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Abstract

We examine the economic impacts of natural resource revenue-sharing systems, where central governments transfer a portion of resource revenue to producing regions. Using a natural experiment in Indonesia, we separately identify the effects of shared revenue and resource extraction. Contrary to Dutch disease concerns, shared oil and gas revenue does not harm local manufacturing firms, while extraction promotes manufacturing growth. Both extraction and shared revenue significantly raise local non-oil GDP. We find suggestive evidence of larger gains from shared revenue in areas without onshore extraction, implying central governments could improve aggregate welfare by channeling more resource revenue toward resource-poor areas.

JEL codes: H77, O13, O14, Q32, Q33
Keywords: Growth, resource booms, decentralization, manufacturing firms, Indonesia, Dutch disease

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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.[1] The majority employ a derivation-based system where a portion of the revenue is distributed to the producing region and, potentially, adjacent areas.[2] Such revenue-sharing schemes aim to satisfy local demands to share extraction benefits, compensate producing regions for environmental damage, and defuse resource-related conflicts (Bauer et al., 2016). However, the economic consequences of these policies remain an open question.

Fiscal windfalls could promote economic growth by financing investments in human and physical capital. On the other hand, windfalls could also drive up input costs for firms, causing the non-resource traded sector to shrink, a phenomenon known as Dutch disease (Corden and Neary, 1982). These dynamics could lower overall growth if the non-resource traded sector is characterized by learning by doing or other positive spillovers (Wijnbergen, 1984). Windfalls could also have different effects in resource-producing and non-producing regions. For instance, resource-producing regions may have better preexisting infrastructure, due to previous public investments facilitating the export of the resource. These areas may also have tight factor markets, thanks to the booming resource sector. In both cases, fiscal windfalls could generate greater benefits in non-producing regions.

Central governments in resource-rich countries therefore face a policy dilemma: whether to share a disproportionate amount of resource revenue with producing regions or distribute the benefits more evenly across the country. Consequently, understanding the economic impacts of revenue sharing, distinct from the direct effects of extraction, as well as their potential interactions, is crucial. However, separately identifying these two effects is challenging, as extraction and revenue are inextricably linked in most contexts.[3]

This paper exploits a national reform in Indonesia to estimate the separate economic effects of shared resource revenue and resource extraction. Prior to 2001, Indonesia was highly centralized, and hydrocarbon-rich regions did not receive a disproportionate share of revenue from natural resource extraction. In 2001, the country implemented comprehensive

[1]The alternative is to pool resource revenue with non-resource revenue in the intergovernmental transfer system.

[2]Derivation-based distribution can be achieved via local taxation or intergovernmental grants. Countries that have implemented a derivation-based system for all or part of their mineral, oil, or gas revenues include Angola, Bolivia, Brazil, Cameroon, Canada (some regions), Chad, China, Colombia, the Democratic Republic of the Congo, Ecuador, Ethiopia, Ghana, Guinea, India, Indonesia, Iraq, Italy, Kyrgyzstan, Madagascar, Malaysia, Mexico, Mongolia, Niger, Nigeria, Papua New Guinea, Peru, the Philippines, South Sudan, Uganda, the United States (some regions), and Venezuela (Bauer, Gankhuyag, Manley, Halling and Venugopal, 2016). A few countries instead use an indicator-based system, tying revenue to local population, poverty, or other characteristics.

[3]In 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 municipalities through which oil and gas is transported, i.e., only municipalities where extraction has an economic footprint.

decentralization reforms, including a revenue-sharing scheme that redirected a significant portion of natural resource revenue to producing districts and other districts within the same province. This reform resulted in a permanent increase in public expenditure for hydrocarbon-rich districts and their neighbors relative to hydrocarbon-poor areas. Crucially, while producing districts were exposed to resource extraction shocks both before and after 2001, the fiscal windfall from shared revenue only occurred post-reform. The introduction of the reform, coupled with specific features of the sharing rule, enables us to separately identify the effects of resource extraction and shared revenue, contributing to our understanding of fiscal federalism and natural resource economics.

We measure resource-related shocks as the interaction between current resource prices and predetermined endowments. For the resource extraction shock, we calculate the product of current oil and gas prices and the district's onshore oil and gas endowments per capita. This measure uses only onshore endowments, as offshore extraction activity has minimal effect on local economies.[4] To quantify the shared oil and gas revenue shock, we interact current resource prices with the district's per capita fiscal endowments (the stock of resources relevant for revenue-sharing purposes) and an indicator for the post-decentralization period. To determine each district's claim on deposits for revenue-sharing purposes, we apply the sharing rule to physical endowments. Notably, many districts are entitled to substantial revenue transfers despite having no onshore production, due to either offshore endowments or the presence of hydrocarbons elsewhere in the province.

In the first part of the analysis, we use manufacturing firm census data spanning 1975–2014 to estimate the effects of extraction and fiscal windfalls on firm outcomes. We find that oil and gas extraction booms increase manufacturing output, value added, labor productivity, and wages. In contrast, fiscal windfalls result in reduced wages, especially in districts without onshore extraction, while showing minimal impact on output or productivity. The results are robust to controlling for a wide array of potential confounders. Notably, while the observed increase in wages due to resource extraction aligns with classic Dutch disease mechanisms, we find no evidence of harm to productivity—a finding that contradicts typical Dutch disease expectations.

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 impacts on all non-oil sectors of the economy, which is crucial given that manufacturing accounts for only about 20 percent of GDP. We find that both resource extraction and fiscal windfalls generate positive economic spillovers, significantly boosting non-oil GDP. Both our baseline regressions and difference-in-differences estimates suggest larger gains from windfalls in districts lacking onshore endowments. However, this evidence is merely suggestive, as the difference is not statistically significant.

A growing empirical literature investigates the economic impacts of natural resource

[4] See,e.g., Caselli and Michaels (2013). In Section 5.2 we test this assumption.

abundance at the subnational level.[5] Our paper contributes to this literature by estimating the separate effects of resource extraction and shared revenue and their interaction, rather than estimating their compound effect. Most relevant to our paper is unpublished work by Cust and Rusli (2015).[6,7] Their paper uses offshore oil and gas production as an instrument for district expenditures in Indonesia, arguing that offshore extraction only affects district outcomes via shared revenue. Using data from 1998 to 2009, they conclude that spending from the fiscal transfers, rather than the effects of (onshore) extraction, drives the observed gains in GDP from oil and gas booms.

Our paper offers several advantages. First, we use microdata on manufacturing firms, not just GDP, enabling us to test for Dutch disease mechanisms. Second, our analysis spans a longer timeframe (1975–2014), encompassing two boom-and-bust cycles: one before and one after the adoption of revenue sharing (Figure 1, Panel (a)). We also employ an extended subnational GDP series (1993–2013). Third, our identification strategy captures both the permanent increase in revenue and expenditure due to the introduction of revenue sharing, and transitory changes from price fluctuations. This contrasts with the strategy in Cust and Rusli (2015), which only captures transitory revenue shocks. This is important, because districts tend to provide lumpy public services based on permanent public revenue (Cassidy, 2023). Thus, we can detect positive effects of shared revenue through improved infrastructure and public services. Finally, we estimate heterogeneous effects of windfalls based on districts' onshore extraction status, providing policy-relevant insights.

A related literature examines the impact of oil and gas revenue transfers to local governments on public expenditure and public service delivery. Recent works in this area include Caselli and Michaels (2013), Besfamille, Jorrat, Manzano, Quiroga and Sanguinetti (2023), Martínez (2023), and Cassidy (2023). 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

[5] See Cust and Poelhekke (2015) for a survey. Recent examples include Michaels (2011), Allcott and Keniston (2017), Feyrer, Mansur and Sacerdote (2017), Cust, Harding and Vézina (2019), Cavalcanti, Da Mata and Toscani (2019), Bartik, Currie, Greenstone and Knittel (2019), Pelzl and Poelhekke (2021), and Benguria, Saffie and Urzua (2024).

[6] Olsson and Valsecchi (2015) estimate the effect of shared oil and gas revenue in Indonesia on several outcomes, including regional GDP, public services, employment, and household consumption. Kraus, Heilmayr and Koch (2024) look at whether Indonesia's palm oil boom leads to Dutch disease. Naylor, Higgins, Edwards and Falcon (2019) examine the influence of various aspects of Indonesia's decentralization reforms on the expansion of the palm oil industry.

[7] Two other studies on natural resource production and Dutch disease in Indonesia do not separately estimate the effects of resource revenues sharing. Cust et al. (2019) find that oil and gas shocks increase wages, employment, firm output, and labor productivity using firm-level data from the 1990 to 2008. As the authors note, their estimates reflect the combined effect of resource extraction and fiscal windfalls. Pelzl and Poelhekke (2021), using the same manufacturing firm census from 1990 to 2009, find that mining booms increase manufacturing wages in districts with labor-intensive mining operations. Mining revenues are also shared with producing districts, so these estimates similarly conflate the effects of revenue and extraction. They find that oil and gas booms do not affect manufacturing wages or employment.

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 extraction 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 revenues (Arndt, 1983). The importance of oil and gas to the local economy varies dramatically across the country, as illustrated by the locations of Indonesia's 775 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 export revenues, 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.[8] 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.[9] This strategy aimed to appease local interests and deter provincial secession,

[8] See Appendix Figures A.1, A.2, and A.3 for more details.

[9] The 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

a concern heightened by East Timor's 1999 independence and fears that resource-rich provinces might seek autonomy if fiscally self-sufficient.

To further mitigate secessionist risks, the central government established a natural resource revenue-sharing scheme with producing regions, fundamentally altering the distribution of resource wealth 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, and 6 percent equally divided among other districts 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 equally divided among other districts in the province.[10] The provincial governments of Aceh, Papua, and West Papua receive an additional 55 percent of oil revenue and 40 percent of gas revenue.[11] For revenue-sharing purposes, the district nearest to the offshore field is designated as the "producing district" and receives the largest share of revenue,[12] 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 extraction. 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 extraction. Notably, many districts are entitled to substantial revenue transfers despite having 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 59 percent being onshore. The special sharing arrangements with Aceh, Papua, and West Papua 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

decentralization.

[10]Starting 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).

[11]Law No. 21 of 2001 granted these concessions to Papua (and later West Papua, 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 West Papua runs through 2027, after which the additional oil revenue will decrease to 35 percent and gas revenue to 10 percent.

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

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, we aggregate variables to 1975 district borders, combining districts that amalgamated after 1975.[13] This results in 282 geographic units. (See Appendix Section A.2 for details.)

3 Data

This section briefly describes our main variables and data sources, with details provided in Appendix Section A.2. (See Appendix Section A.2 for details.) Summary statistics are provided in Appendix Table A.1. 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). 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 establishments in an unbalanced panel, and each firm is in the survey 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). We extend the non-oil GDP series back to 1993 by digitizing archival BPS reports.

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, 2016). Indonesian liquefied natural gas (LNG) price data are from IndexMundi, which sources from World Gas Intelligence and the World Bank.[14] 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.[15]

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

[14] Because 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.2.)

[15] Our endowment variables use the proven reserves of oil and gas fields discovered by 1998, the year before

We define exposure to resource extraction shocks as the current value of predetermined onshore endowments,

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

in constant 2010 USD millions. We focus on the reduced-form impact of $\boldsymbol{P}_t'\boldsymbol{E}_d$, rather than using it as an instrument for oil and gas production, because we expect this variable to affect economic outcomes through channels other than resource production.[16]

We construct fiscal endowments of oil and gas (F_d^{oil} and F_d^{gas}) by applying the revenue-sharing formula to the oil and gas 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.[17]

We define exposure to fiscal shocks as

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

where $\boldsymbol{P}_t'\boldsymbol{F}_d = P_t^{oil}F_d^{oil} + P_t^{gas}F_d^{gas}$. 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).

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 extraction and shared-revenue effects.[18] Moreover, the presence of districts with large fiscal endowments but zero onshore endowments facilitates the identification of interaction effects.

4 Identification Strategy

The Indonesian setting provides advantageous time-series and cross-sectional variation, allowing us to disentangle the effects of resource extraction, shared revenue, and their interaction. We estimate the effects of extraction by comparing the differential responses of economic outcomes to resource price changes across districts with varying onshore endow-

the decentralization law was passed. (The results are very similar when we only use fields discovered by 1975.) We normalize by population in 1980 instead of 1971, the year of the first census, because the 1971 data are incomplete.

[16]For example, an increase in $\boldsymbol{P}_t'\boldsymbol{E}_d$ will lead to greater investment in exploratory drilling and an increase in demand for intermediate goods and labor.

[17]The 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 West Papua as used in the rest of the country.

[18]Appendix Figure A.4 plots the onshore, offshore, and fiscal shocks over time for every district.

ments. The effects of shared revenue are primarily identified from the differential change in outcomes post-decentralization for districts with varying fiscal endowments.

4.1 Manufacturing Firm Outcomes

We begin by estimating a model that relates firm-level outcomes to the two shocks and their interaction. 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.[19] The model is

$$\ln Y_{i,d,t} = \sum_{k=0}^{K} \beta_k \boldsymbol{P}'_{t-k}\boldsymbol{E}_d + \sum_{k=0}^{K} \delta_k \boldsymbol{P}'_{t-k}\boldsymbol{F}_d \cdot 1(t-k \geq 1999)\cdot 1(\boldsymbol{E}_d = \boldsymbol{0}) \\ + \sum_{k=0}^{K} \gamma_k \boldsymbol{P}'_{t-k}\boldsymbol{F}_d \cdot 1(t-k \geq 1999)\cdot 1(\boldsymbol{E}_d > \boldsymbol{0}) + \alpha_i + \phi_{j(i),t} + \lambda_{r(d),t} + \varepsilon_{i,d,t}, \tag{1}$$

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 account for additively separable, time-invariant differences in firm outcome levels, while the industry-by-year effects control for time-varying, industry-specific shocks. The inclusion of region-by-year effects ensures that our parameters are identified from within-region variation, effectively comparing districts in the same region that experience different shocks.

Equation (1) allows fiscal shocks to have different effects based on whether the district's onshore endowment is zero ($\boldsymbol{E}_d = \boldsymbol{0}$) or positive ($\boldsymbol{E}_d > \boldsymbol{0}$). This enables us to test for heterogeneous responses arising from differences in district characteristics associated with the presence of onshore extraction. For example, districts with onshore endowments might possess better preexisting infrastructure built to facilitate the extraction and export of oil and gas. Theoretically, this could result in either higher or lower fiscal multipliers. Additionally, these districts might exhibit tighter factor markets or elevated levels of corruption, both of which tend to reduce fiscal multipliers.

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 new transfers into public services like infrastructure. The dynamic model incorporates the effects of delayed spending.

According to Equation (1), increasing the value of the onshore endowment by 1 million USD per capita raises the outcome by $100 \cdot \beta_0$ log points in the same year. The cumulative four-year impact of a permanent increase in the onshore endowment by 1 million USD per capita is $100 \cdot \sum_{k=0}^{3} \beta_k$ log points. Similarly, the immediate and cumulative impacts of increasing the fiscal endowment by 1 million USD per capita after decentralization are $100 \cdot \delta_0$

[19] We use the seven regions defined by the Central Bureau of Statistics: Sumatra, Java, Nusa Tenggara, Kalimantan, Sulawesi, Maluku, and Papua.

and $100 \cdot \sum_{k=0}^{3} \delta_k$ for districts with zero onshore endowment, and $100 \cdot \gamma_0$ and $100 \cdot \sum_{k=0}^{3} \gamma_k$ for districts with positive onshore endowment.

The magnitude of β_k is not directly comparable to the magnitudes of δ_k and γ_k; each coefficient is a reduced-form effect that operates through a different channel. An increase in the value of the onshore endowment induces more intensive extraction and investment in exploration, whereas an increase in the value of the fiscal endowment is associated with greater local public spending.

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 assumption needed to identify $\{\beta_k\}$, $\{\delta_k\}$, and $\{\gamma_k\}$ is that firms belonging to the same industry and region but located in districts with different endowments would have followed parallel trends, on average, in the absence of price changes or the introduction of revenue-sharing.[20] Under this assumption, which implies strict exogeneity, the parameters can be consistently estimated using the fixed-effects estimator or the first-difference estimator. All tables report standard errors that are robust to heteroskedasticity and clustering at the district level.

One way that the identifying assumption could be violated is if endowments were correlated with other district characteristics that matter for economic dynamics. Appendix Table A.2 reports the cross-sectional relationship between the endowment variables and 11 baseline geographic and demographic characteristics. Districts with larger onshore endowments tend to have flatter terrain and less forestland, while districts with larger fiscal endowments tend to have steeper terrain, more forestland, and coastal access. Both endowments are positively correlated with land area. The correlations with demographic characteristics tend to be weaker. In Section 5.2 ahead, we show that the results are robust to controlling for all 11 baseline covariates, interacted with year effects. We also show that the results are robust to other modifications of Equation (1), such as allowing the effect of onshore extraction shocks to differ before and after 1999, and controlling for offshore extraction shocks.

Another potential threat to our design is that changes in central government spending at the district level around 2001 might be correlated with fiscal endowments. The fiscal shock exposure 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 previously discussed, the fiscal endowment is positively correlated with the change in district government spending around 2001 (Figure 1, Panel (b)). This correlation will match the correlation with the *total* spending change as long as the central government

[20] See Goldsmith-Pinkham, Sorkin and Swift (2020) and Christian and Barrett (2023) for more discussion of identification in these settings. See also Allcott and Keniston (2017) for a discussion of parallel trends in the context of local resource shocks in the United States.

spending change is uncorrelated with fiscal shock exposure.[21] Unfortunately, we cannot estimate this correlation due to a lack of geographically disaggregated central government spending data. However, we find that the fiscal endowment correlates with three of five proxies for baseline central expenditure (Appendix Table A.3). While a correlation between the fiscal endowment and the *level* of national spending does not inherently cause bias, it could portend problems if the national spending change around 2001 systematically relates to its previous level. Therefore, as a further robustness check, we control for the baseline central expenditure proxies interacted with year effects.

While the parallel trends assumption is not testable, we can examine its plausibility by estimating trends over time periods when the shocks were minimal or nonexistent. Oil and gas prices were fairly stable over 1991–1998 (Appendix Figure A.1), so districts with onshore endowments and districts with zero endowments should exhibit similar trends over this period. Similarly, districts with fiscal endowments and districts with zero endowments should be on parallel trends 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. For ease of interpretation, we define three treatment groups based on onshore and fiscal endowments. Let $\overline{E}_d$ be the average of $\boldsymbol{P}_t'\boldsymbol{E}_d$ over 1975–2014, and let $\overline{F}_d$ be the average of $\boldsymbol{P}_t'\boldsymbol{F}_d$ over 2001–2014. Define M as the 80th percentile of $\overline{F}_d$ in the subsample where $\overline{F}_d > 0$ and $\overline{E}_d = 0$. The treatment categories are: (1) positive onshore and fiscal endowments (31 districts), (2) high fiscal endowment and zero onshore endowment (32 districts), and (3) low fiscal endowment and zero onshore endowment (124 districts). Formally, we define indicator variables for these groups:

$$\begin{aligned}
D_{1,d} &= 1(\overline{E}_d > 0) && \text{(positive onshore and fiscal endowments)},\\
D_{2,d} &= 1(\overline{F}_d \geq M \cap \overline{E}_d = 0) && \text{(high fiscal endowment, zero onshore)},\\
D_{3,d} &= 1(0 < \overline{F}_d < M \cap \overline{E}_d = 0) && \text{(low fiscal endowment, zero onshore)}.
\end{aligned}$$

Control districts have zero endowments: $\overline{E}_d = 0$ and $\overline{F}_d = 0$ (95 districts). We chose M to make fiscal endowment distributions similar for groups 1 and 2. In group 1, the fiscal endowment (in USD thousands per capita) has a median of 4.9, mean of 27.4, and maximum of 201.5. For

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

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

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

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

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

group 2, these values are 3.2, 15.8, and 152.2, respectively.[22] Groups 1 and 2 thus experience similar fiscal shocks; any difference between them is attributable to extraction activity or heterogeneous responses to fiscal shocks.

We examine trends in the three treatment groups relative to the control group using the difference-in-differences (DiD) model

$$\ln Y_{i,d,t} = \sum_{s\neq 1998} \beta_s D_{1,d} \cdot 1(t=s) + \sum_{s\neq 1998} \delta_s^h D_{2,d} \cdot 1(t=s) + \sum_{s\neq 1998} \delta_s^\ell D_{3,d} \cdot 1(t=s) + \alpha_i + \phi_{j(i),t} + \lambda_{r(d),t} + \varepsilon_{i,d,t}. \tag{2}$$

The parameters $\{\beta_s\}$, $\{\delta_s^h\}$, and $\{\delta_s^\ell\}$ represent the change in the outcome from 1998 to year s in the three treatment groups relative to districts with zero endowments. Finding $\beta_s = \delta_s^h = \delta_s^\ell = 0$ for $s < 1998$ would indicate that the treatment groups and the control group were on parallel trends prior to the passage of the decentralization law and subsequent rise in oil and gas prices.

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).[23] This is unsurprising, as the average firm is only observed for 9.1 years. While the results from Equation (2) may not mirror those from Equation (1) due to the sample differences, they remain useful for assessing pre-decentralization trends in firm outcomes. Importantly, Equation (2) does not impose the log-linear functional form assumption of Equation (1), allowing for potential nonlinear firm responses to shocks. The event-study evidence thus complements the baseline regression results.

4.2 District Non-Oil GDP

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

[22] Appendix Figure A.5 shows the distributions.
[23] The total sample size is around 203,000 compared to 482,000 for the baseline regression.

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

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

where $Y_{d,t}$ is non-oil GDP in district d in year t, α_d is a district fixed effect, and $\lambda_{r(d),t}$ is a region-by-year effect.[24] We report estimates of the model for $K = 0$ and $K = 3$.

We analyze (log) non-oil GDP trends in treated districts relative to control districts using the model

$$\ln Y_{d,t} = \sum_{s \neq 1998} \beta_s D_{1,d} \cdot 1(t = s) + \sum_{s \neq 1998} \delta_s^h D_{2,d} \cdot 1(t = s) \tag{4}$$
$$+ \sum_{s \neq 1998} \delta_s^\ell D_{3,d} \cdot 1(t = s) + \alpha_d + \lambda_{r(d),t} + \varepsilon_{d,t}.$$

The sample is a nearly balanced panel of 289 districts from 1993 to 2013.[25]

5 Results

5.1 Main Results

Table 1 presents the fixed-effects estimates from the static version ($K = 0$) of Equation (1). The shock exposure variables are measured in USD millions per capita to enhance readability. However, a one-year change of 1 million USD per capita never occurs in the data (Appendix Figure A.4). Therefore, consider instead an increase of 100 thousand USD per capita, which raises the outcome by $10 \cdot \beta_0$ log points.

In response to such an onshore extraction shock, firm output increases by 5.2 log points (S.E. = 0.7 log points) (column 1). Value added increases by 1.8 log points (S.E. = 0.9 log points), but the estimate is only significant at the 10 percent level (column 2). The impact on number of workers is small and statistically insignificant (column 3). Output per worker and value added per worker see economically meaningful increases of 5.1 log points (S.E. = 0.5) and 2.0 log points (S.E. = 0.7 log points), respectively (columns 4 and 5). These responses are consistent with agglomeration effects (Allcott and Keniston, 2017). Wages also rise by 2.3 log points (S.E. = 0.5 log points) (column 6). Overall, the estimates are consistent with

[24] In the GDP analysis, $\boldsymbol{E}_d$ and $\boldsymbol{F}_d$ are based on endowments under the ground in 1993, the first year of the sample.

[25] The only districts with missing non-oil GDP observations are Kabupaten Halmahera Tengah and Kabupaten Bekasi. The BPS documents only report total GDP for Halmahera Tengah over 1993–1996, and the reported non-oil GDP for 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.

resource booms increasing local labor demand and driving up input costs, while at the same time improving firm productivity.

The estimated effects of the fiscal shock are less precise. However, wages experience a statistically significant decrease in response to an increase in the value of the fiscal endowment. For districts with zero onshore endowments, wages fall by 31.7 log points (S.E. = 15.0 log points), while for districts with positive endowments, they decrease by 17.9 log points (S.E. = 5.2 log points). These estimates are perhaps surprising, as one might expect the fiscal shock to drive up labor costs via increased demand. Alternatively, increased spending could improve amenities and attract workers to the region, thereby driving down wages. Across all outcomes, we do not find a statistically significant difference in impacts based on whether a district has any onshore endowment. Contrary to the hypothesized mechanisms (such as differences in preexisting infrastructure quality) discussed in the introduction, we find no evidence that fiscal windfalls promote firm growth more regions without onshore extraction.

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 similar to the static estimates, albeit larger in absolute magnitude. For example, the cumulative impact of an onshore extraction shock on output per worker is 7.8 log points (S.E. = 0.6 log points), compared to the static estimate of 5.1 log points.

Figure 3 plots the estimates of $\{\beta_s\}$, $\{\delta_s^h\}$, and $\{\delta_s^\ell\}$ 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 our shock variables. For the most part, plants in treated districts exhibit trends parallel to those of plants in control districts prior to 1999. However, plants in districts with high fiscal endowments and zero onshore endowments experience downward pretrends in output and value added relative to plants in control districts. We will show ahead that adjusting for a potential driver of differential pretrends—covariate imbalance—leads to broadly similar results.

5.2 Robustness Checks

We next check the robustness of our results to modifying the regression specification in several ways. As a first robustness check, we add baseline covariates interacted with year effects to the model. Table 3 displays the results. In Panel A, we control for time-varying effects of 11 geographic and demographic covariates, while in Panel B, we control for time-varying effects of baseline central government expenditure proxies. The regressions in Panel C add both sets of covariates. The results are generally similar to the baseline estimates.[26]

A potential concern with Equation (1) is that it assumes that extraction shocks have the same effect regardless of the time period. If instead this effect is heterogeneous over time—

[26]Appendix Tables A.4 through A.6 report the corresponding robustness checks for the dynamic specification.

perhaps due to changes in extraction technology—then the estimated impact of the revenue shock over 1999–2014 might simply be picking up on this heterogeneity. To see whether our estimates of δ_0 and γ_0 are biased due to neglecting heterogeneity in β_0 over time, we interact $\boldsymbol{P}_t'\boldsymbol{E}_d$ with indicators for the pre- and post-decentralization time periods. Panel A of Table 4 presents the results. The impacts of onshore extraction shocks before and after 1999 are statistically indistinguishable for four out of six outcomes; we reject equality for value added ($p = 0.070$) and number of workers ($p = 0.005$). More importantly, the estimates of δ_0 and γ_0 do not change much, indicating that our preferred estimates are not meaningfully biased due to neglected heterogeneity in the effect of extraction shocks over time.

Another potential concern centers on the distinction between onshore and offshore endowments. Both endowments matter for revenue sharing, but we have argued that only onshore endowments matter for the direct economic impact of resource extraction. If this assumption is incorrect, then our estimates of δ_0 and γ_0 could reflect the impact of offshore extraction in addition to the impact of shared revenue. Moreover, the direction of this bias would be unclear *a priori*. To test for the presence of this bias, we control for $\boldsymbol{P}_t'\boldsymbol{E}_d^{\text{off}}$, where $\boldsymbol{E}_d^{\text{off}}$ is offshore endowment per capita. The results, presented in Panel B of Table 4, suggest that offshore extraction is not an important confounder. The estimated coefficient on $\boldsymbol{P}_t'\boldsymbol{E}_d^{\text{off}}$ is statistically insignificant for five out of six outcomes, and the estimates of β_0, δ_0, and γ_0 remain similar to the baseline estimates.

The first-difference estimates reported in Appendix Tables A.7 and A.8 are sometimes quite different than the fixed-effects estimates, especially for the static model and the individual coefficients in the dynamic specification. However, the cumulative estimates in the dynamic specification are fairly similar using first differences and fixed effects. There are three exceptions to note: first differences yields larger effects of the fiscal shock on value added and number of workers, and a smaller effect of the fiscal shock on wages.[27]

As a final robustness check, we re-estimate the coefficients using alternative versions of the fiscal endowment variable. These versions incorporate different assumptions about how provinces allocate their shared revenue to districts. The results remain consistent across various allocation assumptions, as demonstrated in Appendix Figure A.6.

5.3 Non-Oil GDP

Table 5 presents the non-oil GDP results based on the static ($K = 0$) and dynamic ($K = 3$) versions of Equation (3). Both the fixed-effects and first-difference estimates suggest that onshore extraction and shared revenue raise non-oil GDP. The fixed-effects estimates of the static effect and the cumulative dynamic effect are similar, so we focus on cumulative effects. Increasing the value of the onshore endowment by 100 thousand USD per capita raises non-oil GDP by 13.2 log points (S.E. = 5.2 log points). A same-sized increase in the fiscal

[27]Appendix Tables A.9 through A.12 report robustness checks for the first-difference estimates.

endowment's value yields different effects depending on the district's onshore endowment. In districts with zero onshore endowments, it increases non-oil GDP by 148.4 log points (S.E. = 56.3 log points), while in districts with positive onshore endowments, the increase is 118.9 log points (S.E. = 40.0 log points). The difference between these two effects is statistically insignificant ($p = 0.658$). Overall, both onshore extraction and shared revenue appear to raise non-oil GDP, with revenue having similar effects regardless of whether extraction takes place in the district.

The first-difference estimates indicate an immediate increase in non-oil GDP of 6.0 log points (S.E. = 2.2 log points) in response to an increase in the value of the onshore endowment. However, the cumulative effect is smaller and statistically insignificant. The cumulative estimates for the fiscal shock, by contrast, are similar to the fixed-effects estimates.

The fixed-effects and first-difference estimates remain similar when controlling for the time-varying impact of baseline geographic and demographic characteristics, central expenditure proxies, or both sets of covariates (Appendix Tables A.13 through A.15). The results are also similar when we employ different assumptions about how provinces distribute their shared revenue across districts (Appendix Figure A.7).

Figure 4 plots the DiD estimates based on Equation (4). Reassuringly, the three treatment groups exhibit parallel trends with the control group (districts with zero endowments) before the 1999 passage of the decentralization law. After 1999, non-oil GDP begins to rise in districts with positive onshore and fiscal endowments relative to districts with zero endowments, coinciding with oil and gas price increases. Districts with large fiscal endowments and zero onshore endowments experience a similar increase in non-oil GDP after 1999. Conversely, districts with small fiscal endowments do not see an increase in non-oil GDP, which is unsurprising given the minimal shared revenue in these districts.

Remarkably, the first two treatment groups both experience a nearly identical long-run increase in non-oil GDP of 33 log points (S.E. = 10 log points) from 1998 to 2013, relative to control districts—despite districts with onshore endowments benefiting from extraction and a somewhat larger fiscal shock. This suggests two, non-mutually exclusive possibilities.

The first possible explanation is that resource extraction has no effect on non-oil GDP. However, the evidence is inconsistent this theory. Tables 1 and 2 show that resource extraction boosts manufacturing output. Additionally, columns 1–3 of Table 5 show positive effects on non-oil GDP. Nevertheless, it is worth noting that the cumulative effect estimated using first differences in column 4 is statistically insignificant.

Second, shared revenue could have a smaller impact on non-oil GDP in districts with positive onshore endowments. The estimates in Table 5 provide limited support for this theory. Shared revenue has a larger immediate effect on non-oil GDP in districts lacking onshore endowments (column 2). This difference in on-impact responses is statistically significant, as the confidence intervals do not overlap. However, the heterogeneity fades over time. While the cumulative response is larger in districts lacking onshore endowments, the difference is smaller and statistically insignificant.

5.4 Mechanisms

We now consider potential mechanisms for our finding that shared revenue appears to boost non-oil GDP more in districts lacking onshore endowments. Candidate mechanisms include differential factor markets, baseline infrastructure, and corruption rates. Our results support differential factor markets as a possible mechanism, but not infrastructure or corruption.

First, fiscal multipliers may be lower in districts with onshore extraction due to their tighter factor markets. Prior work finds that multipliers are generally larger in regions or periods where there is more slack in the economy.[28] The results presented in Tables 1 and 2 provide support for this hypothesis. Onshore extraction drives up manufacturing wages, suggesting that the labor market is especially tight in districts with onshore endowments during periods of high resource prices. Shared revenue lowers wages, but this effect is less pronounced in districts with onshore endowments.

Next, we examine whether superior preexisting infrastructure, possibly built to facilitate the extraction and export of oil and gas, can account for the lower fiscal multiplier observed in districts with onshore endowments. In these districts, the marginal public works project may yield lower returns than in infrastructure-poor districts. However, the results in Appendix Table A.16 indicate that districts with onshore endowments actually possessed inferior preexisting infrastructure. Alternatively, infrastructure may exhibit increasing returns due to network effects, resulting in a larger multiplier in areas with better initial infrastructure. This mechanism, however, appears implausible, as the difference between districts with onshore endowments and those with large fiscal endowments but no onshore endowments is statistically significant for only one out of four infrastructure outcomes when controlling for regional effects.

Finally, differences in corruption could drive the results. Previous research on this topic is mixed, with some studies finding that corruption lowers the fiscal multiplier and others finding it raises the multiplier.[29] In our setting, however, corruption does not appear to be a plausible mechanism. The results presented in Appendix Table A.17 indicate that manufacturing firms' "gift payments" to external parties (a euphemism for bribes in Indonesia) do not differ significantly between districts with onshore endowments and those with fiscal endowments but no onshore endowments. (Columns 3 and 6 present the preferred specifica-

[28] See, e.g., Shoag et al. (2010), Cohen, Coval and Malloy (2011), Auerbach and Gorodnichenko (2012), Nakamura and Steinsson (2014), and Adelino, Cunha and Ferreira (2017). However, some studies find differential multipliers only when comparing deep recessions to strong expansionary periods (Caggiano, Castelnuovo, Colombo and Nodari, 2015), while others find no difference in multipliers across economic conditions (Ramey and Zubairy, 2018).

[29] Haque and Kneller (2015) find that corruption reduces the returns to public spending, hampering economic growth. Additionally, Avellán, Galindo and Leon-Diaz (2020) estimate larger multipliers in countries with higher quality institutions. Conversely, Akcigit, Baslandze and Lotti (2023) and Ficarra (2024) find that corruption can increase growth by easing regulatory and bureaucratic burdens.

tions, which flexibly control for firm size and region fixed effects). Thus, corruption does not appear to be a mechanism.

6 Conclusion

What are the local economic effects of resource extraction and shared resource revenue? While a growing empirical literature examines this question, the vast majority of studies estimate only the compound effect of extraction and shared revenue. In this paper, we exploit the introduction of resource revenue sharing in Indonesia to disentangle these two effects and identify their interaction. This investigation is crucial for evaluating resource revenue-sharing systems, which are prevalent worldwide.

To answer this question, we use firm-level manufacturing data from 1975 to 2014 and district-level non-oil GDP data from 1993 to 2013. We identify the effects of extraction on outcomes by comparing the differential responses 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 for the resource sharing formula.

We find that onshore oil and gas extraction drives up manufacturing wages, yet manufacturing output and productivity still grow in response to greater extraction. Resource extraction also appears to generate positive spillovers to the non-oil sector in aggregate, with non-oil GDP rising as extraction increases. These results are inconsistent with the more severe manifestations of Dutch disease.

Interestingly, shared oil and gas revenue causes manufacturing wages to fall, possibly due to an increase in the supply of workers responding to enhanced amenities funded by this revenue. Shared revenue also significantly boosts non-oil GDP. We find suggestive evidence that districts lacking onshore endowments experienced greater benefits from shared revenue, though we cannot statistically reject equal effects.

Our research highlights the need for future studies to test for heterogeneous effects of shared revenue in other contexts. The presence of such heterogeneous effects would imply that countries should reevaluate their allocation of resource revenue.

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7 Tables

Table 1: Firm Outcomes: Static Model

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.52***	0.18*	0.01	0.51***	0.20***	0.23***
	(0.07)	(0.09)	(0.05)	(0.05)	(0.07)	(0.05)
Fiscal Endow. × Price	0.64	3.63	3.47	−2.82	0.19	−3.17**
× ($t \geq 1999$) × (Onshore = 0)	(4.12)	(4.92)	(2.59)	(3.65)	(3.83)	(1.50)
Fiscal Endow. × Price	−0.62	0.21	0.65*	−1.27**	−0.55	−1.79***
× ($t \geq 1999$) × (Onshore > 0)	(0.60)	(0.75)	(0.36)	(0.50)	(0.65)	(0.52)
p-value: No Interaction Effect	0.758	0.486	0.283	0.665	0.843	0.378
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1) for $K = 0$. The p-values correspond to the test of $H_0: \delta_0 = \gamma_0$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Firm Outcomes: Dynamic Model

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.20***	−0.02	−0.05	0.24***	0.06	0.13***
	(0.07)	(0.08)	(0.05)	(0.06)	(0.06)	(0.03)
Lag 1	0.12**	0.20*	−0.01	0.13***	0.22**	0.01
	(0.06)	(0.11)	(0.02)	(0.05)	(0.09)	(0.03)
Lag 2	0.06	−0.10	0.09***	−0.03	−0.19***	−0.06
	(0.06)	(0.06)	(0.01)	(0.06)	(0.06)	(0.06)
Lag 3	0.46***	0.23***	0.02	0.44***	0.22***	0.25***
	(0.06)	(0.06)	(0.02)	(0.05)	(0.05)	(0.04)
Sum of Lags 0–3	0.84***	0.32***	0.05	0.78***	0.30***	0.33***
	(0.08)	(0.10)	(0.05)	(0.06)	(0.07)	(0.07)
Fiscal Endow. × Price	0.45	0.55	2.93	−2.48	−2.36	−2.21
$\times(t \geq 1999)\times$ (Onshore = 0)	(4.29)	(3.86)	(2.26)	(3.66)	(2.94)	(1.46)
Lag 1	1.38	2.27	0.18	1.20	2.09	1.29
	(1.45)	(2.44)	(0.90)	(1.65)	(2.46)	(1.27)
Lag 2	5.68**	7.02***	0.63	5.05**	6.40***	−1.38
	(2.34)	(2.18)	(0.81)	(2.29)	(1.83)	(2.30)
Lag 3	−7.79**	−5.87*	−0.09	−7.70***	−5.80**	−1.65
	(3.39)	(3.06)	(1.24)	(2.62)	(2.31)	(1.12)
Sum of Lags 0–3	−0.27	3.97	3.66	−3.93	0.34	−3.94**
	(4.34)	(5.47)	(2.72)	(3.91)	(4.44)	(1.82)
Fiscal Endow. × Price	−1.69	−3.85**	1.06	−2.75**	−5.00***	−1.73***
$\times(t \geq 1999)\times$ (Onshore > 0)	(1.14)	(1.61)	(0.69)	(1.25)	(1.58)	(0.58)
Lag 1	1.70	1.78	0.09	1.61	1.66	−0.50
	(1.60)	(2.11)	(0.32)	(1.61)	(1.91)	(0.49)
Lag 2	1.68	3.03**	−0.79**	2.47**	3.85***	−0.08
	(1.36)	(1.45)	(0.40)	(1.15)	(1.40)	(0.95)
Lag 3	−2.36	0.44	0.18	−2.54**	0.24	0.63
	(1.53)	(1.27)	(0.43)	(1.23)	(1.01)	(0.80)
Sum of Lags 0–3	−0.67	1.40*	0.54	−1.21**	0.75	−1.68***
	(0.77)	(0.82)	(0.45)	(0.61)	(0.64)	(0.58)
p-value: No Interaction Effect	0.928	0.640	0.261	0.480	0.926	0.235
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1) for $K = 3$. The p-values correspond to the test of $H_0: \sum_{k=0}^{K} \delta_k = \sum_{k=0}^{K} \gamma_k$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Firm Outcomes: Robustness to Adding Controls

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
	Panel A: Controlling for Baseline Covariates					
Onshore Endow. × Price	0.58***	0.24*	0.00	0.57***	0.27***	0.31***
	(0.12)	(0.14)	(0.06)	(0.08)	(0.10)	(0.06)
Fiscal Endow. × Price $\times(t \geq 1999)\times$ (Onshore = 0)	1.28	3.97	4.43	−3.14	−0.54	−3.04*
	(3.19)	(4.16)	(2.82)	(3.38)	(3.52)	(1.79)
Fiscal Endow. × Price $\times(t \geq 1999)\times$ (Onshore > 0)	−1.40	−0.87	0.79	−2.18**	−1.76*	−2.11***
	(1.11)	(1.20)	(0.54)	(0.90)	(1.03)	(0.67)
p-value: No Interaction Effect	0.370	0.234	0.184	0.761	0.717	0.599
	Panel B: Controlling for Baseline Central Expenditure Proxies					
Onshore Endow. × Price	0.60***	0.29	0.03	0.57***	0.29**	0.24***
	(0.16)	(0.20)	(0.10)	(0.11)	(0.14)	(0.09)
Fiscal Endow. × Price $\times(t \geq 1999)\times$ (Onshore = 0)	0.41	3.17	3.46	−3.06	−0.26	−3.14*
	(3.94)	(4.69)	(2.84)	(3.68)	(3.67)	(1.65)
Fiscal Endow. × Price $\times(t \geq 1999)\times$ (Onshore > 0)	−2.00*	−1.54	0.07	−2.07**	−1.68	−1.73***
	(1.02)	(1.15)	(0.68)	(0.96)	(1.16)	(0.59)
p-value: No Interaction Effect	0.539	0.323	0.250	0.783	0.696	0.423
	Panel C: Controlling for Baseline Covariates and Central Expenditure Proxies					
Onshore Endow. × Price	0.51***	0.23	−0.06	0.57***	0.32*	0.36***
	(0.18)	(0.22)	(0.09)	(0.13)	(0.16)	(0.09)
Fiscal Endow. × Price $\times(t \geq 1999)\times$ (Onshore = 0)	1.35	3.94	4.23	−2.88	−0.38	−2.65
	(3.21)	(4.08)	(2.79)	(3.63)	(3.51)	(1.81)
Fiscal Endow. × Price $\times(t \geq 1999)\times$ (Onshore > 0)	−0.70	−0.60	1.17*	−1.87	−1.87	−2.27***
	(1.39)	(1.50)	(0.61)	(1.24)	(1.38)	(0.62)
p-value: No Interaction Effect	0.508	0.266	0.271	0.776	0.667	0.834
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1) for $K = 0$ after adding baseline covariates interacted with year effects. Panel A estimates control for the variables in Appendix Table A.2, Panel B estimates control for the variables in Appendix Table A.3, and Panel C estimates control for both sets of variables. The p-values correspond to the test of $H_0: \delta_0 = \gamma_0$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: 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) Wage
	Panel A: Allowing for Time-Varying Effects of Extraction					
Onshore Endow. × Price × $(t < 1999)$	0.54***	0.26***	0.08	0.47***	0.22***	0.20***
	(0.07)	(0.10)	(0.07)	(0.05)	(0.05)	(0.02)
Onshore Endow. × Price × $(t \geq 1999)$	0.46***	−0.00	−0.16**	0.62***	0.16	0.29**
	(0.14)	(0.17)	(0.07)	(0.13)	(0.16)	(0.14)
Fiscal Endow. × Price × $(t \geq 1999) \times$ (Onshore = 0)	0.67	3.70	3.54	−2.87	0.21	−3.20**
	(4.13)	(4.94)	(2.61)	(3.65)	(3.84)	(1.50)
Fiscal Endow. × Price × $(t \geq 1999) \times$ (Onshore > 0)	−0.34	1.05	1.43***	−1.78**	−0.37	−2.06***
	(0.88)	(0.98)	(0.52)	(0.72)	(0.89)	(0.78)
p-value: Pre-1999 = Post-1999	0.545	0.070	0.005	0.249	0.685	0.544
p-value: No Interaction Effect	0.807	0.593	0.435	0.762	0.879	0.498
	Panel B: Controlling for Offshore Extraction Shocks					
Onshore Endow. × Price	0.52***	0.18**	0.00	0.52***	0.21***	0.23***
	(0.07)	(0.09)	(0.04)	(0.06)	(0.08)	(0.05)
Offshore Endow. × Price	−0.03	0.13	0.40**	−0.44	−0.26	0.04
	(0.37)	(0.54)	(0.16)	(0.31)	(0.49)	(0.19)
Fiscal Endow. × Price × $(t \geq 1999) \times$ (Onshore = 0)	0.68	3.50	3.05	−2.38	0.46	−3.21**
	(4.00)	(4.81)	(2.65)	(3.48)	(3.68)	(1.50)
Fiscal Endow. × Price × $(t \geq 1999) \times$ (Onshore > 0)	−0.57	0.01	0.04	−0.61	−0.16	−1.84***
	(0.86)	(1.08)	(0.48)	(0.78)	(0.88)	(0.45)
p-value: No Interaction Effect	0.765	0.487	0.258	0.623	0.872	0.383
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in modified versions of Equation (1) for $K = 0$. The "no interaction effect" p-values correspond to the test of $H_0: \delta_0 = \gamma_0$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Log Non-Oil GDP

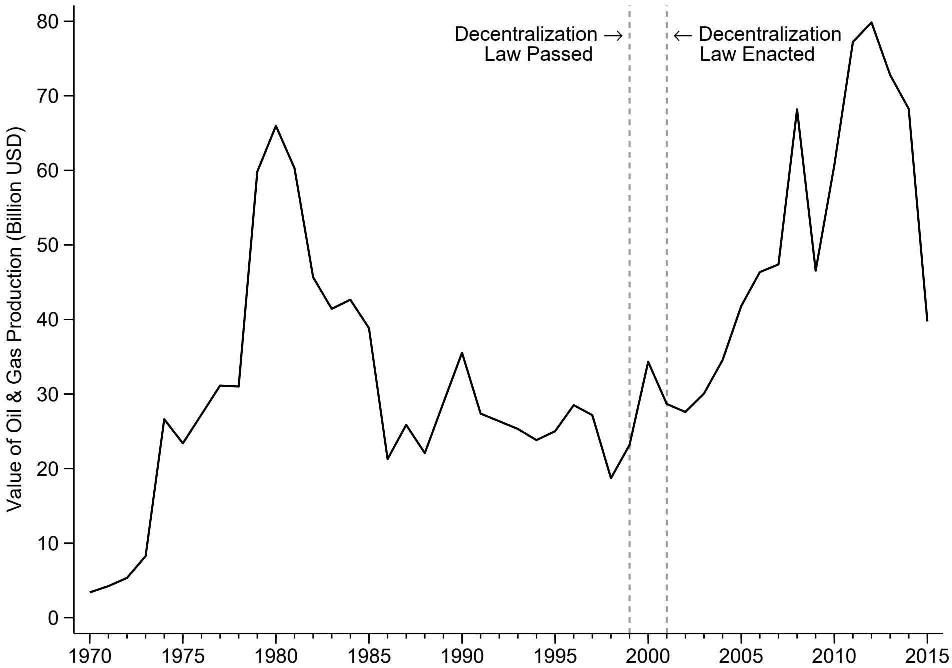
	Fixed Effects		First Difference	
	(1)	(2)	(3)	(4)
Onshore Endow. × Price	1.74^{***}	2.09^{***}	0.53^{***}	0.60^{***}
	(0.50)	(0.47)	(0.20)	(0.22)
Lag 1		0.13		0.02
		(0.13)		(0.14)
Lag 2		0.15		−0.11
		(0.13)		(0.07)
Lag 3		-1.05^{***}		-0.28^{***}
		(0.26)		(0.09)
Sum of Lags 0–3		1.32^{**}		0.22
		(0.52)		(0.41)
Fiscal Endow. × Price	14.41^{***}	10.20^{***}	3.84^{**}	3.96^{**}
$\times (t \geq 1999) \times$ (Onshore = 0)	(4.94)	(2.37)	(1.83)	(1.93)
Lag 1		3.36^{**}		3.60^{**}
		(1.54)		(1.74)
Lag 2		3.18^{*}		2.09^{**}
		(1.63)		(1.04)
Lag 3		−1.90		1.25^{**}
		(2.27)		(0.63)
Sum of Lags 0–3		14.84^{***}		10.89^{**}
		(5.63)		(4.81)
Fiscal Endow. × Price	9.63^{***}	−0.19	1.49^{*}	1.46^{*}
$\times (t \geq 1999) \times$ (Onshore > 0)	(3.45)	(1.88)	(0.80)	(0.79)
Lag 1		4.52^{***}		4.92^{***}
		(1.21)		(1.44)
Lag 2		2.58^{**}		2.36^{**}
		(1.31)		(1.00)
Lag 3		4.98^{***}		2.74^{***}
		(1.27)		(0.90)
Sum of Lags 0–3		11.89^{***}		11.48^{***}
		(4.00)		(3.88)
p-value: No Interaction Effect	0.409	0.658	0.226	0.922
Observations	6,062	6,062	5,773	5,773
Districts	289	289	289	289

Notes: This table presents fixed-effects and first-difference estimates of the coefficients in Equation (3) for $K = 0$ and $K = 3$. The p-values correspond to the test of $H_0: \sum_{k=0}^{K} \delta_k = \sum_{k=0}^{K} \gamma_k$. The sample covers 1993–2013. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1993 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

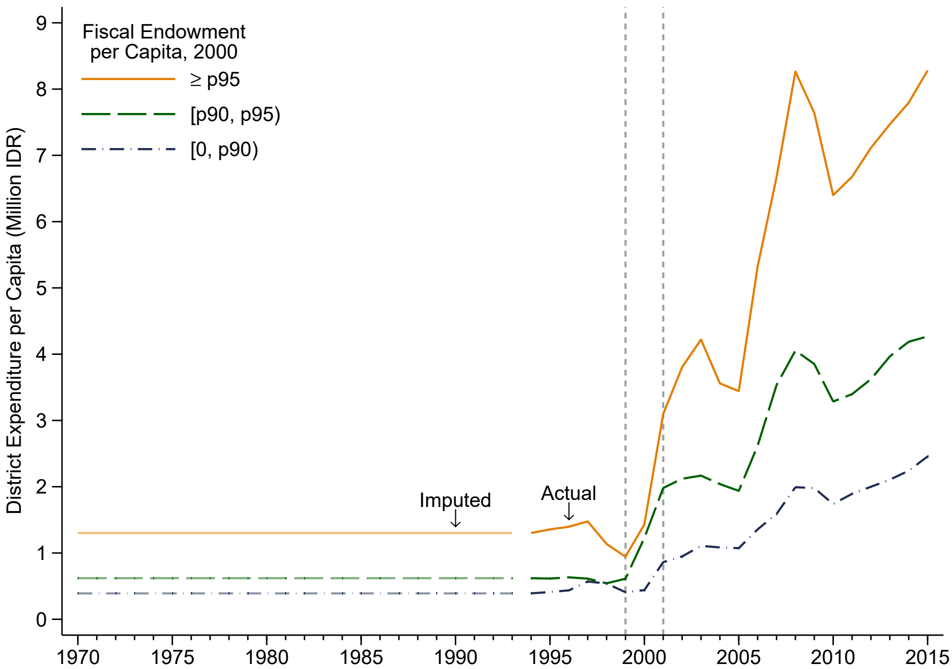
8 Figures

Figure 1: Resource Booms and Revenue Sharing

(a) Value of National Oil and Gas Production



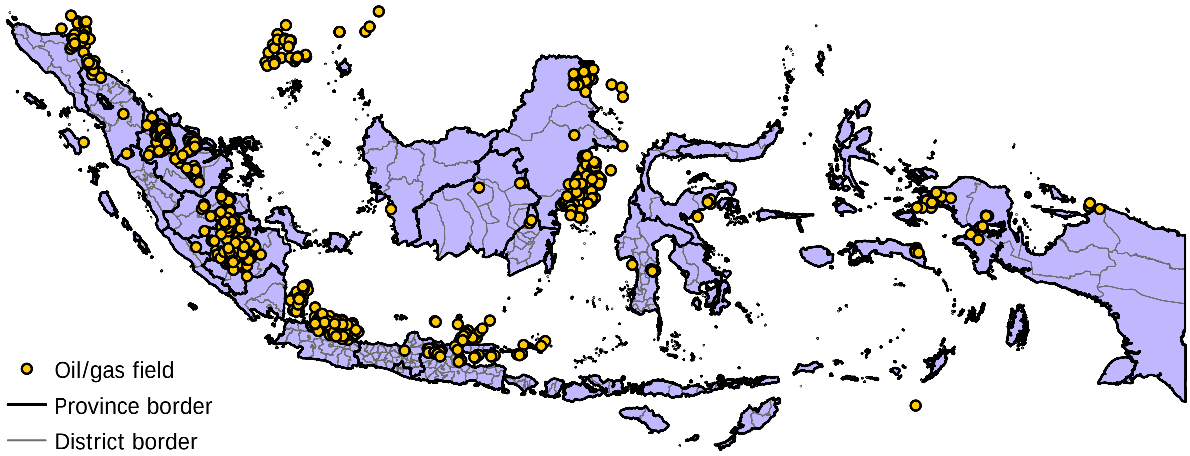
(b) District Public Expenditure by Fiscal Endowment



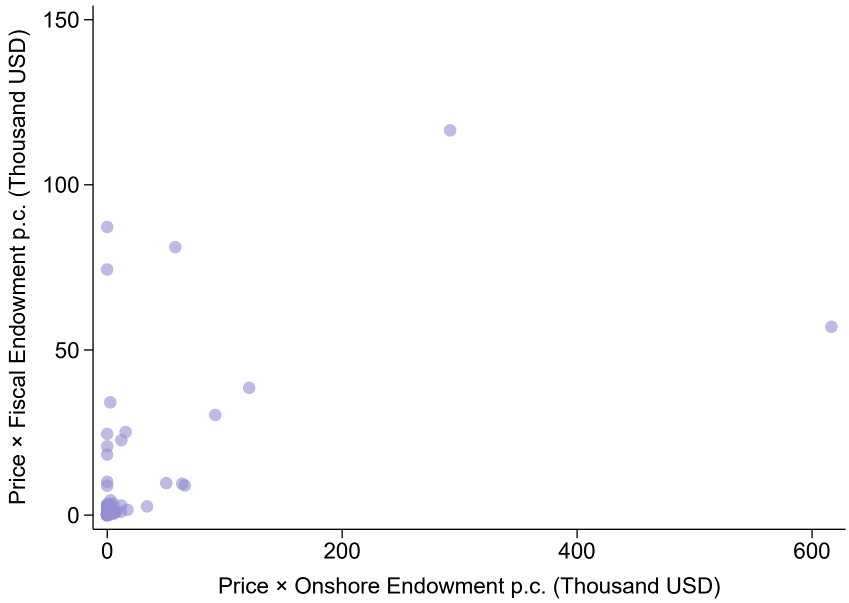
Notes: Panel (a) plots the total value of oil and gas production, in constant 2010 USD billions. Panel (b) plots average expenditure per capita for three groups of districts defined by their fiscal endowment per capita. Public expenditure is expressed in constant 2010 IDR millions per capita. Expenditure values prior to 1994 are imputed using the value in 1994.

Figure 2: Geographic Variation in Hydrocarbon Endowments

(a) Oil and Gas Fields of Indonesia



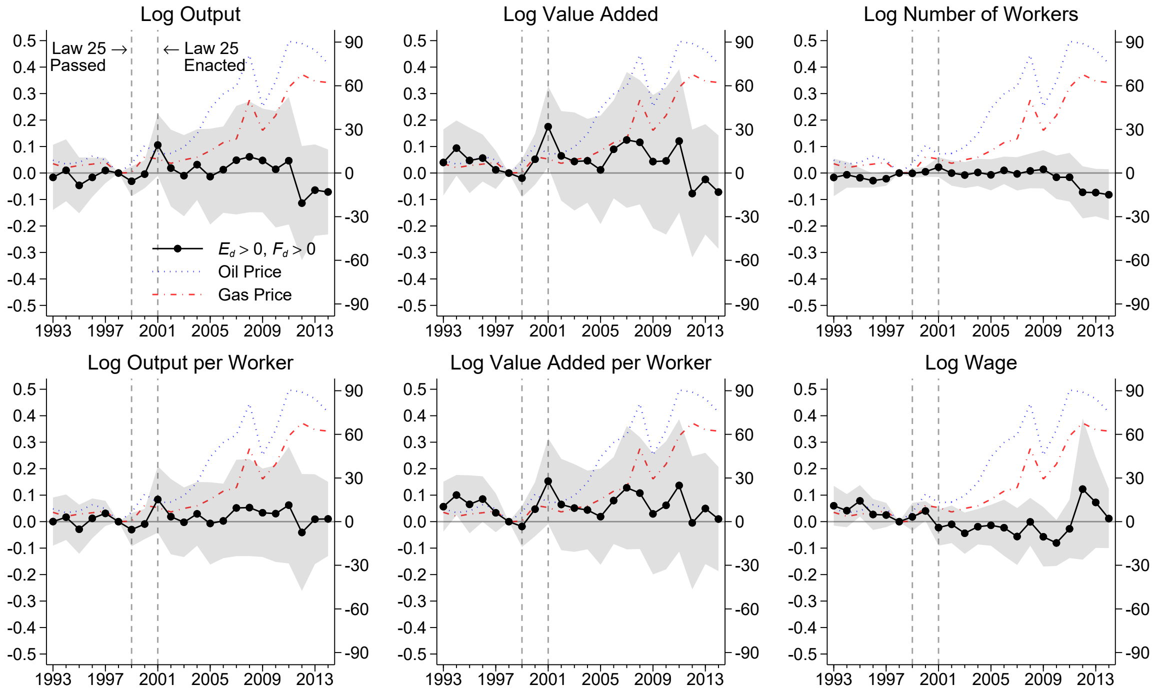
(b) Onshore Endowment vs. Fiscal Endowment



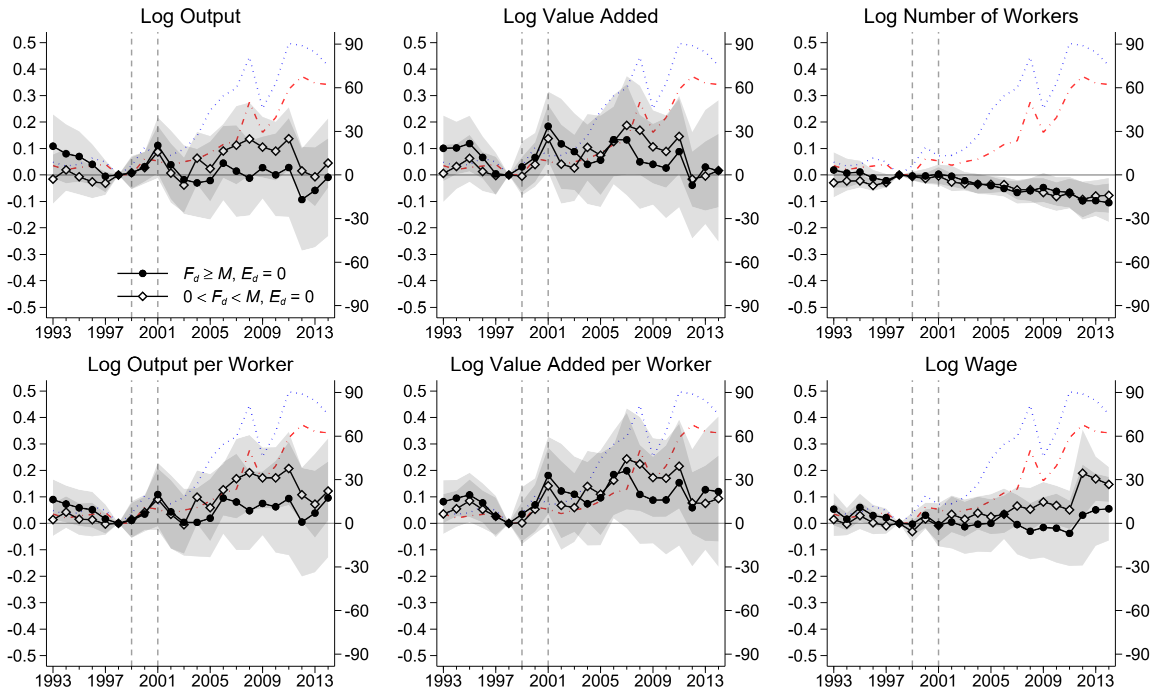
Notes: Panel (a) displays the locations of oil and gas fields along with 1975 province and district borders. Panel (b) plots the fiscal endowment per capita against the onshore endowment per capita. Both variables are based on 2000 oil and gas prices (in constant 2010 USD thousands) and 1980 population.

Figure 3: Difference-in-Differences: Firm Outcomes

(a) Positive Onshore and Fiscal Endowments vs. Zero Endowment

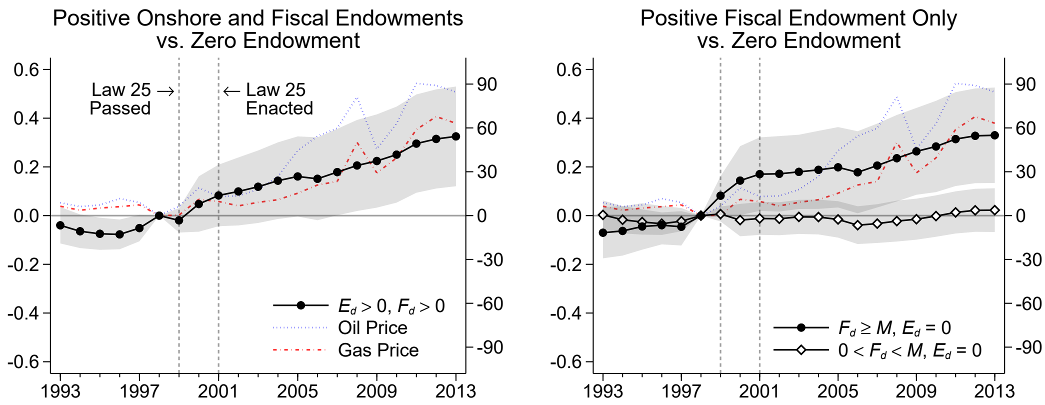


(b) Positive Fiscal Endowment Only vs. Zero Endowment



Notes: This figure plots estimates and 95-percent confidence intervals for $\{\beta_s\}$ (Panel (a)), $\{\delta_s^h\}$ (Panel (b)), and $\{\delta_s^\ell\}$ (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 heteroskedasticity and clustering at the level of 1975 district borders.

Figure 4: Difference-in-Differences: Log Non-Oil GDP



Notes: This figure plots estimates and 95-percent confidence intervals for $\{\beta_s\}$ (left panel), $\{\delta_s^h\}$ (right panel), and $\{\delta_s^\ell\}$ (right panel) 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 heteroskedasticity and clustering at the level of 1993 district borders.

A Appendix (For Online Publication)

Contents

A.1 Details on Oil and Gas Revenue Sharing

This section details the construction of the fiscal endowments, $\boldsymbol{F}_d$, 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 West Papua 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.

Provincial governments 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

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

Formally, let $R^{oil}_{d,t}$ and $R^{gas}_{d,t}$ 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 West Papua, the fiscal endowments per capita according to borders in year t are

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

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

For districts in Aceh, Papua, and West Papua, 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

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

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

Fiscal endowments $F_{d,t}^{oil}$ and $F_{d,t}^{gas}$ can vary over time due to changing district and province borders. Notably, after Papua split into two provinces—Papua and West Papua—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.[30] To avoid bias, we average across year-specific endowments in the post-decentralization period,

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

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

[30]See, 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 (2023).

A.2 Data Appendix

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, and each plant is in the survey 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.[31]

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

[31] Occasionally, 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.

compensation types and frequently appear to be subsumed into other categories.[32] 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 subnational) 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 working 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.

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 2000–2013.[33] We extend the two GDP series back to 1993 by digitizing archival BPS reports.[34] In the GDP dataset, we aggregate all variables to 1993 district borders and normalize endowments by 1980 population.

[32]For 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."

[33]The series ends in 2013 due to a change in methodology. Originally, BPS used the United Nation's System of National Accounts 1993 (SNA 1993). Starting in 2010, BPS produced a new GDP series based on SNA 2008. As a result, it stopped producing the older series after 2013.

[34]The 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." 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 containing no hydrocarbons, the GDP series for 1983–1993 is inconsistent with the series for 1993–2013 using 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 switching to the SNA 1993 methodology in 1993.

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, 2016). Endowments include both proven reserves and (estimated) unproven reserves as reported by Rystad. The extraction shock variable in Equation (1), $\boldsymbol{P}_t'\boldsymbol{E}_d$, uses oil and gas endowments at the start of our sample period in 1975. The fiscal shock variable, $\boldsymbol{P}_t'\boldsymbol{F}_d$, 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.[35] As a result, we need data on local natural gas prices.

Data on liquefied natural gas (LNG) prices in Indonesia are from IndexMundi,[36] which sources from World Gas Intelligence and the World Bank.[37] Because the data only go back 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 7,176 USD per capita (in constant 2010 USD). For offshore endowment, the corresponding percentiles are 0 and 560 USD per capita. Fiscal endowment is less skewed due to the sharing rules. The 25th percentile is 0, the median is 56 USD per capita, the 75th percentile is 347 USD per capita, and the 95th percentile is 9,690 USD 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.

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

[35]For more, please see https://www.eia.gov/todayinenergy/detail.php?id=3310.

[36]See https://www.indexmundi.com/commodities/?commodity=indonesian-liquified-natural-gas&months=360.

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

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 and district land area provided by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). These data cover 1983–2014. Data on district revenues and expenditures are from the Indonesian Ministry of Finance (*Kementerian Keuangan*) and INDO-DAPOER. Data on topography, coastline, and forestland come from the 2011 wave of the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES). Data on infrastructure in 1999 come from PODES 2000.

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.[38] 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.

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).[39] 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 focuses on 282 geographic units, merging some districts to spatially harmonize our data over time. In Central Kalimantan, four pairs of districts (all *kabupaten*) amalgamated in 1985 and separated again in 2002: (1) Katingan and Kotwaringin Timur; (2) Gunung Mas and Kapuas; (3) Barito Timur and Barito Selatan; and (4) Murung Raya and Barito Utara. Additionally, we combine Kabupaten Bogor and Kota Bogor in West Java due to a border change in 1995, in which Kota Bogor expanded to include several subdistricts that previously belonged to Kabupaten Bogor.

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

[39] INDO-DAPOER is hosted at http://databank.worldbank.org/data/reports.aspx?source=1266.

A.3 Tables

Table A.1: Summary Statistics

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.	(5) Obs.
District Covariates (1975 Borders)					
Price × Onshore Endowment p.c. (USD 1000s)	5.35	42.05	0.00	616.50	282
(> 0)	48.66	119.95	0.00	616.50	31
Price × Offshore Endowment p.c. (USD 1000s)	2.94	33.83	0.00	553.59	282
(> 0)	55.28	140.99	0.56	553.59	15
Price × Fiscal Endowment p.c. (USD 1000s)	2.69	12.11	0.00	116.51	282
(> 0)	4.05	14.70	0.00	116.51	187
Land Area (Thousand km^2)	7.02	13.94	0.02	119.04	282
District Has Some Coastline	0.64	0.48	0.00	1.00	282
Share of Villages with Slope > 15°	0.31	0.19	0.00	0.80	282
Share of Villages Located in or Near Forest	0.21	0.18	0.00	0.78	282
Population (Thousands), 1980	532.89	529.95	12.95	5,481.47	282
Share of Population Aged 0–14, 1980	0.41	0.04	0.29	0.50	282
Share of Population Aged 15–64, 1980	0.55	0.03	0.46	0.67	282
Share of Population with Primary Education, 1980	0.23	0.10	0.04	0.60	282
Share of Population with Secondary Education, 1980	0.04	0.04	0.01	0.23	282
Literacy Rate, 1980	0.57	0.11	0.15	0.82	282
Urbanization Rate, 1980	0.24	0.31	0.00	1.00	282
INPRES Schools per 1,000, 1983	1.11	1.33	0.02	9.73	282
INPRES Teachers per 1,000, 1983	5.69	6.77	0.17	44.59	282
INPRES Family Toilets per 1,000, 1983	7.54	21.38	0.00	318.81	282
INPRES Capital Projects per 1,000, 1982–83 Budget	0.52	0.74	0.00	5.67	282
National Capital Projects per 1,000, 1982–83 Budget	0.10	0.17	0.00	1.90	282
Firm Outcomes					
Output (IDR Millions)	40.82	332.33	0	40,430	490,798
Value Added (IDR Millions)	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 Millions)	0.17	0.99	0	486	490,786
Value Added per Worker (IDR Millions)	0.07	0.56	0	290	472,238
Average Wage (IDR Thousands)	12.02	15.96	0	3,864	489,855
District Outcomes (1993 Borders)					
GDP (IDR Billions)	12,472	24,887	228	287,657	6,065
GDP Excluding Oil and Gas (IDR Billions)	11,267	22,094	228	287,657	6,062
Oil and Gas GDP (IDR Billions)	1,211	8,476	0	164,218	6,061
(> 0)	8,149	20,668	0	164,218	901

Notes: The top panel presents statistics for a cross section of districts at 1975 borders. The bottom panel presents statistics at the firm-year level. The price × endowment variables are expressed in constant 2010 USD thousands per capita based on 1980 population and 2000 oil and gas prices. GDP is expressed in constant 2010 IDR billions. Firm output and output per capita are expressed in constant 2010 IDR millions, and average wages are expressed in constant 2010 IDR thousands.

Table A.2: Baseline District Characteristics and Endowments

	(1) Log Area	(2) Coastal	% Villages: (3) Slope > 15°	% Villages: (4) In/Near Forest	(5) Log Pop., 1980	% Population, 1980: (6) Aged 0–14	% Population, 1980: (7) Aged 15–64	% Population, 1980: (8) Primary Edu.	% Population, 1980: (9) Secondary Edu.	% Population, 1980: (10) Literate	% Population, 1980: (11) Urban
Price × Onshore Endowment p.c.	3.83***	0.09	−38.85***	−28.17**	2.78***	1.84	−1.25	−11.18	−3.94	0.17	−26.79
	(1.37)	(0.29)	(11.66)	(13.63)	(0.99)	(1.31)	(1.39)	(7.93)	(2.70)	(8.71)	(31.38)
Price × Fiscal Endowment p.c.	17.71***	5.65***	109.83*	293.24***	−7.03*	−11.52	16.56*	11.91	10.00	−33.50	117.44
	(5.96)	(1.68)	(64.12)	(92.35)	(3.69)	(8.93)	(8.89)	(45.38)	(13.33)	(52.13)	(132.13)
p-value: Both Coeffs = 0	0.000	0.000	0.002	0.005	0.019	0.341	0.146	0.114	0.313	0.560	0.615
Observations	282	282	282	282	282	282	282	282	282	282	282

Notes: This table presents estimates from cross-sectional regressions of baseline characteristics on the two endowment variables, controlling for region fixed effects. 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.3: Baseline Central Government Expenditure Proxies and Endowments

	Centrally Administered Public Services per 1,000 People				
	(1) INPRES Schools, 1983	(2) INPRES Teachers, 1983	(3) INPRES Family Toilets, 1983	(4) INPRES Capital Projects, 1982–83 Budget	(5) National Capital Projects, 1982–83 Budget
Price × Onshore Endowment p.c.	−2.14	3.74	-55.03^{*}	0.49	0.17
	(1.91)	(9.68)	(31.52)	(1.12)	(0.18)
Price × Fiscal Endowment p.c.	37.92^{***}	101.79^{**}	338.74	14.16^{*}	1.77
	(13.07)	(51.36)	(226.97)	(8.19)	(1.27)
p-value: Both Coeffs = 0	0.000	0.035	0.220	0.000	0.000
Observations	282	282	282	282	282

Notes: This table presents estimates from cross-sectional regressions of central government expenditure proxies on the two endowment variables, controlling for region fixed effects. Each price × endowment variable is expressed in constant 2010 USD thousands per capita based on 1980 population and 2000 oil and gas prices. The central expenditure proxies are expressed per 1,000 people based on 1980 population. All variables are measured at 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Dynamic Model: Controlling for Baseline Covariates

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.21**	−0.02	−0.05	0.26***	0.06	0.20***
	(0.10)	(0.12)	(0.05)	(0.08)	(0.10)	(0.05)
Lag 1	0.22***	0.34***	0.02	0.20***	0.32***	0.09**
	(0.07)	(0.13)	(0.03)	(0.06)	(0.11)	(0.04)
Lag 2	−0.03	−0.17*	0.07***	−0.10	−0.23**	−0.10
	(0.09)	(0.09)	(0.02)	(0.08)	(0.09)	(0.07)
Lag 3	0.50***	0.23***	−0.01	0.51***	0.23***	0.21***
	(0.07)	(0.08)	(0.03)	(0.07)	(0.08)	(0.05)
Sum of Lags 0–3	0.90***	0.38**	0.04	0.87***	0.38***	0.40***
	(0.14)	(0.16)	(0.08)	(0.10)	(0.11)	(0.08)
Fiscal Endow. × Price	1.56	1.84	3.37	−1.81	−1.59	−2.63
$\times (t \geq 1999) \times$ (Onshore = 0)	(3.60)	(3.66)	(2.27)	(3.21)	(2.82)	(1.61)
Lag 1	−2.09	−1.56	0.45	−2.54	−2.02	0.51
	(2.46)	(2.93)	(0.83)	(2.59)	(2.88)	(1.67)
Lag 2	7.21**	7.50**	1.46	5.75**	6.04***	−0.06
	(2.97)	(2.94)	(0.91)	(2.41)	(2.34)	(2.01)
Lag 3	−5.86*	−3.15	−0.54	−5.32*	−2.63	−1.26
	(3.55)	(3.48)	(1.30)	(2.81)	(2.76)	(1.93)
Sum of Lags 0–3	0.82	4.64	4.74	−3.92	−0.20	−3.44
	(3.45)	(4.61)	(3.01)	(3.74)	(4.04)	(2.15)
Fiscal Endow. × Price	−1.94	−4.08**	1.12	−3.06**	−5.28***	−2.41***
$\times (t \geq 1999) \times$ (Onshore > 0)	(1.53)	(1.82)	(0.79)	(1.52)	(1.72)	(0.70)
Lag 1	0.07	0.20	0.04	0.03	0.14	−0.58
	(1.47)	(2.01)	(0.38)	(1.55)	(1.85)	(0.70)
Lag 2	3.17**	3.64**	−0.57	3.73***	4.18***	0.85
	(1.53)	(1.70)	(0.45)	(1.34)	(1.61)	(1.03)
Lag 3	−2.69	0.58	0.11	−2.80**	0.47	0.26
	(1.65)	(1.37)	(0.47)	(1.36)	(1.16)	(0.81)
Sum of Lags 0–3	−1.39	0.34	0.71	−2.10**	−0.48	−1.88**
	(1.27)	(1.34)	(0.65)	(1.03)	(1.10)	(0.74)
p-value: No Interaction Effect	0.493	0.336	0.165	0.602	0.942	0.458
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1), additionally controlling for the covariates in Appendix Table A.2 interacted with year effects. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Dynamic Model: Controlling for Baseline Central Expenditure Proxies

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.30**	0.13	−0.01	0.31***	0.16	0.16**
	(0.14)	(0.17)	(0.07)	(0.11)	(0.13)	(0.08)
Lag 1	0.06	0.15	−0.03	0.09	0.19	−0.06
	(0.09)	(0.14)	(0.06)	(0.08)	(0.12)	(0.08)
Lag 2	0.04	−0.16*	0.08**	−0.04	−0.26***	−0.04
	(0.10)	(0.09)	(0.04)	(0.08)	(0.09)	(0.07)
Lag 3	0.49***	0.30***	0.02	0.47***	0.29***	0.28***
	(0.10)	(0.10)	(0.05)	(0.07)	(0.08)	(0.06)
Sum of Lags 0–3	0.89***	0.41*	0.07	0.83***	0.38**	0.35***
	(0.19)	(0.22)	(0.12)	(0.13)	(0.15)	(0.10)
Fiscal Endow. × Price	0.01	0.01	2.27	−2.27	−2.23	−2.21
$\times(t \geq 1999)\times$ (Onshore = 0)	(4.21)	(3.75)	(2.20)	(3.89)	(3.13)	(1.46)
Lag 1	1.45	2.75	0.69	0.77	2.07	2.01
	(1.58)	(2.72)	(0.97)	(1.57)	(2.51)	(1.39)
Lag 2	5.55**	7.13***	0.18	5.38**	6.96***	−0.37
	(2.69)	(2.43)	(0.61)	(2.52)	(2.12)	(1.83)
Lag 3	−7.48**	−6.67**	0.81	−8.29***	−7.50***	−3.64**
	(3.28)	(3.29)	(1.06)	(2.82)	(2.75)	(1.45)
Sum of Lags 0–3	−0.48	3.22	3.94	−4.42	−0.69	−4.21**
	(4.22)	(5.24)	(3.04)	(3.97)	(4.22)	(2.04)
Fiscal Endow. × Price	−3.01**	−5.47***	0.58	−3.58***	−6.07***	−2.06***
$\times(t \geq 1999)\times$ (Onshore > 0)	(1.35)	(1.75)	(0.65)	(1.36)	(1.76)	(0.63)
Lag 1	1.75	1.95	0.25	1.49	1.64	0.13
	(1.67)	(2.06)	(0.40)	(1.58)	(1.81)	(0.87)
Lag 2	1.26	2.51*	−1.21*	2.47**	3.81***	−0.19
	(1.53)	(1.50)	(0.62)	(1.25)	(1.41)	(0.95)
Lag 3	−2.03	0.63	0.29	−2.31*	0.25	0.55
	(1.42)	(1.40)	(0.40)	(1.19)	(1.15)	(0.91)
Sum of Lags 0–3	−2.02*	−0.38	−0.09	−1.93*	−0.37	−1.57**
	(1.19)	(1.37)	(0.79)	(1.08)	(1.29)	(0.66)
p-value: No Interaction Effect	0.718	0.503	0.204	0.528	0.939	0.224
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1), additionally controlling for the variables in Appendix Table A.3 interacted with year effects. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Dynamic Model: Controlling for Baseline Covariates and Central Expenditure Proxies

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.19	0.04	−0.07	0.26*	0.13	0.20**
	(0.17)	(0.20)	(0.07)	(0.13)	(0.17)	(0.09)
Lag 1	0.19*	0.29*	−0.01	0.20**	0.32**	0.11
	(0.10)	(0.15)	(0.05)	(0.09)	(0.13)	(0.08)
Lag 2	−0.05	−0.21*	0.06	−0.10	−0.28**	−0.06
	(0.12)	(0.12)	(0.04)	(0.10)	(0.11)	(0.08)
Lag 3	0.43***	0.17	−0.03	0.46***	0.22**	0.22***
	(0.11)	(0.13)	(0.06)	(0.09)	(0.11)	(0.06)
Sum of Lags 0–3	0.76***	0.30	−0.05	0.82***	0.39**	0.46***
	(0.21)	(0.25)	(0.11)	(0.15)	(0.18)	(0.11)
Fiscal Endow. × Price	1.39	1.90	2.79	−1.40	−0.96	−2.24
$\times(t \geq 1999)\times$ (Onshore = 0)	(3.56)	(3.52)	(2.07)	(3.41)	(2.88)	(1.67)
Lag 1	−1.71	−1.12	0.60	−2.32	−1.74	1.04
	(2.23)	(2.83)	(0.96)	(2.46)	(2.72)	(1.64)
Lag 2	7.67***	8.28***	1.32*	6.35***	6.96***	0.78
	(2.82)	(2.83)	(0.74)	(2.40)	(2.38)	(1.92)
Lag 3	−6.54*	−4.80	0.01	−6.55**	−4.84*	−2.89
	(3.48)	(3.45)	(1.16)	(2.86)	(2.79)	(1.82)
Sum of Lags 0–3	0.81	4.26	4.73	−3.92	−0.58	−3.30
	(3.54)	(4.55)	(3.02)	(4.06)	(4.04)	(2.24)
Fiscal Endow. × Price	−1.96	−4.37**	1.25*	−3.20**	−5.67***	−2.22***
$\times(t \geq 1999)\times$ (Onshore > 0)	(1.71)	(2.06)	(0.74)	(1.54)	(1.88)	(0.69)
Lag 1	0.38	0.43	0.22	0.15	0.14	−0.65
	(1.60)	(1.95)	(0.36)	(1.56)	(1.76)	(0.83)
Lag 2	2.98**	3.50**	−0.69	3.67***	4.26***	0.44
	(1.40)	(1.59)	(0.59)	(1.29)	(1.60)	(1.11)
Lag 3	−1.79	1.30	0.37	−2.17*	0.85	0.31
	(1.37)	(1.36)	(0.38)	(1.24)	(1.22)	(0.85)
Sum of Lags 0–3	−0.39	0.85	1.15	−1.54	−0.42	−2.11***
	(1.52)	(1.71)	(0.70)	(1.44)	(1.64)	(0.75)
p-value: No Interaction Effect	0.725	0.449	0.234	0.550	0.969	0.601
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	280	280	280	280	280	280

Notes: This table presents fixed-effects estimates of the coefficients in Equation (1), additionally controlling for the variables in Appendix Tables A.2 and A.3 interacted with year effects. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Firm Outcomes: First Difference, Static Model

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.22^{***}	0.18^{**}	-0.06^{**}	0.28^{***}	0.26^{**}	0.18^{***}
	(0.04)	(0.08)	(0.03)	(0.05)	(0.10)	(0.02)
Fiscal Endow. × Price	−1.97	−0.51	0.96	−2.94	−1.48	−1.67
$\times (t \geq 1999) \times$ (Onshore = 0)	(2.89)	(2.79)	(0.63)	(2.88)	(2.64)	(1.13)
Fiscal Endow. × Price	−0.94	−1.66	1.10^{**}	-2.04^{**}	−2.80	0.02
$\times (t \geq 1999) \times$ (Onshore > 0)	(0.77)	(1.50)	(0.43)	(0.83)	(1.72)	(0.56)
p-value: No Interaction Effect	0.722	0.699	0.854	0.757	0.655	0.176
Observations	421,924	399,655	421,903	421,903	399,655	421,108
Plants	51,726	50,357	51,725	51,725	50,357	51,627
Districts	280	280	280	280	280	280

Notes: This table presents first-difference estimates of the coefficients in Equation (1) for $K = 0$. The p-values correspond to the test of $H_0: \delta_0 = \gamma_0$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Firm Outcomes: First Difference, Dynamic Model

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.23***	0.18**	−0.06**	0.28***	0.24**	0.18***
	(0.04)	(0.08)	(0.03)	(0.05)	(0.10)	(0.02)
Lag 1	0.09	0.10	−0.08***	0.16**	0.19*	0.02
	(0.08)	(0.13)	(0.03)	(0.07)	(0.11)	(0.03)
Lag 2	0.06	−0.00	0.08***	−0.02	−0.09	−0.06
	(0.06)	(0.06)	(0.01)	(0.06)	(0.06)	(0.06)
Lag 3	0.21***	−0.09	−0.06***	0.27***	−0.02	0.02
	(0.06)	(0.07)	(0.01)	(0.05)	(0.07)	(0.05)
Sum of Lags 0–3	0.58***	0.19	−0.11***	0.69***	0.32***	0.16*
	(0.11)	(0.13)	(0.04)	(0.09)	(0.12)	(0.09)
Fiscal Endow. × Price	−1.70	−0.23	0.80	−2.51	−1.04	−1.74
$\times(t \geq 1999)\times$ (Onshore = 0)	(2.82)	(2.85)	(0.63)	(2.74)	(2.65)	(1.20)
Lag 1	1.17	1.40	0.39	0.78	1.01	0.81
	(1.95)	(2.06)	(0.57)	(1.90)	(2.02)	(1.15)
Lag 2	4.90**	5.83***	−0.01	4.91*	5.84***	−1.41
	(2.39)	(2.01)	(0.56)	(2.62)	(2.03)	(2.51)
Lag 3	1.19	1.96	1.76***	−0.57	0.19	0.22
	(2.10)	(1.72)	(0.46)	(2.20)	(1.70)	(1.45)
Sum of Lags 0–3	5.56	8.95**	2.94***	2.62	6.01*	−2.12
	(5.23)	(3.74)	(1.05)	(5.01)	(3.17)	(2.82)
Fiscal Endow. × Price	−0.83	−1.55	0.98**	−1.81**	−2.56	−0.16
$\times(t \geq 1999)\times$ (Onshore > 0)	(0.70)	(1.46)	(0.41)	(0.76)	(1.68)	(0.48)
Lag 1	2.12	2.43	0.36	1.76	2.06	−0.26
	(2.29)	(2.33)	(0.34)	(2.15)	(2.08)	(0.46)
Lag 2	0.82	1.59	−0.70**	1.53	2.34**	0.12
	(1.07)	(1.00)	(0.30)	(0.99)	(1.01)	(0.77)
Lag 3	0.15	3.38*	0.89***	−0.75	2.43	1.79
	(0.74)	(1.74)	(0.29)	(0.71)	(1.50)	(1.25)
Sum of Lags 0–3	2.27	5.85***	1.53**	0.74	4.27**	1.49
	(2.13)	(2.17)	(0.64)	(1.98)	(2.03)	(1.67)
p-value: No Interaction Effect	0.548	0.440	0.224	0.719	0.612	0.252
Observations	421,924	399,655	421,903	421,903	399,655	421,108
Plants	51,726	50,357	51,725	51,725	50,357	51,627
Districts	280	280	280	280	280	280

Notes: This table presents first-difference estimates of the coefficients in Equation (1) for $K = 3$. The p-values correspond to the test of $H_0: \sum_{k=0}^{K} \delta_k = \sum_{k=0}^{K} \gamma_k$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: First Difference: Robustness to Adding Controls

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
	Panel A: Controlling for Baseline Covariates					
Onshore Endow. × Price	0.18***	0.17	−0.09**	0.26***	0.27**	0.20***
	(0.06)	(0.11)	(0.03)	(0.06)	(0.12)	(0.04)
Fiscal Endow. × Price	−2.27	−0.33	0.48	−2.75	−0.81	−2.46*
$\times (t \geq 1999) \times$ (Onshore = 0)	(3.33)	(3.32)	(0.64)	(3.17)	(3.03)	(1.45)
Fiscal Endow. × Price	−1.05	−1.83	1.11**	−2.16**	−3.01*	−0.70
$\times (t \geq 1999) \times$ (Onshore > 0)	(0.88)	(1.54)	(0.45)	(0.98)	(1.78)	(0.63)
p-value: No Interaction Effect	0.705	0.649	0.381	0.849	0.490	0.197
	Panel B: Controlling for Proxies for Baseline Central Expenditure					
Onshore Endow. × Price	0.31***	0.27*	−0.04	0.35***	0.35**	0.18***
	(0.10)	(0.14)	(0.04)	(0.09)	(0.15)	(0.05)
Fiscal Endow. × Price	−2.02	−0.57	0.87	−2.89	−1.45	−1.88
$\times (t \geq 1999) \times$ (Onshore = 0)	(3.18)	(3.23)	(0.68)	(3.15)	(3.06)	(1.38)
Fiscal Endow. × Price	−1.45	−2.11	1.00**	−2.46**	−3.29*	−0.14
$\times (t \geq 1999) \times$ (Onshore > 0)	(1.07)	(1.62)	(0.48)	(1.12)	(1.89)	(0.79)
p-value: No Interaction Effect	0.856	0.643	0.866	0.888	0.580	0.221
	Panel C: Controlling for Baseline Covariates and Central Expenditure Proxies					
Onshore Endow. × Price	0.25***	0.21	−0.07*	0.33***	0.34**	0.19***
	(0.10)	(0.16)	(0.04)	(0.10)	(0.16)	(0.07)
Fiscal Endow. × Price	−2.33	−0.26	0.54	−2.87	−0.82	−2.55*
$\times (t \geq 1999) \times$ (Onshore = 0)	(3.45)	(3.38)	(0.65)	(3.28)	(3.11)	(1.43)
Fiscal Endow. × Price	−1.42	−1.87	1.13**	−2.55**	−3.21*	−0.39
$\times (t \geq 1999) \times$ (Onshore > 0)	(1.08)	(1.65)	(0.47)	(1.19)	(1.94)	(0.69)
p-value: No Interaction Effect	0.784	0.645	0.429	0.920	0.490	0.096
Observations	421,924	399,655	421,903	421,903	399,655	421,108
Plants	51,726	50,357	51,725	51,725	50,357	51,627
Districts	280	280	280	280	280	280

Notes: This table presents first-difference estimates of the coefficients in Equation (1) for $K = 0$ after adding baseline covariates interacted with year effects. Panel A estimates control for the variables in Appendix Table A.2, Panel B estimates control for the variables in Appendix Table A.3, and Panel C estimates control for both sets of variables. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: First Difference, Dynamic Model: Controlling for Baseline Covariates

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.18***	0.17	−0.08**	0.26***	0.26**	0.19***
	(0.06)	(0.11)	(0.03)	(0.06)	(0.12)	(0.04)
Lag 1	0.15*	0.19	−0.05*	0.20***	0.25**	0.07
	(0.08)	(0.13)	(0.03)	(0.07)	(0.12)	(0.04)
Lag 2	−0.02	−0.02	0.08***	−0.10	−0.11	−0.08
	(0.08)	(0.08)	(0.02)	(0.08)	(0.08)	(0.07)
Lag 3	0.26***	−0.02	−0.06***	0.32***	0.05	0.00
	(0.07)	(0.10)	(0.02)	(0.06)	(0.09)	(0.05)
Sum of Lags 0–3	0.56***	0.32*	−0.12**	0.68***	0.45***	0.18**
	(0.14)	(0.16)	(0.06)	(0.12)	(0.16)	(0.09)
Fiscal Endow. × Price	−2.08	−0.30	0.38	−2.46	−0.68	−2.40
$\times(t \geq 1999)\times$ (Onshore = 0)	(3.34)	(3.48)	(0.64)	(3.12)	(3.15)	(1.66)
Lag 1	−2.58	−2.71	0.52	−3.10	−3.23	0.26
	(3.24)	(3.01)	(0.54)	(3.22)	(2.84)	(1.41)
Lag 2	6.10**	5.75**	0.47	5.63**	5.29**	−0.58
	(2.61)	(2.58)	(0.58)	(2.54)	(2.41)	(2.30)
Lag 3	0.14	1.44	1.53***	−1.39	−0.08	−0.80
	(2.78)	(2.57)	(0.54)	(2.89)	(2.55)	(2.93)
Sum of Lags 0–3	1.58	4.19	2.90**	−1.31	1.29	−3.52
	(5.88)	(4.88)	(1.25)	(5.88)	(4.53)	(3.73)
Fiscal Endow. × Price	−0.85	−1.69	1.01**	−1.86**	−2.75	−0.69
$\times(t \geq 1999)\times$ (Onshore > 0)	(0.82)	(1.50)	(0.43)	(0.92)	(1.76)	(0.62)
Lag 1	1.02	1.37	0.42	0.60	0.94	−0.11
	(2.12)	(2.15)	(0.32)	(2.05)	(1.99)	(0.58)
Lag 2	2.10*	2.02*	−0.65*	2.75**	2.71**	0.62
	(1.20)	(1.19)	(0.37)	(1.15)	(1.18)	(0.89)
Lag 3	−0.55	2.52	0.81**	−1.36*	1.67	0.79
	(0.87)	(1.86)	(0.32)	(0.82)	(1.64)	(1.10)
Sum of Lags 0–3	1.72	4.23*	1.60**	0.12	2.57	0.61
	(2.22)	(2.19)	(0.65)	(2.14)	(2.14)	(1.64)
p-value: No Interaction Effect	0.981	0.993	0.324	0.809	0.769	0.277
Observations	421,924	399,655	421,903	421,903	399,655	421,108
Plants	51,726	50,357	51,725	51,725	50,357	51,627
Districts	280	280	280	280	280	280

Notes: This table presents first-difference estimates of the coefficients in Equation (1), additionally controlling for the covariates in Appendix Table A.2 interacted with year effects. The p-values correspond to the test of $H_0: \sum_{k=0}^{K}\delta_k = \sum_{k=0}^{K}\gamma_k$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.11: First Difference, Dynamic Model: Controlling for Baseline Central Expenditure Proxies

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.31***	0.25*	−0.04	0.35***	0.33**	0.17***
	(0.10)	(0.14)	(0.04)	(0.09)	(0.15)	(0.05)
Lag 1	0.05	0.14	−0.08**	0.13*	0.22*	−0.06
	(0.08)	(0.14)	(0.04)	(0.08)	(0.13)	(0.06)
Lag 2	−0.00	−0.12	0.08**	−0.08	−0.20**	−0.06
	(0.10)	(0.09)	(0.03)	(0.08)	(0.08)	(0.07)
Lag 3	0.22***	0.03	−0.07**	0.29***	0.13	0.11
	(0.08)	(0.11)	(0.03)	(0.08)	(0.11)	(0.08)
Sum of Lags 0–3	0.57***	0.30	−0.11	0.69***	0.48**	0.18
	(0.20)	(0.21)	(0.08)	(0.17)	(0.20)	(0.12)
Fiscal Endow. × Price	−1.86	−0.29	0.66	−2.51	−0.95	−1.69
$\times (t \geq 1999) \times$ (Onshore = 0)	(3.13)	(3.26)	(0.67)	(3.03)	(3.05)	(1.41)
Lag 1	1.54	2.51	0.82	0.73	1.69	1.11
	(1.98)	(2.36)	(0.56)	(2.14)	(2.29)	(1.59)
Lag 2	4.66*	5.83**	−0.59	5.25*	6.42***	−0.78
	(2.71)	(2.28)	(0.48)	(2.70)	(2.19)	(2.08)
Lag 3	1.76	1.95	2.02***	−0.26	−0.08	−1.88
	(1.82)	(1.62)	(0.56)	(2.04)	(1.65)	(1.51)
Sum of Lags 0–3	6.11	10.00**	2.91**	3.20	7.08**	−3.24
	(4.86)	(4.00)	(1.34)	(4.50)	(3.23)	(2.99)
Fiscal Endow. × Price	−1.45	−2.10	0.86*	−2.31**	−3.13*	−0.34
$\times (t \geq 1999) \times$ (Onshore > 0)	(1.01)	(1.58)	(0.46)	(1.06)	(1.87)	(0.76)
Lag 1	2.14	2.61	0.55	1.59	2.05	0.36
	(2.21)	(2.19)	(0.40)	(2.05)	(1.95)	(0.65)
Lag 2	0.80	1.71	−1.02**	1.82	2.78**	−0.01
	(1.38)	(1.26)	(0.49)	(1.14)	(1.11)	(0.88)
Lag 3	0.68	3.68**	1.01***	−0.33	2.56*	1.17
	(0.88)	(1.77)	(0.34)	(0.79)	(1.52)	(1.46)
Sum of Lags 0–3	2.17	5.90***	1.40	0.76	4.25**	1.18
	(2.14)	(2.13)	(0.87)	(1.97)	(2.00)	(1.61)
p-value: No Interaction Effect	0.442	0.318	0.340	0.608	0.393	0.185
Observations	421,924	399,655	421,903	421,903	399,655	421,108
Plants	51,726	50,357	51,725	51,725	50,357	51,627
Districts	280	280	280	280	280	280

Notes: This table presents first-difference estimates of the coefficients in Equation (1), additionally controlling for the variables in Appendix Table A.3 interacted with year effects. The p-values correspond to the test of $H_0 : \sum_{k=0}^{K} \delta_k = \sum_{k=0}^{K} \gamma_k$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: First Difference, Dynamic Model: Controlling for Baseline Covariates and Central Expenditure Proxies

	Log(Outcome)					
	(1) Output	(2) Value Added	(3) Number of Workers	(4) Output per Worker	(5) Value Added per Worker	(6) Wage
Onshore Endow. × Price	0.25***	0.20	−0.07	0.32***	0.32*	0.18***
	(0.10)	(0.16)	(0.04)	(0.10)	(0.16)	(0.07)
Lag 1	0.14	0.24*	−0.05	0.19**	0.29**	0.06
	(0.09)	(0.14)	(0.04)	(0.08)	(0.13)	(0.07)
Lag 2	−0.08	−0.13	0.06*	−0.14	−0.20*	−0.05
	(0.11)	(0.11)	(0.04)	(0.10)	(0.11)	(0.09)
Lag 3	0.18**	−0.02	−0.07**	0.26***	0.07	0.03
	(0.09)	(0.12)	(0.04)	(0.09)	(0.12)	(0.07)
Sum of Lags 0–3	0.50***	0.30	−0.13	0.63***	0.49**	0.23*
	(0.19)	(0.23)	(0.09)	(0.17)	(0.22)	(0.12)
Fiscal Endow. × Price	−2.05	−0.00	0.42	−2.47	−0.43	−2.33
$\times(t \geq 1999)\times$ (Onshore = 0)	(3.46)	(3.53)	(0.65)	(3.23)	(3.21)	(1.52)
Lag 1	−1.55	−1.25	0.80	−2.35	−2.05	0.25
	(2.96)	(2.79)	(0.60)	(3.13)	(2.72)	(1.43)
Lag 2	6.50**	6.41**	0.18	6.32**	6.23**	−0.12
	(2.54)	(2.63)	(0.50)	(2.47)	(2.49)	(2.12)
Lag 3	0.13	0.56	1.70***	−1.57	−1.14	−2.19
	(2.54)	(2.17)	(0.61)	(2.73)	(2.22)	(2.73)
Sum of Lags 0–3	3.03	5.72	3.10**	−0.08	2.60	−4.39
	(5.34)	(4.66)	(1.39)	(5.32)	(4.15)	(3.87)
Fiscal Endow. × Price	−1.26	−1.72	1.03**	−2.28**	−2.95	−0.42
$\times(t \geq 1999)\times$ (Onshore > 0)	(1.02)	(1.61)	(0.44)	(1.11)	(1.91)	(0.69)
Lag 1	1.12	1.37	0.44	0.68	0.93	−0.05
	(2.15)	(2.02)	(0.34)	(2.04)	(1.83)	(0.65)
Lag 2	2.13*	2.32*	−0.70	2.83**	3.06***	0.32
	(1.20)	(1.25)	(0.49)	(1.11)	(1.19)	(1.04)
Lag 3	0.29	3.05*	0.95***	−0.67	2.02	0.77
	(0.87)	(1.66)	(0.33)	(0.84)	(1.48)	(1.12)
Sum of Lags 0–3	2.27	5.03**	1.71**	0.56	3.07	0.62
	(2.21)	(2.12)	(0.76)	(2.20)	(2.18)	(1.58)
p-value: No Interaction Effect	0.886	0.879	0.374	0.905	0.911	0.198
Observations	421,924	399,655	421,903	421,903	399,655	421,108
Plants	51,726	50,357	51,725	51,725	50,357	51,627
Districts	280	280	280	280	280	280

Notes: This table presents first-difference estimates of the coefficients in Equation (1), additionally controlling for the variables in Appendix Tables A.2 and A.3 interacted with year effects. The p-values correspond to the test of $H_0: \sum_{k=0}^{K} \delta_k = \sum_{k=0}^{K} \gamma_k$. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1980 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Log Non-Oil GDP: Controlling for Baseline Covariates

	Fixed Effects		First Difference	
	(1)	(2)	(3)	(4)
Onshore Endow. × Price	1.51^{***}	1.57^{***}	0.46^{**}	0.53^{***}
	(0.53)	(0.55)	(0.18)	(0.20)
Lag 1		0.22		0.16
		(0.17)		(0.18)
Lag 2		0.15		−0.01
		(0.13)		(0.08)
Lag 3		-0.68^{**}		-0.23^{***}
		(0.35)		(0.09)
Sum of Lags 0–3		1.26^{**}		0.45
		(0.54)		(0.47)
Fiscal Endow. × Price	12.85^{***}	10.37^{***}	3.42^{*}	3.48^{*}
$\times(t \geq 1999)\times$ (Onshore = 0)	(4.44)	(2.29)	(1.75)	(1.85)
Lag 1		2.70^{*}		2.87^{*}
		(1.40)		(1.59)
Lag 2		2.51^{*}		1.62^{*}
		(1.47)		(0.97)
Lag 3		−2.72		1.23^{**}
		(2.25)		(0.57)
Sum of Lags 0–3		12.87^{**}		9.20^{**}
		(5.06)		(4.41)
Fiscal Endow. × Price	6.76^{**}	−0.52	0.92	0.85
$\times(t \geq 1999)\times$ (Onshore > 0)	(3.27)	(1.94)	(0.79)	(0.78)
Lag 1		2.93^{**}		3.14^{**}
		(1.23)		(1.42)
Lag 2		1.90		1.64^{*}
		(1.22)		(0.88)
Lag 3		4.25^{***}		2.55^{***}
		(1.19)		(0.83)
Sum of Lags 0–3		8.56^{**}		8.19^{**}
		(3.76)		(3.56)
p-value: No Interaction Effect	0.239	0.468	0.173	0.852
Observations	6,062	6,062	5,773	5,773
Districts	289	289	289	289

Notes: This table presents fixed-effects and first-difference estimates of the coefficients in Equation (3) for $K = 0$ and $K = 3$, additionally controlling for the variables in Appendix Table A.2 interacted with year effects. The sample covers 1993–2013. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1993 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Log Non-Oil GDP: Controlling for Baseline Central Expenditure Proxies

	Fixed Effects		First Difference	
	(1)	(2)	(3)	(4)
Onshore Endow. × Price	1.87***	1.92***	0.59***	0.66***
	(0.60)	(0.53)	(0.19)	(0.21)
Lag 1		0.34*		0.24
		(0.20)		(0.22)
Lag 2		0.14		−0.06
		(0.16)		(0.09)
Lag 3		−0.83***		−0.22**
		(0.29)		(0.10)
Sum of Lags 0–3		1.57**		0.61
		(0.63)		(0.54)
Fiscal Endow. × Price	14.20***	10.23***	3.77**	3.88**
$\times (t \geq 1999) \times$ (Onshore = 0)	(4.60)	(2.37)	(1.67)	(1.78)
Lag 1		3.26**		3.49**
		(1.33)		(1.53)
Lag 2		3.12**		2.06**
		(1.55)		(0.95)
Lag 3		−2.04		1.21**
		(1.88)		(0.53)
Sum of Lags 0–3		14.57***		10.64**
		(5.17)		(4.20)
Fiscal Endow. × Price	8.21**	−0.43	0.79	0.78
$\times (t \geq 1999) \times$ (Onshore > 0)	(3.87)	(2.28)	(0.95)	(0.92)
Lag 1		3.49**		3.83**
		(1.37)		(1.58)
Lag 2		2.55*		2.30**
		(1.37)		(1.01)
Lag 3		4.71***		2.65***
		(1.46)		(1.01)
Sum of Lags 0–3		10.33**		9.56**
		(4.42)		(4.04)
p-value: No Interaction Effect	0.274	0.491	0.107	0.844
Observations	6,062	6,062	5,773	5,773
Districts	289	289	289	289

Notes: This table presents fixed-effects and first-difference estimates of the coefficients in Equation (3) for $K = 0$ and $K = 3$, additionally controlling for the variables in Appendix Table A.3 interacted with year effects. The sample covers 1993–2013. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1993 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Log Non-Oil GDP: Controlling for Baseline Covariates and Central Expenditure Proxies

	Fixed Effects		First Difference	
	(1)	(2)	(3)	(4)
Onshore Endow. × Price	1.38**	1.32**	0.54***	0.61***
	(0.62)	(0.62)	(0.20)	(0.21)
Lag 1		0.31		0.24
		(0.23)		(0.23)
Lag 2		0.06		−0.04
		(0.16)		(0.10)
Lag 3		−0.50		−0.22**
		(0.35)		(0.11)
Sum of Lags 0–3		1.19*		0.59
		(0.65)		(0.56)
Fiscal Endow. × Price	12.92***	10.50***	3.47**	3.54**
$\times (t \geq 1999) \times$ (Onshore = 0)	(4.49)	(2.50)	(1.63)	(1.74)
Lag 1		2.71**		2.87*
		(1.37)		(1.56)
Lag 2		2.55*		1.67*
		(1.43)		(0.91)
Lag 3		−2.85		1.18**
		(1.88)		(0.51)
Sum of Lags 0–3		12.90***		9.26**
		(4.98)		(4.07)
Fiscal Endow. × Price	7.20*	0.13	0.43	0.39
$\times (t \geq 1999) \times$ (Onshore > 0)	(3.70)	(2.26)	(1.01)	(0.98)
Lag 1		2.71**		2.92*
		(1.34)		(1.52)
Lag 2		2.31*		1.86**
		(1.30)		(0.90)
Lag 3		3.83***		2.51***
		(1.30)		(0.93)
Sum of Lags 0–3		8.97**		7.68**
		(4.19)		(3.79)
p-value: No Interaction Effect	0.276	0.502	0.096	0.762
Observations	6,062	6,062	5,773	5,773
Districts	289	289	289	289

Notes: This table presents fixed-effects and first-difference estimates of the coefficients in Equation (3) for $K = 0$ and $K = 3$, additionally controlling for the variables in Appendix Tables A.2 and A.3 interacted with year effects. The sample covers 1993–2013. Each endowment × price variable is measured in constant 2010 USD millions per capita based on 1993 population. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1993 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Baseline Infrastructure and Endowments

	Share of Villages with Paved Road in 1999		Share of Villages with All-Weather Road in 1999		Share of Villages with Street Lights in 1999		Share of Households with Electricity in 1999	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Onshore Endow. > 0)	−0.139***	−0.099*	−0.056	−0.025	0.011	0.013	0.046	0.045
	(0.052)	(0.053)	(0.049)	(0.037)	(0.060)	(0.060)	(0.046)	(0.046)
(Fiscal Endow. ≥ M) × (Onshore = 0)	0.001	0.042	−0.030	−0.001	0.093*	0.075	0.117***	0.101**
	(0.044)	(0.044)	(0.035)	(0.028)	(0.054)	(0.058)	(0.040)	(0.041)
(0 < Fiscal Endow. < M) × (Onshore = 0)	−0.036	−0.076*	0.082***	0.030	0.223***	0.131***	0.157***	0.063*
	(0.035)	(0.042)	(0.022)	(0.023)	(0.043)	(0.050)	(0.031)	(0.034)
Outcome Mean	0.704	0.704	0.911	0.911	0.630	0.630	0.696	0.696
p-value: Onshore = High Fiscal	0.014	0.007	0.626	0.542	0.218	0.311	0.144	0.178
p-value: Onshore = Low Fiscal	0.043	0.662	0.003	0.132	0.000	0.035	0.007	0.644
Region FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	289	289	289	289	289	289	289	289

Notes: This table reports estimates from regressions of pre-decentralization infrastructure outcomes on dummies for the presence of district endowments. The regressions control for region fixed effects, as indicated. The term "village" refers to both predominantly rural villages (*desa*) and predominantly urban wards (*kelurahan*). Standard errors, reported in parentheses, are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.17: Baseline Corruption and Endowments

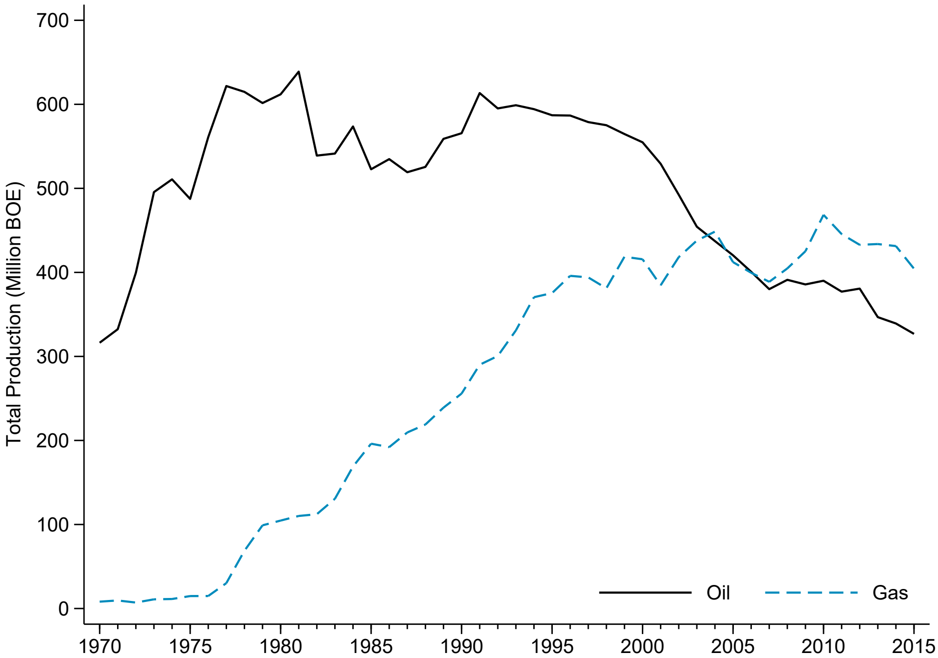
	Probit: Firm Paid Any Gifts in 2000			Poisson: Value of Gifts in 2000		
	(1)	(2)	(3)	(4)	(5)	(6)
(Onshore Endow. > 0)	−0.042	−0.030	−0.061	−0.221	−0.454	−0.373
	(0.118)	(0.106)	(0.112)	(0.389)	(0.397)	(0.461)
(Fiscal Endow. ≥ M) × (Onshore = 0)	0.053	0.055	0.026	0.844*	0.394	0.538
	(0.064)	(0.055)	(0.059)	(0.436)	(0.497)	(0.573)
(0 < Fiscal Endow. < M) × (Onshore = 0)	0.072	0.056	0.031	0.040	0.385	0.385
	(0.044)	(0.039)	(0.042)	(0.395)	(0.336)	(0.335)
Outcome Mean	0.63	0.63	0.63	25.59	25.59	25.59
p-value: Onshore = High Fiscal	0.453	0.450	0.449	0.022	0.170	0.170
p-value: Onshore = Low Fiscal	0.332	0.418	0.434	0.541	0.086	0.146
Quartic in Log Firm Output	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes
Observations	15,392	15,392	15,392	15,392	15,392	15,392
Districts	254	254	254	254	254	254

Notes: This table reports estimates of the cross-sectional relationship between district endowments and gifts paid by manufacturing firms to external parties before decentralization. Columns 1–3 report average marginal effects from a probit regression, where the outcome is an indicator equal to one if the firm paid any gifts. Columns 4–6 report coefficients from an exponential mean model estimated by Poisson quasi-maximum likelihood, where the outcome is the value of gifts paid, in constant 2010 IDR 1 thousand (approximately USD 0.11). The regressions control for a quartic polynomial in log firm output and region fixed effects, as indicated. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering at the level of 1975 district borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

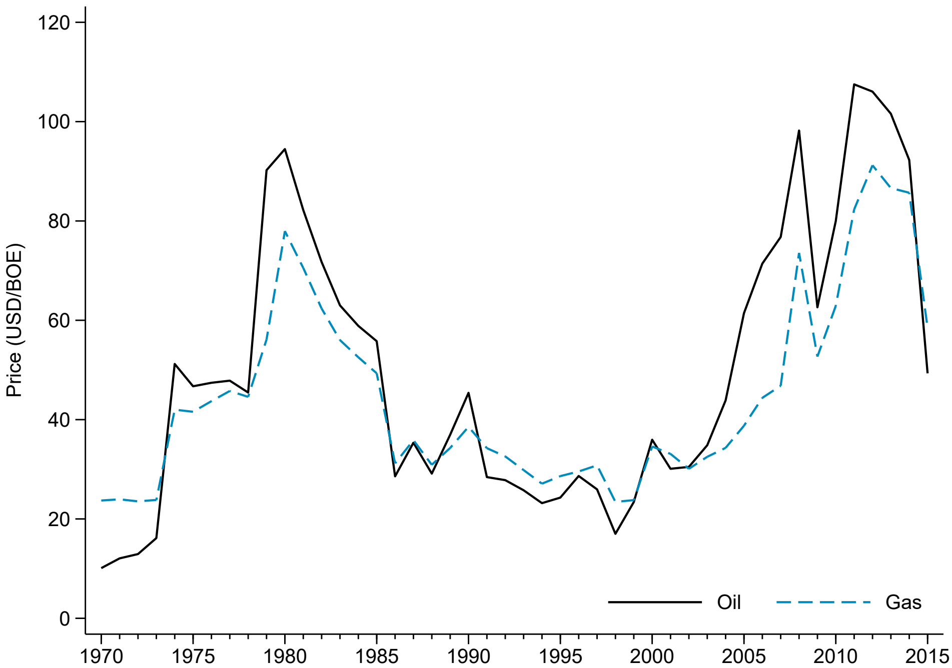
A.4 Figures

Figure A.1: Oil and Gas Production and Prices

(a) Production

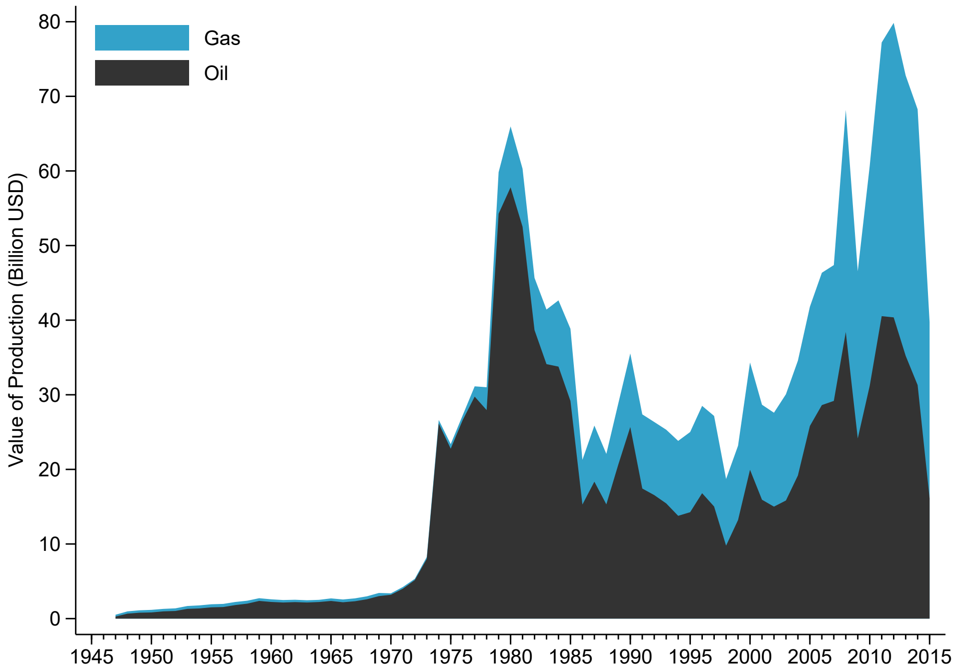


(b) Prices



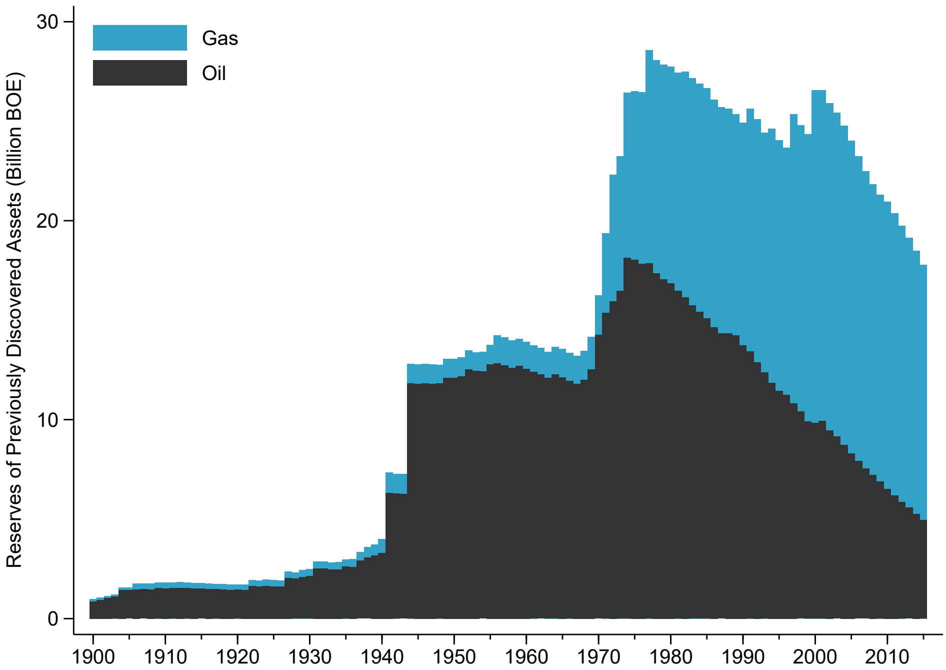
Notes: Prices are expressed in constant 2010 USD.

Figure A.2: Historical Oil and Gas Production



Notes: The value of production is expressed in constant 2010 USD billions. The production data are missing prior to independence in 1947.

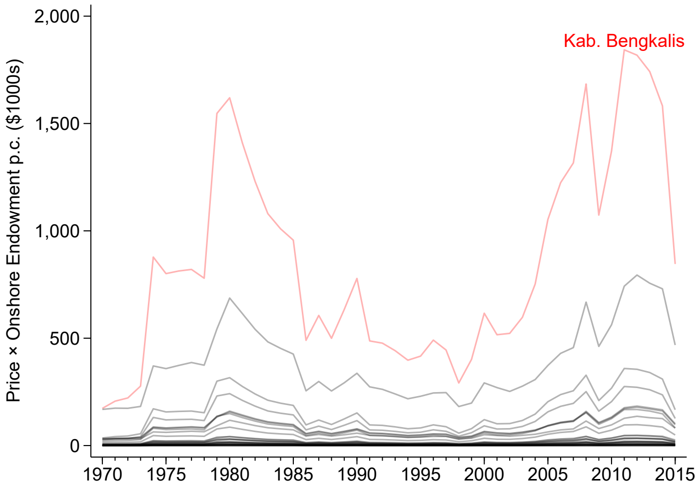
Figure A.3: Historical Oil and Gas Reserves



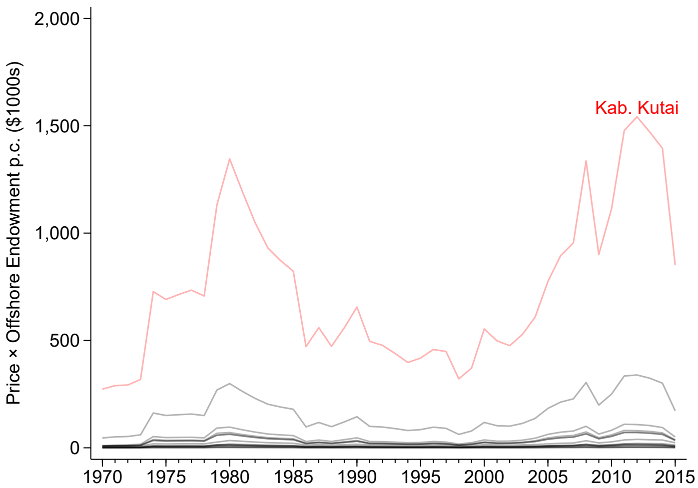
Notes: Reserves are expressed in billions of barrels of oil equivalent.

Figure A.4: Extraction and Fiscal Shocks by District

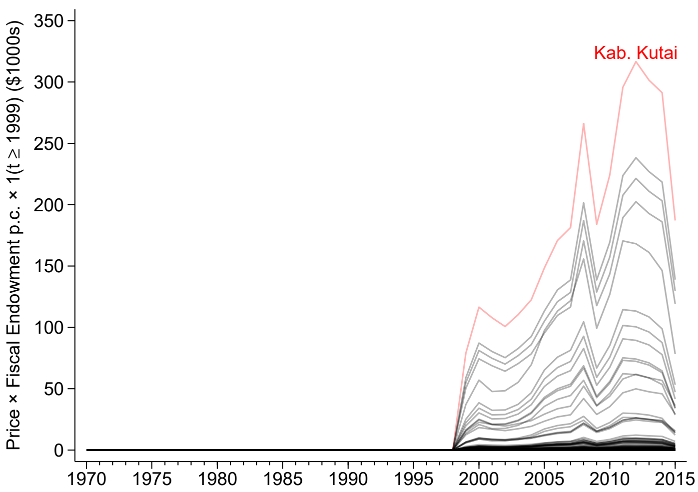
(a) Onshore Shocks



(b) Offshore Shocks

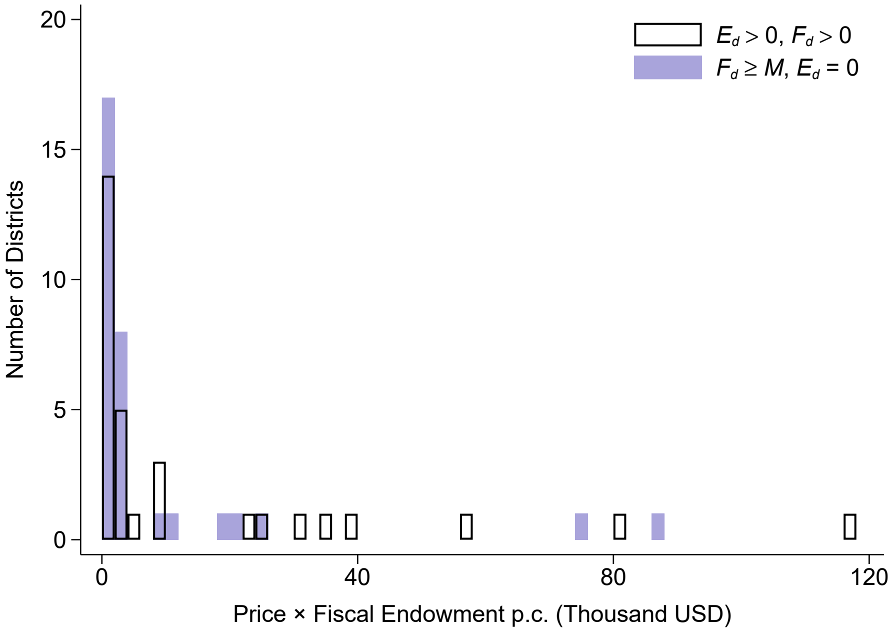


(c) Fiscal Shocks



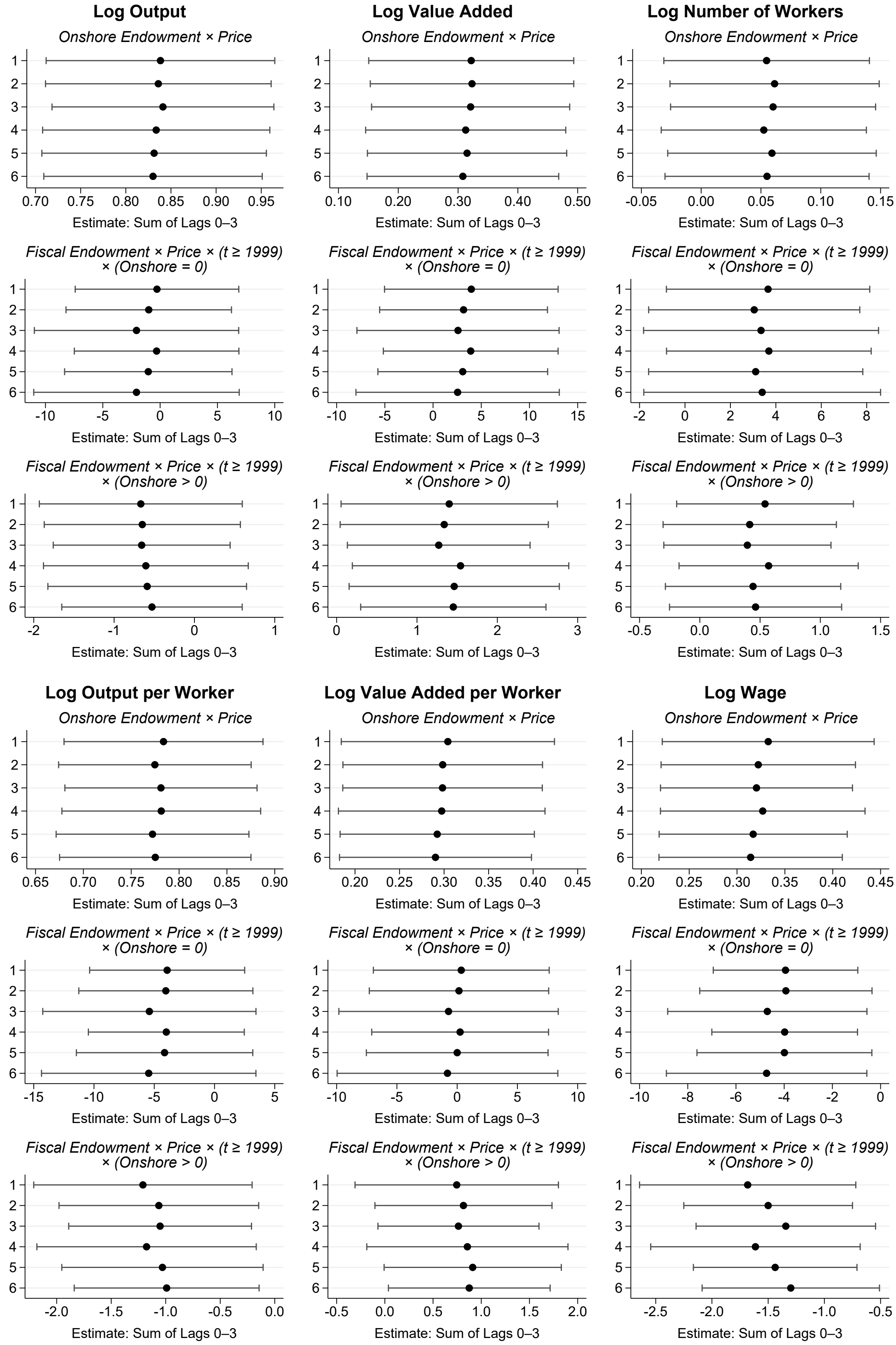
Notes: Panel (a) plots the onshore extraction shock, $\boldsymbol{P}_t'\boldsymbol{E}_d$, separately for each district. Panel (b) plots the offshore extraction shock, while Panel (c) plots the fiscal shock, $\boldsymbol{P}_t'\boldsymbol{F}_d \cdot 1(t \geq 1999)$. Shocks are expressed in constant 2010 USD thousands per capita based on 1980 population.

Figure A.5: Fiscal Endowment Distributions for Difference-in-Differences Treatment Groups



Notes: This figure plots the fiscal endowment distributions for groups 1 and 2 in Equation (2).

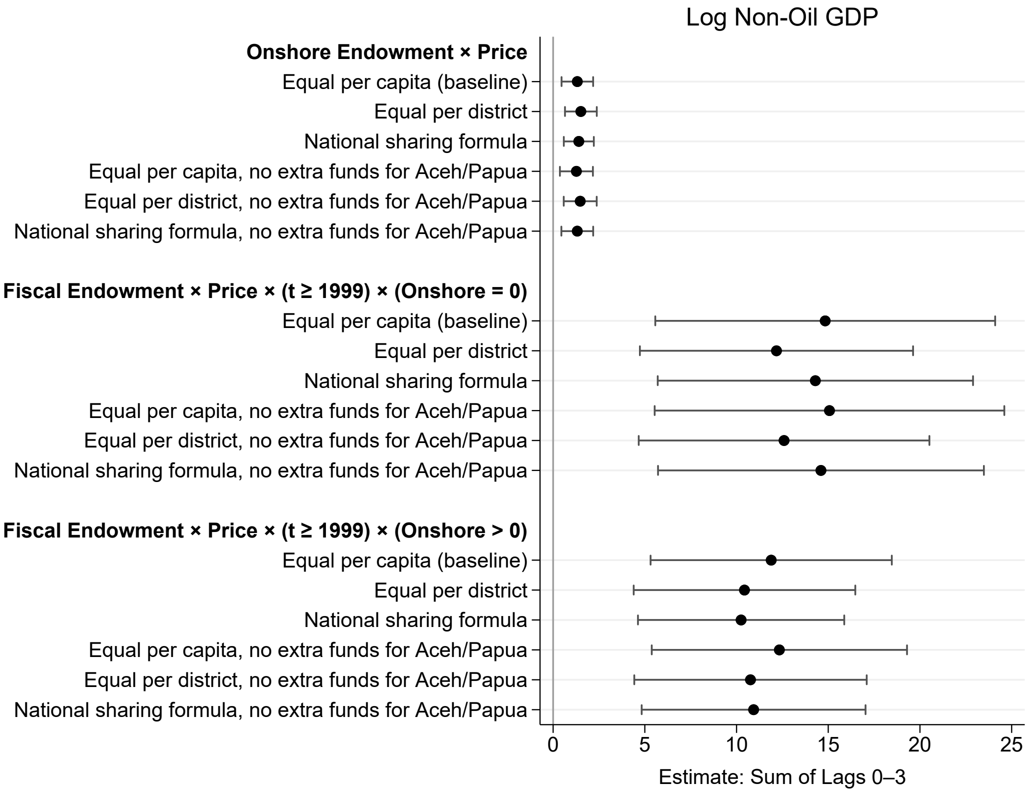
Figure A.6: Varying Assumptions about How Provincial Revenue is Allocated to Districts



Allocation Assumptions

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

Figure A.7: Varying Assumptions about How Provincial Revenue is Allocated to Districts



Notes: This figure plots fixed-effects estimates and 95-percent confidence intervals for $\sum_{k=0}^{3}\beta_k$, $\sum_{k=0}^{3}\delta_k$, and $\sum_{k=0}^{3}\gamma_k$ 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 West Papua that is used in the rest of the country, rather than allocating extra revenue to these regions. Confidence intervals are robust to heteroskedasticity and clustering at the level of 1993 district borders.