In the Beginning was the Curse, and the Curse was Oil

A Revisit in the Economics of the Resource Curse

Universität Hamburg
Fakultät Wirtschafts- und Sozialwissenschaften

Kumulative Dissertation
Zur Erlangung der Würde einer Doktorin
der Wirtschafts- und Sozialwissenschaften
„Dr. rer. pol.“
(gemäß der PromO vom 18. Januar 2017)

vorgelegt von
Phoebe W. Ishak
Aus Kairo, Ägypten
Hamburg, 12 Juli 2022

Prüfungskommision

Vorsitzende: Prof. Dr. Melanie Krause

Erstgutachter: Prof. Dr. Olaf Posch

Zweitgutachter: Prof. Dr. Ulrich Fritsche

Drittegutachter: Prof. Dr. Stefan Voigt

Datum der Disputation: 10 August 2022

„Geschrieben, um dich eine neue Geschichte
für deine Leben vorzustellen“

Paulo Coelho, übersetzt von Arabisch

Für Saad, Mona und Erin

Aknowledgements

First and foremost, I would like to express my gratitude to my supervisors Olaf Posch and Stefan Voigt for their support and guidance. I am profoundly grateful for their constant encouragement and research flexibility. I am also very thankful for the support and guidance of each member of my Ph.D. committee. I am truly honored they have agreed to be part of this process. I wish to offer my sincerest gratitude to Ulrich Fritsche and Melanie Krause for providing me with perceptive and insightful comments that greatly improved my dissertation.

My deepest thanks are reserved to my co-authors Pierre-Guillaume Méon and Mohammad Reza Farazanegan, whom I was fortunate to work with, and who gave me an opportunity to learn from their qualities. I am very much indebted to Hannes Müller for his endless support and for enriching my research experience with new avenues. My colleagues at Universität Hamburg and Universitat Autònoma de Barcelona, who contributed to an inspiring and amicable work environment. A very special regard to Khalid Abu Ismail for his generous advice and thoughtfulness.

I am grateful to the workshop, seminar and conference participants in Berlin, Münster, Hamburg, Göttingen, Melbourne, Switzerland, Barcelona, Warwick and International Monetary Fund, who enhanced my research with critical comments and valuable suggestions. Various anonymous referees, who provided thorough review services, which helped to improve my papers exceptionally. Financial support of the WiSo Graduate School, which enabled a large part of my projects to continue.

For my family, words will never be enough to express all my gratefulness and love. I was blessed with wonderful parents Saad Ishak and Mona Samy. I thank you for your endless encouragement and continous confidence in me. My sisters Erin and Nancy, my brother in law Mamdouh and my little niece Maurine, I thank you for all your love and support. My pastor Father Polikarious, and my friends, Menna Gamal and Joao Azevedo, who always believed in me, especially in times when I was in doubt. Lastly, to the generous soul Ahmed Kheir, for his all along encouragement throughout the journey.

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Introduction

"I call petroleum the devil's excrement. It brings trouble."

Juan Pablo Pérez Alfonso, a co-founder of OPEC, 1976

The seminal papers of Sachs and Warner (1995, 2001) put forward as a stylized fact that "economies with abundant natural resources have tended to grow less rapidly than natural-resource-scarce economies" and gave a rise to what is known as "The Resource Curse". More empirical evidence has then followed showing that point resources, espicially oil, have hightened economic and political failures, and weakened countries' development prospectives particularly in Africa, Middle East and Latin America (e.g. Karl, 1997; Isham et al., 2005; Boschini et al., 2007). This dissertation revisits key questions of the oil resource curse literature in four studies.

Channels of the resource curse have varied. Economists have long argued that resource-abundant countries tend to suffer from low industrial capacity, lack of rule of law and accountability measures, corruption, underdeveloped fiscal and financial institutions, weak educational systems, and civil conflicts, all of which explain their bad growth prospects (see Van der Ploeg (2011) for a review). However, this oil-low growth association has been challenged by a number of studies stressing the importance of national contextual factors (e.g. institutional differences) and/or the disaggregation of local effects of resource abundance within a country (e.g. Mehlum et al. 2006; Allcott and Keniston, 2015). Others have provided evidence that the curse notion depended on the period of observation and on the definition of economic performance. Specifically, when performance is measured by the level of output per capita instead of its growth, the curse in fact appears to be a blessing (Alexeev and Conrad, 2009; Cotet and Tsui, 2013). Another strand of literature suggests that oil rents can prolong authoritarian regimes survival through widespread distribution, repression or freedom concessions (e.g. Ross, 2001; Wright et al., 2013), but it leaves unexplored the effects of different types of the mass threats and size of oil wealth in the final choice of the particular survival strategy.

Building on this, two studies center on the conditioning role of shadow economies in the final impact of resource abundance on macroeconomic outcomes. The first discusses the impact of oil price shocks on the incidence of protest (Chapter 2, co-authored with Mohammad Reza Farzanegan), and the second analyzes the effect of declining oil rents on taxation efforts (Chapter 3, co-authored with Mohammad Reza Farzanegan). The third study is a within-country analysis of the effect of oil and gas revenues on economic activity in Brazilian municipalities, contrasting the distribution of effects between oil-producing municipalities and their neighbors (Chapter 4, co-authored with Pierre-Guillaume Méon). The fourth study investigates the trade-off between resorting to spending and repression in handling social unrest, and asks if this trade-off changes with the size of oil wealth

(Chapter 5). Below, I give a description of each chapter, its findings and contributions to the literature.

Chapter 2 investigates the impact of variations in international oil prices on the incidence of protest, and asks whether the shadow economy can play a mitigation role. Our argument is built on two important strands of economic literature. First, the early work by rentier state theorists arguing that oil rents may create a cursory stability by building social bases whose support hinges upon the continuous availability of funds and distribution networks (Beblawi and Luciani, 1987; Karl, 1997; Ross, 2001). It follows that the fall in oil-fueled-revenues will make it quite difficult for the state to continue engaging in widespread distribution, adversely affecting household incomes and lowering the opportunity cost for engaging in anti-state protest (e.g. Bazzi and Blattmann, 2014). Second, shadow economy might act as a countercyclical device to buffer against social unrest by providing an alternative source of income during economic downturns (e.g. Eilat and Zinnes, 2002; Bajada and Schneider, 2009). In such setting, the existence of the shadow economy will reduce the effect of oil price fluctuations on political instability.

This is the first study to consider the mitigation role of the shadow economy in oil windfalls-political instability nexus. It provides novel evidence on role of shadow economy in soothing protests breakout in oil-dependent countries. Specifically, we find that negative oil price shocks increases the number of protests in oil dependent countries, but the effect is less severe the higher the initial level of the shadow economy. The effect vanishes in countries with a shadow economy representing more than 35% of gross domestic product. This suggests that the shadow economy's capacity to absorb persistent oil price fluctuations without provoking political unrest, should regard it as a mitigation tool rather than an economic burden.

Another key notion of "Oil Resource Curse" literature concerns the weak fiscal capacity of natural-resource-abundant countries, sometimes known as "Fiscal Resource Curse". The presence of oil rents can reduce the state's incentive to strengthen its taxation systems (e.g. Besley and Persson, 2014). A natural question would then be, "What would happen, if oil rents suddenly declines?" In chapter 3, we analyze the conditional response of tax revenues to declining oil rents in the presence of shadow economies. The negative association between resource rents and taxation capacity is well documented. In short-run, the governments may lower tax rates in response to increase in state-accrued resource revenues (James, 2015; Crivelli and Gupta, 2014). This casts its shadows on the long-run efficiency of tax administrations, by having low tax bases and hence, low incentives to invest in market-supporting legal capacity (Besley and Persson, 2011). The fiscal channel is used explain the democracy deficits in oil rich economies. The lower fiscal dependency of the state on citizens may reduce the demand for accountability of the state to the people as well as the political participation of people (Ross, 2001).

We add to this literature by showing that the adjustment of tax efforts in response to declining oil rents is significantly constrained by the initial size of the shadow economy. To the best of our knowledge, the moderating role of the shadow economy in the nexus between tax revenues and negative oil shocks is neglected in the literature. Our theoretical framework demonstrates that negative shocks in oil rents promote tax efforts of the state when the size shadow economy is sufficiently moderate, whereas they have no significant positive impact on tax revenues when the shadow economy is extensive. Empirically, we find that a decline in oil rents following negative oil price shocks cease to have any significant positive impact on tax revenues in countries with shadow economy representing more than 35% of GDP.

In Chapter 4, we consider a specific-country case (i.e. Brazilian municipalities) to test the resource curse hypothesis. It belongs to recent growing within-country studies analyzing the impact of resource windfalls on disaggregated country-level data. Recent studies using within-country data consistently report evidence that natural resource abundance or booms in the price of natural resources improve the economic performance of the areas that produce the natural resources. These findings are documented in both developed and developing countries and at various periods (e.g. Black et al., 2005; Mamo et al., 2019).

In this paper, we push the research frontier forward and ask whether natural resource windfalls can be a curse and a blessing at the same time. More specifically, if oil-producing municipalities are benefiting economically from resource booms, does this mean that non-producing municipalities are also benefiting, or could they be suffering? To answer this question, we argue that the spatial dimension of the effect of natural resources is key, because the benefits and costs of resource windfalls are unevenly distributed across space. Hence, while municipalities that produce oil are likely to benefit from the revenues and economic activities that natural resources generate, they may draw on resources from other municipalities, thereby imposing negative externalities. The balance between the benefits and the costs of natural resources is therefore likely spatially uneven, with positive effects dominating in the vicinity of resource-producing municipalities then fading away as distance grows. Moreover, the net aggregate effect at the regional or country levels may be either insignificant or negative.

Our argument is based on the theoretical models of theoretical models of Moretti (2011), and Allcott and Keniston (2015), the final impact of an exogenous increase in oil and gas revenues in oil-producing municipalities and their neighbors is ambiguous. To explain, an increase in oil and gas revenues allow both oil sector workers and local municipalities governments in oil-producing municipalities to increase their expenditures and demand of intermediate goods. These positive effects may be magnified in the presence of agglomeration economies that would attract additional firms to oil-producing municipalities. On the negative side, the increased demand for labor increases wages, and hence, the rising labor costs reduce the production of non-resource sectors, especially tradable sectors, which may move from oil-producing municipalities to other municipalities. The neighboring municipalities may benefit from the increased state revenues from oil royalties, increased demand for intermediate goods and the relocation of the production of tradeable goods driven away from oil-producing municipalities. These effects, however, are likely to be smaller the more distant a municipality from the oil-producing municipality. Conversely, the rise in the demand for labor in oil-producing municipalities may result in an increase in wages in neighboring municipalities, if the labor is sufficiently mobile. Furthermore, the increase in demand for goods and services in oil-producing municipalities may attracts scarce resources, like capital and skilled labor. This could suppress production and reduce the demand for unskilled labor, resulting in lower wages.

Accordingly, the effect of an increase in oil and gas revenues in both oil-producing and neighboring municipalities is a priori ambiguous and requires an empirical investigation. However, we expect oil spillovers on neighboring municipalities will tend to decrease the further the municipality to oil production activities. To test that, we make use of exogenous variation in international oil and gas prices to Brazilian municipalities and employ spatial econometric techniques. We find that an exogenous increase in oil and gas revenues raises economic activity, measured by night-time light emissions, in oil-producing municipalities, but impose negative spill-overs on neighbouring municipalities. Using census data, we show that oil and gas revenues increase royalties, population, local real prices, and real wages, essentially in manufacturing and services in oil municipalities. However, neighboring municipalities suffer from negative spillovers on wages and prices, and from an increase in crime. Our findings are in line with previous studies finding positive local effects for resource booms, but we additionally complement that by showing that neighboring municipalities suffer from negative spillovers. Moreover, we show that the aggregate effect of natural resources is insignificant when observed at higher levels of aggregation. That may explain why subnational studies report unmitigated positive effects while the findings of cross-country studies are contradictory.

Resource curse literature also addresses the question of whether oil rents can prolong authoritarian regimes survival, offering bulk of evidence suggesting that oil dictatorships are more likely to withstand power challenges than their non-oil counterparts. Different explanations have been given to such finding, all pointing to the use of oil revenues to neutralize power threats. This can be achieved through a number of survival strategies including doling out resources on public goods, enhancing repressive capacities, curbing freedoms and limiting political institutions (e.g. Ross, 2001; Ghandi and Przeworski, 2007; Mesquita and Smith, 2010). With respect to this, in chapter 5, I examine the dictator's trade-off between survival strategies when faced by an imminent threat of overthrown in oil abundant countries. I argue that the resort to one strategy is rather conditional upon the type of posed mass threat, the dictator's time horizon (short run vs. long run) and the size of oil wealth.

Mass threats in dictatorships differ in type and severity, ranging from peaceful gatherings to violent civil conflicts, and can in turn affect the dictator's time horizon. In the short run, the dictator's options are limited to spending and repression (Wintrobe 1998; Annett 2001; Acemoglu et al. 2000). For oil dependent dictatorships, a greater oil wealth can have the potential to allow the dictator to adopt different defense mechanism than a dictator with limited or no oil revenues. By relaxing their budget constraints, oil revenues may lead dictators to increase spending and employ less repression. Such disaggregation of types of the mass threats taking into account the length of the response period and size of oil wealth is widely overlooked in analyzing the dictator's behavior in general and the implications of oil wealth in particular.

The study bases its analysis on the theoretical models of Wintrobe (1998), Annett (2001) and Acemoglu et al. (2000) who argued that the short run response to a fall in mass loyalty is either repression or spending. However, the type and severity of the mass threat were rather left unspecified in their models as well as the type of state finance. Except for Wintrobe (1998), there has been a mutually exclusive employment of either spending or repression to quench mass threats, rather than allowing for mixing between both depending on the intensity of the threat.

To address these analytical shortcomings, I investigate the trade-off between resorting to spending and repression in handling different mass threats, and how this trade-off changes with the presence of oil rents. Using explanatory factor analysis (EFA), I estimate two indices of mass threats namely civil protest and mass violence. Employing a simultaneous equation model, I found that mass violence is countered by both more spending and repression, and civil protest is only met by repression. Moreover, greater oil wealth is only found to provide a wider fiscal space to relatively increase spending at low and intermediate levels of threats. However, as threats intensify, the effect of oil wealth dissipates and oil dictatorships behave the same as their non-oil counterparts. Furthermore, oil wealth had no impact on repression, whether when accounting for threats or not, revealing that oil wealth may not alter the repressive stance of dictators and push them to substitute spending for repression.

The main contributions of this dissertation concern giving more attention to the underlying conditioning context when analyzing the final effect of natural resources, particularly oil rents. Chapters 2 and 3 show that the existence of sizable shadow economies can mitigate the effect of oil price shocks on political instability and tax revenues. Chapter 4 stresses on the importance of within-country disaggregation of resource boom effects among oil-producing sub-nationals and their neighbors. Chapter 5 reconsiders the role of oil rents in prolonging autocratic regime survival after taking into account the type of posed mass threat, the dictator's time horizon (short run vs. long run) and the size of oil wealth.

Oil Price Shocks, Protest and the Shadow Economy: Is there a Mitigation Effect?*

Abstract

In this study, we look at how oil price shocks affect the incidence of protests in a country and how the size of a country's shadow economy influences this relationship. Using panel data from 144 countries, from the period of 1991–2015, we find evidence that negative oil price shocks significantly increase protests in countries with small shadow economies. The effect dissipates as the size of the shadow economy increases and eventually vanishes in countries with a shadow economy representing more than 35% of gross domestic product. Our analysis departs from existing literature by emphasizing the moderating role of a shadow economy on the effects of negative oil shocks on the incidence of protests in oil-dependent economies. The results are robust to various specifications and their broader implications are discussed.

Keywords: conflict, oil price shocks, protest, resource curse, shadow economy

JEL codes: D74, O13, O17, Q34

2.1 Introduction

The effect of natural resource wealth and income fluctuations on political stability has been widely explored in the literature (see Bazzi and Blattman, 2014 for a survey). Resource-rich countries are subject to severe commodity price swings that significantly affect their macroeconomic fundamentals, including political unrest. Widespread political instabilities are associated with a higher risk of investment and capital flight, displacement of population and destruction of infrastructure and social capital. These may have significant negative effects on economic growth. For instance, in the case of the Arab Spring protests in Tunisia, Matta et al (2019) estimated the income lost to range between 5.5 and 6.4 percent of GDP over the period of 2011-2013. Earlier cross-country studies also show sizable negative effects of political instability and conflict on economic growth. Collier (1999) estimates that countries tend to grow, on average, 2.2 percentage points slower during a period of civil war compared to peacetime. Similar negative effects on economic growth, food production and foreign debt burden are shown by Stewart et al. (2001).

In this paper, we focus on whether fluctuations in international oil prices induce political instability in oil-dependent countries, and if the size of the shadow economy can mitigate

*This chapter is co-authored with Mohammad Reza Farzanegan and published as *"Ishak, P. W., and Farzanegan, M. R. (2022). Oil Price Shocks, Protest and the Shadow Economy: Is there a Mitigation Effect? Economics & Politics, 34 (2), 298-321."*

these effects. We argue that oil price fluctuations have a lower impact on political unrest in countries incubating sizable shadow economies. Our argument is built on two important strands of economic literature. One concerns the effect of resource rents on political instability and risk of conflict, and the other considers the role of the shadow economy in buffering against social unrest. Our key result shows that declines in oil rents following negative price shocks increase the incidence of protests in oil-dependent economies, but that this effect is significantly mitigated by the size of the shadow economy.

First, a significant unexpected decline in oil rents in an oil-dependent economy can impose a variety of challenges for political power (Bjorvatn and Farzanegan, 2015). The government, which controls resource rents, often use public sector employment as a redistributive tool and to pacify opposition through patronages. A sudden drop in rents weakens the government's ability to continue making cash transfers and to provide public employment.[1] Negative oil rents shocks increase pressure on the state to cut subsidies, implement the privatization of public assets, reduce the size of the public sector and increase tax burden. For example, the oil-rich countries of the Gulf Cooperation Council (GCC) have started to introduce value added taxes and other fees as a result of negative oil shocks (Edwards, 2020).[2] These developments, especially in oil-rich economies, may increase the risk of social unrest. For example, the 2019 countrywide protests and riots in Iran were the result of increased gasoline prices by the state following a significant drop in oil income under sanctions. A reduction in subsidies and cash payments can also have adverse effects on inequality and poverty indicators (Farzanegan and Habibpour, 2017). A reduction in cash payments can negatively affect the size of the middle class and shift them to lower income deciles, thus reducing the opportunity cost to join the anti-state protests.[3]

Second, although we expect a sudden decline of oil rents in oil-dependent countries to trigger protests and dissatisfaction with the political system, this effect can be moderated by the existence of a shadow economy. A shadow economy can act as a countercyclical device to buffer against social unrest by providing an alternative source of income for disgruntled citizens during an economic downturn (e.g. Eilat and Zinnes, 2002; Boeri and Garibaldi, 2002; Dell'Anno and Solomon, 2007; Bajada and Schneider, 2009).[4] Furthermore, shadow economies may offer leeway from the distortionary activities of the state in the formal economy, such as administrative corruption, leading to enhanced economic activities in the formal sector (Choi and Thum, 2005). The shadow economy acts as an insurance policy against economic volatility by creating jobs and providing profit opportunities for businesses. In this setting, the existence of a shadow economy may increase

[1] As mentioned in Bjorvatn and Farzanegan (2013), governments that control resource rents use public sector employment as a redistributive instrument, mainly for "patronage" purposes to secure re-election or to stay in power (also see Alesina, Baqir, & Easterly, 1998; Auty, 2001; Robinson, Torvik, & Verdier, 2006).

[2] https://www.brookings.edu/events/the-impact-of-low-oil-prices-and-taxation-on-the-gcc-countries/

[3] Farzanegan et al (2017) study the relationship between oil rents and the middle class in Iran. They show that the middle class expands in response to positive shocks in oil income.

[4] Farzanegan (2009) also finds that smuggling of commercial goods (as a part of the shadow economy) has a significant and dampening effect on import prices in the case of Iran.

the opportunity cost of protesting during periods of sluggish growth following a decline of oil rents in oil-dependent economies. More generally, the existence of a shadow economy reduces the effect of oil price fluctuations on political stability.

As such, our contribution is to demonstrate that the joint effect of declining oil rents and the presence of a shadow economy matters in the incidence of protests in countries. To the best of our knowledge, the mitigating role of the shadow economy on the nexus of declining oil rents and political stability is overlooked in the literature. There is little evidence for the mitigating role of the shadow economy on social unrest amid oil-induced economic shocks.[5] In the context of oil-producing countries, investigating such role is particularly appealing not only because of the vulnerability of those economies to external shocks, but also because of the considerable share of informal economies in their official GDP. According to the latest data from Medina and Schneider (2018), the average share of the shadow economy to GDP in oil-producing countries ranges from 11 percent to 62 percent, with an average of around 31 percent, which points to the significant role of the informal sector in those economies.

In measuring political instability, we focus on less violent events of unrest such as protests, measured by the number of anti-government demonstrations, general strikes and riots (Banks and Wilson, 2018). These forms of instability can happen more often in response to macroeconomic shocks and may be the starting point for larger and more intense conflicts and violence, such as revolutions, regime changes or civil wars (e.g., Syria civil war since 2011).[6] Incidences of protests remain less studied compared to more violent events. The recent findings of Bazzi and Blattman (2014) shows oil price shocks to be associated with shorter, less intense civil conflicts instead of new civil war outbreaks. Our macroeconomic shock is given by changes in oil rents, which have a greater impact on economies with higher oil dependence. To measure oil rents shocks, we rely on the variation in international oil prices weighted by the country's time-invariant oil exports share to GDP. This circumvents endogeneity problems associated with the conventional measurements of resource abundance in terms of values of oil production or as a share of resource rents in GDP.

Using panel data on 144 countries, covering the period from 1991 to 2015, we first show that negative oil price shocks increase the incidence of protests. We then proceed to suggest a mechanism to mitigate the protest-induced effects of negative oil shocks. We show that the shadow economy significantly reduces the incidence of protests and that negative oil price shocks cease to have any significant impact on protests in countries with

[5]The literature on the shadow economy either focuses on estimating its size and discerning its causes (e.g., Schneider and Enste, 2000; Medina and Schneider, 2018) or studies its impact on the official economy and vice-versa on either a limited subset of countries or using cross-country variation (e.g. Dell'Anno et al., 2007). Other cross-country studies examine its relationship with other macroeconomic variables, such inequality, inflation and corruption (e.g., Dreher and Schneider, 2010). See Goel and Nelson (2016) for recent review of literature.

[6]Another example is related to Iran. Anti-government protests against the monarchy which started in 1977-78 led to the Islamic Revolution in 1979, followed by eight years of war with Iraq (1980-88). The economic costs of the revolution and war is estimated to be, on average, $3000 per person, per year from 1979 to 1988 (Farzanegan, 2020).

sizable shadow economies (representing more than 35% of GDP). This can be attributed to the safety net provided by the shadow economy as an alternative market to avoid the distortionary interventions of the government in the formal economy.

The rest of the paper is organized as follows: Section 2.2 reviews the two strands of literature which our study connects: the theoretical discussions of both the oil rents-protest nexus and the shadow economy-protest relationship. Section 2.3 describes the data and the empirical strategy. Sections 2.4 and 2.5 present our baseline results and robustness checks. Finally, Section 2.6 summarizes and concludes the paper.

2.2 Conceptual framework: The nexus between oil rents, protests and the shadow economy

Our study seeks to establish a link between two strands of literature: the first explains the relationship between resource rents and conflict, and the second discusses the relationship between the shadow economy and social unrest. In this section, we briefly review the theoretical arguments of each strand separately. We subsequently combine the theoretical arguments and discuss how a shadow economy can affect the relationship between oil rents and conflict.

2.2.1 Oil rents and conflict

Earlier studies are divided on the effects of oil rents on political stability. A large part of the literature examines how an increase in resource rents may increase conflict or the risk of political instability.

The starting point in the literature is the curse of resources. Countries with higher levels of resource rents dependency, on average and in the long term, grow slower than resource-poor economies (for a survey, see Frankel, 2010). In particular, the curse is more relevant and significant when the resource is crude oil (Boschini, Pettersson, & Roine, 2007).[7] The negative effects of oil rents on economic growth may be connected to a higher risk of conflict and incidence of protests. Economic growth may shape the opportunity costs of population planning and conflict participation (de Soysa, 2002). Hence, a dampening effect of income growth on a country's risk of conflict is expected, as found by Brückner and Gradstein (2015).

In addition to the indirect effect of resource rents on conflict through the channel of economic growth, the literature also argues that resource rents directly increase the inci-

[7] The focus of our paper is exclusively on oil rents, which ensures homogeneity in the effects of resource rents on political instability and conflict. This approach is also utilized in other studies, such as Arezki and Brückner (2011). Earlier studies in the resource curse have highlighted the importance of not pooling commodities when we are examining the effects of rents on governance or economic growth indicators. For example, Isham et al. (2005) suggest that while the point resource exporters (e.g., oil) perform relatively poorly in different institutional dimensions, the exporters of diffuse resources such as agricultural products or livestock do not record such a significant negative effect and show more robust economic growth. In his book on the oil curse, Ross (2012) and many others show that properties of oil resources differentiate it from other resources in the context of political economy analysis. van der Ploeg (2011) also provides a survey on this topic.

dence of conflict. For example, Collier and Hoeffler (2012) present possible explanations for the increased effects of rents on conflict. First, rents may increase the value of the state, intensifying conflict over the control of the state (i.e. state as a prize hypothesis). In addition, they refer to the decreased capacity of a state with a resource-rich economy to control corruption, which may increase the risk of conflict. Second, resource rents finance rebel groups and reduce the opportunity costs for rebellion (Collier and Hoeffler, 2004; Le Billon, 2001). One reason is the unequal distribution of rents, leading to economic and political incentives for resource-rich regions to separate from the rest of country. This subsequently increases the risk of conflict (e.g., Ross, 2004, Le Billon, 2001).[8] Rents also affect the fiscal relationship between the state and the people, reducing the government accountability of resource-rich states. By lowering the share of tax revenues in the government budget, resource rents increase the political gap between the state and society (Acemoglu and Robinson, 2000). This in turn may negatively affect the quality of political institutions and increase the incidence of protests as other channels of resolving conflicts are unavailable.

Although it may be true that rents dependency in the long-term directly and /or indirectly increase the risk of civil conflict, we also show examples where rents have bought peace, particularly in the short term. In a study of 29 sub-Saharan countries from 1985 to 2007, Arezki and Gylfason (2013) show that higher resource rents result in fewer internal conflicts and less democratic countries face a lower probability of conflicts after an increase in resource rents. This finding, they argue, is because of the ability of the politically powerful to buy peace through the redistribution of rents to the public. This redistribution happens through higher government spending (e.g., subsidies, public employment). Similarly, Andersen and Aslaksen (2013) show that oil rents extend autocratic regimes. They also find political stability to be less sensitive to rents in democratic countries (see also Fjelde (2009) and Omgba (2009) for similar results). More generally, Basedau and Lay (2009) argue that the constructive effects of resource rents on internal stability have largely been neglected in the study of peace and war. They show that political regimes apply resource rents to purchase peace through patronages (political corruption), by implementing large-scale distributive policies (e.g., subsidies) and by suppressing opposition.

Based on the latter studies, we argue that a sudden decline in oil rents should increase political instability and civil conflict in oil-dependent countries. In contrast to these studies, which focus on occurrences of civil wars and regime breakdowns, we estimate the effect of oil rents shocks on less violent incidences of social unrest (i.e. protests). This remains largely unexplored along with the recent findings of Bazzi and Blattman (2014) which shows oil price shocks to be associated with shorter, less intense civil conflicts. To the best of our knowledge, there is only the study (by Smith (2004)) which examines the impact of oil price shocks on anti-state protests and finds that oil wealth is associated with fewer protests. One concern with this study is that it is based on a cross-country empirical specification, which makes it difficult to make causal inferences. Another concern regards

[8] Farzanegan, Lessmann & Markwardt (2018) show that the increased risk of conflict in resource rich economies can be reduced by promoting political decentralization.

its measure of the time-varying value of oil exports to GDP being partly based on oil production, which makes it endogenous to social unrest.

2.2.2 Shadow economy and conflict

A line of literature discusses the positive effects of a shadow economy on political instability and incidence of protests. A shadow economy can decrease the risk of protests when the formal part of the economy does not absorb the flow of the working-age population due to regulatory burdens and higher labor costs. Increased unemployment in the formal economy is associated with higher labor force participation in the shadow economy (Dobre, Alexandru and Lepas, 2010). Existence of a shadow economy, with far less regulatory framework and significantly lower labor costs, may provide the unemployed population with income that stimulates general demand in the economy. The role of the shadow economy exists in different countries for various reasons and can reduce the economic pains of high unemployment in the formal economy, reducing the incidence of protests and major conflicts (Lucifora and Moriconi, 2012). The earned income in the shadow economy is also spent in the formal economy and may offset some negative effects on effective household demand (Alexandru, Dobre and Ghinararu, 2011). In a sample of 19 Asian countries during the period 1990–2015, Huynh and Nguyen (2020) show that a shadow economy decreases income inequality by increasing the share of income held by the lowest quintile and decreasing the share of income held by the highest quintile. La Porta and Shleifer (2014) study the characteristics of formal and informal entrepreneurs. They show that informal firms are often small, inefficient and run by entrepreneurs with low education attainment. In other words, the informal economy provides opportunities for a segment of society who may face significant challenges in obtaining employment in the formal and competitive economy. Their findings are mostly in line with Lewis' (1954) dual view of informality, implying that official and shadow economies are largely segregated and produce goods with different labor, capital and entrepreneurial inputs and that they also serve different customers. This implies that in the absence of a shadow economy, or its repression by the state, we may expect to observe an increasing gap between the rich (with higher skills) and the poor (with lower educational opportunities), which may result in a higher risk of conflict.

The constructive effect of a shadow economy on decreasing conflict and instability is also observed in the arguments by structuralists when discussing the formal and informal economy nexus. Structuralists believe that by providing cheaper goods and services and offering flexible working conditions, a shadow economy increases the level of competition in the formal economy. In the context of developing countries, a shadow economy helps to maintain economic activities when rent seeking and corruption increase the cost of doing business in the formal sector (Eilat and Zinnes, 2002). A shadow economy may also increase financial resources and help to accumulate entrepreneurial experience for the disadvantaged (Dell'Anno, 2016). The positive effect of the shadow economy on the formal economy may manifest itself in higher opportunity costs of conflict.

However, there is also a line of literature which discusses the risks of a shadow economy on political stability. A sizable shadow economy increases budget deficits by reducing tax revenues, especially in natural resource-poor economies. This negative impact on public finances may reduce the capacity of the state to produce public goods and services, such as security and order, thereby increasing the risk of conflict (Farzanegan and Badreldin, 2017). It can also reduce the capacity of the state to maintain redistribution policies and subsidy payments, with negative consequences for poverty and inequality (Enste, 2003; Chong, and Gradstein, 2007). On the interaction between the formal and shadow economy, we also encounter "dual" views, in contrast with the structuralists. According to the "dual" view hypothesis, shadow economy activities are unfairly competitive to the activities in the formal economy and thus negatively affect market allocation and economic growth. This is also empirically supported (Eilat and Zinnes, 2002). There are a series of allocative inefficiencies at the firm level in the shadow economy. Operating outside of public authorities may endanger the protection of property rights, leading to lower investment activity. Firms in the shadow economy are also forced to operate sub-optimally with limited ownership structures. Such firms are exposed to a higher risk of extortion and public corruption, given their unregistered nature and lack of legal support by public institutions (Dell'Anno, 2016). Moreover, Biswas et al. (2012) show that a shadow economy negatively affects the quality of the business environment, which combined with the "dual" view, may predict greater dissatisfaction with the political system.

2.2.3 Shadow economy as a potential moderator for oil conflicts

So far, we have discussed the different channels through which changes in oil rents and the shadow economy may independently affect political instability. These different arguments show that both fueling and dampening effects of oil rents and shadow economies on social unrest are possible. We aim to show that the size of a shadow economy can be an important moderator on the effect of (negative) changes in oil rents on the incidence of protests, especially in oil-dependent economies.

Decreasing oil rents in oil-based economies force the government to increase taxation and to reduce subsidies and public jobs. Negative oil rents shocks may reduce government spending on education and health, as well as its capacity to establish order and safety and to attract foreign investment. These developments may trigger public dissatisfaction with the performance of the state and increase the incidence of protests and anti-government demonstrations. A shadow economy may decrease the negative effect of falling oil rents on the incidence of protests if it acts as a safety net that provides jobs for the unemployed and as a channel to earn income for segments of the population with lower education, skills, or limited capital.

The existence and size of a shadow economy also limits the capacity of the state to increase tax burdens following a decline in oil rents. Ishak and Farzanegan (2020) examine this issue in a sample of 124 countries from 1991-2015. Their panel data analysis shows that declining oil rents after negative price shocks cease to have any significant positive effects on tax revenues in countries with sizable shadow economies. Considering their findings, one can expect that protests and demonstrations against higher tax burdens following negative oil shocks in oil-based countries is less likely with the presence of a sizable shadow economy. This may also indicate a lower chance of instability, as the shadow economy not only provides a safety net, but also is limits the ability of the state to levy significant taxes. Nevertheless, a significant drop in oil rents may force the state to search for taxation opportunities by repressing the shadow economy and increasing the costs of operating underground through higher penalties. This may backfire and mobilize the low-income households of the shadow economy, leading to more social unrest.

2.2.4 Main hypotheses

To summarize, the theoretical literature implies that both (negative) oil rents shocks and the shadow economy affect the risk of conflict and incidence of protests. Most arguments show that a drop in oil rents in oil-dependent economies increases the incidence and risk of political instability. Higher levels of oil-dependency will have greater negative effects on internal stability. However, higher resource rents, especially in the short term, may also increase the capacity of the political elite to pay off political opposition, maintaining stability and order. Similarly, theoretical predictions of the effects of shadow economies on instability are not conclusive.

Theory is also not conclusive regarding a shadow economy's effect on the relationship between declining oil rents and the incidence of conflict. There are arguments for the constructive and destructive roles of a shadow economy when an oil-based economy faces significant negative oil shocks. Shadow economies can either moderate or increase internal conflicts following decline in oil rents in these economies. On the one hand, if the shadow economy is able to absorb the large number of the unemployed and provide a safety net for the poorer households when the oil economy is in decline, then we may expect to observe a dampening effect. If the shadow economy helps to reduce income inequality and poverty, one may expect to see higher opportunity costs for individuals to enter conflict. On the other hand, if the shadow economy amplifies the budget deficit of a state which is already experiencing dropping oil income, then state capacity is further undermined and stability and order are more tenuous.

In light of these multifaceted arguments, the aim of our paper is to empirically (1) investigate the link between negative oil rents shocks and the incidence of protests and (2) investigate how the size of the shadow economy influences this nexus. Our hypotheses to be tested are formulated as follows:

Hypothesis 1: Negative oil rents shocks are associated with higher incidence of protests in oil-dependent countries, ceteris paribus.

Hypothesis 2: The final effect of negative oil rents shocks on the incidence of protests depends on the size of the shadow economy. Higher levels of the shadow economy (as a share of GDP) moderate the effect of negative oil shocks on the incidence of protests in oil-dependent countries, ceteris paribus.

2.3 Data and empirical strategy

2.3.1 Data

Our panel dataset combines information on oil price shocks, shadow economy and protest over the period 1991 to 2015. Our indicator for oil rent fluctuations is measured by the variation in international oil prices weighted by the degree of oil dependency. Hence, the country-specific measure of annual oil price shocks for country i at time t is constructed by multiplying the time-invariant whole-period average of country i's share of oil exports to GDP δ_i with the annual ln-change in international oil prices $\Delta lnOilPrice_t$ and takes the following form:

$$OilPriceShock_{it} = \delta_i \Delta lnOilPrice_t \tag{2.1}$$

This specification captures that oil price shocks should have a greater impact on countries with greater dependence on oil.[9] The oil exports data are from the United Nations' Comtrade data set reported according to the SITC1 system. The calculated country's share of oil to GDP δ_i was revised so that extreme values are replaced by the second highest value to avoid some of the problems associated with the reported export values that may be inaccurate for specific countries (Feenstra et al., 2005).[10] Data on international oil prices is taken from United Nations Conference on Trade and Development Commodity Statistics (UNCTAD, 2016).

To measure the shadow economy of a country, we rely on data taken from Medina and Schneider (2018), who define the shadow economy as "all the economic activities hidden from official authorities to avoid paying taxes and all social security contributions, to avoid governmental bureaucracy or the burden of regulatory framework, and for institutional reasons including corruption law, the quality of political institutions and weak rule of law." The estimates of the size of the shadow economy, measured as a percentage of GDP, are based on the Multiple Indicators Multiple Causes (MIMIC) model. This empirical approach first treats the shadow economy as an unobserved (latent) variable, identifying multiple causes and indicators for estimating its size. Second, it uses structural equations modeling to estimate the causal relationships between the unobserved variable and the observed indicators. A key advantage of this dataset is that it uses a light intensity approach instead of GDP as an indicator variable and, hence, captures a wider range of economic activities that are not reported by official GDP figures (Farzanegan and Hayo, 2019). A second advantage of this dataset is the inclusion of a longer time span and a wider set of countries.

We rely on data from the Cross-National Time-Series Data Archive (CNTS) (Banks and Wilson, 2018) to construct our indicator for protests. The CNTS dataset measures different types of political instabilities ranging from the less intense incidences of protests to

[9] See Bazzi and Blattman (2014), and Brückner, Ciccone and Tesei (2012) for a similar methodology.

[10] Only 15 observation were modified for The Bahamas (1975-1983), Congo Republic (2015), Oman (1970-1971), Equatorial Guinea (2000-2004) and Qatar (1971-1972).

major events such as civil wars and coups. Given the purpose of this paper, we only select three indicators for less violent events of instabilities: anti-government demonstrations, general strikes and riots. Our measure for protests is a count variable (expressed in logs) calculated by summing the numbers of all demonstrations, strikes and riots that took place in a country in a given year; hence, it captures the magnitude or the intensity of the instability. Table 1 provides the summary statistics for our variables of interest.

Table 2.1: Summary Statistics

Variable	**N**	**Mean**	**SD**	**Min**	**Max**
Oil price shock	3114	0.004	0.02	-0.16	0.18
Protest (log)	3114	0.46	0.78	0	4.98
Shadow Economy (% of GDP) (log)	3114	3.33	0.49	1.82	4.28
GDP per capita (log)	3114	8.46	1.57	4.75	11.63

2.3.2 Empirical strategy

Our conceptual framework hypothesizes that the effect of negative oil price shocks on protests depends on the size of the shadow economy, ceteris paribus. Specifically, a decline in oil rents will increase protests, but the effect is lower the larger the size of the shadow economy. To test these two hypotheses, we estimate the following model:

$$\begin{aligned}\Delta lnProtest_{it} = \alpha_i + \theta_t + \gamma_i t + \beta_1 3 - yearOilPriceShock_{it} + \beta_2 lnSE_{it-3} \\ + \beta_3 OilPriceShock_{it} \times lnSE_{it-3} + \epsilon_{it} \end{aligned} \tag{2.2}$$

The dependent variable $lnProtest_{it}$ is the sum of protest events in a given country i and year t, α_i is country fixed effects, θ_t is year fixed effects and ϵ_{it} is the error term.[11] The $\gamma_i t$ is a country-specific linear time trend to account for potential omitted variables and pre-existing trends. $OilPriceShock_{it}$ is the explanatory variable of interest proxying changes in oil rents. This is measured as the weighted-change in (log) oil prices averaged over the previous three years. Employing the three-year average has the advantage of accounting for the time-dependence of oil shocks, in addition to reducing the role of transitory shocks and measurement error in the explanatory variable. It also allows for the lag response of the outcome variable.[12]

To investigate the mitigating role of the shadow economy, we add the (log) size of the shadow economy (% of GDP), $lnSE_{it-3}$, both by itself and interacted with oil price shocks. This allows us to examine the impact of oil price shocks on protest conditional

[11]Since our econometric specification employ the log of protest, we add "one" to the number of each protest indicator to avoid sample selection bias that would arise from dropping country-year observations with no reported protest event in at least one year.

[12]This is same approach followed by Brückner and Ciccone (2010), Brückner, Ciccone and Tesei (2012) and Caselli and Tesei (2016). Ciccone (2011) shows (in the context of rainfall-induced income shocks) that in the presence of a non-stationary time series, the effect of shocks on conflicts can be uncovered using a specification in differences. Furthermore, given the persistence of oil prices, using contemporaneous year-to-year change without taking into account the effect of shocks in previous years could lead to displacement effects and could incorrectly estimate the effect of a given shock on protests which could be due to previous shocks that had a lag effect.

upon the initial size of the shadow economy. To be consistent with the starting date of the price shock and to address reverse causality, the initial size of the shadow economy is measured at year $t-3$. Hence, if the change in oil prices is measured as the average over the years t, $t-1$, $t-2$ and $t-3$, the size of shadow economy enters at year $t-3$. Table A2 in Appendix A shows that the lagged levels of a shadow economy are uncorrelated with contemporaneous oil price changes, which suggests that a lagged level of the shadow economy can be treated as a predetermined variable whose lagged levels are uncorrelated with the current error term. This mitigates the endogeneity concerns and additionally rules out reverse causality, since it is unlikely for protest at time t to affect the shadow economy at time $t-3$.[13] Furthermore, it should be noted that even if there could be remaining concerns regarding the full exogeneity of the shadow economy, the interaction term between oil price shocks (i.e., the exogenous variable) and the shadow economy will remain consistent (Bun and Harrison, 2019).[14]

In this specification, β_1 captures the linear effect of oil price shocks on protest conditional upon the initial size of the shadow economy. We expect the sign of the linear effect to be negative (i.e. $\beta_1 < 0$), and the sign of the conditional effect to be positive (i.e. $\beta_3 > 0$). Hence, oil price shocks are expected to have a smaller effect on protest proliferation in countries with a relatively larger shadow economy. The time variation stems from movements in international oil prices, allowing the effects to change based on the degree of oil dependency. This helps to circumvent problems associated with using conventional measures of resource wealth, such as export or production levels (typically normalized by GDP or population), which could be spuriously correlated with our outcome of interest. The included country- and year-fixed effects control for all time-invariant country characteristics and global trends. We cluster the standard errors at the country level.[15]

The usage of (non-) differenced specifications is motivated by the time series properties of international oil prices, protests and shadow economies. In Table A1 in Appendix A, we provide formal unit root tests for these variables. The tests cannot reject the null hypothesis of the presence of a unit root in the time series of oil price and protests in levels, but they reject it for their first differences. For the shadow economy, formal tests reject the null hypothesis of the presence of unit roots in levels and its first difference.

[13] We also considered using the second and fourth lags, when choosing the initial level of the shadow economy and results remain robust (available upon request). However, deeper lags severely reduce our sample size, and the second lag does not match the starting date of the shock.

[14] We use the shadow economy in logs to smooth out outliers and obtain a smoother distribution of values, but results remain robust if the shadow economy is entered in levels (see Table 4).

[15] It is worth noting that our empirical specification does not intend to capture variation in absolute levels of protests across countries and does not imply that protests should be attributed primarily to oil price shocks. To eliminate these time-variant cross-country differences in the propensity and drivers of protest occurrence, we need to take the first difference of protests in the presence of country fixed effects, year fixed effects and country-specific time trends to account for secular changes in protest breakouts that vary across countries and over time. In this way, we exploit the annual fluctuations (i.e., growth) of protests to assess the impact of oil price shocks and the mitigating role of the shadow economy.

2.4 Empirical results

2.4.1 A first look at the data

Our main argument is that the effect of a decline in oil rents due to negative price shocks on protests depends on the size of the shadow economy. To get an initial snapshot of this relationship, Figure 1 graphs the life table survival estimates for country groups classified based on the degree of their dependency on oil revenues and the size of their shadow economies. The graph of the protest survival function in Figure 1(A) indicates that the average survival rate for experiencing protests is slightly higher for countries where the share of the shadow economy to GDP is greater than the sample median ("high shadow economy countries") compared to the rest ("low shadow economy countries"). Put differently, high shadow economy countries may have a slightly lower likelihood of witnessing protests than low shadow economy countries. However, the log-rank test for equality of survivor functions fails to reject the null hypothesis that both groups are equal, meaning there is no statistically significant difference for the likelihood of witnessing protests between small and large shadow economy countries. Figure 1(B) plots the protest survival function for countries whose share of oil exports to GDP is greater than the sample median ("high oil dependent countries") against countries with lower dependency on oil ("low oil dependent countries"). This indicates that the average survival rate for experiencing protests is lower for high oil dependent countries and that there is a higher likelihood of witnessing protests in high oil dependent countries compared to low oil dependent countries. The log-rank test for equality of survivor functions rejects the null hypothesis that both groups are equal.

Finally, Figure 1(C) plots the protest survival function for high oil dependent countries with large shadow economies against high oil dependent countries with small shadow economies. This indicates that the average survival rate for experiencing protest is higher for high oil dependent countries with large shadow economies relative to those with small shadow economies. The log-rank test for equality of survivor functions rejects the null hypothesis that both groups are equal.

2.4.2 Baseline results

Table 2.2 presents our main results for estimating equation 2.2. In Column (1), we look at the average effect of the three-year average oil price shocks on protests without controlling for the initial size of the shadow economy. The negative coefficient implies that oil price shocks, on average, have a negative impact on protests, but it is not statistically significant. In Columns (2) and (3), we divide our sample into high shadow economy and low shadow economy countries, based on whether the three-year lagged shadow economy is above or below the median. We see that oil price shocks have no statistically significant effect on protests in the high shadow economy group, but have a negative and statistically significant effect on protests in low shadow economy countries. In addition, the reported Chow test rejects the hypothesis that the estimated coefficient in high shadow economy countries is the same as the coefficient in low shadow economy countries. Hence, it is not surprising

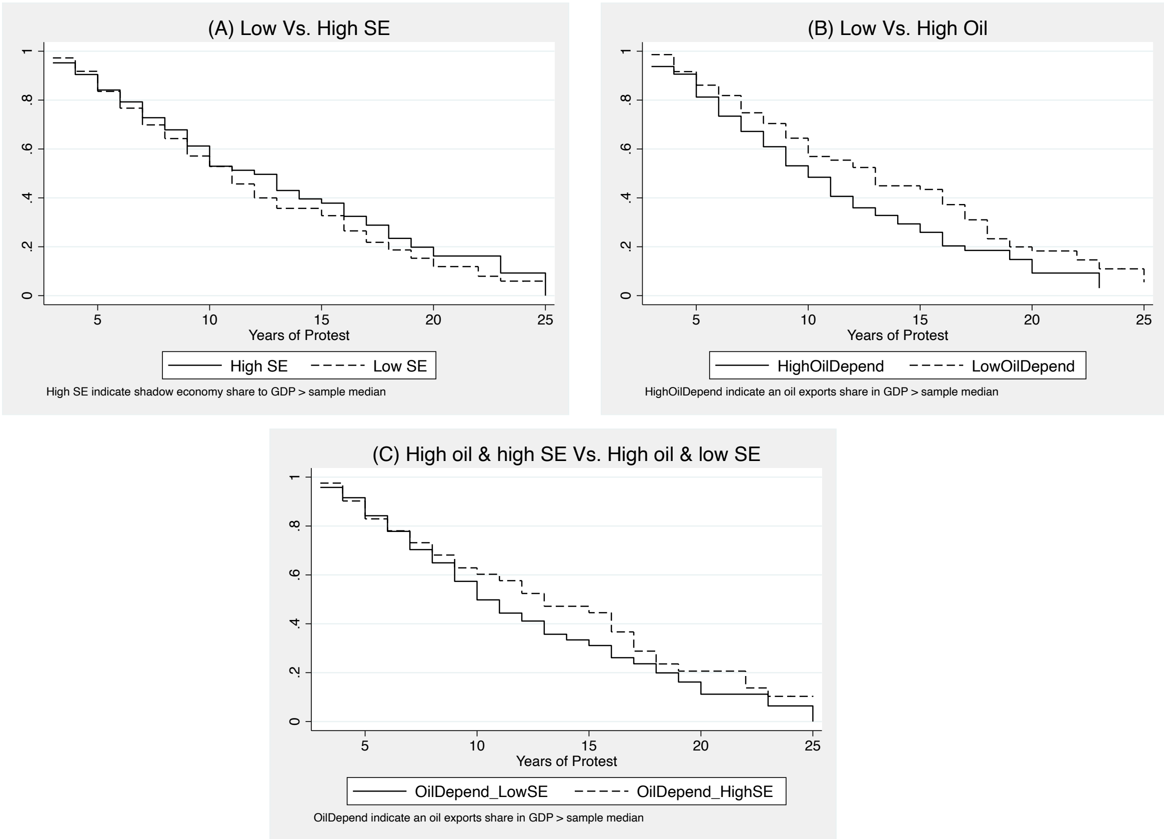


Figure 1: Oil and protest survival functions

Note: Figure 1(A) displays the protest survival function for high shadow economy (SE) countries (i.e., share of the shadow economy to GDP > sample median) vs. low SE countries. Figure 1(B) plots the estimates for high oil dependent countries (i.e., share of oil exports to GDP > sample median) vs. low oil dependent countries. Figure 1(C) plots the protest survival function for high oil dependent countries with high SE against high oil dependent countries with low SE.

to see the average effect of oil price shocks in the full sample to be nil, since the opposing effects found in the low shadow economy and high shadow economy groups offset (with both samples having an equal number of observations).

Column 4 adds the three-year lagged level of (log) shadow economy (as a % of GDP), both by itself and interacted with oil price shocks, as additional control variables. We see that the coefficient of the oil price shocks is still negative and statistically significant, while the interaction term between the oil price shocks and the lagged shadow economy is positive and significant at a 1% significance level. This is in line with our hypotheses that negative oil price shocks significantly increases protests in small shadow economy countries, but the effect dissipates as the initial size of the shadow economy increases. In very high levels of shadow economies, the effect becomes very small and statistically insignificant.[16]

[16] We checked whether the effect of negative oil price shocks differs from positive price shocks by including a dummy in Column (4) that takes the value of 1, if the 3-year average oil price shock is strictly negative. Our coefficients of interest remain unchanged in sign and significance, but the interaction effects were statistically insignificant. Hence, the estimated effect of negative shocks is not statistically significantly different from positive shocks. This follows that our interpretation is also applicable in case of positive oil

To put the coefficients in Column (4) into perspective, we see that in a low-shadow economy country (where the shadow economy is around 7% of GDP), the effect of a 1 percentage point decline in weighted-international oil price implies an increase in protests by 6 percentage points. In a mid-shadow economy country (where the shadow economy is around 32% of GDP), the effect of a 1 percentage point decline in weighted-international oil price implies an increase in protests by 0.64 percentage points. Negative oil price shocks cease to have a significant impact on protests in high-shadow economy countries (where the shadow economy is more than 35% of GDP). Consider Iran, Equatorial Guinea and Nigeria as examples of oil dependent countries, with an average shadow economy of 17.6%, 31% and 56.4% of GDP, respectively. The coefficient estimates imply that in response to a 1 percentage point decline in weighted-international oil prices, protests will increase in Iran by 2.7 percentage points but will only increase by 0.8 percentage points in Equatorial Guinea. The same decline will have no significant impact on protest in Nigeria.

Finally, in Columns (5)-(7), we disaggregate our protest measure into its three indicators: riots, strikes and anti-government demonstrations. We find that the number of both riots and anti-government demonstrations increases significantly following negative oil price shocks, but the effect becomes less severe the larger the initial size of the shadow economy. In contrast, negative oil price shocks have no statistically significant impact on strikes. The estimated coefficients also suggest that oil price shocks have a stronger impact on riots, both quantitatively and qualitatively, relative to anti-government demonstrations and strikes.

Table 2.2: Oil price shocks, protest and shadow economy

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
	Δ ln Protest	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Riots**	**Δ ln Strikes**	**Δ ln Demos**
		High SE	**Low SE**				
Oil price shock, t	-0.589	0.472	-1.584**	-12.406***	-9.202**	-1.883	-6.415*
	(0.437)	(0.742)	(0.696)	(3.973)	(3.763)	(1.341)	(3.739)
SE (log), t-3				0.122	0.103	-0.008	0.005
				(0.158)	(0.109)	(0.057)	(0.135)
Oil price shock × SE (log)				3.391***	2.394**	0.579	1.799*
				(1.097)	(1.034)	(0.363)	(1.073)
Chow test p-value		0.01	0.01				
No. of observations	3,114	1,557	1,557	3,114	3,114	3,114	3,114
No. of countries	144	94	102	144	144	144	144
R-squared	0.055	0.053	0.120	0.056	0.066	0.037	0.053
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country × trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Oil price shock is the average three year ln-change in the oil price multiplied by whole period average oil exports share to GDP (1991-2015). The dependent variable in columns 1-4 is the ln-change of sum of protest events that took place in a given country at a given year; in column 5 is ln-change in the number of riots; in column 6 is ln-change in the number of strikes; and in column 7 is the ln-change in the number of anti government protests.The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects, year fixed effects and country-specific time trend are not reported. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

price shocks.

2.5 Robustness checks

Table 2.3 presents various important robustness checks to our main results (Column (4) in Table 2). One concern is that the international price of oil could be endogenous to major oil producers and exporters, introducing bias to the estimates. Specifically, reverse causality may arise if an intensification of protests disturbs oil production and thus world supply, causing international prices to increase. To account for this, in Column (1), we exclude the quintile of countries with the highest whole-period average oil exports as a share of GDP, while in Column (2) we exclude the top quintile of countries whose share of world oil production exceeds 3%, averaged over the sample period.[17] Additionally, we exclude members of Organization of the Petroleum Exporting Countries (OPEC) in Column (3). In all instances, the coefficients of interest maintain their signs and statistical significance. Next, in Columns (4) and (5), we divide the sample into democratic and autocratic regimes to investigate the heterogeneity of oil price shocks on protests. We base our classification for political regimes on the Polity IV regime database (Marshall and Jaggers, 2016) and follow the convention of classifying countries as democracies (autocracies) if their Polity2 score is strictly positive (negative) (e.g., Persson and Tabellini, 2009; Caselli and Tesei, 2016).[18] Our coefficients of interest remain robust in both samples. Moreover, the Chow test fails to reject the null hypothesis that impact of oil price shocks on protest in autocracies is equal to that of the full sample. Thus, there is no statistically significant difference between autocracies and democracies on the effect of protests following oil price shocks.

In Column (6), we omit the quintile of country-year observations with the highest share of shadow economy to GDP (i.e., more than 55% of GDP) to check whether the results are influenced by extreme observations. Similarly, in Column (7), we drop the quintile of countries with the highest share of shadow economy to GDP, averaged over the whole-period sample. In both cases, our main results remain robust in sign and significance. Finally, in Column (8), we check whether our estimates are sensitive to a specific measure of the shadow economy. We employ an alternative measure for the size of a shadow economy taken from Alm and Embaye (2013), with estimates based on the currency demand method for the period 1984-2006. In our sample, the correlation between Medina and Schneider's (2018) estimates of the shadow economy with Alm and Embaye (2013) is around 0.78. The coefficients of interest remain qualitatively similar despite the smaller sample size.

Table 2.4 conducts further robustness checks. In Column (1), we use a different weight for oil price shocks by employing the mid-period value of oil exports share to GDP. We calculated the average of the closest five years to the year 2003. In Column (2), we restrict our sample to high oil dependent countries, defined as oil exporters whose whole-

[17] The latter group was identified using Ross and Mahdavi (2015) dataset on oil production covering the period 1932–2014, and refers to the top 10 percent of oil producers or countries producing (over the whole-period average) more than 3 percent of world oil production.

[18] Following Bruckner and Ciccone (2010), we adjust Polity2 so that periods of interregnum, coded as 0, and transitionary periods are treated as missing. Such adjustment ensures that instability is not affected by the particular political situation in a given year. The results also remain robust to the inclusion of those periods.

Table 2.3: Oil price shocks, protest and shadow economy – Robustness checks I

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ ln Protest	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**
	Drop major exporters	**Drop major producers**	**Drop OPEC**	**Demo-cracy**	**Auto-cracy**	**Drop extreme SE values**	**Drop high SE countries**	**Alm & Embage SE**
Oil price shock, t	-12.142**	-11.299**	-24.447***	-27.419**	-12.240**	-13.732***	-13.426***	-23.151**
	(5.627)	(4.654)	(6.831)	(12.492)	(5.018)	(5.173)	(4.295)	(10.611)
SE (log), t-3	0.129	0.069	0.073	0.241	0.092	0.028	0.093	0.104
	(0.162)	(0.169)	(0.167)	(0.188)	(0.433)	(0.163)	(0.159)	(0.094)
Oil price shock × SE	3.500**	3.078**	7.059***	7.328**	3.441**	3.772**	3.694***	6.898**
	(1.618)	(1.289)	(1.916)	(3.062)	(1.428)	(1.477)	(1.203)	(3.163)
Chow test p-value				0.53	0.53			
No. of observations	2,979	2,874	2,871	2,090	830	2,828	2,982	2,116
No. of countries	137	133	132	111	56	138	138	108
R-squared	0.054	0.055	0.053	0.074	0.158	0.066	0.056	0.041
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country × trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Oil price shock is the average three year ln-change in the oil price multiplied by whole period average oil exports share to GDP (1991-2015). The dependent variable is the ln-change of sum of protest events that took place in a given country at a given year. In column 1, we exclude the quintile of countries with highest whole-period average oil exports as a share of GDP; in column 2, we exclude quintile of countries whose share of world oil production exeeds 3% averaged over the sample period; in column 6, we exclude the quintile of country-year observations with highest share of shadow economy to GDP; and in column 7, we drop the quintile of countries with the highest share of shadow economy to GDP averaged over the whole period sample. Democratic (autocartic) countries are defined as those whose Polity2 score strictly > (<) zero. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects, year fixed effects and country-specific time trend are not reported. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

Table 2.4: Oil price shocks, protest and shadow economy – Robustness checks II

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δ ln Protest	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**
	Weight average mid-period	**High oil exporters only**	**Developed countries**	**Developing countries**	**Additional natural resources**	**Post-2003 period**	**Placebo test**
Oil price shock, t	-11.814***	-12.494***	-13.207	-27.984***	-12.611***	-19.541***	1.597
	(3.827)	(4.031)	(10.685)	(9.353)	(4.365)	(6.744)	(4.587)
SE (log), t-3	0.118	0.173	0.180	0.089	0.094	0.003	0.048
	(0.158)	(0.235)	(0.197)	(0.242)	(0.163)	(0.349)	(0.114)
Oil price shock × SE	3.266***	3.373***	4.102	7.476***	3.423***	5.118***	-0.360
	(1.073)	(1.095)	(3.445)	(2.494)	(1.208)	(1.830)	(1.382)
Additional natural resources (with interactions)					Yes		
Chow test p-value		0.08	0.13	0.13		0.19	
No. of observations	3,114	1,538	0.083	0.056	3,064	1,850	3,114
No. of countries	144	72	1,055	2,059	143	144	144
R-squared	0.056	0.066	49	95	0.059	0.095	0.042
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country × trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Oil price shock is the average three year ln-change in the oil price multiplied by whole period average oil exports share to GDP (1991-2015). The dependent variable is the ln-change of sum of protest events that took place in a given country at a given year. The additional natural resources are mineral rents, natural gas rents and coal rents, all as a percentage of GDP. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects, year fixed effects and country-specific time trend are not reported. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

period average exports share to GDP is above the median. In Columns (3) and (4), we differentiate between developed and developing countries based on the World's Bank income classification. In Column (5), we account for the additional effects of other natural resources, such as coal, natural gas and minerals, to alleviate the concern that dependence on these natural resources rents may be correlated with oil prices and influence the results. These natural resources are measured by their corresponding rents as a share of GDP (WDI, 2018). This is particularly important to check since oil exporters could also be exporters of other commodities and highly dependent on both.

In all specifications, the signs and statistical significance of our variables of interest remain robust, with no statistically significant differences in effects between developed and developing countries. However, we find a relatively higher effect in high oil dependent countries compared to low oil dependent countries and the difference is statistically significant (Chow test p-value 0.08). The dependence on other natural resource rents and their interactions with the shadow economy are statistically insignificant and small in magnitude (not reported). This means that, on average, a greater dependency on oil- and thus greater exposure to price changes- dominates the effects of dependency on other minerals. In other words, the relationship of interest still holds even conditional on the presence of other minerals.[19]

In Column (6), we restrict our sample to the post-2003 period, which constitutes half of the sample period and captures the oil boom period. The estimated coefficients maintain similar significance levels as the entire sample. The Chow test fails to reject the null hypothesis that the estimated coefficients in this sub-sample is the same as for the full sample. Finally, in Column (7), we conduct a placebo test to make sure that our results are not mechanical or driven by pre-existing trends. We do this by regressing past protest data (1966-1990) on current period oil price shocks. We find that the coefficients of interest have switched signs and lost their significance.

Other robustness checks are reported in Table A3 in Appendix A. We check the robustness of our baseline results to the inclusion of fixed effects and drop in influential observations. Column (1) removes country specific-time trends and Column (2) removes both country fixed effects and country specific-time trends. In all models, our main variables of interest remain robust in sign and significance. In Columns (3) and (4), we omit influential observations using Cook's distance and Welsch distance formulas, respectively. The coefficients of interest show the predicted signs and are statistically significant in all instances.

2.6 Conclusion

Fluctuations in oil prices and rents can have significant implications for political instability in countries. We investigated the incidence of protests to oil rents shocks relationship in 144 countries from 1991 to 2015. Our panel estimations show that there is a significant positive

[19]Note that here we only investigate the interaction between dependency on other natural resources and oil price fluctuations, and not the impact of other commodity price fluctuations. The latter is left for future research to differentiate between the impact of other point resources (coal, gold) and non-point resources (agriculture and food products).

effect of negative oil rents shocks on protests (i.e., anti-government demonstrations, riots, and strikes). Our main contribution, however, is to present the shadow economy as an important moderator in the negative oil shocks-conflict problem.

Our results indicate that negative oil price shocks significantly increase protests in small shadow economy countries, but the effect dissipates as the initial size of the shadow economy increases, eventually vanishing at the highest levels. A larger shadow economy can thus contribute to political stability by providing a complementary source of income for countries that depend on oil revenues and experience negative price shocks. Our results are also in line with findings of Choi and Thum (2005) who show that the presence of a shadow economy mitigates government-induced distortions that may arise after negative price shocks, and as a result, leads to more economic activities in the formal sector. Our results on the constructive role of the shadow economy in mitigating the effects of declining oil rents on conflicts may be seen in contrast with the studies of Loayza (1996) and Johnson et al. (1997). These studies argue that the formal and informal sectors compete for resources and the existence of the shadow economy is viewed as destructive for economic growth.

This finding has several implications. First, such a mitigating role should allow for reconsiderations of permanent calls to eliminate the unofficial economy, by depicting it as a source of evil, a stance that simply conflates causes with symptoms. Governments must recognize that the existence of a shadow economy serves as an implicitly or explicitly integral part of social risk management strategies. Second, even with the justified objective of eliminating inefficiencies in allocating goods and factors in the economy, deregulation and structural adjustment strategies must be designed carefully. Specifically, strategies must be implemented in such a way that a reduction or abolishment of the shadow economy will be complemented by an increase in or establishment of other risk management pillars (social security payments, unemployment insurance, etc.). Third, diversification of production will reduce state dependency on oil revenues and therefore, economic vulnerability to shocks. Thus, industrial diversification strategies can serve as an important complement to strategies aimed at reducing the role of the shadow economy. Ultimately, the existence of the shadow economy is a response to unsound economic policies and inefficient economic structures that fail to shield the economy against shocks, aspects that should be addressed in advance.

Future research should investigate drivers of shadow economy activities in oil dependent countries and investigating the effect of other commodity price shocks. Shadow economies may exist to correct market inefficiencies by providing business opportunities for small-scale firms and low skilled and poorly educated workers. These segments of the population are systemically excluded from official economies due to heavy market regulations, inability to access credit, and poor educational and training services in oil producing countries (Dreher and Schneider, 2010; van der Ploeg, 2011; and Gylfason, 2001).

2.A Appendix A

List of countries included

Albania, Algeria, Angola, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahamas, Bahrain, Bangladesh, Belarus, Belgium, Belize, Benin, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Brunei, Bulgaria, Burundi, Cameroon, Canada, Chad, Chile, China, Colombia, Congo Dem. Rep., Congo Rep., Costa Rica, Cote d'Ivoire, Croatia, Cyprus, Czech Republic, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea-Bissau, Guyana, Honduras, Hungary, Iceland, India, Indonesia, Iran, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Korea Rep., Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Malta, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Saudi Arabia, Senegal, Singapore, Slovakia, Slovenia, Solomon Islands, South Africa, Spain, Sri Lanka, Suriname, Swaziland, Sweden, Switzerland, Tajikistan, Tanzania, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States, Uruguay, Venezuela, Vietnam, Yemen, Zambia, Zimbabwe.

In Table 2.A1, we conduct formal unit root tests for the log of oil prices, protests, the shadow economy and GDP per capita. The tests cannot reject the null hypothesis of the presence of a unit root in the time series of oil prices, protests and GDP in levels, but they reject it for their first differences. Although the tests for the log of protests contradict in the series in levels, they both reject the hypothesis of the presence of unit roots in their first differences. For the shadow economy, formal tests reject the null hypothesis of the presence of unit roots in levels and the first difference.

Table 2.A1: Unit Root Tests

Variable	Ln Oil Prices (Time-Series Tests)		Ln Protest (Panel Data Tests)		Ln Shadow economy (Panel Data Tests)		Ln GDP per capita (Panel Data Tests)	
	Level	**Diff.**	**Level**	**Diff.**	**Level**	**Diff.**	**Level**	**Diff.**
Dickey-Fuller	n.s.	**	n.s.	***	***	***	n.s.	***
Dickey-Fuller-GLS	n.s.	**	-	-	-	-	-	-
Philipps-Perron	n.s.	***	***	***	***	***	n.s.	***

Note: All unit root tests contain trend. For panel data, we apply the fisher type tests. Abbreviation: n.s., not significant at the 10% level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

Table 2.A2 formally checks whether lagged levels of shadow levels can be used as a predetermined variable by regressing the lagged levels of shadow economy at time $t-2$, $t-3$ and $t-4$ on contemporaneous oil price shocks. The results show that the coefficient of oil price shocks is small and statistically insignificant in all models. Hence, this lends us credence that lagged levels of the shadow economy are uncorrelated with the error term

at time t and can be safely treated as a predetermined variable.

Table 2.A2: Correlation oil price shocks and lagged levels of shadow economy

Model	(1)	(2)	(3)
	SE (log), t-2	**SE (log), t-3**	**SE (log), t-4**
Oil price shock, t	-0.097	-0.060	-0.056
	(0.145)	(0.139)	(0.128)
Number of observations	3,114	3,114	2,980
Number of countries	144	144	143
R-squared	0.738	0.725	0.713
Country FE	Yes	Yes	No
Year FE	Yes	Yes	Yes
Country specific-time trend	Yes	No	No

Oil price shock is the average three year ln-change in the oil price multiplied by whole period average oil exports share to GDP (1991-2015). The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects, year fixed effects and country-specific time trend are not reported. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

Table 2.A3 checks the robustness of Equation (2) to the inclusion of fixed effects and removal of influential observations. Column (1) drops the country specific-time trends and Column (2) drops both country fixed effects and country specific-time trends. In all models, our main variables of interest remain robust in sign and significance. In Columns (3) and (4), we omit influential observations using Cook's distance and Welsch distance formulas, respectively. The coefficients of interest show the predicted signs and are statistically significant in all instances.

Table 2.A3: Oil price shocks, protest and shadow economy – Robustness checks III

Model	(1)	(2)	(3)	(4)
	Δ ln Protest	**Δ ln Protest**	**Δ ln Protest**	**Δ ln Protest**
	Drop time trend	**Drop time trend & country FE**	**Drop influential observations (Cook's distance)**	**Drop influential observations (Welsch's distance)**
Oil price shock, t	-10.980***	-8.783***	-4.455*	-7.041**
	(3.906)	(3.076)	(2.697)	(2.929)
Shadow economy (log), t-3	0.001	-0.032***	0.086	0.123
	(0.093)	(0.010)	(0.110)	(0.110)
Oil price shock × SE	2.959***	2.382***	1.229*	2.015**
	(1.058)	(0.836)	(0.739)	(0.832)
Number of observations	3,114	3,114	2,842	2,869
Number of countries	144	144	144	144
R-squared	0.042	0.041	0.103	0.100
Country FE	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country specific-time trend	No	No	Yes	Yes

Oil price shock is the average three year ln-change in the oil price multiplied by whole period average oil exports share to GDP (1991-2015). The dependent variable is the ln-change of sum of protest events that took place in a given country at a given year. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects, year fixed effects and country-specific time trend are not reported. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

The Impact of Declining Oil Rents on Tax Revenues: Does the Shadow Economy Matter?[*]

Abstract

We study the association between oil rents and tax revenues, highlighting the importance of the shadow economy (SE) as a moderating factor in this relationship. Declining oil rents may not lead to higher tax efforts in a state if the SE is sizable. Using a sample of 124 countries from 1991 to 2015, our panel data regression analysis illustrates the moderating role of the SE in the final effect of negative oil rent shocks on tax revenues. A decline in oil rents following negative oil price shocks ceases to have any significant positive impact on tax revenues in countries with an SE representing more than 35% of gross domestic product. The results are robust after controlling for country- and year-fixed effects, other determinants of tax revenues, and using a dynamic model.

Keywords: shadow economy, tax revenues, oil rents, resource curse

JEL codes: Q32; Q35; H26; O17

3.1 Introduction

In the wake of the 2014 oil price crash, oil-producing countries suddenly faced a sharp decline in oil prices that left them in a serious fiscal situation, given that the bulk of their government revenues came from the proceeds of oil.[1] Concerns were raised about the ability of those countries, especially those with a high dependence on oil, to compensate for their fiscal losses and mobilize new revenue sources. Despite attempts by several countries to enact tax reforms (e.g., Kazakhstan, Angola, and Brazil), not all of them were successful in boosting their tax receipts. In this context, this study goes a step further in understanding the relationship between negative oil price shocks and tax revenues. Our goal is to show that the effect of a negative shock on a country's tax revenues depends on the size of the shadow economy (SE).[2] We show that a fall in oil rents does not lead to

[*]This chapter is co-authored with Mohammad Reza Farzanegan and published as *"Ishak, P. W., and Farzanegan, M. R. (2020). The Impact of Declining Oil Rents on Taxation: Does the Shadow Economy Matter? Energy Economics 92, 104925"*

[1]https://blogs.worldbank.org/developmenttalk/what-triggered-oil-price-plunge-2014-2016-and-why-it-failed-deliver-economic-impetus-eight-charts

[2]We follow the definition of an SE presented by Schneider (2005) and Buehn and Schneider (2012 a,b). Their definition of the SE (i.e., an informal economy) covers the production and transactions of "legal" goods and services that are not reported for tax purposes. This definition excludes illegal activities, such as the drug trade and human trafficking. According to Schneider, there are four reasons for economic agents moving from the formal to the SE: (1) evading income-, value-added, and other tax payments; (2) evading payment of social contributions; (3) evading implementation of special labor standards, such as minimum wages and safety and environmental standards in the production process; (4) evading compliance with standard administrative processes, such as completing statistical questionnaires.

higher tax revenues in the presence of a sizable SE. Our study contributes to the resource curse literature by conditioning the effect of changes in oil rents on tax revenues to the existing SE.

We argue that "negative" changes in oil rents could increase the willingness of the state to initiate and implement tax reforms to increase tax revenues, but only conditionally. In particular, we emphasize the relevance of the size of the SE as a key determinant of the impact of declining oil rents on tax revenues. Our suggestive evidence and simple theoretical framework (see Appendix A) demonstrate that negative shocks in oil rents promote the tax revenues of the state when the size of the SE is sufficiently moderate, whereas they have no significant positive impact on tax revenues when the SE is extensive. The existence of the SE offers a safe haven for businesses and people to conceal their economic activities from tax authorities. It follows that a rise in tax rates will have a limited effect in compensating for a decline in government revenues from oil receipts in the presence of large informal economies and the respective low tax bases.

We examine our main hypothesis on the oil rents-tax revenues-SE nexus using panel data from 1991 to 2015 for a sample of 124 countries. We suggest that an expected increase in tax revenues in response to negative changes in oil rents is not happening automatically. The final effect of negative changes in oil rents on tax revenues depends on the size of the SE. In countries with moderately sized SEs, we may expect to observe an increase in government tax revenues following negative oil price shocks. However, if the size of the SE is significant, then we should not experience an increase in tax revenues. For example, the case of Kazakhstan, an oil-based economy, is informative. Between 2014 and 2016, the average crude oil price declined from \$96 to \$50 and then to \$42 per barrel. This significant fall in oil prices led to a decline in oil rents (as a share of gross domestic product (GDP)) in Kazakhstan from 13% in 2014 to 7% in 2016 (WDI, 2016). Such a significant negative shock in oil rents was not associated with an increase in tax revenues, despite the implementation of tax reforms. Tax revenues as a share of GDP, which were 14% in 2014, declined to 10% in 2016. One possible reason for the failure of the Kazakhstan government to increase its tax efforts in response to falling oil rents was the sizable SE. According to Medina and Schneider (2019), the size of the SE (% of GDP) increased from 32% to 37% during the mentioned period.[3]

To the best of our knowledge, the moderating role of the SE in the nexus between tax revenues and negative oil shocks is neglected in the resource curse literature.[4] The resource curse hypothesis implies that resource-based economies, on average and in the long run, have slower rates of economic growth compared to resource-poor countries (Sachs

[3]The importance of dealing with the SE to improve the rate of tax collection in Kazakhstan is discussed here: https://www.worldbank.org/en/news/feature/2020/03/10/towards-a-more-dynamic-economy-revenue-reform-in-kazakhstan

[4]In unreported results, we also checked the effect of positive oil price shocks on tax revenues controlling for GDP per capita (i.e., tax base) and found no statistically significant effect. This could be due to the fact that tax rates in these countries (most of which are developing countries) are already low. Therefore, a rise in oil revenues following positive price shocks will have no significant effect on tax rates but may instead increase public expenditure (as is partly shown by Farzanegan, 2011 for case of Iran). However, future research may examine this issue in more detail.

and Warner, 1995, 2001). To date, much of this literature has studied the effect of an increase in oil rents on growth in developing countries and has shown different transmission mechanisms for the negative growth impacts of higher rents (e.g., Alexeev and Conrad, 2009).[5] Our study particularly relates to the fiscal transmission channel, which stipulates the negative effects of resource rents' dependency on the taxation capacity of the state and the willingness to reform the tax system. However, most of this literature relates to positive oil rent changes and usually reports a negative relationship between tax revenues and resource rents, neglecting the existence of contextual conditional effects. For example, using the U.S. case study, James (2015) argues that, in response to higher resource revenues, the government decreases non-resource tax rates and shows that a \$1 increase in resource revenues results in a \$0.25 decrease in non-resource revenues. Using a sample of resource-rich economies, Crivelli and Gupta (2014) show a significant negative impact from positive changes in resource rents on the taxation of goods and services. We extend this literature by analyzing the impact of negative changes in oil rents on government taxation performance. We further examine the conditional role of the SE in the final effect of negative changes of oil rents on government tax efforts, an aspect that is neglected in the literature. In this regard, we partly relate to recent studies reporting a negative association between SE and tax revenues (e.g., Mazhar and Méon, 2016; Awasthi and Engelschalk, 2018), albeit shedding more lights on the final association between oil rents and tax revenues under different sizes of the SE.

Another strand of literature investigates the long-term negative effects of rent dependency on tax administration. Besley and Persson (2011, p.21) argue that a higher dependence on resource rents (or aid) that flow directly to the government budget may mean that market incomes are smaller. This leads to a smaller tax base, which then diminishes the incentive to invest in a market-supporting legal capacity. This lack of development of state administrations, especially with reference to raising tax revenues, is also related to the rentier state hypothesis introduced initially by Mahdavy (1970) in his case study of Iran and developed in later studies, such as Beblawi and Luciani (1987) (for further discussion, see Besley and Persson, 2013, 2014).[6] We relate to this strand by documenting that the presence of a large SE, and subsequent low tax base, reduces government incentives and/or constrains efforts to develop strong fiscal systems.

Other scholars, such as Ross (2001, 2012), use the fiscal channel to explain democracy deficits in oil-rich economies. The negative effect of rents on political institutions is due to the response of tax revenues to positive changes in oil rents. Higher oil rents may reduce the willingness of the state to tax citizens and cause the postponement of tax reforms. The lower fiscal dependency of the state on citizens may reduce the demand for

[5]For various investigations of the transmission channels of oil curse see Farzanegan and Thum, 2020; Ishak, 2019; Bjorvatn and Farzanegan, 2015; Farzanegan, 2014; Ross, 2012; Bjorvatn et al. 2012; van der Ploeg, 2011; Tsui, 2011; Aslaksen, 2010; Frankel, 2010; Venables, 2010; Mehlum et al. 2006; Hodler, 2006; and Gylfason, 2001, among others.

[6]In a theoretical and empirical investigation, Jensen (2011) also shows that "resource intensification weakens state-building by impeding the state's fiscal capacity." Fiscal capacity is defined as the state's ability to tax.

accountability of the state to the people, as well as the political participation of the people. In a panel of 30 hydrocarbon-producing countries, Bornhorst et al. (2009) empirically examine whether there is evidence of an offset between government revenues from oil and gas-related activities and revenues from other domestic sources. They show that countries that receive large revenues from the exploitation of natural resource endowments reduce their domestic tax effort. They conclude that "there might be significant adjustment costs in moving to a higher level of domestic taxation once resources are depleted." We add to this literature by showing that the adjustment of tax efforts in response to declining resource rents is significantly constrained by the initial size of the SE.

To set the scene, Section 3.2 presents a conceptual framework and some suggestive evidence on the moderating role of the SE on the impact of declining oil rents on tax revenues. In Section 3.3, we discuss our empirical strategy and data. We then proceed to present and discuss the empirical evidence and perform robustness analysis in Section 3.4. We conclude the article in Section 3.5.

3.2 Conceptual framework and descriptive analysis

Our main argument is that the effect of declines in oil rents following negative oil price shocks on a country's tax revenues depends on the size of the SE. To obtain an initial snapshot of the relationship between negative oil price shocks and tax revenues, taking into account the initial size of the SE, Figures 3.1 and 3.2 plot changes in (log) tax revenues to GDP against negative oil price shocks in high- and low-SE countries. We define countries as high (low)-SE countries if the size of the SE is greater (lower) than the median (i.e., the sample median is 32%). Indeed, Figure 3.1 shows hardly any relationship between (log) changes in tax revenues to GDP and negative price shocks in high-SE countries. The slope of the coefficient is equal to 0.69 and is statistically insignificant. Figure 3.2 shows, in contrast, a positive and statistically significant relationship in low-SE countries. The slope of the coefficient is equal to 1.20 and is statistically significant at the 1% significance level.[7] In other words, negative oil price shocks cease to have an impact on tax revenues in high-SE countries, whereas tax revenues respond positively to negative oil price shocks in countries with low SEs.

Looking at countries' tax performance following drops in oil rents offers support for our argument. For illustration, we focused on the two years of 2014 and 2015, when global crude oil prices dropped from $96 to 50$ per barrel. What was the trend of development of tax revenues in oil-producing countries in which the size of the SE as a proportion of GDP was more than the median level of 32% during 2014 and 2015? We find three main cases: Angola shows a decline in oil rents (% of GDP) from 23% to 10% from 2014 to 2015 and, at the same time, a drop in tax revenues as a proportion of GDP from 15% to 12%. In Brazil, we observe a drop in oil rents from 1.8% to 1%. No change was observed

[7] Note, the clustering of some observations around zero is due to the inclusion of low-oil exporters (i.e., low oil export weight). In the robustness checks section (Table 3.4), we check the results after excluding low-oil exporters and results remain robust.

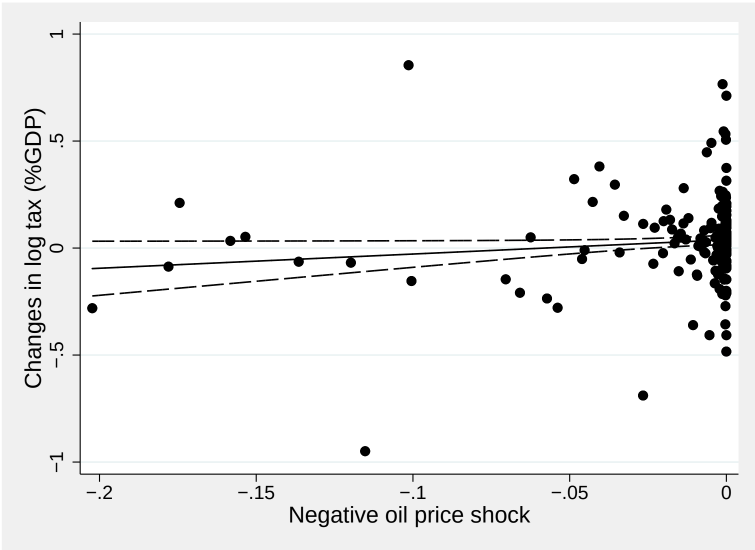


Figure 3.1: Changes in (log) tax revenue to GDP and negative oil price shocks in high-shadow economy countries. A country is considered a high-shadow economy country, if the size of the shadow economy is greater than the median. The dashed lines are the 90% confidence interval.

in tax revenues as a proportion of GDP (it remained constant at 12. 8%). In Egypt, we observe a reduction in oil rents from 6.7% to 2.9%. Likewise, no significant change in tax revenues can be observed (it remained almost constant at 12.5%). The SE in these countries amounts to around 35% of GDP.

Another episode involving a significant drop in oil prices is related to the global financial crisis of 2008–2009, the so-called "Great Recession." The crude oil price decreased from $97 in 2008 to 67$ in 2009. We look at countries in which the SE is more than 32% of GDP (i.e., the median in our sample). Is it possible to identify a meaningful increase in the tax efforts of oil-producing countries with a sizable SE during 2008–2009? In Angola, oil rent share of GDP dropped from 56% to 31% during this period, but the tax revenue share experienced a decline from 27% to 17%. Azerbaijan is another oil-rich economy that experienced a decline in the share of oil rents from 37% to 24% of GDP. However, this country experienced difficulty in increasing its tax efforts, mainly due to its sizable SE (which also expanded from about 44% to 45%). The share of tax revenues as a proportion of the GDP of Azerbaijan also declined from 16.4% to 14.1% in the mentioned period despite implementing tax reforms. Nigeria is another relevant example of an oil-rich economy in which the size of the SE equaled approximately 54% of GDP in 2008–2009. While the share of oil rents in Nigeria dropped from 17% to 9% of GDP, the tax efforts of the state were limited, and tax revenues declined, as a proportion of GDP, from 5.5% in 2008 to 5.1% in 2009. There are further similar examples for the period of 2008–2009, such as Russia, Cote d'Ivoire, and Thailand, among others.

In contrast, tax revenues in oil-producing countries with relatively lower levels of SE (less than the median of 32%) have responded significantly to negative developments in oil prices during different periods owing to their implemented tax reforms. For instance, considering the drop in oil price during 2014–2015, we can observe that Kuwait, which has experienced a drop in oil rents as a share of GDP from 54% to 37%, was able to raise

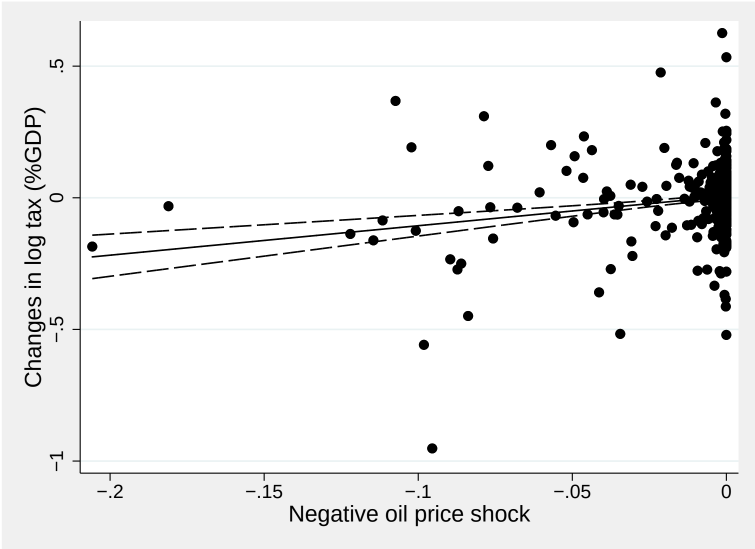


Figure 3.2: Changes in (log) tax revenue to GDP and negative oil price shocks in low-shadow economy countries. A country is considered a low-shadow economy country, if the size of the shadow economy is lower than the median. The dashed lines are the 90% confidence interval.

its tax revenue share of GDP from 0.8% to 1.38%. The size of the SE in Kuwait was approximately 22%. Another example is Mexico, which has an SE size of 29%. Mexico faced a decline in its oil rents as a share of GDP from 4% to 1.6%. However, we can observe an increase in its tax revenues from 10.6% in 2014 to 13% in 2015.

There are other similar examples when considering the negative movement of oil price during the financial crisis of 2008–2009. Oil producing countries with a smaller SE were more able to increase their tax revenues in the short term in response to a drop in oil rents. For example, Qatar, with an SE size of 16%, experienced a decline in oil rents from 31% of GDP in 2008 to 21% in 2009. Tax revenues, as a share of GDP, in this country increased from 16% to 20% for the same period. Iran, with its estimated SE size of approximately 16% of GDP, is another example. The share of oil rents decreased from 31% of GDP to 17% during 2008–2009. We can observe an increase in tax revenues in Iran from 6% to 7.3%. A similar situation can be observed in Oman, with an SE size of 17% in 2009. Its oil rents, as a share of GDP, declined from 38% to 30%, while its tax revenue share increased from 2.4% to 3.4%.

In short, our argument can be summarized in the following two testable hypotheses:

Hypothesis 1: An exogenous decline in international oil price increases tax revenues, ceteris paribus. *Hypothesis 2:* An exogenous decline in international oil price has a smaller impact on tax revenues, where the initial size of the SE is higher, ceteris paribus.

We provide a simple theoretical illustration of the association between tax revenues and negative changes in oil rents in the presence of an SE, as well as a formal representation of the hypotheses in Appendix A. Next, we present the empirical specifications for testing the hypotheses.

3.3 Data and empirical strategy

3.3.1 Data

We use a panel dataset covering 124 countries over the period 1991–2015. Our main specification uses three-year averages of our measures of tax revenues, oil price shocks, SE, and per capita income. This allows us to overcome instances of missing data for some countries, especially tax revenues, and have a more balanced dataset. Nevertheless, our results do not depend on the use of three-year averages.[8] Appendix B presents the list of countries included in the sample. Our measure for oil price shock for country i at time t takes the following form (Eq. 3.1):

$$OilPriceShock_{it} = \delta_i(lnOilPrice_t - lnOilPrice_{t-3}) \tag{3.1}$$

where δ_i represents the whole-period average of country's i share of oil exports to GDP multiplied by the 3-year change of (log) international real oil prices ($lnOilPrice_t$). The construction of the measure captures that oil price shocks will have a greater impact in countries with a higher oil dependency.[9] It also allows us to circumvent problems associated with using conventional measures of oil wealth such as export or production levels (typically normalized by GDP or population) which could be spuriously correlated with our outcome of interest. The oil exports data are from the United Nations' Comtrade dataset reported according to the SITC1 system (UN Comtrade, 2018). Data on international real oil prices is taken from British Petroleum database (BP, 2018). To differentiate negative oil price shocks from positive shocks, we construct a variable that takes the value of three-year ln-change of oil prices, if this value is strictly negative and zero otherwise (see Farzanegan and Markwardt (2009) for similar methodology). Negative oil price shocks are first calculated per year for each country and then collapsed to the three-year average. Formally,

$$NegPriceShock_{it} = min(0, OilPriceShock_{it}) \tag{3.2}$$

Tax revenues are measured by the ratio of tax revenues to GDP, taken from the World Bank's World Development Indicators (WDI, 2018). As we show in the next section, controlling for GDP per capita captures any variations in tax base, so that what remains in this measure is only the variation in tax rate, which is our variable of interest. GDP per capita is taken from the World Development Indicators (WDI, 2018). The share of SE to GDP is taken from Medina and Schneider (2018). The estimates for the size of the SE are based on the MIMIC model. This empirical approach first treats the SE as an unobserved (latent) variable, identifying multiple causes and multiple indicators for estimating its size. Second, it uses a structural equation model to estimate the relationships between

[8] Our results remain robust when using annual data or five-year averages.

[9] See Bazzi and Blattman (2014), Brückner and Ciconne (2010), and Brückner et al. (2012) for similar methodology. We also check whether there are significant differences between net oil importers and exports (see Table 3.5).

the unobserved variable and the observed indicators. A key advantage of this dataset is that it uses a light intensity approach instead of GDP as an indicator variable and, hence, it captures a wider range of economic activities that are not reported by official GDP figures (Farzanegan and Hayo, 2019). A second advantage of this dataset is the inclusion of a longer time span and wider coverage of countries. Table 3.1 provides the summary statistics for our main variables of interest.

Table 3.1: Summary Statistics

Variable	N	Mean	SD	Min	Max
Tax revenue (% of GDP) (log)	799	2.69	0.65	-1.34	4.06
Negative oil price Shock (3-year growth)	799	-0.01	0.02	-0.17	0.00
Shadow economy (% of GDP)	799	28.81	13.38	6.52	70.93
GDP per capita (log)	799	8.86	1.45	5.15	11.58

3.3.2 Empirical strategy

Our conceptual framework hypothesizes that the effect of negative oil price shocks on tax revenues depends on the initial size of the SE, *ceteris paribus*. Specifically, a negative exogenous decline in oil rents will increase tax revenues, but the effect is lower for a larger SE.

To test these two hypotheses, we estimate the following model:

$$\begin{aligned} lnTaxRev_{it} = \alpha_i + \theta_t + \beta_1 NegPriceShock_{it} + \beta_2 SE_{it-1} + \beta_3 NegPriceShock_{it} \\ \times SE_{it-1} + \beta_4 lnGDP_{it} + \epsilon_{it} \end{aligned} \tag{3.11}$$

where α_i is country fixed effects and θ_t is year fixed effects. $lnTaxRev_{it}$ is (log) tax revenue (% of GDP) in country i and year t; NegPriceShock is our proxy for a negative oil rents shock dp; $lnGDP_{it}$ is (log) GDP per capita and ϵ_{it} is a disturbance term. SE_{it-1} is the initial size of the shadow economy (% of GDP) lagged one period to address reverse feedback concerns, since it is less unlikely that tax revenues and price shocks at year t will affect the size of shadow economy at year $t-1$.[10] This suggests that a lagged level of the shadow economy can be treated as a predetermined variable, whose lagged values are uncorrelated with the current error term.[11] In this specification, β_1 captures the linear effect of negative oil price shocks on tax revenues in countries more dependent on oil, and β_3 measures the effect of negative oil price shocks on tax revenue conditional upon the initial size of the shadow economy. According to our theoretical prediction, the sign of the linear effect should be positive ($\beta_1 > 0$) and the sign of the interaction effect should be

[10] As we will show below, our main specification uses three-year averages of our variables of interest; hence, the SE measured at year $t-1$ is the average of years $t-3$, $t-4$, and $t-5$, which further rules out any reverse feedback concerns.

[11] Results remain robust when we use the second lag of the SE, despite the drop in sample size. See Ishak and Fritsche (2019) for their findings on the insignificant response of the lagged SE to oil price shocks. We include the SE in levels rather than in logs to address multicollinearity concerns (i.e., the variance inflation factor (VIF) is, on average, 17.37 when using the (log) SE, whereas it drops to only 6.75 when using the SE in levels. Conventionally, the VIF should not exceed 10, otherwise the model would suffer from multicollinearity).

negative ($\beta_3 < 0$). Hence, the higher initial size of the shadow economy, the lower effect of oil price shocks on tax revenues. Country- and year-fixed effects control for all time-invariant country characteristics and common global trends, and we cluster the standard errors at the country level.

The inclusion of (log) GDP per capita controls for the effect of oil price shocks on GDP (i.e. the denominator in ($\frac{TaxRevenue}{GDP}$)). This ensures that we capture the effect of negative oil prices on the size of tax revenues (i.e. nominator) and not on GDP. Second, as tax revenues can change with changes in either the tax rate or tax base (i.e., output or consumption), controlling for GDP also captures the changes in tax base. Hence, $_1$ and $_3$ measure the unconditional and conditional effects, respectively, of negative oil price shocks on the changes in government efforts to increase tax revenues by increasing tax rates (for a similar approach, see Bhattcharyya and Hodler, 2014; Bhattcharyya and Collier, 2014).

One potential concern regarding our measure for an SE is that its estimated size is based on the multiple indicators multiple causes (MIMIC) approach, which treats tax revenues as a percentage of GDP as one of the drivers of the SE. However, it should be noted that even though our measure for SE could be endogenous, the interaction term between negative oil price shocks (i.e., the exogenous variable) and the SE remains consistent (Bun and Harrison, 2019)..[12]

The usage of (non-) differenced specifications is motivated by the time series properties of international oil prices, tax revenues, SE, and GDP. In Table in Appendix C, we provide formal unit root tests for these variables using both annual data and three-year averages. The tests cannot reject the null hypothesis of the presence of a unit root in the time series of oil price in levels, but they reject it for their first differences. For tax revenue, SE, and GDP, formal tests reject the null hypothesis of the presence of unit roots in levels and first difference.

3.4 Empirical results

Table 3.2 contains our main empirical results. Column 1 looks at the average impact of negative oil price shocks on (log) tax revenues (% of GDP) without controlling for the initial size of the SE. This shows that negative oil price shocks have a positive but statistically insignificant impact on tax revenues. In Columns 2 and 3, we split our sample into high-and low-SE countries, if the lagged size of the SE (% of GDP) is greater or lower than the median, respectively. Column 2 shows a positive but statistically insignificant effect of negative oil price shocks on taxes in high-SE countries. In contrast, the effect is positive and statistically significant in low-SE countries, as reported in Column 3.

In Column 4, instead of sample split, we add the lagged level of the SE, both by itself and interacting with negative oil price shocks. The coefficient of negative oil price shocks is positive and statistically significant at the 5% significance level, while the coefficient of the

[12] We address concerns about the exogeneity of oil price shocks by dropping Organization of the Petroleum Exporting Countries (OPEC) and oil producers with 1% or 3% of world oil production (see section 3.4.2).

interaction term is negative and statistically significant at the 10% significance level. This suggests that negative oil price shocks lead to an increase in the tax revenue share of GDP, but the positive effect is reduced at the higher initial levels of the SE (as % of GDP) in line with our hypotheses. Our main results (based on Column 4) are illustrated in Figure 3.3,

Table 3.2: Negative oil price shocks, taxation and shadow economy

Model	(1)	(2)	(3)	(4)
	lnTax	**lnTax**	**lnTax**	**lnTax**
		High SE	**Low SE**	**Baseline**
Negative price shock, t	0.777	0.754	4.053**	6.389**
	(0.803)	(1.365)	(1.571)	(2.520)
Shadow economy, t-1				0.002
				(0.005)
Negative price shock × Shadow economy				-0.124*
				(0.068)
GDP per capita (log), t	0.309**	0.156	0.324	0.327**
	(0.130)	(0.143)	(0.200)	(0.139)
Number of observations	799	276	523	799
Number of countries	124	63	96	124
R-squared	0.074	0.123	0.119	0.095
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The dependent variable in columns 1-6 is (log) tax revenue (% of GDP); in column 7 is the change in (log) tax revenue (% of GDP). Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP. Columns 2 and 3 differentiate between high and low shadow economy countries, if shadow economy is greater (lower) than the median. The method of estimation in columns is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

which plots the estimated effect of negative oil price shocks on tax revenues conditional on the initial size of the SE, along with the 90% confidence bands. The plot shows that the increase in tax revenues following negative oil price shocks is lower at higher levels of initial size of SE. With no SE, a one-percentage-point weighted decline in international oil price implies an increase in tax revenues of 6.4%. In a low-SE country (SE around 7% of GDP), the effect of a one-percentage-point weighted decline in international oil price leads to an increase in tax revenues of 5.5%. In a mid-SE country (SE around 32% of GDP), the effect of a one-percentage-point weighted decline in international oil price implies an increase in tax revenues of 2.4%. Negative oil price shocks cease to have any significant impact on tax revenues in high-SE countries, where an SE represents more than 35% of GDP. In Appendix B, we present a list of countries with SE representing more than 35% of GDP.

To put things differently, let us consider Iran, Oman, Kazakhstan, and the Republic of Congo as examples of oil-dependent countries with SEs representing, on average, 18%, 19%, 39%, and 50% of GDP, respectively. A one-percentage-point decline in international oil prices increases tax revenues in Iran and Oman by 4% each, but has no significant impact on tax revenues in Kazakhstan and the Republic of Congo.

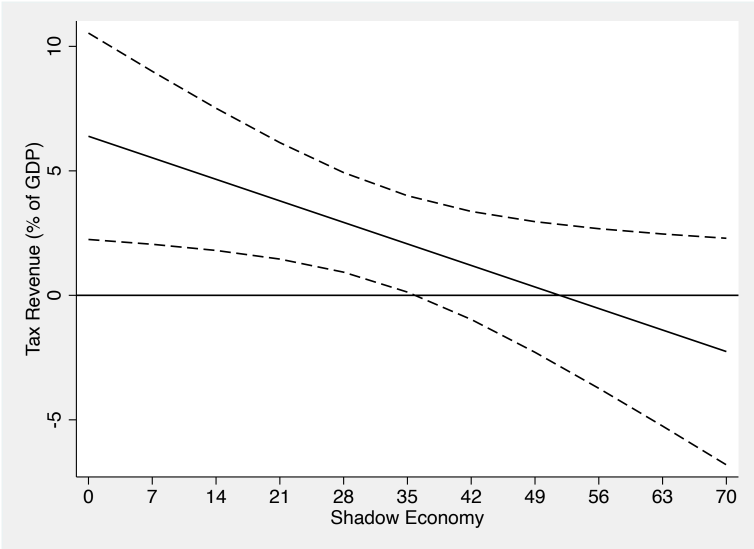


Figure 3.3: Marginal effects of negative oil prices shocks on tax revenue (% of GDP) at different levels of shadow economy (% of GDP). The dashed lines represent the 90% confidence intervals.

3.5 Robustness checks

Our baseline results from the previous section are based on a static model. To allow for dynamics, Columns 1 and 2 of Table 3.3 add the lagged dependent variable as an additional explanatory variable and estimate a dynamic panel model. Column 1 shows the result using ordinary least squares estimation, while Column 2 reports the results using system-generalized method of moments (GMM) estimation..[13] Note that the number of countries has dropped slightly to 119 countries due to the non-availability of data on tax revenues for some countries before 1991. The estimates are close between the two models. Based on Column 1, the coefficient of the dynamic factor is 0.371 (=1 – 0.629), which indicates that tax revenues adjust very slowly over time, so that the long-run effect of a negative oil price shock on tax revenues is approximately 37% higher than the short-run effect. The estimated coefficients from Column 1 imply that, on average, a one-percentage-point weighted decline in international oil price, unconditionally, leads to an increase in tax revenues of 13.9% in the long run..[14] However, conditional on the size of the SE, a one-percentage-point weighted decline in international oil price leads to an increase in tax revenues of 11.3% in low-SE countries in the long run (SE around 7% of GDP) and by two percentage points in mid-SE countries (SE around 32% of GDP) in the long run.

In Column 3, we estimate the model in first differences by employing the one-period change in (log) tax revenues as our dependent variable. The (log) GDP per capita also enters the first difference. Recall, our unit root tests have confirmed the stationarity of the

[13]System-GMM is implemented in a two-step procedure, where the tax revenues variable is instrumented by its second lag in level equation and its first lag in differenced equation, following convention. We do not collapse instruments because the number of instruments (i.e., 39 instruments) is lower than the number of groups (i.e., 119 countries). The overidentifying restriction is not a concern in our case, with the p-value of the Hansen test being 0.41, meaning that we fail to reject the null hypothesis of no overidentifying restriction. We also estimated the first-differences GMM model. The results did not change.

[14]We calculate the long-run effect by dividing the oil shock estimated coefficient by (1- the coefficient of the lag of the dependent variable), which is (5.16/1-0.629).

tax revenues series. Nevertheless, this approach has the advantage of controlling for all country-specific linear trends in tax revenues when combined with country-fixed effects. We continue to find a positive impact of negative oil price shocks on tax revenues, and the impact is higher at the lower levels of the initial size of a SE.

To obtain a stronger sense of the long-run effects and further rule out any stationarity concerns, we additionally estimate a dynamic fixed effect augmented distributed lag (ARDL) model. Column 4 reports the long-run ARDL estimated coefficients and the error correction term, both of which are very close in magnitude to the computed long-run estimates from Column 1 (see above text). In Columns 5 and 6, we address the concern of the presence of cross-sectional dependence among panels..[15] To this end, we estimated a regression with Driscoll-Kraay standard errors, which are robust to both cross-sectional and temporal dependence. Column 5 contains the results for the static model, while Column 6 reports the estimates for the dynamic model. In both cases, the coefficients of the variables of interest remain identical to the baseline estimates in sign, significance, and magnitude. The only difference is that the interaction term in Column 5 becomes significant at the 5% significance level.

Table 3.3: Negative oil price shocks, taxation, and shadow economy - robustness checks I

Model	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	lnTax	**lnTax**	**Δ lnTax**	**Δ lnTax**	**lnTax**	**lnTax**
	OLS	**SYS-GMM**	**OLS**	**FE-ARDL**	**OLS**	**OLS**
	Lagged Taxes	**Lagged Taxes**	**1st difference**	**Long-run estimates**	**Correct for CSD**	**Correct for CSD**
Negative price shock, t	5.165***	7.586**	2.689**	13.992***	6.389***	5.165**
	(1.017)	(3.577)	(1.071)	(4.246)	(1.399)	(1.673)
Shadow economy, t-1	-0.001	0.001	-0.004	-0.009	0.002	-0.001
	(0.002)	(0.001)	(0.003)	(0.008)	(0.003)	(0.002)
Negative price shock × Shadow economy	-0.138***	-0.139*	-0.074*	-0.360***	-0.124**	-0.138**
	(0.024)	(0.073)	(0.043)	(0.101)	(0.043)	(0.049)
GDP per capita (log), t	0.151**	0.025*		0.356***	0.327***	0.151**
	(0.069)	(0.013)		(0.128)	(0.067)	(0.051)
lnTax, t-1	0.629***	0.785***				0.629***
	(0.064)	(0.088)				(0.055)
Δ GDP per capita (log), t			0.325***			
			(0.118)			
Error correction term (ECM)				-0.420***		
				(0.032)		
Number of observations	726	726	726	726	799	726
Number of countries	119	119	119	119	124	119
R-squared	0.500		0.086		0.1	0.500
AR (1)		0.01				
AR (2)		0.10				
Hansen test, p-value		0.41				
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable in columns 1-2, 5 and 6 is (log) tax revenue (% of GDP); in columns 3 and 4 is the change in (log) tax revenue (% of GDP). Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP. Columns 1, 2 and 6 add the lagged tax revenue as an additional explanatory variable. The method of estimation in columns 1, 3, 5 and 6 is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level; in column 2 is System-GMM; in column 4 is FE-ARDL. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

In Table 3.4, we add additional control variables to our baseline model. In Column 1,

[15] Tests for cross-section dependence reject the null hypothesis that errors are weakly cross-sectional dependent.

we add the share of agriculture value added to GDP to control for the fact that economy-dominated agriculture sectors may be difficult to tax in the presence of a large number of subsistence farmers (Gupta, 2007). In Column 2, we add the share of imports and exports to GDP as a proxy for the degree of openness. Trade liberalization could either negatively affect government revenues by reducing tariff receipts or increase revenue mobilization through the elimination of exemptions and improvement in customs procedures (Keen and Simone, 2004). In Column 3, we control for the share of foreign aid receipts to gross national income (GNI). Aid could affect the domestic revenue mobilization efforts depending on the type of aid received and its domestic use (i.e., to finance investments or current consumption). Gupta et al. (2004) find that concessional loans increase domestically generated taxation, while grants exert the opposite impact. In Columns 4–7, we control for different measures of the quality of institutions and state effectiveness (Besley and Persson, 2014). Corruption could reduce tax revenues by facilitating tax evasion (Buehn and Farzanegan, 2012). Low contestability of power, measured by the Polity2 score.[16] and executive constraints from the Polity IV database (Marshall et al., 2018), reduces the incentives of the ruling elite to impose progressive tax rates and deliver efficient public services. Political instability, measured by a durable variable from Polity IV, lowers the ability of the government to impose efficient tax systems and monitor compliance. All factors result in a low tax base and lower tax compliance rates. Finally, in Column 8, we control for social-cultural norms affecting tax morals. Ethnically fractionalized states have a weaker sense of national identity, which in turn weakens their moral obligations toward tax payments (Besley and Persson, 2014). We use a one-year lag of all additional control variables to avoid reverse feedback effects. Throughout all the columns, our main results remain robust in sign and statistical significance..[17]

Table 3.5 presents a number of robustness checks using alternative samples. To the extent that international oil prices are exogenous to specific countries demand or supply shocks, we should get an unbiased estimates for β_1 and β_3 in our baseline specification. Nevertheless, we cannot rule out the possibility of price manipulations triggered by major oil producers..[18] To address that, in columns 1-3 we exclude major oil producers -whose average production exceeds 1% or 3% of world production- and OPEC countries.[19] In columns 4 and 5, we check whether our results driven by high-SE countries or by low oil exporters.

[16] Following Brückner and Ciccone (2010), we adjust Polity2 so that periods of interregnum, coded as 0, and transitionary periods are treated as missing.

[17] We also checked with alternative controls for political institutions and trade openness in Table 3.C3 in Appendix C. Specifically, we used the democracy index developed by Gründler and Krieger (2016) based on machine learning techniques. For trade openness, we employed the KOF globalization index (de facto) and the KOF economic globalization index (de facto). For a survey on tax revenues and globalization as measured by the KOF globalization index, see Potrafke (2015). In all cases, the results remain robust, except that the interaction term becomes insignificant when the democracy index is used. However, the estimated marginal effects, which take into account the estimations of both the main effect and the interaction effect, remain the same as in the baseline results.

[18] For a review of the literature on the market power of the different members of OPEC, see Farzanegan and Raeisian Parvari (2014).

[19] Data on world oil production is calculated using Ross and Mahdavy (2015) oil and gas database. We also excluded the top 10 oil producers based on CIA world Factbook and results remain unchanged.

Table 3.4: Adding additional control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	lnTax	**lnTax**	**lnTax**	**lnTax**	**lnTax**	**lnTax**	**lnTax**	**lnTax**
Negative price shock, t	7.096***	6.325**	9.759***	7.442***	6.912***	7.111***	7.023***	6.447**
	(2.446)	(2.665)	(2.099)	(2.389)	(2.552)	(2.601)	(2.594)	(2.510)
SE, t-1	0.003	0.000	-0.001	-0.001	0.002	0.002	0.002	0.002
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Negative price shock × SE	-0.139**	-0.125*	-0.211***	-0.174***	-0.151*	-0.157*	-0.152*	-0.126*
	(0.069)	(0.072)	(0.048)	(0.057)	(0.077)	(0.080)	(0.078)	(0.067)
GDP pc (log), t	0.469***	0.305**	0.361**	0.295**	0.339**	0.326**	0.326**	0.327**
	(0.156)	(0.152)	(0.157)	(0.147)	(0.144)	(0.143)	(0.143)	(0.138)
Additional Controls	**Agricuilture, value added (%GDP) (log)**	**Trade (%GDP)**	**Aid (%GNI) (log)**	**Corruption (log)**	**Polity2**	**Executive constraints**	**Political instability**	**Ethnicity**
	0.125	0.0001	0.050**	-0.029	0.005	0.002***	0.002	0.004
	(0.085)	(0.001)	(0.021)	(0.060)	(0.006)	(0.000)	(0.002)	(0.020)
Number of observations	753	759	467	720	754	766	766	799
Number of countries	123	122	89	111	119	120	120	124
R-squared	0.153	0.086	0.193	0.099	0.103	0.108	0.104	0.095
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable in columns is (log) tax revenue (% of GDP). Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

In column 4, we exclude the top 1% countries in the size of the shadow economy as a percentage of GDP, averaged over the whole sample period.[20] In column 5, we drop low oil exporters, whose average share of oil exports to GDP is lower than the median. In all instances, our coefficients of interest keep their sign and statistical significance. In columns 6 and 7, we weigh our measure for negative oil price changes (i.e. δ_i) once with country's whole-period average of oil production (% of GDP) and another with the country's whole-period average of oil rents (% of GDP). In both cases, the coefficient of negative oil price shocks remain positive and statistically significant. The conditioning term loses its statistical significant but keeps its positive sign. Nevertheless, the estimated marginal effects of negative oil prices at different levels of shadow economy remain the same as the baseline specification (see Figures C2 and C3 in Appendix C).

Table 3.5: Alternative samples and weights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	lnTax	lnTax	lnTax	lnTax	lnTax	lnTax	lnTax
Negative price shock, t	8.603***	8.831***	7.893***	6.272**	6.417**	3.799*	4.844*
	(2.492)	(2.325)	(2.279)	(2.566)	(2.514)	(1.939)	(2.798)
SE, t-1	0.004	0.002	0.002	-0.001	0.001	0.004	0.005
	(0.005)	(0.005)	(0.005)	(0.004)	(0.008)	(0.007)	(0.007)
Negative price shock × SE	-0.151**	-0.133*	-0.156**	-0.121*	-0.124*	-0.064	-0.076
	(0.072)	(0.075)	(0.063)	(0.068)	(0.067)	(0.054)	(0.090)
GDP pc (log), t	0.351**	0.285**	0.332**	0.257**	0.460**	0.401**	0.385**
	(0.140)	(0.137)	(0.144)	(0.126)	(0.183)	(0.169)	(0.164)
Omitted obers-vations/weights for shocks	**OPEC**	**Top 1% producers**	**Top 3% producers**	**Top 1% SE**	**Low oil exporters**	**weighted by oil production (% GDP)**	**weighted by oil rents (% GDP)**
Number of observations	763	708	753	786	387	529	559
Number of countries	118	110	117	122	59	79	84
R-squared	0.108	0.097	0.104	0.078	0.162	0.122	0.121
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is (log) tax revenue (% of GDP). Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP in columns 1-5; by whole-period average oil production share to GDP in column 6; and by whole-period average oil rents share to GDP in column 7. Column 4 excludes the top 1% shadow economy countries averaged over the whole period; Column 5 excludes low oil exporters with whole-period average oil exports share to GDP lower than the median. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

In Table 3.6, we check whether our estimated effects differ with country characteristics. As low-oil exporters are net oil importers, a decline in international oil prices represents a reduction in their costs of production and, in turn, an increase in output and government revenues. This could reverse the relationship, so that net oil importers may lower tax rates in periods of low international oil prices. We have shown that the exclusion of low-oil exporters does not change the results, with our variables of interest remaining stable in magnitude, sign, and significance. To further address this, Column 1 in Table 3.6 includes an indicator for net oil importers interacting with our main variable of interest. Net oil importers are defined as those whose whole-period average oil exports are strictly negative.

[20] Exclusion of observations corresponding to the highest 1% and 5% of shadow economy values do not change our results.

All interaction terms are statistically insignificant (not shown for brevity) and the reported Chow test fails to reject the null hypothesis of equality of estimated effect in both net oil importers and net oil exporters. In contrast, our baseline estimates remain robust. An alternative explanation for such an insignificant difference could be the fact that some net oil importers are financially dependent on net oil exporters in terms of remittances and aid, so that negative oil price shocks could also negatively affect government revenues, which balance out the positive effects of the decline in production costs.

Table 3.6: Country characteristics and alternative data frequency

Model	(1)	(2)	(3)	(4)
	lnTax	**lnTax**	**lnTax**	**lnTax**
Negative price shock, t	6.243**	7.211***	1.612**	12.536*
	(2.556)	(2.407)	(0.708)	(7.212)
Shadow economy, t-1	-0.001	0.003	0.002	0.001
	(0.007)	(0.007)	(0.004)	(0.005)
Negative price shock x Shadow economy	-0.121*	-0.128*	-0.041**	-0.272*
	(0.071)	(0.076)	(0.020)	(0.160)
GDP per capita (log), t	0.313**	0.339**	0.330**	0.391**
	(0.138)	(0.144)	(0.146)	(0.151)
	Dummy for net importers + interaction	**Dummy for democracies + interaction**	**Annual observations**	**5-year average**
Chow test (P value)	0.11	0.14		
Number of observations	799	799	2,114	530
Number of countries	124	124	124	124
R-squared	0.115	0.106	0.077	0.172
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The dependent variable is (log) tax revenue (% of GDP. Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP. Columns 1 & 2 uses the 3-year average observations; column 3 uses annual observations; and column 4 uses 5-year average observations. In columns 1 & 2, the net importers and democracy dummies are included on their own and intereacted with our variables of interest, respectively. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

As previously mentioned, low contestability of power reduces the incentives of the ruling elites to impose progressive tax rates and deliver efficient public services, which is the case in autocracies. In Column 2, we differentiate between democracies and autocracies by including an interaction dummy variable that takes the value of 1 if the lagged Polity2 score is strictly positive (negative) for democracies (autocracies). The dummy is included on its own and interacts with our variable of interest. The results reported in Column 2 show that our estimated coefficients preserved their signs and statistical significance. The included dummies and their interactions were statistically insignificant (not shown) and the reported Chow test failed to reject the null hypothesis of equality of estimated effect in both democracies and autocracies. We proceed in Columns 3 and 4 by showing that our results are not sensitive to the choice of data frequency. In Column 3, we use annual observations, and in Column 4, we employ five-year average observations. Our results remain robust.

Other robustness checks are reported in Table 3.C2 in Appendix C, including the dropping of fixed effects and using alternative transformations for GDP per capita. First, as the SE size exhibits low variation over time, it could be the case that adding fixed effects drives down the estimated coefficients for the SE, especially in the presence of measurement error. This is not a major concern for the identification strategy because our main variable of interest is the interaction term between the initial level of SE and negative price shocks. Nevertheless, in Columns 1 and 2 of Table 3.C2, we drop country-fixed effects and both country- and year-fixed effects, respectively. The estimated coefficients remain qualitatively similar but greater in magnitude. The latter is due to the exclusion of fixed effects, which control for time-invariant country characteristics and common time-varying shocks. Hence, their exclusion results in an upward bias in the estimates. Second, despite the importance of controlling for fluctuations in GDP as laid down in the empirical specification section, the downside is that it could create a linear relationship between the left side and the right side of the equation, since the GDP is also included in the denominator of the dependent variable. To tackle this, we follow two approaches. In Column 3 of Table 3.C2, we replace log GDP per capita with its value in levels, and in Column 4, we employ the one-year lagged log GDP per capita. The results remain robust in both cases.

3.6 Conclusion

We studied how the impact of falling oil rents on tax revenues may be contingent on the size of the SE. Employing a conceptual framework and presenting ample examples, in addition to a simple theoretical illustration, we demonstrate that declining oil rents are less likely to increase the tax receipts of governments under a sizable SE.

To test our main hypothesis on the moderating role of the SE in the final effect of negative oil shocks on tax receipts, we use panel data covering the period 1991–2015 and more than 120 countries. Our main hypothesis is supported by the data. In particular, the positive effects of falling oil rents on tax revenues decrease with higher levels of SE. Our main results hold when we control for the effects of income, agriculture value added to GDP, trade, aid, ethnicity, time-varying common shocks, country-fixed effects, and quality of institutions (corruption, democracy, and political stability). Moreover, our main results, based on country- and year-fixed effect regressions, are robust after addressing endogeneity and using different estimation methods, such as dynamic fixed effect ARDL and GMM models.

Our results contribute to the debate on ambiguity in the role of the SE in oil-dependent countries. On the one hand, the existence of an SE can correct market inefficiencies and allow workers to cope with economic volatility (Ishak and Fritsche, 2020), but on the other hand, our findings suggest that it may impede government taxation efforts during economic downturns. In this regard, policymakers are well-advised to analyze the SE and its drivers, as well as weigh the benefits and costs for its existence. Allowing for a limited role of the SE can be conditionally beneficial if its size is kept under control. At the same time, the government is recommended to embark on labor market reforms, in terms of increasing labor productivity, reducing obstacles for firm entry, offering a flexible regulatory environment, and employee social protection. This will allow for a reduction in the role of the SE without losing its benefits.

Future research may also investigate how a decline in oil revenues following the negative economic shocks from the coronavirus pandemic in 2020 or economic sanctions may affect the tax efforts of oil-based economies, considering the size of the SE.

3.A Appendix A - Theoretical framework

We develop a simple model showing the moderating effect of the SE in the oil rent-taxation nexus. Specifically, the SE is incorporated as a determinant of the final impact of oil rents on taxation efforts. The intuition is that negative shocks to oil rents increase the government's incentive to invest in tax efforts to compensate for the decline in oil receipts. However, such an ability to raise tax revenues is constrained in the presence of large informal economies, which, in turn, implies low tax bases. It follows that a decline in oil rents will have a lower impact on tax efforts to raise tax rates and tax revenues, the greater the size of the SE.

To model these effects, let us consider that the government has the choice between using oil rents or taxation to finance public goods. The difference between both sources lies in the fact that taxation is distortive and creates welfare losses (Bornhorst et al., 2009). Hence, the government will depend on oil rents as a first option. If oil rents suddenly declined, the government would resort to taxing firms. We assume that a rational citizen will understand that using oil rents for financing public goods is also considered a tax. If the money were not used for this purpose, he/she could receive a transfer instead.[21] As such, the government's total received tax revenues encompass both oil rents and imposed corporate taxes.

Suppose that the individual's utility function takes the following form $U(Y, T)$, where Y denotes private net income (i.e. consumption) and G is a government transfer or the size of the received public good. The government receives an exogenous amount of oil rents of pR, where p is the international oil price and R is amount of oil extraction. The government can tax firms, with each firm n is charged a tax rate of τ. However, the government can only tax firms operating in the official economy. The share of the firms in the shadow economy is SE, with $0 < SE < 1$. The total tax revenues collected (T) is thus

$$T = [n - nSE]\tau \tag{a.1}$$

We assume that the number of firms operating in the official and SE (i.e., tax base) $[n - nSE]$ remain constant (i.e., fixed at their given initial level) to avoid leakages into the informal market or dropping out of business in response to tax rate increases and to allow for a proportional relationship between tax revenues, T, and tax rate, τ. In other

[21] We follow Jensen's (2011) line of reasoning in defining rational citizen's preferences for direct transfers or provision of public goods.

words, we assume that an increase in (say by 1%) will lead to an increase in T by the same amount.

It then follows that the size of the public good (G) is

$$G = pR + [1 - SE]\tau n \tag{a.2}$$

and net income (Y) is

$$Y = [(1 - SE)(1 - \tau) + SE]n = n[1 - \tau(1 - SE)] \tag{a.3}$$

For simplicity, we assume a Cobb-Douglas utility function $U(Y, T) = Y^a T^{1-a}$. The individual's utility function then becomes

$$U = [n[1 - \tau(1 - SE)]]^a [pR + [1 - SE]\tau n]^{1-a} \tag{a.4}$$

With τ and $SE \in [0, 1]$. The first order condition of the maximization problem, $dU/d\tau$, assuming an internal solution implies that

$$\frac{dU}{dY} - \frac{dU}{dT} = 0 \tag{a.5}$$

This means that the marginal utility from private consumption and marginal utility of public good should be equal.[22] Under this condition, it yields

$$a[pR + [1 - SE]\tau n] - [1 - a][n[1 - \tau(1 - SE)]] = 0 \tag{a.6}$$

Conducting comparative statistics to the above condition (a.5) using the implicit-function theorem, we get

$$\frac{d\tau}{dp} = -aR[\frac{1}{n(1 - SE)}] \tag{a.7}$$

with $SE < 1$, which makes

$$\frac{d\tau}{dp} < 0 \tag{a.8}$$

To see how the change in SE affects $\frac{d\tau}{dp}$, we get

$$\frac{d}{dSE}(\frac{d\tau}{dp}) = \frac{-aRn}{n^2(1 - SE)^2} < 0 \tag{a.9}$$

Thus, the final impact of a change in p depends on SE, that is, the initial level of the SE. Based on equations a.8 and a.9, we can therefore formulate the following two hypotheses:

Observation 1: An exogenous decline in international oil price p increases tax rate τ and, consequently, tax revenues T, ceteris paribus.

[22] We also checked for internal solutions assuming U(Y,0) or U(0,T), but both solutions were rejected for having contradictory signs.

Observation 2: An exogenous decline in international oil price p has a smaller impact on tax rate τ and tax revenues T, the higher the size of the SE, ceteris paribus.

Observation 1 is based on equation a.8, while observation 2 is based on equation a.9.

3.B Appendix B - List of countries and definitions

3.B1 List of countries with SE representing more than 35% of GDP

Albania, Algeria, Angola, Armenia, Azerbaijan, Bahamas, Bangladesh, Belarus, Belize, Benin, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Bulgaria, Burundi, Colombia, Congo Rep., Cote d'Ivoire, Croatia, Cyprus, Dominican Republic, Egypt, El Salvador, Ethiopia, Georgia, Ghana, Guatemala, Honduras, Jamaica, Kazakhstan, Madagascar, Malawi, Malaysia, Mali, Mexico, Moldova, Morocco, Myanmar, Nicaragua, Nigeria, Pakistan, Papua New Guinea, Paraguay, Peru, Philippines, Romania, Russia, Rwanda, Senegal, Sri Lanka, Suriname, Tanzania, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Ukraine, Uruguay, Zambia, Zimbabwe.

3.B2 Control variables: Definitions and sources

- Agriculture value added (% of GDP): Agriculture corresponds to ISIC divisions 1-5 and includes forestry, hunting, and fishing, as well as cultivation of crops and livestock production. Value added is the net output of a sector after adding up all outputs and subtracting intermediate inputs. It is calculated without making deductions for depreciation of fabricated assets or depletion and degradation of natural resources. Source: WDI (2018).

- Trade (% of GDP): The sum of exports and imports of goods and services measured as a share of gross domestic product. Source: WDI (2018).

- Aid (% of GNI): The share of official development assistance (ODA) to GNI. It consists of disbursements of loans made on concessional terms (net of repayments of principal) and grants by official agencies of the members of the Development Assistance Committee (DAC), by multilateral institutions, and by non-DAC countries in the DAC list of ODA recipients. Source: OECD (2018).

- Corruption: An index for corruption perceptions ranging from 1 to 6, with higher values indicating less corruption. Source: ICRG (2018).

- Polity2: An index measuring the quality of political institutions. It ranges from 1 to 10, with higher values indicating better institutional quality. Source: Marshall et al. (2018).

- Executive constraints: An index measuring imposed constraints on the powers of the executive. It ranges from 1 to 7, with higher values indicating more executive constraints. Source: Marshall et al. (2018).

- Political instability: measured by "Durable" variable counting the number of years since the most recent regime change that alters essential authority characteristics, as defined by a three-point change in Polity2 in a three-year period or less. Source: Marshall et al. (2018).
- Ethnicity: A measure for ethnic fractionalization taken from Fearon (2003).

3.C Appendix C - Additional tables and figures

Table 3.C1: Unit root tests

Variable	Log Oil Prices (Time-Series Tests)		Log Tax revenues (Panel Data Tests)		Shadow economy (Panel Data Tests)		Log GDP per capita (Panel Data Tests)	
Annual data	**Level**	**Diff.**	**Level**	**Diff.**	**Level**	**Diff.**	**Level**	**Diff.**
Dickey-Fuller	n.s.	**	***	***	***	***	***	***
Dickey-Fuller-GLS	n.s.	**	-	-	-	-	-	-
Philipps-Perron	n.s.	***	***	***	***	***	***	***
3-year Average			**Level**	**Diff.**	**Level**	**Diff.**	**Level**	**Diff.**
Dickey-Fuller			***	***	***	***	***	***
Philipps-Perron			***	***	***	***	***	***

Note: All unit root tests contain trend. For panel data, we apply the fisher type tests. Abbreviation: n.s., not significant at the 10% level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

Table 3.C2: Further robustness checks I

Model	(1)	(2)	(3)	(4)
	lnTax	lnTax	lnTax	lnTax
	No country FE	No country & year FE	GDP per capita in Levels	lagged GDP per capita
Negative price shock, t	29.260***	28.878***	6.806**	7.326***
	(10.317)	(10.221)	(2.610)	(2.362)
Shadow economy, t-1	-0.002	-0.003	-0.008*	0.003
	(0.004)	(0.004)	(0.004)	(0.005)
Negative price shock × Shadow economy	-0.506*	-0.500*	-0.134*	-0.148**
	(0.278)	(0.276)	(0.068)	(0.059)
GDP per capita (log), t	0.057	0.056		
	(0.044)	(0.043)		
GDP per capita (level), t			0.0001	
			(0.000)	
GDP per capita (log), t-1				0.351**
				(0.150)
Number of observations	799	799	799	793
Number of countries	124	124	124	124
R-squared	0.173	0.170	0.051	0.093
Country FE	No	No	Yes	Yes
Year FE	Yes	No	Yes	Yes

The dependent variable in columns is (log) tax revenue (% of GDP). Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

Table 3.C3: Further robustness checks II

Model	**(1)**	**(2)**	**(3)**
	lnTax	**lnTax**	**lnTax**
	Alternative political institutions index	**Alternative measure for trade openness**	**Alternative measure for trade openness**
	OLS	**OLS**	**OLS**
Negative price shock, t	6.502**	6.120**	6.161**
	(2.721)	(2.479)	(2.535)
Shadow economy, t-1	0.001	0.002	0.002
	(0.005)	(0.005)	(0.005)
Negative price shock × Shadow economy	-0.131	-0.117*	-0.115*
	(0.092)	(0.067)	(0.068)
GDP per capita (log), t	0.313**	0.284**	0.311**
	(0.143)	(0.133)	(0.136)
Gründler and Krieger (2016), t-1	-0.058		
	(0.083)		
KOF globalization index (de facto), t-1		0.005*	
		(0.003)	
KOF economic globalization index (de facto), t-1			0.002
			(0.002)
Number of observations	774	799	799
Number of countries	119	124	124
R-squared	0.092	0.104	0.102
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

The dependent variable in columns is (log) tax revenue (% of GDP). Negative oil price shock is the 3-year growth of log oil price multiplied by whole-period average oil exports share to GDP. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the country level. Country fixed effects and year fixed effects are not reported. Significantly different from zero at *90% confidence, **95% confidence, ***99% confidence.

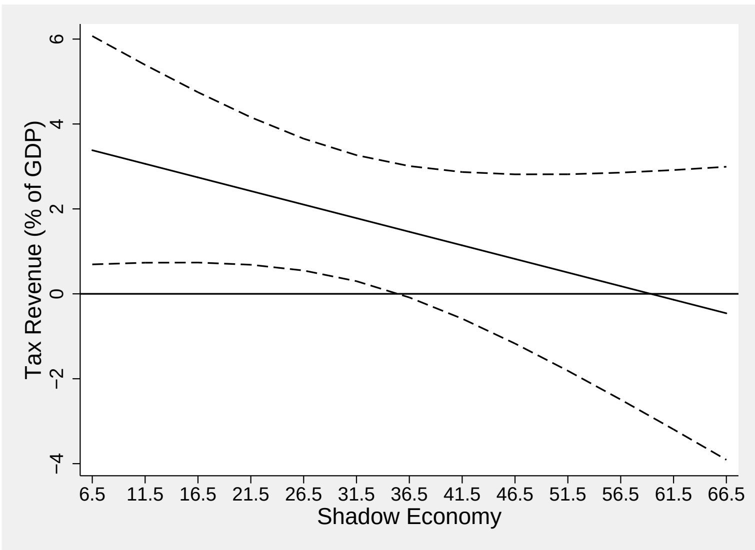


Figure 3.C1: Marginal estimates for column 6 in Table 3.5

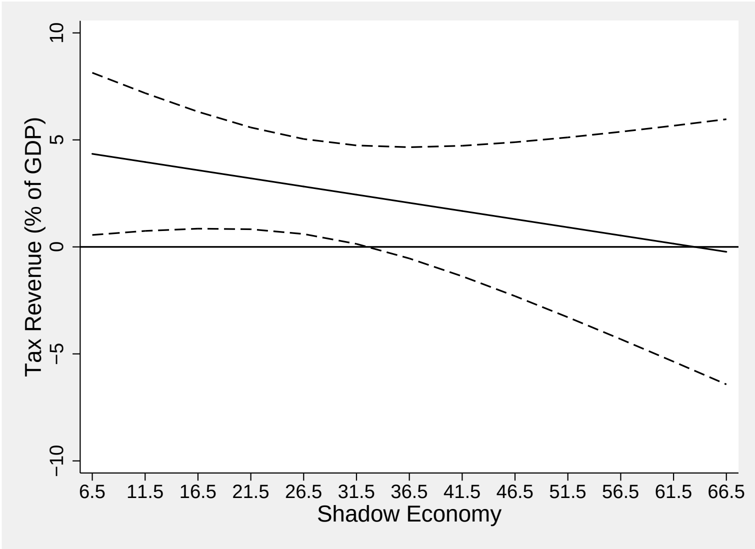


Figure 3.C2: Marginal estimates for column 7 in Table 3.5

A Resource-Rich Neighbor is a Misfortune: The Spatial Distribution of the Resource Curse in Brazil*

Abstract

We study the spatial distribution of the effect of oil and gas revenues on Brazilian municipalities, using variations in the international prices of oil and gas to establish causality. Oil and gas revenues increase economic activity, measured by night-time light emissions, in oil-producing municipalities but impose negative spill-overs on neighbouring municipalities. Spill-overs dominate beyond 150 km from oil activities and compensate direct effects in micro-regions. In oil municipalities, oil and gas revenues increase royalties, population, local real prices, crime, and real wages, essentially in manufacturing and services. Spillovers are negative on wages and prices and positive on royalties and crime.

Keywords: Natural resources curse, oil, spill-over effects, Night-time lights, Brazil

JEL codes: O11, O13, Q32

Introduction

The existence of the natural resource curse as a firmly established stylized fact has tarnished. At the country level, the evidence is mixed. While early work by Sachs and Warner (1995, 2001) or Auty (2001) reported that growth was lower in resource-rich countries, Alexeev and Conrad (2009) provided evidence that the curse depended on the period of observation and on the definition of economic performance. Specifically, when performance is measured by the level of output per capita instead of its growth, which allows to take into account the total effect of natural resources, the curse in fact appears to be a blessing. Cotet and Tsui (2013) confirmed the finding. Moreover, studies using within-country data consistently report evidence that natural resource abundance or booms in the price of natural resources improve the economic performance of the areas that produce the natural resources. The finding applies to both developed and developing countries and various periods. In the US, for instance, Black et al. (2005) and Allcot and Keniston (2018) observed that the 1970s oil-price boom increased production in oil-, gas-, and coal-producing counties, while Michaels (2011) reported that counties better endowed with oil experienced a faster growth of employment in both mining and manufacturing. By the same token, Aragón and Rud (2013) observed that the discovery and exploitation of a goldmine in Peru increased local incomes. Gradstein and Klemp (2016) and Mamo

*This chapter is co-authored with Pierre-Guillaume Méon and is accepted for publication in *Economic Development and Cultural Change* journal. Parts of the chapter were written while I was visiting the Economics Department at Universitat Autònoma de Barcelona.

et al. (2018) report that natural resources increased economic performance proxied by night-time light emissions, respectively for oil in Brazil and mining in Africa. Natural resources therefore appear to be a blessing for the subnational areas that produce them. Does it mean that natural resources are a curse for no one?

In this paper, we argue to the contrary. The spatial dimension of the effect of natural resources is key, because the benefits and costs of resource windfalls are unevenly distributed across space. Specifically, municipalities that produce oil likely benefit from the activity and revenues that natural resources generate and likely grow. However, to do so, they draw on resources from other municipalities, thereby imposing negative externalities. They may also impose costs in the guise of crime, corruption, or conflict. The balance between the benefits and the costs of natural resources is therefore likely spatially uneven, with positive effects dominating in the vicinity of resource-producing municipalities then fading away as distance grows. As a result, some non-resource-producing municipalities may suffer from resource windfalls. Moreover, the net aggregate effect may be either insignificant or negative despite the direct effect being positive. Consequently, the effect of natural resources measured at the local level may be positive while it may turn out insignificant or even negative when observed at higher levels of aggregation, such as regions or countries. That may explain why subnational studies report unmitigated positive effects while the findings of cross-country studies are contradictory.

We test that hypothesis using data on oil and gas revenues in Brazilian municipalities. Brazil is an appealing case for several reasons. First, it is the tenth larger oil producer in the world without being an OPEC member. This implies that while oil matters to Brazil, its influence on world prices is limited. Furthermore, there is an official distinction between oil-producing and non-oil producing municipalities.[1] Brazil has designed a system whereby oil and gas revenues automatically accrue to oil-producing municipalities and states in the form of royalties. Municipalities are therefore the natural unit of observation of the effect of oil on performance.[2] Finally, the features of oil production in Brazil allows us to identify the causal impact of oil and gas revenues on Brazilian municipalities. We do so by exploiting the fact that fluctuations in oil and gas prices are exogenous to whatever happens in each municipality. We can therefore compute an exogenous component of oil and gas revenues by multiplying the initial level of production of each municipality by the prices of oil and gas. As those prices are determined on global markets, the result of that product can safely be considered exogenous to Brazilian municipalities.

To study the geographic dimension and the spill-over effects of oil, we follow Mamo et al. (2019) and chiefly estimate a spatial Durbin model (SDM).[3] That model relates the performance of a municipality to its own oil revenues and to those of adjacent municipalities. It therefore allows to determine the sign and the magnitude of the effects of oil revenues on both oil-producing and non-oil producing municipalities. As a complement,

[1] In Brazil, gas production is a by-product of oil production. For conciseness' sake, we therefore refer to oil-production or oil-producing municipalities even though municipalities may produce both oil and gas.

[2] See Caselli and Michaels (2013) for an overview of oil production and oil-producing municipalities in Brazil.

[3] See also Harari and La Ferrara (2018) for a similar procedure.

we also model spillovers using distance measures to the nearest oil municipality to more precisely study how spillovers vary as one moves away from oil-producing municipalities. We also look at wider units of observation, specifically micro- and macro-administrative units. By construction, those units include both oil-producing and non-oil-producing municipalities. By studying the effect of oil on those aggregated units, one de facto observes the sum of the direct effect of oil and its spillovers. Our workhorse measure of economic performance is night-time light emissions, which has been used as a proxy for GDP at the sub-national level, for instance by Henderson et al. (2012), Hodler and Raschky (2014), Mamo et al. (2018), and, specifically on Brazil, by Gradstein and Klemp (2017). We, however, complement that baseline measure by a series of variables that are measured at the municipality level, to investigate the channels of transmission of oil to economic performance.

Our paper chiefly contributes to the literature that investigates the effect of natural resources on economic performance at the subnational level (e.g., Black et al., 2005, Allcot and Keniston, 2018, Michaels, 2011, Gradstein and Klemp, 2016). It also contributes to the literature that investigates the spatial spillovers of natural resources, be it on economic performance (Aragón and Rud, 2013, Mamo et al., 2019), or gender relations (Kotsadam and Tolonen, 2016). We in particular complement the papers of Mamo et al. (2019) and Gradstein and Klemp (2016). While Mamo et al. (2019) focus on mining in Sub-Saharan Africa, we consider Brazil, which allows us to consider observations that are institutionally homogeneous and to take advantage of the specificities of the distribution of oil revenues among Brazilian municipalities. We share our focus on oil and light emissions in Brazil with Gradstein and Klemp (2016) but depart from their analysis insofar as we work at the level of individual municipalities instead of reconstructed cells. Second, we consider the revenues of both oil and gas, as opposed to oil only. Although gas was essentially a byproduct of oil production and the Brazilian oil production barely covered Brazilian consumption over most of our period of study, it still contributed to the revenue windfall from hydrocarbon production. Third, and more importantly, our main focus is on the spatial spillovers between municipalities of oil and gas revenues. The fact that oil and gas revenues essentially accrue to oil-producing municipalities makes municipalities the natural level of analysis. Moreover, we use several techniques designed to capture those spillovers and study how they change across space. Third, our explanatory variable is the product of current oil and gas prices and initial oil and gas productions, which is in line with the rule used to share oil and gas revenues among Brazilian municipalities, while Gradstein and Klemp (2016) consider the product of oil price and distance to the nearest oil field. Our explanatory variable can therefore better approximate oil and gas revenues in a municipality, which are a function of both oil and gas prices and quantities. Finally, working at the level of municipalities allows us to consider a series of dependent variables in addition to light emissions and thereby study the channels of transmission of spillovers. We find that oil and gas revenues have a positive direct effect on economic activity in oil municipalities, with a 10 percent increase in revenues boosting economic activity by about 1.4 percent more in oil producing municipalities, on average. In contrast, non-oil municipalities are shown to suffer negative spillovers from oil and gas revenues, with the spillover effect being nearly of the same magnitude as the direct impact. Further, by employing distance-weighted measures of oil and gas revenues, we find negative spillovers to become stronger relative to direct effects, the further the municipality from oil production locations. According to our estimates, the negative significant spillovers starts to dominate in municipalities located at around 150 km from oil activities. We confirm that oil and gas revenues affect royalties in both oil and neighboring municipalities. They also increase population, real wages, in particular in the manufacturing sector and in services, as well as local real prices and crime. Moreover, we observe negative spillovers of oil and gas revenues to wages and prices and positive spillovers on crime in neighboring municipalities.

The remainder of the paper is organized as follows. Section 4.2 presents our theoretical framework. Section 4.3 describes the data and presents our empirical strategy. Section 4.4 presents our main empirical results and robustness checks, followed by section 4.5, which further investigates the nature of spillover effects. Finally, section 4.6 concludes.

4.2 The spatial distribution of the effect of oil and gas revenues: A framework

4.2.1 Effect on oil-producing municipalities

The effect of an increase in oil and gas revenues may be gauged thanks to the theoretical frameworks provided by Moretti (2011) to investigate the arrival of a new producer in a municipality and Allcott and Keniston (2015) to discuss nature resources booms. On the positive side, an increase in oil and gas revenues allow oil sector workers to increase their demand of intermediate goods, that may be partly produced locally. Moreover, the revenues of oil-producing municipalities should increase, allowing them to increase their expenditures, resulting in an increased demand for goods and services. The direct effect of an increase in oil and gas revenues on local production is therefore likely positive. Moretti (2011) and Allcott and Keniston (2015) moreover stress that that positive effect may be magnified in the presence of agglomeration economies that would attract additional firms to oil-producing municipalities.

However, the framework of Moretti (2011) and Allcott and Keniston (2015) also implies that general equilibrium effects may mitigate direct and agglomeration effects. First, the increased demand for labor increases wages, hence the cost of labor. Whereas rising wages allow the booming oil-sector to attract workers and increase its production, rising labor costs reduce the production of other sectors. That effect may be particularly harmful to producers of goods that are traded with other municipalities, because those producers are unable to increase their prices. As a result, the production of tradeable goods may move from oil-producing municipalities to other municipalities. Second, the prices of goods and services and of housing in oil-producing municipalities may increase due to the increase in labor costs, further increasing production costs for local producers and driving them away.

The relative magnitude of direct and general equilibrium effects is ambiguous. Accordingly, the effect of an increase in oil and gas revenues is a priori ambiguous. Whether it increases or decreases local production is therefore an empirical matter.

4.2.2 Spatial spillovers of oil and gas revenues

One may at first pass doubt that oil and gas revenues have any effect at all on other municipalities, if natural resources extraction comes in the guise of "enclaves" with high productivity but limited spillovers, as McMillan et al. (2014) document. Yet, whereas neighboring municipalities may not benefit from the direct effect of increased oil and gas revenues, they may be affected indirectly, because the indirect effects of oil and gas revenues may spread geographically. On the positive side, neighboring municipalities may benefit from the increased demand for intermediate goods from oil-producing municipalities. How far those effects may spread depends on the nature of the spread of the supply chain of oil producers. Unless transportation costs are negligible, those supply-chain effects are likely smaller the more distant a municipality from the oil-producing municipality. Moreover, neighboring municipalities may benefit from the relocation of the production of tradeable goods driven away from oil-producing municipalities by rising wages.

Another positive effect is specific to the way in which oil revenues are shared among Brazilian municipalities. Although the bulk of royalties accrues to oil-producing municipalities, neighboring municipalities are entitled to a small share of royalties if their land was used for oil post-product operations (i.e., storage and transportation).[4] Accordingly, neighboring municipalities may also benefit from a small increase in public expenditures leading to an increase in the demand for goods and services.

Conversely, neighboring municipalities may suffer from general equilibrium effects. In particular, the rise in the demand for labor in oil-producing municipalities is likely to spillover to neighboring municipalities, if workers are sufficiently mobile across municipalities, also resulting in an increase in prices. In addition, workers may migrate to oil-producing municipalities to meet the increased demand for labor in the oil sector. Another source of negative spillovers would materialize if the increase in demand for goods and services in oil-producing municipalities also attracts scarce resources, like capital and skilled labor. That could worsen bottlenecks in non-oil municipalities and further decrease production. That could even reduce the demand for unskilled labor, resulting in lower wages.

Again, increased oil revenues can generate positive or negative spillovers on neighboring municipalities with the total effect being ambiguous. That ambiguity notwithstanding, it stands to reason that whatever spillovers there are will decrease with distance to oil production, as transport costs will dampen the effect of oil revenues on the relocation of tradeable goods and the migration of workers. We should therefore expect those effects to be strongest in the immediate vicinity of oil-municipalities.

[4] See Monteiro and Ferraz (2010) for a detailed description of the distribution of oil royalties.

4.3 Empirical strategy and data

4.3.1 Empirical strategy

To gauge the direct and spill-over effects of oil and gas revenues on Brazilian municipalities, we estimate the following Spatial Durbin Model (SDM):

$$\begin{aligned} Y_{ist} = \rho W Y_{ist} + \alpha_1 log(Revenues_{ist}) + \alpha_2 W log(Revenues_{ist}) \\ + \beta X_{ist} + \gamma W X_{ist} + \delta_i + \eta_t + \epsilon_{ist} \end{aligned} \tag{4.1}$$

Where Y_{ist} is the relevant measure of economic activity of municipality i in state s and year t; δ_i is municipality fixed effects; η_t is year fixed effects; X_{ist} is a vector of time-varying controls and ϵ_{ist} is the error term. Municipality fixed effects control for the differences across municipalities that are constant over time, while year fixed effects control for changes in the variables that affect all municipalities in the same year.

The main explanatory variable, $log(Revenues_{ist})$, is the logarithm of oil and gas revenues computed as $Oil_{is,1992} \times P_t^{oil} + Gas_{is,1992} \times P_t^{gas}$, where $Oil_{is,1992}$ and $Gas_{is,1992}$ are the oil and gas production levels in municipality i and state s in 1992, and P_t^{oil} and P_t^{gas} are the international oil prices of oil and gas in year t. The spatial dimension of economic activity and spillovers of oil revenues are captured by the contiguity spatially weight matrix W, which defines potential interactions between each pair of municipalities. Two municipalities are considered as neighbors if they share a common border. A contiguous municipality is assigned a weight of 1, while non-neighbors are assigned a weight of zero. Accordingly, WY_{ist} is the average economic activity in the municipalities that are adjacent to municipality i in state s at time t and parameter ρ reflects the strength of spatial dependence in economic activities. By the same token, $Wlog(Revenues_{ist})$ denotes the logarithm of oil and gas revenues averaged over municipality i's neighbors.

The first parameter of interest is α_1. It captures the direct effect on the activity of a municipality of the oil and gas revenues of that municipality. The second parameter of interest is α_2, which measures the spillover on a municipality of oil and gas revenues in neighboring municipalities. Note that if $\rho = \alpha_2 = \gamma = 0$, then the model reduces to a standard linear regression model.

The weight matrix W implies that economic activity in a municipality is affected not only by the production and oil activity of its direct neighbors but also by all the other municipalities in the sample, because their neighbors have neighbors and so on. The magnitudes of α_1, and $\alpha_2$can therefore not be interpreted directly. We, therefore, compute and separately report the average effect of oil and gas revenues on economic activities within oil municipalities (direct effect), the average spillover effect to neighboring municipalities (indirect effect), and the average total effect (direct effect + indirect effect). The direct effect takes into account the feedback effects from neighboring municipalities in response to oil and gas revenues in municipality i. The total average effect measure can be interpreted as the average total impact of oil and gas revenues on economic activities in a typical municipality if all municipalities had oil activities (Pace and LeSage, 2006).[5]

Beside the fact that the main aim of the paper is to distinguish the direct effect of oil and gas revenues on oil producing municipalities from the spillovers that may affect neighboring municipalities, ignoring the spatial terms in Equation 1 would bias the estimates of the causal effects of oil revenues on economic activity in the presence of spillovers. Specifically, the effect would be overestimated in the presence of negative spillovers and underestimated in the presence of positive spillovers.

To the extent that the prices of oil and gas are determined on the world market and Brazilian municipalities are therefore price takers, prices are exogenous to the local factors in Brazilian municipalities. Exogeneity is further ensured by fixing the level of oil production to its level at the beginning of the sample period. Doing so overcomes endogeneity problems that would appear if oil discovery efforts and extraction rates were correlated with economic activity. The variation of the value of oil production given by the product of the price of oil and initial production level is therefore plausibly exogeneous and OLS estimates of α_1 and α_2 should be unbiased and reflect causal effects.[6]

4.3.2 Data

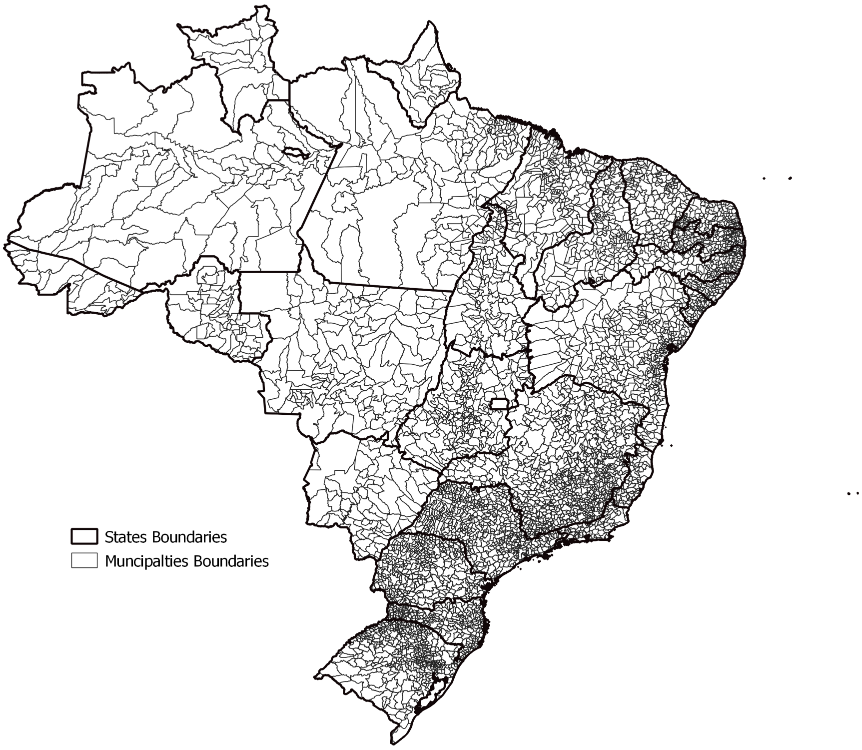
Our baseline dependent variable, economic activity, is measured using night-time light emissions. It comes from the National Oceanic and Atmospheric Administration's (NOAA) National Geophysical Data Center. We use the Defense Meteorological Satellite Program Operational Line scan System (DMSP-OLS) dataset providing a satellite-year dataset for the time period 1992 to 2013. It has been used as a proxy for GDP in recent studies (e.g. Doll et al., 2006; Henderson et al., 2012; Hodler and Raschky, 2014; Michalopoulos and Papaioannou, 2014; Keola et al., 2015). Night light emissions exhibit a strong correlation with GDP per capita (Henderson et al., 2012). The data is available at a very fine spatial resolution of approximately 1 square kilometer (30-arc seconds). It can therefore be aggregated at the level of Brazilian municipalities, which are our unit of observation. Figure 4.1 describes the borders of those municipalities.

To construct the dataset, NOAA processes daily images taken by the U.S. Department of Defense weather satellites orbiting the earth 14 times per day. Each satellite observes every location on earth every night at some point in time between 20:30 and 22:00. NOAA removes observations biased by strong sources of natural light, e.g., the summer months when the sun sets late, light activity related to the northern and southern lights, and forest fires. Observations obscured by clouds are also excluded. The filtered daily images are then averaged for the entire year producing light intensity data ranging from 0 (no light) to 63, with higher values indicating greater luminosity. The result is a measure of night light intensity that only reflects human (economic) activity.[7]

[5] Another interpretation could be that the total average effect measures the total cumulative impact arising from oil activities in municipality i on the economic activity of all other municipalities (on average) (Pace and LeSage, 2006).

[6] Similar procedures were used by Dube and Vargas (2013) and Nunn and Qian (2014).

[7] See Henderson et al., 2012 for more technical information about the construction of the dataset.



Notes: This map shows all Brazilian states and their corresponding municipalities (IBGE).

Figure 4.1: Brazilian municipalities' boundaries

Night-lights data has a number of advantages over using official municipal GDP figures computed by the Brazilian national bureau of statistics (Instituto Brasileiro de Geografia e Estatística-IBGE). First, the data has a longer time-series, as the official municipal GDP figures are only available on an annual basis from 1999 onward. Second, night-time lights data captures any type of economic activity, both official and non-official, especially at the sub-national level where official statistics are otherwise lacking or unavailable, in contrast to official GDP figures. Third, municipal GDP is not computed directly in Brazil. It is instead inferred from state GDP divided among municipalities according to a number of reference variables. For oil producing municipalities, the reference variable used to assigni municipal industry GDP is the same as the one used to assign oil production to municipalities, creating a tautological correlation between oil production and official GDP. Using night-time light emissions therefore allows us to study a relationship that is not a spurious artefact of the way Brazilian authorities impute state GDP to municipalities. Our data on oil production comes from Agencia Nacional de Petroleo (ANP), which provides information on oil output, prices, and oil fields locations on a monthly basis.[8] We use this data to determine municipalities located over these oil fields and their production shares. We define oil municipalities as municipalities located over at least one oil field or part of it, with oil fields being in production phase in 1992.

[8]Each oil field produce oil and natural gas. Therefore, throughout the article we use "oil" to refer to both oil and natural gas.

Notes: This map shows the locations of oil producing municipalities and their corresponding states. Oil municipalities are defined as municipalities that started oil production in 1992. We exclude all municipalities that did not exist in 1991. Data from IBGE and ANP.

Figure 4.2: Brazilian oil-producing states and municipalities' locations

International oil and gas prices were retrieved from International Financial Statistics.[9] Figure 4.2 shows the locations of the oil-producing municipalities and the corresponding states. In appendix 4.A1, we explain in detail how we construct the annual oil production series for each municipality.[10] Population, in logarithm, is controlled for in all regressions given its strong correlation with economic activity (Mamo et al., 2019). Data on annual municipality population is obtained from inter-census population estimates provided by IBGE. Because the SDM approach requires a strongly balanced dataset, missing municipal-year observations are filled in using linear interpolation to avoid the drop of municipalities with missing years. Table 4.1 reports the summary statistics for our variables of interest.

Table 4.1: Summary statistics

Variable	N	Mean	SD	Min	Max
Night-time lights	42680	4.99	9.25	0	63
Revenues (log)	42680	0.97	4.44	0	28.61
Population (log)	42680	9.71	1.13	6.58	16.29
Distance to oil muncipality	42680	249.89	176.89	0.00	724.39

Our period of analysis is 1992-2013. Our final baseline sample covers 1940 municipalities among which 91 produce oil. To consider a reasonably homogenous sample, we focus on

[9]It should be noted that some oil municipalities are set to report zero oil production, because they started producing after 1992.

[10]We follow Caselli and Michaels (2013) in constructing the oil production figures for oil producing municipalities.

the nine oil-producing states, namely Ceará (9 oil-producing municipalities), Rio Grande do Norte (16 municipalities), Alagoas (9 municipalities), Sergipe (19 municipalities), Bahia (26 municipalities), Espírito Santo (4 municipalities), Rio de Janeiro (5 municipalities), Sao Paulo (1 municipality), and Paraná (2 municipalities).[11] The number and the size of Brazilian municipalities changed during the 1990s owing to the split and the merging of municipalities. We therefore restrict our sample to municipalities that existed in 1991 onward and exclude all municipalities that were founded afterwards.[12] In all specifications, we cluster the standard errors at the municipal level.

4.4 Baseline results

4.4.1 A first look at the data

Figure 4.3 plots the average night-time light levels, net of municipality and year fixed effects, against international oil price separately for oil producing municipalities, in Panel A, and non-oil producing, in Panel B. Panel C reports the results pooling all Brazilian municipalities.[13] The figure shows that the price of oil correlates positively with night-time light emissions in oil producing municipalities but negatively in non-oil producing municipalities. This finding suggests that while oil-producing municipalities benefit from an increase in the price of oil, other municipalities suffer from it.

In both plots, the nonparametric line suggests that the relationship is linear. Plotting all municipalities together, we see no relationship between the price of oil and night-time lights with the nonparametric line being flat. Accordingly, the previously depicted positive and negative relationships cancel out each other when combined together.[14]

4.4.2 Empirical findings

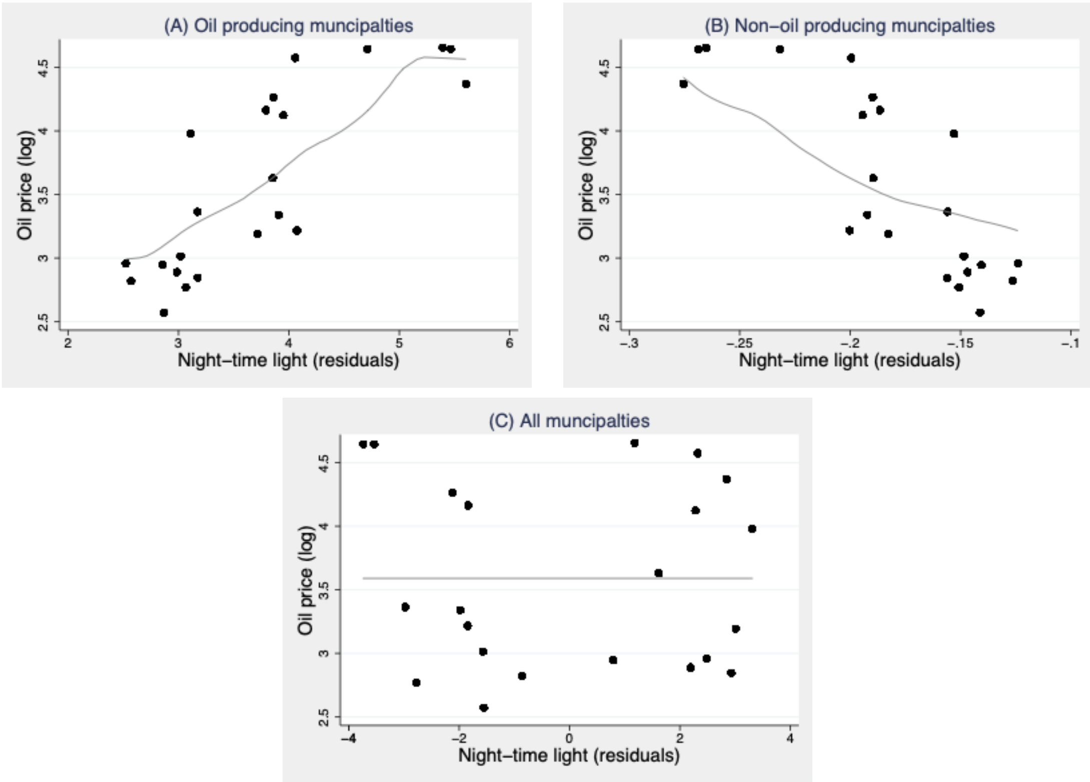
Table 4.2 reports the results from estimating Equation 4.1 using various sample compositions and variable definitions. The baseline sample is restricted to municipalities located in the nine oil-producing states. In Column 1, oil production is fixed at 1992 levels, the production of municipalities that started producing after 1992 being set to zero. Column 1 reports a restricted version of Equation 1 where α_1 and α_2 are set to zero. In other words, the column reports the OLS estimates of Equation 4.1. It shows that the coefficient of oil and gas revenues is positive and statistically significant at the one-percent level. Thus, a rise in oil and gas revenues increases economic activity, as measured by light emissions,

[11] Ten other municipalities were also producing oil in 1992. However, eight out of these municipalities did not exist in 1991, but were carved out of existing municipalities and thus dropped. One municipality located in the state of Amazonas is excluded given its remote location from other oil-municipalities. The last municipality "Grossos" in state of Rio Grande do Norte is excluded given the unavailability of data on night light data.

[12] As robustness check, we also use the full sample of 2065 municipalities located in the oil producing states.

[13] The estimation proceeds in two steps. In the first step, we regress night-time light on municipality fixed effects and year fixed effects. Then we take the residuals and use the nonparametric local polynomial estimator to examine the relationship between oil price and annually averaged night-time light.

[14] We get the same results when we use international gas prices (results not reported for brevity).



Notes: The graph shows the relationship between oil price and annually averaged night-time light in (A) oil producing municipalities; (B) non-oil producing municipalities; and (C) All Brazilian municipalities. The solid line represents the nonparametric local polynomial fit computed using an Epanechnikov kernel

Figure 4.3: Effect of oil production on economic activity

in oil producing municipalities. That finding is in line with previous studies reporting a positive effect of natural resources on economic activity in areas where production is located (Black et al., 2005, Allcot and Keniston, 2018, Michaels, 2011, Aragón and Rud, 2013, Gradstein and Klemp, 2016, and Mamo et al., 2019).

Columns 2 to 4 explicitly tackle spatial spillovers by reporting the spatial Durbin model estimates of Equation 4.1, including the spatial lags of oil revenues and night-time light variables. In all those models, the spatial autoregressive parameter ρ is positive and statistically significant beyond the one-percent level, reflecting a strong correlation in economic activity across municipalities. Accordingly, a contemporaneous increase in economic activity in a neighboring municipality induces a 0.17 point increase in economic activity in the municipality itself. Column 2 reports the estimation of Equation 4.1 on the baseline sample. The coefficient of oil and gas revenues remains positive and statistically significant at the one-percent level of significance. The key new insight is provided by the coefficient of the spatial lag of oil and gas revenues, which is statistically significant at the five-percent level suggesting that the effect of oil and gas revenues spills over to neighboring municipalities. In addition, its magnitude is similar to the magnitude of the coefficient of oil and gas revenues in the municipality, indicating that economic activity in neighboring municipalities is expanding by less than the average that one would expect from the positive spatial correlation pattern observed in night-time lights as previously indicated by autoregressive parameter ρ.

Table 4.2: Effect of oil and gas revenues on economic activity

	(1)	(2)	(3)	(4)	(5)
	Light	**Light**	**Light**	**Light**	**Light**
	OLS	**SDM**	**SDM**	**SDM**	**SDM**
	Baseline	**Baseline**	**Subsample**	**Oil dummy**	**Oil dummy**
Panel A: Estimated Coefficients					
ln(Revenues)	0.675***	0.735***	0.752***		
	(0.216)	(0.214)	(0.215)		
W(ln(Revenues))		-0.771**	-0.791**		
		(0.352)	(0.345)		
ln(Oil price) x oil dummy				0.796***	
				(0.190)	
W(ln(Oil price) x oil dummy)				-0.721**	
				(0.291)	
ln(Gas price) x oil dummy					0.978***
					(0.240)
W(ln(Gas price) x oil dummy)					-0.932**
					(0.365)
W(light)		0.168***	0.156***	0.169***	0.169***
		(0.018)	(0.018)	(0.018)	(0.018)
Number of observations	42,680	42,680	42,064	42,680	42,680
Number of municipalities	1,940	1,940	1,912	1,940	1,940
R-squared	0.476	0.478	0.247	0.328	0.326
lnPopulation	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Panel B: Direct and Indirect effects from SDM					
Direct Effect		0.711***	0.729***	0.774***	0.950***
		(0.217)	(0.218)	(0.193)	(0.244)
Indirect effect		-0.791*	-0.807*	-0.707**	-0.931**
		(0.432)	(0.423)	(0.354)	(0.458)
Total effect		0.080	-0.078	0.068	0.020
		(0.529)	(0.522)	(0.451)	(0.572)

Dependent variable is mean night-time light intensity. The sample is restricted to municipalities located in 9 oil-producing states. In column 1, oil municipalities that have started production after 1992 are set to zero. Column 3 excludes oil muncipalities that have started production after 1992. Columns 4 and 5 replace oil production in column 1 by a dummy indicating oil muncipalties that started production from 1992 onward. The method of estimation in column 1 is OLS; in columns 2-5 is spatial Durbin model (SDM) based on a spatial weights row-standardized contiguity matrix W that assigns 1 to municipalities that share a common border. Robust standard errors reported in parentheses and clustered at the municipal level. Panel B reports estimates of direct, indirect and total effects from SDM, with standard errors computed by Monte Carlo standard errors using 100 replications (Lesage and Pace, 2009). Significantly different from zero at *10% significance, **5% significance, ***1% significance.

Panel B reports the estimates of the implied direct effect of oil and gas revenues within an oil municipality, of the indirect average spillover effect in the neighboring municipalities, and of the total average effect, which is the sum of direct and indirect effects. It thereby gives the quantitative significance of our effects. The direct effect of oil and gas revenues within oil municipalities is positive and statistically significant. The coefficient of the direct effect of oil and gas revenues is about 13% lower than in Column 1 and implies that a 10 percent increase in oil and gas revenues boosts economic activity by 0.07 luminosity points. With an average night-time light emission of 4.99 in our sample, that coefficient translates to an average 1.4 percent more increase in economic activity in oil-producing municipalities. The indirect spillover effect in neighboring municipalities is negative and significant, suggesting that oil and gas revenues reduce economic activity in neighboring municipalities. Furthermore, the spillover effect is nearly of the same magnitude as the direct effect. As a result, the total average effect is small and statistically insignificant, since the positive direct effect and negative indirect effect cancel each other out.

Considering that all municipalities that had not started producing oil in 1992 received no revenues throughout the period even though a municipality may have started its production in the following year may result in an attenuation bias. As an alternative, in Column 3, we estimate Equation 4.1 on a subsample of municipalities excluding municipalities that started producing oil after 1992. The coefficients of oil and gas revenues and of their spatial lag both slightly increase in magnitude but keep their signs and significance levels. The magnitudes of the direct, indirect, and total average effects also increase, and their signs and levels of statistical significance also remain unchanged, showing that our baseline finding was not driven by our hypothesis on the onset of oil production.

To address concerns of an attenuation bias due to measurement error, we replace oil production in Column 4 by a dummy variable that takes a value of 1, if the municipality is producing oil from 1992 onward and multiply it by the international price of oil. The coefficients of oil and gas revenues and of their spatial lag remain stable in sign and significance, as well as their associated direct, indirect and total effects. Similarly, in Column 5, we multiply the dummy indicating oil municipalities by the log of international gas price and our results remain.

Overall, our results suggest large negative spillover effects from oil and gas revenues. A ten-percent increase in oil and gas revenues reduces economic activity in neighboring municipalities by around 0.08 units of luminosity on average. Ignoring those spillovers would give a biased picture of the total effect of oil and gas revenues. While oil producing municipalities seem to benefit from an increase in oil and gas revenues, neighboring oil municipalities suffer from it. As a result, the aggregate effect of oil and gas revenues on economic activity is negligible, which may explain the contradictory findings of the literature (Sachs and Warner, 1995, 2001, Auty, 2001, Alexeev and Conrad, 2009).

4.4.3 Robustness checks

In our baseline specification, we examine the effect of the sum of oil and gas revenues on night-time lights. Yet, oil and gas revenues may affect the economy differently. In Columns 1 and 2 of Table 4.3, we therefore differentiate the effect of revenues generated by the production of the two hydrocarbons. In both models, we still see a positive direct impact of oil and gas revenues on night-time light emissions within producing municipalities, whereas the neighboring municipalities bear negative spillovers and the total effect remains small and statistically insignificant.Looking at the distribution of night-time light emissions, we find that around 99 percent of observations lie between 0 and 54, with about 1 percent of observations

Table 4.3: Effect of oil and gas revenues on economic activity – robustness checks I

	(1)	(2)	(3)	(4)	(5)
	Light	**Light**	**Light**	**Light**	**Light**
	Oil production only	**Gas production only**	**Winzorising extreme values**	**Lagged oil revenues**	**Add dynamics**
Panel A: Estimated Coefficients					
ln(Oil revenues)	0.702***				
	(0.186)				
W(ln(Oil revenues))	-0.582*				
	(0.307)				
ln(Gas revenues)		0.804***			
		(0.231)			
W(ln(Gas revenues))		-0.768**			
		(0.374)			
ln(Revenues)			0.737***		0.204***
			(0.213)		(0.078)
W(ln(Revenues))			-0.790**		-0.112
			(0.349)		(0.114)
ln(Revenues), t-1				0.764***	
				(0.214)	
W(ln(Revenues)), t-1				-0.721**	
				(0.353)	
W(light)	0.169***	0.168***	0.162***	0.170***	0.125***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.011)
Light, t-1					0.793***
					(0.013)
Number of observations	42,680	42,680	42,680	40,740	40,740
Number of muncipalties	1,940	1,940	1,940	1,940	1,940
R-squared	0.287	0.255	0.265	0.257	0.963
lnPopulation	Yes	Yes	Yes	Yes	Yes
Muncipalty FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Panel B: Direct and Indirect effects from SDM					
Direct Effect	0.685***	0.781***	0.713***	0.743***	0.194***
	(0.189)	(0.235)	(0.216)	(0.216)	(0.066)
Direct Effect (Long run)					0.945***
					(0.365)
Indirect effect	-0.567	-0.778*	-0.812*	-0.711*	-0.110
	(0.370)	(0.463)	(0.425)	(0.411)	(0.134)
Indirect effect (Long run)					-0.037
					(1.501)
Total effect	0.118	0.003	-0.098	0.032	0.084
	(0.462)	(0.566)	(0.517)	(0.489)	(0.148)
Total effect (Long run)					0.908
					(1.717)

Dependent variable is mean night-time light intensity. The sample is restricted to municipalities located in 9 oil-producing states. All columns shows the estimates of a spatial Durbin model (SDM) employing a row standardized contiguity matrix. Estimates are based on a spatial weights contiguity matrix W that assigns 1 to municipalities that share a common border. Column 1 estimates the effect of oil revenues on night-time light. Column 2 estimates the effect of gas revenues on night-time light. In column 3, night-light data is winsorized by replacing extreme values with the next highest/lowest observation. Column 4 employs the one-year lagged oil and gas revenues as an explanatory variable. In column 5, we add lagged night-time lights. Robust standard errors reported in parentheses and clustered at the municipal level. Panel B reports estimates of direct, indirect and total effects from SDM, with standard errors computed by Monte Carlo standard errors using 100 replications (Lesage and Pace, 2009). Significantly different from zero at *10% significance, **5% significance, ***1% significance.

taking the extreme values of 0 and beyond 54. To make sure that our findings are not driven by those extreme observations, we winsorize extreme values by replacing them with the next highest (lowest) observation. This ensures that all observations are used and the effect of possibly spurious outliers is reduced. The second column of Table 4.3 contains the results of a regression estimating Equation 4.1 with winsorized night-time lights. The main results do not differ: the direct effect of oil and gas revenues on economic activity is positive and significant within oil municipality; the spillover effect to neighboring municipalities is negative and quantitatively larger than the direct effect; and the overall effect is small and insignificant.

One may argue that the effect of oil and gas revenues only materializes with a lag and that using its contemporaneous value underestimates it. In Column 3 of Table 4.3, we therefore lagged oil revenues by one year. Again, the estimated coefficients and the associated effects change neither in sign, magnitude, or statistical significance. This ensures that the baseline results were not driven by the lag structure.[15]

Equation 4.1 is static and therefore does not take the inertia of economic activity into account. We therefore estimated a fully dynamic autoregressive SDM by adding the lagged dependent variable to the explanatory variables. The coefficient of lagged night-time lights is positive and statistically significant. However, although the coefficient of oil revenues drops in size, it remains statistically significant. The coefficient on spatial lagged oil revenues remains negative, but loses its statistical significance. However, the estimated direct, indirect and total effects remain unchanged. The long-run direct effect is larger than the short-run effect, suggesting that night-time light adjusts very slowly, so that the direct effect of oil revenues on economic activity in oil municipalities is around 400% larger in the long run.

Table 4.4 reports a series of robustness checks pertaining to the sample of municipalities. We have so far restricted our sample to the municipalities that existed throughout our period of study, thereby overlooking municipalities that appeared during the period. To make sure that this did not drive our baseline results, we included all municipalities in the 9-oil producing states taking into account the ones that did not exist in 1991. Column 1 of Table 4.4 shows that the estimated coefficient on oil and gas revenues becomes slightly larger, but the coefficient of its spatial lag increases by around 17 percent. The same is true for their associated estimated effects. All coefficients remain unchanged in their signs and statistically significance.

Another way to make sure that our results are not driven by outliers is to simply drop extreme observations. We accordingly dropped the top 10% oil producing municipalities in terms of oil production and estimated Equation 4.1 anew.[16] The outcome of that estimation is reported in Column 2 of Table 4.4. Again, dropping large producers of oil does not affect the baseline results, in terms of either statistical or quantitative significance.[17]

[15] We also checked with using the 2-year lag and results remain robust (results reported in appendix 4.A2).

[16] The dropped oil municipalities are Açu, Areia Branca, Alagoinhas, Catu, Cabo Frio, Campos dos Goytacazes, Casimiro de Abreu, Macaé, and Quissamã.

[17] Further robustness checks reported in appendix 4.A2 include dropping capital municipalities and lights

Table 4.4: Effect of oil and gas revenues on economic activity – robustness checks II

	(1)	(2)	(3)	(4)	(5)
	Light	**Light**	**Light**	**Light**	**Light**
	All munci-paltities	**Drop top 1% oil producers**	**Sub-sample (1992-2002)**	**Sub-sample (2003-2013)**	**Placebo test**
Panel A: Estimated Coefficients					
ln(Revenues)	0.749***	0.775***	1.119***	0.828**	0.084
	(0.214)	(0.237)	(0.372)	(0.385)	(0.125)
W(ln(Revenues))	-0.902**	-0.983***	-0.768	-0.807*	-0.130
	(0.352)	(0.315)	(0.485)	(0.435)	(0.157)
W(light)	0.178***	0.169***	0.156***	0.227***	0.016***
	(0.017)	(0.018)	(0.017)	(0.020)	(0.005)
Number of observations	45,430	42,482	21,340	21,340	42,680
Number of muncipalties	2,065	1,931	1,940	1,940	1,940
R-squared	0.248	0.269	0.096	0.234	0.048
lnPopulation	Yes	Yes	Yes	Yes	Yes
Muncipalty FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Panel B: Direct and Indirect effects from SDM					
Direct Effect	0.721***	0.744***	1.099***	0.797**	0.084
	(0.217)	(0.241)	(0.376)	(0.389)	(0.126)
Indirect effect	-0.944**	-1.009**	-0.759	-0.837*	-0.138
	(0.441)	(0.399)	(0.545)	(0.500)	(0.164)
Total effect	-0.223	-0.264	0.340	-0.040	-0.054
	(0.538)	(0.520)	(0.705)	(0.683)	(0.230)

Dependent variable is mean night-time light intensity. The sample is restricted to municipalities located in 9 oil-producing states. All columns shows the estimates of a spatial Durbin model (SDM) employing a row statndardized contiguity matrix. Estimates are based on a spatial weights contiguity matrix W that assigns 1 to municipalities that share a common border. In column 1, we include all muncipalties in the 9-oil producing states, including the ones that did not exist in 1991. Column 2 excludes the top 1% oil producing municipalities. Columns 3 and 4 restrict the sample period to two periods, (1992-2002) and (2003-2013) respectivelly. In column 5, night-time light are randomely reshuffled among muncipalties. Panel B reports SDM estimates for the direct effect in oil municipality, the indirect average spillover effect in neighboring municipalities and the total average effect, which is the sum of direct and indirect effects, respectively. Robust standard errors reported in parentheses and clustered at the municipal level. Panel B reports estimates of direct, indirect and total effects from SDM, with standard errors computed by Monte Carlo standard errors using 100 replications (Lesage and Pace, 2009). Significantly different from zero at *10% significance, **5% significance, ***1% significance.

To see if the baseline results are driven by a specific period of time, we split the sample into two sub-periods of equal length, specifically pre-2003 and post-2003 and run a regression for each period separately. The two regressions are reported in Columns 3 and 4 of Table 4.4. The coefficient of oil and gas revenues is positive and statistically significant in both regressions. The spatial lag of oil and gas revenues is statistically significant only for the post-2003 period. This finding may be driven by the smaller size of the sample or suggest that spillovers essentially materialized in the second period. The estimated direct, indirect, and total effects for both samples follow the same patterns. In addition, the positive direct effect of oil revenues is always compensated by the negative spillover effect, so that the total effect is statistically insignificant, like in our baseline results.

In Column 5, we address the concern that time-varying omitted variables could be driving the estimated relationship between economic activity and oil revenues. Indeed, it could be the case that the residual unobserved heterogeneity still co-moves with the world oil prices despite the wide array of fixed effects we include. To rule that possibility out, we perform a placebo test by randomly reshuffling night-time light observations among mu-

> 99th percentile. Our main results remain robust.

nicipalities before estimating Equation 4.1 again. Although the coefficient of the spatially lagged dependent variable remains positive and statistically significant, the coefficients of oil revenues and of their spatial lag as well as the estimated direct and indirect effect all become statistically insignificant and very small in magnitude. As the placebo test results in insignificant results, we can safely conclude that our baseline results are not driven by co-movements in oil prices and economic activity.

4.5 Extensions

4.5.1 How spillovers fade with distance

We have so far taken the spatial dimension of spillovers into account by defining proximity as adjacency, in line with the SDM. To go beyond that definition and see how far oil production in one municipality affects other municipalities, we complement our baseline results by using crow-fly distance to the nearest oil-producing municipality. To allow the relationship to be non-linear, we define exposure to oil production in ring k as:

$$Exposure_{isk} = log(Revenues_{js}) \times ring_{ijk}$$

where $ring_{ijk}$ is a dummy variable set to one if a municipality i in state s lies within a certain range k from oil municipality j.

To define the rings' width, we choose a baseline distance cut-offs of 50, 100, 150, 200, 250, 300, and more than 300 km.[18] Choosing a 50 km distance as a starting point is fairly reasonable for two reasons: First, the number of municipalities below 50 km distance is small, hence introducing more noise and reducing the power of the results; Second, given that oil municipalities are moderately spatially clustered, by employing distance below 50 km, we could be end up capturing the effect of the oil municipality itself rather than the surrounding non-oil areas. The latter concern is known as overglow phenomenon of night-time lights driven by lights emanating from the oil industry itself (i.e. gas flares or construction sites). Such overglow effects become smaller after 50 km (Pinkovskiy, 2017).[19]

The spillover effect is then estimated by running the following regression:

$$Y_{ist} = \beta_1 log(Revenues_{ist}) + \sum_k \beta_k log(Exposure_{isk}) + \varphi X_{ist} + \alpha_i + \vartheta_t + \mu_{ist} \tag{1}$$

where α_i and ϑ_t are municipality and year fixed effects. β_1 measures the direct effect of oil and gas revenues on economic activity in oil municipalities (i.e. ring = 0 km) and β_k captures the spillover effect of oil production on economic activity in municipalities located within a certain ring from oil municipalities. Total spillover effects are, therefore, estimated by examining the effect of oil revenues aggregately within all rings at once, with each ring excluding from its range the preceding one. For instance, if we are at the 100 km radius, we exclude from it the 50 km radius and so on.

[18] Distance thresholds are calculated based on the centroids of municipalities.

[19] As a robustness check, we also tried cut-offs of 100, 200, 300, and more than 300 km and results remain

Table 4.5: Effect of oil and gas revenues on economic activity – extensions

	(1)	(2)	(3)	(4)	(5)	(6)
	Light	**Light**	**Light**	**Light**	**Light**	**Light**
	Ring	**Distance to oil muncipality**	**Distance to oil muncipality**	**Micro-regions**	**Micro-regions**	**Macro-regions**
ln(Revenues)	0.443**			0.075	-3.279***	-5.555
	(0.225)			(1.306)	(1.058)	(7.800)
Exposure $\leq$ 50	-0.066					
	(0.296)					
Exposure $\leq$ 100	-0.260					
	(0.351)					
Exposure $\leq$ 150	-0.604**					
	(0.291)					
Exposure $\leq$ 200	-0.328					
	(0.310)					
Exposure $\leq$ 250	0.278					
	(0.332)					
Exposure $\leq$ 300	-0.075					
	(0.200)					
Exposure > 300	-0.254*					
	(0.137)					
ln(Oil Price)		2.048***				
		(0.062)				
ln(Oil Price) x Distance to oil muncipalty		-0.001***				
		(0.000)				
ln(Gas Price)			2.457***			
			(0.076)			
ln(Gas Price) x Distance to oil muncipalty			-0.001***			
			(0.000)			
Number of observations	42,680	42,680	42,680	5,346	5,346	1,298
Number of muncipalties/regions	1,940	1,940	1,940	243	243	59
R-squared	0.481	0.478	0.477	0.592	0.574	0.673
lnPopulation	Yes	Yes	Yes	Yes	Yes	Yes
Muncipalty FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variable is mean night-light intensity. The sample is restricted to municipalities located in 9 oil-producing states. In all columns, oil municipalities that have started production after 1992 are set to zero. The unit of observation in columns 1, 2 and 3 is muncipality; in columns 4 and 5 is micro unit; in column 6 is macro unit. In column 1, the spillover effects is estimated using the ring approach as described in main text. In column 2, light pixels from oil municipalities are dropped. The method of estimation is ordinary lease squares. Robust standard errors reported in parentheses are clustered at the municipal level in columns 1, 2 and 3; at the micro leve in columns 4 and 5; at the macro level in column 6. Significantly different from zero at *10% significance, **5% significance, ***1% significance.

Table 4.5 presents the results from estimating Equation 1. The direct effect of oil revenues on economic activity in oil municipalities remains positive and statistically significant. As we move away from oil municipalities, the effect becomes statistically insignificant in the 50 and 100 km rings. We observe a negative effect of oil statistically significant at the five-percent level in the ring ranging from 100 to 150 km. The effect is statistically insignificant at standard levels in the next three rings, specifically 150-200 km, 200-250 km, and 250-300 km. Beyond 300 km, the spillover effect of oil revenues is negative and weakly statistically significant at the ten-percent level.

In the second column of Table 4.5, we follow Gradstein and Klemp's (2016) specification and replace the distance dummies by a continuous measure of crow-fly distance, which we interact with the international price of oil. The direct effect of the price of oil on economic activity is positive and statistically significant at the one-percent level. The estimated coefficient implies that a 10 percent increase in oil price raises economic activity

unchanged. The results are reported in appendix 4.A2.

by 0.20 percentage point in oil-producing municipalities, that is when distance is zero. The interaction effect is negative and significant at the one-percent level, suggesting that the positive effect of oil fades as one moves away from oil municipalities. Quantitatively, the average distance to an oil producing municipality is 291 km, so that the coefficient of the conditional effect implies that a 10 percent increase in oil price increase economic activity in a non-oil municipality with an average distance from an oil municipality by 0.18 points. This is 10 percent lower than the increase in economic activity in oil municipalities. This result replicates the finding of Gradstein and Klemp (2016) the effect fades with distance but remains positive. In contrast to our previous findings, the effect of oil never turns out negative. The linear specification that Gradstein and Klemp (2016) use, and that we use here, therefore hides the negative spillovers that we observe with the SDM and the non-linear model. The result is not confined to using oil price, in fact we obtain the same results in Column 3, when interacting the distance to oil municipality with the international price of gas. The coefficients of (log) gas price and interaction term are almost of the same magnitude as in Column 2.

To further explore the general equilibrium effects of oil production, we follow Mamo et al. (2019) and redefine the unit of observation to correspond to the next higher administrative level. The idea here is that by doing so we define geographic units that internalize a part of the spillover effects. The larger the administrative unit, the larger the share of spillover effects that is internalized. We therefore conduct our analysis at the micro-region level, which is a grouping of economically integrated contiguous municipalities with similar geographic and productive characteristics, then at the level of macro-regions, the next highest administrative level. Micro-regions closely parallel the notion of local economies and have been widely used as the units of analysis in the literature on effects of trade liberalization on local labor markets in Brazil (Dix-Carneiro and Kovak, 2015; Dix-Carneiro, Soares and Ulyssea, 2018). The average micro-region in our sample comprises eight municipalities with a maximum number of 41 municipalities and an average size of 5,000 square kilometers. Macro-regions feature, on average, 33 municipalities and have an average size of 22,000 square kilometers.

When estimating Equation 4.1 on micro- and macro- region, we expect the coefficient of oil revenues to be smaller than in the baseline regressions or even statistically insignificant, owing to the fact the positive direct effects from oil and gas revenues and the negative spillovers may cancel out. Column 3 of Table 5 reports the outcome of that estimation. In line with our previous results signaling the presence of negative spillovers, the coefficient of oil and gas revenues is statistically insignificant. In the aggregate, the direct positive effect and negative spillover effects therefore cancel out. Column 4 reports the results obtained using macro-regions, the next highest administrative level, as our unit of observation. The coefficient of interest remains statistically insignificant. In addition, it is now negative, suggesting that it internalizes a larger share of negative spillovers. Overall, the results of those extra regressions confirm that oil and gas revenues generate large and negative spatial spillovers on neighboring municipalities, rendering the total impact of oil revenues on economic activities in all municipalities insignificant.

Finally, we follow another specification used by Mamo et al. (2019) and try to measure spillover effects only by simply excluding from the aggregation by microregion night-time light pixels adhering to oil municipalities before re-estimating Equation 4.1. As we exclude the activity from the municipalities that directly benefit from oil production and our results point to negative spillovers, we expect the effect to be negative and statistically significant. The outcome of that regression is reported

4.5.2 Transmission channels of oil and gas revenues to the economic activity

We now turn to the drivers behind the effect of oil revenues. To do so, we consider a series of alternative dependent variables that may all be affected by oil production. Municipalities that neighbor an oil-producing municipality are also entitled to a small share of royalties, if their land was used for oil post-product operations (i.e., storage and transportation). Data on oil royalties (measured in real terms and expressed in US dollars) are obtained from Agencia Nacional de Petroleo (ANP) for the period 1999-2013. In Column 1 of Table 4.6, we look at the effect of oil revenues on total royalties received. The estimated coefficient of the direct effect is positive and statistically significant at the one-percent level. The coefficient of its spatial lag is positive, but much smaller in magnitude and significance. Quantitatively, a 10 percent increase in oil and gas revenues results in a 7 percent increase in royalties received by oil municipalities compared to 1 percent increase in neighboring municipalities. The total effect is positive and statistically significant, and mostly driven by the direct effect, confirming that royalties are to a large extent a local phenomenon and exert limited spillovers. The coefficient of the spatial lag of royalties is positive and significant but is very small in size suggesting limited correlation between royalties across municipalities. Together with the finding that oil and gas revenues impose a negative spillover effect on the night-time light emissions of non-oil municipalities, this finding shows that the small royalties that are paid to non-oil municipalities do not compensate other mechanisms that affect them negatively.

In Column 2, we examine the impact of oil revenues on population and find that they significantly increase the population of oil municipalities but have no statistically significant impact on neighboring municipalities. A 10 percent increase in oil and gas revenues leads to 0.1 percent increase in the size of the population in oil municipalities but there we find no evidence of a drain of workers in neighboring municipalities. Movements of population from non-oil municipalities to oil-producing municipalities are therefore not the channel of transmission of the spillovers from oil and gas revenues. The increase in population therefore likely originates in non-neighboring municipalities.

In Columns 3 to 6, we test a specific prediction of the theoretical models developed by Moretti (2011) and Allcott and Keniston (2015). They argue that the rise in labor demand following resource booms will increase wages in resource-related sectors. To examine that, we rely on data on wages gathered by Registro Anual de Informaes Sociais (RAIS), an official micro database for all registered formal workers during the period 2002-2013. The wages are expressed in real terms, with nominal figures deflated by CPI index provided by IPEA (Índice Nacional de Preços ao Consumidor - INPC) and transformed to US dollars. Besides looking at overall wages, we also explore heterogeneous effects by sector, namely the tradable sector, defined as the sum of the manufacturing and agriculture sectors, and non-tradeable sectors, defined as the service sector. Column 3 uses the average wage in all sectors as dependent variable. In that column, the coefficients of oil and gas revenues is positive, meaning that wages in oil producing municipalities are statistically significantly increasing with oil and gas revenues. The spatial lag of the independent variable exhibits a negative but statistically insignificant coefficient, suggesting that there are no systematic spillovers for oil and gas revenues on wages in general.

In neighboring municipalities, wages are adversely affected by oil revenues and the impact is particularly strong –both in magnitude and statistical significance- for the service sector. The coefficient for the direct effects indicates that a 10 percent increase in oil revenues increases real wages, manufacturing and service wages in oil producing municipalities by 6.6, 9.2 and 25.7 US dollars, respectively, while agriculture sector's wages are left unaffected. In contrast, manufacturing, agriculture and service wages in neighboring municipalities significantly decline by 7.4, 12.6, and 102 US dollars for each 10 percent increase in oil revenues. The total effect of oil revenues on wages is positive, but statistically insignificant for overall and manufacturing wages, meaning that the uptick in wages witnessed by oil municipalities is compensated by a decline in wages in neighboring municipalities. For the service and agriculture sectors, the coefficient of the indirect effect is greater in size than the coefficient of the direct effect, so that the total effect is negative and statistically significant.

In Column 6, we examine the effect of oil and gas revenues on local prices. Following the reported increase in sectoral wages, we would expect a rise in the demand for locally traded goods, which in turn increase their prices. Following Aragon and Rud (2013), we test this proposition using the prices of locally produced agriculture goods. Local prices are based on the Municipal Agricultural Production survey (Produção Agrícola Municipal), obtained from IPEA for the period 1992-2010 and expressed in US dollars real terms. To determine the main agriculture crop for each municipality and its price, we follow Berman et al. (2017) and define it as the one with the highest total production value over the entire period (evaluated at 2000 prices). We then divide the production value of the main crop by production (measured in tons) to get the local crop price per ton. As expected, local prices in oil municipalities increase with oil revenues, whereas they decline in neighboring municipalities. A 10 percent rise in oil and gas revenues increases local prices by 2 dollars per ton, while it reduces them in neighboring municipalities by almost the same amount.

Finally, in Column 7, we analyze the impact of oil and gas revenues on crime. Crime rates are measured by the number of homicides per 100,000 inhabitants in a given year and obtained from IPEA during the period 1992-2013.[20] Previous studies identify two

[20]Homicides rates is a good proxy for crime rates given that lack of data on other forms of non-violent crimes (i.e. property crimes such as theft, robbery and burglary) at the municipal level. The same measure

opposite effects of natural resources on criminal activities (see for example Dube and Vargass, 2013). On the one hand, increased royalties increase incomes, hence the incentives to engage in theft, expropriation, and illegal activities in general. This effect is referred to as the rapacity effect. On the other hand, a deteriorating economic situation reduces the opportunity costs of committing crimes and can thus lead to a surge in crime. In Column 7, both oil and gas revenues and their spatial lag exhibit a positive coefficient. Although the coefficient of the spatial lag of oil and gas revenues is statistically insignificant at conventional levels, their marginal effect reported in the bottom panel of the table is statistically significant at the ten-percent level. Accordingly, an increase in oil and gas revenues increases crime in both oil municipalities and neighboring municipalities, suggesting that crime is one of the spillovers of oil and gas revenues. This is reminiscent of Caselli and Michaels (2013) who find that oil windfalls in Brazil increase the incidence of illegal activities by local politicians.

Overall, the results suggest that oil revenues exhibit positive and negative effects, where the balance between both effects depends on the proximity to oil municipalities. In oil municipalities, the positive effects of oil revenues on royalties, wages, local prices and population suppress the effect of negative externalities found in the proliferation of criminal activities. As result, we see a positive effect on the overall economic activity. In neighboring municipalities, the severe decline in wages and local prices is amplified by the increase in crime rates and hence, we see a negative effect in the overall economic activity.

4.6 Conclusion

We study the spatial distribution of the effect of oil and gas revenues on Brazilian municipalities. Using variations in the international prices of oil and gas to establish causality, we find that oil and gas revenues increase economic activity in oil-producing municipalities. However, we also observe that oil and gas revenues in a municipality impose a negative spill-over on neighboring municipalities. Both gas and oil revenues contribute to those effects. Moreover, our main finding is not driven by outliers or sample selection and survives various specifications allowing for a dynamic relationship. A placebo test shows that it is not spurious.

The finding that oil and gas revenues benefit oil-producing municipalities is in line with the within-country literature that emphasizes that areas endowed with natural resources tend to benefit from it. In that respect, it shows that oil-producing municipalities do not suffer from a resource curse. However, neighboring municipalities do. Accordingly, oil-producing municipalities impose a negative spillover on their neighbors. We observe that those spillovers become larger relative to direct effects the further from oil production a municipality is located and dominate in municipalities located beyond 150 km from oil activities.

was used by Dix-Carneiro et al. (2018) in estimating the impact of trade shocks on crime rates. We refer to their paper for more discussion on the high correlation between homicides rates and other types of crime at the state level.

Table 4.6: Table 6 – Drivers of economic activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Royalty)	**Log(Population)**	**Real Wages-All Sectors**	**Real Wages-Manufacturing**	**Real Wages-Agriculture**	**Real Wages-Service**	**Local real prices**	**Log(Crime rate)**
Panel A: Estimated Coefficients								
ln(Revenues)	0.694***	0.095***	65.952***	92.514*	-13.064	257.331*	20.297***	0.308***
	(0.149)	(0.012)	(19.32)	(54.469)	(14.261)	(134.875)	(2.543)	(0.041)
W(ln(Revenues))	0.075	-0.007	-26.188	-84.773**	-113.353***	-927.823***	-20.147**	0.352***
	(0.052)	(0.027)	(22.876)	(42.542)	(17.402)	(233.099)	(9.722)	(0.081)
W(Dependent Varibale)	0.012	0.152***	0.111***	0.063***	0.141***	0.218***	0.046*	0.168***
	(0.008)	(0.017)	(0.015)	(0.020)	(0.018)	(0.014)	(0.025)	(0.007)
Number of observations	29,100	42,680	23,286	18,888	20,472	23,280	31,977	42,680
Number of muncipalties	1,940	1,940	1,940	1,574	1,706	1,940	1,683	1,940
R-squared	0.952	0.478	0.866	0.096	0.148	0.359	0.119	0.054
lnPopulation	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Muncipalty FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Direct and Indirect effects from SDM								
Direct Effect	0.695***	0.095***	65.753***	91.711*	-16.865	218.044*	20.089***	0.322***
	(0.150)	(0.012)	(19.314)	(54.610)	(14.125)	(134.620)	(2.509)	(0.042)
Indirect effect	0.081*	0.011	-15.432	-73.906*	-125.665***	-1,022.539***	-20.950**	0.454***
	(0.052)	(0.032)	(24.998)	(41.438)	(18.888)	(270.262)	(8.568)	(0.099)
Total effect	0.777***	0.106***	50.303*	17.805	-142.529***	-804.495***	-0.860	0.776***
	(0.137)	(0.035)	(30.364)	(68.856)	(22.680)	(306.618)	(8.506)	(0.117)

The dependent variable in columns 1 is (log) royalty (in real terms); in column 2 is (log) population size; in columns 3 is real average monthly wages in all sectors; in column 4 is real average monthly wages in manufacturing sector; in column 5 and 6 is real average monthly wages in agriculture and service sectors, respectitively; in column 7 is real prices of locally produced agriculture goods and in column 7 is (log) crime rate (per 100,000 inhabitants). The sample is restricted to municipalities located in 9 oil-producing states. In all columns, hydrocarbon municipalities that have started production after 1992 are set to zero. The method of estimation is a spatial Durbin model (SDM) employing a row standardized contiguity matrix. Estimates are based on a spatial weights contiguity matrix W that assigns 1 to municipalities that share a common border. Robust standard errors reported in parentheses and clustered at the municipal level. Panel B reports estimates of direct, indirect and total effects from SDM, with standard errors computed by Monte Carlo standard errors using 100 replications (Lesage and Pace, 2009). Significantly different from zero at *10% significance, **5% significance, ***1% significance.

When studying how oil and gas revenues affect non-oil municipalities, we confirm that oil and gas revenues increase royalties in neighboring municipalities. They also increase population, real wages, in particular in the manufacturing sector and in agriculture and services, as well as local real prices and crime. Moreover, we observe negative spillovers of oil and gas revenues to wages and prices and positive spillovers on crime in neighboring municipalities. A consequence of finding negative spillovers of oil and gas revenues on non-oil municipalities is that the sum of direct and spillover effects is close to zero. Accordingly, we can observe no effect of oil and gas revenues on economic activity at higher levels of aggregation, as direct and spillover effects cancel out. This finding may explain the conflicting results of the cross-country literature. In any case, they point out to the distributive effects of natural resources. Our results uncover the geographic dimension of those distributive effects. We also find that oil revenues affect wages and prices, which hints at possible distributive effects between factors of production. They warrant future research.

4.A Appendix

4.A1 Oil and gas data

Our data on oil and gas production come from Brazilian Agencia Nacional de Petroleo (ANP), which publishes data on oil and gas production and reference prices by oil field on a monthly basis. The data is extracted from the exploration and production of oil and gas database (Banco de Dados de Exploração e produção de óleo e gás – BDEP). The reference price is the maximum between the actual sale price of the oil extracted in a particular field and an imputed sale price based on prevailing world-market prices for oil with similar chemical composition. These prices are only available from 1999 onward, which is the reason why we use international oil prices, taken from International Financial Statistics (IFS), to allow us to have a longer time series and to match the available data on night-lights. Given that, we calculate the monthly production value for each oil field from 1992 based on the following formula: oil production in 1992 $\times$ international oil price + gas production in 1992 $\times$ international gas price. The value of annual output is then the sum of the year of the monthly output values. Next, we assign these output values to municipalities based on two approaches. The first approach allocates offshore oil values to municipalities using the Percentuais Médios de Confrontação for December 2013, which lists the assigned quotas for municipalities facing offshore oil fields. These quotas show little variation over time, and our results are robust to using quotas from different month, namely July 2007. The second approach assigns onshore oil fields to municipalities. To do that, we first georeferenced onshore fields (that were in production phase in October 2018) using GIS information disclosed by ANP's BDEP database. If oil field is located within the boundaries of only one municipality, its output is fully assigned to this particular municipality. If, on the other hand, an oil field is located over a number of municipalities' boundaries, we divide the oil production based on the percentage of the occupied area by

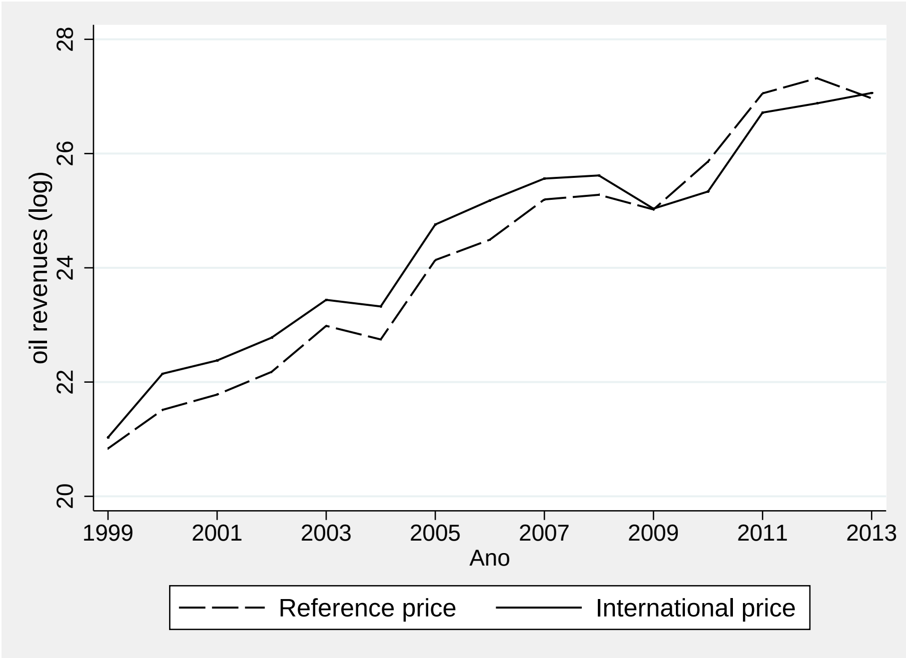


Figure 4.A1: Oil revenues (in Brazilian Real) calculated by Brazilian reference price (dashed line) and international oil prices (solid line)

the oil field in each municipality.

To check we calculation, we compared municipal oil revenues obtained using Brazilian reference prices (in Brazilian Real) with the one we constructed using international oil prices. For the latter series, we converted the values from US dollars to Brazilian Real. Figure 4.A1 plots the evolution of oil revenues from the two series. For the period 1999-2013, the two series were almost the same with the degree of correlation reaching 0.9986.

4.A2 Further robustness checks

Table 4.A1: Further robustness checks

	(1)	(2)	(3)	(4)
	Light	**Light**	**Light**	**Light**
	SDM	**SDM**	**SDM**	**OLS**
	Drop lights > 99th percentile	**Drop capital cities**	**Lagged oil revenues**	**Ring cut offs**
Panel A: Estimated Coefficients				
ln(Revenues)	0.762***	0.682***		0.535**
	(0.205)	(0.212)		(0.223)
W(ln(Revenues))	-0.957***	-0.812**		
	(0.278)	(0.357)		
ln(Revenues), t-2			0.842***	
			(0.215)	
W(ln(Revenues)), t-2			-0.734**	
			(0.370)	
W(light)	0.212***	0.169***	0.174***	
	(0.018)	(0.018)	(0.018)	
Exposure $\leq$ 100				-0.313
				(0.339)
Exposure $\leq$ 200				-0.625**
				(0.303)
Exposure $\leq$ 300				-0.165
				(0.303)
Exposure > 300				-0.637**
				(0.287)
Number of observations	41,844	42,482	38,800	42,680
Number of muncipalties	1,902	1,931	1,940	1,940
R-squared	0.212	0.231	0.243	0.480
lnPopulation	Yes	Yes	Yes	Yes
Muncipalty FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Panel B: Direct and Indirect effects from SDM				
Direct Effect	0.724***	0.657***	0.820***	
	(0.210)	(0.215)	(0.216)	
Indirect effect	-0.987***	-0.848*	-0.703	
	(0.367)	(0.436)	(0.455)	
Total effect	-0.263	-0.191	0.117	
	(0.476)	(0.526)	(0.528)	

Dependent variable is mean night-time light intensity. Columns 1-3 show the estimates of a spatial Durbin model (SDM) employing a row statndardized contiguity matrix. The method of estimation in column 4 is ordinary least sqaures. Robust standard errors reported in parentheses and clustered at the municipal level. Significantly different from zero at *10% significance, **5% significance, ***1% significance.

Autocratic Survival Strategies: Does Oil make a Difference?*

Abstract

This paper examines the behavior of dictators when faced by an imminent threat of being overthrown in oil abundant countries. In the short run, the dictator's survival strategies is argued to be confined to public spending and repression, whereas the choice of their levels is conditional upon the intensity of the mass threat (i.e. civil protest vs. mass violence) and the size of oil wealth. The empirical results indicate a possibility of mixing between spending and repression, and that oil wealth allows for differences in their employed levels in face of the same threat. Using a dataset of authoritarian regimes in 88 countries from 1981 to 2006, I found that mass violence is handled through increasing both spending and repression, whereas civil protest is only met by repression. Furthermore, greater oil wealth is found to provide a wider fiscal space to relatively increase spending, but only at low and intermediate levels of mass threats. As the threats intensify, the effect of oil wealth dissipates and oil wealth dictatorships behave the same as their non-oil wealth counterparts.

Keywords: Autocratic Survival, Oil Rents, Public Spending, Repression

JEL codes: H50, P48, Q34, D74

5.1 Introduction

Recently, there has been a revived interest in exploring the link between oil rents and authoritarian regimes survival, with the bulk of literature suggesting that oil dictatorships are more likely to withstand political power challenges than their non-oil counterparts.[1] With oil proceeds, dictators have the financial privilege that could allow them to neutralize mass threats by doling out resources on public goods, enhancing repressive capacities, providing less freedoms and reducing the need for political institutions (Ross 2001; Ghandi and Przeworski 2007; Mesquita and Smith 2010). However, dictators in general do not always follow the same strategy, when defending their thrones. The leader, acting rationally, will always choose the strategy that allows him to preserve his power at the lowest cost

*This chapter has been published as *Ishak, P. W. (2019). Autocratic Survival Strategies: Does Oil Make a Difference? Peace Economics, Peace Science and Public Policy 25(2), 1-22.*

[1] See Ross 2001; Aslaksen 2010; Ramsay 2011; Tsui 2011; Ayanwu and Erhijakpor 2014; Smith 2004; Ulfelder 2007; Wiens el al. 2014; Wright et al. 2013; Anderson and Aslaksen 2012; Ross 2009; Cuaresma el al. 2010; Morrison 2015.

based on the severity of the political threat and the size of their financial resources, particularly oil wealth.[2] For instance, the variation in the dictators' responses to preeminent threats can be depicted amid the 2011 Arab uprisings. Dictators facing ongoing revolutions or even imminent threats of it embarked different responses. In Tunisia and Egypt, the dictators, upon losing the support of their armed forces, decide to peacefully step down handing the authority to transitional governments guarded by the military. In Syria and Libya, results turned violent with both Bashar al-Assad and Muammar Gaddafi fiercely refusing to concede following the escalating instabilities and vowed instead to resort to force to crush protestors. In contrast, Gulf countries, with the exception of Bahrain, have been able to placate potential opposition through raising state salaries, providing new state jobs, revoking proposed subsidies cuts and offering direct handouts to citizens. In Saudi Arabia, for example, in February and March 2011, King Abdullah announced new spending plans of more than 100 US billion dollars.[3]

Oil driven spending may have allowed leaders in Gulf countries to shield their palaces from mass discontent, but in low oil production states, leaders decided to either step down or vowed to defeat the protesters, which could imply that a higher size of oil rents makes spending more allured to repression. Furthermore, the resolution made by some leaders to step down came as a last resort, after repressive forces, promises to cut prices and to allow more political freedoms have all failed to make protestors withdraw their demands for change.[4] A matter that reveals the existence of short-term survival strategies, mainly spending and repression, as opposed to long-term ones in the form of freedoms or political concessions. Both remarks insatiate an important question: Under which circumstances will a particular survival strategy (-ies) become mostly effective to deter power challenges from below? As such, will a dictator follow the same measures when dealing with a peaceful small-scale protest as when facing a nationwide revolution? In this article, I argue that the resort to one strategy is rather conditional upon the type of posed mass threat, the dictator's time horizon (short run vs. long run) and the size of oil wealth.

Mass threats in dictatorships differ in type and severity, ranging from peaceful gatherings to violent acts of defiance and creating in turn political instabilities. Such instabilities affect the dictator's time horizon, forcing him to take short run responses that could substantially differ from the ones taken over a longer period of time. In the short run, the dictator's options are limited to the use of financial rewards (i.e. spending) and repressing defected groups (Wintrobe, 1998; Annett, 2001; Acemoglu et al., 2000), while in the long run, spending and repression, being financially costly to sustain, could relaxed for a less costly strategies such as suppression of freedoms or agreements for power sharing (Ghandi and Przeworski 2007; Mesquita and Smith 2010; Conrad 2011). For oil dependent dictatorships, a greater oil wealth can have the potential to allow the dictator to adopt different defense mechanism than a dictator with limited or no oil revenues. By relaxing

[2]As it will be explained below, oil wealth refers to the value of oil production in per capita terms. I also use the terms oil wealth, oil abundance and financial resources interchangeably.

[3]see F. Gregory Gause III (2011).

[4]See "Tunisia pushes out its strongman: could other Arab countries follow?" Time, 14 January 2011.

their budget constraints, oil revenues may lead dictators to increase spending and employ less repression.

Such disaggregation of types of the mass threats taking into account the length of the response period and size of oil wealth is widely overlooked in analyzing the dictator's behavior in general and the implications of oil wealth in particular. First, the institutional explanation for dictatorial stability leaves out the length of period available for a dictator to take action. When political instabilities are not taken into account, the relationship is underspecified, for not assessing the appropriateness of institutional reform within a short run horizon. Second, previous cross-country empirical studies estimating the impact of oil rents on spending, as a mechanism for regime durability, ignored the existence of political threats and did not account for repression (or other strategy) as a possible substitute (Ross, 2001; Jenson and Wantchekon, 2004; Morrison, 2009; 2015). Third, using measures for oil dependence (i.e. oil exports as a percentage of GDP) does not allow for testing the distributive and repressive differences among oil dictatorships based on the size of their population.

To the best of my knowledge, there are three studies that specifically analyzed the short term respond to political threats, which is either consumption or repression. The first is the theoretical model of Wintrobe (1998), where he argued that the short run response to a fall in mass loyalty is to repress. However, the type and severity of the mass threat was rather left unspecified in his model. The second study is by Annette (2001), where he found that political instabilities increase public spending to absorb dissidence. But, his indicator for instability was conflating less intense incidences of instabilities with violent events, and does not distinguish elite threats from mass threats by grouping coups with revolutions. As such, it fails to discern the corresponding impact for each threat type on spending, besides depending on cross-sectional analysis, which carries the concern on whether the casual mechanism can be confidently identified. Thirdly, Acemoglu et al. (2000) theoretically focused on the respond of dictators to revolutions via repression. In these studies, the type of state finance in influencing the leader's decision was left analyzed and except for Wintrobe (1998), there has been a mutually exclusive employment of either spending or repression to quench mass threats, rather than allowing for mixing between both depending on the intensity of the threat.

To address these analytic shortcomings and further push the research frontier, this article has two main goals: (1) investigate the dictator's short run response to different mass threats, (2) investigate whether the oil dictators' response shall differ from their non-oil counterparts and among each other, (3) analyze the possibility of mixing between repression and spending. Hence, I first estimate the simultaneous within-country effect of the civil protest and mass violence indices on public spending and repression levels. Then, I tackle the impact of the size of oil wealth on the resulting outcomes. Using time series-cross section data from 88 authoritarian countries between 1981 and 2006 and correcting for simultaneity, I found that mass violence is countered by both more spending and repression, and civil protest is only met by repression. Moreover, greater oil wealth is only found to provide a wider fiscal space to relatively increase spending at low and intermediate levels of threats. However, as threats intensify, the effect of oil wealth dissipates and oil dictatorships behave the same as their non-oil counterparts. Furthermore, oil wealth had no impact on repression, whether when accounting for threats or not, revealing that oil wealth may not alter the repressive stance of dictators and push them to substitute spending for repression.

The outline of this paper is as follows. In the first section, the different types of mass threats that exist in dictatorships are discerned along with the dictator's available short run and long run responses, followed by a discussion on how a dictator can employ spending and repression in responding to mass threats in the short run. Next, I discuss how oil dictatorships could differ from their non-oil counterparts and among themselves in terms of financial endowments, which in turn might influence their corresponding survival strategies. In the third and fourth sections, data and methodology for estimating mass threats indices and the testing of the proposed hypothesis are discussed. Next, the results of employed models and robustness tests are presented and finally section 5.8 concludes.

5.2 Dictatorial threats and survival strategies

Dictators' main aim is to preserve their power in the direst dismal conditions, where power challenges are posed from different actors and on various amplitudes, leaving the dictator in a constant state of anxiously defending his throne (Svolik, 2012). Such situation is commonly referred as "the dictator's dilemma" which arises; when the dictator is not sure whether his people, both mases and elites, genuinely support him or only show a commanded support, especially in the absence of rules for succession and power alteration, leading to an ever present sense of insecurity (Wintrobe, 1998). To ease this mission, the dictator must carefully identify the source and estimate the severity of each political threat, upon which he can determine the suitable type of response. Given this fact, autocrats face two types of threats: those originating from their ruling elites and those coming from the masses (Ghandi and Przeworski, 2007; Ghandi, 2008; Svolik, 2012). Generally, a dictator responds to a specific potential threat with the following strategies or a combination of them: public spending, repression, freedoms and power sharing agreements (e.g. creating legislatures or providing opposition more political freedoms) (Wintrobe, 1998; Ghandi and Przeworski, 2007; Ghandi, 2008; Mesquita and Smith, 2010; Conrad, 2011).

Nevertheless, not all strategies are feasible in the short run. For instance, establishing political institutions as a guarantee for power sharing requires longer periods of negotiations especially in the presence of powerful elites and opposition groups, since they alter the rules of the game and force the dictator to concede part of his powers to keep his throne, a process that unfolds over time and not instantaneously. In other words, the severity of the power threats can shorten the dictator's time horizon pushing him to take short-term actions that differ substantially from long-term strategies that aim at uprooting sources of the threat. Knowing that, it makes sense to differentiate between short run and long run dictators' responses. Following Wintrobe (1998), Annett 2001 and Acemoglu et al. (2000), the most effective strategies to implement in the short run are spending and repression . To reduce the complexity pertaining to having different forms of threats and strategies, the focus of this study would be on disentangling the different forms of mass threats and analyzing only the corresponding short run dictator's response, which is the mix between spending and physical repression.[5]

Bottom up threats stemming from the masses vary in nature and form. This implies that small-scale or peaceful anti-government gatherings like protests and strikes aimed at demonstrating sentiments of discontent with the function of the government should be distinguished from more organized violent acts of defiance. In this regard, Conrad (2011) and Vreeland (2008) emphasized the role of the organization of masses in influencing the dictator's respond, where more organized defected groups are met by more spending and more repression. Protests can show up spontaneously with leaderless masses suddenly taking into the streets at their own discretion. Hence, the leader's short run response shall be an immediate resort to force, since the protests are rather diffused and has little effect on the longevity of the regime (Wintrobe, 1998; Conrad, 2011). Repression has in this context an effective deterrence effect, since it discourages other groups from taking similar discourses.[6]

Mass violence such as revolutions, on the other hand, requires more organized groups to defy the state. The immediate resort to repression in this case has eminent consequences. On one hand, engaging in state violence could lead to an escalation of more violent acts against the state that can be perceived as a sign of regime incompetence and provoke military coups to bring stability back (Wintrobe, 1998; Escribà-Folch, 2013). On the other hand, once mass violence erupts, it means that participators have already solved to manage their collective action problems (Escribà-Folch, 2013). Alternatively, the dictator can respond in a divide and conquer fashion by increasing public spending to placate public support for anti-state groups and trying to stimulate the acquiescence of their leaders with material spoils (Annett, 2001; Mesquita and Smith, 2010). A third strategy could entail a mix between repression and spending, such that both spending and repression are allowed to increase in respond to mass violence. For instance, more repression can be directed to specific defected groups such as armed factions, while more spending is doled on other sectors to uproot potential support to these groups. Being financially costly though subject spending and repression to trade-offs, such that the employment of one strategy causes a change in the level of provision for the other especially if the dictator's is running under budget constraints (Wintrobe, 1998; Conrad, 2011; Caselli and Cunningham, 2009).

In sum, power conflicts are always present in dictatorships and in order for the leader

[5]A third possible strategy is freedom concessions, which will considered in the robustness checks in relation to spending and repression.

[6]As perfectly put by Wintrobe (1998) "the optimal response to a fall in loyalty is to expand repression in the short run". In this regard, I make no distinction between tinpots' and totalitarians' short run responses to a sudden fall in loyalty, as depicted in Wintrobe's model. Since my argument is based on the intensity of the threat (i.e. negative shock) rather than on the type of dictator, a thing that was not tackled in Wintrobe's model.

to defend his palace, he must carefully define the source of threat and evaluate its severity. However, in contrast to previous studies (e.g. Wintrobe, 1998; Acemoglu et al., 2000; Mesquita and Smith, 2010), where is always a clear cut a mutually exclusive employing of either spending or repression as a respond to mass threats, I argue here that there is also a possibility of mixing between both when the intensity of the threat is taken into account. Thus, not all resources are devoted to repression and the likelihood of doling out more resources on repression should never count out the possibility of increasing spending depending on the targeted group. Hence, depending on the intensity of the threat, the above discussion suggests the following testable hypotheses:

Hypothesis 1: In the short run, a dictator responds to civil protests with relatively less public spending and more repression independent of the size of oil wealth.

Hypothesis 2: In the short run, a dictator responds to mass violence with relatively more public spending and more repression independent of the size of oil wealth.

5.3 Oil and autocratic survival

Dictators in oil-endowed countries resemble their opponents in other dictatorships, with regard to the constant present of power threats and the type of strategies employed to eliminate power challengers. However, they differ in the size of the financial resources available at their disposal, mainly the oil wealth, providing them the potential to behave differently in the short run in response to mass erupts. In other words, heterogeneities in the employed levels of repression and spending may exist based on the size of oil wealth.

As postulated by the rentier state theory, oil autocracies deploy oil proceedings in "buying off political consensus", hence obviating the need of the state to resort to taxation to generate the revenues required to reward their supporters. Such policy propels state autonomy and waives state accountability obligations in terms of setting up political institutions or allowing civil freedoms (Beblawi and Luciani, 1987; Mahdavy, 1970; Ghandi, 2008; Mesquita and Smith, 2010). The result is enclaved distributive economies residing upon capital-intensive sectors and having few forward and backward linkages with other sectors that eventually spur the consequent revenues on their population (Dunning, 2008).[7] Given that, in order to preserve their grip on power, oil leaders generally depend on two main strategies: repression and cooptation (Wintrobe, 1998). On one hand, large investment in military and internal security apparatus shield the regime from possible popular upheavals (Ross, 2001). On the other hand, engaging in wide spread public spending can quench popular demands for political change (Wright el al., 2013). Likewise, relying on a repressive security apparatus is costly. In their study, Acemoglu et al. (2010) argue that natural resources can equip the leader to finance repression and thus increase likelihood of regime survival. Conversely, military can stage a coup against the current

[7]Empirical studies managed to find that oil pushes downward the levels of democracy, or similarly increase authoritarian levels, and such impact is driven by large scale distribution rather than repression (Ross, 2001; Jenson and Wantchekon, 2004; Aslaksen, 2010; Ramsay, 2011; Tsui, 2011; Ayanwu and Erhijakpor, 2014). Other studies (Smith, 2004; Ulfelder, 2007; Wiens el al., 2014; Wright et al., 2013; Anderson and Aslaksen, 2012; Ross, 2009; Cuaresma el al., 2010; Morrison, 2015).

leader to install their military dictatorship and hence reduces chances of regime durability. However, the key advantage of having oil rents is that it can mediate the negative consequences of resorting to repression. Having a larger fiscal space can relax the dictator's budget constraint and allows for substituting public spending for force to tackle mass dissidence, thus making the former a more effective option in the short run and reduces the likelihood of violence escalation (Elbadawi and Makdisi, 2013).

Yet, not all oil dictatorships are similar, for heterogeneities also exist among them in terms of the extent of oil wealth given the size of their population. Thus, although a country might be highly dependent on oil in terms of the percentage of oil exports to its total exports, or GDP, having big populations could dampen the country's total oil wealth per capita and in turn its distributive abilities. So, higher resource dependence is not equivalent to higher wealth. In their study, Basedau and Lacher (2006) found countries with high oil revenues per capita are more stable, experience less political instability than countries with low oil revenues per capita, even if both share the same level of oil dependence. Accordingly, a dictator resting on large oil wealth and facing low population, can afford spending on public goods, while maintaining a lower level of repression. In other words, the larger the size of oil resources per capita, the better off the authoritarian leaders, since higher resources can be distributed to their smaller population, thereby deterring incentives of rebellion and making repression not their first favored option (Elbadawi and Makdisi, 2013).[8] To conclude, differences in the financial privileges between oil-endowed and non-oil autocracies as well as within oil autocracies themselves may influence the dictator's behavior towards social upheavals in the short run in terms of the employed levels of spending and repression. Hence, taking into account the size of oil wealth, the following hypotheses are tested:

Hypothesis 3: A greater size of oil wealth increases spending and reduce repression, independent of the size of mass threats.

Hypothesis 4: In the short run, an oil dictator responds to civil protests by higher public spending and lower repression relative to non-oil dictator.

Hypothesis 5: In the short run, an oil dictator responds to mass violence by higher public spending and lower repression relative to non-oil dictator.

5.4 Mass threats in dictatorships: Measurement and estimated indices

Mass threats in this study are defined as "any peaceful or violent mass attempts aimed at overthrowing the head of the state". Hence, unlike previous empirical literature on political instabilities (Cukierman et al,. 1992; Alesina et al., 1996), the propensity of

[8] The idea that a higher oil wealth per capita can be associated with more redistribution is analogues to Bueno de Mesquita et al. (2003) theory on the ruler's choice between providing private or public goods. The choice is based on the relative size of the wining coalition to the selectorate. The bigger the selectorate (in our case the population) relative to the winning coalition (in our case the elites), the more costly is to provide public goods to the whole population, and the ruler will opt for more private goods distribution to his selected supporters.

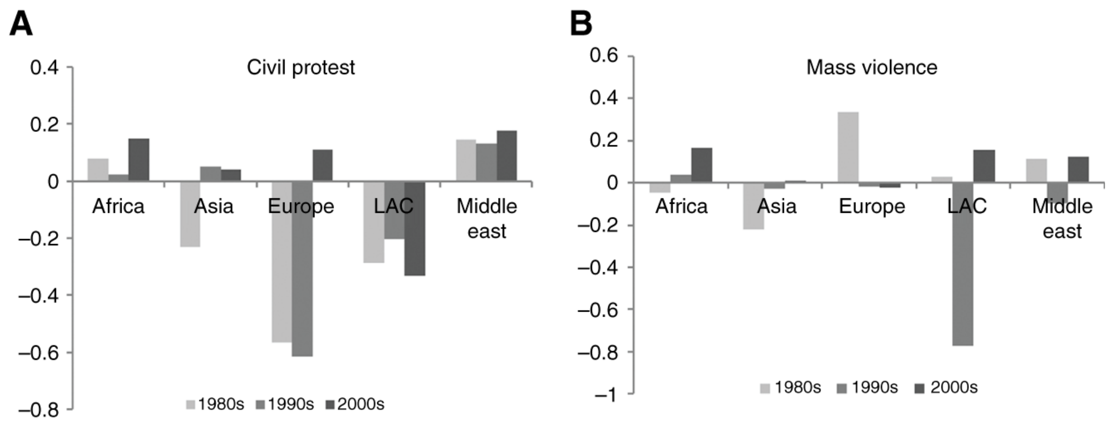


Figure 5.1: Social stability across and within regions

observing a leader turnover is not necessary, but only an event that threatens to bring down the dictator is sufficient. Building on this logic, an index for political instabilities is constructed to capture different dimensions of mass threats, mainly their less and more violent forms, and allowing to disentangle various factors that could endanger the survival of a dictator (Vaniers and Gupta, 1986; Hibbs, 1973). Therefore, instances that reflect elite's defections and internal palace attempts to topple the leader such as cabinet shuffles and coups are excluded (Jong-A-Pin, 2009). Accordingly, the following indicators were selected: assassinations, general strikes, guerrilla warfare, riots, revolutions, anti-government demonstrations, civil war and minor civil conflicts. Data sources are Cross-National Time-Series Data Archive (CNTS) taken from Databanks International (Banks, 2018) and International Peace Research Institute (Gleditsch et al., 2002). Based on data availability and in align with other employed indicators, the period of analysis covers the years 1981-2006.[9]

Each indicator on its own constitute an imperfect reflection of mass threats. For instance, a country could be rendered stable in a certain period due to the lack of civil wars. However, when taking into account the frequent occurrence of strikes and riots, the picture can dramatically change. In my model, it is necessary to differentiate between the severity of social disruptions that would force the dictator to resort to either public spending or repression and to group together factors of similar nature. In contrast to previous studies in which principal component analysis (PCA) was used (Alesina and Perotti, 1996; Perotti, 1996; Blanco and Grier, 2009), I employ instead explanatory factor analysis (EFA) to build the index (Annett, 2001; Jong-A-Pin, 2009). EFA differs in the sense that it assumes an underlying model that could explain the unobserved latent variable (in our case mass threats) and extracts only common variance to the observed indicators taking into account the existence of unique variance that pertains only to the single indicator. Definitions of the used indicators and the operationalization of explanatory factor analysis are found in appendices 5.A1 and 5.A2.

The results of the factor analysis give rise to two indices of political instabilities steam-

[9]The period of analysis is restricted to 2006, because of the data availability of UCDP/PRIO Armed Conflict Dataset 3.1 covering incidences of civil strife (Gleditsch et al., 2002).

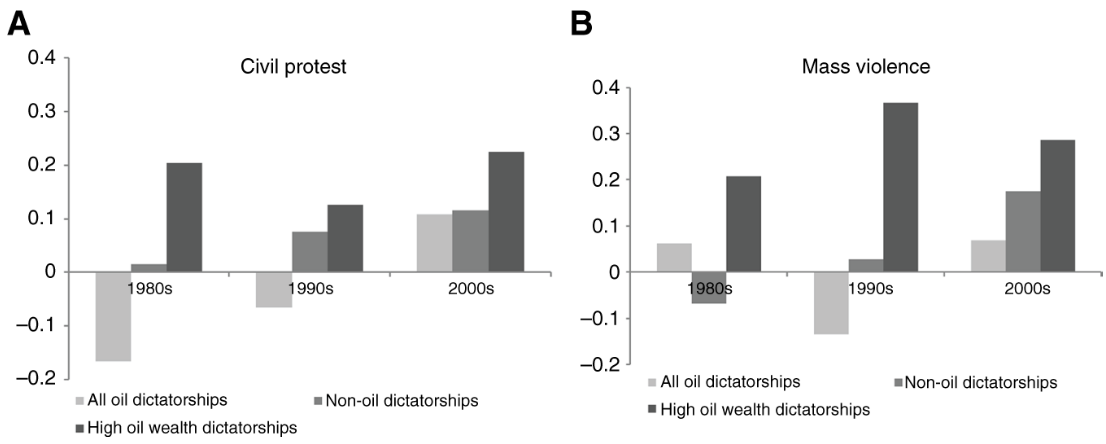


Figure 5.2: Social stability across oil and non-oil dictatorships

ing from the populace during the period (1981-2006), namely civil protest and mass violence. The two indices exhibit low correlation among them, which indicate that each corresponds to a different dimension of political instability, despite the fact that the correlation coefficient is significantly different from 0 (see Table 5A2 in appendix 5.A2). To ease interpretation, the scores of the two indices are multiplied by minus 1 so that higher values would indicate more stability. For further insights, Figure 5.1 outlines the indices scores across world regions based on decade-average scores.[10] It indeed shows that the state of political stability differs across different parts of the world and even within regions overtime. It also shows that a region could show more stability on one dimension relative to the other. For instance, the most stable region is appears to be the Middle East in terms of civil protest based on average score over the three decades, but it was replaced by Europe in case of mass violence.

To get a first snapshot of the relationship between oil and stability, Figure 5.2 classifies countries into oil producing dictatorships, non-oil dictatorships and richest quartile of oil producing dictatorships in per capita terms (i.e. oil rich low population). Oil dictatorships refer to countries combing between active oil production and authoritarian regimes. Countries are classified to be "High oil wealth" countries, if the whole-period-average of their oil wealth corresponds to the highest 3rd quartile (73 percentile).[11]

5.5 Empirical strategy

5.5.1 Model specification

To examine the effect of mass threats and oil endowments on the different political paths a dictator can take to foster his throne, the preceding hypotheses regarding public spending and state repression as possible mechanisms to nullify existing threats in the short run are tested. I start with two separate dynamic linear regression models with country and

[10] Only the last period is truncated to 2006. The regions are: Middle East and North Africa, Latin America, Asia, Sub-Saharan Africa, and Europe.

[11] This corresponds to a monetary value of current US$1000 or more in per capita terms. The same cutoff points were given by Cammett et al. (2015) and Basedau and Lacher (2006).

year fixed effects. Our dependent variables are the levels of public spending and repression regressed over the two indices of mass threats along with the measure for oil wealth (per capita):

$$Spending_{it} = \alpha_i + \delta_t + \beta_1 MT_{it-1} + \beta_2 Oil_{it-2} + \beta_3 X_{1,it} + \epsilon_{1,it} \tag{5.1}$$

$$Repression_{it} = \alpha_i + \delta_t + \beta_1 MT_{it-1} + \beta_2 Oil_{it-2} + \beta_3 X_{2,it} + \epsilon_{2,it} \tag{5.2}$$

Where α_i and δ_t are country and year fixed effects, MT is the two constructed mass threats indices in country i and year t, Oil is the measure for a country's oil wealth in per capita terms, X is a set of all other control variables that may affect the two dependent variables and ϵ_{it} is an error term. As previously indicated, it is also possible that the dictator can resort to both tools at the same time depending on the severity of the threat, hence simultaneous changes in levels of spending and repression can occur. In addition, being both financially costly imply that the employment of one strategy necessitates a change in the provision of the others, hence the two survival strategies are deemed to be interdependent (Conrad, 2011). To capture this interdependence and correct for simultaneity, I estimate a simultaneous equation model (SEM) in which the two endogenous variables spending and repression shall appear as the dependent variable in one equation, while being included as additional explanatory variables in the other two equations. The structural equations for SEM take the following form:

$$Spending_{it} = \alpha_i + \delta_t + \beta_1 Z_{1,it} + \gamma_1 Rep_{it} + \epsilon_{1,it} \tag{5.3}$$

$$Repression_{it} = \alpha_i + \delta_t + \beta_2 Z_{2,it} + \gamma_2 Spend_{it} + \epsilon_{2,it} \tag{5.4}$$

where Z_1 and Z_2 are the set of covariates that affect each dependent variable including MT indices and Oil. This system of equations is estimated using least squares two-stage instrumental-variables regression with country and year fixed effects, where each of the two endogenous spending and repression variables is instrumented by their lagged levels. Since it is a dynamic simultaneous system, there might be some concern regarding a potential suffering from Nickell bias (Nickell, 1981), which arises when a lagged dependent variable is included along with OLS fixed-effects in large N-small T samples, causing correlation between the former and the error term. To address this concern, the system of equations is also estimated using system-GMM (Blundell & Bond, 1998). To illustrate, in the first stage, the below equations are estimated using all the covariates (Z_1, Z_2) from equations 5.3 and 5.4 to get the fitted values for *Spend* and *Rep*:

$$\widehat{Spending}_{it} = \alpha_1 Z_{1,it} + \alpha_2 Z_{2,it} + \epsilon_{4,it} \tag{5.5}$$

$$\widehat{Repression}_{it} = \alpha_3 Z_{1,it} + \alpha_4 Z_{2,it} + \epsilon_{5,it} \tag{5.6}$$

In the second stage, the fitted values obtained from equations 5.5 and 5.6 are then substituted for *Spend* and *Rep* in equations 5.3 and 5.4 to get unbiased estimates of γ_1 and γ_1. The identification of this system requires specific instruments for spending and repression to be included in one equation but excluded from the other, Z_1 and Z_2. These specific instruments are the lagged levels of spending and repression.

Recognizing that a potential feedback effect might exist in the sense that instances of mass threats may drive leaders to increase spending and repression to prevent an escalation of the situation and at the same time, higher spending and repression can bring stabilization to the regime. This in turn renders mass threats indices to become endogenous and allows the causality to work in either direction.[12] Adding to that, oil wealth as a function of the ability to extract and distribute oil can be affected by popular uprisings which may cause disruption in oil production and adversely affect the proceeding rents. Furthermore, a dictator facing ongoing instabilities may want to increase oil production to maximize his revenues (Wright el al., 2013). Both incidences make oil wealth endogenous as well. To address that, the MT indices are lagged by one year, whereas the oil variable is two-years lagged to help reduce the reverse feedback effect and allows for possible lags in the reaction of the dictator to political threats. Finally, to test the hypotheses that oil dictatorships can behave differently to non-oil dictators in face of mass threats depending on the size of oil wealth, each of the MT indices is interacted with oil wealth to examine whether the impact of oil wealth on spending and repression, and so the dictator's response, varies with the intensity of mass threats.[13]

5.5.2 Data and descriptive analysis

I use time series-cross sectional (TSCS) data on autocratic regimes and the type of leadership from Cheibub, Ghandi and Vreelands (CGV) (2010). CGV distinguishes between different types of dictatorships based on the characteristics of their leaders, namely civilian, military or monarch. The main sample consists of 88 countries for the years 1981 until 2006.[14] Periods of major civil war, foreign occupation or the collapse of state authority are excluded on the grounds of failing to be qualified as either democratic or dictatorship. I identify these periods using the dataset on autocratic breakdown and regime transition from Geddes, Wright and Frantz (GWF, 2014), whereas cases of civil wars are not excluded

[12] Annett (2001); Escribà-Folch (2013).

[13] Political instabilities in a country could be driven by many causes (e.g. inequality, income shocks. . . .etc.) and in this model, the focus is on the existence of instabilities rather than investigating the causes. Hence, political instabilities are not instrumented. However, to account for periods when low oil prices negatively affects state revenues and the dictator's ability to buy off opposition, in the robustness checks I restrict the sample to only oil bust years.

[14] The original CGV dataset consists of around 130 autocratic regimes. Unfortunately, I only managed to include 88 countries given the lack of data on consumption and repression for the dropped countries during the sample period. The excluded countries compromise some of the major oil producers including Saudi Arabia, Libya, Angola and Equatorial Guinea. Although this might get us into a potential selection bias, I argue that this shall not of a major concern given that most of the dropped countries (28 out of 42 countries) has experienced an uninterrupted autocratic rule during the sampled period and score very low levels of political instabilities. Nevertheless, I acknowledge the fact that the unavailability of data for some countries presents a serious limitation to this paper.

if the governor still holds grip on most of his territory.[15] Table 5.1 shows the descriptive analysis for the variables from the baseline specification.

Table 5.1: Summary statistics

Variable	N	Mean	SD	Min	Max
Civil Protest Index	1294	0.03	0.99	-0.27	15.52
Mass Violence Index	1294	-0.04	0.83	-0.49	5.88
Oil wealth per capita (in current US $)	1298	572.72	1812.78	0	18115.42
Oil wealth per capita (Log)	1298	2.52	3.02	0	9.80
Public spending (Log)	1298	20.88	1.88	16.58	26.67
Repression (inverse)	1298	3.92	2.02	0	8
GDP per capita (Log)	1298	7.43	1.27	4.88	11.11
All Dictatorships	26	86.38	12.21	70.00	108.00
Oil Dictatorships	26	46.69	2.85	42.00	51.00
OilDictatorships (%)	26	54.68	4.92	47.17	61.11
Oil Dictotrships (annual change)	25	-0.28	1.57	-4	3

On average, oil dictatorships constitute around 54.7 percent of all sample dictatorships and over the whole period, the share ranged between 47.2 percent and 61.1 percent. The majority of oil dictators has, on average, a civilian background (49.3 percent) followed by military personnel (29.2 percent) and finally, monarchy (21.5 percent). The observed number of dictatorships has relatively high standard deviation of 11 points, while number of oil dictatorships shows a low variation of 2.7 points. This is confirmed in Figure 5.3, which traces the evolution of dictatorships over the sample period and shows that while dictatorships in general were witnessing a steady decline in number, oil dictatorships posed resilient with average change hovering around -0.28 points. Summary statistics presented in Table 5.1 show that the average oil revenues per capita is around 573 current US dollars, with very large cross-country and time variation. If we restrict the sample to only oil producing countries, the average oil rents per capita jumps to 1614 current US dollars. The majority of dictatorships are associated with oil production. In 1068 out of 1298 dictator-year observation, a non-zero value of per capita oil revenues is observed. The maximum value of oil rents per capita is scored by Qatar.

To examine the effect of spending and repression as mechanisms to cool off mass political threats, Figures 5.4 and 5.5 depict the change in the levels of spending and repression along with the average score of both mass threat indices in a number of countries. In Nigeria and Algeria (Figure 5.4), the increase in the level of threats occurred parallel to the decline in government spending, whereas their consequent fall was witnessed shortly after the boost in spending. A close co-movement can also be seen between threats and repression. The rise in repression goes hand in hand with the upsurge in the threats level in both countries. In Indonesia (Figure 5.5), the steady rise in spending kept the level of threats low. However, once a sudden fall in spending was recorded, threats aggravated and at the same time, repression picked up. Gabon has witnessed little instabilities all over the whole period, but the uptick in threats followed the steady decline in spending and was brought back down by increased repression. The latter remained quite high even when

[15]For example Bosnia (1992-1995) was at a state of civil war, thus lacking 'any sovereign political authority' (Svolik, 2012).

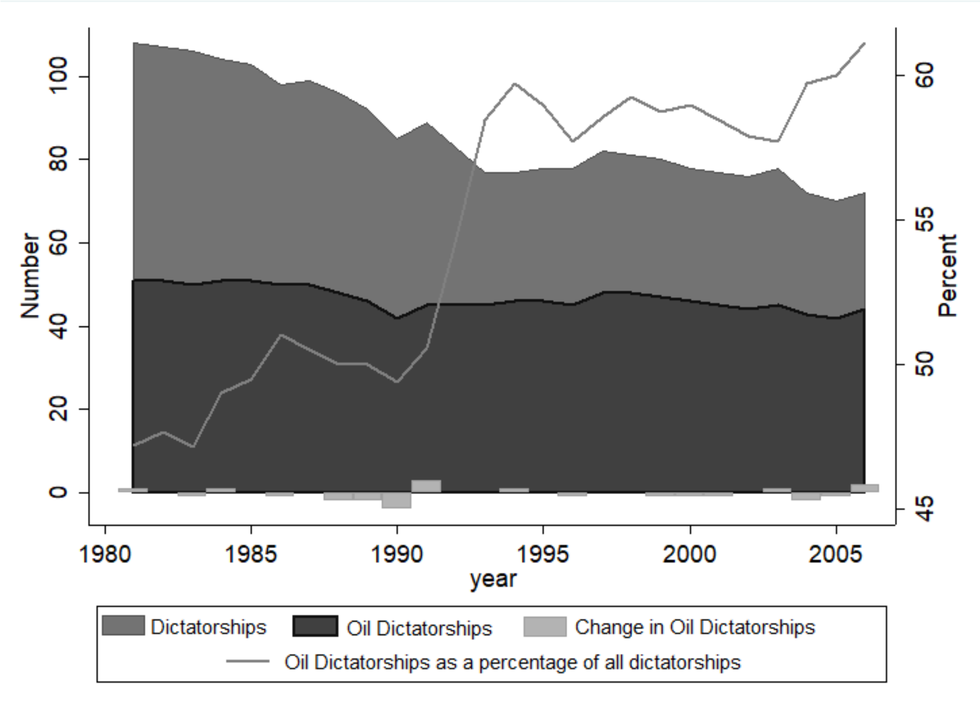


Figure 5.3: The number and change in oil dictatorships, 1981-2006

the spending started to rise again.

To measure public spending, an indicator that captures only current state spending on public services, such as health, education, infrastructure…etc., should be the one of interest. Current expenditures are presumed to be a first choice for a dictator to correct for political failures, since dictators have full control over state funds allocations and because current spending have an immediate effect on cooling off disgruntled demands especially during times of critical unrest, unlike capital expenditures which require time to show an impact. To measure that, the "General government final consumption (current US Dollars)" indicator obtained from World Bank's World Development Indicators (WDI, 2016) is used. It covers all government current expenditures including expenditure on national defense and security, but exclude military capital expenditure. State Repression is measured by Physical integrity index (CIRI, 2014) build up from torture, extrajudicial killing, political imprisonment and disappearance indicators, and ranging from 0 (no respect) to 8 (full respect). The index is rescaled so that 0 would then indicate no government repression and 8 indicate highest government repression.

The main independent variables are the composite index of mass threats divided into two sub-indices and oil income per capita. To measure oil, I rely on (log) total oil income per capita data (measured in current US Dollars) taken from Haber and Menaldo dataset (2011). This measure is obtained by multiplying the level of oil production by world oil prices and then dividing by the number of population. The explanatory power of this measure allows for assessing whether the responses of oil producing dictators differ from non-oil producing and as well as within oil producers themselves. By incorporating the size of the population, it evaluates in turn the distributive capabilities of oil producing

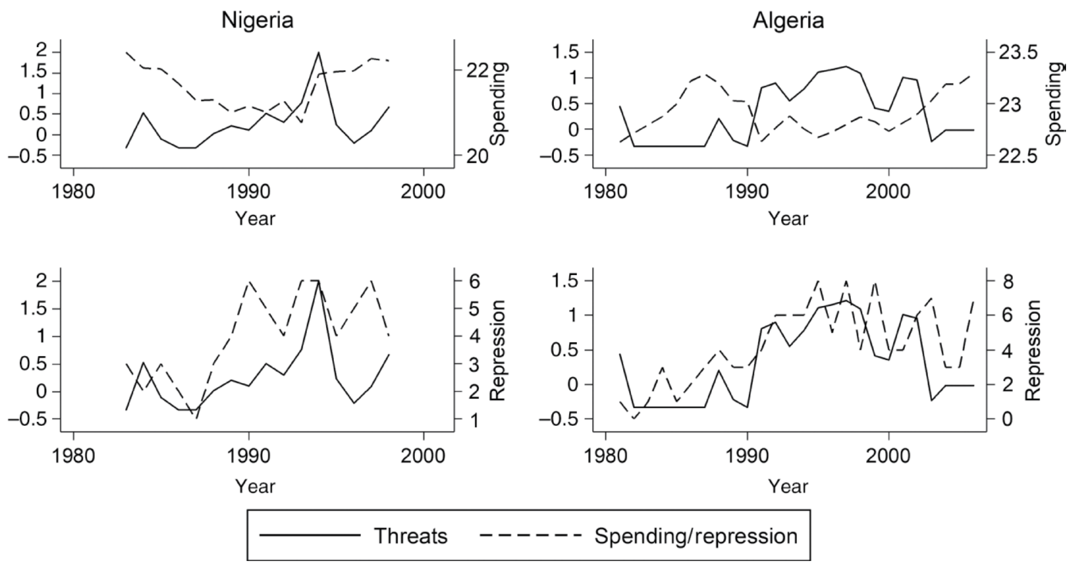


Figure 5.4: Mass threats, spending and repression

countries.[16] Finally, because the ability of the leader to finance public spending depends on the overall economic environment, it is expected that wealthier countries are better equipped to increase consumption spoils to their populace.[17] Thus, a control variable for country's wealth measured by GDP per capita is included (WDI, 2016).[18]

5.6 Empirical results

Table 5.2 reports the results of two separate fixed-effects models with public spending and repression as dependent variables. The coefficients for mass violence and civil protest are only positive and statistically significant in the repression models. Hence, ignoring the trade-offs between strategies suggest that dictators are more likely in the short run to resort to repression irrespective of the type of the threat. Oil wealth, on the other hand, has a statistically significant positive effect on spending only. This comes in line with Morrison (2015), who found a positive relationship between non-tax revenue, including oil revenues, and government spending.[19] The results are quite close across the OLS and system-GMM estimations.

To account for the simultaneous changes in both spending and repression given the

[16] To address endogeneity issues, I also consider using oil reserves and time-invariant measure for the level of oil production as alternative measures for oil wealth.

[17] To give further insights on the distinct conditional effects of oil wealth and GDP on the trade-off between repression and consumption, I report in the appendix 5.A3 the impact of political instabilities conditional upon the initial level of the GDP, while controlling for oil wealth.

[18] A high correlation was found between (log) oil wealth per capita and (log) GDP per capita of about 70% along with a high variance inflation error (VIF) of 90 points, causing the model to suffer from multicollinearity and reducing the precision of the estimated coefficients. Such high correlation is expected to exist since oil wealth contributes greatly to a country's economic growth, especially in non-developing countries. Hence, GDP per capita is used instead of its natural log (52% correlation and VIF is of 2 points). It should be noted that dropping GDP per capita leave our main results unchanged (results available upon request).

[19] See also Ross, 2001; Smith, 2004; Jenson and Wantchekon 2004; Basedau and Lacher, 2006; Aslaksen, 2010; Ramsay, 2011; Tsui 2011; Ulfelder, 2007.

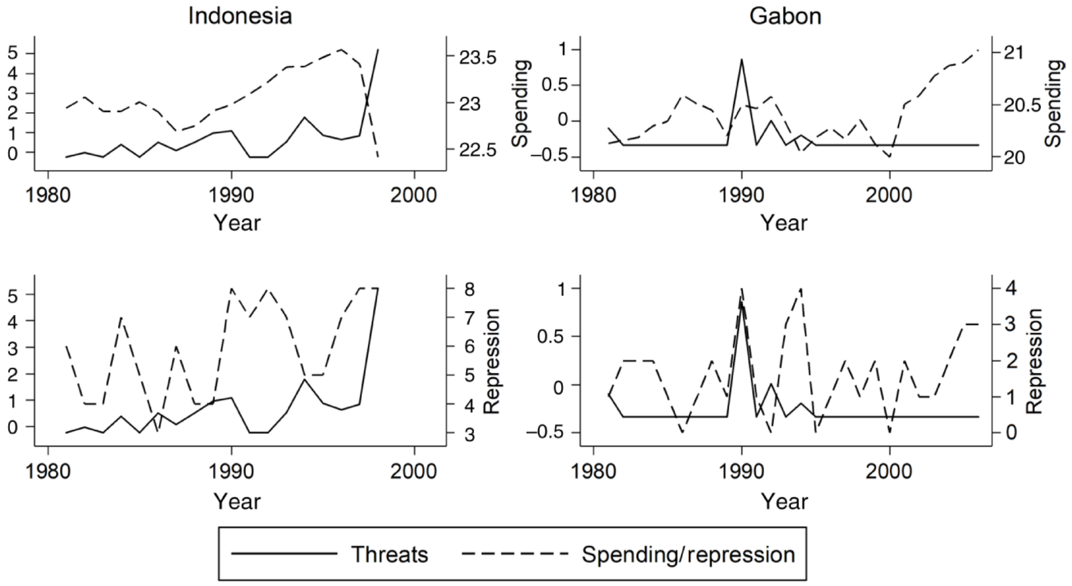


Figure 5.5: Mass threats, spending and repression

presence of mass threats and test the cross-tradeoffs, Table 5.3 shows the results for the two-stage equation model estimated by least squares and system-GMM with spending and repression instrumented by their lagged levels. Column 1 reports the estimates for the average effect of mass violence on spending, which is positive and statistically significant, while the coefficient for civil protest is positive but weakly significant (let alone not robust) and with much smaller magnitude than mass violence. This suggests that, independent of oil wealth, dictators are more likely in the short run to dole out more resources in terms of spending and repression when confronted with violent groups than when dealing with peaceful or small-scale acts of disobedience, confirming Annett's (2001) results that state also increases spending in face of political unrest. Based on column 2, the size of the effect on spending implies that one-point standard deviation increase in mass violence increases the log of spending by 0.02 points in the short run and by around 0.11 points in the long run. Having positive and significant point estimates of oil wealth suggests that higher oil wealth per capita is associated with higher spending. A higher level of oil wealth may therefore allow for directing domestic consumption towards buying political consent. For instance, a one-point standard deviation increase in the log of oil revenues leads to a 0.09 point increase in the log of government spending in the short run and 0.47 point increase in the long run.

In the repression models, the corresponding effects of both indices in column 4 are quite high, indicating that dictators do not hesitate in using force to root out potential threats independent of their severity and source and more repression is shown in face of organized violence than protest (Wintrobe, 1998; Vreeland, 2008). For instance in column 4, a one-point increase in violence leads to 0.23-point increase in repression in the short run and around 0.59-point increase in the long run, compared to a short run 0.17-point increase in response to civil protest and a long run 0.45-point. When it comes to oil wealth, it had on average a positive but insignificant effect on repression, a result that could reflect that leaders in oil wealthy countries do not differ from other dictators when it comes to the use of repression independent of type of threats. This partially coincides with Smith (2004) findings that repression do not fully account for explaining regime durability in oil dictatorships, for these countries employ the same level of repression as the rest of dictatorships. Columns 1 and 4 also reports the first-stage F-statistic for both models; the statistic is around 76 and 606 for spending and repression respectively, which is above the threshold of 10 recommended by Staiger and Stock(1997), indicating that weak instruments does not represent a concern. Interestingly to see that there is a statistically

Table 5.2: Effect of mass threats and oil wealth on public spending and repression I

	Spending		**Repression**	
	(1)	**(2)**	**(3)**	**(4)**
	OLS	**SYS-GMM**	**OLS**	**SYS-GMM**
Civil Protest, t-1	0.002	0.028	0.173**	0.261***
	(0.005)	(0.021)	(0.067)	(0.059)
Mass Violence, t-1	0.006	0.017	0.227**	0.421***
	(0.009)	(0.012)	(0.092)	(0.134)
Oil Per capita (log), t-2	0.026**	0.021*	0.016	0.075
	(0.012)	(0.014)	(0.068)	(0.049)
GDP per capita, t-1	0.0001**	0.0001	-0.0001	-0.0001***
	(0.0001)	(0.0001)	(0.000)	(0.0001)
Dependent Variable, t-1	0.818***	0.949***	0.390***	0.486***
	(0.034)	(0.033)	(0.044)	(0.127)
AR(1)		0.00		0.00
AR(2)		0.367		0.983
Hansen overid test, p-value		0.435		0.269
Number of observations	1296	1296	1296	1296
Adjusted R2	0.793		0.285	
Number of Countries	88	88	88	88
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The method of estimation in columns 1 and 3 is least squares; columns 2 and 4 is system-GMM; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. In column 2, the lagged dependent variable is instrumented by only its second lag at the differenced equation and its first lag at the level equation. In column 3, the lagged dependent variable is instrumented by only its third lag at the difference equation and second lag at the level equation. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

significant negative association between repression and spending in the spending referring to the financial constraints entailing the provision of both strategies. In fact, a 1-point standard deviation increase in repression causes log of spending to reduce by 0.07 points in the short run and by 0.36 points in the long run. In contrast, the coefficient of spending in the repression model is negative, but statistically insignificant. This suggests that repression can crowd out the financial resources available to support public spending, whereas public spending does not seem to reduce the level of repression employed. In other words, repression can substitute spending, but spending can complement repression. Other results show that GDP per capita is positively significant in the spending model, meaning that dictators having more resources at their disposal can use them to solidify their rule by increasing consumption (Wintrobe, 1998). Finally, the results indicate that spending and repression have reached their steady state levels (i.e. positive estimates), with the variation in their current levels being highly explained by their previous year's levels. To test whether the size of the oil wealth relatively affects the spending and repression levels across different types and levels of threats, the (log) oil wealth is then interacted

Table 5.3: Effect of mass threats and oil wealth on public spending and repression II

	Spending			**Repression**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	IV-2SLS	**IV-2SLS**	**SYS-GMM**	**IV-2SLS**	**IV-2SLS**	**SYS-GMM**
Civil Protest, t-1	0.011*	0.012	0.027*	0.174***	0.118**	0.388**
	(0.006)	(0.009)	(0.016)	(0.066)	(0.058)	(0.151)
Mass Violence, t-1	0.022**	0.021*	0.020*	0.230***	0.235***	0.553***
	(0.010)	(0.011)	(0.011)	(0.088)	(0.073)	(0.135)
Oil Per capita (log), t-2	0.028**	0.028**	0.017**	0.024	0.041	0.120
	(0.013)	(0.012)	(0.008)	(0.071)	(0.074)	(0.115)
Civil Protest*Oil per capita		0.001	0.001		-0.046*	-0.003
		(0.004)	(0.004)		(0.024)	(0.031)
Mass Violence*Oil per capita		0.002	0.005		-0.038	-0.027
		(0.004)	(0.004)		(0.024)	(0.043)
GDP per capita, t-1	0.0001**	0.0001**	0.0001	-0.0001	-0.0001	-0.0001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Spending, t				-0.093	-0.078	-0.123
				(0.156)	(0.149)	(0.285)
Repression, t	-0.035***	-0.034***	-0.006			
	(0.012)	(0.013)	(0.006)			
Dependent Variable, t-1	0.811***	0.810***	0.957***	0.387***	0.376***	0.341***
	(0.031)	(0.031)	(0.021)	(0.044)	(0.042)	(0.072)
First-stage F-statistic	75.72	77.62		606.46	606.18	
AR(1)			0.000			0.000
AR(2)			0.383			0.606
Hansen overid test, p-value			0.990			0.204
Joint F-statistic, p-value		0.81	0.34		0.09	0.81
Number of observations	1,296	1,296	1,296	1,296	1,296	1,296
Number of countries	88	88	88	88	88	88
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

The method of estimation in columns 1, 2, 4 1nd 5 is two-stage least squares; columns 3 and 6 is two-stage GMM; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log), t-2 in columns 2, 3, 5 and 6 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. In column 3, the lagged dependent variable is instrumented by its second lag at the differenced equation and its first lag at the level equation. In column 6, the lagged dependent variable is instrumented by all lags at level and differenced equation and "collapse" command is used to reduce the instrument set. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

with indices of civil protest and mass violence. To take in account the variation within the oil producing countries and to ease interpretation, the (log) oil wealth is calculated as the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. By this construction, we are now looking at the effect of political instabilities in an average oil producing country. The two-stage least squares results are shown in Table 3, columns 2 and 5. In the spending model, the coefficients of the main variables remained almost unchanged, both in significance and magnitude, except for civil protest, which posed insignificant. Hence, civil protests are less likely to be countered by more spending suggesting that using state coffers to satisfy protesters is seen to be a costly strategy (Wintrobe, 1998; Conrad, 2011). The conditioning variables were of expected positive sign but statistically insignificant. Similarly was the case in the repression model, with the conditioning variables having an expected negative signs, but either weakly or statistically insignificant point estimates. The F-test of the joint significance of the interaction terms fail to reject the null hypothesis that both terms are not jointly different from zero in the spending model (P-value==0.81), while in the repression model, the null hypothesis is barely rejected at 10 percent significance level model (P-value==0.09). The size of the effect of protest and violence on repression, when conditional effects of oil wealth are taken into account has slightly changed. Based on column 5, a one-point increase in violence leads to 0.24-point increase in repression in the short run and around 0.64-point increase in the long run, compared to a short run 0.12-point increase in response to civil protest and a long run 0.32-point. The F-test of the joint significance of the interaction terms fail to reject the null hypothesis that both term are not jointly different from zero in the spending model (P-value==0.81), while in the repression model, the null hypothesis is barely rejected at 10 percent significance level model (P-value==0.09). The columns also reports first-stage F-statistic for both models and it is well above the recommended threshold. The results from system-GMM estimation, shown in columns 3 and 6, are very close to the original least squares results. As in the previous model, the resemblance of the estimated results across least squares and system-GMM is not surprising given the time dimension of the sample, where Nickell Bias becomes smaller when T is 20 years or more (Beck and Katz 2011).[20]

Nevertheless, such results do not provide any information about the responses of oil dictators given the type and size of mass threat. To analyze that, Figure 5.6 show the conditional marginal effects of a change in oil wealth on the levels of repression and spending at the different levels of mass threats based on models 2 and 5 in Table 5.3 together with 95% confidence bands. Figures 5.6(a) and 5.6(c) shows the conditional marginal effects of greater oil wealth on spending and repression at various intensity levels of civil protest. The solid line depicts a change in the marginal effect as the intensity of the threat goes up, indicating that countries with relatively greater oil wealth increase spending and reduce repression. However, the increase in spending is only significantly different from zero at very low levels on the 95% confidence interval and the reduction in repression is not statistically significant at all levels of mass threat. This indicates that at low levels of threats, a dictator with greater oil wealth can nullify the threats by relatively more spending, however as the threat intensifies, such marginal effect vanishes making the dictator behave the same as dictators with lower oil wealth. In contrast, a dictator with greater oil wealth responds with the same level of repression prevailing in low oil wealth countries at all levels of civil protest. Similarly, Figures 5.6(b) and 5.6(d) shows the corresponding marginal effects of oil wealth at various levels of mass violence. The marginal effect on spending appears to be positive statistically significant at low and intermediate levels of mass threat, while the effect on repression is negative but insignificant at all levels of mass violence. In line with hypotheses 1 and 2, a dictator will respond in the short run with relatively more spending and repression in face of violence than in case of civil protest, independent of the size of oil wealth and civil protest are more likely to be met by increased repression only. Both results confirm the possibility of increasing both spending and repression, rather than preferring one to the other depending on the severity of the threat. In addition, oil dictators were found to differ from their non-oil counterparts in

[20] Given that, the two-stage least squares method becomes more preferable to system-GMM, where the number of instruments in the latter method tend to increase with T leading to overfitting and failing to expunge the endogeneity of the variable. Hence, unreliable estimates could be produced, especially if N gets smaller (Roodman 2009a; 2009b).

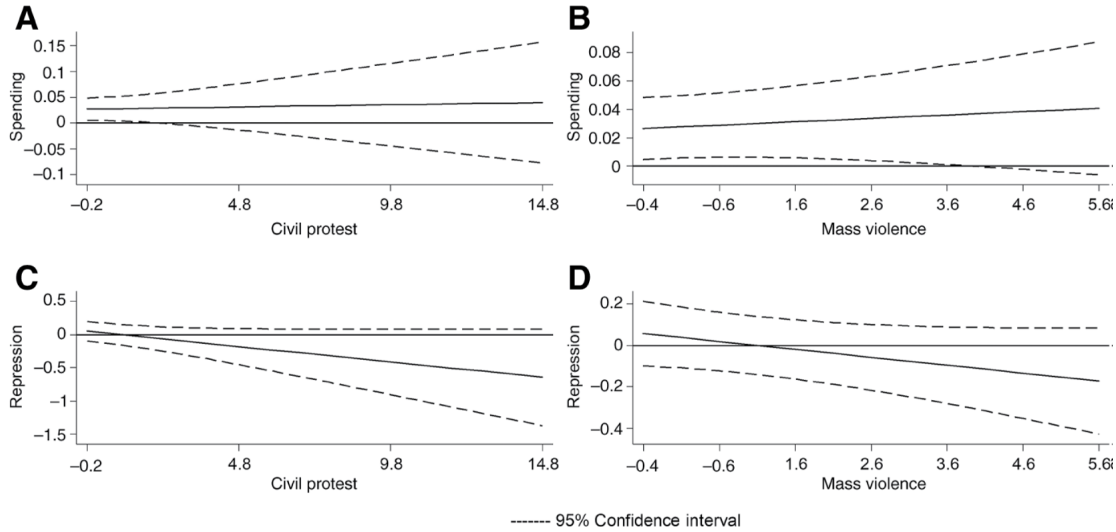


Figure 5.6: Conditional marginal effects at different levels of civil protest and mass violence

their financial capacities to engage in large scale spending given the size of their population, but in contrast to hypothesis 3, they resemble the latter group in terms of their repressive stance independent of threats. Furthermore, the hypotheses on oil dictatorships stated that greater oil wealth allows countries to relatively increase spending and lower repression in face of mass threats. However, the results contradicts these hypotheses. In all incidences of mass threats, greater oil wealth had a relatively positive marginal impact on spending at low and intermediate levels of threats, while showing no difference on the level of employed repression as compared to non-oil dictators. This leads to an interesting result that all dictatorships exhibit the same behavior when faced by either extensive mass violence or civil protest and the financial advantage of having a greater of oil wealth vanishes as threat intensifies. Hence, there is no reason to believe that a very high size of oil wealth makes countries less repressive or drives them to substitute spending for force. The latter result is even more aligned with recent studies which found that resource windfalls may in contrast increase repression through relaxing the dictator's budget constraint against power challengers (Tsui, 2010) or because they increase the value of staying in power (Caselli et al., 2016).

5.7 Robustness checks

In this section, the results from a number of performed robustness checks are reported. Specifically, I start with the two-stage least squares baseline specification in Table 3 (columns 2 and 5) and then apply the following modification one at a time: (a) add institutional variables; (b) drop major oil producers (OPEC); (c) differentiate between boom and bust periods; (d) use alternative measures for oil wealth, government spending and repression.[21] Freedoms concession or suppression can be an alternative strategy to

[21] Other robustness checks are reported in Appendix 5.A3 including: the use of another classification for autocracy; adding additional instruments; drop dynamic effects; and adding additional economic and demographic variables.

Table 5.4: Type of dictator, degree of civil freedoms and country-specific time trend

	Spending			Repression		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Civil Protest, t-1	0.012	0.012	0.012	0.120**	0.108**	0.120**
	(0.009)	(0.009)	(0.009)	(0.058)	(0.055)	(0.057)
Mass Violence, t-1	0.021*	0.020*	0.021*	0.244***	0.202***	0.236***
	(0.011)	(0.011)	(0.011)	(0.073)	(0.072)	(0.073)
Oil Per capita (log), t-2	0.027**	0.029**	0.027**	0.038	0.060	0.036
	(0.012)	(0.012)	(0.012)	(0.075)	(0.067)	(0.073)
Civil Protest × Oil per capita	0.001	0.001	0.001	-0.045*	-0.040*	-0.046*
	(0.004)	(0.004)	(0.004)	(0.025)	(0.023)	(0.024)
Mass Violence × Oil per capita	0.002	0.002	0.002	-0.040*	-0.036	-0.039
	(0.004)	(0.004)	(0.004)	(0.024)	(0.023)	(0.024)
GDP per capita, t-1	0.0001**	0.0001**	0.0001**	-0.0001	-0.0001	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Civil liberties, t-1		-0.013			-0.211***	
		(0.011)			(0.051)	
Military dictator, t-1	0.010			0.315		
	(0.056)			(0.206)		
Spending, t				-0.092	-0.054	-0.078
				(0.152)	(0.154)	(0.150)
Repression, t	-0.034***	-0.038***	-0.034***			
	(0.013)	(0.013)	(0.013)			
Dependent Variable, t-1	0.810***	0.811***	0.810***	0.374***	0.350***	0.376***
	(0.031)	(0.032)	(0.032)	(0.042)	(0.043)	(0.042)
First-stage F-statistic	76.99	67.09	78.02	633.21	607.58	594.70
Joint F-statistic, p-value	0.82	0.81	0.83	0.09	0.13	0.09
Number of observations	1,296	1,296	1,296	1,296	1,296	1,296
Number of countries	88	88	88	88	88	88
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific time trend	No	No	Yes	No	No	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log), t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

either repression or spending. In addition, the ability of the populace to pose a threat to the dictator depends on the institutional setting inside the regime. For instance, people would be deterred from protesting if the expected retaliation, in terms of higher repression, would be severe. To account for these, a measure for the levels of civil freedoms is included, which assesses the state of freedoms of expression, associational and organizational rights and freedom of movement in given year (Freedom House, 2016). The inverse of this index is used, so that 1 would indicate no respect for civil freedoms and 7 indicate full respect. Furthermore, dictators may respond differently to threats depending on their background. In this regard, military dictatorships, as a ruling doctrine based on the "management of violence", have a competitive advantage in the use of force and in cases of political threats, more repression is employed (Wintrobe, 1998). On the contrary and despite their use of violence, civilian dictators and monarchs may rely more on the distribution of public goods and on maintaining patronage networks as their primary survival strategy (Wright el al. 2013). Therefore, a Military Leader dummy variable to control for the dictator's type is also included (Ghandi and Przeworski, 2006; 2007). Both institutional variables are lagged by one year to be consistent with the starting date of the mass threat. The results are reported in Table 5.4. The results are identical to the baseline specification, where the sign and the statistical significance of each of the main independent variables remain the same. The Military Leader dummy variable had no significant effect on both spending

Table 5.5: Oil bust and boom periods, and excluding OPEC countries

	Spending			Repression		
	Bust Period	**Boom Period**	**Excluding OPEC**	**Bust Period**	**Boom Period**	**Excluding OPEC**
Civil Protest, t-1	0.012	0.019	0.008	0.102*	0.261***	0.089
	(0.013)	(0.013)	(0.007)	(0.059)	(0.094)	(0.066)
Mass Violence, t-1	0.024*	0.037	0.018*	0.170**	0.349**	0.185***
	(0.014)	(0.024)	(0.011)	(0.072)	(0.153)	(0.057)
Oil Per capita (log), t-2	-0.027	0.017*	0.026**	-0.102	0.141	0.051
	(0.038)	(0.011)	(0.012)	(0.141)	(0.100)	(0.076)
Civil Protest × Oil per capita	0.003	0.007	-0.001	-0.049**	-0.038	-0.059**
	(0.006)	(0.006)	(0.003)	(0.024)	(0.055)	(0.028)
Mass Violence × Oil per capita	-0.003	-0.005	0.002	-0.029	-0.048	-0.059***
	(0.004)	(0.006)	(0.004)	(0.026)	(0.039)	(0.019)
GDP per capita, t-1	0.0001***	0.0001	0.0001***	-0.0001	-0.0001	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Spending, t				0.140	-0.180	-0.063
				(0.340)	(0.197)	(0.146)
Repression, t	-0.053**	-0.038	-0.031**			
	(0.026)	(0.026)	(0.013)			
Dependent Variable, t-1	0.677***	0.883***	0.819***	0.287***	0.316***	0.388***
	(0.037)	(0.056)	(0.034)	(0.065)	(0.057)	(0.046)
First-stage F-statistic	21.71	30.16	70.16	331.63	220.44	517.02
Joint F-statistic, p-value	0.56	0.35	0.82	0.09	0.40	0.00
Number of observations	729	552	1,158	729	552	1,158
Number of countries	77	80	78	77	80	78
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log), t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Columns 1 and 3 restrict sample to bust period (1986-1999); columns 2 and 4 restrict the sample to boom periods (1981-1985) and (2000-2006); Columns 3 and 6 exclude OPEC countries. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

and repression, whereas civil freedoms had a strong negative significant relationship with repression, but not with spending. As such, freedoms can an alternative to physical forms of state repression, but are less likely to be a substitute to spending. Additionally, in columns 3 and 6, we control for country-specific time trends to reduce potential omitted variable bias arising from focusing on the reduced form estimates. In this way, if political instabilities are not randomly distributed, including a country-specific time trend ensures that repression is not simply upward trending towards them. Our key estimates remain quantitatively and statistically the same.[22] The value of oil wealth, being a function of oil prices and oil production, could be severely affected by downturns in international oil prices, i.e. bust periods. This could in turn damage the ability of the dictator to offer material spoils in exchange for political consent. On the other hand, boom periods generate expectations of higher subsequent public spending, which if not occurred, could lead to social unrest. Furthermore, higher oil prices can drive military actions against the regime, fed by the greed to capture the accumulated oil revenues. To examine that, Table 5.5 divides the sample into oil bust and boom periods. Bust period is defined as all years between 1986 and 1999 in which international oil prices were at their lowest (Barsky and kilian, 2004), while boom periods refers to the periods from 1981-1985 and 2000-2006. The downside of splitting the sample, however, is the entailed loss of observations, which

[22]Furthermore, if there are time-variant factors that jointly affect the explanatory and the response variables, adding year fixed effects removes any specific association between both variables, which are already controlled for.

Table 5.6: Using oil reserves at different lags

	Spending				Repression			
	Oil Reserves t-2	Oil Reserve t-3	Oil Reserves t-4	Oil Reserves t-5	Oil Reserves t-2	Oil Reserves t-3	Oil Reserves t-4	Oil Reserves t-5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Civil Protest, t-1	0.003	0.001	-0.000	-0.001	0.149***	0.187***	0.193***	0.164**
	(0.007)	(0.007)	(0.008)	(0.008)	(0.058)	(0.055)	(0.072)	(0.075)
Mass Violenc, t-1	0.024**	0.026**	0.029**	0.029**	0.256***	0.240***	0.217***	0.186**
	(0.012)	(0.012)	(0.013)	(0.012)	(0.069)	(0.076)	(0.079)	(0.081)
Oil Res. pc (log), t-2	0.092**	0.102**	0.081**	0.105**	-0.297	-0.195	-0.332	-0.210
	(0.045)	(0.043)	(0.039)	(0.041)	(0.208)	(0.188)	(0.233)	(0.237)
Civil Protest × Reserves	0.004	-0.003	-0.001	-0.003	-0.060	-0.050	-0.018	-0.023
	(0.008)	(0.009)	(0.008)	(0.008)	(0.050)	(0.045)	(0.044)	(0.048)
Mass Violence × Reserves	0.004	0.002	0.002	0.004	-0.075**	-0.085***	-0.079**	-0.064*
	(0.006)	(0.007)	(0.007)	(0.007)	(0.032)	(0.032)	(0.034)	(0.033)
GDP pc (log), t-1	0.313***	0.353***	0.373***	0.414***	0.092	0.123	0.125	-0.187
	(0.056)	(0.062)	(0.063)	(0.069)	(0.309)	(0.328)	(0.358)	(0.384)
Spending, t					-0.153	-0.089	-0.074	0.068
					(0.185)	(0.197)	(0.208)	(0.226)
Repression, t	-0.026**	-0.032**	-0.035**	-0.031*				
	(0.012)	(0.014)	(0.016)	(0.017)				
Dependent Variable, t-1	0.735***	0.719***	0.717***	0.686***	0.377***	0.349***	0.330***	0.325***
	(0.025)	(0.027)	(0.029)	(0.032)	(0.044)	(0.048)	(0.049)	(0.049)
First-stage F-statistic	72.29	53.38	45.72	43.07	796.95	624.68	572.31	423.16
Joint F-statistic, p-value	0.75	0.90	0.94	0.80	0.03	0.02	0.05	0.13
Number of observations	1,277	1,196	1,116	1,043	1,277	1,196	1,116	1,043
Number of countries	87	84	82	82	87	84	82	82
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil Res. pc (log), t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Columns 1 and 5 show the effect of oil reserves at year t-2 on spending and repression at year t. Columns 2 and 6 show the effect of oil reserves at year t-3 on spending and repression at year t. Columns 3 and 7 show the effect of oil reserves at year t-4 on spending and repression at year t. Columns 4 and 8 show the effect of oil reserves at year t-5 on spending and repression at year t. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

can affect the results. Nevertheless, during bust period, mass violence remain positive and statistical significant, while oil wealth lost its significance in the spending model. The opposite occurs during the booms periods, with mass violence becoming insignificant and oil wealth gaining statistical significance. In the repression model, the effect of mass threats on repression remains the same during both periods, and the oil wealth's impact is still insignificant. This implies that during periods of financial crisis and the inability of oil revenues to fund public spending, public dissidence can support violence against the state. As such, the state responds mass violence with more spending to placate public support to it and also with more repression. During boom periods, the state is in a stronger financial position vis-à-vis opponents and therefore, refuses to tolerate mass violence and regards repression as the suitable respond.

Additionally, international oil prices are also affected by the internal political status in major oil world producers, which could cast is shadow on the value of oil returns. To address this concern, Table 5.5 excludes OPEC countries from the sample. The key estimates remain the same in the spending model, but civil protest lost its significance in the repression model. This could be rather due to the drop in the sample size. The F-test of the joint significance of the interaction terms rejects the null hypothesis that both terms are not jointly different from zero in the repression model. This, however, does not change our main conclusion that oil wealth does not drive the dictator to employ less repression, given the oil wealth variable is statistically insignificant.

Table 5.7: Using oil wealth per capita (Ross Mahdavi, 2015) and the value of average of oil production

	Spending		Repression	
	Oil Wealth (Ross et al. 2015)	Value of average Oil Production	Oil Wealth (Ross et al. 2015)	Value of average Oil Production
	(1)	(2)	(3)	(4)
Civil Protest, t-1	0.012	0.004	0.119**	0.168*
	(0.009)	(0.008)	(0.058)	(0.087)
Mass Violence, t-1	0.019*	0.020*	0.232***	0.276***
	(0.011)	(0.012)	(0.071)	(0.075)
Oil Per capita (log), t-2	0.032***	0.123***	0.119	0.062
	(0.012)	(0.035)	(0.077)	(0.271)
Civil Protest × Oil per capita	0.001	0.001	-0.044*	0.000
	(0.004)	(0.001)	(0.023)	(0.011)
Mass Violence × Oil per capita	0.002	0.000	-0.038*	-0.015**
	(0.003)	(0.001)	(0.023)	(0.007)
GDP per capita, t-1	0.000**	0.000**	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Spending, t			-0.110	-0.086
			(0.145)	(0.154)
Repression, t	-0.032***	-0.033***		
	(0.013)	(0.012)		
Dependent Variable, t-1	0.808***	0.812***	0.384***	0.384***
	(0.031)	(0.030)	(0.042)	(0.043)
First-stage F-statistic	83.46	77.84	609.98	654.77
Joint F-statistic, p-value	0.84	0.66	0.09	0.09
Number of observations	1,325	1,326	1,325	1,326
Number of countries	89	89	89	89
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log), t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Columns 1 and 3 reports estimate using oil wealth variable from Ross et. (2015). Columns 2 and 4 reports estimates using the country's (time-invariant) whole period average level of oil producation multiplied by time varying oil price. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

Despite the fact that oil wealth enters the model one year before the start date of mass erupts, there might be still some concern regarding the full exogeneity of this measure, since production might be affected by mass erupts as well expectations for mass erupts. To address this, total oil reserves (log), as proxy for time-fixed measure of oil abundance, is used instead. This variable enters the empirical specification with 3 years lags to account for the possibility that having higher reserves do not automatically imply the ability to extract and produce oil, since the latter is also function of the degree of economic development, geography and trade relations. I also consider using 2, 4 and 5-years lags. The main results remain robust in all specifications in Table 5.6.

In Table 5.7, two additional alternative measures for oil wealth are used. One is the oil revenues per capita taken from Ross et al. (2015). The second is constructed by multiplying the country's (time-invariant) whole period average level of oil production with (time-varying) oil price. Such measure avoids the endogeneity of oil production, by making use of the relative exogeneity of oil prices. The main results remain unchanged.

Additional robustness checks are reported in Table 5.8. First, the measure for government spending in current prices used in our baseline specification is replaced by its

Table 5.8: Using government expenditures (% of GDP) and political terror scale

	Spending		**Repression**	
	Gov. Exp. (% of GDP)	**PTS**	**Gov. Exp. (% of GDP)**	**PTS**
	(1)	**(2)**	**(3)**	**(4)**
Civil Protest, t-1	0.143**	0.016	0.113**	0.063**
	(0.072)	(0.010)	(0.056)	(0.032)
Mass Violence, t-1	0.063	0.020*	0.210***	0.116***
	(0.214)	(0.011)	(0.069)	(0.036)
Oil Per capita (log), t-2	0.357*	0.045***	0.038	0.033
	(0.205)	(0.012)	(0.065)	(0.023)
Civil Protest × Oil per capita	0.050	0.002	-0.046*	-0.016
	(0.031)	(0.004)	(0.024)	(0.013)
Mass Violence × Oil per capita	-0.081	0.005	-0.039*	-0.019
	(0.096)	(0.003)	(0.021)	(0.013)
GDP per capita, t-1		0.0001**		-0.0001***
		(0.000)		(0.000)
Spending, t			0.016	-0.075
			(0.012)	(0.077)
Repression, t	-0.145	-0.050**		
	(0.192)	(0.023)		
Dependent Variable, t-1	0.676***	0.791***	0.391***	0.456***
	(0.036)	(0.037)	(0.040)	(0.040)
First-stage F-statistic	83.64	126.49	338.93	474.69
Joint F-statistic, p-value	0.13	0.29	0.05	0.18
Number of observations	1,364	1,343	1,364	1,343
Number of countries	90	91	90	91
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log), t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Columns 1 and 3 report estimates when general government expenditures (% GDP) is used; columns 2 and 4 report estimates using political terror scale as an indicator for repression. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

counterpart measure as a percentage of GDP. One advantage of this measure is that it includes an indirect measure of the economic size of the country instead of controlling for it separately in the model using real GDP per capita. Therefore, GDP per capita is dropped. The downside, however, is that it could make it difficult to remove business cycles effects given that we are look at year-to-year effects. In other word, it may be hard to decide whether the estimated result is due a change in public spending (numerator) or in GDP (denominator). In column 1, the estimated coefficient for mass violence is positive, but statistically insignificant, whereas the coefficient for civil protest is positive and statistically significant. This could be due to the balancing forces between simultaneous changes in public spending and their base GDP. Recall, civil protest has no significant impact on public spending as shown in our main model, whereas it can have negative effects on the GDP. Hence, reducing the GDP could increase the share of public spending to GDP in face of civil protest, but not the absolute size of public spending. The results in column 2 for the repression model remain the same. Second, Political Terror Scale (PTS) is used as an alternative indicator for repression in columns 2 and 4 (Gibney and Dalton, 1996; Wood and Gibney, 2010). The PTS ranges from 1 to 5, with high values indicating high levels of terror. Results remain robust to this specification.

5.8 Conclusion

The first implication of this study is that the type of threat matters when it comes to the dictator's decision to perceive a certain strategy. This rather takes us away from the general notion contending repression and spending as natural traits of dictatorships, to conceiving them as means ready for employment when needed and subject to tradeoffs with other strategies depending on the dictator's time horizon. In line with Wintrobe (1998) and Annett (2001), the short run responses to political threats are either spending or repression. The empirical results predict a rather simultaneous increase in both spending and repression in face of mass violence, and that repression is preferred over spending in case of mass protests. As such, instead of engaging in spending or repression in mutually exclusive fashion as indicated in previous studies, the novelty of the results here lies in revealing a third possibility of mixing between both depending on the intensity of the threat.

The second implication suggests that oil dictatorship are in the end dictators who gained their name because they cannot be removed by popular vote (Ghandi and Przeworski, 2007), but the difference lies in their source and size of their incomes. Hence, under the same circumstances, they will always follow the same dictatorial strategies independent of the size of their oil wealth. As the results show, the size of oil wealth may have a relative impact on increasing spending, but only at low and intermediate levels of threats, but as the threats intensify, the effect of oil wealth dissipates and oil dictatorships behave the same as their non-oil counterparts. Additionally, oil wealth showed no impact on repression, whether independent or conditional on threats. The gives a rise to an important conclusion that even if oil wealth might have the potential to alter the distributive and repressive capacities of dictatorships, there is still no strong evidence to believe that such endowments can make countries less repressive or drives them to substitute spending for force when faced by severe power challenges.

In a broader perspective, this study offers an extension to Wintrobe's (1998) modeling of dictatorial response to negative shocks that reduce populace's loyalty to the regime. By differentiating between the intensities of mass threats, the corresponding responses are extended to include not only exclusive resorting to repression or spending, but also a mix between both. Furthermore, the size of financial resources can allow for differences in the levels of employed repression and spending in face of the same threat. In this regard, it was shown oil dictators differ in the levels of the employed spending and repression at very low and intermediate levels of threats.

These results are quite useful in understanding the dictatorial behavior in different settings and offers further explanation for the duration of authoritarian regimes, especially in oil dependent countries. Furthermore, instead of identifying the strategies for political survival, equally important is the understanding of the circumstances under which dictators can choose their responding mechanism. The disaggregation of threats into elites and masses, small scale, peaceful and violent is useful in studying not the dictators' behavior, but also the reaction of different society groups to dictatorial policies. Additionally, specifying the dictator's time horizon gives more insights about the tradeoff between perused policies. For instance, financial constraints can reduce the ability of the leader to sustain both spending and repression in the long run, a situation that could drive him to crackdown on freedoms or offer political concessions to contain future threats. A point of departure would be through looking at the institutional setting and the composition of opposition groups in these states. Although I made no argument about the extent of the organization of political opposition, it is worthy to study that. As pointed by Smith (2004), oil dictatorships might have developed non-repressive mechanisms that allow them to stay in power. Finally, the typology of oil dictatorships based on the size of their rents and population can be incorporated in other studies such as foreign aid. The benchmark should not be the degree on dependence on the source of income, but the capacity to distribute on the population. More research is needed to explore whether the same results can be reached with other types of rents mainly minerals and foreign aid.

5.A Appendix

5.A1 Variables: definitions and sources

Table 5.A1: List of variables, definitions and sources

Indicator	Definition	Source
Assassination	Any politically motivated murder or attempted murder of a high government official or politician.	Databanks International (2013)
Strikes	Any strike of 1,000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority.	Databanks International (2013)
Guerilla Warfare	Any armed activity, sabotage, or bombings carried on by independent bands of citizens or irregular forces and aimed at the overthrow of the present regime.	Databanks International (2013)
Riots	Any violent demonstration or clash of more than 100 citizens involving the use of physical force.	Databanks International (2013)
Revolutions	Any illegal or forced change in the top government elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government.	Databanks International (2013)
Demonstrations	Any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority, excluding demonstrations of a distinctly anti-foreign nature.	Databanks International (2013)
Minor Civil Conflicts	Dummy variable that takes on the value 1 if a country is observed as having at least one intra-state conflict between the government of the state and internal opposition groups with at least 25 battle-related deaths in a given year and without foreign intervention, 0 otherwise.	Gleditsch et al. (2002)
Civil War	Dummy variable that takes on the value 1 if a country is observed as having at least one intra-state conflict between the government of the state and internal opposition groups with at least 1,000 battle-related deaths in a given year and without foreign intervention, 0 otherwise.	Gleditsch et al. (2002)

5.A2 Factor analysis: Estimation and results

Factor analysis was estimated using maximum likelihood.[23] To decide on the number of factors, the graphical scree test is firstly used. The number of factors is determined based on their corresponding eigenvalues and according to Kaiser criteria, the chosen factor should have an eigenvalue greater than 1. It can be seen in Figure 5.A1 that there are three factors whose eigenvalues are larger than 1. However, models with more than two factors are considered to be a "Heywood case" indicating a poorly specified model. Hence, it could be said that two factors are deemed to be appropriate. Second, the performing of a Likelihood Ratio (LR) lead to the rejection of the null-hypothesis meaning that the two factor model is favored over the unrestricted model. Finally, using the Akaike's information criterion and the Schwarz criterion, the model with the lowest value was chosen.

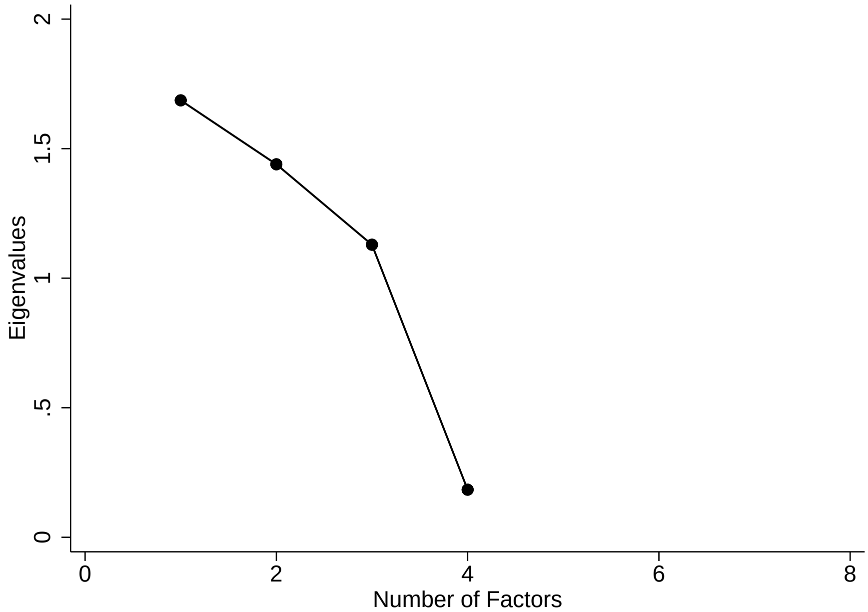


Figure 5.A1: Scree plot of factors and their corresponding eigenvalues

I assume that dimensions of political instability are correlated, since it is plausible to imagine that uncontained low scale incidences of strikes and riots can develop over time and give a rise to more violent events as revolution and assassinations. Accordingly, I apply Oblimin rotation method, which minimizes correlation between estimated communalities in the factor loadings matrix; such that each indicator is assigned to the factor for which it has the highest loading. The estimated rotated factor loadings are shown in table 5.A2. The first factor has high loadings for small scale incidences of instabilities or civil protest indicators, while the second indicator reflect a more wide spread violence and can be labeled as mass violence. To check the robustness of our results, I started by excluding one indicator at a time and then dropping one world region at a time. The results turned out to be robust for all sample alteration. Figure 5.A2 depicts the variation of the indices over time based on average number of observations and shows signs of co-movement between

[23]I follow the methodology of Jong-A-Pin (2009).

them.

Table 5.A2: Rotated factor loadings matrix

Indicators	Mass Civil Protest	Political Mass Vioelnce	Unique Variance
Assassination	0.11	0.26	0.92
Strikes	0.45	0.04	0.80
Guerilla Warfare	-0.01	0.80	0.37
Riots	0.87	-0.03	0.24
Revolutions	0.02	0.61	0.63
Demonstrations	0.82	0.03	0.32
Minor Civil Conflicts	0.06	0.20	0.95
Civil War	-0.01	0.61	0.63

Note: The method of estimation for factor loadings is Maximum Likelihood (ML) Oblimin rotation.

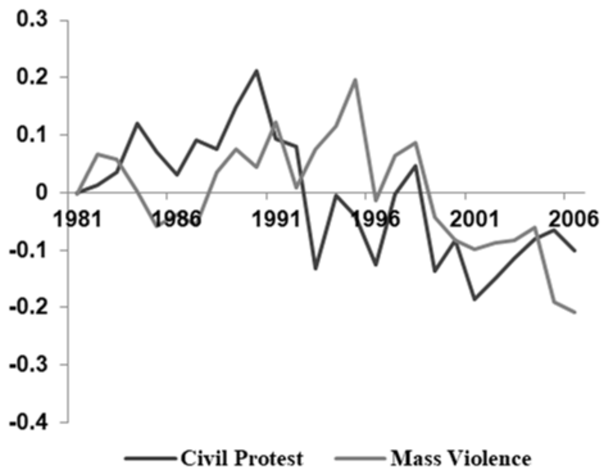


Figure 5.A2: Variation of mass threats indices over time (1981-2006)

5.A3 Further robustness checks

It might be well maintained that the results could be sensitive to the employed classification of autocracy and that heterogeneities could exist among well entrenched and more mild forms of autocracies. To address that, the results are replicated using two different classifications for autocratic regimes as shown in Table 5.A3. First, countries are classified as autocracies if their Polity2 score is strictly negative (Brückner and Ciconne, 2010; Caselli and Tesei, 2016). Such classification allows for analyzing the respond of a dictator in the most restrictive forms of autocracy. Second, I use the data on autocratic regimes from Geddes, Wright and Frantz, which focus on the institutional characteristics of the regime rather than the background of the incumbent (GWF, 2014). For countries with negative Polity2 scores, civil protest and mass violence have no impact on spending, whereas the magnitude of both indices are higher in the repression model. The point estimate of oil wealth in the spending model is also larger than its estimate in the baseline model. Using GWF classification, the results remained unchanged in both models. This reveals an interesting result that the most repressive forms of autocracies tend to respond with more repression, whereas mild forms of autocracies tend to mix between spending and repression.[24]

Table 5.A3: Different classifications for autocracies

	Spending		Repression	
	Polity2 < 0	**GWF**	**Polity2 < 0**	**GWF**
	(1)	**(2)**	**(3)**	**(4)**
Civil Protest, t-1	0.002	0.007	0.127**	0.093*
	(0.011)	(0.009)	(0.064)	(0.055)
Mass Violence, t-1	0.012	0.024*	0.272***	0.250***
	(0.017)	(0.013)	(0.094)	(0.074)
Oil Per capita (log), t-2	0.038***	0.031***	0.076	0.090
	(0.013)	(0.012)	(0.065)	(0.070)
Civil Protest × Oil per capita	0.006	0.002	-0.016	-0.030
	(0.006)	(0.004)	(0.036)	(0.025)
Mass Violence × Oil per capita	0.003	0.003	-0.016	-0.049**
	(0.005)	(0.004)	(0.030)	(0.024)
GDP per capita, t-1	0.0001**	0.0001**	-0.0001*	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)
Spending, t			0.055	-0.106
			(0.176)	(0.154)
Repression, t	-0.026*	-0.039**		
	(0.014)	(0.015)		
Dependent Variable, t-1	0.801***	0.812***	0.368***	0.343***
	(0.033)	(0.033)	(0.048)	(0.043)
First-stage F-statistic	56.16	64.1	553.01	560.48
Joint F-statistic, p-value	0.54	0.77	0.82	0.11
Number of observations	1,043	1,184	1,043	1,184
Number of counteries	78	81	78	81
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log)t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Columns 1 and 3 report estimates for autocracies with strictly negative Polity2 score. Columns 2 and 4 report when GWF classification of autocracy is used. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

In the baseline specification, both spending and repression are instrumented by their lagged levels. In table 5.A4, additional instruments are added to both models. In particular, additional instruments controlling for trade openness and demographics are included in the spending model. Trade openness (log), measured as the sum of imports and exports as a percentage of GDP (WDI, 2016), tackles the cited relationship between trade dependence and the size of public sector (Rodrik, 1998). Demographic variables, in terms of age distribution of younger and elder population, address the fact that elder population, on average, demand more resources to be devoted to providing pensions (Perotti, 1996), whereas younger population demand more spending on education, and in both cases public spending is driven up. To account for such differences, a measure for Dependent Population is included, constructed by adding the percentage of population whose ages are between 0 and 14 years old, and percentage of population over 65 years old (WDI, 2016). In the repression model, two binary variables are included indicating whether an interstate war has taken place in the country in the past year (Gleditsch et al,. 2002) and whether there are major ongoing civil or ethnic conflicts in at least four boarding states (Marshall, 2017). The latter variable account for recent evidence showing that instability in neighboring states can lead the regime to be increase repression as a precautionary

[24]Following Caselli et al. (2016), polity2 score is adjusted, so that periods of anarchy and interregnum are codded as missing instead of 0. Results remain unchanged.

measure (Escribà-Folch, 2013).

Table 5.A4: Adding additional instruments and dropping dynamics

	Spending		Repression	
	Additional Instruments	Drop dynamics	Additional Instruments	Drop dynamics
	(1)	(2)	(3)	(4)
Civil Protest, t-1	0.004	0.023	0.142***	0.178**
	(0.008)	(0.018)	(0.050)	(0.079)
Mass Violence, t-1	0.023**	0.046*	0.224***	0.455***
	(0.011)	(0.026)	(0.074)	(0.096)
Oil Per capita (log), t-2	0.021*	0.094**	0.023	0.099
	(0.012)	(0.040)	(0.071)	(0.112)
Civil Protest × Oil per capita	-0.001	0.002	-0.039*	-0.062*
	(0.004)	(0.006)	(0.023)	(0.032)
Mass Violence × Oil per capita	0.001	0.006	-0.041*	-0.079**
	(0.004)	(0.010)	(0.023)	(0.036)
GDP per capita, t-1	0.000**	0.000***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Trade (log) (% GDP), t-1	0.127***			
	(0.032)			
Dependent population, t-1	0.001			
	(0.004)			
Interstate war, t-1			0.411*	
			(0.223)	
Neighbouring's instability, t-1			-0.503**	
			(0.240)	
Spending, t			-0.126	-0.222
			(0.151)	(0.205)
Repression, t	-0.029**	-0.071**		
	(0.012)	(0.028)		
Dependent Variable, t-1	0.823***		0.371***	
	(0.029)		(0.042)	
First-stage F-statistic	27.20	80.97	249.27	610.65
Hansen J-statistic	0.98		0.27	
Joint F-statistic, p-value	0.96	0.77	0.09	0.05
Number of observations	1,294	1,304	1,294	1,308
Number of countries	88	88	88	89
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log)t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Columns 1 and 3 report estimates when adding additional instruments. Columns 2 and 4 report estimates when lagged dependent variables is dropped. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

The results reported in Table 5.A4 remain similar to the baseline specification. Although the conditional variables in the repression model show stronger statistical significance, as long as the oil wealth variable remain statistically insignificant, the conditional impact of oil wealth on repression remain the same. The table also reports the first-stage F-statistic for both models and it is well above the recommended threshold. Tests of overidentifying restrictions are also performed, where the p-value of the Hansen test reported fail to reject the null hypothesis that the instruments are valid. Furthermore, results from first stage shows a strong positive statistical relationship between neighboring instability and repression, and between trade and spending. Another robustness check is performed by removing the lagged dependent variable, hence shifting the model into its static version. The statistical significance of the main variables of interest remains the same, but the point estimates are quite larger in magnitude.

Table 5.A5 performs more robustness checks by including additional covariates re-

flecting annual inflation, urbanization and regime duration in government spending and repression models.[25] To illustrate, Alesina et al. (1998) points out that urbanization is a strong determinants of defense spending and government consumption, respectively. On the other hand, macroeconomic stability measured by annual inflation assesses the ability of the leader to provide financial spoils and finance his security apparatus (Conrad, 2011). The baseline results remain robust to the inclusion of urbanization and ethnic fractionalization measures. Yet, the inclusion of inflation caused mass violence to lose its statistical significance in the spending model indicating that economic hardships reduce the ability of the leader to increase spending to cool off mass dissidence. Table 5.A5 also address the concern that the population size might be affected by mass uprisings and the corresponding dictatorial response; hence, the size of oil wealth per capita might be affected as well. Hence, the size of population (log) is measured by the number of people living in a given country in a given year (WDI, 2016) is also controlled for and the no change in the main results is encountered.

Table 5.A5: Adding additional control variables

	Spending				Repression			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Civil Protest, t-1	0.012	0.009	0.006	0.012	0.117**	0.123**	0.144**	0.120**
	(0.009)	(0.009)	(0.008)	(0.009)	(0.058)	(0.056)	(0.059)	(0.057)
Mass Violence, t-1	0.020*	0.022*	0.017	0.020*	0.227***	0.231***	0.210***	0.239***
	(0.011)	(0.012)	(0.013)	(0.011)	(0.073)	(0.073)	(0.078)	(0.073)
Oil Per capita (log), t-2	0.028**	0.026**	0.028**	0.027**	0.044	0.041	0.022	0.043
	(0.013)	(0.012)	(0.012)	(0.012)	(0.079)	(0.075)	(0.105)	(0.075)
Civil Protest × Oil pc	0.001	-0.000	-0.000	0.001	-0.048**	-0.041*	-0.040*	-0.045*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.024)	(0.024)	(0.024)	(0.025)
Mass Violence × Oil pc	0.002	0.003	0.004	0.002	-0.038	-0.036	-0.039	-0.037
	(0.004)	(0.004)	(0.004)	(0.004)	(0.024)	(0.024)	(0.027)	(0.024)
GDP per capita, t-1	0.0001**	0.0001**	0.0001**	0.0001**	-0.0001	-0.0001	-0.0001	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Population (log), t-1	-0.183*				-1.303**			
	(0.096)				(0.652)			
Urbanization (log), t-1		0.110**				0.456		
		(0.049)				(0.359)		
Inflation (log), t-1			-0.019*				-0.060	
			(0.010)				(0.050)	
Regime Duration, t-1				0.018				-0.079
				(0.029)				(0.175)
Spending, t					-0.093	-0.099	-0.189	-0.075
					(0.151)	(0.153)	(0.183)	(0.148)
Repression, t	-0.036***	-0.036***	-0.016	-0.034***				
	(0.013)	(0.013)	(0.012)	(0.013)				
Dependent Variable, t-1	0.809***	0.806***	0.798***	0.810***	0.370***	0.372***	0.380***	0.377***
	(0.031)	(0.030)	(0.027)	(0.031)	(0.042)	(0.043)	(0.050)	(0.043)
First-stage F-statistic	78.14	75.87	56.66	77.64	604.87	640.58	871.82	615.56
Joint F-statistic, p-value	0.82	0.66	0.59	0.84	0.09	0.13	0.15	0.11
Number of observations	1,296	1,295	975	1,296	1,296	1,295	975	1,296
Number of countries	88	88	77	88	88	88	77	88
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil per capita (log)t-2 is the country's oil wealth at t-2 minus the average oil wealth among autocracies at t-2. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

The duration of the regime may account for the dictator's respond to mass threats.

[25] The data for annual inflation and urbanization levels are taken from World Bank World Development Indicators database (WDI, 2016), while fractionalization is obtained from Roeder's (2001).

In columns 4 and 8, the results are tested when accounting for the age of the regime. Regime's duration is measured by a dummy variable that takes on the value 1, if the variable 'Durable' is 0 in polity IV dataset, which means that a new regime has started or that the state is in anarchy, 0 otherwise. The main results remain robust. Results are also checked after dropping GDP per capita and no change is reported.

In the baseline specification, using oil wealth per capita allows for comparing oil producers against non-oil producers in their trade-off between consumption and repression in face of mass instabilities. In our sample, including mostly developing countries and limited diversified economies, GDP per capita and oil wealth are highly correlated. In other words, high-income countries are by large (but not all) the same countries having the highest oil wealth per capita. This may conflate the conditional effects of oil wealth with high income. To give further insights, Table 5.A6 reports the impact of political instabilities conditional upon the initial level of the GDP, while controlling for oil wealth. Hence, we are comparing the conditional effect of high-income, keeping the size of the oil wealth constant. The coefficient of the GDP per capita is positive and statistically significant in the spending model, while being negative and statistically insignificant in the repression model. This indicates that high income is associated with more spending, but not necessarily with lower repression.

Table 5.A6: Conditional effects of GDP per capita

	Spending	**Repression**
	(1)	**(2)**
Civil Protest, t-1	0.005	0.129***
	(0.006)	(0.048)
Mass Violence, t-1	0.023**	0.212***
	(0.010)	(0.075)
GDP per capita (log), t-2	0.223***	-0.030
	(0.050)	(0.261)
Civil Protest × GDP per capita	0.0001	0.106**
	(0.005)	(0.047)
Mass Violence × GDP per capita	-0.001	-0.122***
	(0.008)	(0.047)
Oil per capita, t-1	0.0001	-0.0001
	(0.0001)	(0.0001)
Spending, t		-0.079
		(0.194)
Repression, t	-0.035***	
	(0.013)	
Dependent Variable, t-1	0.748***	0.374***
	(0.028)	(0.044)
First-stage F-statistic	72.24	644.88
Number of observations	1,296	1,296
Number of countries	88	88
Country FE	Yes	Yes
Year FE	Yes	Yes

The method of estimation is two-stage least squares; t-statistics reported in parentheses are based on Huber-robust standard errors clustered at the country level. Oil Production (log)t-2 is the country's (time-invariant) whole period average level of oil producation multiplied by time varying oil price. Country and year fixed effects are not reported. The asterisks denote:*p<0.10, **p<0.05, ***p<0.01.

The conditional marginal effects is shown in figure 5.A3, depicting the marginal effects of a change in income on the levels of repression and spending at the different levels of mass threats together with 95% confidence bands. Figures 5.A3(A) and 5.A3(B) indicate that higher income is associated with more spending at all levels of mass threat. In Figures

5.A3(C) and 5.A3(D), the marginal effect on repression appears to be positive statistically significant at high levels of civil protest, while the effect on repression is negative but insignificant at all levels of mass violence. Taken together, the results show difference between the conditional effects oil wealth and high income, referring to distinct responses of oil wealthy regimes. High-income countries spend relatively more at all levels of threats, and repress more at higher levels of civil protest. In contrast to our main results, finding greater oil wealth per capita to relatively increase spending at low and intermediate levels of mass threats, and to employ the same level of repression as non-oil dictators.

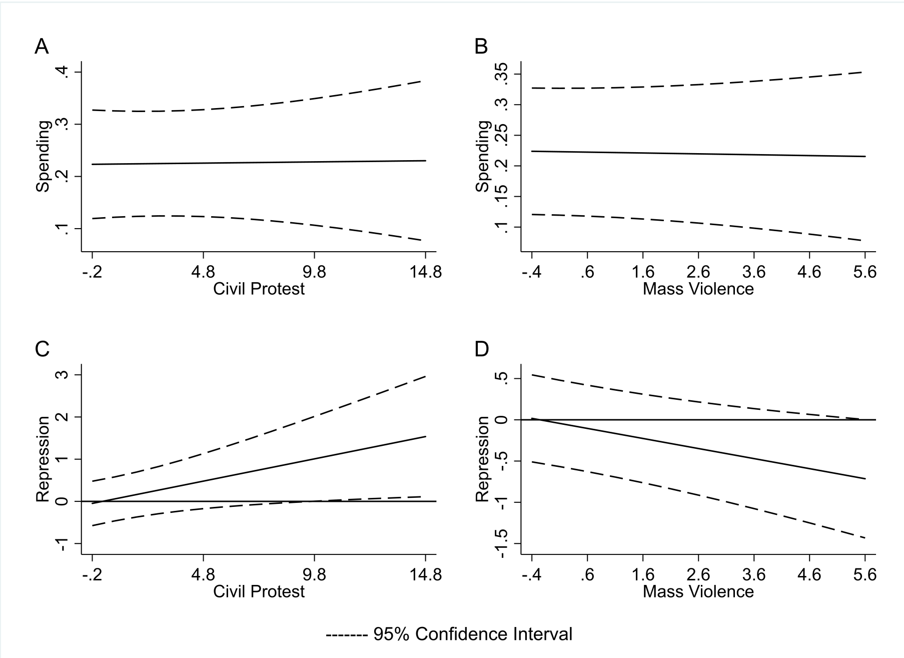


Figure 5.A3: Conditional marginal effects at different levels of civil protest and mass violence

Chapter 6.

Conclusion

This dissertation has sought to revisit key questions of resource curse literature. The main aim was to shed the light on the underlying conditioning context, when evaluating the final impact of natural resource abundance on a country's political stability, tax revenues and economic activity. I focus on one type of natural resources, that is "oil rents", which has occupied the bulk space of resource curse studies and is often cited to be main culprit of economic and political failures for many countries in Africa, Middle East and Latin America. In three of my papers, I identify two of these conditioning factors: (1) the shadow economy and (2) the spatial distribution of the oil rents. The fourth study reconsiders the role of oil rents in prolonging autocratic regime survival after taking into account the type of posed mass threat, the dictator's time horizon (short run vs. long run) and the size of oil wealth. In these studies, I have examined national- and local-level evidence, employed different analytical models including panel-, time-series and spatial-econometric techniques and worked with survey and geographical data.

The main findings of this dissertation can be summarized as follows. Chapters 2 and 3 show that the existence of sizable shadow economies can mitigate the effect of oil price shocks on anti-government protests and tax revenues. Specifically, a higher size of the shadow economy can reduce the effect of negative oil price shocks on protest proliferation. Furthermore, negative shocks in oil rents tend to promote tax efforts of the state when the size shadow economy is sufficiently moderate, whereas they have no significant positive impact on tax revenues when the shadow economy is representing more than 35% of GDP.

Chapter 4 stresses on the importance of within-country disaggregation of resource boom effects among oil-producing sub-nationals and their neighbors. It documents that oil-producing municipalities benefit from an increase in economic activity following oil and gas revenue booms, while neighboring municipalities suffer from a corresponding slash in economic activities. At the Micro-level, the positive direct of higher economic activities is balanced off by the negative spill-overs, so that the aggregate effect of an increase in oil and gas revenues is null. Chapter 5 shows that the trade-off between public spending and repression is conditional upon the intensity of the posed mass threat (i.e. civil protest vs. mass violence) and the size of oil wealth. It finds that mass violence is handled through increasing both spending and repression, whereas civil protest is only met by repression. Furthermore, greater oil wealth can relatively increase spending, but only at low and intermediate levels of mass threats. As the threats intensify, the effect of oil wealth dissipates and oil wealth autocracies behave the same as their non-oil wealth counterparts.

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Summaries

Chapter 2. Oil Price Shocks, Protest and the Shadow Economy: Is there a Mitigation Effect?

We study the impact of variations of international oil prices on the incidence of protest, while exploring the role of the shadow economy (SE) as a mitigating factor. On one hand, a significant unexpected decline in oil rents in an oil-dependent economy can impose a variety of challenges for political power. The government, which controls resource rents, often use public sector employment as a redistributive tool to pacify opposition through patronages. It follows that a sudden drop in rents weakens the government's ability to continue making cash transfers and to provide public employment, increasing the risk of social unrest. On the other hand, SE can act as a countercyclical device to buffer against social unrest by providing an alternative source of income for disgruntled citizens during an economic downturn. Furthermore, SE may offer leeway from the distortionary activities of the state in the formal economy, such as administrative corruption, leading to enhanced economic activities in the formal sector. Hence, the SE can simply act as an insurance policy against economic volatility by creating jobs and providing profit opportunities for businesses, which consequently increase the opportunity cost of protesting. Using panel data from 144 countries, from the period of 1991-2015, we find evidence that negative oil price shocks significantly increase protests in countries with small SE. However, the effect dissipates as the size of the SE increases and eventually vanishes in countries with a SE representing more than 35% of gross domestic product. This suggests that the shadow economy's capacity to absorb persistent oil price fluctuations without provoking political unrest, should regard it as a mitigation tool rather than an economic burden.

Wir untersuchen die Auswirkungen von Schwankungen der internationalen Ölpreise auf die Häufigkeit von Protesten und untersuchen gleichzeitig die Rolle der Schattenwirtschaft (SW) als mildernden Faktor. Einerseits kann ein signifikanter, unerwarteter Rückgang der Öleinnahmen in einer ölabhängigen Volkswirtschaft eine Reihe von Herausforderungen für die politische Macht mit sich bringen. Die Regierung, die die Ressourcenrenten kontrolliert, setzt die Beschäftigung im öffentlichen Sektor häufig als Umverteilungsinstrument ein, um die Opposition durch Klientelismus zu besänftigen. Daraus folgt, dass ein plötzlicher Rückgang der Renten die Fähigkeit der Regierung schwächt, weiterhin Geldtransfers zu leisten und öffentliche Arbeitsplätze zu schaffen, wodurch das Risiko sozialer Unruhen steigt. Andererseits kann die Schattenwirtschaft als antizyklisches Mittel gegen soziale Unruhen wirken, indem sie verärgerten Bürgern während eines Wirtschaftsabschwungs eine alternative Einkommensquelle bietet. Darüber hinaus kann die Schattenwirtschaft einen Spielraum vor den verzerrenden Aktivitäten des Staates in der formellen Wirtschaft bieten, wie z.B. Korruption in der Verwaltung, was zu verstärkten wirtschaftlichen Aktivitäten im formellen Sektor führt. Die Schattenwirtschaft kann also einfach

als Versicherungspolice gegen wirtschaftliche Schwankungen fungieren, indem sie Arbeitsplätze schafft und Gewinnmöglichkeiten für Unternehmen bietet, die folglich die Opportunitätskosten von Protesten erhöhen. Anhand von Paneldaten aus 144 Ländern aus dem Zeitraum 1991-2015 finden wir Belege dafür, dass negative Ölpreisschocks die Proteste in Ländern mit kleinen Schattenwirtschaften deutlich erhöhen. Der Effekt schwindet jedoch mit zunehmender Größe der Schattenwirtschaft und verschwindet schließlich in Ländern mit einer Schattenwirtschaft, die mehr als 35% des Bruttoinlandsprodukts ausmacht. Dies deutet darauf hin, dass die Fähigkeit der Schattenwirtschaft, anhaltende Ölpreisschwankungen aufzufangen, ohne politische Unruhen zu provozieren, eher als ein Instrument zur Abschwächung der Krise denn als eine wirtschaftliche Belastung angesehen werden sollte.

Chapter 3. The Impact of Declining Oil Rents on Tax Revenues: Does the Shadow Economy Matter?

We study the association between oil rents and tax revenues, highlighting the importance of the shadow economy (SE) as a moderating factor. Our suggestive evidence and theoretical framework demonstrate that negative shocks in oil rents promote the tax revenues of the state when the size of the SE is sufficiently moderate, whereas they have no significant positive impact on tax revenues when the SE is extensive. The existence of the SE offers a safe haven for businesses and people to conceal their economic activities from tax authorities. It follows that a rise in tax rates will have a limited effect in compensating for a decline in government revenues from oil receipts in the presence of large informal economies and the respective low tax bases. Using a sample of 124 countries from 1991 to 2015, our panel data regression analysis illustrates the moderating role of the SE in the final effect of negative oil rent shocks on tax revenues. A decline in oil rents following negative oil price shocks ceases to have any significant positive impact on tax revenues in countries with an SE representing more than 35% of gross domestic product. The results are robust after controlling country and year fixed effects, other determinants of tax revenues and using a dynamic model.

Wir untersuchen den Zusammenhang zwischen Öleinnahmen und Steuereinnahmen, wobei wir die Bedeutung der Schattenwirtschaft (SW) als moderierenden Faktor hervorheben. Unsere suggestiven Belege und unser theoretischer Rahmen zeigen, dass negative Schocks bei den Öleinnahmen die Steuereinnahmen des Staates erhöhen, wenn die Größe der Schattenwirtschaft hinreichend moderat ist, während sie keine signifikanten positiven Auswirkungen auf die Steuereinnahmen haben, wenn die Schattenwirtschaft umfangreich ist. Die Existenz der SW bietet einen sicheren Hafen für Unternehmen und Personen, die ihre wirtschaftlichen Aktivitäten vor den Steuerbehörden verbergen wollen. Daraus folgt, dass eine Erhöhung der Steuersätze nur eine begrenzte Wirkung hat, wenn es darum geht, einen Rückgang der Staatseinnahmen aus Öleinnahmen auszugleichen, wenn es eine große Schattenwirtschaft und eine entsprechend niedrige Bemessungsgrundlage gibt. Anhand einer Stichprobe von 124 Ländern aus den Jahren 1991 bis 2015 veranschaulicht unsere Paneldaten-Regressionsanalyse die moderierende Rolle der SW bei den endgültigen Auswirkungen negativer Öleinnahmenschocks auf die Steuereinnahmen. Ein Rückgang der Öleinnahmen nach negativen Ölpreisschocks hat in Ländern mit einer SW von mehr als 35% des Bruttoinlandsprodukts keine signifikanten positiven Auswirkungen mehr auf die Steuereinnahmen. Die Ergebnisse sind robust, wenn man länder- und jahresfixe Effekte sowie andere Determinanten der Steuereinnahmen kontrolliert und ein dynamisches Modell verwendet.

Chapter 4. A Resource-Rich Neighbor is a Misfortune: The Spatial Distribution of the Resource Curse in Brazil

We study the spatial distribution of the effect of oil and gas revenues on Brazilian municipalities, using variations in the international prices of oil and gas to establish causality. Oil and gas revenues increase economic activity, measured by night-time light emissions, in oil-producing municipalities but impose negative spill-overs on neighbouring municipalities. Spill-overs dominate beyond 150 km from oil activities and compensate direct effects in micro-regions. In oil municipalities, oil and gas revenues increase royalties, population, local real prices, crime, and real wages, essentially in manufacturing and services. Spill-overs are negative on wages and prices and positive on royalties and crime.

Wir untersuchen die räumliche Verteilung der Einnahmen aus dem Öl- und Gasgeschäft der brasilianischen Gemeinden und nutzen die Variationen der internationalen Öl- und Gaspreise zur Feststellung der Kausalität. Die Öl- und Gaseinnahmen erhöhen die wirtschaftliche Aktivität, gemessen an den nächtlichen Lichtemissionen, in den ölproduzierenden Gemeinden, führen aber zu negativen Auswirkungen auf die benachbarten Gemeinden. Spillovers dominieren in einer Entfernung von mehr als 150 km von den Ölaktivitäten und kompensieren die direkten Auswirkungen in Mikroregionen. In den Ölkommunen erhöhen die Öl- und Gaseinnahmen die Lizenzgebühren, die Bevölkerung, die lokalen Realpreise, die Kriminalität und die Reallöhne, vor allem im verarbeitenden Sektor und im Dienstleistungssektor. Spillovers wirken sich negativ auf Löhne und Preise und positiv auf Lizenzgebühren und Kriminalität aus.

Chapter 5. Autocratic Survival Strategies: Does Oil make a Difference?

This paper examines the behavior of dictators when faced by an imminent threat of being overthrown in oil abundant countries. In the short run, the dictator's survival strategies is argued to be confined to public spending and repression, whereas the choice of their levels is conditional upon the intensity of the mass threat (i.e. civil protest vs. mass violence) and the size of oil wealth. The empirical results indicate a possibility of mixing between spending and repression, and that oil wealth allows for differences in their employed levels in face of the same threat. Using a dataset of authoritarian regimes in 88 countries from 1981 to 2006, I found that mass violence is handled through increasing both spending and repression, whereas civil protest is only met by repression. Furthermore, greater oil wealth is found to provide a wider fiscal space to relatively increase spending, but only at low and intermediate levels of mass threats. As the threats intensify, the effect of oil wealth dissipates and oil wealth dictatorships behave the same as their non-oil wealth counterparts.

Dieses Papier untersucht das Verhalten von Diktatoren angesichts der drohenden Gefahr, in Ländern mit hohem Ölreichtum gestürzt zu werden. Kurzfristig werden die Überlebensstrategien des Diktators auf öffentliche Ausgaben und Repressionen beschränkt, während die Wahl ihrer Höhe von der Intensität der Massenbedrohung (d.h. ziviler Protest vs. Massengewalt) und dem Umfang des Ölreichtums abhängt. Die empirischen Ergebnisse zeigen eine Möglichkeit der Vermischung von Ausgaben und Repression, und dass der Ölreichtum angesichts der gleichen Bedrohung Unterschiede in der Beschäftigungsquote zulässt. Anhand eines Datensatzes über autoritäre Regime in 88 Ländern von 1981 bis 2006 fand ich heraus, dass Massengewalt durch Erhöhung der Ausgaben und Repressionen bewältigt wird, während der zivile Protest nur durch Repression begegnet wird. Darüber hinaus wird festgestellt, dass ein größerer Ölreichtum einen größeren fiskalischen Spielraum für eine relative Erhöhung der Ausgaben bietet, aber nur bei niedrigen und mittleren Niveaus von Massenbedrohungen. Mit zunehmender Verschärfung der Bedrohungen verschwindet die Wirkung des Ölreichtums, und Ölreichtum-Diktaturen verhalten sich genauso wie ihre Pendants, die keinen Ölreichtum haben.

List of Publications

Ishak, P. W., and Méon, PG (2022). A Resource-Rich Neighbour is a Misfortune: The Spatial Distribution of the Resource Curse in Brazil. Economic Development and Cultural Change, *forthcoming*

Ishak, P. W., and Farzanegan, M. R. (2022). Oil Price Shocks, Protest and the Shadow Economy: Is there a Mitigation Effect? *Economics & Politics*, 34 (2), 298-321.

Ishak, P. W., and Farzanegan, M. R. (2020). The Impact of Declining Oil Rents on Taxation: Does the Shadow Economy Matter? *Energy Economics* 92, 104925.

Ishak, P. W. (2019). Autocratic survival strategies: Does oil make a difference? *Peace Economics, Peace Science and Public Policy*, 25, 1–22.