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Palm oil and the politics of deforestation in Indonesia ☆

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ABSTRACT

This paper studies the interactions between political and economic incentives to foster forest conversion in Indonesian districts. Using a district–level panel data set from 2001 to 2016, we analyze variation in remotely sensed forest losses as well as measures of land-use licensing. We link these outcomes to political incentives arising before idiosyncratically–timed local mayoral elections as well as to price exposure measures based on oil palm soil suitability combined with global price variations for palm oil. Empirical results document increases of about 4% in deforestation in the year prior to local mayoral elections on average. Additionally, palm oil plays a crucial role in driving deforestation dynamics. Deforestation rates increase by 7% in places that experience a one standard deviation increase in local price exposure, but no upcoming elections. These effects are amplified to almost 19% larger forest losses in places that experience pre-election years and a standard deviation higher palm oil price exposure at the same time. We thus find clear evidence for economic and political incentives reinforcing each other as drivers of forest loss and land conversion for oil palm cultivation.

1. Introduction

Food production has driven forest losses globally at an accelerating pace since the early 2000s (Lambin and Meyfroidt, 2011; Pendrill et al., 2019). This process has been slowed down in countries with effective environmental institutions (Lambin et al., 2014) and commitment to sustainable agriculture (Ceddia et al., 2014). Under weak institutions, local governments can decide to support, ignore, or even hinder federal conservation efforts and are often the *de facto* sole managers of their environmental resources. With high profits from agricultural production, local politicians have to balance the benefits from forest conservation against commercial gains from agricultural expansion and their own political interests.

The existing economic literature has addressed several dimensions of the political economy of deforestation, highlighting especially the importance of the decentralization and district proliferation process as drivers of deforestation in Indonesia

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(Burgess et al., 2012; Alesina et al., 2019) and pre-election increases in deforestation in Brazil (Pailler, 2018). In this paper, we provide the first causal evidence on the interaction between agricultural and pre-election incentives to deforest. Specifically, we combine variation in the local incentives to grow oil palm with historically determined idiosyncratically timed mayoral elections in Indonesian districts, and ask whether these two types of processes reinforce each other.

During the past two decades, Indonesia became the primary global producer of palm oil.[1] Home to one of the largest remaining tropical rain-forests in the world, Indonesia has also experienced accelerated deforestation dynamics, with a 13% loss of forests since 2000 (Austin et al., 2017; Hansen et al., 2013). The palm oil boom has been accompanied by sweeping decentralization reforms at the beginning of the 2000s, which have given rise to new local elites and a system of "money politics" that is rife with clientelism (Aspinall and Sukmajati, 2016). The local policy debate has emphasized the role of corruption of local administrations for the excessive expansion of oil palm plantations (Tempo, 2018). Anecdotal evidence suggests that agribusinesses and especially oil palm corporations play a crucial role in financing local mayoral elections, the so-called *pilkada* (Kompas, 2020).[2] High profits from palm oil are therefore likely to spur local politicians to support deforestation activities. These incentives will be reinforced right before elections when voters are the most responsive to handouts (Drazen and Eslava, 2010).

Our empirical analysis studies deforestation dynamics in a panel of Indonesian districts over the years 2001–2016. Conditional on district and year fixed effects, we link deforestation to two sets of incentives: pre-election incentives driven by the timing of elections of local mayors, and agricultural incentives for expanding oil palm cultivation areas. After the sudden resignation of Suharto in 1998, Indonesia began a staggered process of democratically replacing local mayors as all mayors appointed by Suharto were allowed to finish their existing terms before leaving office (Martinez-Bravo et al., 2017). This introduced a quasi-random timing of five-year mayoral election cycles across Indonesia, which we use to identify the pre-election effects. Agricultural incentives are measured by a shift-share exposure measure combining global market price fluctuations of palm oil with time invariant local agro-climatic conditions to grow oil palm.

Our results document that deforestation increases by about 4.2% in the year before local elections at the baseline. Districts that are exposed to one standard deviation larger increases in palm oil prices experience 7.1% higher deforestation rates. Both incentives reinforce each other, leading to a total of 18.8% of additional forest losses in districts that face a standard deviation higher palm oil price exposure in the year before local mayoral elections. We also show that this link has been only statistically significant after direct mayoral elections were introduced. We fail to find differences in deforestation before and in the aftermath of district splits, indicating that simple administrative overburden is less likely to explain these results. Excess deforestation induced by political and agricultural incentives is prevalent on the agriculturally most productive forest areas (lower density and non-primary forests). Results on transitions between different land-use types also show descriptively that this interaction seems to matter most in the short run and in areas that are directly converted to industrial oil palm plantations.

Our paper contributes to three major strands of literature. First, it is related to the growing conceptual and empirical literature on the relationship between institutions and the environment (Ostrom, 1990; Fredriksson et al., 2010; Cabrales and Hauk, 2011), and more specifically, on the impact of local governments on forest conservation (Lemos and Agrawal, 2006; Ribot et al., 2006; Luttrell et al., 2014; Sills et al., 2015; Cisneros et al., 2015, 2016; Arima et al., 2014). Focusing on local elections and deforestation (Burgess et al., 2012; Pailler, 2018; Alesina et al., 2019), we contribute to this literature by providing the first causal identification of how political incentives interact with agricultural incentives. Second, this paper also relates to the literature on the environmental externalities of agricultural goods production, which documents that demand shocks and technological advances in agriculture lead to negative impacts on the environment (Angelsen, 2007; Barreto and Silva, 2010; Hargrave and Kis-Katos, 2013; Krishna et al., 2017; Gatto et al., 2017; Busch et al., 2015). We contribute to this literature by showing that the prospects of future economic benefits drive land-use decisions. Finally, our paper also speaks to the broader literature on decentralization and political budget cycles. While decentralization was seen as a tool in bringing decision makers closer to the public, introducing accountability, and improving the provision of public goods, it also opens the possibility for rent seeking and capture of the political process by local elites (Bardhan, 2002; Bardhan and Mookherjee, 2006; Martinez-Bravo et al., 2017; Burgess et al., 2012). We contribute to this literature by showing how decentralized environmental management results in an increased depletion of natural resources before mayoral elections.

The remainder of the paper is structured as follows. Section 2 sketches a theoretical framework and describes the policy environment. Section 3 presents the data and outlines the empirical approach. Section 4 presents the empirical results, discusses identification issues and possible mechanisms, while section 5 provides a more general discussion of the results. Section 6 concludes.

2. Theoretical and institutional background

2.1. Agricultural incentives, elections and deforestation

Deforestation is the human conversion of forested land into alternative uses (cf. Fearnside, 2017), and predominantly, into

[1] Since 2013, Indonesia has supplied more than 27 megatonnes of palm oil—half of total world production—to the global food, cosmetic, and bio-diesel industries. (FAO, 2018; Corley, 2009; Pin Koh, 2007).

[2] See for instance, https://www.kompas.com/tren/read/2020/09/23/085738165/pilkada-dan-konsesi-sawit?page=all and https://majalah.tempo.co/read/kolom/156643/korupsi-politik-perizinan-sawit.

agricultural land. It arises when the demand for new agricultural land is met by supply of farmland from previously forested areas (cf. von Thünen, 1875; Angelsen, 2007). A shift in the demand for agricultural goods will increase the demand for new land and thereby deforestation. Population growth, economic development, and changing consumption patterns are among the most researched drivers of deforestation (cf. Andersen, 1996; Hargrave and Kis-Katos, 2013; Rajão et al., 2020). Supply shifts on the agricultural market, driven by new technologies or infrastructure investments also show significant impacts on deforestation dynamics as they increase the marginal profitability of the input factor land (cf. Pfaff, 1999; Arima et al., 2007; Losos et al., 2019; Barber et al., 2014).

The supply of new land from deforestation depends on the size of the remaining forest area and on the effective policy environment. Access and use restrictions on forest landscapes (e.g., protected areas) limit the supply of new land from public forests. On private forest land, governments often aim to implement incentive policies that increase the owners' valuation of forest conservation (e.g., via payments for environmental services). The success of such conservation policies will depend on whether complementary environmental monitoring and enforcement mechanisms are in place (Lambin et al., 2014; Börner et al., 2015b, 2017). The supply of new land from deforestation is therefore a function of politicians' efforts to limit the conversion of public forests to private land, to maintain protected areas, to monitor land use and enforce regulations. The demand for new land will additionally be affected by policies that raise agricultural profitability (e.g., by providing fertilizer or credit subsides). With discretionary power, politicians may use these policies to their own political benefit, also in illicit ways.

The theory of political budget cycles posits that politicians manipulate a range of policy tools just before elections in order to increase their re-election chances (Alt and Rose, 2009; Haan and Klomp, 2013). The original idea of political business cycles by Nordhaus (1975) relies on imperfectly informed voters who make decisions based on politicians' past performance, placing higher weights on the most recent years before elections. Under the assumption of such 'retrospectively myopic' voters, any increases in voters' utility just before elections will increase the politicians' chances for re-election. Further theoretical arguments stress that before elections politicians may want to signal competence by increasing public spending in general (Rogoff, 1990), or shifting spending to the most tangible public goods (Drazen and Eslava, 2010). Beyond causing changes in public spending patterns, pre-election periods have also been linked to increases in corruption (Shi and Svensson, 2006; Alt and Rose, 2009). Especially in young democracies with low levels of political accountability, politicians may increase corrupt activities before elections in order to raise funds for electoral campaigns built on direct handouts or vote buying (cf. Pereira et al., 2009; Khemani, 2004; Aidt et al., 2020).

As far as agricultural development provides benefits to the electorate, we expect political support for the agricultural sector to increase right before elections (e.g., via subsidies, new forest conversion licences, or reduced enforcement activities). The benefits of forest conversion for agriculture before elections are more likely to arise immediately while negative consequences from the loss of environmental services materialize in the medium run. In a setting, where politicians can choose from multiple policies to influence voters, we expect them to focus on the most profitable strategies (Holmstrom and Milgrom, 1991). They will support forest harming activities before elections when and where the prospective profits from land conversion are relatively high. The empirical analysis of this interplay between agricultural profitability and pre-election incentives lies at the core of this paper.

2.2. Decentralized environmental governance in Indonesia

After the fall of the authoritarian Suharto regime in 1999, Indonesia underwent a rapid decentralization and democratization process. At its cornerstone lay a large-scale shift of administrative and fiscal authority to local (district) administrations in 2001 (World Bank, 2003). A large share of public services was shifted from the national to the district level,[3] the share of public sector employees increased rapidly, and fiscal transfers from the central government reached three quarters of all fiscal spending (World Bank, 2003; Kis-Katos and Sjahrir, 2017; Gonschorek et al., 2018).

In parallel, the decentralization process introduced incentives to create new administrative units by splitting existing districts. The so-called *pemekaran*, or district proliferation process, led to the creation of a large number of new administrative units, whereby district splits were triggered by ethnic heterogeneity, expected fiscal, or natural resource rents as well as geographic factors (Fitrani et al., 2005).

Decentralization also transferred environmental and land-use decision-making to local administrations, providing considerable power over the profitable timber, logging and oil palm sectors. District administrations gained considerable autonomy over the supply of agricultural licences as well as forestry revenues (Smith et al., 2003; Barr et al., 2006). Even after subsequent re-centralization efforts, some districts continued to issue licences illegally, and new concessions still take into account the official recommendation by district administrations (Ribot et al., 2006). In order to establish a new oil palm plantation, districts have to issue a permit for companies to initiate negotiations with rural landholders and the Ministry of Forestry (EIA, 2014). When concessions are targeted at public state forests, districts play a crucial role in the release of those forest areas for agricultural production (Barr et al., 2006; Sahide and Giessen, 2015). Overlapping competencies between the different tiers of the government blur the divisions of responsibilities and the *de facto* area of oil palm plantations is substantially larger than the official figures (Kartodihardjo, 2018).

[3] Law 22/1999 transferred substantial responsibility related to public works, the provision of health, education and culture, agriculture, communication, industry and trade, capital investment, environment and land to districts.

In 1999, following the first democratic presidential elections since 1955, new local district parliaments were formed with democratically elected members who then appointed new mayors. Direct elections of district mayors were introduced in 2005 (Sjahrir et al., 2014). From the start of the decentralization process, all old-regime mayors were first allowed to complete their full five-year term, which resulted in a historically determined staggered regime change (Martinez-Bravo et al., 2017), and an idiosyncratic and asynchronous election cycle for local mayors throughout our period of analysis (Sjahrir et al., 2013). These newly decentralized political processes became especially susceptible to local elite capture (Bardhan and Mookherjee, 2005). The introduction of direct elections for mayors in 2005 has been linked to a consolidation of old political elites, a reduction in political competition and thereby lower governance outcomes, higher fiscal spending, and an increase in forest losses (Sjahrir et al., 2013; Burgess et al., 2012; Alesina et al., 2019).

Scholars have repeatedly upheld corruption as one of the most prevalent drivers of deforestation in Indonesia (Smith et al., 2003; Amacher et al., 2012). Corruption scandals often involve mayors who receive bribes to ignore illegal logging or to issue forest concessions, but oil palm corporations have also been directly involved in illegal election financing. Within the Indonesian system of "money politics" (Aspinall and Sukmajati, 2016), political parties ask candidates running for office for contributions worth millions of dollars (Mongaby, 2018), which leaves them often in debt and eager to accept financing from large corporations. This entrenchment has lead to an ever growing entanglement between politics and the palm oil industry that finance political campaigns in return for new plantation licences (Mongaby, 2018). To further illustrate this point, section A.3 in the Appendix provides a short list of corruption scandals linked to the oil palm sector that were covered by online newspapers.

3. Data and empirical approach

3.1. Data and descriptive trends

Our spatial units of analysis are Indonesian districts, which became crucial decision-making units since decentralization. The panel data frame spans 16 years, from 2001 to 2016. Out of the 514 districts that existed in 2016, we restrict our main analysis to those 397 that were substantially forested (had an initial forest cover of at least 40%) in 2000. We deal with the ongoing district proliferation process by fixing our district frame to the end of the observation period, building a balanced panel of geographic entities as observed in 2016, and adding controls for district splits to our models. Our empirical analysis combines time-variant remotely-sensed data on forest cover, measures of the local exposure to variation in prices of palm oil (and other agricultural crops), information on the timing of local mayoral elections, and the administrative district splitting process, as well as further information on land-use licenses and local conditions. Appendix A.1 presents a more complete description of the data generating procedures. Descriptive statistics are displayed in Table A1 in the Appendix.

Our main dependent variable of interest is the yearly size of newly deforested area within each district. It is derived from the database by Hansen et al. (2013) that is based on satellite observations and provides yearly raster files at a 30-m resolution for the years of 2000 until 2016. Over the last fifteen years, Indonesia has experienced a steep increase in yearly deforestation, with substantial fluctuation in deforestation rates from year-to-year, which will be exploited in our empirical analysis (see Fig. 1a). Fig. 2 maps the spatial distribution of total forest loss over the full time period per district.[4] It reveals a strong concentration of deforestation within two islands, Sumatra and Kalimantan, which were also the most strongly affected by the expansion of oil palm plantations over the last decades. At the same time, the maps also show substantial within-island variation in deforestation.

We measure political incentives by relying on the idiosyncratic timing of mayoral elections within each district. Local mayoral elections usually take place every five years and the starting year for each cycle has been determined by historical chance (Martinez-Bravo et al., 2017). Fig. 1b shows substantial variation in the yearly number of elections across Indonesia as a whole, which we use to identify the local effects of the idiosyncratic election timing. Fig. A2 in the Appendix displays the data by month for the time period of direct mayoral elections. Within our sample, we identify up to 1247 pre-election years, based on at most five completed or upcoming mayoral elections for each district. Most mayoral election cycles have been regular, with only fewer substantial deviations from the five-year cycle. We address the sensitivity of our results to deviations from a fully mechanical cycle in section 4.2. Descriptively, Fig. A3 in the appendix shows that conditionally on district and year fixed effects, average deforestation has been the highest in the pre-election year as compared to all other years.[5]

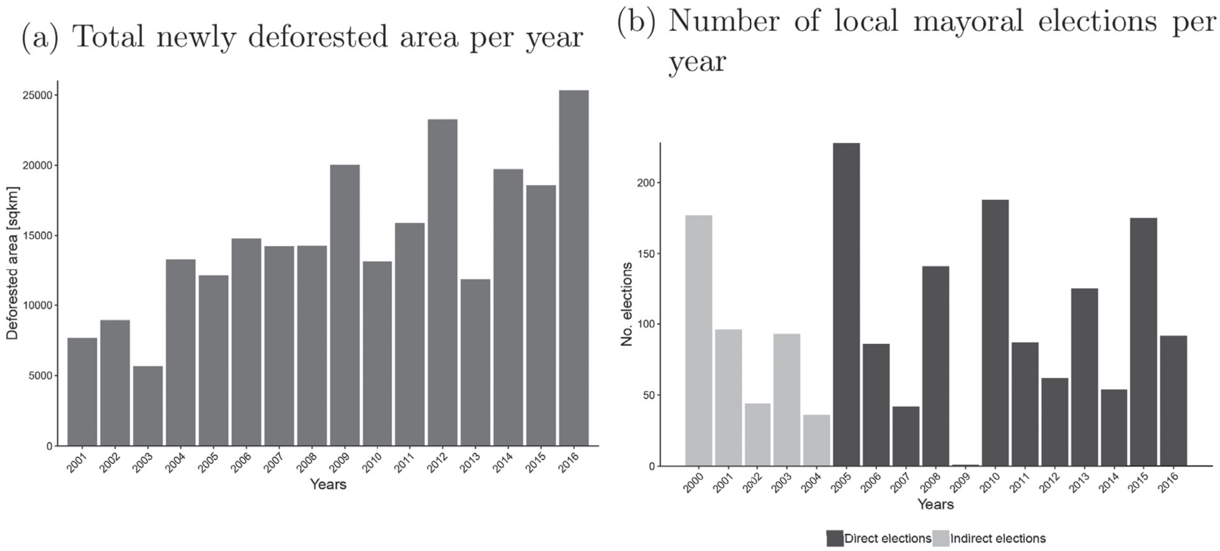
We measure economic incentives through a price exposure measure, P_{dt}^{Exp}, that varies across districts d and years t. It is derived by interacting local agricultural suitability with world market price variation of palm oil:

$$P_{dt}^{Exp} = S_d \times P_t, \tag{1}$$

where S_d denotes the time-invariant average agricultural suitability for growing oil palm in a given district d (mapped in Fig. A5 in the Appendix). P_t is computed as the deviation of the current world market price for palm oil in year t from its average over the past five years. Fig. 3 shows the time trends in global palm oil price deviations in real terms (converted to Indonesian Rupiah), as well as a weighted average of prices of other main crops used as a comparison (FAO/IIASA, 2012, see Appendix A.1 for

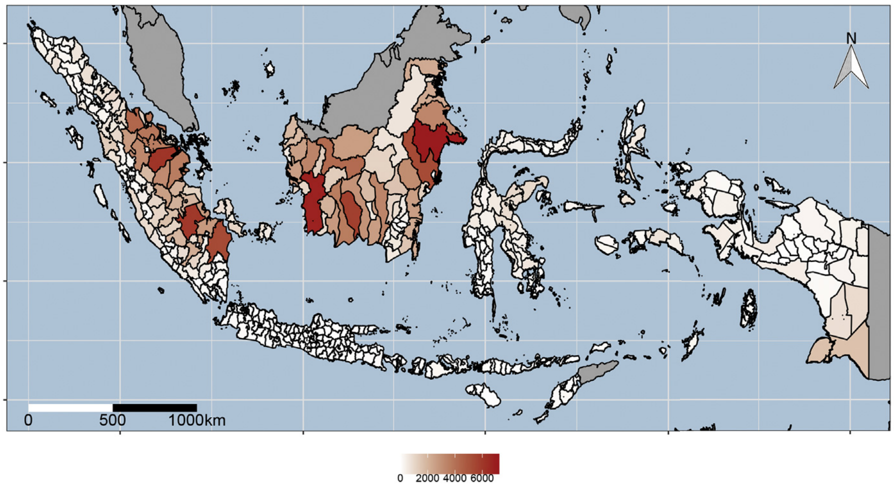
[4] As a comparison, Fig. A1 in the Appendix displays a map based on the pixel-wise raw data used to calculate aggregate measures of deforestation.

[5] While deforestation in the pre-election year is statistically significantly higher than in all other years when estimated separately in panel (a), corrections for multiple variable testing in panel (b) show that we do not have enough statistical power to estimate a full deforestation cycle on its own.



Note: Panel (a) depicts annual forest losses based on data form Hansen et al. (2013). Panel (b) depicts the number of yearly district elections based on data from KPU Election registries and Wikipedia.

Fig. 1. Deforestation dynamics and mayoral election timing.



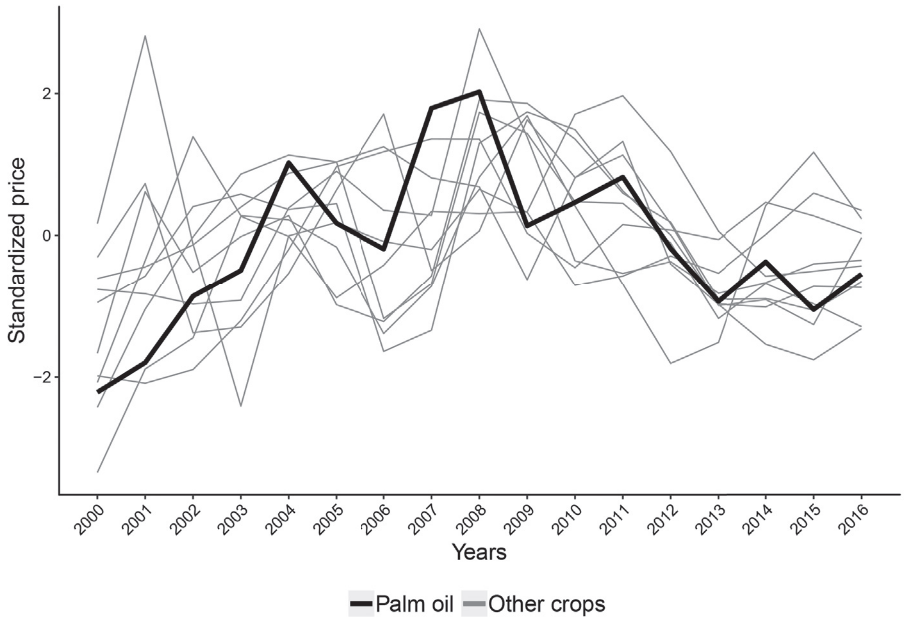
Note: Figure depicts the total square kilometer of forest loss within each district, based on Hansen et al.'s (2013) global raster maps and district layers from GISPEDIA (2018).

Fig. 2. Total deforestation 2001–2016 (per district).

further details).[6] All observed price variables are stationary but show substantial fluctuation over time. Palm oil prices increased in several waves, peaking first in 2004, then in 2008 and in 2011. By the end of our time period, they were more or less back to their starting levels. The underlying hypothesis is one of backward-looking adjustment: market participants observe the profitability of oil palm over recent years and adjust their economic decisions to convert forest land when prices deviate from the prices in the past. The global price trend is not exogenous to the Indonesia-wide trend of aggregate deforestation. We discuss the implications of this and test for the robustness of the main results by implementing an IV procedure in section 4.2.

Among further controls, we allow for differential trends in selected conditions—soil suitability for growing oil palm and initial forest area—which plays an important role from the perspective of causal identification (see section 4.2). We also control for the administrative process of district proliferation in a flexible way, relying on data from World Bank (2019).

[6] Using US dollar prices instead results in similar dynamics and closely comparable empirical results (not presented; cf. Fig. A6 in the Appendix).



Note: The thin grey lines plot the yearly variation in the normalized real local exposure value of global crop prices of ten major agricultural crops; the thick grey line plots their weighted average (using SNA-based production shares). The thick black line plots variation in real local exposure value of the global palm oil price.

Fig. 3. Standardized price trends of palm oil and other major crops.

3.2. Empirical model

Our empirical strategy relates the dynamics of district-level deforestation in a panel data setting to two potential determinants of deforestation: idiosyncratic variation in the timing of local mayoral elections at the district level and local variation in economic incentives to convert land to oil palm plantations. We are especially interested in whether and to which extent these two forces reinforce each other.

Our main dependent variable is expressed as the inverse hyperbolic sine of the newly deforested area in district d and year t, D_{dt}.[7] We regress this outcome on an indicator of local election timing, a measure for the variation in price incentives, and the interaction of these two variables, plus further controls:

$$D_{dt} = \quad \alpha E_{dt+1} + \beta P^{Exp}_{dt} + \mu E_{dt+1} \times P^{Exp}_{dt} + \mathbf{X}_{dt}\gamma + t \times \mathbf{Z}_{d0}\delta + \lambda_d + \xi_t + \varepsilon_{dt} \tag{2}$$

We follow the literature on political budget cycles (e.g., Shi and Svensson, 2006; Brender and Drazen, 2005; Sjahrir et al., 2013; Aidt et al., 2020) by focusing on the time period in the run-up to elections to test for different deforestation levels compared to all other years. To capture this, the pre-election indicator, E_{dt+1}, turns to one in the year preceding the local mayoral elections and remains zero otherwise.

The effects of economic incentives are captured by our palm oil price exposure variable, P^{Exp}_{dt}, measuring the potential exposure of the local economy to new variations in global market prices of palm oil. Price exposure varies across districts and across time due to differences in soil suitability for growing oil palm and time variation in global palm oil prices (cf. section 3.1). To investigate whether the economic and political incentives reinforce each other, we focus on the interaction of the electoral cycle with palm oil price exposure. By that, our main coefficient of interest is given by μ, which describes the interaction effect between political and agricultural price incentives. Positive values for α and β would show that both political and economic considerations contribute to deforestation at the district level, whereas a positive μ would imply that the two types of incentives reinforce each other.

All regressions include district fixed effects, λ_d, that control for all sources of time invariant district heterogeneity, and year fixed effects, ξ_t, that control for average fluctuations in deforestation due to macroeconomic and common policy shocks. Additionally, our preferred specifications control for the ongoing district splitting process, $\mathbf{X}_{dt}$, by adding a split indicator for parent

[7] The inverse hyperbolic sine function transforms the size of the yearly newly deforested area, d_{dt}, as $\ln(d_{dt} + \sqrt{d_{dt} + 1})$. As compared to a log-transformation, it has the advantage of being defined at zero and yielding near-zero positive values for very small deforestation levels, but allowing for interpreting small coefficients in percent similarly to a log transformation.

Table 1
Baseline: Local exposure to palm oil prices and elections.

	(1)	(2)	(3)	(4)	(5)
Pre-election year	0.053**		0.053**	0.044*	0.042*
	(0.023)		(0.024)	(0.025)	(0.025)
Palm oil price exposure		0.080***	0.080***	0.082***	0.071**
		(0.031)	(0.031)	(0.031)	(0.032)
Pre-election year × Palm oil price exposure					0.075**
					(0.036)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	Yes	Yes
Observations	6352	6352	6352	6352	6352
Adj. R^2	0.889	0.887	0.887	0.890	0.890

Note: The table shows the effects of palm oil price incentives and election incentives on deforestation (measured as the inverse hyperbolic sine of yearly forest losses), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

and child districts separately, together with further two lags and two leads for each. The controls for the timing of the district splitting process capture all variation in deforestation pressure that is driven by the changes in the administrative and fiscal policy environment at the district level.

Moreover, we allow for differential time trends by selected initial conditions, $\mathbf{Z}_{d0}$, which include the initial forest size within the district as well as the initial oil palm suitability index. This ensures that our results are not merely reflecting differential trends in deforestation across structurally different districts. Allowing for differential trends by initial conditions plays also a relevant role from an identification perspective (as discussed in section 4.2). All standard errors are clustered at the level of 251 original parent districts in order to control for correlated outcomes across newly formed districts that used to belong to the same parent district before.

4. Results

4.1. Baseline results

Table 1 presents our main estimates based on equation (2), adding controls step-wise. In column (1) we compare the pre-election year to all other years by conditioning our estimates on district and year fixed effects only. The result reveals statistically significantly higher deforestation rates in the year before local mayoral elections. The positive pre-election coefficient shows an increase in forest losses by about 5% in districts with upcoming mayoral elections in the next year as compared to all other years. Column (2) includes instead our control for agricultural incentives that drive land conversion to oil palm plantations. Palm oil price exposure, as described before, combines time variation in palm oil prices on the world markets with spatial variation in the agro-climatic suitability for growing oil palm in each district. Results show a clear positive link between local exposure to palm oil price variation and forest loss. A one standard deviation higher local palm oil price exposure results in about 8% more deforestation in a district. When added together in column (3), the pre-election and the palm oil price exposure coefficients stay precisely the same as there is no substantial correlation between these two dynamics, underlining the quasi-experimental nature of the election cycle.

Results change only marginally in column (4), when we control for varying deforestation trends by initial oil palm suitability and forest size as well as for the time dynamics of the district splitting process. Table A2 in the Appendix displays the full results. It confirms that deforestation trends increase with the size of the initial forest cover as well as oil palm suitability. Interestingly, the district splitting process did not result in descriptively higher deforestation around the splits. Neither parent, nor child districts experience higher deforestation rates neither right before nor after the administrative splits occur. When all these controls are included, the pre-election coefficient becomes somewhat smaller and hence also less precisely estimated, whereas the coefficient on palm oil price exposure remains the same.

The main result of this paper is presented in column (5) of Table 1 and focuses on the interaction between political and economic incentives. The interaction of the pre-election indicator with the palm oil price exposure shows a highly significant positive coefficient. Hence, agricultural incentives to convert land to oil palm plantations play a larger role before elections, or, pre-election incentives result in more deforestation in times and places when and where the agricultural conditions favor land conversion to oil palm. While a one standard-deviation (SD) increase in palm oil price exposure increases deforestation by 7.1%, this effect doubles in pre-election years. When taken together, a pre-election year in a place experiencing a one SD higher price exposure results in 18.8% higher deforestation. We will retain the specification of this column as our preferred baseline specification throughout the following analyses.

4.2. Robustness and identification issues

In our setting, causal identification of the interaction between pre-election and price incentive effects relies on two uncorrelated sources of identifying variation: election timing and local exposure to palm oil price variation. In what follows, we discuss and test both sources of variation using an array of sensitivity, IV and placebo tests, and shortly address some further concerns.

Timing of mayoral elections. The variation in political incentives due to election timing is identified by asynchronous local elections across Indonesia, which result in idiosyncratic variation in the timing of local electoral races (and hence pre-election year effects). The largest part of this variation has been historically induced by the timing of when the first post-Suharto majors entered into office as past executives were let to serve their full five-year term first (Skoufias et al., 2011; Sjahrir et al., 2013). The early timing of the transition did not depend on the performance of the previous mayors or local institutional quality and has been argued to be fully exogenous (Martinez-Bravo, 2014; Martinez-Bravo et al., 2017). Table A3 in the Appendix provides further evidence on the orthogonality of the idiosyncratic election timing to deforestation by showing no difference between districts that adopted direct mayoral elections early on versus districts that introduced direct elections only in later years.

If timing of the mayoral elections stayed entirely mechanical after this idiosyncratic transition process, we would find exact five-year cycles in all districts. In reality, shifts in the timing of mayoral elections did arise sporadically, either for administrative or political reasons. A handful of elections were called early due to corruption scandals, and at times elections were also shorty postponed due to administrative considerations, e.g, in order not to overlap with the national elections held in 2009 (as can also be seen in the timing of monthly elections in Fig. A2 in the Appendix). Such adjustments were relatively infrequent and 94% of all parent districts remained within a five to six year cycle.[8] The predominant reason for deviations from the fixed cycle arises instead from district splits as newly formed child districts often elect a new mayor earlier.[9]

Such irregular elections could also go along with changes in the administrative capacity and lead to excess deforestation before elections that does not only reflect the role of election incentives. We ensure that our results are robust to deviations from the fully mechanical timing of elections in two ways. In our preferred specifications, we control for the timing of district splits in a fully flexible way, including two lags and leads separately for parent and child districts. This helps us to distinguish between changes in deforestation that happen before elections and changes that may accompany the district splitting process (and elections in new entities linked to that). However, in selected cases, corruption scandals may have forced some districts to pre-emptively introduce new elections. In these cases, election timing is not mechanical anymore as it is prone to strategic behavior by corrupt administrations. Here, the pre-election year effect could also reflect a generally worse institutional environment beyond the simple election incentives. This does not invalidate the average result, but affects its interpretation: instead of political incentives, a bad institutional environment could also be behind excess deforestation before elections.

To show that our results are not driven by such irregularities in election timing, Table A4 in the Appendix repeats the main specification for observations within the regular cycle only, for non-splitting districts as well as for districts that never deviated from the fixed election cycle. In this selective sample, we can be more certain that a pre-election year effect does not reflect general institutional challenges or the effect of district splits but is linked to politicians' pre-election behaviour.[10] Estimates of the political and price interaction in the more restrictive sample of regular elections increase in magnitude, ensuring that our main result of interest is not driven by irregularities in election timing. However, in the most restrictive specification that focuses only on districts that always followed a 5-year cycle precisely (and hence were neither newly formed during the time period, nor faced end-of-year elections or other delays), only the interaction stays significant.

Shift-share measure of economic incentives. Our price exposure measure (see equation (1)) consists of an interaction that can also be understood as a shift-share measure (Jaeger et al., 2018). The spatial variation in soil suitability can be conceptualized as a single 'share' component, whereas the 'shift' is derived from time variation in the global market price of palm oil.[11] Soil suitability is not fully exogenous, only predetermined, as local geo-climatic conditions can be expected to be correlated with a host of other location-specific characteristics that may affect deforestation pressure. That is why it is crucial to control first of all for location and time fixed effects. More importantly, in all our regressions we control for the initial suitability interacted with a time trend (which is also in the spirit of the suggestions by Borusyak et al., 2018 and Goldsmith-Pinkham et al., 2020). This helps to make sure that our shift-share measure is not driven by underlying trends that are due to further local factors, spuriously correlated with soil suitability.

The 'shift' component of our price exposure measure reflects the global market price variation of palm oil. This poses one additional challenge. Indonesia is the worlds' largest palm oil producer and exporter, producing about 54% of total world output in 2015/16 (USDA, 2019). The world market supply of palm oil has been increasing continuously through the ongoing extension of oil palm area. Demand shortages during the financial crisis and later the relatively lower pace of demand expansion have

[8] Six year cycles are predominantly found in districts that held elections right after New Year. In the latest years, the central government has pushed towards synchronising local elections and about half of all mayoral elections took place on the 9th of December in 2020. Such tendencies are not yet present in our time frame, which ends in 2016, as also shown in the monthly election timing in Fig. A2 in the Appendix.

[9] After out of all 151 district separations between 2001 and 2016 (cf. Fig. A4 in the Appendix), 38% of the first elections in the newly formed child districts still coincide with the parental election cycle, but the rest occurs earlier.

[10] Excluding all newly formed district parts from our analysis comes at a cost as hopes to exploit natural resources have been an important motivation behind district splits (Fitrani et al., 2005; Burgess et al., 2012).

[11] Other studies on the oil palm expansion in Indonesia rely on oil palm suitability as a share and the national trend of oil palm area expansion as a single shift (Kubitza and Gehrke, 2018; Krishna and Kubitza, 2021).

Table 2
Sensitivity: Instrumenting for palm oil price exposure.

Dependent variable:	*Asinh* Deforestation	Palm oil price exposure	Palm oil price exposure × Pre-el. year	*Asinh* Deforestation
	OLS (1)	1st stage (2)	1st stage (3)	2nd stage (4)
Pre-election year	0.042*	0.013	0.038**	0.047*
	(0.025)	(0.012)	(0.019)	(0.025)
Palm oil price exposure	0.071**			0.003
	(0.032)			(0.047)
Pre-election year × Palm oil price exposure	0.075**			0.155*
	(0.036)			(0.094)
Oil palm suitability × Trade-weighted global GDP		0.033***	0.001	
		(0.000)	(0.001)	
Pre-election year × Oil palm suitability × Trade-weighted global GDP		0.000	0.026***	
		(0.001)	(0.003)	
Sanderson-Windmeijer mult. F-stat.		1452.6	84.1	
Kleibergen-Paap Wald F-stat.:				39.3
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Observations	6352	6352	6352	6352

Note: The table shows the effects of palm oil price incentives and election incentives on deforestation (measured as the inverse hyperbolic sine of yearly forest losses), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Estimates absorb district and year fixed effects following Baum et al. (2002) and Correia (2016), using the Stata packages *ivreghdfe* and *ivreg2*. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. The critical value for a weak identification test resulting in maximal 10% of bias in IV coefficients is 7.0 (Stock and Yogo, 2002). Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

started to put downward pressures on the world market price of palm oil. Assuming a less than perfectly elastic demand for palm oil, the agricultural expansion of oil palm area (and hence deforestation) are likely to have contributed to a lowering of the world market price. This induced a negative reverse correlation between aggregate deforestation and palm oil prices. We control for common aggregate fluctuations using year fixed effects and only link differences in agro-climatic price exposure across districts to changes in deforestation. Nonetheless, the endogenous price reaction (driven by oil palm expansion induced deforestation) will lead us to under-estimate the relative importance of palm oil price variation driving the demand for new land. We therefore interpret our estimates as a lower bound of the possible price incentive effects.

As a further sensitivity check, we also provide an upper bound of the price estimate. We use an instrumental variable (IV) approach that potentially over-estimates the effects of palm oil price incentives by linking them to fluctuations in global demand for agricultural commodities. We generate our instrument by interacting local oil palm suitability with potential shifts in global demand for Indonesian palm oil, rather than palm oil prices. We approximate this demand component by a trade-weighted global GDP measure, which includes all trading partners that have been importing oil seeds from Indonesia over an initial period, and weights their real GDP figures by each country's market share in total Indonesian oil seed exports (see Appendix A.1 for a more detailed explanation). This approach (wrongly) assumes that all global demand and business cycle effects would go through the global demand for palm oil (and not through that of other crops with potentially correlated local suitability).

Table 2 presents the IV results. The instruments are sufficiently strong for predicting variation in palm oil price exposure and its interaction at the first stage (resulting in a joint Kleinberger-Paap F-statistic of 39.3). However, as these IV coefficients are likely over-estimated, we consider them only as a useful sensitivity check. The estimated coefficients on elections and prices decrease and lose significance compared to the baseline results (Table 1), whereas the price coefficients interacted with the pre-election indicator increase substantially. These results show that, in particular, the pre-election effects of palm oil prices, which utilize the quasi-experimental variation from elections, also persist if we use a global demand driven measure of the variation in palm oil prices.

A further concern of omitted variable bias arises because palm oil prices may also proxy for a further range of agricultural incentives to plant other crops. Although oil palm has indeed played a central role in the Indonesian economy, incentives to plant other crops can be expected to also influence deforestation. We check for the robustness of our results by also controlling for the fluctuation in further crop prices. Just like for palm oil, we measure variation in local crop price exposure for ten other main agricultural crops by interacting deviations in yearly prices from past five-year averages with a local suitability index (as in equation (1)). To reduce the dimensionality of the comparison, we combine the individual price indices by weighting them by the relative national importance of each crop into a single *Other crop price exposure* measure (see Appendix A.1 for further detail). As for the weights, we contrast results using FAO production data with data from the Indonesian System of National Accounts (SNA).

Table 3 contrasts the role of palm oil with a combined measure of other price incentives, which also show a strong correlation with deforestation. A one standard deviation increase in the weighted price exposure measure is linked to about 2–8% more

Table 3
Sensitivity: Palm oil vs. other agricultural crop prices.

	(1)	(2)	(3)	(4)
Pre-election year	0.045*	0.041	0.047*	0.041
	(0.025)	(0.025)	(0.025)	(0.025)
Palm oil price exposure		0.051*		0.059**
		(0.028)		(0.029)
Pre-election year × Palm oil price exposure		0.083**		0.079*
		(0.039)		(0.042)
Other crop price exposure	0.083*	0.054	0.071	0.026
	(0.045)	(0.040)	(0.044)	(0.041)
Pre-election year × Other crop price exposure	0.019	−0.017	0.045	−0.008
	(0.031)	(0.032)	(0.034)	(0.039)
Source of crop weights	FAO	FAO	SNA	SNA
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Observations	6352	6352	6352	6352
Adj. R^2	0.890	0.890	0.889	0.890

Note: The table shows the effects of palm oil price incentives and election incentives on deforestation (measured as the inverse hyperbolic sine of yearly forest losses), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Other agricultural price exposure is a weighted average of individual crop price exposures, weighted by their relative economic importance within Indonesia in year 1995–2000 using the FAO statistics and in year 2000 using the Indonesian System of National Accounts (SNA) data by the Central Bureau of Statistics (BPS). Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size, the local oil palm suitability index and a weighted average of other crop suitability indices. Palm oil price exposure and other crop price exposure have been normalized by subtracting their mean and dividing by their standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

deforestation, which is only significant when weighting crops using FAO weights (in column 1). The interaction between the pre-election year and the aggregated crop price incentives turns out insignificant. When accounting for palm oil prices as well as other crop prices together (in columns 2 and 4), the interaction of pre-election incentives is robustly linked to palm oil prices, while staying insignificant for the combined price index of other crops. This corroborates our expectation that palm oil may indeed have a more special role in explaining the political economy of deforestation in Indonesia. Alternatively, Table A5 in the Appendix presents pairwise tests between price exposure of each of the other important agricultural crops and palm oil price exposure. Results on other crop price exposures are only sporadically significant and there is no other crop that would dominate the pre-election effects of oil palm.

The measure of palm oil price exposure crucially relies on the local geoclimatic suitability to grow oil palm. However, this may also simply measure agricultural suitability in general, in which case our price exposure measures again would not be palm-oil specific, preventing us from giving a causal interpretation to the estimated effects. We test for this by generating alternative (false) palm oil price exposure measures that substitute for the oil palm-specific geo-climatic variation using other crop suitability indices, and multiply them with palm oil prices. Table A6 presents the placebo estimates that re-run the baseline estimates (from equation (2)) using these false measures. The price exposure coefficients remain positive and significant, indicating that for the general price effect, oil palm suitability cannot be clearly distinguished from overall agricultural suitability, as both are highly correlated. However, the interaction coefficient between the pre-election year indicator and the alternative price exposure remains small and insignificant across all placebo regressions. This strengthens our claim that it is the interplay of incentives to plant oil palm with political incentives that drives deforestation rates in Indonesia.

Further concerns. Our main results from Table 1 are robust to how deforestation is measured, and to which districts we include in the analysis. The interaction between price exposure and electoral incentives persists irrespective of what canopy density cut-offs we use to define a forest, or which inclusion criteria we use to define our district sample. As a further concern, the estimated significance levels in our base model may be too high due to under-rejection issues. We check for the sensitivity of our estimates using different clustering methods and correcting for multiple hypothesis testing, which also addresses the overrejection problem in shift-share designs (Adão et al., 2019). Adjustments render our estimates less significant, but our main interaction coefficient of interest remains significant at the 90% level in all specifications. A more detailed description of all these results is presented in section A.2.

4.3. Political mechanisms and policy instruments

Our main results show that political incentives and the demand for palm oil are jointly driving forest losses in Indonesia. While this finding seems to be fairly robust, it does not pinpoint yet any clear mechanisms for why deforestation increases before elections, and especially in times of rising palm oil prices. When the next elections are drawing nearer, local politicians may target local economic development in general in order to signal their competence towards their voters. Additionally, local administrations may increase the sales of licenses that allow land conversion before elections in order to collect additional funds

Table 4
Politics and policies: Direct vs. indirect elections.

	(1)	(2)	(3)
Pre-election year (indirect)	−0.039	−0.042	0.043
	(0.083)	(0.084)	(0.033)
Pre-election year (direct)	0.057*	0.056*	0.042
	(0.032)	(0.032)	(0.033)
Palm oil price exposure		0.082***	0.073**
		(0.031)	(0.032)
Pre-election year (indirect) × Palm oil price exposure			−0.006
			(0.118)
Pre-election year (direct) × Palm oil price exposure			0.085**
			(0.041)
District fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Further controls	Yes	Yes	Yes
Observations	6352	6352	6352
Adj. R^2	0.889	0.890	0.890

Note: The table shows the effect of price incentives and the effect direct and indirect elections on deforestation (measured as the inverse hyperbolic sine of yearly forest losses), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

that can be used to finance direct hand-outs or other policies that are valued by the local constituencies in the short run. Or, as suggested by anecdotal evidence (see section 2.2), local politicians may also be more directly connected to the oil palm sector and may especially rely on their patronage networks for support when elections are drawing nearer.

In what follows, we present a series of further results that investigate the potential role of political dynamics as well as local policy actions that can be linked to deforestation. First, we compare whether incentives differ before direct and indirect elections and test whether mayoral term limits contribute to explaining the pre-election effect. Second, we use information on agricultural concession areas to analyse descriptively whether deforestation happens on future concession areas. Third, we use information on the yearly amount of newly-allocated licenses to extract timber or wood fiber from the forest in order to see whether policy actions are also directly linked to the economic incentives established above.

Our main results did not distinguish between direct mayoral elections that were introduced in 2005 and the earlier system, where mayors were indirectly appointed by democratically elected local parliaments. Table 4 decomposes the pre-election effects within these two electoral systems, distinguishing between the effects of direct and indirect elections. We expect to find stronger political incentives to pursue populist policies in the regime with direct elections, as they establish a closer link between politicians' actions and their electoral success (Bardhan, 1997). This could increase deforestation pressure directly if the benefits of land conversion are fairly widespread, or indirectly, if revenues from potentially corrupt activities linked to land conversion are then used for financing hand-outs or other voter-pleasing policies before elections. The results in Table 4 are as expected, showing much clearer increases in deforestation before direct elections, as well as in interactions with palm oil prices.

District mayors face a two-term limit for being in office. Second term mayors and first term mayors re-running for office potentially face different incentives to engage in corrupt activities. We collected historical information on Indonesian mayors by scraping Wikipedia pages of Indonesian districts. Due to gaps in the timelines, we can identify if mayors are in their first or second term in 82% of all elections. In years without any information on the mayor incumbency, we assume the mayors are in their first term, which could underestimate the effect of second term mayors in our sample. Results are shown in Table 5. The indicator for second term mayors in column (1) is positive but insignificant, and remains insignificant when interacted with palm oil price exposure in columns (2) and (3). Thus, we find no differences in deforestation resulting from a two-term limit. This may be due to political elites exchanging positions in public office, as we can see descriptively that vice mayors are frequently inaugurated after the second term of their predecessors, but may also reflect the increasingly dynastic nature of Indonesian politics (The Economist, 2020).[12] In such a setting, local administrations may equally overuse forest resources before elections either to support their own campaigns or to help the electoral bid of the vice mayor or even another relative.

To win elections, politicians can support the oil palm sector by reducing environmental enforcement efforts, subsidizing agricultural inputs, or handing out new agricultural licences. We further investigate deforestation dynamics focusing on the location of deforestation within vs. outside of future concession licences. We classify deforestation according to the latest observed localization of official concession areas (in 2014–2017, see Appendix A.1 for details).[13] We are able to distinguish between three types of concessions for agricultural land use: concessions for logging, for wood fiber extraction (which basically results in managed forest use), and for oil palm plantations. By that, we investigate how deforestation patterns differ on land types that ended up

[12] https://www.economist.com/asia/2020/12/03/indonesian-politics-is-becoming-a-family-affair.

[13] Fig. A7 in the Appendix displays the final concession boundaries, whereas Fig. A8 shows the trends in newly established concession areas over time.

Table 5
Politics and policies: The role of second term mayors.

	(1)	(2)	(3)
Palm oil price exposure		0.081***	0.079**
		(0.031)	(0.033)
Second term mayor	0.035	0.032	0.031
	(0.032)	(0.032)	(0.032)
Palm oil price exposure × Second term mayor			0.011
			(0.029)
District fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Further controls	Yes	Yes	Yes
Observations	6352	6352	6352
Adj. R^2	0.889	0.890	0.890

Note: The table shows the effect of palm oil price incentives and the institutional effect of second term mayors on deforestation (measured as the inverse hyperbolic sine of yearly forest losses), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

as a concession area for a specific use around the end of our period of analysis. A substantial drawback of this approach is that we cannot distinguish between the timing of deforestation and concessions relative to each other. As we are comparing a large number of outcomes to each other, in brackets we also report standard errors that correct for multiple hypothesis testing across models (Benjamini and Hochberg, 1995). This reduces the significance of each result substantially and makes the comparison at best useful for descriptive purposes only.

In column (1) of Table 6, we estimate the effect of palm oil prices and pre-election incentives on deforestation located on area that never received agricultural concession licences. While price incentives significantly increase deforestation in those areas, political incentives are insignificant but of similar magnitude as our baseline results. In column (2), on areas that were later licensed under one of the three concession types, we observe significantly positive estimates for both palm oil prices and our main interaction of interest. Differentiating further by the type of concession, we see significantly more forest loss on oil palm concessions due to price and interaction effects, contributing to more deforestation when political and economic incentives are aligned. However, we also see significantly less deforestation before elections at the baseline (or, when price incentives are less favourable, see column 3). This negative pre-election effect at baseline for deforestation on non-concession areas could reflect several reasons. It could arise from spillover effects if political incentives drive logging activities out of already licensed areas and into not-yet-licensed areas. It could also indicate substitution effects if politicians in high suitability (and hence later converted) areas engage in other means of revenue generation before elections as long as deforestation is not the most profitable activity to pursue. Unfortunately, as our oil palm concession data is time invariant, we cannot further analyse the relative timing of concession areas and deforestation, and hence are unable to demonstrate actual spillover dynamics. Finally, no similar dynamics can be observed on areas that end up with logging or wood fiber concessions (see columns 4 and 5).

A more direct link between electoral incentives and policy mechanisms is investigated in Table 7, where we use the legal timing of new concessions for logging and extracting wood fiber to estimate the effect of prices and election incentives on the size of newly licensed concession areas. Although logging and wood fiber licences are officially designated for forest good production, they could still serve as a transition stage before setting up oil palm plantations or as a disguise for oil palm areas. The baseline palm oil price exposure coefficients in this setting are generally insignificant.[14] Unlike in all previous specifications, wood fiber licences are higher not only before but also during and after elections, with a peak in the post-election year (see column 1). The dynamics of logging concessions move in the opposite direction but not significantly so (column 3). The interaction between prices and elections is significantly positive after elections (column 2) for wood fiber, but not for logging licences (column 4). In combination with the effects on deforestation by concession type, this provides suggestive evidence that licensing and deforestation go hand-in-hand but also shows that the actual licenses may follow with a delay. Local politicians may either encourage or ignore deforestation before elections and then legalize the new oil palm areas afterwards. In this process, they seem to make use of wood fiber concessions, which are direct substitutes for the use of the increasingly scarce land resources.

4.4. Ecological impacts and land-use dynamics

Table 8 investigates the importance of our results from an ecological perspective, distinguishing between deforestation on different biomes and on primary and non-primary forests. First, we categorize the initial forest area into five mutually exclusive biomes: lowland, upland, montane, wetland and peatland (following Margono et al., 2014; Gumbricht et al., 2017) (also dis-

[14] Table A7 in the Appendix shows a somewhat more pronounced relationship between timber price exposure and wood and logging concessions.

Table 6
Politics and policies: Deforestation by final legal concession status.

Dependent:	Forest losses outside concessions	Forest losses on final concession areas Any	Oil palm	Logging	Fibre
	(1)	(2)	(3)	(4)	(5)
Pre-election year	0.040	−0.062	−0.082	−0.008	0.016
	(0.027)	(0.046)	(0.036)**	(0.044)	(0.021)
	[0.034]	[0.056]	[0.049]*	[0.044]	[0.025]
Palm oil price exposure	0.068	0.075	0.055	0.040	0.028
	(0.032)**	(0.035)**	(0.026)**	(0.036)	(0.031)
	[0.040]*	[0.044]*	[0.033]*	[0.045]	[0.037]
Pre-election year × Palm oil price exposure	0.057	0.079	0.070	0.062	0.022
	(0.039)	(0.035)**	(0.028)**	(0.037)*	(0.034)
	[0.049]	[0.047]*	[0.041]*	[0.048]	[0.037]
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes
Observations (Districts)	6352	6352	6352	6352	6352
Adj. R^2	0.876	0.984	0.985	0.983	0.984

Note: The table shows the effect of palm oil price incentives and elections on deforestation (measured as the inverse hyperbolic sine of yearly forest losses within existing agricultural concessions areas in 2015), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Standard errors in brackets are corrected to account for multiple variable testing across columns following (Benjamini and Hochberg, 1995). Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

Table 7
Politics and policies: New wood fiber and logging concessions.

Dependent variable:	*asinh* New wood fiber concessions		*asinh* New logging concessions	
	(1)	(2)	(3)	(4)
Pre-election year	0.382*		0.189	
	(0.197)		(0.197)	
Election year	0.359***		−0.088	
	(0.135)		(0.154)	
Post-election year	0.533***	0.355**	−0.136	−0.166
	(0.162)	(0.152)	(0.150)	(0.149)
Palm oil price exposure (PE)	0.184	0.121	−0.082	−0.082
	(0.124)	(0.106)	(0.130)	(0.110)
PE × Pre-election year	−0.064		0.044	
	(0.196)		(0.138)	
PE × Election year	−0.191**		−0.042	
	(0.133)		(0.160)	
PE × Post-election year	0.185	0.242*	−0.086	0.090
	(0.172)	(0.158)	(0.140)	(0.127)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Observations	5558	5558	5558	5558
Adj. R^2	0.168	0.167	0.186	0.186

Note: The table shows the effect of palm oil price incentives and elections on the yearly expansion of agricultural concession areas (timber and logging), across 397 districts between 2001 and 2015, with an initially forest cover of at least 40% in 2000. Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size, the local oil palm suitability index and initial primary forest size to proxy the potential of high value timber. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

played in Fig. A9 in the Appendix). These five biomes are of different agricultural value, with lowland and wetland areas being especially suited for agricultural production. The last two columns distinguish between primary forest areas that lie within and non-primary forest areas that lie outside of the tropical rainforest. Forest losses on primary forest areas measure the irreversible loss of tropical rainforests and hence are expected to induce more negative ecological effects (Garg, 2019). As before, standard errors in brackets correct for multiple hypothesis testing across models.

In lowland areas, the results are very close in magnitude to the baseline coefficients from Table 1, showing increases in deforestation with increasing palm oil price exposure and also a significant interaction of price exposure with upcoming elec-

Table 8
Ecology and land use: Ecological differences in deforestation.

Dependent variable:	Lowland forest loss	Upland forest loss	Montane forest loss	Wetland forest loss	Peatland forest loss	Primary forest loss	Non-prim. forest loss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-election year	0.043	0.017	0.055	0.035	0.042	0.074	0.054
	(0.028)	(0.046)	(0.069)	(0.065)	(0.062)	(0.044)*	(0.026)**
	[0.044]	[0.055]	[0.155]	[0.114]	[0.128]	[0.068]	[0.042]
Palm oil price exposure	0.079	0.029	0.022	0.018	0.072	0.042	0.067
	(0.029)***	(0.053)	(0.046)	(0.051)	(0.055)	(0.046)	(0.033)**
	[0.043]*	[0.095]	[0.072]	[0.059]	[0.094]	[0.115]	[0.051]
Pre-election	0.078	−0.042	−0.027	0.140	0.015	0.052	0.069
year × Palm oil price	(0.041)*	(0.055)	(0.073)	(0.051)***	(0.062)	(0.059)	(0.033)*
exposure	[0.060]	[0.116]	[0.088]	[0.077]*	[0.062]	[0.144]	[0.051]
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6352	6352	6352	6352	6352	6352	6352
Adj. R^2	0.908	0.869	0.872	0.867	0.835	0.930	0.877

Note: The table shows effects of price incentives and election incentives on different types of deforestation (measured as the inverse hyperbolic sine of yearly forest losses), across 397 districts between 2001 and 2016, with an initially forest cover of at least 40% in 2000. Forest losses are classified by biome location (lowland, upland, montane, wetland, peatland) and forest type (primary and non-primary). Estimates account for district and year fixed effects. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Standard errors in brackets are corrected to account for multiple variable testing across columns following (Benjamini and Hochberg, 1995). Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

tions. However, only the palm oil price coefficient retains significance after correcting for multiple hypothesis testing. The other biome showing similar dynamics is wetland, the agricultural conversion of which is somewhat more challenging but also highly profitable. Here the interaction of palm oil price exposure with the pre-election indicator turns out more significant and larger in magnitude, documenting that agricultural incentives matter mainly before elections. This interaction coefficient also stays significant when using the stricter standard errors. On the agriculturally less valuable upland, montane, and peatland areas, point estimates on prices and their interactions with the pre-election indicator are substantially smaller and insignificant. The last two columns of Table 8 do not show strong differences between primary and non-primary forest areas. Taken together, even though statistically relatively weaker, these results provide a useful consistency check for our baseline results, showing agricultural incentives before elections to be more visibly linked to deforestation in prime agricultural areas.

New land-use maps enable us to also perform an exploratory analysis of the dynamics of converting forests into industrial-scale oil palm plantations. Combining remote sensing with visual interpretation, Austin et al. (2017) produce maps of industrial oil palm plantations in five-year intervals from 2000 to 2015 for the islands of Sumatra, Kalimantan, and Papua (see Fig. A10 in the Appendix). Based on their data, we identify deforestation that is located on pre-existing oil palm plantations, newly created plantations, or other area. These maps also allow us to analyze when deforested area is being converted into oil palm plantations, distinguishing between short-term conversion (that happens within a 1–5 year window) and mid-term conversion (within a 6–15 year window). We assume that forest losses that are only identified as oil palm plantations at a later point in time must have had an alternative transitory use in the meanwhile.[15] The regression results based on these maps can only be considered as indicative for two reasons. First, splitting deforestation by future land-use type introduces an element of endogenous selection into our sample splits and results could also reflect spillovers from one land-use type to another. Second, splitting up our sample will require us to engage in a series of multiple comparisons, which may again end up under-powered once we correct for multiple hypothesis testing.

Table 9 shows the results using the alternative deforestation measures that differentiate among forest loss by the location and timing of oil palm plantations. When we correct for multiple hypothesis tests across the six models (Benjamini and Hochberg, 1995), none of the coefficients retain their significance. Nonetheless, the relative magnitudes of the results are indicative for the general patterns of oil palm induced deforestation. Column (1) reproduces our main result for total deforestation in the reduced sample located on the three islands. It shows positive though insignificant palm oil price exposure and pre-election coefficients, and a positive marginally significant coefficient on the interaction between electoral and palm oil price incentives. This result is mirrored in the bulk of the sample, representing forest losses located in areas that did not become industrial-scale oil palm plantations until 2015 (see column 2), as well as in areas that have been converted to oil palm plantations (see column 3). The interaction effect between political and economic incentives increases substantially when we focus on deforested areas that became directly converted to oil palm within five years (see column 4). For forest areas that where converted to oil palm in the

[15] Descriptive statistics on forest loss in Table A8 in the Appendix show that within the reduced sample of 231 districts (on three islands), 5% of total forest loss is located on pre-existing oil palm plantations, 24% on new plantations, and 72% is located outside of industrial-scale oil palm plantations.

Table 9
Ecology and land use: Oil palm expansion and deforestation.

Dependent:	Total forest loss	Forest losses on *non*-oil palm	Forest losses on *new* oil palm areas by 2015			Forest losses on *existing* oil palm
			Any	Short-run	Long-run	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-election year	0.029	0.041	−0.079	−0.155	−0.164	−0.001
	(0.025)	(0.024)*	(0.058)	(0.086)*	(0.090)*	(0.048)
	[0.030]	[0.032]	[0.079]	[0.122]	[0.129]	[0.048]
Palm oil price exposure	0.051	0.047	0.030	0.313	−0.165	−0.026
	(0.039)	(0.038)	(0.088)	(0.123)**	(0.096)*	(0.051)
	[0.051]	[0.047]	[0.164]	[0.218]	[0.129]	[0.081]
Pre-election year × Palm oil price exposure	0.059	0.057	0.163	0.359	0.148	−0.001
	(0.033)*	(0.032)*	(0.066)**	(0.158)**	(0.114)	(0.043)
	[0.046]	[0.044]	[0.113]	[0.256]	[0.147]	[0.043]
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.905	0.897	0.963	0.875	0.957	0.976
Observations	3465	3465	3465	3465	2310	3465

Note: The table shows the effect of palm oil price incentives and elections on forest clearings for oil palm plantations (measured as the inverse hyperbolic sine of yearly forest losses located on remotely sensed oil palm plantations in 2000, 2005, 2010 and 2015), across 231 districts between 2001 and 2016, with an initially forest cover of at least 40% on the islands Sumatra, Kalimantan, and Papua. Further controls include indicators of district splits (separately for mother and child districts) as well as time trends varying by initial forest size and the local oil palm suitability index. Palm oil price exposure has been normalized by subtracting its mean and dividing by its standard deviation. Robust standard errors are clustered on a level of 251 original parent districts and reported in parentheses. Standard errors in brackets are corrected to account for multiple variable testing across columns following (Benjamini and Hochberg, 1995). Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

long-run (more than 5 years after deforestation), effects decline and become insignificant.[16] Finally, column (6) of Table 9 can be considered a placebo check. It shows deforestation in areas that have already been converted to oil palm at the beginning of our period. Here "deforestation" may reflect replanting or the conversion of an oil palm plantation to some other use. In this specification, the interaction effect becomes virtually zero, indicating that neither election nor palm oil price incentives play a role in the agricultural decisions on replanting or conversion for alternative use. The results suggest that politicians do not simply focus more strongly on promoting agricultural activities (like replanting) before elections, but mainly influence the immediate conversion of forests to new oil palm plantations.

5. Discussion

Our results show that agricultural incentives to grow oil palm and election incentives reinforce each other and significantly drive forest losses in Indonesia. Deforestation increases by 4.2% in years with high price exposure levels and by 7.1% in years before mayoral district elections. The joint effect amounts to 18.8% of forest losses when districts experience an increase in palm oil price exposure within the pre-election year. Our results are robust to controlling for differential trends based on initial conditions (forest cover and soil suitability) and the district-splitting process. The interaction between political and economic incentives is more pronounced after the introduction of direct mayoral elections in 2005. No statistical evidence for changes in forest losses is found in newly formed administrative districts or in districts with second term mayors. When focusing on land-use transitions more specifically, the interaction of economic and political incentives fuels deforestation more strongly on areas that are converted to oil palm in the short run and on areas that are to be officially licensed as oil palm plantation by the end of our time period. Finally, we show that election and palm oil price incentives contribute to the issuing of new wood fiber concessions after elections.

Our findings confirm the political budget cycle theory that predicts higher public spending before elections (Nordhaus, 1975; Alt and Rose, 2009). Increasing deforestation in pre-election years is indicative of district administrations reducing environmental enforcement efforts, handing out additional agricultural concession licences, or intensifying the support of the oil palm sector. These pre-election effects are also in line with the empirical literature on political budget cycles that shows increases in corrupt activities right before elections to generate funds for handouts and vote buying (Pailler, 2018; Khemani, 2004; Pereira et al., 2009; Aidt et al., 2020). The recent literature on Indonesia's decentralization and democratization process has shown similar context dependent outcomes as a result of political changes (Burgess et al., 2012; Alesina et al., 2019; Martinez-Bravo et al., 2017; Gonschorek, 2021). The central role of agricultural incentives for forest conservation found here confirms previous

[16] The negative baseline pre-election effect in these two specifications would suggest even (marginally significant) reductions in deforestation before elections on areas that subsequently become converted to oil palm when price incentives are unfavourable. However, this effect turns into strongly positive in the short run, once price incentives favour the transition to oil palm.

empirical work conducted in Indonesia and Brazil (Hargrave and Kis-Katos, 2013; Assunção et al., 2015; Gatto et al., 2017).

Our results do not give a clear indication as to whether before elections local politicians are facing an administrative over-burden, engage in corrupt deals to generate funds, or simply try to signal competence to their electorate. However, the nature of Indonesian "money politics" (Aspinall and Berenschot, 2019; Aspinall and van Klinken, 2010), together with the rich anecdotal evidence on the entrenchment between the oil palm sector and local politics (see section 2 and Appendix A.3) supports the interpretation of our findings as signs of political favoritism between the oil palm sector and local political elites. While politicians may try to secure election funds also in other ways (e.g., through corruption in public procurement), the strong pre-election effect in years of high palm oil prices suggest that politicians are shifting their rent extraction activities to the more profitable oil palm sector. At the same time, deforestation does not vary around district splits, which suggests that a simple lack of administrative capacity is less likely to drive the pre-election effect. Finally, the increase in wood fibre concessions in the year after elections, points to an exchange between reduced pre-election enforcement and a post-election legalization of already deforested areas.

The transition to large-scale oil palm agriculture has been accompanied by large losses in natural landscapes, negative ecological outcomes, and reductions in biodiversity services (Austin et al, 2017, 2019; Clough et al., 2016; Denmead et al., 2017; Busch et al., 2015; Chen et al., 2019). In addition to the environmental externalities, the expansion of the palm oil industry has been related to land consolidation, conflict over traditional land rights, and water scarcity (Colchester et al., 2007; Rist et al., 2010; Abram et al., 2017; Merten et al., 2016). At the same time, local populations also suffer from detrimental health effects from forest fires for land clearing (Frankenberg et al., 2005; Koplitz et al., 2016; Marlier et al., 2015; Rangel and Vogl, 2016). However, these ecological and partly also social costs stand in a clear trade-off to substantial local economic benefits of the oil palm transition in Indonesia (Drescher et al., 2016; Grass et al., 2020). Among others, the adoption of oil palm increased land productivity, farm income, female labor force participation, and educational attainment (Krishna et al., 2017; Gatto et al., 2017; Klasen et al., 2016; Dib et al., 2018; Kubitza and Gehrke, 2018; Kubitza et al., 2018). Nonetheless, a land use transition process that is strongly driven by politicians' short term self-interest is unlikely to result in outcomes that adequately factor in social and environmental externalities.

While indicative of a link between pre-election effects and the oil palm sector, our empirical results are subject to a range of limitations. Our main results potentially under-estimate the demand effects of palm oil price variation as the expansion of oil palm in Indonesia must have put a downward pressure on palm oil prices. Our IV strategy presents an upper-bound of our estimates, but is likely over-estimated as it assumes that global demand shocks are only effected trough the oil palm sector. Further results that try to pinpoint on which types of areas deforestation occurs, engage in a wide range of multiple comparisons with limited statistical power. Finally, results that condition deforestation on final land-use concession types also face the limitation of slicing up the sample based on ex post (and hence endogenous) outcomes. Nonetheless, these further pieces of often descriptive evidence lend general support to our main result.

The external effects from the loss of tropical rainforests are increasingly perceived at the local as well as global level. There have been some attempts to slow down the extent of deforestation for oil palm at the national level. In 2009, Indonesia committed to substantial reductions in greenhouse gas emissions (by 26% in 2020). The Ministry of Forestry implemented a moratorium on new agricultural licenses in 2011, although with a limited effectiveness (Busch et al., 2015). In parallel, palm oil certification standards from non-governmental organizations have been successful at mitigating the negative effects of agricultural production (Carlson et al., 2017; Miteva et al., 2015). Our results also suggest that falling prices for palm oil will reduce the pressure to deforest especially in high-suitability areas. Such reductions in deforestation pressure will be relatively speaking the largest before elections. So reducing global demand for palm oil could mitigate excessive deforestation in this setting. Finally, increasing the opportunity costs of deforestation thorough federal enforcement campaigns could also help to shift local incentives and reduce deforestation rates (Börner et al., 2015a; Cisneros et al., 2015; Arima et al., 2014).

6. Conclusion

Our paper investigates how agricultural and political incentives drive deforestation in Indonesian districts. Using a panel over 16 years, we show that deforestation is higher in the year before mayoral elections than in other years within the mayoral election term. Deforestation is also larger in those regions that are more exposed to improving price incentives to grow oil palm, but also other relevant agricultural crops. More substantially, the political and the agricultural incentives interact: Deforestation increases by more before elections in those districts that are more exposed to favourable palm oil price shocks whereas other relevant crops do not seem to have similar pre-election effects.

These results provide evidence that local politicians expect to receive short-term electoral benefits from promoting agricultural (and in the specific Indonesian context, oil-palm driven) development. They also support anecdotal evidence on a special link between the dominant oil palm sector and local politicians in Indonesia, and help to add nuance to the literature on political budget cycles by emphasizing the role of natural resource exploitation and land-use transition before elections. They also point out that increases in centralized monitoring or in local awareness around the election process, together with changes in the rules of election financing by local mayors, may be needed in order to slow down the accelerating economic and political incentives and to mitigate excess deforestation in Indonesia.

Precise data on the timing of oil palm licenses would help to identify whether the land-use licenses for oil palm move together with deforestation (and pre-election and price incentives) or whether they are only used to legalize *de facto* land-use change ex post. Detailed data on yearly variation in land-use types could also help to more clearly identify the dynamics of land-use transition that follow deforestation. Finally, data on election outcomes could help us to assess whether the observed pre-election changes also provide measurable political benefits to the incumbent politicians or politicians related to them. These open questions leave potentially fruitful avenues for future research.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jeem.2021.102453.

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