The Political Economy of Deforestation in the Tropics*

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Abstract

Logging of tropical forests accounts for almost one-fifth of greenhouse gas emissions worldwide and threatens some of the world's most diverse ecosystems. This paper demonstrates that local-level political economy substantially affects the rate of tropical deforestation in Indonesia. Using a novel MODIS satellite-based dataset that tracks annual changes in forest cover over an 8-year period, we find three main results. First, we show that increasing numbers of political jurisdictions leads to increased deforestation. This effect, particularly for illegal logging, is consistent with a model of Cournot competition between jurisdictions determining how much wood to extract from their forests. Second, we demonstrate the existence of "political logging cycles," where illegal logging increases dramatically in the years leading up to local elections. Third, we show that, for local government officials, logging and other sources of rents are short-run substitutes, but that this affect disappears over time as the political equilibrium shifts. The results document how local political economy forces lead to substantial deviations from optimal logging practices and demonstrate how the economics of corruption can drive natural resource extraction.

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1 Introduction

Satellite imagery reveals vast expanses of forest extending across the Amazon Basin, the Congo Basin, and South East Asia. Unlike the great forests in the Northern hemisphere, these tropical forests have been experiencing rapid rates of deforestation (Hansen and DeFries 2004). In fact, relative to a baseline of 1900 the majority of tropical forest has already been felled, with the rate of deforestation accelerating in the last two decades (Holmes 2002; FWI/GFW 2002; Hansen et al. 2008).

Understanding what lies behind tropical deforestation is important not just for reasons of preserving biodiversity, but also because of its critical role in global climate change (Stern 2006; Nabuurs et al. 2007). Tropical deforestation accounts for almost 20 percent of global emissions of greenhouse gases (Hooijer et al. (2006); IPCC (2007); Kindermann et al. (2008)). This is more than is contributed globally by the transportation sector as a whole, and is roughly equivalent to the total greenhouse gas contribution of the United States. In fact, tropical deforestation places Indonesia just behind the US and China as the third largest producer of greenhouse gases worldwide.

While there is an extensive literature on the optimal management of forest resources (e.g., Dasgupta and Heal 1974, Samuelson 1976, Dasgupta 1982, Brown 2000), and while most countries' official policy seeks to implement these types of sustainable logging systems, actual practice diverges significantly from best practice. Local bureaucrats and politicians have much to gain by allowing logging to take place outside official concessions (Barr et al. 2006) or by sanctioning the transport and processing of illegally harvested logs (Casson 2001a). On net, in many cases over fifty percent of the wood yield involves some illegal action – the figure for Indonesia, for example, is estimated at 60-80% (CIFOR 2004). In this context, viewing deforestation as the result of optimal forest extraction policies implemented by a central planner misses the reality of what happens on the ground. Instead, what matters are the incentives that local politicians and bureaucrats face to either protect tropical forests or to allow their destruction.

This paper investigates how these local political economy incentives affect deforestation in Indonesia, home to one of the largest and most valuable tropical forest reserves in the world (FWI/GFW 2002). Although all Indonesian forests are legally owned by the national government, local district governments have a substantial de facto role in forest administration, particularly as the gatekeepers for illegal logging. By using imagery from the MODIS satellite, which was put into orbit in December 1999, we are able to monitor, at a 250m by 250m resolution, what has happened to forest cover on an annual basis across the whole of Indonesia for the period 2000 to 2008 (Hansen et al. 2009). The fineness at which we can monitor forests also allows to compare and contrast deforestation across localities and in four land use zones – the production and conversion zones where logging is legal (within specific concessions) and the conservation and protection zones (where logging is strictly illegal).

Using this data, we investigate how the incentives faced by local bureaucrats and politicians affect the rate of deforestation. First, we show that the rate of deforestation in a province is increasing in the number of political jurisdictions. Between 1998 and 2008, the number of districts in Indonesia increased by 65 percent, from 292 to 483, with districts splits occurring at different times in different parts of the country. Using the MODIS satellite data, we estimate that subdividing a province by adding one more district increases the overall deforestation rate in that province by 7.8 percent, with the increase coming at roughly equal rates in forest zones where logging may be legal or illegal (production and conversion) and zones where all logging is illegal (conservation and protection).

While the increase deforestation in the production and conversion zones (where logging is legal or illegal) could be due to a combination of many forces, including changes in how the central government allocates the legal quotas across jurisdictions, we argue that the increase in deforestation in the conservation and protection zones (where deforestation is illegal) suggests that Indonesian district governments may be engaging in Cournot competition in determining how much wood to extract from their forests. Consistent with the Cournot model, we show that the increase in political jurisdictions drives down prices in the local wood market: adding one more district to a province reduces local prices by 3.3 percent, implying a local demand elasticity for logs of about 2.1. A back-of-the envelope calculation suggests that the increase in deforestation we observe is consistent with what a Cournot model would predict given this elasticity. We also show that the increase in illegal logging is not just due a decline in enforcement, as the changes occur equally in the old and new parts of the district and impact of the new jurisdiction only becomes stronger with time. Combined, this suggests that the patterns of illegal logging are governed, in part, by the industrial organization of corruption (Shleifer and Vishny 1993, Olken and Barron 2009).

Second, we test whether local election pressures influence the rate of deforestation. Starting in 2005, local district heads began to be chosen through direct popular elections rather than being indirectly selected by the local legislature. When direct elections first arrived in a district was determined by when the district head's term came to an end, and the timing of these terms, in turn, was determined by the timing of district head appointments under Soeharto (Skoufias et al. 2010). This introduces asynchronicity in district elections which is plausibly orthogonal to patterns of forest loss and which we exploit to examine whether logging, and in particular illegal logging, increases in the run-up to these elections. Using this approach, we document a "political logging cycle" where local governments become more permissive *vis a vis* logging in the years leading up to elections. We find that deforestation in zones where all logging is illegal increases by as much as 42 percent in the year prior to an election.

Third, just as the rents from facilitating logging may become more or less valuable depending on where governments are in the political cycle, their value (and hence the incentive to allow logging) will depend on what alternative sources of rents governments have access to. Oil and gas reserves are highly unevenly distributed across Indonesia and the revenue sharing rules put in place by post-Soeharto governments, which give greater weight to the districts and provinces where these resources emanated from, mean that the distribution of revenue from these sources is also highly unequal. We exploit the variable availability of oil and gas revenues over time and space to examine whether they blunt or sharpen incentives to extract forest resources both immediately after these hydrocarbon resources become available and over the medium term. Consistent with other examples in the economics of corruption (Olken 2007, Niehaus and Sukhtankar 2009), we find that these two alternate sources of rents are substitutes in the short-run. In the medium term, however, this effect disappears. We provide suggestive evidence that the effect disappears over time because the higher oil and gas rents lead over time to a new, higher rent-extraction political equilibrium (as in **?**)

These results document that the incentives faced by local politicians and bureaucrats – the potential rents they can obtain from restricting logging vs. allowing more, the timing of rent extraction with regard to political needs, and the availability of alternative sources of rents – strongly affect patterns of deforestation in Indonesia. If optimal logging rules were being followed, none of these factors should matter. The fact that they do highlights the lack of full control central governments have over natural resources in developing countries, and suggests that incorporating the incentive compatibility constraints for local agents of the state is crucial to designing effective forestry policies.

The remainder of this paper is organized as follows. In the next section we discuss the background on how political change and deforestation in Indonesia and on how we study these processes using a variety of data sets. Section 3 examines how the splitting of districts affected deforestation, which we interpret in the light a model of Cournot competition. In Section 4 we study the interaction between patterns of deforestation and the timing of elections. Section 5 investigates whether having access to alternative sources of public finance incentivizes or disincentivizes districts to engage in logging. Section 6 concludes.

2 Background and Data

Indonesia comprises an archipelago of islands in South-East Asia stretching from the Indian Ocean to the Pacific Ocean. It is a vast country. From tip-to-tip (from Sabang in Aceh to Merauke in Papua), Indonesia is 3250 miles across; this is the same as the distance from Tampa, Florida to Juneau, Alaska. The conditions in Indonesia are ideal for the growth of forests and without the involvement of humans, Indonesia would be largely covered in forest.

In this section we first trace out the dramatic political changes that Indonesia has experienced in its recent past, and document how these change have resulted in a tug of war over the control of the forest sector. We then outline how we monitor forest loss using satellite data, and discuss how we capture political changes in our data. This section thus prepares the ground for the analysis of the political economy of deforestation which ensues in the subsequent three sections.

2.1 Background

2.1.1 Decentralization in Post-Soeharto Indonesia

The East Asian crisis brought to an end the thirty-two regime of President Soeharto on May 21st, 1998. He and his family had governed Indonesia as a personal fiefdom since 1967, and particularly in later years his New Order regime had become synonymous with the Soeharto family extracting rents from all key sources of economic activity in the country (Fisman 2001).

Soeharto's departure ushered in one of the most radical reconfigurations of a modern state (Bertrand 2008), combining a democratic transition with a radical decentralization of power. Amidst fears that the multi-ethnic country would break apart, substantial administrative and fiscal authority was devolved to the approximately 300 district governments.[1] Off-Java regions which were rich in natural resources like forests, and oil and gas were particularly strident in their demands and wanted systems of control over these resources to be revised and for more of the revenue from their extraction to accrue to them (Cohen 1998, Tadjoeddin et al. 2001, WB 2003, Hofman and Kaiser 2004, Wulan et al. 2004). The decentralization laws, which were passed in 1999 and took effect in 2001, devolved approximately 25% of the national budget to the districts in the form of block grants and dramatically increased their authority over almost all sectors of government. Local governments also received a substantial share of the natural resource royalties originating from their district.[2] Districts were administered by *Bupatis* (district heads), who were in turn indirectly selected by local legislatures.

The allure of self-government where districts could enjoy significant new political and fiscal powers led to a significant amount of district splitting. The total number of districts increased from 292 in 1998 to 498 in 2009. In contrast, the number of districts in Indonesia had remained largely unchanged during the New Order regime (1967-1999) (BPS 2007). District splits thus represented a significant mechanism for the further decentralization of power in the country (Cohen 2003; Fitrani et al. (2005)). What they also did, however, was to introduce a certain amount of disorganization as many districts lacked the human resources, technical capacities and institutional structures to take on these new administrative powers (Tambunan 2000).

Soon after decentralization took effect, pressure mounted for a new reform, since it was felt that the 1999 regional governance law gave too much control to the local parliament and, thus, made the system susceptible to corruption (Mietzner 2007) and elite capture (Erb and Sulistiyanto 2009). Consequently, in 2004 a revised decentralization law considerably increased accountability by introducing direct election of the district head. Direct elections

[1] Unusually, Indonesian decentralization transferred power to the approximately 300 district governments, rather than the approximately 30 provincial governments, since districts, unlike provinces, were perceived to be too small for separatist tendencies (Hull 1999; Niessen 1999).

[2] In particular, an oil-producing district receives 6% of oil royalties and 12% of natural gas royalties; a further 6% (oil) and 12% (gas) is shared equally among all other districts in the same province. Districts are allocated 80% of both the one-off license fee for large-scale timber concessions (*IHPH*) and the Forest Resource Rent Provision (*PSDH*), a second volume-based royalty. Specifically, the producing and non-producing districts are each allocated 32% of the royalties. Furthermore, the district that contains the concession can keep 64% of the *IHPH* fee with the rest going to the central government. Exceptions to this rule were made for the separatist provinces of Aceh (Special Autonomy Law 18 of 2001) and Papua (Special Autonomy Law 21 of 2001), who received substantially larger shares. For a detailed discussion of Indonesia's transfer system refer to Brodjonegoro and Martinez-Vazquez (2002).

were to be held after the previous district head selected by the previous system had served their full tenure. The tenure of appointed district heads, in turn, was dependent on when the terms of district heads appointed under Soeharto had to come to an end. This introduces asynchronicity in district elections.[3] Since the timing was driven by idiosyncratic factors from previous decades, it can be viewed as plausibly exogenous with respect to forest loss; indeed Skoufias et al. 2010 demonstrate that the timing of district elections is uncorrelated to virtually all pre-existing socioeconomic or geographic characteristics.

2.1.2 Implications for the Forest Sector

During the Soeharto regime, the 1967 Basic Forestry Law (ROI 1967) gave the national government the exclusive right of forest exploitation in the so-called 'Forest Estate' (*Kawasan Hutan*); an area of 143 million hectares equivalent to three-quarters of the nation's territory (Barber and Churchill 1987; Barber 1990). The entire Forest Estate was managed by the central Ministry of Forestry, based in Jakarta. The Ministry in turn awarded a small group of forestry conglomerates (with close links to the regime's senior leadership) most of the timber extraction concessions in the Forest Estate, amounting to an area of about 69 million hectares inside the area designated as 'Production Forest' (CIFOR 2004). These exploitation rights were non-transferrable, issued for up to 20 years and required the logging companies to manage the forest sustainably through selective logging (ROI 1970). The second category inside the Forest Estate was the 'Conversion Forest', in which the largest wood producers could use 'Wood Utilization Permits' (*Izin Pemanfaatan Kayu* or *IPK*) to clear-cut the forest and set up plantations for industrial timber, oil palm or other estate crops. Logging was prohibited in the remaining zones of the Forest Estate, which were designated for watershed protection (the 'Protection Forest') and biodiversity protection (the 'Conservation Forest').

The control over these forest zones changed with the passing of the Regional Autonomy Laws in 1999. In particular, the primary change was that the district forest departments became part of the district government, answerable to the head of the district, rather than a division of the central Ministry of Forestry.

The district forest office is the main point of control over much of the forest estate, both in terms of authorizing and monitoring legal logging and in terms of controlling illegal logging. For legal logging, the precise role of the district forest office varies depending on the forest zone. For production forest, for example, the district forest office works with concession holders to develop, monitor, and enforce annual cutting plans.[4] For conversion forest, the district government initiates proposals to the central government that land be converted from forest to other uses, such as oil palm, and is responsible for ensuring that conversion is

[3] For instance, only one-third of all (434) districts held direct elections in June 2005. By 2007, about 30% of all districts still had a district head that had not been elected directly.

[4] In particular, each year the concession holder, working with the district forest office, proposes an annual cutting plan (*Renana Kerja Tebang*), based on a survey they conduct in coordination with the district forest office to determine how much can be sustainably cut. The district government then negotiates the cutting plan with the national Forest Ministry, which coordinates all of the annual cutting plans nationwide to ensure that they do not exceed the total national annual allowable cut.

carried out in the designated areas only.[5]

Given their central role in enforcing forest policy, the district forest office is the key gatekeeper for illegal logging in these zones. For example, a district forest office employee is supposed to be stationed at the gate of every concession to monitor all logs leaving the concession, and at the entrance of all saw mills to check all logs entering the saw mills. Extracting more than the legal quota from a concession, or bringing illegally sourced logs into a mill, therefore requires the complicity of the district forest office.

District forest officials also play a key role in controlling deforestation in the protection and conservation areas. For protection forest, the district forest office has the responsibility to patrol and ensure that no illegal logging is taking place. Conservation forest – much of which is national parks – is the only part of the forest estate legally still under central control. However, since the district forest office enforces the processing of logs at sawmills and monitors transportation of logs, logging in those zones also requires the de facto acquiescence of the district forest office.[6] Anecdotal evidence confirms that district governments play an important role in facilitating illegal logging (Casson and Obidzinski 2002, Smith et al. 2003, Soetarto et al. 2003) Estimates suggest that illegal logging makes up as much as 60-80% of total logging in Indonesia, making illegal logging a US $1 billion a year market (CIFOR 2004), suggesting that these forces play a substantial role in determining the total amount of deforestation.

2.2 Data

2.2.1 Constructing the satellite dataset

Given the prevalence of illegal logging, it is crucial to develop a measure of deforestation that encompasses both legal and illegal logging. To do so, we use data from the MODIS satellites to construct an annual measure of forest change for each year from 2001-2008. The resulting dataset traces, at 250m by 250m resolution, the patterns of deforestation across the entire country over time. This section describes how the forest change dataset is constructed from the raw satellite images.

There are two main challenges in constructing satellite-based images of deforestation. First, humid tropical regions like Indonesia have persistent cloud cover that shrouds the re-

[5] In addition, during the period from 1999-2002, district governments were legally allowed to issue a variety of small-scale, short-term forestry permits themselves, without central government approval. These licenses, both for the 'Production' and 'Conversion Forest', often directly overlapped with the large-scale logging concessions and sometimes even the boundaries of national parks and protected areas (see, e.g., Barr et al. (2001), Casson (2001b), McCarthy (2001), Obidzinski and Barr (2003), Samsu et al. (2004) and Yasmi et al. (2005)). In 2002, under pressure from the main forest concession holders, the national government revoked the right of district governments to issue these small-scale permits. Note that we have verified that the main results in the paper are robust to dropping 2001, so that they are identified only from the period 2002-2008 where districts had no de jure power over forest licenses. See the Appendix for tables.

[6] Local police can also play an important role, since they can also instigate enforcement actions for illegal logging (or threaten to do so). Police are not directly answerable to the head of the district, but are organized on the district-by-district level.

gion year round. This makes it impossible to use high-spatial resolution sensors, like Landsat, which are usually used to measure forest cover change (Asner 2001; Ju and Roy 2008) – since these satellites typically only revisit the same area once every 1-2 weeks, cloud-free images are rarely recorded. Instead, it is necessary to draw on moderate-resolution sensors, such as the MODerate Resolution Imaging Spectroradiometer (MODIS) that pass over the same spot every 1-2 days. This considerably increases the likelihood of obtaining some good quality images, but at the cost of 250m by 250m resolution instead of the approximately 40m resolution available via Landsat. We start with the basic thirty-two day composites of the MODIS Land Surface Reflectance bands (Vermote et al. 2002) and the MODIS Land Surface Temperature Product (Wan et al. 2002) available on the NASA website, which aggregate daily images into monthly images to reduce cloud effects, and then we further aggregate them into annual composites to produce a cloud-free image of each pixel.

Second, one needs to take the composited MODIS images and build a computer algorithm to discriminate between forest and non-forest. For each pixel, the MODIS satellite collects 36 "bands," each of which measures the strength of electromagnetic radiation in a particular part of the spectrum, so each pixel is essentially a 36-dimensional representation of the average electromagnetic radiation coming from a particular 250m by 250m spot. By contrast, the human eye, with its three types of cones, measures only three "bands", which correspond to roughly to blue, green, and red areas of the visual spectrum, so the raw MODIS data is considerably richer than just a visual image at comparable resolution.

The key idea of remote sensing is developing an algorithm that identifies what signatures or set of signatures – i.e., what combinations of means and correlations among various parts of the 36-dimensions of spectrum that MODIS sees – best discriminate between forest and non-forest. For example, plants absorb electromagnetic radiation in the red visual range for use in photosynthesis, but reflect or scatter radiation in the near-infrared range. One common metric therefore examines the so-called NDVI (normalized difference vegetation index), which captures the difference in intensity between light in the red range and in the near-infrared range, and therefore identifies one signature for plant life (Gausman 1977; Tucker 1979; Curran 1980)

In practice, one can do much better than using NDVI by exploiting additional dimensions of the data (see Wulder (1998) for a literature review). For example, forests tend to be cooler than surrounding areas, so bands that measure temperature can also be used (Gholz 1982). Moreover, trees have different spectral signatures than other types of crops and plants (Curran 1980). To take maximal advantage of the richness of the MODIS data, we use a statistical learning procedure known as a "tree bagging algorithm" to determine which spectral signatures best correspond to forest (Breiman et al. 1984; Breiman 1996).

Specifically, we start with much higher resolution "training" images. For each of these images (at 30m by 30m resolution), experts have manually examined the image and coded each cell into forest, non-forest, or forest change (deforestation). We then apply the statistical tree-bagging algorithm to automatically group the MODIS data into naturally occurring groups that share common electromagnetic signatures, and then determine which of these sets of signatures corresponds to the manually-coded forest, non-forest, or forest change cells in the training dataset. This is akin to a regression, except that it allows for complex correlations between bands to be used in the prediction, rather than just means, and allows very flexible functional forms.

One then can extrapolate over the entire MODIS dataset to predict, for each year, the probability that a given pixel was deforested. We code a pixel as deforested if the probability exceeds 90% in any year; once it is coded as deforested, we consider it deforested forever. The reason for this is that, especially in a humid tropical environment like Indonesia, once the original forest is cleared other crops or scrub brush emerge quickly; since the forest takes at least several decades to regrow, this regrowth is not actual tree cover. Deforestation thus is often represented by a pixel that is "green" one year, "brown" the next year, and then "green" again. Given this, Hansen et al. (2009) have shown that the key to detecting true forest change is the high probability of being deforested in a single year, rather than appearing "brown" year after year.

The final output are annual forest change estimates for 2001-2008 for each of the 34.6 million pixels that make up Indonesia. Note that these estimates will provide a lower bound for forest change, as a 250m by 250m pixel is only coded as deforested if the majority of the area represented by the pixel is felled. This will reliably pick up clear-cutting, but will not necessarily capture selective logging if the forest canopy remains largely intact, and therefore may under-estimate total logging. They are instead to be treated as an indicator of likely forest change. The measure will also capture deforestation due to large-scale burns, which can be either intentional (for land clearing purposes, usually after logging of valuable trees has already taken place) or unintentional.[7] This cell-level data is then summed by district and forest zone (i.e., the four forest categories in the 'Forest Estate': the 'Production', 'Conversion', 'Protection' and 'Conservation Forest'). This yields our final left-hand-side variable $deforest_{dzt}$, which counts the number of cells likely to have been deforested in district d in forest zone z and year t.

Figure 1 gives an idea of what our underlying forest cover data looks like. To do this we zoom in onto a small area, since the detailed nature of this dataset makes it impossible to visualize the 34.6 million pixels that make up Indonesia in a single map. It focuses on one of the main hotspots of deforestation during this time period (Hansen et al. 2009), namely the province of Riau on the island of Sumatra. The deforested cells are indicated in red, forest cover is shown in green and non-forest cover in yellow. The map clearly shows that substantial amounts of forest have been deforested during the period from 2001 to 2008. Furthermore, forest clearing seems to spread out from initial areas of logging, as access will be easier from already logged plots.

In addition to the satellite data, we also examine official logging statistics from the annual 'Statistics of Forest and Concession Estate' (*Statistik Perusahaan Hak Pengusahaan Hutan*), published by the Indonesian Central Bureau of Statistics for 1994-2007. These statistics report the quantity of logs cut at the province level and the associated price by wood type,

[7] However, we show in Section 3 below that we obtain remarkably similar results in the Production zone for the satellite-based deforestation measure and official logging statistics, suggesting that much of what we are picking up is, indeed, logging.

for 114 different types of wood.[8] Because they are derived from production, they include both clear-felling as well as selective logging; on the other hand, they capture only logging that was officially reported by the forest concessions, and so likely miss most illegal logging. Since they report the wood cut from the production forest, they should be compared to the satellite data from the 'Production' zone. This data also includes data on the price of woods; since market prices are determined by both legal and illegal logging, these prices will reflect the market equilibrium for both types. We use this second dataset as a consistency check for our satellite data and to examine impacts on prices, as described in further detail in Section 3 below.

2.2.2 Descriptive statistics of forest change

Figure 2 illustrates the distribution of pixels coded as likely deforested at the district level across Indonesia over time. In particular, it shows the number of cells coded as likely deforested at the district level in 2001 and 2008. We focus our analysis on the main forest islands of Indonesia: moving from West to East, these are Sumatra, Kalimantan, Sulawesi and Papua. The remaining islands (Java, Bali, NTB/NTT, and Maluku), shown in white, have negligible forest cover in the baseline period and are not included in our sample. In this map, low levels of likely deforestation are shaded in green, whereas high levels of likely deforestation are indicated in orange and red. The figures suggest that most of the deforestation occurs in Kalimantan and in the lowlands of Sumatra along its eastern coast. From 2001 to 2008, there is a shift in deforestation in Kalimantan from the West to the East, and there is an intensification in deforestation in Sumatra, particularly in the provinces of Riau and Jambi in the east-center of the island. There is also some intensive deforestation in the Southern part of Papua in 2001, but high deforestation rates are not maintained in this area over time.

Table 1 reports the trends in forest cover over time in more detail, and Table 2 displays the summary statistics for our main measure of deforestation. The data in both tables is reported for the entire 'Forest Estate', the subcategories of the 'Forest Estate' where logging may be legal ('Production/Conversion Forest') and where all logging is illegal ('Conservation/Protection Forest') as well as the individual subcategories of the 'Forest Estate'. Table 1 shows the changes in the forest area measured in MODIS pixels (each of which represents an area approximately 250m by 250m). Total deforestation between 2000 and 2008 amounts to 783,040 pixels. Although MODIS pixel change does not detect all forest change, as some forest change occurs below the level detectable by MODIS (Hansen et al. 2009), to gauge the magnitude of this, it is worth noting that 783,040 pixels represents 48,940 square kilometers; this is roughly twice the size of Vermont.

Most of this change occurs in the 'Production Forest', where 486,000 pixels (representing an area of 4.2 million hectares) were coded as likely deforested. Much smaller changes are reported for the other forest zones: 179,000 pixels were deforested in the 'Conversion

[8] We drop the 'other' (*Lainnya)* and 'mixed wood' (*Rimba Campuran)* category, since their composition varies considerably across provinces and over time.

Forest' and only 116,000 pixels were deforested in the 'Conservation' and 'Protection Forest' combined. However, this last estimate will only provide a lower bound of the actual changes on the ground, since logging is prohibited in these parts of the 'Forest Estate'. To the extent illegal logging is selective and, thus, occurs on a much smaller scale, moderate resolution sensors like MODIS will underestimate these changes.

Table 2 shows the summary statistics of our main left-hand side variable, $deforest_{dzt}$, which counts the number of cells likely deforested for district d in forest zone z and year t. On average, 113 pixels (the equivalent of 704 hectares) are deforested annually at the district level. However, the variance of 464 pixels (4 times the mean) suggests that there is a lot of variability in deforestation both across years and districts. The pattern of the results mimics the previous findings, i.e. most of the changes occur in the 'Production Forest', where on average 232 pixels (representing 1,451 hectares) are coded as likely deforested in each district and year.

2.2.3 Political Economy Data

To capture increasing competition in the wood market, we take advantage of the extensive partitioning of districts following the collapse of the New Order regime. Figure 3 illustrates the distribution of district splits in our forest island sample. It displays the total number of districts that the original 1990 district partitioned into by 2008. High numbers of splits (3-7) are denoted by orange and red in the figure, whereas low numbers (0-2) of splits are denoted by blue and green. It is evident from this map that district splits happen all over the country. Most districts split at least once or twice, so that very few of the 1990 districts remain intact. In addition, the map suggests that the largest districts in 1990 split into more new administrative units.

We construct two sets of variables for the districts and provinces using the official publications on regency and municipality codes of Statistics Indonesia (*Badan Pusat Statistik* or *BPS*).[9] Note that we use the 1990 boundaries as a reference point, because 17 new districts were formed between 1990 and 1999 (BPS 2007).[10] For the province-level data, we simply calculate the total number districts and municipalities within the 1990 boundaries of province p on island i in year t, $NumDistrictsInProv_{pit}$.[11] In addition, we construct two more variables at the district level. Firstly, we count into how many districts and municipalities the original 1990 district d on island i split in a year t, $NumOwnDistricts_{dit}$. Secondly, we sum across all the other districts within the same province, $NumOtherDistricts_{dit}$.

We also obtain other district-level covariates as follows. To examine the impact of polit-

[9] The most up-to-date lists of regency and municipality codes is available on the bps webpage at `http://dds.bps.go.id/eng/aboutus.php?mstkab=1` .

[10] During the Soeharto regime, only 3 new *kabupaten* or *kota* were created outside of Jakarta prior to 1990: Kota Ambon (PPRI No. 13 Thn. 1979), Kota Batam (PPRI No. 34. Thn. 1983), and Kab. Aceh Tenggara (UURI NO. 4 Thn. 1984). Jakarta itself was split into 5 city parts in 1978.

[11] Each province is located on only one of the four islands – Sumatra, Kalimantan, Sulawesi, and Papua. We use the island subscript, i, as we will allow for differential time trends by island in the empirical analysis below.

ical election cycles, we obtain district-level election schedules obtained from the Centre for Electoral Reform (CETRO)[12], and use them to construct a dummy for the year the election for district head was held, $Election_{dit}$. To examine the impact of other sources of rents available to district governments, we examine oil and gas revenues per capita at the district level, $PCOilandGas_{dt}$.[13] We obtain the revenue data from the Indonesian Ministry of Finance (*Menteri Keuangan*) webpage (`http://www.djpk.depkeu.go.id/datadjpk/57/`) and the population data for 2008, which is published by the Indonesian Central Bureau of Statistics. It is important to note that new districts often do not record their own share of revenue for the first few years after the split, as the district is not fully functioning yet. We therefore allocate each new district the revenue share of its originating district until it reports its own share of revenue for the first time.

Figure 5 displays oil and gas revenue per capita in 2008 at the district-level. These natural resources are much more spatially concentrated than forest, so that most districts receive none or very little revenue shown as blue and green respectively. The districts that receive the largest share of revenue from oil and gas extraction are located in Eastern Kalimantan and in the province of Riau on Sumatra. Moreover, the map shows that there is some heterogeneity across districts within each province, where provinces are delineated with thick black borders. These differences are due to the revised revenue sharing rules, where the producing and non-producing districts each receive the same percentage of oil and gas revenue, which is then split evenly between the districts in each category (ROI 1999). Since the non-producing districts are usually larger in number, their final share of revenue will be smaller.

3 Cournot competition between districts

3.1 Theoretical Framework

Although there is a large literature on optimal forest management, the forestry literature tends to consider how an optimal central planner should manage forest resources, trading off the growth rate of trees with discounting (e.g., Samuelson 1976, Dasgupta 1982; see Brown 2000 for a survey).[14] In this paper, we consider what happens instead if, instead of a central planner making optimal forest extraction decisions, forest decisions are made by individual actors – in our case, district governments. We begin by examining how the number of jurisdictions affects the rate of extraction.

[12] CETRO is an Indonesian NGO (`http://www.cetro.or.id/newweb/index.php`). We use the most up-to-date district-level election schedule available, which provides election dates up to 2011.

[13] Oil and gas is by far the largest source of natural resource rents for districts. For instance, in 2008 the average district-level revenue from oil and gas was 114.515 billion rupiah, whereas the corresponding figure for forestry was 5.302 billion rupiah.

[14] The other strand of the literature considers multiple actors with competing property rights over the same forest (e.g. Larson and Bromley 1990, Ligon and Narain 1999), but to the best of our knowledge none consider the type of oligopolistic competition we study here where each actor has full control rights over its own forest and strategic interactions occur through the product market.

For simplicity, in this section we abstract away from issues involved in tree regrowth and instead treat forests as an exhaustible natural resource. This is consistent with substantial de-facto logging practice in many tropical forests, including those in Indonesia, where virgin forests are heavily logged, and then either left in a degraded state or converted to a non-forest use, such as palm plantations. This type of non-sustainable clear-cutting and land conversion is also the type of forestry we will primarily be able to observe in the satellite data.[15]

We suppose that each period, district governments choose the quantity of forest to extract. As discussed above, this can occur in a variety of ways: by determining how many illegal log transport permits to issue, how many conversion permits to issue, etc. Once they determine quantities, prices are determined through the market. We assume that transport costs across different parts of Indonesia, the need to process logs locally before export (Indonesia bans the export of raw, unprocessed logs), and capacity constraints at local sawmills combine to generate local downward-sloping demand curves for logs in each market; this assumption is discussed in more detail below.

The problem districts face is thus that of oligopolistic competition in a nonrenewable natural resource. Lewis and Schmalensee (1980) show that many of the standard, static Cournot results generalize to this setting. In particular, they show that a greater number of actors in a market – in our case, more districts – leads to lower prices and greater resource extraction.[16] We will test this implication in the empirical section below.

3.2 Empirical Tests

To test for Cournot competition between districts, we will take advantage of the fact that the number of districts has increased dramatically over the period we study. As discussed above, across all of Indonesia, the number of districts increased from 292 prior to decentralization to 483 in 2008. The increase is even more dramatic in the forest islands (Sumatra, Kalimantan, Sulawesi, and Papua) that are the focus of this study – from 146 districts prior to decentralization to 311 districts in 2008, an increase of 213%. We exploit the staggered timing of these changes in administrative boundaries to identify the relationship between the number of administrative units and logging and to test the theoretical model outlined above.

As analyzed in detail in Fitrani et al. (2005), the splitting of districts was driven by three principal factors: geographic area, ethnic clustering, and the size of the government

[15] One could generalize the model to allow forests to regrow at some slow rate; we speculate that this would not substantially affect the qualitative predictions we consider here, which concern the strategic interactions between districts.

[16] Because the resource is subsequently depleted more quickly with more actors, they also show that the price then subsequently rises more quickly with higher N than with lower N as the resource moves more quickly towards exhaustion. In our case, since the rate of extraction is small relative to the reserves (e.g., about 0.5% per year, see Section 2.2.2 above), the increase in prices may happen too slowly to be observed in our data.

sector.[17] From the perspective of this paper, the key question is not whether a district splits, but rather the timing of the split. Several idiosyncratic factors appear to influence the timing. First, the process of splitting a district is quite cumbersome, involving a number of preliminary steps (e.g., formal agreement of the district legislature, the district head, the provincial governor, and the provincial legislature; documentation of the new districts' ability to meet fiscal requirements; documenting a reason for the split (ROI 2004) and, ultimately, the passage of a special law by the national parliament for each split that will take place. The amount of time each of these steps take varies, which in turn influences the total amount of time required. Moreover, there was a national moratorium on splits from 2004 (when the criteria for splits were revised) through 2007. This moratorium also creates plausibly exogenous delays in timing of splits, as many districts that may have been close to completing the process in 2004 had their split postponed by three years due to the moratorium.[18] In the empirical analysis below, we test empirically for whether the timing of these splits are associated with pre-trends in deforestation, though *a priori* there is little reason to believe they would be.

To test the predictions of the theory, a key question is what definition we should use for the "market" for wood products. While wood and wood products are traded on international markets (and hence, one would expect the market to be global), there are several factors that make wood markets in Indonesia more local. In particular, since 2001 Indonesia has banned the export of raw logs. Instead, all timber felled in Indonesia must first be transported (either by river, when possible, or by road) to local saw mills, plywood mills, and paper mills, where it is processed before export. These factors imply that prices may differ across regions. We focus on the province as the key definition of a market, since provincial boundaries are coincident with the major river watersheds used for transporting logs.

We will examine several empirical predictions of the Cournot theory outlined above. First, taking a province as a measure of the market, we use panel data to test whether the number of districts in the province affects the prices and quantity of wood felled in the province. For this purpose, we will use our two complementary sources of forestry data. For our primary measure of deforestation, we will use the MODIS satellite based data, which captures both legal and illegal deforestation. To examine the impact on prices and estimate elasticites, we will also examine the official forestry statistics.

[17] Specifically, the Soeharto era districts were often quite large, so naturally they find that districts that were larger geographically are more likely to split to make administration easier. Second, there are often ethnic tensions in Indonesia, particularly off Java. Those districts where the different ethnic groups were clustered geographically were more likely to split. Finally, the block grant fiscal transfer (DAU) had a fixed-component per district. While this gives all districts an incentive to split, they find that it is particularly likely in those districts with a large wage bill, who presumably are in greater need of the revenue. The find little consistent relationship between natural resources and splitting, with positive coefficients in the 1998-2000 period and negative coefficients in the 2001-2003 period, implying zero effect on average. Details of these regressions can be found Fitrani et al. (2005).

[18] Unfortunately, we do not observe when the district began the process of filing for a split, as we only observe the date the final split law was passed by the Parliament, so we cannot exploit this three-year moratorium directly as an instrument.

Specifically, for the satellite-based forestry data, since our key dependent variable is a count – i.e., how many pixels were deforested in a given year – we will run a fixed-effects Poisson Quasi-Maximum Likelihood count model (Hausman et al. 1984, Wooldridge 1999; see also Wooldridge 2002), with robust standard errors clustered at the 1990 province boundaries. Specifically, this estimates, by MLE, equations such that

$$\mathbf{E}\left(deforest_{pit}\right) = \mu_{pi} \exp\left(\beta NumDistrictsInProv_{pit} + \eta_{it}\right) \tag{1}$$

where $deforest_{pit}$ is the number of pixels deforested in province p (located on island i) in year t, $NumDistrictsInProv_{pit}$ counts the total number of districts in province p in year t, μ_{pi} is a province fixed-effect, and η_{it} is an island×year fixed effect.[19] The coefficient β in equation (1) represents the semi-elasticity of deforestation with respect to the number of districts in the province. The reason we use the Poisson QML count specification for the satellite data, rather than estimate a log dependent variable with OLS, is that we have many observations (more than 25%) where the dependent variable is 0, so a count model is more appropriate. The Poisson QML count model in (1) is robust to arbitrary distributional assumptions, so long as the conditional mean is specified by (1). The robust standard are clustered at the 1990 province boundaries.

For the price (and quantity) data from the official production statistics, we will run an analogous OLS fixed effects regression, as follows:

$$\log(y_{wipt}) = \beta NumDistrictsInProv_{pit} + \mu_{wpi} + \eta_{wit} + \varepsilon_{wipt}, \tag{2}$$

where y_{wipt} is the price or the quantity of wood type w harvested in province p and year t. The regression also controls for wood-type-by-province and wood-type-by-island-by-year fixed effects, μ_{wp} and η_{wit} respectively. Since there is a substantial variation in quantity of wood across wood species and provinces – the 5th percentile of the quantity variable is 42 m^3, whereas the 95th percentile of the quantity variable is 204,804 m^3 – this regression is weighted by the volume of production of wood type w in province p in the first year that we have data. Note that if one takes logs of equation (1), the coefficient β in equation (1) is directly comparable to the coefficient β in equation (2); both represent the semi-elasticity of deforestation with respect to the number of districts in the province.[20]

Second, we will examine the impact of splits at the district level. In particular, we will test whether splits affect deforestation in the district that splits vs. how it affects deforestation in the remainder of the province. We estimate via Poisson QML a model such that:

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp(\beta NumOwnDistricts_{dit} + \gamma NumOtherDistricts_{dit} + \eta_{it}) \tag{3}$$

[19] As discussed above, there are four islands in our sample: Sumatra, Kalimantan, Sulawesi, and Papua. Each province is located on only one island.

[20] The only difference is that equation (2) is weighted by initial volumes in production ($deforest_{wp0}$), whereas the Poisson model implicity uses contemporaneous volumes for weights ($deforest_{wpt}$) (see VerHoef and Boveng 2007). We show below that using contemporaneous weights when estimating equation (2) produces virutally identical results.

where $deforest_{dit}$ is the number of cells cleared in district d (located on island i) between year $t-1$ and t, $NumOwnDistricts_{dit}$ counts into how many districts the original 1990 district d split into by year t, and $NumOtherDistricts_{dit}$ counts into how many other districts there are within the same province in year t. It also includes district * forest zone fixed effects μ_{di} and island-by-year fixed effects η_{it}. An observation is based on the 1990 district boundaries, and the robust standard errors are now clustered at the 1990 district boundaries. The conditional log-likelihood function is again estimated separately by land use zones.

There are several potential alternative possible explanations for why increasing the number of jurisdictions could increase the rate of deforestation. First, as discussed above, the amount of legal logging in production and conversion zones is determined by a negotiation between the districts and the center. One could imagine that in such a negotiation, increasing the number of districts in a province could increase that province's bargaining power in these negotiations. For illegal logging, however, this negotiation force should not be important. To rule out this explanation as driving the results, we will therefore test for whether we find these increases in logging in zones where we know all logging is illegal.

Second, increasing the number of jurisdictions could result temporarily in a decline in enforcement capacity as new district government sets up its own district forest office. To rule out this explanation as driving the results, we will test for whether the increase in logging we observe is temporary or permanent. Specifically, we will examine lags of the $NumDistricts$ variables to test for whether the increase in logging we observe declines over the subsequent 3 years after the split takes place (which would be consistent with a temporary decline in enforcement capacity). We will also examine whether the increase in deforestation is greater in the new part of the district (i.e., the part of the district which after the split will be governed from a new district capital) as opposed to the old part of the district (i.e. the part of the district which after the split will be governed by the same forest office as before the split). If enforcement capacity was driving the results, we would expect the increase in deforestation to be greater in the new part of the district, but if it was driven by Cournot forces, we would not expect differential results between the old and new parts of the district.

Finally, with some additional assumptions, the simple static Cournot model can be used to generate quantitative predictions that can be tested against the data. Specifically, if we assume constant marginal costs and a constant elasticity of demand, we can derive how large quantitatively the increase in deforestation in response to increasing jurisdictions should be if it was driven by Cournot forces, and see whether it matches the empirical estimates. We explore this calculation in Section 3.6 below.

3.3 Results using the satellite data at the province level

Table 3 begins by estimating equation (1). The table reports the findings separately for each subcategory of the 'Forest Estate'. Column 1 presents all categories of the Forest Estate together, column 2 presents results for the zones where legal logging can take place (i.e., the 'Production' and 'Conversion' zones), and column 3 presents results for the zones where no legal logging can take place (i.e., the 'Conservation' and 'Protection' zones).[21] Columns 4-7 report the estimates for each zone individually.

The total estimated impact of district splits on deforestation is shown in column 1 of Panel A. We find that the annual rate of deforestation increases by 3.61% if an additional district is formed within a province.

Looking across the various zones of the forest estate, the point estimates suggest broadly similar impacts on extraction in the zones where logging could be legal or illegal (production: 5.33%, statistically significant at 1%; conversion: 2.83%, not statistically significant) and in one of the zones where deforestation is clearly illegal (conservation: 7.86%, statistically significant at 10%). This suggests that the impact of the increasing number of political jurisdictions is not merely being driven by changes in the allocation of legal cutting rights, but that something is happening with regard to illegal logging as well.

Panel B reports the estimates of the medium-run impact of district splits by including 3 lags of the $NumDistrictsInProv_{pit}$ variable.[22] In virtually all cases, the medium-run impact estimated by calculating the sum of the immediate effect and all 3 lags is even larger than in the main specification. For example, three years after the split, a district split increase deforestation in the entire 'Forest Estate' by 7.89%. The estimates for deforestation in legal and illegal logging zones, reported in Columns 2 and 3, respectively are now both significant and of similar magnitude – 7.83% on average for the production and conversion zones (where logging could be legal or illegal) and 9.00% for the conservation and protection zones (where all logging is illegal). The fact that the cumulative effect on logging three years after the split is even larger than the immediate impact, especially in the zones where all logging is illegal, suggests that the impact is not merely being driven by declines in enforcement associated with new district creation.

An important potential concern is that the timing of splits is correlated with pre-trends in logging. To investigate this, Table 4 tests for the presence of differential trends in the data by including three leads of the $NumDistrictsInProv_{pit}$ variable. We find that the our main results are robust to the inclusion of leads. Furthermore, and most importantly, the p-value of the joint significance test for the leads is large and statistically insignificant for all zones (ranging from 0.20 to 0.71, depending on specification), suggesting that there are not substantial pre-trends. (By contrast, the p-value of the joint significance test for the immediate and lagged effects of the number of districts is statistically significant, ranging from <0.001 to 0.08, depending on specification). In contrast to the sum of the lags, the sum of the leads is also statistically insignificant in all specifications. These results are reassuring, as they suggest that the results are indeed picking up the causal impact of district splits on both legal and illegal logging in the 'Forest Estate' and are not being driven by unobserved trends.

[21] As discussed above, since the Poisson model weights each observation by the quantity, when we combine observations from multiple zones we obtain the correct weighted average effect.

[22] The results do not change substantially if we use five lags instead.

3.4 Impacts on prices

If Cournot theory outlined in Section (3.1) is important, we would expect increasing numbers of political jurisdictions to not only increase quantities of deforestation, but also to decrease prices. To examine this, we turn to the official production data. This data captures the value and quantity of all logs from the official forest concession reports, separately for each species, province, and year. By dividing value by quantity, we can obtain the price the concession obtained for the wood. Although the official production statistics will not capture illegal logging, the prices concessions receive for their legally felled timber should reflect the prevailing market prices in the area, which will be determined by the quantities of both legal and illegal logging.

Table 5 estimates equation (2), using the data on prices and quantities from the official forest concession reports. Columns 1 and 2 provide the estimates for our main specification, which includes all wood types and covers the period 2001-2007.[23] Columns 3 and 4 show the results for the same sample period, but restrict attention to a balanced panel of wood types, where we observe production of the wood type in all years for a given province. Columns 5 and 6 present the results for all wood types for a longer time horizon that also includes the years of the pre-decentralization period for which the official logging publications were also available, i.e. for 1994-2007. Panel A displays the estimates for the contemporaneous effect (i.e., estimating equation 2 with no lags), and Panel B estimates the medium-run impact by including 3 lags of the number of districts variable. Columns 1, 3, and 5 present equations where the natural log of prices are the dependent variables, and columns 2, 4, and 6 present equations where the natural log of quantities are the dependent variables.

Consistent with the theory, the main results in columns 1 and 2 of Panel A show that adding one additional district in a province decreases prices by 1.7% and increases the quantity of logs felled by 8.9%. Similar results are obtained for the alternative samples shown in columns 3 through 6. The results therefore clearly show a decline in prices, consistent with there being at least some element of a local, provincial market for wood products.

Since increasing the number of districts is essentially a supply shock, one can infer the slope of the demand curve from the ratio of $dLnQuantity$ to $dLnPrice$. Combining the estimates from columns 1 and 2 implies a demand elasticity of -5.24. However, since the official production statistics miss illegal logging, a more reliable estimate of the elasticity can be found by taking the price effects from the official data and the quantity effects from the satellite estimates in Table 3. Using the satellite data estimates in Table 3 that adding an additional district increases quantities by 3.61%, we obtain a demand elasticity of -2.12. Given that markets are separated only by transportation costs, we would expect that demand for forest products should be quite elastic, consistent with the high elasticites we find in the data.

Panel B estimates the medium-run impact of the number of districts on prices and quan-

[23] Data is not yet available for 2008, so this is the most comparable time period to that used in the satellite data analysis below.

tities by including 3 lags of the $NumDistrictsInProv_{pit}$ variable.[24] The medium-run impact estimated by calculating the sum of the immediate effect and all 3 lags is even larger than in the main specification, as at the end of 3 years prices have fallen by 3.29% and quantities increased by 13.1%. The estimated coefficients also become even more precisely estimated. Since the estimate from the satellite data of the medium-run impact in Panel B of Table 3 on total quantities is 7.89%, the estimated medium run elasticity is 2.39 – almost exactly the same as the short-run elasticity estimate of -2.12.

We have also verified that these results are robust to a variety of alternate specifications. In particular, we have shown that the results are similar if, instead of weighting by the quantity in the first year, we instead weight by current quantities. This weighting is most similar to the one applied by the Poisson Quasi-Maximum Likelihood. We have also shown that the results are robust to excluding from the district count *kotamadya* (major cities), which do not control any forest and hence should not affect logging. A falsification test where we include only *kotamadya* shows no impact, as one would expect. Finally, we have repeated analysis of leads of district splits in Table 4 above for the official data. The medium-run impact of district splits on prices and quantities is robust to the inclusion of leads and is similar in magnitude and significance to Table 5. For our main specification (columns 1 and 2), both the sum of the leads and the p-value from a joint F-test of all three leads together are statistically insignificant, indicating that there are no pre-trends in our main specification. While there is scattered evidence of significant effects on the leads in alternate specifications (equivalent to columns 3-6), in the main time period and specification we examine – 2001 through 2007 – we find no evidence of significant pre-period differential trends. These results are all shown in the Appendix.

3.5 Results for the satellite data at the district level

Since the satellite data show us deforestation at a very fine pixel level, we can further disaggregate logging by district as well as forest zone. This allows us to do two things. First, we separately estimate the direct effect of a district splitting – i.e., the impact in the district that splits itself – from the indirect of the district splitting – i.e., the impact on logging on other districts in the same province. Second, we can further test the degree to which changes in enforcement are driving the results (as opposed to market forces) by examining whether the increase in deforestation following district splits is higher in the new part of the district (where a new district office is being set up) as opposed to the old part of the district (which inherits the district office from before.)

3.5.1 Direct vs. indirect effects of district creation

The results from estimating equation (3) are shown in Table 6, and paint a very different picture for direct and indirect effects of district splits for the production/conversion zones and the conservation/protection zones. For direct effects – e.g., the impact of a split on

[24] The results do not change substantially if we use five lags instead.

the district that splits – the overall impact effect shown in Panel A is negative (though insignificant). This is driven by substantial decline in deforestation in the production zone – a decline of 21.1%. On the other hand, there appears to be an increase in illegal logging – deforestation in the conservation zone (i.e., national parks) increases by 13.6% – when the district splits.

Panel B shows, however, that the pattern of these direct effects begins to change over time. By the time the district has been in existence for three years, deforestation in legal logging zones begins to increase, partially offsetting the initial declines, so that the third lag on the number of district splits is positive and statistically significant. While the net effect (the sum of the lags) is not distinguishable from zero, the p-value on a joint test of the contemporary effect and all 3 lags in the legal logging zones (column 2) is < 0.01, suggesting that the pattern we observe – a decline in deforestation initially, followed by an increase – is indeed highly statistically significant. Meanwhile, deforestation in illegal logging zones continues to intensify, so that the net effect in illegal logging zones is an increase of 25.1% (Panel A, column 3, sum of lags), driven by a 37% increase in conservation zones (column 6) and a 13% increase in protection zones (column 7). On net, the total increase in deforestation after 3 years (shown in column 1) is 3.2%, though this is not statistically significant.[25]

For indirect effects, i.e., the effect on other districts in the same province, by contrast, the impact on deforestation is positive and immediate, and is concentrated in the legal logging zones. The impact effect of a district splitting is to increase overall logging by 7% in all other districts in the province (Panel A, column 1); the medium-run impact is 9.5% (Panel B, column 1, sum of lags). There are no statistically significant impacts in illegal logging zones outside of the district that splits.

The difference between the direct and indirect effects of a new district forming suggests a consistent explanation for the results in this section along the following lines. When a district splits, the initial disorganization initially disrupts legal logging activities. Other districts within the same province increase logging immediately. This may reflect a combination of three forces: other districts increasing the quantity of illegal logging in response to the lower extraction from the district that split; other districts further increasing extraction as they anticipate that prices will fall once the new districts are fully established and begin to log more; and the central government reallocating legal production quotas to the other districts in the province. Of these, the first is an example of static Cournot effects; the second is an example of dynamic Cournot effects with a non-exhaustible resource as in Lewis and Schmalensee (1980); and the third is a direct political economy influence. Some combination of all three may be taking place.

For the conservation and protection zones, where we know all logging is illegal, the im-

[25] The Appendix shows that the main results are robust to the inclusion of the leads, and that we do not find a significant sum of leads for the $NumOtherDistricts_{dit}$ variable. In almost all specifications in the Appendix table, we do not find statistically significant effects on either the sum of the leads, or on the joint test of significance of all leads. The only exceptions are the sum of the leads for own splits in the conservation zone (Column 6) and for other splits in the conversion zone, but given that we find significance in only 3 out of the 28 lead tests we consider it is likely that these are just noise, rather than true differential trends.

pacts begin in the own district immediately and intensify over time. As with the provincial level results, the fact that the impacts on illegal logging intensify over time, rather than decline, suggests that this is not merely driven by a decline in enforcement capability associated with the new district's formation. In a benchmark static Cournot model, with equal and constant marginal costs, we would expect that the district that splits should experience an increase in its own production, which is what we observe; the impact on other districts in the same province in such a model is theoretically ambiguous.

3.5.2 New vs. old parts of the district

As discussed above, an alternative explanation for the increase in rent extraction following the creation of a new district is a decline in enforcement capability as the new districts offices are set up. If this was the case, one would expect that there would be an initial increase in extraction, which would then decline over time as enforcement capabilities returned. All the evidence thus far has suggested the opposite: in fact, deforestation increases over time after the district splits.

A further test of the enforcement hypotheses is to look within the district that splits at new vs. old parts of the district. When a district splits, one of the new districts (the original district, or *kabupaten induk*) retains the original capital city and infrastructure; the remaining new districts (*kabupaten baru*) need to set up brand new capital cities, legislatures, and bureaucracies (including district forest offices). In forestry (as in most things), though the original district provides support to the new districts while they are establishing their new offices, one would imagine that monitoring and enforcement capacity would be weaker in the parts of the district handled by the new office than the parts handled by the old office. If lack of enforcement was driving the increase deforestation, we would therefore expect it to be worse in the new part of the district.

To examine this, we re-estimate a version of equation (3). We define an observation based on the final 2008 district borders, and create a dummy variable for each 2008 border-district, $HasOriginalCapital$, which takes the value of 1 if the original capital of the 1990-era district is located within the boundaries of the 2008 district, and 0 otherwise. $HasOriginalCapital$ therefore takes value 1 for the original district and value 0 for the new districts.[26] We then estimate

$$\begin{aligned}\mathbf{E}\left(deforest_{dit}\right) &= \mu_{di}\exp(\beta NumOwnDistricts_{dit} + \\ &\quad \gamma NumOwnDistricts_{dit} \times HasOriginalCapital_i + \eta_{it})\end{aligned} \tag{4}$$

Note that $NumOwnDistricts_{dit}$ is still defined based on the 1990 district borders, as above. Robust standard errors are also clustered at the 1990 district border level.

The results are presented in Table 7. In Panel A, which examines the contemporaneous effect, the coefficient on the interaction term γ (the coefficient on $NumOwnDistricts_{dit} \times$

[26] If districts split multiple times over the period we study, for simplicity $HasOriginalCapital$ traces the location of the original, 1990 district throughout. We have verified that the results are similar if we also trace the 2003 capital throughout.

$HasOriginalCapital_i$) is not statistically significant either overall or in any of the forest zones, suggesting no detectable differences between the old and new parts of districts. In Panel B, which examines the lag structure, we do find that the interaction term γ is negative in the year of the split in the conservation and protection zones, indicating increased deforestation in the year of the shock in the new part of the districts relative to the old part. However, when we examine the sum of the lags, which shows the net impact after 3 years, we find no negative interactions. In fact, the point estimate on sum of the lags is positive in all cases (and statistically significant in the conservation zone). Thus, on net after three years, we find no differences in deforestation between old and new parts of the district, and if anything, more deforestation in the old part of the district. This fact – combined with the evidence shown above that deforestation increases in the medium run – suggests that the increases in deforestation we are observing are unlikely to be primarily driven by declines in enforcement.

3.6 Interpreting magnitudes in a Cournot framework

The empirical analysis above showed that as the number of independent jurisdictions within a province increases, the quantity of deforestation produced in that province increases and the price of wood in that province falls, as one would expect from a model of Cournot competition. Specifically, focussing on the satellite data (which captures both legal and illegal extraction), the overall semi-elasticity of quantity produced with respect to the number of jurisdictions was 0.036 in the short run and 0.079 in the medium run. The estimated price elasticity of demand was around 2.1 in both the short and medium run.

In this section we examine whether these magnitudes are broadly consistent with what would expect from a stylized, textbook Cournot model. The point is not that a simple model will provide an exact description of our setting, but rather just a consistency check that the magnitudes we estimate are broadly consistent with what theory might predict.

To be concrete, suppose we have n identical districts in the province, each of whom faces marginal cost c of producing wood. Suppose the inverse demand function is $p(Q)$ where Q is the total quantity of wood produced in the province. Each district i solves

$$max_{q_i} q_i p \left(\sum q \right) - c q_i \tag{5}$$

The first order condition is

$$q_i p' + p - c = 0 \tag{6}$$

Rewriting and substituting $Q = n q_i$ yields the familiar Cournot equation:

$$\frac{(p-c)}{p} = \frac{1}{n\varepsilon} \tag{7}$$

where ε is the price elasticity of demand.

To derive a formula for the semi-elasticity of quantity with respect to the number of districts, we need to posit a functional form for the inverse demand function. Suppose the elasticity of demand is pinned down by the availability of substitute sources of wood in neighboring markets, so we have constant elasticity of demand, i.e. $p = \frac{a}{q^{\lambda}}$, where $\varepsilon = \frac{1}{\lambda}$. Substituting $p = \frac{a}{q^{\lambda}}$ into equation (6), taking derivatives, and simplifying yields:

$$\frac{1}{Q}\frac{dQ}{dn} = \frac{1}{n^2 - n\lambda} \tag{8}$$

Are the empirical estimates broadly with equations (7) and (8)? In the beginning of our period (2001), we have 116 districts in 21 provinces who are producing logs, so on average we have $n = 5.5$. Substituting the empirical elasticity estimates and the number of districts into equation (8) suggests that the semi-elasticity of quantity with respect to the number of districts ($\frac{1}{Q}\frac{dQ}{dn}$) should be approximately 0.036. Empirically, we estimate using the satellite data that $\frac{1}{Q}\frac{dQ}{dn}$ is 0.035 in the short run and 0.079 in the medium run. The short-run estimate exactly matches the theoretical prediction, and more generally, these estimates are of the same order of magnitude as that predicted by the theory.

Checking the other prediction – the prediction about the markup in equation (7) – is necessarily more speculative, since we do not observe the markup directly. Substituting our estimates into equation (7) suggests that the markup ($\frac{(p-c)}{p}$) should be around 0.09.

How can we estimate the markup in practice? One way to gauge the markup is to look at the bribes charged by corrupt officials who determine q_i. As discussed in Section 2.1.2, within a district, there are many small firms who are willing to fell wood illegally, but they must bribe district officials to obtain an illegal transport permit in order to do so. Suppose that the district sells q_i illegal log transport permits to these small firms in return for bribes. In equilibrium, the firms will be willing to pay up to the full markup, $p - c$, in the form of bribes b.[27]

How large are the bribes b in practice? Direct estimates are scant, but Casson and Obidzinski (2002) estimate that they are of the same order of magnitude as the a relatively small share of the total price, consistent with what equation (7) would suggest. Based on fieldwork in Kalimantan, Casson and Obidzinski estimate that in one district the bribe to receive an illegal wood transport permit is \$22 / m3 of wood. They also note that district officials only require sawmills to purchase these illegal permits for 20% of the wood they process, so the effective bribe required is about \$4 / m3. Since wood prices vary from \$120 to \$250 / m3, the bribes are equal to between 0.01 and 0.03 of the total price of the wood. This is only the transport permit: there are also (presumably) additional bribes to fell the wood. If the additional bribes are similar in magnitude, that would mean that the total bribe is between 0.02 to 0.06 the total price of the wood. In a second district, the district government levies official "fees" on illegal timber of about \$20 / m3, or between 0.08 and 0.16 of the total price. Although in this second case the fees go to the district treasury, they mention that district officials get some return from collecting these fees in the form of higher popularity with their constituents. Although these data are admittedly very rough,

[27] Formally, the district governments solve $\max_{q_i} bq_i$, and free entry among firms ensures that in equilibrium $b = p - c$, so this problem ends up being identical to (5).

the share captured by the district officials appears on the same rough order of magnitude as the 0.03 and 0.09 range predicted by the theory.

On net, the results in this story suggest that the amount of deforestation districts allow may be driven by competition in the product market for wood, as would be predicted by a Cournot-style model. Several points of evidence provide evidence in favor of the competition-style story compared to alternative explanations. First, the fact that increasing jurisdictions not only increases quantities, but also reduces prices, confirms that there is to some degree a downward sloping demand curve for logs in each province. Second, the fact that this occurs in zones where all logging is illegal suggests that this is not merely an artifact of changing allocation rules from the central government. Third, the facts that the impact of new jurisdictions on deforestation rates increases over time, rather than decreases, and the fact that deforestation is not more likely to occur in the new part of the district suggest that declines in enforcement in the illegal logging zones are not primarily driving the results. Finally, a back of the envelope calculation suggests that the quantitative impact of increased political jurisdictions on deforestation is consistent with what one would expect from a simple Cournot model given the equilibrium elasticities observed in the data.

4 Political logging cycles

4.1 Empirical tests

The literature on political business cycles suggests that politicians tend to increase expenditures and postpone tax increases in the years leading up to elections, both at the national level (e.g., Nordhaus 1975, MacRae 1977, Alesina 1987, Rogoff and Sibert 1988, Akhmedov and Zhuravskaya 2004) and at the local level (e.g., Poterba 1994, Besley and Case 1995, Levitt 1997, Finkelstein 2009). This section examines whether political cycles affect not only the legal actions by the state, but the state's permissiveness towards illegal activity. In particular, we examine whether logging in general, and illegal logging in particular, increases in the years leading up to a district election.

To do so, we take advantage of the fact that the timing of district-level elections in Indonesia varies from district-to-district. As discussed in Section 2.1.1, prior to 2005, the heads of districts (known as *Bupati*) were indirectly selected by the district parliament. Starting in 2005, Bupatis were to be directly elected by the population in special elections (ROI 2004). Crucially, the direct elections of Bupatis were phased in as the prior Bupati's term expired, so that some districts had their first direct elections as early as 2005 while others had them as late as 2010.[28] As documented in detail by Skoufias et al. (2010), the timing of these direct elections was determined exclusively by when a Bupati's term expired, which was in turn driven by idiosyncratic factors, such as retirements and appointments of existing Bupatis to other posts, that determined Bupati appointments under the pre-1998

[28]No direct elections for Bupatsi were held in 2009, as national Presidential elections were held that year. Those Bupatis whose term was ending in 2009 were extended on an interim basis and direct elections were held in 2010 instead.

Soeharto regime (Emmerson 1999). Skoufias et al. (2010) examine this empirically and verify that the resulting timing of local elections is uncorrelated with a host of economic, social, and geographic characteristics.[29]

To estimate the impact of elections on logging, we use the satellite data and estimate fixed-effects Poisson QMLE models on the various subcategories of the 'Forest Estate' that estimates the following equation:

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp\left(\sum_{j=t-2}^{t+2} \beta_j Election_{dij} + \eta_{it}\right) \tag{9}$$

where j indexes leads and lags of the *Election* variable, which is a dummy for a Bupati election taking place. As in equation (3) above, we include district fixed effects and island-by-year fixed effects, and cluster standard errors at the 1990 district level, but since elections take place at the district boundaries at any point in time, we use the 2008 district boundaries (interacted with forest zone and year) as the unit of observation. We include up to 2 leads and 2 lags of the *Election* variable to fully capture the 5 year election cycle.[30] Note that since the official forestry statistics are only at the province level, whereas our variation is in the timing of elections within provinces, we cannot use the official forestry statistics dataset for this purpose.

4.2 Results

The results from estimating equation (9) are shown in Table 8. Panel A shows the impact effect of elections (i.e., no leads and lags); Panel B presents the results with 2 leads and lags of the *Election* variable. As before, we present results for the entire 'Forest Estate,' as well as broken down by land use zone.

The results show clear evidence of a political logging cycle in the illegal forest zones. Focusing on column (3) of Panel B, which shows the impact on the conservation and protection zones where no legal logging is allowed, we find that illegal logging increases dramatically in the years leading up to an election: by 29% 2 years prior to the election and by 42% in the year before the election. Illegal logging then falls dramatically (by 36%) in the election year and does not resume thereafter. Looking zone-by-zone, we see that the pattern is strongest statistically in the protection zone (column 7), but that the point estimates suggest a very similar pattern in the conservation zone as well (column 6).

[29]Specifically, Skoufias et al. (2010) run a regression of the probability of holding a direct election by 2007 and regress it on the end date of the previous Bupati's term and the following variables: unemployment rate, log real per capita district GDP, log real per capita district GDP without oil and gas, share of minerals in district GDP, share of energy in district GDP, dummy for district having oil and gas, share of population that is urban, share of asphalt roads in the district, share of rock roads in the district, access to telephones, distance to provincial capital, dummy for being a split district, share of mountainous areas in the district, share of coastal areas for the district, share of valley areas in the district, a city dummy, and 5 island dummies. Other than the end date of the previous Bupati's term, only 1 of the 21 variables they consider (a Sulawesi island dummy) is statistically significant at the 10% level. See Table A-1 of Skoufias et al. (2010).

[30]The omitted category is therefore the years prior to 2 years before the first direct election.

There are several possible explanations for the increase in illegal logging in the years leading up to the elections. One potential explanation is that logging was permitted or facilitated by district officials in return for campaign funds.[31] A second explanation is that district officials simply reduced enforcement of logging in the conservation and protection zones in order to increase their popularity and win votes. Since these two stories are observationally equivalent in terms of the predicted impact on deforestation, it is not possible to tease them apart empirically.

Turning at the zones where logging may be legal or illegal (conversion and production), we see a different pattern. In the conversion zone, we find a 40% increase in logging in the year of the election and a 57% increase in the year following the election. We find no impact on the production zone. According to Barr et al. (2006), many district governments have redirected their interest towards the development of oil palm plantations and other agroindustrial estates in recent years. It is possible that the observed increase in clear-cutting in the 'Conversion Forest' after the election is a repayment for favors during the election campaign. Alternatively, it could be an attempt to grab first rents upon being elected. Once again, these stories are observationally equivalent, so it is not possible to tease them apart empirically with the existing data. Since the effects in the conservation/protection zone and the production/conversion zones have different patterns, column (1) shows little impact overall.

5 Substitutes or complements? Logging vs. other sources of rents

5.1 Empirical implementation

An important question in the economics of corruption is how corrupt officials with multiple opportunities for rent extraction respond if one type of corruption becomes harder or easier. If corrupt officials behave like profit maximizing firms, and there are no spillovers from one type of corrupt activity to the other, then they would optimize separately on each dimension, and there would be no impact of a change in one type of corruption opportunity on the other type of corruption.

More generally, however, one could imagine effects going in either direction. If corrupt officials worry about being detected, and if being detected means the official loses both types of corruption opportunities, then the two types of corruption will appear to be substitutes, and increasing corruption opportunities on one dimension will lower them on the other dimension. On the other hand, if there are fixed costs of being corrupt (for example, those with a low disutility from being corrupt selecting into the civil service), multiple corruption opportunities could be complements. The two existing studies that have examined this ques-

[31] Although we know of no direct qualitative evidence for this link, Greenpeace Indonesia (2009) has asserted that political parties ammassed campaign funds for the 2009 general election through facilitating illegal logging.

tion empirically (Olken 2007 and Niehaus and Sukhtankar 2009) have both found evidence that alternative forms of corruption appear to be substitutes.

In this section, we examine this question by examining how logging responds to changes in another source of local rents for district governments: oil and gas revenues. Under Indonesia's Fiscal Balancing Law (ROI 1999), a fraction of all oil and gas royalties received by the central government is rebated back to districts, with half of the rebate going to the district that produces the oil and gas and the other half of the rebate being shared equally among all other districts in the same province. This can amount to a substantial amount of revenue – as much as US$729.63 per capita in the highest district – which can in turn be a tempting source of rents for district officials.[32] Moreover, the precise amount of oil and gas revenue allocated to each district varies substantially over time as oil and gas production fluctuates, oil and gas prices change, and district boundaries change. The idea that oil revenues are a source of illegal rents is consistent with findings from other contexts (e.g., **?**, **?**).

A key distinction between our context and the existing literature is that while the existing literature (Olken 2007 and Niehaus and Sukhtankar 2009) studies short-run substitution from one type of corruption to another, our setting allows us to examine both the short and medium run. If the fixed costs of corruption are important, adjustment may take time, and the short and medium-run effects could be quite different.

To examine the short-run impact of oil and gas rents on illegal logging, we estimate a version of equation (3). Since district splits influence oil and gas prices through the sharing formula, we control for district splits directly, and estimate the following equation:

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp\left(\beta PCOilandGas_{dit} + \gamma Numdistricts_{dit} + \eta_{it}\right) \tag{10}$$

where $PCOilandGas_{dit}$ is the per-capita oil and gas revenue received by the district (in US$). Note that in computing $Numdistricts_{dit}$ when estimating (10), we count a district as having split only when it reports receiving its own oil and gas revenue.[33] Each observation is a district (using the 2008 borders) $\times$ forest zone $\times$ year. As above, μ_{di} is a district fixed-effect, η_{it} is an island$\times$year fixed effect. We report robust standard errors are reported adjusted for clustering at the 1990 district boundaries. Since district oil and gas sharing revenue is, on average, 20 times larger than that generated by the forestry sector, one would

[32] District government officials have recently been exposed in a wide variety of strategies to capture rents from the oil and gas revenue sharing fund. In Kabupaten Kutai Kartanegara, for example, the national Anti-Corruption Commission recently documented that in 2001 the Bupati simply issued a decree giving himself, top district government officials, and district parliamentarians an official monthly stipend equal to 3 percent of the amount the government received in oil and gas revenue, amounting to over US$9 million over a 4 year period (KaltimPost 2009b, KaltimPost 2009a). In Kabupaten Natuna, Sumatra, a former Bupati was arrested in 2009 by the Anti-Corruption Commission for allegedly embezzling US$8 million in oil and gas revenue funds, by appropriating the funds to a fake committee that he never set up (Kompas 2009). In Kabupaten Karawang, West Java, in 2004 the Bupati allegedly deposited US$600,000 in oil and gas revenue sharing funds into his personal account rather than the district treasury (KoranTempo 2006).

[33] As described above, *de facto* establishment of a district takes 1-3 after the official *de jure* implementation. Since we care about district splits in this case because they affect the oil and gas allocation formula, it is important to control here for the *de facto* date the district split took effect, as that is the date the oil and gas formula would be affected.

not expect forestry decisions to influence oil and gas choices, so we would expect oil and gas revenue to be exogenous with respect to deforestation. To examine the medium-run impacts of oil and gas rents on illegal logging, we estimate (10) as above, but include 3 lags of $PCOilandGas_{dit}$.[34]

5.2 Results

The results from estimating equation (10) are shown in Table 9. Panel A, which shows the immediate impact effect of oil and gas revenue on logging, confirms evidence of short-run substitution between deforestation and oil and gas rents. Specifically, each US$1 of per-capita oil and gas rents received by the district reduces logging by 0.3%. These effects are found in both the legal logging zones (0.3% in production/conversion; column 2) and in the illegal logging zones (0.6% in the conservation/protection zones). To interpret the magnitudes, note that the standard deviation of $PCOilandGas_{dit}$ after removing district fixed effects is 23.7; so a one-standard deviation change in $PCOilandGas_{dit}$ decreases deforestation by 7.1% in the production/conversion zones and by 14.2 percent in the conservation/protection zones.[35]

Panel B shows, however, that the short-run and medium-run effects are quite different. While the immediate effect of oil and gas revenue on logging is still negative (0.5% per US$1, Panel B, Column 1), the sum of the lags is now positive and statistically insignificant. That is, after three years, the total medium-run effect of US$1 of per-capita oil and gas rents is to increase logging by 0.2%. Once again, this shift occurs equally in the legal logging zones (0.2%, column 2) and illegal logging zones (0.1%, column 3). While none of these effects are statistically significant, we can reject the null hypothesis that the sum of the lags and the immediate effect are the same at the 1% level. This suggests that the short and medium-run impacts are different, and in the medium run, oil and gas rents and rents from logging are no longer substitutes.

An important question is why the effects might change over time. One natural hypothesis is that the higher oil and gas rents attract a different type of politician to office who is more interested in rent extraction. These politicians would then extract more rents on all

[34] Note that we do not have district-level data for *PCOilandGas* prior to 2001, so there is a question of how to assign lag values of *PCOilandGas* in the early years of our sample. Prior to the new revenue sharing rules taking effect in 2001, there was very little of this type of revenue sharing with districts. For example, in 2000 (prior to decentralization), for all of Indonesia, the total for all royalties (oil and gas plus other revenue sharing) shared with districts was 538 billion. In 2001, the first year of the new revenue sharig regime, it was 9,312 billion Rupiah. Given that total revenue sharing prior to 2001 was less than 5% of the value in 2001 and after, we assume that oil and gas revenue was 0 prior to 2001 in computing lags. Using missing values for these lags instead produces qualitatively similar results in aggregate, though the reversal between short and long run is now limited only to the production / convserion zone (see Appendix Table 4 in the online appendix).

[35] One might be concerned that these effects reflect labor market substitution, as labor moves into the oil production sector when prices are high. However, we have verified that the same results separately both for oil producers and non-oil producers, where the results for non-oil producers are driven only by the revenue sharing they receive from other oil producing districts in the same province, suggesting this is not driven by labor market factors.

dimensions, both from the oil and gas sector and from forests. To investigate this hypothesis, we begin by interacting oil and gas revenues with a dummy that captures whether the new direct election for district heads has taken place or not, i.e.

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp\left(\begin{array}{c} \beta PCOilandGas_{dit} + \delta PostElection_{dit} \\ +\pi PCOilandGas \times PostElection_{dit} + \gamma Numdistricts_{dit} + \eta_{it} \end{array}\right) \tag{11}$$

The key coefficient of interest is π, which captures how the coefficient on $PCOilandGas$ changes after the direct election. We continue to control for $NumDistricts$ as in equation (10).

The results are presented in Table 10. The results show that π is positive, i.e. the negative effect of oil and gas revenues on logging attenuates once the direct election is held. Specifically, the point estimates suggest that 35% of the substitution effect between oil and gas revenues and forest extraction disappears once the direct election is held. This provides suggestive evidence that the medium-term reversal in the negative oil and gas effect is mitigated through a change in the political equilibrium.

What about the political equilibrium might be changing? In results shown in the online appendix (see Appendix Table 2), we find that higher oil and gas revenues lead to fewer candidates running in the direct election, and instead lead to the new Bupati representing a larger coalition of parties, using data from Skoufias et al. (2010) We find no impact on the probability the incumbent is re-elected. It is possible that these larger coalitions engage in more rent extraction as they have more people with whom to share the spoils of office. Consistent with this, we also find evidence that having fewer candidates or a larger coalition is associated with a greater increase in logging, though the effects are only statistically significant in some forest zones and only in some specifications (see Appendix Table 3). Together, these results, as well as the results in Tables 9 and 10, suggest that the higher political rents lead to a change in the political equilibrium, which in turn undoes the short-run substitution between oil rents and forest extraction.

6 Conclusions

This paper has demonstrated how the incentives faced by local politicians and bureaucrats play an important role in determining the rate of deforestation in Indonesia. Using a novel MODIS satellite-based dataset that tracks deforestation on an annual level across the whole of Indonesia, we have shown that the rates of deforestation are influenced by the return local officials face in the market for logs, by their short electoral needs, and by the availability of alternative sources of rent extraction.

More broadly, the results in the paper demonstrate that the pattern of forest cutting in Indonesia is the not result of some optimal forest management model implemented by the Ministry of Forestry. The decisions that matter for whether trees are cut down or not take place not just in ministerial meeting rooms but also are a result of the push and shove of local politics.

To the extent one wants to slow the rate of tropical deforestation, these results matter. They suggest that unless the incentives of local politicians and bureaucrats are taken centrally into account then central or donor driven policies to counter deforestation may be ineffective. In particular, the recently ratified Reducing Emissions from Deforestation and Forest Degradation (REDD) initiative provides countries with a source of funding to reduce deforestation that is likely to grow considerably over time. Indeed, Indonesia is to be the first recipient of REDD funds, having signed a US$1 billion REDD agreement with Norway in 2009. However, unless REDD programs are designed taking into account those local actors who currently derive considerable benefits from legal and illegal logging, it is unlikely to be effective.

Though instructive in term of revealing how political economy plays a central role in the deforestation process our paper very much leaves open this central question of how best to counter this process. But the results imply taking the financial and electoral pressures upon local politicians and bureaucrats is a crucial step to sustainable tropical forestry.

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Figure 1: Forest cover change in the province of Riau, 2001-2008

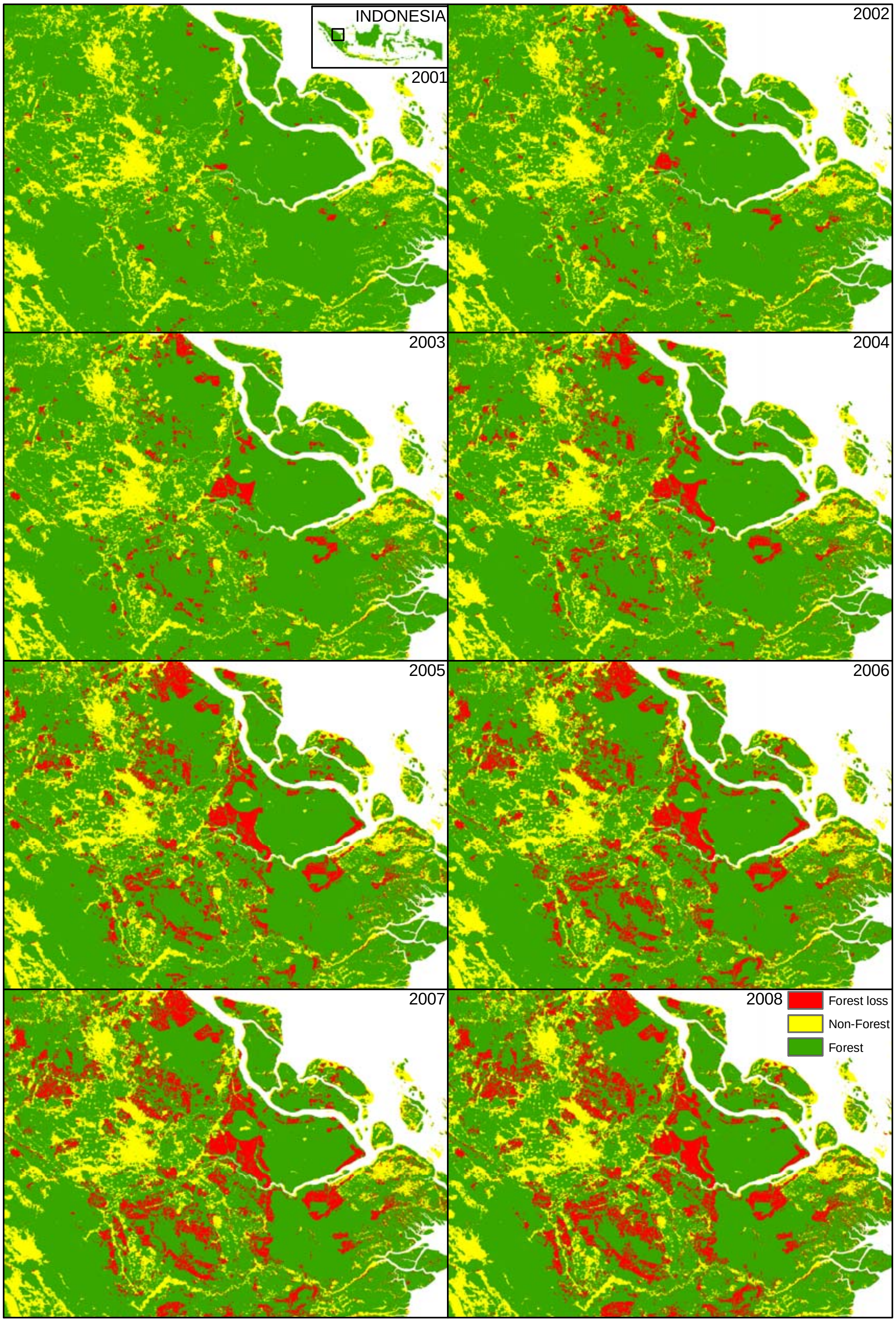

Figure 2: District-level logging in Indonesia using the 2008 district boundaries, 2001 and 2008

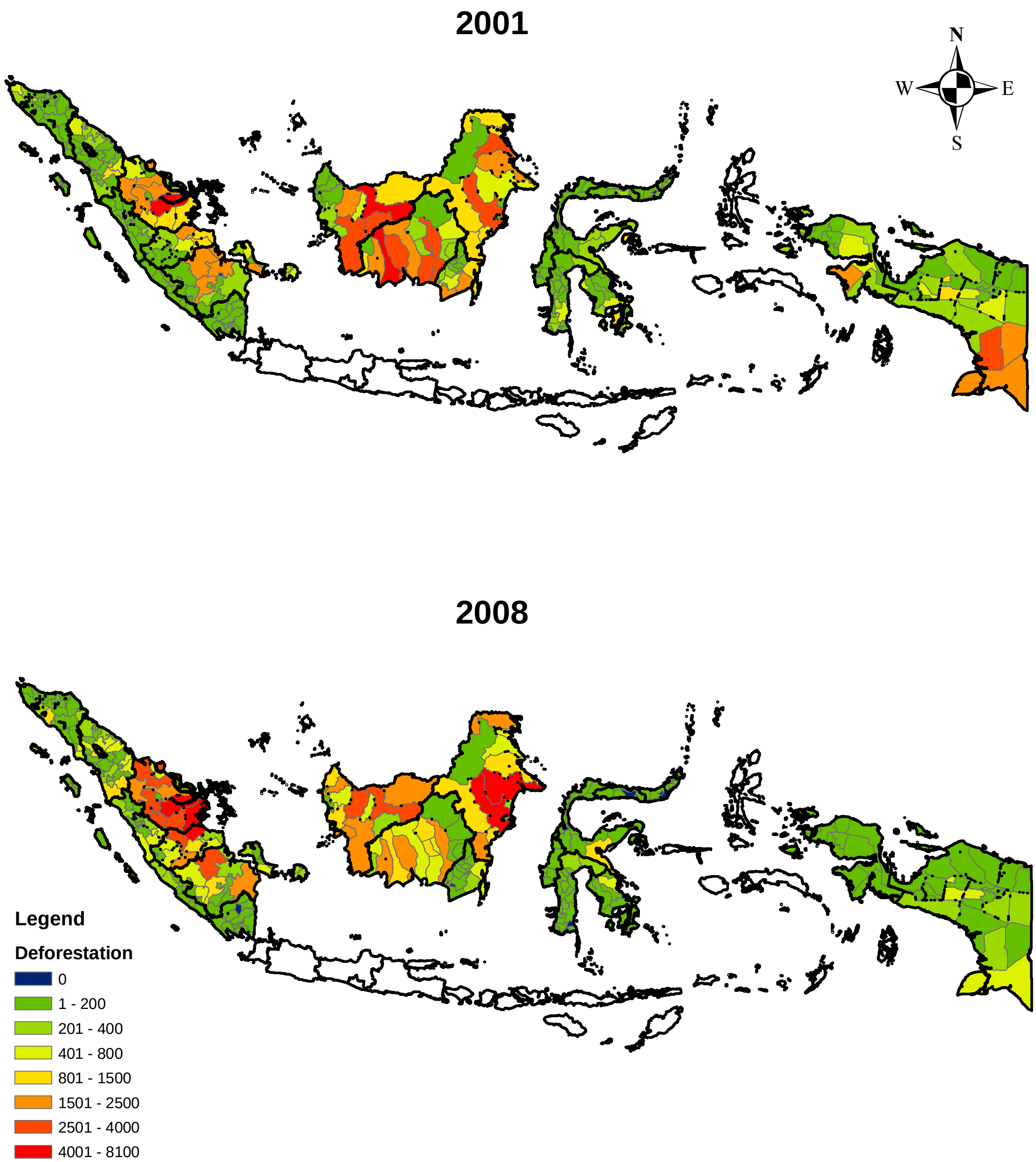

Figure 3: Total number of district splits using the 1990 district boundaries

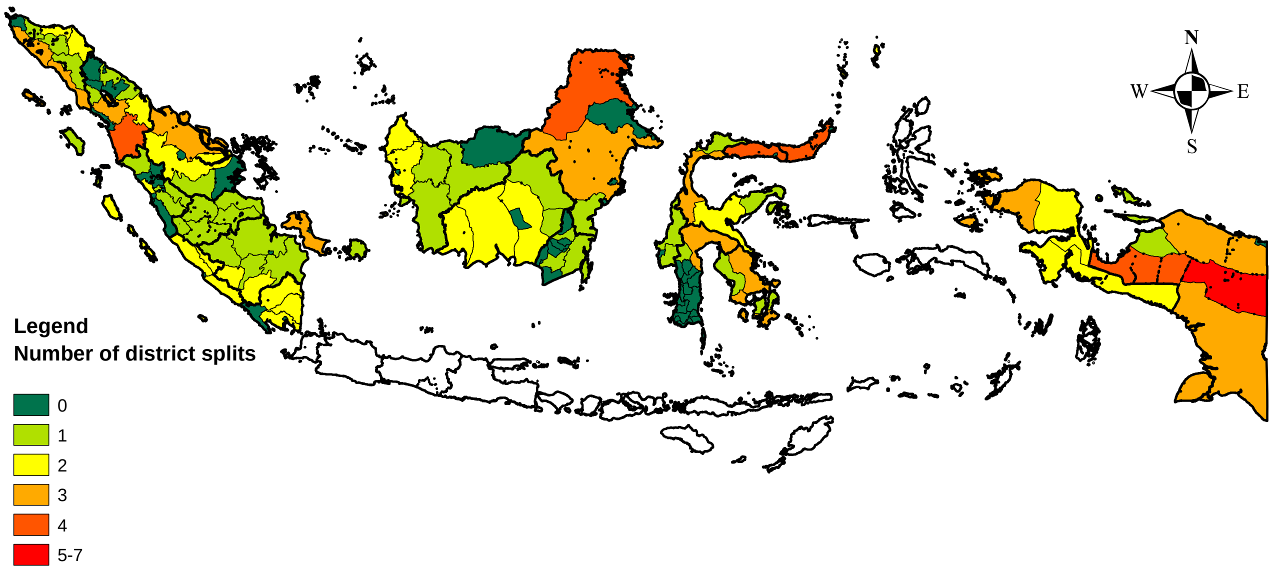

Figure 4: Oil and gas revenue per capita using the 2008 district boundaries, 2008

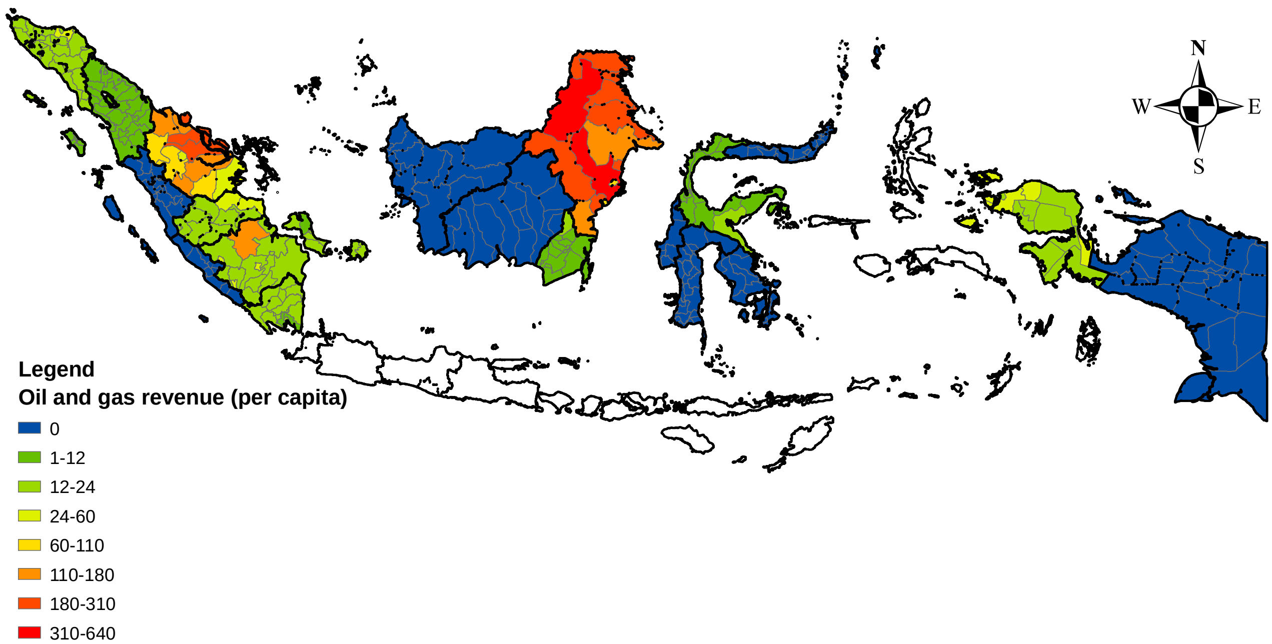

Table 1: Summary statistics

Year	(1) Total land pixels	(2) 2000	(3) 2001	(4) 2002	(5) 2003	(6) 2004	(7) 2005	(8) 2006	(9) 2007	(10) 2008	(11) Change 2008-2000
All Forest	18,986,240	17,567,200	17,493,600	17,353,440	17,287,520	17,199,840	17,115,200	16,946,560	16,855,840	16,784,160	-783,040
Production/Conversion	11,894,240	10,865,280	10,803,360	10,697,280	10,640,320	10,567,840	10,492,640	10,348,320	10,264,640	10,199,200	-666,080
Conservation/Protection	7,092,000	6,701,760	6,690,240	6,656,160	6,647,200	6,631,840	6,622,560	6,598,080	6,591,200	6,584,960	-116,960
Conversion	3,098,080	2,652,160	2,633,600	2,607,040	2,591,520	2,570,400	2,545,920	2,512,640	2,490,560	2,472,800	-179,360
Production	8,796,320	8,213,120	8,169,760	8,090,240	8,048,800	7,997,440	7,946,720	7,835,680	7,774,080	7,726,400	-486,720
Conservation	2,731,840	2,515,200	2,510,720	2,490,240	2,485,920	2,478,400	2,475,520	2,460,960	2,457,120	2,454,880	-60,320
Protection	4,360,000	4,186,560	4,179,520	4,165,920	4,161,120	4,153,440	4,147,040	4,137,120	4,134,080	4,129,920	-56,640
Changes in all forest			-73,440	-140,320	-65,920	-87,680	-84,640	-168,640	-90,720	-71,680	-783,040

Notes: The forest dataset has been constructed from MODIS satellite images, as described in Section 2.2.1. It counts the total number of forest pixels by year and forest zone. The units are the number of MODIS pixels in each class, where a MODIS pixel represents an area approximately 250m * 250m in size.

Table 2: Summary statistics of pixels deforested in 1000HA by district×year

Logging	(1) All Forest	(2) Production/Conversion	(3) Conservation/Protection	(4) Conversion	(5) Production	(6) Conservation	(7) Protection
Mean	113	203	32	152	232	40	26
Standard deviation	464	641	164	423	735	221	106
Observations	6952	3280	3672	1184	2096	1520	2152

Notes: The forest dataset has been constructed from MODIS satellite images, as described in Section 2.2.1. It counts the total number of forest cells by year and forest zone. The variable shown here is the key dependent variable analyzed in Sections 3-5.

Table 3: Satellite data on impact of splits, province level

VARIABLES	(1) All Forest	(2) Production/Conversion	(3) Conservation/Protection	(4) Conversion	(5) Production	(6) Conservation	(7) Protection
Panel A							
Number of districts	0.0361**	0.0424**	0.0391	0.0283	0.0533***	0.0786*	0.00645
in province	(0.0160)	(0.0180)	(0.0317)	(0.0333)	(0.0199)	(0.0415)	(0.0322)
Observations	672	336	336	128	168	144	168
Panel B: Lags							
Number of districts	0.0370	0.0435	0.0833***	0.0447	0.0523	0.0959**	0.0657*
in province	(0.0284)	(0.0332)	(0.0299)	(0.0420)	(0.0350)	(0.0417)	(0.0377)
Lag 1	0.0405	0.0434	-0.129**	0.00823	0.0419	-0.170	-0.0732
	(0.0446)	(0.0461)	(0.0651)	(0.0641)	(0.0434)	(0.130)	(0.0623)
Lag 2	-0.0717***	-0.0740***	0.0186	-0.0883**	-0.0625**	0.111	-0.0851
	(0.0265)	(0.0250)	(0.0762)	(0.0346)	(0.0257)	(0.153)	(0.0679)
Lag 3	0.0731*	0.0654	0.117*	0.107	0.0476	0.0889	0.141**
	(0.0397)	(0.0399)	(0.0610)	(0.0880)	(0.0357)	(0.0614)	(0.0610)
Observations	672	336	336	128	168	144	168
Joint p	4.75e-06	6.95e-08	0.0235	0.0428	0.000923	0.0486	0.0665
Sum of lags	0.0789***	0.0783***	0.0900**	0.0712	0.0793***	0.125**	0.0484
	(0.0200)	(0.0190)	(0.0400)	(0.0616)	(0.0214)	(0.0611)	(0.0357)

Notes: The forest dataset has been constructed from MODIS satellite images, as described in Section 2.2.1. It counts the total number of forest cells by year and forest zone. Note that 1000HA = 10 square kilometres. *Number of districts in province* variable counts the number of districts within each province. The regression also includes province and island-by-year fixed effects. The robust standard errors are clustered at the 1990 province boundaries and reported in parentheses. *** 0.01, ** 0.05, * 0.1

Table 4: Satellite data on impact of splits, leads

VARIABLES	(1) All Forest	(2) Production/Conversion	(3) Conservation/Protection	(4) Conversion	(5) Production	(6) Conservation	(7) Protection
Number of districts	0.0406	0.0444	0.0882**	-0.0105	0.0637	0.138***	0.00976
in province	(0.0396)	(0.0460)	(0.0352)	(0.0304)	(0.0491)	(0.0490)	(0.0614)
Lag 1	0.0244	0.0202	-0.105	-0.0126	0.0166	-0.124	-0.0517
	(0.0480)	(0.0511)	(0.0692)	(0.0834)	(0.0473)	(0.104)	(0.0773)
Lag 2	-0.0603	-0.0547	-0.00237	-0.0712	-0.0395	0.0400	-0.0822
	(0.0385)	(0.0362)	(0.0853)	(0.0588)	(0.0336)	(0.122)	(0.0819)
Lag 3	0.0856*	0.0755	0.135	0.148	0.0584	0.156*	0.134
	(0.0518)	(0.0494)	(0.0884)	(0.123)	(0.0413)	(0.0924)	(0.0947)
Lead 1	0.0879	0.0925	0.0498	0.324*	0.0370	0.172	0.0444
	(0.114)	(0.120)	(0.136)	(0.173)	(0.110)	(0.138)	(0.136)
Lead 2	-0.118	-0.156	-0.0141	-0.257	-0.149	0.132	-0.0897
	(0.137)	(0.136)	(0.163)	(0.180)	(0.131)	(0.180)	(0.170)
Lead 3	0.0364	0.0635	-0.0432	0.117	0.0689	-0.157	0.0180
	(0.107)	(0.104)	(0.103)	(0.130)	(0.103)	(0.115)	(0.112)
Observations	504	252	252	96	126	108	126
Joint p	0.000251	0	0.0129	0	0	2.72e-09	0.0817
Sum of lags	0.0903***	0.0854***	0.116*	0.0536	0.0992***	0.210**	0.00992
	(0.0281)	(0.0239)	(0.0663)	(0.0677)	(0.0223)	(0.0944)	(0.0774)
Sum of leads	0.00586	-5.16e-05	-0.00758	0.184	-0.0432	0.147	-0.0274
	(0.0663)	(0.0587)	(0.0976)	(0.132)	(0.0566)	(0.148)	(0.0889)
Joint p leads	0.714	0.660	0.608	0.201	0.296	0.430	0.550

Notes: See Notes to Table 5. *** 0.01, ** 0.05, * 0.1

Table 5: Impact of District Splits on Prices and Quantities: Legal Logging Data

	(1)	(2)	(3)	(4)	(5)	(6)
	2001-2007		2001-2007		1994-2007	
	All wood observations		Balanced panel of wood observations		All wood observations	
VARIABLES	Log Price	Log Quantity	Log Price	Log Quantity	Log Price	Log Quantity
Panel A						
Number of districts	-0.017*	0.089**	-0.019*	0.106**	-0.023**	0.081***
in province	(0.009)	(0.041)	(0.010)	(0.036)	(0.009)	(0.016)
Observations	1003	1003	532	532	2355	2355
Panel B: Lags						
Number of districts	-0.025**	0.098	-0.027**	0.126	-0.029***	0.071***
in province	(0.010)	(0.074)	(0.012)	(0.078)	(0.008)	(0.023)
Lag 1	0.010**	-0.041	0.009	-0.035	0.010**	-0.001
	(0.004)	(0.036)	(0.005)	(0.041)	(0.004)	(0.035)
Lag 2	-0.001	0.041	-0.001	0.018	0.000	0.017
	(0.008)	(0.045)	(0.009)	(0.021)	(0.004)	(0.027)
Lag 3	-0.017**	0.033	-0.017**	0.043	-0.015*	0.029
	(0.006)	(0.044)	(0.007)	(0.040)	(0.008)	(0.037)
Observations	1003	1003	532	532	1960	1960
Joint p	0.00271	0.000533	0.00756	0.000583	0.000109	0.00645
Sum of lags	-0.0329***	0.131**	-0.0361**	0.153**	-0.0339**	0.117***
	(0.0103)	(0.0527)	(0.0116)	(0.0505)	(0.0131)	(0.0363)

Notes: The log price and log quantity data has been compiled from the `Statistics of Forest and Concession Estate'. The *Number of districts in province* variable counts the number of *kabupaten* and *kota* within each province. The regression also includes wood-type-by-province and wood-type-by-island-by-year fixed effects and are weighted by the first volume reported by wood type and province. The robust standard errors are clustered at the 1990 province boundaries and reported in parentheses. *** 0.01, ** 0.05, * 0.1

Table 6: District level analysis: direct vs. indirect effects

VARIABLES	(1) All Forest	(2) Production/Conversion	(3) Conservation/Protection	(4) Conversion	(5) Production	(6) Conservation	(7) Protection
Panel A							
Number of districts in	-0.102	-0.172*	0.0663	-0.0174	-0.211**	0.136*	-0.0284
original district boundaries	(0.0778)	(0.0913)	(0.0519)	(0.150)	(0.0864)	(0.0767)	(0.0839)
Number of districts	0.0701**	0.0967***	0.0336	0.0380	0.122***	0.0677	0.0138
elsewhere in province	(0.0275)	(0.0311)	(0.0308)	(0.0486)	(0.0326)	(0.0452)	(0.0315)
Observations	3152	1488	1664	536	952	688	976
Panel B: Lags							
Number of districts in	-0.0627	-0.0984	0.107**	0.0139	-0.133	0.151*	0.0421
original district boundaries	(0.0830)	(0.103)	(0.0542)	(0.154)	(0.0969)	(0.0874)	(0.0576)
Lag 1	-0.0185	-0.0780	-0.0739	0.207	-0.140	-0.0828	-0.0259
	(0.130)	(0.159)	(0.103)	(0.239)	(0.141)	(0.138)	(0.0806)
Lag 2	-0.0767	-0.129	0.0252	-0.438	-0.0625	0.153	-0.143
	(0.115)	(0.151)	(0.0956)	(0.287)	(0.133)	(0.161)	(0.103)
Lag 3	0.190***	0.218***	0.193**	0.154	0.243***	0.148	0.261**
	(0.0669)	(0.0737)	(0.0794)	(0.138)	(0.0789)	(0.0940)	(0.106)
Number of districts	0.0702*	0.0901**	0.0883***	0.0356	0.116***	0.105**	0.0771**
elsewhere in province	(0.0371)	(0.0434)	(0.0316)	(0.0599)	(0.0384)	(0.0436)	(0.0357)
Lag 1	0.0582	0.0802	-0.140**	-0.0296	0.0946	-0.194*	-0.0803
	(0.0584)	(0.0643)	(0.0572)	(0.0872)	(0.0619)	(0.100)	(0.0556)
Lag 2	-0.0656	-0.0535	0.0207	-0.00584	-0.0517	0.101	-0.0573
	(0.0477)	(0.0520)	(0.0780)	(0.0668)	(0.0543)	(0.119)	(0.0973)
Lag 3	0.0322	0.0111	0.0935	0.0932	-0.0238	0.0732	0.0972
	(0.0396)	(0.0426)	(0.0584)	(0.0776)	(0.0445)	(0.0527)	(0.0629)
Observations	3152	1488	1664	536	952	688	976
Joint p original	0.0632	0.00753	0.0555	0.119	0.00465	0.212	0.0120
Sum of lags original	0.0323	-0.0867	0.251***	-0.0623	-0.0929	0.370**	0.133**
	(0.114)	(0.115)	(0.0964)	(0.193)	(0.115)	(0.176)	(0.0680)
Joint p elsewhere	0.0100	0.00331	0.0265	0.589	0.00118	0.00983	0.130
Sum of lags elsewhere	0.0951**	0.128***	0.0622	0.0934	0.135***	0.0851	0.0367
	(0.0390)	(0.0432)	(0.0385)	(0.0586)	(0.0480)	(0.0654)	0.0311)

Notes: See Notes to Table 5. A unit of observation is a 1990-borders district * forest zone. Robust standard errors clustered at 1990 district borders in parentheses. *Number of districts in original district boundaries* variable counts the number of districts the district split into and the *Number of districts elsewhere in province* variable counts the number of districts all other districts within the same province split into. The regression also includes district-by-forest zone and island-by-year fixed effects. *** 0.01, ** 0.05, * 0.1

Table 7: District level analysis: new vs. old part of the district

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	All Forest	Production/Conversion	Conservation/Protection	Conversion	Production	Conservation	Protection
Panel A							
Number of districts in	-0.0527	-0.0707	0.0594	0.0336	-0.0730	0.124**	-0.0209
original district boundaries	(0.0774)	(0.0993)	(0.0520)	(0.160)	(0.107)	(0.0591)	(0.100)
Number of districts	-0.0383	-0.0508	-0.00116	-0.0190	-0.0740	0.00417	-0.125
in orig. district boundaries	(0.0836)	(0.0846)	(0.0855)	(0.118)	(0.107)	(0.0469)	(0.0802)
× has original capital in 2008							
Observations	5488	2512	2816	896	1568	1072	1616
Panel B: Lags							
Number of districts in	-0.00514	-0.0192	0.113	0.0654	-0.0229	0.182**	0.0305
original district boundaries	(0.0943)	(0.128)	(0.0690)	(0.178)	(0.127)	(0.0843)	(0.0526)
Lag 1	0.106	0.106	0.0600	0.371	0.0430	0.0526	0.104
	(0.150)	(0.176)	(0.126)	(0.252)	(0.142)	(0.126)	(0.0830)
Lag 2	-0.285***	-0.366***	-0.105*	-0.777***	-0.268**	-0.0312	-0.204**
	(0.110)	(0.131)	(0.0617)	(0.244)	(0.120)	(0.0681)	(0.102)
Lag 3	0.207***	0.260***	0.101	0.334**	0.211**	0.0405	0.191
	(0.0671)	(0.0741)	(0.0849)	(0.144)	(0.0850)	(0.119)	(0.129)
Number of districts	-0.167*	-0.177	-0.138***	-0.134	-0.201	-0.189**	-0.169***
× has original capital in 2008	(0.0879)	(0.127)	(0.0440)	(0.101)	(0.160)	(0.0932)	(0.0546)
Lag 1	-0.0160	-0.00289	-0.168*	-0.0485	-0.00209	-0.122	-0.154
	(0.126)	(0.160)	(0.0885)	(0.188)	(0.169)	(0.105)	(0.124)
Lag 2	0.357***	0.416***	0.313***	0.446***	0.412***	0.340***	0.0818
	(0.0744)	(0.0987)	(0.0940)	(0.0879)	(0.141)	(0.113)	(0.0859)
Lag 3	-0.107	-0.202*	0.178*	-0.217**	-0.196*	0.180	0.251**
	(0.0871)	(0.104)	(0.0971)	(0.102)	(0.115)	(0.141)	(0.127)
Observations	5488	2512	2816	896	1568	1072	1616
Joint p original	0.000419	0.000183	0.0882	<0.001	0.0683	0.137	0.0856
Sum of lags original	0.0219	-0.0188	0.168*	-0.00702	-0.0372	0.244	0.122
	(0.114)	(0.121)	(0.0910)	(0.229)	(0.121)	(0.149)	(0.0910)
Joint p interaction	0	<0.001	<0.001	<0.001	<0.001	0.0139	0.00623
Sum of lags interaction	0.0668	0.0339	0.186	0.0463	0.0127	0.210**	0.0103
	(0.121)	(0.0898)	(0.158)	(0.171)	(0.0886)	(0.105)	(0.147)

Notes: See Notes to Table 5. A unit of observation is a 2008-borders district * forest zone. Robust standard errors clustered at 1990 district borders in parentheses. *Number of districts in original district boundaries* variable counts the number of districts the original 1990 district split into as of year *t* and the *Has original capital in 2008* is a dummy for whether the capital city of the original 1990 district is located within the borders of the district in 2008. The regression also includes 2008 district-by-forest zone and island-by-year fixed effects. *** 0.01, ** 0.05, * 0.1

Table 8: Elections

VARIABLES	(1) All Forest	(2) Production/Conversion	(3) Conservation/Protection	(4) Conversion	(5) Production	(6) Conservation	(7) Protection
Panel A							
ElectionYear	-0.133	-0.0732	-0.593***	0.124	-0.128	-0.398***	-0.658***
	(0.0959)	(0.112)	(0.155)	(0.156)	(0.107)	(0.117)	(0.214)
Observations	6464	3064	3400	1112	1952	1360	2040
Panel B: Leads & Lags							
ElectionYear	0.0277	0.0804	-0.364**	0.405*	-0.00920	-0.125	-0.493***
	(0.142)	(0.155)	(0.152)	(0.241)	(0.151)	(0.187)	(0.183)
Lead 1	0.200	0.173	0.427**	0.242	0.134	0.244	0.501**
	(0.130)	(0.140)	(0.216)	(0.226)	(0.146)	(0.171)	(0.220)
Lead 2	0.131	0.120	0.294**	0.295	0.0869	0.223	0.300**
	(0.166)	(0.185)	(0.130)	(0.223)	(0.184)	(0.149)	(0.134)
Lag 1	0.282*	0.305*	0.140	0.579**	0.235	0.352	-0.111
	(0.155)	(0.170)	(0.217)	(0.236)	(0.186)	(0.282)	(0.201)
Lag 2	-0.0427	-0.0463	0.0180	0.0896	-0.0671	0.0892	-0.103
	(0.173)	(0.193)	(0.266)	(0.302)	(0.205)	(0.339)	(0.236)
Observations	6464	3064	3400	1112	1952	1360	2040
Lags Joint p	0.00305	0.00447	0.000358	1.61e-06	0.0383	0.0695	0.0257
Sum of lags	0.267	0.339	-0.206	1.074	0.158	0.315	-0.708
	(0.429)	(0.470)	(0.547)	(0.733)	(0.489)	(0.664)	(0.500)
Leads Joint p	0.291	0.458	0.0598	0.413	0.641	0.252	0.0418
Sum of leads	0.331	0.293	0.721**	0.536	0.221	0.468*	0.801**
	(0.270)	(0.295)	(0.314)	(0.418)	(0.302)	(0.283)	(0.320)

Notes: See Notes to Table 5. A unit of observation is a 2008-borders district * forest zone. Robust standard errors clustered at 1990 district borders in parentheses. *ElectionYear* variable is a dummy equal to 1 if the district holds district head election that year. The regression also includes district-by-forest zone and island-by-year fixed effects. *** 0.01, ** 0.05, * 0.1

Table 9: Substitutes or Complements?

VARIABLES	(1) All Forest	(2) Production/Conversion	(3) Conservation/Protection	(4) Conversion	(5) Production	(6) Conservation	(7) Protection
Panel A							
Oil and Gas Revenue	-0.00316**	-0.00284*	-0.00597**	-0.00912***	-0.00220	-0.00474**	-0.00986***
per capita	(0.00160)	(0.00165)	(0.00252)	(0.00165)	(0.00146)	(0.00218)	(0.00147)
Observations	6464	3064	3400	1112	1952	1360	2040
Panel B: Lags							
Oil and Gas Revenue	-0.00492***	-0.00432**	-0.0113***	-0.0115***	-0.00362**	-0.0109***	-0.0118***
per capita	(0.00186)	(0.00190)	(0.00257)	(0.00181)	(0.00174)	(0.00368)	(0.00181)
Lag 1	0.000652	8.87e-05	0.00561***	0.00423**	0.000245	0.00797***	-0.00149
	(0.00103)	(0.00126)	(0.00113)	(0.00201)	(0.00106)	(0.00147)	(0.00177)
Lag 2	0.00112	0.00132	0.000731	-0.00112	0.00166	0.00206	0.00103
	(0.00130)	(0.00151)	(0.00138)	(0.00177)	(0.00155)	(0.00144)	(0.00174)
Lag 3	0.00519***	0.00530***	0.00574	0.0119***	0.00401***	-8.39e-05	0.0140***
	(0.00163)	(0.00160)	(0.00372)	(0.00307)	(0.00150)	(0.00288)	(0.00527)
	6464	3064	3400	1112	1952	1360	2040
Joint p	1.08e-07	4.56e-08	0	0	3.39e-06	5.91e-10	0
Sum of lags	0.00205	0.00240	0.000768	0.00344	0.00230	-0.000962	0.00172
	(0.00134)	(0.00154)	(0.00195)	(0.00347)	(0.00155)	(0.00210)	(0.00448)
Sum of lags = immed. effect p-value	<0.001	<0.001	<0.001	<0.001	<0.0010	0.003	0.0171

Notes: See Notes to Table 5. *Oil and Gas Revenue per capita* variable reports the value of per capita revenue from oil and gas extraction at the district-level in US dollars. A unit of observation is a 2008-borders district * forest zone. Robust standard errors clustered at 1990 district borders in parentheses. The regression also includes district-by-forest zone and island-by-year fixed effects and the number of districts the 1990 district has split into by year *t* (and 3 lags of this variable in Panel B). *** 0.01, ** 0.05, * 0.1

Table 10: Oil before and after direct elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	All Forest	Production/Conversion	Conservation/Protection	Conversion	Production	Conservation	Protection
Panel A							
Oil and Gas Revenue	-0.00523***	-0.00457***	-0.0122***	-0.0115***	-0.00369**	-0.0124***	-0.0123***
per capita	(0.00143)	(0.00159)	(0.00174)	(0.00300)	(0.00155)	(0.00275)	(0.00178)
Post-election	0.0218	0.0240	0.0299	-0.0352	0.0552	0.277	-0.208
	(0.110)	(0.118)	(0.217)	(0.187)	(0.125)	(0.263)	(0.168)
Oil and Gas ×	0.00175*	0.00147	0.00517***	0.00253	0.00121	0.00527**	0.00325*
Post-election	(0.000989)	(0.000976)	(0.00180)	(0.00171)	(0.000923)	(0.00246)	(0.00179)
	6403	3037	3366	1099	1938	1346	2020
	0.00128	0.0161	<0.001	<0.001	0.0579	<0.001	<0.001
Oil + Oil * Post-election	-0.00348***	-0.00310**	-0.00698***	-0.00892***	-0.00248*	-0.00713***	-0.00904***
	(0.00129)	(0.00140)	(0.00134)	(0.00174)	(0.00127)	(0.00144)	(0.00137)

Notes: See Notes to Table 10. Robust standard errors clustered at 1990 district borders in parentheses. The regression also includes district-by-forest zone and island-by-year fixed effects and the number of districts the 1990 district has split into by year *t* (and 3 lags of this variable in Panel B). *** 0.01, ** 0.05, * 0.1