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THE POLITICAL ECONOMY OF DEFORESTATION IN THE TROPICS

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ABSTRACT

Tropical deforestation accounts for almost one-fifth of greenhouse gas emissions worldwide and threatens the world's most diverse ecosystems. The prevalence of illegal forest extraction in the tropics suggests that understanding the incentives of local bureaucrats and politicians who enforce forest policy may be critical to understanding tropical deforestation. We find support for this thesis using a novel satellite-based dataset that tracks annual changes in forest cover across eight years of institutional change in post-Soeharto Indonesia. Increases in the numbers of political jurisdictions are associated with increased deforestation and with lower prices in local wood markets, consistent with a model of Cournot competition between jurisdictions. Illegal logging increases dramatically in the years leading up to local elections, suggesting the presence of "political logging cycles". And, illegal logging and rents from unevenly distributed oil and gas revenues are short run substitutes, but this effect dissapears over time as political turnover occurs. The results illustrate how incentives faced by local government officials affect deforestation, and provide an example of how standard economic theories can explain illegal behavior.

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1 Introduction

Viewed from space two great bands of green – the equatorial, tropical forests and northern, temperate and boreal forests – encircle the globe. Deforestation has been extremely rapid in tropical forests relative to their northern counterparts. One reason for this is the greater prevalence of illegal extraction which often negates or overturns attempts to sustain forest cover in tropical areas. Understanding why illegal extraction is often sanctioned or facilitated is therefore likely to be central to countering tropical deforestation.

The current importance attached to understanding the determinants tropical deforestation stems from a growing realization that the disappearance of these forests will have impacts that extend beyond national boundaries. Globally, deforestation accounts for almost 20 percent of annual emissions of greenhouse gases, with the bulk of this coming from tropical forests. To put this in perspective, deforestation contributes more greenhouse gas emissions than the global transportation sector, and roughly the same amount of emissions as the entire United States. Tropical forests are also the most biodiverse environments on the planet and their disappearance brings with it with a mass extinction of species which deprives future generations of the value associated with this genetic diversity.[1] These dual concerns of climate change and biodiversity have served to put tropical deforestation, and particularly understanding how to counter illegal extraction, towards the top of the current global policy agenda (Hansen and DeFries 2004; Stern 2006; Nabuurs et al. 2007; IPCC 2007; Kindermann et al. 2008).

The vast majority of tropical forests are owned and managed by national governments, which in turn rely on local bureaucrats and politicians to enforce national logging rules. Central monitoring of these local officials is imperfect, and these officials can (and do) allow deforestation above and beyond the amount officially sanctioned by the central government. As a result, it is not uncommon in tropical areas for the majority of the wood extracted to involve some illegal action.[2]

This paper uses Indonesian data to examine three forces which may affect the decision by local bureaucrats and politicians to allow more or less logging in their jurisdictions: the number of administrative jurisdictions in the area, which may affect the degree of market power each jurisdiction has in the local wood market; the timing of local elections, which may affect local officials' effective discount rates; and the availability of alternative sources of rents for local officials, which may affect the costs local officials face from being caught in illegal activity.

Indonesia is, in many ways, an ideal context for such a study. It contains one of the largest stands of tropical forest in the world.[3] Rapid deforestation places it just behind the

[1] Despite covering only 7% of the earth's surface these forests are home to more than half of known plant and animal species (Urquhart 2001).

[2] In Indonesia, for example, up to 60 to 80 percent wood yield may involve some illegal action – much of which may be condoned in some form by these local officials (CIFOR 2004).

[3] The other big stands of tropical forest in the world are in the Amazon Basin (mainly Brazil) and the Congo Basin (mainly Demoratic Republic of Congo).

US and China as the third largest producer of greenhouse gases worldwide.[4] And the unique features of post-Soeharto institutions and institutional change generate plausibly exogenous variation in all three of the incentive channels mentioned above.

Since so much deforestation in Indonesia is a result of illegal logging, we cannot rely on official production statistics to capture deforestation. Instead, we use a novel dataset that we constructed from MODIS satellite imagery which allows us to capture deforestation across the entire country. Using these data, we can detect deforestation at a 250 meter by 250 meter resolution annually for all of Indonesia from 2001 to 2008 (Hansen et al. 2009). We combine the pixel-level data on deforestation from our MODIS data with GIS data on district boundaries and land-use classifications to construct a dataset that captures deforestation across localities and across four land use zones – the production and conversion zones where some amount of logging is legal (for specific amounts within specific concessions), and the conservation and protection zones (where all logging is strictly illegal).

To test the impact of the number of political jurisdictions on deforestation, we take advantage of the fact that Indonesia has experienced a remarkable increase in the number of divisions over the past decade. Between 1998 and 2008, the number of districts in the main forest islands of Indonesia more than doubled, from 146 districts in 1998 to 311 districts in 2008. Exploiting the differential timing of these district splits, we estimate that subdividing a province by adding one more district increases the overall deforestation rate in that province by 7.8 percent. The increase appears in both land use zones where logging can be either legal or illegal, as well as in the land use zones where all logging is illegal.

While there are multiple reasons why subdividing administrative jurisdictions could increase deforestation, the evidence appears consistent with a model in which Indonesian district governments engage in Cournot-style competition in determining how much wood to extract from their forests. We show that the increase in administrative jurisdictions drives down prices in the local wood market: adding one more district to a province reduces local prices in the province by 3.3 percent, implying a local demand elasticity for logs of about 2.3. A back-of-the-envelope calculation suggests that the magnitude of the increase in deforestation we observe is consistent with what a simple, static Cournot model would predict given this elasticity.

To test for the presence of political cycles, we exploit the fact that, starting in 2005, district heads began to be chosen through direct popular elections rather than being indirectly selected by the local legislature. These direct elections were staggered, with the timing determined by when the previous district head's term came to an end, which in turn was determined by the timing of district head appointments under Soeharto (Skoufias et al. 2010). Using these asynchronous local elections, we document a "political logging cycle" where logging increases in the years leading up to local elections. Specifically, deforestation in the land-use zones where all logging is illegal increases by as much as 42 percent in the year prior to an election. The pattern is consistent with the idea that local officials' effective discount rate increases in the years leading up to an election, either because of an intense

[4]This prompted Norway to sign in 2010 an agreement with Indonesia for reducing emissions from deforestation and forest degradation (REDD) worth US$ 1 billion.

need for campaign funds or votes or because their time in power may soon be coming to an end.

To test for substitution between illegal logging and other sources of rent extraction, we exploit changes in a district's oil and gas revenue-sharing receipts over time. Oil and gas reserves are highly unevenly distributed across Indonesia, and the revenue sharing rules put in place by post-Soeharto governments mean that the amount of revenue a district receives in a given year depends on oil and gas prices, production in own and surrounding districts, and the number of districts in the province. Consistent with the existing literature on short-run substitution between alternate forms of corruption (Olken 2007, Niehaus and Sukhtankar 2009), we find that rents from illegal logging and the potential for rents from oil and gas revenue sharing are substitutes in the short-run. In the medium term, however, we show that over half of this effect disappears. We provide suggestive evidence that the effect disappears over time because the higher oil and gas rents lead over time to the formation of new, higher rent-extraction political coalitions (as in Brollo et al. 2009).

The results in this study provide new evidence on how potentially corrupt bureaucrats and politicians respond to incentives. All three of the main results in the paper are consistent with rent maximization by local officials: as an official's market power diminishes (due to district splits), he increases the rate of rent extraction; as his discount rate increases (due to an upcoming election), he increases rent extraction; and, as alternative sources of rents increase (due to increased oil and gas revenue), so that he has more to lose from being found engaging in illegal activity in the forest sector, he decreases rent extraction. The results thus provide an example of how potentially illegal behavior can be explained by standard economic models (as in Becker and Stigler 1974, Shleifer and Vishny 1993, and Olken and Barron 2009).

The remainder of this paper is organized as follows. In the next section we discuss the background on institutional 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 of 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 changes 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 year regime of President Soeharto on May 21st, 1998. Soeharto had governed Indonesia since 1967, and 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.[5] Off-Java regions which were rich in natural resources like forests, oil and gas were particularly strident in their demands 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, with some fraction of royalties going to the producing district, some fraction being shared equally among all other districts in the same province, and the rest remaining with Jakarta. 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, as well as a high fixed fee, low per-capita fee structure in the block grant formulas, led to a significant amount of district splitting. The total number of districts increased from 292 in 1998 to 483 in 2008. 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). At the same time, they also introduced 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 district parliament

[5] 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).

and, thus, made the system susceptible to corruption and elite capture (Mietzner 2007; Erb and Sulistiyanto 2009). Consequently, in 2004 a revised decentralization law considerably increased accountability by introducing direct election of the district head. Direct elections were to be held after the previous district head selected by the previous system had served his 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.[6] 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 with 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 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 (ROI 1967; Barber and Churchill 1987; Barber 1990). This is a substantial amount of forest: by comparison, it is roughly equivalent to the U.S. states of California, Montana, and Texas put together, and is roughly double the size of the U.S. national forest system.

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, were issued for up to 30 years and required the logging companies to manage the forest sustainably through selective logging. 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 (the *bupati*), 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

[6] 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.

holders to develop, monitor, and enforce annual cutting plans.[7] 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 carried out in the designated areas only.[8]

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. All legally felled logs require a transport permit from the district forest office, which is not only checked at sawmills and export points, but also verified at regular road checkpoints and at occasional roadblocks. Extracting more than the legal quota from a concession, transporting it, or bringing illegally sourced logs into a mill, therefore requires the complicity of the district forest office. The district forest office are also supposed to conduct regular spot-checks in the forest to ensure that the trees that were felled match those specified in the annual cutting plan, and that no additional trees are felled.

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.[9]

Anecdotal evidence confirms that district governments play an important role in facilitating illegal logging in a variety of ways. For example, district heads have been found to allow logging to take place outside official concessions (Barr et al. 2006), to facilitate the creation of new oil palm plantations inside national forest areas, and to sanction the transport and processing of illegally harvested logs (Casson 2001a). District officials also have been known

[7] 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.

[8] 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 Appendix Tables 13 - 15 in the online appendix for these results.

[9] Local police can also play an important role, since they can also instigate enforcement actions for illegal logging (or threaten to do so).

to issue conversion permits to clear cut forest and plant oil palm on their own, even though they do not have the legal authority to do so (CIFOR 2011). Estimates suggest that illegal logging makes up as much as 60-80% of total logging in Indonesia, making illegal logging a roughly US $1 billion a year market, suggesting that these forces play a substantial role in determining the total amount of deforestation (CIFOR 2004).

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 sensor to construct an annual measure of forest change for each year from 2001-2008. The resulting dataset traces, at a spatial resolution of 250 meters by 250 meters, the patterns of forest clearing 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 region year round. This makes it difficult to use high-spatial resolution sensors, like Landsat, which have been used to measure annual forest cover change in less cloudy environments (INPE 2002). Since these satellites typically only revisit the same area once every 1-2 weeks, cloud-free images are less frequently recorded in Indonesia. An alternative to this is to draw on moderate spatial 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 cloud-free observations, but at a coarser spatial resolution of 250 meters by 250 meters instead of the 30 meter spatial resolution available via Landsat.

To generate the data used in this paper, MODIS thirty-two day composites were used as inputs and included data from the MODIS land bands (blue (459–479 nm), green (545–565 nm), red (620–670 nm), near-infrared (841–876 nm), and mid-infrared (1230–1250, 1628–1652, 2105–2155 nm)) (Vermote et al. 2002), as well as data from the MODIS land surface temperature product (Wan et al. 2002). Composite imagery represent the best land observation over the compositing period, in this case 32 days. To produce a more generalized annual feature space that enabled the extension of spectral signatures to regional and interannual scales, the 32 day composites were transformed to multitemporal annual metrics. Annual metrics capture the salient features of vegetation growth and senescence without reference to specific time of year and have been shown to perform as well or better than time-sequential composites in mapping large areas (Hansen et al. 2003).

For each annual interval, a total of 438 image inputs were used (146 metrics per year plus their calculated differences) (Hansen et al. 2005). This amount of information, in effect 438 dimensions for each 250 meter by 250 meter pixel, is used to quantify forest cover loss per year for that pixel. By contrast, the human eye, with its three types of cones, measures only three bands, which correspond roughly to the blue, green, and red areas of the visual spectrum. The MODIS-derived data set is thus considerably richer than just a series of visual images at comparable resolution. The next step is to take the composited MODIS inputs and implement a computer algorithm to discriminate between forest and non-forest. The key idea of remote sensing is developing an algorithm that identifies what spectral signatures or set of signatures – i.e., what combinations of MODIS-derived spectral and temporal information – best discriminate forest cover and its loss. For example, plants absorb electromagnetic radiation in the visual red part of the electromagnetic spectrum, but reflect or scatter radiation in the near-infrared part. One common metric for measuring vegetation productivity is the NDVI (normalized difference vegetation index), which captures the difference in reflectance of the near-infrared and red parts of the electromagnetic spectrum, and is a useful spectral signature for indicating the presence or absence of vegetation (Tucker 1979). Foster and Rosenzweig's pioneering work relating forest cover to economic factors in India, for example, used satellite-based NDVI measures to detect forest change (Foster and Rosenzweig 2003).

In practice, one can do much better than using NDVI by exploiting additional dimensions of the data. For example, forests tend to be cooler than surrounding areas, so bands that measure temperature can also be used. Moreover, trees have different spectral signatures than other types of crops and plants (Jensen 1995). To take maximal advantage of the richness of the MODIS data, we use a statistical learning procedure known as a decision tree bagging algorithm to determine which spectral signatures best correspond to forest (Breiman 1996).

Specifically, we start with much higher resolution training images. For each of these images (consisting of best available Landsat data), experts classify each pixel as having experienced forest cover loss (clearing) or not. We then relate these labels to corresponding MODIS data using the decision tree algorithm. The decision tree algorithm is a non-linear, hierarchical tool for recursively partitioning a data set into less and less varying subsets regarding the variable of interest, in this case forest cover loss. The method makes no assumptions on the distribution of the data in spectral space, allowing for the robust and precise division of the spectral data into estimates of forest cover loss using a series of nested partitioning rules. One then extrapolates the derived rule set over the entire MODIS dataset to predict, for each year, a per pixel probability of forest cover loss. We code a pixel as cleared if the estimated probability of deforestation exceeds 90%.

The final outputs 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 250 meter by 250 meter 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 will under-estimate total logging. Identified change is 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.

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, to obtain data on prices we also examine 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 and value of logs cut at the province level and the associated price by wood type, for 114 different types of wood.[10] 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. We divide value by quantity to obtain 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.

[10] We drop the 'other' (*Lainnya)* and 'mixed wood' (*Rimba Campuran)* category, since their composition varies considerably across provinces and 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 250 meters by 250 meters). 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)), 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 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.[11] 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.

[11] 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.

We use the official date that the national parliament approved the formation of a new district to code the number of districts present at a given area at t. For the province-level data, we simply calculate the total number districts and municipalities in province p on island i in year t, $NumDistrictsInProv_{pit}$.[12] 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 into in 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 political election cycles, we obtain district-level election schedules obtained from the Centre for Electoral Reform (CETRO)[13], 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, $PCOilandGas_{dt}$.[14] We obtain the revenue data from the Indonesian Ministry of Finance webpage (`http://www.djpk.depkeu.go.id/datadjpk/57/`) and the population data for 2008 from 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 4 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 Increases in Political Jurisdictions

In this section, we consider the implications of subdividing political jurisdictions for deforestation. As discussed above, across all of Indonesia, the number of districts increased from

[12]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.

[13]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.

[14]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 114.5 billion rupiah, whereas the corresponding figure for forestry was 5.3 billion rupiah.

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 113%. We exploit the staggered timing of these changes in administrative boundaries to identify the relationship between the number of administrative units and deforestation.

What would theory predict about the impact of subdividing a political jurisdiction? 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, 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.[15] Lewis and Schmalensee (1980) show that many of the standard, static Cournot results generalize to this dynamic 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 prediction empirically below, consider whether the magnitudes appear consistent with what one would expect from a Cournot model, and consider several alternative explanations for the results, such as changes in enforcement at the time districts split.

3.1 Empirical Specifications

To examine the impact of the number of political jurisdictions, we examine how deforestation responds when a district is subdivided to create new administrative jurisdictions. In doing so, a key question is what determines the timing of these district splits. 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 sector.[17] Since all analysis

[15] 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.

[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.

[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-

in this paper is identified from the timing of the splits, not whether they occur, however, the key question from the perspective of this paper is not whether a district splits, but rather what determines the timing of the split.

Several idiosyncratic factors appear to influence the timing of the splits. 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 takes 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 show empirically that the timing of these splits is not are associated with pre-trends in deforestation, though *a priori* there is little reason to believe they would be. In Appendix Table 1, we also show that the year a district split is uncorrelated with factors such as population, area, oil and gas revenues, share of land that is forested, or the pre-period rate of deforestation.

To test the Cournot 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 by water. Province boundaries are also coincident with mountain ranges which make transporting logs across provinces by road generally more difficult than transporting logs by road within provinces. Provincial boundaries are also the smallest level at which our price data is available.

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

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. They 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 in 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.

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 elasticities, we will examine the official forestry statistics.

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 by province to account for arbitrarily serial correlation over time within provinces. 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. Including island×year fixed effects allows for flexible time trends in deforestation across different parts of the country over time.[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 province boundaries.[20] We estimate this equation separately by land use zones.

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

[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] Note that province borders changed over our sample period. In 1990 (i.e., under Soeharto), there were 17 provinces in our sample area; in 2001, at the start of our data, there were 19 provinces in our sample area, and in 2008, at the end of our data, there were 21 provinces in our sample area. Districts are not split across province lines. Since the finer provinces correspond more naturally to geographic units (e.g., West Sulawesi; West Papua), in our main specifications we use the finer 21-province definitions for the analysis, but cluster standard errors at the original 17-province level. If we use the 17-province level 1990-era borders for the analysis instead, the estimates with no lags attenuate, but the estimates with lags remain virtually unchanged. See Appendix Tables 2-4.

weighted by the volume of production of wood type w in province p in the first year that we have data, so the coefficient is approximately interpretable as the effect on average prices in the province. 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.[21]

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:

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

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 clustered at the 1990 district boundaries. The conditional log-likelihood function is again estimated separately by land use zones.

3.2 Impacts on Quantities

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 pooled 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).[22] 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 3.85% 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: 5.35%, statistically significant at 1%; conversion: 3.87%, not statistically significant) and in one of the zones where deforestation is clearly illegal (conservation: 9.76%, statistically significant at 5%). 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.

[21] 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 in Appendix Table 6 that using contemporaneous weights when estimating equation (2) produces virutally identical results.

[22] 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.

Panel B reports the estimates of the medium-run impact of district splits by including 3 lags of the $NumDistrictsInProv_{pit}$ variable.[23] 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 increases deforestation in the entire 'Forest Estate' by 8.22%. 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 – 8.09% on average for the production and conversion zones (where logging could be legal or illegal) and 10.1% 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 a 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 no 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 highly statistically significant in all specifications). 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.

3.3 Impacts on Prices

If the 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 reports results from estimating 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.[24]

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

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

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 8.4%, though the impact on prices is not statistically significant. Panel B estimates the medium-run impact of the number of districts on prices and quantities by including 3 lags of the $NumDistrictsInProv_{pit}$ variable.[25] 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 3.4% and quantities increased by 13.5%, and the impact on prices is now statistically significant at the 5% level. Similar results are obtained for the alternative samples shown in Columns 3 through 6, and the price effect becomes statistically significant in both Panel A and B when we use the entire sample.

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 3.85%, we obtain a demand elasticity of -2.27. Alternatively, using the medium-run estimates – the increase in quantities of 8.22% from Panel B of Table 3 and the increase in prices of 3.4% from Panel B of Table 5 – we obtain an estimated medium run elasticity of -2.41 – almost exactly the same as the short-run elasticity estimate of -2.27. Given that the downward sloping demand curve within a province is determined by transportation costs across provincial boundaries, we would expect that demand for forest products should be reasonably elastic, consistent with the high elasticities we find in the data.

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 (See Appendix Tables

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

5 and 6). A falsification test where we include only *kotamadya* shows no impact in most specifications, though the results are very noisy given the small number of cities. Finally, we have repeated analysis of leads of district splits in from Table 4 above for the official data in Appendix Table 6. 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 one of the alternate specifications (Column 5 of Appendix Table 6), in the main time period and specification we examine – 2001 through 2007 – we find no evidence of significant pre-period differential trends.

3.4 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 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 0.0385 in the short run and 0.0822 in the medium run. The estimated price elasticity of demand was around 2.3 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 a continuum of logging firms in each district d, each of whom can extract logs at marginal cost c. To extract logs, each firms needs to secure a permit from the district government, at cost b per unit extracted.[26] Suppose the inverse demand function is $P(Q)$ where Q is the total quantity of wood produced in the province. Each firm f in district d solves

$$max_{q_{fd}} p(Q)\, q_{fd} - cq_{fd} - bq_{fd}.$$

Firms are thus willing to pay bribes up to $b = p(Q) - c$ to obtain logging permits.

We assume that each district government determines the quantity of permits to issue in its district, and then sells the permits to firms. Each district d solves

$$max_{q_d} b(q_d)\, q_d$$

Substituting yields the familiar Cournot equation

$$max_{q_d} q_d p\left(\sum q\right) - cq_d \tag{4}$$

[26]Since the cost structure for firms is constant across firms and linear in quantities, the optimal price structure for bribes that districts will set will be linear in equantities as well.

The first order condition is

$$q_d p' + p - c = 0 \tag{5}$$

Suppose there are n identical districts in the province, so that total quantity $Q = nq_d$. Rewriting and substituting $Q = nq_d$ yields the familiar Cournot equation:

$$\frac{(p-c)}{p} = \frac{1}{n\varepsilon} \tag{6}$$

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 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 (5), taking derivatives, and simplifying yields:

$$\frac{1}{Q}\frac{dQ}{dn} = \frac{1}{n^2 - n\lambda} \tag{7}$$

Are the empirical estimates broadly with equations (6) and (7)? In the beginning of our period (2001), we have 116 districts in 21 provinces who are producing logs, so on average we have $n = 5.2$. Substituting the empirical elasticity estimates and the number of districts into equation (7) suggests that the semi-elasticity of quantity with respect to the number of districts ($\frac{1}{Q}\frac{dQ}{dn}$) should be approximately 0.034. Empirically, we estimate using the satellite data that $\frac{1}{Q}\frac{dQ}{dn}$ is 0.038 in the short run and 0.082 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 (6) – is necessarily more speculative, since we do not observe the markup directly. Substituting our estimates into equation (6) suggests that the markup ($\frac{(p-c)}{p}$) should be around 0.08.

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_d. 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_d illegal log transport permits to these small firms in return for bribes. In equilibrium, as in the simple model, 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 (6) would suggest. Based on fieldwork in Kalimantan, Casson and Obidzinski (2002) estimate that in one district the bribe to receive an illegal wood transport permit is \$22/m^3 of wood. They also note that district officials only require sawmills to purchase these illegal permits for 20% of the wood

[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 (4).

they process, so the effective bribe required is about $\$4/\text{m}^3$. Since wood prices vary from $\$120$ to $\$250/\text{m}^3$, 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 that they study, the district government levies official "fees" on illegal timber of about $\$20/\text{m}^3$, 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, they suggest that the bribes collected are quite small as a share of the total value of the woods, and are on the same rough order of magnitude as the 0.08 range predicted by the theory.

3.5 Direct versus Indirect Effects of District Splits

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 separately estimate the direct effect of a district splitting – i.e., the impact in the district that splits itself – from the indirect effect of the district splitting – i.e., the impact on logging on other districts in the same province.

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 – the impact of a split on 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 20.4%. On the other hand, there appears to be an increase in logging in illegal zones – deforestation in the conservation zone (i.e., national parks) increases by 14.1% – 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 25.5% (Panel B, Column 3, sum of lags), driven by a 37.7% increase in conservation zones (Column 6) and a 13.5% increase in protection zones (Column 7). On net, the total increase in deforestation in the district that splits after 3 years (shown in Column 1) is 3.6%, though this is not statistically significant.[28] In results shown in the Appendix, if we look even further

[28] Appendix Table 8 shows that the main results are robust to the inclusion of the leads, and that we do

out and include 5 lags in the model, the total increase in the deforestation in the district that splits after 5 years is 8.9%, though once again this is not statistically significant (see Appendix Table 7).

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 occurs in both legal and illegal logging zones. The impact effect of a district splitting is to increase overall logging by 6.8% in all other districts in the province (Panel A, Column 1) and the medium-run impact is 9.5% (Panel B, Column 1, sum of lags).

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, as (for example) forest officials are reassigned. Other districts within the same province increase logging immediately. This may reflect a combination of two forces: other districts increasing the quantity of illegal logging in response to the lower extraction from the district that split and other districts further increasing extraction as they anticipate that prices will fall once the new districts are fully established and begin to log more. Of these, the first is an example of static Cournot effects and the second is an example of dynamic Cournot effects with a non-exhaustible resource as in Lewis and Schmalensee (1980). Both forces may be in action at a given time.

For the conservation and protection zones, where we know all logging is illegal, the impacts 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.

3.6 Alternative Explanations

The results in this section suggest that having more political jurisdictions is associated with an increased rate of deforestation and lower prices in wood markets. Although we have focused on Cournot competition between districts as one plausible interpretation of these findings, there are several alternative explanations as well. This section considers several of these alternative explanations.

not find a significant sum of leads for the $NumOtherDistricts_{dit}$ variable. In almost all specifications in Appendix Table 8, 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 is the sum of the leads for own splits in the conservation zone (Column 6), but given that we find significance at the 10 percent level in only 3 out of the 28 lead tests we consider it is likely that these are just noise, rather than true differential trends.

3.6.1 Enforcement

One possible alternative explanation is that the creation of a new district could result temporarily in a decline in enforcement capacity as a new district government sets up its own district forest office. There are, however, several pieces of evidence against this idea that a decline in enforcement is responsible for the increase in deforestation associated with the creation of new districts. First, if enforcement was the issue, we would expect that there would be a large increase in deforestation initially, with declines over time as the new districts established themselves. Instead we see the opposite pattern: an increase in deforestation initially that intensifies over time.

Second, we can test 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. In results shown in the Appendix, we show that there is little differential impacts between the new and old parts of the district, and if anything, there is a stronger effect in the old part of the district (see Appendix Table 9). Combined, these results suggest that a decline in enforcement due to the creation of a new district is unlikely to be driving the results.

3.6.2 Changes in the Assignment of Central Logging Quotas

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, so that the province as a whole receives a higher legal cut quota.

While this explanation could explain changes in the production and conversion zones, for illegal logging, however, this negotiation force should not be present. As shown in Table 3 above, we find increases in the rate of deforestation of approximately equal magnitude in the land use zones where logging should be legal or illegal (production and conversion) and the zone where no logging should take place (conservation and protection). Moreover, in production zones, legal logging is the selective felling of individual trees, not the type of clearing of 250 meter by 250 meter pixels that should appear in our MODIS satellite data. While these reallocations of legal logging quotas may be taking place, they do not seem to be the main driver of these results.

3.7 Discussion

On net, the results in this section suggest that increasing the number of districts increases the rate of deforestation, as would be predicted by a Cournot-style model of competition between districts. Although we can not rule out all possible stories, several points of evidence provide suggestive evidence in favor of the Cournot-type 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.

These findings also speak to a recent literature that has suggested that decentralized management of forests at the community level may lead to less deforestation (Somanathan et al. 2009, Baland et al. 2010). The results here results provide a counter-example to this idea, and suggest that, where districts may be large enough to have some market power in wood markets, and where district officials can obtain rents from allowing illegal logging but not necessarily from preserving forests for future generations, subdividing jurisdictions may lead to more deforestation.

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.[29] As documented in detail by Skoufias et al. (2010), the

[29] No direct elections for Bupati were held in 2009, as national Presidential elections were held that year.

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 Bupatis to posts during the pre-1998 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.[30]

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:

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp\left(\sum_{j=t-2}^{t+2} \beta_j Election_{dij} + \eta_{it}\right) \tag{8}$$

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 in force at any point in time, we use the finest boundaries we have (i.e. 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.[31] 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 (8) are shown in Table 7. 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

Those Bupatis whose term was ending in 2009 were extended on an interim basis and direct elections were held in 2010 instead.

[30] 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).

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

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).

There are several possible explanations for the increase in illegal logging in the years leading up to the elections. One set of explanations has to do with the elections increasing the politician's effective discount rates. For example, district heads who think they are likely to lose re-election may ramp up illegal logging in the years before leaving office, since the main penalty from being caught in illegal logging is being removed from office, which may happen anyway because of the election. Their effective discount rates might also increase since, if they lose reelection, they would forfeit the opportunity to collect bribes from the selling trees in the future, so they may wish to sell them all now.[32]

A second set of explanations has to do more directly with the campaign. For example, district officials may permit logging in return for funds to fight elections.[33] Alternatively, perhaps 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 sets of explanations 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 or funds during the election campaign. Alternatively, it could be an attempt to grab 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.

[32] To test this theory, one would ideally like to find X variables that predict the probability of an incumbent's re-election. Unfortunately, we have not found variables with enough predictive power on incumbent's re-election to do so. Interacting the election cycle variables with whether the incumbent actually won re-election produces largely inconclusive results, which would be consistent with the incumbent's re-election probability being hard to predict.

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

5 Substitutes or Complements? Logging versus Other Potential 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 classical 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 question 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.[34] 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., Brollo et al. 2009, Caselli and Michaels 2009).

A key distinction between our context and the existing literature is that while the existing

[34] 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, East Kalimantan, for example, the national Anti-Corruption Commission recently documented that in 2001 the Bupati 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 simply deposited US$600,000 in oil and gas revenue sharing funds into his personal account rather than the district treasury (KoranTempo 2006).

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:

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp\left(\beta PCOilandGas_{dit} + \gamma Numdistricts_{dit} + \eta_{it}\right) \tag{9}$$

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 (9), we count a district as having split only when it reports receiving its own oil and gas revenue.[35] 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 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 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 (9) as above, but include 3 lags of $PCOilandGas_{dit}$.[36]

5.2 Results

The results from estimating equation (9) are shown in Table 8. 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;

[35] As described above, *de facto* establishment of a district takes 1-3 years 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.

[36] 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 12 in the online appendix).

so a one-standard deviation change in $PCOilandGas_{dit}$ decreases deforestation by 7.1% in the production/conversion zones and by 14.2% in the conservation/protection zones.[37]

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 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.

$$\mathbf{E}\left(deforest_{dit}\right) = \mu_{di} \exp \begin{pmatrix} \beta PCOilandGas_{dit} + \delta PostElection_{dit} \\ +\pi PCOilandGas \times PostElection_{dit} + \gamma Numdistricts_{dit} + \eta_{it} \end{pmatrix} \tag{10}$$

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 (9).

The results are presented in Table 9. 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 Appendix Table 10, 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

[37] 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.

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 11). Together, these results, as well as the results in Tables 8 and 9, 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. The idea that oil rents affects outcomes by affecting who is in office echoes recent findings from Brazil (Brollo et al. 2009).

6 Conclusions

The world's tropical forests are rapidly disappearing and climate change and biodiversity concerns have made countering tropical deforestation a key global policy challenge. In common with other natural resources that fall under national ownership, command and control systems for forests in tropical countries are typified by weak governance. Monitoring of local bureaucrats and politicians who *de facto* control forest extraction, including that which is not officially sanctioned, is often imperfect. In these situations the decision to extract or conserve forests may be affected by the return these officials face in timber markets, by their short-term electoral needs, and by the availability of potential alternative sources of rent extraction.

Where these incentives do not line up with national forestry policy illegal extraction can become widespread and actual extraction can exceed planned extraction. By combining detailed satellite imagery with data on competition between jurisdictions, elections and local resource rents we have shown that local political economy factors are critical to understanding the pattern of tropical deforestation in Indonesia, home to some of the largest tropical forest reserves in the world. We find that increases in the numbers of political jurisdictions are associated with increased deforestation. Illegal logging increases dramatically in the years leading up to local elections. And having access to rents from local oil and gas reserves dampens incentives to engage in illegal logging in the short but not the medium term.

The results in this paper suggest that, to the extent that policy makers seek to encourage conservation in countries like Indonesia, Brazil and the Democratic Republic of Congo – which contain the last great stands of tropical forest – central government policies though necessary may not be sufficient. Therefore the raft of measures under the REDD – Reducing Emissions from Deforestation and Forest Degradation – banner, which are now a central plank in efforts to combat global climate change and biodiversity loss, may not work unless they also take on board the decisions of local bureaucrats and politicians. Similarly the broader class of Payment for Environmental Services (PES) schemes (see Ferraro 2002; Jack et al. 2008; Wunder 2008; Bond 2009) will need to beyond the formal owners of the forest resource (e.g. central government) and consider how to properly incentivize local officials who are currently enjoying rents from the removal of this resource.

Illegal extraction is often thought to be a core problem for natural resources management in the tropics. These issues apply not just to forests, but also to fisheries and conservation efforts directed at particular species of animals or plants. The combination of standard economic theories with innovative means of monitoring illegal extraction can offer powerful insights into what drives this behavior.

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

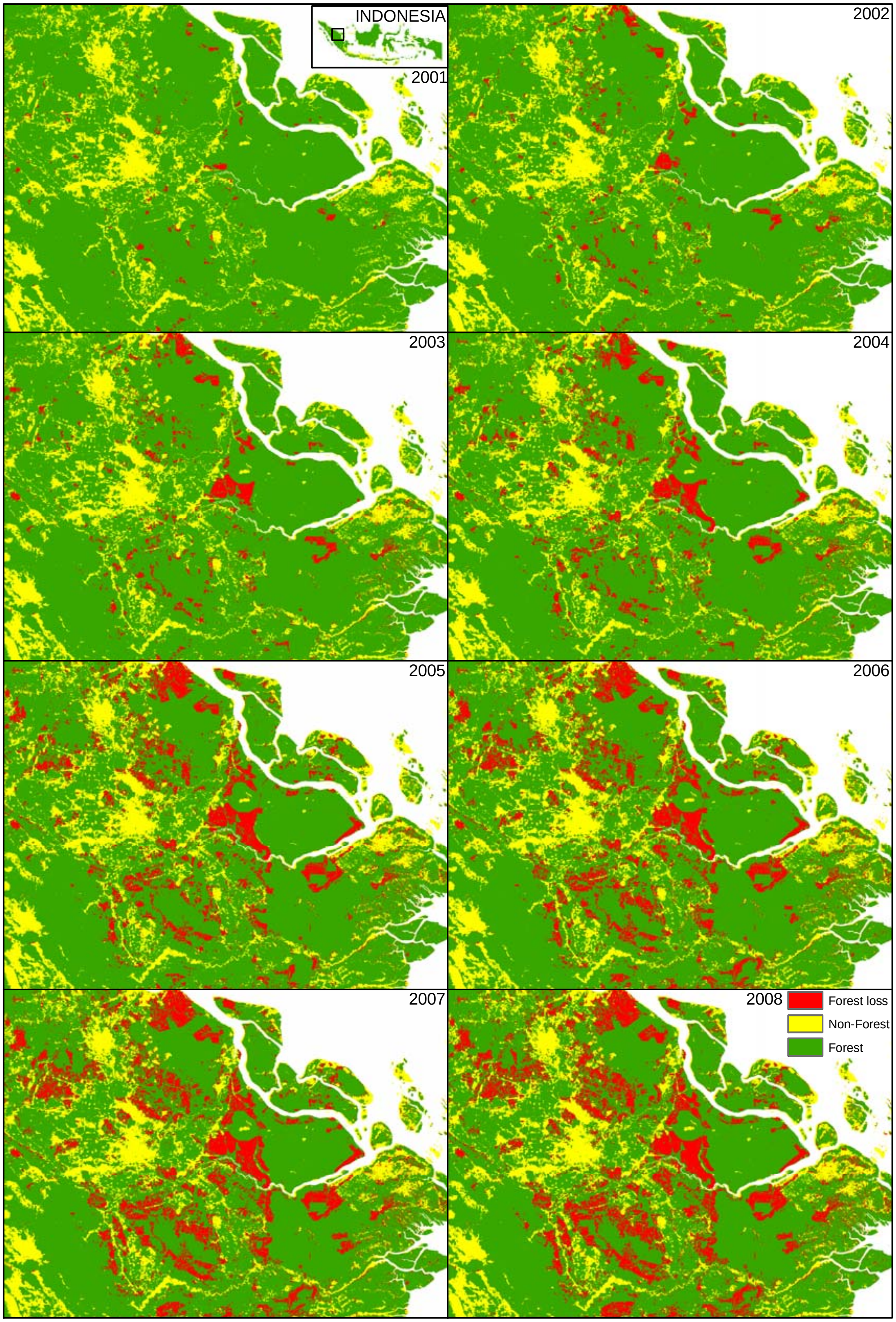

Figure 2: District-level logging in Indonesia using the 2008 district boundaries, 2001 and 2008

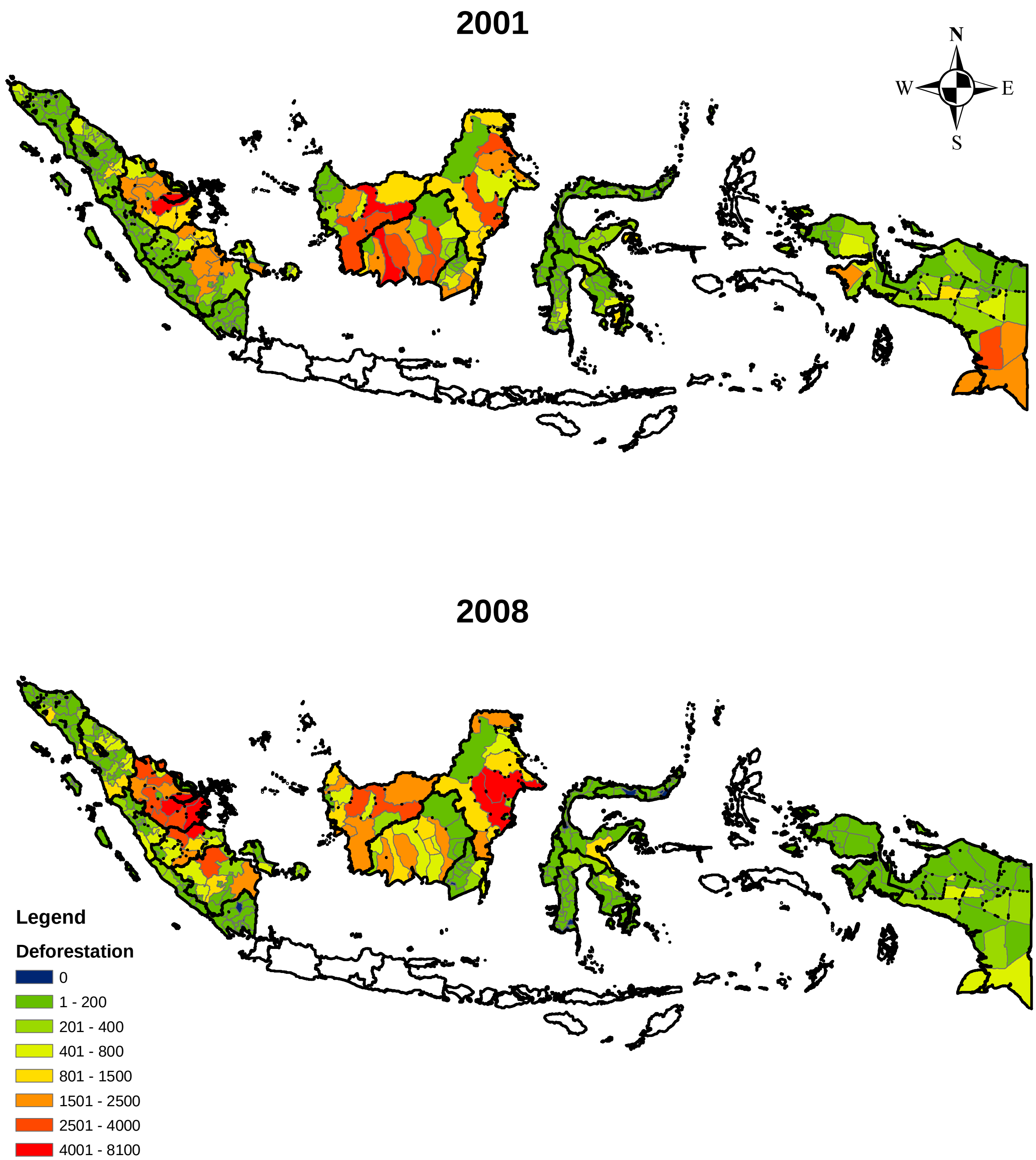

Figure 3: Total number of district splits using the 1990 district boundaries

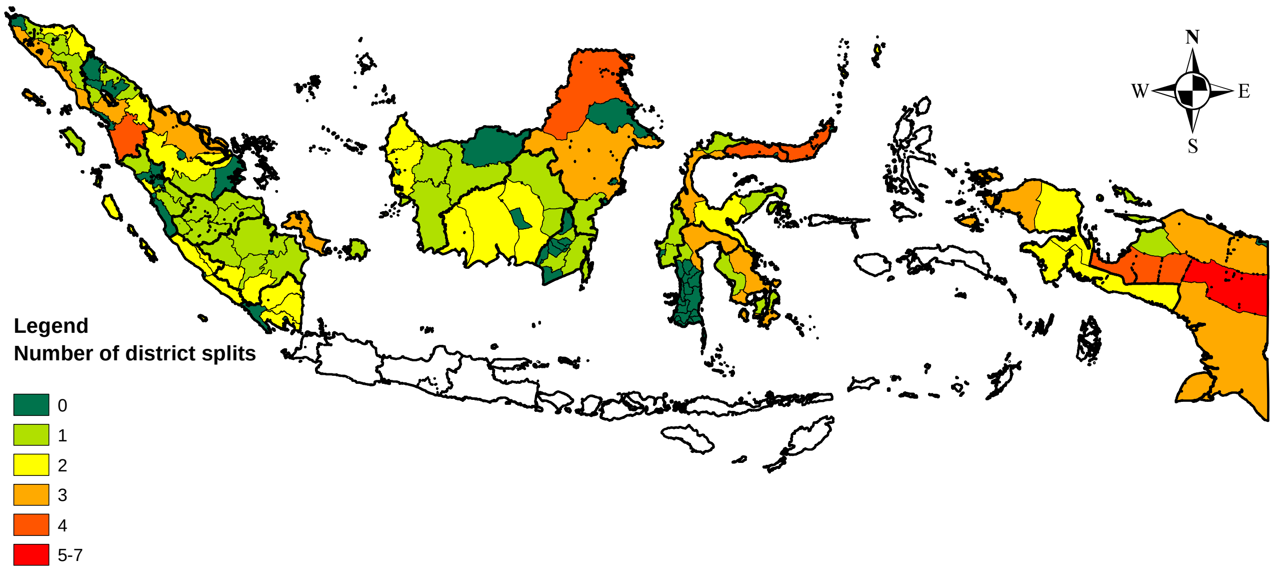

Figure 4: Oil and gas revenue per capita using the 2008 district boundaries, 2008

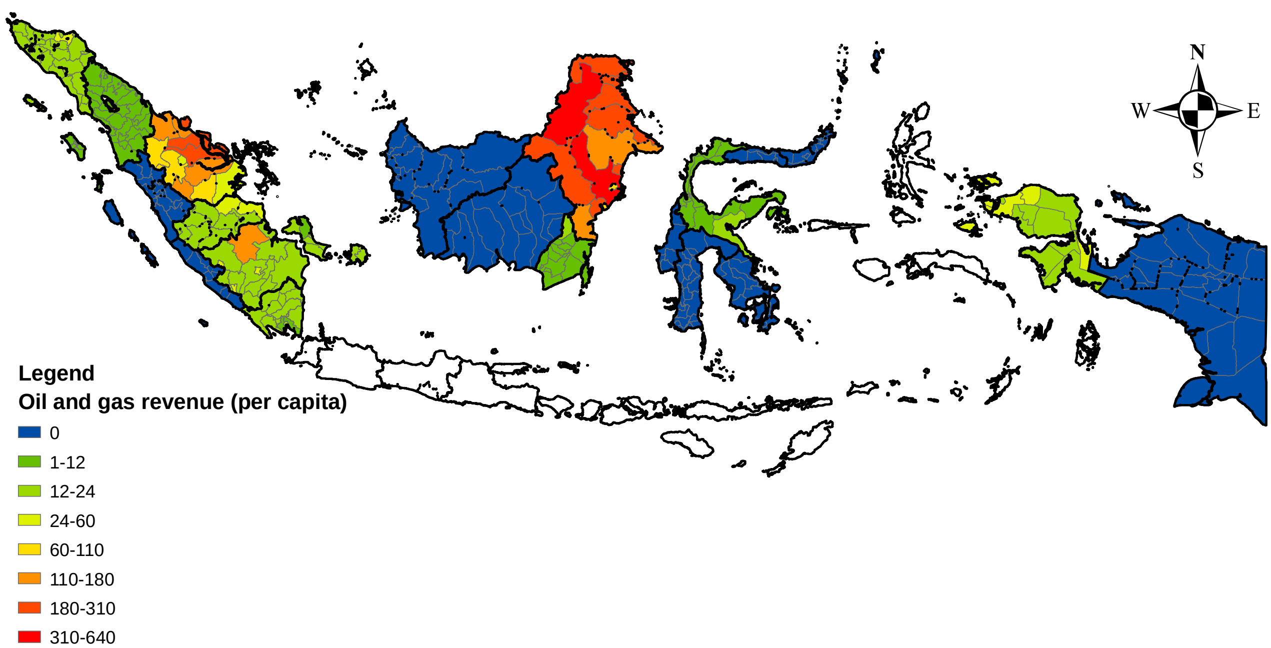

Table 1: Summary Statistics

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 pixels 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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	All Forest	Production/Conversion	Conservation/Protection	Conversion	Production	Conservation	Protection
Panel A							
Number of districts	0.0385**	0.0443**	0.0472	0.0387	0.0535***	0.0976**	0.00870
in province	(0.0160)	(0.0179)	(0.0331)	(0.0305)	(0.0199)	(0.0411)	(0.0349)
Observations	608	296	312	128	168	144	168
Panel B: Lags							
Number of districts	0.0385	0.0448	0.0900***	0.0538	0.0520	0.113***	0.0691*
in province	(0.0287)	(0.0333)	(0.0294)	(0.0398)	(0.0352)	(0.0391)	(0.0393)
Lag 1	0.0425	0.0448	-0.127*	0.0117	0.0426	-0.160	-0.0776
	(0.0459)	(0.0477)	(0.0672)	(0.0653)	(0.0448)	(0.131)	(0.0635)
Lag 2	-0.0723***	-0.0747***	0.0209	-0.0925***	-0.0624**	0.104	-0.0780
	(0.0271)	(0.0254)	(0.0808)	(0.0356)	(0.0258)	(0.157)	(0.0765)
Lag 3	0.0735*	0.0660	0.118*	0.112	0.0472	0.0949	0.138**
	(0.0435)	(0.0436)	(0.0665)	(0.0892)	(0.0387)	(0.0634)	(0.0670)
Observations	608	296	312	128	168	144	168
Joint p	<0.001	<0.001	0.0162	<0.001	<0.001	0.0205	0.0610
Sum of contemp. + lags	0.0822***	0.0809***	0.101**	0.0850	0.0795***	0.151***	0.0513
	(0.0204)	(0.0193)	(0.0426)	(0.0594)	(0.0217)	(0.0575)	(0.0373)

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	0.0390	0.0433	0.0844**	-0.0155	0.0631	0.124**	0.0173
in province	(0.0389)	(0.0455)	(0.0379)	(0.0351)	(0.0491)	(0.0550)	(0.0633)
Lag 1	0.0245	0.0205	-0.110	-0.0146	0.0171	-0.130	-0.0595
	(0.0504)	(0.0534)	(0.0738)	(0.0853)	(0.0492)	(0.112)	(0.0794)
Lag 2	-0.0574	-0.0532	0.0108	-0.0646	-0.0389	0.0651	-0.0737
	(0.0366)	(0.0347)	(0.0902)	(0.0565)	(0.0332)	(0.135)	(0.0866)
Lag 3	0.0844	0.0749	0.131	0.148	0.0578	0.141	0.132
	(0.0551)	(0.0530)	(0.0935)	(0.121)	(0.0440)	(0.0962)	(0.105)
Lead 1	0.0891	0.0930	0.0522	0.329*	0.0371	0.167	0.0444
	(0.109)	(0.115)	(0.135)	(0.170)	(0.106)	(0.137)	(0.147)
Lead 2	-0.137	-0.168	-0.0601	-0.315*	-0.152	0.0347	-0.103
	(0.149)	(0.149)	(0.187)	(0.185)	(0.145)	(0.232)	(0.207)
Lead 3	0.0527	0.0740	-0.00308	0.173	0.0708	-0.0549	0.0260
	(0.105)	(0.103)	(0.120)	(0.120)	(0.106)	(0.153)	(0.133)
Observations	456	222	234	96	126	108	126
Joint p contemp. + lags	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	0.00133
Sum of contemp. + lags	0.0904***	0.0855***	0.116*	0.0534	0.0991***	0.200**	0.0164
	(0.0279)	(0.0238)	(0.0667)	(0.0681)	(0.0224)	(0.0972)	(0.0771)
Joint p leads	0.939	0.993	0.909	0.165	0.413	0.334	0.708
Sum of leads	0.00488	-0.000459	-0.0110	0.188	-0.0439	0.147	-0.0326
	(0.0635)	(0.0555)	(0.0968)	(0.135)	(0.0537)	(0.152)	(0.0870)

Notes: See Notes to Table 3. *** 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 All wood observations		2001-2007 Balanced panel of wood observations		1994-2007 All wood observations	
VARIABLES	Log Price	Log Quantity	Log Price	Log Quantity	Log Price	Log Quantity
Panel A						
Number of districts	-0.017	0.084*	-0.019	0.103**	-0.024**	0.080***
in province	(0.012)	(0.044)	(0.013)	(0.039)	(0.010)	(0.017)
Observations	1003	1003	532	532	2355	2355
Panel B: Lags						
Number of districts	-0.025*	0.096	-0.029	0.123	-0.031***	0.072**
in province	(0.014)	(0.076)	(0.016)	(0.082)	(0.009)	(0.024)
Lag 1	0.010***	-0.039	0.009**	-0.033	0.011***	-0.004
	(0.003)	(0.034)	(0.004)	(0.041)	(0.003)	(0.034)
Lag 2	-0.001	0.040	-0.001	0.021	-0.000	0.019
	(0.009)	(0.041)	(0.010)	(0.022)	(0.005)	(0.028)
Lag 3	-0.017**	0.038	-0.018*	0.045	-0.015*	0.033
	(0.007)	(0.042)	(0.008)	(0.044)	(0.008)	(0.036)
Observations	1003	1003	532	532	1960	1960
Joint p contemp. + lags	0.000917	0.000477	0.00366	0.000724	6.74e-05	0.00890
Sum of contemp. + lags	-0.0336**	0.135**	-0.0384**	0.156**	-0.0344**	0.119***
	(0.0134)	(0.0561)	(0.0150)	(0.0592)	(0.0139)	(0.0383)

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 versus Indirect Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	All Forest	Production/Conversion	Conservation/Protection	Conversion	Production	Conservation	Protection
Panel A							
Number of districts in	-0.0984	-0.166*	0.0680	-0.0144	-0.204**	0.141*	-0.0281
original district boundaries	(0.0779)	(0.0934)	(0.0522)	(0.148)	(0.0893)	(0.0768)	(0.0845)
Number of districts	0.0680**	0.0937***	0.0363	0.0386	0.118***	0.0757*	0.0130
elsewhere in province	(0.0277)	(0.0317)	(0.0314)	(0.0493)	(0.0331)	(0.0443)	(0.0330)
Observations	3152	1488	1664	536	952	688	976
Panel B: Lags							
Number of districts in							
original district boundaries	-0.0590	-0.0921	0.111**	0.0182	-0.125	0.157*	0.0465
Lag 1	(0.0834)	(0.105)	(0.0542)	(0.153)	(0.100)	(0.0874)	(0.0586)
	-0.0185	-0.0775	-0.0766	0.207	-0.141	-0.0847	-0.0305
Lag 2	(0.130)	(0.159)	(0.104)	(0.240)	(0.142)	(0.142)	(0.0805)
	-0.0772	-0.127	0.0249	-0.436	-0.0611	0.153	-0.143
Lag 3	(0.115)	(0.151)	(0.0969)	(0.285)	(0.132)	(0.165)	(0.104)
	0.190***	0.217***	0.196**	0.157	0.241***	0.152	0.262**
Number of districts	(0.0669)	(0.0735)	(0.0795)	(0.139)	(0.0785)	(0.0939)	(0.106)
elsewhere in province	0.0676*	0.0864*	0.0919***	0.0366	0.111***	0.113***	0.0802**
Lag 1	(0.0376)	(0.0442)	(0.0318)	(0.0608)	(0.0393)	(0.0422)	(0.0374)
	0.0601	0.0819	-0.142**	-0.0298	0.0971	-0.192*	-0.0858
Lag 2	(0.0589)	(0.0646)	(0.0589)	(0.0878)	(0.0622)	(0.103)	(0.0570)
	-0.0656	-0.0543	0.0215	-0.00852	-0.0521	0.0987	-0.0550
Lag 3	(0.0479)	(0.0520)	(0.0799)	(0.0666)	(0.0543)	(0.122)	(0.0992)
	0.0328	0.0122	0.0954	0.0974	-0.0232	0.0765	0.0979
	(0.0398)	(0.0427)	(0.0599)	(0.0787)	(0.0445)	(0.0537)	(0.0648)
Observations	3152	1488	1664	536	952	688	976
Joint p original	0.0644	0.00805	0.0460	0.117	0.00576	0.195	0.0115
Sum of lags original	0.0356	-0.0794	0.255***	-0.0546	-0.0866	0.377**	0.135**
	(0.114)	(0.115)	(0.0962)	(0.190)	(0.116)	(0.177)	(0.0683)
Joint p elsewhere	0.0126	0.00453	0.0245	0.586	0.00189	0.0105	0.136
Sum of lags elsewhere	0.0948**	0.126***	0.0668*	0.0957	0.132***	0.0963	0.0373
	(0.0391)	(0.0434)	(0.0385)	(0.0599)	(0.0480)	(0.0636)	(0.0322)

Notes: See Notes to Table 3. 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: 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
Joint p contemp. + lags	0.00305	0.00447	0.000358	1.61e-06	0.0383	0.0695	0.0257
Sum of contemp. + 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)
Joint p leads	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 3. 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 8: 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 contemp. + 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 contemp + lags = contemp. effect p-value	<0.001	<0.001	<0.001	<0.001	<0.0010	0.003	0.0171

Notes: See Notes to Table 3. *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 9: 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 8. 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