THE ECONOMIC JOURNAL

The Economic Journal, **135** (*August*), 2017–2053 https://doi.org/10.1093/ej/ueaf022
Advance Access Publication Date: 11 April 2025

REVENUE PERSISTENCE AND PUBLIC SERVICE DELIVERY*

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I exploit unusual policy variation in Indonesia to examine how local responses to intergovernmental grants depend on their persistence. A national reform generated permanent increases in the general grant that were larger for less densely populated districts, while hydrocarbon-rich districts experienced transitory shocks to shared resource revenue. Public service delivery strongly responded to the permanent shock, but not to the transitory shocks, consistent with districts providing lumpy public services as a function of lifetime fiscal resources. The timing and composition of expenditure responses are consistent with this mechanism. The results suggest that the underwhelming effects of natural resource revenue found in previous studies could be due, in part, to forward-looking behaviour by local governments.

> Citizens perceive the granting of intergovernmental fiscal transfers as the magical art of passing money from one government to another and seeing it vanish into thin air. These perceptions are well grounded in reality in developing countries.
>
> Anwar Shah, '*A Practitioner's Guide to Intergovernmental Fiscal Transfers*' (2006, p. 17)

How local governments respond to intergovernmental transfers is a fundamental question in public finance. The issue is especially salient in developing countries, where transfers finance around 60% of subnational expenditure compared to only a third in OECD countries (Shah and Shah, 2006). However, a widespread concern in academic and policy circles is that transfers may fail to stimulate improvements in public service delivery in developing countries. Some recent

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This paper was received on 17 August 2023 and accepted on 18 March 2025. The Editor was Sule Alan.

The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The author was granted an exemption to publish parts of their data because access to these data is restricted. However, the author provided the Journal with temporary access to the data, which enabled the Journal to run their codes. The codes for the parts subject to exemption are also available on the Journal repository. The restricted access data and these codes were also checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.15020648.

A previous version of this paper was circulated with the title, 'Do Intergovernmental Grants Improve Public Service Delivery in Developing Countries?'.

I am grateful to the editor, Sule Alan, and two anonymous referees for valuable comments that improved the paper. I also thank David Agrawal, David Albouy, Martín Besfamille, Chris Blattman, Hoyt Bleakley, Will Boning, Anne Brockmeyer, Alecia Cassidy, Connor Cole, Jim Cust, Mark Dincecco, François Gerard, Diana Greenwald, Steve Hamilton, Jim Hines, Brad Humphreys, Byung-Cheol Kim, Shin Kimura, Joana Naritomi, Ricardo Perez-Truglia, Pablo Querubín, Monica Singhal, Joel Slemrod, Ugo Troiano, Tejaswi Velayudhan, Dean Yang, and seminar participants at the University of Michigan at Ann Arbor, University of Michigan at Dearborn, Northwestern University, Office of Tax Analysis at the US Department of the Treasury, George Washington University, University of Illinois at Urbana-Champaign, University of Wyoming, Florida International University, University of Alabama, National Tax Association Annual Conference, Southern Economic Association Annual Conference, and International Institute of Public Finance Annual Congress for helpful comments. I am grateful to Monica Martinez-Bravo and Andreas Stegmann for generously sharing data. I thank Lauren Johnson for excellent research assistance. I gratefully acknowledge financial support from the University of Michigan Library, the University of Michigan Rackham Graduate School, and the Michigan Institute for Teaching and Research in Economics (MITRE).

studies have found the impact of transfers to be smaller than expected (Caselli and Michaels, 2013) or non-existent (Gadenne, 2017; Martínez, 2023). The typical explanations are corruption and waste.[1]

In this paper I argue that the impact of transfers on public services depends on the persistence of the revenue source. Transitory shocks to a volatile transfer, such as shared natural resource revenue, have a small impact on the local government's intertemporal budget constraint. A forward-looking government therefore may not invest in new structures or hire frontline workers in response to an increase in such a transfer. By contrast, a permanent increase in transfers should produce noticeable improvements in public services, as long as corruption is not all-encompassing. Intertemporal optimisation could thus explain, at least in part, the small estimated impact of volatile transfers on public service delivery.

To test this theory, I compare local government responses to permanent and transitory shocks to transfers in Indonesia. The country's largest intergovernmental transfer, the general grant, is highly persistent. A change in the allocation formula in 2006 resulted in permanent increases in this grant that were larger for less densely populated districts. I exploit the sharp increase in the revenue gradient in land area per capita to estimate the causal effects of a *permanent* increase in fiscal transfers. The second-largest transfer is the oil and gas grant, which is tied to local hydrocarbon extraction and exhibits significant transitory variation in hydrocarbon-rich areas. I exploit the central government's royalty-sharing rule, spatial variation in initial hydrocarbon endowments and time-series variation in aggregate revenue from this grant to estimate the causal effects of *transitory* shocks to fiscal transfers.

The permanent increase in the general grant stimulated greater provision of public schools, health facilities and personnel, and local roads. Increasing the grant by 1 million rupiah (IDR) (approximately USD100) per capita improved overall public service delivery by 0.6 SD, relative to pre-reform levels. By contrast, transitory shocks to the oil and gas grant had small effects, increasing overall public service delivery by 0.1 SD. We can statistically reject equal responses of overall public service delivery to the two grants at the 1% level.

The results are consistent with a model in which local governments provide lumpy public goods and services as a function of lifetime fiscal resources. The mean-reverting nature of the oil and gas grant implies that current-year changes have a small impact on lifetime resources. Even if the government has a high discount rate, it will be hesitant to increase spending on structures such as schools, which require a large upfront investment and a future stream of maintenance expenditure, or on employees that enjoy significant job security, when oil and gas revenue increases. Holding fixed the size of the initial shock, more persistent increases in revenue are more likely to stimulate greater investment and hiring of frontline workers.

Supporting this mechanism, the expenditure response to the general grant is hump-shaped over time and overshoots at its peak, increasing by about 1.60 rupiah for every rupiah of revenue, indicating large upfront investments. Hydrocarbon-rich districts do not perfectly smooth their spending, but the expenditure response to the oil and gas grant is around one-third of the response to the general grant. Furthermore, the gap in the responses is smaller for more discretionary and less lumpy categories of spending, and larger for capital and personnel expenditure.

I consider other potential mechanisms. Differences in administration are unlikely to explain the results, as the two grants are subject to the same rules and oversight by the central government.

[1] For examples of local officials misappropriating funds from the centre, see, e.g., Reinikka and Svensson (2004), Olken (2007), Ferraz and Finan (2008; 2011) and Brollo *et al.* (2013).

Another possibility is that district responses are non-linear in the size of the initial shock, or asymmetric with respect to increases and decreases in transfers. I test for these two mechanisms and find little evidence that they drive the results. I also show that the results are not driven by changes in political competition or differential pre-trends.

Alternatively, hydrocarbon-poor districts may spend their funds more efficiently than hydrocarbon-rich districts, regardless of the source. This difference in spending efficiency could provide an explanation for the results, given that the permanent shock to the general grant was confined to hydrocarbon-poor areas. However, I examine a wide array of governance indicators and find no evidence that governance is better in hydrocarbon-poor districts. While waste and corruption undoubtedly plague many district governments in Indonesia, these problems are not worse on average in hydrocarbon-rich districts. Other characteristics—such as urbanisation, education level and GDP per capita—differ for districts exposed to the general grant shock compared to hydrocarbon-rich districts. However, adjusting for covariate imbalance and allowing for heterogeneous responses based on these factors yields results that are similar to the baseline results. Furthermore, hydrocarbon-rich districts experience steady improvement in public services relative to hydrocarbon-poor districts following the *permanent* revenue increase caused by the introduction of resource-revenue sharing in 2001. The evidence thus points to revenue persistence, rather than baseline district characteristics, as the driving force behind the results.

The Indonesian setting offers many advantages. First and foremost is the unique policy variation: the two most important intergovernmental transfers were subject to shocks of differing persistence, but were otherwise comparable. Second, there are a large number of district governments—over 300—with broad spending authority in the areas of education, health and infrastructure. Third, districts had no control over income taxes and little control over property taxes during the study period. This virtually eliminates an important margin of response to revenue shocks—tax cuts—and enables the analysis to isolate the decision of how much to spend rather than save, and when to spend. Fourth, rich data on fiscal outcomes and public services over 1993–2014 make it possible to examine dynamic responses to fiscal transfers along many margins.

The results are informative for decentralisation policy around the world. International organisations have pushed for greater fiscal decentralisation in the developing world (World Bank, 1999; United Nations, 2009), but central governments have generally been hesitant to devolve tax responsibilities to local governments. An important question is whether central governments in developing countries should cede more tax authority to subnational governments or continue to rely on grants (Gadenne and Singhal, 2014). Knowing the impact of intergovernmental transfers on public service delivery, and what type of variation in transfers can yield this information, is an important first step.

This paper contributes to multiple literatures in development and public finance. First, it contributes to the literature that examines whether intergovernmental transfers actually improve public service delivery. Caselli and Michaels (2013) find that shared oil and gas revenue failed to stimulate improvements in public services in Brazilian municipalities. However, Litschig and Morrison (2013) show that in an earlier period in Brazil, a formula-based, general-purpose transfer improved education outcomes. Interestingly, they exploit a large shock to the transfer that lasted for four years, making it relatively persistent.[2] Gadenne (2017) and Martínez (2023) examine whether increases in local tax revenue lead to better outcomes than increases in transfers

[2] Olsson and Valsecchi (2015) provide earlier evidence that Indonesia's oil and gas grant improved public service delivery using a shorter panel and a different empirical strategy than the present paper.

in Brazil and Colombia, respectively. Both studies conclude that tax revenue stimulates improvements in public service delivery, but transfers do not, arguing that citizens hold politicians more accountable for how they spend tax revenue.[3] These studies do not report the persistence of the revenue sources, so differences in persistence could, in theory, contribute to the results.

Second, this paper is related to research on the so-called flypaper effect, the empirical regularity that local governments have a greater propensity to spend out of non-matching grants than out of local private income.[4] My work differs from this literature in three ways. First, these papers focus on a single grant, while I compare responses to two different grants. Second, researchers typically ask how much grant revenue was spent versus passed on to citizens via tax cuts, whereas I focus on how the expenditure response depends on the persistence of the grant in a setting where local governments have little control over tax rates. Third, this literature focuses on reported expenditure by local governments, whereas I also employ measures of public service provision.

To the best of my knowledge, the only other study that compares two grants with differing persistence is concurrent work by Besfamille *et al.* (2023). They find that Argentinian provinces significantly adjust expenditure in response to changes in a relatively persistent grant based on shared tax revenue, but not in response to changes in volatile hydrocarbon royalties. Their fiscal results are therefore qualitatively similar to mine. The main difference between the two papers is that Besfamille *et al.* (2023) examine total spending and debt, whereas I study both fiscal outcomes and measures of actual public service delivery.

Finally, this research contributes to the literature on the resource curse (Van der Ploeg, 2011). One concern in this literature is that the volatility and sheer size of resource-related transfers will lead to wasteful and volatile local spending (Cust and Viale, 2016; Natural Resource Governance Institute, 2016). If this concern is well founded, then central governments should smooth revenue on behalf of local governments and distribute the funds from resource extraction more evenly across regions. I contribute to this debate by showing that in the context of Indonesia, natural resource revenue and less volatile general-purpose grants promote public service delivery to a similar degree, after properly accounting for the persistence of revenue shocks. Local governments thus seem capable of managing the volatility of natural resource revenue.

The rest of the paper proceeds as follows. Section 1 provides background information on institutions and local public finance in Indonesia following the country's transition to democracy. Section 2 introduces a conceptual framework that generates testable hypotheses about how local governments will respond to permanent and transitory revenue shocks. Section 3 presents the fiscal responses to the two grants and Section 4 presents the impacts on public service delivery. Section 5 provides a discussion of the results and Section 6 concludes.

1. Policy Context

1.1. *Democratisation and Decentralisation*

The 1997 Asian financial crisis exposed long-standing political grievances in Indonesia, triggering demonstrations and civil unrest throughout the country. These protests culminated in the

[3] In a related study, Borge *et al.* (2015) find that natural resource revenue and non-resource revenue have similar effects on spending efficiency in Norwegian municipalities.

[4] See Hines and Thaler (1995) and Inman (2008) for summaries of the literature. Recent contributions include Knight (2002), Gordon (2004), Baicker (2005), Dahlberg *et al.* (2008), Lutz (2010), Cascio *et al.* (2013), Gennari and Messina (2014), Lundqvist (2015), Vegh and Vuletin (2015), Dahlby and Ferede (2016), Liu and Ma (2016), Leduc and Wilson (2017), Solé-Ollé and Viladecans-Marsal (2019) and Helm and Stuhler (2022).

resignation of President Suharto in May of 1998, marking the end of three decades of centralised, authoritarian rule. In 1999, democratic elections were held at the national and subnational levels, and the central government passed a law devolving significant autonomy and fiscal resources to subnational governments starting in 2001 (Law No. 22/1999 and Law No. 25/1999).

Indonesia has four levels of subnational public administration: province, district, subdistrict and village. Districts are responsible for the majority of subnational policymaking; provinces primarily play a coordinating role and subdistricts (*kecamatan*) implement district policies. Districts are classified as either rural districts (*kabupaten*) or urban districts (*kota*), but both types operate under the same political and fiscal institutions. The central government empowered districts, rather than provinces, partly because it feared that some provinces would attempt to secede if given autonomy.[5]

Starting in 1999, district parliaments were directly elected through a proportional representation system. The district heads ('mayors') previously appointed by Suharto were allowed to complete their five-year terms, after which the local parliament appointed the mayor. Starting in 2005, voters directly elected the mayor. Incumbent mayors were permitted to finish their terms before direct elections could be held, resulting in a staggered rollout of direct elections across districts from 2005 to 2008. Mayors can serve a maximum of two five-year terms.

The 'Big Bang' decentralisation reforms of 2001 devolved significant expenditure authority to districts, so that Indonesia now ranks as one of the most decentralised countries in the developing world (Shah *et al.*, 2012). Districts provide public goods and services in the areas of education, health and local infrastructure. However, own-source revenue accounts for only 7% of total district revenue, so public expenditure is primarily financed by intergovernmental grants.[6] Online Appendix Table A.1 provides summary statistics on district revenue, expenditure and public goods and services.

Most local funding comes from an unconditional, non-matching transfer known as the General Allocation Fund (*Dana Alokasi Umum*), or 'general grant' for short. This grant accounts for over half of district revenue on average. A minority of districts receive significant shared natural resource revenue (*Dana Bagi Hasil Sumber Daya Alam*), which is tied to local extraction of natural resources. The most important grant of this type is the oil and gas grant. I discuss these two revenue sources in detail ahead. A small portion of expenditure is financed by conditional, matching transfers known as special allocation grants (*Dana Alokasi Khusus*), provided by the central government on a discretionary basis.

Districts are prohibited from introducing income taxes—individual or corporate—which are solely within the purview of the central government. However, districts receive a portion of the tax revenue collected within the district. Shared tax revenue accounts for around 7% of the district budget. From 2001 to 2010, the central government also exercised sole authority over the property tax. Between 2011 and 2014, the property tax was gradually decentralised to the districts, with most districts receiving this authority in 2014. This reform apparently had little impact in practice, at least over the study period: case studies suggest that districts were reluctant to deviate from pre-decentralisation tax rates (Von Haldenwang, 2017). Overall, local tax rates

[5] Indeed, Timor-Leste gained independence in 1999, and secessionist sentiment was strong in other peripheral regions of the country. Empowering the smaller districts made coordination more challenging for would-be secessionists. As Eckardt and Shah (2006, p. 235) note, 'Strengthening local governments would facilitate strengthening political and economic union while addressing long-felt local grievances'.

[6] Own-source revenue mostly consists of business licence fees, hotel and restaurant taxes and utility fees.

are not an important margin of adjustment to revenue shocks at the district level over the study period.

Following decentralisation, subnational borrowing has been minimal, for three reasons. First, the central government banned foreign borrowing by districts and must pre-approve domestic borrowing (Blöndal *et al.*, 2009). Second, many districts have poor credit ratings. Finally, district governments have had difficulty spending all of their transfer revenue in a timely fashion, leading to a buildup of reserves (World Bank, 2007, pp. 127–8). Current revenue and reserves typically suffice to finance capital projects and smooth current expenditure.

The number of districts has grown from 341 in 2001 to 514 in 2014, due to district splitting.[7] The central government imposed two moratoria on splitting during the analysis period, the first from 2004 to 2006 and the second from 2009 to 2012. As a consequence, no splits occurred in 2006, the year that the general grant and the oil and gas grant experienced their largest shocks, as discussed ahead. General grant revenue typically increases in per capita terms in both the original ('parent') district and the new ('child') district(s) after a split, due to the nature of the formula. The baseline regressions flexibly control for district splits, though the results are robust to omitting these controls.[8]

Indonesia ushered in a second era of decentralisation with the 2014 Village Law, which increased fiscal transfers to village governments and expanded their authority to provide public services, starting in 2015. I focus on the period 2001–14 to hold the federal structure constant.

To ensure that all districts in the sample operate under the same institutional environment, I omit provinces that have a special administrative or fiscal arrangement with the central government. The final sample contains 348 districts from 29 provinces. (See Online Appendix Section A.4 for details.)

1.2. *General Grant*

The general grant is intended to equalise district capacity to provide local public services.[9] Each year the central government sets the total budget for the grant and allocates funds according to a formula. Half of the grant pool funds the 'basic allocation', which covers the civil service wage bill. The basic allocation increases one-for-one with wage costs, but central regulations on recruitment and staffing prevent exorbitant spending on public employees that would otherwise occur due to the structure of the grant (Shah *et al.*, 2012). The remaining half of the grant pool is allocated according to the 'fiscal gap': the difference between expenditure needs and fiscal capacity. Expenditure needs are calculated as a weighted sum of indices related to population, land area, poverty and construction costs. Fiscal capacity is defined as a weighted sum of imputed own-source revenue, shared tax revenue and shared natural resource revenue. (See Online Appendix Section A.2 for details.) After paying civil servant wages, districts have complete discretion over how to spend the grant.

In 2006 the central government significantly increased the budget for the general grant. The grant budget depends on forecasts of the national government's long-term budget health, and a key parameter in these forecasts is the assumed future oil price. For years, the central government had

[7] See Fitrani *et al.* (2005), Burgess *et al.* (2012) and Bazzi and Gudgeon (2021) for details.

[8] An alternative approach would be to aggregate district outcomes to level of 2001 borders. I do not do this because the proper unit of analysis for my research question is a government, not a section of land.

[9] Equalisation grants have the potential to promote equity by targeting areas populated by households with low earning potential. In real-world contexts, such as in Canada, such grants often distort household location decisions and fall short of equity goals (Albouy, 2012).

deliberately underestimated the oil price to reduce its transfer obligations (Lewis and Oosterman, 2009). A rapidly falling debt-to-GDP ratio since 1999 created space for expanding transfers (World Bank, 2007, p. 10). In 2006 the general grant budget increased by 44% after the central government increased the oil price assumption from USD30 per barrel to USD60 per barrel (Agustina *et al.*, 2012). That same year the central government changed the allocation formula, reducing the weight assigned to population and increasing the weight assigned to land area. Both the increase in the budget and the change in the allocation formula were announced in October of 2004 (Law No. 33/2004).

The change in general grant revenue per capita dictated by the formula adjustment and budget increase was roughly linear in district land area per capita. (See Online Appendix Section A.2.) Districts rich in oil and gas resources should have experienced a decline in general grant funds at this time, due to a rise in oil and gas revenue. However, a hold-harmless provision froze the general grant allocation in place for these resource-abundant districts (World Bank, 2007, p. 121). Changes to the grant budget and formula in years other than 2006 were minor, so the reform-driven variation in general grant revenue per capita ($G_{d,t}$) is approximately

$$G_{d,t} \approx \theta_d + \pi A_d \cdot N_d \cdot 1(t \geq 2006),$$

where $\pi > 0$, A_d is land area per capita in district d in 2006, N_d is an indicator for not being located in a hydrocarbon-rich province, and $1(t \geq 2006)$ is an indicator for years 2006 and later.[10]

The parameter θ_d captures the (approximately) time-invariant grant amount in district d before 2006. This amount varies across districts due to differences in district characteristics that enter into the grant formula. Starting in 2006, the general grant is predicted to increase to $\theta_d + \pi A_d \cdot N_d$. Consequently, in hydrocarbon-poor provinces, the post-2006 increase is proportional to district land area per capita and remains constant over time.

Data on district land area and population come from the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). I collected data on intergovernmental grants from reports by the Ministry of Finance (*Kementerian Keuangan*). See Online Appendix Section A.4 for details on data sources and variable construction.

Panel (a) of Figure 1 shows that while less densely populated districts already received more general grant revenue per capita than more densely populated districts prior to the reform, the gap permanently widened in 2006 in hydrocarbon-poor provinces. This gap was roughly constant over 2006–14. By contrast, in hydrocarbon-rich provinces the gap was roughly constant over time, and there was no permanent increase in the general grant. The reform therefore created significant cross-district variation in the size of a permanent shock to the general grant within hydrocarbon-poor provinces.

The 2006 reform was intended to increase fiscal equalisation across regions. There is little indication that political considerations determined the nature of the reform. Conceivably, members of the national legislature representing less densely populated districts could have used the reform to help their own re-election prospects or the prospects of incumbents in the district legislatures. The timing of the reform is inconsistent with this story, however, as elections for both the national and district legislatures took place in 1999, 2004, 2009 and 2014.

Alternatively, members of the national legislature may have wanted to improve the re-election prospects of incumbent mayors in less densely populated districts. If this were the case, then

[10] The hydrocarbon-rich provinces are Kalimantan Timur, Riau, Kepulauan Riau, Sumatera Selatan and Jambi. (See Online Appendix Figure A.1.)

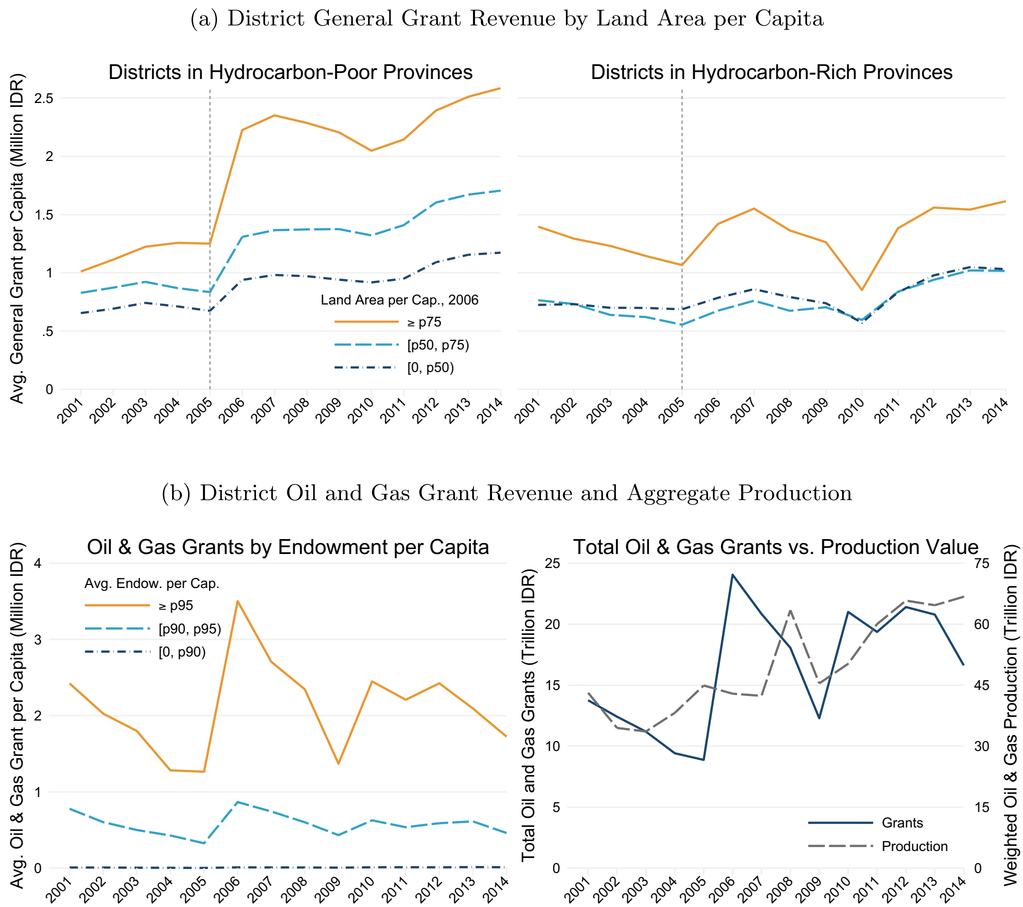


Fig. 1. *Permanent and Transitory Shocks to Grant Revenue. Panel (a) plots average general grant revenue per capita for districts located in hydrocarbon-poor provinces (left panel) and hydrocarbon-rich provinces (right panel) and divided according to land area per capita in 2006. Panel (b) plots average oil and gas grant revenue for districts divided according to average hydrocarbon endowment per capita (left panel) and total oil and gas grants and the weighted value of production (right panel), where the value of oil production is given a weight of 0.062 and the value of gas production is given a weight of 0.122. Grants are expressed in constant 2010 IDR 1 million, and oil and gas production is expressed in constant 2010 IDR 1 trillion. The hydrocarbon-rich provinces are Kalimantan Timur, Riau, Kepulauan Riau, Sumatera Selatan and Jambi. The vertical dashed line indicates the timing of the general grant reform.*

one would expect to see a disproportionate number of mayoral elections taking place in these districts in 2006. In reality, among resource-poor provinces, the average land area per capita of districts with mayoral elections in 2006 is statistically indistinguishable from the average land area per capita of districts with mayoral elections in 2005, 2007 or 2008.[11] This is unsurprising, as the timing of direct mayoral elections was largely determined by idiosyncratic historical factors

[11] Results available upon request.

(Martínez-Bravo *et al.*, 2017). Overall, there is little reason to believe that the timing or size of the general grant reform were motivated by political considerations.

1.3. *Oil and Gas Grant*

The central government shares revenue (i.e., royalties and taxes) that it collects from natural resource extraction within the district and province. Oil and natural gas are by far the largest sources of natural resource revenue in Indonesia. According to the sharing rule, 15.5% of oil revenue collected within a district is redistributed to subnational governments: 3.1% goes to the provincial government, 6.2% goes to the producing district, and the remaining 6.2% is evenly divided among the other districts located in the same province. The sharing rule for natural gas is more generous: 6.1% goes to the provincial government, 12.2% goes to the producing district and another 12.2% is divided equally among the other districts in the province. Despite the less generous sharing rule, shared oil revenue exceeds shared gas revenue on average due to the higher value of oil production. Districts have complete discretion over how to spend the oil and gas grant.[12]

The oil and gas grant is derived from current, realised oil and gas revenue collected by the central government. In principle, it should fluctuate in tandem with the current value of district oil and gas production. The central government transfers this revenue to districts on a quarterly basis, using estimated profits for the current quarter and an adjustment for profit forecast errors from the previous quarter. In practice, however, these payments are sometimes delayed due to various factors. Late reporting of profit forecasts by the Ministry of Energy and Mineral Resources contributes to these delays (Agustina *et al.*, 2012). Additionally, cash flow problems at Pertamina, the state-owned oil and gas company, may have exacerbated the problem (World Bank, 2007, p. 15).

Using the proprietary Rystad UCube database (Rystad Energy, 2016), I calculate the total economically recoverable oil and gas resources in each district as of 2000 (and known in 2000)—prior to fiscal decentralisation. I then convert physical endowments into monetary values using the average prices of oil and gas over 2001–14, insert these variables into the revenue-sharing formula in place of actual oil and gas revenue, and divide by district population. The resulting variable, denoted by $E_{d,t}$, represents the pre-determined oil and gas endowment to which district d has a claim for revenue-sharing purposes in year t, in constant 2010 IDR (billions) per capita. This variable can change over time due to changes in district population, district borders or province borders. To ensure that the instrument is not influenced by population changes or the splitting of districts or provinces, I use the average endowment per capita over 2001–14, denoted by E_d. Online Appendix Section A.3 provides more details on the sharing rule and the endowment variable.

Panel (b) of Figure 1 illustrates that districts in the top 5% of hydrocarbon endowment per capita received large oil and gas grants with sharp year-to-year fluctuations, particularly during 2005–9. Districts between the 90th and 95th percentiles of endowment per capita received significantly smaller grants, while those in the bottom 90% received virtually none.

[12] Technically, 0.5% of the oil and gas revenue collected by the central government is distributed to subnational governments as a earmarked grant for elementary education (Law No. 33/2004). The earmarked portion accounts for around 3% of the district's oil grant, and 2% of the district's gas grant. This earmarking is unlikely to influence district spending decisions, as earmarked funds are extremely small relative to total education spending, which represents over one-third of the district budget on average.

The figure also compares total oil and gas grant revenue against the weighted value of oil and gas production, where oil production value is weighted at 0.062 and gas production value at 0.122.[13] This weighted production value should be roughly proportional to the central government's transfer obligations as dictated by the sharing rule. However, the two time series do not track each other—not even with a lag (Online Appendix Table A.2). This lack of synchronisation could be attributed to payment delays of varying duration.

The variation in the oil and gas grant driven by endowments and central government policies is captured by $E_d \cdot \tilde{H}_{(-d),t}$, where $\tilde{H}_{(-d),t}$ represents aggregate oil and gas grants excluding own-district grant revenue. Due to the central government's deviations from its own disbursement rule, the uncertainty of future revenue shocks could stem from both volatile resource prices and payment delays of uncertain duration. The results should be interpreted in light of this fact. It is important to note, however, that district-specific discretionary policy—such as prioritising payments to certain districts—will not bias the estimates, as the instrument uses aggregate grants excluding own grants.

1.4. *Geographic Variation in Exposure to Grant Shocks*

Figure 2 displays the spatial variation in district exposure to shocks to the two grants. Every region except for Java contains districts with high exposure to the general grant reform—that is, low population density. Furthermore, there is rich within-region variation in land area per capita in all regions except for Java. Oil and gas endowments are fairly geographically concentrated, with five provinces containing the bulk of the deposits and around one third of districts having an endowment of zero. Still, there is significant cross-district variation in endowments within most regions and within hydrocarbon-rich provinces.

1.5. *Magnitude and Persistence of Grant Shocks*

Both the general grant and oil and gas grant are unconditional, non-matching and subject to the same level of central-government oversight. Hence, they differ only in their time-series variation, which has two components: (i) the initial magnitude of shocks and (ii) the persistence of shocks. In the subsample of districts with high exposure to one of the two grants, the initial magnitude of the shocks is similar on average for the two grants. (See Online Appendix Section A.5 for details.) By contrast, the shock persistence is much higher for the general grant. In a dynamic panel model, the autoregressive coefficients for the general grant nearly sum to one, implying almost 'perfect' persistence. The estimates for the oil and gas grant are less precise, but the totality of the evidence suggests the oil and gas grant is significantly less persistent than the general grant. (See Online Appendix Section A.6 for details.)

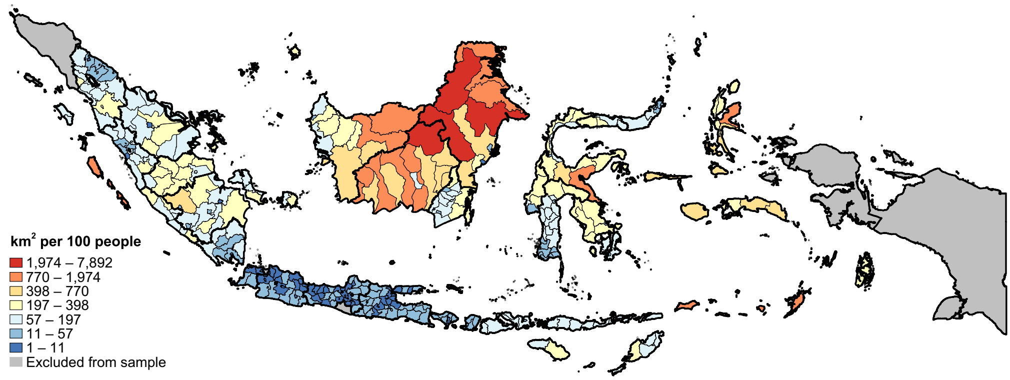
2. Conceptual Framework

This section outlines a model of local government behaviour and proposes three hypotheses that will be tested in Sections 3 and 4. (See Online Appendix Section A.1 for a fully developed model.)

Assume the local government provides a non-durable good and a lumpy durable good. Fiscal transfers serve as the sole source of public revenue. The government chooses the path of the two

[13] Data on oil and gas production also come from Rystad Energy (2016).

(a) Land Area per Capita in 2006



(b) Average Hydrocarbon Endowment per Capita

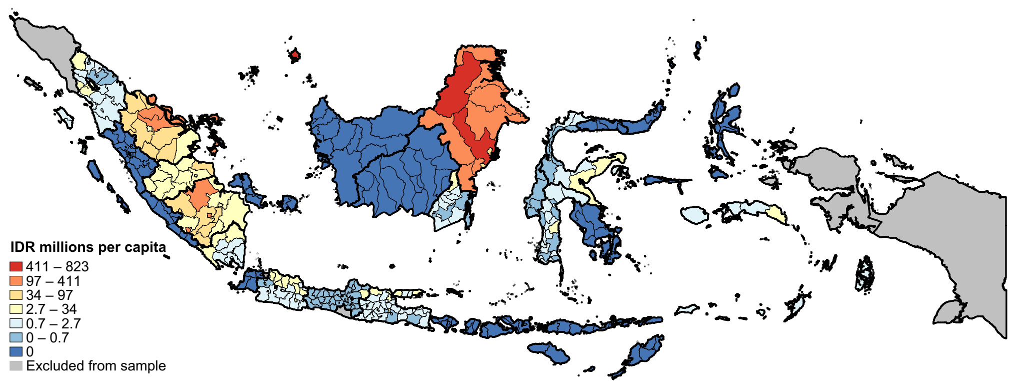


Fig. 2. *District Exposure to Grant Revenue Shocks. District borders (thin lines) and province borders (thick lines) are displayed as they existed in 2006. Average hydrocarbon endowment per capita is calculated according to (A.7) in the Online Appendix. Bins are shaded based on the 25th, 50th, 75th, 90th, 95th and 99th percentiles.*

goods to maximise the sum of citizens' discounted lifetime utility, subject to the government's intertemporal budget constraint. Under Cobb–Douglas utility, the optimal provision of each good is proportional to the government's stock of lifetime resources, which includes the present discounted value of the stream of transfer revenue. Three hypotheses emerge from the model.

HYPOTHESIS 1. *The spending response to a revenue shock is increasing in the persistence of the shock.*

Holding the initial size of the revenue shock fixed, a more persistent shock has a larger impact on lifetime resources and, therefore, stimulates a greater spending response.

HYPOTHESIS 2. *If transfers are perfectly persistent, then spending 'overshoots', initially increasing more than one-for-one with current transfers.*

When the durable good increases, the fiscal response will be front-loaded due to the upfront investment required to increase the stock of durables.

HYPOTHESIS 3. *Durable good provision increases only in response to large increases in lifetime resources.*

The front-loaded spending response described in Hypothesis 2 only occurs if the revenue shock induces a sufficiently large increase in lifetime resources. This is because investment is lumpy: the government incurs a fixed cost whenever it makes a large adjustment to the stock of durables. (Small adjustments, such as routine maintenance, do not incur the fixed cost.)

Both the size of the initial shock and its persistence matter for the composition of the spending response, as both affect the change in lifetime resources. As discussed in the previous section, among Indonesian districts that are highly exposed to shocks to either the general grant or the oil and gas grant, the size of the initial shock is similar for both grants. Therefore, shocks to the two grants have different impacts on behaviour primarily because of differences in persistence.

An important omission from the model is bureaucratic delay. District governments in Indonesia sometimes receive transfers late in the year, face delays in the process of getting budgets approved by the province, and have difficulty procuring goods and services in a timely manner. Fiscal responses thus may occur with a lag. The empirical tests discussed ahead allow for lagged responses.

Another important consideration is corruption. Local officials may appropriate a portion of the fiscal transfers for private consumption, driving a wedge between reported spending and actual public good provision. In the presence of corruption, the qualitative predictions of the model still hold, as long as the share of resources appropriated by government officials does not vary markedly with the persistence of transfers.[14] In Section 4.5, I show that the level of corruption is similar in districts with high exposure to the permanent grant shock and districts with high exposure to the transitory grant shocks.

A final consideration is asymmetric responses. Public good provision may respond differently to increases and decreases in transfers, possibly because reducing the stock of durables is more costly than increasing the stock. This could matter empirically, because the oil and gas grant experienced both increases and decreases, whereas the general grant only experienced an increase. I allow for asymmetric responses in a robustness check ahead.

3. Fiscal Responses

3.1. *Empirical Strategy*

I begin the empirical analysis by estimating the dynamic fiscal responses to the general grant and the oil and gas grant, with the goal of testing the theoretical predictions. Data on district revenue and expenditure come from the Ministry of Finance and INDO-DAPOER. All fiscal variables are expressed in constant 2010 IDR 1 million (approximately USD100) per capita.

[14] For example, if the local government's felicity function is $\lambda(\gamma \log C_t + (1-\gamma)\log D_t) + (1-\lambda)\log S_t$, where C_t is the non-durable good, D_t is the durable good and S_t is rents, then public good provision is a share λ of the provision under no corruption, and similar comparative statics obtain.

I estimate the direct projections (Jordà, 2005)

$$\begin{aligned} Y_{d,t+h} - Y_{d,t-k} &= \beta_h (G_{d,t} - G_{d,t-k}) + \delta_h (H_{d,t} - H_{d,t-k}) \\ &\quad + \boldsymbol{\phi}_h' (\boldsymbol{X}_{d,t} - \boldsymbol{X}_{d,t-k}) + \lambda_{r(d),t,h} + \varepsilon_{d,t,h}, \end{aligned} \tag{1}$$

where $Y_{d,t}$ is total expenditure in district d and year t, $G_{d,t}$ is general grant revenue, and $H_{d,t}$ is oil and gas grant revenue (H stands for 'hydrocarbon'). The covariates $\boldsymbol{X}_{d,t}$ are indicators for whether the district has split, interacted with indicators for whether the district is a parent or a child district, as well as three lags of these variables.[15] The model also controls for district fixed effects (via differencing) and horizon-specific region-by-year effects, $\lambda_{r(d),t,h}$.[16]

The index $k \in \{1, 2\}$ represents the duration of the revenue shock considered, and h represents the time horizon of the expenditure response. The horizon-specific coefficients β_h and δ_h represent the per-dollar effect of a k-year change in the general grant and the oil and gas grant, respectively, on expenditure h years later.

Both grants could be endogenous in (1). The general grant is likely endogenous because it is a function of the civil service wage bill and fiscal need. An adverse shock that increases fiscal need would lead to an increase in the general grant while also potentially affecting local demand for public services or local capacity to provide those services. The oil and gas grant could also be endogenous if it responds to the local business environment, local economic shocks, conflict or other factors that affect district expenditure and public services. Furthermore, grant amounts could, in theory, deviate from the allocations prescribed by law due to political manipulation. Such deviations could reflect the relative bargaining power of the district, introducing another source of endogeneity.

In light of these concerns, I estimate β_h and δ_h using instrumental variables (IV) that capture the exogenous variation in grants described in Sections 1.2 and 1.3. In levels, the 2×1 vector of excluded instruments is

$$\boldsymbol{Z}_{d,t} \equiv \big(A_d \cdot N_d \cdot 1(t \geq 2006), \quad E_d \cdot \tilde{H}_{(-d),t}\big),$$

where A_d is land area per capita in 2006, N_d is an indicator for not being located in a hydrocarbon-rich province, E_d is average hydrocarbon endowment per capita over 2001–14 and $\tilde{H}_{(-d),t}$ is aggregate oil and gas grants excluding own-district grants. I then take the k-year difference of the instruments, $\boldsymbol{Z}_{d,t} - \boldsymbol{Z}_{d,t-k}$, to mirror the grant variables in (1).

The instrument for the general grant, $A_d \cdot N_d \cdot 1(t \geq 2006)$, is relevant because the permanent increase in the general grant dictated by the 2006 reform was proportional to land area per capita among districts in hydrocarbon-poor provinces. Intuitively, the IV estimator compares the change in the general grant revenue gradient in land area per capita to the change in the corresponding spending gradient for districts in hydrocarbon-poor provinces. The key identifying assumption is that the spending gradient in land area per capita would not have changed in the absence of the 2006 reform. This assumption allows the level of spending to be correlated with land area per capita, but it rules out any correlation between land area per capita and *changes* in spending due to factors other than the 2006 reform. Put another way, it states that outcomes in districts

[15] The split year is the first full calendar year following the passage of the legislation creating the new district(s). The construction of the covariates is motivated by the patterns observed in the data: general grant revenue steadily increases in the three years after a split (relative to non-splitting districts) then levels off, with child districts seeing larger increases (Cassidy and Velayudhan, 2024).

[16] Following the Central Bureau of Statistics, I code seven regions: Sumatra, Java, Nusa Tenggara, Kalimantan, Sulawesi, Maluku and Papua.

with different population densities would have followed parallel paths over time in the absence of the reform. While this assumption is not testable, it would be more plausible if the spending gradient in land area per capita were constant over time prior to the reform, and if there were no confounding policy changes that were systematically related to the 2006 reform. I test for a constant pre-reform gradient and examine confounding policies ahead.

The instrument for the oil and gas grant, $E_d \cdot \tilde{H}_{(-d),t}$, captures variation due to pre-determined hydrocarbon endowment per capita and national revenue-sharing policy. The key identifying assumption is that outcomes in districts with different average per capita endowments would have followed parallel trends in the absence of shocks to the oil and gas grant. This rules out omitted factors that vary over time and differentially affect districts with different endowment levels. One concern is that districts with better political institutions and leadership may attract more oil and gas exploration, increasing known endowment (Arezki *et al.*, 2019; Cassidy, 2019; Cust and Harding, 2019). The instrument avoids contamination along these lines by measuring endowment known as of 2000, prior to fiscal decentralisation. Before 2001, the central government was the sole actor negotiating with oil and gas companies, so incentives to explore were roughly uniform across the country.[17] It is therefore plausible that pre-determined endowment is uncorrelated with the unobserved quality of governance.

A second concern is that district-level oil and gas production may be correlated with the instrument, leading to estimates that conflate the effects of production and shared revenue. However, as already discussed, aggregate oil and gas grant revenue does not fluctuate in tandem with aggregate oil and gas production—or its lags—apparently because of payment delays of varying length (Figure 1(b) and Online Appendix Table A.2).

The identifying assumptions do *not* imply that we should expect districts to exhibit constant spending gradients in average endowment per capita over *any* period after decentralisation. The reason is that shocks to the oil and gas grant occurred in every year starting in 2001; there is no 'pre-shock' period under decentralisation.

3.2. *Reduced-Form Effects over Time*

I first present graphical evidence by plotting the reduced-form impacts of exposure to grant shocks over time. To do so, I estimate the regression

$$Y_{d,t} = \sum_{s \neq 2005} \theta_s A_d \cdot N_d \cdot D_t^s + \sum_{s \neq 2005} \gamma_s E_d \cdot D_t^s + \boldsymbol{\pi}' \boldsymbol{X}_{d,t} + \alpha_d + \lambda_{r(d),t} + u_{d,t}, \tag{2}$$

where D_t^s is an indicator that equals one if $t = s$, α_d is a district fixed effect and A_d, N_d, E_d and $\boldsymbol{X}_{d,t}$ are as defined in the previous section. The coefficient θ_s captures the change in the spending gradient in exposure to the general grant reform between 2005 and year s. Similarly, γ_s captures the change in the spending gradient in exposure to the oil and gas grant between 2005 and year s. I also estimate (2) with the grants as outcomes to visualise the time-varying effects of exposure on grant revenue.[18]

[17] Separatist violence in Aceh and Papua has disrupted resource extraction in the past, but these regions are excluded from the sample due to their special fiscal arrangements with the central government.

[18] Equation (2) could instead be expressed as a system of long-difference equations,

$$Y_{d,t} - Y_{d,2005} = \theta_t A_d \cdot N_d + \gamma_t E_d + \boldsymbol{\pi}'(\boldsymbol{X}_{d,t} - \boldsymbol{X}_{d,2005}) + (\lambda_{r(d),t} - \lambda_{r(d),2005}) + (u_{d,t} - u_{d,2005}),$$

for $t \neq 2005$, highlighting the interpretation of θ_t and γ_t as changes in gradients from 2005 to t. Expressing the equation in levels allows me to jointly estimate (θ_t, γ_t) for all $t \neq 2005$ in a single step.

Throughout the paper I report standard errors that are robust to heteroscedasticity and two-way clustering at the district and province-by-year levels to account for within-district serial correlation and cross-district correlation within provinces in a given year (Cameron *et al.*, 2011). The within-district correlation is due to the persistence of fiscal variables and unobservables over time. The cross-district correlation could arise from the fact that, in any given year, non-producing districts located in the same province are entitled to the same amount of oil and gas grant revenue.

Figure 3 displays point estimates and 95% confidence intervals for the parameters in (2). Figure 3(a) plots the estimates of $\{\theta_s\}$ separately for total expenditure (circles) and general grant revenue (diamonds). The estimates confirm that districts with greater land area per capita experienced larger permanent increases in general grant revenue starting in 2006. These districts responded by sharply increasing expenditure in 2006. This expenditure response grew over the next three years before partially subsiding in 2010. The estimates for $s < 2005$ are close to zero and statistically insignificant, implying that the spending gradient in exposure to the general grant reform was constant prior to the reform. This suggests that the reform did not target districts based on pre-existing fiscal trends, and that there were no anticipatory effects. Thus, the estimates support the plausibility of the identifying assumption for the general grant.

Figure 3(b) plots the estimates of $\{\gamma_s\}$ separately for total expenditure (circles) and oil and gas grant revenue (diamonds). Hydrocarbon-rich districts experienced sharp, transitory changes in the oil and gas grant, especially over 2005–9. The figure suggests that expenditure responds somewhat to these shocks, though the response appears to be less than one-for-one and is spread out over several years. Overall, expenditure in hydrocarbon-rich districts evolves more smoothly over time than the oil and gas grant.

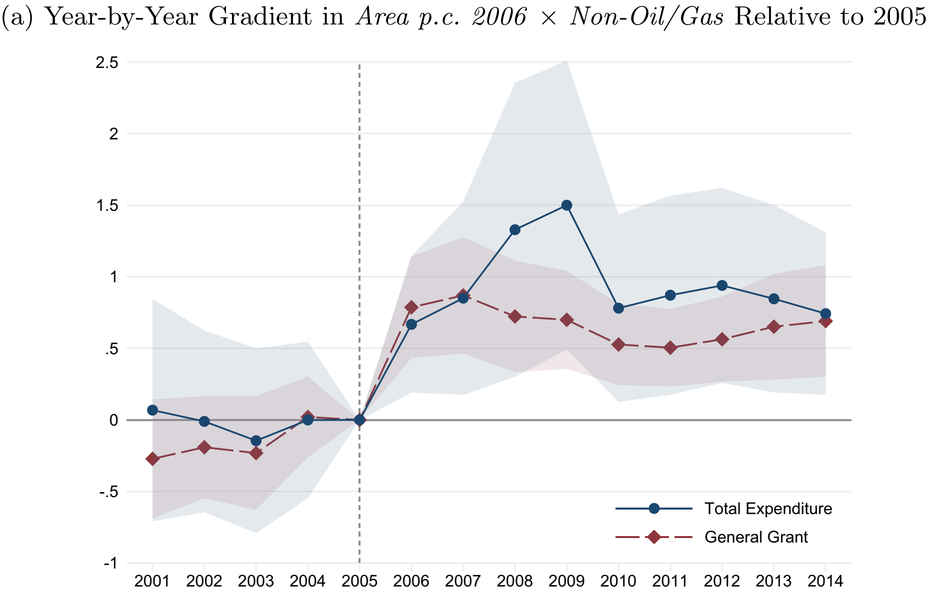
3.3. *Main Results*

Table 1 presents the first-stage results. Panel A reports estimates based on one-year changes ($k = 1$). The first instrument, $A_d \cdot N_d \cdot 1(t \geq 2006)$, has a positive and highly significant effect on general grant revenue per capita (p.c.), with a point estimate of 0.77 and a 0.08 SE. The magnitude and statistical significance of this estimate are similar when the second instrument, $E_d \cdot \tilde{H}_{(-d),t}$, is included. The second instrument has a positive and highly significant effect on oil and gas grant revenue per capita, with a point estimate of 0.59 and a 0.04 SE. Similarly, this first-stage effect is insensitive to the inclusion of the first instrument. Using two-year changes produces similar estimates (Panel B).

Table 2 reports the IV estimates of β_h and δ_h from (1) for different horizons h.[19] I focus on the results for one-year changes in grants (Panel A), as the results for two-year changes are qualitatively similar (Panel B). The point estimate of 0.69 (SE = 0.11) in the first row and first column indicates that an increase in the general grant by 1 rupiah per capita immediately raises total expenditure by 0.69 rupiah per capita. Columns 2–6 show that the expenditure response to the general grant steadily grows for three years, peaking at 1.63 (SE = 0.27), before declining to 0.83 (SE = 0.20) five years after the shock.

Total expenditure is less responsive to the oil and gas grant, initially increasing by 0.23 (SE = 0.07) and peaking at 0.55 (SE = 0.15) two years later. The response falls slightly to 0.49 (SE = 0.06) in year three before dropping sharply to 0.16 (SE = 0.07) the next year. An increase in the oil and gas grant initially leads to a sharp increase in the budget surplus

[19] The estimates that do not control for $\boldsymbol{X}$ are similar and are reported in Online Appendix Table A.4. Online Appendix Table A.5 reports the ordinary least squares estimates for comparison.



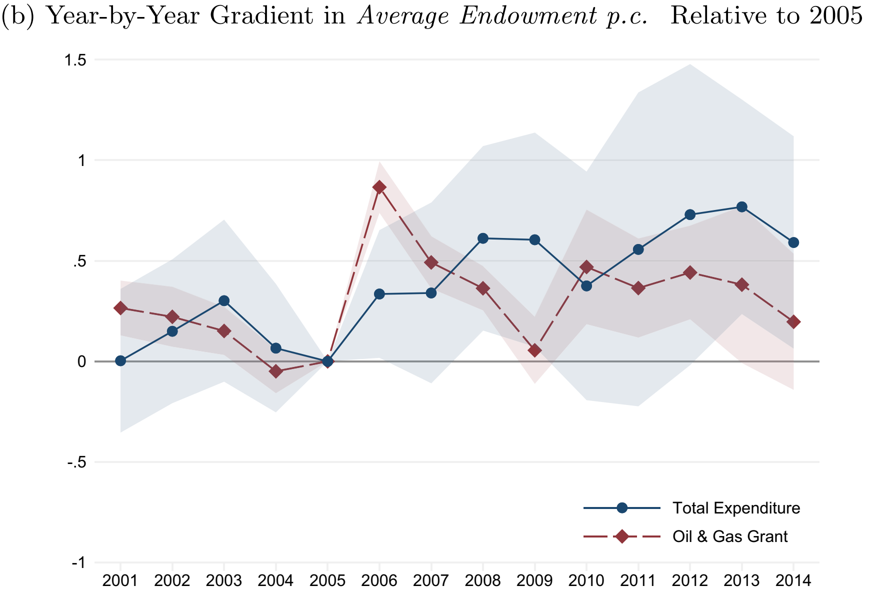


Fig. 3. *Reduced-Form Effects of Grant Exposure on Fiscal Variables over Time. This figure displays point estimates and 95% confidence intervals for parameters from (2). The circles are estimates of* $\{\theta_s\}_{s\in\mathcal{S}}$ *in panel (a) and* $\{\gamma_s\}_{s\in\mathcal{S}}$ *in panel (b) when the outcome is total expenditure per capita. The diamonds in panel (a) are estimates of* $\{\theta_s\}_{s\in\mathcal{S}}$ *when the outcome is general grant revenue per capita, and the diamonds in panel (b) are estimates of* $\{\gamma_s\}_{s\in\mathcal{S}}$ *when the outcome is oil and gas grant revenue per capita. Average hydrocarbon endowment per capita is measured in constant 2010 IDR 100 millions to make the vertical axes of the two graphs similar. Confidence intervals are robust to heteroscedasticity and two-way clustering by district and province-by-year.*

Table 1. *First-Stage Estimates.*

	General grant p.c.		Oil and gas grant p.c.	
	(1)	(2)	(3)	(4)
	Panel A: One-year changes ($k = 1$)			
Area p.c. 2006	0.77***	0.73***		−0.01
× Non-oil/gas × year ≥ 2006	(0.08)	(0.09)		(0.03)
Avg. endowment p.c.		−0.03	0.59***	0.59***
× Agg. oil and gas grant excl. own		(0.03)	(0.04)	(0.04)
Observations	4,290	4,290	4,290	4,290
District clusters	348	348	348	348
Prov. × year clusters	358	358	358	358
	Panel B: Two-year changes ($k = 2$)			
Area p.c. 2006	0.80***	0.85***		0.04
× Non-oil/gas × year ≥ 2006	(0.13)	(0.13)		(0.04)
Avg. endowment p.c.		0.05	0.57***	0.57***
× Agg. oil and gas grant excl. own		(0.05)	(0.04)	(0.04)
Observations	3,957	3,957	3,957	3,957
District clusters	348	348	348	348
Prov. × year clusters	332	332	332	332

Notes: This table presents first-stage estimates based on one-year differences (Panel A) and two-year differences (Panel B) of the variables. To improve readability, land area per capita is measured in tens of square kilometres per capita, and aggregate oil and gas grants are measured in 2010 IDR trillions. Each regression controls for region-by-year effects and indicators for whether the district has split, defined separately for parent and child districts, as well as three lags of these indicators. Standard errors, reported in parentheses, are robust to heteroscedasticity and two-way clustering by district and province-by-year. $^{***}p < 0.01$.

(Online Appendix Table A.6). However, districts reduce the surplus three years after a revenue increase, likely due to the significant reduction in this grant over 2007–9 following the sharp increase in 2006 (Figure 1(b)). Both results are consistent with expenditure smoothing.

The Sanderson and Windmeijer (2016) F-statistic, which tests for weak identification of individual coefficients on the endogenous variables, ranges from 71 to 94 for the general grant, and 77 to 177 for the oil and gas grant, indicating that the structural parameters are strongly identified.

Each column in Table 2 reports p-values from testing two hypotheses. The first null hypothesis, $H_0: \beta_h = \delta_h$, is motivated by Hypothesis 1, which states that the spending response to a revenue shock is increasing in the persistence of the shock ($\beta_h > \delta_h$). The second null hypothesis, $H_0: \beta_h \leq 1$, is motivated by Hypothesis 2, which states that persistent revenue shocks will produce a greater than one-for-one spending response ($\beta_h > 1$) if the shock is sufficiently persistent. Because $h \in \{0, 1, \ldots, 5\}$, each hypothesis is actually a family of six hypotheses. The more hypotheses one tests, the greater the probability of rejecting at least one true null hypothesis in the family, known as the family-wise error rate (FWER). To address this concern, the table reports adjusted p-values based on the Holm step-down method (Holm, 1979), which fixes the FWER rather than merely fixing the significance level of each individual hypothesis test. The Holm method is conservative and allows for arbitrary dependence between hypothesis tests.[20] For comparison, the table also reports conventional (unadjusted) p-values.

[20] For a family of M hypotheses with unadjusted p-values ordered so that $p_1 \geq p_2 \geq \ldots \geq p_M$, the Holm step-down procedure begins by adjusting the smallest p-value as $p_M^H = \min\{1, Mp_M\}$. Each ensuing p-value is adjusted as $p_j^H = \min\{1, jp_j\}$ if $\min\{1, jp_j\}$ is greater than or equal to all previously adjusted p-values, $p_{j+1}^H, \ldots, p_M^H$. Otherwise, it is set to the maximum of the previously adjusted p-values.

Table 2. *Dynamic Responses of Total Expenditure to Grants.*

	Response of total expenditure per capita after h years					
	$h = 0$ (1)	$h = 1$ (2)	$h = 2$ (3)	$h = 3$ (4)	$h = 4$ (5)	$h = 5$ (6)
	Panel A: One-year changes in grants ($k = 1$)					
General grant p.c.	0.69***	0.86***	1.45***	1.63***	0.75***	0.83***
	(0.11)	(0.21)	(0.34)	(0.27)	(0.18)	(0.20)
Oil and gas grant p.c.	0.23***	0.30***	0.55***	0.49***	0.16**	0.28
	(0.07)	(0.10)	(0.15)	(0.06)	(0.07)	(0.19)
H_0: gen. = oil and gas						
Unadjusted p-value	0.000	0.001	0.005	0.000	0.000	0.000
Adjusted p-value	*0.000*	*0.003*	*0.005*	*0.000*	*0.001*	*0.000*
H_0: gen. grant ≤ 1						
Unadjusted p-value	0.997	0.744	0.096	0.009	0.913	0.801
Adjusted p-value	*1.000*	*1.000*	*0.482*	*0.056*	*1.000*	*1.000*
SW F-stat.: gen. grant	71.0	73.0	78.8	87.9	77.1	93.6
SW F-stat.: oil and gas	89.1	77.3	89.9	93.1	107.2	177.1
Observations	4,290	3,957	3,612	3,272	2,924	2,579
District clusters	348	348	348	348	348	348
Prov. × year clusters	358	330	302	274	246	218
	Panel B: Two-year changes in grants ($k = 2$)					
General grant p.c.	0.70***	1.03***	1.46***	1.01***	0.63***	0.74***
	(0.14)	(0.25)	(0.25)	(0.21)	(0.14)	(0.18)
Oil and gas grant p.c.	0.12*	0.31***	0.34***	0.15**	0.10	0.30
	(0.07)	(0.11)	(0.04)	(0.07)	(0.16)	(0.21)
H_0: gen. = oil and gas						
Unadjusted p-value	0.000	0.011	0.000	0.000	0.000	0.015
Adjusted p-value	*0.000*	*0.022*	*0.000*	*0.001*	*0.000*	*0.022*
H_0: gen. grant ≤ 1						
Unadjusted p-value	0.986	0.456	0.035	0.487	0.996	0.923
Adjusted p-value	*1.000*	*1.000*	*0.209*	*1.000*	*1.000*	*1.000*
SW F-stat.: gen. grant	41.7	42.6	43.2	43.8	44.1	44.3
SW F-stat.: oil and gas	416.5	379.7	371.5	386.1	365.2	372.2
Observations	3,957	3,612	3,272	2,924	2,579	2,237
District clusters	348	348	348	348	348	348
Prov. × year clusters	332	304	276	248	220	192

Notes: This table reports IV estimates of β_h and δ_h in (1). Panel A presents estimates based on one-year changes in grants and Panel B presents estimates based on two-year changes in grants. Each regression controls for region-by-year effects and indicators for whether the district has split, defined separately for parent and child districts, as well as three lags of these indicators. Sanderson and Windmeijer (2016) first-stage F-statistics are reported for each endogenous variable. Adjusted p-values use the Holm correction for multiple hypothesis testing. Standard errors, reported in parentheses, are robust to heteroscedasticity and two-way clustering by district and province-by-year. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

There is strong evidence against $H_0: \beta_h = \delta_h$, which is rejected at the 1% level for all horizons in the specification with one-year changes in grants (Table 2, Panel A). For the specification with two-year changes in grants (Table 2, Panel B), the hypothesis is rejected at either the 1% or 5% level, depending on the horizon. The general grant clearly induced a larger expenditure response than the oil and gas grant. The second hypothesis, $H_0: \beta_h \leq 1$, is rejected at horizon $h = 3$ at the 1% level using unadjusted p-values ($p = 0.009$) and at the 10% level using the Holm method ($p = 0.056$). This represents evidence of an overshooting expenditure response to the general grant. The patterns are similar in the specification with two-year changes in grants (Table 2, Panel B), but the hypothesis can only be rejected at the 5% level using unadjusted p-values ($p = 0.035$) (at horizon $h = 2$). Overall, the fiscal results are consistent with Hypotheses 1 and 2.

Table 3. *Mean Responses of Expenditure Categories to Grants.*

	Mean responses: $\frac{1}{6}\sum_{h=0}^{5}\beta_h$ and $\frac{1}{6}\sum_{h=0}^{5}\delta_h$				
	(1)	(2)	(3)	(4)	(5)
	Panel A: Expenditure by economic classification				
	Total	Personnel	Capital	Goods and services	Other
General grant p.c.	1.04***	0.18**	0.54***	0.18***	0.09***
	(0.14)	(0.09)	(0.08)	(0.07)	(0.03)
Oil and gas grant p.c.	0.31***	0.02	0.15***	0.06***	0.02
	(0.05)	(0.02)	(0.03)	(0.01)	(0.02)
Baseline budget share	1.000	0.584	0.164	0.177	0.075
H_0: gen. = oil and gas					
Unadjusted p-value	0.000	0.091	0.000	0.070	0.003
Adjusted p-value	*0.000*	*0.141*	*0.000*	*0.141*	*0.009*
SW F-stat.: gen. grant	93.9	93.1	83.6	74.0	74.3
SW F-stat.: oil and gas	184.4	164.4	235.8	249.2	260.1
Observations	2,595	2,484	2,580	2,444	2,422
District clusters	348	348	348	348	348
Prov. × year clusters	218	218	218	218	218
	Panel B: Expenditure by function				
	Education	Administration	Infrastructure	Health	Agriculture
General grant p.c.	0.22***	0.39***	0.47***	0.08**	0.11***
	(0.06)	(0.08)	(0.13)	(0.03)	(0.02)
Oil and gas grant p.c.	0.06***	0.19**	0.16***	0.03**	0.01
	(0.02)	(0.10)	(0.02)	(0.01)	(0.01)
Baseline budget share	0.379	0.309	0.113	0.066	0.029
H_0: gen. = oil and gas					
Unadjusted p-value	0.019	0.035	0.016	0.114	0.000
Adjusted p-value	*0.062*	*0.070*	*0.062*	*0.114*	*0.000*
SW F-stat.: gen. grant	69.0	69.0	68.2	69.0	68.5
SW F-stat.: oil and gas	244.3	244.4	246.3	244.3	244.2
Observations	1,776	1,776	1,772	1,776	1,767
District clusters	347	347	346	347	347
Prov. × year clusters	162	162	162	162	162

Notes: This table reports IV estimates of the mean responses of different categories of expenditure (per capita) to the general grant, $\sum_{h=0}^{5}\beta_h/6$, and to the oil and gas grant, $\sum_{h=0}^{5}\delta_h/6$, obtained by replacing the outcome in (1) with $\sum_{h=0}^{5}(Y_{d,t+h} - Y_{d,t-k})/6$. All estimates estimates are based on one-year changes in grants. Baseline budget shares are measured in 2001. Each regression controls for region-by-year effects and indicators for whether the district has split, defined separately for parent and child districts, as well as three lags of these indicators. Sanderson and Windmeijer (2016) first-stage F-statistics are reported for each endogenous variable. Standard errors, reported in parentheses, are robust to heteroscedasticity and two-way clustering by district and province-by-year. ** $p < 0.05$, *** $p < 0.01$.

3.4. *Composition of Expenditure Responses*

To better understand the role of lumpy expenditure, I next examine the composition of expenditure responses. Table 3 presents the mean responses of different categories of expenditure over horizons 0 through 5. (Online Appendix Figure A.3 plots the dynamic responses.) Panel A breaks down expenditure by economic classification in order of budget share: total, personnel, capital, goods and services, and 'other'.[21] The average difference in the responses of total expenditure to the two grants is 0.73 rupiah per capita (1.04 for the general grant versus 0.31 for the oil and gas grant). Capital expenditure accounts for over half of this difference, with a gap of 0.39 rupiah per capita (0.54 versus 0.15)—despite representing only 16% of district budgets

[21] The 'other' category includes unplanned spending, interest payments and discretionary financial assistance and donations (Sjahrir *et al.*, 2013).

at baseline. The next largest gap (0.16) is found in personnel spending, which could involve significant long-term commitments due to the difficulty of firing public employees.[22] The gap is smallest for goods and services (0.12) and 'other' expenditure (0.07), which likely contain less lumpy and more discretionary items. The evidence is thus consistent with lumpy investment and committed expenditure on personnel driving the different responses to the two grants.

Table 3, Panel B, summarises the responses for the five largest functional categories of expenditure ranked by budget share: education, administration, infrastructure, health and agriculture.[23] (Note that these categories are not exhaustive.) Infrastructure spending exhibits the biggest difference, with a gap of 0.31 rupiah per capita (0.47 versus 0.16). This is consistent with the result for capital expenditure and further underscores the importance of lumpy investment.

3.5. *Robustness Checks*

3.5.1. *Pre-trends and confounders*

As already mentioned, the key identifying assumption is that the relationship between expenditure and exposure to the grant shocks, as determined by land area per capita and average hydrocarbon endowment per capita, would have been constant over time in the absence of shocks to the grants. While the assumption is not testable, one implication is that districts with varying exposure to the grant shocks would have experienced similar spending trends over periods when no grant shocks occurred. This implication is not testable for the oil and gas grant, which experienced shocks in every period. However, it is testable for the general grant, which maintained a roughly time-invariant relationship with land area per capita over 2001–5. As already discussed, the spending gradient in land area per capita was constant over time prior to 2006 (Figure 3), which is consistent with the identifying assumption.

The identifying assumption could also be violated if other policy or economic shocks coincided with the grant shocks and differed in their intensity according to district exposure to the grant shocks. For example, the estimated response to the oil and gas grant would be biased if changes in oil and gas production both correlated with changes in the grant and influenced expenditure. However, as already discussed, this is unlikely to be an important source of bias, as there is no clear relationship between changes in hydrocarbon production and changes in the oil and gas grant, even allowing for lagged effects (Figure 1(b) and Online Appendix Table A.2).[24] Furthermore, the estimates hardly change when I control for district-level oil and gas production per capita (Online Appendix Table A.7).

Alternatively, the estimates could be biased if grant shocks were correlated with changes in other sources of revenue. To conserve space, Online Appendix Table A.8 presents estimates of the mean responses of alternative revenue sources over horizons 0 through 5. An additional 1 rupiah per capita of general grant revenue is associated with an additional 0.07 rupiah per capita (SE = 0.03) of the special grant in the specification with one-year shocks. This effect is half as large, and statistically insignificant, in the specification with two-year shocks. The responses of own-source

[22] In field interviews, public-sector midwives in Yogyakarta said that they could earn significantly more in the private sector, but stayed in the public sector due to job security (Untied Nations Population Fund Indonesia, 2014, p. 47).

[23] Functional expenditure comprises the sum of capital, personnel, goods and services, and other expenditure related to a particular function. Each functional category can therefore include spending on items traditionally categorised as infrastructure. For example, education expenditure includes spending on school buildings. The infrastructure category encompasses other types of infrastructure, such as roads, that do not fall under the other functional categories.

[24] The time-series estimates in Online Appendix Table A.2 indicate either a positive or negative correlation, depending on the number of lags included. These estimates are merely suggestive due to the extremely small sample size.

revenue and shared tax revenue are small in magnitude and statistically indistinguishable for the general grant and the oil and gas grant. Because the special grant is an earmarked, discretionary transfer, one may be concerned that this grant targeted districts that benefited the most from the general grant reform. Any bias due to this grant is necessarily small, given the small magnitude of the point estimate. Nevertheless, I re-estimate the model controlling for the special grant, noting that the endogeneity of this grant could introduce a new source of bias. The estimates reported in Online Appendix Table A.9 are slightly smaller than the baseline estimates, but the general pattern is very similar. Overall, there is little indication that other sources of revenue cause significant bias.

Another potential confounder is district splitting. As previously mentioned, general grant revenue per capita tends to increase following splits. Furthermore, districts with greater land area are more likely to split. Given the identification strategy, any factor that differentially impacts districts with greater land area per capita after 2006 is a potential source of bias. Therefore, districts that split after the first moratorium ended in 2006 are of particular concern. The baseline specification addresses this concern by flexibly controlling for the dynamic impact of splitting. As an alternative check, I drop districts that split after the first moratorium. The results are similar to the baseline estimates (Online Appendix Table A.10).

3.5.2. *Functional form*

The estimates could also be biased if the functional form of (1) is incorrect. In particular, the assumption that spending responds symmetrically to increases and decreases in revenue might not hold, due to downward rigidities in expenditure. Asymmetric spending responses could lead to a mistaken conclusion that spending responds more to the general grant, because the effect of the general grant is identified from a single increase whereas the effect of the oil and gas grant is identified from several increases and decreases. To examine whether this is an important source of bias, I estimate the model

$$\begin{aligned} Y_{d,t+h} - Y_{d,t-k} = \beta_h (G_{d,t} - G_{d,t-k}) &+ \delta_h^+ (H_{d,t} - H_{d,t-k})^+ \\ &+ \delta_h^- (H_{d,t} - H_{d,t-k})^- + \boldsymbol{\phi}_h' (\boldsymbol{X}_{d,t} - \boldsymbol{X}_{d,t-k}) + \lambda_{r(d),t,h} + \varepsilon_{d,t,h}. \end{aligned} \tag{3}$$

Equation (3) allows for asymmetric responses to increases and decreases in the oil and gas grant, denoted by $(H_{d,t} - H_{d,t-k})^+ \equiv \max\{0, H_{d,t} - H_{d,t-k}\}$ and $(H_{d,t} - H_{d,t-k})^- \equiv \min\{0, H_{d,t} - H_{d,t-k}\}$. (The instrument $E_d \cdot \tilde{H}_{(-d),t}$ is likewise partitioned into increases and decreases.)

Online Appendix Table A.11 presents the results. Focusing on one-year changes in grants (Panel A), expenditure increases significantly in response to increases in the oil and gas grant, while the response to decreases in the oil and gas grant is weaker and potentially negative. However, the null hypothesis of symmetry ($\delta_h^+ = \delta_h^-$) is never rejected at conventional levels, even using unadjusted p-values. The imprecision of the estimates of δ_h^- appear to drive this result. However, the null hypothesis that *increases* in the two grants induce the same response ($\beta_h = \delta_h^+$) is rejected at the 1% level for horizons $h = 0, \ldots, 3$. This hypothesis is also rejected at horizon $h = 4$, but only at the 10% level using the unadjusted p-value. Thus, while spending may respond asymmetrically to increases and decreases in the oil and gas grant, this asymmetry does not drive the baseline results. Expenditure responds more to the general grant than to increases in the oil and gas grant.

3.5.3. *Instrument construction*

Finally, I examine the robustness of the results to alternative constructions of the general grant instrument. The baseline specification uses a triple interaction: $A_d \cdot N_d \cdot 1(t \geq 2006)$. Online Appendix Table A.12 shows that the results are similar when using the double interaction $A_d \cdot 1(t \geq 2006)$ as the instrument instead. When retaining the baseline (triple interaction) instrument, but controlling for the lower-order interaction $N_d \cdot 1(t \geq 2006)$, the estimates again remain similar to the baseline results (Online Appendix Table A.13).[25] Furthermore, the estimates for the general grant remain similar to the baseline estimates when hydrocarbon-rich provinces are omitted from the sample (Online Appendix Table A.14).

4. Public Service Delivery Responses

4.1. *Empirical Strategy*

Having established that the fiscal responses to the two grants are consistent with the theory, I next examine the impacts on public service delivery. Data on public goods and services come from the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES), a triennial census that is intended to cover every village in Indonesia. I merge villages across six survey waves from 1999 to 2014, producing a balanced panel of around 44,000 villages located in districts in the analysis sample. I then aggregate outcomes to the district level. (See Online Appendix Section A.4 for details.)

The outcomes of interest are public schools, health facilities, health personnel and paved roads. I focus on these outcomes due to data availability and the fact that district governments are responsible for either provision (education and health) or financing (local roads) of these services.[26] All of the measures of public service delivery involve either lumpy investment (schools, health clinics, paved roads) or committed expenditure (health personnel). The theory predicts that the general grant will have a larger impact on these outcomes than the oil and gas grant, and that the outcomes may not respond at all to the oil and gas grant.

Because outcomes are observed only every three years, I aggregate grant revenue over time by taking three-year averages. For year t in which public service delivery is observed, let $\overline{G}_{d,t}$ denote average general grant revenue in district d across years t, $t-1$ and $t-2$, and let $\overline{H}_{d,t}$ denote the corresponding three-year average of the oil and gas grant.[27] I apply the same transformation to the instruments and estimate the direct projections

$$\begin{aligned} Y_{d,t+h} - Y_{d,t-3} &= \beta_h(\overline{G}_{d,t} - \overline{G}_{d,t-3}) + \delta_h(\overline{H}_{d,t} - \overline{H}_{d,t-3}) \\ &\quad + \boldsymbol{\phi}_h'(\boldsymbol{X}_{d,t} - \boldsymbol{X}_{d,t-3}) + \lambda_{r(d),t,h} + \varepsilon_{d,t,h}, \end{aligned} \tag{4}$$

for $h \in \{0, 3, 6\}$. Differencing removes district fixed effects, and region-by-year effects control for arbitrary regional differences in the evolution of public services over time. Equation (4) allows grants to have lagged effects, due to lagged expenditure responses or time to build.

As previously discussed, the key identifying assumption is that districts with different exposure to the grant shocks would have experienced similar trends in public service delivery in the absence of shocks to the grants. Apart from the concerns discussed in the context of fiscal responses, one

[25] Note that the lower-order interaction $A_d \cdot N_d$ is eliminated through differencing.

[26] District and village governments both contribute to local infrastructure. Districts finance upgrades and procure engineers while villages finance and implement maintenance projects (World Bank, 2010).

[27] In 2002 $\overline{G}_{d,t}$ and $\overline{H}_{d,t}$ are measured as two-year averages, because the grants did not exist in 2000.

potential problem is that less developed areas could be experiencing catch-up growth in public services over this period. If public service delivery trends differed for districts with different population densities for reasons other than the general grant reform, the estimates would be biased. Catch-up growth in public services would likely produce differential trends prior to the reform, however. I test for differential pre-trends ahead.

4.2. *Reduced-Form Effects over Time*

I begin by estimating the reduced-form impacts of exposure to the two grants on public service delivery using the regression

$$Y_{d,t} = \sum_{s \in \mathcal{S}} \theta_s A_d \cdot N_d \cdot D_t^s + \sum_{s \in \mathcal{S}} \gamma_s E_d \cdot D_t^s + \boldsymbol{\pi}' \boldsymbol{X}_{d,t} + \alpha_d + \lambda_{r(d),t} + u_{d,t}, \tag{5}$$

where D_t^s is an indicator that equals one if $t = s$. The set $\mathcal{S}$ includes all available survey years except for the reference year, 2005. Thus, θ_s and γ_s measure the change in the gradients of Y in exposure to the general grant reform and exposure to the oil and gas grant, respectively, between 2005 and year s.

Figure 4 displays point estimates and 95% confidence intervals for the parameters in (5). Figure 4(a) plots the estimates of $\{\theta_s\}$. This gradient is roughly constant over time prior to 2006, which means that pre-trends were similar for districts with different exposure to the general grant reform.[28] For almost all outcomes, the gradient increases after 2006, suggesting that the permanent increase in the general grant increased public service delivery. The only exception is public primary schools per capita, for which the gradient decreases after 2006. This decrease is smaller than the increase in the gradient of public secondary schools per capita. As shown in Online Appendix Figure A.4, the gradient of school access, measured as the share of villages with at least one school, did not change for public primary schools, whereas it increased for public kindergartens and secondary schools. This suggests that the decrease in the gradient of public primary schools is due to a reduction in schools in villages that already had multiple schools.

To assess overall responses, I construct a public services index, defined as the average of the seven public good outcomes after standardising each outcome by its baseline mean and standard deviation. For this index, the gradient is constant prior to 2006 and steadily grows after 2006, implying an increase in overall public service delivery in response to the general grant.

Panel (b) of Figure 4 displays the estimates of $\{\gamma_s\}$. Despite the large increase in the oil and gas grant in 2006, only the gradient of doctors per capita sharply increases from 2005 to 2008. The gradients of public secondary schools per capita and access to paved roads steadily grow over the entire sample period, but changes in these gradients do not coincide with the sharp changes in the oil and gas grant. The reduced-form evidence is inconsistent with investment responding to transitory shocks to revenue. However, it is consistent with public services responding to the permanent increase in oil and gas revenue starting in 2001, as discussed below.[29]

[28] There is a slight upward pre-trend in the gradients of public secondary schools and paved roads. I address this issue in Section 4.4.

[29] Note that the magnitudes are not comparable across panels (a) and (b) of Figure 4, because the exposure variables are measured in different units.

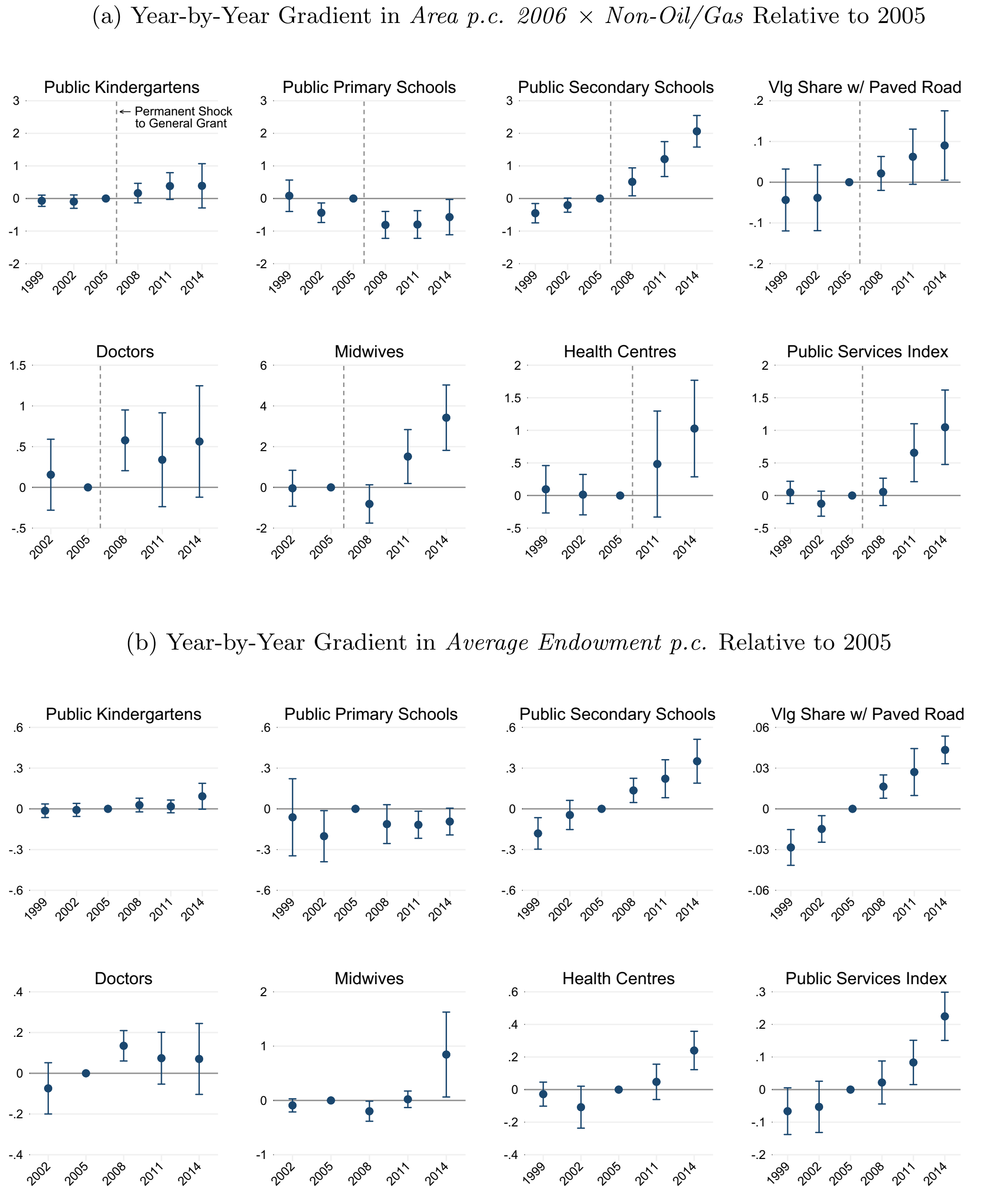


Fig. 4. *Reduced-Form Effects of Grant Exposure on Public Service Delivery over Time. This figure displays point estimates and 95% confidence intervals for* $\{\theta_s\}_{s\in\mathcal{S}}$ *in panel (a) and* $\{\gamma_s\}_{s\in\mathcal{S}}$ *in panel (b) in (5). The reference year is 2005. Average hydrocarbon endowment per capita is measured in constant 2010 IDR 100 millions to make the vertical axes in the two panels similar. Confidence intervals are robust to heteroscedasticity and two-way clustering by district and province-by-year.*

4.3. *Main Results*

Table 4 reports IV estimates of the mean responses to the two grants, $\sum_{h\in\{0,3,6\}}\beta_h/3$ and $\sum_{h\in\{0,3,6\}}\delta_h/3$, to conserve space. (Online Appendix Figure A.5 plots the dynamic responses.) These estimates represent the average change in public service delivery over the short and medium term due to an increase in grant revenue by IDR 1 million ($\approx$ USD100) per capita. For context, total revenue per capita averages around 2 million IDR per capita over the sample period. All outcome variables involve either lumpy investment or committed expenditure. Therefore, Hypothesis 3 predicts that the general grant will increase every public service, but the oil and gas grant will have no effect on any public service. Each column in Table 4 reports p-values from testing the hypothesis that the grants have equal effects.

Columns 1–3 report the estimates for public schools. The mean response of public kindergartens to the general grant is 0.336 (SE = 0.182), which means that increasing the general grant by IDR 1 million per capita raises the number of kindergartens per 10,000 people by 0.34. This is a large increase relative to the baseline mean of 0.19. Surprisingly, the provision of public primary schools falls in response to the general grant, with a mean response of −0.766 (SE = 0.263). However, this effect is small relative to the baseline mean of around 8. The mean response of public secondary schools is 1.299 (SE = 0.190), which represents a doubling relative to the baseline mean of 1.2. Overall, the general grant significantly increases the provision of public schools, as the increase in public kindergartens and secondary schools is over twice a large as the reduction in primary schools. By contrast, the mean response to the oil and gas grant is small and statistically insignificant for public kindergartens and secondary schools. The effect of the oil and gas grant on public primary schools (−0.184) is negative and statistically significant, but small in magnitude.[30]

Columns 4–6 report the estimates for health personnel and facilities. The mean response to the general grant is 0.517 (SE = 0.271) for doctors, 1.346 (SE = 0.654) for midwives and 0.834 (SE = 0.494) for health care centres. These effects range from one quarter to one third of the baseline mean of the respective outcomes. The mean responses to the oil and gas grant are less than a third as large, yet they are statistically significant for midwives and health care centres. The outcome in column 7 is the share of villages where the main road is paved. At baseline, the average share is 0.64. Increasing the general grant by IDR 1 million per capita raises this share by 0.051 (SE = 0.026). The effect of the oil and gas grant is about half as large at 0.026 (SE = 0.012).

For six out of the seven outcomes considered, the general grant has a positive and economically large effect. For one of these outcomes—public secondary schools—we can statistically reject equal responses to the two grants using both conventional and adjusted p-values. However, for the most part we fail to reject equal responses. This is perhaps unsurprising, considering the large number of outcomes and the noise associated with any given measure of public service delivery.

Column 8 reports the mean responses of the public services index. The response to the general grant is 0.593 (SE = 0.162), meaning that overall public service delivery increases by about 0.6 SD. By contrast, the response to the oil and gas grant is 0.115 (SE = 0.118). The hypothesis of equal responses to the two grants is easily rejected ($p = 0.006$). In sum, the general grant stimulates across-the-board improvements in public services, whereas the oil and gas grant does not. Thus, the evidence is consistent with Hypothesis 3.

[30] The effect of the general grant on the total number of public schools is 0.869 (SE = 0.466), while the effect of the oil and gas grant is 0.128 (SE = 0.255). (Result not reported.)

Table 4. *Mean Responses of Public Service Delivery to Grants.*

	Public schools per 10,000 people			Health personnel and facilities per 10,000 people			Share of villages	Index
	Kindergarten (1)	Primary (2)	Secondary (3)	Doctors (4)	Midwives (5)	Health centres (6)	Paved road (7)	(8)
General grant p.c.	0.336*	−0.766***	1.299***	0.517*	1.345**	0.834*	0.051*	0.593***
	(0.182)	(0.263)	(0.190)	(0.271)	(0.654)	(0.494)	(0.026)	(0.162)
Oil and gas grant p.c.	0.067	−0.184***	0.246	0.100	0.364***	0.245*	0.026**	0.115
	(0.084)	(0.060)	(0.221)	(0.140)	(0.073)	(0.141)	(0.012)	(0.118)
Baseline mean outcome	0.191	8.011	1.220	1.665	5.701	2.599	0.641	−0.001
H_0: gen. = oil and gas								
Unadjusted p-value	0.145	0.016	0.000	0.108	0.125	0.202	0.334	0.006
Adjusted p-value	*0.541*	*0.093*	*0.000*	*0.541*	*0.541*	*0.541*	*0.541*	
SW F-stat.: gen. grant	63.6	63.6	63.6	64.2	64.2	64.2	63.6	63.6
SW F-stat.: oil and gas	77.5	77.5	77.5	76.2	76.2	131.6	77.5	77.5
Observations	1,392	1,392	1,392	1,388	1,388	1,044	1,392	1,392
District clusters	348	348	348	347	347	348	348	348
Prov. × year clusters	111	111	111	111	111	83	111	111

Notes: This table reports IV estimates of the mean responses of public service delivery to the general grant, $\sum_{h\in\{0,3,6\}} \beta_h/3$, and to the oil and gas grant, $\sum_{h\in\{0,3,6\}} \delta_h/3$, obtained by replacing the outcome in (4) with $\sum_{h\in\{0,3,6\}}(Y_{d,t+h} - Y_{d,t-3})/3$. Because the data on health care centres are missing in 2008, β_0 and δ_0 are not identifiable for this outcome, so the table reports $\sum_{h\in\{3,6\}} \beta_h/2$ and $\sum_{h\in\{3,6\}} \delta_h/2$. Each regression controls for region-by-year effects and indicators for whether the district has split, defined separately for parent and child districts, as well as three lags of these indicators. The baseline mean of the outcome variable is measured in 2002. Adjusted p-values use the Holm correction for multiple hypothesis testing. Sanderson and Windmeijer (2016) first-stage F-statistics are reported for each endogenous variable. Standard errors, reported in parentheses, are robust to heteroscedasticity and two-way clustering by district and province-by-year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4. *Robustness Checks*

The potential sources of bias in estimating β_h and δ_h in (4) are similar to those discussed for the fiscal responses. The estimates of β_h would be biased if the gradient in exposure to the general grant reform would have increased after 2006 in the absence of the reform, perhaps due to differential pre-trends. Reassuringly, this gradient is roughly constant over time prior to 2006 for most public services and for the overall index (Figure 4(a)). This suggests that the estimated impacts of the general grant are not driven by pre-existing trends in services.[31]

However, the gradients of public secondary schools and paved roads are trending slightly upward before 2006, raising the question of whether pre-trends drive the results for these outcomes. I address this concern in two ways. First, I control for time-varying effects of baseline district characteristics. Specifically, I control for year effects interacted with the following variables, all measured in 2000: ethnic fractionalisation, urbanisation rate, share of population aged 15–64, share of population with a primary education, share of population with a secondary education and log GDP per capita. Adding these controls nearly eliminates the pre-trend for public secondary schools, yet the gradient still increases sharply after 2006 (Online Appendix Figure A.6(a)). However, there is still an upward (statistically insignificant) pre-trend in paved roads, and the gradient now increases by a smaller amount after 2006 for this outcome. The results for the other outcomes do not change much. The estimated impact of the general grant is slightly larger for health centres and slightly smaller for the other outcomes (Online Appendix Table A.15). Overall, the results do not appear to be driven by differential trends owing to baseline differences in district characteristics.

As a second approach to addressing bias due to differential trends, I conduct a sensitivity analysis following Rambachan and Roth (2023).[32] The idea is to assume that the confounding factors that produced a non-constant gradient in the post-reform period are similar in magnitude to the confounding factors in the pre-reform period. Online Appendix Figure A.7 displays 95% confidence intervals for θ_{2014} in (5), allowing the maximum post-treatment violation of the constant gradient assumption to be up to M times the maximum pre-treatment violation of the constant gradient assumption.[33] The result for public secondary schools is highly robust, despite this outcome exhibiting statistically significant differential pre-trends. The 95% confidence interval for θ_{2014} is [0.96,3.50] when allowing post-reform violations of the constant gradient assumption to be no larger than the maximal pre-reform violation ($M = 1$). To overturn the conclusion that θ_{2014} is statistically significant, one would need to allow post-reform violations to be up to twice as large as the maximal pre-reform violation ($M = 2$). This robustness is due to the fact that pre-trends for public secondary schools are precisely estimated and small in magnitude relative to the post-reform change in the gradient. The robustness of the other results varies based on the size and precision of the pre-trend estimates. The conclusion that overall public service delivery

[31] Recall that trends in the gradient in average hydrocarbon endowment per capita are not informative for the identifying assumptions, because the oil and gas grant experienced shocks in every period.

[32] I use the Stata package `honestdid`. See https://github.com/mcaceresb/stata-honestdid.

[33] Formally, let ζ_t denote the change in the gradient in exposure to the general grant reform from 2005 to year t that would have occurred in the absence of the reform (ζ_{2005} is normalised to zero). For $t < 2005$, ζ_t is identified as the differential pre-trend in the gradient. For $t > 2005$, ζ_t quantifies the (hypothetical) bias in our estimate of θ_t in (5) due to a violation of the constant gradient assumption. For a given M, the confidence interval reported in Online Appendix Figure A.7 is robust to $\zeta = (\zeta_{1999}, \zeta_{2002}, \ldots, \zeta_{2014})$ such that

$$\zeta \in \left\{\zeta : \forall t \geq 2005,\ |\zeta_{t+3} - \zeta_t| \leq M \cdot \max_{s \leq 2005} |\zeta_s - \zeta_{s-3}|\right\}.$$

increased in response to the general grant shock depends on the assumption that post-reform violations of the constant gradient assumption are no larger than the maximal pre-reform violation ($M = 1$).

The estimates are similar when no controls are included or when special grant revenue is added to the set of controls (Online Appendix Tables A.16 and A.17). Controlling for district oil and gas production has virtually no impact on the results (Online Appendix Table A.18), while dropping districts that split after the first moratorium also yields similar results (Online Appendix Table A.19). When I allow for asymmetric responses to increases and decreases in the oil and gas grant, I find that public service delivery generally responds more to the general grant than to increases in the oil and gas grant (Online Appendix Table A.20). The ordinary least squares (OLS) estimates also suggest that public service delivery responds more to the general grant, but the point estimates for the general grant are smaller than the IV estimates (Online Appendix Table A.21). This is consistent with the general grant endogenously increasing in response to negative shocks at the district level. Similar results obtained when using the double interaction $A_d \cdot 1(t \geq 2006)$ as the instrument (Online Appendix Table A.22), when retaining the baseline (triple interaction) instrument, but controlling for the lower-order interaction $N_d \cdot 1(t \geq 2006)$ (Online Appendix Table A.23), and when dropping hydrocarbon-rich provinces (Online Appendix Table A.24). Finally, the two grants continue to have statistically different effects on the public services index when outcomes are dropped from the index one at a time (Online Appendix Table A.25).

4.5. *Other Potential Mechanisms*

The results presented thus far are consistent with districts adjusting lumpy public services in response to large changes in lifetime fiscal resources. Still, there are other potential explanations. Local officials might simply be wasting or embezzling a larger portion of the oil and gas grant when compared to the general grant. As previously noted, both grants are subject to the same regulations and oversight by the central government. Nevertheless, factors beyond administrative oversight could contribute to varying levels of misappropriation between the grants. For example, the grants could have different impacts on political competition, or hydrocarbon-rich districts could be more corrupt at baseline. I test for these mechanisms in the next two subsections. Another possibility is that hydrocarbon-rich districts have a lower marginal propensity to provide public services out of grants compared to districts exposed to the general grant shock, owing to differences in urbanisation, education, GDP or other characteristics. I examine this hypothesis in the third subsection. In the fourth and final subsection, I evaluate an implication of many alternative mechanisms: a *permanent* increase in the oil and gas grant should also fail to stimulate greater public service delivery.

4.5.1. *Effects on political competition*

The two grants could have different effects on local politics, which could impact how revenue is translated into services. For example, the oil and gas grant might have a larger negative impact on political competition than the general grant. The reduction in competition could then lead to worse governance.

Online Appendix Table A.26 reports IV estimates of the effects of the two grants on different measures of political competition. For the first outcome (number of candidates), higher values indicate greater competition. For the remaining outcomes (Herfindahl Index of vote shares, size of winning coalition, re-election of incumbent and margin of victory), higher values indicate less competition. I estimate three versions of the model: the first assuming that grants in the election year affect the outcomes, the second assuming that grants in the year before the election affect the outcomes and the third assuming that average grants over the mayoral term affect the outcomes. The reason is that the appropriate timing is unclear, as elections happen any time from January to December and grants are disbursed in instalments throughout the year. The estimates indicate that neither grant has a strong effect on political competition. If anything, the general grant reduces political competition more than the oil and gas grant. We reject the hypothesis (at the 10% level) that the grants have equal effects in only two out of 15 regressions. Political competition therefore does not seem to explain the results for public service delivery.

4.5.2. *Baseline differences in corruption*

Another potential mechanism is differences in baseline corruption. If hydrocarbon-rich districts are especially corrupt—in line with the resource curse literature—then funds from all sources are more likely to go missing in such places. This would be a problem because the responses to the general grant are identified by comparing hydrocarbon-poor districts with varying population densities, whereas the responses to the oil and gas grant are identified by comparing hydrocarbon-rich and hydrocarbon-poor districts. Baseline differences in corruption could therefore drive the empirical results.

An ideal test would examine the association between exposure to the grant shocks and a wide array of corruption variables in 2000—the year prior to fiscal decentralisation. However, only one measure of corruption is available in 2000: bribes paid by manufacturing firms. I supplement these data with a richer dataset on corruption outcomes measured in 2007 and 2010, as described ahead.

I measure corruption in 2000 using establishment-level data from the Indonesian manufacturing survey of large- and medium-sized firms (*Survei Industri Besar/Sedang*). Establishments report the value of ‘gifts, charitable contributions, donations, etc.’ paid to external parties, which I interpret as bribes to local officials following Henderson and Kuncoro (2006; 2011).[34]

Table 5 presents the results.[35] In columns 1–3 the outcome is an indicator equal to one if the firm paid any gifts in 2000, while in columns 4–6 the outcome is the value of gifts paid in that year. The regressions in Panel A use binary measures of exposure to the grant shocks, which are easy to interpret. Since the results are similar whether or not I control for (log) firm revenue or region effects, I focus on the results controlling for both. Compared to districts with low exposure to both grants, the probability of paying any bribe is 16 percentage points lower in districts with high exposure to the general grant shock, and 16 percentage points lower in hydrocarbon-rich districts (column 3). Exposure to the general grant shock is positively associated with the value of bribes paid, while exposure to the oil and gas grant is negatively associated with bribe value, though both associations are statistically insignificant (column 6). Similar qualitative patterns emerge using the continuous measures of exposure (Panel B).[36]

Next I examine the corruption variables contained in the Economic Governance Survey conducted by KPPOD (Regional Autonomy Watch) and the Asia Foundation. The survey consists

[34] Cassidy and Velayudhan (2024) validate this interpretation by showing that within-firm variation in gift-giving is positively correlated with firm activities that require permits or licences from the local government.

[35] The number of districts falls because districts are only identified at 2000 borders and some districts do not contain any large- or medium-sized manufacturing establishments.

[36] The point estimates are similar, albeit less precise, when I drop firms for which a non-zero share of the capital is owned by any level of government.

Table 5. *Baseline Corruption and Exposure to Grant Shocks: Firm-Level Estimates.*

	Probit: Firm paid any gifts in 2000			Poisson: Value of gifts in 2000		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Binary measures of exposure					
Top 25% area p.c. 2006	−0.132**	−0.125*	−0.158*	−0.185	−0.309	0.376
× Non-oil/gas	(0.066)	(0.065)	(0.084)	(0.481)	(0.402)	(0.327)
Top 5% endowment p.c.	−0.174***	−0.118**	−0.163***	−0.462	−1.510**	−0.897
	(0.057)	(0.057)	(0.060)	(0.480)	(0.646)	(0.557)
	Panel B: Continuous measures of exposure					
Area p.c. 2006	−0.138	−0.145	−0.290	−0.998	−0.744	1.433*
× Non-oil/gas	(0.154)	(0.147)	(0.180)	(0.982)	(0.877)	(0.830)
Endowment p.c.	−0.875**	−0.631*	−0.992**	−0.381	−7.920	−3.009
	(0.427)	(0.353)	(0.417)	(2.313)	(5.579)	(3.285)
Log firm revenue	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes
Outcome mean	0.63	0.63	0.63	49.24	49.24	49.24
Observations	17,251	17,251	17,251	17,251	17,251	17,251
Districts (2000 borders)	278	278	278	278	278	278

Notes: This table reports establishment-level estimates of the cross-sectional relationship between exposure to the grant shocks and gifts paid by manufacturing firms to external parties in 2000. 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 USD0.11). The regressions in Panel A use binary measures of exposure to the grant shocks, while the regressions in Panel B use continuous measures. The regressions control for log firm revenue and region fixed effects, as indicated. Standard errors, reported in parentheses, are robust to heteroscedasticity and clustering by district at 2000 borders. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of two waves, enumerated in 2007 and 2010, and is designed to measure the effects of local governance on the business environment. I focus on survey questions in the following three areas: perceptions of local government corruption, informal costs and payments in exchange for security. I regress each firm-level outcome on indicators for high exposure to each grant, controlling for (log) firm employment and region-by-survey-wave effects.

Figure 5 displays the results. With one exception, districts with high exposure to the grants have similar or lower levels of measured corruption relative to districts with low exposure to both grants. In particular, there is no evidence that hydrocarbon-rich districts are more corrupt than hydrocarbon-poor districts. If anything, corruption is slightly lower in hydrocarbon-rich districts. The results are qualitatively similar when using continuous measures of exposure to the grant shocks (not reported).

While measuring corruption is always challenging, the available evidence suggests that hydrocarbon-rich districts were not more corrupt than hydrocarbon-poor districts in general, or districts with high exposure to the general grant shock in particular. Baseline differences in corruption are therefore unlikely to explain the results for public services.

4.5.3. *Baseline differences in other characteristics*

While baseline levels of corruption do not explain the results, other characteristics might. Online Appendix Table A.27 shows that districts with significant exposure to the general grant shock tend to be less urbanised, have lower education levels and possess lower GDP per capita, compared to hydrocarbon-rich districts.[37] If the marginal propensity to provide public services out of grants

[37] Districts with high exposure to the general grant shock also cover 12.7% more land area (SE = 21.2%) and have 21.9% higher populations (SE = 17.7%), implying a small difference in population density.

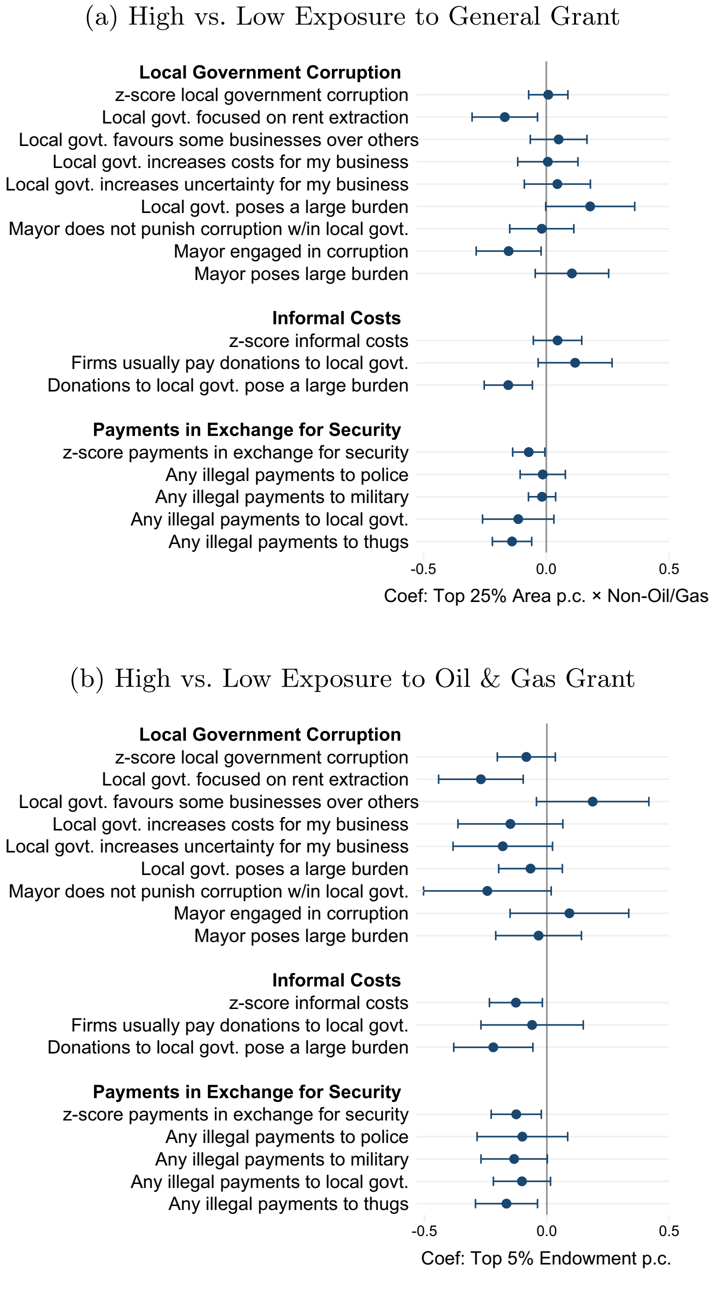


Fig. 5. *Midline Corruption and Exposure to Grant Shocks. This figure plots estimates of* β *in panel (a) and* δ *in panel (b) in* $Y_{f,d,t} = \beta HighGen_d + \delta HighOG_d + \alpha \log Emp_{f,d,t} + \lambda_{r(d),t} + \varepsilon_{f,d,t}$*, using firm-level data from the Economic Governance Survey waves of 2007 and 2010.* $HighGen_d$ *is an indicator equal to one if the district is in the top 25% in terms of land area per capita and is located in a resource-poor province.* $HighOG_d$ *is an indicator equal to one if the district is in the top 5% in terms of average hydrocarbon endowment per capita.* $Emp_{f,d,t}$ *is the number of employees in the firm, and the* $\lambda_{r(d),t}$ *are region-by-survey-wave effects. All firm outcomes are standardised to have a mean of zero and 1 SD. Confidence intervals of 95% are reported.*

is heterogeneous across districts, then imbalance in these characteristics could lead to different responses to the two grants.[38] To address this concern, I allow the responses to the grants to depend on baseline covariates, and I evaluate both responses at the same value of the covariates.

Given the use of continuous regressors and instruments in the empirical model, the most natural way to correct for covariate imbalance and accommodate heterogeneous effects is through a (parametric) regression adjustment approach. As in Section 4.4 I control for time-varying effects of baseline characteristics, but now I also include interactions between the grants and these covariates. The regression is

$$\begin{aligned} Y_{d,t+h} - Y_{d,t-3} = {} & \beta_h(\overline{G}_{d,t} - \overline{G}_{d,t-3}) + \delta_h(\overline{H}_{d,t} - \overline{H}_{d,t-3}) \\ & + \boldsymbol{\theta}_h'(\overline{G}_{d,t} - \overline{G}_{d,t-3}) \cdot \dot{\boldsymbol{W}}_d + \boldsymbol{\gamma}_h'(\overline{H}_{d,t} - \overline{H}_{d,t-3}) \cdot \dot{\boldsymbol{W}}_d \\ & + \sum_{s \in \mathcal{S}} \pi_{h,s} \boldsymbol{W}_d \cdot D_t^s + \boldsymbol{\phi}_h'(\boldsymbol{X}_{d,t} - \boldsymbol{X}_{d,t-3}) + \lambda_{r(d),t,h} + \varepsilon_{d,t,h}, \end{aligned} \quad (6)$$

where D_t^s is an indicator that equals one if $t = s$, and $\mathcal{S}$ is the set of PODES survey years. The baseline covariates $\boldsymbol{W}_d$ correspond to those described in Section 4.4. The interaction term covariates, $\dot{\boldsymbol{W}}_d$, are demeaned using average values for districts with high exposure to one of the two grants (i.e., either top 25% land area per capita in 2006 and not located in a hydrocarbon-rich province, or top 5% average hydrocarbon endowment per capita). The coefficients β_h and δ_h thus represent the grant effects for an average district with substantial exposure to either grant. The parameters $\boldsymbol{\theta}_h$ and $\boldsymbol{\gamma}_h$ describe how marginal changes in the covariates influence the responses to the grants. Interactions between $\dot{\boldsymbol{W}}_d$ and the baseline instruments are added to the instrument set so that the parameters remain exactly identified.

Online Appendix Table A.28 shows that the results based on (6) are qualitatively similar to the baseline results. For both grants, the estimated responses become somewhat smaller for public schools, health centres and paved roads, but larger for doctors and midwives. The general grant continues to have a larger effect on all outcomes except for public primary schools. The standard errors are generally larger compared to the baseline estimates, which is unsurprising given the reduction in degrees of freedom and the addition of ten endogenous variables and ten instruments. Nevertheless, the impact of the general grant on overall public service delivery (0.54 SD) remains statistically significant at the 1% level. By contrast, the overall impact of the oil and gas grant (0.02 SD) is statistically insignificant and differs from the effect of the general grant at the 10% level. Heterogeneous responses to grants according to unbalanced covariates thus do not drive the baseline results.

4.5.4. *Other differences between the grants*

It is not possible to anticipate every potential mechanism, unrelated to persistence, that could explain the baseline results. However, it is possible to test a common implication of these mechanisms: a permanent increase in the oil and gas grant should also fail to stimulate an increase in public services.

Decentralisation induced a large permanent increase in the oil and gas revenue received by district governments. Prior to 2001, districts received virtually no revenue from local natural resource extraction. Unfortunately, it is not possible to credibly estimate the per-dollar impact of

[38] The table also shows that baseline public service provision was lower in hydrocarbon-rich districts. This would imply a greater marginal propensity to provide public services, so it cannot explain the baseline results.

(a) Year-by-Year Gradient in *Average Endowment p.c.* Relative to 1999

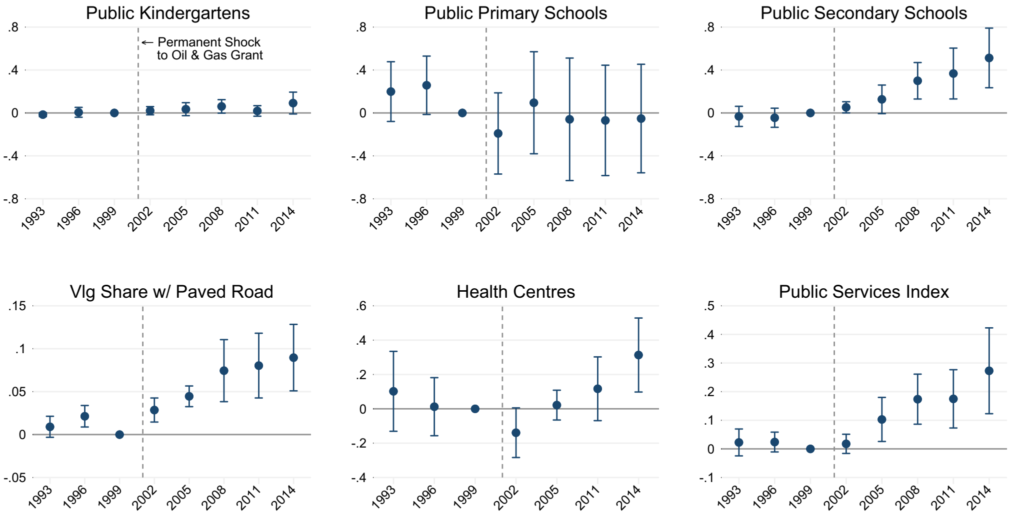


Fig. 6. *Reduced-Form Effects of Oil and Gas Grant Exposure on Public Service Delivery. This figure displays point estimates and 95% confidence intervals for* $\{\gamma_s\}_{s \in S}$ *in (5), where the reference year is 1999. The regressions additionally control for year effects interacted with the following variables (measured in 2000): ethnic fractionalisation, urbanisation rate, share of population aged 15–64, share of population with a primary education, share of population with a secondary education and log GDP per capita. Average hydrocarbon endowment per capita is measured in constant 2010 IDR 100 millions. Confidence intervals are robust to heteroscedasticity and two-way clustering by district and province-by-year.*

permanent oil and gas revenue using (4). Many policies and institutions changed during decentralisation, which could have had differential impacts on hydrocarbon-rich versus hydrocarbon-poor districts. The modern grant system also did not exist prior to 2001, so it is unclear how to measure the change in revenue around decentralisation. At a minimum, though, it is possible to test whether public service delivery improved in hydrocarbon-rich districts relative to hydrocarbon-poor districts following decentralisation.

Figure 4 shows that this is indeed the case: the gradient in average endowment per capita increases over time following decentralisation for most public services, and it clearly increases for the public services index (Figure 4(b)). Of course, these results could reflect differential development trajectories in hydrocarbon-rich versus hydrocarbon-poor districts that would have occurred in the absence of the revenue-sharing policy. I therefore add two more years of pre-decentralisation data using the 1993 and 1996 waves of the village census in order to test for differential pre-trends. (See Online Appendix Section A.4 for details.) I then estimate (5), using 1999 as the reference year.

Figure 6 displays the results.[39] The estimates suggest that trends in public service delivery did not significantly differ by average endowment per capita prior to decentralisation. In particular, the public services index has very similar (and statistically indistinguishable) gradients in 1993,

[39] It is not possible to estimate pre-trends for the number of doctors and midwives, as these data are missing in 1999.

1996 and 1999. Furthermore, the gradient increases in 2002 and continues to increase in the following years, suggesting that the permanent increase in oil and gas revenue significantly increased public service delivery, albeit with a lag.

Given the many changes that occurred around decentralisation, these results are speculative. Still, they are consistent with the idea that revenue persistence explains the results for public services.

5. Discussion

The finding that the general grant stimulates greater public services stands in contrast to the large literature arguing that non-tax revenue hinders government performance. It also goes against recent causal evidence from Gadenne (2017) and Martínez (2023) that local taxes, but not grants, improve public service delivery. In both of those papers, the shock to tax revenue appears to be permanent: Gadenne (2017) exploits the rollout of tax-capacity investments, while Martínez (2023) uses upward revisions to assessed property values. A key question is whether the shocks to non-tax revenue exhibit similar persistence. In Gadenne (2017) fiscal transfers increase when municipal population crosses a cut-off over time. The amount of time that crossing municipalities spend just above the cut-off is similar to the amount of time that municipalities are observed in a tax-capacity programme in her sample, so the revenue shocks could have similar persistence. Martínez (2023) exploits shocks to shared oil and gas revenue, which are clearly transitory. My results underscore the importance of revenue persistence in a setting where the two revenue sources are subject to the same level of accountability.

On the fiscal side, Besfamille *et al.* (2023) find similar qualitative results for Argentinian provinces: spending responds more to the relatively more persistent grant (based on shared tax revenue) than to hydrocarbon royalties. However, the results differ in absolute terms, with Indonesian districts exhibiting a higher marginal propensity to spend out of hydrocarbon revenue compared to Argentinian provinces. The institutional context might explain this difference. While the degree of local tax autonomy is similarly limited in both countries, Argentinian provinces are much larger in terms of population and land area. Argentinian provinces also had considerable experience managing volatile revenue, as the royalty-sharing regime was established 20 years before the beginning of the study period in Besfamille *et al.* (2023). Further research is needed to understand why local governments smooth expenditure to differing degrees.

Interestingly, Andersen and Sørensen (2019) find that permanent shocks to general-purpose grants and natural resource revenue have similar effects on local employment in Norway. Their setting is admittedly very different than Indonesia, but their findings are consistent with the notion that the persistence of revenue shocks is what matters—not the source of revenue per se.

6. Conclusion

Indonesian districts experienced large shocks to unconditional grants in the period following decentralisation. Districts with greater land area per capita and few natural resources saw a larger permanent increase in the general grant starting in 2006. Districts richly endowed with hydrocarbons experienced large swings in the oil and gas grant. Public service delivery strongly responded to the general grant, but not to the oil and gas grant, suggesting that local governments consider the persistence of revenue shocks when adjusting lumpy public goods and services. The timing and composition of fiscal responses support this interpretation: the general grant stimulated a larger and more immediate expenditure response, especially for infrastructure investment. Other potential mechanisms fail to explain the results. Revenue persistence is an important, yet neglected, determinant of how public service delivery responds to revenue shocks.

The results are relevant for national fiscal policy. For example, increasing intergovernmental transfers during economic downturns could be more effective at stimulating the economy when the increase is permanent. The results also put shared natural resource revenue in a more positive light. Hydrocarbon-rich districts spend out of the permanent component of the oil and gas grant; volatile resource revenue need not lead to volatile local spending.

If local responses to revenue shocks depend on the shock's impact on lifetime fiscal resources, then both the initial size and the persistence of the shock should matter. This paper studies a context in which revenue shocks were similar in size, but differed in persistence. An interesting question for future work is whether responses differ according to the initial size of the shock, holding persistence fixed. Future research should also examine how local governments respond to different types of revenue shocks in contexts with significant local taxation, where governments have an additional margin of response—tax cuts.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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Advance Access Publication Date: 11 April 2025