Do Natural Resources Influence Who Comes to Power, and How?*

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Abstract

Do natural resources impair institutional outcomes? Existing work studies how natural resources influence the behavior of leaders in power. We study how they influence who comes to power. Our analysis focuses on oil price shocks and local democracy in Colombia, a country mired in civil conflict. We find that when the price of oil rises internationally, legislators affiliated with right-wing paramilitary groups win office more in oil-producing municipalities. These effects are larger in conflict-ridden locations, where armed groups are poised to intervene in local elections. Consistent with such intervention, positive price shocks also reduce electoral competition: fewer centrist candidates run for office, and fewer centrist legislators are elected to office. In essence, there is diminished representation at the center. Our findings highlight how natural resources undermine democracy by distorting elections, and suggest that conflict leaves the political sector vulnerable to the resource curse.

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Introduction

Do natural resources lead countries to develop faster? Paradoxically, many have argued that they instead hinder development (Sachs and Warner 1995 and 2001; Sala-i-Martin and Subramanian 2013; Gelb 1988). A key reason behind this hindrance may lie in how natural resources affect politicians' incentives (Ross 1999; Robinson et al. 2006; Caselli and Cunningham 2009; Mehlum et al. 2006). For example, theory suggests that they may lower accountability by easing taxation (Madhavy 1970; Huntington 1991; and Morrison 2007), increasing repression (Caselli and Tesei 2011) or allowing leaders to buy off the opposition (Acemoglu et al. 2004). Alternatively, they may exacerbate redistributive demands, leading those who hold power to maintain autocracy (Boix 2003). Given these potential consequences on the political sector, many cross-national studies have tried to assess how natural resources affect democratic development. This rich literature has found mixed results[1].

Theoretical work in this area illuminates how natural resources distort politicians' behavior once in power. But do they also distort who comes to power? And, how they come to power?[2] After all, the desire to control windfall revenues may motivate influential groups to seize power through coercive strategies. These groups may constrict electoral participation in a bid to alter election outcomes. Or, they may turn to violence in the fight for power, skewing election outcomes toward those willing to use force.

This question holds global relevance — from Iraq to Nigeria to Burma, there is no shortage of countries with both natural resources and armed actors poised to intervene in electoral politics. We answer this question within one institutional context — Colombia — since this facilitates clean identification. We harness data on local elections in nearly 1,000 municipalities over 1997-2007. We determine whether movements in the international price

[1] The range of findings include: negative effects (Barro 1999; Ross 2001; Jensen and Wantchekon 2004; Ramsay 2006; Aslaksen 2008; Tsui 2011; Brückner Ciccone and Tesei 2011); heterogenous effects (Dunning 2008; Caselli and Tesei 2011; Ross 2012; and Andersen and Ross 2012); and insignificant effects (Herb 2005; Alexeev and Conrad 2009; Haber and Menaldo 2011; Wacziarg 2011). Morrison (2009) also finds that oil revenue exerts postive effects on regime stability.

[2] Caselli and Cunnginham (2009) address this question theoretically but posit that greater rents will incentivize more candidates to run for office, increasing electoral competition.

of oil influence election outcomes differentially in more oil-dependent municipalities.

This strategy poses at least three identification advantages relative to cross-national analysis. First, the price of oil is exogenous to small producers such as Colombia, but this is questionable for large producers in a global sample. Second, municipalities are more similar than countries, so there are fewer potential confounds. Third, by using standardized election outcomes, we circumvent concerns that cross-national measures such as Freedom House and Polity may not be perfectly comparable across countries.

The Colombian context offers several features that make it especially well-suited to examining this topic. Its long internal conflict has bred many illegal armed groups that seek to control both politics and rents from natural resources. For example, during our study period, left-wing guerrillas fought right-wing paramilitary groups. Both armed groups use politically-targeted violence, but paramilitaries also intervene directly in elections. They assassinate candidates, and help allied politicians gain office in exchange for favorable policies (Acemoglu et al. 2013). Recent data tracks whether legislators are affiliated with political parties that colluded with paramilitary groups (Fergusson et al. 2013).

Drawing on this data, we show that oil price shocks alter the political equilibrium. A rise in the price of oil leads to the differential election of pro-paramilitary legislators in more oil-dependent areas. These effects go hand-in-hand with lower competition in local elections: positive oil price shocks widen the vote margin of winners. Also, fewer candidates run for office, particularly from non-extreme political parties. Correspondingly, fewer centrist mayors are elected, reducing representation at the center.

We also demonstrate that violence plays a role in these distortions – which past work on the resource curse has largely ignored. First, price shocks boost aggregate municipal revenue and the presence of paramilitaries in oil areas. This is consistent with an account in which illicit groups try to control territories flush with resource rents, and intervene in elections to achieve control. Second, the election effects are stronger in locations with high levels of past conflict, where armed groups have better infrastructure for intervening forcefully.

We cast evidence against several alternative accounts. For example, the election of pro-paramilitary legislators does not accompany a general shift to the right. In fact, positive oil price shocks *reduce* the election of center-right mayors, and exert no significant impacts on the election of pro-environmental mayors. Also, there are no discernible impacts on average wages or wage inequality, which rules out economic voting.

Our analysis also examines other dimensions of the resource curse, focusing on the actions of those in power. But we do not observe significant impacts of oil price shocks on general party-level incumbency[3]. We also do not find effects on tax revenue or spending, including on public employment. This suggests a limited role of patronage networks.

Our paper complements a handful of other within-country studies of the political resource curse. Three focus on Brazil, and find effects on outcomes such as corruption (Caselli and Michaels 2013; Brollo et al. 2013) and incumbency over the short run (Monteiro and Ferraz 2012). Looking in the U.S., Goldberg et. al (2008) also suggest that resource dependence affects sitting governors' vote shares. The biggest difference between these papers and our paper is that we examine the role of extra-legal forces. Our findings highlight how natural resources can induce groups to use extreme strategies, distorting who holds power as measured by political affiliation[4].

These results are in one institutional context. Do they apply more broadly? To get a preliminary sense of this, we look at cross-country data. Using x-polity scores (Vreeland 2008), we again find that oil price shocks exert larger effects on democracy among countries that experienced more conflict at the outset of the sample period. These results are of course only meant to be suggestive correlations since a full cross-country analysis would require its own attention to identification and causality concerns.

Still, seeing a similar pattern internationally— of a stronger resource curse in conflict areas — does support the view that Colombia is not an anomalous example.

[3]Mayors can't run for immediate re-election, which restricts potential incumbency in our context.

[4]Monteiro and Ferraz (2012) and Brollo et al. (2013) look at candidate characteristics such as education, but neither considers the party affiliation of elected legislators.

In the remainder of the paper, we provide background on the Colombian context; lay out the mechanisms linking oil prices to elections; describe the empirical strategy and data; present the results; and conclude.

Background

In this section, we provide an overview of democracy, oil production and political conflict in Colombia.

Democracy and Local Government. Colombia has held national elections regularly since independence, except for two periods of military intervention (the last of which ended in 1958). Since 1988, the direct popular election of mayors, governors and local councils have taken place at regular pre-determined intervals of two to four years. We focus on the election of mayors and councils as these positions vary at the municipality level, and we aim to identify the impact of municipal oil dependence[5].

Notably, mayors are not allowed to run for immediate re-election (Dávila 2009) which limits individual-level incumbency[6]. Mayors are also considered more powerful than the councils. Given their limited functions,[7] a recent debate has even emerged about abolishing these entities[8]. Council size varies by municipal population. Elections for council positions are municipality-wide, and candidates are elected through a list system.

The Oil Sector. Oil is Colombia's largest export. Over the period of our analysis, municipalities received revenue from oil production based on the following allocation process. Foreign oil companies operating in Colombia were required to pay the government royalties amounting to 50 percent of their oil export values. An explicit revenue sharing agreement divided these royalties across the central, departmental and municipal governments[9]. The

[5] Governors are department level positions, and there are 33 such departments.

[6] Non-consecutive re-election is also relatively uncommon, occurring in only 5 percent of the elections events in our sample.

[7] http://www.citymayors.com/mayors/colombian-mayors.html.

[8] http://www.eltiempo.com/archivo/documento/MAM-1305716

[9] The government places 80 percent of the oil royalties into an Oil Stabilization Fund. As codified in

amount given to each municipality was proportional to its production level. As of 1996, royalty revenues from oil and other natural resources, termed "regalias", have been categorized separately in the fiscal accounts.

The Colombian Conflict. The Colombian conflict includes three key actors: leftist guerrillas, right-wing paramilitary groups, and the state. The guerrilla insurgency was launched in the 1960s. During our sample period, it was led by the Armed Revolutionary Forces of Colombia (FARC) and the National Liberation Army (ELN)[10]. Independent paramilitary groups emerged to fight the guerrillas during the late 1980s. In 1994, the Peasant Self-Defense Forces of Córdoba and Urabá (ACCU) began crafting regional alliances with other paramilitary groups. They formed an umbrella organization in 1997 called the United Self-Defense Forces of Colombia (AUC). The paramilitaries were declared illegal as they violently targeted civilians after this consolidation. While some factions colluded unofficially with the military brigades (HRW 2000), they held no official affiliation with government forces.

Both the guerrillas and paramilitaries rely on the cocaine trade for financing. They also siphon rents from natural resources and aim to control territories with these resources. Commodity price shocks have been a key determinant of violent attacks by armed groups, with different actors specializing in predation over different natural resources (Dube and Vargas 2013). Paramilitary groups are known to be particularly strong in the oil region. Audits show that oil and gas royalties are often missing from municipalities where they exert influence (HRW 2005a). Armed groups siphon revenue either by colluding with aligned politicians, or extorting resources under threat of force, for example, by kidnapping and assassinating mayors (El Tiempo 2007)[11]. The guerrillas also blow up oil pipelines, which is another way

Law 141, of the remaining amount, 32 percent goes to the central government, 47.5 percent goes to the department, and 12.5 percent goes to the municipality.

[10] Other revolutionary groups such as M-19 and Quintín Lame also joined during the 1980s. Most of these other groups demobilized and formed political parties in the early 1990s. For example, the M-19 movement formed the M-19 Democratic Alliance political party.

[11] This form of budgetary predation became especially important after a major decentralization in 1991 transferred more fiscal resources to local governments (Sanchez and Palau 2006). As such, decentralization of fiscal resources to the municipal level has been shown to increase conflict (Chacón 2014).

in which resources and armed groups may be connected.

Paramilitary Intervention in Elections. When the AUC was launched, the paramilitary groups made a strategic decision to influence electoral outcomes. They formed explicit pacts with politicians to support particular candidates. For example, the Pacto de Ralito called for a "refounding of the country" and was signed by prominent paramilitary leaders and more than 50 politicians including senators, mayors and local councilors (Lopez and Sevillano 2008). The large number of links between paramilitaries and politicians were revealed by the media in the "para-politics scandal".

The evidence indicates that paramilitary organizations used many strategies to achieve their political ends[12]. They helped allied candidates by providing illegal financing, or by intimidating and assassinating opponents (Lopez 2010)[13]. They also bought votes, stuffed ballots, and used voter IDs of the deceased (Valencia 2007). Additionally, they coerced voters by threatening violence or carrying out massacres. Sometimes, they did this to get votes for their preferred candidates; other times, to keep people from voting (BBC, 2002).

Which politicians enjoyed paramilitary support? As documented by Acemoglu et al. (2013), the small, new political parties that emerged in Colombia in the 2000s were the parties that came to be affiliated with paramilitary groups — where paramilitaries were present, vote shares for these third parties increased. Fergusson et al. (2013) also present evidence that greater media exposure had no impact on the degree of collusion.

Mechanisms

There are several different pathways through which natural resources can affect institutional outcomes. They may influence the process through which leaders win office, and thus,

[12] Much of this evidence comes from the confiscation of a laptop belonging to the paramilitary leader, "Jorge 40". See: http://www.semana.com/on-line/articulo/el-computador-jorge-40-puede-inicio-nuevo-proceso-8000/81379-3

[13] As an example, Jorge 40's computer revealed a recording of Carlos Maria Garcia Davila, a fellow paramilitary member, coordinating with politicians on important electoral campaign in the Caribe Coast (Pedraza Saravia and Olaya 2011).

determine who gains power. Alternatively, they may influence the actions of those who already hold office. We examine each of these pathways below.

The Rise to Power

When the price of oil rises, more rents are up for grabs in oil-producing areas. In countries such as Colombia, with explicit sharing agreements, greater revenue accrues within the coffers of oil-rich municipalities. This boosts the value of controlling these locations, incentivizing armed groups to wrest political control. Indeed, past theoretical work shows that stealable resources promote conflict owing to predation incentives (Grossman 1991; Dal Bó and Dal Bó 2011).

The bid to wrest control, in turn, motivates armed groups to intervene in elections. Getting favored candidates into office has clear benefits, since these officials are the key to accessing municipal revenue. In Colombia, mayors decide on the allocation of public contracts. They are also positioned to divert public funds toward allies.

Armed groups can accomplish their goal of manipulating elections through several strategies. First, they can finance and support politicians from parties aligned with their political agenda or intimidate and even assassinate politicians from non-aligned parties. Targeting potential contenders will reduce the number of candidates running from other parties. This scare-off effect will serve to reduce the competitiveness of elections.

Second, armed groups can also manipulate the electorate. They can either intimidate voters to keep them away from the polls, or buy their votes[14]. Both the reduction in candidates and manipulation of voters will in turn, influence the political affiliation of elected legislators. This is the political selection effect.

But, what types of candidates will get selected into office? In our institutional context, the paramilitaries are known to operate more in oil areas and explicitly target elections. This generates two additional implications. If electoral intervention is the work of paramil-

[14]In fact, both strategies have been documented in Colombia. Thus, the impact on voter turnout is theoretically ambiguous in this context.

itary groups, then positive oil price shocks should also increase officials aligned with pro-paramilitary parties in oil-dependent areas. There should also be a corresponding increase in the presence of paramilitary groups.

Finally, the decision to target elections may interact with the presence of armed conflict. Violent groups may already have a base of operations in locations with recurrent conflict; and, they may have more tacit knowledge of how to target opponents in these places. Both factors will lower the cost of forceful intervention in these places. Thus oil price shocks should exert larger effect in locations that have previously experienced more conflict.

The Actions of those in Power

Natural resources may also affect institutional outcomes through the actions of those who already hold power. Under the canonical rentier mechanism, natural resources allow officials to buy off political support through light taxation and increased spending (Madhavy 1970; Huntington 1991), particularly on patronage. For example, Robinson et al. (2006) theoretically show that politicians inefficiently expand public sector employment during resource booms. These spending and taxation patterns are held to lower accountability as citizens become politically disengaged and make weak demands for representation.

If rentier mechanisms are at play, we should see lower tax revenue in municipal coffers as municipal governments lower taxes in non-resource sectors; and, we should see significant increases in total spending, particularly on municipal employment.

A number of theories also posit that natural resources entrench leaders. As Boix (2013) points out, these resources are fixed factors, for which there is little threat of exit. This feature exacerbates demands for redistribution, curbing leader's incentives to allow democratic politics. Other accounts of entrenchment emphasize that office-holders can spend resources buying off the opposition (Acemoglu et al. 2004), or repressing their opponents (Caselli and Tesei 2011; Ross 2011).

If natural resources equip office-holders with the incentives and means to retain office,

then we should observe general incumbency advantages in elections outcomes. Although individual-level incumbency is legally restricted in our context, party-level incumbency is possible.

Predictions

In summary, the accounts above imply five empirical predictions. First, if leader selection is important then positive oil price shocks should boost the election of pro-paramilitary legislators, and these effects should be larger in high conflict locations. Second, if these effects reflect intervention by paramilitary groups, oil price shocks should lower electoral competition. Third, they should boost paramilitary activity in oil rich areas.

Fourth, if rentier type effects are at play, then positive oil price shocks should differentially lower tax revenue and increase municipal spending in areas such as public employment. Fifth, if general entrenchment effects are important, then these price shocks should lead to more re-election of incumbent parties.

Importantly, all of these effects should be strongest in municipalities that produce more oil as positive oil price increases are, by definition, larger in these areas.

In testing the predictions on political selection, we draw on both mayoral and local council elections. In testing competition, we focus on just mayoral elections, for which we have better measures. The margin of victory can only be defined for these races. Also, the interpretation of candidates running for office is cleaner since council elections occur through a list system.

Empirical Strategy

We use a difference-in-differences empirical strategy to test our predictions. We assess whether changes in the international oil price exert differential impacts among municipalities that produce more oil.

Our cross-sectional variation is based on the amount of oil produced in each municipality in 1993. We define oil dependence as the value of oil produced in per capita terms, in 1993[15]. During that year, 57 municipalities produced oil. Figure A.1 in the Online Appendix shows the quartiles of oil production across municipal locations.

This variable circumvents endogeneity concerns for a number of reasons. It reflects the spatial distribution of oil reserves and precedes the start of the sample period. Thus, it does not reflect potentially endogenous oil discovery or extraction undertaken in response to election outcomes. It also precedes the 1997 election by several years, which minimizes concerns that it reflects extraction decisions made with the aim of influencing political outcomes in the run-up to the first election in our sample. Finally, it precedes paramilitary consolidation which started with the expansion of ACCU in 1994, and culminated in the formation of the AUC in 1997.

The time variation in our empirical strategy is the international price of oil. Importantly, this price is exogenous to Colombia's production, as the country holds less than one percent of the world oil market. Figure A.2 shows the oil price over time.

The estimating equation that represents our empirical strategy is:

$$y_{jrt} = \alpha_j + \beta_t + \delta_r t + (Oil_{jr} \times OilPrice_t)\lambda + \mathbf{X}_{jrt}\phi + \varepsilon_{jrt} \quad (1)$$

where y_{jrt} are elections-related outcomes in municipality j, region r and year t; α_j are municipality fixed effects; β_t are year fixed effects; and $\mathbf{X}_{jrt}$ are time-varying controls which always include the natural log of population. Oil_{jr} is the oil dependence of municipality j in region r during 1993; $OilPrice_t$ is the natural log of the international price of oil in real terms in year t. λ captures the differential effect of the oil price on political outcomes in municipalities producing more oil. Note that the constituent terms do not appear in equation (1) since municipality fixed effects control for and absorb the municipal-level Oil_{jr} variable

[15] This is defined as barrels of oil produced in each municipality in 1993, multiplied by the per barrel international oil price in 1993, scaled by the municipal population in millions 1993.

while year effects control for and absorb the annual-level $OilPrice_t$ variable[16].

$\delta_r t$ are linear time trends in the four major geographic regions[17]. These trends account for the fact that natural resources are concentrated in particular regions which may have experienced different trends in institutional outcomes, based on varying rates of economic growth or armed group presence. For example, oil is concentrated in the Southeastern region, and guerrilla presence may have increased there in the latter part of our sample period, when the government seized control of the Demobilized Zone (DMZ), pushing the FARC eastward toward Venezuela[18].

We estimate equation (1) using OLS. Since boundaries changed in a potentially endogenous manner over this period, we use a concordance (Dube and Vargas 2013) to aggregate municipalities to their boundaries in 1988, a pre-sample period year. In all specifications, standard errors are clustered at this original municipality level to control for serial correlation over time within these units.

Data

We utilize data on oil production, prices and a number of elections-related outcomes. Data on mayoral and local council elections come from the Colombian national elections council, Registraduría Nacional del Estado Civil[19]. In our sample period, elections were held in 1997, 2000, 2003 and 2007. We follow the standard elections data and avoid using irregular elections that didn't occur on the official election day since their timing may be endogenous. In examining the political competition channel, we use the number of candidates running for office in mayoral elections. We also calculate the margin of victory, defined as the difference in vote share between the elected candidate and first runner-up. In addition, we

[16] We examine the effect of prices in levels versus growth since a growth specification may lead to an excess focus on short-run effects by capturing only year-to-year changes.

[17] These are: Andean, Caribbean, Southeastern and Pacific.

[18] The DMZ comprises five municipalities in Southern Colombia that the FARC were allowed to administer over 1999-2002.

[19] http://www.registraduria.gov.co/

classify whether winners of mayoral elections are from center-right, center-left, or extreme left political parties. Extreme left parties include the socialist party, Polo Democrático (Polo), and those historically associated with the guerrillas such as the UP or M19. As we discuss below, our results are insensitive to removing Polo from this classification.

We also define whether elected municipal officials are affiliated with pro-paramilitary parties on the basis of an original event-based dataset collected by Fergusson et al. (2013). This extraordinary data records all news events from *El Tiempo,* Colombia's leading newspaper, in which national-level politicians in the Congress are accused of collaborating with paramilitary groups over 1997-2010. We follow the two-fold approach of Dube and Vargas (2013) to generate our measures. First, a party is coded as pro-paramilitary for the duration of the sample if one of its national-level politicians is accused of paramilitary collaboration in the Fergusson et al. data. This party classification is then combined with the Registraduría data to code the share of local councils from pro-paramilitary parties, and whether the mayor is from a pro-paramilitary party.

This approach can generate measurement error along two dimensions. First, it is possible that a national-level legislator may have colluded with paramilitary groups, but the rest of his party is not paramilitary aligned. This is unlikely to be pervasive for the following reasons. First, national legislators tend to be leading, influential members of their political parties, so their position typically serves as a good proxy for the positions of other party members. This is especially true for our context, since the paramilitary affiliated parties typically tend to be small (Acemoglu et al. 2013). Second, as discussed in the background, paramilitary groups and political parties embarked on a coordinated effort to influence politics, with national level politicians signing documents such as the *Pacto de Ralito.* Implementing these plans would require coordination within parties, rather than one-off acts of collusion between isolated politicians and armed groups.

A second type of measurement error may arise because there are parties running in local elections that do not have national level representation. If some of these parties are

pro-paramilitary, they may be missing from the classification. Suppose this measurement error is like white noise, simply adding mean zero error. This would create noisy estimates, biasing our results toward zero. Suppose that the measurement error had some systematic bias, such as over or under-measuring pro-paramilitary parties. If this mismeasurement is uncorrelated with oil dependence, again there would be no problem. Finally, suppose this mismeasurement were correlated with oil dependence. Even in this case, it cannot produce our results: while our empirical strategy includes both cross-sectional variation and time variation, we are careful to hold the the pro-paramilitary classification constant over the sample period. As a result, even a correlation of this error with oil dependence would not bias the results since the pro-paramilitary measure itself can't respond to changes in oil prices[20]. Also, note that municipality fixed effects sweep out any time-invariant effect such as the general accurracy with which parties can be classified in each municipality.

Our data on municipal spending and revenue, including on natural resource regalias, span 1997-2005 and come from the National Planning Department (NPD). We also use data on paramilitary and guerrilla activity, which are available over the same period. We define whether paramilitaries, FARC or ELN are active in a given municipality-year based on data from the Center for Study of Economic Development (CEDE)[21], which records whether these groups undertook activities such as arson, attacks on private property, kidnappings, blocking transport routes, injuring members of the armed forces, or carrying out political homicides.

Our conflict data is from Dube and Vargas (2013) and originates from the Conflict Analysis Resource Center (CERAC). This data covers war-related episodes in over 950 Colombian municipalities over 1988-2005. It is event-based, drawing from 25 major newspapers, and

[20] This is also why we would want to avoid using a measure that classifies individual local-level politicians as paramilitary-affiliated. Consider two mayors colluding with paramilitary groups, one of whom is in an oil municipality. When the price of oil rises, and oil areas become more valuable, greater scrutiny by the media and political adversaries may lead to higher rates of discovery of paramilitary collusion for the politician in the oil area. Our time-invariant party-level measure circumvents this type of potentially endogenous classification.

[21] This dataset originates from the Observatory of Human Rights of the Vice-Presidency of Colombia and is constructed on the basis of reports from the Administrative Department of Security (DAS), the Colombian security agency.

oral reports on political violence from a network of Catholic priests operating in rural areas. We use three measures of conflict: number of paramilitary attacks, guerrilla attacks and total clashes between the armed groups and the armed groups and the state. These are used to define a high conflict sample, which includes municipalities in which any of these variables exceed their mean over 1988-1992.

We also use income data from household surveys called the *Encuesta Nacional de Hogares* (ENH). This survey is carried out by the Departamento Administrativo Nacional de Estadistica (DANE), and includes data on a representative sample of 23 departments in the four regions of Colombia. The employment module gathers repeated cross-sectional data on labor market outcomes. We conduct our analysis with individuals above age 14, the official working age in Colombia. We use data from 1998-2005, when earnings data are collected in a comparable manner[22]. We divide real monthly earnings for wage and salaried workers by monthly hours to obtain the (log) hourly wage in real terms. We trim out the top and bottom 1% outliers in this variable and also use it to generate two municipal-level variables: the (log) ratio of wages at the 90th vs. 10th percentile of the municipal wage distribution; and the equivalent ratio for wages at the 75th vs. 25th percentile of the distribution.

In terms of our independent variables, our measure of oil production comes from the Ministry of Mines and Energy (MME)[23]. This is defined as the average daily production of barrels of crude oil, in hundreds of thousands of barrels, in each municipality in 1993. The international price of crude oil is obtained from the International Financial Statistics (IFS) and is measured in thousands of 2012 pesos per barrel. We also use data on coca production in 1994 from the Dirección Nacional de Estupefacientes (DNE), and data on municipal population from DANE[24]. Table A.1 in the Online Appendix presents the descriptive statistics of the key variables in the analysis. Approximately 15% of mayorships and 11% of local councils are composed of pro-paramilitary legislators over this period.

[22]The ENH survey was also conducted in 1996 and 1997 but the earnings measures from these years exclude in-kind earnings and are thus not directly comparable to remaining sample years.

[23]http://www.minminas.gov.co/

[24]https://www.dane.gov.co/

Characteristics of Oil and Non-Oil Municipalities

Table 1 examines the cross-sectional characteristics of municipalities that did and didn't produce oil in 1993[25]. The table shows that measures of poverty and service delivery, including the number of hospitals, tax offices and police stations in 1997, do not vary significantly across these two groups.

However, the table also suggests that oil locations are larger as measured by population, and differ on geographic dimensions such as height. Though the indicator of coca cultivation does not vary significantly at conventional levels, the mean difference (4%) is substantial considering the overall sample mean of 7%. These differences could confound the effects if electoral dynamics differ in the coca belt, highly populated areas, or geographic areas with oil, in a manner correlated with the price of oil. We therefore control for time-varying (log) population; as well as elevation and the 1994 coca indicator interacted with the price of oil. These latter interactions constitute our additional controls.

Results

Oil Price Shocks and Pro-Paramilitary Legislators

We examine the effect of oil price shocks on institutional outcomes by estimating equation (1) . Table 2 gauges the impact on the election of politicians from pro-paramilitary parties. The first two columns look at mayoral elections, and the next two columns look at local council elections[26].

The results show that when the price of oil rises, pro-paramilitary legislators get elected differentially in more oil-dependent municipalities. The results do not change with the inclusion of our additional controls, and the effects are substantial. The coefficient of .081 in

[25] The picture looks the same if we instead compare municipalities above and below mean oil dependence in 1993.

[26] The pro-paramilitary council share variable is unavailable for 2007. Thus our analysis of council outcomes include the 1997 to 2003 elections.

column (2) tells us that a 10% increase in the price boosts the likelihood of a pro-paramilitary mayor by .008 more in the average oil municipality (with oil dependence 1.01), as compared to a non-oil municipality. This represents a 5.5% increase above the pro-paramilitary mayor mean (.15). Since the price of oil rose by 130% over 1997-2007, this implies a 72% greater chance of getting at least one pro-paramilitary mayor over the four elections in our sample. This is a per-election effect of 18%.

The effect on the council share is also substantial, though smaller in magnitude. The coefficient in column (4) suggests that a 130% price hike differentially increases the share of pro-paramilitary councilors by 39% above the mean (of .11). This is a per-election effect of 9.8%. The difference in magnitudes may arise because mayors are more powerful than local councils. So, it would be more strategic for armed groups to target mayorships.

The Role of Conflict

If the impacts on pro-paramilitary legislators reflect intervention by the armed groups, these effects should be stronger in locations with high levels of violence. This is where armed groups should have more experience and infrastructure for carrying out forceful interventions.

To test this, we define a high conflict sample where armed group attacks or clashes exceeded their average levels in the pre-sample period (1988-1992)[27]. A total of 37 percent of our municipalities fall into this group. Average oil dependence is higher here, but 19 of the 57 oil-producing municipalities are actually located in the low conflict sample. Thus, there is meaningful variation in oil presence within both groups.

Columns (5)-(8) of Table 2 examine the split sample effects. The results show a clear pattern. Oil price shocks exert larger and more precisely estimated effects in the high conflict sample, for both election outcomes. The estimate in column (8) suggests that a 130% price

[27] 1988 is the first year for which conflict data is available, and 1992 is the year preceding the 1993 oil production measure. Specifying this time period enables us to capture the dramatic rise in violence in the first half of the 1990s. For example, average total clashes increased 3-fold from .19 to .57 between 1988 and 1992. The pattern of results also look the same if we measure conflict for other time periods such as the first three years through 1990.

increase results in a 51% differential increase in the pro-paramilitary council share variable (or 13% per-election effect), in the high conflict sample. In contrast, the coefficient in column (7) is negative and statistically insignificant for the low conflict sample. Overall, the results from Table 2 provide strong support for our first empirical prediction, that oil price shocks will induce political selection, and that the effect will be larger in places that have experienced violence.

Political Competition and Representation at the Center

To explore if our main effects reflect armed group interference in elections, we analyze the competitiveness of local elections. We calculate the vote margin of the winner and track the number of candidates running for mayoral elections[28].

Table 3 presents these results. As shown in column (1), the oil price shock widens the margin of victory substantially. The coefficient implies that a 130% oil price increase induces a differential per-election effect of 4.7%. We also see effects on the number of candidates. On average, there are 3.9 candidates running for mayoral elections. The coefficient of -.239 in column (2) tells us that a 130% oil price rise will induce a 2% per-election reduction. These two effects, on the margin of victory and candidates, provide strong support for our second empirical prediction, on the competitiveness of local elections.

In the next three columns we decompose the candidate effect by examining those affiliated with the extreme political parties as well as the non-extreme center. Columns (3) and (4) show that there are no significant reductions in candidates from either pro-paramilitary parties or extreme left parties, which includes those historically associated with the guerrillas. In contrast, column (5) shows that there were significant reductions in candidates from the other political parties comprising the non-extreme center.

These effects are consistent with the scare-off effect, in which paramilitary groups suc-

[28] As discussed earlier, we focus on the mayoral elections for this analysis since the vote margin of the winner can be defined, and because the council elections occur via list systems, which makes it difficult to interpret results around the number of candidates running for office.

cessfully drive contenders out of office. The non-effects on extreme-left candidates square with strategic incentives: areas where pro-guerrilla mayors stand a chance of winning would be extremely costly to convert into pro-paramilitary wins, so targeting the center is more strategic.

Does this scare-off effect translate into fewer mayorships controlled by centrist parties? Columns (6)-(8) of Table 3 show that there are in fact, significant reductions in the election of center-right and center-left mayors. In contrast, there are no impacts on extreme-left mayors[29]. Taken together, these results suggest that oil rents facilitate the rise of pro-paramilitary mayors, while pushing out centrist mayors. As such, they document a hollowing of the center.

Revenue and the Presence of Armed Groups

Do the effects on political selection and competition reflect the work of armed groups seeking control of revenue in oil-rich areas? To answer this question, Table 4 gauges whether oil price shocks influence revenue and the likelihood that paramilitary and guerrilla groups are active within oil-dependent municipalities.

Columns (1)-(2) demonstrate substantial increases in both regalias revenue and total revenue. The coefficients suggest that a 130% oil price hike boosts these outcomes by 26% and 9% more in the average oil municipality, as compared to a non-oil municipality, over the sample period. Columns (3)-(5) also show that the oil price shock differentially increased the likelihood of paramilitary activity, without affecting the likelihood of either FARC or ELN activity. These results support our third empirical prediction, on revenue and the activation of paramilitaries in oil areas.

Since we conceptualize revenue to be a key mechanism, an alternative approach is instrumenting revenue with the oil price interaction. We then lose the 2007 election, since we don't have revenue data for that year. Appendix Table A.2 presents these results for our

[29] These results do not change if we re-classify Polo, the socialist party, as center-left vs. extreme-left. These estimates are available upon request.

main outcomes (pro-paramilitary mayor, council share, margin of victory, candidates, and paramilitary activity). All of our effects remain significant, verifying the robustness of the results to this alternate approach.

Other Aspects of the Resource Curse

In Table 5, we examine other dimensions of the resource curse stemming from the actions of those who hold office. First, we look at rentier effects. Columns (1)-(3) look at tax revenue, total spending and spending on personnel in municipal governments. We find no significant effects on these variables. For total spending, the coefficient is positive, but statistically indistinguishable from zero. For spending on personnel, the coefficient is negative, suggesting limited impacts on patronage networks. Second, we consider whether oil price shocks lead to general incumbency effects in mayoral elections. But, in column (4), we find that there is no greater tendency to re-elect whichever party is in power[30]. For this institutional context, we find little evidence of our fourth and fifth empirical predictions.

Alternative Accounts

Finally, we subject our results to further robustness checks and present evidence against alternative accounts.

Figure A.2 shows that the price of oil rose linearly for the majority of our sample period, with the exception of two dips. If institutional outcomes also trended differentially in oil producing municipalities, for some reason besides the oil price rise, this could potentially confound our estimates. While our main specifications control for linear trends by region, in Table A.3, we add linear trends by whether municipalities produced oil in 1993. This could be an over-control since these trends will partly control for electoral trend responses to changes in the oil price. However, we find that all of our main outcomes remain statistically

[30]This restricts our elections sample to the 2000-2007 period. We verify that our main effect holds with the same magnitude and remains statistically significant in this sub-sample.

significant with the exception of the pro-paramilitary council share, for which we have a smaller sample. Even this estimate is not statistically distinguishable from the baseline specification in Table 2-column (4) at the 5% level.

If paramilitary groups were already more active in the oil region in the beginning of the period, then it could be their presence, rather than oil dependence, that drives the estimated effects. To account for this confound, we measure whether any armed group (FARC, ELN or paramilitary) was active in each municipality over 1988-1992, and derive an equivalent measure for whether paramilitaries (only) were active in this pre-period. In Table A.3, we control for the interaction of each of these variables with the oil price. Our results remain unaffected.

Another alternative account posits that oil price shocks may lead to the election of pro-paramilitary legislators by shifting voter's preferences to the right, rather than influencing armed group activity. For example, if oil price hikes boost wages or make the wage distribution more unequal, this could lead to greater support for right-wing parties favoring conservative economic policies. Alternatively, if higher oil value is perceived to generate economic benefits, this may generate backlash against those with more pro-environmental leanings.

We present three pieces of evidence against this alternative mechanism. First, Table 5 examines the impact on wages and wage distribution, drawing on household-level survey data over 1998-2005. Column (5) presents an individual-level wage regression, controlling for standard demographic variables[31]. This shows that oil price shocks did not exert significant effects on average wages within a given municipality. This is consistent with the fact that the oil sector is not labor intensive, and therefore employs relatively few individuals within the municipal workforce. Columns (6)-(7) present results on the ratio of wages at the 90th vs. 10th percentile of the municipal wage distribution, and the 75th percentile vs. 25th percentile of this wage distribution. These are municipal-level regressions which control for

[31] These are: gender, age, age squared, whether the respondent was married and years of education.

municipal averages of the demographic variables in column (5). These results also show there were no discernible impacts on wage dispersion. This casts doubt on the idea that our results reflect changes in economic conditions that shift voters to the right.

Second, if oil price changes shifted environmental preferences, this should influence the election of legislators from the Colombian Green Party. Yet, in columns (8)-(9) of Table 5, we show that there were no significant effects on either the number of Green Party candidates or the election of Green Party mayors. Third, Table 3-column (6) showed that oil price shocks *reduced* the election of mayors from center-right political parties. This directly counters the idea that voter preferences shifted to the right more generally.

Another account suggests that the oil shock may have led to the election of pro-paramilitary politicians by generating backlash against the guerrillas for their role in blowing up oil pipelines. However, Table 4 showed that oil price shocks do not significantly boost guerrilla activity in the oil region. Also, we do not observe decreases in the number of extreme left candidates or mayors (in Table 3). Both results are inconsistent with the backlash account.

External Validity

The results above provide robust support for the idea that oil price shocks induce armed groups to intervene in local elections, distorting the democratic process, particularly in conflict-ridden locations. However, these results are from one institutional context, which raises concerns regarding their external validity.

There are of course many qualitative accounts of countries where armed groups predate on natural resources and intervene in electoral politics. In Nigeria, paramilitary groups have fought over oil revenue, and intervened violently in elections to support particular candidates over the past two decades (HRW 2005b). In India, the Maoist Naxalites, who target mining areas, have used bombings to disrupt recent elections (Chawla 2015). In Afghanistan, the Taliban continue taking cuts from mining (Vittori 2014) and remain resolute in curbing electoral participation.

Quantifying these patterns through a complete cross-national analysis is beyond the scope of our paper. But we present some suggestive correlations using cross-country data. We provide more detail in the Online Appendix.

We use data on x-polity, a democracy measure developed by Vreeland (2008) which purges polity of components directly related to conflict. We combine this with data on oil reserves, oil prices, and civil war (from PRIO), which record whether countries experienced a conflict event which resulted in at least 25 or 1000 battle-related deaths[32]. We split the sample by whether countries had any conflict over the 1950s and 1960s, defining this as our pre-period[33]. Table A.5 shows the descriptive statistics of these variables.

We estimate a cross-country regression which closely matches our within-country specification. We interact oil reserves per capita (averaged over the pre-period) with the international oil price, controlling for country and year fixed effects, as well as income per-capita[34]. The sample period covers 1970-2004. Table A.6 presents the results. As in Colombia, we again find that oil price shocks exert a significantly larger effect on democracy in the conflict sample, using either the Prio 25 or Prio 1000 measure. By showing the same pattern in the international data as in the Colombia data, these estimates suggest that our findings are not anomalous to Colombia.

These specifications should be taken as exploratory since they do not solve potential endogeneity concerns, especially regarding the international price of oil. Also, much of our sample is from after 1980, when governments capture oil rents, and the oil curse tends to emerge (Andersen and Ross 2014). Consequently, our cross-national findings may not hold over longer time-horizons, where there is scant evidence of the resource curse (Haber and Menaldo 2011). However, the concentrated effects we observe in conflict locations may also help explain why the resource curse is not a ubiquitous phenomenon. This should be the

[32]See Appendix for greater detail on data sources and variables.

[33]PRIO data begins in the 1950s, but the first decade doesn't include many conflict events. Extending the pre-period into the 1960s allows us to capture more conflict-affected locations. But we find the same pattern of results if we define the pre-period as ending in 1965.

[34]Since income is potentially endogenous to oil, we also verify that the results are not dependent on the inclusion of this control.

subject of further research.

Conclusion

This paper has examined how natural resource dependence influences institutions using a within-country approach. While much of the past literature has focused on incumbent behavior, we ask whether resource reliance can influence the electoral process, and determine who comes to power.

Focusing on Colombian politics, we find that oil price shocks reduce electoral competition and promote the election of legislators from right-wing pro-paramilitary parties. These effects are larger in conflict-affected locations, and correspond to the higher presence of paramilitary groups in oil-rich areas. Our results are consistent with an account in which armed groups intervene forcefully in local elections with the aim of controlling resource rich regions. Overall, they suggest that the combined effect of conflict and natural resources may prove particularly inimical to local democracy.

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Table 1: Charactersitics of Oil and Non-Oil Municipalities

	Mean in oil municipalities	Mean in non-oil municipalities	Difference
Unsatisfied Basic Needs 1993 (poverty index)	53.108	55.217	2.109
Secondary education 1993	0.45	0.472	0.022
Police stations, 1997	37.807	47.563	9.756
Judicial offices, 1997	81.265	104.843	23.578
Post offices, 1997	41.04	43.369	2.329
Rural development banks, 1997	46.711	45.895	-0.816
Schools, 1997	283.97	430.737	146.767
Hospitals, 1997	21.063	24.889	3.826
Tax Offices, 1997	17.757	19.924	2.167
Length of primary rivers	272,554.29	349070.615	76,516.330
Land inequality (gini)	0.687	0.696	0.009
Located in Demilitarized Zone (indicator)	0.004	0	-0.004
Log population 1997	-4.299	-3.75	0.549***
Elevation	1,271.46	446.459	-825.003***
Cultivated coca, indicator, 1994	0.048	0.088	0.040

Notes. Oil refers to municipalities that produced oil in 1993. Non-oil refers to municipalities that did not produce oil in 1993. *** is significant at the 1% level, ** is significant at the 5% level, * is significant at the 10% level.

Table 2: Oil Price Shocks and the Election of Pro-Paramilitary Legislators

	(1) Pro-para mayor	(2) Pro-para mayor	(3) Pro-para council share	(4) Pro-para council share	(5) Pro-para mayor	(6) Pro-para mayor	(7) Pro-para council share	(8) Pro-para council share
Oil dependence × log oil price	0.0811***	0.0806***	0.0407**	0.0330**	0.0473	0.0831***	-0.0276	0.0400***
	[0.022]	[0.022]	[0.016]	[0.015]	[0.079]	[0.021]	[0.030]	[0.015]
Observations	3,659	3,659	2,964	2,964	2,290	1,369	1,862	1,102
Number of municipalities	959	959	998	998	599	360	626	372
Election Years	1997-2007	1997-2007	1997-2003	1997-2003	1997-2007	1997-2007	1997-2003	1997-2003
Additional controls		Y		Y	Y	Y	Y	Y
Sample	**Full**	**Full**	**Full**	**Full**	**Low conflict**	**High conflict**	**Low conflict**	**High conflict**
Mean oil dependence	1.01	1.01	1.01	1.01	0.7	1.18	0.7	1.18

Notes. Standard errors clustered at the municipality level are shown in parentheses. Variables not shown in all specifications include municipality and year fixed effects, linear trends by region and log of population. Additional controls include the interaction of elevation and municipalities cultivating coca in 1994 with the price of oil. The high conflict sample includes municipalities where average attacks/clashes exceeded the mean between 1988-1992. Mean oil dependence is the mean for municipalities that produced oil in 1993. *** is significant at the 1% level, ** is significant at the 5% level, * is significant at the 10% level.

Table 3: Political Competition and Representation at the Center

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Margin of victory	Number of candidates				Mayor		
		All	*Pro-para*	*Extreme-left*	*Other*	*Center-right*	*Center-left*	*Extreme-left*
Oil dependence × log oil price	0.024***	-0.239***	0.037	0.011	-0.217**	-0.056***	-0.047***	0.002
	[0.009]	[0.084]	[0.045]	[0.048]	[0.095]	[0.019]	[0.018]	[0.004]
Observations	3,597	3,712	3,703	3,702	3,703	3,710	3,710	3,710
Number of municipalities	957	967	965	965	965	967	967	967
Election Years	1997-2007	1997-2007	1997-2007	1997-2007	1997-2007	1997-2007	1997-2007	1997-2007
Additional Controls	Y	Y	Y	Y	Y	Y	Y	Y

Notes. See Table 2 for table notes.

Table 4: Oil Price Shocks, Revenue and the Presence of Armed Groups

	(1) Log regalias revenue	(2) Log total revenue	(3) Paramilitary activity	(4) FARC activity	(5) ELN activity
Oil dependence × log oil price	0.201***	0.068**	0.024*	-0.018	-0.022
	[0.063]	[0.030]	[0.013]	[0.015]	[0.015]
Observations	3,427	8,234	9,063	9,063	9,063
Number of municipalities	683	1,007	1,007	1,007	1,007
Years in sample	1997-2005	1997-2005	1997-2005	1997-2005	1997-2005
Additional Controls	Y	Y	Y	Y	Y

Notes. See Table 2 for table notes.

Table 5: Examining Alternative Accounts and Theories of the Resource Curse

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Other Aspects of the Resource Curse				**Alternative Accounts**				
	Log tax revenue	Log total spending	Log personnel spending	Party re-elected	Log wage	Log wage ratio (90/10)	Log wage ratio (75/25)	Number of green candidates	Green mayor
Oil dependence × log oil price	0.027	0.044	-0.020	-0.015	0.021	-0.002	0.009	0.037	0.00001
	[0.025]	[0.029]	[0.034]	[0.037]	[0.016]	[0.030]	[0.009]	[0.029]	[0.002]
Observations	8,219	8,397	8,291	2,662	207,835	1,050	1,050	3,703	3,710
Number of municipalities	1,006	1,007	1,007	905	229	229	229	967	967
Sample period	1997-2005	1997-2005	1997-2005	2000-2007	1998-2005	1998-2005	1998-2005	1997-2007	1997-2007
Additional Demographic Controls					Y	Y	Y		
Additional Municipal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes. See Table 2 for table notes. Additional demographic controls include gender, education, if the respondent is married, age and its square. Column (5) is an individual level wage regression and columns (6)-(7) are municipal level regressions with municipal averages of the demographic controls.

A Supporting Information Appendix

In this appendix, we present: the descriptive statistics of key variables used in our paper (Table A.1); a map of the spatial distribution of oil production in 1993 (Figure A.1) and the price of oil over our sample period (Figure A.2). We also conduct additional robustness checks of our main findings, which we detail below. These include: instrumenting revenue with oil price shocks; controls for linear trends by oil producing municipalities; and controls for pre-period armed group presence. Finally, we present supplemental analysis of a cross-country analog to our within-Colombia analysis.

A.1 Instrumenting Revenue

Our paper conceptualizes revenue as a key part of the mechanism, and posits that revenue surges attract paramilitary groups which in turn influences elections. So, an alternate approach is examining how changes in revenue — instrumented by oil price shocks — affects these outcomes. One drawback to this approach is that it necessarily excludes election year 2007 since we lack revenue data for that year. This is why we prefer to estimate the direct impacts of the price shocks as our primary specification.

In implementing the IV approach, we treat log total revenue as the endogenous variable. We opt to instrument total revenue rather than regalias revenue since places with no natural resources receive zero regalias, leading to their exclusion from the log specification.

Table A.2 presents the IV specification for our five main outcomes, estimated using 2SLS. All our results remaining significant, including those related to mayoral elections, for which we now have a reduced sample. Thus our findings are robust to this alternate approach.

A.2 Additional Controls

As shown in Figure A.2, the price of oil trended upward for much of our sample period. If institutional outcomes also trended upward differentially in oil producing municipalities – for some reason other than the oil price increase – this could upward bias our estimates. Table A.3 adds in a linear trend by whether municipalities produced oil in 1993. The effect for all mayoral outcomes remain statistically significant. The effect on the council share, for which we have a smaller sample, becomes statistically

insignificant at conventional levels (the p-value is .101 when other additional controls are omitted, and .163 when these other controls are included). However, even the coefficient in column (7) is not statistically distinguishable at the 5% level from the baseline specification in column (4) of Table 2, suggesting that the drop in significance reflects a power limitation.

A second potential concern stems from the fact that armed groups tend to be active in certain geographic regions. Of course, if their presence is uncorrelated with oil dependence, this cannot bias our estimates. If their pre-period presence is correlated with oil, but they do not differentially alter their activities in these locations over time in a way that covaries with the oil price, this again cannot bias the estimates. However, if they increase activities in locations with pre-period presence as the price of oil changes, then this could create bias.

This is particularly a concern for paramilitary groups since they have been operating in the oil region since their emergence. Our account is that paramilitary groups increase their activities in *oil* locations when the price of oil rises. But, if paramilitaries were already more active in oil municipalities prior to the study period, we have to discern whether oil price hikes lead them to boost activities in oil locations vs. locations where they were already present.

To distinguish the effect of armed group presence from oil presence, we measure whether any armed group (FARC, ELN or paramilitary) was active in each municipality over 1988-1992. We derive an equivalent indicator for whether just the paramilitary groups were active during this pre-period. We then control for the interaction of these variables with the oil price. Table A.4 presents these results. Our results remain unaffected by the inclusion of these controls. In fact, the magnitude of the coefficients for the elections outcomes are in line with the baseline estimates in Tables 2 and 3. The results on paramilitary activity in columns (5) and (10) help clarify that when the price of oil rises, paramilitary activity increases in the oil dependent municipalities, even after accounting for the potential correlation between oil-dependence and paramilitary presence in the pre-period.

A.3 Cross-Country Exercise

Here, we present a simple exercise to validate our findings from Columbia using cross-national data.

We draw on data from several sources. We measure democracy using x-polity, which was developed by Vreeland (2008). The original polity score includes five components. Three measure factors

related to the chief executive — if chief executives are chosen through a competitive process, if their recruitment can be considered an open process, and if there are any constraints placed on their power. The remaining two components measure the competitiveness and regulation of political participation. As Vreeland (2008) points out, this latter group directly considers whether political violence, including civil war, affects participation. Thus, x-polity removes these two components from the measure.

To facilitate the use of the x-polity measure in time-series analyses, we follow the same procedure used to transform the polity variable into polity2. We apply a "fix" that converts instances of "standardized authority codes" (i.e., -66, -77, and -88) to scores within the x-polity range, (i.e., -6 to +7). We refer to the resulting variable as x-polity2.

We use the PRIO Armed Conflict Dataset, tracking civil war incidents as of 1950. There are two relevant measures. Prio 25 equals one if a country experienced a conflict event that resulted in at least 25 battle-related deaths, while Prio 1000 indicates if a country experienced an event resulting in at least 1000 battle-related deaths. We define our pre-period to cover the 1950s and 1960s. We then code whether countries experienced any conflict event over this pre-period, for both measures. Using this process to define the conflict sample helps ensure that measured conflict is not a direct response to either contemporaneous oil price shocks or democracy in the country.

Data on oil reserves and population are from Haber and Menaldo (2011). We create an average oil reserves per capita over the pre-period (covering the 1950s/1960s), again to circumvent concerns that contemporaneous oil production and remaining reserves may respond to a country's regime. Finally, the international price of crude oil is obtained from the International Financial Statistics (IFS) and is measured in dollars per barrel. Table A.5 shows descriptive statistics for these variables.

We use this data to estimate a regression that is similar to our within-country specification. We interact the average reserves per capita with the log international oil price. We include country and year fixed effects, and control for log income per capita. Our sample period extends from 1970-2004. Table A.6 presents the results.

We first show the effect in the full sample and then proceed to the split sample. As with our within country specification, we again find that the negative relationship between oil price shocks and democracy is substantially larger in the conflict sample, as compared to the no-conflict sample.

We interpret these effects in light of different means in different groups. Consider the Prio 25 split sample results. In the sample without any conflict over 1950s/60s, mean oil reserves per capita for oil producers is 6 and the mean x-polity score is 1.6. The coefficient in column (2) tells us that a 130% price increase implies a 2.4% differential reduction in the x-polity score of the average oil producer, as compared to a non-oil producing country in this group. In the conflict sample, mean oil reserves is .329 for oil producers and mean x-polity is .49. Thus, the coefficient in column (3) tells us that a 130% price increase implies a 47% differential reduction in the democracy score of the average oil producer.

We find that the results are insensitive to a series of other changes. In defining the pre-period and sample period, we face a trade-off — extending the pre-period allows us to capture more conflict affected countries, but shrinks the sample, reducing power. However, the results look similar if we perturb the dividing year to 1965 instead of 1970. The findings also look similar if we use the average oil reserves per capita over the entire sample instead of the pre-period sample. They also do not depend on the inclusion of per-capita income, which is a potentially endogenous control. Finally, while x-polity has the great advantage that it eliminates components that are conflict-related, it also eliminates the components linked to political participation. So we also examine impacts on polity2, and find similar results.[1]

These results are suggestive since we do not conduct a full-blown cross-country analysis. This would require its own attention to endogeneity concerns, especially on the price of oil. In addition, these effects are estimated for a time period after 1970, when the oil curse tends to emerge (Andersen and Ross 2014). But of course, these patterns may not be visible over longer time horizons, as Haber and Menaldo (2011) have shown. However, we think these correlations are informative because they show a similar pattern of results in the international data as in the Colombia data. These patterns suggest that the political resource curse tends to be more severe in conflict-affected locations.

[1] All of these estimates are available from the authors upon request.

References

[1] Andersen, Jørgen J. and Michael L. Ross. 2014, "The Big Oil Change: A closer look at the Haber-Menaldo analysis."' *Comparative Political Studies* November 2014.

[2] Haber, Stephen and Victor Menaldo. 2011, "Do Natural Resources Fuel Authoritarianism? A Reappraisal of the Resource Curse." *American Political Science Review* 105(1): 1-26.

[3] Vreeland, James. 2008, "The Effect of Political Regime on Civil War: Unpacking Anocracy." *Journal of Conflict Resolution* 2008. 52 (3): 401-425.

Table A.1: Summary Statistics of Key Variables

	Mean	Stdv Dev.	Min.	Max.	N
Panel-level variables					
Pro-para mayor	0.148	0.356	0	1	3659
Pro-para council share	0.111	0.191	0	1	2964
Number of candidates	3.91	2.286	1	37	3712
Number of pro-para candidates	0.645	1.039	0	13	3703
Number of extreme-left candidates	0.136	0.366	0	4	3702
Number of other candidates	3.128	1.807	0	24	3703
Number of green candidates	0.045	0.21	0	2	3703
Extreme-left mayor	0.014	0.118	0	1	3710
Center-left mayor	0.353	0.478	0	1	3710
Center-right mayor	0.307	0.461	0	1	3710
Green mayor	0.006	0.075	0	1	3710
Margin of victory	0.169	0.156	0	1	3597
Incumbent party	0.321	0.467	0	1	2662
Log regalias revenue (millions of pesos)	4.812	2.704	-2.259	11.026	3427
Log total revenue (millions of pesos)	8.608	0.957	4.769	15.581	8234
Log tax revenue (millions of pesos)	5.964	1.758	-1.465	14.666	8219
Log total spending (millions of pesos)	8.615	0.962	0.679	15.706	8397
Log personnel spending (millions of pesos)	6.376	1.044	-0.929	13.039	8291
Log wage	7.712	.755	5.218	9.954	207835
Log wage ratio ($90^{th}/10^{th}$ percentile)	1.753	.543	0	3.717	1099
Log wage ratio ($75^{th}/25^{th}$ percentile)	.802	.345	0	2.733	1099
Paramilitary activity	0.446	0.497	0	1	9063
FARC activity	0.385	0.487	0	1	9063
ELN activity	0.19	0.392	0	1	9063
Log population (millions)	-4.246	1.125	-8.512	1.953	4088
Cross-sectional variables					
Oil dependence, 1993	0.056	0.549	0	10.839	1022
Cultivated coca indicator, 1994	0.05	0.218	0	1	1022
Elevation	1225.449	1171.15	2	25221	1022
Above mean conflict, 1988-1992	0.374	0.484	0	1	3659
Annual-level variables					
Log oil price, thousands of 2012 pesos/barrel	10.883	0.651	10.064	12.138	24

Notes. Note that mean log population is negative since average municipal population, in millions, is a fraction.

Table A.2: Instrumenting Revenue

	(1) Pro-para mayor	(2) Pro-para council share	(3) Number of candidates	(4) Margin of victory	(5) Paramilitary activity
Log total revenue	0.594**	0.130*	-1.790*	0.192***	0.379*
	[0.293]	[0.068]	[1.009]	[0.074]	[0.199]
Observations	2,294	2,568	2,323	2,246	8,219
Number of municipalities	846	941	852	839	1,005
Sample Years	1997-2005	1997-2003	1997-2005	1997-2005	1997-2005
Additional Controls	Y	Y	Y	Y	Y
First stage kP F-statistic	4.708	5.063	4.772	4.545	5.271

Notes. Standard errors clustered at the municipality level are shown in parentheses. Variables not shown in all specifications include municipality and year fixed effects, linear trends by region and log of population. Additional controls include the interaction of elevation and municipalities cultivating coca in 1994 with the price of oil. All specifications instrument log total revenue with an interaction of municipal oil dependence and annual log price. First stage kP F-statistic shows the Kleibergen-Paap rk Wald F statistic.*** is significant at the 1% level, ** is significant at the 5% level, * is significant at the 10% level.

Table A.3: Robustness to Linear Trends by Oil-producing Municipality

	(1) Pro-para mayor	(2) Pro-para council share	(3) Margin of victory	(4) Number of candidates	(5) Paramilitary activity	(6) Pro-para mayor	(7) Pro-para council share	(8) Margin of victory	(9) Number of candidates	(10) Paramilitary activity
Oil dependence x log oil price	0.063***	0.024	0.025**	-0.302***	0.029*	0.064***	0.020	0.026***	-0.298***	0.028*
	[0.022]	[0.015]	[0.010]	[0.089]	[0.016]	[0.023]	[0.014]	[0.010]	[0.086]	[0.015]
Observations	3,659	2,964	3,597	3,712	9,063	3,659	2,964	3,597	3,712	9,063
Number of municipalities	959	998	957	967	1,007	959	998	957	967	1,007
Sample Years	1997-2007	1997-2003	1997-2007	1997-2007	1997-2005	1997-2007	1997-2003	1997-2007	1997-2007	1997-2005
Trends by Oil Producer	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional Controls						Y	Y	Y	Y	Y

Notes. Standard errors clustered at the municipality level are shown in parentheses. Variables not shown in all specifications include municipality and year fixed effects, linear trends by region and log of population. Additional controls include the interaction of elevation and municipalities cultivating coca in 1994 with the price of oil. Trends by oil producer include an interaction of year with an indicator of whether the municipality produced oil in 1993. *** is significant at the 1% level, ** is significant at the 5% level, * is significant at the 10% level.

Table A.4: Controlling for Initial Armed Group Presence

	(1) Pro-para mayor	(2) Pro-para council share	(3) Margin of victory	(4) Number of candidates	(5) Paramilitary activity	(6) Pro-para mayor	(7) Pro-para council share	(8) Margin of victory	(9) Number of candidates	(10) Paramilitary activity
Oil dependence x log oil price	0.082***	0.033**	0.025***	-0.236***	0.026*	0.081***	0.032**	0.025***	-0.242***	0.028**
	[0.022]	[0.015]	[0.009]	[0.084]	[0.013]	[0.022]	[0.015]	[0.009]	[0.083]	[0.014]
Observations	3,651	2,958	3,590	3,704	9,063	3,651	2,958	3,590	3,704	9,063
Number of municipalities	957	996	955	965	1,007	957	996	955	965	1,007
Sample Years	1997-2007	1997-2003	1997-2007	1997-2007	1997-2005	1997-2007	1997-2003	1997-2007	1997-2007	1997-2005
Additional Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Any armed group 88-92 x log oil price	Y	Y	Y	Y	Y					
Paramilitary 88-92 x log oil price						Y	Y	Y	Y	Y

Notes. Standard errors clustered at the municipality level are shown in parentheses. Variables not shown in all specifications include municipality and year fixed effects, linear trends by region and log of population. Additional controls include the interaction of elevation and municipalities cultivating coca in 1994 with the price of oil. Any armed group is a municipal-level indicator for any activity by the paramilitaries, the FARC or the ELN in the pre-period 1988-1992. Paramilitary is a municipal-level indicator for any paramilitary activity in the pre-period 1988-1992. *** is significant at the 1% level, ** is significant at the 5% level, * is significant at the 10% level.

Table A.5: Summary Statistics of Cross-National Variables

	Mean	Stdv Dev.	Min.	Max.	N
Panel-level variables					
Xpolity2	1.289	4.931	-6	7	3901
Log income per capita	7.245	1.561	4.046	10.939	3901
Cross-sectional variables					
Prio 25 conflict indicator, 1950-1969	0.275	0.448	0	1	120
Prio 1000 conflict indicator, 1950-1969	0.1	0.301	0	1	120
Oil reserves per capita, 1950-1969	1.929	15.466	0	163.296	120
Annual-level variables					
Log oil price, thousands of USD	2.274	1.306	0.582	4.575	59

Table A.6: Oil Price Shocks, Conflict and Democracy: Cross-National Evidence

	(1) Xpolity2	(2) Xpolity2	(3) Xpolity2	(4) Xpolity2	(5) Xpolity2
Oil reserves × log oil price	-0.006***	-0.005***	-0.539**	-0.006***	-0.431***
	(0.002)	(0.001)	(0.238)	(0.002)	(0.058)
Observations	3,901	2,783	1,118	3,539	362
Number of countries	124	89	35	112	12
R-squared	0.780	0.796	0.741	0.778	0.782
Country sample	**Full**	**No Conflict**	**Conflict**	**No Conflict**	**Conflict**
Conflict measure		Prio 25	Prio 25	Prio 1000	Prio 1000
Country fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y

Notes. The sample period is 1970-2004. Oil reserves per capita are calculated over 1950-1969. The conflict sample includes countries that experienced conflict over the period 1950-1969. All specifications control for log GDP per capita. Standard errors are clustered at the country level.

Figure A.1: Oil Production in 1993

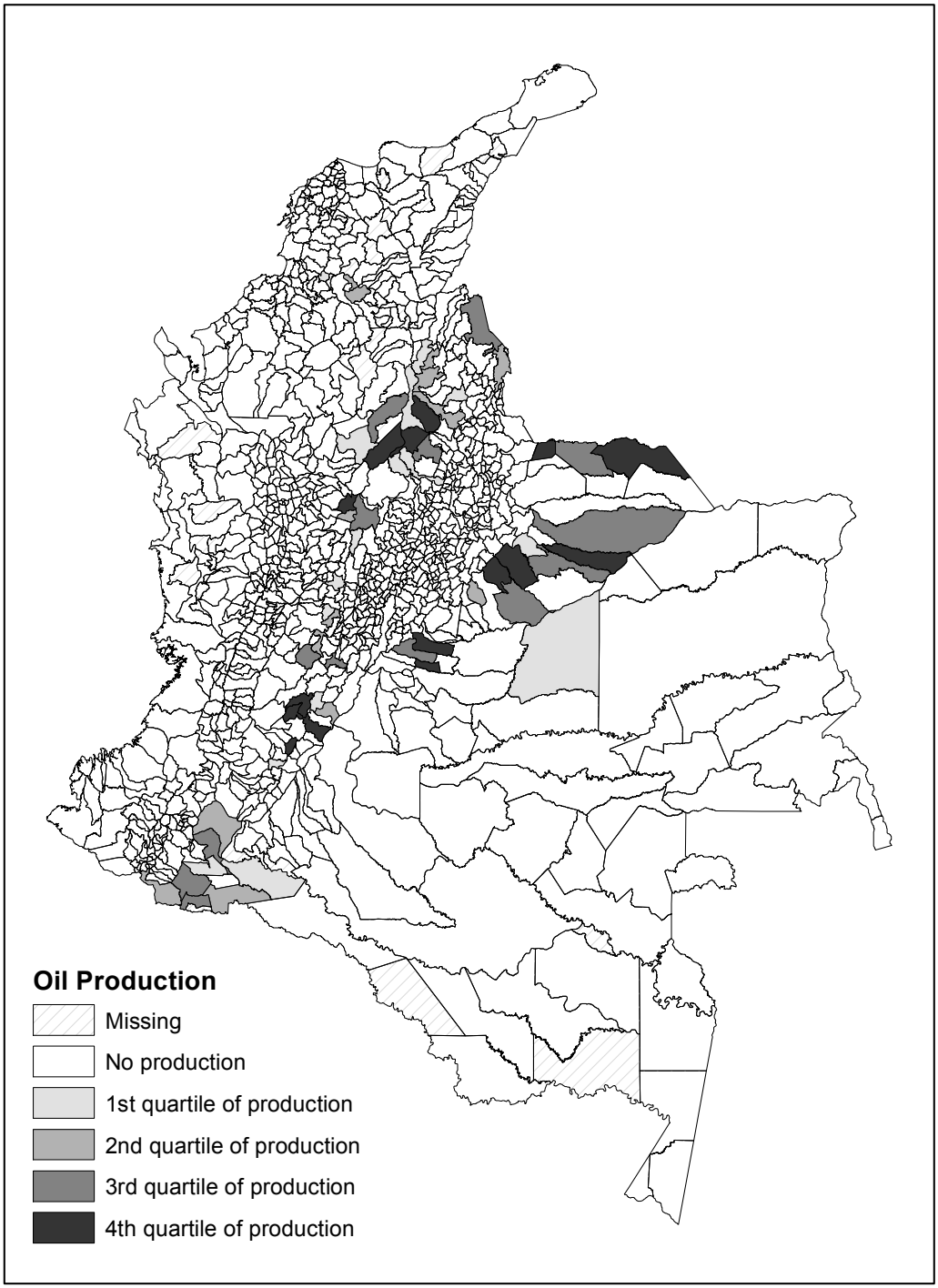

Figure A.2: The International Price of Oil