THE ECONOMIC JOURNAL

The Economic Journal, 1–34 DOI: 10.1093/ej/uez009
Advance Access Publication Date: 16 May 2019

THE LONG-RUN EFFECTS OF OIL WEALTH ON DEVELOPMENT: EVIDENCE FROM PETROLEUM GEOLOGY*

Traviss Cassidy

We estimate the long-run effects of oil wealth on development by exploiting spatial variation in sedimentary basins—areas where petroleum can potentially form. Instrumental variables estimates indicate that oil production impedes democracy and fiscal capacity development, increases corruption, and raises GDP per capita without significantly harming the non-resource sectors of the economy. We find no evidence that oil production increases internal armed conflict, coup attempts, or political purges. In several specifications failure to account for endogeneity leads to substantial underestimation of the adverse effects of oil, suggesting that countries with higher-quality political institutions and greater fiscal capacity disproportionately select into oil production.

Does natural resource abundance promote or hinder economic and political development? Despite decades of research, the question remains largely unresolved.[1] Much of the disagreement owes to the difficulty of identifying exogenous variation in resource wealth.[2] Country-level resource exploration and extraction are endogenous to political, institutional, and economic conditions.[3] Recent contributions to the literature have exploited subnational data and short-term fluctuations

* Corresponding author: Traviss Cassidy, Department of Economics, Finance, and Legal Studies, University of Alabama, 361 Stadium Dr., Alston Hall 271, Tuscaloosa, AL 35487. Email: tmcassidy@cba.ua.edu

This paper was received on 3 October 2016 and accepted on 13 July 2018. The Editor was Kjell Salvanes.

I thank the editor, Kjell Salvanes, and two anonymous referees for many helpful comments. I also thank Alecia Cassidy, Ugo Troiano, and Dean Yang for their helpful comments and encouragement, as well as Sutirtha Bagchi, Hoyt Bleakley, Matias Cattaneo, Adam Cole, Micah DelVecchio, Mark Dincecco, Jim Hines, John Holden, Niels Johannesen, Lutz Kilian, Hamid Mohtadi, Michael Ross, Joel Slemrod, Dmitriy Stolyarov, Mike Waite, and participants of the Midwest Economic Association Annual Meeting; the Midwest International Economic Development Conference; the Michigan Tax Invitational; the University of Michigan's Informal Development Seminar; Health, History, Demography, and Development Seminar; Public Finance Lunch Seminar; and Political Economy Workshop for helpful comments. All mistakes are my own. A previous version of this article was titled, 'The Long-Run Effects of Oil Wealth on Democracy and Fiscal Capacity.'

[1] Early studies argued that resource wealth lowered economic growth via the Dutch Disease (Corden and Neary, 1982; Sachs and Warner, 1995, 1999, 2001), but recent studies call the Dutch Disease hypothesis into question, showing that oil discovery and production can cause positive spillovers for manufacturing and boost aggregate investment and employment (Michaels, 2011; Arezki *et al.*, 2016; Allcott and Keniston, 2017). Smith (2015) and Arezki *et al.* (2016) both present evidence that oil wealth raises GDP, using cross-country panel data. Influential early studies in the political science literature claimed that resource rents promoted authoritarianism (Ross, 2001; Jensen and Wantchekon, 2004). However, Herb (2005) and Haber and Menaldo (2011) argue that there is no robust relationship between oil rents and democracy. See, however, responses to the latter study by Andersen and Ross (2014) and Wiens *et al.* (2014). Alexeev and Conrad (2009) argue that the negative cross-sectional association between oil and the quality of institutions disappears after controlling for (instrumented) GDP. Brückner *et al.* (2012) present evidence that oil exports improve democratic institutions. For recent surveys of the resource curse literature, see Torvik (2009), Frankel (2010), van der Ploeg (2011), and Ross (2015). See Cust and Poelhekke (2015) for a survey of the subnational evidence for the resource curse.

[2] See, for example, the discussions in Brunnschweiler and Bulte (2008) and van der Ploeg and Poelhekke (2010).

[3] David and Wright (1997) argue that the United States became the world's premier mineral producer from 1870 to 1910 not because of a fortuitous mineral endowment relative to other countries, but because its superior technology and institutions allowed it to more efficiently extract resources. Bohn and Deacon (2000) find that democratic institutions and political stability positively affect investment in oil exploration. Cust and Harding (2017) show that when oil is potentially located on a national border, 95% more exploratory drilling occurs in the country with relatively better institutions.

in world resource prices in order to identify short-run causal effects of resource income.[4] However, several important outcomes, such as the political regime and fiscal capacity of the central government, require analysis at the national level. Furthermore, the interaction among resource wealth and economic and political variables may develop over long periods of time. Political and fiscal institutions develop and consolidate over many years—as do their effects.[5] In addition, both the 'greed' and 'grievance' motives for conflict (Collier and Hoeffler, 2004) can be deeply rooted in the presence of resource endowments. Therefore, the long-run effects of natural resources are of great interest. Understanding how natural resource wealth affects long-run development will inform not only domestic resource policy (e.g., royalties and drilling rights) but also federal transfer policy and foreign aid, as natural resource revenue and other forms of non-tax revenue are believed to have similar effects (e.g., Djankov *et al.*, 2008; Brollo *et al.*, 2013).

This article examines the long-run effects of oil wealth on development using a new identification strategy that exploits the geological characteristics of countries. Hydrocarbons—notably crude oil and natural gas—are produced by the heating and compression of organic matter buried within sedimentary basins. Our instrumental variables approach uses new data on the spatial distribution of sedimentary basins to isolate exogenous cross-country variation in oil wealth.[6]

Addressing endogeneity is crucial in this context because the sign of the bias of ordinary least squares is a priori ambiguous. If wealthier or more democratic countries attract greater private investment in resource exploration and production, perhaps due to their stronger property-rights protections, then the estimated effect of resource wealth on development will be biased upwards (Cust and Harding, 2017). On the other hand, if low-income or less democratic countries have more lax regulation of the resource sector or are governed by politicians who personally benefit from rapid extraction rates, then the estimate will be biased downwards (Robinson *et al.*, 2006).

Other studies have used instrumental variables (Brunnschweiler and Bulte, 2008; van der Ploeg and Poelhekke, 2010; Tsui, 2011; Borge *et al.*, 2015), price shocks (Brückner *et al.*, 2012; Dube and Vargas, 2013; Caselli and Tesei, 2016; Carreri and Dube, 2017; Andersen *et al.*, 2017), and giant oil field discoveries (Lei and Michaels, 2014; Smith, 2015; Arezki *et al.*, 2016) to estimate the causal effects of natural resource abundance. Panel models have the advantage of controlling for unit fixed effects but potentially present two disadvantages: they typically only recover short-run effects and they may be biased if institutions influence the timing of resource discoveries and production.

Consistent estimation of long-run effects requires a source of cross-country variation in resource wealth that is orthogonal to institutional quality and other important country characteristics. Previous cross-country studies have used initial subsoil assets as an instrument for resource wealth (e.g., van der Ploeg and Poelhekke, 2010; Tsui, 2011). However, these measures of *known* resource endowment could depend on exploration effort, which endogenously responds to economic and political conditions. We aim to improve upon this strategy by focusing on geological features that cannot respond to economic or political factors.

[4] Subnational studies include Vicente (2010), Michaels (2011), Litschig (2012), Monteiro and Ferraz (2012), Aragón and Rud (2013), Brollo *et al.* (2013), Caselli and Michaels (2013), Dube and Vargas (2013), Aragón and Rud (2016), Allcott and Keniston (2017), Carreri and Dube (2017), and Aragón *et al.* (2018). For empirical strategies that exploit price shocks, see Brückner *et al.* (2012), Dube and Vargas (2013), Caselli and Tesei (2016), Andersen *et al.* (2017), and Carreri and Dube (2017).

[5] See Besley and Persson (2011b) for a model of fiscal capacity as a stock variable, and see Persson and Tabellini (2009) on the implications of democratic capital.

[6] Bartik *et al.* (2017) use an index of geological suitability for hydrocarbons accessible by fracking to predict the prevalence of fracking at the U.S. county level.

The instrumental variables estimates indicate that an increase in average annual oil production from 1966 to 2008 significantly reduces the level of democracy in 2008 as well as the average level of democracy from 1966 to 2008. Increasing oil production also leads to more corruption and reduces average tax revenue as a share of GDP from 2000 to 2008. The corresponding OLS estimates understate the negative effects of oil, suggesting that countries with better political institutions and greater state capacity disproportionately select into oil production. The evidence on internal armed conflict, coup attempts, and purges is less conclusive. Finally, we find evidence that oil production raises GDP and does not significantly harm the non-resource sectors of the economy. The results are consistent with recent research showing that oil negatively impacts political institutions without leading to noticeably worse economic outcomes on average (Ross, 2012). The results are robust to controlling for region fixed effects and a wide variety of geographic covariates.

The potential weakness of our empirical strategy is that, even after controlling for geographic confounders, predetermined correlates of development may still be correlated with our instrument, owing to the lumpy distribution of sedimentary basins around the world. We take this concern very seriously and explore the sensitivity of our estimates to controlling for other predetermined characteristics. Out of nine important predetermined characteristics considered, only one—the percentage of the population that was Muslim in 1950—is strongly correlated with the instrument. Controlling for this variable attenuates the estimated effects of oil production on institutions (which remain negative), strengthens the estimated positive effects on GDP, and has little impact on the estimated effects on conflict and tax revenue. While the instrument is not perfect, placebo tests reassuringly show no significant correlation between sedimentary basins and democracy or population density in years when world oil production was minimal.

Several studies have argued that natural resources have heterogeneous effects which depend on country-specific factors, such as institutions.[7] Following this literature, we test for heterogeneous effects, finding that the negative long-run effects of oil wealth on democracy and tax revenue are concentrated in the subsample of countries with weak institutional constraints on executive decision-making from 1950 to 1965. Interestingly, countries with weak executive constraints from 1950 to 1965 benefited the most from oil in terms of income, probably reflecting the fact that lower-income countries have the highest potential GDP gains from oil (Smith, 2015). We view the evidence on heterogeneous effects as suggestive rather than causal, because initial institutions may be correlated with unobserved country characteristics which affect modern-day outcomes.

The results on heterogeneous effects of oil on democracy are most similar to those of Tsui (2011) and Caselli and Tesei (2016), who find that resource wealth causes non-democracies to become less democratic but has no effect on the political regime in democracies. Unlike those studies, however, we condition on initial rather than contemporary political institutions to (partially) alleviate concerns about the endogeneity of political institutions. Theory predicts that natural resource wealth will have heterogeneous effects on corruption and conflict depending on the quality of institutions (Bhattacharyya and Hodler, 2010; Besley and Persson, 2011a). However, our empirical results provide little support for these predictions. Our finding that oil

[7] For the argument that the effect of natural resources on income depends on the quality of institutions, see, e.g., Lane and Tornell (1996), Tornell and Lane (1999), Mehlum *et al.* (2006), Robinson *et al.* (2006), and Boschini *et al.* (2007). Other studies emphasise that resource rents influence politician behaviour in different ways depending on preexisting political institutions; see, e.g., Aslaksen and Torvik (2006), Bhattacharyya and Hodler (2010), Tsui (2011), Andersen and Aslaksen (2013), and Caselli and Tesei (2016).

wealth reduces fiscal capacity is related to the theoretical predictions of Besley and Persson (2009a, 2010, 2011b) and is consistent with previous empirical studies (Jensen, 2011; Cárdenas *et al.*, 2011). To our knowledge this is the first article to empirically test how the effect of oil on tax revenue depends on initial institutions. Recent research on fiscal capacity and natural resources emphasises the role of the marginal value of public funds (Besley and Persson, 2011b; Jensen, 2011), however our results are more consistent with a 'rentier state' model (Mahdavy, 1970; Ross, 2001) which focuses on an autocrat's ability to use public finance to produce a quiescent population.

The article proceeds as follows. Section 1 provides background information on petroleum geology and describes the construction of the instrumental variable. Section 2 describes the data, Section 3 describes the identification strategy, Section 4 presents the main results, Section 5 discusses the evidence of heterogeneous effects, and Section 6 concludes.

1. Petroleum Geology and Instrumental Variables

1.1. *Formation of Hydrocarbons*

This section provides a brief overview of petroleum geology and defines the instrumental variable. There are five geological prerequisites for oil reservoir formation. First, there must be a *source rock*, a sedimentary rock rich in organic material deposited by algae and zooplankton millions of years ago. Source rocks form within a sedimentary basin—a region of the earth's crust characterised by prolonged subsidence, in which tectonic movements cause the surface area to sink and sediments from surrounding regions to fill in the depressed area (Southard, 2007). Extreme heat and pressure convert the buried organic material into hydrocarbons, notably natural gas and crude oil (Kvenvolden, 2006). Second, a *migration pathway* must connect the source rock to an area where the reservoir will form. For example, this migration pathway may be a fracture caused by seismic activity. Third, a *reservoir rock* must be located along the migration pathway. This highly porous and permeable rock, usually a sandstone or carbonate, collects and absorbs the migrating hydrocarbons (Chen, 2009). Fourth, a highly impermeable *caprock* must seal the hydrocarbons within the reservoir rock, preventing the hydrocarbons from leaking to the surface and dissipating. The final requirement is the presence of what is known as a *trap*, which concentrates the hydrocarbons in specific locations where they can be exploited (Allen and Allen, 2005).[8]

1.2. *Sedimentary Basin Classification*

The Fugro Robertson, Ltd. (2013) Tellus GIS database provides the name, location, description, and geological classification of every onshore and offshore sedimentary basin. See Figure 1 for a map of the basins. Geologists rely on three general techniques to collect data on sedimentary basins: (*i*) surface mapping; (*ii*) core sampling; and (*iii*) subsurface geophysics such as seismic profiling (Southard, 2007). Aerial photographs provide a base map of the surface, and survey work on the ground complements the photographs in the construction of surface maps (Marjoribanks, 2010, ch. 2). Core sampling involves the removal of a cylindrical piece of subsurface material using a drill. Geologists use seismic air guns to initiate seismic waves underground. They use

[8] I am indebted to Mike Waite, a former geophysicist at Chevron, for explaining this process to me.

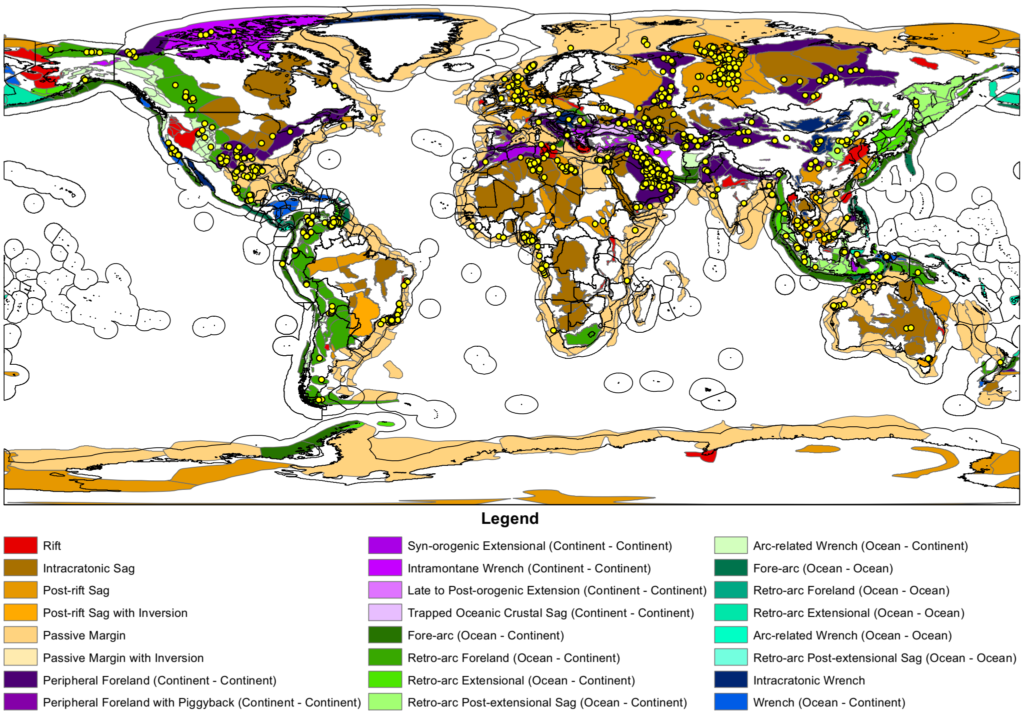

Fig. 1. *Sedimentary Basins and Giant Oil and Gas Fields.*
Notes: Coloured areas represent sedimentary basins, and yellow dots represent giant oil and gas fields. The GIS data on sedimentary basins come from Fugro Robertson, Ltd. (2013), and the GIS data on giant oil and gas fields come from Horn (2004).

seismic detectors to record the arrival of the waves at different points under the surface. Geologists then use the data collected by the seismic detectors to draw seismic profiles (Britannica, 2015).

Fugro Robertson, Ltd. (2013) divides sedimentary basins into 24 classification groups according to their plate-tectonic environment, primary mechanism of subsidence, and other details regarding the nature of faulting and subsidence and the relative location of the basin on the tectonic plate. Each basin forms in one of three general plate-tectonic environments. The first is a divergent environment, in which adjacent tectonic plates pull away from each other. The second is a convergent environment, in which tectonic plates collide head on, causing one plate to pass underneath the other in a process known as subduction. Convergent environments are further divided according to whether they feature continental plates, oceanic plates, or both. The third is a wrench environment, in which adjacent tectonic plates move in opposite, parallel directions, rubbing alongside each other. The mechanism of subsidence is mechanical (aka 'tectonic'), thermal, or thermo-mechanical. Mechanical subsidence is caused by the movement of tectonic plates due to faulting. Thermal subsidence is caused by the thickening of the earth's crust due to cooling of the underlying mantle, which causes the crust to become denser than its surroundings. Thermo-mechanical subsidence is caused by some combination of the aforementioned mechanical and thermal processes.

Table B.3 in Appendix B lists the name, classification code, and plate-tectonic environment (‘sub-regime’) of each of the 24 Fugro Robertson basin types. The classification code consists of two or three elements. The first element indicates the general plate-tectonic environment. It takes the value of ‘D’ for ‘Divergent,’ ‘C’ for ‘Convergent,’ and ‘W’ for ‘Wrench.’ For codes consisting of three elements, the second element indicates the involvement of continental tectonic plates, oceanic tectonic plates, or both. A second-element value of 1 indicates the presence of two continental plates, 2 indicates the presence of one continental and one oceanic plate, and 3 indicates the presence of two oceanic plates. For example, a basin with code starting with ‘C.1’ exists in an environment in which two continental plates are converging, while a basin with code starting with ‘C.2’ exists in an environment in which a continental plate and an oceanic plate are converging. For codes consisting of three elements, the third element indicates the location of the basin relative to the plates and areas of faulting. For example, codes ending in ‘F’ indicate foreland basins, which are formed adjacent to a mountain range caused by the subduction of two plates. The code ‘C.1.F’ corresponds to a foreland basin formed in the context of two continental plates colliding, while ‘C.3.F’ corresponds to a foreland basin formed from the collision of two oceanic plates. To give another example, codes ending in ‘E’ indicate extensional basins, which are formed in areas characterised by the stretching of the crust or lithosphere. For codes consisting of only two elements, the second element indicates the location of the basin relative to the plates and areas of faulting. In sum, the final element of the code indicates local characteristics of the basin formation, while the preceding elements of the code indicate global characteristics of the plate-tectonic environment.

One common basin type is C.1.F or ‘peripheral foreland basin,’ which exists in a convergent plate-tectonic environment and is characterised by a mechanical subsidence mechanism. Peripheral foreland basins are found adjacent to mountain ranges formed by the subduction of two continental plates. Large peripheral foreland basins exist in the Persian Gulf and Arabian Peninsula, adjacent to the Zagros mountains in Iran. Another common basin type is D.4 or ‘passive margin basin,’ which forms within a divergent plate-tectonic environment and features a thermal subsidence mechanism. Passive margins occupy areas where an oceanic plate and a continental plate have diverged, such as the eastern coastlines of the Americas and all coastlines of Africa, among other places.

1.3. *Instrument Construction*

The next task is to specify the candidate instrument sets. The composition of each instrument set depends on two choices. The first choice is how to aggregate the 24 Fugro Robertson basin categories into a smaller number of exhaustive and mutually exclusive basin categories. Aggregating the basin categories is reasonable a priori as many of the disaggregated categories account for a very small fraction of the earth’s surface area and thus are unlikely to have much predictive power. The second choice is which aggregate basin categories to include in the set of instruments. Section 3 describes the instrument selection procedure.

We pursue two approaches to basin aggregation. The first is based on the global characteristics of the basin environment—the general plate-tectonic environment and primary mechanism of subsidence. Fugro Robertson, Ltd. (2013) provides a grouping that assigns each basin type to one of five plate-tectonic environments—divergent, convergent continent-continent, convergent ocean-continent, convergent ocean-ocean, and wrench—and one of three subsidence categories—

mechanical, thermo-mechanical, and thermal. This method results in eight groups of basin types that actually exist, as shown in Table B.4 in Appendix B.[9]

The second approach is based on the local characteristics of the basin as indicated by the final element of the Fugro Robertson, Ltd. (2013) code. As already mentioned, the local characteristics involve the location of the basin relative to the plates and areas of faulting. This approach produces ten basin groups, as shown in Table B.5 in Appendix. Appendix D provides maps of the aggregated basin categories.

We assign values of each aggregate basin type to countries by calculating the log of the sovereign area (in square kilometers) per 1,000 inhabitants in 1960 covered by the basin.[10] Sovereign territory is inclusive of maritime boundaries. Data on country land borders are from Erle and Gilles (2013), and data on maritime borders are from the Flanders Marine Institute (2013).[11]

2. Other Data Sources

This section describes the other data sources used in the empirical analysis. The sample period is 1966–2008.[12] Data on oil production, our primary measure of oil wealth, come from Ross (2013), who cleaned and compiled data from the US Geological Survey, the US Energy Information Administration's International Energy Statistics, the World Bank, and the BP Statistical Review. This data set covers 172 countries, of which 96 have produced oil, from 1932 to 2011.[13] Oil production is measured as the log of average annual metric tons per 1,000 inhabitants from 1966 to 2008.

To ensure that the basin instrument satisfies the exclusion restriction, we include controls for geographic features that are possibly correlated with both sedimentary basins and economic and political outcomes. The basin variable will naturally be correlated with the physical size of the country, so we include a control for total land area calculated from GIS data. Gallup *et al.* (1998) show that countries with more land in the tropics and less access to waterways tended to grow more slowly over their sample period. We use their data to construct a measure of land area in the tropics. Data on country coastline are obtained from the CIA World Factbook (CIA, 2015). We also use data on the area of mountainous land from Fearon and Laitin (2003), who argue that mountainous terrain is associated with higher levels of insurgency and civil war. Finally, we control for soil quality, which could influence development directly through its effect on agricultural productivity, or indirectly through the division of labour and the evolution of gender norms (Alesina *et al.*, 2013). We use the FAO's Global Agro-Ecological Zones (GAEZ)

[9] Basins with convergent ocean-ocean tectonics and thermal subsidence covered only 1,331 square kilometers of sovereign area among countries in the sample, which is several orders of magnitude less than any other basin group defined by the tectonic environment and subsidence mechanism. These basins exists in essentially just one country included in the sample. (St. Kitts and Nevis contains 1,329 square kilometers of this basin type, while Venezuela contains two square kilometers.) We therefore combine these basins with those with convergent ocean-ocean tectonics and mechanical subsidence.

[10] All geographic variables are normalised by population in 1960, prior to the sample period, because population may be endogenous to oil production through changes in migration (Michaels, 2011) or fertility (Ross, 2008).

[11] All geographic calculations use the Cylindrical Equal Area projected coordinate system, which preserves area measure.

[12] The sample ends in 2008 to avoid the depths of the Great Recession.

[13] An advantage of this data set is that it also includes information on oil exports as well as natural gas production and exports. Natural gas often accumulates near crude oil reservoirs, so the sedimentary basin instrument also predicts natural gas endowment. The empirical analysis focuses on oil production to facilitate comparison to past studies, however the results are very similar when the explanatory variable is oil and gas production.

database (Fischer *et al.*, 2002) to calculate each country's land area containing 'good' soil.[14] Soil quality depends on nutrient availability, nutrient retention capacity, rooting conditions, oxygen availability, presence of excess salts, toxicity, and workability.

As with the *Basin* variables, all geographic controls measuring surface area are expressed as the log of the surface area (in square kilometers) per 1,000 inhabitants in 1960. The coastline variable is expressed as the log of the coastline (in kilometers) per 1,000 inhabitants in 1960.[15] Data on population come from Maddison (2013).

We measure democracy using the standard POLITY2 index from the Polity IV database (Marshall and Gurr, 2014), which depends on qualities of executive recruitment, constraints on executive authority, and political competition. The index takes integer values from −10 to 10. POLITY2 codes cases of foreign 'interruption' as missing and cases of 'interregnum,' or anarchy, as zero. Furthermore, the POLITY2 score is prorated starting from zero during periods of transition following interruption or interregnum. This can give the false impression that, say, a period of anarchy in an autocratic country represents a movement towards democracy. We follow the recent literature (Brückner and Ciccone, 2011; Caselli and Tesei, 2016) and code periods of interregnum as missing. Furthermore, we prorate the score during periods of transition starting from the most recent non-missing POLITY2 score. We normalise POLITY2 to take values between zero and one, with one being the most democratic. Two different democracy outcomes are used: (*i*) democracy in 2008; and (*ii*) average democracy from 1966 to 2008 in years in which the country was independent. The measure of executive constraints is the *XCONST* variable from the Polity IV database, also normalised to take values between zero and one. This variable measures the 'extent of institutionalised constraints on the decision-making powers of chief executives,' where the constraints can be imposed by any accountability group (Marshall and Gurr, 2014).

Data on corruption and conflict come from several sources. Our corruption measure comes from the Political Risk Services (PRS) and focuses on corruption within the political system.[16] The index ranges from zero to six, with higher numbers indicating less corruption. We recode the corruption variable to be six minus the PRS index, so that the new variable ranges from zero to six, with higher numbers indicating *more* corruption. We measure corruption in 2008. Three variables capture different aspects of political conflict. First, we use the UCDP/PRIO data set (Gleditsch *et al.*, 2002) to calculate the number of internal or internationalised internal armed conflicts per year in which the country was independent from 1966 to 2008. The data set counts only conflicts in which the government is a party and which involve at least 25 battle-related deaths. Second, we use the Polity IV database (Marshall and Marshall, 2016) to count the number of (failed or successful) coup attempts per year in which the country was independent from 1966

[14] The GAEZ database divides zones according to the moisture regime (dry, moist, sub-humid, or humid) and soil quality (good, moderate, or poor). We define 'good soil' as soil with 'good' quality falling in any of the moisture regimes. We use the most recent version of the database available, version 3.0.

[15] Due to the presence of zero values, each 'log' transformation in the empirical analysis is in fact a differentiable and monotonic transformation $h(w) = \log(w)$ for $w > w_0$ and $h(w) = \log(w_0) - 1 + w/w_0$ for $w \leq w_0$. In practice w_0 is set equal to the minimum positive value of the random variable observed in the sample.

[16] According to the Political Risk Services, the measure accounts for excessive patronage, nepotism, job reservations, 'favour-for-favours', secret party funding, and suspiciously close ties between politics and business.

to 2008.[17] Finally, we use the data set by Banks and Wilson (2016) to calculate the number of purges per year in which the country was independent from 1966 to 2008.[18]

Revenue data come from the ICTD Government Revenue Dataset, compiled by Prichard *et al.* (2014) on behalf of the International Centre for Tax and Development (ICTD). The series covers the period 1980–2013 for 204 countries, although a nontrivial amount of data are missing, particularly in earlier years. Previously available cross-country tax and revenue data sets were plagued by many missing observations, inconsistent accounting definitions, and inadequate decomposition of tax and revenue by source, among other problems. In particular, accounting treatment of natural resource revenue is notoriously variable across countries, making cross-country analysis difficult. The authors of the ICTD data set combined and manually cleaned data from several international databases, improving data coverage and consistency. For the purposes of this article, the ICTD data set is particularly valuable because it is based on a standardised approach to revenue from natural resources.[19] We focus on two government revenue outcomes: total revenue and tax revenue. All revenue variables exclude social contributions. Total revenue is the sum of all tax and non-tax revenue. Crucially, total revenue includes both resource tax revenue (e.g., corporate taxes paid by private natural-resource firms) and non-tax resource revenue (e.g., royalties paid by private companies and profits from state-owned natural-resource companies). Following the ICTD classification, tax revenue is defined as the sum of all non-resource tax revenue.[20] To maximise sample size and smooth out fluctuations due to business cycles, revenue variables are measured as the log of their average share of GDP from 2000 to 2008.

Fiscal capacity—the state's maximum administrative ability to collect tax revenue—is unobservable. Following the empirical fiscal-capacity literature (Besley and Persson, 2011b; Jensen, 2011; Cárdenas *et al.*, 2011), we use tax revenue as a proxy for fiscal capacity. Tax revenue collection requires investment in tax administration and entails higher information and enforcement costs than other forms of revenue, such as natural-resource royalties (Besley and Persson, 2011b). We thus expect variation in tax revenue to largely reflect variation in the state's administrative capacity to collect taxes.

We measure the log of GDP per capita in 2008 (constant 2011 international dollars) using the World Bank's World Development Indicators.

We construct subcomponents of GDP per capita—non-oil GDP per capita, non-oil/gas GDP per capita, non-resource GDP per capita, and manufacturing GDP per capita—using GDP per capita and GDP share data from the World Bank's World Development Indicators. For example, non-resource GDP per capita is constructed by multiplying GDP per capita by one minus the share of natural resource rents (value of production less production costs) in GDP. Similarly, manufacturing GDP is constructed by multiplying GDP per capita by the share of manufacturing

[17] A coup is defined as a 'forceful seizure of executive authority and office by a dissident/opposition faction within the country's ruling or political elites that results in a substantial change in the executive leadership and the policies of the prior regime (although not necessarily in the nature of regime authority or mode of governance)' (Marshall and Marshall, 2016).

[18] A purge is defined as 'any systematic elimination by jailing or execution of political opposition within the ranks of the regime or the opposition' (Banks and Wilson, 2016).

[19] Despite the extensive efforts made to construct a reliable data set, some problems remain due to the limitations of primary sources. In some cases the data appear not credible, and in other cases it is impossible to isolate natural resource revenue from other types of revenue. These problematic observations are flagged in the data set and are excluded from the empirical analysis.

[20] This definition is conceptually appealing, as we are interested in how resource wealth affects investments in state capacity. Taxing a few large resource firms requires much less administrative capacity than, say, enforcing a personal income tax.

value added in GDP. Subcomponents of GDP per capita are also measured in 2008 and in log scale.

3. Identification Strategy

3.1. *Estimating Equations*

This section describes the identification strategy. We estimate the effect of oil wealth on country outcomes using sedimentary basin areas as instruments. The estimating equations are:

$$y_{cr} = \beta Oil_{cr} + \boldsymbol{\delta}'\boldsymbol{x}_{cr} + \alpha_r + \varepsilon_{cr}$$

$$Oil_{cr} = \boldsymbol{\pi}'\boldsymbol{Basin}_{cr} + \boldsymbol{\phi}'\boldsymbol{x}_{cr} + \lambda_r + \xi_{cr},$$

where c indexes countries and r indexes regions. The variable y represents a country-level outcome, such as level of democracy or tax revenue. *Oil* is a measure of average annual oil production per capita over the period of interest.[21]

$\boldsymbol{Basin}$ is a possibly multidimensional vector of sedimentary basin measures. The main threat to identification is the possibility that some geographic features omitted from the model are correlated with elements of $\boldsymbol{Basin}$ and development outcomes. We address this concern by controlling for several geographic characteristics that have been shown to be correlated with economic and political development.[22] The vector $\boldsymbol{x}$ comprises total land area, mountainous area, tropical area, good-soil area, and length of coastline. The parameter α_r represents an unobserved region-specific determinant of development.[23] We eliminate the potential bias produced by α_r by including region indicator variables.

The first identifying assumption of the model is that, conditional on the set of geographic covariates, $\boldsymbol{Basin}$ is independent of potential development outcomes and potential selection into oil discovery. Informally, this assumption says that $\boldsymbol{Basin}$ does not have a direct effect on development outside the channel of oil discovery, and that basin prevalence is not systematically related to country exploration technology or any other propensity for discovery, after controlling for geographic covariates. Given that we control for geographic features that are both plausibly correlated with $\boldsymbol{Basin}$ and may affect development outcomes, the first assumption is likely to hold. The second identifying assumption is that increasing the prevalence of sedimentary basins would never cause a country to produce less oil, for example because of lower exploration effort. This is the familiar monotonicity assumption (Imbens and Angrist, 1994; Angrist and Imbens, 1995). It is likely to hold in all but the most implausible scenarios. The final identifying assumption is that $\boldsymbol{Basin}$ and *Oil* have non-zero correlation. If these assumptions hold, then the two-stage least squares estimand identifies the average causal effect of *Oil* on y in countries where a marginal change in basin area induces a change in *Oil* (Angrist and Imbens, 1995).

Our identification strategy is related to studies which use a measure of the initial resource endowment as an instrument for resource wealth over a specific time period (van der Ploeg and Poelhekke, 2010; Tsui, 2011). The resource endowment of a country is typically measured as the sum of cumulative resource discoveries and a geological estimate of undiscovered subsoil

[21] We focus on the effect of oil production, because the results are very similar for other measures of oil abundance, such as oil discovery, oil reserves, oil endowment, and oil and gas production. These results are available upon request.

[22] See Gallup *et al.* (1998) for geographic correlates of economic development, and see Fearon and Laitin (2003) for the correlation between mountainous terrain and insurgency.

[23] The regions are Africa, Europe/Northern America/Oceania, Asia, and Latin America/Caribbean.

resources. The disadvantage of this measure is that known resource endowments represent a non-random sample of true resource endowments. Resource discovery depends on exploration effort, which is likely to be correlated with country characteristics such as property rights institutions (Bohn and Deacon, 2000; Cust and Harding, 2017; Arezki *et al.,* 2019). Hence the difference between true endowment and known endowment is a function of country characteristics that influence development. In contrast, sedimentary basins cannot respond to country-level political or economic conditions.[24] The next section discusses robustness checks comparing estimates using the basin instrument to estimates using the oil endowment instrument from Tsui (2011).

In contrast to the empirical strategy presented here, researchers commonly use commodity price shocks, either directly (Caselli and Tesei, 2016) or interacted with a time-invariant measure of resource abundance (Brückner *et al.*, 2012; Dube and Vargas, 2013; Andersen *et al.*, 2017; Carreri and Dube, 2017), as a source of exogenous variation in resource wealth. The strategy appears very credible when applied to subnational data. However, in cross-country studies, the approach raises two concerns. First, the commodity price may not be exogenous to all countries. Producers with significant market share, such as members of OPEC, may adjust production to manipulate prices in response to changing global or domestic economic conditions. This concern is alleviated by dropping large producers from the sample, but at the expense of external validity. Second, the time-invariant measure of resource abundance, usually calculated in an initial period or averaged over several periods, is endogenous in cross-country regressions for reasons already mentioned. Identification issues aside, the price-shock strategy is suited for estimating the short-run effects of natural resources, whereas this article is focused on long-run effects.

3.2. *Instrument Selection*

No definitive ranking of sedimentary basin types by hydrocarbon potential exists in the petroleum geology literature.[25] Therefore, we pursue a data-driven procedure for instrument selection. In selecting a set of valid instrumental variables, the researcher generally faces a trade-off between bias and efficiency. Starting from a baseline set of valid instruments, adding additional valid instruments potentially improves asymptotic efficiency (Wooldridge, 2010, pp. 229–230). However, the finite-sample bias of 2SLS generally grows with the number of instruments used (Donald and Newey, 2001), posing a particularly severe problem when the added instruments are weak (Bound *et al.*, 1995). Furthermore, the presence of weak instruments can render inference based on the standard normal approximations invalid (Staiger and Stock, 1997; Stock and Yogo, 2003). In light of these concerns, we search for the (possibly singleton) set of instruments that maximises the first-stage F statistic, rather than including all possible instruments. In this way we prioritise minimizing bias and making valid inferences over maximizing efficiency. Specifically, for each of the two basin aggregation methods described in Section 1, we estimate a first-stage regression for every possible subset of basins. For each regression, we calculate the Kleibergen and Paap (2006) robust *rk* Wald F statistic for the excluded instruments.

The main results will be based on the set of instruments that maximises this F statistic, though we will also report results using the F statistic-maximizing instrument set for each set

[24] In principle, there could be some relationship between the collection of data on sedimentary basins and unobserved determinants of oil production or country outcomes. In subsection 4.5 we discuss why this is unlikely to be a source of bias. Subsection 4.5 also considers the possibility that predetermined correlates of development might be correlated with sedimentary basins.

[25] Kingston *et al.* (1983) admit that ‘there is no magic formula which can separate sedimentary basins into oil-and-gas-prone versus barren’.

size. It is important to note that the instrument selection procedure does not invalidate second-stage inference. The reason is that model selection is performed at the service of predicting oil production, not second-stage outcomes.

4. Empirical Results

4.1. *Descriptive Statistics*

Table B.7 in Appendix B provides general summary statistics.[26] Average democracy in 2008 (0.69) greatly exceeds average democracy in 1966 (0.44), reflecting a general trend towards democratisation. Table 1 summarises variables separately according to whether the country produced any oil from 1966 to 2008. In the sample period, 96 countries had positive oil production and 76 had zero production. In 1966 average democracy in non-oil countries was three percentage points higher than average democracy in oil countries. By 2008 this difference had increased to seven percentage points, though neither difference is statistically significant ($p = 0.677$, $p = 0.182$). Corruption levels and the number of coup attempts were similar in the two groups, however oil countries had more internal conflict and purges ($p = 0.068$, $p = 0.067$). While oil countries had greater total revenue as a proportion of GDP from 2000 to 2008 compared to non-oil countries ($p < 0.001$), total non-resource tax revenue was lower in oil countries than in non-oil countries ($p = 0.179$). Oil countries tended to be richer than non-oil countries, both in 1966 ($p < 0.001$) and in 2008 ($p < 0.001$). Average executive constraints from 1950 to 1965 were slightly stronger in oil countries, although the difference is statistically significant ($p = 0.594$). Unsurprisingly, all sedimentary basin measures are higher for oil countries, with the exception of the relatively rare convergent ocean-ocean basins, though the difference in average values is statistically significant. Average land area, coastline, mountainous area, and good-soil area are statistically indistinguishable in the two groups, although oil countries contain less tropical area on average ($p = 0.029$). It is important to note that the categories mask considerable heterogeneity in production levels, as the distribution of oil production is highly skewed.

4.2. *First-Stage Results*

Table 2 presents the first-stage results for the effect of the basin variables on oil production. To conserve space, the table reports results for the three top-performing (in terms of first-stage F statistic) instrument sets for each approach to basin aggregation—global characteristics or local characteristics. Tables B.8 and B.9 in Appendix B present the first-stage results for all 18 instrument sets considered. Each column in Table 2 reports the Kleibergen and Paap (2006) robust *rk* Wald F statistic, which tests for weak identification and is robust to heteroskedasticity. In each table, column N reports the results using the instrument set of size N that maximises the first-stage F statistic.

The first group of instruments in Table 2 are aggregate categories based on global characteristics: the general plate-tectonic environment and primary mechanism of subsidence. The singleton instrument set that maximises this F statistic is the basin type with convergent continent-continent tectonics and mechanical subsidence, which achieves an F statistic of 25.3. The aforementioned basin type, together with the basin type with convergent ocean-continent tectonics and thermal subsidence, constitute the two-instrument set that maximises the F statistic, achieving an F statis-

[26] Data and replication code are available in the supporting material posted on the publisher's website.

Table 1. *Summary Statistics by Oil Presence.*

	Oil countries	Non-oil countries	Difference	*p*-value
Democracy, 2008	0.66	0.73	− 0.07	0.182
Democracy, 1966	0.43	0.46	− 0.03	0.677
Avg. democracy, 1966–2008	0.52	0.54	− 0.02	0.683
Corruption, 2008	3.38	3.52	− 0.14	0.508
Internal conflicts per year, 1966–2008	0.27	0.13	0.13*	0.068
Coup attempts per year, 1966–2008	0.05	0.07	− 0.01	0.330
Purges per year, 1966–2008	0.08	0.04	0.04*	0.067
Total revenue, 2000–2008 (log avg.)	− 1.41	− 1.68	0.27***	0.000
Tax revenue, 2000–2008 (log avg.)	− 2.04	− 1.89	− 0.14	0.179
GDP, 2008 (log p.c.)	9.46	8.58	0.88***	0.000
GDP, 1966 (log p.c.)	8.01	7.24	0.77***	0.000
Non-oil GDP, 2008 (log p.c.)	9.34	9.11	0.23	0.300
Non-oil/gas GDP, 2008 (log p.c.)	9.36	9.11	0.25	0.256
Non-resource GDP, 2008 (log p.c.)	9.27	8.51	0.76***	0.000
Manufacturing GDP, 2008 (log p.c.)	7.39	6.51	0.88***	0.000
Executive constraints, 1950–1965	0.48	0.45	0.04	0.594
Oil production, 1966–2008 (log avg. p.c.)	− 1.60	− 9.03	7.42***	0.000
Oil endowment (log p.c.)	− 8.78	− 11.93	3.15***	0.000
Convergent C-C mechanical area (log p.c.)	− 7.37	− 9.61	2.23***	0.000
Convergent O-C thermal area (log p.c.)	− 8.74	− 8.99	0.25**	0.021
Convergent O-C mechanical area (log p.c.)	− 8.81	− 10.64	1.83***	0.001
Convergent O-O area (log p.c.)	− 9.92	− 9.63	− 0.29	0.428
Divergent thermal area (log p.c.)	− 5.90	− 7.29	1.38*	0.066
Wrench mechanical area (log p.c.)	− 12.82	− 13.80	0.97*	0.084
Divergent mechanical area (log p.c.)	− 10.43	− 10.89	0.46	0.264
Convergent C-C thermo-mechanical area (log p.c.)	− 8.40	− 8.68	0.28**	0.047
Foreland area (log p.c.)	− 5.96	− 8.47	2.51***	0.000
Intracratonic sag area (log p.c.)	− 10.05	− 12.21	2.16***	0.007
Passive margin area (log p.c.)	− 8.26	− 9.45	1.19	0.136
Convergent sag area (log p.c.)	− 14.39	− 15.98	1.59***	0.001
Post-rift sag area (log p.c.)	− 9.57	− 10.97	1.40**	0.010
Wrench area (log p.c.)	− 12.82	− 13.80	0.97*	0.084
Extensional area (log p.c.)	− 9.82	− 9.99	0.17	0.589
Convergent wrench area (log p.c.)	− 11.47	− 11.93	0.46	0.355
Fore-arc area (log p.c.)	− 10.03	− 10.75	0.72	0.141
Rift area (log p.c.)	− 10.43	− 10.89	0.46	0.264
Land area (log p.c.)	− 3.09	− 3.41	0.32	0.185
Coastline (log p.c.)	− 9.22	− 9.51	0.29	0.528
Mountainous area (log p.c.)	− 6.23	− 6.92	0.69	0.132
Tropical area (log p.c.)	− 6.77	− 5.49	− 1.28**	0.030
Good soil area (log p.c.)	− 6.70	− 6.91	0.21	0.599
Observations	96	76		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tic of 17.6. Inspection of Table B.8 in Appendix B reveals that, with one exception, adding an additional instrument reduces the F statistic. When every instrument is included, the F statistic equals 9.4.

The second group of instruments in Table 2 are aggregate categories based on the final element of the Fugro Tellus code, which indicates local characteristics of the depositional environment. The singleton instrument set that maximises the first-stage F statistic is the foreland basin type, which achieves an F statistic of 16.4. Foreland basins and intracratonic sag basins constitute the two-instrument set that maximises the first-stage F statistic, achieving an F statistic of 17. From columns 2–10 in Table B.9 in Appendix B, the F statistic declines monotonically in the number of instruments included, equaling only 6.6 when every instrument is included.

Table 2. *First-Stage Estimates for Optimal Sets of Basin Instruments.*

	Log avg. oil production per capita, 1966–2008					
	(1)	(2)	(3)	(4)	(5)	(6)
Global characteristics						
Convergent C-C mechanical	0.599***	0.592***	0.589***			
	(0.119)	(0.119)	(0.124)			
Convergent O-C thermal		0.589***				
		(0.175)				
Convergent O-C mechanical			0.359***			
			(0.084)			
Convergent O-O			− 0.362**			
			(0.139)			
Local Characteristics						
Foreland				0.576***	0.608***	0.613***
				(0.142)	(0.143)	(0.139)
Intracratonic sag					0.213***	0.209***
					(0.069)	(0.068)
Passive margin						0.091
						(0.076)
Observations	157	157	157	157	157	157
R^2	0.318	0.327	0.394	0.315	0.357	0.364
F statistic	25.3	17.6	18.9	16.4	17.0	14.4

Notes: All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Comparing the results across all instrument sets, the instrument set that maximises the *F* statistic is the singleton basin type with convergent continent-continent tectonics and mechanical subsidence. The baseline second-stage results will be based on this instrument set, though we report results using the other instrument sets in Appendix C. The optimal instrument set's *F* statistic of 25.3 indicates that strong-instrument asymptotic theory applies. Nonetheless, to be conservative we also report 95% Anderson and Rubin (1949) confidence intervals for the coefficient on oil. Unlike the usual Wald test, the Anderson-Rubin test has correct size in the presence of weak instruments.

4.3. *Second-Stage Results*

Tables 3 and 4 present the main second-stage results. In each table, Panel A presents the OLS estimates and Panel B presents the IV estimates. Below the IV estimates in Panel B, we report the *p*-value to a test of whether oil production is endogenous. The endogeneity test is the Hansen (1982) overidentification test of the null hypothesis that oil production is exogenous. The test is valid under the assumption that *Basin* is exogenous.[27]

4.3.1. *Political resource curse*

Table 3 presents tests of the political resource curse hypothesis. The regressions presented in the first two columns provide strong evidence that oil wealth impedes democracy. The IV estimates indicate that a 1% increase in average annual oil production per capita from 1966 to 2008 reduces the level of democracy in 2008 by 0.038. The same increase in oil production reduces average

[27] The test is essentially a heteroskedasticity-robust version of the usual Durbin-Wu-Hausman test of the difference between OLS and IV.

Table 3. *Testing for a Political Resource Curse.*

	Democracy 2008	Avg. democracy 1966–2008	Corruption 2008	Internal conflict 1966–2008	Coup attempts 1966–2008	Purges 1966–2008	Total revenue 2000–2008	Tax revenue 2000–2008
	Panel A: Ordinary Least Squares							
Oil production	– 0.019***	– 0.014***	0.032	0.012**	0.001	0.003	0.032***	– 0.044***
	(0.005)	(0.004)	(0.023)	(0.006)	(0.002)	(0.002)	(0.007)	(0.012)
Observations	157	160	136	172	160	172	165	167
R^2	0.441	0.536	0.334	0.204	0.203	0.093	0.463	0.471
	Panel B: Two-Stage Least Squares							
Oil production	– 0.038**	– 0.039***	0.136**	0.007	0.002	– 0.001	0.021	– 0.163***
	(0.017)	(0.015)	(0.060)	(0.018)	(0.003)	(0.005)	(0.016)	(0.039)
Observations	157	160	136	172	160	172	165	167
F statistic	25.3	26.7	23.2	31.4	26.7	31.4	29.3	27.3
A-R 95% CI	[−0.081, −0.008]	[−00.077, −0.014]	[0.027, 0.280]	[−0.030, 0.045]	[−0.004, 0.009]	[−0.011, 0.010]	[−0.015, 0.053]	[−0.268, −0.100]
Oil exog.	0.209	0.063	0.053	0.780	0.578	0.445	0.446	0.000

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. The IV specifications use *Basin* as an instrument for oil production. The *F* statistic is the Kleibergen and Paap (2006) *rk* statistic, which tests for weak identification and is robust to heteroskedasticity. The A-R 95% confidence interval is based on the Anderson and Rubin (1949) χ^2 test of the null hypothesis that the coefficients on the endogenous variables in the structural equation are jointly equal to zero. The A-R test is robust to the presence of weak instruments. The *p*-value of the test of the endogeneity of oil wealth is from the Hansen (1982) overidentification test of the null hypothesis that oil wealth is exogenous. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. *Testing for an Economic Resource Course.*

	GDP, 2008	Non-oil GDP, 2008	Non-oil/gas GDP, 2008	Non-resource GDP, 2008	Manufacturing GDP, 2008
	Panel A: Ordinary Least Squares				
Oil production	0.092***	0.043**	0.040**	0.071***	0.076***
	(0.015)	(0.017)	(0.017)	(0.016)	(0.021)
Observations	166	132	129	166	145
R^2	0.661	0.623	0.608	0.657	0.599
	Panel B: Two-Stage Least Squares				
Oil production	0.074*	0.045	0.039	0.054	– 0.037
	(0.040)	(0.044)	(0.044)	(0.041)	(0.075)
Observations	166	132	129	166	145
F statistic	29.3	20.4	20.1	29.3	14.3
A-R 95% CI	[−0.010, 0.157]	[−0.047, 0.139]	[−0.055, 0.132]	[−0.033, 0.137]	[−0.246, 0.097]
Oil exog.	0.632	0.963	0.967	0.646	0.079

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. The IV specifications use *Basin* as an instrument for oil production. The *F* statistic is the Kleibergen and Paap (2006) *rk* statistic, which tests for weak identification and is robust to heteroskedasticity. The A-R 95% confidence interval is based on the Anderson and Rubin (1949) χ^2 test of the null hypothesis that the coefficients on the endogenous variables in the structural equation are jointly equal to zero. The A-R test is robust to the presence of weak instruments. The *p*-value of the test of the endogeneity of oil wealth is from the Hansen (1982) overidentification test of the null hypothesis that oil wealth is exogenous. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

democracy during 1966 to 2008 by 0.039. The effects are statistically significant at the 5% and 1% levels, respectively, and appear to be large in political-economic terms. An increase in oil production by one standard deviation (4.24 log points) reduces the 2008 democracy score by 0.16, or half a standard deviation. This is roughly equal to the difference between the scores of Colombia or Kenya (0.85) and the USA (1.0). In both democracy specifications, the OLS estimates are smaller in absolute magnitude than the IV estimates; in the second specification we can statistically reject the exogeneity of oil production ($p = 0.063$), although in the first we cannot.

The results in column 3 suggest that oil wealth increases corruption, consistent with conventional wisdom and previous empirical evidence (e.g., Bhattacharyya and Hodler, 2010). An increase in oil production by one standard deviation increases corruption by 0.58 points, or half a standard deviation. The OLS estimates are much smaller in absolute magnitude and are statistically insignificant. The discrepancy between the OLS and IV results is consistent with more corrupt countries attracting less oil exploration and production, perhaps due to a poor business environment. In this specification we can statistically reject the exogeneity of oil production ($p = 0.053$).

The results in columns 4–6 provide little evidence that oil wealth increases conflict—contrary to conventional wisdom, though consistent with previous research (Cotet and Tsui, 2013). The OLS results suggest that oil wealth has a positive and significant effect on internal armed conflict, though the corresponding IV estimate is half the size of OLS and is statistically insignificant. Both the OLS and IV regressions find that the effect of oil wealth on coup attempts and purges is statistically insignificant.

Columns 7 and 8 examine the effect of oil production on government revenue. The IV estimate of the effect of oil production on total government revenue is positive but statistically insignificant. In contrast, the IV estimate of the effect of oil production on tax revenue is negative and statistically significant. A 1% increase in oil production causes a 0.16% reduction in tax revenue as a share of GDP from 2000 to 2008. The effect on tax revenue is significant at the 1% level. An increase in oil production by one standard deviation causes a decline in tax revenue by 0.69 log points, or one standard deviation. This is roughly the difference between Burundi (−2.01) and France (−1.32). The corresponding OLS estimates are much smaller in absolute magnitude. The Hansen (1982) test decisively rejects the exogeneity of oil production in the tax revenue specification ($p < 0.001$) but not the total revenue specification.

4.3.2. *Economic resource curse*

Table 4 presents tests of the economic resource curse hypothesis. Column 1 presents results for (log) GDP per capita, while columns 2–5 present results for disaggregated measures of (log) GDP per capita. Both the OLS and IV estimates indicate that oil wealth raises GDP. According to the IV estimate, a 1% increase in average oil production per capita raises GDP per capita in 2008 by 0.07%. The effect is statistically significant at the 10% level. Raising oil production by one standard deviation causes an increase in GDP by 0.31 log points, or 0.25 standard deviations. This is roughly the difference between Norway (11.09) and Ireland (10.78) or between Algeria (9.45) and Ecuador (9.14).

The results in column 1 could be consistent with oil wealth harming the non-resource sectors of the economy, as long as the positive effects on the resource sector outweigh the negative effects on the non-resource sectors. The OLS results in columns 2–5 indicate that oil wealth actually raises non-resource GDP and manufacturing GDP. The IV estimates for non-resource GDP are similar to the OLS estimates, though less precise. Together they suggest that a 1% increase on oil production raises non-resource GDP by 0.05 to 0.07%. The OLS and IV estimates of the effect of oil wealth on manufacturing significantly diverge. The OLS estimate indicates that a 1% increase on oil production raises manufacturing GDP by almost 0.08%, and this estimate is significant at the 1% level. On the other hand, the IV estimate is negative and statistically insignificant. We reject the exogeneity of oil production in the manufacturing GDP equation ($p = 0.079$).

In four of the 13 specifications, the Hansen (1982) test rejects the exogeneity of oil production at the 10% level. This outcome is unlikely to be due simply to chance or multiple hypothesis testing. For example, if oil production were in fact exogenous in each of the 13 regressions, the probability of rejecting the null hypothesis of exogeneity at the 10% level in four or more of the specifications is 0.034 (assuming the tests are independent).[28] Furthermore, whenever the OLS and IV estimates diverge considerably, OLS understates the negative effects of oil relative to IV. Thus the results are consistent with the possibility that countries with stronger political and fiscal institutions disproportionately select into oil discovery and production.

4.4. *Varying the Instrument Set*

The results discussed so far are based on the optimal (singleton) instrument set which maximises the first-stage F statistic. We now consider how the results change when the instrument set changes. Figures C.4 and C.5 in Appendix C plot the second-stage results for the political and economic outcomes, respectively, using instrument sets categorised according to the general plate-tectonic environment and primary mechanism of subsidence. The results based on N instruments use the instrument set of size N that achieves the highest first-stage F statistic. For each outcome, the grey, dashed line indicates the value of the corresponding OLS estimate. As Table B.8 in Appendix B shows, each of the eight instrument sets is at least moderately strong, however the first (singleton) instrument set is significantly stronger than the others, with a first-stage F statistic of 25.3. Because of this, along with the fact that the bias of 2SLS generally increases with the number of instruments (Donald and Newey, 2001), we would expect results based on the first instrument set to have lower bias, but also lower precision, compared to results based on the other instrument sets. Consistent with this prediction, the estimated effects of oil production on democracy, average democracy, corruption index, and tax revenue, are further from the OLS results and less precise when using one instrument—or even two instruments—compared to estimates based on larger instrument sets. Adding additional, weaker instruments pushes the 2SLS estimates toward the OLS estimate, which we expect to be biased upwards for democracy and tax revenue and downwards for corruption. The estimates of the effect of oil production on internal conflict and purges show a somewhat different pattern: estimates based on small instrument sets imply effects roughly equal to zero, while estimates based on larger instrument sets imply positive and marginally significant effects. The point estimates for coup attempts and total revenue do not change much as the instrument set varies. For every measure of GDP, the point estimates based on smaller instrument sets are smaller than the point estimates based on larger instrument sets. This pattern is especially apparent for non-resource GDP and manufacturing GDP. Similar to the results for democracy, corruption, and tax revenue, the GDP results are consistent with the fact that richer countries with stronger institutions engage in more resource exploration and production.

Figures C.6 and C.7 in Appendix C plot the second-stage results for the political and economic outcomes, respectively, using instrument sets categorised according to the local properties of the depositional environment. Once again, results based on N instruments use the instrument set of size N that achieves the highest first-stage F statistic. The coefficient patterns are qualitatively

[28] Under the stated assumptions, the number of rejections, W, has a binomial distribution with $n = 13$ and $p = 0.1$. Therefore, $P(W \geq 4) = 0.034$.

similar to those in Figures C.4 and C.5 in Appendix C. The main difference is that the estimates based on different instrument sets diverge less from each other, perhaps because the smaller instrument sets are weaker than in the case of the tectonic-subsidence grouping. Another difference is that the sign and statistical significance of the estimated effect of oil production is less sensitive to the instrument set—at least for the political outcomes—than when instrument sets based on the tectonic-subsidence grouping are used. In fact, nearly every instrument set implies that oil production has a negative and significant effect on democracy, average democracy, and tax revenue; a positive and significant effect on corruption, internal conflict, purges, and total revenue; and an insignificant effect on coup attempts. The preponderance of the evidence suggests that OLS understates the adverse political effects of oil production, though the OLS and 2SLS estimates often are not statistically different from one another. In the GDP equations, by contrast, the OLS and 2SLS results are similar for most instrument sets and do not suggest that OLS is systematically biased in one direction or another.

4.5. *Validity of the Instrument*

4.5.1. *Measurement*

We now consider several potential objections to the validity of the *Basin* instrument. The first relates to measurement. Two of the three methods used to map sedimentary basins—core sampling and seismic profiling—require the use of advanced technology and physical access to the area under investigation. One might therefore worry that the precision or reliability of the basin data is increasing in 'good' institutions like property rights protections. In that case the *variance* of the basin measurement error would be decreasing in the quality of institutions. However, it does not follow that the measurement error is *correlated* with the quality of institutions, so the above form of measurement error need not produce asymptotic bias.

Another version of the measurement argument supposes that basin area is systematically underestimated in countries with poor institutions, invalidating the instrument. This argument is unconvincing for two reasons. First, it is inconsistent with the pattern of basin coverage by region. Table B.6 Appendix B summarises the portion of sovereign area covered by sedimentary basins separately for seven regions defined by common geographical location and history. Basin coverage is actually higher on average in Eastern Europe and Central Asia (0.67) and the Middle East and North Africa (0.86)—areas associated with relatively weak property-rights protections—than in the extensively prospected areas of Northern, Central, Western, and Southern Europe and Neo-Europes (0.57).[29] This pattern is visually confirmed in Figure 1. Second, even if basin area were underestimated in countries with poor institutions, the vast majority of the conclusions drawn in this article would hold up. This type of non-classical measurement error would cause the IV estimates to understate the effects of oil on democracy, corruption, conflict, and fiscal capacity, so that the estimated coefficients would often provide informative (absolute) lower bounds on the true effect.[30]

[29] The 'Neo-Europes' are Australia, Canada, New Zealand, and the United States.

[30] Let Z, Z^*, and X be the measured *Basin*, the true *Basin*, and *Oil*, respectively, after netting out the control variables using population projections. If the measurement error in *Basin*, e, is uncorrelated with the control variables, then $Z = Z^* + e$ (Wooldridge, 2010, p. 29) and the probability limit of $\hat{\beta}_{IV}$ is $\beta + \text{Cov}(\varepsilon, e)/\,\text{Cov}(X, Z)$. Because $\text{Cov}(X, Z)$ is positive, the sign of the bias depends on the sign of $\text{Cov}(\varepsilon, e)$. For 'good' outcome variables like democracy, the example in the text implies that the bias is positive, whereas for 'bad' outcome variables like conflict, the bias is negative.

4.5.2. *Reverse-engineering of the basin classification*
The next potential objection is that sedimentary basin classification could be reverse-engineered: the known presence or absence of hydrocarbons may influence how geologists categorise a basin, based on their knowledge of other hydrocarbon-rich or hydrocarbon-poor basins. Therefore, some of the correlation between hydrocarbons and particular basin types may be spurious rather than based on true geological features.

This issue is unlikely to invalidate our results for two reasons. First, reverse-engineering of basin categories would bias the 2SLS estimates towards the OLS estimates. The intuition is simple: in the most extreme case of reverse-engineering, a few basin types would have 100% hydrocarbon success rates and would jointly predict oil production perfectly, causing the 2SLS estimates to equal the OLS estimates. To the extent that the 2SLS and OLS estimates differ, the 2SLS estimates still provide useful bounds on the true effects of oil production. Second, as already discussed, Figures C.4–C.7 in Appendix C show that the results are broadly similar whether instruments are constructed based on global characteristics of basins or local characteristics. It is unlikely that both the global and local categorisations of basins could be reverse-engineered.

4.5.3. *Predetermined confounders*
Another potential objection is that *Basin* could be correlated with omitted determinants of development, causing an asymptotic bias of unknown sign. To explore this possibility, Table 5 reports the results from regressing several predetermined variables on the basin instrument, controls, and region effects. The first outcome is the urbanisation rate in 1850, which is the last year in the series provided by Chandler (1987). The next outcome is an indicator for a British legal origin, taken from William Easterly's Global Development Network Growth Database (Easterly, 2001). The third outcome is an indicator for having a legacy as a communist country, taken from the list of communist countries in Kornai (1992). The next three outcomes measure the percentage of the population that was Christian, Muslim, or Hindu in 1950. These data come from the World Religion Database (Johnson and Grim, 2017). The final three outcomes are measures of ethnic, religious, and linguistic fractionalisation produced by Alesina *et al.* (2003). Seven of the nine estimated coefficients on *Basin* are statistically insignificant, suggesting that the instrument is uncorrelated with historical determinants of long-run economic development, legal origin, communist legacy, the presence of Christians or Hindus, or religious or linguistic fractionalisation. The basin instrument has a strong, positive correlation with the percentage of the population that was Muslim in 1950. A large portion of this correlation is driven by the religious composition and presence of basins in the Middle East and North Africa; adding a dummy variable for this region causes the coefficient on *Basin* to fall by half.[31] The basin instrument also has a positive correlation with ethnic fractionalisation that is significant at the 10% level. It is therefore important to examine how the main results change when we control for these two variables.

Table 6 reports the main results for the political outcomes using the optimal instrument while controlling for the percentage of the population that was Muslim in 1950. The OLS estimates of the effect of oil production on the political outcomes generally move slightly closer to zero while maintaining the same pattern of signs and similar levels of statistical significance: oil production still has a negative and significant effect on democracy, average democracy, and tax revenue, while having a positive and significant effect on internal conflict and total revenue.

[31] Result not shown but available upon request.

Table 5. *Partial Correlation between Basin and Predetermined Variables.*

	Urbanisation, 1850	British legal origin	Communist legacy	Percentage Christian, 1950	Percentage Muslim, 1950	Percentage Hindu, 1950	Fractionalisation:		
							Ethnic	Religious	Linguistic
Convergent C-C mechanical	−0.050	0.003	−0.014	−0.357	5.759***	0.015	0.015*	0.002	0.008
	(0.277)	(0.015)	(0.012)	(0.597)	(0.865)	(0.214)	(0.008)	(0.008)	(0.007)
Observations	84	163	172	172	172	172	171	172	165
R^2	0.449	0.086	0.060	0.800	0.495	0.078	0.407	0.093	0.402

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. *Testing for a Political Resource Curse (Controlling for Percentage Muslim in 1950).*

	Democracy 2008	Avg. democracy 1966–2008	Corruption 2008	Internal conflict 1966–2008	Coup attempts 1966–2008	Purges 1966–2008	Total revenue 2000–2008	Tax revenue 2000–2008
	Panel A: Ordinary Least Squares							
Oil production	− 0.014***	− 0.011**	0.018	0.012*	− 0.000	0.003	0.034***	− 0.033***
	(0.005)	(0.004)	(0.022)	(0.006)	(0.002)	(0.002)	(0.007)	(0.010)
Observations	157	160	136	172	160	172	165	167
R^2	0.477	0.567	0.359	0.204	0.231	0.093	0.464	0.530
	Panel B: Two-Stage Least Squares							
Oil production	− 0.016	− 0.025	0.093	0.003	− 0.003	− 0.001	0.023	− 0.138***
	(0.019)	(0.015)	(0.077)	(0.022)	(0.005)	(0.006)	(0.020)	(0.044)
Observations	157	160	136	172	160	172	165	167
F statistic	12.4	15.3	10.6	18.9	15.3	18.9	16.3	14.8
A-R 95% CI	[−0.062, 0.025]	[−0.067, 0.004]	[−0.073, 0.298]	[−0.047, 0.048]	[−0.014, 0.006]	[−0.015, 0.012]	[−0.025, 0.064]	[−0.274, −0.068]
Oil exog.	0.893	0.339	0.318	0.675	0.470	0.488	0.585	0.006

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. The IV specifications use *Basin* as an instrument for oil production. The *F* statistic is the Kleibergen and Paap (2006) *rk* statistic, which tests for weak identification and is robust to heteroskedasticity. The A-R 95% confidence interval is based on the Anderson and Rubin (1949) χ^2 test of the null hypothesis that the coefficients on the endogenous variables in the structural equation are jointly equal to zero. The A-R test is robust to the presence of weak instruments. The *p*-value of the test of the endogeneity of oil wealth is from the Hansen (1982) overidentification test of the null hypothesis that oil wealth is exogenous. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Controlling for the Muslim population causes the 2SLS estimates to become more imprecise, due to a weakened first stage. The 2SLS estimates for the effect of oil production on democracy, average democracy, and corruption all move towards zero while remaining greater than the OLS point estimates in absolute value, once again suggesting that OLS may understate the adverse effects of oil wealth on institutions. These three point estimates are now statistically insignificant. Given that OLS likely provides an upper bound on the effect of oil production on democracy, we are still able to conclude that oil impedes democracy. Controlling for Muslim population pushes the 2SLS estimate of the effect of oil production on tax revenue slightly closer to zero, however this estimate remains sizable and highly significant.

Table 7 reports the main results for the economic outcomes using the optimal instrument while controlling for the percentage of the population that was Muslim in 1950. Both the OLS and 2SLS estimates are broadly similar to those in the baseline specification, in terms of both magnitude and significance. Controlling for Muslim population leads to slightly larger positive estimated effects of oil production on GDP.

Overall, Tables 6 and 7 suggest that the baseline 2SLS estimates may have slightly overstated the adverse effects of oil wealth on democracy and taxation while still providing strong evidence that such adverse effects exist. The results weaken the original claim that the OLS results for average democracy and corruption were substantially biased, while confirming the claim that the OLS results for taxation were substantially biased. The robustness check confirms the baseline OLS and 2SLS estimates for the GDP regressions.

Are the above results limited to the optimal instrument, or do they apply to all instrument sets? Figures C.8–C.11 in Appendix C replicate the main results using optimal instrument sets of different sizes while controlling for the percentage of the population that was Muslim in 1950. The pattern of coefficient estimates based on different instrument sets is very similar to

Table 7. *Testing for an Economic Resource Course (Controlling for Percentage Muslim in 1950).*

	GDP, 2008	Non-oil GDP, 2008	Non-oil/gas GDP, 2008	Non-resource GDP, 2008	Manufacturing GDP, 2008
	Panel A: Ordinary Least Squares				
Oil production	0.099***	0.049***	0.045**	0.078***	0.090***
	(0.015)	(0.017)	(0.017)	(0.015)	(0.021)
Observations	166	132	129	166	145
R^2	0.668	0.628	0.611	0.665	0.616
	Panel B: Two-Stage Least Squares				
Oil production	0.112**	0.087	0.074	0.096*	0.011
	(0.051)	(0.060)	(0.061)	(0.052)	(0.097)
Observations	166	132	129	166	145
F statistic	17.1	11.7	10.8	17.1	6.8
A-R 95% CI	[0.005, 0.228]	[−0.033, 0.245]	[−0.058, 0.234]	[−0.012, 0.216]	[−0.344, 0.224]
Oil exog.	0.793	0.499	0.631	0.735	0.401

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. The IV specifications use *Basin* as an instrument for oil production. The *F* statistic is the Kleibergen and Paap (2006) *rk* statistic, which tests for weak identification and is robust to heteroskedasticity. The A-R 95% confidence interval is based on the Anderson and Rubin (1949) χ^2 test of the null hypothesis that the coefficients on the endogenous variables in the structural equation are jointly equal to zero. The A-R test is robust to the presence of weak instruments. The *p*-value of the test of the endogeneity of oil wealth is from the Hansen (1982) overidentification test of the null hypothesis that oil wealth is exogenous. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the pattern in the original figures. The two main differences are that some point estimates move slightly closer to zero, and the confidence intervals of all point estimates grow.

Tables B.10 and B.11 in Appendix B report the main results using the optimal instrument while controlling for ethnic fractionalisation. The results are remarkably similar to the baseline results, which is perhaps unsurprising given that the partial correlation between *Basin* and ethnic fractionalisation is weak. Figures C.12–C.15 in Appendix C confirm that the results using different instrument sets hardly change when we control for ethnic fractionalisation.

4.5.4. *Placebo tests*

While it is reassuring that our conclusions do not change significantly when accounting for the influence of potential confounders described above, there may be other determinants of political and economic development that are correlated with the basin instrument. We address this possibility with two placebo tests. If *Basin* impacts development only through the channel of oil wealth, then it should have no impact on economic and political outcomes in years when oil was not a commercially valuable commodity or when world oil production was minimal. Before 1859 the value of oil was modest. The year 1859 saw both the first modern oil well (by Edwin Drake) and the first commercially successful internal-combustion engine (by Étienne Lenoir) (Britannica, 2015). Prior to 1920 no country produced a significant amount of oil, defined as $100 per capita (in constant 2007 USD) (Andersen and Ross, 2014). In 1940 there were three significant oil producers, and by 1950 there were ten. For context, 56 countries were significant oil producers in 2008 (Ross, 2013).

Figure 2 plots estimates of the reduced-form effect of *Basin* on political and economic development in different years, controlling for geography and climate. Panel (a) presents the effect of *Basin* on the polity index. To examine the changing influence of the basin instrument over

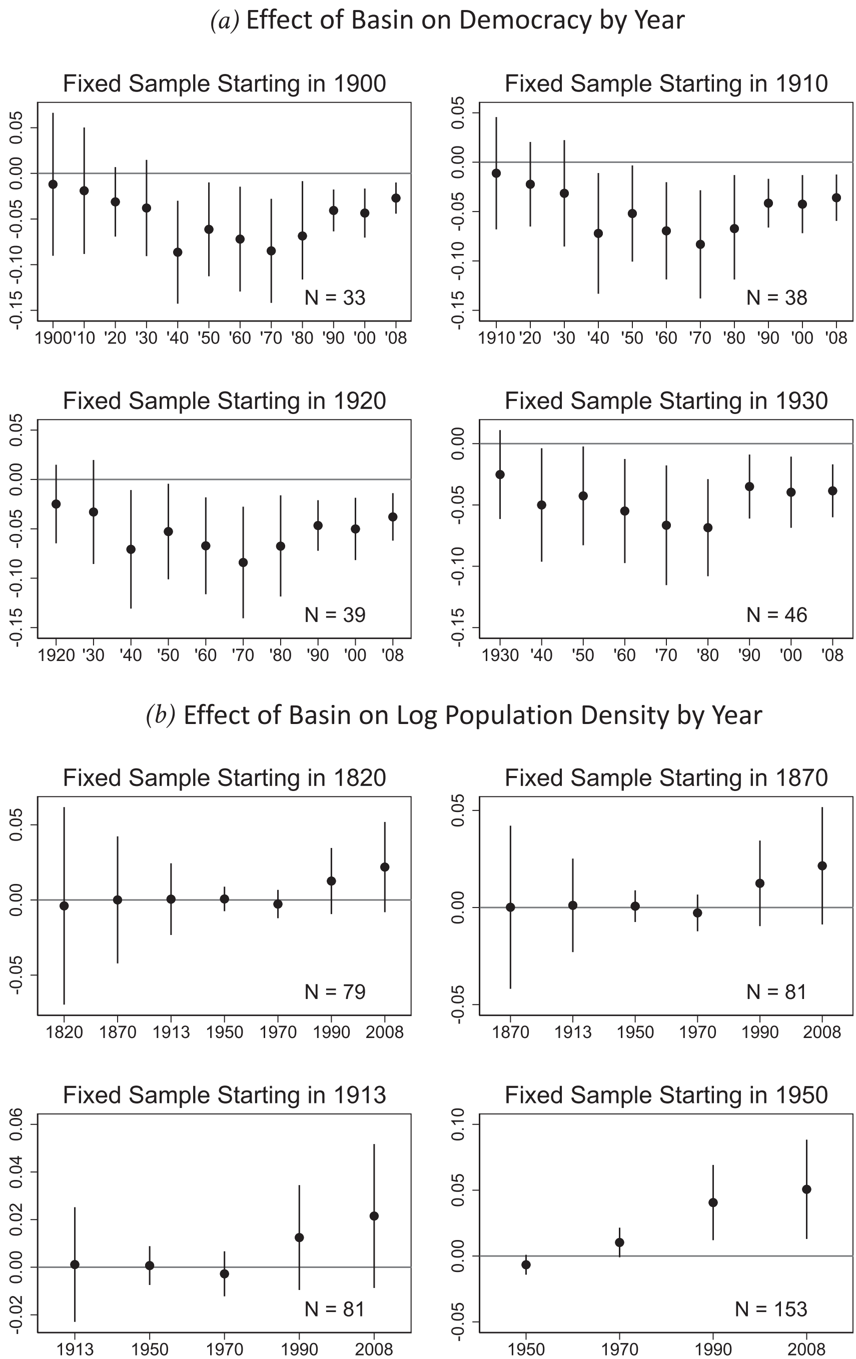

Fig. 2. *Placebo Tests.*

Notes: This figure plots point estimates and 95% confidence intervals for the reduced-form effect of the optimally chosen *Basin* variable over time, controlling for geography and climate. In each graph, the sample of countries is fixed. The outcome variable in Panel (a) is democracy, and the outcome variable in Panel (b) is log population density.

time, we fix the sample of countries. The four graphs are based on fixed country samples starting in 1900, 1910, 1920, and 1930. All four graphs tell the same story: prior to 1940, the effect of *Basin* on democracy was statistically indistinguishable from zero. Starting in 1940, *Basin* had a negative and statistically significant effect, and this negative effect persisted to 2008.

Panel (b) is similar, presenting four graphs of the reduced-form effect of *Basin* on log population density over time. We focus on population density because GDP data prior to 1950 are available only for a small number of countries. Prior to 1950, the availability of population data from Maddison (2013) varies considerably from year to year. We choose to measure log population density in the years 1820, 1870, and 1913 because these are the only years prior to 1950 for which population data are available for more than 65 countries. The graphs suggest that *Basin* had no influence on log population density prior to 1970; the effect of *Basin* on log population density becomes positive and statistically significant starting in 1990.

Together, the results in Figure 2 suggest that *Basin* did not influence political and economic development in periods in which the value of oil production was insignificant. These results strengthen the claim that the baseline results are not simply driven by omitted variables that are correlated with both *Basin* and long-run development.

4.5.5. *Predetermined borders*

The validity of the basin instrument rests on the assumption that national borders were drawn without consideration for the locations of sedimentary basins. The most plausible violation of this assumption would occur in geographic regions where modern borders were established after the discovery of oil. If oil-field acquisition (hence basin acquisition) via border changes were systematically related to potential outcomes—e.g., if more economically or militarily powerful countries acquired more oil fields through territorial conquest or delimiting colonial dependencies—then the IV estimator would be inconsistent for the treatment effect of interest.

To address this concern we replicate the main analysis on the subsample that excludes any country whose land borders could have plausibly been influenced by the location of oil fields.[32] We first record the year of the earliest known oil discovery for each country, according to Thieme *et al.* (2007) and Lujala *et al.* (2007). We then record the year of the earliest establishment of modern borders, using the information in Strang (1991), Britannica (2015), and CIA (2015). It is important to note that the modern borders of most former colonies and former satellite states were drawn decades before independence. Finally, we record the dates of all changes to homeland territory (as opposed to dependency territory) since 1816, according to Tir *et al.* (1998). Using data on country contiguity from Correlates of War Project (2007; described in Stinnett *et al.*, 2002) to identify neighbouring countries, we implement the following procedure:

1. Exclude country *A* if country *A* first discovered oil before its modern borders were set.
2. Exclude country *A* if country *A*'s neighbour, country *B*, first discovered oil before country *A*'s modern borders were set, *and* country *B*'s modern borders were not set prior to the discovery.
3. To minimise unnecessary exclusions, include countries that were to be excluded according to Rule 1 or Rule 2 if either (a) there are no known onshore oil fields within 200 kilometers of the border in question, or (b) there are no land basins within 200 kilometers of the border in question.

[32] We focus this robustness check on land borders for tractability, as maritime borders are often ambiguous or disputed.

Table 8. *Testing for a Political Resource Curse: Subsample with Predetermined Borders.*

	Democracy 2008	Avg. democracy 1966–2008	Corruption 2008	Internal conflict 1966–2008	Coup attempts 1966–2008	Purges 1966–2008	Total revenue 2000–2008	Tax revenue 2000–2008
	Panel A: Ordinary Least Squares							
Oil production	− 0.017***	− 0.018***	0.044	0.014**	0.002	0.002	0.020**	− 0.043***
	(0.006)	(0.006)	(0.030)	(0.006)	(0.002)	(0.002)	(0.009)	(0.014)
Observations	96	97	80	108	97	108	104	105
R^2	0.395	0.580	0.376	0.219	0.201	0.119	0.441	0.467
	Panel B: Two-Stage Least Squares							
Oil production	− 0.035**	− 0.042***	0.151**	0.009	0.001	− 0.000	0.006	− 0.138***
	(0.014)	(0.012)	(0.068)	(0.012)	(0.003)	(0.008)	(0.019)	(0.047)
Observations	96	97	80	108	97	108	104	105
F statistic	21.7	21.7	20.4	27.8	21.7	27.8	30.3	25.4
A-R 95% CI	[−0.067, −0.008]	[−0.071, −0.021]	[0.018, 0.311]	[−0.016, 0.035]	[−0.006, 0.009]	[−0.017, 0.017]	[−0.035, 0.044]	[−0.260, −0.058]
Oil exog.	0.157	0.038	0.086	0.637	0.698	0.809	0.387	0.033

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. The IV specifications use *Basin* as an instrument for oil production. The *F* statistic is the Kleibergen and Paap (2006) *rk* statistic, which tests for weak identification and is robust to heteroskedasticity. The A-R 95% confidence interval is based on the Anderson and Rubin (1949) χ^2 test of the null hypothesis that the coefficients on the endogenous variables in the structural equation are jointly equal to zero. The A-R test is robust to the presence of weak instruments. The *p*-value of the test of the endogeneity of oil wealth is from the Hansen (1982) overidentification test of the null hypothesis that oil wealth is exogenous. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4. Include countries with borders set prior to 1859, even if they qualify for exclusion according to Rule 1 or Rule 2.[33]

The procedure results in the exclusion of 61 countries from the baseline sample of 157 countries. Tables 8 and 9 report regression results based on the sample of countries with borders that were not plausibly influenced by the location of oil fields or basins. The results are remarkably similar to the results from the full sample, both qualitatively and quantitatively. The broad similarity of the results to the main results suggests that countries with borders drawn after the discovery of oil are not systematically different than countries with borders drawn before the discovery of oil.

4.6. *Comparison to Endowment Instrument*

The closest predecessor to the identification strategy in this article is Tsui (2011), who uses oil endowment as an instrument for oil discovery. To facilitate comparison between Tsui (2011) and this article, we normalise the oil endowment variable in the same manner that we normalise the basin variables: *Endowment* is the (log of) total oil endowment in millions of barrels divided by 1960 population.[34] As mentioned in the introduction, there are a priori reasons to worry that known oil endowment is endogenous. We find suggestive statistical evidence that this is indeed the case. Tables B.12 and B.13 in Appendix B compare the OLS results, 2SLS results based on *Endowment*, and 2SLS results based on *Basin*. The first-stage *F* statistic on *Endowment* is extremely large—410 in the full sample—and IV estimates using *Endowment* are almost

[33] Before 1859 petroleum was arguably not a very valuable commodity and thus would not have influenced border formation. The year 1859 saw both the first modern oil well (by Edwin Drake) and the first commercially successful internal-combustion engine (by Étienne Lenoir) (Britannica, 2015).

[34] The data on oil endowment is shared online by Cotet and Tsui (2013).

Table 9. *Testing for an Economic Resource Course: Subsample with Predetermined Borders.*

	GDP, 2008	Non-oil GDP, 2008	Non-oil/gas GDP, 2008	Non-resource GDP, 2008	Manufacturing GDP, 2008
	Panel A: Ordinary Least Squares				
Oil production	0.081***	0.036	0.036	0.062***	0.049
	(0.020)	(0.023)	(0.023)	(0.021)	(0.031)
Observations	105	73	70	105	89
R^2	0.686	0.647	0.632	0.678	0.596
	Panel B: Two-Stage Least Squares				
Oil production	0.112**	0.099*	0.096	0.107**	0.006
	(0.050)	(0.059)	(0.060)	(0.051)	(0.084)
Observations	105	73	70	105	89
F statistic	30.4	19.4	18.5	30.4	13.9
A-R 95% CI	[0.020, 0.229]	[−0.005, 0.254]	[−0.008, 0.252]	[0.013, 0.227]	[−0.181, 0.196]
Oil exog.	0.484	0.225	0.239	0.331	0.577

Notes: See Appendix B for variable definitions. All specifications include geographic controls (land area, coastline, and mountainous area), climatic controls (tropical area and good soil area), and region fixed effects. The IV specifications use *Basin* as an instrument for oil production. The *F* statistic is the Kleibergen and Paap (2006) *rk* statistic, which tests for weak identification and is robust to heteroskedasticity. The A-R 95% confidence interval is based on the Anderson and Rubin (1949) χ^2 test of the null hypothesis that the coefficients on the endogenous variables in the structural equation are jointly equal to zero. The A-R test is robust to the presence of weak instruments. The *p*-value of the test of the endogeneity of oil wealth is from the Hansen (1982) overidentification test of the null hypothesis that oil wealth is exogenous. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

always closer than IV estimates using *Basin* to the OLS estimates. In addition, the Hansen (1982) overidentification test rejects the exogeneity of *Endowment* in the average democracy, corruption, tax revenue, and manufacturing GDP specifications, though it fails to reject exogeneity in the other specifications.[35] Nonetheless, the *Endowment* and *Basin* instruments produce the same qualitative conclusions, providing support for the political resource curse hypothesis and rejecting the economic resource curse hypothesis.

4.7. *Discussion*

The estimated negative effects of oil production on democracy and tax revenue indicate that oil wealth has a tendency to degrade—or retard the development of—democratic institutions and fiscal capacity over the long run. Oil wealth increases the value of holding political power, which in theory could make a coup d'état more attractive in the eyes of potential usurpers. However, the resource revenue also strengthens the government's hand, potentially funding investment in defence.[36] The results of this section suggest that oil wealth increases government repression in the form of purges.[37] However, in equilibrium oil wealth does not lead to more coup attempts. This result is consistent with the model of Tsui (2010), which predicts that the number of political insurgents will be independent of the size of resource wealth.[38] The reason is that an increase in resource wealth induces the ruler to invest in political entry barriers which deter potential insurgents.

[35] This overidentification test evaluates the exogeneity of *Endowment* under the assumption that *Basin* is exogenous.
[36] Cotet and Tsui (2013) find that oil discoveries increase military spending in nondemocratic countries.
[37] Note that we find a positive, significant effect of oil production on purges using many instrument sets of size greater than one. The evidence on internal armed conflict is inconclusive.
[38] This result depends on the counterinsurgent technology having constant returns to scale.

The fact that OLS underestimates the pernicious effects of oil on democracy and tax revenue suggests that countries with better political institutions and greater state capacity have a greater propensity to select into oil production.[39] The results are consistent with recent evidence that the drilling decisions of international oil companies are highly sensitive to the quality of national institutions (Cust and Harding, 2017).

5. Heterogeneous Effects by Executive Constraints

5.1. *Theory*

Several political economy models predict that the political and economic effects of natural resource wealth will depend on the quality of institutions. In some models institutions determine the extent to which incumbents can spend resources to increase their likelihood of staying in power. The degree to which resource booms promote autocracy or resource misallocation within the economy thus depends on institutions (Robinson *et al.*, 2006; Caselli and Tesei, 2016). In a similar vein, democratic institutions determine the degree to which popular support (or lack thereof) affects the incumbent's chances of staying in power. While resource booms increase the scope of corruption, incumbents are less likely to embezzle state funds when democratic institutions are strong (Bhattacharyya and Hodler, 2010). In addition, resource rents are more likely to promote repression and civil war when political checks and balances are weak (Besley and Persson, 2009b, 2011b). Finally, resource abundance can reduce economic growth when institutions favour rent-seeking over productive activities (Mehlum *et al.*, 2006).[40]

In Appendix A we present a theoretical model that predicts that institutions will determine the effect of resource revenue on the incumbent's *joint* decision over the political regime and tax policy. An autocrat faces the threat of a popular uprising and must decide whether to allow a transition to democracy or suppress the movement using bribes. Under democracy the median voter, who is poor, chooses a positive tax rate. When the autocrat chooses to suppress democracy, his optimal strategy involves bribing the rich citizens and setting taxes equal to zero. Both the autocrat's ability and willingness to suppress democracy increase in the amount of resource rents accruing to the autocrat. However, executive constraints create transaction costs associated with stealing resource rents from government coffers and making bribes. As a result, a resource boom increases the likelihood that autocracy and low taxation persist *if and only if* executive constraints are sufficiently weak. See Appendix A for details.

5.2. *Evidence*

To test the implications of the theoretical models described above, we estimate the effects of oil production, allowing for heterogeneity in the response according to the strength of executive constraints. We construct a measure of initial executive constraints by averaging each country's *XCONST* score (Polity IV) from 1950 to 1965.[41] The variable *XCONST* is measured on a scale

[39] Prospecting intensity probably accounts for most of the differential selection into oil production. While known subsoil assets in the OECD countries are valued at around US$265,000 per square kilometer, in sub-Saharan Africa known subsoil assets are valued at only US$45,000 per square kilometer (Collier and Laroche, 2015).

[40] See Tsui (2010) for a model that combines the economic and political dimensions of the resource curse while modeling institutions as the deadweight costs associated with rent appropriate and political entry deterrence.

[41] Naturally, the sample is restricted to countries with at least one observation of *XCONST* from 1950 to 1965. We use a 16-year average to reduce noise and maximise sample size.

of one to seven, with one indicating 'unlimited authority', three indicating 'slight to moderate limitation on executive authority', five indicating 'substantial limitations on executive authority', and seven indicating 'executive parity or subordination'. Numbers two, four, and six denote intermediate categories. We construct an indicator variable, *weak constraints*, which equals one for countries that averaged a score of three or lower from 1950 to 1965. In our sample the median score for average *XCONST* over this period is three.

We split the sample into two subsamples—countries with relatively strong executive constraints and those with relatively weak constraints—and estimate the structural equation separately for each subsample. We then compare the IV estimates obtained in each subsample. While we have data on democracy in 2008 for 157 countries, we observe *weak constraints* for only 116 countries. This is because countries that gained independence after 1965 have missing values for *XCONST* for all years from 1950 to 1965.

Tables B.14 and B.15 in Appendix B present the results of the heterogeneity analysis. The validity of the exercise relies on the assumption that *weak constraints* is uncorrelated with unobserved determinants of development. In Appendix B we show that *weak constraints* is, for the most part, uncorrelated with the different sedimentary basin measures. Nonetheless, exogeneity is a strong assumption, and the results in this section should be interpreted with caution. The optimal basin instrument in the full-sample analysis leads to excessively small first-stage *F* statistics in the subsample analysis. We therefore report results based on the instrument set {Foreland, Intracratonic Sag}, which produces modestly sized first-stage *F* statistics in the subsamples. We checked the results using the seven strongest instrument sets according to Tables B.8 and B.9 and the pattern of second-stage coefficients is very similar using different instrument sets.

5.2.1. *Political resource curse*

Table B.14 in Appendix B presents the results of the heterogeneity analysis for the political outcomes. As shown in Panel A, in the sample of strong-constraints countries, oil production has a statistically insignificant effect on each political variable, with the exception of total revenue. In contrast, Panel B shows that, in the sample of weak-constraints countries, oil production has a statistically significant effect on six of the eight outcomes, reducing democracy in 2008, average democracy from 1966 to 2008, and tax revenue; and increasing internal conflict, purges, and total revenue. The effects of oil production on corruption and coup attempts are statistically insignificant in the sample of weak-constraints countries.

A 1% increase in oil production reduces the level of democracy in 2008 by 0.044. The effect is significant at the 5% level. In the weak-constraints sample, an increase in oil production of one standard deviation (4.24 log points) reduces 2008 democracy by 0.19, or 0.59 standard deviations.[42] This is roughly equal to the difference between the scores of Algeria (0.6) and Malawi (0.8).[43] The negative effect of oil on democracy in 2008 is smaller in magnitude (−0.027) and statistically insignificant in the strong-constraints sample.

Oil production also has a large effect on tax revenue in the sample of weak-constraints countries. A 1% increase in oil production reduces the tax-revenue-to-GDP ratio by 0.108% in the sample of countries with weak constraints. The estimate is significant at the 5% level. Among countries with weak constraints, increasing oil production by one standard deviation (4.24 log points) reduces

[42] The standard deviation of 2008 democracy in the weak-constraints sample is 0.31.

[43] While both Algeria and Malawi had weak executive constraints from 1950 to 1965, Algeria produced a significant amount of oil from 1966 to 2008, and Malawi produced no oil.

the tax revenue share of GDP by 0.46, or 0.67 standard deviations, which is roughly equal to the difference in tax revenue between Nicaragua (−1.83) and Mexico (−2.33).[44] The negative effect of oil on tax revenue is smaller in magnitude (−0.012) and statistically insignificant in the strong-constraints sample.

Overall, the results suggest that the adverse political consequences of oil wealth are concentrated in the sample of countries with weak initial constraints on the executive. While the point estimates in the two subsamples often differ substantially, Panel C shows that we are unable to reject equality of the point estimates in any of the equations, perhaps owing to the small sample sizes.

5.2.2. *Economic resource curse*

Table B.15 Appendix B presents the results of the heterogeneity analysis for the economic outcomes. As shown in Panel A, in the sample of strong-constraints countries, oil production has a positive effect on each economic variable, though each coefficient is statistically insignificant. The point estimates for the sample of weak-constraints countries, reported in Panel B, are slightly larger than those in Panel A, and they are all significant at least at the 10% level. In the weak-constraints sample, a 1% increase in oil production raises GDP per capita by 0.15%, and the effect is almost identical for manufacturing GDP—contrary to the Dutch Disease hypothesis.

5.2.3. *Weak constraints*

The heterogeneity analysis would be invalid if, for example, pre-1966 oil production affected both *weak constraints* and post-1966 democracy and tax revenue. However, the *Basin* measures have virtually no statistical association with *weak constraints*, as shown in Appendix B. The only basin types that have a statistically significant association with *weak constraints* are convergent ocean-ocean basins, convergent wrench basins, and fore-arc basins. In all three cases, the association with *weak constraints* is negative, which contradicts the claim that pre-1966 oil production adversely affected pre-1966 institutions.

5.3. *Discussion*

We find evidence that the long-run effects of oil wealth on development may be heterogeneous. In particular, the adverse effects of oil on democracy and fiscal capacity are concentrated in the subsample of countries that had weak executive constraints from 1950 to 1965. This result is consistent with other recent findings. Tsui (2011) finds that the discovery of oil impeded democratisation only in countries that were non-democratic at the time of discovery. Similarly, Caselli and Tesei (2016) show that resource windfalls cause autocratic countries to become even more autocratic, whereas they have no effect on the regime in democratic countries or in deeply entrenched autocracies. Finally, Andersen and Aslaksen (2013) show that oil wealth positively affects political survival (measured as the leader's duration in office) in intermediate and autocratic regimes, but not in democracies. In contrast to the results of Bhattacharyya and Hodler (2010), we do not observe heterogeneous effects of oil on corruption. Neither does the effect of oil on conflict seem to differ according to institutional quality. The finding that oil has a larger positive effect on GDP in weak-constraints countries is consistent with other evidence that

[44] Both Nicaragua and Mexico had weak executive constraints from 1950 to 1965. From 1966 to 2008 oil production was substantial in Mexico and nil in Nicaragua.

less developed countries have the largest GDP gains from oil production (Alexeev and Conrad, 2009; Smith, 2015).

In order to identify a heterogeneous effect of oil, the dimension of heterogeneity (e.g., political institutions) must be uncorrelated with unobserved determinants of future political outcomes and oil wealth. Of course, this assumption is unlikely to hold. However, unlike the studies mentioned above, we condition on initial rather than contemporaneous political institutions to (partially) alleviate concerns about the simultaneity of political institutions and resource production.

The heterogeneity results are interesting in light of the recent literature on the determinants of fiscal capacity. Previous empirical studies find that resource wealth tends to negatively impact tax revenue (Cárdenas *et al.*, 2011; Jensen, 2011; Crivelli and Gupta, 2014). However, these studies do not test for heterogeneous effects. The fiscal capacity model of Besley and Persson (2009a, 2010, 2011b) predicts that a 'common-interest' state emerges when institutions are 'cohesive' enough. In their model, institutional cohesion depends on the ability of the group in power to redistribute resources away from the group not in power. In a common-interest state, politicians invest in fiscal capacity because they know that future capacity to tax will be used to raise funds for common-interest public goods rather than for redistributing income away from the group not in power. Because the marginal utility from public goods is assumed to be declining, a relaxation of the government's budget constraint due to a resource windfall causes the group in power to invest less in fiscal capacity. When institutions are not cohesive, no group invests in fiscal capacity, regardless of the level of resource revenue. Therefore, the model predicts that resource rents lower future tax revenue only in countries with cohesive institutions. In contrast, we find that the negative effect of oil production on future tax revenue is strongest in countries that lack cohesive institutions. Our results are not wholly inconsistent with the fiscal capacity model, however they do underscore the importance of low taxation as a means of political survival.

6. Conclusion

Using a new instrumental variables approach, we find that oil wealth impedes democracy, increases corruption, reduces taxation, and raises GDP without significantly harming the non-resource sectors of the economy. We find no evidence that oil wealth increases internal armed conflict, coup attempts, or political purges. In several specifications OLS substantially underestimates the detrimental effects of oil, suggesting that countries with better institutions disproportionately select into oil discovery and production. Controlling for the percentage of the population that was Muslim in 1950 attenuates the estimates for democracy and corruption, though oil production still appears to adversely affect these outcomes. For outcomes such as democracy and fiscal capacity, the initial strength of executive constraints appears to determine whether subsequent oil production is a curse or a blessing. However, initial institutions seem to matter less for how oil affects corruption, conflict, and purges. Despite suffering a political resource curse, countries with weak initial institutions saw the greatest economic gains from oil wealth, at least in aggregate terms.

This article's identification strategy is useful to researchers studying the long-run impact of oil wealth on any outcome in cross-country data. The strategy can also be applied to subnational analyses—granted that the geographic units of analysis are large—because the spatial distribution of sedimentary basins generates exogenous within-country variation in oil endowment. Furthermore, the general idea of the strategy—that geophysical processes provide useful identifying variation in resource wealth—may prove useful for studying the effects of mineral resources in other contexts. Recent examples in this vein include Fernihough and O'Rourke (2014) and Bartik *et al.* (2017), who exploit geological information to study the economic effects of coal and fracking, respectively.

One limitation of this study is that it does not cleanly identify how the potential economic benefits of resource extraction vary with institutional quality. The fact that countries that started with weak institutions experienced the largest economic gains from oil wealth probably owes more to the initial poverty of these countries than to the pure mediating effect of institutions. Future work should examine this mediating effect by using variation in institutions that is orthogonal to both resource wealth and economic conditions. Such an analysis may be possible at the country level using 'exogenous' democratic transitions (Pozuelo *et al.*, 2016) or at the subnational level in countries that experienced regional variation in the timing of institutional reforms, such as Indonesia (Skoufias *et al.*, 2014).

University of Alabama

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

Online Appendix
Replication Package

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