

Resource Booms, Revenue Sharing, and Growth*

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Abstract

Using a natural experiment in Indonesia, we estimate the separate economic effects of natural resource booms and shared resource revenue. Contrary to Dutch disease concerns, oil and gas booms promote manufacturing growth, and shared revenue does not harm local manufacturing firms. Shared revenue significantly raises local non-oil GDP, but resource booms do not. Supply-side factors help explain the results: shared revenue increases local population and firm entry, while resource booms do not. Oil and gas booms thus benefit local economies largely through shared revenue. Where the revenue is spent matters more for local growth than where the resources are extracted.

JEL codes: H77, O13, O14, Q32, Q33

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1 Introduction

In over 30 countries spanning five continents, special rules govern the distribution of natural resource revenue to subnational governments. The majority employ a derivation-based system in which a portion of the revenue is distributed to the producing region and, potentially, adjacent areas.¹ Such revenue-sharing schemes aim to satisfy local demands to share in extraction benefits, compensate producing regions for environmental damage, and defuse resource-related conflicts (Bauer et al., 2016).

Evaluating revenue-sharing systems requires knowing both the effects of shared revenue on recipient jurisdictions and the direct effects of resource booms on producing regions.² For example, setting appropriate compensation through transfers requires knowing the direct benefits and harms of booms to local economies. More generally, their separate magnitudes inform policymakers' decisions about the level of extraction and the size of transfers.

Theoretical predictions for each channel are ambiguous. Resource booms could crowd out tradable manufacturing by driving up input costs—so-called Dutch disease (Corden and Neary, 1982)—or they could increase manufacturing productivity through agglomeration effects (Allcott and Keniston, 2018). Shared revenue could similarly drive up firm costs by stimulating local demand, but it could also promote growth by financing investments in human and physical capital. Separately identifying these two effects is challenging, however, because resource booms and shared revenue are inextricably linked in most contexts.³

This paper exploits a national reform in Indonesia to identify the separate economic effects of resource booms and shared resource revenue. Prior to 2001, Indonesia was highly centralized, and hydrocarbon-rich regions did not receive a disproportionate share of revenue from oil and gas extraction (Shah, Qureshi, Binder and Zou, 1994; World Bank, 1996). In 2001, the country implemented comprehensive decentralization reforms, including a revenue-sharing scheme that redirected a substantial portion of oil and gas revenue to producing districts and other districts within the same province. The reform permanently increased public expenditure in hydrocarbon-rich districts and their neighbors relative to hydrocarbon-poor districts. Whereas districts producing onshore oil and gas were exposed to resource booms and busts both before and after 2001, the fiscal windfall from shared revenue began only after the reform. The reform, coupled with specific features of the sharing rule, enables

¹Derivation-based distribution can be achieved through local taxation or intergovernmental grants. The four largest countries by population—and seven of the top 10—implement a derivation-based system for all or part of their mineral, oil, or gas revenues. See Bauer, Gankhuyag, Manley, Halling and Venugopal (2016) for the full list. A few countries instead use an indicator-based system, tying revenue to local population, poverty, or other characteristics.

²We define resource booms to include all resource-related activities induced by an increase in the resource price. In the case of oil and gas, this includes drilling new wells in proven fields and investing in exploration of unproven fields, as well as increased extraction.

³In a standard setting where subnational governments receive a fixed portion of the royalties from oil production within the jurisdiction, shared revenue and local oil production would be roughly proportional. For example, in Colombia (prior to 2012) and Brazil, royalties are allocated to producing municipalities and to municipalities through which oil and gas are transported.

us to separately identify the effects of resource booms and shared revenue.

We measure resource-related shocks as the interaction between current resource prices and predetermined endowments. For resource booms, we calculate the product of oil and gas prices and the district's onshore oil and gas endowments per capita. This measure uses only onshore endowments, as offshore production has minimal direct effects on local economies.⁴ To quantify the shared oil and gas revenue shock, we apply the statutory sharing rule to each district's physical endowments to obtain its per capita fiscal endowment, then interact this measure with resource prices and an indicator for the post-decentralization period. Because the rule allocates a portion of revenue from offshore production and from production elsewhere in the province to non-producing districts, many districts have substantial fiscal endowments despite no onshore production of their own.

In the first part of the analysis, we use manufacturing firm census data spanning 1975–2014 to estimate the effects of resource booms and fiscal windfalls on firm outcomes. We find that fiscal windfalls do not significantly affect output or productivity, but they do reduce wages. By contrast, oil and gas booms increase manufacturing output, productivity, and wages. While the wage effect of resource booms aligns with classic Dutch disease mechanisms, booms actually increase productivity—a finding that contradicts typical Dutch disease expectations. We show that firms connected to the oil and gas sector drive these results.

The second part of the analysis uses data on district GDP excluding oil and gas (termed “non-oil GDP”) from 1993 to 2013, constructed in part from archival government documents. These data enable us to examine aggregate effects on all non-oil sectors of the economy, which is crucial given that manufacturing accounts for only about 20 percent of GDP in Indonesia. We find that fiscal windfalls significantly boost non-oil GDP, but resource booms have zero impact. We reject the hypothesis of equal effects at the one percent level. The implied fiscal multiplier of shared revenue is about 1.5.

Supply-side mechanisms help explain the results. Fiscal windfalls significantly increase district population, while the effect of resource booms is virtually zero. A plausible reason is improved public amenities from increased local government spending. Windfalls also promote entry by manufacturing plants. While shared revenue did not benefit incumbent firms, it may have promoted overall manufacturing growth via firm entry. Entry by both firms and individuals helps explain the significant non-oil GDP response to shared revenue.

Taken together, the results suggest that resource booms benefit manufacturing while having zero aggregate impact on non-oil GDP. By contrast, fiscal windfalls stimulate aggregate economic growth, in part by attracting workers and encouraging firm entry. The local benefits of oil and gas booms in Indonesia come largely from shared revenue rather than spillovers from the oil and gas sector. This justifies compensatory transfers to oil-rich areas,

⁴The reason is that labor and intermediate goods for offshore production are unlikely to be sourced from the nearest coastal district. [Caselli and Michaels \(2013\)](#) and [Cavalcanti, Da Mata and Toscani \(2019\)](#) provide evidence that offshore production does not stimulate growth in non-resource sectors in Brazil. In Section 5.4 we test this assumption.

which do not automatically benefit—at least in aggregate terms—from extraction activity.

Our results are informative for how fiscal policy mediates the effects of resource price shocks in developing country settings. Where the revenue is spent matters more for local growth than where the resources are extracted. The results can thus inform policy in the many countries that currently share resource revenue or that are considering introducing such a system. Furthermore, as many countries have introduced or changed revenue-sharing rules in recent years, our identification strategy can be applied in other settings.⁵

A growing empirical literature investigates the economic impacts of natural resource abundance at the subnational level.⁶ Our paper contributes to this literature by estimating the separate effects of resource booms and shared revenue rather than their compound effect. We also make three specific contributions to the study of resource booms and shared revenue.

First, we estimate the impact of resource booms on non-resource GDP. Recent research examines the impacts of booms on manufacturing firms (e.g., [Cust et al., 2019](#); [Pelzl and Poelhekke, 2021](#)). This work tests important Dutch disease mechanisms, but its focus on manufacturing gives an incomplete picture of the economy. Manufacturing accounts for only around 20 percent of GDP in Indonesia and a smaller share in advanced economies. Other studies analyze the responses of wages, income, house prices, and employment ([Feyrer et al., 2017](#); [Allcott and Keniston, 2018](#); [Bartik et al., 2019](#)), but lack GDP data at the relevant level of analysis. We use district non-oil GDP data to quantify aggregate impacts across all non-oil sectors.

Second, we exploit a permanent resource revenue shock, in addition to transitory shocks. This distinction matters for output multipliers ([Baxter and King, 1993](#)). Permanent revenue shocks are more likely to induce lumpy infrastructure investments ([Cassidy, 2025](#)). Infrastructure spending, in turn, has a larger long-run multiplier than general government purchases ([Ramey, 2021](#)). Most prior work exploits transitory resource revenue shocks (e.g., [Olsson and Valsecchi, 2015](#); [Cust and Rusli, 2015](#); [Bancalari and Rud, 2024](#)) and thus identifies a different multiplier than ours. A notable exception is [Caselli and Michaels \(2013\)](#), who exploit persistent differences in oil revenue transfers and offshore production across Brazilian municipalities. Our study has two primary advantages relative to theirs: non-oil GDP data and panel variation in oil transfers and production.⁷ Our strategy identifies the long-run impact of introducing revenue sharing.

⁵Countries that have significantly reformed their resource revenue-sharing rules in recent decades include Bolivia (2007), Colombia (2012), the Democratic Republic of Congo (2018), Ghana (1993, 2016), Kyrgyzstan (2013), Mexico (2007, 2018), Mongolia (2013, 2016), Mozambique (2013, 2022), Nigeria (1999), Peru (2001–2004), the Philippines (1991), and Uganda (2003, 2022).

⁶See [Cust and Poelhekke \(2015\)](#) for a survey. Recent examples include [Michaels \(2011\)](#), [Aragón and Rud \(2013\)](#), [Jacobsen and Parker \(2014\)](#), [Loayza and Rigolini \(2016\)](#), [Allcott and Keniston \(2018\)](#), [Feyrer, Mansur and Sacerdote \(2017\)](#), [Cust, Harding and Vézina \(2019\)](#), [Cavalcanti et al. \(2019\)](#), [Bartik, Currie, Greenstone and Knittel \(2019\)](#), [Gradstein and Klemp \(2020\)](#), [Pelzl and Poelhekke \(2021\)](#), [Jacobsen, Parker and Winikoff \(2023\)](#), and [Benguria, Saffie and Urzua \(2024\)](#).

⁷Based on their estimates for total GDP and a back-of-the-envelope calculation, they conclude that shared revenue has no impact on non-oil GDP, in sharp contrast to our results.

Third, we illuminate mechanisms through which resource revenue stimulates the local economy. In Indonesia, fiscal windfalls lead to population increases and firm entry—supply-side changes that contribute to the multiplier alongside the usual demand-side channels. These outcomes are commonly examined in the resource-boom literature but rarely in work on resource revenue.⁸ The inflow of firms and workers into high-transfer districts is consistent with welfare gains from improved public amenities.

Our paper is closely related to four papers studying Indonesia. [Cust et al. \(2019\)](#) and [Pelzl and Poelhekke \(2021\)](#) estimate the effects of resource booms on manufacturing firms. Both estimate the combined effect of resource booms and fiscal windfalls, whereas we separately identify these two effects. We also use a longer manufacturing panel (1975–2014, as compared to 1990–2009), allowing us to exploit the late-1970s and early-1980s oil boom and bust.

Two unpublished papers estimate the economic effects of shared oil and gas revenue in Indonesia. [Olsson and Valsecchi \(2015\)](#) estimate the effects of shared revenue on non-oil GDP and other outcomes. [Cust and Rusli \(2015\)](#) estimate the effects of both shared revenue and onshore oil and gas production on non-oil GDP. We employ a different identification strategy from these papers. Instead of treating oil and gas revenue or production as exogenous, we use predetermined endowments interacted with resource prices as our exogenous shocks. As noted above, we also exploit the permanent revenue shock from the 2001 reform rather than transitory variation alone.⁹

A related literature examines the impact of resource revenue transfers on local public expenditure and public services. Recent works in this area include [Caselli and Michaels \(2013\)](#), [Borge, Parmer and Torvik \(2015\)](#), [Besfamille, Jorrat, Manzano, Quiroga and Sanguinetti \(2023\)](#), [Martínez \(2023\)](#), and [Cassidy \(2025\)](#). Our paper focuses on the impact of shared revenue on manufacturing output and economic growth, but considers changes in public service delivery as one potential mechanism.

The paper proceeds as follows. Section 2 provides background information on oil and gas production and revenue-sharing rules in Indonesia. Section 3 then summarizes the data construction. We describe our strategy for separately identifying the effects of resource booms and shared revenue in Section 4, and Section 5 discusses the results. Section 6 provides concluding remarks.

2 Institutional Background

Indonesia's economic growth has been closely tied to oil export revenues due to its oil dependence. By the early 1980s, the oil sector accounted for 78 percent of total exports, with oil company taxes comprising 70 percent of government revenue ([Arndt, 1983](#)). The importance

⁸An exception is [Caselli and Michaels \(2013\)](#), who find that municipal population does not respond to oil transfers in Brazil.

⁹We also use an extended subnational GDP series (1993–2013), constructed in part from archival reports. The GDP series in [Olsson and Valsecchi \(2015\)](#) spans 2001–2010, and the one in [Cust and Rusli \(2015\)](#) spans 1998–2009.

of oil and gas to the local economy varies dramatically across the country, as illustrated by the locations of Indonesia's 1,157 oil and gas fields in Panel (a) of Figure 2.

A sharp increase in the value of oil and gas production in the 1970s (Figure 1, Panel (a)) boosted oil and gas exports, and GDP grew by an average of 8 percent per year. Consequently, the World Bank reclassified Indonesia as a middle-income country by 1981, a stark contrast to its status as one of the world's poorest countries at the decade's start (Arndt, 1983). Despite steady oil production and growing gas production throughout the 1980s and 1990s, the value of production plummeted due to a collapse in the oil price.¹⁰ The country subsequently experienced another price-driven boom-and-bust cycle from 1998 to 2015. In hydrocarbon-rich areas, these price fluctuations are expected to have significant economic impacts, resulting in shocks of varying magnitudes across different regions.

Indonesia's political institutions changed dramatically over the analysis period. Policy-making was highly centralized under President Suharto's authoritarian rule from 1966 to 1998. The central government paid regional civil servant salaries via Autonomous Region Subsidies (*Subsidi Daerah Otonom*, or SDO), while earmarked President's Instruction grants (*Instruksi Presiden*, or INPRES) funded routine and development expenditures (World Bank, 1990). Regional spending was centrally administered with minimal local government input (Silver, Azis and Schroeder, 2001). Crucially, though oil revenue funded these expenditures, central priorities—not production locations—dictated regional allocations. For example, the massive 1973–1979 primary school construction program (INPRES *Sekolah Dasar*) was bankrolled by oil windfalls, but schools were allocated in proportion to the number of primary-aged unenrolled children in the district (Duflo, 2001).

After Suharto's resignation in 1998, Indonesia transitioned to democracy and implemented sweeping decentralization reforms. Law No. 25 of 1999, effective from 2001, granted significant autonomy over public expenditures to over 300 districts, bypassing the 30 larger provinces.¹¹ This strategy aimed to appease local interests and deter regional secession, a concern heightened by East Timor's 1999 independence and fears that resource-rich provinces might secede if fiscally self-sufficient.

To further mitigate secessionist risks, the central government established a revenue-sharing scheme with producing regions, fundamentally altering the distribution of resource revenue across Indonesia. The scheme allocates 15 percent of oil revenue to subnational governments: 3 percent to the provincial government, 6 percent to the producing district government, and 6 percent equally divided among the other district governments in the province. Gas revenue sharing is more generous, with 30 percent transferred to subnational governments: 6 percent to the province, 12 percent to the producing district, and 12 percent

¹⁰See Appendix Figures A.3, A.4, and A.5 for more details.

¹¹The Village Law (Law No. 6 of 2014), implemented in 2015, transferred some policymaking authority and fiscal resources from districts to villages. Our analysis concludes in 2014 to focus on the initial phase of decentralization.

equally divided among the other districts in the province.¹² The provincial governments of Aceh, Papua, and Papua Barat receive an additional 55 percent of oil revenue and 40 percent of gas revenue.¹³ For revenue-sharing purposes, the district nearest to the offshore field is designated as the “producing district” and receives the largest share of revenue,¹⁴ even though the labor and intermediate goods used for extraction are unlikely to be sourced from that district.

Decentralization introduced time-series variation in shared revenue distinct from fluctuations in resource production. While oil and gas production experienced boom and bust cycles both before and after decentralization, districts received no shared revenue until 2001. Subsequently, substantial fiscal transfers flowed into hydrocarbon-rich districts and their neighbors. Panel (b) of Figure 1 illustrates a significant increase in district government expenditures per capita following the 2001 decentralization, particularly in hydrocarbon-rich areas.

Decentralization also generated distinct cross-sectional variation in shared revenue and onshore production. Notably, many districts are entitled to substantial revenue transfers despite having little or no onshore production, due to either offshore endowments or the presence of hydrocarbons elsewhere in the province. Indonesia has a mix of onshore and offshore fields (Panel (a) of Figure 2), with 60 percent being onshore. The special sharing arrangements with Aceh, Papua, and Papua Barat provinces generate additional variation in shared revenue independent of local endowments.

From 2001, district expenditure was funded by multiple revenue sources. The primary funding source was the General Allocation Grant (*Dana Alokasi Umum*), an unconditional, non-matching grant allocated based on fiscal capacity and expenditure needs. Natural resource revenue, including oil, gas, timber, and minerals, constitutes the second largest funding source. The central government also distributes Special Allocation Grants (*Dana Alokasi Khusus*), earmarked transfers that comprise a minor portion of district budgets. Districts generate own-source revenue from license and utility fees and taxes on hotels and restaurants, averaging seven percent of their total revenue. An additional seven percent comes from the central government’s sharing of tax revenue collected from local firms and individuals.

From 1975 to 2014, the number of districts in Indonesia increased from 287 to 514, with most of the increase occurring after 1998. To maintain a consistent geographic unit of analysis,

¹²Starting in 2009, subnational governments received an additional 0.5 percent of oil and gas revenue: 0.1 percent to the province, 0.2 percent to the producing district, and 0.2 percent to other districts in the province. These funds were earmarked for education spending (Law No. 33 of 2004).

¹³Law No. 21 of 2001 granted these concessions to Papua (and later Papua Barat, after it was split off). Law No. 18 of 2001 (superseded by Law No. 11 of 2006) granted similar concessions to Aceh. Both arrangements were responses to armed conflicts in these regions. The additional revenue sharing began in 2002. The agreement with Aceh lasts indefinitely, while the agreement with Papua and Papua Barat runs through 2027, after which the additional oil revenue will decrease to 35 percent and gas revenue to 10 percent.

¹⁴Provided district territory extends to within four nautical miles of the field (Law No. 22/1999).

we aggregate variables to 1975 district borders, combining districts that amalgamated after 1975.¹⁵ This results in 280 geographic units. (See Appendix Section A.3 for details.)

3 Data

This section briefly describes our main variables and data sources. (See Appendix Section A.3 for details.) Appendix Table A.3 reports the summary statistics. Our sample includes all of Indonesia except for East Timor, which gained independence in 1999.

We use data from the Indonesian manufacturing census of large and medium-sized firms (*Survei Industri Besar/Sedang*, or IBS) spanning 1975–2014 (Central Bureau of Statistics, Indonesia, 1975–2014). IBS covers the universe of manufacturing plants with at least 20 employees. For simplicity, we use the terms “firm” and “plant” interchangeably, even though we cannot link plants belonging to the same firm. The dataset contains information on output, value added, wages, number of employees, 5-digit industry, legal status, district of operation, and other information. There are 76,194 separate plants in an unbalanced panel; plants are observed for an average of 9.1 years. For all firm-years, we construct a 2-digit industry code that maps onto the 1999–2009 coding scheme; it is similar to codes in ISIC rev. 3.

We construct a nearly balanced panel of district non-oil GDP from 2000 to 2013 using data from the Central Bureau of Statistics (*Badan Pusat Statistik*, or BPS) and the World Bank’s Indonesia Database for Policy and Economic Research (INDO-DAPOER). The series ends in 2013, because BPS stopped reporting district non-oil GDP. We extend the series back to 1993 by digitizing archival BPS reports. (See Appendix Section A.3.)

We measure exposure to resource-related shocks using data on resource prices and endowments. Data on oil prices and oil and gas endowments come from Rystad Energy’s proprietary UCube database (Rystad Energy, 2019).¹⁶ Indonesian liquefied natural gas (LNG) price data are from IndexMundi, which sources from World Gas Intelligence and the World Bank.¹⁷

Let E_d^{oil} and E_d^{gas} denote the onshore oil and gas endowments under the ground in district d in 1975, in millions of barrels of oil equivalent per capita based on 1980 population.¹⁸ We use oil and gas fields discovered by 1998, one year before the decentralization law was passed. This provides a fairly comprehensive measure of true endowments, while avoiding bias due to selective exploration in the post-decentralization period, when district governments more

¹⁵The GDP analysis is performed at the level of 1993 borders, as the GDP series starts in 1993.

¹⁶The UCube database has since been replaced by a similar service called Upstream Solution. UCube provided upstream information about oil and gas exploration and production at the field level in Indonesia (and globally) via the Cube Browser. Information is collected from company and government reports. When information is unavailable, analysts conduct research and make estimates. Well-level data is also available at additional cost. UCube was the basis for the EIA’s global upstream model (Rystad Energy, 2014).

¹⁷Because the Indonesian LNG price data are only available back to 1994, we use West Texas Intermediate (WTI) oil prices to backcast LNG prices for earlier years. (See Appendix Section A.3.)

¹⁸We use 1980 population because the data from the prior census (in 1971) are incomplete.

actively engaged with oil and gas companies. A less comprehensive measure based on fields discovered by 1975 would further avoid bias from exploration that responds to economic conditions, while potentially missing relevant cross-district variation in endowments. The results are very similar when we only use fields discovered by 1975. (See Section 5.)¹⁹

Define exposure to onshore shocks as the current value of predetermined onshore endowments,

$$\mathbf{P}'_t \mathbf{E}_d = P_t^{\text{oil}} E_d^{\text{oil}} + P_t^{\text{gas}} E_d^{\text{gas}},$$

in constant 2010 USD millions. We focus on the reduced-form impact of $\mathbf{P}'_t \mathbf{E}_d$, rather than using it as an instrument for onshore production, because we expect this variable to affect economic outcomes through channels other than resource production. For example, an increase in $\mathbf{P}'_t \mathbf{E}_d$ will lead to greater investment in exploratory drilling and an increase in demand for intermediate goods and labor.

We construct fiscal endowments of oil and gas, F_d^{oil} and F_d^{gas} , by applying the revenue-sharing formula to endowments, then dividing by 1980 population to express them in per capita terms. (See Appendix Section A.1 for detailed formulas.) Our baseline fiscal endowment variables assume provincial revenue is allocated (via spending) to districts proportionally to population. Our results are robust to alternative allocation assumptions.²⁰

We define exposure to fiscal shocks as

$$\mathbf{P}'_t \mathbf{F}_d \cdot 1(t \geq 1999),$$

where $\mathbf{P}'_t \mathbf{F}_d = P_t^{\text{oil}} F_d^{\text{oil}} + P_t^{\text{gas}} F_d^{\text{gas}}$. This measure captures the differential impact of decentralization on areas with larger claims on hydrocarbon endowments. We use 1999, the year the reform was passed, rather than 2001, when it was enacted, as anticipated future changes in government spending can affect current economic activity (Ramey, 2011).²¹

Panel (b) of Figure 2 plots fiscal endowment against onshore endowment using 2000 prices. The revenue-sharing rules generate substantial variation in fiscal endowments for a given onshore endowment value, aiding in the separate identification of resource-boom and shared-revenue effects.²²

Panel (b) of Figure 2 identifies two high-leverage observations: (1) Kabupaten Kutai, which has the largest fiscal endowment, and (2) Kabupaten Bengkalis, which has the largest onshore endowment. Because these districts could be influential in the results, we check robustness

¹⁹A third approach uses all fields discovered by 2014. This approach has two downsides. First, it introduces bias due to exploration in the post-decentralization period. Second, it incorporates irrelevant variation in endowments: a field discovered in 2010 likely will not produce oil or gas during the sample period, as the average production lag following giant field discoveries is 5.4 years (Arezki, Ramey and Sheng, 2016).

²⁰The results are similar when using two alternative allocation assumptions: (1) equal amounts to each district, and (2) greater amounts to producing districts following the national government's sharing formula. The results are also robust to applying the same sharing rule to Aceh, Papua, and Papua Barat as used in the rest of the country.

²¹As we show ahead, the results are similar when we use 2001 instead.

²²Appendix Figure A.6 plots the onshore, offshore, and fiscal shocks over time for every district.

to dropping them.

4 Identification Strategy

The Indonesian setting provides time-series and cross-sectional variation that allows us to disentangle the effects of resource booms and shared revenue. We identify the effects of resource booms by comparing responses to resource price changes across districts with different onshore endowments. We identify the effects of shared revenue primarily by comparing changes in outcomes after decentralization across districts with different fiscal endowments.

4.1 Manufacturing Firm Outcomes

We begin by estimating a model that relates firm-level outcomes to the two shocks. Let $Y_{i,d,t}$ be an outcome (e.g., real output) in year t for firm i in district d . Additionally, let $j(i)$ denote the industry of firm i , and $r(d)$ the region of district d .²³ The model is

$$\begin{aligned} \ln Y_{i,d,t} = & \sum_{k=0}^K \beta_k \mathbf{P}'_{t-k} \mathbf{E}_d + \sum_{k=0}^K \delta_k \mathbf{P}'_{t-k} \mathbf{F}_d \cdot 1(t-k \geq 1999) \\ & + \mathbf{X}'_d \boldsymbol{\pi}_t + \alpha_i + \phi_{j(i),t} + \lambda_{r(d),t} + \varepsilon_{i,d,t}, \end{aligned} \quad (1)$$

which includes firm fixed effects (α_i), industry-by-year effects ($\phi_{j(i),t}$), and region-by-year effects ($\lambda_{r(d),t}$). The firm fixed effects absorb time-invariant differences in firm outcome levels, while the industry-by-year and region-by-year effects absorb industry-specific shocks and regional shocks, respectively. The model also controls for the time-varying effects of baseline district covariates, \mathbf{X}_d , which we discuss ahead.

We estimate static ($K = 0$) and dynamic ($K = 3$) versions of the model. Allowing for dynamic effects is potentially important, as firms may take multiple years to adjust to shocks, and district governments may require several years to translate transfers into public services such as infrastructure. The dynamic model captures the lagged effects of shocks.

According to Equation (1), a USD 1 million per capita increase in the onshore endowment value raises the outcome by $100 \cdot \beta_0$ log points in the same year. Therefore, a more realistic increase of USD 10,000 per capita raises the outcome by about β_0 percent.²⁴ The cumulative four-year impact of a permanent USD 10,000 per capita increase in the onshore endowment value is about $\sum_{k=0}^3 \beta_k$ percent. Similarly, the immediate and cumulative impacts of a permanent USD 10,000 per capita increase in the fiscal endowment value after decentralization are δ_0 and $\sum_{k=0}^3 \delta_k$ percent, respectively.

The magnitudes of β_k and δ_k are not directly comparable. Each coefficient is a reduced-form effect that operates through a different channel. An increase in the onshore endowment value affects the local economy through greater investment and employment in the oil and

²³We use the seven regions defined by the Central Bureau of Statistics: Sumatera, Jawa, Nusa Tenggara, Kalimantan, Sulawesi, Maluku, and Papua.

²⁴Over 1999–2014, the onshore endowment value per capita averaged USD 10,800 (SD: USD 86,010), and the fiscal endowment value per capita averaged USD 6,510 (SD: USD 24,410) (Appendix Table A.3).

gas sector, as well as spillovers to other industries via factor price changes and agglomeration. By contrast, an increase in the fiscal endowment value is associated with greater local public spending. Nevertheless, in Section 5 we provide guidance for comparing the effects of typical onshore and fiscal shocks.

Equation (1) is analogous to a difference-in-differences model, with endowments measuring exposure to a national shock and prices quantifying the magnitude of the shock. Accordingly, the key identifying assumption for $\{\beta_k\}$ and $\{\delta_k\}$ is that firms in the same industry and region but located in districts with different endowments would have followed parallel trends, on average, in the absence of resource price changes or the introduction of revenue-sharing.²⁵ Under this assumption, which implies strict exogeneity, the fixed-effects estimator is consistent for the slope parameters. We report standard errors clustered at the district level.

One way the identifying assumption could be violated is if endowments were correlated with other district characteristics that matter for economic dynamics. Appendix Table A.4 reports the cross-sectional relationship between the endowment variables and baseline geographic and demographic characteristics. Naturally, both endowments are positively correlated with land area, but the relationship is statistically significant only for onshore endowments. Districts with larger onshore endowments tend to have flatter terrain and larger populations, while districts with larger fiscal endowments tend to have more forestland, greater coal formation area, smaller populations, a greater share of working-age adults, and coastal access. We reject the hypothesis that the endowment coefficients are jointly zero at the five percent level for all geographic characteristics except coal formation area. By contrast, we fail to reject this hypothesis for all demographic characteristics except log population.

To address this concern, our baseline specification controls for the time-varying effects of district characteristics X_d that are correlated with at least one of the endowment variables: log land area, terrain slope, forestland, coastal access, coal formation area per capita in 1980, log population in 1980, and population share aged 15–64 in 1980 (Appendix Table A.4). We show ahead that the results are robust to controlling for all baseline covariates.

Another potential threat to our design is that other national policy changes starting around 1999 could differentially affect districts with different endowments. Central government fiscal policy and other features of decentralization are two such potential confounders. We discuss each in turn.

First, the change in central government spending at the district level around 2001 could be correlated with fiscal endowments. The fiscal shock variable is meant to be proportional to the change in *total* government spending—national and subnational—induced by decentralization, as this is what matters for economic outcomes. As discussed above, the fiscal

²⁵See Goldsmith-Pinkham, Sorkin and Swift (2020) and Christian and Barrett (2023) for further discussion of identification in these settings. See also Allcott and Keniston (2018) for a discussion of parallel trends in the context of local resource shocks in the United States.

endowment is positively correlated with the change in district government spending around 2001 (Figure 1, Panel (b)). This correlation will equal the correlation with the *total* spending change as long as the central government spending change is uncorrelated with the fiscal shock. (See Appendix Section A.2 for details.)

We cannot estimate this correlation because we lack geographically disaggregated central government spending data. We do find, however, that the fiscal endowment is correlated with two of five proxies for baseline central expenditure (Appendix Table A.4). Moreover, the two endowment variables are jointly significant at the five percent level for three of five proxies. While a correlation between endowments and the *level* of central spending does not necessarily cause bias, it could portend problems if the central spending change around 2001 is systematically related to its previous level. Therefore, as a further robustness check, we control for the baseline central expenditure proxies interacted with year dummies. We also report robustness checks controlling for the district poverty rate in 1999 interacted with year dummies, as the central government spending change could plausibly be related to poverty through changes in spending priorities.²⁶

Second, other aspects of decentralization could differentially affect districts with different endowments. Two salient aspects are district splitting and intergovernmental competition. As discussed in Section 2, the number of districts in Indonesia increased dramatically over the sample period, with most district splits occurring after 1998. Previous research has shown that splits harm economic growth (Cassidy and Velayudhan, 2026). If the propensity to split is correlated with endowments, our estimates could be biased. We therefore perform robustness checks controlling for splits in different ways.

Decentralization also may have promoted intergovernmental competition, whereby districts compete for mobile capital. Districts with onshore endowments plausibly have weaker incentives to compete because of the geographic immobility of firms linked to the oil and gas industry (Pepinsky and Wihardja, 2011). Districts without onshore endowments therefore may have adopted more business-friendly policies (e.g., less red tape and corruption) than their counterparts with onshore oil and gas. We address this concern by estimating the effect of the fiscal shock on the subsample of districts with no onshore endowments.

A final threat is that Equation (1) could be misspecified because it assumes that onshore shocks have the same effect before and after decentralization, and that offshore shocks have no direct economic effects. If onshore shocks have heterogeneous effects over time—perhaps because of changes in extraction technology—then the estimated impact of the fiscal shock over 1999–2014 might simply reflect this heterogeneity. In addition, if offshore booms directly affect the economy of the nearest coastal district, the estimated effects of the fiscal shock could reflect the impact of offshore booms in addition to the impact of shared revenue. We show below that the results are robust to relaxing these functional form assumptions.

²⁶District-level poverty rates are unavailable in 1980, unlike the other demographic covariates. To our knowledge, BPS first reported district-level poverty rates in 1999. See Appendix Section A.3 for details.

While the parallel trends assumption is not testable, we can examine its plausibility by estimating outcome gradients in the endowment variables over time periods when the shocks were minimal or nonexistent. Oil and gas prices were fairly stable over 1991–1998 (Appendix Figure A.3), so the outcome gradient in the onshore endowment should be roughly constant over this period. Similarly, the outcome gradient in the fiscal endowment should be constant prior to the announcement of revenue-sharing in 1999.

We estimate an event-study specification to examine firm trends around the passage of the decentralization law. The model is

$$\ln Y_{i,d,t} = \sum_{s \neq 1998} \beta_s \overline{\mathbf{P}}' \mathbf{E}_d \cdot 1(t = s) + \sum_{s \neq 1998} \delta_s \overline{\mathbf{P}}'_* \mathbf{F}_d \cdot 1(t = s) + \mathbf{X}'_d \boldsymbol{\pi}_t + \alpha_i + \phi_{j(i),t} + \lambda_{r(d),t} + \varepsilon_{i,d,t}, \quad (2)$$

where $\overline{\mathbf{P}}$ is the vector of average oil and gas prices over 1975–2014, and $\overline{\mathbf{P}}'_*$ is the vector of average oil and gas prices over the decentralization period, 2001–2014. The omitted reference period is 1998. The coefficient β_s represents the change in the gradient in onshore endowment between 1998 and year s . Similarly, δ_s captures the change in the gradient in fiscal endowment between 1998 and year s . Finding $\beta_s = \delta_s = 0$ for $s < 1998$ would indicate that firms located in districts with different exposure to onshore and fiscal shocks were on similar trends prior to the introduction of revenue sharing and subsequent price shocks.

To estimate the parameters in Equation (2), we focus on the period 1993–2014 and only include firms that were observed in 1998, the base year for differences. This sample (about 15,000 firms) is smaller than that used for Equation (1) (about 52,000 firms).²⁷ This is unsurprising, as the average firm is only observed for 9.1 years.

4.2 District Non-Oil GDP

Finally, we estimate the impacts of resource booms and revenue sharing on non-oil GDP. This allows us to study the effects on the entire non-oil sector of the economy. The firm-level analysis may miss important effects of resource booms and fiscal shocks for two reasons. First, manufacturing accounts for only about 20 percent of GDP in Indonesia, and impacts could vary by sector. Second, the manufacturing sector may produce innovation spillovers for the rest of the economy.

We estimate the impacts on non-oil GDP using the model

$$\ln Y_{d,t} = \sum_{k=0}^K \beta_k \mathbf{P}'_{t-k} \mathbf{E}_d + \sum_{k=0}^K \delta_k \mathbf{P}'_{t-k} \mathbf{F}_d \cdot 1(t - k \geq 1999) + \mathbf{X}'_d \boldsymbol{\pi}_t + \alpha_d + \lambda_{r(d),t} + \varepsilon_{d,t}, \quad (3)$$

where $Y_{d,t}$ is non-oil GDP in district d in year t and α_d is a district fixed effect.²⁸ We analyze

²⁷The total sample size is around 203,000 compared to 482,000 for the baseline regression.

²⁸In the GDP analysis, \mathbf{E}_d and \mathbf{F}_d are based on endowments under the ground in 1993, the first year of the sample.

non-oil GDP trends by shock exposure using the model

$$\ln Y_{d,t} = \sum_{s \neq 1998} \beta_s \overline{P}' E_d \cdot 1(t=s) + \sum_{s \neq 1998} \delta_s \overline{P}'_* F_d \cdot 1(t=s) + X'_d \pi_t + \alpha_d + \lambda_{r(d),t} + \varepsilon_{d,t}, \quad (4)$$

where \overline{P}' and \overline{P}'_* are defined as in Equation (2). The sample is a nearly balanced panel of 288 districts from 1993 to 2013.²⁹

5 Results

5.1 Onshore Production and District Expenditure

We first present quasi-first-stage results to aid in the interpretation of the reduced-form estimates. We consider the effect of increasing the value of the (onshore or fiscal) endowment per capita by USD 10,000. For onshore shocks, USD 10,000 is slightly above the mean and equal to about 0.12 standard deviations. For fiscal shocks, this amount represents more than twice the mean and about 0.45 standard deviations (Table A.3).

Panel (a) of Figure 3 shows that a USD 10,000 increase in the value of the onshore endowment per capita is associated with a USD 114 increase in the value of onshore oil and gas production per capita, or about 0.08 standard deviations. Similarly, Panel (b) of Figure 3 shows that a USD 10,000 increase in the value of the fiscal endowment per capita is associated with a USD 72 increase in district expenditure per capita, or about 0.17 standard deviations. Column 1 of Table 1 displays the cumulative effects of the shocks on total expenditure based on Equation (3). A permanent USD 10,000 increase in the value of the fiscal endowment per capita raises total district expenditure per capita by USD 88.³⁰

5.2 Comparing Shock Magnitudes

While the quasi-first-stage results provide context for interpreting the effects of onshore and fiscal shocks in isolation, they do not facilitate direct comparison of the two shocks. One may want to know whether the direct economic effects of resource booms matter more or less than the indirect effects via spending of shared revenue. However, it would be inappropriate to simply compare β_k with δ_k , because a USD 10,000 increase in the onshore endowment means something different than a USD 10,000 increase in the fiscal endowment. To avoid this problem, we instead compare the effects of *typical* shocks observed in the data. We quantify typical shocks using two approaches.

First, consider the ratio $P'_t F_d / (P'_t E_d)$ for districts with $E_d > 0$. This ratio gives the fiscal shock per USD of onshore shock in a given district. In the subsample of districts with onshore endowments, the median of this ratio over 1999–2014 is 0.36. This means that for the median

²⁹Only two districts (at 1993 borders) have incomplete non-oil GDP data. The BPS documents only report total GDP for Kabupaten Halmahera Tengah over 1993–1996, and the reported non-oil GDP for Kabupaten Bekasi over 1993–1995 excludes its child district, Kota Bekasi, which became autonomous in 1996. We could not find any GDP figures for Kota Bekasi over 1993–1995.

³⁰The estimates are similar when omitting high-leverage districts (Appendix Table A.5).

district, the fiscal endowment value increases by USD 3,600 for every USD 10,000 increase in the onshore endowment value. (The ratio is quite stable over time because oil and gas prices are highly correlated.) This suggests we should compare the onshore shock coefficient to 0.36 times the fiscal shock coefficient.

The second approach compares one-standard-deviation increases in the endowment values. The standard deviation of $P'_t F_d$ over 1999–2014 is 0.28 times the standard deviation of $P'_t E_d$ over the same period (Appendix Table A.3). Therefore, to examine whether one-standard-deviation increases in each endowment value have equivalent effects, we would compare the onshore shock coefficient to 0.28 times the fiscal shock coefficient.

The idea behind both approaches is that because a USD 10,000 increase represents a proportionally larger shock to the fiscal endowment than to the onshore endowment, the fiscal coefficient must be scaled down to enable meaningful comparisons. For simplicity, we choose a round number between the two ratios—namely, 0.3—and report estimates and standard errors for $\beta - 0.3 \cdot \delta$, where $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$.

5.3 Manufacturing Outcomes

Panel A of Table 2 presents the fixed-effects estimates from the static version ($K = 0$) of Equation (1). A USD 10,000 per capita increase in the onshore endowment value raises firm output by 0.45 percent (S.E. = 0.09 percent) (column 1). The impacts on value added and the number of workers are small and statistically insignificant (columns 2 and 3). Output per worker and value added per worker increase by 0.51 percent (S.E. = 0.07 percent) and 0.15 percent (S.E. = 0.08 percent), respectively (columns 4 and 5). These responses are consistent with agglomeration effects (Allcott and Keniston, 2018). Wages also rise by 0.26 percent (S.E. = 0.06 percent) (column 6). Overall, the estimates suggest that resource booms increase local labor demand and drive up input costs, while simultaneously improving firm productivity.

The estimated effects of the fiscal shock are less precise. There is little evidence that fiscal windfalls significantly increase firm output or productivity. In fact, output per worker and average wages both decline in response to the fiscal shock, and these effects are statistically significant. A USD 10,000 per capita increase in the fiscal endowment value reduces output per worker by 2.32 percent (S.E. = 1.08 percent) and average wages by 3.85 percent (S.E. = 0.63 percent). These estimates are surprising, as one might expect the fiscal shock to drive up wages via increased demand. Alternatively, increased spending could improve local amenities and attract workers to the region, thereby reducing wages. We explore this possibility in Section 5.9.

Overall, onshore shocks and fiscal shocks have contrasting effects. Onshore shocks increase labor productivity and wages, while fiscal shocks reduce them. Furthermore, typical onshore and fiscal shocks have statistically distinguishable effects on these outcomes.

Panel B of Table 2 presents fixed-effects estimates based on the dynamic version ($K = 3$) of Equation (1). While the effects of the shocks do appear to be dynamic, the cumulative

effects are qualitatively similar to the static estimates, albeit larger in absolute magnitude in some cases. For example, the cumulative impact of the onshore shock on output per worker is 0.78 percent (S.E. = 0.08 percent), compared to 0.51 percent in the static specification. For firm output, output per worker, and wages, we reject equal effects of typical onshore and fiscal shocks.

Figure 4 plots estimates of $\{\beta_s\}$ and $\{0.3 \cdot \delta_s\}$ from Equation (2) to visualize firm trends before and after decentralization and the subsequent rise in oil and gas prices. The figure also displays oil and gas prices, as well as the years when the decentralization law was passed and enacted, to highlight the sources of time variation in the shock variables. The gradient in onshore endowment per capita is roughly constant before 1999 for all outcomes, then rises for firm output, labor productivity, and wages as resource prices rise. Similarly, the gradient in fiscal endowment per capita is roughly constant prior to decentralization for all outcomes. After decentralization, the wage gradient falls. The graphical evidence is consistent with the results in Table 2.

5.4 Manufacturing Outcomes: Robustness

The Online Appendix reports results from several robustness checks.

Appendix Table A.6 shows that the results are very similar when we define the first year of the fiscal shock to be 2001, the year decentralization was enacted. Appendix Tables A.7 and A.8 display the estimates after omitting high-leverage districts. Dropping Kutai yields broadly similar estimates. Dropping Bengkalis increases the absolute magnitudes of the estimates while reducing their precision. The conclusion that onshore shocks benefit firms more than fiscal shocks is robust to the exclusion of high-leverage districts.

The results are also similar when we use endowment variables based only on fields discovered by 1975 (Appendix Table A.9). This is reassuring, as it suggests that subsequent discoveries were unrelated to local economic trends, eliminating one possible source of bias in the baseline estimates.

Appendix Tables A.10 and A.11 display estimates controlling for additional baseline covariates interacted with year effects in the static and dynamic specifications, respectively. In Panel A, we control for 11 geographic and demographic covariates, while in Panel B, we control for baseline central government expenditure proxies. The regressions in Panel C add both sets of covariates. In Panel D, we omit all covariates. The results are similar to the baseline estimates. Similarly, the results remain similar when we control for the district poverty rate in 1999 or omit the poorest districts from the sample (Appendix Table A.12).

Appendix Tables A.13 and A.14 report estimates that control for district splits in three ways: controlling for separate post-split dummies for the first district split and the first split in the post-decentralization period (1999 and later) (Panel A), controlling for the log number of districts contained within original district borders (Panel B), and controlling for a dummy for having ever split over 1975–2014 interacted with year effects (Panel C). In all cases, the

results are similar to the baseline estimates.

Next we examine whether the estimated effects of the fiscal shock are biased because decentralization had differential effects on districts with onshore endowments compared to those without. Appendix Table A.15 reports estimates of $\{\delta_k\}$ when omitting districts with any onshore endowments. The estimates for output, value added, number of workers, and value added per worker all increase, though their standard errors are large and only the effect for number of workers is statistically significant. The estimates for output per worker and average wage are similar to the baseline estimates. While the imprecision of the estimates makes it difficult to draw firm conclusions, this robustness check suggests the baseline estimates may have underestimated the effect of the fiscal shock on firm employment.

To see whether our estimate of δ_0 is biased by neglecting heterogeneity in β_0 over time, we interact $P'_t E_d$ with indicators for the pre- and post-decentralization periods. Panel A of Appendix Table A.16 presents the results. The impacts of onshore booms before and after 1999 are statistically distinguishable from each other for only two out of six outcomes: we reject equality for number of workers ($p = 0.015$) and average wage ($p = 0.027$). More importantly, the estimates of δ_0 do not change much, indicating that our preferred estimates are not meaningfully biased by neglected heterogeneity in the effect of onshore shocks over time.

To test whether offshore booms bias our estimates, we control for $P'_t E_d^{\text{off}}$, where E_d^{off} is offshore endowment per capita. The results, presented in Panel B of Table A.16, suggest that offshore booms are not an important confounder. The estimated coefficient on $P'_t E_d^{\text{off}}$ is statistically insignificant for five out of six outcomes, and the estimates of β_0 and δ_0 remain similar to the baseline estimates.

As a final robustness check, we re-estimate the coefficients using alternative versions of the fiscal endowment variable. These versions make different assumptions about how provinces allocate their shared revenue to districts via spending. The results remain consistent across various allocation assumptions (Appendix Figure A.8).

5.5 Manufacturing Outcomes: Heterogeneity

We also examine if firm responses depend on whether the firm's sector is connected to the oil and gas industry. We classify sectors as connected or unconnected based primarily on input-output tables constructed by Indonesia's Central Bureau of Statistics (Central Bureau of Statistics, Indonesia, 2005). We identify sectors 23 (Fuel industry, oil and coal refining, petroleum products, and nuclear fuel), 24 (Chemicals and chemical products), 27 (Basic metal), and 28 (Metal goods, not machinery and equipment) as most connected to the oil and gas industry. (See Appendix Section A.3 for details.)

We estimate the model

$$\begin{aligned} \ln Y_{i,d,t} = & (1 - C_{j(i)}) \cdot \left(\sum_{k=0}^K \beta_k^N P'_{t-k} E_d + \sum_{k=0}^K \delta_k^N P'_{t-k} F_d \cdot 1(t-k \geq 1999) \right) \\ & + C_{j(i)} \cdot \left(\sum_{k=0}^K \beta_k^C P'_{t-k} E_d + \sum_{k=0}^K \delta_k^C P'_{t-k} F_d \cdot 1(t-k \geq 1999) \right) \\ & + \alpha_i + \phi_{j(i),t} + \lambda_{r(d),t} + \varepsilon_{i,d,t}, \end{aligned} \quad (5)$$

where $C_{j(i)}$ is an indicator equal to one if industry j is connected to the oil and gas industry. We are interested in the hypotheses $H_0: \sum_{k=0}^K \beta_k^N = \sum_{k=0}^K \beta_k^C$ and $H_0: \sum_{k=0}^K \delta_k^N = \sum_{k=0}^K \delta_k^C$.

Panel A of Table 3 presents our main heterogeneity results. We find that onshore shocks have different effects depending on whether the firm is connected to the oil and gas industry. Connected firms experience larger increases in output, value added, output per worker, value added per worker, and average wages. They also see a decline in employment, while unconnected industries do not. The differences are statistically significant for employment, output per worker, and value added per worker. These results suggest that oil-connected firms are driving the main firm-level estimates in Table 2.

By contrast, fiscal shocks do not differentially affect connected firms. This makes sense, as we do not have strong ex-ante reasons to expect that fiscal effects would flow to a particular set of industries. While governments might use fiscal windfalls to further stimulate oil-connected industries—or support struggling unconnected industries—we find evidence of neither.

In Panel B of Table 3, we add sector 25 (Rubber and plastic products) to the set of oil-connected industries. Although our method of identifying oil-connected industries does not suggest that this industry is connected, we consider its inclusion because plastic is largely derived from oil.³¹ (See Appendix Section A.3 for details.) When including “Rubber and plastic products” as a connected industry, many differences are attenuated, which is consistent with our data-driven classification of this industry as unconnected.

5.6 Non-Oil GDP

Table 4 presents the non-oil GDP results based on Equation (3) for different lag specifications. To summarize the dynamic impacts, we focus on the cumulative effect of permanent onshore and fiscal shocks. Across all specifications, the impact of onshore shocks on non-oil GDP is virtually zero. By contrast, the impact of fiscal shocks is large and statistically significant, with the estimated cumulative effect growing somewhat as more lags are included. According to the model with $K = 3$ (column 4), a permanent USD 10,000 per capita increase in the fiscal endowment value raises non-oil GDP by 5.64 percent (S.E. = 1.56 percent).

³¹However, it is likely that industry relationships between oil and plastic production are complicated by the fact that oil is traded globally at low cost. There may be limited cost savings in the plastics industry associated with being located near oil production. We also note that a boom in oil due to higher prices would likely drive up input costs in the plastics industry.

In contrast to the manufacturing firm results, typical fiscal shocks raise non-oil GDP by more than typical onshore shocks. For example, when $K = 3$ (column 4), an onshore shock of USD 10,000 reduces non-oil GDP by 0.03 percent (S.E. = 0.24 percent), while a fiscal shock of USD 3,000 raises non-oil GDP by 1.69 percent (S.E. = 0.47 percent). The difference is statistically significant at the one percent level. When fewer lags are included, the corresponding difference is statistically significant at the five percent level.

Figure 5 plots the event-study estimates based on Equation (4). The gradient in the onshore endowment is nearly constant in all years both before and after decentralization and the subsequent rise in resource prices. This is consistent with the zero effects of onshore shocks found in Table 4. The gradient in the fiscal endowment increases gradually after the introduction of revenue sharing, then sharply in 2006 when shared revenue and district expenditure increased again. Again, this pattern is consistent with the large positive estimates in Table 4.

The gradient in the fiscal endowment also shows a slight upward trend from 1993 to 1995 before leveling off over 1995–1998. While this pretrend is statistically insignificant, it is worth considering whether preexisting trends drive the results. Appendix Figure A.7 plots the event-study estimates after adding additional controls or dropping districts that experienced large spikes in non-oil GDP due to either administrative reclassification or large-scale mine openings. The results show that the pretrend in the fiscal endowment is entirely driven by Kabupaten Berau, which saw the opening of major coal mines in 1992, 1995, and 2001. Dropping this district has virtually no impact on the post-1999 gradient, leading us to conclude that preexisting trends do not drive the estimates for fiscal shocks.

5.7 Non-Oil GDP: Robustness

We next subject the non-oil GDP results to several robustness checks. The results are very similar when we use 2001 as the first year of the fiscal shock (Appendix Table A.17), omit high-leverage districts (Appendix Table A.18), base the endowment variables on fields discovered by the start of the sample in 1993 (Appendix Table A.19), or change the covariate set (Appendix Table A.20). Controlling for poverty in 1999 also does not meaningfully change the results (Appendix Table A.21), nor does controlling for district splits (Appendix Table A.22). Omitting districts with onshore endowments slightly increases the estimated effect of the fiscal shock (Appendix Table A.23).

Changing our assumptions about how provinces distribute their shared revenue across districts induces some meaningful variation in the estimates (Appendix Figure A.10). Assuming that provinces distribute funds equally across districts (as opposed to in proportion to population) increases the estimate for onshore shocks somewhat and decreases the estimate for fiscal shocks. When we instead assume that provinces mimic the national sharing formula, giving more funds to oil-rich districts, the estimate for the onshore shock does not change while the estimate for the fiscal shock falls somewhat. Importantly, while the gap between

the fiscal and onshore effects narrows in both scenarios, fiscal shocks continue to have larger effects. By contrast, all three alternative measures that give Aceh and Papua the same formula weights as the rest of the country cause the gap between the fiscal and onshore estimates to widen. Overall, the conclusion that fiscal shocks increase non-oil GDP while onshore shocks do not remains robust to changing assumptions about revenue allocation.

5.8 Fiscal Multiplier

Next we connect our results to the literature on subnational fiscal multipliers. These studies exploit regional spending shocks to identify an “open economy relative multiplier” (Nakamura and Steinsson, 2014), which differences out national fiscal and monetary policy. Such relative multipliers can provide a lower bound for the deficit-financed, no-monetary-policy-response national multiplier (Chodorow-Reich, 2019).

Let $Y_{d,t}$ denote non-oil GDP, and let $G_{d,t}$ denote total district expenditure. We estimate cumulative multipliers (Ramey, 2016) using the specification

$$\sum_{j=0}^h y_{d,t+j} = \delta_h \sum_{j=0}^h g_{d,t+j} + \mathbf{X}'_{d,t} \beta + \lambda_{r(d),t} + \varepsilon_{d,t,h}, \quad (6)$$

where

$$y_{d,t+j} = \frac{Y_{d,t+j} - Y_{d,t-1}}{Y_{d,1998}}, \quad g_{d,t+j} = \frac{G_{d,t+j} - G_{d,t-1}}{Y_{d,1998}}.$$

We apply the same transformation to the fiscal shock, expressing it as a cumulative change over h years relative to 1998 non-oil GDP.³² We then use the transformed fiscal shock as an instrument for $\sum_{j=0}^h g_{d,t+j}$. The regression controls for the onshore shock, transformed in the same manner. (Differencing eliminates district fixed effects.)

Table 5 presents the results. The on-impact multiplier ($h = 0$) is 1.20 (S.E. = 0.60). This is statistically distinguishable from 0 but not from 1 according to the weak-IV-robust Anderson and Rubin (1949) test. The multiplier grows over time, peaking at 1.55 (S.E. = 0.77) at $h = 3$, but remains statistically indistinguishable from 1. The estimates are similar when we omit high-leverage districts (Appendix Table A.24). These multipliers are somewhat smaller than the median multiplier of 1.9 in the papers surveyed by Chodorow-Reich (2019), though we lack sufficient precision to reject equality.

5.9 Mechanisms

So far we have shown that onshore shocks increase the output, productivity, and wages of manufacturing firms without promoting aggregate economic growth, while fiscal shocks raise manufacturing wages and significantly increase overall growth. We next test for potential

³²We scale by non-oil GDP in the year before decentralization (1998), rather than in the initial year (1993), because two districts (Halmahera Tengah and Kabupaten Bekasi) are missing non-oil GDP data for 1993–1995. The results are similar when we scale by 1993 non-oil GDP and omit these districts.

mechanisms behind these results.

Population Growth

As mentioned earlier, the fiscal shock could reduce wages through improvements in local amenities that attract workers to the region. Table 6 shows that the fiscal shock increases district population while the onshore shock does not. According to the model with $K = 3$ (column 4), a permanent USD 10,000 per capita increase in the fiscal endowment value increases population by 1.36 percent (S.E. = 0.38 percent). The corresponding estimate for the onshore shock is virtually zero.³³ The results suggest that the fiscal shock raised non-oil GDP while lowering manufacturing wages in part by increasing local labor supply. The population effects could reflect changes in fertility and mortality in addition to migration, however.

Firm Entry

Improvements in amenities from the fiscal shock could also attract firms. This mechanism could promote aggregate growth without the fiscal shock increasing the output of incumbent firms. To examine firm entry and exit, we estimate exponential-mean models of the number of manufacturing plants, entries, and exits at both the market (district-industry) and district level.³⁴ The results in Table 7 suggest that the fiscal shock increases entry whereas the onshore shock does not. Although the fiscal shock did not benefit incumbent firms, as shown by the within-firm estimates, it may have promoted manufacturing growth overall via firm entry.

Other Features of Decentralization

The onshore and fiscal shocks could also affect economic outcomes through their interaction with other features of decentralization. District splitting was one of the most prominent such features. As already shown, our results are robust to controlling for splits in various ways. However, if endowments or shocks affect the propensity to split, splitting could be a mechanism behind our results.

Appendix Table A.25 reports estimates of the cross-sectional relationship between endowments and splitting (columns 1–2) and the dynamic relationship between the shocks and the propensity to split for the first time in a given year (column 3). Across all specifications, both endowments are uncorrelated with splitting, and we fail to reject equal effects of comparably sized onshore and fiscal shocks on the probability of splitting. Using the estimates in column 2, a one-standard-deviation increase in the onshore endowment is associated with a 0.027 higher probability of splitting after decentralization (S.E. = 0.063), and a one-standard-deviation increase in the fiscal endowment is associated with a 0.030

³³The regressions in Table 6 control for all geographic and demographic covariates, to avoid overestimating the effect of the fiscal shock on population. Panel (a) of Appendix Figure A.11 shows that when only the baseline covariates are included, the gradient in the fiscal endowment increases over 1980–1990 before becoming constant over 1990–1998. Including all covariates virtually eliminates these pretrends while modestly reducing the post-1999 gradient (Panel (b)).

³⁴Some “entries” could reflect firm growth, as the sample only includes firms with at least 20 employees (Section 3).

higher probability (S.E. = 0.033).³⁵ The 95-percent confidence intervals are $[-0.096, 0.150]$ for the onshore shock and $[-0.035, 0.095]$ for the fiscal shock. Thus, while we cannot reject the hypothesis that splitting is uncorrelated with endowments, we also cannot rule out economically meaningful correlations. Splitting is a potential mechanism, though it appears less likely than the previous two.

6 Conclusion

In this paper, we exploit the introduction of resource revenue sharing in Indonesia to estimate the separate effects of resource booms and shared resource revenue on local economies. Most prior studies estimate only the compound effect. Disentangling the two effects is important for designing resource revenue-sharing systems.

We use plant-level manufacturing data from 1975 to 2014 and district-level non-oil GDP data from 1993 to 2013. We identify the effects of resource booms from the differential response of economic outcomes to resource price changes across districts with varying onshore endowments. We identify the effects of shared revenue from the differential change in outcomes after the introduction of resource revenue sharing across districts with varying endowments relevant to the revenue-sharing formula.

We find that onshore oil and gas booms increase manufacturing wages, yet manufacturing output and productivity still grow. These effects are strongest in sectors that are upstream or downstream of the oil and gas industry. The results are inconsistent with the more severe manifestations of Dutch disease. However, broader positive spillovers are absent: onshore booms have no impact on non-oil GDP.

By contrast, shared oil and gas revenue does not significantly affect manufacturing output or productivity, but it does reduce wages. Shared revenue also raises non-oil GDP. The effects of onshore and fiscal shocks on non-oil GDP are statistically distinguishable. Supply-side factors help explain the results: fiscal shocks increase local population and firm entry, possibly because of improvements in local amenities, but onshore shocks do not.

Taken together, our results imply that most of the local benefits of oil and gas booms in Indonesia operate through their effect on shared revenue. Oil-rich areas are not automatically compensated for extraction externalities through economic growth. Any compensation that policymakers feel these regions are owed—for environmental or other reasons—must be delivered through the intergovernmental transfer system.

Research on Brazil has reached opposite conclusions: [Caselli and Michaels \(2013\)](#) find that shared oil revenue has no effect on non-oil sectors or population, and [Cavalcanti et al. \(2019\)](#) find that onshore oil booms have positive spillovers to the services sector. Identifying the features of these settings that drive the different findings, and expanding the evidence

³⁵First convert the standard deviations reported in Appendix Table A.3 to USD millions by dividing by 1,000, then multiply by the average marginal effect reported in Appendix Table A.25. The results are similar when we formally calculate the change in the splitting probability from increasing the endowment from zero to its standard deviation.

base to other countries, are priorities for future research. Our identification strategy can be applied to other countries that have introduced, eliminated, or modified their revenue-sharing rules over time. For example, in 2012 Colombia switched from a derivation-based system to an indicator-based system for sharing oil royalties, shifting revenue away from oil-producing areas.³⁶

Declaration of Generative AI and AI-Assisted Technologies in the Writing Process

During the preparation of this work the authors used Claude in order to edit the document. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

³⁶Álvarez, Castañeda and Hofstetter Gascón (2025) exploit this reform to estimate the effect of shared revenue on local taxes.

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7 Tables

Table 1: District Expenditure

	District Expenditure by Function, Scaled by 1980 Population									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Admin.	Edu.	Infra.	Health	Agri.	Env.	Law & Order	Social Prot.	Tourism & Culture
Onshore Endow. \times Price (β)	-0.51** (0.26)	-0.20** (0.10)	-0.14** (0.06)	-0.14* (0.08)	-0.07** (0.03)	-0.05** (0.02)	0.01 (0.01)	-0.02*** (0.01)	-0.01** (0.00)	-0.01 (0.00)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	8.80*** (1.03)	2.95*** (0.40)	1.99*** (0.24)	2.48*** (0.41)	0.98*** (0.15)	0.51*** (0.11)	0.16*** (0.04)	0.17*** (0.03)	0.14*** (0.01)	0.07*** (0.01)
$\beta - 0.3 \cdot \delta$	-3.15*** (0.50)	-1.08*** (0.19)	-0.73*** (0.12)	-0.88*** (0.19)	-0.37*** (0.07)	-0.20*** (0.05)	-0.04** (0.02)	-0.07*** (0.01)	-0.05*** (0.01)	-0.03*** (0.01)
Observations	5,588	5,437	5,438	5,430	5,420	4,729	5,199	4,517	4,896	4,901
Districts	275	275	275	275	275	275	275	275	275	275

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (3) for $K = 3$, where the outcomes are district expenditure on different functions. Expenditure is expressed in constant 2010 USD thousands and scaled by 1980 population. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the change in spending in USD due to a USD 1 thousand increase in the value of the endowment. The sample covers 1994–2014. The five districts in Jakarta are missing, as these districts are managed by the province. Most districts are missing law and order expenditure for 2003–2004, and all districts are missing agriculture expenditure for 2013–2014. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Firm Outcomes

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Static Model (K = 0)</i>						
Onshore Endow. \times Price (β)	0.45*** (0.09)	0.05 (0.10)	-0.06 (0.06)	0.51*** (0.07)	0.15* (0.08)	0.26*** (0.06)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-1.09 (1.28)	-0.47 (1.47)	1.23 (0.87)	-2.32** (1.08)	-1.82 (1.23)	-3.85*** (0.63)
$\beta - 0.3 \cdot \delta$	0.78* (0.45)	0.19 (0.51)	-0.43 (0.31)	1.21*** (0.37)	0.69* (0.42)	1.41*** (0.23)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278
<i>Panel B: Dynamic Model (K = 3)</i>						
Onshore Endow. \times Price	0.12 (0.09)	-0.18* (0.11)	-0.14*** (0.05)	0.26*** (0.08)	-0.01 (0.09)	0.14*** (0.05)
Lag 1	0.16** (0.06)	0.29*** (0.11)	0.01 (0.03)	0.15*** (0.06)	0.29*** (0.10)	0.07 (0.05)
Lag 2	-0.01 (0.09)	-0.17* (0.09)	0.10*** (0.02)	-0.11 (0.08)	-0.27*** (0.09)	-0.09 (0.06)
Lag 3	0.50*** (0.09)	0.25*** (0.09)	0.02 (0.03)	0.48*** (0.08)	0.24*** (0.08)	0.22*** (0.05)
Sum of Lags 0-K (β)	0.78*** (0.11)	0.19* (0.11)	-0.01 (0.07)	0.78*** (0.08)	0.24*** (0.09)	0.35*** (0.07)
Fiscal Endow. \times Price $\times (t \geq 1999)$	-2.93* (1.67)	-3.80* (2.08)	2.13*** (0.75)	-5.06*** (1.37)	-6.01*** (1.73)	-2.59*** (0.66)
Lag 1	1.76 (1.61)	0.88 (2.12)	-0.17 (0.39)	1.93 (1.48)	1.01 (1.94)	-2.16** (0.84)
Lag 2	3.70** (1.81)	4.66** (1.96)	-1.03** (0.43)	4.73*** (1.63)	5.73*** (1.85)	-0.15 (1.25)
Lag 3	-3.26* (1.90)	-0.83 (1.71)	-0.04 (0.49)	-3.22* (1.67)	-0.84 (1.48)	1.00 (1.03)
Sum of Lags 0-K (δ)	-0.73 (1.52)	0.91 (1.71)	0.90 (0.99)	-1.63 (1.30)	-0.12 (1.50)	-3.90*** (0.90)
$\beta - 0.3 \cdot \delta$	0.99* (0.53)	-0.08 (0.59)	-0.28 (0.35)	1.27*** (0.45)	0.28 (0.51)	1.52*** (0.33)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1). Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Firm Outcomes: Heterogeneity by Oil-Connectedness

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Baseline Measure of Oil-Connectedness</i>						
Onshore Endow. × Price						
× Not Connected (β^N)	0.75*** (0.11)	0.16 (0.11)	0.00 (0.07)	0.75*** (0.09)	0.20** (0.09)	0.34*** (0.07)
× Connected (β^C)	1.59** (0.77)	1.19* (0.70)	-0.47** (0.23)	2.06*** (0.60)	1.69*** (0.55)	0.64** (0.30)
Fiscal Endow. × Price × ($t \geq 2001$)						
× Not Connected ($0.3 \cdot \delta^N$)	-0.20 (0.46)	0.32 (0.51)	0.25 (0.30)	-0.45 (0.40)	0.02 (0.45)	-1.17*** (0.27)
× Connected ($0.3 \cdot \delta^C$)	-0.39 (1.54)	-0.20 (1.23)	0.50 (0.60)	-0.88 (1.36)	-0.73 (1.18)	-1.12 (0.79)
p -value: $\beta^N = \beta^C$	0.291	0.149	0.050	0.031	0.006	0.295
p -value: $\delta^N = \delta^C$	0.903	0.641	0.635	0.755	0.496	0.946
<i>Panel B: Measure Adding Rubber and Plastic Industry</i>						
Onshore Endow. × Price						
× Not Connected (β^N)	0.77*** (0.11)	0.18 (0.11)	0.00 (0.07)	0.76*** (0.08)	0.22** (0.09)	0.33*** (0.07)
× Connected (β^C)	0.59 (0.43)	0.26 (0.42)	-0.47*** (0.16)	1.06*** (0.41)	0.75* (0.43)	0.55*** (0.21)
Fiscal Endow. × Price × ($t \geq 2001$)						
× Not Connected ($0.3 \cdot \delta^N$)	-0.10 (0.48)	0.39 (0.51)	0.38 (0.34)	-0.48 (0.39)	-0.03 (0.43)	-1.13*** (0.27)
× Connected ($0.3 \cdot \delta^C$)	-1.07 (0.99)	-0.58 (0.74)	-0.44 (0.40)	-0.63 (1.02)	-0.17 (0.80)	-1.50*** (0.51)
p -value: $\beta^N = \beta^C$	0.690	0.839	0.001	0.471	0.205	0.283
p -value: $\delta^N = \delta^C$	0.313	0.048	0.014	0.881	0.813	0.440
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of $\beta^N = \sum_{k=0}^3 \beta_k^N$, $\beta^C = \sum_{k=0}^3 \beta_k^C$, $0.3 \cdot \delta^N = 0.3 \cdot \sum_{k=0}^3 \delta_k^N$, and $0.3 \cdot \delta^C = 0.3 \cdot \sum_{k=0}^3 \delta_k^C$ in Equation (5). In Panel (a) we define oil-connected industries as sectors 23 (fuel, oil and coal refining, petroleum products, and nuclear fuel), 24 (chemicals and chemical products), 27 (basic metal), 28 (metal goods, not machinery and equipment), and 29 (machinery and equipment). In Panel (b) we add sector 25 (rubber and plastic products industry) to the set of oil-connected industries. (See Appendix Table A.1.) Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Log Non-Oil GDP

	Log Non-Oil GDP			
	(1)	(2)	(3)	(4)
Onshore Endow. \times Price	-0.01 (0.27)	-0.11 (0.23)	-0.11 (0.25)	-0.11 (0.30)
Lag 1		0.12 (0.11)	0.11 (0.10)	0.10 (0.10)
Lag 2			0.00 (0.12)	0.06 (0.07)
Lag 3				-0.09 (0.20)
Sum of Lags 0- K (β)	-0.01 (0.27)	0.01 (0.27)	-0.00 (0.27)	-0.03 (0.24)
Fiscal Endow. \times Price \times ($t \geq 1999$)	4.85*** (1.38)	3.39*** (0.79)	2.98*** (0.75)	2.36*** (0.77)
Lag 1		1.62** (0.79)	0.20 (0.60)	0.24 (0.59)
Lag 2			2.11*** (0.73)	0.93** (0.36)
Lag 3				2.12** (0.86)
Sum of Lags 0- K (δ)	4.85*** (1.38)	5.01*** (1.44)	5.29*** (1.50)	5.64*** (1.56)
$\beta - 0.3 \cdot \delta$	-1.46** (0.61)	-1.50** (0.64)	-1.59** (0.66)	-1.72*** (0.67)
Observations	6,041	6,041	6,041	6,041
Districts	288	288	288	288

Notes: This table presents fixed-effects estimates of the coefficients in Equation (3) for $K \in \{0, 1, 2, 3\}$. The sample covers 1993–2013. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Fiscal Multiplier

	Cumulative Multiplier after h Years			
	$h = 0$ (1)	$h = 1$ (2)	$h = 2$ (3)	$h = 3$ (4)
Multiplier (δ_h)	1.20** (0.60)	1.39* (0.72)	1.52** (0.76)	1.55** (0.77)
AR p -value: $\delta_h = 1$	0.704	0.520	0.385	0.349
AR p -value: $\delta_h = 0$	0.024	0.031	0.032	0.039
First-stage F -statistic	5.8	7.9	6.3	5.5
Observations	5,263	4,952	4,642	4,333
Districts	283	283	283	283

Notes: This table presents instrumental-variables estimates of δ_h in Equation (6). The instrument is $P_t' F_d \cdot 1(t \geq 1999)$, expressed as a cumulative change over h years relative to 1998 non-oil GDP. All regressions control for $P_t' E_d$, expressed using the same transformation. Reported p -values are from the [Anderson and Rubin \(1949\)](#) test, which is robust to the presence of weak instruments. The first-stage F -statistic is the [Kleibergen and Paap \(2006\)](#) rk statistic. The sample covers 1994–2013. The five districts in Jakarta are missing, as these districts are managed by the province. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Log Population

	Log Population			
	(1)	(2)	(3)	(4)
Onshore Endow. \times Price	-0.01 (0.06)	0.04 (0.10)	0.03 (0.10)	0.04 (0.10)
Lag 1		-0.05 (0.05)	-0.02 (0.04)	-0.02 (0.04)
Lag 2			-0.04* (0.02)	0.00 (0.02)
Lag 3				-0.05 (0.04)
Sum of Lags 0- K (β)	-0.01 (0.06)	-0.01 (0.06)	-0.02 (0.06)	-0.03 (0.06)
Fiscal Endow. \times Price \times ($t \geq 1999$)	1.23*** (0.38)	0.54 (0.43)	0.50 (0.43)	0.44 (0.48)
Lag 1		0.75** (0.30)	0.44* (0.26)	0.45* (0.27)
Lag 2			0.39** (0.17)	0.18 (0.13)
Lag 3				0.28 (0.29)
Sum of Lags 0- K (δ)	1.23*** (0.38)	1.29*** (0.38)	1.32*** (0.39)	1.36*** (0.38)
$\beta - 0.3 \cdot \delta$	-0.38** (0.15)	-0.40*** (0.15)	-0.42*** (0.15)	-0.44*** (0.14)
Observations	9,800	9,800	9,800	9,800
Districts	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (3) for $K \in \{0, 1, 2, 3\}$, controlling for all baseline characteristics in Appendix Table A.4. The sample is at level of 1975 district borders and covers 1980–2014. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

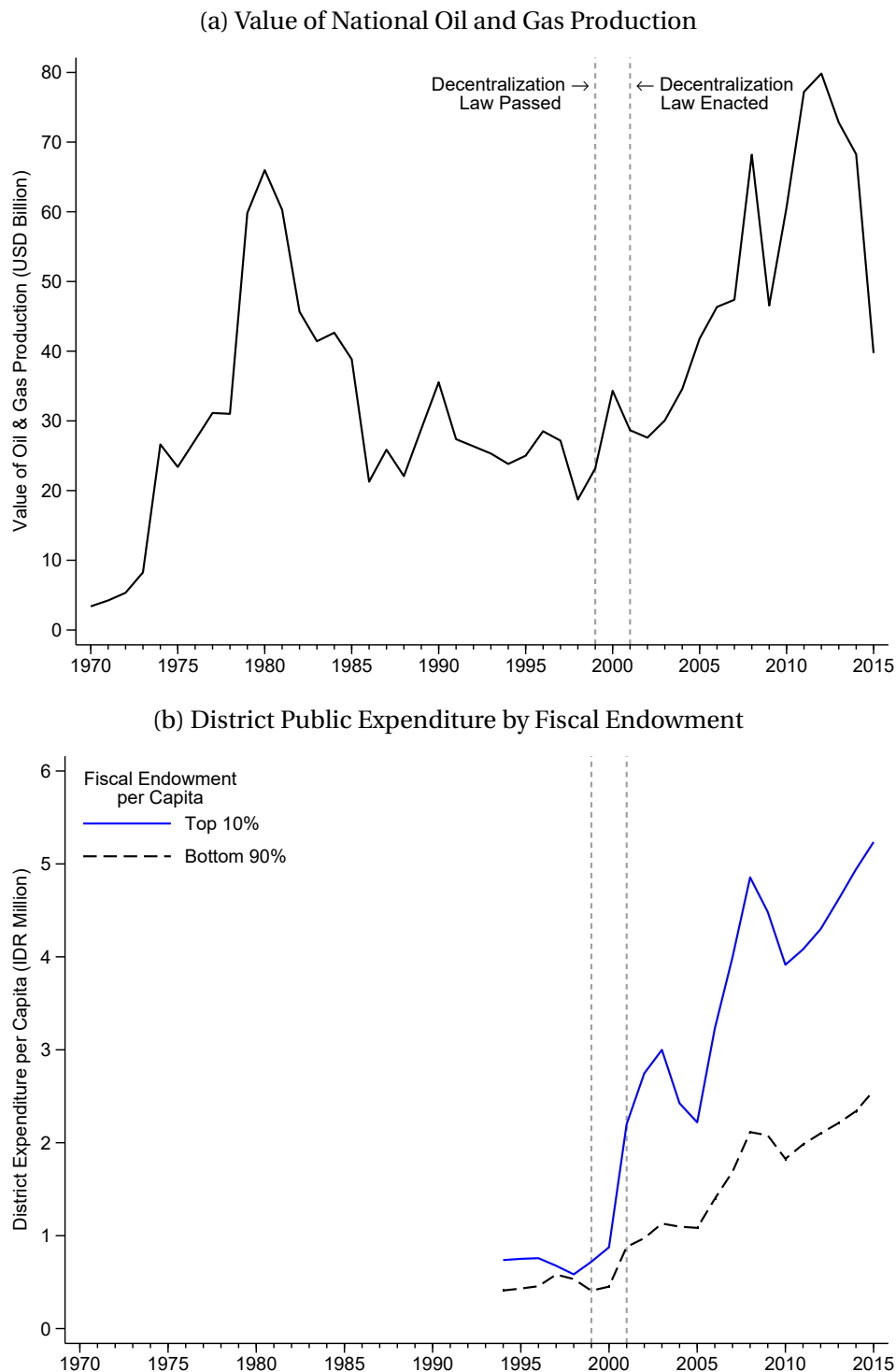
Table 7: Firm Entry and Exit: Dynamic Model

	E(Outcome \mathbf{X}) = $\exp(\mathbf{X}'\boldsymbol{\theta})$		
	(1) Plants	(2) Entries	(3) Exits
<i>Panel A: Market Level</i>			
Onshore Endow. \times Price (β)	-0.24 (0.31)	-0.48** (0.21)	0.20 (0.29)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	3.41 (2.92)	5.85*** (1.87)	-2.43 (2.61)
$\beta - 0.3 \cdot \delta$	-1.26 (1.18)	-2.24*** (0.72)	0.93 (1.03)
Observations	116,207	102,412	93,918
Markets	2,935	2,709	2,535
Districts	280	279	280
<i>Panel B: District Level</i>			
Onshore Endow. \times Price (β)	-0.31 (0.33)	-0.34 (0.26)	0.26 (0.29)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	3.56 (3.08)	5.74*** (1.90)	-3.14 (2.53)
$\beta - 0.3 \cdot \delta$	-1.38 (1.25)	-2.06*** (0.73)	1.20 (0.99)
Observations	11,186	10,702	10,658
Districts	280	279	280

Notes: Panel A presents estimates of $\beta = \sum_{k=0}^3 \beta_k$ and $\delta = \sum_{k=0}^3 \delta_k$ in the model $E(Y_{m,d,t} | \mathbf{X}) = \exp(\sum_{k=0}^3 \beta_k \mathbf{P}'_{t-k} \mathbf{E}_d + \sum_{k=0}^3 \delta_k \mathbf{P}'_{t-k} \mathbf{F}_d \cdot 1(t-k \geq 1999) + \alpha_m + \phi_{j(m),t} + \lambda_{r(d),t})$, where m indexes markets (district-industry pairs). Panel B presents estimates of $\beta = \sum_{k=0}^3 \beta_k$ and $\delta = \sum_{k=0}^3 \delta_k$ in the model $E(Y_{d,t} | \mathbf{X}) = \exp(\sum_{k=0}^3 \beta_k \mathbf{P}'_{t-k} \mathbf{E}_d + \sum_{k=0}^3 \delta_k \mathbf{P}'_{t-k} \mathbf{F}_d \cdot 1(t-k \geq 1999) + \alpha_d + \lambda_{r(d),t})$. Estimates are obtained via fixed-effects Poisson quasi-maximum likelihood. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

8 Figures

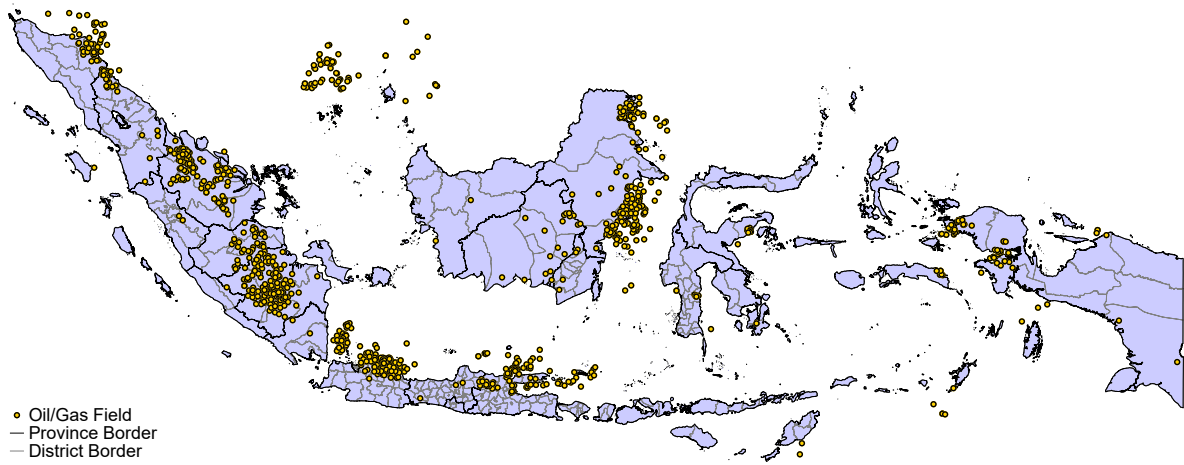
Figure 1: Resource Booms and Revenue Sharing



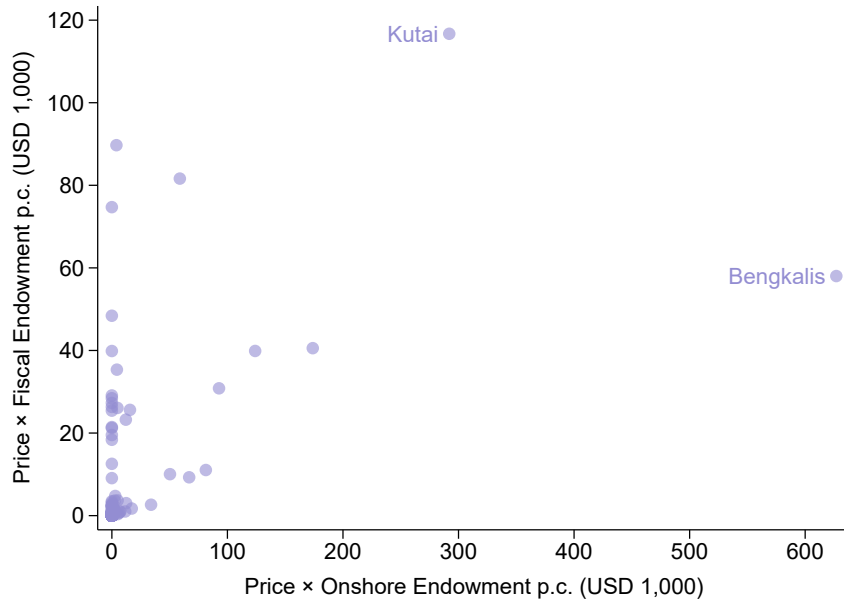
Notes: Panel (a) plots the total value of oil and gas production, in constant 2010 USD billions. Panel (b) plots average expenditure per capita for groups of districts defined by their fiscal endowment per capita. Public expenditure is expressed in constant 2010 IDR millions per capita.

Figure 2: Geographic Variation in Hydrocarbon Endowments

(a) Oil and Gas Fields of Indonesia

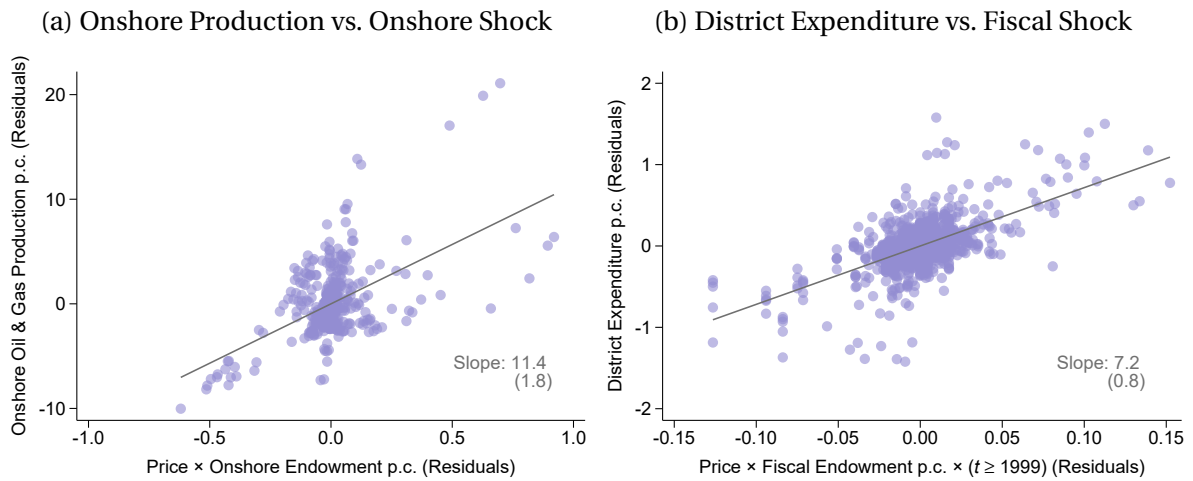


(b) Onshore Endowment vs. Fiscal Endowment



Notes: Panel (a) displays the locations of oil and gas fields along with 1975 province and district borders. Panel (b) plots the fiscal endowment per capita against the onshore endowment per capita. Both variables are based on 2000 oil and gas prices (in constant 2010 USD thousands) and 1980 population.

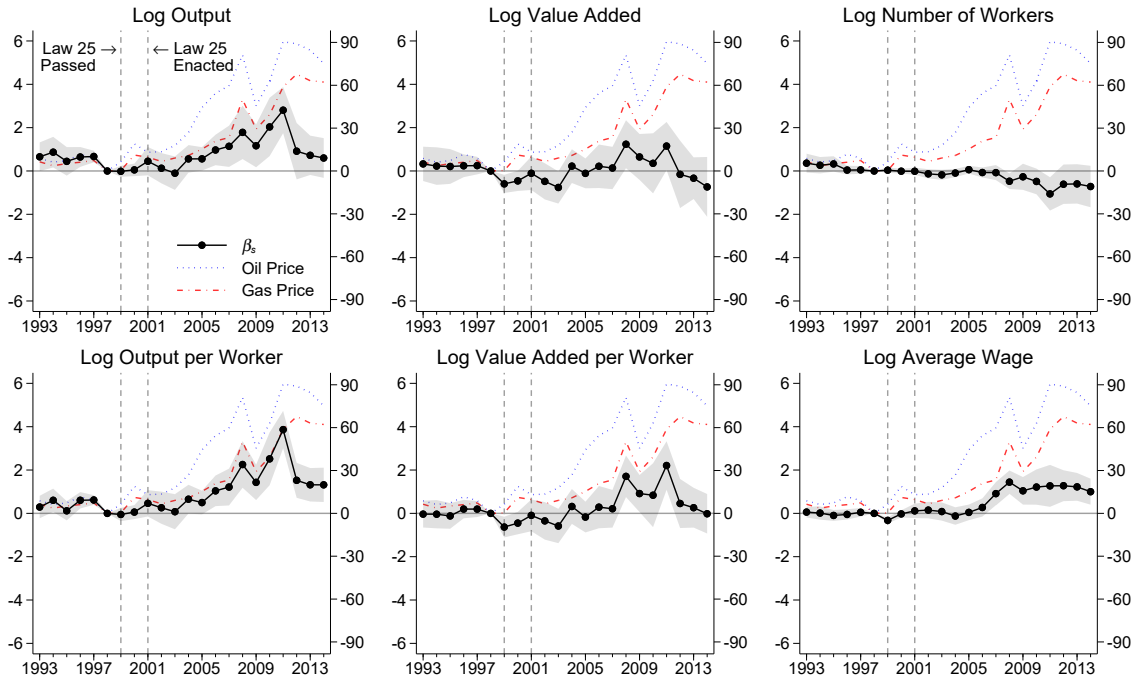
Figure 3: Relationship Between Shocks, Onshore Production, and District Expenditure



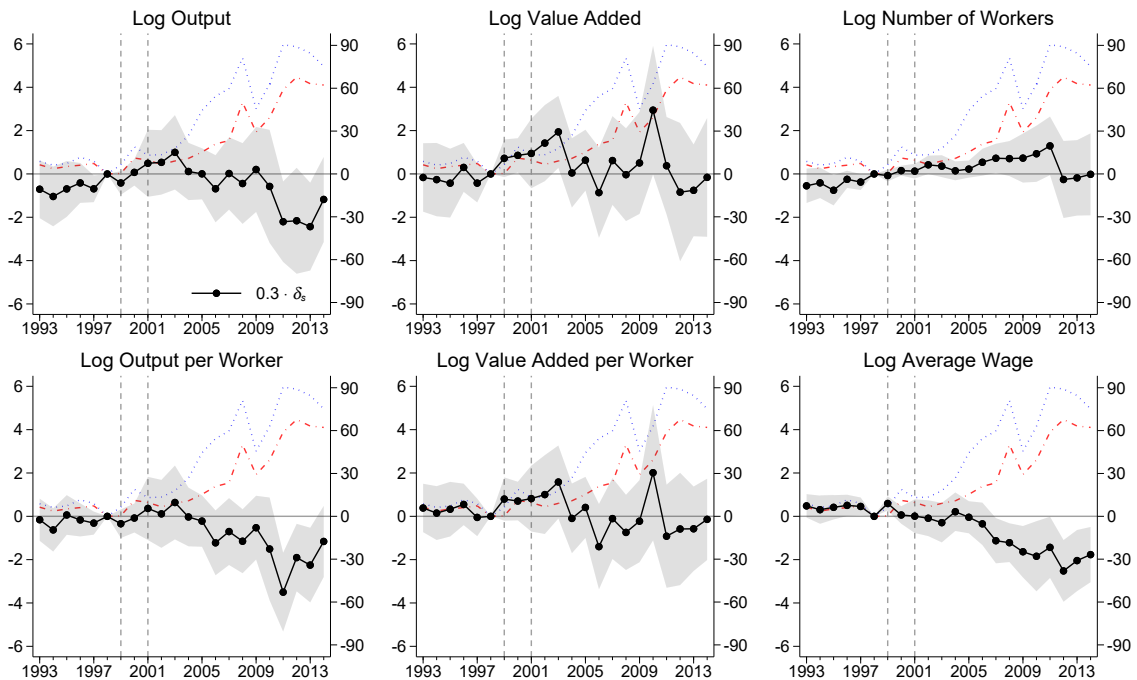
Notes: This figure plots residuals from regressing each variable on district and region-by-year dummies. The sample in Panel (a) covers 1975–2014, and the sample in Panel (b) covers 1994–2014. Onshore oil and gas production and district expenditure are expressed in constant 2010 USD thousands per capita, and each endowment × price variable is measured in constant 2010 USD millions per capita, all based on 1980 population.

Figure 4: Event Study: Firm Outcomes

(a) Year-by-Year Gradient in Onshore Endowment Relative to 1998

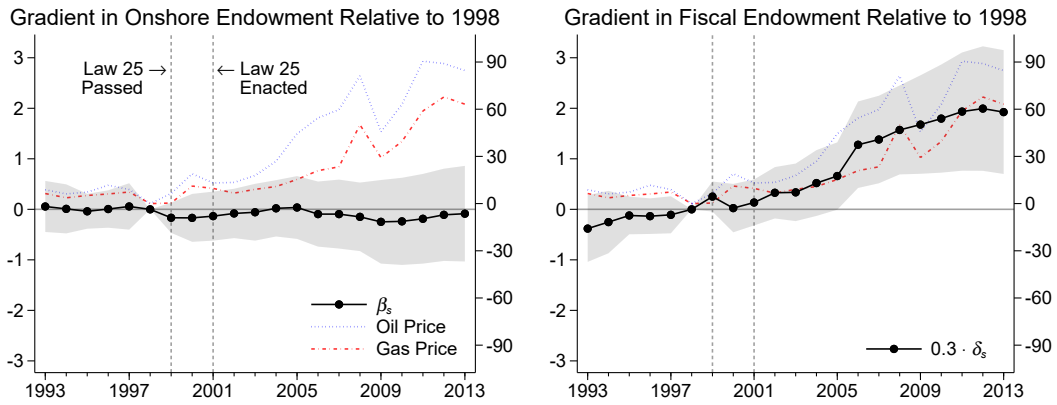


(b) Year-by-Year Gradient in Fiscal Endowment Relative to 1998



Notes: This figure plots estimates and 95-percent confidence intervals for $\{\beta_s\}$ (Panel (a)) and $\{0.3 \cdot \delta_s\}$ (Panel (b)) in Equation (2) (left axis). Vertical dashed lines indicate the years the decentralization law was passed and enacted. Oil and gas prices are expressed in deviations from their 1998 values, in constant 2010 USD per barrel of oil equivalent (right axis). Confidence intervals are robust to clustering at the level of 1975 district borders.

Figure 5: Event Study: Log Non-Oil GDP



Notes: This figure plots estimates and 95-percent confidence intervals for $\{\beta_s\}$ and $\{0.3 \cdot \delta_s\}$ in Equation (4) (left axis). Vertical dashed lines indicate the years the decentralization law was passed and enacted. Oil and gas prices are expressed in deviations from their 1998 values, in constant 2010 USD per barrel of oil equivalent (right axis). Confidence intervals are robust to clustering at the level of 1993 district borders.

A Appendix (For Online Publication)

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A.1 Details on Oil and Gas Revenue Sharing

This section details the construction of the fiscal endowments, F_d , in Equation (1). As described in Section 2, the central government shares 15 percent of oil revenue with subnational governments: 3 percent goes to the provincial government, 6 percent goes to the producing district, and the remaining 6 percent is evenly split by the other districts in the province. Thirty percent of gas revenue is shared: 6 percent to the province, 12 percent to the producing district, and 12 percent evenly divided among the other districts in the province. The provincial governments of Aceh, Papua, and Papua Barat each receive an additional 55 percent of oil revenue and 40 percent of gas revenue. The fiscal endowment variables are designed to capture the district's exposure to the spending shock induced by revenue sharing.

Provinces have discretion over how they spend their shared resource revenue within their borders. However, they do not report spending figures broken down by geographical location. Therefore, to quantify the district-level spending shocks induced by revenue sharing, we need to make an assumption about the spatial distribution of provincial expenditures. Our baseline fiscal endowment measure assumes that provinces spend their resource revenue on districts in proportion to their population. Nevertheless, our findings remain consistent when alternative assumptions are made, such as equal spending per district or allocations based on the national revenue sharing formula.

We construct fiscal endowments by replacing actual resource revenue collected by the central government with physical endowments in the revenue-sharing formula. Informally, fiscal endowment per capita is

$$\frac{\text{District Claim on Own Endowment} + \text{District Claim on Endowments of Other Districts} + \text{Provincial Endowment Allocated to District}}{\text{District Population in 1980}}.$$

Formally, let $R_{d,t}^{\text{oil}}$ and $R_{d,t}^{\text{gas}}$ be the physical endowments of oil and gas under the ground in 1975 and belonging to district d according to borders in year t . Both variables include onshore and offshore resources and are expressed in millions of barrels of oil equivalent. Let $p(d)$ index the province of district d , let $N_{p(d),t}$ be the number of districts in province $p(d)$ in year t , and let $S_{d,t}$ be district d 's share of provincial population in year t according to borders in year t . For a district d located in a province other than Aceh, Papua, or Papua Barat, the fiscal endowments per capita according to borders in year t are

$$F_{d,t}^{\text{oil}} = \frac{1}{\text{Pop}_{d,1980}} \left(0.06 \cdot R_{d,t}^{\text{oil}} + \frac{0.06}{N_{p(d),t} - 1} \cdot \sum_{\substack{j \neq d \\ p(j)=p(d)}} R_{j,t}^{\text{oil}} + 0.03 \cdot S_{d,t} \cdot \sum_j R_{j,t}^{\text{oil}} \right),$$

$$F_{d,t}^{\text{gas}} = \frac{1}{\text{Pop}_{d,1980}} \left(0.12 \cdot R_{d,t}^{\text{gas}} + \frac{0.12}{N_{p(d),t} - 1} \cdot \sum_{\substack{j \neq d \\ p(j)=p(d)}} R_{j,t}^{\text{gas}} + 0.06 \cdot S_{d,t} \cdot \sum_j R_{j,t}^{\text{gas}} \right).$$

For districts in Aceh, Papua, and Papua Barat, the formula is modified to account for the additional 55 percent of oil revenue and 40 percent of gas revenue received by the province, yielding

$$F_{d,t}^{\text{oil}} = \frac{1}{\text{Pop}_{d,1980}} \left(0.06 \cdot R_{d,t}^{\text{oil}} + \frac{0.06}{N_{p(d),t} - 1} \cdot \sum_{\substack{j \neq d \\ p(j)=p(d)}} R_{j,t}^{\text{oil}} + 0.58 \cdot S_{d,t} \cdot \sum_j R_{j,t}^{\text{oil}} \right),$$

$$F_{d,t}^{\text{gas}} = \frac{1}{\text{Pop}_{d,1980}} \left(0.12 \cdot R_{d,t}^{\text{gas}} + \frac{0.12}{N_{p(d),t} - 1} \cdot \sum_{\substack{j \neq d \\ p(j)=p(d)}} R_{j,t}^{\text{gas}} + 0.46 \cdot S_{d,t} \cdot \sum_j R_{j,t}^{\text{gas}} \right).$$

Fiscal endowments $F_{d,t}^{\text{oil}}$ and $F_{d,t}^{\text{gas}}$ can vary over time due to changing district and province borders. Notably, after Papua split into two provinces—Papua and Papua Barat—in 2003, non-producing districts located in the new province of Papua saw their fiscal endowments drop to nearly zero, as most of Indonesian Papua's oil and gas is located in the west (Figure 2). Border changes can directly affect economic and political outcomes.³⁷ To avoid bias, we average across year-specific endowments in the post-decentralization period,

$$F_d^{\text{oil}} = \frac{1}{14} \sum_{t=2001}^{2014} F_{d,t}^{\text{oil}}, \quad F_d^{\text{gas}} = \frac{1}{14} \sum_{t=2001}^{2014} F_{d,t}^{\text{gas}}.$$

As a result, the time variation in value of fiscal endowments, $\mathbf{P}'_t \mathbf{F}_d$, is solely due to price changes.

³⁷See, e.g., Burgess, Hansen, Olken, Potapov and Sieber (2012); Lewis (2017); Alesina, Gennaioli and Lovo (2019); Bazzi and Gudgeon (2021); Singhania (2022); Cassidy and Velayudhan (2026).

A.2 National Government Expenditure

In Section 4, we claim that the fiscal endowment will have the same correlation with the change in district spending and the change in *total* government spending, from before to after 2001, as long as the central government spending change is uncorrelated with the fiscal endowment. We provide the supporting argument below.

Consider the regression of total government spending at the district level on shock $S_{d,t} = \mathbf{P}'_t \mathbf{F}_d \cdot 1(t \geq 1999)$,

$$G_{d,t} = \beta S_{d,t} + \alpha_d + \lambda_{r(d),t} + \varepsilon_{d,t}.$$

Letting $\tilde{S}_{d,t}$ and $\tilde{G}_{d,t}$ denote residuals from regressing $S_{d,t}$ and $G_{d,t}$ on the district and region-by-year dummies, we have

$$\text{plim } \hat{\beta}_{\text{FE}} = \frac{\sum_{t=1}^T \text{E} \tilde{S}_{d,t} \tilde{G}_{d,t}}{\sum_{t=1}^T \text{E} \tilde{S}_{d,t}^2}.$$

Total spending is the sum of national and subnational spending, $G_{d,t}^N$ and $G_{d,t}^S$. Since $\tilde{G}_{d,t} = \tilde{G}_{d,t}^N + \tilde{G}_{d,t}^S$, using $G_{d,t}^S$ as the outcome variable would yield the same probability limit as long as $\sum_{t=1}^T \text{E} \tilde{S}_{d,t} \tilde{G}_{d,t}^N = 0$, because in that case $\sum_{t=1}^T \text{E} \tilde{S}_{d,t} \tilde{G}_{d,t} = \sum_{t=1}^T \text{E} \tilde{S}_{d,t} \tilde{G}_{d,t}^S$.

A.3 Data Appendix

A.3.1 Manufacturing Plants

We use data from the Indonesian manufacturing census of large and medium-sized firms (*Survei Industri Besar/Sedang*, or IBS) spanning 1975–2014 (Central Bureau of Statistics, Indonesia, 1975–2014). These data are produced by Indonesia’s Central Bureau of Statistics (*Badan Pusat Statistik*, or BPS) and cover the universe of manufacturing plants with at least 20 employees. The dataset contains information on wages, energy use, capital, number of employees, 5-digit industry, legal status, district of operation, and other information. There are 76,194 separate plants in an unbalanced panel; plants are observed for an average of 9.1 years. Although we use the words “firm” and “plant” interchangeably, we cannot link plants that belong to the same firm.

For all plant-years, we construct a 2-digit industry code that maps onto the 1999–2009 coding scheme; it is similar to codes in ISIC rev. 3. For 1999–2009, the 2-digit industry code is the first two digits of the 5-digit industry code. The Indonesian industry code scheme underwent three overhauls during the analysis period: 1989–1990, 1998–1999, and 2009–2010. We use a data-driven approach to map codes from earlier and later years. For example, if plants with the 5-digit product code 33212 in 1998 generally had a 2-digit code of 17 in 1999, we use that mapping.³⁸

³⁸Occasionally, 5-digit product codes frequently mapped into multiple 2-digit industries. In these cases, plants with a two-year track-record were assigned that plant’s 2-digit code. Plants without a two-year track-record were assigned the most common mapping. Additionally, if a plant reported product code A in years 1 and 3, but product code B in year 2, we assume that this plant straddles industries or there was a coding error. We change year 2’s product code to A.

While all districts that existed in 1975 are represented in our plant-level data, some newer districts do not have plant entries. This could be because some rural districts are quite small (e.g., around 15,000 people), but also because there may be some coverage issues within the data. Our intuition suggests that any potential bias from incomplete coverage is likely to be small because the coverage issues would need to be correlated with oil prices/production and they would need to affect the types of plants that are being surveyed within a district (e.g., only successful plants are being interviewed in some regions, while less successful plants are skipped). Instead, to the extent that coverage issues occur, it appears that entire districts may not be covered for some years. We have no evidence that any potential coverage issues are correlated with plants in a way that would bias our central estimates.

Wage variables are also constructed using the Indonesian Manufacturing Census. For both production and non-production workers within a given plant-year, we calculate total wages, total benefits, and total compensation. We also calculate total payments to all workers. While the precise wage subcategories that are reported each year vary, there is less deviance at the aggregated levels we use. Categories that drop out in some years are typically minor compensation types and frequently appear to be subsumed into other categories.³⁹ Our final proxy for average wages is total payments to all employees divided by the number of employees.

Finally, we also use the output, value added, and employee count variables reported in the Indonesian Manufacturing Census. Output and value added are reported directly, while employee counts are broken down by production and non-production roles. Of note, value added is unavailable for 1976–1978.

We exclude plants that are fully or partially owned by the government (national or sub-national) in more than 10 percent of the years that the plant is observed, as these plants may be insulated from market forces. (Around 7.5 percent of plants are government-owned, according to this definition.) Consequently, one district is omitted from the sample, as only government-owned plants are observed in that district. We also exclude plants that experience a one-year change in output, output per worker, value added, or value added per worker exceeding 500 log points in absolute value; a change in wages exceeding 300 log points in absolute value; or a change in number of employees exceeding 200 log points in absolute value.

A.3.2 Oil-Connected Industries

This section describes our process for determining which two-digit sectors are most connected to the oil and gas industry. We use this to investigate whether some of our main results are driven by sectors that are well-connected.

³⁹For example, the 2000 census reports subcategories for (i) “wage/salary,” (ii) “overtime, gifts, bonuses, and other similar payments,” and (iii) “company’s contribution to pension funds, social security, accident allowance, insurance, and other benefits,” while the 2001 census only reports subcategories for (i) “wage/salary” and (ii) “other incentive.”

We determine that the manufacturing sectors most related to the oil industry are 23 (Fuel industry, oil and coal refining, petroleum products, and nuclear fuel) and 24 (Chemicals and chemical products). The next most related sectors are 27 (Basic metal) and 28 (Metal goods, not machinery and equipment). We also run a robustness check that includes sector 25 (Rubber and plastic products industry) as related to the oil industry. Appendix Table A.1 lists all two-digit manufacturing sectors.

Table A.1: Translation of KBLI 2005 Two-Digit Manufacturing Industry Codes

Code	Sector Name
15	Food
16	Tobacco
17	Textiles
18	Ready-made clothing
19	Leather, leather goods, and footwear
20	Wood, wood products (excluding furniture), and woven goods from materials like rattan
21	Paper and paper products
22	Publishing, printing and reproduction of recorded media
23	Fuel, oil and coal refining, petroleum products, and nuclear fuel
24	Chemicals and chemical products
25	Rubber and plastic products
26	Non-metallic mining
27	Basic metal
28	Metal goods, not machinery and equipment
29	Machinery and equipment
30	Office equipment, accounting, and other electrical machinery
31	Other electrical machinery and its equipment
32	Radio, television, and communication equipment
33	Medical, measuring, navigation, or optical equipment
34	Motor vehicles
35	Transportation equipment, other than motor vehicles with 4+ wheels
36	Furniture and other processing
37	Recycling

Notes: Two-digit manufacturing industry codes are translated from the KBLI 2005, produced by Indonesia's Central Bureau of Statistics. These codes only include industries in the manufacturing sector, and exclude other sectors like agriculture, transportation, and services. Translations have been lightly edited for length and clarity.

To make this determination, we analyze input-output (IO) tables constructed by Indonesia's Central Bureau of Statistics ([Central Bureau of Statistics, Indonesia, 2005](#)). We consider the 2005 iteration, focusing on the set of tables that divide the economy into 66 sectors.⁴⁰ Because many of these 66 sectors are outside of manufacturing, it is only modestly more granular than the sectors in our manufacturing data.

We ascertain which of the 66 sectors are most related to the "Oil, gas and geothermal" (25) and "Petroleum refinery" (41) sectors, examining both upstream and downstream effects. Appendix Table A.2 details the full set of 66 sectors.

IO sector 25 is upstream of sectors 25 (itself, with a technical coefficient of 0.1209),

⁴⁰It also includes tables based on 19 sectors (which are too coarse for our purposes) and 175 sectors (which are too granular).

39 (0.4603), 40 (0.2280), 41 (0.3889), 44 (0.0806), 45 (0.1153), and 51 (0.0746). It is only downstream of itself (again, 0.1209). Using the crosswalk between the 66-sector codes and the KBLI 2005 codes (which form the basis of our manufacturing data and can be found in Appendix Table A.1), we see that these relationships correspond to sectors 23 (Fuel, oil and coal refining, petroleum products, and nuclear fuel) and 24 (Chemicals and chemical products) in the KBLI 2005 two-digit manufacturing industry codes. They also modestly correspond to KBLI sectors 27 (Basic metal) and 28 (Metal goods, not machinery and equipment), as well as a range of non-manufacturing sectors.

IO Sector 41 (Petroleum refinery) is upstream of many manufacturing sectors, but generally with weak relationships. The strongest relationships are with KBLI sectors 17 (Textiles), 24 (Chemicals and chemical products), 26 (Non-metallic mining), 27 (Basic metal), 28 (Metal goods, not machinery and equipment). Of note, IO Sector 41 has much stronger relationships with many transportation-related industries, but these are not in the manufacturing sector. It is only meaningfully downstream of sector 25 (Oil, gas, and geothermal).

We include the rubber and plastic products industry (KBLI sector 25) as a related industry in a robustness check. We do this for two reasons. First, plastic is directly derived from oil. Second, in the input-output tables this industry (IO sector 42) has a meaningful relationship with IO sector 66 (Unclear activities / limitations). It is plausible that some of these unclear transactions are actually with the oil industry but were difficult to classify because the underlying documentation is ambiguous. The rubber and plastic products industry may therefore be meaningfully connected to oil and gas.⁴¹ However, we also note that the “rubber” part of sector 25 is large and unlikely to be closely linked to the oil and gas industry.

For comparison, [Allcott and Keniston \(2018\)](#) analyze input-output tables from the United States at the four-digit industry level. Their analysis is considerably more granular than we are able to achieve in our context; they have a larger set of firms to analyze and also have fewer industry code changes to contend with. They find that the downstream sectors most connected (> 0.05) to the U.S. hydrocarbons industry are: Petroleum refining (0.69), Products of petroleum and coal not elsewhere classified (0.31), Nitrogenous and phosphatic fertilizers (0.081), and Carbon black (0.062). The upstream sectors most connected (> 0.05) to the U.S. hydrocarbons industry are: Oil and gas field machinery and equipment (0.23), Hydraulic cement (0.12), Ground or treated minerals (0.086), and Chemicals and chemical preparations not elsewhere classified (0.066).⁴² While we take the comparison with a grain of salt due to

⁴¹Indeed, our prior was that the three sectors most closely connected to the oil industry are: 23 (Fuel industry, oil and coal refining, petroleum products, and nuclear fuel), 24 (Chemical industry and chemical products), and 25 (Rubber and plastic products industry).

⁴²Of note, in the Indonesian context we find a weak relationship between the oil and gas industry and sector 29, “machinery and equipment.” This contrasts with the strong relationship [Allcott and Keniston \(2018\)](#) see between the oil and gas sector and “Oil and gas field machinery and equipment.” There are several possible reasons for this difference. First, we note the relevant code in [Allcott and Keniston \(2018\)](#) is considerably narrower, strengthening the estimated relationship. Second, it is possible that firms in Indonesia purchase more of their oil and gas machinery and equipment from foreign firms than firms in the United States.

Table A.2: Translation of 66-Sector Division for the 2005 KBLI Input-Output Tables

Code	Sector Name	Code	Sector Name
1	Rice	34	Cigarette industry
2	Legumes	35	Spinning industry
3	Corn	36	Textile, clothing and leather industry
4	Tubers	37	Bamboo, wood and rattan industry
5	Vegetables and fruits	38	Paper and paper products industry
6	Other food crops	39	Fertilizer and pesticide industry
7	Rubber	40	Chemical industry
8	Sugar cane	41	Petroleum refinery
9	Coconut	42	Rubber and plastic goods industry
10	Palm oil	43	Industry of goods from non-metallic minerals
11	Tobacco	44	Cement industry
12	Coffee	45	Iron and steel base industry
13	Tea	46	Non-ferrous base metal industry
14	Cloves	47	Metal goods industry
15	Fiber crops	48	Machinery, tools and electrical equipment
16	Other plantation crops	49	Transportation equipment industry and repairs
17	Other crops	50	Other goods industry not classified elsewhere
18	Livestock	51	Electricity, gas and clean water
19	Cutting animal	52	Buildings
20	Poultry and its products	53	Trade
21	Wood	54	Restaurants and hotels
22	Other forest products	55	Railway transportation
23	Fisheries	56	Land transportation
24	Coal and ore mining (metal)	57	Water transportation
25	Oil, gas and geothermal	58	Air transportation
26	Mining and excavation (others)	59	Transportation support services
27	Food processing and preservation	60	Communications
28	Oil and fat industry	61	Financial institutions
29	Rice milling industry	62	Construction and service businesses
30	Flour industry (all types)	63	Defense
31	Sugar industry	64	Social community services
32	Other food industry	65	Other services
33	Beverage industry	66	Unclear activities / limitations

Notes: Two-digit sector codes are translated from the KBLI 2005, produced by Indonesia's Central Bureau of Statistics. These codes represent the entire Indonesian economy, and include the agricultural, manufacturing, services, and transportation sectors, among others. Translations have been lightly edited for length and clarity.

the different levels of granularity and contexts, we view their analysis as suggestive that our list of oil-connected industries is reasonable.

A.3.3 District GDP

We construct a nearly balanced panel of district GDP using multiple sources. BPS produces estimates of two versions of district GDP—total GDP and GDP excluding oil and gas—using census data, survey data, and administrative records. The World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER) disseminates these data for 2000 onwards. The district non-oil GDP series ends in 2013, because BPS stopped reporting this variable. We extend the GDP data back to 1993 by digitizing archival BPS reports.⁴³

⁴³The reports are available at BPS's Online Public Access Catalogue (<https://perpustakaan.bps.go.id/opac>). We found the reports by using the search query "produk domestik regional bruto kabupaten/kota di indonesia."

To capture real growth, we use non-oil GDP measured in constant prices. The archival data from 1993 to 2000 are expressed in constant 1993 prices, while the World Bank data from 2000 to 2013 are expressed in constant 2000 prices. We combine the two series using chain linking. Specifically, we extend the newer series backwards in time using the level of the newer series in 2000 and the annual growth rates of the older series.

Formally, let Y_t^{old} be GDP in year t in constant 1993 prices, and let Y_t^{new} be GDP in year t in constant 2000 prices. Define the backfilled GDP series in constant 2000 prices as

$$\tilde{Y}_t^{\text{new}} = \frac{Y_{2000}^{\text{new}}}{\prod_{s=t+1}^{2000} (1 + g_s^{\text{old}})}$$

for $t \in \{1993, \dots, 1999\}$, where $1 + g_s^{\text{old}} = Y_s^{\text{old}} / Y_{s-1}^{\text{old}}$ is the gross growth rate in year s based on the older series. This ensures that real growth rates are always calculated within-series and that there is no artificial discontinuity in GDP in 2000.

In the GDP dataset, we aggregate all variables to 1993 district borders and normalize endowments by 1980 population.

A.3.4 Oil and Gas Data

Data on the Brent oil price and oil and gas endowments and production come from Rystad Energy's proprietary UCube database (Rystad Energy, 2019). Endowments include both proven reserves and (estimated) unproven reserves as reported by Rystad. The onshore shock variable in Equation (1), $P_t' E_d$, uses oil and gas endowments at the start of our sample period in 1975. The fiscal shock variable, $P_t' F_d$, uses oil and gas endowments in 2000, the year before decentralization was enacted.

While oil prices closely track each other globally, natural gas prices are regional. Because oil is energy dense, trading oil is relatively inexpensive. Global oil prices therefore track each other very closely, and Brent oil prices do an excellent job of approximating oil prices in Indonesia. Natural gas, however, is voluminous, expensive to trade, and natural gas prices vary substantially by region.⁴⁴ As a result, we need data on local natural gas prices.

Data on liquefied natural gas (LNG) prices in Indonesia are from IndexMundi,⁴⁵ which sources from World Gas Intelligence and the World Bank.⁴⁶ Because the data only go back

BPS also provides a document reporting district GDP over 1983–1993. However, we choose not to use this document due to inconsistencies in the data. First, the document does not report separate series for total GDP and GDP excluding oil and gas. For some districts containing hydrocarbons, reported GDP clearly excludes oil and gas, while for others, the GDP figures clearly include oil and gas. Second, even for districts with no hydrocarbons, the GDP series for 1983–1993 is inconsistent with the series for 1993–2013 in subsequent reports. For example, 1993 GDP reported in the 1983–1993 document tends to be much smaller than 1993 GDP (all in current prices) reported in subsequent documents published in 1997. This is likely due to BPS adopting the System of National Accounts 1993 (SNA 1993) methodology in 1993.

⁴⁴For more, please see <https://www.eia.gov/todayinenergy/detail.php?id=3310>.

⁴⁵See <https://www.indexmundi.com/commodities/?commodity=indonesian-liquefied-natural-gas&months=360>.

⁴⁶We have tried to purchase the data directly from World Gas Intelligence, but they have not responded to multiple inquiries.

to 1994, we backcast earlier prices using the relationship between West Texas Intermediate (WTI) oil prices and Indonesian LNG prices. We use WTI prices instead of another region's gas prices because oil prices are a good proxy for the cost of energy over the long run, while one region's gas prices are not particularly informative about another region's gas prices. Further, LNG prices are generally contracted over long periods of time, with contracts often tied directly to world oil prices.

Like most non-renewable natural resources, oil and gas deposits are geographically concentrated, yielding a skewed distribution across districts (Figure 2). Based on 2000 oil and gas prices, the 75th percentile of onshore endowment is 0 and the 95th percentile is USD 7,176 per capita (in constant 2010 USD). For offshore endowment, the corresponding percentiles are USD 0 and 560 per capita. Fiscal endowment is less skewed due to the sharing rules. The 25th percentile is 0, the median is USD 56 per capita, the 75th percentile is USD 347 per capita, and the 95th percentile is USD 9,690 per capita (in constant 2010 USD). When restricting to positive values only, the 5th and 95th percentile ranges for onshore, offshore, and fiscal endowment are [0.022, 291.955], [0.560, 553.587], and [0.003, 24.598] respectively, in USD thousands per capita.

A.3.5 Other District Variables

We use microdata from the Population Census (*Sensus Penduduk*) and the Intercensal Population Survey (*Survei Penduduk Antar Sensus*) to measure district population and employment by sector. The census years are 1971, 1980, 1990, 2000, and 2010; and the intercensal surveys take place in 1976, 1985, 1995, and 2005. These data are produced by BPS and disseminated by the Integrated Public Use Microdata Series (IPUMS) International ([Minnesota Population Center, 2020](#)). We also use annual population estimates from the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER), covering 1983–2014. We combine this series with census population in 1980, and we log-linearly interpolate missing population values.

Official 1999 district-level poverty rates are calculated by BPS and obtained from INDO-DAPOER.⁴⁷ District-level poverty rates in 1999 are missing for Jakarta, so we assign the province-level poverty rate to these districts.⁴⁸

Data on land area come from INDO-DAPOER, while data on topography, coastline, and forestland come from the 2011 wave of the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES). Data on coal-bearing formations come from the *Badan Geologi Formasi*

⁴⁷BPS began calculating district-level poverty rates in 1999, using a two-step procedure. First, it estimated the poverty line using the SUSENAS consumption module, which had comprehensive expenditure data but was only representative at the province level. Second, it calculated the percentage of households below the poverty line using the larger SUSENAS core survey, which was representative at the district level. The core survey had less comprehensive expenditure data, leading to underestimation of consumption. As a result, BPS had to calibrate the poverty line to account for this underestimation before calculating the poverty rate. In principle, BPS could have applied this same methodology to 1993 or 1996 data, but to the best of our knowledge it did not.

⁴⁸This is likely because BPS initially calculated district-level poverty rates for the purpose of allocating central transfers to autonomous districts, and Jakarta's districts are not autonomous.

Pembawa Batubara polygon layer from ESDM One Map (Badan Geologi, 2019). We measure coal formations in square kilometers per 1,000 inhabitants in 1980. Data on infrastructure in 1999 come from PODES 2000.

Data on district expenditures come from INDO-DAPOER and the Ministry of Finance (*Kementerian Keuangan*). The INDO-DAPOER data span 2001–2014. We fill in any missing data and extend the series back to 1994 using Ministry of Finance budget reports. The five districts in Jakarta lack expenditure data, as these districts are managed by the province. Most districts are missing law and order expenditure for 2003–2004, and all districts are missing agriculture expenditure for 2013–2014.

We construct five proxies for baseline central government expenditure using PODES 1983. The first four proxies are measures of public services provided by the centrally administered Presidential Instruction (*Instruksi Presiden*, or INPRES) program. The INPRES school construction program (INPRES *Sekolah Dasar*) built over 61,000 primary schools between 1973 and 1979 (Duflo, 2001). Our school variable therefore reflects the stock of INPRES primary schools as of 1979. We also construct measures of the number of INPRES teachers and family toilets in 1983, and the number of completed or ongoing INPRES capital projects in the 1982–1983 budget.⁴⁹ The fifth proxy is the number of completed or ongoing national capital projects in the 1982–1983 budget. These projects are larger in scale than the INPRES projects.

A.3.6 Spatial Harmonization

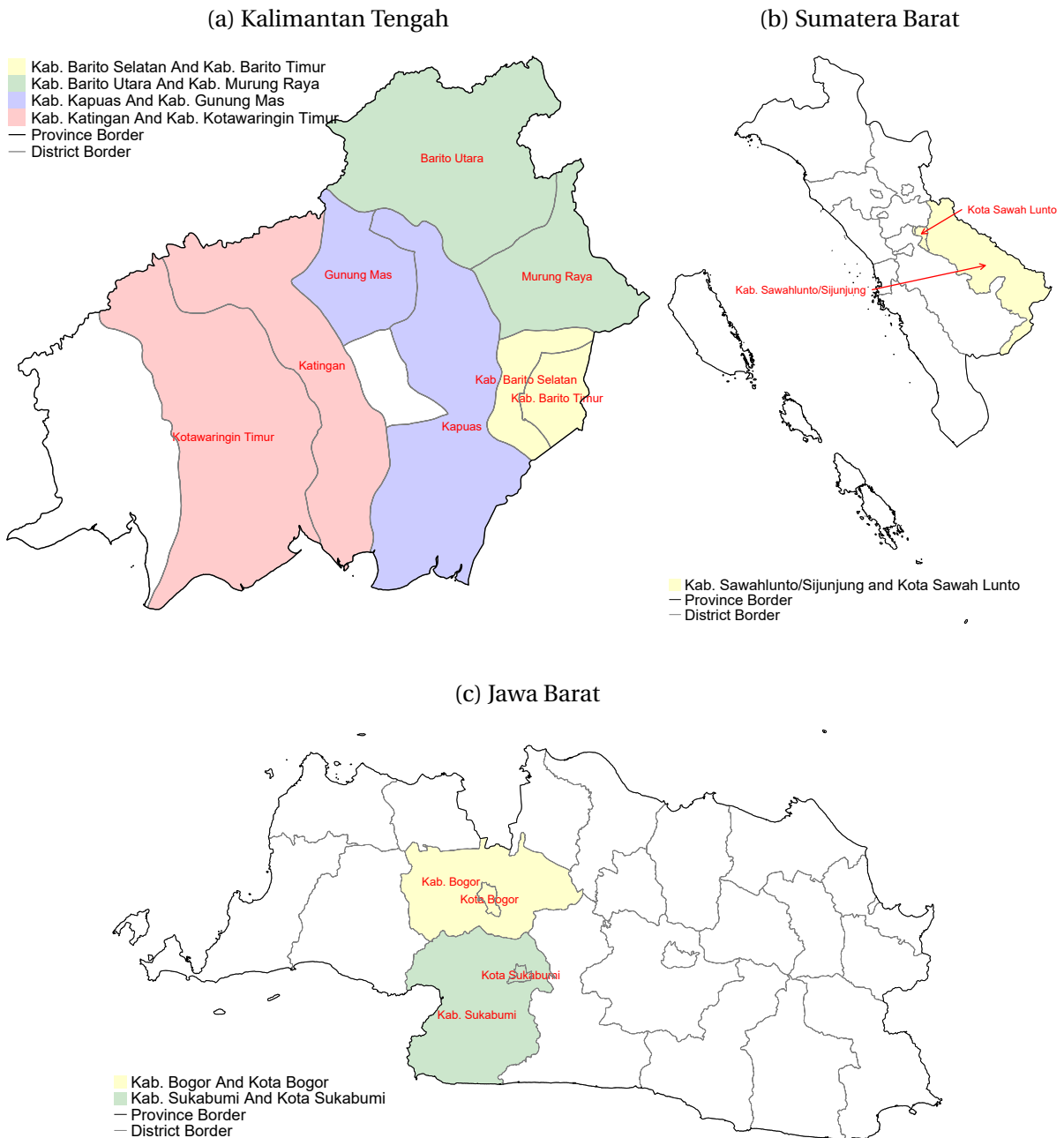
The number of districts in Indonesia increased dramatically over the analysis period. To construct consistent geographic units, we use the district crosswalk for 1993–2014 provided by the World Bank's *Indonesia Database For Policy And Economic Research* (2015) (INDO-DAPOER).⁵⁰ We modify and extend the crosswalk back to 1970 using data from Olken (2009) and the Village Potential Statistics (*Pendataan Potensi Desa*). Using the crosswalk, we assign each establishment to a district according to 1975 borders.

In 1975, there were 287 districts. However, our analysis uses 280 geographic units, merging some districts to spatially harmonize our data over time. In Kalimantan Tengah, four pairs of districts (all *kabupaten*) amalgamated in 1985 and separated again in 2002: (1) Katingan and Kotawaringin Timur; (2) Gunung Mas and Kapuas; (3) Barito Timur and Barito Selatan; and (4) Murung Raya and Barito Utara. We amalgamate the districts in each pair to maintain consistent borders. Additionally, we combine Kabupaten Bogor with Kota Bogor, and we combine Kabupaten Sukabumi with Kota Sukabumi—all in Jawa Barat. For both pairs of districts, the smaller kota expanded in 1995 to include several subdistricts that previously belonged to the larger kabupaten. Similarly, we combine Kabupaten Sawahlunto/Sijunjung with Kota Sawahlunto (in Sumatera Barat) because the smaller kota expanded into the larger kabupaten in 1990. Figure A.1 displays our spatial harmonization process for 1975 borders.

⁴⁹PODES 1983 also contains information on INPRES public toilets, but these data appear to be unreliable.

⁵⁰INDO-DAPOER is hosted at <http://databank.worldbank.org/data/reports.aspx?source=1266>.

Figure A.1: Spatial Harmonization of Districts, 1975 Borders

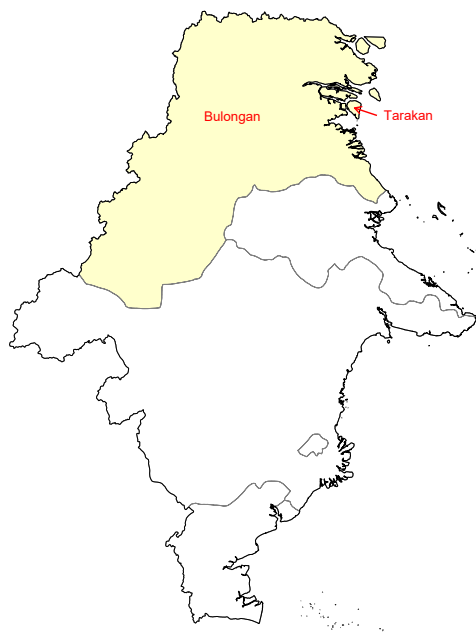


Notes: This figure depicts the districts we combined in Kalimantan Tengah (Panel a), Jawa Barat (Panel b), and Sumatera Barat (Panel c) to geographically harmonize our sample of 1975 district borders. Shapefiles used are provided by [Minnesota Population Center \(2020\)](#), and incorporate geographies from the 1976, 1980, and 1985 maps.

In 1993, we begin with 292 districts. We combine Tarakan with Bulongan (in Kalimantan Timur), Donggala with Palu (in Sulawesi Tengah), and the same Jawa Barat districts we did in 1975 to ensure that geographies are harmonized across the years 1993–2014. The process yields 288 districts which we use in our analysis. Figure A.2 displays our spatial harmonization process for 1993.

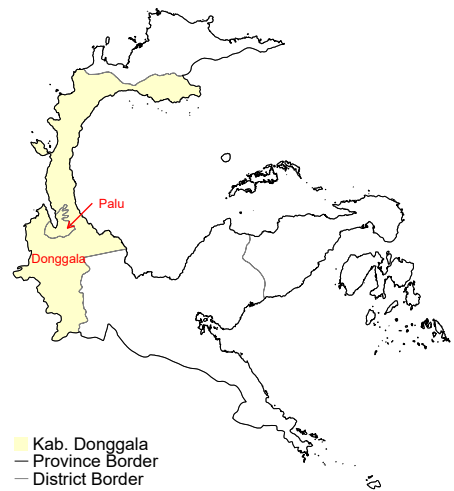
Figure A.2: Spatial Harmonization of Districts, 1993 Borders

(a) Kalimantan Timur



■ Kab. Bulongan
— Province Border
— District Border

(b) Sulawesi Tengah



■ Kab. Donggala
— Province Border
— District Border

Notes: This figure depicts the districts we combined in Kalimantan Timur (Panel a) and Sulawesi Tengah (Panel b) to geographically harmonize our sample of 1993 district borders. Shapefiles used are provided by [Minnesota Population Center \(2020\)](#), and use 1995 geographies.

A.4 Additional Results: Tables

Table A.3: Summary Statistics

	(1)	(2)	(3)	(4)	(5)
	Mean	Std. Dev.	Min.	Max.	Obs.
<i>Firm Outcomes, 1975–2014</i>					
Output (IDR Million)	40.82	332.33	0	40,430	490,798
Value Added (IDR Million)	16.52	194.19	0	43,226	472,238
Number of Workers	145.61	520.32	1	40,850	490,786
Output per Worker (IDR Million)	0.17	0.99	0	486	490,786
Value Added per Worker (IDR Million)	0.07	0.56	0	290	472,238
Average Wage (IDR 1,000)	12.02	15.96	0	3,864	489,855
<i>District GDP, 1993–2013 (1993 Borders)</i>					
GDP (IDR Billion)	14,147	26,627	248	287,657	6,044
GDP Excluding Oil and Gas (IDR Billion)	12,787	23,712	248	287,657	6,041
Oil and Gas GDP (IDR Billion)	1,367	9,201	0	164,218	6,040
(> 0)	9,161	22,283	0	164,218	901
<i>District Expenditures, 1994–2014 (1975 Borders)</i>					
Total Expenditure p.c. (USD)	314.46	424.78	2.64	5,187.34	5,864
Administration Expenditure p.c. (USD)	81.41	156.82	0.00	2,413.63	5,715
Education Expenditure p.c. (USD)	72.42	84.77	0.00	812.79	5,716
Infrastructure Expenditure p.c. (USD)	55.80	102.37	0.10	1,624.77	5,708
Health Expenditure p.c. (USD)	26.56	43.07	0.02	496.45	5,698
Agriculture Expenditure p.c. (USD)	9.57	19.13	0.00	278.15	4,732
Environment Expenditure p.c. (USD)	5.93	10.25	0.00	143.39	5,476
Law and Order Expenditure p.c. (USD)	3.42	7.18	0.00	141.72	4,794
Social Protection Expenditure p.c. (USD)	3.42	7.77	0.00	105.29	5,176
Tourism and Culture Expenditure p.c. (USD)	2.02	4.32	0.00	69.00	5,176
<i>Onshore Oil and Gas Production, 1975–2014 (1975 Borders)</i>					
Onshore Oil and Gas Production p.c. (USD)	193.56	1,442.85	0.00	39,632.33	11,200
(> 0)	1,537.47	3,805.12	0.03	39,632.33	1,410
<i>District Shocks, 1999–2014 (1975 Borders)</i>					
Price × Onshore Endowment p.c. (USD 1,000)	10.80	86.01	0.00	1,874.59	4,480
(> 0)	81.71	224.18	0.00	1,874.59	592
Price × Offshore Endowment p.c. (USD 1,000)	5.44	62.99	0.00	1,542.65	4,480
(> 0)	95.23	247.21	0.64	1,542.65	256
Price × Fiscal Endowment p.c. (USD 1,000)	6.51	24.41	0.00	317.18	4,480
(> 0)	9.30	28.72	0.00	317.18	3,136
<i>District Covariates (1975 Borders)</i>					
Land Area (1,000 km ²)	7.07	13.98	0.02	119.04	280
District Has Some Coastline	0.65	0.48	0.00	1.00	280
Share of Villages with Slope > 15°	0.31	0.18	0.00	0.80	280
Share of Villages Located in or Near Forest	0.21	0.18	0.00	0.78	280
Coal Formation Area (km ² per 1,000), 1980	4.23	19.43	0.00	170.86	280
Population (1,000), 1980	539.80	548.60	23.82	5,481.47	280
Share of Population Aged 0–14, 1980	0.41	0.04	0.29	0.50	280
Share of Population Aged 15–64, 1980	0.55	0.03	0.46	0.67	280
Share of Population with Primary Education, 1980	0.23	0.10	0.04	0.60	280
Share of Population with Secondary Education, 1980	0.04	0.04	0.01	0.23	280
Literacy Rate, 1980	0.56	0.11	0.15	0.82	280
Urbanization Rate, 1980	0.23	0.31	0.00	1.00	280
Poverty Rate, 1999	0.24	0.14	0.02	0.78	280
INPRES Schools per 1,000, 1983	1.12	1.34	0.02	9.73	280
INPRES Teachers per 1,000, 1983	5.75	6.83	0.17	44.59	280
INPRES Family Toilets per 1,000, 1983	7.58	21.46	0.00	318.81	280
INPRES Capital Projects per 1,000, 1982–83 Budget	0.53	0.75	0.00	5.67	280
National Capital Projects per 1,000, 1982–83 Budget	0.10	0.18	0.00	1.90	280

Notes: The top four panels present statistics for outcomes at the firm-year and district-year levels. The bottom panel presents statistics for the cross section of districts. Firm output and output per capita are expressed in constant 2010 IDR millions, and average wages are expressed in constant 2010 IDR thousands. GDP is expressed in constant 2010 IDR billions. District expenditures are expressed in constant 2010 USD per capita based on 1980 population. The price × endowment variables are expressed in constant 2010 USD thousands per capita based on 1980 population.

Table A.4: Baseline Characteristics and Central Expenditure Proxies

	<i>Covariates:</i>		(3) Joint Test <i>p</i> -value	(4) Obs.
	(1) Onshore Endow. × Price	(2) Fiscal Endow. × Price		
<i>Outcome Variables: Baseline Characteristics</i>				
Log Land Area	4.64** (1.82)	11.68 (8.65)	0.000	280
% Villages with Slope > 15°	-32.99** (14.20)	57.70 (81.42)	0.010	280
% Villages Located in or Near Forest	-23.14 (15.08)	234.18*** (90.87)	0.033	280
District Has Some Coastline	0.03 (0.29)	6.67*** (1.59)	0.000	280
Coal Formation Area (km ² per 1,000), 1980	-10.43 (57.80)	590.04* (315.52)	0.137	280
Log Population, 1980	3.34*** (1.25)	-10.44*** (4.02)	0.011	280
% Population Aged 0–14, 1980	3.23 (2.46)	-22.61 (14.86)	0.316	280
% Population Aged 15–64, 1980	-2.89 (2.70)	28.57* (15.64)	0.128	280
% Population with Primary Education, 1980	-19.40 (12.12)	58.80 (63.07)	0.225	280
% Population with Secondary Education, 1980	-7.27 (5.41)	28.84 (26.92)	0.406	280
Literacy Rate (%), 1980	-2.64 (8.40)	-30.98 (51.60)	0.303	280
Urbanization Rate (%), 1980	-26.54 (33.98)	82.46 (144.90)	0.734	280
<i>Outcome Variable: Poverty, Pre-Decentralization</i>				
Poverty Rate (%), 1999	1.96 (10.30)	-16.35 (59.50)	0.961	280
<i>Outcome Variables: Central Expenditure Proxies</i>				
INPRES Schools per 1,000, 1983	-0.60 (1.78)	29.01** (11.46)	0.000	280
INPRES Teachers per 1,000, 1983	6.68 (9.62)	81.73* (47.09)	0.060	280
INPRES Family Toilets per 1,000, 1983	-35.49 (21.72)	206.13 (183.46)	0.238	280
INPRES Capital Projects per 1,000, 1982–83 Budget	0.97 (0.88)	10.27 (6.74)	0.000	280
National Capital Projects per 1,000, 1982–83 Budget	0.24 (0.17)	1.48 (1.06)	0.001	280

Notes: This table presents estimates from cross-sectional regressions of baseline characteristics on the two endowment variables, controlling for region fixed effects. Each row is a separate regression. Column 3 reports the *p*-value from the test that the endowment coefficients are jointly zero. Each price × endowment variable is expressed in constant 2010 USD thousands per capita based on 1980 population and 2000 oil and gas prices. All variables are measured at 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: District Expenditure: Dynamic Model, Robustness to Omitting High-Leverage Districts

	District Expenditure by Function, Scaled by 1980 Population									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Admin.	Edu.	Infra.	Health	Agri.	Env.	Law & Order	Social Prot.	Tourism & Culture
<i>Panel A: Omitting Kutai</i>										
Onshore Endow. × Price (β)	-0.45** (0.19)	-0.17** (0.08)	-0.14*** (0.05)	-0.12* (0.07)	-0.07** (0.03)	-0.06*** (0.02)	0.01 (0.01)	-0.02*** (0.01)	-0.01** (0.00)	-0.00 (0.00)
Fiscal Endow. × Price × (t ≥ 1999) (δ)	9.17*** (1.15)	2.96*** (0.49)	2.12*** (0.24)	2.52*** (0.50)	1.04*** (0.16)	0.56*** (0.12)	0.19*** (0.04)	0.18*** (0.03)	0.14*** (0.02)	0.07*** (0.02)
Observations	5,569	5,419	5,420	5,412	5,402	4,714	5,182	4,501	4,881	4,886
Districts	274	274	274	274	274	274	274	274	274	274
<i>Panel B: Omitting Bengkalis</i>										
Onshore Endow. × Price (β)	-1.80*** (0.55)	-0.67*** (0.22)	-0.43*** (0.12)	-0.30 (0.25)	-0.21** (0.08)	-0.16*** (0.06)	-0.06*** (0.02)	-0.06*** (0.01)	-0.03* (0.01)	-0.02 (0.02)
Fiscal Endow. × Price × (t ≥ 1999) (δ)	9.53*** (1.08)	3.22*** (0.47)	2.18*** (0.23)	2.56*** (0.50)	1.08*** (0.15)	0.57*** (0.12)	0.21*** (0.04)	0.20*** (0.03)	0.15*** (0.01)	0.08*** (0.02)
Observations	5,568	5,417	5,418	5,410	5,400	4,712	5,179	4,501	4,879	4,883
Districts	274	274	274	274	274	274	274	274	274	274
<i>Panel C: Omitting Kutai and Bengkalis</i>										
Onshore Endow. × Price (β)	-1.51** (0.68)	-0.64** (0.27)	-0.46*** (0.14)	-0.18 (0.27)	-0.27*** (0.07)	-0.17*** (0.07)	-0.03* (0.02)	-0.06*** (0.02)	-0.04*** (0.01)	-0.00 (0.02)
Fiscal Endow. × Price × (t ≥ 1999) (δ)	9.46*** (1.17)	3.10*** (0.51)	2.22*** (0.23)	2.52*** (0.53)	1.10*** (0.15)	0.59*** (0.12)	0.21*** (0.04)	0.20*** (0.03)	0.15*** (0.02)	0.07*** (0.02)
Observations	5,549	5,399	5,400	5,392	5,382	4,697	5,162	4,485	4,864	4,868
Districts	273	273	273	273	273	273	273	273	273	273

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (3) for $K = 3$, where the outcomes are district expenditure on different functions. The regressions omit high-leverage districts, as indicated. Expenditure is expressed in constant 2010 USD thousands and scaled by 1980 population. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the change in spending in USD due to a USD 1 thousand increase in the value of the endowment. The sample covers 1994–2014. The five districts in Jakarta are missing, as these districts are managed by the province. Most districts are missing law and order expenditure for 2003–2004, and all districts are missing agriculture expenditure for 2013–2014. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Firm Outcomes: Robustness to Starting Fiscal Shock in 2001

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Static Model (K = 0)</i>						
Onshore Endow. \times Price (β)	0.42*** (0.09)	0.03 (0.10)	-0.06 (0.05)	0.48*** (0.08)	0.13 (0.08)	0.25*** (0.06)
Fiscal Endow. \times Price $\times (t \geq 2001)$ (δ)	-0.53 (1.34)	-0.11 (1.48)	1.06 (0.82)	-1.59 (1.26)	-1.29 (1.34)	-3.66*** (0.77)
$\beta - 0.3 \cdot \delta$	0.58 (0.47)	0.06 (0.51)	-0.38 (0.29)	0.96** (0.44)	0.51 (0.46)	1.35*** (0.28)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278
<i>Panel B: Dynamic Model (K = 3)</i>						
Onshore Endow. \times Price	-0.06 (0.10)	-0.34*** (0.09)	-0.11*** (0.04)	0.05 (0.10)	-0.19** (0.09)	0.10* (0.06)
Lag 1	0.34*** (0.07)	0.40*** (0.06)	-0.01 (0.03)	0.35*** (0.06)	0.43*** (0.06)	0.08** (0.04)
Lag 2	0.11* (0.07)	-0.03 (0.08)	0.10*** (0.02)	0.02 (0.06)	-0.13* (0.07)	-0.03 (0.05)
Lag 3	0.44*** (0.07)	0.19** (0.08)	0.05** (0.02)	0.40*** (0.06)	0.15** (0.07)	0.21*** (0.04)
Sum of Lags 0-K (β)	0.83*** (0.12)	0.23** (0.12)	0.02 (0.07)	0.81*** (0.09)	0.26*** (0.10)	0.37*** (0.07)
Fiscal Endow. \times Price $\times (t \geq 2001)$	1.90 (1.74)	-0.24 (1.56)	2.34*** (0.64)	-0.44 (1.90)	-2.64 (1.66)	-1.82** (0.83)
Lag 1	-1.66 (1.23)	-0.97 (1.42)	-0.09 (0.34)	-1.57 (1.09)	-0.90 (1.24)	-2.61*** (0.88)
Lag 2	0.89 (1.22)	2.05* (1.22)	-1.33*** (0.29)	2.22** (1.08)	3.38*** (1.14)	-1.02 (0.92)
Lag 3	-2.73** (1.37)	-0.85 (1.64)	-0.46 (0.47)	-2.27* (1.32)	-0.45 (1.53)	1.54 (0.97)
Sum of Lags 0-K (δ)	-1.60 (1.51)	0.00 (1.67)	0.47 (0.95)	-2.07 (1.28)	-0.61 (1.44)	-3.91*** (0.89)
$\beta - 0.3 \cdot \delta$	1.31** (0.54)	0.23 (0.58)	-0.12 (0.34)	1.43*** (0.45)	0.44 (0.50)	1.54*** (0.33)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1), measuring the fiscal shock as $P_t^i F_d \cdot 1(t \geq 2001)$. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Firm Outcomes: Static Model, Robustness to Omitting High-Leverage Districts

	Log(Outcome)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output	Value Added	Number of Workers	Output per Worker	Value Added per Worker	Avg. Wage
<i>Panel A: Omitting Kutai</i>						
Onshore Endow. \times Price (β)	0.46*** (0.09)	0.04 (0.10)	-0.06 (0.06)	0.52*** (0.08)	0.15* (0.08)	0.25*** (0.06)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.70 (1.36)	0.08 (1.57)	1.22 (0.88)	-1.92* (1.14)	-1.28 (1.28)	-3.85*** (0.67)
$\beta - 0.3 \cdot \delta$	0.67 (0.47)	0.02 (0.54)	-0.43 (0.31)	1.10*** (0.39)	0.53 (0.44)	1.41*** (0.24)
Observations	481,342	462,147	481,330	481,330	462,147	480,422
Plants	52,120	51,254	52,120	52,120	51,254	52,022
Districts	277	277	277	277	277	277
<i>Panel B: Omitting Bengkalis</i>						
Onshore Endow. \times Price (β)	0.75 (0.84)	0.26 (0.77)	-0.44 (0.38)	1.20* (0.70)	0.60 (0.62)	1.08* (0.62)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.79 (1.59)	0.12 (1.77)	2.12** (1.04)	-2.93** (1.38)	-1.94 (1.56)	-4.74*** (0.79)
$\beta - 0.3 \cdot \delta$	0.98 (1.14)	0.23 (1.05)	-1.07* (0.61)	2.08** (0.97)	1.19 (0.89)	2.50*** (0.78)
Observations	480,340	461,190	480,328	480,328	461,190	479,421
Plants	51,995	51,129	51,995	51,995	51,129	51,897
Districts	277	277	277	277	277	277
<i>Panel C: Omitting Kutai and Bengkalis</i>						
Onshore Endow. \times Price (β)	1.53* (0.81)	0.80 (0.75)	-0.41 (0.41)	1.97*** (0.73)	1.18* (0.62)	1.11 (0.77)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.70 (1.71)	0.55 (1.98)	2.16** (1.03)	-2.87* (1.49)	-1.59 (1.70)	-4.81*** (0.88)
$\beta - 0.3 \cdot \delta$	1.74 (1.16)	0.64 (1.16)	-1.06* (0.63)	2.83*** (1.03)	1.66* (1.00)	2.55*** (0.95)
Observations	479,887	460,743	479,875	479,875	460,743	478,968
Plants	51,933	51,067	51,933	51,933	51,067	51,835
Districts	276	276	276	276	276	276

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1) for $K = 0$ after dropping high-leverage districts. Kabupaten Kutai is dropped in Panel A, Kabupaten Bengkalis is dropped in Panel B, and both are dropped in Panel C. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Firm Outcomes: Dynamic Model, Robustness to Omitting High-Leverage Districts

	Log(Outcome)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output	Value Added	Number of Workers	Output per Worker	Value Added per Worker	Avg. Wage
<i>Panel A: Omitting Kutai</i>						
Onshore Endow. \times Price (β)	0.79*** (0.11)	0.19* (0.12)	-0.00 (0.06)	0.79*** (0.09)	0.25** (0.10)	0.34*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.33 (1.58)	1.41 (1.80)	0.89 (0.99)	-1.22 (1.35)	0.37 (1.54)	-3.81*** (0.93)
$\beta - 0.3 \cdot \delta$	0.89 (0.55)	-0.23 (0.62)	-0.27 (0.35)	1.16** (0.47)	0.14 (0.53)	1.48*** (0.34)
Observations	481,342	462,147	481,330	481,330	462,147	480,422
Plants	52,120	51,254	52,120	52,120	51,254	52,022
Districts	277	277	277	277	277	277
<i>Panel B: Omitting Bengkalis</i>						
Onshore Endow. \times Price (β)	0.57 (0.99)	0.37 (0.88)	-0.50 (0.43)	1.08 (0.81)	0.77 (0.72)	1.18 (0.77)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.19 (1.92)	1.13 (2.09)	1.90 (1.20)	-2.10 (1.66)	-0.70 (1.81)	-5.13*** (1.06)
$\beta - 0.3 \cdot \delta$	0.62 (1.35)	0.03 (1.18)	-1.07 (0.69)	1.71 (1.14)	0.98 (1.00)	2.72*** (1.00)
Observations	480,340	461,190	480,328	480,328	461,190	479,421
Plants	51,995	51,129	51,995	51,995	51,129	51,897
Districts	277	277	277	277	277	277
<i>Panel C: Omitting Kutai and Bengkalis</i>						
Onshore Endow. \times Price (β)	1.25 (1.01)	0.94 (0.90)	-0.52 (0.49)	1.79** (0.88)	1.44* (0.76)	1.31 (0.99)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.04 (2.03)	1.41 (2.30)	1.95* (1.13)	-2.00 (1.80)	-0.52 (1.97)	-5.13*** (1.17)
$\beta - 0.3 \cdot \delta$	1.26 (1.41)	0.52 (1.35)	-1.10 (0.69)	2.39* (1.28)	1.59 (1.20)	2.85** (1.25)
Observations	479,887	460,743	479,875	479,875	460,743	478,968
Plants	51,933	51,067	51,933	51,933	51,067	51,835
Districts	276	276	276	276	276	276

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$, $\delta = \sum_{k=0}^K \delta_k$, and $\gamma = \sum_{k=0}^K \gamma_k$ in Equation (1) for $K = 3$ after dropping high-leverage districts. Kabupaten Kutai is dropped in Panel A, Kabupaten Bengkalis is dropped in Panel B, and both are dropped in Panel C. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Firm Outcomes: Only Using Fields Discovered by 1975

	Log(Outcome)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output	Value Added	Number of Workers	Output per Worker	Value Added per Worker	Avg. Wage
<i>Panel A: Static Model (K = 0)</i>						
Onshore Endow. × Price (β)	0.48*** (0.10)	0.06 (0.11)	-0.05 (0.06)	0.53*** (0.08)	0.15* (0.09)	0.26*** (0.06)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	-1.61 (1.55)	-0.91 (1.69)	0.96 (0.96)	-2.57* (1.37)	-2.03 (1.48)	-4.37*** (0.85)
$\beta - 0.3 \cdot \delta$	0.96* (0.54)	0.33 (0.59)	-0.34 (0.34)	1.30*** (0.47)	0.76 (0.51)	1.57*** (0.30)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278
<i>Panel B: Dynamic Model (K = 3)</i>						
Onshore Endow. × Price	0.16 (0.10)	-0.14 (0.12)	-0.13** (0.06)	0.29*** (0.08)	0.02 (0.10)	0.15*** (0.06)
Lag 1	0.13* (0.08)	0.26** (0.13)	0.00 (0.03)	0.12* (0.06)	0.27** (0.11)	0.09 (0.05)
Lag 2	-0.03 (0.10)	-0.18* (0.10)	0.13*** (0.02)	-0.15* (0.09)	-0.32*** (0.10)	-0.13* (0.07)
Lag 3	0.55*** (0.10)	0.26*** (0.10)	0.01 (0.03)	0.54*** (0.09)	0.26*** (0.09)	0.23*** (0.06)
Sum of Lags 0–K (β)	0.81*** (0.12)	0.20 (0.12)	0.01 (0.07)	0.80*** (0.09)	0.23** (0.10)	0.34*** (0.07)
Fiscal Endow. × Price × ($t \geq 1999$)	-4.31** (1.83)	-5.34** (2.34)	2.04** (0.91)	-6.35*** (1.47)	-7.49*** (1.90)	-3.27*** (0.77)
Lag 1	2.77 (1.86)	1.83 (2.51)	-0.02 (0.46)	2.80 (1.71)	1.80 (2.29)	-2.63** (1.04)
Lag 2	4.22* (2.22)	5.08** (2.39)	-1.54*** (0.49)	5.75*** (1.92)	6.67*** (2.15)	0.39 (1.52)
Lag 3	-3.70 (2.37)	-0.69 (2.16)	0.06 (0.62)	-3.76* (2.02)	-0.83 (1.80)	1.26 (1.26)
Sum of Lags 0–K (δ)	-1.02 (1.88)	0.88 (2.00)	0.54 (1.09)	-1.55 (1.67)	0.16 (1.80)	-4.24*** (1.18)
$\beta - 0.3 \cdot \delta$	1.12* (0.66)	-0.07 (0.69)	-0.15 (0.38)	1.26** (0.57)	0.18 (0.61)	1.61*** (0.40)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1). Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Endowments are based on oil and gas fields discovered by 1975. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Firm Outcomes: Static Model, Robustness to Changing Covariate Set

	Log(Outcome)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output	Value Added	Number of Workers	Output per Worker	Value Added per Worker	Avg. Wage
<i>Panel A: Controlling for Additional Demographic Covariates</i>						
Onshore Endow. \times Price (β)	0.55*** (0.11)	0.19 (0.12)	-0.05 (0.06)	0.60*** (0.08)	0.27*** (0.09)	0.33*** (0.06)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-1.37 (1.54)	-1.08 (1.70)	1.33 (0.98)	-2.70** (1.21)	-2.54* (1.36)	-3.45*** (0.64)
$\beta - 0.3 \cdot \delta$	0.96* (0.54)	0.51 (0.59)	-0.45 (0.35)	1.41*** (0.42)	1.03** (0.47)	1.37*** (0.23)
<i>Panel B: Controlling for Baseline Central Expenditure Proxies</i>						
Onshore Endow. \times Price (β)	0.41** (0.16)	0.01 (0.17)	-0.12 (0.08)	0.53*** (0.12)	0.17 (0.13)	0.32*** (0.09)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.83 (1.52)	-0.41 (1.63)	1.64* (0.86)	-2.47* (1.30)	-2.18 (1.41)	-3.77*** (0.74)
$\beta - 0.3 \cdot \delta$	0.66 (0.56)	0.14 (0.60)	-0.61* (0.32)	1.27*** (0.47)	0.83* (0.50)	1.46*** (0.29)
<i>Panel C: Controlling for Additional Covariates, Central Expenditure Proxies</i>						
Onshore Endow. \times Price (β)	0.42** (0.17)	0.08 (0.18)	-0.12 (0.09)	0.54*** (0.13)	0.24* (0.14)	0.34*** (0.09)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.66 (1.69)	-0.53 (1.80)	1.68* (0.94)	-2.34* (1.42)	-2.35 (1.54)	-3.20*** (0.74)
$\beta - 0.3 \cdot \delta$	0.61 (0.62)	0.24 (0.66)	-0.63* (0.35)	1.24** (0.51)	0.94* (0.56)	1.30*** (0.28)
<i>Panel D: Omitting All Covariates</i>						
Onshore Endow. \times Price (β)	0.50*** (0.07)	0.13 (0.09)	-0.00 (0.05)	0.50*** (0.06)	0.16** (0.06)	0.23*** (0.05)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.25 (0.80)	0.95 (0.93)	0.81* (0.44)	-1.06 (0.68)	0.05 (0.76)	-1.90*** (0.51)
$\beta - 0.3 \cdot \delta$	0.57** (0.29)	-0.16 (0.32)	-0.25 (0.16)	0.82*** (0.25)	0.15 (0.26)	0.80*** (0.17)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1) for $K = 0$ after adding baseline covariates interacted with year effects. The estimates in Panel A control for all baseline characteristics in Appendix Table A.4, Panel B estimates control for the central expenditure proxies in Appendix Table A.4, Panel C estimates control for both sets of variables, and Panel D estimates omit all covariates. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Firm Outcomes: Dynamic Model, Robustness to Changing Covariate Set

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Controlling for Additional Demographic Covariates</i>						
Onshore Endow. \times Price (β)	0.85*** (0.14)	0.33** (0.14)	-0.01 (0.08)	0.86*** (0.10)	0.37*** (0.11)	0.42*** (0.07)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-1.02 (1.79)	0.27 (1.95)	1.07 (1.13)	-2.09 (1.44)	-0.95 (1.61)	-3.31*** (0.87)
$\beta - 0.3 \cdot \delta$	1.16* (0.64)	0.25 (0.68)	-0.33 (0.40)	1.49*** (0.50)	0.66 (0.55)	1.42*** (0.31)
<i>Panel B: Controlling for Baseline Central Expenditure Proxies</i>						
Onshore Endow. \times Price (β)	0.67*** (0.18)	0.11 (0.19)	-0.10 (0.10)	0.77*** (0.14)	0.26* (0.14)	0.44*** (0.10)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.17 (1.73)	1.21 (1.90)	1.44 (1.00)	-1.61 (1.50)	-0.39 (1.67)	-3.90*** (0.91)
$\beta - 0.3 \cdot \delta$	0.72 (0.64)	-0.25 (0.69)	-0.53 (0.37)	1.25** (0.54)	0.38 (0.59)	1.61*** (0.34)
<i>Panel C: Controlling for Additional Covariates, Central Expenditure Proxies</i>						
Onshore Endow. \times Price (β)	0.64*** (0.19)	0.16 (0.20)	-0.13 (0.10)	0.77*** (0.15)	0.33** (0.15)	0.45*** (0.10)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	0.04 (1.95)	1.13 (2.13)	1.55 (1.11)	-1.51 (1.65)	-0.59 (1.84)	-3.06*** (0.93)
$\beta - 0.3 \cdot \delta$	0.63 (0.71)	-0.18 (0.77)	-0.59 (0.41)	1.22** (0.59)	0.50 (0.66)	1.36*** (0.35)
<i>Panel D: Omitting All Covariates</i>						
Onshore Endow. \times Price (β)	0.81*** (0.08)	0.28*** (0.10)	0.04 (0.05)	0.77*** (0.07)	0.28*** (0.08)	0.34*** (0.07)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.25 (0.90)	2.01** (0.98)	0.71 (0.48)	-0.96 (0.78)	1.21 (0.82)	-1.82*** (0.59)
$\beta - 0.3 \cdot \delta$	0.88*** (0.32)	-0.32 (0.35)	-0.18 (0.17)	1.06*** (0.28)	-0.08 (0.30)	0.88*** (0.21)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (1) for $K = 3$ after adding baseline covariates interacted with year effects. The estimates in Panel A control for all baseline characteristics in Appendix Table A.4, Panel B estimates control for the central expenditure proxies in Appendix Table A.4, Panel C estimates control for both sets of variables, and Panel D estimates omit all covariates. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Firm Outcomes: Dynamic Model, Robustness to Controlling for Poverty

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Controlling for Poverty Rate in 1999</i>						
Onshore Endow. \times Price (β)	0.82*** (0.12)	0.27** (0.12)	0.01 (0.07)	0.80*** (0.09)	0.29*** (0.10)	0.38*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.95 (1.60)	0.52 (1.80)	0.61 (1.01)	-1.56 (1.39)	-0.21 (1.60)	-3.55*** (0.88)
$\beta - 0.3 \cdot \delta$	1.10** (0.56)	0.11 (0.61)	-0.17 (0.36)	1.27*** (0.47)	0.35 (0.53)	1.45*** (0.32)
<i>Panel B: Omitting Poorest 5 Percent of Districts</i>						
Onshore Endow. \times Price (β)	0.77*** (0.11)	0.19* (0.11)	-0.01 (0.07)	0.78*** (0.08)	0.24*** (0.09)	0.35*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.74 (1.54)	0.92 (1.72)	0.90 (1.00)	-1.63 (1.31)	-0.11 (1.50)	-3.91*** (0.90)
$\beta - 0.3 \cdot \delta$	0.99* (0.54)	-0.09 (0.59)	-0.28 (0.35)	1.27*** (0.45)	0.27 (0.51)	1.52*** (0.33)
Observations	476,824	457,824	476,812	476,812	457,824	475,957
Plants	51,560	50,708	51,560	51,560	50,708	51,465
Districts	259	259	259	259	259	259
<i>Panel C: Omitting Poorest 10 Percent of Districts</i>						
Onshore Endow. \times Price (β)	0.81*** (0.11)	0.20* (0.12)	0.00 (0.06)	0.81*** (0.09)	0.24** (0.10)	0.37*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.94 (1.58)	0.94 (1.79)	0.98 (1.05)	-1.91 (1.33)	-0.17 (1.55)	-4.03*** (0.88)
$\beta - 0.3 \cdot \delta$	1.09** (0.55)	-0.08 (0.61)	-0.29 (0.37)	1.38*** (0.46)	0.29 (0.52)	1.58*** (0.33)
Observations	468,283	449,624	468,271	468,271	449,624	467,424
Plants	50,512	49,680	50,512	50,512	49,680	50,418
Districts	246	246	246	246	246	246
<i>Panel D: Omitting Poorest 25 Percent of Districts</i>						
Onshore Endow. \times Price (β)	0.79*** (0.13)	0.16 (0.14)	-0.00 (0.07)	0.79*** (0.09)	0.21* (0.11)	0.32*** (0.08)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.83 (1.81)	0.97 (2.04)	1.32 (1.17)	-2.14 (1.47)	-0.51 (1.70)	-4.01*** (1.00)
$\beta - 0.3 \cdot \delta$	1.04 (0.64)	-0.13 (0.71)	-0.40 (0.41)	1.44*** (0.50)	0.36 (0.59)	1.52*** (0.36)
Observations	421,427	405,178	421,420	421,420	405,178	420,900
Plants	44,778	44,061	44,778	44,778	44,061	44,706
Districts	206	206	206	206	206	206

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (1) for $K = 3$. The estimates in Panel A control for the district poverty rate in 1999 interacted with year effects. Panels B, C, and D drop subsamples of poor districts. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Firm Outcomes: Static Model, Robustness to Controlling for District Splits

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Controlling for Post-Split Dummies</i>						
Onshore Endow. \times Price (β)	0.45*** (0.09)	0.06 (0.10)	-0.07 (0.06)	0.52*** (0.07)	0.16** (0.08)	0.26*** (0.06)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-1.20 (1.30)	-0.73 (1.48)	1.23 (0.88)	-2.44** (1.06)	-2.08* (1.20)	-3.90*** (0.64)
$\beta - 0.3 \cdot \delta$	0.81* (0.45)	0.28 (0.51)	-0.44 (0.31)	1.25*** (0.36)	0.78* (0.41)	1.43*** (0.24)
<i>Panel B: Controlling for Log Number of Districts</i>						
Onshore Endow. \times Price (β)	0.51*** (0.09)	0.09 (0.10)	-0.05 (0.06)	0.56*** (0.07)	0.18** (0.08)	0.27*** (0.06)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-2.15 (1.35)	-1.79 (1.55)	0.81 (0.91)	-2.96*** (1.11)	-2.74** (1.29)	-3.81*** (0.62)
$\beta - 0.3 \cdot \delta$	1.16** (0.47)	0.63 (0.52)	-0.29 (0.32)	1.45*** (0.38)	1.00** (0.44)	1.42*** (0.23)
<i>Panel C: Controlling for Ever Split \times Year Dummies</i>						
Onshore Endow. \times Price (β)	0.48*** (0.10)	0.13 (0.10)	-0.06 (0.06)	0.53*** (0.07)	0.22*** (0.08)	0.27*** (0.05)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.86 (1.31)	-0.60 (1.52)	1.29 (0.90)	-2.15** (1.09)	-2.01 (1.24)	-3.41*** (0.57)
$\beta - 0.3 \cdot \delta$	0.74 (0.46)	0.31 (0.52)	-0.44 (0.32)	1.18*** (0.38)	0.82* (0.42)	1.30*** (0.21)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1) for $K = 0$ after adding controls for district splits. The estimates in Panel A control for a dummy that equals one after the first district split and a dummy that equals one after the first split in the post-decentralization period (1999 and later). Panel B estimates control for the log number of districts contained within original district borders. Panel C estimates control for a dummy for ever splitting during 1975–2014 interacted with year dummies. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Firm Outcomes: Dynamic Model, Robustness to Controlling for District Splits

	Log(Outcome)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output	Value Added	Number of Workers	Output per Worker	Value Added per Worker	Avg. Wage
<i>Panel A: Controlling for Post-Split Dummies</i>						
Onshore Endow. \times Price (β)	0.78*** (0.11)	0.20* (0.11)	-0.01 (0.07)	0.79*** (0.08)	0.25*** (0.09)	0.35*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.80 (1.54)	0.70 (1.71)	0.91 (1.01)	-1.70 (1.27)	-0.33 (1.45)	-3.92*** (0.90)
$\beta - 0.3 \cdot \delta$	1.02* (0.54)	-0.01 (0.58)	-0.28 (0.36)	1.30*** (0.43)	0.35 (0.49)	1.52*** (0.33)
<i>Panel B: Controlling for Log Number of Districts</i>						
Onshore Endow. \times Price (β)	0.86*** (0.11)	0.27** (0.11)	0.01 (0.07)	0.85*** (0.09)	0.30*** (0.10)	0.37*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-1.64 (1.59)	-0.37 (1.76)	0.48 (1.06)	-2.12 (1.30)	-1.01 (1.50)	-3.71*** (0.84)
$\beta - 0.3 \cdot \delta$	1.36** (0.56)	0.38 (0.60)	-0.13 (0.38)	1.49*** (0.45)	0.61 (0.51)	1.48*** (0.30)
<i>Panel C: Controlling for Ever Split \times Year Dummies</i>						
Onshore Endow. \times Price (β)	0.81*** (0.11)	0.28*** (0.11)	-0.00 (0.07)	0.81*** (0.08)	0.32*** (0.09)	0.37*** (0.07)
Fiscal Endow. \times Price \times ($t \geq 1999$) (δ)	-0.55 (1.56)	0.72 (1.74)	0.95 (1.04)	-1.50 (1.31)	-0.36 (1.50)	-3.35*** (0.83)
$\beta - 0.3 \cdot \delta$	0.97* (0.55)	0.07 (0.60)	-0.29 (0.37)	1.26*** (0.45)	0.43 (0.51)	1.38*** (0.30)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (1) for $K = 3$ after adding controls for district splits. The estimates in Panel A control for a dummy that equals one after the first district split and a dummy that equals one after the first split in the post-decentralization period (1999 and later). Panel B estimates control for the log number of districts contained within original district borders. Panel C estimates control for a dummy for ever splitting during 1975–2014 interacted with year dummies. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Firm Outcomes: Robustness to Omitting Districts with Onshore Endowments

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Static Model (K = 0)</i>						
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	2.73 (3.73)	6.61 (4.88)	5.34** (2.10)	-2.61 (3.07)	1.30 (3.84)	-4.04*** (1.37)
Observations	402,675	386,015	402,668	402,668	386,015	401,825
Plants	43,731	42,963	43,731	43,731	42,963	43,639
Districts	241	241	241	241	241	241
<i>Panel B: Dynamic Model (K = 3)</i>						
Fiscal Endow. × Price × ($t \geq 1999$)	-1.76 (3.81)	1.86 (3.67)	4.38*** (1.65)	-6.14** (3.13)	-2.49 (3.24)	-3.41** (1.41)
Lag 1	1.92 (2.72)	1.97 (3.29)	1.03 (0.72)	0.88 (2.69)	0.94 (3.07)	-0.47 (1.68)
Lag 2	8.89** (3.55)	10.10*** (3.65)	0.39 (1.05)	8.51*** (3.14)	9.72*** (3.14)	-1.81 (2.82)
Lag 3	-4.92 (3.40)	-5.88** (2.78)	-0.21 (1.18)	-4.71 (3.01)	-5.68** (2.60)	1.59 (2.68)
Sum of Lags 0- K (δ)	4.12 (4.78)	8.05 (5.94)	5.58** (2.42)	-1.46 (4.03)	2.49 (4.68)	-4.11** (1.90)
Observations	402,675	386,015	402,668	402,668	386,015	401,825
Plants	43,731	42,963	43,731	43,731	42,963	43,639
Districts	241	241	241	241	241	241

Notes: This table presents fixed-effects estimates of $\{\delta_k\}$ in Equation (1), omitting districts with any onshore endowments. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Firm Outcomes: Robustness to Alternative Specifications of Shocks

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Avg. Wage
<i>Panel A: Allowing for Time-Varying Effects of Onshore Booms</i>						
Onshore Endow. \times Price $\times (t < 1999)$ (β^{pre})	0.50*** (0.10)	0.16 (0.12)	0.03 (0.05)	0.47*** (0.07)	0.18** (0.09)	0.18*** (0.06)
Onshore Endow. \times Price $\times (t \geq 1999)$ (β^{post})	0.36** (0.16)	-0.16 (0.18)	-0.25** (0.11)	0.61*** (0.14)	0.10 (0.15)	0.42*** (0.09)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.63 (1.58)	0.58 (1.87)	2.16** (1.06)	-2.79** (1.37)	-1.56 (1.62)	-4.66*** (0.76)
p -value: $\beta^{\text{pre}} = \beta^{\text{post}}$	0.451	0.137	0.015	0.347	0.651	0.027
<i>Panel B: Controlling for Offshore Booms</i>						
Onshore Endow. \times Price (β)	0.46*** (0.09)	0.06 (0.10)	-0.07 (0.06)	0.53*** (0.08)	0.17* (0.09)	0.26*** (0.06)
Offshore Endow. \times Price	-0.55 (0.55)	-1.11* (0.60)	0.59 (0.37)	-1.14** (0.46)	-1.77*** (0.57)	-0.71* (0.37)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	-0.91 (1.33)	-0.10 (1.54)	1.04 (0.82)	-1.95* (1.11)	-1.24 (1.27)	-3.63*** (0.66)
$\beta - 0.3 \cdot \delta$	0.73 (0.46)	0.09 (0.53)	-0.38 (0.30)	1.11*** (0.39)	0.54 (0.44)	1.35*** (0.25)
Observations	481,795	462,594	481,783	481,783	462,594	480,875
Plants	52,182	51,316	52,182	52,182	51,316	52,084
Districts	278	278	278	278	278	278

Notes: This table presents fixed-effects estimates of the coefficients in modified versions of Equation (1) for $K = 0$. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Log Non-Oil GDP: Robustness to Starting Fiscal Shock in 2001

	Log Non-Oil GDP			
	(1)	(2)	(3)	(4)
Onshore Endow. \times Price	-0.02 (0.26)	0.02 (0.22)	-0.00 (0.23)	0.00 (0.29)
Lag 1		-0.05 (0.10)	-0.05 (0.08)	-0.04 (0.08)
Lag 2			0.02 (0.10)	0.09 (0.07)
Lag 3				-0.12 (0.18)
Sum of Lags 0- K (β)	-0.02 (0.26)	-0.03 (0.27)	-0.03 (0.27)	-0.08 (0.25)
Fiscal Endow. \times Price \times ($t \geq 2001$)	4.64*** (1.29)	2.51*** (0.85)	2.07*** (0.80)	1.48* (0.87)
Lag 1		2.38*** (0.65)	1.44*** (0.42)	1.19*** (0.40)
Lag 2			1.60*** (0.62)	0.77** (0.39)
Lag 3				2.06** (0.88)
Sum of Lags 0- K (δ)	4.64*** (1.29)	4.89*** (1.35)	5.12*** (1.41)	5.50*** (1.47)
$\beta - 0.3 \cdot \delta$	-1.41** (0.59)	-1.50** (0.62)	-1.57** (0.64)	-1.73*** (0.65)
Observations	6,041	6,041	6,041	6,041
Districts	288	288	288	288

Notes: This table presents fixed-effects estimates of the coefficients in Equation (3) for $K \in \{0, 1, 2, 3\}$, measuring the fiscal shock as $P_t^i F_d \cdot 1(t \geq 2001)$. The sample covers 1993–2013. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Log Non-Oil GDP: Robustness to Omitting High-Leverage Districts

	Log Non-Oil GDP			
	(1) All Districts	(2) Omitting Kutai	(3) Omitting Bengkalis	(4) Omitting Kutai and Bengkalis
Onshore Endow. \times Price	-0.11 (0.30)	-0.17 (0.32)	-0.74 (1.21)	-1.50 (1.71)
Lag 1	0.10 (0.10)	0.07 (0.08)	0.17 (0.39)	0.00 (0.42)
Lag 2	0.06 (0.07)	0.08 (0.07)	0.15 (0.27)	0.27 (0.38)
Lag 3	-0.09 (0.20)	-0.03 (0.22)	0.31 (0.87)	0.79 (1.26)
Sum of Lags 0– K (β)	-0.03 (0.24)	-0.05 (0.24)	-0.11 (1.12)	-0.44 (1.36)
Fiscal Endow. \times Price \times ($t \geq 1999$)	2.36*** (0.77)	2.68*** (0.87)	2.62*** (0.90)	2.95*** (0.88)
Lag 1	0.24 (0.59)	0.48 (0.73)	0.20 (0.72)	0.50 (0.77)
Lag 2	0.93** (0.36)	0.91** (0.40)	0.96** (0.42)	0.92** (0.41)
Lag 3	2.12** (0.86)	1.49* (0.77)	1.98** (0.89)	1.35 (0.85)
Sum of Lags 0– K (δ)	5.64*** (1.56)	5.56*** (1.74)	5.76*** (1.69)	5.73*** (1.75)
$\beta - 0.3 \cdot \delta$	-1.72*** (0.67)	-1.71** (0.72)	-1.84 (1.45)	-2.16 (1.65)
Observations	6,041	6,020	6,020	5,999
Districts	288	287	287	286

Notes: This table presents fixed-effects estimates of the coefficients in Equation (3) for $K = 3$ and different samples of districts. The sample covers 1993–2013. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.19: Log Non-Oil GDP: Only Using Fields Discovered by 1993

	Log Non-Oil GDP			
	(1) All Districts	(2) Omitting Kutai	(3) Omitting Bengkalis	(4) Omitting Kutai and Bengkalis
Onshore Endow. \times Price	-0.11 (0.31)	-0.17 (0.32)	-0.75 (1.32)	-1.60 (1.92)
Lag 1	0.10 (0.09)	0.06 (0.08)	0.15 (0.42)	-0.06 (0.47)
Lag 2	0.06 (0.07)	0.07 (0.07)	0.10 (0.29)	0.20 (0.43)
Lag 3	-0.10 (0.20)	-0.04 (0.21)	0.22 (0.93)	0.70 (1.42)
Sum of Lags 0– K (β)	-0.06 (0.24)	-0.07 (0.25)	-0.28 (1.21)	-0.77 (1.49)
Fiscal Endow. \times Price \times ($t \geq 1999$)	2.37*** (0.77)	2.69*** (0.87)	2.64*** (0.93)	3.00*** (0.90)
Lag 1	0.24 (0.60)	0.48 (0.73)	0.21 (0.73)	0.52 (0.77)
Lag 2	0.94** (0.37)	0.92** (0.40)	0.98** (0.44)	0.94** (0.43)
Lag 3	2.14** (0.87)	1.51* (0.77)	2.05** (0.90)	1.40 (0.87)
Sum of Lags 0– K (δ)	5.69*** (1.58)	5.61*** (1.76)	5.87*** (1.72)	5.86*** (1.78)
$\beta - 0.3 \cdot \delta$	-1.77*** (0.67)	-1.76** (0.73)	-2.04 (1.55)	-2.53 (1.80)
Observations	6,041	6,020	6,020	5,999
Districts	288	287	287	286

Notes: This table presents fixed-effects estimates of the coefficients in Equation (3) for $K = 3$ and different samples of districts. The sample covers 1993–2013. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Endowments are based on oil and gas fields discovered by 1993. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.20: Log Non-Oil GDP: Robustness to Changing Covariate Set

	Log Non-Oil GDP			
	(1) All Districts	(2) Omitting Kutai	(3) Omitting Bengkalis	(4) Omitting Kutai and Bengkalis
<i>Panel A: Controlling for Additional Demographic Covariates</i>				
Onshore Endow. × Price (β)	-0.04 (0.25)	-0.06 (0.24)	0.18 (1.16)	-0.24 (1.31)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	4.99*** (1.62)	4.67** (1.88)	4.95*** (1.80)	4.82** (1.91)
$\beta - 0.3 \cdot \delta$	-1.54** (0.69)	-1.46* (0.76)	-1.31 (1.53)	-1.68 (1.65)
<i>Panel B: Controlling for Central Expenditure Proxies</i>				
Onshore Endow. × Price (β)	0.09 (0.31)	0.09 (0.30)	-0.32 (1.12)	-0.49 (1.49)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	5.03*** (1.70)	5.10*** (1.91)	5.33*** (1.84)	5.33*** (1.92)
$\beta - 0.3 \cdot \delta$	-1.42* (0.76)	-1.44* (0.80)	-1.92 (1.52)	-2.09 (1.79)
<i>Panel C: Controlling for Additional Covariates, Central Expenditure Proxies</i>				
Onshore Endow. × Price (β)	0.04 (0.30)	0.03 (0.29)	-0.08 (1.12)	-0.37 (1.42)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	4.67*** (1.64)	4.52** (1.93)	4.84*** (1.83)	4.75** (1.95)
$\beta - 0.3 \cdot \delta$	-1.36* (0.74)	-1.33* (0.80)	-1.53 (1.53)	-1.79 (1.74)
<i>Panel D: Omitting All Covariates</i>				
Onshore Endow. × Price (β)	0.15 (0.22)	0.15 (0.22)	0.12 (0.92)	0.17 (1.33)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	4.76*** (1.26)	4.89*** (1.50)	4.86*** (1.48)	4.92*** (1.55)
$\beta - 0.3 \cdot \delta$	-1.28** (0.58)	-1.32** (0.63)	-1.33 (1.29)	-1.31 (1.59)
Observations	6,041	6,020	6,020	5,999
Districts	288	287	287	286

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (3) for $K = 3$ and different samples of districts, after adding baseline covariates interacted with year effects. The estimates in Panel A control for the baseline characteristics in Appendix Table A.4, Panel B estimates control for the central expenditure proxies in Appendix Table A.4, Panel C estimates control for both sets of variables, and Panel D estimates omit all covariates. The sample covers 1993–2013. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.21: Log Non-Oil GDP: Robustness to Controlling for Poverty

	Log Non-Oil GDP			
	(1) All Districts	(2) Omitting Kutai	(3) Omitting Bengkalis	(4) Omitting Kutai and Bengkalis
<i>Panel A: Controlling for Poverty Rate in 1999</i>				
Onshore Endow. × Price (β)	-0.04 (0.24)	-0.06 (0.24)	-0.05 (1.12)	-0.39 (1.37)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	5.44*** (1.55)	5.32*** (1.72)	5.52*** (1.68)	5.47*** (1.75)
$\beta - 0.3 \cdot \delta$	-1.67** (0.66)	-1.65** (0.72)	-1.70 (1.46)	-2.03 (1.66)
Observations	6,041	6,020	6,020	5,999
Districts	288	287	287	286
<i>Panel B: Omitting Poorest 5 Percent of Districts</i>				
Onshore Endow. × Price (β)	-0.07 (0.25)	-0.05 (0.24)	-0.66 (0.90)	-0.68 (1.33)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	5.26*** (1.48)	5.41*** (1.64)	5.61*** (1.61)	5.61*** (1.64)
$\beta - 0.3 \cdot \delta$	-1.64** (0.65)	-1.67** (0.68)	-2.34* (1.26)	-2.36 (1.60)
Observations	5,726	5,705	5,705	5,684
Districts	273	272	272	271
<i>Panel C: Omitting Poorest 10 Percent of Districts</i>				
Onshore Endow. × Price (β)	-0.05 (0.25)	-0.03 (0.24)	-0.61 (0.91)	-0.61 (1.35)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	5.25*** (1.48)	5.41*** (1.64)	5.58*** (1.61)	5.60*** (1.65)
$\beta - 0.3 \cdot \delta$	-1.63** (0.65)	-1.65** (0.68)	-2.29* (1.27)	-2.29 (1.62)
Observations	5,436	5,415	5,415	5,394
Districts	259	258	258	257
<i>Panel D: Omitting Poorest 25 Percent of Districts</i>				
Onshore Endow. × Price (β)	0.00 (0.24)	0.02 (0.24)	-0.41 (1.06)	-0.61 (1.61)
Fiscal Endow. × Price × ($t \geq 1999$) (δ)	5.21*** (1.54)	5.14*** (1.70)	5.52*** (1.71)	5.45*** (1.75)
$\beta - 0.3 \cdot \delta$	-1.56** (0.67)	-1.52** (0.71)	-2.06 (1.43)	-2.25 (1.92)
Observations	4,512	4,491	4,491	4,470
Districts	215	214	214	213

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (3) for $K = 3$ and different samples of districts, after adding poverty in 1999 interacted with year effects (Panel A) or omitting high-poverty districts (Panels B–D). The sample covers 1993–2013. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.22: Log Non-Oil GDP: Robustness to Controlling for District Splits

	Log Non-Oil GDP			
	(1) All Districts	(2) Omitting Kutai	(3) Omitting Bengkalis	(4) Omitting Kutai and Bengkalis
<i>Panel A: Controlling for Post-Split Dummies</i>				
Onshore Endow. \times Price (β)	-0.02 (0.25)	-0.03 (0.24)	-0.07 (1.13)	-0.38 (1.38)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	5.67*** (1.57)	5.60*** (1.76)	5.79*** (1.70)	5.76*** (1.78)
$\beta - 0.3 \cdot \delta$	-1.72** (0.67)	-1.71** (0.73)	-1.81 (1.46)	-2.11 (1.67)
<i>Panel B: Controlling for Controlling for Log Number of Districts</i>				
Onshore Endow. \times Price (β)	0.05 (0.24)	0.05 (0.23)	-0.03 (1.11)	-0.18 (1.42)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	5.63*** (1.57)	5.63*** (1.73)	5.80*** (1.70)	5.80*** (1.76)
$\beta - 0.3 \cdot \delta$	-1.64** (0.66)	-1.64** (0.70)	-1.76 (1.44)	-1.92 (1.70)
<i>Panel C: Controlling for Ever Split \times Year Dummies</i>				
Onshore Endow. \times Price (β)	-0.02 (0.25)	-0.04 (0.25)	-0.10 (1.15)	-0.44 (1.39)
Fiscal Endow. \times Price $\times (t \geq 1999)$ (δ)	5.50*** (1.61)	5.42*** (1.80)	5.62*** (1.75)	5.59*** (1.82)
$\beta - 0.3 \cdot \delta$	-1.68** (0.69)	-1.67** (0.75)	-1.79 (1.50)	-2.12 (1.70)
Observations	6,041	6,020	6,020	5,999
Districts	288	287	287	286

Notes: This table presents fixed-effects estimates of $\beta = \sum_{k=0}^K \beta_k$ and $\delta = \sum_{k=0}^K \delta_k$ in Equation (3) for $K = 3$ and different samples of districts after adding controls for district splits. The estimates in Panel A control for a dummy that equals one after the first district split and a dummy that equals one after the first split in the post-decentralization period (1999 and later). Panel B estimates control for the log number of districts contained within original district borders. Panel C estimates control for a dummy for ever splitting during 1975–2014 interacted with year dummies. The sample covers 1993–2013. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.23: Log Non-Oil GDP: Robustness to Omitting Districts with Onshore Endowments

	Log Non-Oil GDP			
	(1)	(2)	(3)	(4)
Fiscal Endow. \times Price $\times (t \geq 2001)$	5.46** (2.30)	3.34*** (1.16)	3.10*** (1.01)	2.87*** (0.88)
Lag 1		2.34* (1.37)	1.53* (0.80)	1.54* (0.80)
Lag 2			1.20 (1.05)	0.81 (0.59)
Lag 3				0.74 (1.14)
Sum of Lags 0– K (δ)	5.46** (2.30)	5.68** (2.42)	5.83** (2.54)	5.96** (2.69)
Observations	5,267	5,267	5,267	5,267
Districts	251	251	251	251

Notes: This table presents fixed-effects estimates of $\{\delta_k\}$ Equation (3) for $K \in \{0, 1, 2, 3\}$, omitting districts with any onshore endowments. The sample covers 1993–2013. Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1993 population. Each coefficient therefore represents the percentage change in the outcome due to an increase in the value of the endowment by USD 10,000 per capita. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.24: Fiscal Multipliers: Robustness to Omitting High-Leverage Districts

	Cumulative Multiplier after h Years			
	$h = 0$ (1)	$h = 1$ (2)	$h = 2$ (3)	$h = 3$ (4)
<i>Panel A: Omitting Kutai</i>				
Multiplier (δ_h)	1.11** (0.56)	1.32* (0.71)	1.49* (0.76)	1.58** (0.80)
AR p -value: $\delta_h = 1$	0.829	0.602	0.424	0.342
AR p -value: $\delta_h = 0$	0.040	0.044	0.040	0.042
First-stage F -statistic	5.6	7.8	6.0	5.3
Observations	5,247	4,938	4,630	4,323
Districts	282	282	282	282
<i>Panel B: Omitting Bengkalis</i>				
Multiplier (δ_h)	1.16** (0.58)	1.35* (0.70)	1.50** (0.75)	1.57** (0.78)
AR p -value: $\delta_h = 1$	0.767	0.564	0.401	0.339
AR p -value: $\delta_h = 0$	0.028	0.033	0.032	0.037
First-stage F -statistic	6.0	8.4	6.5	5.6
Observations	5,244	4,934	4,625	4,317
Districts	282	282	282	282
<i>Panel C: Omitting Kutai and Bengkalis</i>				
Multiplier (δ_h)	1.09* (0.56)	1.30* (0.69)	1.49** (0.76)	1.60** (0.81)
AR p -value: $\delta_h = 1$	0.862	0.621	0.423	0.325
AR p -value: $\delta_h = 0$	0.042	0.042	0.038	0.038
First-stage F -statistic	5.9	8.4	6.3	5.4
Observations	5,228	4,920	4,613	4,307
Districts	281	281	281	281

Notes: This table presents instrumental-variables estimates of δ_h in Equation (6). The instrument is $P_t' F_d \cdot 1(t \geq 1999)$, expressed as a cumulative change over h years relative to 1998 non-oil GDP. All regressions control for $P_t' E_d$, expressed using the same transformation. Reported p -values are from the [Anderson and Rubin \(1949\)](#) test, which is robust to the presence of weak instruments. The first-stage F -statistic is the [Kleibergen and Paap \(2006\)](#) rk statistic. The sample covers 1994–2013. The five districts in Jakarta are missing, as these districts are managed by the province. Standard errors, reported in parentheses, are clustered at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

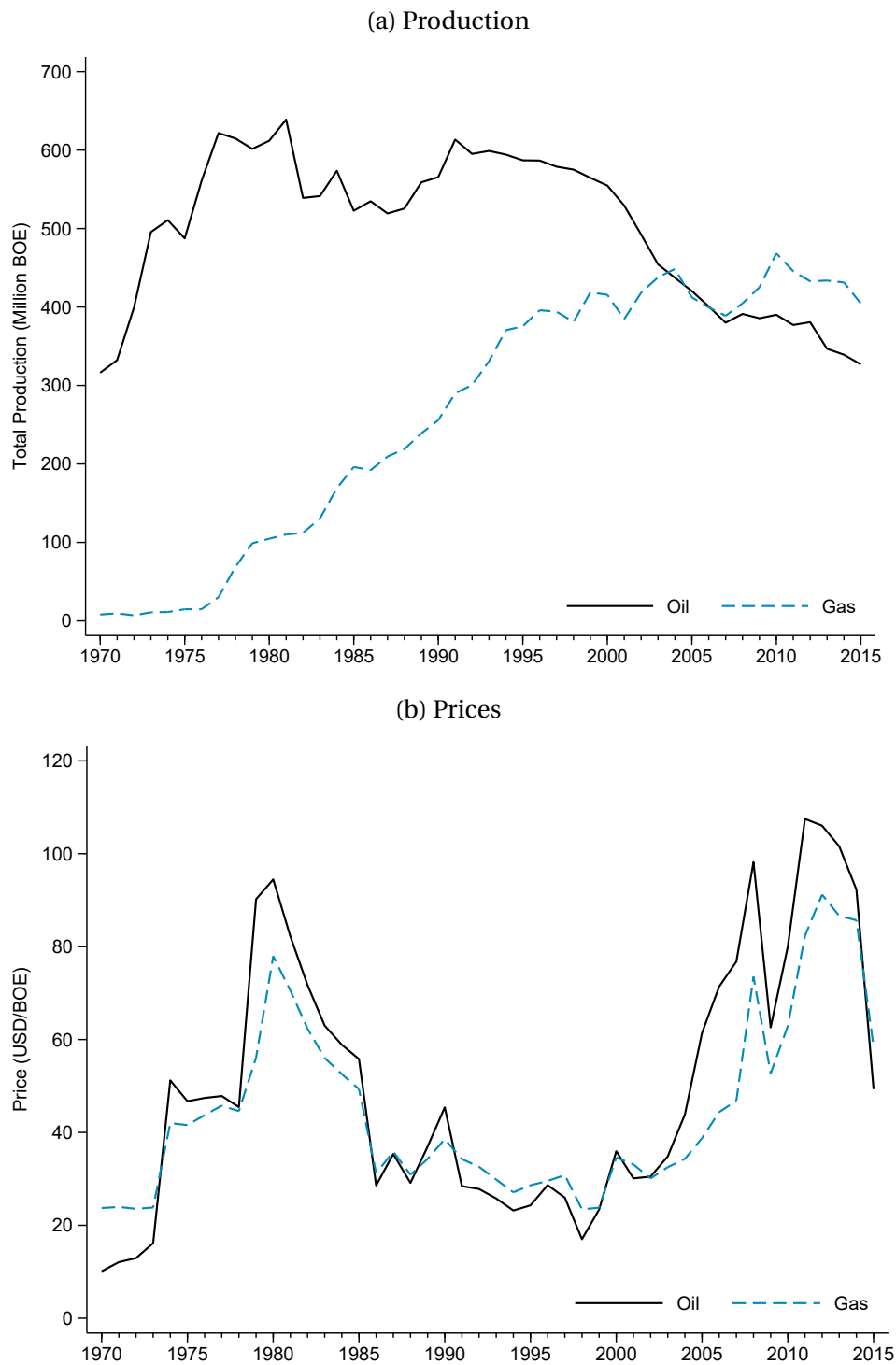
Table A.25: Association Between Shocks and District Splitting

	Cross-Sectional Model		Hazard Model
	(1) Ever Split	(2) Ever Split Post-1999	(3) First Split Post-1999
Onshore Endow. \times Price	0.12 (0.71)	0.31 (0.73)	1.03 (3.24)
Lag 1			-5.14 (5.41)
Lag 2			4.33 (3.87)
Lag 3			0.77 (4.57)
Sum of Lags 0– K (β)	0.12 (0.71)	0.31 (0.73)	0.99 (0.69)
Fiscal Endow. \times Price \times ($t \geq 1999$)	2.56 (1.83)	1.23 (1.36)	1.05 (3.00)
Lag 1			0.17 (4.44)
Lag 2			1.10 (6.53)
Lag 3			-1.46 (6.96)
Sum of Lags 0– K (δ)	2.56 (1.83)	1.23 (1.36)	0.86 (1.00)
Outcome Mean	0.389	0.368	0.010
p -value: $\beta = 0.3 \cdot \delta$	0.550	0.949	0.351
Observations	271	271	1,105
Districts	271	271	280

Notes: This table reports average marginal effects (AMEs) and sums of AMEs from probit regressions. The estimates in columns 1–2 use the cross-section of districts in the year 2000 and control for region effects. (Nine districts are missing, because all districts in Papua split at some point, so the Papua dummy perfectly predicts the outcome.) The estimates in column 3 are from a hazard model applied to panel data, controlling for region-by-year effects. All regressions control for the baseline covariates included in Equation (1). *Ever Split* is an indicator equal to one if the district experienced a split during 1975–2014, while *Ever Split Post-1999* is an indicator equal to one if the district experienced a split during 1999–2014. *First Split Post-1999* equals zero in years prior to the first split during 1999–2014, equals one in the year the district first split during that period, and is set to missing thereafter. The split year is defined as the first full calendar year following the passage of legislation creating the new district(s). Each endowment \times price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.5 Additional Results: Figures

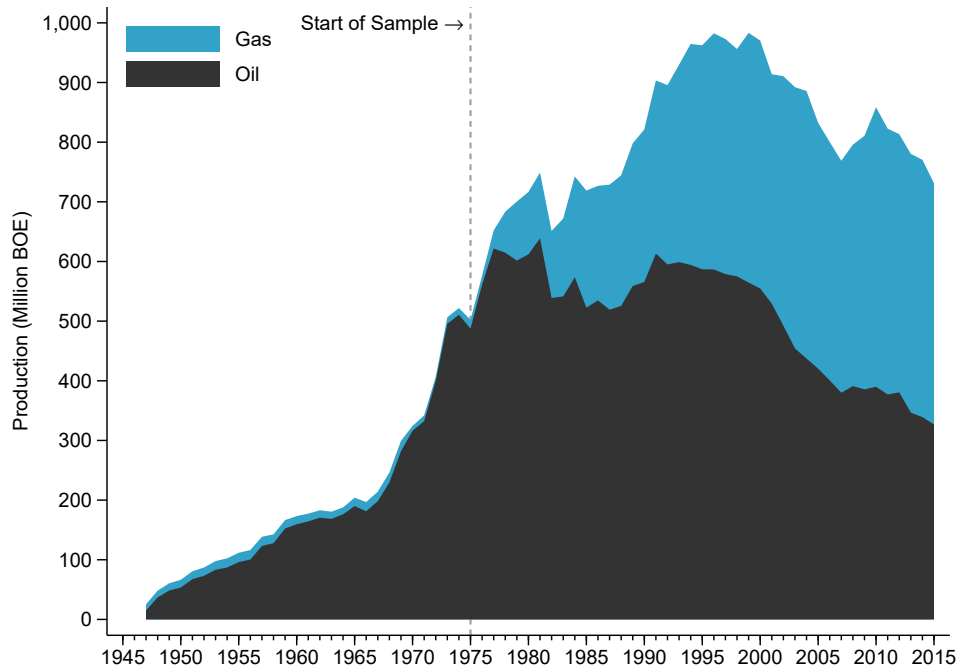
Figure A.3: Oil and Gas Production and Prices



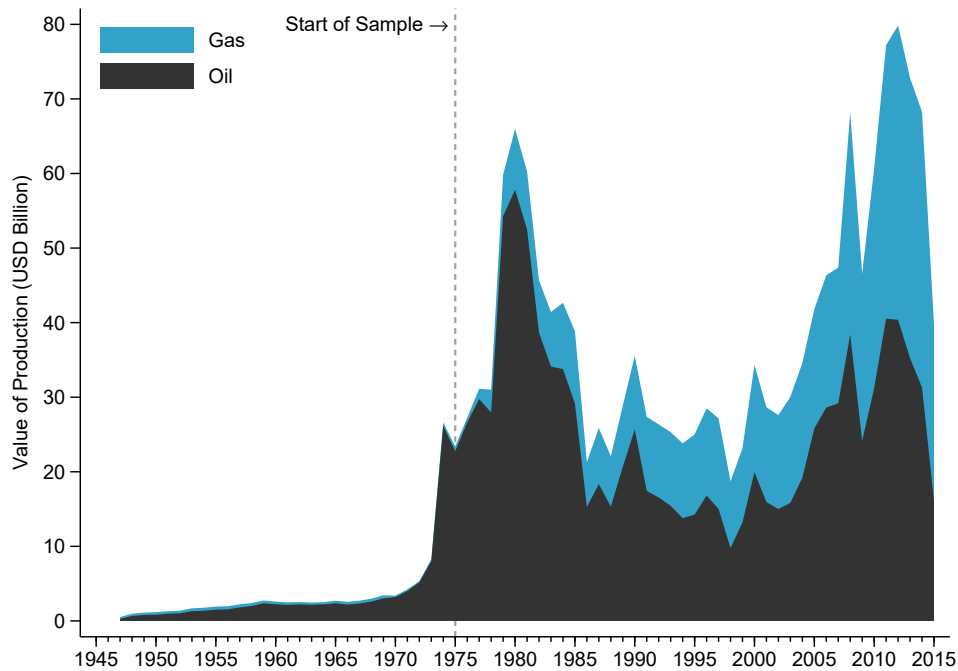
Notes: Prices are expressed in constant 2010 USD.

Figure A.4: Historical Oil and Gas Production

(a) Production

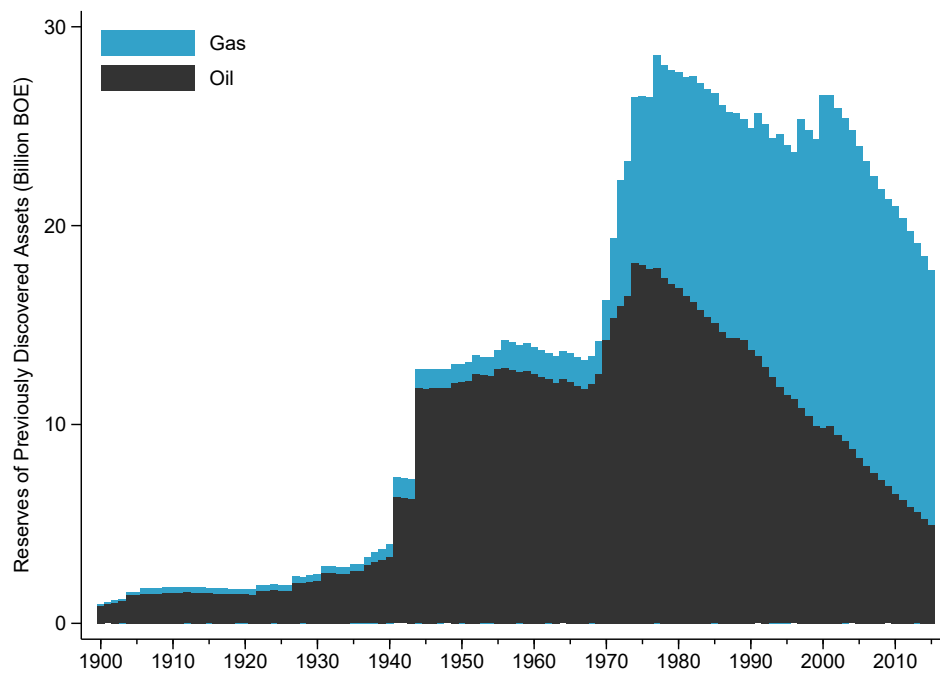


(b) Value of Production



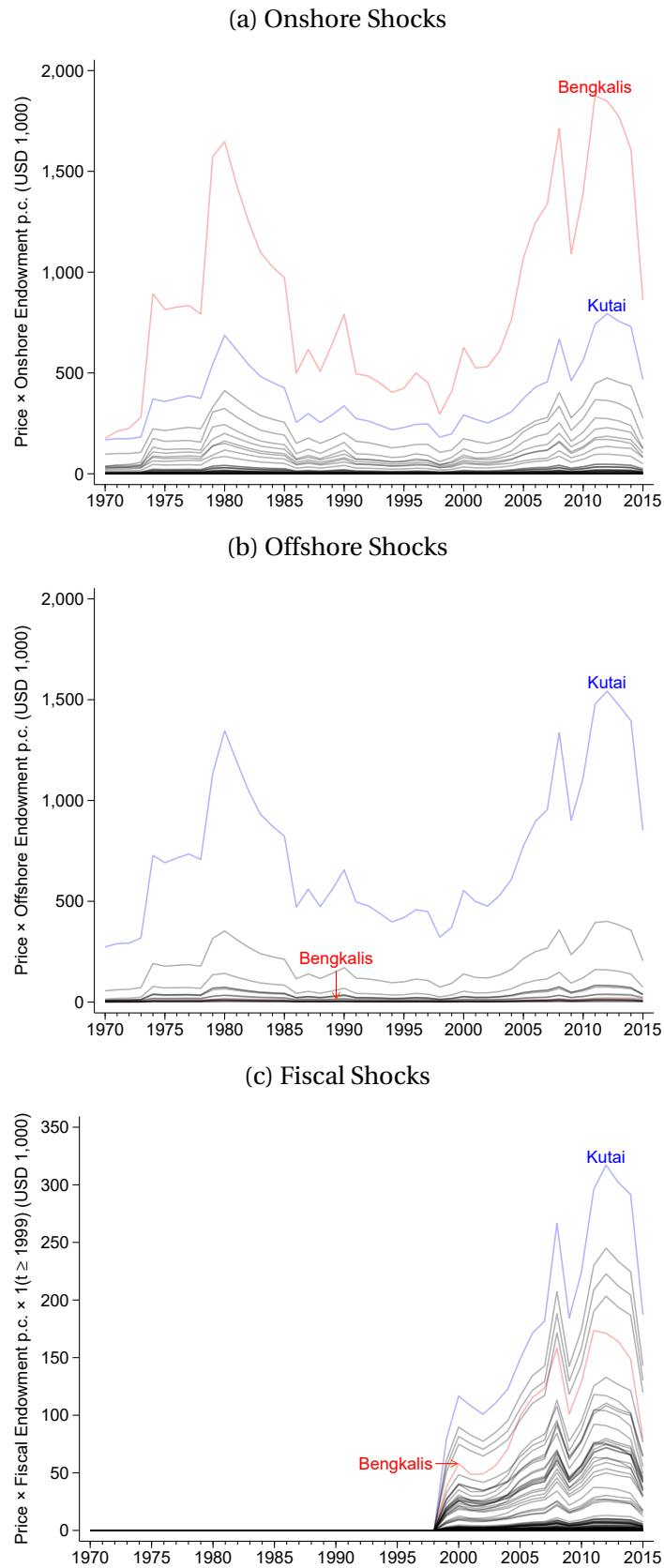
Notes: In Panel (b) production is expressed in millions of barrels of oil equivalent. In Panel (a) the value of production is expressed in constant 2010 USD billions. The production data are missing prior to independence in 1947.

Figure A.5: Historical Oil and Gas Reserves



Notes: Reserves are expressed in billions of barrels of oil equivalent.

Figure A.6: Onshore, Offshore, and Fiscal Shocks by District

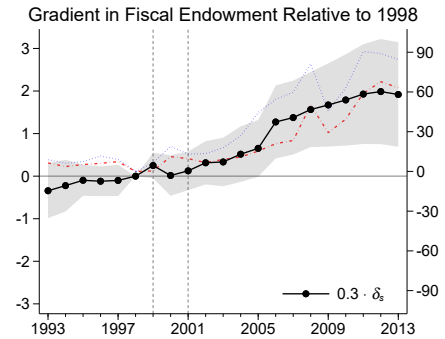
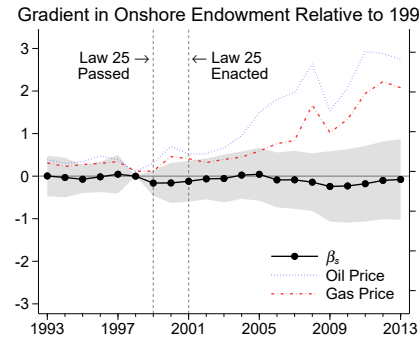
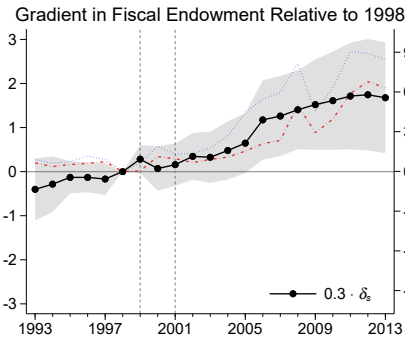
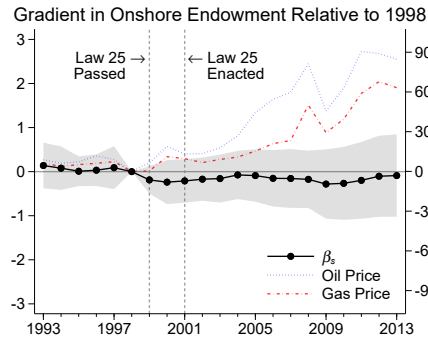


Notes: Panel (a) plots the onshore shock, $P_t' E_d$, separately for each district. Panel (b) plots the offshore shock, while Panel (c) plots the fiscal shock, $P_t' F_d \cdot 1(t \geq 1999)$. Shocks are expressed in constant 2010 USD thousands per capita based on 1980 population.

Figure A.7: Event Study Robustness: Log Non-Oil GDP

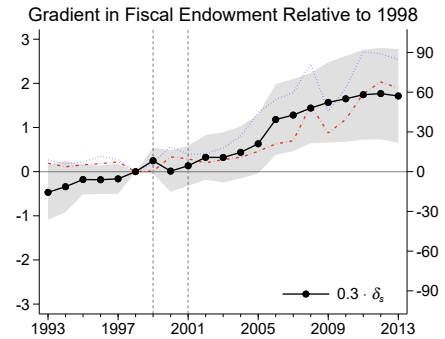
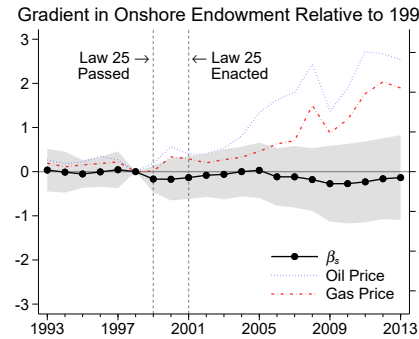
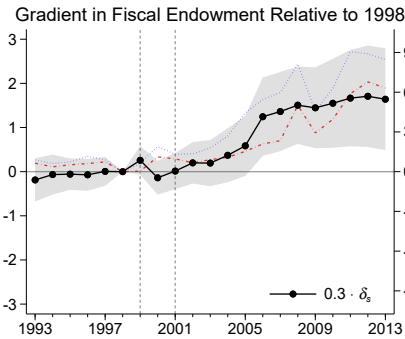
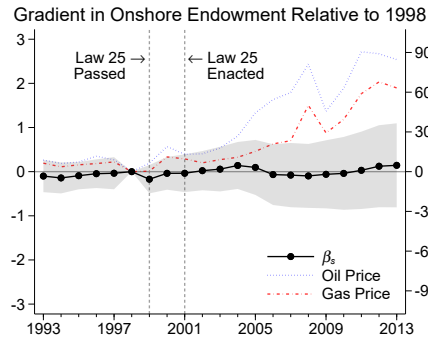
(a) Controlling for Additional Demographic Covariates

(b) Drop Kota Batam (Administrative Reclassification)



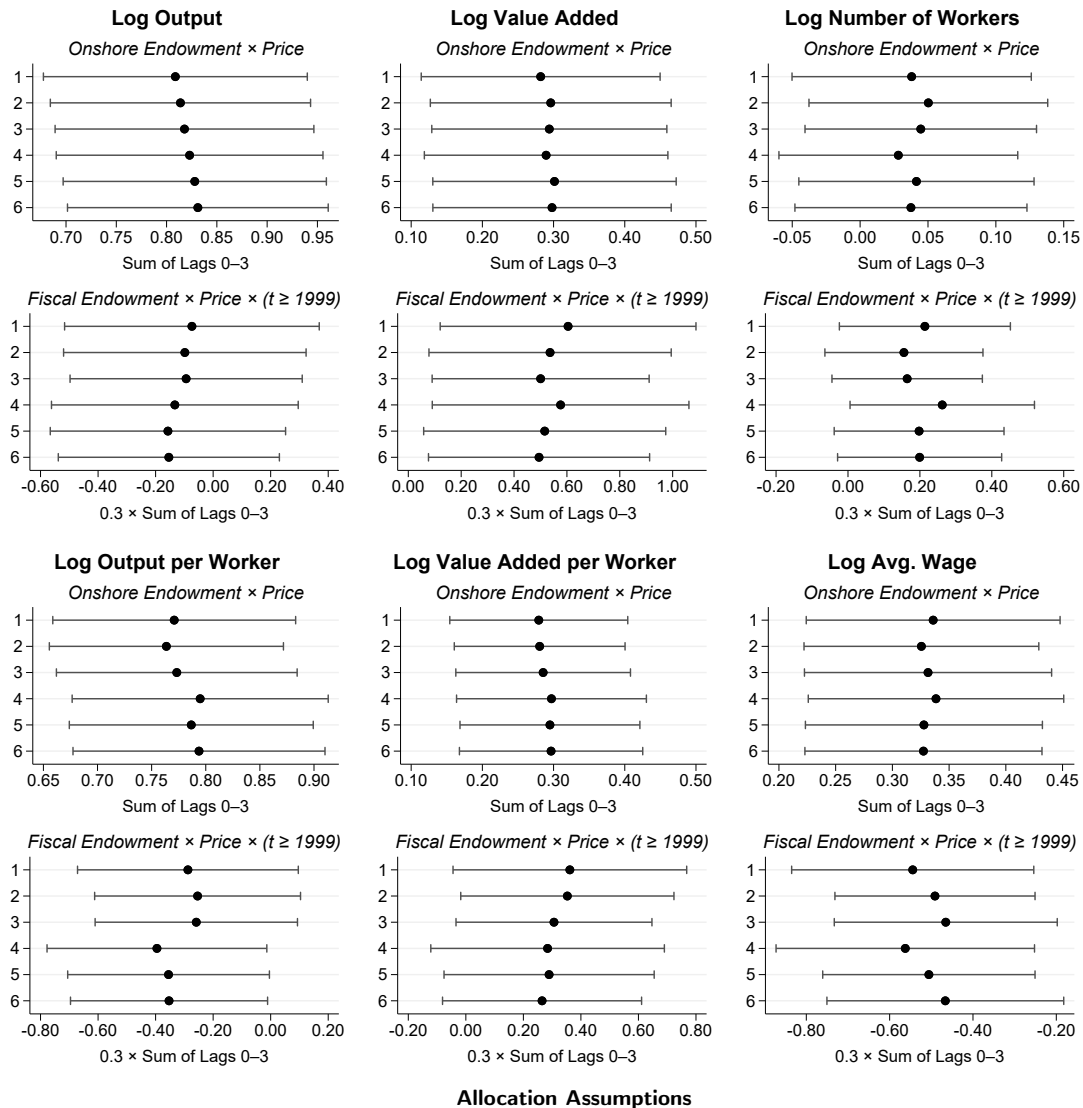
(c) Drop Kabupaten Berau (Several Coal Mine Openings)

(d) Drop Kabupaten Fak-Fak (Grasberg Gold Mine Opening)



Notes: This figure plots estimates and 95-percent confidence intervals for $\{\beta_s\}$ and $\{0.3 \cdot \delta_s\}$ in Equation (4) (left axis). Vertical dashed lines indicate the years the decentralization law was passed and enacted. The estimates in Panel A control for all baseline characteristics in Appendix Table A.4 interacted with year effects. Panels B–D drop districts that experienced spikes in non-oil GDP due to causes unrelated to oil or gas. Panel B drops Kota Batam, which was converted from an administrative municipality to an autonomous municipality in 1999. Panel C drops Kabupaten Berau, which saw major coal mine openings in 1992, 1995, and 2001. Panel D drops Kabupaten Fak-Fak, which experienced high non-oil GDP volatility due to the massive Grasberg gold and copper mine (located in child district Mimika). Oil and gas prices are expressed in deviations from their 1998 values, in constant 2010 USD per barrel of oil equivalent (right axis). Confidence intervals are robust to clustering at the level of 1993 district borders.

Figure A.8: Varying Assumptions about How Provincial Revenue is Allocated to Districts

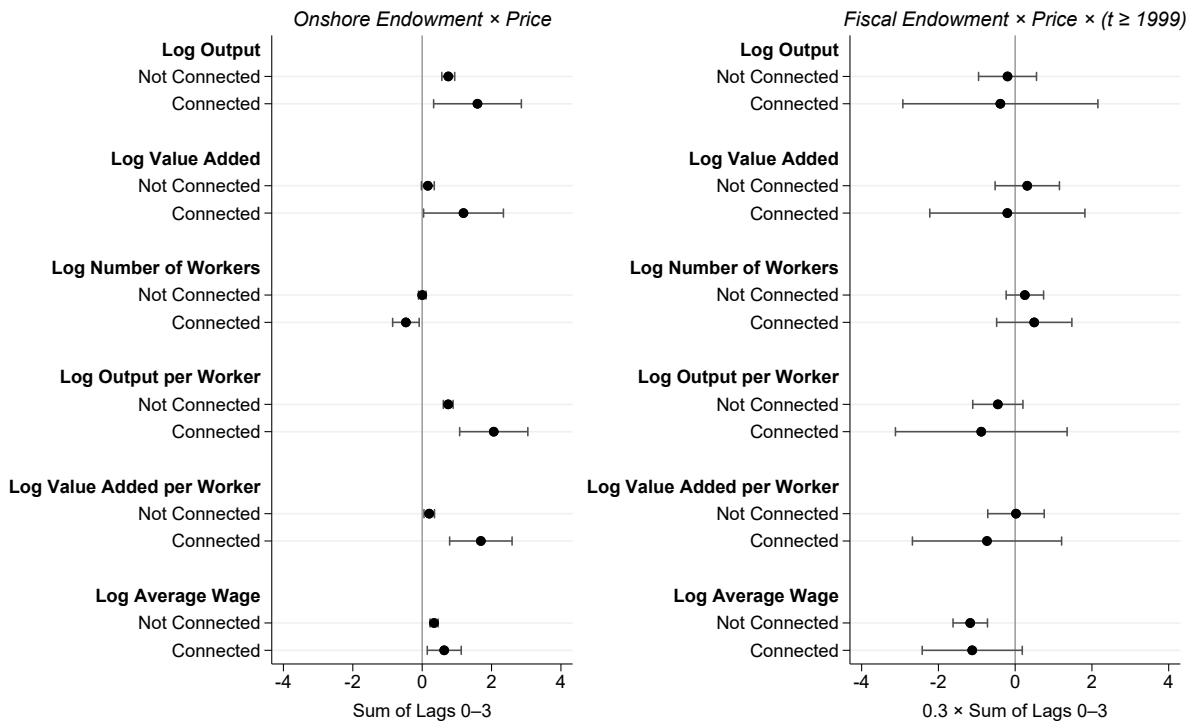


- Allocation Assumptions**
- 1: Equal per capita (baseline)
 - 2: Equal per district
 - 3: National sharing formula
 - 4: Equal per capita, no extra funds for Aceh/Papua
 - 5: Equal per district, no extra funds for Aceh/Papua
 - 6: National sharing formula, no extra funds for Aceh/Papua

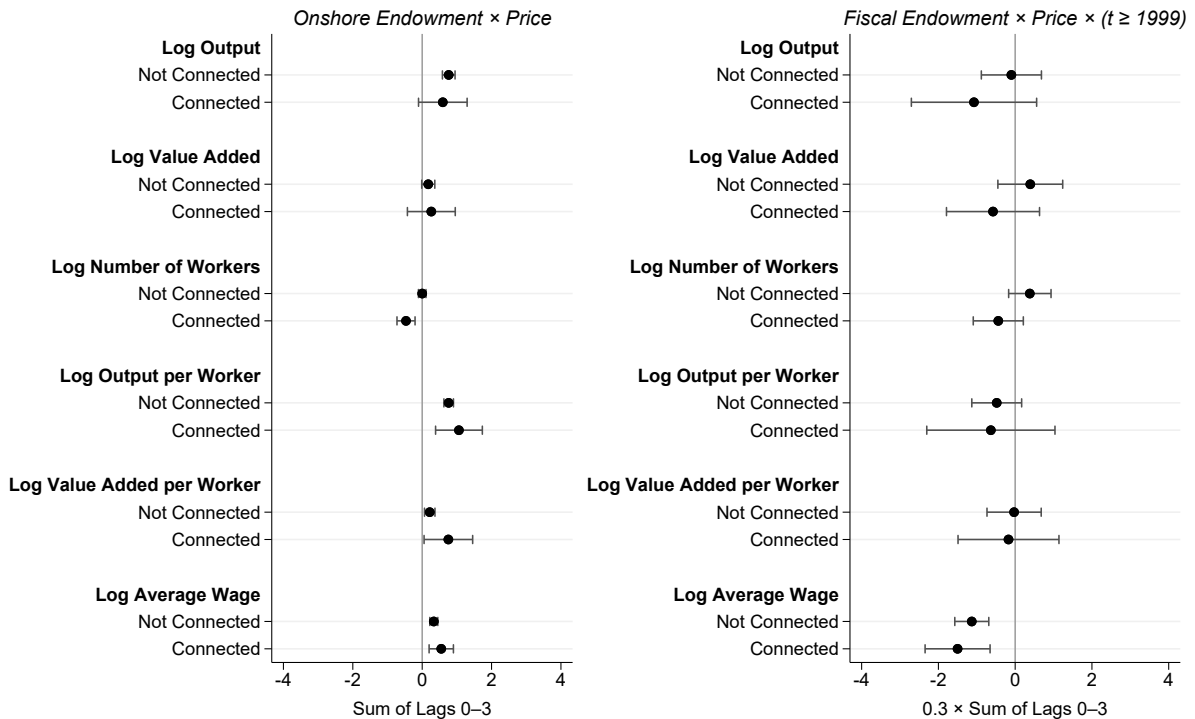
Notes: This figure plots fixed-effects estimates and 95-percent confidence intervals for $\sum_{k=0}^3 \beta_k$ and $0.3 \cdot \sum_{k=0}^3 \delta_k$ in Equation (1), using six versions of the fiscal endowment variable. Each employs a different assumption about how provincial revenue is distributed across districts. The first three assumptions are: (1) equal per capita amounts (baseline), (2) equal amounts to each district, and (3) greater amounts to producing districts following the national government's sharing formula. Assumptions 4–6 mirror assumptions 1–3, except they apply the same sharing rule to Aceh, Papua, and Papua Barat that is used in the rest of the country, rather than allocating extra revenue to these regions. The fiscal shock coefficients are scaled by 0.3 to facilitate comparison of the effects of typical onshore and fiscal shocks. Confidence intervals are robust to clustering at the level of 1975 district borders.

Figure A.9: Heterogeneous Effects by Oil-Industry Connectedness

(a) Baseline Measure of Oil-Connectedness

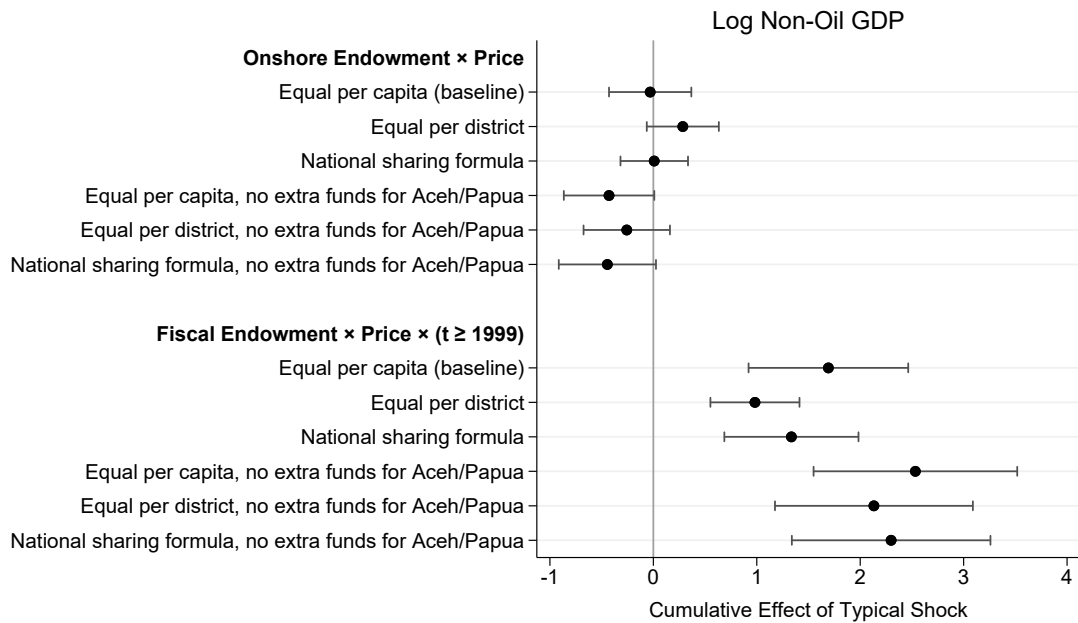


(b) Measure Adding Rubber and Plastic Industry



Notes: This figure plots estimates and 95-percent confidence intervals for $\sum_{k=0}^3 \beta_k^N$, $\sum_{k=0}^3 \beta_k^C$, $0.3 \cdot \sum_{k=0}^3 \delta_k^N$, and $0.3 \cdot \sum_{k=0}^3 \delta_k^C$ in Equation (5). In Panel (a) we define oil-connected industries as sectors 23 (fuel, oil and coal refining, petroleum products, and nuclear fuel), 24 (chemicals and chemical products), 27 (basic metal), 28 (metal goods, not machinery and equipment), and 29 (machinery and equipment). In Panel (b) we add sector 25 (rubber and plastic products industry) to the set of oil-connected industries. (See Appendix Table A.1.)

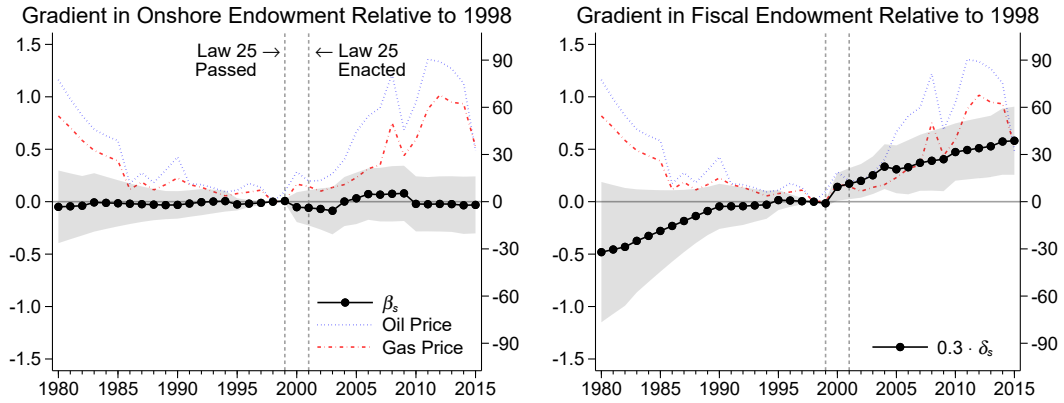
Figure A.10: Varying Assumptions about How Provincial Revenue is Allocated to Districts



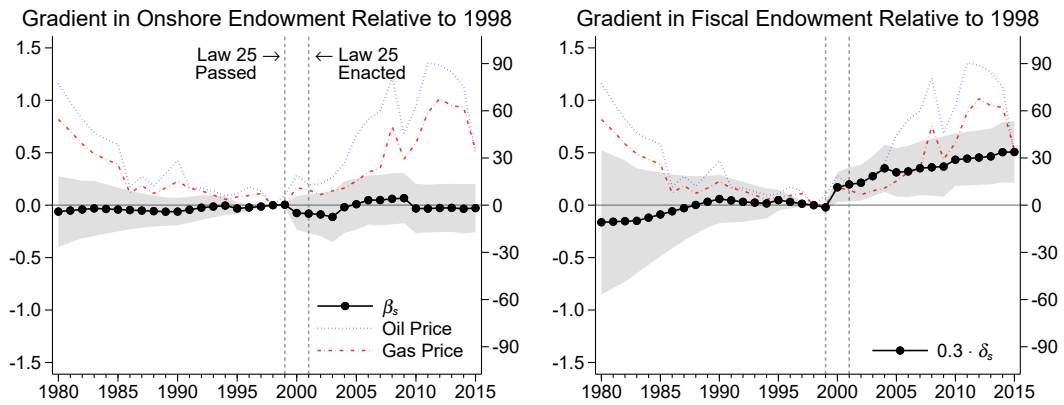
Notes: This figure plots fixed-effects estimates and 95-percent confidence intervals for $\sum_{k=0}^3 \beta_k$ and $0.3 \cdot \sum_{k=0}^3 \delta_k$ in Equation (3), using six versions of the fiscal endowment variable. Each employs a different assumption about how provincial revenue is distributed across districts. The first three assumptions are: (1) equal per capita amounts (baseline), (2) equal amounts to each district, and (3) greater amounts to producing districts following the national government's sharing formula. Assumptions 4–6 mirror assumptions 1–3, except they apply the same sharing rule to Aceh, Papua, and Papua Barat that is used in the rest of the country, rather than allocating extra revenue to these regions. The fiscal shock coefficients are scaled by 0.3 to facilitate comparison of the effects of typical onshore and fiscal shocks. Confidence intervals are robust to clustering at the level of 1993 district borders.

Figure A.11: Event Study: Log Population

(a) Baseline Specification



(b) Controlling for Additional Demographic Covariates



Notes: This figure plots estimates and 95-percent confidence intervals for $\{\beta_s\}$ and $\{0.3 \cdot \delta_s\}$ in Equation (4) (left axis). Vertical dashed lines indicate the years the decentralization law was passed and enacted. Oil and gas prices are expressed in deviations from their 1998 values, in constant 2010 USD per barrel of oil equivalent (right axis). Confidence intervals are robust to clustering at the level of 1975 district borders.