee et al. 2016; Gaveau et al. 2022).

Observing illegality is generally challenging, and this is all the truer in the outer islands of Indonesia where the line between legality and illegality is blurred by weak institutions and complex rules (Paoli et al. 2013). We focus on deforestation for industrial plantations, where illegality can be more clearly defined than for smallholders and can be summarized by the absence

[10]This assumption is stronger for smallholder agents (type $A$), who can be more numerous in a 900 ha plantation site, but this is not critical because our main analyses do not compare smallholder and industrial plantations.

[11]This corresponds to the official forest definition by the Government of Indonesia (MoF 2008; Austin et al. 2017)

of the Business Use Right (HGU), or concession, which is contingent on the other legal steps. However, the available map of oil palm concessions is reportedly incomplete (Greenpeace 2011). Therefore, we focus on mitigating commission errors in illegal deforestation and assume that omission errors are conditionally independent from price elasticity (Appendix C.2). To mitigate commission errors, we consider plantation sites as illegal only if, in addition to being outside a known concession, they are inside a forest zone where oil palm is forbidden under the Indonesian legal land designation (*Indonesia legal classification* 2023). We define legal industrial plantations as being within a concession.[12]

### 3.3 Measurements of price signals: a model of plantation-mill relationships

To measure the fresh fruit bunches (FFB) and crude palm oil (CPO) price signals perceived by plantation agents, we observe mill-gate prices and model mills' differential influence through space and time in plantation sites.

**A primary microeconomic panel of geolocalized palm oil mills.** We produce an original input-output geo-referenced data set of Indonesian palm oil mills from 1998 to 2015. For that we merge the Universal Mill List (UML) and the Indonesian manufacturing census (IBS) by leveraging input-output variables and village identifiers usually not provided to researchers with the IBS (Appendix C.3.1). We geo-localize 466 IBS mills with their coordinates in the UML, and 121 additional ones, not matched with the UML, at their village centroids. The geo-localized mills are not statistically different from the larger set of mills in the IBS (Table A.3). With the IBS input-output variables, we observe the value and volume of all intra-annual mill-gate transactions, including fresh fruit bunches purchases and crude palm oil sales. We take the mean unitary value at mill-year-product level and call it the mill-gate price. The time series of the average mill-gate CPO prices follow that of the international CPO prices (Figure B.3).

**The set of reachable mills.** The quantity of oil derived from a ton of palm fruit increases with the quality of the fruits and decreases rapidly in transport from plantation to mill (Byerlee et al. 2016). Therefore, plantation agents can expect to sell only among mills surrounding the plantation site. For every plantation site and year, we determine the set of reachable mills

[12] The commission error implied by this loose definition of legality is not critical since we study legal deforestation mostly in comparison to illegal deforestation, which is the focus of this paper.

as being within an euclidean distance (catchment radius). We assume that mills beyond this distance have no influence.[13] Taking from the literature (Appendix C.3.1), our preferred catchment radius is $30\,\mathrm{km}$ in Sumatra and $50\,\mathrm{km}$ in Kalimantan. This balances a trade-off: a too short catchment radius implies observing too few of the plantation sites experiencing deforestation, biasing our observations towards areas near palm oil mills. On the other hand, a too large catchment radius spuriously relates plantations to many mills that have in fact no influence, introducing measurement error in price signals. This trade-off justifies that we assume a different catchment radius for Sumatra and Kalimantan. First, in Sumatra, most deforestation occurs within $30\,\mathrm{km}$ of mills, while in Kalimantan a significant share occurs farther away (Table A.1). Second, the higher mill concentration in Sumatra reduces the likelihood that a plantation will be influenced by prices from mills located farther than $30\,\mathrm{km}$ away.

**Differential mill influence.** While several mills can be reached from a plantation site, not all are equally influential. Plantation agents may expect different sales plans, from selling to one mill exclusively, to partial off-take agreements with some of the reachable mills, to selling on the local spot market only. We model the relative influence of reachable mills as invert-Euclidean distances standardized among reachable mills in a given year. We use these as weights to average mill-gate prices into the annual price signal perceived in a plantation site and year. This Von Thünenian model of influence is consistent over the unobserved heterogeneity in the expected sales plans of plantation agents, because the weights approximate the expectable, relative transport costs within a set of reachable mills (including constant fuel costs and fruit quality decline).

**Medium-run expectations and price signals.** Oil palm trees become productive three years after planting and yield fruits for twenty years or more, so prospective plantation agents form price expectations in the medium- to long-run. We assume that prices in the four most recent years make the best information available in this context. This balances a trade-off between implausibly assuming naive expectations, where only the current price makes all the information, and implausibly assuming that ancient mill-gate price information is fully accessible.[14] More-

[13] This implies potentially lower price signal variance in smaller sets of reachable mills, but it is benign since for causal and statistical inference we cluster observations by set of reachable mills.

[14] The literature on palm oil price forecasting is not informative for the present case, because it focuses on intra-annual forecasting and on identifying best models and lags, using macro price series (Rahim et al. 2018).

over, a price elasticity in this medium-run time scale is relevant to policy instruments typically enforced for more than a year, but not necessarily expected to last more than a political mandate. We thus model price expectations as the moving average of the annual price signals in the current and the three previous years, which we call the *medium-run price signal* (formula in Appendix C.3.2). We weight equally each year of information for the sake of simplicity and since allowing flexible weights yields very similar results (Section 5.3).[15]

# 4 Estimation and identification

## 4.1 Estimation strategy

The conceptual framework outlined above can be summarized in a reduced-form relationship between deforestation on the left-hand side, and the crude palm oil (CPO) price signal and a structural error term on the right-hand side.[16] We model this reduced-form as follows:

$$Deforestation^k_{i\omega dt} = exp(\alpha^k ln(Price^{medium}_{i\omega dt}) + \beta^k X_{i\omega dt} + \lambda^k_\omega + \gamma^k_{dt} + e^k_{i\omega dt}) \tag{2}$$

$Deforestation^k_{i\omega dt}$ is deforestation for $k =$ industrial, smallholder or all plantations, in plantation site $i$, with the set of reachable mills $\omega$, in district $d$, from 2002 to 2014 ($t = 1, ..., 13$). $Price^{medium}_{i\omega dt}$ is our measure price signal. In our main specification, $Price^{medium} = Price^{medium,CPO}$, and thus $\alpha^k = \alpha^k_{CPO}$ is the crude palm oil price elasticity of deforestation for smallholder ($k = A$), industrial ($k = B$), or all ($k = \{A, B\}$) plantations. $\alpha^k_{CPO}$ is a reduced-form parameter with respect to the more structural elasticities in the right hand side of Equation 1. To document some of these mechanisms, in alternative specifications we add the price signal of fresh fruit bunches (FFB), i.e. $Price^{medium} = [Price^{medium,FFB}, Price^{medium,CPO}]$.

$X_{i\omega dt}$ is a vector of controls, comprising the number of known reachable mills in our main specification. We decompose the other determinants of deforestation into fixed effects at the set of reachable mills level ($\lambda^k_\omega$) district-year fixed effects ($\gamma^k_{dt}$) and residuals ($e^k_{i\omega dt}$).

[15] This simplification is notably helpful in interaction analyses. Moreover, dynamic structural (Scott 2014; Araujo et al. 2021; Hsiao 2022), or cross-sectional (Souza Rodrigues 2019) estimations would constrain the heterogeneity and mechanism analyses that make this paper's contribution.

[16] Which comprises all the other determinants of deforestation — probably including perceived information on investment costs (e.g., of land acquisition and conversion), discount rates, operating costs (e.g., of labor, energy and fertilizers), institutional costs (either fixed or marginal, positive or negative, formal or not), opportunity costs, and attainable yields.

$Deforestation^k_{i\omega dt}$ is a count of non-negative integers (deforestation pixel-events) that may be null for a significant proportion of observations. Effectively, Table 1 shows that it is positively skewed, with a substantial amount of zero values. Therefore, we estimate Equation 2 as an exponential mean model, by Poisson Quasi maximum likelihood (Wooldridge 1999) (Appendix D.1). This imposes weaker distributional assumptions, as it only requires the mean (and not the variance) to be correctly specified. It is also more robust to distributional assumptions than negative binomial models for count data (Wooldridge 2002), and more appropriate than the inverse hyperbolic sine transformation given the small values of the measured land use change responses for many observations (Bellemare and Wichman 2020). Finally, unlike for zero-inflated Poisson models, the available algorithms to fit quasi-Poisson models accommodate fixed effects well.

**Statistical inference.** To estimate standard errors, we allow arbitrary correlations within clusters of observations. Following Abadie et al. (2022), we define clusters at the level where the treatment assignment is as-good-as-random. In our case, this is the set of reachable mills (Section 4.2 and Appendix D.3). On average, nine plantation sites over eight years have the same set of reachable mills; and the median set has two plantation sites over eight years.

### 4.2 Identification strategy

Absent a sharp experimental framework, it is not straightforward to interpret causally an observed correlation between prices and deforestation. Yet, with a set of assumptions and empirical specifications, we can isolate and leverage variation in the data that identifies the causal elasticity parameter $\alpha$.

**The source of identifying variation.** The variation in price signal, our treatment variable, arises from the interaction of two variation sources. The first one is the spatial distribution of mills and plantations - i.e., the differences between plantation sites in their relative distances to reachable mills. The second source of variation is the differential mill-gate crude palm oil (CPO) prices. This relates our price signal regressor to a shift-share treatment (Appendix D.3). In such setting, for $\hat{\alpha}^k$ to identify the price elasticity, it is sufficient that the mill-gate CPO prices (the shifts) be conditionally exogenous to plantation-mill distance weights (the shares) and to deforestation in plantation sites (Borusyak et al. 2022).

These conditions can hold because the conditional variation in mill-gate CPO prices is driven by downstream dynamics that mills cannot influence due to the CPO market structure. Such variation exists (Appendix C.3.1), because downstream dynamics are passed on to mills differentially according to their selling schedules within a year, which are not necessarily synchronized. Indeed, mills mainly sell CPO via off-take agreements in futures or forward markets for periods of typically three to nine months, and marginally through spot-market sales (Wiggs et al. 2020; EBRD 2024). As a result, in a given year any two mills may face different combinations of intra-annual market prices. This variation can be used for identification in our case if, as we assume, the interaction between the intra-annual timing of mills' selling deals, and the deal prices, is exogenous. We argue that this assumption holds, essentially because the CPO market gives no bargaining power to mills over their deals with the concentrated trading and refining groups (Section 2.1). CPO buyers can thus impose their conditions (Purnomo et al. 2018; Wiggs et al. 2020; EBRD 2024), including a common oligopoly price based on intra-annual dynamics in aggregate supply and demand, and idiosyncratic contract lengths according to downstream logistics, financial optimization, business strategies and macroeconomic dynamics.[17] The components of this interaction are not observable, as the corporate ties between mills, traders and refineries are still mostly hidden from the public and the terms of their transactions are even more so. Therefore, we endeavor to isolate the identifying variation by including highly resolved district-year and local FFB market fixed effects in Equation 2. This effectively restrict comparisons to plantation sites that only differ in their relative distances to mills facing idiosyncratic price departures from the annual district average.

**Isolating the identifying variation** As district-year fixed effects remove common variation to observations in the same year and district, the remaining variation in price signals can be interpreted as departures from a district market price. In line with the above characterization of the sector, there is substantial variation in these departures, with a standard deviation from the district market price of 138 USD/ton (Appendix C.3.1). These departures are independent from macro-drivers of prices on markets larger than a district, and that could spuriously correlate with macro-level drivers of deforestation.[18] Departures being annual, they are also indepen-

[17] The price-setting power of CPO buyers is confirmed in an interview of the head of the Indonesian Palm Oil Association (PwC 2023).

[18] For instance, large-scale meteorological events like El Niño, affect both annual agronomic palm conditions and international prices of palm oil and substitutable crops, in turn affecting the Indonesian market prices (Rahman

dent from district-specific dynamics. This is crucial, because district political cycles can explain deforestation and prices through general equilibrium effects on the district markets for land, labor and energy.[19]

The local market fixed effects further restrict comparisons between price signal departures within the same local FFB market, i.e. at plantation sites with the same set of reachable mills. This controls for systematic differences in local determinants of price departures that can also correlate with deforestation, including: i) agro-climatic conditions — typically constant in time and common to larger areas than a plantation site; ii) FFB quality differences sufficient to explain CPO quality differences — which are limited given the high level of standardization and homogeneity of the CPO commodity (Byerlee et al. 2016); iii) remoteness — capturing transport costs from the local FFB market to CPO buyers, and compliance costs due to the intensity of regulatory or civil society monitoring; iv) the number of reachable mills — continuously capturing differences between frontier and mature markets, including local infrastructures like roads (Hughes 2018) and hence a time-varying part of transport costs. Since the sets of reachable mills are made only of the mills we have geo-localized, we further control for the number of all known reachable mills per the UML (2018).

**Reverse causality.** Even within local FFB markets and relative to annual district averages, deforestation in the past may explain both current deforestation and CPO prices. Such dynamic reverse causality could arise in three scenarios, provided there is a significant auto-regressive process between current deforestation and deforestation removed at least five years in the past[20] First, mills could locally contain production (and deforestation) to later obtain higher prices, or bargain higher prices to accommodate local price increases. This scenario is implausible to the extent that mills have no seller market power (Section 2.1).[21] In the second scenario, local deforestation spikes that later raise FFB supplies could depress FFB prices and/or allow mills to achieve economies of scales, which they could transform into price competitiveness for growth. Local spikes in FFB supplies could also give mills some leeway to realize opportunistic

et al. 2013; Sanders et al. 2014; Santeramo and Searle 2019).

[19]As Indonesian district jurisdictions are powerful in the administration of land and the control over land can unlock substantial revenues, deforestation has been shown to respond to the decentralization of authority on land, district splits, and district political cycles (Burgess et al. 2012; Cisneros et al. 2021; Project 2025).

[20]Recall the lag between deforestation and supply; and that our measurement of price signal comprises 1- to 4-year lagged prices.

[21]The average mill in our data produces about 0.1% of global supply in the period (Table A.3).

deals within a year. However, this is unlikely because as demand was booming, the palm oil market was demand-driven, such that all supply would meet demand at profitable prices. In this non-equilibrium state, mills did not need translate input price drops or productivity gains into lower selling prices.[22] Third, CPO buyers could track local supply spikes to offer lower prices to mills with excess CPO.

Empirical assessments further comfort us in dispelling reverse causality. First, we check that the price elasticity estimates are robust to controlling for past deforestation (Section 5.3). In addition, we run falsification tests and find no evidence that mill-gate FFB inputs affect CPO prices. The falsification tests are mill-level regressions of CPO prices on current and previous FFB volumes. These yield economically and statistically insignificant coefficient estimates, thus not falsifying our counterarguments to the two first scenarios above, and indicating that none of the three scenarios is strong enough to show up in the data. It is consistent with our main assumption that mill-gate CPO prices are driven by downstream dynamics and not by upstream supplies. More precisely, combined with evidence of economies of scale in the milling sector (Man and Baharum 2011; Byerlee et al. 2016), these results corroborate that mills did not turn economies of scale into more competitive CPO prices. This empirical support against reverse causality holds for similar and alternative variation scales as in the main regressions — i.e. in level and log of CPO prices (Table A.4 and A.5 respectively), and for between- and within-mill deviations from annual district averages (odd and even columns).

# 5 Results

We present our estimates of the crude palm oil price elasticity of deforestation for the different plantation types in Table 2. Column (6) features the overall 1.5 price elasticity of deforestation for any plantation. It shows that pooling together all types of plantations — industrial, illegal or not, and smallholder plantations — deforestation reacts positively to crude palm oil price signals. This is generally larger than the estimates in the literature (Appendix E). More specifically, deforestation for unregulated plantations responds positively to price signals too, with a price elasticity of 2.4 (column 5). Next, we unpack this result for the two kinds of unregulated deforestation — for illegal industrial and for smallholder plantations. We then check the robustness of the results and gauge price effects at scale.

[22] See Box 2.1. in Byerlee et al. (2016) for an analysis of mill profitability.

Table 2: Crude palm oil price elasticity of deforestation across plantation types

| Deforestation for: | (1) Industrial plantations Legal | (2) Industrial plantations Illegal | (3) Industrial plantations All | (4) Smallholder plantations | (5) Unregulated plantations | (6) All |
|---|---|---|---|---|---|---|
| **Elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | 0.66 | 4.35 | 1.79 | 1.37 | 2.4 | 1.5 |
| 95% CI | [-1.21; 2.53] | [1.5; 7.21] | [0.27; 3.31] | [0.18; 2.57] | [0.97; 3.82] | [0.37; 2.62] |
| Observations | 20532 | 18801 | 65368 | 20721 | 28505 | 71926 |
| Clusters | 533 | 523 | 1143 | 529 | 927 | 1441 |

NOTE. This table shows our main estimates of the crude palm oil (CPO) price elasticity of deforestation. They are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of CPO prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

## 5.1 The price elasticity of industrial plantations

### 5.1.1 Illegal industrial deforestation.

In Table 2, column (3) indicates that deforestation for industrial plantations increases by 1.79% in response to a 1% increase in crude palm oil price signals. This includes deforestation for all industrial plantations, including with unknown legal status. Zooming in on deforestation for industrial plantations identified as illegal, we find a 4.3 price elasticity (column 2). Hence, as medium-run CPO prices signal higher profitability in the future, companies accelerate deforestation, including in areas where it is illegal. We explore the dynamics of this response, before comparing with legal deforestation.

First, we split the analysis between immediate conversion (0-4 years between forest clearing and palm planting) and transitional conversion (5-12 years). Table A.6 shows the price elasticities of industrial deforestation by these conversion dynamics, in the period 2002-2010 when transitional conversion can be observed. We find that the price elasticity of immediate conversion for illegal industrial plantations is positive and substantial (column 2). Another striking result from Table A.6 is the significant price elasticity of transitional conversion for illegal industrial plantations (column 5). It indicates that some of the illegal forest clearing encouraged by price signals is not converted to industrial plantations within 4 years, but after delays of 5 years or more.[23] Such delays may be due to financial complications or government interven-

[23] Gaveau et al. (2022) report that delays are marginal in aggregate terms. Here, we find that transitional conversion events, albeit small (2.2 ha per grid cell-year on average) are not uncommon.

tions limiting plantation development after forest is cleared.[24] Although barely documented, the numerous conflicts with local communities may also delay development operations after forest clearing (Berenschot et al. 2024).

Second, we estimate the conditional partial effects of the contemporaneous and the price signals, and of their interaction. The contemporaneous price signal is the annual price signal (Equation 3) in the year deforestation occurs. The previous price signal averages the annual price signals in the three previous years. We find that industrial deforestation altogether responds positively to contemporaneous price signals, and not clearly to previous price signals (Table A.7 column 3). This is also the case of deforestation for illegal industrial plantations, but with a clearer response to previous price signals in this case (column 2). More precisely, Table A.8 (columns 2 and 3), shows that industrial deforestation is sensitive to the price signals in the two most recent years (conditional on the other annual price signals). For industrial plantations altogether, the moderating effect of contemporaneous price signals on previous ones is positive (Table A.7 column 3). This means that the effect of previous price signals on deforestation increases with an increase in contemporaneous price signals (or vice-versa). These results indicate that price developments in the short-run weigh significantly in the expectations formed by industrial plantation agents about the profitability of their perennial and yield-lagging crop. Yet, older price signals do play a role too, as they reinforce the influence of contemporaneous ones when they agree.

Lastly, in another analysis where we regress deforestation on the variability in the price signal,[25] we find additional results to understand industrial deforestation. We find that price signal variability deters industrial deforestation (column 3 in Table A.9). This is consistent with forest having a positive option value for oil palm companies, as Lundberg and Abman (2022) demonstrate and find in a non-subsistence smallholder context.

#### 5.1.2 Contrast with legal deforestation.

On the other hand, we estimate that the price elasticity of deforestation for industrial plantations that are legal is below one and not statistically different from zero (Table 2, column 1). Hence, there is no evidence that legal industrial deforestation responds to price signals.

[24] An extensively documented case is the Tanah Merah project (`https://news.mongabay.com/2020/03/new-player-starts-clearing-rainforest-in-worlds-biggest-oil-palm-project/`).

[25] We measure variability as the standard deviation in the annual price signals making the medium-run price signal. In these regressions we control for the level of price signal.

This is unsurprising, because the spatio-temporal dynamics of legal deforestation are bound to inflexible administrative and political processes. In particular, companies are required to develop the industrial plantation rapidly after obtaining the Location Permit (Ijin Lokasi), which they obtain only following previous licensing steps; and the plantation demarcation depends on environmental suitability assessments and community consultation (Paoli et al. 2013). The negative price elasticity in column (1) of Table A.6 nevertheless indicates that with higher prices, companies put legal immediate conversion on hold. Moreover, this opposite effect to illegal deforestation is consistent with the hypothesis that price signals induce companies to escalate regulation bypassing — which Setiawan et al. (2016) show is not difficult — *instead* of waiting for every authorization and miss out on economic opportunities. This reallocation might occur spatially, through the reallocation of resources, or temporally, by circumventing previously observed legal procedures, and revert to legal expansion as prices decline.

To test this hypothesis further, we investigate the effect of price signals on the distribution of industrial deforestation between illegal and legal plantation sites. We quantify this distribution as the relative difference between the predicted averages of illegal and legal industrial deforestation. To estimate the effect of price signals on this difference, we pool illegal and legal industrial deforestation and augment the main regression with a term of interaction between price signal and the indicator of illegality. Illegal deforestation tends to occur in areas with higher initial forest cover, fewer reachable mills, and more public ownership of reachable mills (Table 1), and this may also moderate the effect of price signals on industrial deforestation. Therefore, we control for these covariates of illegality, for their interactions with price signals, and for differential trends on initial forest cover. Table 3, shows the estimated partial effects of price signals, illegality and their interaction on deforestation for industrial plantations.[26] Columns (1)-(2) show that local market fixed effects absorb too much variation to find evidence that price signals affect the distribution of deforestation between legal and illegal plantation sites. However, releasing some of this variation — including changes over time in deforestation and price signals as local markets evolve — we do find a redistribution effect (columns 3-6).[27] Overall, we estimate that deforestation for illegal plantations is 59% to 84% lower than for legal plantations at average price signals. A 1% increase in average price signals reduces this difference by 0.3 to 1.1 percentage points. Hence, this analysis indicates that

[26] We also report p-values of tests of residual variance equality between illegal and legal deforestation, indicating no risk of spurious inference associated with the interaction coefficient (Wooldridge 2002).

[27] Recall that all specifications allow for endogenous political economy down to the district level.

price signals influence to some extent whether industrial plantations expand illegally or legally (within concessions at least). Deforestation operations, which are on average larger in legal areas, seem to be reduced in favor of illegal cuts when price signals rise.

Table 3: Effect of crude palm oil price signal on the distribution of deforestation between illegal and legal industrial plantations

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | Industrial plantations | | | | | |
| **Partial effect of:** | | | | | | |
| *Illegal*CPO price signal* | | | | | | |
| Estimate | 0.39 | 0.32 | 1.11 | 0.69 | 1.12 | 0.79 |
| CI | [-0.05; 0.84] | [-0.05; 0.68] | [0.37; 1.85] | [0.12; 1.25] | [0.44; 1.79] | [0.31; 1.26] |
| *Illegal* | | | | | | |
| Estimate | -78.96 | -84.04 | -59.14 | -73.06 | -66.87 | -77.85 |
| CI | [-87.23; -70.69] | [-91.35; -76.73] | [-71.14; -47.14] | [-81.29; -64.82] | [-76.39; -57.35] | [-83.75; -71.96] |
| *CPO price signal* | | | | | | |
| Estimate | 2.34 | 2.97 | 1.63 | 1.53 | 1.4 | 1.41 |
| CI | [0.77; 3.91] | [1.27; 4.66] | [0.23; 3.04] | [0.18; 2.88] | [0.16; 2.63] | [0.27; 2.55] |
| Extra controls | | X | | X | | X |
| **Fixed effects:** | | | | | | |
| Set of reachable mills | X | X | | | | |
| Subdistrict | | | X | X | | |
| District-year | X | X | X | X | X | X |
| **p-value in test of:** | | | | | | |
| Residual variance equality | 0.118 | 0.116 | 0.669 | 0.565 | 0.541 | 0.44 |
| Observations | 39214 | 38511 | 37167 | 36506 | 39214 | 38636 |
| Clusters | 928 | 921 | 928 | 923 | 928 | 923 |

NOTE. This table shows our estimates of the effect of crude palm oil (CPO) price signals on the distribution of deforestation between illegal and legal industrial deforestation, expressed in percentage points (rows under "Illegal*CPO price signal"). They express a difference in percentage difference (relative difference x 100) between deforestation for illegal and for legal industrial plantations, associated with a 1% increase in average signals. Rows under "Illegal" report the percentage difference (the distribution of illegal deforestation) at baseline average price signals. Rows under "CPO price signal" report the partial effects of a 1% increase in price signals on industrial deforestation in percentage change, including the interaction effect of illegality at the average illegality rate. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with industrial oil palm plantations. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. P-values are for tests of residual variance equality between illegal and legal deforestation. Regressions without extra controls include only the number of all known reachable mills as covariate, while extra controls are: the percentages of initial forest cover, domestic private ownership, foreign ownership and CPO exports at reachable mills (on distance-weighted average); interactions between all covariates and price signals; and a flexible trend on the initial forest cover. All estimates are derived from a generalized linear model of the quasi-Poisson family. Regressions include combinations of fixed effects, from geo-localized IBS reachable-mills, subdistrict and district-year fixed effects. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50 km (30 km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

## 5.2 The price elasticity of smallholder plantations

We find that a 1% increase in crude palm oil (CPO) price signals increases deforestation for smallholder plantations by 1.4% (Table 2, column 4). This indicates that smallholders are elastic to fresh fruit bunches (FFB) prices and/or that more influential plantation agents (with or without milling capacity) steer deforestation for smallholder plantations in the same direction as CPO price signals. To disentangle these potential mechanisms, we add the price signal of FFB in the regression.[28] Price signals are not as exogenous for FFB as for CPO though. Notably, FFB quality may increase both deforestation and FFB prices. In this case, the omitted variable bias

[28] I.e. $Price_{idt}^{medium} = [Price_{idt}^{medium,FFB}, Price_{idt}^{medium,CPO}]$ in Equation 2.

would most likely be positive and the FFB price elasticity estimate would be an upper bound. Since smallholder plantations have no seller power, it is unlikely that they contain deforestation to maintain FFB prices high. Therefore, we can exclude that such reverse causality introduces a negative bias here.[29] We estimate a negative elasticity of deforestation for smallholder plantations to FFB prices signals (Table 4), including conditional on CPO prices (columns 2-4). Since we believe this result to be an upper bound, it means that FFB price signals reduce deforestation for smallholder plantations.

Table 4: Fresh fruit bunches price elasticity of deforestation for smallholder plantations

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Deforestation for:** | Smallholder plantations | | | |
| **Elasticity to:** | | | | |
| *FFB price signal* | | | | |
| Estimate | -2.02 | -1.92 | -1.94 | -1.97 |
| 95% CI | [-3.61; -0.43] | [-3.48; -0.36] | [-3.51; -0.37] | [-3.54; -0.39] |
| *CPO price signal* | | | | |
| Estimate | | 1.71 | 1.71 | 1.71 |
| 95% CI | | [-0.14; 3.57] | [-0.14; 3.57] | [-0.15; 3.56] |
| **Additional controls** | | | | |
| Industrial expansion proxy | | | | X |
| ... interacted with FFB price | | | X | X |
| Observations | 18207 | 15880 | 15880 | 15880 |
| Clusters | 501 | 484 | 484 | 484 |

NOTE. This table shows our estimates of the fresh fruit bunches (FFB) price signal partial effects on deforestation for smallholder plantations. They are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signal. We show partial effect estimates for the crude palm oil (CPO) price signal for informative purpose only, as they only have a control role here. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of either FFB or CPO prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with smallholder oil palm plantations. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50 km (30 km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

We can derive several conclusions from this negative effect, within our conceptual framework and under the assumption that the pass-through from CPO to FFB prices is classically non-negative (Section 2.2).[30]

First, plugging it ($\hat{\iota}^A_{FFB} + \hat{\phi}^B_{FFB} < 0$) in Equation 1 with the former result that CPO price signals accelerate deforestation for smallholder plantations ($\hat{\alpha}^A_{CPO} = 1.37$ in Table 2, column 4), one infers that $\phi^B_{CPO} > 0$. This means that industrial plantation agents respond to an increase in

[29] Because such reverse causality cannot be excluded for industrial plantations, we do not present results about FFB price signals in the case of industrial deforestation.

[30] The extreme case of a null pass-through corresponds to a monopsonic mill paying FFB at plantations' marginal cost irrespective of its CPO price. Estimating the pass-through is beyond the scope of this paper as it would require a quasi-experimental setting at the mill level and a model of local mill oligopsonies.

CPO prices specifically (i.e. at constant FFB prices) by accelerating deforestation for smallholder plantations.

Second, our analysis of dynamic effects shows that, unlike for industrial plantations, it is the previous years of price signals that primarily count in expectations formed to deforest for smallholder plantations (column 4 in Table A.7 and A.8). This suggests that in times of contemporaneous CPO price spikes, companies prioritize deforestation for their own industrial plantations and allocate forest land to smallholder plantations in the following years.

Third, when CPO prices increase, the reduction in deforestation for smallholder plantations caused by the passed-through increase in FFB price signals ($\varepsilon_{CPO}^{FFB} \times (\iota_{FFB}^{A} + \phi_{FFB}^{B}) < 0$) is more than compensated by the influence of companies in response to CPO price signals specifically ($\phi_{CPO}^{B} > 0$). Hence, this influence is possibly strong relative to the CPO-to-FFB price pass-through $\varepsilon_{CPO}^{FFB}$ (assumed non-negative). Another possibility is that the reaction of companies to FFB prices specifically ($\phi_{FFB}^{B}$)) has an opposite effect on deforestation for smallholder plantations than smallholders' own response (where "own" means net of the expansion facilitated/hindered by industrial actors, i.e. $\iota_{FFB}^{A}$).

Fourth, to disentangle which of these mechanisms primarily drives the negative effect, we additionally control for the immediate conversion of forest to industrial plantations, as a proxy for the eviction of smallholder expansion by industrial plantations (Table 4 column 3). Additionally, we control for the interaction of this proxy with the FFB price signal (column 4). We find that the effect of FFB price signals on deforestation for smallholder plantations remains negative when the eviction effect ($\phi_{FFB}^{B}$) is controlled. Therefore, we conclude that smallholders have a negative price elasticity in terms of their own deforestation decisions

While the complexity of smallholder decisions calls for cautious interpretations (Jelsma et al. 2019), the present result advances empirical knowledge on the matter and is consistent with previous studies and theory indicating an intensive margin response of smallholders. In theory, low yields and incomplete expansion agency imply that intensification may offer higher returns than expansion. Moreover, intensification offers a swifter and thus surer mean to benefit from FFB price heights, because higher yields can be achieved rapidly by increasing labor and fertilizer inputs, two major yield-limiting factors (Lee et al. 2014b; Euler et al. 2016a; Hoffmann et al. 2017). In turn, as demand for farm labor (in both smallholder and industrial plantations) rises with FFB prices, smallholders' own workforce for expansion, and therefore expansion, may diminish. In times of relatively lower FFB prices, smallholders may also prioritize deforestation for the purpose of securing land, rather than focusing on production (Ogahara et al. 2022). In reality, independent smallholder yields are low, heterogeneous in space (Monzon et al. 2023), and tend to increase over time.[31] Our result being identified with space-time variation, it is thus consistent with the proposition that smallholders respond to the growing FFB demand and price incentives by intensifying production instead of expanding (in their own decision).

## 5.3 Robustness

We check the robustness of the results against a range of alternative methods, for all deforestation and for both kinds of unregulated deforestation (Figures B.4, B.5, B.6). In each of the three cases, the main estimate of the crude palm oil price elasticity appears at the center of the distribution of alternative estimates. We leave a complete discussion of these estimates for Appendix F.1. Here, we discuss additional robustness checks of key assumptions.

First, we check that the results hold when allowing annual price signals to have different dynamic effects (Table A.8) and when price signals are expressed in level instead of logarithm (Table A.10) — two conditions to consider our identification strategy as a shift-share (Appendix D.3).

Second, to assess the sensitivity of the results to potential reverse causality (which we argued is unlikely in Section 4.2), we estimate the price elasticity conditional on past deforestation. We measure past deforestation in four alternative ways as the combinations of two specifications of spatial scale and two specifications of temporal depth. As spatial scales we consider either past deforestation in plantation site $i$, or in its eight closest neighbors. As temporal depths, we consider either deforestation in $t-5$, i.e. four years before the latest annual price signal in our main specification (in $t-1$), or averaged from $t-5$ to $t-8$, i.e. over the four years before each of the annual price signal in our main specification ($t-1$ to $t-4$). The longer lags reduce the time period covered in the sample available for estimation. Table A.11 shows that price elasticity estimates in the same period are very similar with or without these controls. This brings further confidence that reverse causality does not threaten causal identification.

[31]Statistics of evolution for smallholder yields are missing, but cross-sectional statistics from large surveys of independent smallholders at different points in time show an increase in yearly FFB yields from 11.0 ton/ha in the early 2010s (Molenaar et al. 2013) to 14.4 ton/ha in 2012-15 (Dalheimer et al. 2022) and 13.9 ton/ha in 2020-21 (Monzon et al. 2023).

## 5.4 Back-of-the-envelope scaling-up

To gauge the magnitudes of our results we estimate the effects that the estimated 1.5 price elasticity of deforestation implies at the level of the whole country of Indonesia for counterfactual price changes (see Appendix F.2 for external validity checks). We allow effects in all plantation sites where deforestation is possible in Sumatra and Kalimantan, which we define as being within 82 km of at least one known palm oil mill (as from the UML). This follows Heilmayr et al. (2020), who analyzed from RSPO audit reports that 99% of mills' supply bases were within this Euclidean distance. Because in this area many plantation sites are actually unlikely to experience deforestation (either because there is no forest or because of unsuitability to oil palms), we excluded those that never experienced any deforestation from 2002 to 2014 (which is probably conservative regarding the total extent of primary forest where oil palm can be grown in the country). Note that, for the sake of simplicity, we count in the scaling area the plantation sites where deforestation occurred before the first mill opened in the catchment radius - i.e., at the extensive margin. Hence, we aggregate our results over 11396 $3 3\times 3\,\mathrm{km}$ plantation sites in Sumatra and Kalimantan. We assume that this population of plantation sites has the same average deforestation as in our sample (Table A.2). Under this assumption, we multiply by the scaling factor to estimate a baseline total deforestation of 132835 ha.

Table 5 shows the aggregated annual effects of different counterfactual crude palm oil price changes on deforestation in Indonesia. For different price changes, we quantify the relative change in average deforestation, the scaled effect on deforestation, and the corresponding potential revenue from a CO2 payment. The effect is scaled based on the aggregation factor presented above. We estimate corresponding carbon pricing revenues from a potential result-based payment for reducing emissions from deforestation. We apply an average of $638\ ton\ CO2\ ha^{-1}$ emissions due to deforestation (Guillaume et al. 2018).[32] CO2 revenues are based on the USD 5/ton CO2 agreed price Norway paid to Indonesia for its recently avoided deforestation .[33]

[32]We apply the 44/12 C to CO2 conversion factor to their $174\ Mg\ C\ ha^{-1}$ lost in conversion of Sumatra rainforests into oil palm monocultures.

[33]https://www.regjeringen.no/en/aktuelt/noreg-betaler-530-millionar-for-redusert-avskoging-i-indonesia/id2722135/

Table 5: Annual counterfactual effects of crude palm oil price changes on deforestation

| | +1 std. dev. | -1% | -19% |
|---|---|---|---|
| Relative change (%) | 7.63 | -1.49 | -29 |
| Total change (ha) | 10137 | -1978 | -38522 |
| Potential CO2 revenues (million USD) | -32.3 | 6.3 | 122.9 |

NOTE. This table shows scaled-up effects of counterfactual changes in crude palm oil (CPO) price signals. To compute total change effects, we apply relative changes to average deforestation from our main econometric model, with a scaling factor of 11396, equal to the number of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan within 82 km to any known palm oil mill where deforestation occurred at least once between 2002 and 2014. Potential CO2 revenues correspond to result-based payments paid at a price of USD 5 per ton CO2 avoided, assuming average emissions of 174 ton C per hectare deforested.

Hence, given a 1.5 price elasticity of deforestation, we estimate that average variations (+5%) in CPO price signals incentivize Indonesian oil palm plantations to clear 10.1 kha of primary forest annually.[34] In the presence of a result-based payment scheme, this represents a yearly opportunity cost of USD 32.3 million. To curb annual deforestation 29% below the 2002-2014 average with price incentives alone, price signals for individual plantations should be lowered by 19%.[35]

## 6 Discussion and conclusion

To conclude, we summarize our main findings, we state the main methodological limitations and we propose further research avenues. Finally, we discuss the policy relevance of our results.

**Summary.** In this study, we estimate different price elasticities of primary forest conversion to oil palm plantations in Indonesia. We find that medium-run crude palm oil (CPO) price signals have a positive effect on deforestation in the Indonesian oil palm sector. The overall price elasticity is 1.5. Unregulated deforestation — for smallholder plantations or for illegal industrial plantations — reacts positively to crude palm oil prices. On the other hand, smallholders seem to deforest less as they receive higher fresh fruit bunch (FFB) price signals.

[34]We compute standard deviations in our price signal regressor variable, in the estimating sample, after removing variations in fixed effect dimensions (Mummolo and Peterson 2018).

**Limitations.** Our country-wide deforestation-price elasticity estimates advance the existing literature in terms of internal validity and of analyses of heterogeneity and mechanisms. Yet, they necessarily rely on observational data, which prevents us from ascertaining that our estimates exactly identify the causal price elasticity parameters of interest.

We believe we use one of the most accurate measurement of the true price incentives privately observed by oil palm plantations in Indonesia to date. However, some measurement error remains. First, we observe the annual mean unitary values and not the prices that mills publicly disclose (at a higher frequency than annually). Second, our sample of geo-localized IBS mills is not exhaustive. Therefore, in areas with mills both from and not from our sample, our price signal measurement is incomplete. We do not suspect any of these to be prone to systematic measurement error. In particular, Table A.3 shows that there is no systematic difference between the IBS mills we have geo-localized and the others.

In terms of heterogeneity, investigating the empirics of unregulated economic behaviors, while being important in this context, is essentially limited by data availability. In particular, our measurement of legality in deforestation for industrial plantations is subject to measurement error, and this approximation may additionally correlate with multiple factors of price signal heterogeneity that we are not sure to control completely. Therefore, our results on the effect of prices on the distribution of deforestation between legal and illegal plantations should be taken with caution and further investigated in future work. For smallholder plantations, we are not able to look into the local diversity of politically relevant situations.

In terms of mechanisms, we are not able to disentangle the price elasticities of deforestation for industrial plantations with and without milling capacity. Besides, we do not capture indirect land use change triggered by price changes.

Regarding external validity, our study may be limited by the exclusion of the extensive margin in our analysis, i.e., deforestation occurring where no mill is already operating. We would expect that such deforestation is less price elastic, because it depends more on other elements that determine the mill establishment, like capital availability, or the regional political economy and infrastructure (Hsiao 2022; Kraus et al. 2022). Yet, the large structural gap between installed milling capacity and actual CPO production (Pirard et al. 2020) indicates a significant risk of deforestation at the intensive margin (where milling capacity is already installed), for which our estimates are valid. Extending our conclusions in time should also be done with caution, since our study does not cover recent developments in oil palm-related policies, such as the biofuel mandates, (Kharina et al. 2016) or the *No Deforestation, No Peat, No Exploitation* commitments from the private sector (Pirard et al. 2015).

Finally, we emphasize that we estimate a price elasticity based on price variations unrelated with any conservation commitment. Therefore, our results cannot serve to evaluate the price incentives provided by the Roundtable on Sustainable Palm Oil (RSPO). Yet, it seems to us that given our elasticity estimates, the RSPO premiums — ranging from 2% (Levin 2012) to 7% (Preusser 2015) — are insufficient to promptly reach zero-deforestation palm oil.

**Further research.** We do not attempt in this paper to properly simulate policy effects on deforestation through prices. We do not model a separation between deforestation-free and deforestation-based markets (and prices) that is caused by a label or by downstream due diligence on sustainability. Hence, our study does not provide strong insights into the incentivizing scheme of the RSPO. We leave such efforts to future research. Moreover, our new, spatially explicit, microeconomic panel dataset of palm oil mills can be useful to study the economic causes of other important phenomena in Indonesia, like land conflicts or intentional forest and peat fires. It can also help improve the understanding of the economics of palm oil mills, whose operations have remained a black box so far. Finally, our identification strategy being grounded on the 'hourglass' industrial organization common to tropical commodity supply chains (zu Ermgassen et al. 2022), it may inspire the design of similar studies in other contexts.

## 6.1 Policy Relevance

There are three main implications from our results that are relevant to sustainable and fair policy in the Indonesian palm oil context.

**Deforestation left unregulated will follow prices.** First, extrapolating our general result to the last decade suggests that the downward trend in palm oil prices is at least partly responsible for the decrease in deforestation rates. As a corollary, a surge in price signals in the future (e.g. because of long-term growth in palm oil demand or downturns in the supply of vegetable oil and fuel substitutes) could overtake supply-side conservation efforts. This is especially likely as unregulated deforestation — for illegal and for smallholder plantations — remains weakly addressed by existing institutionally-reliant interventions (Heilmayr et al. 2020;

Drost et al. 2021; Groom et al. 2022).[36] Our results indicate that this is all the more likely that industrial plantations just by-pass regulations to seize economic opportunities signaled by prices in the medium-run. [37]

**Feasible price interventions can steer unregulated deforestation.** Since maintaining low prices through international demand requires challenging coordination (Hsiao 2022), upstream price intervention is a promising policy option to effectively govern deforestation. Our results confirm the potential effectiveness of such intervention, as they show that deforestation — for unregulated plantations in particular — decelerates in reaction to lower mill-gate CPO prices. Our back-of-the-envelope estimation indicates that a 19% tax levied uniformly on palm oil mills could curb deforestation 29% under the critically high average of 2002-2014.[38] This would credit Indonesia a minimum of USD 120 million a year for avoided emissions. In addition, we find that a uniform mill-tax would curb deforestation only for illegal industrial plantations and for smallholder plantations, thus possibly fostering structural institutional and agricultural change.

Yet, the effectiveness and equity of a mill-tax on CPO would vary with its design. In a generic context alike the present one, Heine et al. (2020) discuss extensively how such tax could be redistributed to incentivize conservation even further. In particular, they suggest targeting choke points in the value chain, much alike palm oil mills, where the state has sufficient capacity to levy a tax and that cannot be circumvented by upstream producers. They propose that the tax be refunded against proof of sustainable production. Reversing the burden of the proof accommodates possibly weak monitoring capacity of the tax authority. This effectively enlists the entity at the choke point as a "voluntary private enforcer" of the state's desired policy (Heine

[36] Adding incentives to the conservation policy toolkit has been advocated in the Amazonian context for similar reasons (Godar et al. 2014).

[37] Currently, an export tax applies to CPO and its revenues are meant to support rural development. Our results suggest that the export tax has contributed to avoid illegal deforestation. However, the effect of the export tax on CPO price signals is not clear, for at least three reasons: the tax varies with international prices; the domestic demand for CPO has increased; and the tax embeds no conservation incentives.

[38] General-equilibrium effects could modulate the effect of such a tax in either direction. For instance, if the elasticity of demand for deforestation-based palm oil is low, producers could transfer the burden of the tax to consumers and thus be less affected on equilibrium. A tax on palm oil only may also achieve lower emission reductions than we estimate if forest is left vulnerable to uncontrolled production of another commodity. Finally, our results suggest that a tax would disincentivize illegal deforestation but not necessarily legal deforestation. The effect of the tax therefore depends on the regulatory framework and its capacity to handle the additional demand for legal deforestation. Moreover, we also find that the conservation benefits of a tax could be mitigated if the tax has a stabilizing effect on prices.

et al. 2020). Our results indicate that palm oil mills could serve as such entities, as they steer deforestation, in particular for illegal and for smallholder plantations, upon price changes. Moreover, such link between the tax level and conservation outcomes would strengthen incentives beyond the elasticity we estimate here from non-linked price differentials. More generally, structural change — like smallholder formalization — in remote and unregulated segments of the palm oil supply chain may be governed through such feebate targeted at the locally potent palm oil mill (Heine et al. 2020).

**Protecting smallholder revenues further reduces deforestation.** Regarding equity, how revenues from the tax, and possibly from international compensation for avoided emissions, are redistributed to land and palm oil-reliant communities (including smallholders) is critical. Our main results indicate that a CPO tax would lead palm oil mills to restrict forest clearing for smallholder plantations. While this is a welcome impact from a gross environmental perspective, it is not from an environmental justice perspective, nor from an economic development one. Importantly, our results indicate that smallholders respond to higher fresh fruit prices by intensifying their plantations but decide to expand in the forest when prices are low. This expansive reaction to a drop in prices induced by the tax would be more than compensated by milling actors constraining smallholders' access to new land. Together, these results imply that both the effectiveness and equity of a tax on crude palm oil could be improved by augmenting it with protection or redistribution schemes that prevent smallholders' prices or incomes to be affected.

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# Appendix

## A Tables

Table A.1: Deforestation for oil palm plantations accumulated over 2002-2014, in kha.

| **Plantations in:** | Sample | 30km from sample mill | 50km from sample mill | Total |
|---|---|---|---|---|
| Sumatra | 221.72 | 564.55 | 702.02 | 801.40 |
| Kalimantan | 150.32 | 321.92 | 565.81 | 1015.62 |
| Both | 372.05 | 886.47 | 1267.83 | 1817.02 |

NOTE. This table shows measures of accumulated deforestation from 2002 to 2014 in different groups of plantation sites in Indonesia. Deforestation is counted as primary forest loss eventually replaced with oil palm plantations (either industrial, in 2015, or smallholder in 2014). The first column on the left, called "Sample", describes the group of plantation sites that have non-missing price signal information and that we hence use in our main analysis. Sample mills are the 587 palm oil processing plants from the Indonesian manufacturing census that we have geo-localized. The last column on the right, called "Total", describes the group of all plantation sites across the study area, including with missing price information or located away from a sample mill, and thus represents total deforestation for oil palm.

Table A.2: Descriptive statistics of all-deforestation samples with and without missing values

| | **Without missing values** | | | **With missing values** | | | **t test** | **KS test** |
|---|---|---|---|---|---|---|---|---|
| | # grid cells = 12687<br># grid cell-year = 71926 | | | # grid cells = 22570<br># grid cell-year = 215667 | | | | |
| | mean | std.dev. | median [min; max] | mean | std.dev. | median [min; max] | p-value | p-value |
| Deforestation (ha) | 5.17 | 29.6 | 0 [0; 847.5] | 5.18 | 30.95 | 0 [0; 903.1] | 0.984 | 0.000 |
| Price signal ($/tCPO) | 672.9 | 92.46 | 671.7 [349.8; 926.4] | 673.9 | 92.42 | 673.2 [349.8; 926.4] | 0.031 | 0.276 |
| # reachable mills | 7.79 | 5.12 | 7 [1; 37] | 5.72 | 4.3 | 5 [1; 37] | 0.000 | 0.000 |

NOTE. This table shows descriptive statistics of the variables used in our main regression, for the sample of deforestation for all plantations actually used in estimations (without missing values), and the same sample but without removing observations with missing values. # means "number of". The two right-most columns show p-values of Welch two-sided t-tests, where the null hypothesis is that the true difference in means between the two groups is null, and the groups' variances are not assumed to be equal; and p-values of Kolmogorov-Smirnov tests where the null hypothesis is that the variables in the two groups are drawn from the same continuous distribution.

## Table A.3: Descriptive statistics of palm oil mills in the Indonesian manufacturing census

| | **Geo-localized IBS palm oil mills n = 587 mills** | | | **All IBS palm oil mills n = 930 mills** | | | **t-test** | **KS test** |
|---|---|---|---|---|---|---|---|---|
| | mean | std.dev. | median [min; max] | mean | std.dev. | median [min; max] | p-value | p-value |
| First year in IBS | 1999 | 8.19 | 2001 [1975; 2015] | 2000 | 8.78 | 2002 [1975; 2015] | 0.000 | 0.000 |
| FFB MUV (USD/ton) | 124.7 | 35.69 | 127.4 [16.84; 241.5] | 123.3 | 35.73 | 125.8 [16.84; 242.2] | 0.108 | 0.274 |
| FFB input (ton) | 149047 | 115114 | 133193 [0; 1035319] | 148035 | 114416 | 132552 [0; 1035319] | 0.692 | 1.000 |
| CPO MUV (USD/ton | 684.9 | 172.5 | 706.8 [170.1; 1191] | 679.8 | 173.4 | 700.8 [170.1; 1191] | 0.192 | 0.287 |
| CPO output (ton) | 36082 | 24384 | 32902 [0.64; 179142] | 35795 | 24363 | 32389 [0.64; 179142] | 0.587 | 0.999 |
| PKO MUV (USD/ton) | 399.9 | 140 | 389.4 [12.53; 827] | 398.4 | 139.8 | 386 [12.53; 832.9] | 0.676 | 1.000 |
| PKO output (ton) | 8441 | 8918 | 6917 [0.11; 96775] | 8368 | 8861 | 6846 [0.11; 96775] | 0.724 | 1.000 |
| CPO export share (%) | 16.85 | 33.37 | 0 [0; 100] | 15.75 | 32.55 | 0 [0; 100] | 0.072 | 0.375 |
| Central government ownership (%) | 15.39 | 35.48 | 0 [0; 100] | 14.64 | 34.76 | 0 [0; 100] | 0.227 | 0.961 |
| Local government ownership (%) | 2.25 | 14.65 | 0 [0; 100] | 2.1 | 14.17 | 0 [0; 100] | 0.562 | 1.000 |
| National private ownership (%) | 65.75 | 46.02 | 100 [0; 100] | 66.76 | 45.7 | 100 [0; 100] | 0.214 | 0.831 |
| Foreign ownership (%) | 16.62 | 34.89 | 0 [0; 100] | 16.51 | 34.88 | 0 [0; 100] | 0.862 | 1.000 |

NOTE. This table reports summary statistics for a set of variables from the Indonesian manufacturing census (IBS), at the palm oil mill level, annually in 1998-2015. The sample of geo-localized IBS palm oil mills is a sub-sample of all IBS palm oil mills. IBS palm oil mills are identified here as IBS plants that report crude palm oil (CPO) or palm kernel oil (PKO) outputs, or fresh fruit bunches (FFB) inputs at least one year, and are not in Java or Bali islands. MUV means mean unitary value (value/volume). USD means US dollar and is 2010-constant. We report p-values of Welch two-sided t-tests where the null hypothesis is that the true difference in means between the two groups is null, and the groups' variances are not assumed to be equal; and p-values of Kolmogorov-Smirnov tests where the null hypothesis is that the variables in the two groups are drawn from the same continuous distribution.
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## Table A.4: Mill-level regressions of crude palm oil output prices on fresh fruit bunches input volumes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | CPO output price | | | | | | | | | |
| **Regression coefficient of:** | | | | | | | | | | |
| *FFB input kton in year t* | | | | | | | | | | |
| Estimate | 0.023 | 0.064 | -0.001 | 0.051 | 0.016 | -0.004 | -0.013 | -0.061 | | |
| 95% CI | [-0.029; 0.075] | [-0.0005; 0.128] | [-0.067; 0.065] | [-0.015; 0.118] | [-0.059; 0.091] | [-0.084; 0.075] | [-0.091; 0.066] | [-0.155; 0.033] | | |
| *FFB input kton in year t-1* | | | | | | | | | | |
| Estimate | | | 0.017 | 0.053 | 0.008 | 0.026 | 0.037 | 0.003 | | |
| 95% CI | | | [-0.046; 0.080] | [-0.012; 0.118] | [-0.058; 0.073] | [-0.047; 0.099] | [-0.046; 0.120] | [-0.085; 0.091] | | |
| *FFB input kton in year t-2* | | | | | | | | | | |
| Estimate | | | | | -0.027 | 0.018 | -0.001 | -0.009 | | |
| 95% CI | | | | | [-0.091; 0.036] | [-0.060; 0.096] | [-0.080; 0.078] | [-0.111; 0.092] | | |
| *FFB input kton in year t-3* | | | | | | | | | | |
| Estimate | | | | | | | -0.084 | -0.041 | | |
| 95% CI | | | | | | | [-0.196; 0.027] | [-0.172; 0.090] | | |
| *FFB input kton, 4 past year average* | | | | | | | | | | |
| Estimate | | | | | | | | | -0.056 | -0.112 |
| 95% CI | | | | | | | | | [-0.157; 0.046] | [-0.307; 0.083] |
| **Fixed-effects:** | | | | | | | | | | |
| District-year | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mill | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Observations | 3,150 | 3,150 | 2,212 | 2,212 | 1,603 | 1,603 | 1,179 | 1,179 | 1,179 | 1,179 |
| Clusters | 340 | 340 | 300 | 300 | 270 | 270 | 241 | 241 | 241 | 241 |

NOTE. This table shows estimation results of ten regressions of the mill-gate price of the crude palm oil (CPO) output of mills on contemporaneous and past (lagged) fresh fruit bunches (FFB) inputs, in thousand tons (kton). The unit of observation is the mill-year. All models are estimated by ordinary least squares (OLS) with district-year fixed effects and even models additionally have mill fixed effects.
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Table A.5: Mill-level regressions of the log of crude palm oil output prices on fresh fruit bunches input volumes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Log of CPO output price | | | | | | | | | |
| **Regression coefficient of:** | | | | | | | | | | |
| *FFB input kton in year t* | | | | | | | | | | |
| Estimate | 3.31e-5 | 0.0001 | -4.64e-6 | 9.54e-5 | 3.02e-5 | 7.28e-6 | -1.83e-5 | -8.86e-5 | | |
| 95% CI | [-5.32e-5; 0.0001] | [-7.2e-6; 0.0002] | [-0.0001; 0.0001] | [-1.71e-5; 0.0002] | [-9.74e-5; 0.0002] | [-0.0001; 0.0001] | [-0.0001; 0.0001] | [-0.0002; 6.61e-5] | | |
| *FFB input kton in year t-1* | | | | | | | | | | |
| Estimate | | | 1.78e-5 | 9.12e-5 | 9.01e-6 | 3.67e-5 | 4.99e-5 | 9.86e-7 | | |
| 95% CI | | | [-8.64e-5; 0.0001] | [-1.12e-5; 0.0002] | [-0.0001; 0.0001] | [-7.71e-5; 0.0002] | [-9.17e-5; 0.0002] | [-0.0001; 0.0001] | | |
| *FFB input kton in year t-2* | | | | | | | | | | |
| Estimate | | | | | -6.52e-5 | 1.53e-5 | -1.29e-5 | -3.29e-5 | | |
| 95% CI | | | | | [-0.0002; 4.62e-5] | [-0.0001; 0.0001] | [-0.0002; 0.0001] | [-0.0002; 0.0001] | | |
| *FFB input kton in year t-3* | | | | | | | | | | |
| Estimate | | | | | | | -0.0001 | -5.02e-5 | | |
| 95% CI | | | | | | | [-0.0003; 6.2e-5] | [-0.0003; 0.0002] | | |
| *FFB input kton, 4 past year average* | | | | | | | | | | |
| Estimate | | | | | | | | | -9.87e-5 | -0.0002 |
| 95% CI | | | | | | | | | [-0.0003; 7.8e-5] | [-0.0005; 0.0001] |
| **Fixed-effects:** | | | | | | | | | | |
| District-year | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mill | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Observations | 3,150 | 3,150 | 2,212 | 2,212 | 1,603 | 1,603 | 1,179 | 1,179 | 1,179 | 1,179 |
| Clusters | 340 | 340 | 300 | 300 | 270 | 270 | 241 | 241 | 241 | 241 |

NOTE. This table shows estimation results of ten regressions of the mill-gate price of the crude palm oil (CPO) output of mills, taken to the natural logarithm, on contemporaneous and past (lagged) fresh fruit bunches (FFB) inputs, in thousand tons (kton). The unit of observation is the mill-year. All models are estimated by ordinary least squares (OLS) with district-year fixed effects and even models additionally have mill fixed effects.

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Table A.6: Crude palm oil price elasticity of deforestation for industrial plantations, by conversion dynamics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | Industrial plantations | | | | | |
| | Immediate conversion | | | Transitional conversion | | |
| | Legal | Illegal | All | Legal | Illegal | All |
| **Elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | -4.25 | 7.96 | -1.6 | -1.55 | 5.56 | 1.26 |
| 95% CI | [-7.37; -1.14] | [2.1; 13.82] | [-4.91; 1.7] | [-4.71; 1.6] | [2.07; 9.04] | [-1; 3.52] |
| Observations | 9386 | 6229 | 29749 | 11478 | 9032 | 36347 |
| Clusters | 356 | 268 | 706 | 367 | 354 | 817 |

NOTE. This table shows our estimates of the price elasticity of deforestation in industrial oil palm plantations. They are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate immediate from transitional conversion based on the time lapse between forest loss and plantation development. The cut-off point is 4 years. We can observe transitional conversion only up to 2010. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

Table A.7: Short- and medium-run crude palm oil price elasticity of deforestation across plantation types

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Elasticity to:** | | | | | | |
| *Short-run price signal* | | | | | | |
| Estimate | 0.643 | 1.557 | 1.072 | 0.345 | 0.889 | 0.773 |
| 95% CI | [-0.207; 1.493] | [0.687; 2.427] | [0.571; 1.573] | [-0.224; 0.914] | [0.392; 1.387] | [0.379; 1.168] |
| *Medium-run price signal* | | | | | | |
| Estimate | 0.378 | 2.315 | 0.751 | 1.05 | 1.349 | 0.77 |
| 95% CI | [-1.003; 1.759] | [-0.125; 4.755] | [-0.469; 1.971] | [0.16; 1.94] | [0.234; 2.464] | [-0.131; 1.671] |
| **Interaction** | | | | | | |
| Estimate | 0.029 | 0.029 | 0.029 | 0.035 | 0.03 | 0.027 |
| 95% CI | [-0.004; 0.061] | [-0.03; 0.089] | [0.002; 0.056] | [-0.002; 0.072] | [0.004; 0.055] | [0.008; 0.047] |
| Observations | 20532 | 18801 | 65368 | 20721 | 28505 | 71926 |
| Clusters | 533 | 523 | 1143 | 529 | 927 | 1441 |

NOTE. This table shows our estimates of the short- and medium-run crude palm oil (CPO) price elasticity of deforestation. They are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The short-run price signal is measured as the inverse-distance weighted average of CPO prices at the gates of reachable mills. The medium-run price signal is the 4-year average of short-run price signals. The last block of rows shows estimates of the partial effects of the interaction of both, evaluated at the sample mean. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

Table A.8: Cumulative and annual crude palm oil price elasticity of deforestation across plantation types

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Cumulative elasticity to:** | | | | | | |
| *Annual CPO price signals* | | | | | | |
| Estimate | 0.96 | 3.91 | 1.94 | 1.47 | 2.24 | 1.51 |
| 95% CI | [-0.77; 2.68] | [1.41; 6.41] | [0.61; 3.27] | [0.39; 2.54] | [0.96; 3.52] | [0.5; 2.53] |
| **Annual elasticity to:** | | | | | | |
| *CPO price signal in t* | | | | | | |
| Estimate | 0.51 | 1.58 | 1.03 | 0.35 | 0.9 | 0.73 |
| 95% CI | [-0.28; 1.3] | [0.66; 2.5] | [0.54; 1.51] | [-0.23; 0.93] | [0.39; 1.41] | [0.34; 1.13] |
| *CPO price signal in t-1* | | | | | | |
| Estimate | 0.22 | 1.2 | 0.75 | 0.21 | 0.68 | 0.62 |
| 95% CI | [-0.34; 0.77] | [0.34; 2.06] | [0.29; 1.21] | [-0.34; 0.76] | [0.21; 1.15] | [0.27; 0.98] |
| *CPO price signal in t-2* | | | | | | |
| Estimate | 0.13 | 0.68 | 0.31 | 0.38 | 0.44 | 0.29 |
| 95% CI | [-0.55; 0.81] | [-0.26; 1.62] | [-0.2; 0.82] | [-0.06; 0.81] | [-0.01; 0.89] | [-0.11; 0.68] |
| *CPO price signal in t-3* | | | | | | |
| Estimate | 0.1 | 0.45 | -0.14 | 0.53 | 0.22 | -0.13 |
| 95% CI | [-0.63; 0.82] | [-0.42; 1.33] | [-0.64; 0.35] | [0.1; 0.96] | [-0.24; 0.68] | [-0.51; 0.24] |
| Observations | 20532 | 18801 | 65368 | 20721 | 28505 | 71926 |
| Clusters | 533 | 523 | 1143 | 529 | 927 | 1441 |

NOTE. This table shows the elasticities of deforestation to the contemporaneous price signal and to the price signals in the three past years. The four elasticities are estimated jointly. The cumulative elasticity is the sum of these four elasticities. Price elasticity estimates are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The annual price signal is measured as the inverse-distance weighted average of crude palm oil prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

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Table A.9: Effects of crude palm oil price variability on deforestation across plantation types

| Deforestation for: | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Average Partial Effect of:** | | | | | | |
| *CPO price signal variability* | | | | | | |
| Estimate | -0.17 | -0.36 | -0.25 | 0.1 | 0.01 | -0.11 |
| 95% CI | [-0.38; 0.05] | [-0.75; 0.03] | [-0.45; -0.04] | [-0.07; 0.28] | [-0.17; 0.19] | [-0.26; 0.04] |
| Observations | 20532 | 18801 | 65368 | 20721 | 28505 | 71926 |
| Clusters | 533 | 523 | 1143 | 529 | 927 | 1441 |

NOTE. This table shows points of percentage change in average deforestation associated with a change in the standard deviation of annual price signals in the 4 previous years. Annual price signals are measured as the inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as forest loss outside the 2000 primary forest extent, eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

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Table A.10: Crude palm oil price semi-elasticity of deforestation across plantation types

| Deforestation for: | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Semi-elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | 0.12 | 0.63 | 0.3 | 0.21 | 0.36 | 0.25 |
| 95% CI | [-0.18; 0.42] | [0.21; 1.05] | [0.06; 0.53] | [0.04; 0.38] | [0.16; 0.57] | [0.08; 0.41] |
| Observations | 20532 | 18801 | 65368 | 20721 | 28505 | 71926 |
| Clusters | 533 | 523 | 1143 | 529 | 927 | 1441 |

NOTE. This table shows estimates of the price semi-elasticity of deforestation. They are to be interpreted as points of percentage change in average deforestation associated with a USD 1 increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

Table A.11: Crude palm oil price elasticity of deforestation for all plantations, conditional on past deforestation

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | All plantations | | | | | |
| | Sample period: 2006-2014 | | | Sample period: 2009-2014 | | |
| **Elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | 2.58 | 2.57 | 2.61 | 3.27 | 3.26 | 3.31 |
| 95% CI | [1.22; 3.94] | [1.22; 3.93] | [1.25; 3.97] | [1.84; 4.69] | [1.84; 4.67] | [1.88; 4.74] |
| **Conditional on deforestation:** | | | | | | |
| *the year preceding price signal* | | | | | | |
| In plantation site i | X | | | | | |
| In i's 8 neighbors | | X | | | | |
| *the four years preceding price signal* | | | | | | |
| In plantation site i | | | | X | | |
| In i's 8 neighbors | | | | | X | |
| Observations | 53926 | 53926 | 53926 | 38743 | 38743 | 38743 |
| Clusters | 1380 | 1380 | 1380 | 1272 | 1272 | 1272 |

NOTE. This table shows estimates of the crude palm oil (CPO) price elasticity of deforestation, conditional on different measurements of past deforestation, as well as not conditional on past deforestation but with the same time period restrictions. Estimates are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of CPO prices at the gates of reachable mills. Current deforestation, the outcome variable, is measured as primary forest loss eventually replaced with both industrial and smallholder oil palm plantations, without distinction of legality. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\text{km}$ grid cells in Sumatra and Kalimantan. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.
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Table A.12: Crude palm oil price elasticity of deforestation across plantation types, moderated by local market development

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | -0.33 | 3.87 | 0.48 | 0.31 | 1.96 | 0.59 |
| 95% CI | [-1.76; 1.11] | [0.98; 6.75] | [-0.8; 1.76] | [-1.29; 1.91] | [0.43; 3.49] | [-0.44; 1.62] |
| **Regression coefficient of:** | | | | | | |
| *# reachable mills* | | | | | | |
| Estimate | 0.287 | 0.185 | 0.083 | -0.008 | 0.038 | -0.008 |
| 95% CI | [-0.070; 0.645] | [-0.317; 0.687] | [-0.191; 0.357] | [-0.340; 0.323] | [-0.232; 0.307] | [-0.233; 0.217] |
| Observations | 23038 | 25040 | 79348 | 34873 | 35651 | 84194 |
| Clusters | 881 | 1314 | 2392 | 1510 | 1709 | 2593 |

NOTE. This table shows our estimates of the price elasticity of deforestation, along with estimated coefficients of interactions between the price signal and the annual count of all known reachable mills. Price elasticity is estimated as the total partial effect of price signal and therefore accounts for the interaction coefficients. It is to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\text{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.

Table A.13: Crude palm oil price elasticity of deforestation across plantation types, by island

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Deforestation for:** | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Sumatra elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | 3.02 | 4.9 | 2.86 | 1.39 | 2.42 | 1.77 |
| 95% CI | [0.06; 5.98] | [1.54; 8.26] | [0.87; 4.86] | [0.19; 2.58] | [0.93; 3.92] | [0.47; 3.07] |
| Observations | 5305 | 8556 | 26873 | 16050 | 17136 | 32443 |
| Clusters | 208 | 352 | 680 | 512 | 743 | 972 |
| **Kalimantan elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | -0.04 | 1.74 | 0.92 | -6.16 | 1.9 | 0.97 |
| 95% CI | [-2.24; 2.15] | [-2.93; 6.41] | [-1.23; 3.07] | [-34.53; 22.22] | [-2.38; 6.18] | [-1.15; 3.08] |
| Observations | 15219 | 10245 | 38087 | 4427 | 11281 | 38941 |
| Clusters | 325 | 171 | 465 | 17 | 184 | 472 |

NOTE. For legal and illegal industrial plantations in Kalimantan the GLM algorithm did not converge, even with high number of iterations. Estimates are presented for informative purpose but should be taken with caution.
This table shows our estimates of the price elasticity of deforestation by island. They are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as primary forest loss eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining primary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.
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Table A.14: Crude palm oil price elasticity of secondary forest loss across plantation types

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Secondary forest loss for: | Industrial plantations | | | Smallholder plantations | Unregulated plantations | All |
| | Legal | Illegal | All | | | |
| **Elasticity to:** | | | | | | |
| *CPO price signal* | | | | | | |
| Estimate | 0.5 | 0.53 | 0.28 | -0.61 | -0.02 | 0.15 |
| 95% CI | [-1.23; 2.22] | [-2.3; 3.36] | [-0.88; 1.43] | [-1.28; 0.06] | [-0.88; 0.84] | [-0.72; 1.01] |
| Observations | 36269 | 25336 | 125797 | 72517 | 60862 | 139870 |
| Clusters | 942 | 792 | 2508 | 1669 | 2175 | 3212 |

NOTE. This table shows our estimates of the price elasticity of deforestation in secondary forest. They are to be interpreted as points of percentage change in average deforestation associated with a 1% increase in price signals. The price signal is measured as the 4-year average of annual inverse-distance weighted averages of crude palm oil prices at the gates of reachable mills. Deforestation is measured as forest loss outside the 2000 primary forest extent, eventually replaced with oil palm plantations. We differentiate industrial from smallholder plantations based on scale and landscape criteria. We consider industrial plantations legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation. Information about the legal status is not always available. All estimates are derived from a generalized linear model of the quasi-Poisson family. All regressions include (geo-localized IBS) reachable-mills and district-year fixed effects, as well as the annual count of all known reachable mills as covariate. Sample observations are annual records of $3 \times 3\,\mathrm{km}$ grid cells in Sumatra and Kalimantan from 2002 to 2014. They all have a positive extent of remaining secondary forest, and are within a 50km (30km in Sumatra) radius from at least one of our sample mills. 95% confidence intervals (CI) are based on standard errors computed with the delta method and clustered at the set of reachable mills.
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# B Figures

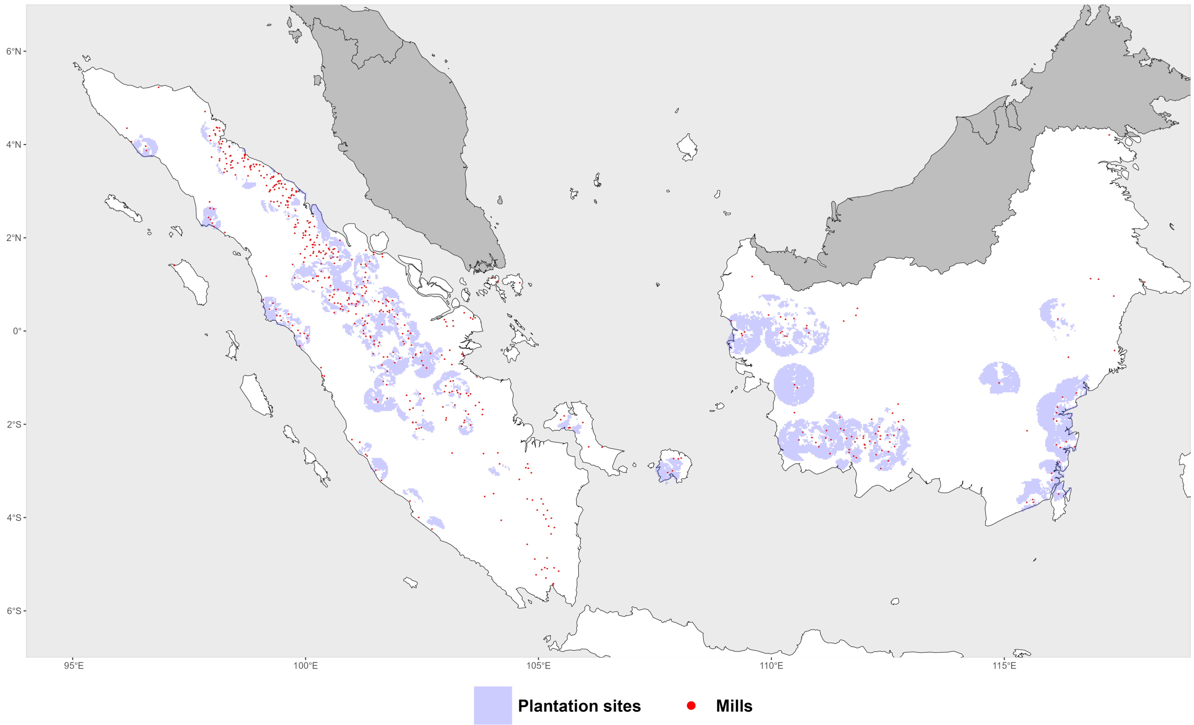


Figure B.1: Palm oil mills and plantation sites in the analysis

NOTE. This figure maps the samples of palm oil mills (red dots) and plantations (light blue area) used in the analysis to estimate the price elasticity of deforestation. The geographical area highlighted in white includes the Indonesian islands of Sumatra (left) and Kalimantan (right).

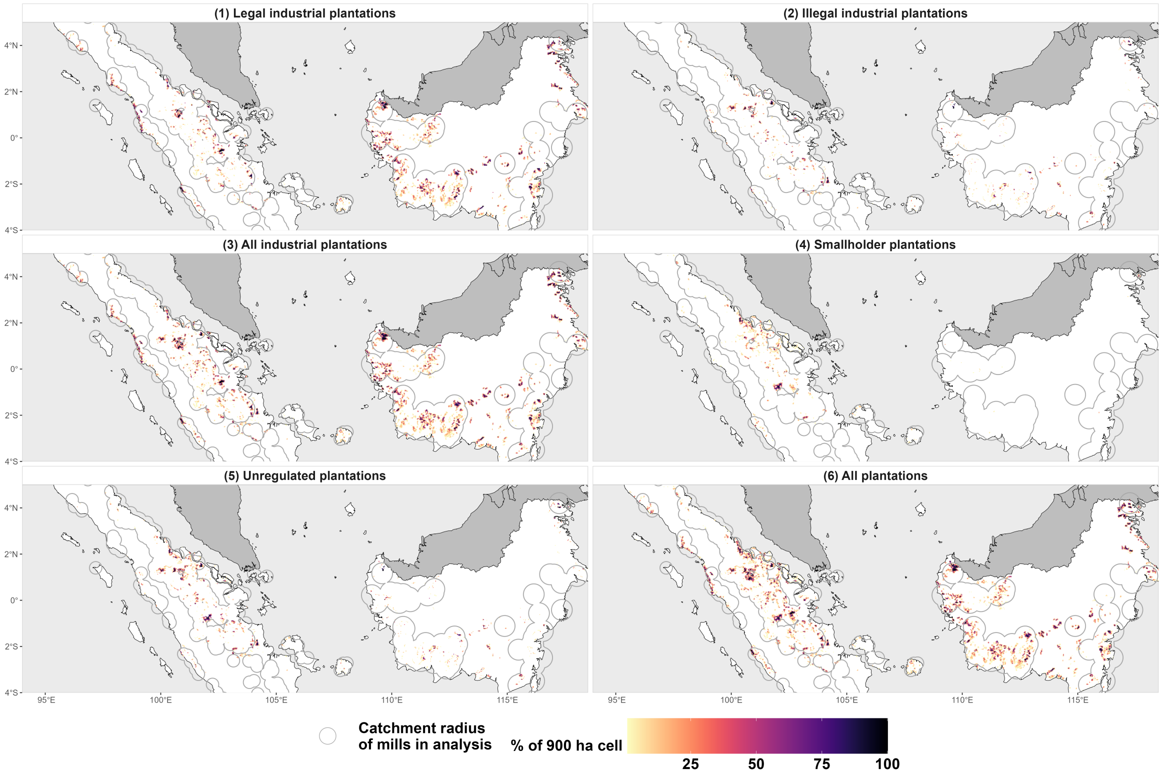


Figure B.2: Cumulative oil palm deforestation by plantation type

NOTE. The geographical area highlighted in white includes the Indonesian islands of Sumatra (left) and Kalimantan (right). Cumulative deforestation is measured as primary forest loss from 2002 to 2014 and eventually replaced with industrial oil palm plantations. Industrial and smallholder plantations are differentiated by scale and landscape criteria. Industrial plantations are considered legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation.

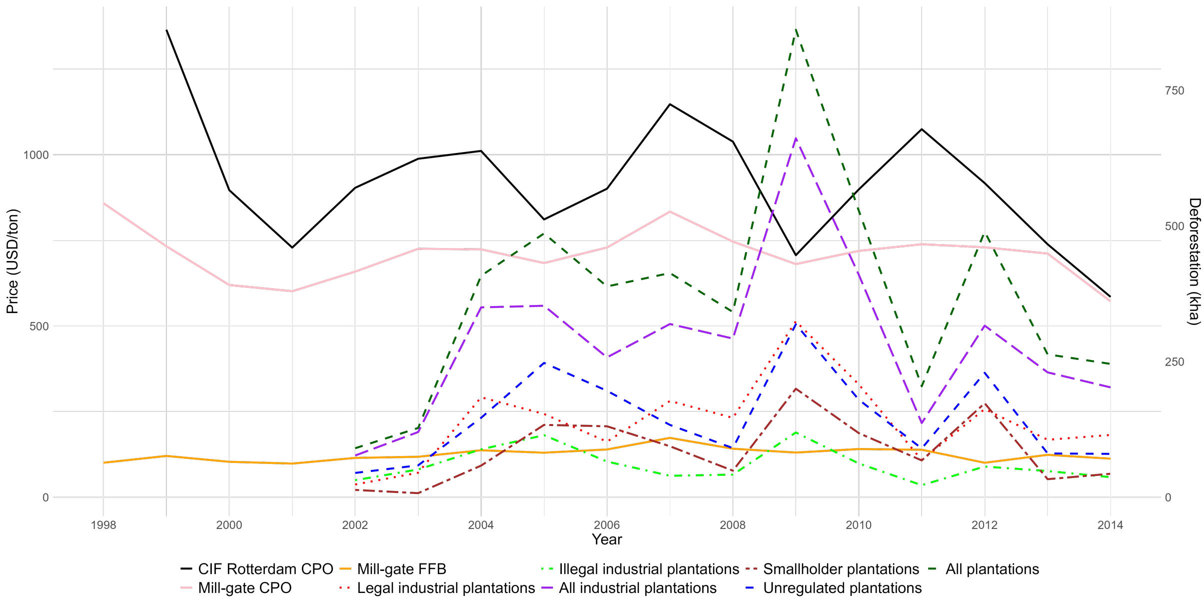


Figure B.3: Time series of prices and deforestation

NOTE. The deforestation time series are in the samples used in the main analyses to estimate the crude palm oil price elasticity of deforestation for different plantation types. Deforestation is measured as primary forest loss from 2002 to 2014 and eventually replaced with industrial oil palm plantations. Industrial and smallholder plantations are differentiated by scale and landscape criteria. Industrial plantations are considered legal if they are within a known oil palm concession, and illegal if they are outside a concession and inside a permanent forest zone designation.

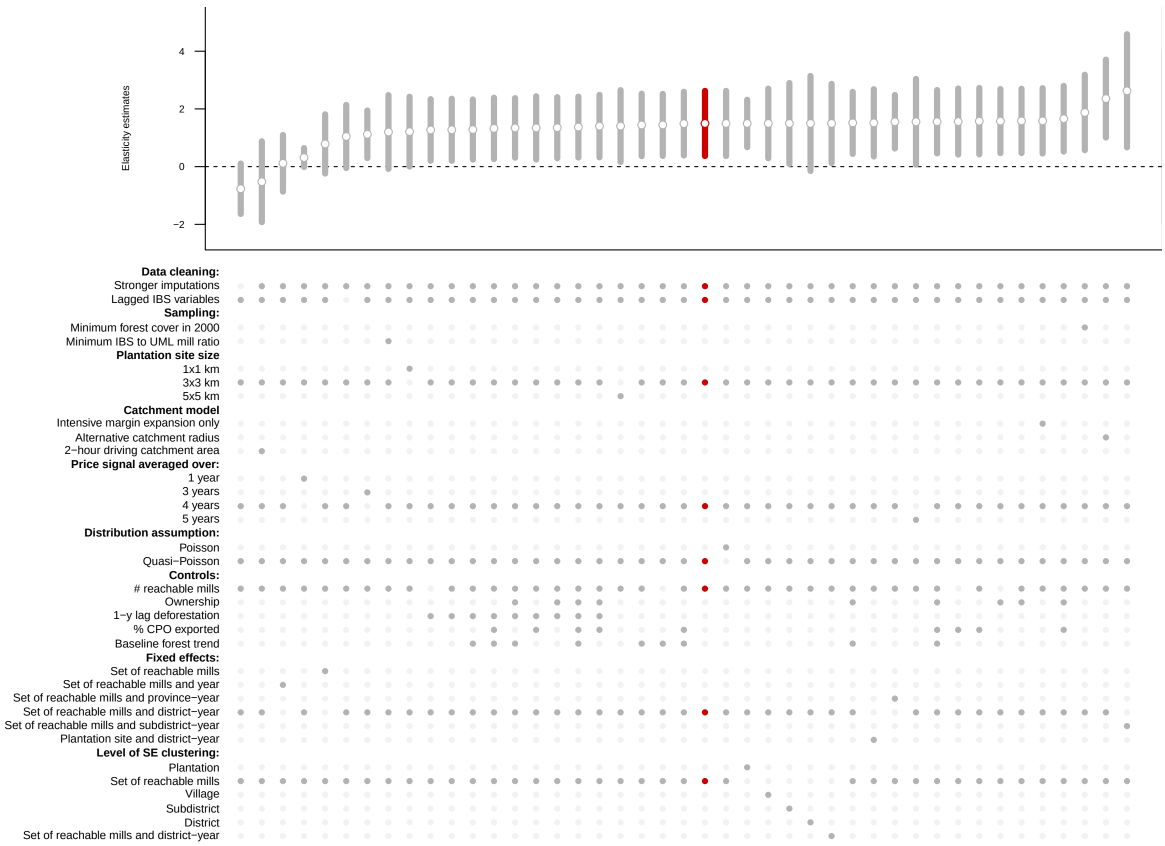


Figure B.4: Estimates of the crude palm oil price elasticity of deforestation for all plantations under different specifications

NOTE. This figure shows point estimates (white dots in upper panel) of the overall Indonesian price elasticity of deforestation estimated in this paper. Grey bars in the upper panel represent 95% confidence intervals. Darker marks in the lower panel mean that the corresponding vertical estimate is derived from a model that has the corresponding horizontal feature. The main specification is highlighted.
The minimum forest cover in 2000 is 50%. The IBS to UML mill ratio designates the number of mills from our sample relative to the total number of known reachable mills. It is also set to 50% (included). Alternative catchment radius is 50km in Sumatra and 30km in Kalimantan.
We are grateful to Ariel Ortiz-Bobea for providing the code to make this chart openly at: `https://github.com/ArielOrtizBobea/spec_chart/blob/master/spec_chart_function.R`

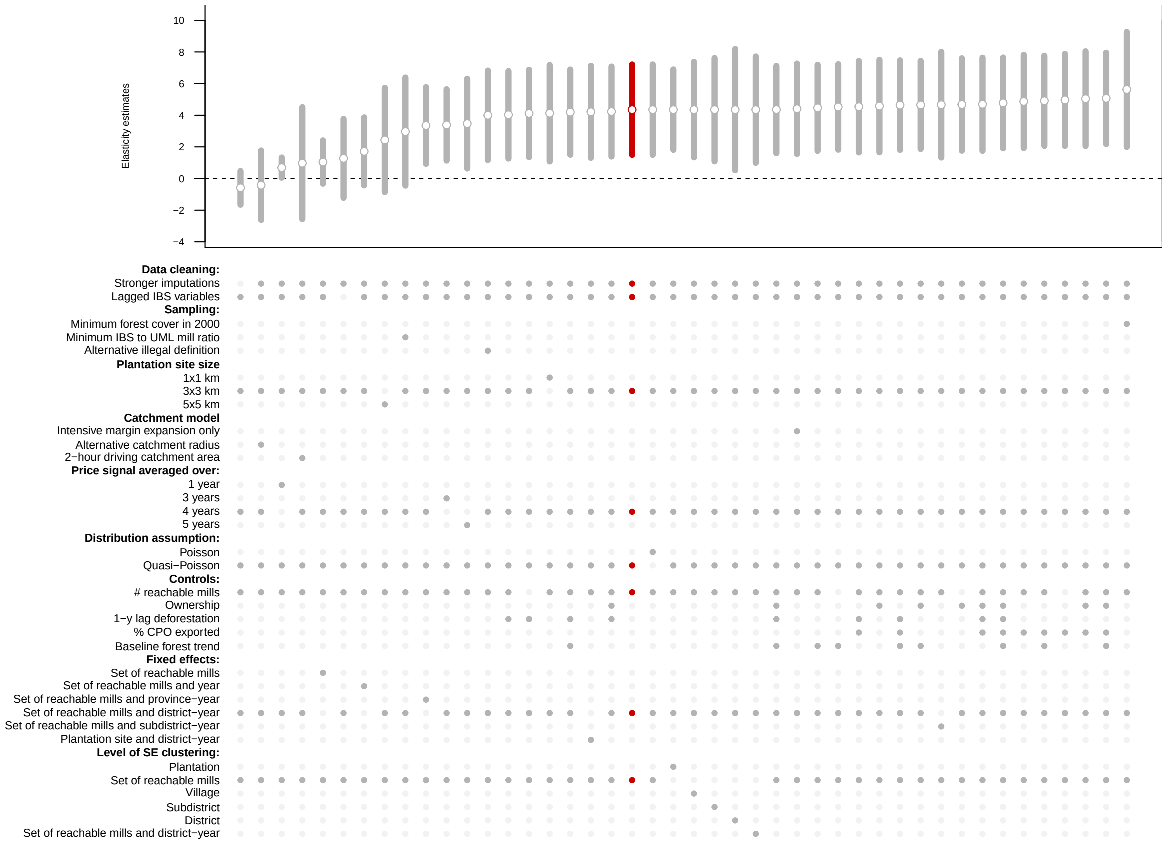


Figure B.5: Estimates of the crude palm oil price elasticity of deforestation for illegal industrial plantations under different specifications

NOTE. This figure shows point estimates (white dots in upper panel) of the price elasticity of deforestation for illegal industrial plantations estimated in this paper. Grey bars in the upper panel represent 95% confidence intervals. Darker marks in the lower panel mean that the corresponding vertical estimate is derived from a model that has the corresponding horizontal feature. The main specification is highlighted.
The minimum forest cover in 2000 is 50%. The IBS to UML mill ratio designates the number of mills from our sample relative to the total number of known reachable mills. It is also set to 50% (included). Alternative catchment radius is 50km in Sumatra and 30km in Kalimantan.
We are grateful to Ariel Ortiz-Bobea for providing the code to make this chart openly at: `https://github.com/ArielOrtizBobea/spec_chart/blob/master/spec_chart_function.R`

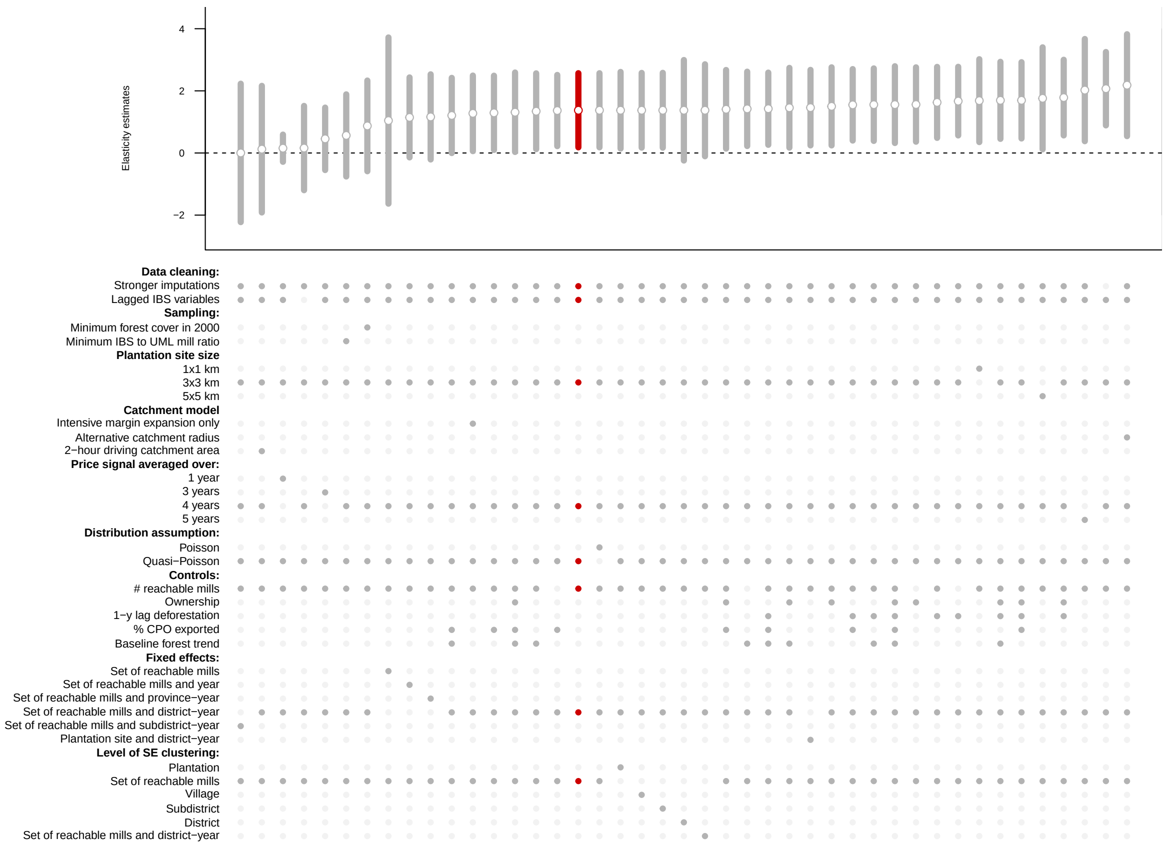


Figure B.6: Estimates of the crude palm oil price elasticity of deforestation for smallholder plantations under different specifications

NOTE. This figure shows point estimates (white dots in upper panel) of the Indonesian price elasticity of deforestation for smallholder plantations estimated in this paper. Grey bars in the upper panel represent 95% confidence intervals. Darker marks in the lower panel mean that the corresponding vertical estimate is derived from a model that has the corresponding horizontal feature. The main specification is highlighted.
The minimum forest cover in 2000 is 50%. The IBS to UML mill ratio designates the number of mills from our sample relative to the total number of known reachable mills. It is also set to 50% (included). Alternative catchment radius is 50km in Sumatra and 30km in Kalimantan.
We are grateful to Ariel Ortiz-Bobea for providing the code to make this chart openly at: `https://github.com/ArielOrtizBobea/spec_chart/blob/master/spec_chart_function.R`

# C Data sample and measurements

This complements the homonym section in the main text and gives additional details and explanations about the sampling strategy, data and measurements. The first subsection documents our original micro-economic dataset of geo-localized palm oil mills.

## C.1 Data sample

In the main estimations, the sample we use is restricted in several dimensions. First, we limit observations to Sumatra and Kalimantan from 2002 to 2014. We do not include observations from other Indonesian islands because of data scarcity. In Papua, we have very few observations and in other islands, data on oil palm plantation extents are lacking. We do not include observations of deforestation for industrial plantations in 2015, although these are available, in order to use a panel of the same length as on deforestation for smallholder plantations which is observable until 2014. We exclude observations before 2002 as we observe 4-year average price signals starting in 2002.

Second, we include only observations of grid cells from years when at least one geo-localized mill from the manufacturing census is reachable. We note that it is common that mills need a minimal fruit supply basis to operate. This implies that at usual mill capacity and plantation yield, a minimum plantation size of ca. 3000 hectares is developed alongside any new mill opening (Paoli et al. 2013). Because of the lag between planting and harvesting, deforestation for these integrated plantations occurs before the mill starts operating. Such deforestation does probably not occur because of price signals from reachable mills already operating. In a robustness check, we find similar results after excluding plantation sites in the direct vicinity (3000 hectares) of a mill before it starts operating (Appendix F.1).

Third, we remove plantation sites within RSPO certified concessions, as we expect both price and deforestation dynamics to differ systematically in these plantation sites, including before certification procurement. RSPO stands for the Roundtable on Sustainable Palm Oil, the main voluntary certification scheme for palm oil. Few certifications were issued in the first years of the RSPO, from 2009 to 2014. Thus, this sampling exclusion applies to very few observations during our study period. We observe certified areas using data from Carlson et al. (2018). NDPE (No Deforestation No Peat No Exploitation), another supply chain initiative, was not already implemented during our study period.

Fourth, we remove annual records as soon as one of the variables in Equation 2 has a missing value. For the price signal variable, this is the case if no reachable mill reported both the volume and monetary value of its sales in one of the four previous years, or if they did but we removed them in the data cleaning step (Appendix C.3.1). In our case, this has a particular influence on the final sample, because the likelihood that a price signal value is missing decreases with the number of reachable mills. Thus, removing observations with missing values implies that we tend to sample fewer grid cells in remote areas. Another particular implication of removing observations with missing values in our case is that we do not sample records of grid cells in the first 4 years after the first reachable mill is established (as our main, medium-run, price signal measure runs over 4 years). Table A.2 shows this removes about 10000 grid cells from the final sample, making a significant difference for the distribution of deforestation, the average price signal, and for the number of reachable mills, in particular. This is not surprising, since inclusion in the sample is a function of the number of reachable mills. We argue that this necessary sampling step does not risk introducing a selection bias, as we control in our regressions precisely for the criterion behind it: the number of reachable mills.

Finally, our quasi-Poisson estimation procedure (see Section 4.1) removes observations from clusters in the fixed effect dimensions that have a constantly null outcome (i.e. no deforestation during our study period). This naturally mitigates zero-inflation.

## C.2 Measurements of deforestation for oil palm plantations

In this section, we explain how we construct our measures of land use change from forest to oil palm plantation (referred to as 'deforestation' here).[39]

**Forest loss.** We use maps from the Global Forest Change (GFC) dataset (Hansen et al. 2013). They cover the whole of Indonesia with a resolution of 1 arc-second per pixel (i.e. 27.8 x 27.6 meter pixels with our projection) annually from 2001 to 2018. A forest loss event is defined at the pixel level, as the year when complete removal of tree canopy cover (with a minimum height of 5m) is observed where such cover was still present in 2000. A minimum canopy cover threshold defines what is counted as forest in 2000 at the pixel level. This approach does not count forest

[39] All rasters used in this study are aligned with the resolution of forest loss maps from Hansen et al. (2013) and all spatial data are projected with a Cylindrical Equal Area projection centered on Indonesia (longitude = 115, latitude = 0).

degradation as deforestation, because the tree loss pixel-event is counted only once, the year a near-zero canopy closure is observed Hansen et al. (2013). Moreover, the GFC dataset does not enable us to distinguish between 2000 tree canopy cover (and hence loss) in primary forest, secondary forest, or tree plantations.

**Primary forest extent in 2000.** The map we use to measure primary forest extent in 2000 comes from Margono et al. (2014). It covers the whole country, with the same resolution as the GFC data set. Primary forest in 2000 is a subset of the 2000 tree canopy cover from the GFC data set, with canopy cover of at least 30%. It is defined as "mature natural forest cover that has not been completely cleared in recent history and consisted of a contiguous block of 5ha or more" (Margono et al. 2014). Two primary forest types are distinguished: intact and degraded. The former, following Potapov et al. (2008), shows no sign of alteration by humans, while the second has been subjected to human disturbances, such as selective logging. They correspond to the Indonesian Ministry of Forestry's primary and secondary forest cover types (Margono et al. 2014). In this study, we regroup them.

In Table A.14 we present alternative results for secondary forest loss. We define secondary forest in 2000, at the pixel level, as tree canopy cover of at least 30 percent, outside primary forest, and notably outside 2000 industrial oil palm plantations (as observed by Austin et al. (2017)).[40]

**Oil palm plantations.** In this study, we use two different maps, from Austin et al. (2017) and Petersen et al. (2016). These maps have been produced by visual interpretation of Landsat imagery. They both recognize areas with signs of future cultivation as plantations. The former product, from Austin et al. (2017), includes only large-scale oil palm plantations and covers the regions of Sumatra, Kalimantan, and Papua for the years 1995, 2000, 2005, 2010 and 2015, with a 250m pixel resolution. The latter product, from Petersen et al. (2016), includes and distinguishes between large plantations of more than 100ha, mid-size plantations and small-size plantations. It is a snapshot of the whole of Indonesia, computed with images from 2013 and 2014. Mid and small-size plantations are mosaic landscapes. Mid-size plantation mosaic landscapes are

[40] This ensures that canopy closure removals within already existing plantations (i.e., palm replacements) are not counted as deforestation. This approach is the best we can do in the absence of other tree plantation maps for 2000, but it still has some pitfalls. For instance, if an area was covered with another plantation crop (like timber) in 2000, cleared and converted to an oil palm plantation before 2015, it would be mistakenly counted as deforestation.

at least 100 hectares wide, have oil palm patches between 10 and 100 hectares, comprising at least 50% of the landscape. Small-size plantation mosaic landscapes have oil palm patches smaller than 10 hectares, again comprising at least 50% of the landscape. In our main analysis, we use the maps from Austin et al. (2017) to study industrial plantations, and we pool small and mid-sized plantation maps from Petersen et al. (2016) to study smallholder plantations. Where these map sources overlap, we characterize plantations as industrial, as remote sensing for this landscape is less error-prone.

**Measuring deforestation.** We combine these data sets to compute annual maps of deforestation for oil palm plantations. Our main forest definition at the pixel level, hence determining our baseline forest extent in 2000, is any (i.e., intact or degraded) primary forest.[41] This corresponds to the official forest definition by the Government of Indonesia (MoF 2008; Austin et al. 2017) which justifies that this is retained in our main analysis.

Then, annual (primary or secondary) forest loss pixel events observed within the 2000 baseline forest extent are deemed deforestation events if they later fall within an oil palm plantation. This means that we count a deforestation pixel-event the year the forest is cleared, and not the year the palm trees are planted or when they become productive. Both plantation data sets recognize areas with signs of future cultivation as oil palm plantations. Hence, we can observe deforestation up to 2014, the latest common year for both industrial and smallholder plantations. Since the first year in our sample for analysis is 2002, this sequence allows a time laps of up to 12 years between forest clearing and detectable productive palm trees.

**Measuring illegal deforestation.** To measure illegal deforestation we overlay a map of oil palm concessions and a map of legal land use designation. To minimize the commission errors due to the data incompleteness, we impose a combination of conditions: we deem deforestation as illegal if it occurs outside a known concession *and* inside a forest zone designation where oil palm cultivation is forbidden. The latter is any of Permanent Production Forest (*Hutan Produksi Tetap*), Limited Production Forest (*Hutan Produksi Terbatas*), Conservation Forest (*Hutan Konservasi*) or Protection Forest (*Hutan Lindung*). Concretely, those umbrella groups comprise the following classes in our data: HL, HP, HPT, HK, KSA, KPA, KSAL, CA, SM, TN, TWA, Tahura,

[41]We routinely exclude pixels categorized as industrial plantations in 2000, although the primary forest map should already exclude them.

KSAL, KPAL CAL, SML, TNL, TWAL, TB and Hutan Cadagan. See (MoF 2019) for definitions of specific acronyms.

A further issue is that both maps are composite snapshots — they do not specify the date the concessions were issued, nor changes in land designation. Different settings could thus lead to commission error in observing illegal deforestation. Where the land designation snapshot precedes a reclassification into convertible forest, or the concession snapshot precedes a concession issuance, we could incorrectly deem deforestation occurring afterwards as illegal. Such commission error in illegal deforestation due to the snapshots being outdated is probably limited though. The land designation and the concession maps are based on 2010 official data and the Moratorium on new concessions in primary forest came into force in 2011. New concessions have been issued by local governments despite the moratorium, but this may be considered part of questionable legal processes, especially with respect to the central government (Enrici and Hubacek 2016). Moreover, alternatively using a map of concessions in 2020[42] to identify illegal deforestation shows that our results are robust to concession issuance post-2010 (Figure B.5). The opposite problem of too recent snapshots causing commission error in illegal deforestation is even more unlikely, as it would emerge from cases of land reclassification into protected forest, or concession revocation, after deforestation occurred.[43] Finally, note that too recent snapshots risk to yield higher commission errors in legal deforestation. For this reason, we do not use the 2020 concession map in our main analysis, and even with the 2010 concession map, our results on legal deforestation should be taken more cautiously.

**Immediate and transitional conversion.** For industrial plantations only, we can observe the time lapse between the forest loss event and the year when oil palms are observed for the first time in the half-decadal data from Austin et al. (2017). Conversion is deemed immediate if the time lapse is between 0 and 4 years. It is deemed transitional if the time lapse is between 5 and 12 years. We observe that immediate conversion represents two thirds of the deforestation we measure in 2002-2010 (when we can observe transitional conversion).

[42](*Greenpeace Kepo Hutan Public Downloads - Google Drive* 2023)

[43]Revocation is intended in cases of chronic non-compliance with the plantation development laws (Paoli et al. 2013). Moreover, there is no anecdotal support for significant oil palm concession revocation in the period of interest (Morel et al. 2016; Mongabay 2022).

## C.3 Measurements of price signals: a model of plantation-mill relationships

### C.3.1 A primary microeconomic panel of geolocalized palm oil mills

We semi-manually matched two existing data sets to produce an original, spatially explicit, microeconomic data set of palm oil mills in Indonesia from 1998 to 2015.

**Indonesian manufacturing census (IBS).** The Indonesian manufacturing census (IBS) is issued by the Indonesian office of statistics (BPS).[44] It reports annual establishment-level data for all manufacturing facilities employing at least 20 employees.[45] We identified palm oil mills with 9-digit commodity codes from 1998 to 2015. We use KKI codes 151410102 or 151410103 for crude palm oil and crude palm kernel oil respectively, and 011340101 or 011340501 for fresh fruit bunches. The variables available in the manufacturing census and used in our analysis are geographic variables;[46] mill-level input and output quantities and values at the 9-digit commodity level; mill-level ownership shares across four categories (national public, regional public, domestic private and foreign private); and product-level export shares.

**Mill-gate prices: definitions and cross-sectional variation.** We measure $P_{mt}$, the price received or paid by mill $m$ in year $t$ for CPO or FFB respectively, as the mean unitary value. The mean unitary value is the monetary value of a mill's CPO output, or FFB input, divided by the corresponding volume. It reflects the average price of transactions made in a year and reported in IBS. We express these mill-gate prices in 2010-constant USD/ton. In the study period, the mean mill-gate CPO price over geo-localized mills is 685 USD/ton, with a standard deviation of 172 USD/ton (A.3). The within year standard deviation of mill-gate CPO prices is 149 USD/ton. The within district-year variation is 138 USD/ton. Hence, most of the variation in mill-gate CPO prices is in the cross-section, between mills of the same district. Such violation of the law of one price has two explanations. The first one is consistent with our characterization of the CPO market: the multi-months off-take agreements between mills and their buyers imply that arbitrages are not completely eliminated from an annual perspective. This is furthered by the bargaining power of CPO buyers over mills and by the speculative nature of some of these pur-

[44] The data has also been referred to as *Statistik Industri* in the literature

[45] The average mill in IBS has 137 employees, and 75% of the mills have more than 87 employees. Thus, we are not worried that the 20-employee threshold is a threat in terms of selection bias.

[46] The data we obtained from BPS provided the district (*kabupaten*) information over the 1998-2015 period. However, the sub-district (kecamatan) and the village (desa) information were provided over 1998-2010 only.

chases. The second explanation is the presence of transport cost differentials between mills within the same district, which we control with the local-market fixed effects.

For FFB, the mean mill-gate price is 125 USD/ton, with a standard deviation of 36 USD/ton (A.3). The within year standard deviation is 30 USD/ton, and within province-year it is 27 USD/ton. This is in line with other observations in the literature (Section 2.1), that effective FFB prices depart from the ruled levels.

**Cleaning IBS data** We use two main routines to clean input and output quantity and value variables: we remove duplicates, and we remove outliers. For each routine, we construct two cleaned variables: one with the stronger imputations (suffixed "imp1"), and one with the weaker imputations (suffixed "imp2"). The one with the stronger imputations described a more modified sample, in an attempt to reduce statistical noise (the term "removed" means "is given a missing value" throughout the paragraph). For duplicates within a firm identifier, imp1-variables observations are removed if either quantity or value is duplicated. For imp2-variables, observations are removed only if both quantity and value are duplicated. For duplicates within a year, imp1- and imp2-variables observations are removed only if both quantity and value are duplicated.

We define statistical outliers as observations that, within a year, are higher than $p75+1.5iqr$ where $p75$ is the $75^{th}$ percentile value and $iqr$ is the interquartile range. We define outliers as observations of quantity variables that are statistical outliers and fail one of three tests. The first test asks whether the observation's input-output ratio is also a statistical outlier. The second test asks whether the observation's crude palm oil-palm kernel oil ratio is a statistical outlier. The third test asks whether an observation's variation rate with respect to the previous period is an outlier. This procedure allows us to use all available information to deem an observation an outlier. For value variables, this is not possible and we deem an observation an outlier as long as it is a statistical outlier within a year. We express all monetary values used in the analysis in 2010 USD. We then compute price variables as mean unitary values: the ratios of quantities and values. We finally remove observations whose price variables are either upper or lower statistical outliers. Removing price upper outliers removes observations whose quantity is mismeasured (too low) relative to value, or whose value is mismeasured (too high though not outlier) relative to a true small quantity. Removing price lower outliers removes observations whose value is mismeasured (too low) relative to quantity, or whose quantity is mismeasured (too high though not outlier) relative to a true small value.

In addition, we lag all variables from the Indonesian manufacturing census, including prices, by one year. This merely aims at correcting a measurement lag. We do this because remotely sensed annual deforestation does not necessarily represent the actual state at the end of the year, while IBS variables should, a priori, reflect census respondents' observations for the whole year. Because this does not have conceptual implications for our empirical strategy, we do not annotate these lags or refer to them further.

Finally, with these cleaned variables, we identified 930 plants as palm oil mills, based on the criteria that they sourced FFB at least once or sold CPO or PKO at least once, and that they are not located in Java or in Bali.

**Universal Mill List (UML).** In the latest version we use, the Universal Mill List features 1140 Indonesian palm oil mills, with their names and coordinates (UML 2018). We merge the UML with a newer data set of palm oil mills (Benedict et al. 2023), containing information on parent companies and establishment dates, but we further refer to the whole data set as the UML.

**Matching the manufacturing census and the UML.** We matched the palm oil mills from these two data sets to make the manufacturing census economic data spatially explicit. The matching strategy leverages a third document: the manufacturing directories. This is a list of manufacturing establishments, with their names, 5-digit industry codes, main commodity names, addresses (often incomplete), and number of workers. Although they are edited annually, we could find them only for years 2003, 2006, 2009-2015. Since the number of workers in the directories is sourced from the manufacturing census (although with many lags, leads, and inconsistencies between the two), we used this variable together with district (and village when available) information to match mills from the manufacturing census with manufacturing directories' names. These names were then used to match the manufacturing census mills with UML coordinates. All conflicts were resolved after a case-by-case investigation. Finally, we match 466 mills from the manufacturing census with a UML palm oil mill (and four more which never reported CPO or PKO output, nor FFB input, or are located in Java)

There are 464 palm oil mills from the manufacturing census that could not be matched with the UML by the method explained above. Out of these, we approximate the geo-localization of the 121 additional mills for which village information is reported in the manufacturing census. To do so, we use the centroids of the polygons of the most recent valid village identifier. Because, in Indonesia, since 2000, there is a trend to village splits rather than to village mergers, the most recent information also tends to be the most spatially accurate.[47]

### C.3.2 Differential mill influence and medium-run expectations

**The catchment radius parameter in the literature.** The existing literature helps us get a sense of magnitudes for catchment radii of palm oil mills. According to Harris et al. (2013), only 15.3% of oil palms are farther than $30\,\mathrm{km}$ from a mill. This study is based on Gunarso et al. (2013) for plantation data and Global Forest Watch for palm oil mill data, for Indonesia, Malaysia, and Papua New Guinea. 44.5% of oil palms are within $10\,\mathrm{km}$ of a mill, and 8.1% are farther than $50\,\mathrm{km}$. The Center for International Forestry Research (CIFOR), in its online atlas (`https://atlas.cifor.org/borneo/#en`) applies a $10\,\mathrm{km}$ buffer around mills. In Peninsular Malaysia, a region comparable to Sumatra, Shevade and Loboda (2019) report almost no deforestation due to oil palms beyond $40\,\mathrm{km}$ to a mill.

**Price signal formulas.** We calculate the annual price signal as:

$$Price_{it}^{annual} = \sum_{\mathbb{M}_{it}} \frac{d_{im}^{-1}}{\sum_{\mathbb{M}_{it}} d_{in}^{-1}} * P_{mt} \tag{3}$$

where each plantation site $i$ can reach a set of mills $\mathbb{M}_{it}$ in year $t$, each at a distance $d_{im}$ having a mill-gate price $P_{mt}$. Then, we calculate the annually perceived medium-run price signal as:

$$Price_{it}^{medium} = \frac{1}{4} \sum_{l=3}^{0} Price_{it-l}^{annual} \tag{4}$$

[47] Due to administrative village splits, plants do not necessarily report their correct village names or codes every year. This can be particularly misleading because codes for "parent" villages may be re-used in the next iteration but for different villages than their "child" villages. Therefore, we deemed that the village information a plant reported in a given year was valid if the corresponding "parent" village (in 2000) matched with the mode of all annual village information reported by the plant (also expressed in "parent" village).

# D Estimation and identification

## D.1 Estimation strategy

**Functional form and estimation** In this study, we estimate an exponential mean model by Poisson Quasi maximum likelihood. The Poisson distributional assumption has been made elsewhere in statistical studies of (Indonesian) deforestation (e.g., Burgess et al. (2012), Busch et al. (2012), and Busch et al. (2015)). Hence, we also seek comparability of our results with, in particular, Busch et al. (2015). The quasi-Poisson distribution imposes weaker assumptions on our data, as it only requires the mean (and not the variance) to be correctly specified. We use the standard log-link function. We estimate Equation 2 with the feglm algorithm from the R package *fixest*. This method estimates generalized linear models using weighted ordinary least squares (OLS) estimations with demeaning along fixed effect dimensions in the OLS steps and no presence of the incidental parameter problem (Bergé 2018).

## D.2 Partial effects

In all regressions, the price signal variable is scaled to the natural logarithm. The partial effects of price signals on deforestation are computed as the relative difference between predicted deforestation at the sample means, with and without a 1% increase in the price signal, multiplied by 100 (hence, all estimates are scaled to percentage points). From Equation 2, this simplifies to $100(1.01^{\hat{\alpha}} - 1)$% and hence does not depend on sample means (Barrera-Gomez and Basagana 2015). This only slightly differs from the exponential of regression coefficients as it gauges the effect for a "full" 1% change in a right-hand-side variable and not for an infinitesimal change. We present results this way because it is more consistent with computation of effects for larger changes (e.g., one standard deviation) or when second-order terms are included on the right-hand side. We estimate the variance of the partial effect with the delta method (Greene 2012).

To investigate synergies, we use interaction terms: right-hand-side variables computed as the product of the treatment (price signal here) and an interacting variable which is also featured in the right-hand side. Because our model is not linear, the informative estimate is the partial effect of the interaction term, not its coefficient (Ai and Norton 2003). Hence, interaction estimates discussed in Section 5 and displayed in Tables A.7, 4 and A.12 are second-order cross-derivatives of predicted deforestation, evaluated at the sample mean.

## D.3 Shift-share identification in a simplified regression

Here, we discuss how our identification strategy relates to the canonical shift-share setting in Borusyak et al. (2022, 2023). We proceed in three steps: first, we highlight the shift and the share components of our price signal treatment variable; second, we show and interpret how fixed effects isolate the exogenous variation in price signal in a simplified regression framework more akin to the canonical setting; third, we discuss how our main setting departs from the canonical one.

**Price signal as a shift-share treatment.** To see the shift and the share components in the price signal treatment variable, let us rewrite Equation 3 as:

$$Price^{short}_{it\omega} = \sum_m \frac{d^{-1}_{im}}{\sum_{\mathbb{M}_\omega} d^{-1}_{in}} \mathbb{1}[m \in \mathbb{M}_\omega] P_{mt} = \sum_m s_{itm} \mathbb{1}[m \in \mathbb{M}_\omega] P_{mt} \tag{5}$$

Where $\omega$ indexes unique sets of reachable mills, i.e. what we call local fresh fruit bunches (FFB) markets. The same mill can be in several sets, but no two sets comprise exactly all the same mills. For instance, when a mill $m_2$ enters a local market where $m_1$ already operates, $\mathbb{M}_{\omega_0} = [m_1]$ and $\mathbb{M}_{\omega_1} = [m_1, m_2]$. We call the sets of reachable mills by the more intuitive name of "local markets". $\sum_m$ means that $m$ runs over all mills in the country, including those that are not reachable from plantation site $i$ at time $t$. Equation 5 reflects the extension to panel data proposed by Borusyak et al. (2022), but here shares do not vary across years but across the sets of reachable mills $\omega$ (which can be constant across several years for a given plantation site).

**The role of fixed effects.** Borusyak et al. (2022) explain that a regression specification that includes $\Omega - 1$ dummies for the $\omega$-indexed clusters allows for "non-random cluster-average shocks". In other words, this allows to assume only that mill-gate prices be as-good-as-randomly assigned within, and not necessarily across, such clusters. This is what the local market fixed effects in our identification strategy do. To show how they isolate exogenous variation in the price signal variable, we consider a regression of deforestation on the annual price signal (i.e., not the 4-year average) in level (i.e., not in log) from Equation 5. In such a regression, local market fixed effects imply demeaning regression variables at this level (which fixest effectively implements in the Quasi-Poisson regression — see Appendix D.1). We write below the demeaned, simplified price signal. To ease the reading, we add the local market level subscript $\omega$ and we drop the superscripts $k$ and $short$ without implication. Because the shares sum up to one, we can write:

$$\tilde{Price}_{it\omega} = \sum_m s_{itm} \mathbb{1}[m \in \mathbb{M}_\omega] P_{mt} - \bar{Price}_\omega = \sum_m s_{itm} \mathbb{1}[m \in \mathbb{M}_\omega](P_{mt} - \bar{Price}_\omega) \tag{6}$$

Equation 6 shows how the fixed effect demeaning of the price signal corresponds to the recentering proposed by Borusyak et al. (2023) — precisely, in their *Case 2*: "Complete shares with controls". Moreover, noting $\mathbb{N}_\omega$ and $\mathbb{T}_\omega$ respectively the sets of plantation sites and years of local market $\omega$ (of sizes $N_\omega$ and $T_\omega$), we can write the local market price signal average:

$$\bar{Price}_\omega = \frac{1}{N_\omega T_\omega} \sum_{\mathbb{N}_\omega} \sum_{\mathbb{T}_\omega} \sum_{\mathbb{M}_\omega} s_{itm} P_{tm} = \frac{1}{N_\omega} \sum_{\mathbb{N}_\omega} \sum_{\mathbb{M}_\omega} s_{i\bar{t}_\omega m} P_{\bar{t}_\omega m} \tag{7}$$

because shares are constant within the time period of a local market — i.e., $\forall t \in \mathbb{T}_\omega, s_{itm} = s_{i\bar{t}_\omega m}$ — and with $P_{\bar{t}_\omega m} = \frac{1}{T_\omega} \sum_{\mathbb{T}_\omega} P_{mt}$. Thus,

$$\bar{Price}_\omega = \sum_{\mathbb{M}_\omega} s_{\bar{i}_\omega \bar{t}_\omega m} P_{\bar{t}_\omega m} \tag{8}$$

where $s_{\bar{i}_\omega \bar{t}_\omega m}$ are shares that sum up to one, because $s_{i\bar{t}_\omega m}$ do. Hence, $\bar{Price}_\omega$ is a weighted average of prices at the gates of mills in local market $\omega$, for the time this market remains unchanged, with weights on every mill reflecting the average distance across plantation sites in this local market. In the full regression that specifies district-year fixed effects in addition to local market fixed effects, one can think about $Price_{it\omega}$ as already demeaned by the district-year average, thus reflecting annual departures from the district market. In practice, this is not completely exact to the extent that some local markets may cross district borders. Given that district borders do not infringe the circulation of FFB systematically, we consider this approximation to be inconsequential in our empirical setting.

**Departures from the canonical setting.** The annual, in-level price signal variable allows to easily formalize the role of fixed effects for identification thanks to the resemblance with the canonical shift-share setting. However, to be relevant, our main specification includes instead the logarithm of the 4-year averaged (medium-run) price signal. These two departures respectively correspond to the non-linear and non-anonymous extensions to which the recentering (by fixed effects in our case) solution applies (Borusyak et al. 2023). Specifically, we note that the 4-year averaged price signal (i.e., averaging $Price^{short}_{it\omega}$ over the four past years as in Equation 4) does not equal the share-weighted average of the 4-year averaged mill-gate prices, because the set of reachable mills may not be constant during the four past years. Table A.8 shows that in practice, the specification with annual price signals as treatment variables yields similar results than the main specification with the 4-year average. Regarding non-linearity, Table A.10 shows that expressing the price signal variable in level instead of log yields qualitatively identical results.

Finally, we note that while theoretical shift-share settings are most commonly associated with two-stage instrumental variable estimation, reduced-form applications are not less valid: Borusyak and Hull (2020) refer explicitly to shift-share "instruments or treatments" throughout; Borusyak et al. (2022) mention the reduced form as a special case; and Borusyak et al. (2023) state that their framework includes the reduced-form case under a similar standard exclusion restriction that the formula (here the reachable mills distance weights) captures all causal channels from mill-gate prices on plantation-site deforestation – a condition we argue is met conditionally on our set of fixed effects (Section 4.2). In similar settings, shift-share methods and insights are stated to apply to the reduced-form case (Goldsmith-Pinkham et al. 2020; Alvarez et al. 2022).

# E Comparison with existing estimates

Here, we attempt to compare our findings with the closest estimates in the literature. Yet, we remark that none of the studies discussed here have provided a price elasticity of deforestation as their main estimate. Therefore, they may naturally have focused less on identification concerns about this parameter. The first (in time) study we can compare our estimates to, is Wheeler et al. (2013). They estimate a log-log regression of deforestation on a time series of palm oil futures prices and other economic variables. We can compare our estimated price elasticity to their model coefficient of 0.816. Using our spatial variation, we hence find a price elasticity twice as large as theirs. We shall note that this difference may also come from differences in the measure of deforestation between our two studies.

Comparing with Busch et al. (2015) requires more assumptions, because this study provides an estimate of the effect of agricultural revenue - and not price - on deforestation. They find that an additional USD 100 (in 2005 USD) is associated with a 1.02-1.18% increase in deforestation. Converting to 2010 USD, assuming an average yield of 3.5 ton CPO per hectare (Khatiwada et al. 2018) and an average price of USD 680/ton CPO over the period (based on our own data), we convert their estimates into a 0.13-0.15 price elasticity.[48] This is lower but comparable to our estimated 1.8 price elasticity of deforestation in industrial plantations, which is the most similar setting to theirs. One should note that the agricultural revenue in Busch et al. (2015) is computed at each land parcel for the most potentially lucrative crop, which is oil palm 69% of the time.

In Cisneros et al. (2021) the effect of price exposure (calculated as the interaction of international prices and suitability for oil palm) on deforestation is expressed for one standard deviation. Thus, in order to compare our analyses to theirs, we compute our partial effects for one standard deviation in our data (remaining after fixed-effect variations are absorbed). In their study, a one standard deviation higher palm oil price exposure results in an 8% increase in deforestation. This is exactly equivalent to the effect of one standard deviation in our setting (corresponding to our main 1.5 price elasticity estimate). However, for the two studies to be more aligned, we compare our price elasticity in industrial plantations (10.2% increase in deforestation for a one-standard-deviation increase in price signals) to their estimated effect

[48]We convert the additional USD 100 to a $100 * USD100/(0.518 * 3.5 * 680) \approx 8.110924$ percentage change in CPO prices (where 0.518 is approximately the deflator we use). We then scale the associated percentage change in deforestation - either 1.02 or 1.18% - by this relative price change.

of price exposure on deforestation in new industrial oil palm plantations by 2015 (3% and imprecise). Hence, here too, our research setting seems to capture a larger effect of prices on deforestation in the Indonesian oil palm sector.

# F Internal and external validity

## F.1 Internal validity

Here, we explain and discuss the estimates we find under a battery of alternative estimation and identification strategies, for all deforestation (Figure B.4), deforestation for illegal industrial plantations (Figure B.5) and deforestation for smallholder plantations (Figure B.6). We explain why these different specifications are relevant and why we do not prefer them for our main analysis. We generally check single departures from the main specification and not combinations of alternative specifications.

**IBS data cleaning.** We check two departures from our main analysis in terms of preparation of IBS variables.

The first departure is the imputation described in Appendix C to clean price variables. In our main analysis, we use a stronger imputation, in order to reduce statistical noise due to duplicates. The softer cleaning choice appears to not only cause more statistical noise in the regressors (that would "just" yield an attenuation bias). It also maintain many mill-level price observations that have systematic measurement error. All these mill observations (829), that are removed in building the main dataset, are duplicates in terms of either CPO quantity or CPO value, (but not both). In most cases (816), the observation of quantity is duplicated from another year, while the value of the output is (presumably) correctly reported to the Indonesian office of statistics (BPS). We do not speculate here about the different ways how this measurement error can be systematically associated with deforestation. Rather, we argue that such source of variation, because it is not well understood, should not enter our main analysis.

The second data preparation we check is lag-adjusting price signals. In our main analysis, we lag IBS variables to correct for a suspected measurement lag between them and the remote sensing forest loss measurement in the outcome variable. Indeed, IBS variables are representative of a whole year, since they are census-based measurements. On the other hand, true forest loss events may not be detected instantly, in particular because of haze and seasonal clouds (Gaveau et al. 2022). Moreover, events could occur at the beginning of the year and be spuriously counted as a decision taken this year. Not taking this into account (i.e., not lagging IBS variables) yields a slightly lower and less precise estimate, including for industrial illegal plantations. For smallholder plantations, it yields an estimate close to zero, as it excludes the effect of the lagged influence of companies discussed in Section 5.

**Sampling.** We report the price elasticity estimates for two additional sampling conditions. In our main analysis, no such conditions are applied. Both conditions yield very similar estimates to the main one.

Under the first additional condition, we include in the sample only plantations where more than 50% of the area was covered with primary forest in 2000. This condition is relevant because it makes the sample more homogeneous in terms of initial land use. It is not included in our main analysis because it also limits the external validity of our results.

Under the second condition, we include in the sample only the plantations for which the set of known reachable mills comprises at least 50% of IBS geo-localized mills. This excludes plantations for which the measurement error is too high due to our geo-localized IBS mill data set not being exhaustive. In our main analysis, we do not apply this condition for the sake of generality and simplicity.

Both sampling conditions reduce the sample size of deforestation for smallholder plantations to the extent that the associated estimates become statistically insignificant. The minimum forest cover may be especially restrictive if deforestation for smallholder plantations tends to occur in older deforestation frontiers where little forest remains.

**Plantation site size.** In the main analysis, we define the cross-sectional unit of analysis – plantation sites – as arbitrarily delineated 3 x 3 km grid cells. To check the robustness of the results to this choice of spatial resolution, we re-run the analysis with deforestation and price signals calculated in grid cells two-third smaller (1 x 1 km) or two-third larger (5 x 5 km). This tends to yield slightly less precise but largely comparable estimates.

**Catchment modeling.** How we model the true relationships between mills and plantations is a critical point in our analysis. Therefore, we explore three alternatives to the model used in our main estimation strategy - catchment radii of 30 km in Sumatra and 50 km in Kalimantan.

The first alternative consists in the assumption that plantations are only influenced by prices at the nearest mill. This is the simplest model possible. Not surprisingly, it is very imprecise. This estimate's confidence interval is so large that we do not feature it in Figure B.4 for the sake of readability.

The second one consists in removing from the analysis extensive margin deforestation. As defined in Section 2.2, we deem deforestation to be at the extensive margin if it occurs close to a mill — i.e. in one of the four nearest grid cells to this mill. Four 900 ha grid cells make a 3600 ha area which is enough to supply an average mill (Paoli et al. 2013) — and prior to its establishment date. Extensive margin deforestation likely results from integrated plantation-mill decisions that are not much influenced by price signals from other mills. Indeed, the price elasticity of intensive margin deforestation is higher than that of deforestation at both extensive and intensive margins. For the sake of generality and simplicity, we consider deforestation at both margins in our main analysis.

The third alternative is a different catchment radius in each island: 50 km in Sumatra and 30 km in Kalimantan. In Section 4.1, we discuss the size of the catchment radius and the reason why it should be lower in Sumatra than in Kalimantan. The alternative catchment radii yield a higher estimate.[49] However, in the case of illegal deforestation, the alternative yields a much closer and imprecise estimate. This suggests that in Sumatra, where the elasticity is otherwise clearest (Table A.13), associating illegal deforestation with mills between 30 and 50 km introduces significant noise.

Finally, we model the catchment area of each mill not as a circle defined by a radius, but as the set of plantations that can reach the mill within two hours of driving (see Harahap et al. (2019) for a discussion on the driving time.[50]). This modeling is highly relevant because often, mills, although close to plantations in straight line distance, may actually not be reachable in time by trucks following weaving roads (and the opposite is also true). However, this modeling is not done in our main, preferred analysis because it may introduce endogeneity. Indeed, plantations likely expand (and hence deforest more) in parts of districts where the road infrastructure is better, while in the same area, prices are probably affected by the better access to markets enabled by better roads. This bias should be attenuated in our main analysis as we arbitrarily draw a line beyond which plantations are not connected to a mill although the road infrastructure would actually make the mill's prices influence deforestation. The estimate under this catchment area model is negative in the case of all deforestation, positive in the cases

[49]We also get an estimate under a 10 km catchment radius assumption, but here again we do not present it in Figure B.4 as the confidence interval is so wide that it complicates the reading of the whole figure.

[50]Harahap et al. (2019) use a four-hour constraint, grounding on `https://goldenagri.com.sg/plantation-mill-24-hours/` Here we present a twice shorter constraint because the estimation with the four-hour constraint yields too large a confidence interval to be displayed next to the other estimates.

of deforestation for illegal industrial and for smallholder plantations, and imprecise in all cases. This may result from the aforementioned endogeneity and significant discrepancies between the road network available in our data (OpenStreetMap) and the actual road and track network used by palm oil producers and intermediaries.

**Price signal time average.** As explained in Section 4.1, our main measure of price signal is a 4-year average of annual price signals. We present here price elasticity estimates with different time average lengths.

Unsurprisingly, the short-run price signal measure alone yields a non-significant estimate. Indeed, we expect the development of perennial crops to have little responsiveness to annual variations. This is confirmed by the narrow confidence interval.

The price elasticity point estimate increases with the average length of the price signal time, while precision decreases. With a 5-year average, too much noise enters the price signal measure and the price elasticity becomes less precise.

In the case of deforestation for smallholder plantations, the price elasticity estimate decreases with the amount of lagged prices in the price signal. Not accounting for the fourth lag fails to capture the lagged influence of companies on the expansion of smallholder plantations.

**Distributional assumptions.** Our preferred distributional assumption is a quasi-Poisson distribution (which allows the variance to be different from the mean). A Poisson distribution assumption yields the same point estimate and very similar standard errors. This suggests that our data are not subject to over- or under-dispersion.

**Control variables.** We explore specifications with all combinations of control variables. These include the control on the number of known reachable mills specified in our main specification and four additional control variables.

The first one is 1-year lagged deforestation. Deforestation has been often shown to be an auto-regressive process, and indeed we find that, in our data, lagged deforestation is positively correlated with current deforestation (results available upon request). Furthermore, we expect that prices from the 4 past years that we average in our price signal measure also influenced past deforestation. Indeed, in our data, we find that a price signal measured as an average of prices over 3 years does influence deforestation (cf. the above paragraph on different time average lengths). However, we do not believe that 1-year lagged deforestation can impact price signals (because of the time lag between planting and harvesting). Therefore, we suspect 1-year lagged deforestation to be an intermediate factor. We find that neither the magnitude nor the precision of our estimate varies with the inclusion of 1-year lagged deforestation. Thus, we conclude that the effect we measure is not inflated by the spurious accumulation of intermediate effects by which past prices would cause past deforestation that would then cause present deforestation.

Second, we control for the (inverse-distance weighted) average share of crude palm oil (CPO) exported by reachable mills. This proxies plantation exposure to the Indonesian export tax (Rifin 2014) and to international supply chains and hence might control for additional potentially confounding systematic differences between plantations. Adding it to the main control set yields a similar estimate.

Third, we control for the average ownership shares of reachable mills (with invert-distance weights as for prices): the share of domestic private capital and the share of foreign capital (we exclude the share of public capital to avoid perfect collinearity). We might be concerned that, for instance, local government mills have different deforestation motivations than foreign mills, while also having different marketing conditions. However, ownership changes may react to price shocks, while also being endogenous to local conditions. This makes ownership shares potential colliders, or "bad controls", that we prefer to exclude from our main analysis. Including them in the regression yields a similar estimate.

Fourth, we control for the baseline forest trend. This is built as an interaction between the primary forest cover in 2000 and the year. It captures differential trends between plantations with different initial land uses. These trends likely explain deforestation. If they are also correlated with price signals, they can bias our estimate. However, adding them to the main control set yields a similar estimate.

With each of these controls, we also test to remove the control on the number of all known reachable mills. This mill-density control may be a collider (so-called "bad control"), if local market development and deforestation have a common cause (like past deforestation) and if local market development *results* from local prices. However, variations in medium-run price signal shocks are unlikely to influence the timing and location of a multi-million dollar investment in a new mill (see Kraus et al. (2022) for a detailed discussion of the drivers thereof.) We find indeed a similar estimate for specifications without this control. Moreover, adding a control for past deforestation in the last 4 years to mitigate the endogeneity of mill density and deforestation does not change the results.

In addition, as mentioned in Section 4.2, to address concerns of reverse causality bias, we test the robustness of our results to specifications that include a control for 5- to 8-year lagged deforestation. As for the 1-year lagged deforestation control, this is motivated by the suspected auto-regressive process of deforestation. But here, we aim to block the confounding effect of past deforestation on current prices through reverse causality. We measure past deforestation in plantation site $i$ or in it's 8 nearest plantation sites. The latter captures the potential bias that could arise from global spatial spillovers (LeSage 2014). These spillovers occur when deforestation in surrounding areas affects local deforestation. They are likely to occur (Robalino and Pfaff 2012; Shevade and Loboda 2019), and in particular it is possible that surrounding deforestation in the past, (i.e., temporally and spatially lagged) affects current local deforestation. For both spatial scales, we measure past deforestation in two temporal depths: either 5-year lagged, or 5- to 8-year lagged averaged. The latter captures endogeneity with past deforestation for all 1- to 4-year lagged annual price signals in our main price signal measurement, as well as some of the cumulative effect of past deforestation on the more recent price signals. Because deforestation as we measure it can only be observed as of 2001, the two temporal specifications imply that the samples available for estimation cover the periods 2005-2014 and 2009-2014 respectively. The results of this robustness check are presented in Table A.11. We find a 2.6 price elasticity point estimate conditional on 5-year lagged deforestation, either in plantation site or in neighboring ones. Estimating our main specification over the same period yields a similar 2.6 point estimate. We find a 3.3 price elasticity point estimate conditional on average 5- to 8-year lagged deforestation, either in plantation site or in neighboring ones. Estimating our main specification over the same period yields a similar 3.3 point estimate. Hence, this robustness check makes us more confident that our results are not confounded by dynamic reverse causality.

**Fixed effects.** Our main analysis uses a combination of the set of reachable mills and district-year fixed effects, as we believe that most price endogeneity arises at the district level. Different fixed effects absorb variations at different levels. The set of reachable mills fixed effects alone remove little time heterogeneity, thus allowing aggregate shocks to confound the estimate, leading to a lower estimate. Adding a year fixed effect additionally controls for country-wide annual shocks that would apparently introduce a positive bias. Adding, rather, local-year fixed effects, i.e., ruling out common confounding shocks at the level of province, district, subdistrict or village, yields positive estimates. These are precise in the case of province-year and district-year fixed effects, larger but less precise in the case of subdistrict-year fixed effects, and very imprecise in the case of village-year fixed-effects (which we do not display in Figure B.4 in order to better read it). This shows that most of the effect of price signals on deforestation is at play above the village-year level. Not controlling for common shocks at a local level yields imprecise results in the cases of deforestation for illegal industrial and smallholder plantations. Finally, holding the price departure with respect to the district market level, we change the other fixed effect from the set of reachable mills to the plantation level. This bans inter-plantation comparisons (contemporaneous or not) from identification. However, it introduces a new type of identifying comparisons: within the same plantation, but across years when it can reach a different set of mills. The resulting estimate is very similar to the main one.

**Clustering.** We show in Figure B.4 how allowing correlations in standard errors within different observation clusters affects confidence intervals. Price elasticity estimates are statistically different from zero with more clusters than in our main analysis - i.e., with plantation and village clusters. They also remain significant with larger and hence fewer clusters; namely, with district clusters and two-way plantation and district-year clusters.

### F.2 External validity

To assess the external validity of the results, we run three additional analyses. First, we assess whether the price elasticity varies with the size of the local market by estimating the coefficient of the interaction between price signals and the number of reachable mills, not the partial effect (Buis 2010). We find no evidence of contingency between price elasticity and local market development (Table A.12). This suggests that the price elasticity results apply to a large range of local market development levels. Hence, even if our sample is restricted to plantations within 30 km (50 km in Kalimantan) from at least one mill to reduce noise in estimation, the results may be extrapolated to plantations in even more remote areas.

Second, We estimate the price elasticities of deforestation for Sumatra and Kalimantan separately. Table A.13 shows that estimates are comparable in both islands, but less precise for Kalimantan. The exception is the price elasticity of deforestation for smallholder plantations which has a negative point estimate in Kalimantan and is extremely imprecise. Possible reasons for these imprecise estimates include: fewer clusters and observations; a lower share of geolocalized palm oil mills (40% against 60% in Sumatra) implying more noise in the price signal variable; and possibly a truly different influence of price incentives.

Third, we estimate our main model on a measure of secondary forest replaced by oil palm plantations. Table A.14 shows that deforestation in such forests is generally not price elastic. We see two non-exclusive potential explanations for this absence of effect (given the large number of observations and clusters, we do not attribute it to a lack of statistical power). First, secondary forest is, by definition of primary forest, more scattered. Deforestation in secondary forest plots may be decided marginally, at too small a scale for price signals to be significantly influential. Second, our measurement of secondary forest loss (Appendix C.2) includes cuts in existing tree plantations (except industrial oil palm plantations which we can remove from the baseline), and palm rotations or conversions from other tree crops to oil palm may respond differently to price signals. In the case of smallholder plantations, the negative and significant (at 10%) estimate suggests that smallholders delay the replanting of old trees when prices are high, as hypothesized in (Zainal Abidin et al. 2018) for instance.