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Abstract

I study the poverty impacts of plantation-based agricultural growth, focusing on Indonesian palm oil. Using rich new administrative panel data, I exploit exogenous variation arising from Indonesia's unique institutions and the data's longitudinal features to identify causal effects. Increasing the palm oil share of land in a district by ten percentage points corresponds to a ten percent reduction in its poverty rate, and a narrowing of the poverty gap. Effects are similar across regions and at the province level. Oil palm expansion tends to be followed by a sustained boost to the value of agricultural output, manufacturing output, and district GDP.

Keywords: palm oil, cash crop, plantation, agriculture, poverty, Indonesia
JEL codes: C23, C26, I32, Q15, Q18

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Is palm oil good for the poor? Palm oil is the world's most consumed vegetable oil. Crude palm oil is derived from the reddish pulp of the fruit of the oil palm, a plantation-based, labor-intensive cash crop originating from Africa (*elaeis guineensis*) and the Americas (*elaeis oleifera*), mostly grown in developing countries today. Millions of people across Asia, South America, and Africa earn income from oil palms, which yield more oil from less land than any other crop, from relatively little inputs. While oil palm is one of the most economically attractive uses for land in humid lowland tropics, the palm oil industry is one of the world's most socially contested, particularly in its largest producer, Indonesia. The palm oil sector consistently faces controversy related to tropical deforestation, endangered wildlife, displaced people, and local conflicts.[1]

In this article I ask whether the world's largest modern plantation-based agricultural expansion has been pro-poor. I study the effects of Indonesia's rapid increase in palm oil land in the 2000s on district poverty outcomes using rich new longitudinal data. Blending administrative statistics with detailed household survey data covering all Indonesian districts, I exploit the spatial and temporal variation in the size and speed of oil palm expansion and poverty reduction across Indonesia in the 2000s. The main empirical challenge relates to potential omitted variables affecting palm oil expansion and poverty. However, districts must apply to the central government to convert land into palm oil plantations, generating random delays in approval timing *within* districts, and to a lesser extent, uncertain responses across districts. I principally rely on the plausible identification assumption that the within district changes in palm oil land are exogenous to future poverty, conditional on district and island-by-year fixed effects. Consonant instrumental variable estimates robust to potential unobservable heterogeneity are also provided. I find increasing the district share of palm oil land by ten percentage points causes the poverty rate to fall on average ten percent more than in similar districts in the same region without oil palm expansion. Poverty gaps also

[1]See Corley and Tinker (2003) for history and physiology, and Rival and Levang (2014) for physiology and recent developments in Asia. Dennis et al (2005), Koh and Wilcove (2007, 2008), and Busch et al (2015), discuss environmental impacts, and McCarthy et al (2011), and Rist et al (2010) discuss local social impacts.

significantly narrow. Results are consistent over different time horizons and in all major oil palm growing regions, with effects strongest in the rural outer islands where initial poverty rates were highest. Similar patterns are observed at the province level. Large and smallholder plantations both reduce poverty, but with different dynamics. The poverty impacts of new large plantations appear to have a consistent time profile, but I find no evidence of immediate impacts from smallholder expansion. Smallholder impacts are however similar to those from large plantations after a few years. Oil palm expansions correspond to a small boost in the value of agricultural output, manufacturing output, and district GDP, suggesting positive spillovers through local production or consumption linkages.

This article makes two main contributions. Although the role of the agriculture in economic development and poverty reduction has been widely studied (Dercon (2009) and Dercon and Gollin (2014) provide reviews), little attention has been paid to plantation agriculture, despite its growing ubiquity in developing countries (Barbier, 1989; Maxwell and Fernando, 1989; Pryor, 1982; Tiffen and Mortimore, 1990). Existing theories and evidence do not naturally carry over. For example, cash crops seldom feed modern sector labor (c.f., Lewis, 1954; Schultz, 1964). The potential for agricultural demand-led industrialization is also unclear (c.f., Johnston and Mellor, 1961; Ranis and Fei, 1961; Adelman, 1984). Consumption (backwards) linkages are likely greater than traditional agriculture (e.g., through higher income effects from higher yields), but low technology, skill, and processing requirements imply limited production (forward) linkages. Recent studies on local multipliers may be more relevant (Moretti, 2010; Hornbeck and Keskin, 2015), although these frameworks have not been widely applied in rural developing country contexts.[2] Turning to my focus in this paper, poverty, agricultural growth is generally believed to directly reduce poverty more than other types of growth (Ravallion and Chen, 2003; Kraay, 2006; Ravallion and Chen, 2007; Christiaensen et al, 2012) but the poverty

[2] To the best of my knowledge, theoretical frameworks on local labor markets and local multipliers have not been adapted to settings with imperfect substitutability between imports and local consumables, immobile factors of production, and abundant unskilled labor.

benefits of large-scale agricultural development are more contested (Quizon and Binswanger, 1986; Anriquez and Lopez, 2007; Maertens and Swinnen, 2009). Critical mechanisms responsible for past agriculture-led poverty reduction (e.g., agricultural technology growth, initial agricultural infrastructure, and human capital conditions (Ravallion and Datt, 2002)) are less likely for plantations. This is the first nationwide study of the link between the plantation-based cash crops and welfare, using the world's largest modern plantation sector expansion as a case study. I focus on Indonesia, but findings should be informative to other developing countries looking towards plantation agriculture for poverty reduction.

My second contribution relates to the ongoing policy debate on the development impacts of the palm oil industry. While the environmental costs of palm oil are well documented, whether Indonesia's dramatic shift in land use towards palm oil has brought benefits to the poorest is the subject of much speculation but yet no systematic quantitative inquiry (McCarthy et al, 2011; Rist et al, 2010; Budidarsono et al, 2012). Existing qualitative narratives and geographically-narrow case studies provide a rich source of descriptive evidence but no basis for causal inference, with weak and narrow internal validity. Using a new nationwide district panel covering the most significant period of oil palm expansion, I address these critical limitations of the existing evidence.

1 Indonesia's palm oil expansion

The modern world's largest plantation-based agricultural expansion is taking place in Indonesia. The third most populous developing country after China and India, Indonesia supplied more than 40 percent of the 60.54 million metric tons of palm oil produced in 2014–15. Global palm oil production has doubled every decade since the 1960s, surpassing soy bean oil in 2007 to become the world's dominant vegetable oil (US Department of Agriculture, 2015). With a comparative advantage in unskilled labor intensive goods and proximity to India and China (the largest purchasers), Indonesia was well placed to capitalize on the growing demand. Palm oil has been Indonesia's largest agricultural export for the last two decades, directly employing between two and three million people in 2011.[3] Indonesia's rapid increase in palm oil production has come almost exclusively through land area expansion (i.e., 92 percent), rather than intensification and higher yields (Gaskell, 2015). From 2001–2009 palm oil land increased from around five million hectares to just under 17 million hectares, 8.7 percent of Indonesia. Land must be taken from some other use to develop oil palm plantations and the opportunity costs of the land used to grow oil palm are widely documented (Burgess et al, 2012; Busch et al, 2015; Carlson et al, 2013; Gibbs et al, 2010; Hunt, 2010; Koh et al, 2011; Rival and Levang, 2014; and Wheeler et al, 2013). Land use is the central policy issue, and the fundamental constraint to further sectoral growth. But there is a paucity of credible evidence on the welfare impacts of changing land use patterns, particularly concerning the unprecedented plantation expansion characterizing the modern Indonesian economy.

Three decades of economic growth and structural change since the 1970s saw broad-based benefits and poverty reduction across Indonesia (Hill, 1996). Rural poverty reduction was mostly driven by agricultural growth (Suryahadi et al, 2009; de Silva and Sumarto, 2014). Suryahardi et al (2009) decompose provincial growth into urban and rural growth by sector to find rural agricultural growth strongly reduces rural poverty, but urban services growth has the largest impacts on both urban and rural poverty due to strong inter-sectoral and regional linkages. Since the Asian Financial Crisis and the fall of Suharto in 1997, economic growth and poverty reduction have both slowed. Rapid structural change and a rising manufacturing share of GDP slowed to a halt with the mining and palm oil booms of the 2000s. The poverty headcount has continued to fall, but it is unclear how much progress can be attributed to palm oil. The rural agricultural sector has had highest poverty incidence since independence (consistently accounting for around one third of the poverty headcount), and almost 100 million Indonesians still lived below or near the poverty line in 2014 (World Bank, 2015).[4]

[3] Indonesia's population is almost 250 million.

[4] Manning (2010), Miranti (2010), Suryahadi et al (2003), Manning and Sumarto (2011), and Wetterberg et al (1999) provide comprehensive accounts of the evolution and determinants of poverty in Indonesia.

Whether palm oil-based economic growth reduces poverty depends on its relative importance to the economy and poor people (Loayza and Raddatz, 2010). Palm oil growth is likely to be pro-poor if poor people are employed, have access to land to become smallholders, or benefit from related economic development. On employment, Labor intensity is the critical factor shaping the poverty elasticity of sectoral growth in Indonesia and oil palm is a labor-intensive cash crop requiring little skill or capital (Thorbecke and Jung, 1996). Oil palm farmers and plantation workers can earn more than other low skilled workers and returns to labor are estimated to be two–seven times the average agricultural wage (Budidarsono et al, 2012). Large plantations employ roughly two people for every five hectares and in 2010, just 1.7 million (out of 250 million) people worked on palm oil plantations, receiving a wage just higher than the Rp 6,400/hour earned on food crop farms; most of the returns to labor on large plantations return to employers and landowners (Burke and Resosudarmo, 2012). However, half of Indonesia's reported oil palm plantation area is managed by smallholders, usually with one–two hectares each. Smallholder plantation area has grown much faster than company and state-owned plantation area since 2000 (McCarthy et al, 2011), accounting for a larger share of the palm oil related labor market. Smallholders tend to report improved yields, profits, nutrition, and incomes after entering the sector (Budidarsono et al, 2012). But those living below the poverty line area also more likely be be landless and unable to legally become smallholder oil palm farmers, making potential spillovers (i.e., production and consumption linkages) the critical factor shaping the poverty elasticity of oil palm expansion. Existing case studies argue this cannot be the case, emphasizing little observed improvement in rural livelihoods, heightened social tensions, exploitation and human rights violations, and enclave palm oil industries delivering little benefit to local communities (Obidzinski et al, 2014). Ultimately the question of whether palm oil is good for Indonesia's poor is an empirical one.

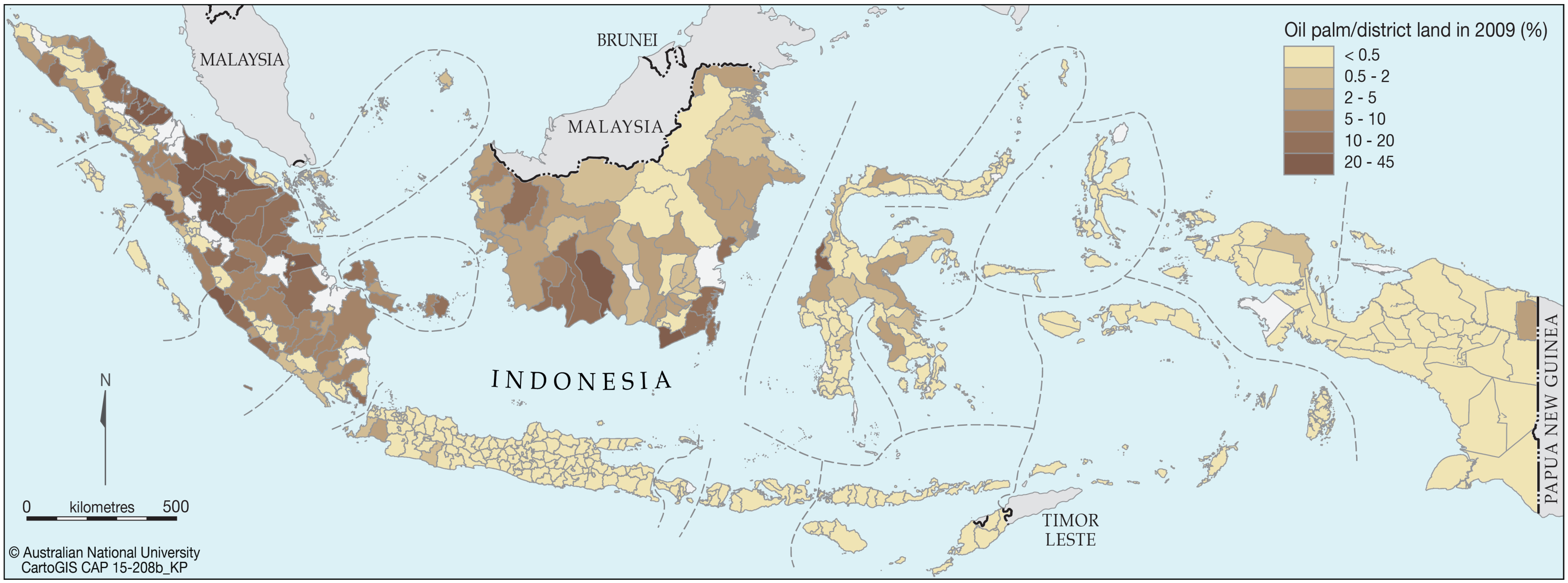
2 Data

2.1 Palm oil data

My main explanatory variable is official district palm oil land as a share of total district area. Total district palm oil land area in hectares is taken from the Tree Crop Statistics of Indonesia for Oil Palm. Produced by the Department of Agriculture annually since 1996, data cover land of varying condition (e.g., damaged, immature, and mature plantations) and ownership (smallholder, province, and state owned).[5] I convert palm oil land area to a share of total district area to focus on changing compositions: comparing palm oil land to other uses for land. As palm oil expansion has been predominantly in rural areas, the comparison tends to be against other types of agriculture and rural livelihoods. The identifying variation used throughout this paper relates changing land use patterns at the district level, and the spatial distribution of palm oil land as a share of total district area in 2009 is presented in Figure 1. Districts across Sumatra and Kalimantan (the two larger northwestern islands) use a greater share of land for palm oil than Sulawesi, Java, Papua, and the other islands. While data on official palm oil land are likely to be be imperfect, focusing on plantation land declared by the Indonesian Government has greatest tractability. Alternative satellite data cannot distinguish between mature plantations and forests, making it ill-suited for this study. For the parts of Indonesia where satellite data on plantation areas have been field verified, small unofficial, informal, and illegal oil palm developments tend to place alongside and proportional to officially declared plantations.

[5]Districts with no palm oil land are missing values in the original data, so I recode them as zeros to retain the baseline and control districts. Before recoding as zeros, I cross check these data against other sources for official plantation figures and gain strong anecdotal evidence from public officials that data are more or less nationally exhaustive. There are no large jumps from the imputed zero values. All increase gradually. Similar results are obtained if I drop all districts with no oil palm, focusing only on changes within districts with oil palm land. Unless otherwise stated, subnational official Indonesian data are taken from the World Bank (2015), from which my main results are easily reproduced.

Figure 1: Palm oil land as a share of district area, 2010



2.2 Poverty data

My primary outcome variable is the district poverty rate from 2002 to 2010, taken from Indonesia's central statistics agency, *Badan Pusat Statistik* (BPS), and the central social policy target for Indonesian governments and development agencies. The poverty rate is the share of the total district population living below the poverty line, i.e. the poverty headcount divided by the population. The expenditure-based poverty line varies by district and period, linked by a universal consumption requirement—mostly caloric. Poverty figures are derived from the consumption module of BPS's high quality, district representative national socio-economic survey (SUSENAS), covering 1,178,494 people across all provinces in 2010. SUSENAS is agnostic to whether consumption goods are purchased in formal or informal markets (i.e., not greatly affected by regional price differences), and a consistent method is used to calculate poverty rates for the entire period under study (i.e., the method changed in 1998 and 2011). I provide additional estimates using the depth of poverty (the average gap between the expenditure of poor people and the poverty line, known as the poverty gap index) and the poverty headcount (the poverty rate not denominated by population) from the same source. District poverty rates in 2010 are presented in Figure 2. Most poor people live on Java, where there are fewer oil palms, but poverty rates are highest in the rural periphery. Perhaps counterintuitively, the largest reductions in district poverty rates from 2002–2010 occurred in districts in Sumatra, Sulawesi, and smaller islands (e.g., Papua), not districts in Kalimantan or Java where significant oil palm expansions and urbanization have respectively taken place.

Figure 2: District Poverty Rates, 2010

2.3 *Pemekaran*

Indonesian districts are well-defined legal and geographical units with district level administrations reflecting local economies. A district panel provides temporal and spatial variation well suited to identify aggregate impacts within districts. Indonesia underwent one of the world's largest reconfigurations of a modern state with the fall of President Suharto in 1997, democratizing and decentralizing power to around 300 district governments. New political and fiscal powers drove the number of districts to proliferate from 292 in 1998 to almost 500 in 2015, a process known as *pemekaran*. Fitriani, Hofman, and Kaiser (2005) provide a detailed account of *pemekaran*, highlighting how district splits followed sub-district (i.e., *kecamatan*) boundaries and did not affect neighboring districts' borders. I combine the SUSENAS-derived district level poverty data with the official district palm oil statistics and apply year-2001 district boundaries to obtain a nationally-exhaustive balanced panel of 341 constant geographic units.[6]

3 Empirical strategy

I relate the share of district area used for palm oil plantations to district poverty rates with the equation:

$$ln(y_{d,t}) = \beta P_{d,t-1} + \delta_d + \tau_{i,t} + \gamma X_{d,t-1} + \varepsilon_{d,t} \tag{1}$$

$y_{d,t}$ denotes poverty in district d at time t, logged to account for data skewedness. $P_{d,t-1}$ is the palm oil land percentage of total district area, lagged by one year because poverty is measured in the middle of the year and palm oil at the end, and not logged to retain zeros. β is the effect of an additional percentage point of palm oil land as a share of total district land on the district poverty rate, i.e., on the extensive margin.

[6] In most Indonesian data, districts retain the original names and codes after splitting and reducing in size. Care is needed to avoid applying district fixed effects to such units, and the problem is far more pronounced at the village level. In international data, this equates to letting the USSR series continue without its former members instead of creating a new series for Russia. Alternative district definitions yield similar results, but constant land area units allow an uninterrupted panel dataset.

δ_d are district fixed effects, removing time-invariant, district-specific sources of bias that could jointly affect land use changes and poverty outcomes within districts, e.g., geography, climate, history, institutions, and culture. $\tau_{i,t}$ are island–year fixed effects capturing time-varying shocks common to each island grouping, e.g., economic growth and business cycles, international commodity prices, national elections and political shocks, and major policy changes such as large regional infrastructure investments. Island groups are defined as Java, Sumatra, Kalimantan, Sulawesi, with remaining islands grouped together. Island–year fixed effects focus my comparison to districts within the same island group, and relax the parallel trends assumption to flexible regional poverty trends.

$\gamma X_{d,t-1}$ is a vector of control variables, empty in my main estimates to maintain an exhaustive national sample, and because most potential controls are not plausibly predetermined or independent of changes in palm oil land and poverty (e.g., income, employment, government spending, literacy rates, whose inclusion would prevent a causal interpretation). Plausibly exogenous controls like election dummies and agro-climatic variables are sometimes weakly correlated with poverty but have no bearing on my parameter of interest and are not available for the complete panel. Estimates including a wide range of covariates are presented as robustness checks later in the article (Table 3) and in the Online Appendix (Table 2). Social policy in Indonesia is strongly targeted but the spatial distribution of the targeting is relatively unchanged from 2001–2010 (i.e., mostly captured by the district fixed effects). To be clear, potentially confounding new social programs were mostly implemented nationally (e.g., the Raskin rice subsidy, the National Program for Community Empowerment, unconditional cash transfers, and scholarships–captured by island–year fixed effects), or piloted in a few villages before national roll-out and unlikely to significantly bias nationwide panel estimates (e.g., conditional cash transfers). $\varepsilon_{d,t}$ is a heteroskedasticity-consistent error term, clustered at the district level to allow arbitrary correlation within districts over time.[7]

[7] Bertrand et al (2004) discuss problems arising in panel estimates when serial correlation is unaddressed. I consider larger cluster robust errors a more conservative basis for inference and hypothesis testing, with

I estimate equation one with the classic mean-differenced within estimator. Coefficients are driven by the variation over time within each district, rather than at a particular point in time across districts. While the implicit time to effect in panel estimators is approximately the temporal dimension (e.g., an annual panel gives short-run, contemporaneous estimates), within estimation also picks up level effects (c.f., first differences assume effects in the same year), an important consideration given that impacts from more palm oil land are unlikely to be fully realised immediately. After land conversion has been approved (discussed in the following section), smallholders need time to switch livelihood, prepare land, and plant oil palms. Likewise companies must establish the necessary infrastructure, prepare the land, hire workers, plant crops, then harvest fruit at least two years later (it takes five to seven years for the oil palms to reach a productive state). The price paid for a fresh fruit bunch also increases with tree maturity, set weekly and published in local newspapers, i.e., per hectare incomes likely increase with time.[8]

To gauge longer-term magnitudes, I estimate the long-difference equation:

$$ln(y_{d,2010}) - ln(y_{d,2002}) = \beta(P_{d,2009} - P_{d,2001}) + \delta_i + \gamma X_{d,2000} + \varepsilon_{d,t} \qquad (2)$$

$ln(y_{d,2010}) - ln(y_{d,2002})$ is change in the logged poverty rate from 2002–2010 in district d. $P_{d,2009} - P_{d,2001}$ is the change in the share of total district area used for palm oil plantations from 2001–2009. δ_i are island group dummies and $\gamma X_{d,2000}$ is a vector of initial conditions controls to allow variable trends by region and initial conditions, namely log district per capita incomes and poverty rates in 2000. Time-invariant sources of confoundedness are again differenced out. β has a similar semi-elasticity interpretation to that in equation one, and $\varepsilon_{d,t}$ is the robust error term, clustered at the province level to capture potential spatial correlation.

weaker assumptions and better finite sample properties than more efficient counterparts. Using normal independently and identically distributed (a.k.a., i.i.d.) standard errors, clustering at the province and island levels, and making other adjustments (e.g., panel-specific autocorrelation, bootstrap, and jackknife standard errors) gives similar coefficients and standard errors (see the Online Appendix Table 7).

[8]Note that district fixed effects likely capture systematic differences across local markets.

3.1 Identification

$\widehat{\beta}$ in equation one has a causal interpretation if there are no time and district varying omitted variables correlated with $y_{d,t}$ and $P_{d,t-1}$ influential enough to systematically shift poverty trends within island groups. Formally, the identification assumption is:

$$\mathbb{E}(\varepsilon_{d,t}|P_{d,t-1}, \delta_d, \tau_{i,t}, \gamma X_{d,t-1}) = 0 \tag{3}$$

Identification is achieved using panel data methods with district and island–year fixed effects, relying on the plausible assumption that the *timing* of changes in official palm oil land is exogenous to changes in district poverty outcomes, conditional on fixed effects. A conditional independence assumption is justified for two main reasons.

Firstly, changes in official palm oil land provide a plausibly exogenous source of identifying variation minimizing potential omitted variable bias. Equation one focuses on the poverty response from the *timing* of district palm oil expansions (rather than whether expansions occur), so the critical issue is what determines the timing. Palm oil land declared by the Department of Agriculture reflects plantation sector land use decisions made through the large, decentralized bureaucracy. Indonesian land use regulations are complicated, especially since decentralization. The *Regional Autonomy Laws 1999* saw district forest departments become answerable to *bupatis* (district heads), rather than a Ministry of Forestry division answerable to the central government. *Bupatis* must apply to the central government (i.e., the ministries of agriculture, forestry, and industry, depending on the circumstances) for approval to convert land into oil palm plantations. This process involves identifying areas for plantations, attracting investors, securing approval from the district parliament, making a formal request for land conversion to the central government, central agencies working through the request, the district receiving approval, and the new plantation land being reported the following year in the Department of Agriculture's Tree Crop Statistics. Each step is influenced by idiosyncratic factors generating variation in the time taken for each step, which in turn influences the total time, and results in delays of *random* length.[9] Jakarta's responses to districts' requests and the variation *across* districts is also unlikely to be significantly affected by local socioeconomic conditions. Jakarta's unresponsiveness to local conditions and poor spatial coordination capabilities are well documented, even after direct *bupati* elections were introduced through the 2000s (McLeod, 2005; Lewis, 2005, 2010; Sjahrir et al, 2013, 2014).[10] Finally, is palm oil expansion likely to take place regardless of Jakarta's approval? That is, do the timing and decisions actually matter? Significant illegitimate plantation expansion is unlikely because of the immense logistical and processing infrastructure required. Large investments, particularly by foreign companies, are unlikely to proceed in high-risk (e.g., expropriation) environments without formal approval and support. Smallholders require the same infrastructure as large plantations and face large establishment costs until their first harvest (e.g., loss of current livelihoods and up to two years without income), making illegitimate smallholder-led expansion a risky endeavour.

The second main reason a conditional independence assumption is suitable is that selection into palm oil *across* districts is likely to mostly relate to time-invariant characteristics already captured by district fixed effects or differenced out, including history, culture, location, soil, climate, and other growing conditions. Major legal and regulatory changes, like moratoriums, are effectively captured by the island–year fixed effects and dissipate in the long-differences.

A causal interpretation of equation two's $\widehat{\beta}$ relies on the same parallel trends assumption. Consistent estimates are obtained if no omitted variables systematically shift poverty trends

[9]My institution-related argument for plausibly exogenous variation follows from and is similar to Burgess et al (2012), who argue administrative lags from central to district governments render district splits exogenous to various province and district level outcomes. Further background on Indonesia's unique resource management institutions is provided in Resosudarmo (2005), which includes several chapters on natural resource management policies for the forestry sectors.

[10]Even given the bureaucratic and biological lags, a "lagged identification" strategy would be misled (i.e., arguing exogenous Granger causation of future poverty). Although the endogenous component (e.g., feedback from districts) takes place before new palm oil land is declared, moving endogeneity back in time simply shifts the identification assumption from selection on covariates, to no dynamics among unobservables. The key issue is that the number of years taken to process districts' requests to convert land into palm oil plantations is random and the disconnect between local and central policy making makes the outcome of requests somewhat random.

within island groups after allowing for differential poverty trends by island and initial conditions. Several robustness checks on the credibility of the identification assumption follow my main results and, as unobservable heterogeneity is impossible to rule out in non-randomized observational studies, consonant instrumental variable estimates are provided.

4 Results

My main result is using a greater share of district area for palm oil production corresponds to lower district poverty rates and reductions in the depth of poverty. Column 1 of Table 1 presents a first-differenced annual panel estimate, focusing on contemporaneous year to year changes. A ten percentage point increase in the district share of land used for palm oil in one year corresponds to a small poverty reduction of 3 percent of the poverty rate the next year, statistically significant at the five percent level. Such immediate effects must come through channels other than the production and sale of the crop, for example through payments to communities and waged labor to establish plantations. Column 2 presents my preferred within fixed effects estimate. A ten percentage point increase in the share of land used for palm oil at the mean (e.g., from 10 to 20 percent of district area) corresponds to a poverty reduction of seven percent of the poverty rate: more than twice the first difference estimate and more precise. Column 3 presents the nine-year long-difference estimate, finding that a district with ten percentage points more of its land used for palm oil plantations has a poverty rate ten percent lower than otherwise similar districts. Columns 4–6 of Table 1 show that increasing the share of land used for palm oil also corresponds to significant reductions in the depth of poverty.

Table 1: Poverty impacts of palm oil expansion

Dependent variable	*Log district poverty rate*			*Log district poverty gap index*		
Estimator	FD	FE	LD	FD	FE	LD
Column	1	2	3	4	5	6
Palm oil land / district area	-0.003**	-0.007***	-0.012***	-0.010***	-0.014***	-0.016***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)
District FEs	Y	Y	N	Y	Y	N
Island–year FEs	Y	Y	N	Y	Y	N
Initial condition controls	N	N	Y	N	N	Y
Districts	341	341	335	341	341	335
Observations	3040	3386	335	2705	3051	335

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Estimators are first difference (FD), within fixed effects (FE), and long difference (LD). Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for panel estimators and at the island level for LDs. Initial condition controls refer to log district per capita income and log poverty rate in 2000.

4.1 Alternative explanations

Estimates presented in Table 1 could be driven by a changing denominator, where districts increasing oil palm land experience higher population growth, inwards migration of wealthier people, or outwards migration of poor people. All three cases would show a false reduction in the poverty rate. In Table 2 I present results from fixed effects and long-difference estimates using the logged population (Columns 1 and 2) and logged number of poor people (Columns 3 and 4) as dependent variables. Column 1 provides no evidence of any short-term change in population arising from oil palm land expansion. Column 2 shows that over nine years districts with large palm oil expansion tend to have slightly larger populations, as expected if incomes rise, although statistically significant at only the ten percent level. Even if there is higher population growth and an influx of wealthy migrants (inward movement of poor people is however more plausible), Columns 3 and 4 show more oil palm land corresponds to a large reduction in the total number of poor people in each district. I cannot rule out poor people leaving and being replaced by inward migrants, but this appears unlikely. In Columns 5 and 6 I include per capita palm oil production, highly statistically significant and of a large magnitude. Per capita palm oil production renders the palm oil land share coefficient insignificant, suggesting the principal channel for the effects observed in Table 1 is the production of palm oil and not some other factor correlated with districts converting land to palm oil plantations. Similar results are obtained if one and a half times the official poverty line is used to calculate the poverty rate, and if the mean household expenditure of the bottom twenty percent of the population is used as the dependent variable.

4.2 Placebos, additional controls, and district trends

I test the credibility of my identification assumption in four distinct ways: an in-time placebo test; coefficient stability to additional covariates; district-by-district time trends; and instrumental variables. Column 1 of Table 3 presents an in-time placebo test, estimating equation one using future values of palm oil land as the explanatory variable. The coefficient

Table 2: Population, Poor People, and Production

Dependent variable	*Log district population*		*Log number of poor*		*Log district poverty rate*	
Estimator	FE	LD	FE	LD	FE	LD
Column	1	2	3	4	5	6
Palm oil land / district area	-0.0007	0.003*	-0.011***	-0.009***	-0.004*	-0.007
	(0.0007)	(0.002)	(0.003)	(0.003)	(0.002)	(0.004)
Per capita palm oil production					-0.190***	-0.125**
					(0.054)	(0.054)
District FEs	Y	N	Y	N	Y	N
Island-year FEs	Y	N	Y	N	Y	N
Initial condition controls	N	Y	N	Y	N	Y
Districts	341	335	341	335	341	335
Observations	3689	335	3045	335	3386	335

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Estimators are within fixed effects (FE) and long difference (LD). Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for panel estimators and at the island level for long differences. Initial condition controls refer to log district per capita income and the log poverty rate in 2000, and an island group fixed effect.

on future palm oil land is statistically insignificant, suggesting long-term confounding factors correlated with land use change (e.g., economic growth) are unlikely to drive the main result.

Columns 2–5 of Table 3 show how the main fixed effects estimates are robust to the inclusion of a range of key covariates. I present only estimates with an interesting interpretation. Column 2 includes the first lag of the district poverty rate, as districts with higher poverty rates may reduce poverty more easily (recall that my long-difference estimates control for initial per capita income and poverty levels). The estimated coefficient on oil palm land is of a slightly lesser magnitude but not statistically different. Convergence is not driving my main fixed effects result. Column 3 of Table 5 shows how the main result is similar if I include a new, unique set of electricity variables: installed megawatt capacity; combined installed megawatt capacity of all neighbouring districts; and the share of households with access to electricity from the state utility and other sources.[11] Electricity-related variables are well suited to proxy the general level of economic development and capabilities, with less endogeneity problems than income variables due to long and cumbersome plant approval processes.

Many palm oil producing districts are also rich in oil and gas, and natural resources have in no small part driven local politics and institutional changes since decentralization. These processes could jointly affect poverty and agrarian change. In Column 4 of Table 5 I show the main result is unchanged if I include per capita oil and gas value-added, per capita general purpose transfers from the central government (*Dana Alokasi Umum*), district oil and gas revenue, the total number district splits (*pemekaran*) in each collapsed district, and a dummy variable for years following the staggered introduction of direct *bupati* elections across districts.[12]

Many palm oil plantations are on land once primary forest. Forestry income could bias my

[11]Electricity variables taken from Sparrow, Grimm, and Tasciotti (2015), and the sample falls from 341 to 260 districts in Column 3 of Table 5 because 1993 boundaries are used to construct this data.

[12]These fiscal and political variables are taken from Burgess et al (2012), covering only the more forested islands of Sumatra, Kalimantan, Sulawesi, and Papua. Forestry-related variables used in Column 5 of Table 3 are obtained from the same source.

estimates downwards (i.e., show greater reductions in poverty), and potential social harms associated with deforestation (e.g., malaria, conflict) could bias my estimates upwards. The best available measure of deforestation is the change in pixel level tree cover constructed from Moderate Resolution Imaging Spectroradiometer (MODIS) satellite imagery, detailed in Burgess et al (2012). While MODIS data cannot disentangle primary forest from plantations (i.e., it is distinctly not a measure of deforestation in the Indonesian context), it can still serve as a useful proxy for observed changes in forestry and the natural environment. In Column 5 of Table 3 I estimate the short-term poverty impacts of more palm oil land holding changes in tree cover and forestry income constant, partialling out potential effects through these channels. I control for revenue from forestry, the annual change in pixels of tree cover, lagged pixels of tree cover (i.e., forest stock), district splits, and direct election dummies. The estimated poverty impacts of additional palm oil land are slightly larger with no change in tree cover or income from forestry. Column 6 controls for per capita district income, government revenue, and government expenditure in the full samples (data are taken from World Bank (2015)), yielding similar results and implying distributional changes are likely to be an important part of the story.

In Column 6 I relax the parallel trends assumption and include district-by-district time trends along with the district and island–year fixed effects. As expected, the coefficient is smaller with this extremely rich control vector, but still negative and statistically significant at the ten percent level. Long-difference estimates are similarly robust to controlling for income growth, different measures of tree cover change, and using different start and finish years (see Online Appendix Table 2). Using the common heuristic that coefficient stability to additional controls is informative about omitted variable bias, I also estimated equations one and two using the entire suite of controls (around 200 variables) available in the World Bank's (2015) first public sub-national data base, the Indonesia Database for Policy and Economic Research (DAPOER). Not systematically selected to control for poverty determinants, DAPOER variables can be considered selected at random with respect to my two variables of interest, with omitted variables also omitted at random. Coefficients on palm oil land are remarkably stable: this exercise and related proportional selection bias calculations (Oster, 2015) suggest it is highly unlikely an omitted variable overturns the main result, i.e., the identification assumption is likely satisfied (see Altonji et al (2005), Bellows and Miguel (2009), and Oster (2015) for theory and applications). As unobservable heterogeneity is impossible to rule out in observational studies, I present instrumental variable estimates robust to any such problematic omitted variables in the following section.

4.3 Instrumental variable estimates

I employ two distinct instrumental variable strategies. For panel fixed effects estimates, I exploit demonstration effects arising from early adoption. I instrument the within district changes in palm oil land with a district-specific linear trend increasing in districts' initial palm oil land, exploiting that fact that initial conditions matter for future growth trajectories. Oil palms historically expanded more in areas where plantations were already established due to better access to pre-existing knowledge networks, materials, processing facilities, and other infrastructure necessary to develop oil palm plantations. I use Fuller's (1977) median-unbiased limited information maximum likelihood (LIML) estimator for all instrumental variables estimates, presented in Table 4. Column 1 presents the short-run, local average partial effect identified from existing oil palm activity. A ten percentage point increase in the share of district land used for palm oil at the mean, exclusively due to that district's steeper palm oil land expansion trajectory, corresponds to an almost 40 percent reduction in the poverty rate.[13] While least squares tends to be biased towards zero and there are theoretical reasons to suspect a downwards bias, the larger instrumental variable estimates have an alternative explanation: early adopters on average achieved more rapid poverty reduction. This local average partial effect is quite widely applicable, with 71 of

[13]Using the poverty gap index, the percentage of the population below 1.5 times the poverty line, and the mean household expenditures of the bottom 20 percent of the population as a dependent variable all give similar results.

Table 3: Robustness—Panel Fixed Effects

Dependent variable: log district poverty rate							
Column	1	2	3	4	5	6	7
Palm oil land / district area	0.001	-0.006***	-0.008***	-0.010***	-0.009***	-0.007***	-0.003*
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
In-time placebo test	Y	N	N	N	N	N	N
Lag poverty control	N	Y	N	N	N	N	N
Electricity controls	N	N	Y	N	N	N	N
Political, fiscal, oil, and gas controls	N	N	N	Y	N	N	N
Forest & political controls	N	N	N	N	Y	N	N
Income & revenue controls	N	N	N	N	N	Y	N
District time trends	N	N	N	N	N	N	Y
Districts	341	341	260	181	181	335	336
Observations	2704	2699	2321	1445	1445	3227	3386

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Within estimator with district and island–year FEs used for all estimates. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level. Each group of control variables is enumerated in the text.

the 341 districts across four of the five island groups using some share of their land for oil palm plantations in 2000. The first-stage coefficient is positive, of a decent magnitude, and statistically significant at the 0.1 percent level, suggesting non-convergence in oil palm expansion and strong, positive early-adopter effects. The Kleibergen-Paap (2006) rk Wald F statistic is near the Staiger and Stock (1997) rule of thumb of an excluded-F statistic of greater than ten, but as LIML is median-unbiased magnitudes can still be reliably understood in the presence of a potentially weak instrument (Murray, 2006). The relevant exclusion restriction for Column 1 is that initial palm oil adoption is uncorrelated with each district's poverty trajectory except through palm oil growth, after controlling for district and year fixed effects. This assumption is fundamentally untestable but it is informative to include initial district-specific linear trends based on initial poverty rates to capture other potential channels. Column 2 shows the main result is virtually unchanged when 339 district-specific linear poverty trends are included.

My long-difference instrumental variable strategy exploits two additional factors constraining oil palm expansion in Indonesia: district rainfall in 2000, because oil palms require humid low land tropics; and average district slope, because less mountainous terrain is more suitable for large-scale plantation agriculture.[14] While rainfall and slope may affect poverty through channels other than palm oil land (e.g., labor and agricultural productivity), Figure 4 shows little correlation between these instruments (rain and slope) and the long-differenced poverty rate in districts with no oil palm, i.e., untreated districts. I cannot rule out rain and slope affecting poverty during this period through channels other than palm oil land in treated districts, but it appears unlikely. My preferred over-identified estimate is presented in Column 6, with exactly-identified estimates are presented in Columns 3–5 to illustrate the local average partial effect and relative second-stage contribution of each instrument. All magnitudes are are consistent with those from least squares in Table 1. The Kleibergen-Paap (2006) rk Wald F statistic for my preferred estimate in Column

[14]Rainfall and slope variables are taken from Wheeler et al (2013), where I collapse districts accordingly and convert monthly rainfall data into an annual totals.

6 demonstrates strong identification, exceeding the most conservative Stock and Yogo (2005) critical values. The Hansen J statistic of 5.16 does not reject the null hypothesis over-identifying restrictions are valid, providing some further empirical support for the exclusion restrictions. Further robustness checks are provided in the Online Appendix.[15]

5 Effect Heterogeneity

Existing qualitative studies examining the poverty implications of oil palm expansion in Indonesia emphasise context-specific heterogeneity (McCarthy, 2010). My main results confirm that, on average and across all of Indonesia, growth in the palm oil sector has tended to reduce poverty. In this section I assess whether my main estimates mask systematic heterogeneity by region and sector.

Table 5 presents the effects of additional palm oil land on district poverty rates by each of Indonesia's five main regions. I present full-sample within and long-difference estimates, interacting the island dummies with my main palm oil land share variable. Each interaction term represents the contribution of the variation of changes in palm oil land in each island group to the main effects presented in Table 1. I drop the main effects (i.e., not interacted) to allow a more straightforward interpretation of the estimated coefficients. Column 1 of Table 5 presents the within estimate including district and year fixed effects. The coefficients on the interaction terms for regions with little palm oil (Java and small other islands) are, as expected, statistically insignificant. Across the palm oil producing regions of Sumatra, Kalimantan, and Sulawesi districts have, on average, experienced short-run poverty reductions as a result of oil palm expansion. Districts in Sulawesi, where initial poverty was highest, experienced the largest reductions in district poverty rates. The long-difference estimate presented in Column 2 paints a similar picture; the only difference

[15] Results were also qualitatively verified through focus group discussions, semi-structured interviews, and practical observations in six villages across four districts in Sumatra, each with varying engagement with oil palm. People reported improved living standards from recent success with oil palm, and their villages tended to be wealthier and have better infrastructure and services than neighboring rural villages.

Figure 3: IVs and Poverty Reduction (Y axis) in Untreated Districts

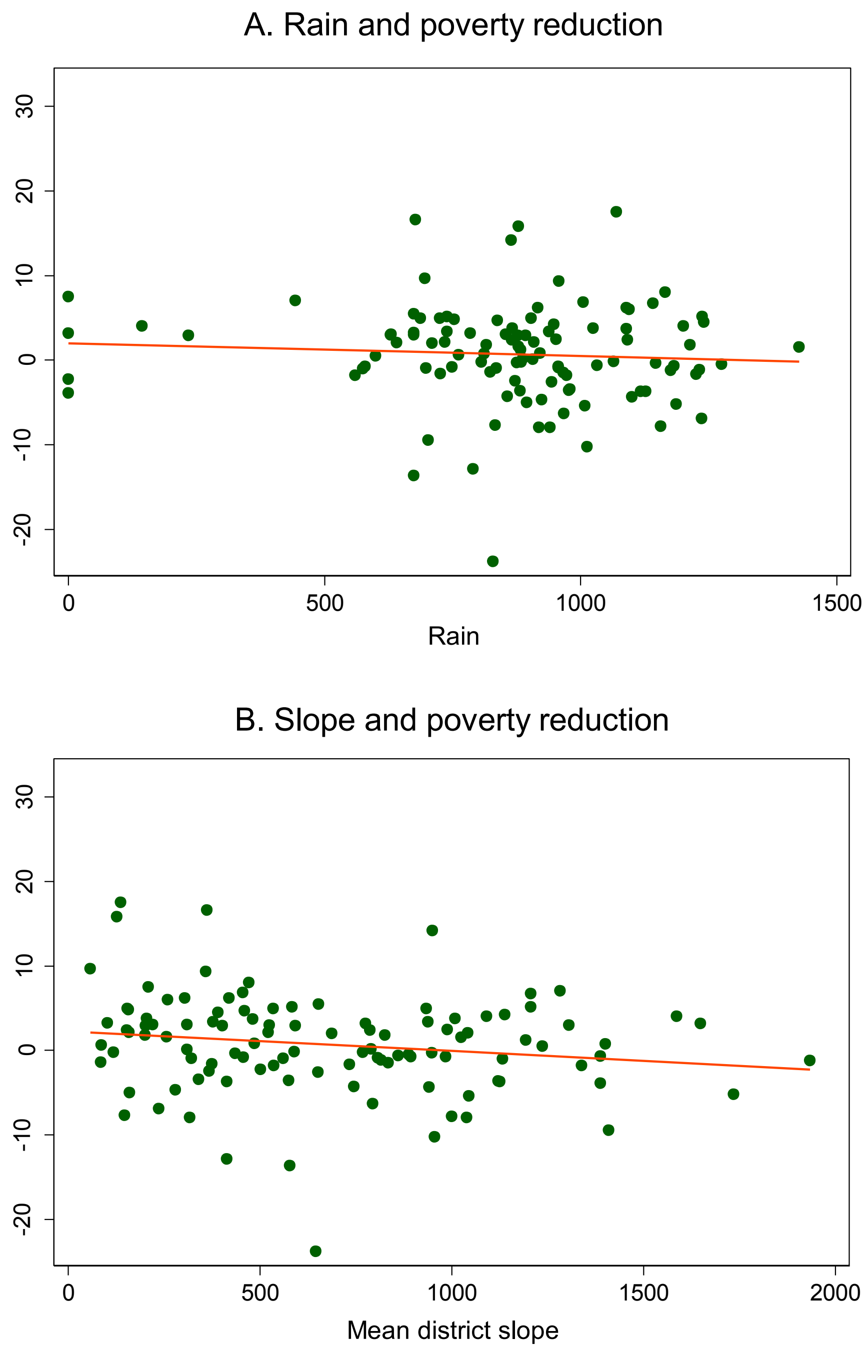


Table 4: Robustness: Instrument Variable Estimates

Dependent variable: log district poverty rate						
LIML estimator	FE	FE	LD	LD	LD	LD
Column	1	2	3	4	5	6
Palm oil land / district area	-0.037***	-0.019*	-0.012**	-0.028***	-0.017	-0.013**
	(0.014)	(0.010)	(0.005)	(0.009)	(0.013)	(0.005)
District & year FEs	Y	Y	N	N	N	N
Initial poverty trends	N	Y	N	N	N	N
Initial conditions controls	N	N	Y	Y	Y	Y
Districts	341	339	335	275	274	274
Observations	3386	3367	335	275	274	274
First-stage coefficients and diagnostics						
Initial palm land trends	0.21***	0.21***				
Initial palm share			3.167***			2.849***
Rain				0.004***		0.002***
Slope					-0.002***	-0.002***
Excluded-F statistic	8.8	8.2	12.3	34.6	9.4	16.6
10% max Fuller bias critical value	19.4	19.4	19.4	19.4	19.4	7.9
30% max Fuller bias critical value	12.7	12.7	12.7	12.7	12.7	5.6
Hansen J statistic						5.2

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Estimators are the within fixed effects (FE) and long difference (LD), both estimated with the limited information maximum likelihood (LIML) instrumental variable (IV) estimator. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for FE estimates and island level for LD estimates. FE LIML instrument palm oil land with the initial share of palm oil land interacted with the year dummies. LD LIML instrument palm oil land with the share of palm oil land in 2000, total district annual rainfall in 2000, and the average slope of the district. Initial condition controls refer to initial per capita income, initial poverty, and island dummies. Excluded-F refers to the Kleibergen-Paap Wald rk F statistic statistic from the first stage regressions, the robust analogue of the Cragg-Donald statistic.

is that magnitudes for Kalimantan and Sulawesi are similar in the short and long run, but for Sumatra the long run effects are twice the magnitude of the short run effects. The most plausible explanation for this difference is that in Kalimantan and Sulawesi, most recent oil palm expansion has come through large industrial-scale plantations, whereas in Sumatra it relates mostly to smallholders.

Smallholder-based agricultural development is often preferred to large company-led expansion, as it is seen to engage more with local people, create more jobs per hectare, and drive "community development" (see, e.g., International Finance Corporation (2013), or Dercon and Gollin (2014) for reviews). While smallholder oil palm farmers in Indonesia tend to be much larger scale than smallholders in other countries, I follow the common smallholder/large plantation dichotomy.[16] Oil palm is generally a highly productive crop but Indonesian smallholders have been reported to: have per hectare yields up to 40 percent lower than industrial estates; struggle to exploit economies of scale; and use inefficient practices restricting yields and incomes (Burke and Resosudarmo, 2012; Lee et al, 2013). On the other hand, large industrial plantations are usually between 5,000–20,000 hectares and intensively managed to maximize efficiency (Corley and Tinker, 2003). Naturally the sectors may have heterogeneous poverty impacts.

Table 6 compares the poverty impacts of additional state-owned, private company, and smallholder oil palm land. Data are taken from the Tree Crop Statistics for Indonesia from the Department of Agriculture. As data are only available for a few years, I use wider panel (e.g., two-yearly and four-yearly panels) within estimation with district and island–year fixed effects to assess dynamics (c.f., long-differences). Columns 1–6 of Table 6 show similar coefficient magnitudes and dynamics in large state owned and private plantations, consistent in magnitude with the long-difference presented in Table 1. Effects for smallholders are more variable. In Column 7, there is no detectable short-run relationship between more smallholder

[16] Individual oil palm farmers usually manage no less than two hectares, often work in large cooperatives, and are usually found in close association with larger plantations, thus better classified as small to medium enterprises.

Table 5: Regional Heterogeneity

Dependent variable: log district poverty rate		
Estimator	FE	LD
Equation	1	2
Java*palm oil land/district area	0.013	-0.032
	(0.015)	(0.042)
Sumatra*palm oil land/district area	-0.006***	-0.012***
	(0.002)	(0.004)
Kalimantan*palm oil land/district area	-0.014***	-0.010**
	(0.003)	(0.005)
Sulawesi palm oil land/district area	-0.045***	-0.053***
	(0.007)	(0.017)
Other*palm oil land/district area	-0.036	-0.004
	(0.128)	(0.054)
District and year FEs	Y	Y
Initial conditions controls	N	N
Districts	341	335
Observations	3386	335

Stars denote statistical significance at the 10, 5, and 1 percent levels. Full sample (Column 3) is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Island groupings are defined as districts from Java, Sumatra, Kalimantan, Sulawesi, and with remaining islands grouped together. Estimators are within (FE) with district and year FEs, and long difference (LD) with initial log poverty and log per capita income controls. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for FE estimates and the province level for LD estimates. Island*palm interaction terms interact the island dummy for each island with the main palm oil land share variable, such that each interaction term represents the contribution of the variation of changes in palm oil land in each island group to the main effects presented in Table 1. Main effects (i.e., not interacted) are dropped for a more straightforward interpretation.

land and district poverty rates, perhaps because oil palms take at least two years to bear fruit (c.f., large state and company plantations immediately hire labor to establish and work on the plantations, and often build local infrastructure and community facilities for their workers). In Columns 8 and 9 the time to effect is extended to two and four years and the estimated smallholder coefficients are similar to other sectors. When all sectoral shares are included in the same regression, private company land renders the coefficients on state-owned and smallholder palm oil land statistically insignificant (See Online Appendix Table 11).

6 Wider impacts

The oil palm is a high-yielding crop which intensively uses low-skilled labor, but the proportion of people directly employed in the palm oil sector is small. The principal channels for growth in the palm oil sector to affect local welfare and poverty are potential forward (production) and backward (consumption) linkages. In this section I apply the same econometric approach to evaluate whether oil palm expansion generates district-level aggregate demand spillovers, or poverty reduction at a level of aggregation higher than the district–the province.

Table 7 examines the economic impacts of oil palm expansion on the value of district agricultural output, manufacturing output, and total GDP (all official BPS data). First, note that while the estimated coefficients are mostly positive and statistically significant, effects are not large in magnitude. Oil palm expansion does not appear to correspond to a major economic boom at the district level. For the two sectors most directly involved in the palm oil industry (i.e., agriculture and manufacturing), Columns 1–4 show small, persistent, and statistically significant increases in the value of output. A 10 percentage point increase in the share of land used for palm oil plantations corresponds a seven percent increase in the value of agricultural output and a four percent increase in the value of manufacturing output relative to similar districts. Considering aggregate district GDP in Column 5, an annual

Table 6: Heterogeneity by Plantation Type

Dependent variable: log district poverty rate									
Sector	State owned plantation land			Private company plantation land			Smallholder plantation land		
Panel width	Annual	2-yearly	4-yearly	Annual	2-yearly	4-yearly	Annual	2-yearly	4-yearly
Column	1	2	3	4	5	6	7	8	9
Palm oil land/ district area	-0.011**	-0.011***	-0.011**	-0.012***	-0.012***	-0.011***	-0.004	-0.011**	-0.012**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)	(0.004)
N	3009	1674	1004	3009	1674	1004	3009	1674	1004

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level. Within estimator with district and island–year fixed effects used throughout. Palm oil land data for each sector is taken from the Tree Crop Statistics of Indonesia, Department of Agriculture.

panel fixed effects estimate finds no statistically significant immediate effect, implying some short-run crowding out and reallocation of factors of production. Allowing some time for economic changes to manifest, the long-difference estimate in Column 6 shows increasing the share of district land used for oil palm by 10 percentage points corresponds to an average increase in non-oil and gas real GDP of 2.4 percent relative to districts without oil palm expansion. Over the medium term, potential local Dutch disease effects and the crowding out of other economic activity appear to be at least fully offset, with net economic effects small and positive. Longer-term development implications of local structural change towards agro-industries are unclear and a potential area for further research.

In Table 8 I estimate my main reduced-form relationship of interest using analogous province-level data. Columns 1 and 2 present short-run effects, focusing on changes within each province over time. In Column 1, I include island-specific poverty trends, and the coefficient is larger than the main district-level estimates, statistically significant at the ten percent level. Column 2 replaces the trends with the more flexible island–year fixed effects, reducing the magnitude to an order similar to the main results in Table 1. A long-difference estimate is presented in Column 3, including island dummies to allow for different regional poverty trends. Provinces with a ten percentage point increase in their share of land used for palm oil have experienced, on average, a 13 percent greater reduction in the poverty rate from 2002–2010. The three estimates presented in Table 8 suggest the robust relationship between oil palm expansion and poverty reduction observed within districts across Indonesia may apply more broadly, at least at the province level.

Table 7: Effects of oil palm expansion on sectoral and total district GDP

Dependent variable	*Log agricultural output*		*Log manufacturing output*		*Log GDP (excl. oil, gas)*	
Estimator	FE	LD	FE	LD	FE	LD
Palm oil land / district area	0.004**	0.007***	0.005***	0.004*	0.001	0.002**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
District FEs	Y	N	Y	N	Y	N
Island–year FEs	Y	N	Y	N	Y	N
Initial conditions controls	N	Y	N	Y	N	Y
N observations	3410	342	3410	342	3410	342

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Estimators are within fixed effects (FE) and long difference (LD) estimators. District fixed effects included throughout. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for panel estimators and at the island level for long differences. Initial conditions controls refer to log district per capita income and log poverty rate in 2000, and island group dummies. GDP data are official government statistics taken from the World Bank (2015).

Table 8: Province level Results

Estimator	FE	FE	LD
Column	1	2	3
Palm oil land / district area	-0.014*	-0.007**	-0.013**
	(0.006)	(0.003)	(0.004)
Linear island trends	Y	N	N
Island–year FEs	N	Y	N
Island dummies	N	Y	Y
Observations	319	319	30

Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual balanced panel of Indonesian provinces from 2002–2010, with palm oil land lagged one period (i.e., 2001–2009). Estimates are the within estimator with province level fixed effects (FE) and the long difference estimator. Heteroskedasticitiy-robust standard errors are in parentheses, clustered at the province level for FE estimates and the island level for LD. Data taken from the World Bank (2015).

7 Conclusion

This paper's objective was to quantify the contribution of palm oil expansion to poverty reduction in Indonesia. While there have been clear environmental consequences associated with Indonesia's rapid increase in palm oil production, on average Indonesian districts using more of their land for oil palm have experienced greater reductions in the rate and depth of poverty, a pattern observed across all palm oil producing regions and at the province level. Table 9 shows the estimated contribution of palm oil to poverty reduction in the ten districts experiencing the most dramatic change in the share of district land used for palm oil plantations. In these ten districts, my estimates account for over 70 percent of the observed poverty reduction. These districts also have some of the lowest poverty prevalence rates in Indonesia, e.g., Tanah Laut, South Kalimantan had a poverty rate of 5.1 percent in 2010, and Deli Serdang, North Sumatra 6.6 percent, less than half the national district average of 13.8 percent. There is clearly scope for future palm oil-based agricultural growth to contribute to rural poverty reduction, particularly in areas with little to moderate levels of oil palm plantations established.

The main limitation of this study is my focus on effects *within* the same district, not spillovers across regions or nationally. Since decentralization, Indonesia has continued to rapidly urbanize without much further industralization (Vollrath, Gollin, and Jedwab, 2015). Most palm oil companies are based in capital cities and general equilibrium effects are not well understood, particularly backward linkages to the non-tradable sectors. A detailed investigation of the changing labor market dynamics associated with the contemporaneous palm oil and mining booms and related structural change would be worthwhile, and in particular, developing a better understanding of the longer-term economic consequences of pro-poor primary sector growth.

Table 9: Top Ten Oil Palm Expansions

District, province	Palm oil land/area 2001–2009 change	Poverty rate 2002–2010 change		Contribution
		Estimated	Actual	
Rokan Hulu, Riau	35.8	-12.4	-15.8	78%
Asahan, N. Sumatra	34.5	-6.5	-3.9	100%
Labuhan Batu, N. Sumatra	34.0	-6.2	-2.5	100%
Tanah Laut, S. Kalimantan	23.9	-2.5	-3.8	68%
Deli Serdang, N. Sumatra	21.4	-2.6	-3.3	77%
Simalungun, N. Sumatra	19.9	-4.5	-8.1	55%
Kampar, Riau	19.4	-3.6	-5.2	71%
Kuantan Singingi, Riau	19.3	-6.3	-14.9	43%
Pasaman, W. Sumatra	17.2	-2.9	-3.9	74%
Langkat, N. Sumatra	16.7	-4.1	-9.8	42%

The estimated change in the poverty rate from 2002–2010 is obtained by applying the estimated long-difference semi-elasticity of -0.012 in Table 1 to the change in palm oil land as a share of district area in the preceding column. The estimated contribution of oil palm expansion is capped at 100% for Asahan and Labuhan Batu, which experienced less poverty reduction than predicted, i.e., without oil palm expansion, poverty could have increased.

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Is plantation agriculture good for the poor?
Online Appendix

Ryan Edwards

August 24, 2015

1 Summary of robustness checks

Key robustness checks and sensitivity analyses are summarized below, with the **threat** followed by a description of the robustness check undertaken. Some results from these exercises are presented in the main body of the paper or in the following Appendix Tables.

Exclusion restrictions for instrumental variable estimates may be violated: There is no reduced-form relationship in "non-treated" sub-samples, i.e., Java and all districts with no oil palm land (Figure 3). Similar results are obtained if additional covariates and trends proxying potential omitted variables are added, and Over-identification diagnostics are sound (Table 4).

Generalizability and external validity: Estimated using province-level data (Table 8), and using per capita palm oil production instead of the district palm oil land share of total district area (Appendix Table 9).

Long-differences may be sensitive to start and finish years: Estimated using alternative start and finish years (Appendix Table 2).

Period-specific effects: Interacted palm oil land with year dummies and crude palm oil price, and estimated before 2005 and after 2005, when there was a noticeable productivity improvement (Appendix Table 5).

Poverty persistent and serial correlation: While long-differences all control for initial poverty, additional estimates control for initial poverty in the panel set up and and interacted the treatment variable with initial conditions in both the panel and long-difference estimates (Appendix Table 3).

Sensitive to dependent variable (poverty measure) used: Estimated with the log poverty gap index, number of poor people, and the average household expenditures of the bottom twenty percent as dependent variables all logged and not logged (Table 1, Table 2, and Appendix Table 8).

Sensitivity to district definitions used and collapsing procedures: Estimated with alternative district definitions, i.e., in the original form, and creating new districts after splits.

Sensitive to explanatory variable (palm oil measure) used: Estimated using palm oil land in hectares and per capita; and for production in tons, value, and per capita terms (Appendix Table 9)

Sensitive to functional forms and estimators: Estimated with linear-linear (Appendix Table 6), and log-log specifications, noting that the log-log specification drops the control group. Single-equation models are estimated with first differences, random effects, and generalized least squares estimators, and instrumental variable models with a different instrumental variable estimators.

Sensitive to initial oil palm land conditions: Interacted palm oil land with initial poverty and initial palm oil land (Appendix Table 3), and mean district and province yields per hectare.

Sensitive to instruments used: For the panel initial conditions-specific linear trends instrument, lagged land, rainfall, and forest-related instruments give similar results under strong enough instruments, as does system and difference GMM, and interacting with world crude palm oil price instead of the year dummies. For the long-differences, variations on those used in the paper and described above give similar results.

Sensitive to standard errors used: All results presented in the paper are insensitive to standard errors used (Appendix Table 7).

Serial correlation and path-dependence in oil palm land: Estimated using wider panels, which minimizes this problem. Standard errors are clustered to allow correlation over time in the panel estimates, and long-difference estimates are robust to including initial palm oil land as a control variable.

Time-varying omitted variable bias: Placebo tests with future treatment values; included province and district-specific time trends; included province-year fixed effects; checked main coefficient stability to all DAPOER controls, including fiscal transfers, income, health and education, employment, and many other indicators.

Urban bias: Estimated: without Jakarta districts, without districts in Java (Appendix Table 5), and without all cities (kotas).

2 Appendix Tables

Table 1: Panel Summary Statistics

Variable		2002	2010	All years	Mean difference
Palm oil land / district area (%)	Mean	0.58	2.65	1.3	2.07
	SD	1.61	6.35	4.01	4.74
	N	341	341	3386	
District poverty rate (%)	Mean	19.94	13.82	16.74	-6.12
	SD	11.57	7.3	9.5	-4.27
	N	335	341	3386	

Summary statistics are for the balanced panel of constant geographic units, where district boundaries are reset to those at the start of the panel period for consistency. Palm oil land as a share of district area is lagged by one year, as it is in my estimates. Data are official Indonesian Government data, obtained through the World Bank's Indonesian Database for Policy and Economic Research online public portal.

Table 2: Robustness–Long-Differences with Additional Covariates

Dependent variable: log district poverty rate					
Column	1	2	3	4	5
Palm oil land / district area	-0.012***	-0.011***	-0.011***	-0.013***	-0.014***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Island FEs	Y	Y	Y	Y	Y
Initial poverty & per capita income controls	Y	Y	Y	Y	Y
Differenced log district GDP control	Y	N	N	N	N
Burgess tree cover controls	N	Y	N	N	N
Wheeler tree cover controls	N	N	Y	N	N
Earlier first year	N	N	N	Y	N
Early final year	N	N	N	N	Y
Observations / districts	335	181	335	336	334

Columns 1-3 show coefficients virtually unchanged controlling for potential long-run effects through the forestry sector and economic growth, and Columns 4 and 5 show long-difference estimates are insensitive to the start and finish year. Stars denote statistical significance at the 10, 5, and 1 percent levels. Main sample is for the years 2002 and 2010, with palm oil land lagged one period (i.e., 2001 and 2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticity-robust standard errors are in parentheses, clustered at the province level. Burgess et al (2012) controls include initial tree cover to capture forest stock and the change in tree cover to proxy deforestation and changes in the forestry sector. Wheeler et al (2012) use similar satellite data to Burgess et al (2012) but with greater regional coverage.

Table 3: Non-Linearities–Panel Estimates with Interactions

Dependent variable: log district poverty rate				
Column	1	2	3	4
Palm oil land / district area (palm land)	-0.010**	-0.009***	-0.016***	0.014**
	(0.004)	(0.003)	(0.004)	(0.007)
Palm land * palm land	0.00009			
	(0.0001)			
Initial palm land (2000) * palm land		0.0009		
		(0.0006)		
Initial poverty (2000) * palm land			0.0003***	
			(0.0001)	
Initial rural population share * palm land				-0.0003***
				(0.0001)
District fixed FEs	Y	Y	Y	Y
Island-year FEs	Y	Y	Y	Y
Observations	3386	3386	3367	3054

This table investigates non-linearities in the response of poverty to additional palm oil land in my main panel estimates. The main effect is consistent across all estimates, but there is evidence of some non-linearity at higher levels of initial poverty (Column 3) and with higher rural population shares (Column 4). Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Within estimation with district and island–year fixed effects is used throughout, and heteroskedasticity-robust district-clustered standard errors are in parentheses.

Table 4: Non-Linearities–Quadratics and Diminishing Returns

Dependent variable: log district poverty rate				
Sample	Annual	Two-yearly	Four-yearly	Long difference
Column	1	2	3	4
Palm oil land / district area	-0.010**	-0.025***	-0.021**	-0.027***
	(0.005)	(0.008)	(0.009)	(0.008)
Quadratic term	0.00009	0.0004*	0.0003	0.0007***
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
District FEs	Y	Y	Y	N
Island–year FEs	Y	Y	Y	N
Additional controls	N	N	N	Y
N districts	341	341	341	335
N obs	3386	1692	1016	335

Columns 2 and Column 4 of this table show diminishing returns in terms of poverty reduction at very high levels of palm oil land share, weak in the short run but statistically significant at the one per cent level in the long run. That is, at very high levels of palm oil land shares, additional oil palm expansion appears to exacerbate poverty. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticitiy-robust standard errors are in parentheses, clustered at the district level for panel estimates and the island level for long differences. Columns 1-3 present results using from panel fixed effects estimation, and column 4 from an OLS long difference. Additional controls are initial poverty rates, initial per capita incomes, and island dummies.

Table 5: Robustness–Alternative Samples

Dep var: log district poverty rate								
Sample	No Java				2006–2010		2001–2005	
Estimator	FE	FE LIML	LD	LD LIML	FE	FE LIML	FE	FE LIML
Column	1	2	3	4	5	6	7	8
Palm oil land / district area	-0.007***	-0.039**	-0.012***	-0.012**	-0.006**	-0.056*	-0.002	-0.116***
	(0.002)	(0.016)	(0.003)	(0.006)	(0.003)	(0.034)	(0.002)	(0.034)
District and year fixed effects	Y	Y	N	N	Y	Y	Y	Y
Island–year fixed effects	Y	N	N	N	Y	N	Y	N
Additional controls	N	N	Y	Y	N	N	N	N
Excluded-F statistic		7.770		14.98		4.185		17.06
Observations	2295	2295	230	175	1683	1683	2044	2044

Columns 1–4 show that my main estimates in Table 2 and the additional instrumental variable estimates are robust to including Indonesia's most populous island with little palm oil land, Java. Columns 5–8 use the first and second half of the sample period, as there was an increase in overall palm oil productivity between the periods. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for all panel estimators. Long difference standard errors are robust clustered at the island level. FE refers to fixed effects and LIML limited information maximum likelihood IV estimator. Panel LIML estimates instrument palm oil land with the initial share of palm oil land interacted with the year dummy, and long difference LIML estimates instrument palm oil land with the initial share of palm oil land, total district annual rainfall, and the average slope of the district. Additional controls are initial palm oil land, initial income, and island dummies. Excluded-F refers to the Kleibergen-Paap Wald rk F statistic obtained from first stage regressions.

Table 6: Robustness–Linear-Linear Functional Form

Dependent variable	*District poverty rate*			*District poverty gap index*		
Estimator	FD	FE	LD	FD	FE	LD
Column	1	2	3	4	5	6
Palm oil land / district area	-0.022*	-0.061**	-0.131*	-0.020***	-0.019*	-0.020
	(0.012)	(0.026)	(0.069)	(0.006)	(0.010)	(0.014)
District FEs	Y	Y	N	Y	Y	N
Island-year FEs	Y	Y	N	Y	Y	N
Initial income control	N	N	Y	N	N	Y
Districts	341	341	335	341	341	335
Observations	3040	3386	335	2705	3051	335

This table shows that the main results are insensitive to whether a log-linear or a linear functional form is adopted. My preferred estimates follow convention and use logs, and the poverty variables are in any case skewed. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Estimators are first difference (FD), within fixed effects (FE), and long difference (LD). Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for panel estimators and at the island level for LDs. Initial condition controls refer to log district per capita income and log poverty rate in 2000.

Table 7: Robustness–Alternative Standard Errors

Dependent variable	*Log poverty rate*						
Estimator	FE	FE	FE	FE	FE	LD	LD
Column	1	2	3	4	5	6	7
Palm oil land / district area	-0.007***	-0.007***	-0.007**	-0.007***	-0.007***	-0.012***	-0.012***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
District FEs	Y	Y	Y	Y	Y	N	N
Island–year FEs	Y	Y	Y	Y	Y	N	N
Island FEs	Y	Y	Y	Y	Y	Y	Y
Initial conditions controls	N	N	N	N	N	Y	Y
Standard errors	Normal	Robust	Prov-cluster	Bootstrap	Jackknife	Normal	Robust
Observations	3386	3386	3386	3386	3386	335	335

This table shows the main results in Table 2 are insensitive to the standard errors used. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year-2001 parent districts. Estimators are first difference (FD), within fixed effects (FE), and long difference (LD). Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level for panel estimators and at the island level for LDs. Initial conditions controls refer to log district per capita income and log poverty rate in 2000.

Table 8: Additional IV Estimates–Log Poverty Gap Index

LIML estimator	FE	LD	LD	LD	LD
Column	1	2	3	4	5
Palm oil land / district area	-0.021***	-0.016*	-0.021	-0.049*	-0.018**
	(0.007)	(0.008)	(0.015)	(0.026)	(0.008)
District and year FEs	Y	N	N	N	N
Island FEs	N	Y	Y	Y	Y
Initial conditions controls	N	Y	Y	Y	Y
Excluded instrument	Land-year	Land	Rain	Slope	All
Excluded-F	12.17	12.31	34.61	9.43	16.61
Observations	3051	335	275	274	274

This table shows that similar results observed for instrumental variable estimates of the impact of additional palm oil land on district poverty rates carry over to the depth of poverty, as measured by the poverty gap index. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level in Column 1 and at the province level in Columns 2–5. LIML refers to the limited information maximum likelihood instrumental variable estimator, FE fixed effects, and LD long-differences. Column 1 instruments palm oil land with the initial share of palm oil land interacted with the year dummy, Column 2 the share of district land used for palm oil in 2000, Column 3, district rainfall in 2000, Column 4 average district slope, and Column 5 uses all LD instruments. Initial condition controls are initial poverty rates, initial per capita incomes, and island dummies. Excluded-F refers to the Kleibergen-Paap Wald rk F statistic obtained from first-stage regressions.

Table 9: Additional Estimates–Palm Oil Production

Dependent variable	Log district poverty rate				Log district poverty gap index			
Estimator	FE	FE LIML	LD	LD LIML	FE	FE LIML	LD	LD LIML
Column	1	2	3	4	5	6	7	8
Per capita palm oil production (tons)	-0.212***	-0.594***	-0.195***	-0.427***	-0.241***	-0.813***	-0.262***	-0.565***
	(0.054)	(0.159)	(0.043)	(0.118)	(0.054)	(0.229)	(0.069)	(0.206)
District and year FEs	Y	Y	N	N	Y	Y	N	N
Island–year FEs	Y	N	N	N	Y	N	N	N
Initial conditions controls	N	N	Y	Y	N	N	Y	Y
Excluded-F statistic		8.03		12.01		8.3		12.01
Districts	341	341	335	274	341	341	335	274
Observations	3386	3386	335	274	3051	3051	335	274

This table shows the effects observed for additional palm oil land as a share of total district area carry over to actual palm oil production in tons, and converted to per capita terms to scale. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. LIML refers to the limited information maximum likelihood instrumental variable estimator, FE fixed effects, and LD long-differences.Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level in for FE estimate and at the province level for LDs. FE LIML instruments, and LD LIML the share of district land used for palm oil, district rainfall, and average district slope in 2000. Initial condition controls are initial poverty rates, initial per capita incomes, and island dummies. Excluded-F refers to the Kleibergen-Paap Wald rk F statistic obtained from first-stage regressions.

Table 10: Heterogeneity–By Region

Dependent variable: log district poverty rate			
Sample	Island	Island	All
Estimator	FE	LD	FE
Column	1	2	3
Panel A: Java			
Palm oil land / district area	0.015 (0.016)	-0.035 (0.047)	-0.007*** (0.002)
Island–palm interaction			0.021 (0.015)
N observations	1091	105	3386
Panel B: Sumatra			
Palm oil land / district area	-0.007*** (0.002)	-0.011*** (0.004)	-0.016*** (0.005)
Island–palm interaction			0.010** (0.005)
N observations	960	96	3386
Panel C: Kalimantan			
Palm oil land / district area	-0.007* (0.003)	-0.009** (0.004)	-0.006*** (0.001)
Island–palm interaction			-0.008** (0.004)
N observations	379	37	3386
Panel D: Sulawesi			
Palm oil land / district area	-0.05*** (0.008)	-0.05*** (0.016)	-0.006*** (0.002)
Island–palm interaction			-0.039*** (0.007)
N observations	450	45	3386
Panel E: Other islands			
Palm oil land / district area	-0.039 (0.130)	0.345** (0.171)	-0.007*** (0.002)
Island–palm interaction			-0.026 (0.128)
N observations	506	51	3386

This table provides consonant sub-samples estimates by region to supplement to the full-sample estimates with interaction terms presented in the paper. Stars denote statistical significance at the 10, 5, and 1 percent levels. Full sample (Column 3) is an annual 341 district panel, 2002–2010. Palm oil land is lagged one period (i.e., 2001–2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Estimators are the within estimator (FE) with district and year fixed effects, and the long-difference (LD) estimator with initial log poverty and log per capita income controls. Heteroskedasticity-robust standard errors are in parentheses, clustered at the district level.

Table 11: Heterogeneity–By Sector

Depvar: log poverty rate								
Panel width	Annual	4-yearly	Annual	4-yearly	Annual	4-yearly	Annual	4-yearly
Column	1	2	3	4	5	6	7	8
State-owned palm oil land	-0.0109**	-0.0111**					-0.00286	-0.00231
	(0.00423)	(0.00432)					(0.00499)	(0.00563)
Private palm oil land			-0.0122***	-0.0114***			-0.0121***	-0.00934**
			(0.00307)	(0.00349)			(0.00319)	(0.00436)
Smallholder palm oil land					-0.00360	-0.0106**	0.00169	-0.00362
					(0.00219)	(0.00449)	(0.00208)	(0.00562)
District and island-year FEs	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3009	1004	3009	1004	3009	1004	3009	1004

This table presents the same sectoral estimates as those in the paper, but additional Columns 7 and 8, which show that the poverty impacts of private palm oil land dominate those from other sectors when all sectors are included in the one regression. Palm oil land variables are all plantation area shares of total district areas. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticitiy-robust standard errors are in parentheses, clustered at the district level. Panel fixed effects estimator used throughout, with district, year, and island-year fixed effects.

Table 12: Heterogeneity–By Frontier and Mosaic Expansion

Dependent variable: log district poverty rate								
Sample	Full sample				Mosaic districts		Frontier districts	
Panel width	Annual		2-year	4-yearly	Annual			
Estimator	FE	FE	FE	FE	FE	LIML	FE	LIML
Column	1	2	3	4	5	6	7	8
Palm oil land/ district area	-0.007***	-0.006***	-0.009**	-0.007*	-0.008***	-0.030***	-0.003	-0.137**
	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.010)	(0.003)	(0.054)
Frontier interaction		-0.002	-0.012**	-0.012**				
		(0.004)	(0.005)	(0.005)				
Island-year FEs	Y	Y	Y	Y	Y	N	Y	N
Excluded-F						6.99		5.09
Observations	3386	3386	1692	1016	2761	2761	625	625

This table examines heterogeneity by forestry typologies categorizing the two distinct phases of palm oil expansion, ‘frontier” and “mosaic” expansions. Forest frontier expansion is characterized by low population densities, large-scale enterprises with government concessions, the conversion of recently logged forests to plantations and often a reliance on migrant labour. Mosaic expansions tend to occur in areas with higher population densities, few large blocks of land available for large enterprises, and mostly smallholder-based expansion. New developments are diffuse and patchwork in nature, with many farmers converting other types land into oil palm plantations (e.g., from rubber agroforestry, rubber monoculture, or rice fields). Frontier expansions typically occur before mosaic expansions, which take place after the frontier has “closed”. Using census population data, I identify typical frontier districts as those with a 2001 population density of less than 40 people per square kilometer, which is the minimum required to grow and cultivate oil palms (i.e., lower population densities require migrants labor). Columns 1–4 present estimates including an interaction term between palm oil land share and frontier districts, and Columns 5–8 split the full sample into ”mosaic” and ”frontier” districts. Frontier districts appear to have experienced more rapid poverty reduction, consistent with the regional results. Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticitiy-robust standard errors are in parentheses, clustered at the district level. FE refers to fixed effects and LIML limited information maximum likelihood instrumental variable estimator. LIML estimates instrument palm oil land with the initial share of palm oil land interacted with the year dummy, and Excluded-F refers to the Kleibergen-Paap Wald rf F statistic obtained from first-stage regressions.

Table 13: Heterogeneity–By Land Quality

Dependent variable: log district poverty rate									
Palm oil land quality		Damaged			Immature			Mature	
Panel width	Annual	2-yearly	4-yearly	Annual	2-yearly	4-yearly	Annual	2-yearly	4-yearly
Column	1	2	3	4	5	6	7	8	9
Palm oil land/ district area	-0.142**	-0.141**	-0.074	-0.025***	-0.025***	-0.035***	-0.014***	-0.014***	-0.011**
	(0.066)	(0.069)	(0.083)	(0.009)	(0.009)	(0.013)	(0.004)	(0.005)	(0.004)
Observations	3009	1674	1004	3009	1674	1004	3009	1674	1004

This table shows how increasing the share of district land used for damaged plantations has the largest short-term impacts on district poverty, tapering off over time. Immature and mature plantations have broadly consistent impacts over 4 years: the former slightly increases (perhaps from "learning by doing and the increased productivity from trees coming of age) and the latter slightly decreases (as poverty gains would have already been made). Stars denote statistical significance at the 10, 5, and 1 percent levels. Sample is an annual 341 district panel, 2002-2010. Palm oil land is lagged one period (i.e., 2001-2009). 2001 district boundaries are used, with new districts collapsed into year 2001 parent districts. Heteroskedasticitiy-robust standard errors are in parentheses, clustered at the district level. The within panel estimator with district and island-by-year fixed effects is used in all estimates.

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