Petroleum Resources and Internal Armed Conflict

May Countries' Institutional Legacy or Conflict Legacy Condition the Conflict-Inducing Effect of Oil?

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Abstract

What explains oil countries' susceptibility to experience internal armed conflict? While this topic has received considerable scholarly attention in recent years, the question remains unsettled. This thesis introduces the perspective that differences in countries' institutional and societal starting points may explain the divergent experience of armed conflict among oil countries. Two potential conditioning circumstances of the relationship between oil income and risk of armed conflict are explored: a country's *institutional legacy* and a country's *conflict legacy* at the commencement of oil income. Differences in countries' institutional legacy is argued to anticipate divergent institutional trajectories in the wake of oil, with differential implications for conflict risk. Conflict legacy, on the other hand, is argued to proxy unusual opportunities of rebellion against oil-financed regimes with the best of available means to buy-off or repress opposition. The merit of these arguments is tested by logistic regression analysis of armed conflict onset among 170 oil and non-oil countries in the years 1961 to 2007. Findings indicate that for two indicators of institutional legacy: *initial bureaucratic control* and *initial education attainment*, there is partial support that institutional legacy mediates the effect of oil income on conflict risk. Under certain conditions, there is significant support that oil increases conflict risk among countries at the lower scale of initial institutions, while this effect is reduced and becomes inseparable from zero at the robust end of initial institutions. Conflict legacy, however, is found *not* to mediate the effect of oil, but rather to have a significant individual impact on conflict risk regardless of size of oil income. In sum, these findings suggest some new answers to the question of why the curse of armed conflict befalls some, but not other oil countries. Further, they support the notion that the relationship between oil income and conflict risk may be explained by the synergies between states' institutional capacities and oil income, at the same time as they underscore that the impact of oil on institutions and conflict risk may not be unidirectional.

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The contents and errors of this thesis are my responsibility.

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1 Introduction

This thesis engages in the scholarly debate on why oil-producing countries are more prone to experience internal armed conflict than other countries. The overarching aim of the thesis is to examine whether differences in countries' institutional legacies or conflict legacies at the commencement of oil income may account for the divergent experience of armed conflict among oil countries.

While a range of studies concur on the finding that that oil wealth stands out as a salient determinant of internal armed conflict onset (de Soysa, 2002; de Soysa & Neumayer, 2007; Fearon, 2005; Fearon & Laitin, 2003; Humphreys, 2005; Lujala, 2010; Ross, 2006, 2012), there is no consensus within the literature as to exactly *how* the identified relationship between oil wealth and heightened risk of armed conflict comes about. Suggested explanations tend to follow two contrasting paths: First, *rebel-centered* explanations which focus on petroleum resources as a financial or motivational source of rebel organizations; and second, *state weakness*-centered explanations which assert that the effect of oil on conflict risk works indirectly by weakening state institutions and economic performance.[1]

Despite an array of suggested causal explanations, the question of which mechanisms mainly underlie and generate the relationship remains unsettled. To date, scholars disagree on this issue, and empirical analyses provide no unambiguous support of either causal story. In addition, present theorizing leave two *puzzles* unresolved. First, previous literature has little to say about why oil engenders ferocious armed conflict in some countries, while other oil-producing countries emerge untouched by this curse. Second, there are opposing projections on the issue of feasibility of rebellion in oil wealthy countries: While one branch of explanations emphasize that oil induces fragile institutions and offers financial sources of organizing rebellion, another branch holds that oil wealth provides regimes with exceptional opportunities to repress or co-opt potential opposition. In light of such contradicting notions, rebels' capacity to mount armed challenge to regimes with oil-financed counterinsurgency capacities and co-optative opportunities stands out as paradoxical.

[1] Rebel-centered causal explanations include the *honeypot* mechanism, the *availability of finance* mechanism, and the oil-induced *grievance* mechanism. State weakness-centered explanations include the *bureaucratic weakness* mechanism, the *government detachment* mechanism, the *rentier state* mechanism and the *vulnerable economy* mechanism.

Addressing the gaps in our knowledge on the relationship between oil wealth and armed conflict, this thesis engages in the search of salient conditioning circumstances. I will introduce a perspective which has been missing in the debate so far; that countries' very different societal and institutional starting points when oil income commences may be distinguishing for their subsequent propensity to experience armed conflict. With reference to both rebel-centered and state-weakness centered causal arguments; two conditions stand out as potentially salient in conditioning the effect of oil on conflict risk: a country's initial institutional capacity (*institutional legacy*) and a country's recent history of organized armed conflict (*conflict legacy*) when oil income commences. This leads me to the following research objective:

Research objective: This thesis explores two conditioning factors in the relationship between petroleum resources and conflict risk. The two are a country's *institutional legacy*, and a country's *conflict legacy*, at the point in time when oil income commences.

Refining explanations of the relationship between oil and armed conflict is not only desirable for the purpose of theory improvement. Petroleum reserves are increasingly being explored in poor and unstable countries. Kenya, Ghana, Ethiopia, Uganda, Tanzania, and Mozambique are among the countries anticipating to become petroleum exporting countries within the coming few years (Ross, 2012, p. 10). Improving our knowledge of the relationship between oil and armed conflict may aid policymaking aimed at helping these countries avoid the potential pitfalls and dangerous side-effects of oil.

How may institutional legacy and conflict legacy be expected to condition the effect of oil on countries exposure to armed conflict? What do these conditions *add* to present conceptualizations of the relationship?

First, institutional legacy, here understood as the initial capacity of state institutions in terms of bureaucratic control and quality of public service provision; may anticipate whether or not state-centered and rebel-centered mechanisms will be triggered following oil income. While state-centered explanations hold that oil income regularly induce bureaucratic weakness and unaccountable governments, which in turn increase feasibility and motivations for rebellion, I argue that the robustness of such institutions at the commencement of oil income may play a distinguishing role for whether such institutional trajectories will arise or not.

Why may differences in countries' conflict legacy be an important conditioning factor for the effect of oil on conflict risk? Presence of armed conflict may serve as an indicator of deficient state capacity, which according to present theory has poor prospects for improving following the onset of oil income into government coffers. Perhaps more important, however, is that structural and organizational legacies of conflict that persist in the post-conflict environment may serve as an important aid of future rebellion following oil. If the capacity of rebels' to organize armed revolt against oil-financed regimes is a paradox, as is indicated by opposing projections on this point in previous theorizing, then organizational legacies and conflict-specific capital inherited from previous conflict may be the answer.

The observable implications of my theoretical arguments are specified in three hypotheses:

> **H1:** The conflict-inducing effect of oil income will be higher in countries with low bureaucratic control than in countries with high bureaucratic control at the commencement of oil income
>
> **H2:** The conflict-inducing effect of oil income may be lower in countries with high quality of public service provision than in countries with low quality of public service provision at the commencement of oil income
>
> **H3.** Oil income increases conflict risk more severely in countries with a legacy of armed conflict than in countries without such a conflict legacy at the commencement of oil income

To test these expectations against empirical observations I conduct a logistic regression analysis of armed conflict onsets among 170 oil and non-oil countries within the years 1961-2007, as well as in a sample of 100 oil countries within the same period. Comprehensive data on the value of oil produced in countries worldwide since 1960 is obtained from Ross (2012). Countries' institutional legacy is proxied by indicators of bureaucratic quality and education attainment ratios, measured at the initial year of oil income. Conflict legacy is assessed on the basis of data on armed conflicts since 1946, measured within a period of ten years preceding first oil income.

Main Findings

The findings obtained in the empirical analysis partially support the proposition that the effect of oil on conflict risk is conditional on countries' institutional legacy (H1 and H2). For both measures of institutional legacy, findings indicate that the conflict-inducing effect of oil income is highest among countries with a fragile institutional legacy in terms of initial

bureaucratic control and initial education attainment. The better performance of initial institutions, the lower is the estimated conflict-inducing effect of oil income. Under certain conditions, there is significant support that oil increases conflict risk among countries at the lower scale of initial institutions, while this effect is reduced and becomes inseparable from zero at the robust end of initial institutions.

Findings do not support the proposition that the effect of oil may be conditional on countries' conflict legacy (H3). When countries with an *unknown* conflict legacy are treated as a separate category, having or not having a conflict legacy does not differentiate the effect of oil on conflict risk. Yet, although conflict legacy can't be understood to mediate the effect of oil on conflict risk, there is clear support that conflict legacy prior to oil independently adds to conflict risk, regardless of the size of oil income. Graphed results show that for any sizes of oil income, predicted probability of onset is substantially higher among countries with a conflict legacy than without. Bivariate analysis of the relationship between armed conflict onset and pre-oil conflict legacy among oil countries support the significance of this finding.

Robustness tests show that the consistency of these findings is not threatened by high multicollinearity among explanatory variables, and that they are not driven by a few influential cases. Rather, exclusion of a few influential observations yields stronger support of the theoretical argument that initial institutions may distinguish the effect of oil. Initial education is found to significantly condition the effect of oil on conflict risk when the logistic regression assumption of no large outliers is addressed.

Structure of the Thesis

Chapter 2 defines a number of core concepts used throughout the thesis. Chapter 3 lays out my own theoretical argument and hypotheses, after identifying current knowledge gaps in the study of oil and internal armed conflict. Chapter 4 develops the quantitative research design applied in the test of hypotheses. Results, interpretations and robustness tests are presented in Chapter 5, while Chapter 6 concludes the endeavor.

2 Definitions

This section defines a number of central concepts, in order to be specific about their meaning within the context of the theoretical framework employed here. The most important concepts include internal armed conflict (and conflict legacy), petroleum resources, institutional capacity (and institutional legacy), and causal mechanisms.[2]

2.1 Internal Armed Conflict

The main phenomenon of interest in this thesis is internal armed conflict. I follow the definition employed by the Uppsala/PRIO Conflict Data Program (Gleditsch et al., 2002).[3] Their theoretical definition differentiates intra-state conflicts from armed conflicts that occur between states, and from other forms of violence that occur within states.

Most importantly, internal armed conflict is here defined as a phenomenon that involves armed competition for state control between the government of a state and an organized opposition group, which challenges a present regime's control over central government or a part of the territory.[4] Distinguishing internal armed conflict from other types of violence such as terrorism, genocide or organized crime, or even international war, may however not be uncomplicated, and the lines between internal armed conflict as it is defined here and other types of violence occurring within states sometimes come across as arbitrary (Sambanis, 2004, p. 815). It is also not a trivial question whether petroleum resources may relate to other forms of political violence, such as interstate war and non-state violence.[5]

Yet, when studying the relationship between petroleum resources and armed conflict, it does make sense to study the causes and variations of internal armed conflict separately from those

[2] This section discusses conceptual delimitations of the terms used. Operational definitions are found in section 4.2 of the Research Design.

[3] See also the full definition at the UCDP website: http://www.pcr.uu.se/research/ucdp/definitions/

[4] As such, it may be regarded distinct from armed contestation between non-state groups. Short of stated political incompatibilities, criminal armed violence also falls outside the definition. The organized aspect differentiates internal armed conflict from spontaneous forms of violence that might take place between civilians and government.

[5] Petroleum resources may for instance provide motivation for interstate invasion, such as in Iraq's attempt to annex Kuwait in 1990; or give rise to border disputes, such as presently occurring between Lebanon and Israel following discovery of off-shore petroleum resources, Thailand and Cambodia, etc. Petroleum extraction has also been at the core of for lethal non-state violence, such as in the dispute between Huaorani and Tagaeri communities in remote jungle areas in Ecuador (UCDP Conflict Encyclopedia, 2014).

of inter-state or non-state armed conflict: They involve different sets of actors, and plausibly also different sorts of causal mechanisms.

The term *conflict legacy* is here employed to refer to a country's experience of internal armed conflict during the years preceding first oil income.

2.2 Petroleum Resources

This thesis focuses exclusively on the relationship between petroleum resources and armed conflict, and leaves the question of the impact of other natural resources on conflict risk unaddressed.

While natural resources was studied as a unitary condition with a purported unitary impact on conflict risk in the initial stages of the debate on natural resources and civil war, there are both empirical and conceptual justifications for studying the effect of petroleum resources separately. Empirically, there is robust support for a relationship between petroleum resources and conflict, while the same is not true for natural resources in general (de Soysa, 2002; Elbadawi & Sambanis, 2002; Fearon & Laitin, 2003; Hegre & Sambanis, 2006).

Conceptually, there are clear advantages with studying the impact of petroleum resources separately from natural resources in general. Natural resources are a broad category which encompasses a wide range of different resources, such as timber, agricultural and aquacultural produce, petroleum, and so forth. These resources differ greatly in terms of physical features, modes of extraction, "lootability", trading options, as well as in the opportunity of revenue they represent for rulers and rebels.[6] There is a commonly recognized point within the literature that different types of resources may vary in their impact on armed conflict, by different causal paths (see for instance de Soysa & Neumayer, 2007, p. 205; Le Billon, 2001, p. 570; Lujala et al., 2005, p. 542; Lujala & Rustad, 2012, p. 10; Snyder & Bhavnani, 2005, p. 568). Studying petroleum resources separately thus facilitates more precise causal argumentation and empirical examination.

So what are the defining features of petroleum as a natural resource? Materially, petroleum refers to different types of hydrocarbon compounds, including crude oil and natural gas,

[6] This point has been particularly pronounced in criticisms of the widely used primary commodity exports measure (Fearon, 2005, p. 486; Lujala et al., 2005, p. 542; Ross, 2006, p. 272).

which are naturally occurring and found in upper strata of the earth.[7] In Snyder and Bhavnanis' typology (2005, p. 568), petroleum represents a typical "non-lootable" natural resource, which requires large amounts of capital, technology and infrastructure to extract. Non-lootable resources may not be easily or profitably exploited by individuals or groups, except if they control the state.[8] Oil and gas typically presents favorable revenue opportunities for governments, as the high barriers to entry facilitates a great degree of government control over actors involved in the extraction process.[9]

As reminded by Le Billon (2001, p. 565), the value assigned to natural endowments such as oil are products of historical processes of social construction, and derive from human desires, needs and practices. Thus, the value of petroleum arises from its intrinsic role in most practices of modern human life (transport and machinery fuel, heating, production of thousands of things such as synthetic fabrics for clothes and equipment, tires, refrigerators, makeup, medicines, etc.).

The massive size of revenue generated by oil sales also differentiates oil from other types of natural resources. The size of government revenues in oil-producing countries (as fraction of GDP) is on average nearly 50% greater than that of non-oil countries (Ross, 2012, pp. 5, 27). The source of this revenue is not unimportant: oil-rents are considered an "easy" source of riches, unreliant on the state's ability to raise tax from its population. The magnitude and source of rents accruing to governments from petroleum sales is both a defining feature of this natural resource and an argument for studying the impact of oil apart from other natural resources.

2.3 Institutional Capacity

A central proposition of this thesis is that states' legacy of institutional capacity may distinguish the conflict-inducing effect of oil. This places my thesis within the scholarly debate on (state) institutional capacity and civil war.

[7] I use the terms petroleum and oil interchangeably throughout the thesis.
[8] This stands in contrast to more readily available natural resources such as surface diamonds and gemstones.
[9] Governments may establish monopoly control either by nationalizing their oil industry, or by monitoring and taxing private companies involved in the extraction process.

In the study of civil conflict, state capacity and institutional capacity are vague theoretical concepts, which may subsume a range of different aspects. Attempts to systematize the different connotations include Hendrix (2010), and Fjelde and de Soysa (2009). Both contributions places relevance on how the performance of state institutions may affect the motive-and-opportunity structure of would-be rebels, contrasting capacities of repression or accommodation as alternative routes to limit the likelihood of rebellion.

In this thesis, I particularly target two aspects of state institutional capacity: bureaucratic control and public service provision. These two aspects may be representative of the two different aspects of state capacity mentioned above: repressive/administrative capacity and accommodative/co-optative capacity.

A state's bureaucratic control may indicate its ability to monitor and administer its population, and the degree of people's submission to state regulation. In Fearon and Laitins' account (2003: 79), administrative and organizational strength is at the core of governments' capacity to detect and deter rebellions before they materialize. This capacity not only rests on states' ability to collect and manage information about its citizens, but is also sustained by the geographical reach of government institutions into rural areas and ability stay informed about goings-on at the local level.

Public service provision may closely relate to an alternative source of state capacity, namely the ability of the state to accommodate its people. In Fjelde and de Soysa (2009, p. 9), co-optation of potential opposition by political goods expenditure is argued to one of the main routes by which governments retain the loyalty of segments of society. In my theoretical framework presented further on, the quality of public service provision proxies the reciprocity of state-society relations. Implicit is the argument that a government that is able to learn and respond to citizens' needs, will reduce motivations for revolt relative to a government that is detached from its populace.

The term *institutional legacy* in this context refers to the quality of bureaucratic control and of public service provision in a country at the point in time when oil income commences.

2.4 Mechanisms

As causal mechanisms are a central focus of this thesis, I will in the following say a few words on what they conceptually are.

In everyday terms, mechanisms may be understood as the causal path (or causal narrative) linking a purported explanatory condition to an observed outcome. Mechanisms are central components of any causal explanation of social outcomes (Elster, 1989, p. 3).[10] While no standard agreed upon definition of mechanisms is available, many definitions place social mechanisms somewhere in between the law-like relations of natural science and mere particularistic accounts of single events.[11] Causal mechanisms in social sciences are usually conceptualized as probabilistic rather than deterministic causal paths, yet occurring in recurring and recognizable patterns.[12]

In this thesis, the mechanism term is used to denote the different causal paths presented as explanations of the correlation between petroleum resources and armed conflict.[13]

[10] Gerring (2005, p. 171) defines mechanisms as "the causal narrative" linking cause to effect, and constitutes one of several formal criteria of causal explanations in social sciences.

[11] The term is found to carry different connotations among different users, possibly reflecting different ontological and epistemological positions. I refer to Mahoney (2001, p. 579) for a review of different definitions of mechanisms, as well as to Hedström and Swedberg (1996) for an attempt to conceptualize the differences.

[12] Elster's (1998) attempt to pin down the core of the term may serve as an example of this reasoning: "Roughly speaking, mechanisms are frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences" (Elster, 1998, p. 45).

[13] In the review of previous literature, the terms causal mechanism, causal argument and causal explanation are used interchangeably to denote mechanisms that are suggested to explain the oil-armed conflict relationship. While it may occur as imprecise to mix these terms, in reality the distinct mechanisms form central components of distinct causal arguments of what explains the relationship.

3 Theory

This chapter will lay out the theoretical foundations of my research objective, and present the theoretical claim of this thesis. A thorough review of prevailing explanations in previous literature reveals that there are critical gaps in our ability to explain the established relationship between oil and conflict risk. There is disagreement among scholars on whether *state-centered* or *rebel-centered* mechanisms mainly account for this relationship. A joint theoretical shortcoming of the explanations is that they fail to address why oil induces conflict in some countries, but not in others.

The perspective introduced by this thesis is that differences in countries' institutional and societal starting points may importantly anticipate the effect of oil on conflict risk. More specifically, I argue that countries' *institutional legacy* and *conflict legacy* at the commencement of oil income may plausibly condition the effect of oil income on conflict risk. First, variations in countries' *institutional legacy* may influence whether or not the conflict-inducing mechanisms suggested in previous literature will be triggered following oil income. While many theorists claim that oil income prevents the development of institutional capacity facilitative of societal peace, including bureaucratic control and reciprocity of state-society relations, I argue that this is a plausible path only from a fragile institutional starting point. In countries with robust institutional capacity developed prior to the commencement of oil, such institutions may be expected to persist rather than vane (upheld by shared norms, practices, expectations, organizational structures, physical offices etc.), and offset the conflict-inducing effects of oil revenue.

The second proposed conditioning factor, *conflict legacy*, is suggested to answer the paradox of rebel capacity of organizing armed revolt against oil-financed regimes with unusual means to repress or buy-off political opposition. It is argued that facilitative legacies from a previous conflict such as organizational capital and a latent support base among the population could be distinguishing for the capacity of rebels to mobilize against the regime in the wake of oil.

Structure of the Chapter

In the following, I begin with an examination of the variety of causal arguments and findings presented in research on this topic thus far. The section reveals a number of shortcomings in available theoretical explanations of the oil-armed conflict relationship. The second section

introduces my own theoretical perspective which addresses the gaps identified in previous research. The theoretical claims presented in this section will be summarized in a few testable hypotheses.

3.1 Knowledge Gaps in the Study of Oil and Armed Conflict

The scholarly debate on petroleum resources and armed conflict may be viewed as a branch of a larger debate on the role played by natural resources for countries' exposure to civil war.[14] While the claim of a general conflict-inducing effect of natural resources has proved non-robust in most empirical studies (de Soysa, 2002; Elbadawi & Sambanis, 2002; Fearon & Laitin, 2003; Hegre & Sambanis, 2006), *petroleum resources* have been found to be a particularly robust and sizeable determinant of armed conflict across a notable number of studies (de Soysa & Neumayer, 2007; Fearon, 2005; Humphreys, 2005; Lujala, 2010; Ross, 2006, 2012).[15] Although the conflict-inducing effect of oil has been corroborated by numerous quantitative and qualitative works, the question of which causal path, or causal mechanism, mainly underlies and generates this relationship is far from settled.

In the following, I will review prominent explanations of the oil-armed conflict relationship which figure in the literature. The purpose of the section is to show that there are a number of problems with present explanations that need to be solved if we are to improve our understanding of this relationship. The review identifies three main shortcomings of available explanations. First, scholars disagree on whether rebel-centered or state-centered mechanisms mainly generate the relationship, and empirical findings provide no conclusive support to either story. Second, the suggested mechanisms may not explain why oil only in some instances bring about armed conflict, while in other instances not. Third, rebels' capacity to

[14] This debate gained momentum with Collier and Hoeffler's (1998, 1999; 2004 and more) identification of natural resources as the single most important determinant of civil war, coupled with the controversial claim that rebel greed and financial opportunities better explains the occurrence of conflict.

[15] In a decisive critique of Collier and Hoeffler's studies on natural resources, Fearon (2005) show how these theorists own results become insignificant when their missing data are recovered and sample frame is adjusted. Fearon suggests that the positive relation between natural resources and armed conflict identified by Collier and Hoeffler may be due to omitted variable bias: Oil is a major component of primary commodity exports, and also correlates significantly with conflict risk. Even in Collier and Hoeffler's (2004) own study, only oil correlates significantly with conflict risk when they disaggregate the primary commodity exports variable into different types of commodities (Collier & Hoeffler, 2004, p. 580).

mount armed challenge to oil-financed regimes remains a paradox which none of the suggested mechanisms are adequately able to resolve.

In previous literature, explanations of the relationship between oil and armed conflict tend to follow two main lines: rebel-centered and state-centered explanations.[16] In rebel-centered explanations, oil directly influences rebel's motivations or financial opportunities to carry out armed insurgency. In state-centered explanations, the effect of oil runs indirectly via the detrimental impact of oil income on state institutions and countries' economic performance. The review will first present the various rebel-centered and state-centered explanations, before I discuss their theoretical and empirical shortcomings in the subsequent section.

3.1.1 Rebel-Centered Explanations

Rebel-centered explanations of the relationship between petroleum resources and armed conflict include the *honeypot* argument, the *availability of finance* argument and the *grievance* argument.

The Honeypot Mechanism

The honeypot argument posits that presence of petroleum resources may directly influence motivations for contenders to seize power because such high value natural resources increases the value controlled by incumbent authorities and thereby increases the "prize" of state control. Some scholars have argued that this may motivate armed contest over government (Fearon, 2005; Fearon & Laitin, 2003).[17] Others argue that this motive in particular incites

[16] Due to the large number of explanations of the oil-armed conflict relationship found in previous literature, the division into rebel-centered and state-centered explanations may occur as a simplification. Yet, similar categorizations of explanations recur in the literature (see for instance Basedau & Wegenast, 2009, p. 38; de Soysa & Neumayer, 2007, p. 202; Lujala, 2010, p. 16). One problem with the distinction is that it is not always so clear whether the focus is primarily on the state or on rebels' incentives or capacities, as the explanations often comment both. Mechanism reviews are also found in Humphreys (2005) and Ross (2004a, 2004b, 2006).

[17] In Fearon's words, "easy riches from oil make the state a more tempting prize relative to working in the regular economy"(Fearon, 2005, p. 487). Yet this is only half of Fearon (2005), and Fearon & Laitins' (2003) causal argument, as their focus is on oil's detrimental impact on states' administrative and bureaucratic capacity.

secessionist attempts as the potential economic gains from controlling a region increase considerably with the availability of petroleum resources (Collier & Hoeffler, 2012, p. 4).[18]

The Availability of Finance Mechanism

The availability of finance (or "looting") argument is rooted in the initially publicized "greed"-based or economic opportunity explanation of the relationship between natural resources and armed conflict. Natural resources may represent a source of finance which may aid rebels in overcoming organizational barriers to insurgency (Collier & Hoeffler, 2004). While this argument may explain insurgencies in countries with easily lootable resources such as alluvial diamonds (Lujala et al., 2005), it appears of less relevance in the case of petroleum resources, as their non-lootable characteristics limit their revenue raising potential to rebel groups (Snyder & Bhavnani, 2005, p. 568). Yet, petroleum resources may provide indirect opportunities of rebel finance through theft and extortion from oil companies (Collier & Hoeffler, 2012, p. 6; Ross, 2006, p. 281), or by the sales of future exploitation rights to companies (so-called "booty futures") (Ross, 2004a, p. 58). However, such activities may not improve the relative capacity of rebels vis-à-vis the government, given incomparable levels of funds accruing to each side.[19] Moreover, ransom and other types of payments from oil companies to rebel groups have become increasingly difficult due to legislation and increased public pressure (Collier & Hoeffler, 2012, p. 6).

The Grievance Mechanism

The final rebel-centered argument connecting petroleum resources to armed conflict is that focusing on grievances, which occurs in different versions.[20] First, oil extraction may generate a sense of relative deprivation among the populace insofar as increasing expectations for redistribution, deliverance of public goods and work opportunities are unmet (Humphreys, 2005). It is conceivable that such expectations may become particularly salient in the regions which harbor the resource reserves, due to locals' perceptions of ownership to the resource in

[18] According to Le Billon's typology of natural resources, oil constitutes a "point resource" (meaning that it is geographically concentrated and exploitation involves extractive industries), and is likely to motivate rather than finance rebellion. The physical location, whether proximate to the power center or peripheral, will further influence whether it incites armed contest over government or secession (Le Billon, 2001, p. 573).

[19] This point is also emphasized by Fearon (2005, p. 502), Thies (2010, p. 323), and Snyder & Bhavnani (2005)

[20] This section only covers the grievance related arguments that propose a direct effect of oil production on social grievances, not the arguments that emphasize features of governance as a primary stage in the causal path.

question. A related argument holds that social and environmental degradation emanating from the extraction process, combined with the failure to bring gainful returns to local communities, incite rebellions in resource rich regions (Lujala & Rustad, 2012, p. 8).[21]

3.1.2 State-Centered Explanations

Turning to the other main branch of causal explanations: State-centered arguments purport that the conflict-inducing effect of oil runs indirectly via the detrimental impact of oil on state capacity. This argument also comes in different versions; the *bureaucratic weakness* argument, the *government detachment* argument, and the *vulnerable economy* argument. Central to each version, however, is the notion that availability of oil income creates states with weaker state institutions due to a reduced need of extracting taxes from the population.[22] Lacking incentives to develop socially intrusive institutions to collect taxes from citizens, the institutions facilitative of societal peace are also undermined.

A related, but contrasting argument is offered by *rentier-state theory*, which holds that oil wealth provides the means for regimes to *reduce* the risk of armed opposition through repression or co-optation. The implication of this argument relating to conflict risk thus contradicts the other arguments.

The Bureaucratic Weakness Mechanism

First, the state weakness argument proposed by Fearon and Laitin (2003) and Fearon (2005) focuses on how oil revenues' reductive impact on bureaucratic institutions increase the feasibility of rebellion in oil wealthy states. When states' financial dependence on taxes from the population diminishes, they fail to develop the administrative and bureaucratic control throughout their territories that would otherwise be requisite to their capacity to tax. This in turn increases both the feasibility and the likelihood of rebellion, as states' administrative presence and control throughout territories is at the core of their counterinsurgency capacity in terms of their ability to forestall the emergence of insurgencies. Oil countries, they argue, tend

[21] Social and environmental problems associated with oil production may include land expropriation, pollution of drinking water, -soil, hunting and fishing grounds, rapid labor migrations causing social disruption as well as low work opportunities for locals (Ross, 2004a, p. 41).

[22] This stage in the causal chain is a central element of- and borrowed from rentier state theory: for elaborate reviews of this theory see Ross (1999, p. 312); Smith (2004, p. 233).

to have weaker bureaucratic institutions than other countries with the same per capita income, and this explains their higher propensity to experience insurgencies.[23]

The Government Detachment Mechanism

Next, the government detachment argument[24] focuses on how oil-funded governments' reduced need of taxation creates states with weak state-society linkages and weak accountability of rulers. The impact on conflict risk is rather complicated however; as the literature diverges over which direction the relationship takes. On one hand, this mechanism may increase the risk of armed conflict by engendering grievances and making the state unable to redress such grievances. On the other hand, rentier state theory suggests that oil-rich governments that are less accountable to their constituencies may more freely use oil rents strategically to prevent armed opposition.

Oil-funded states that do not depend on raising tax from the population may bypass an important mechanism for strengthening the reciprocity of state-society linkages and state responsiveness to societal demands. Taxation provides citizens with information of government activity; create incentives to monitor government behavior, and demands for return provisions such as participation, accountability and services (Humphreys, 2005, p. 512). States that are financially dependent on continued taxation have incentives to comply with such demands; cooperation being less costly than coercion. In contrast, governments financed by non-tax revenue, such as oil-funded states, have lower incentives to comply with demands of accountability and representation, as they do not rely on continual taxation. As a consequence, governments less dependent on tax extraction for their survival may become more detached from their constituencies, and less responsive to their demands (Collier & Hoeffler, 2012, p. 7; Humphreys, 2005, p. 512).

[23] Fearon and Laitin proxy state strenght by GDP per capita, and argue that oil-producing countries have lower state capacity than other countries at the same GDP per capita. One problem of this measure is that it is pretty distant to the phenomenon which they theoretically claim to define states' counterinsurgency capacity: namely institutional outreach and bureacratic control. It might seem that their measure is based on a theoretical inference that oil countries by rule or nature have lower institutional outreach and control throughout territories rather than it being a valid and unambigous measure to really test the implications of their argument. Another problem suggested by (Ross, 2004a, p. 36) is that GDP per capita might be endogenous to conflict. When armed conflict is anticipated or takes place, manufacturing business may leave and reduce economic output. This way, lowered GDP may be a result of conflict rather than a valid predictor of armed conflict.

[24] This argument resembles - but is not equivalent to - rentier state theory. Rentier state theory was developed from studies of oil-wealthy Middle-Eastern countries, and suggests particular theoretical propositions which are not shared by the argument described here. The specific propositions of the rentier state theory are returned to.

Lack of structures of interaction between rulers and ruled, and lack of incentives to respond to societal demands may plausibly inhibit the ability of governments to resolve grievances among the public. Failure to address increasing aspirations for redistribution and gains from high value resources may add tension and increase the likelihood of violent conflict through a relative deprivation mechanism.

The Rentier State Mechanism

The rentier state theory points out an opposite path. While oil-rich governments may become more disconnected from their electorate, they may employ oil rents strategically to pacify potential opposition: for instance via increased expenditure on patronage, social welfare spending, bolstering their repressive apparatus, and preventing the formation of independent social groups or social capital (Basedau & Lay, 2009). Following this reasoning, oil wealth may be utilized to decrease the risk of armed opposition through strategic co-optation and corruption (Fjelde, 2009), and prolong the durability of oil wealthy authoritarian regimes (Smith, 2004).[25]

The Vulnerable Economy Mechanism

A final version of state-centered causal explanations of the oil–armed conflict link focuses on the damaging impacts of oil income on countries' economies. Development economists have long argued that countries' richness in natural resources in general, and in oil in particular, paradoxically hampers economic growth (Sachs & Warner, 1995). Economic dutch disease, combined with governance related conditions such as economic mismanagement; corruption and private rent-seeking are often cited as causes of such economic outcomes.[26] Low per capita income, and low economic growth, in turn, has been identified among the most robust

[25] Via a similar process, oil wealth may influence the risk of conflict indirectly by blocking democratic transitions. Oil wealth is found to strongly inhibit democratic transitions when oil-producing countries are compared with non-oil countries (Andersen & Ross, 2013; Ross, 2012). The causal mechanism is about the same as the one depicted above: oil rich governments may use oil rents to relieve social pressures that would otherwise promote accountability and representative government. Yet, oil countries' failure to democratize may not imply a unidirectional impact on conflict risk, and the potential link between oil wealth, democracy and conflict has not been further examined in current conflict literature.

[26] Economic outcomes such as low economic growth in resource abundant countries are not inevitable, but mediated by policy choices made by political elites, and institutions that constrain the way resource revenues are spent. Mehlum et al. (2006) demonstrate that the effect of natural resource revenues on countries performance in economic growth importantly depends on the quality of political institutions, whether they stimulate unproductive "grabbing" hampering growth, or productive entrepreneurship which stimulate growth.

determinants of armed conflict (Collier et al., 2009; Fearon & Laitin, 2003; Hegre & Sambanis, 2006).[27]

The suggested link to armed conflict is that low economic opportunities in the regular economy make recruitment to insurgent groups unusually cheap. Another explanation, suggested by Humphreys (2005), is that failure to develop thick domestic trade networks in resource dependent economies heightens the risk of conflict, because internal trade generates high incentives to maintain peace within societies, while low levels of internal trade may imply a comparably lower extent of such incentives.

3.1.3 Continuing Empirical and Theoretical Puzzles

With so many mechanisms suggested to explain how petroleum resources increase the risk of armed conflict, what is left to explain? Closer inspection of the theoretical literature and empirical analyses supporting it reveals that the question of which mechanism mainly underlies and produces the observed correlation is actually far from settled. The purpose of the following section is to show that continued attention to this topic is justified, given that (1) there is no clear empirical support of any of the causal mechanisms; scholars disagree on this issue, and (2) there are remaining theoretical puzzles that need to be addressed. In particular, the explanations insufficiently explain variation in the dependent variable. Moreover, there is a considerable theoretical ambiguity as to whether oil wealth facilitates- or reduces the feasibility of rebellion.

Contradictory and Inconclusive Empirical Findings

None of the mechanisms presented above are substantiated by conclusive empirical support. While theorists like Humphreys (2005) and de Soysa and Neumayer (2007) present findings in support of the state-centered mechanisms, other theorists, including Ross (2012) and Lujala (2010), present findings in support of rebel-centered explanations while they object to the soundness of state-centered mechanisms. Findings on the impact of *natural* resources on secessionist conflict, presented in alleged support of rebel-centered mechanisms, prove

[27] Other economic maladies argued to befall oil dependent economies with implications for conflict risk include exposure to trade shocks due to volatile world market prizes (Ross, 2006, p. 291), enclave economies in which oil industry dominates while other sectors are crowded out due to Dutch disease and failure to employ policies for economic diversification (Dunning, 2005, p. 453).

unrobust in similar analyses on *petroleum* resources and conflict type. These points are further elucidated in the discussion below.

Humphreys (2005) finds that past oil income, albeit not oil reserves, significantly increases the risk of conflict onset. In his interpretation, this supports the weak state mechanism, while it challenges the honeypot (or rebel greed) argument.[28] Yet, this is not the only possible interpretation. Correlation between past oil income and increased risk of armed conflict may also run through a rebel grievance causal path, possibly also rebel finance, however theoretically unlikely due to the limited lootability of oil.

De Soysa and Neumayer (2007) are like Humphreys in favor of the weak state mechanism, based on their distinction between energy rents and mineral rents as independent variables.[29] The latter is constructed to capture the size of a country's non-lootable rents in a given year, whereas the former is supposed to capture the size of lootable rents. As they find that only energy rents, but not mineral rents, significantly increase the likelihood of conflict, they conclude that this supports the weak state mechanism while it challenges the looting mechanism. It must be noted, however, that they do not even consider the possibility of other causal mechanisms to underlie the relation between energy rents and armed conflict. As such, they do provide further support of the oil-armed conflict link, but may not provide further support for their favored mechanism.

Theorists that favor rebel-centered explanations include Lujala (2010) and Ross (2012). An important empirical justification for this is that when disaggregating the independent variable, only on-shore oil production significantly increases the risk of armed conflict onset, while off-shore oil production has no such malign effects (Lujala, 2010, p. 25; Ross, 2012, p. 164). If it was the detrimental effects of large oil revenues on state institutions that mattered, rents from off-shore oil production should be just as harmful as rents from on-shore production. When only on-shore oil matters, this may indicate that rebels' access to oil is the main distinguishing condition, working on conflict risk via rebels' motivation and capacities (Lujala, 2010).[30] [31]

[28]This is so because the weak state mechanism may be triggered by prior earned oil income, while the honeypot or rebel greed mechanism may be triggered by the anticipation of future oil income (Humphreys, 2005, p. 519)

[29] Energy consists of oil, gas and coal, whereas minerals include bauxite, copper, iron ore, lead, nickel, phosphate rock, tin, zinc, gold and silver (de Soysa & Neumayer, 2007, p. 206).

[30] The correlation between on-shore oil production and conflict does not enable distinction between different rebel-centered explanations, meaning that the honeypot, grievance or finance argument may be equally plausible.

Another claim in alleged support of rebel-centered explanations is that resource wealth in particular induces secessionist conflicts (Collier & Hoeffler, 2006; Ross, 2006, p. 289). While this is often argued to favor the honeypot argument as resource-wealth heightens the value of controlling a resource-rich region, it may not be distinguishable from rebel grievance or -funding arguments as they also hypothesize local effects. In any case, the robustness of this finding has been challenged in the particular case of petroleum resources. Ross (2012, p. 185) finds that oil income significantly increase the risk of both conflicts over government and separatist conflicts by a largely equal amount.

The economically oriented version of state-centered explanations also fails the test of empirical scrutiny. If oil revenues' detrimental impact on economic growth fully explained the relationship, the correlation between oil income and armed conflict would disappear when controlling for growth. This is not the case (see for instance de Soysa & Neumayer, 2007, p. 212). If oil countries' lack of economic diversification and sparse domestic trade networks explained the relationship, controlling for this could reveal the nature of these relationships. Yet, Humphreys (2005, p. 524) finds that while domestic economic structure (measured by the degree of agricultural dependence of economies) matters for countries' conflict risk, this condition does not reduce the conflict-inducing effect of oil production.[32] In other words, the effect of oil not limited to the condition of domestic economic structure.[33]

Theoretical Shortcomings of the Explanations

In addition to disagreements over mechanisms and inconclusive empirical findings, the causal paths suggested in previous literature are also theoretically unsatisfying. Two main problems may be noted: they do not address how variation in the dependent variable comes about, and

[31] Another interesting finding provided by Lujala (2010), is that when petroleum reserves and -production are located within a conflict area, this nearly doubles the duration of conflict. This may be taken to support rebel-centered arguments in general. Yet, production is not a necessary condition, located reserves are sufficient to bring about this effect. The latter finding in particular supports the honeypot argument, as both rebel grievance and looting opportunity is largely a consequence of production.

[32] In fact, controlling for agricultural dependence increases rather than decreases the coefficient for oil production (Humphreys, 2005, p. 525-526).

[33] What Humphreys does not address is whether the conflict-inducing effect of oil may be *conditional* on sparse economic networks. The combination of oil income and low economic diversification may make armed conflict more likely as both motive (heightened prize of state control) and low opportunity cost is present. This could also better capture economic inequality in society which may induce conflict by increasing grievances.

they disagree on (and underestimate) the paradox of rebel's capacity to carry out armed insurgency against oil rich governments.

Neither rebel-centered explanations nor state-centered explanations address why petroleum resources stimulate armed conflict in some instances, but not in others. If rebels' increased motivations due to heightened prize of state control or perceived grievances explained the relationship, why does this come about in some countries, but not in others? State-centered causal explanations similarly appear to suggest that oil income almost unavoidably creates states with weaker state structures, reduced accountability, and obstructed growth; promoting the impression that this is an inevitable path towards conflict. The many cases where oil wealth has not been followed by armed conflict clearly demonstrate the need to locate conditioning circumstances. Thus, more attention to explain the divergent experience of conflict in oil countries is warranted.

The review of literature also exposed an important theoretical disagreement concerning whether oil wealth improves or disrupts states' abilities to prevent armed insurgencies. Proponents of the weak state hypothesis argue that limited incentives to develop administrative and bureaucratic capacity increases the feasibility of rebellion in oil countries. Contrary to this, proponents of the rentier state theory emphasize that oil wealth provides governments with sizeable rents that may be utilized to prevent the formation of violent challengers to the regime, by methods such as coercion and co-optation. Empirical findings to substantiate the latter are available. Fjelde (2009) finds that high levels of political corruption limit the conflict-inducing effect of oil income. Ross (2003, p. 13) finds that military expenditures in oil- and mineral-rich states are between two and four times as great as that of oil- and mineral-poor states. This could signify a bolstering rather than a weakening of counterinsurgency capacity. As the heightened prize of state control presumably also provides governments with high incentives to prevent loss of power, the capacity of rebel groups to stage armed insurgencies in oil rich countries remains puzzling.

Summary of the Review and Research Gap

In sum, this review of previous research reveals that the question of which causal path primarily connects petroleum resources to heightened danger of civil war is far from settled. Scholars disagree over whether the conflict-inducing effect of oil primarily works via rebels' incentives or capacities or via weakened state institutions and economy. Empirically, there is

no unequivocal support of either causal story. Theoretically, the suggested mechanisms jointly come short in explaining two continuing puzzles: (1) why so many oil-producing countries are able to escape the seemingly inevitable path toward armed conflict; and (2) the paradox of rebels' capacity to stage and sustain armed insurgency against oil-financed governments that enjoy the best available means to sustain repressive capacities and strategic co-optation to prevent opposition.

Table 3.1: Summary of Mechanisms

	Mechanism	*Impact on conflict risk*	*Potential overlap with other mechanisms?*	*Empirical support?*
Rebel-centered	***Honeypot***	+	All state-centered, (and may be difficult to distinguish empirically from *grievance*)	Supportive: Effect of reserves on duration (Lujala) And on-shore oil, but not off shore oil matters (Ross, Lujala): supports rebel-centered arguments in general. Unsupportive: no effect of reserves on onset (Humphreys)
	Finance	+	*Weak bureaucratic control*	Supportive: A few case studies (eg. Republic of Congo, Nigeria); and effect of on-shore oil
	Grievance	+	*Government detachment*	Supportive: Case studies, effect of on-shore oil
State -centered	***Weak bureaucratic control***	+	*Finance*, *honeypot*, all state-centered mechanisms	Supportive: Humphreys, effect of past production increase risk. Unsupportive: only on-shore oil increases conflict risk.
	Government detachment	+	*Grievance*, *honeypot*, other state-centered	As above
	Rentier state	-	No (similar to *government detachment*, but opposing predictions)	Support: Case studies, Fjelde, Smith. Unsupportive: Oil increases overall risk of conflict.
	Vulnerable economy	+	*Honeypot*, *Grievance*, all state-centered	Supportive: Basedau and Lay Unsupportive: Humphreys

A subject that has received little attention so far is that countries' highly different institutional and societal starting points may be quite influential for their propensity to experience armed conflict following the introduction of oil. While several explanations of the relationship between oil and armed conflict emphasize the role of states' institutional capacity, no

contributions have addressed how variations in countries' initial institutional capacity may mediate the conflict-inducing effect of oil.[34] Another potentially salient issue that has remained unaddressed is how countries' varying experience of armed conflict prior to oil may affect the risk of conflict outbreak in oil-producing countries. Previous conflict may serve as an indicator of deficient state capacity both in terms of coercive control and legitimacy of rule, but may also indicate an important source of rebel capacity. Latent conflict structures (previously mobilized groups, rebel skills, presence of arms etc.) represent important sources of organizational capital which may have been underestimated as a predictor of armed conflict following oil.

In the following section, I introduce the perspective that oil countries' divergent institutional legacies and conflict legacies are key to understand the divergent experience of armed conflict in oil-producing countries. More precisely, I argue that the conditions of institutional strength and organizational legacies of conflict at the time when oil revenue ensue may determine whether oil becomes a blessing or a curse. A particular advantage of this perspective is that it suggests an answer to the paradox of rebel capacity, and a way to understand the origin of divergent trajectories in oil's impact on conflict risk via states' institutional capacity.

3.2 The Importance of Divergent Starting Points

Sudan and Cameroon are two African oil-producing countries with a markedly different history of armed conflict following oil production. While Sudan represents a prominent example of the volatile mix of petroleum and armed conflict, Cameroon has been nearly wholly able to avoid internal armed conflict during its time as an oil-producer.

[34] Anecdotic addresses to this point have been made. Smith (2004, p. 243) in his concluding remarks suggests that the impact of oil revenues may vary according to the domestic political setting into which they are introduced, in particular whether the regime was already consolidated or not. Karl (1997, p. 13) notes that petro-state problems may be avoided when state-building has taken place prior to the introduction of oil revenues. As part of a wider research endeavor on natural resources and armed conflict, Humphreys (2005, p. 528) addresses empirically whether the impact of oil rents may be conditional on measures state strength. The potential theoretical implications of his findings however remain underdeveloped and are overshadowed by the larger research objective and multitude of other findings. In addition, his three measures of state strength have clear limitations; most notably they are all likely to be endogenous to conflict. The first two are derived from the Polity index of political regime, which as exposed by Vreeland (2008) operationalize regime with direct reference to political violence and civil war. The third measure of state strength used by Humphreys refer to the Weberianness of state structures (p. 527), but is measured at a fixed point in the late 1990ies and includes no time series.

In Sudan, oil was discovered in 1978. Production did not commence until the mid-1990s, reaching full scale and exports to the international market by 1999 (ECOS, 2008, p. 18). Disagreement over the country's petroleum resources has been an important aspect of the conflict among the Sudanese government and the rebel groups (mainly the Sudan People's Liberation Movement/Army (SPLM/A)) over the years. Yet, oil was not what sparked the conflict initially. In fact, Sudan has experienced internal armed conflict nearly constantly since independence in 1956.[35] Since independence the country has been characterized by extreme centralization, with power and resources located to Karthoum, while other areas have been left marginalized. The divide between the Arab/Muslim north and the African/Christian South has also been evident in terms of economic development. Compared with the north, the south has always been remote and underdeveloped (UCDP Conflict Encyclopedia, 2014). This has not improved much despite soaring national income since 2000. While oil rents have enriched an elite minority, most people of Sudan and South Sudan have seen few benefits (Rolandsen, 2012, p. 73).

Exemplifying the peaceful oil producer, Cameroon started to produce oil in 1977-1978.[36] From its peak of production with an oil income about $200 per capita annually during the 1980ies, Cameroon has in later years produced oil within the range of $40-$130 per capita which is around the median value of about $70 among oil countries. With the exception of a two-day failed quo attempt in 1984, Cameroon has been remarkably peaceful compared to other low-income oil countries.[37] The decade leading up to first oil income was also free from organized political violence. An armed independence struggle in French Cameroon (northern part) had seen its two main goals accomplished with independence from France in 1960 and unification with British Cameroon (southern part) in 1961. Activities of the independence movement UPC (Union of the Populations of Cameroon) stemmed, as key leaders either were coopted or killed by the new regime of the unified Cameroons (DeLancey, 1989, p. 39). The years that followed were marked by centralization of state power on the hands of President Ahmadou Ahidjo. A highly authoritarian political system emerged; concentrating political

[35] During 1963-1972 a territorial conflict concerning Southern Sudan took place. Conflict over government has been active in the years 1971, 1976 and 1983 until present, taking place in the South and in the Darfur region. In 2011, South Sudan achieved independence, but fighting has continued in both countries since (UCDP Conflict Encyclopedia, 2014).

[36] In Ross' data, first oil income is recorded in 1977, while other sources state 1978 as the first year of production.

[37] Throughout the period of study, GDP/cap in Cameroon is continuously around or below the 25th percentile of GDP/cap of all countries.

and economic power on the President by utilizing key tactics of coalition formation and cooptation, patronage networks and repression (DeLancey, 1989, pp. 52-63).

At the same time as widespread patronage and repression may have induced stability, the regime also appears to have been capable in terms of public goods provision. According to DeLancey (1989, p. 91), the regime had a "drive toward universal primary education" since independence, which led to a considerable increase in education facilities and school enrolment in the period. According to the "Africa South of the Sahara" 1986 yearbook published by Europa Publications Limited, Cameroon had achieved one of the highest rates of school attendance in Africa, as a result of long term policy (p. 319).

What may explain different experiences of armed conflict among oil-producing countries such as Sudan and Cameroon? This section lays out the argument that in order to understand why petroleum resources in some cases induces armed conflict, but not in others, it is necessary to pay attention to domestic conditions already in place when the country discovers oil and rents start to flow into government coffers. In particular, two conditions stand out as potentially salient in mediating the effect of oil on conflict risk: first, a country's legacy of institutional capacity, and second, a country's potential legacy of armed conflict prior to the entry of oil. These conditions may extend and improve both rebel-centered and state-centered explanations, as will be shown in the discussion below.

3.2.1 Institutional Legacy

As reviewed above, many theorists suggests that the impact of oil on conflict risk primarily run via oil revenues inhibiting impact on different aspects of states' institutional capacity. In particular, oil was hypothesized to increase the risk of conflict by forestalling the development of state institutional capacities such as (1) bureaucratic strength (2) institutional capacity to engage and respond to citizens (reciprocal institutions), and (3) sound economic performance.

The arguments centering on oil's impact on states' institutional capacity remain poorly tested empirically. If it is the damages on countries' state institutions that actually explain oil countries' heightened risk of armed conflict, this could be tested by using exogenous measures of the critical institutions. This has rarely been done: Fearon and Laitin's (2003) analysis, for instance, is founded on the assumption that oil countries by definition have a

lower state capacity compared with other countries on the same level of GDP/per capita. In their analysis, oil is by itself used as an indicator of poor institutional outreach and bureaucratic control, although this critical aspect of state capacity may vary greatly among oil countries.

Another important shortcoming of the state-centered explanations is that they fail to take into account that institutional destruction, poor economic performance, and heightened risk of conflict is not an inevitable outcome of oil. They also do not address how differences in states' initial level of institutional capacity may play out on the mentioned mechanisms. The postulations of the state-centered explanations describe that oil income inhibits the formation and development of institutions central to maintain societal peace, but the explanations say nothing about how oil income will influence such institutions in countries where they have already been established.

Addressing this shortcoming, my contention is that variations in countries' institutional capacities at the point in time when oil income ensues ("institutional legacy") may importantly mediate whether the state-centered mechanisms, and rebel-centered mechanisms, will be triggered or not following oil income.

In simple terms, I claim that different institutional starting points may anticipate different institutional trajectories following oil wealth, with crucial implications for conflict risk: Countries with entrenched bureaucratic institutions and strong reciprocal institutions in place prior to oil may be able to resist the detrimental impact of oil revenue, and neither rebel incentives nor feasibility of rebellion may increase in the wake of oil. To the contrary, in countries with poorly developed bureaucracy and reciprocal institutions prior to oil, the causal paths suggested by state-centered and rebel-centered mechanisms may more plausibly be triggered and conflict risk increase.

Since the focus on initial institutional context by definition entails a primary focus on the state, the convenient distinction between rebel-centered and state-centered arguments hitherto upheld will now be more difficult to pursue. Increasingly, the two theoretical focuses are found to reflect two sides of the same coin, indicating that they should be seen as complementary rather than opposing. In the following will try to explain how differences in the context of state's institutional capacity may determine whether state-centered and rebel-centered mechanisms will be triggered or not.

Institutional Legacy, Bureaucratic Weakness and Rebel Capacity

The weak state mechanism portrays that oil income makes rebellion more feasible by weakening state bureaucratic and institutional control due to oil countries' reduced need for extracting taxes from the population. The degree to which countries have developed robust bureaucratic control *prior* to onset of oil income may however influence whether this trajectory is realized or not.

Oil income may be more likely to preclude the development of bureaucratic institutions where they are not present rather than to corrupt existing institutions. This suggests different institutional trajectories following oil wealth depending on the country's initial bureaucratic capacity. In countries with entrenched bureaucratic presence and control throughout territories (originally developed to sustain capacity to tax) prior to oil, these institutions may be expected to persist rather than wane, for reasons that may be summed up as organizational inertia.[38] In countries with strong bureaucratic institutions to facilitate tax-capacity established before the onset of oil revenue, a combination of domestic norms, institutions, procedures of national budgeting, people employed in bureaucracy etc. may be expected to sustain the continuation of this practice rather than abandoning it. Hence, in circumstances where robust institutions have already been established, oil income may not be expected to trigger the bureaucratic weakness mechanism. In these situations, the feasibility of rebellion may not automatically go up as a consequence of oil income to the government.

On the other hand, if bureaucratic control is poorly developed prior to oil, the bureaucratic weakness mechanism as it is presented above may more plausibly be triggered. In countries with poor bureaucratic capacity prior to oil, oil income may limit the state's incentives to extend its tax-extractive apparatuses and bureaucratic control throughout territories in order to raise finance. In such settings, oil may forestall development of a crucial source of the state's counterinsurgency capacity, and the feasibility of rebellion and relative capacity of rebels to organize armed insurgency increase.

Hypothesis 1: *The conflict-inducing effect of oil income will be higher in countries with low bureaucratic control than in countries with high bureaucratic control at the commencement of oil income*

[38] Organizational inertia refers to the tendency of mature organizations to continue along their current trajectories, also during change in external conditions.

Institutional Legacy and the Government Detachment Mechanism

The government detachment mechanism portrays that oil income brings about states that are more detached from the public, less accountable, less responsive to social needs and thereby less able to resolve social grievances. Exactly what types of state institutions facilitate accountability and responsiveness on the part of the government is not clearly specified in the literature referring to this mechanism. Yet, it is conceivable that the quality of most kinds of public institutions designed to respond to the needs of citizens may indicate the level of reciprocity of the state to society. As oil rents are expected to in particular heighten redistributive pressures on the part of the state, the quality of redistributive institutions providing public goods may be relevant to consider in this regard.

The claim of this thesis is that the quality of such institutions in place *at the time of* onset of oil revenue may importantly condition whether the government detachment mechanism will be triggered or not. As in the case of the bureaucratic control mechanism, the government detachment mechanism postulates that oil income - by reducing the need for taxation – inhibits the formation and development of institutions that facilitate government accountability and reciprocity to society, while it says nothing on the impact of oil rents on such institutions where they are already in place. I propose that the onset of non-tax revenue such as oil rents need not corrode such capacities if they have already been developed. If a state has developed the institutional capacity to provide high quality public goods like general education and public health care *prior* to oil, this is a plausible indication of well-developed responsiveness to social needs on the part of the state. The same norms, principles and institutions underlying this capacity may be expected to guide the management of ensuing oil revenues, and improve the prospects that oil revenues will be utilized for public good. In other words, in contexts where institutions facilitating governments' reciprocity to society and distribution of public goods are of high quality prior to the entry of oil, the propositions of the government detachment mechanism appear less plausible.

If countries lack such institutions when oil rents start to flood into government coffers, the scenarios portrayed by the government detachment mechanism may more plausibly be triggered. Onset of large oil income in contexts where such institutions are missing may prevent that they ever emerge, following the logic that government's financial reliance on taxation is the primary motive for creating institutions facilitating accountability of rulers and reciprocity towards society.

Institutional Legacy, Rebel Grievance and Rebel Honeypot

Complementing the arguments above, pure rent-seeking motivations on the part of rebels (the honeypot mechanism) and the rebel grievance mechanism may also be alleviated by the presence of robust bureaucratic institutions and well-functioning redistributive institutions.

The honeypot argument posits that that the presence or promise of oil rents increases the value of the state as a target, thereby increasing the risk of armed attempts at governmental power or regional sovereignty. However, the degree to which credible institutional constraints are in place to control elites' use of such rents may importantly determine the size of the "prize" of state control. In countries with robust bureaucratic control, and well-functioning redistributive institutions, such institutions may indicate a capacity to oversee and constrain the management of public funds. Where such constraining institutions are present, private gains from violent attainment of power should be low, and the honeypot mechanism appears implausible.

The rebel-centered grievance mechanism emphasizes how the discovery and extraction of oil may generate higher expectations for redistribution and gainful returns, which may engender conflict if such aspirations are unfulfilled. The robustness of institutions facilitating conversion of public funds into public goods may plausibly condition whether the rebel grievance mechanism will be triggered or not.

Hypothesis 2: *The conflict-inducing effect of oil income may be lower in countries with high quality of public service provision than in countries with low quality of public service provision at the commencement of oil income*

3.2.2 Armed Conflict Legacy

As pointed out in the discussion of continuing theoretical puzzles, the question of rebel groups' capacity to stage armed insurgency against oil financed governments is far from settled. Opposing theoretical projections include on the one hand Fearon and Laitin (2003), who propose that oil wealth increases the feasibility of rebellion by weakening states' bureaucratic control and hence counterinsurgency capacity. On the other hand, proponents of the rentier state theory suggest that oil wealthy regimes may prevent armed challenges by using oil rents to strengthen repressive apparatuses and buy off opposition. Governments of oil-producing countries are identified as remarkably inclined to invest in military capability,

and their incentives for preventing loss of their position should be comparable to rebels tempted by the honeypot.

Addressing this puzzle of rebels' capacity to organize armed revolt against oil-financed regimes; I propose that the possible presence of latent conflict structures from a previous armed mobilization deserves attention. Thus far, the possible connection between armed conflict following oil production and armed conflict *prior* to oil has not been considered.

Why may previous armed conflict be interesting to consider in the oil-armed conflict relationship? With reference to the state-centered explanations of the oil-armed conflict relationship, a previous armed conflict may be indicative of deficient state capacity.[39] A perhaps more important perspective, however, is that organizational and structural legacies persisting from a previous mobilization may significantly add to the feasibility of rebellion in oil-producing countries. Thus, this condition suggests an answer to the previously identified puzzle of rebel capacity in oil-producing countries.

How may prior armed conflict indicate enhanced viability of rebellion following oil? The feasibility of rebellion may rest on the ability of a dissident organization to gain the support of a considerable segment of the population, and to recruit a sufficient number of active members. The occurrence of armed conflict is indicative of the presence of "multiple sovereignties", a concept originally used by Tilly (1978). This condition obtains when one or more armed challengers to the government emerge which commands the support of a significant segment of the population (Tilly, 1978, pp. 192, 200). As argued by (Mason et al., 2011, p. 172), the degree to which the condition of multiple sovereignties persist in society after the formal end of a conflict may define the capacity of rebel groups to resume armed conflict at a later stage. Insofar as the condition of multiple sovereignties[40] persists in the post-conflict society, this plausibly makes the mobilization of human and material resources necessary to resume insurgency at a later stage more feasible.

A previous conflict may indicate the presence of unique resources aiding rebel capacity for renewed insurgency. Collier and Hoeffler (2004, p. 569) emphasize that so-called conflict-

[39] In addition, armed conflict may further weaken state capacity, by destroying economic infrastructure and diverting human capital as well as finances away from productive activity. Armed conflict reduces production and trade, and thereby hampers economic output and growth, implying that the causes and consequences of armed conflict are reciprocal (Collier et al., 2003).

[40] Including organizational infrastructure and popular support base of an insurgency

specific capital inherited from a previous conflict improve the opportunities for rebellion. Similar to the multiple sovereignty approach they emphasize the importance of organizational capital, but they also point to the persistence of weapon stocks and rebel skills as capacity enhancing resources.[41]

Social psychology theory on structural changes that occur during escalation of conflict also supports the idea that armed conflict may be more feasible in settings where former escalation has taken place. Armed conflict may be seen as the result of a broader process of conflict escalation. Rubin et al. (1994, p. 133) describe a range of structural changes that occur within society during escalation of conflict, including development of organizational infrastructure, hostile perceptions and attitudes among adversaries, and community polarization. Such structural changes tend to persist long after violence cease and conflict formally ends, and they make the escalation of future conflict more likely and more severe.

Political issues related to petroleum resources (ownership, distribution of rents, job opportunities, etc.) may become more contentious and difficult to resolve when introduced in settings where such structural changes (mobilized groups, antagonistic attitudes and mistrust) have developed within society during a previously escalated conflict. In line with this thought, Rubin et al. (1994) note that a central feature of escalation is that more and more issues are incorporated into the contested incompatibility. In countries where a recent armed conflict has taken place and escalation structures persist, political issues related to the discovery and exploitation of oil may merge into antagonists incompatibility and be more flammable than when introduced into settings with no prior escalation.

Hypothesis 3: *Oil income increases conflict risk more severely in countries with a recent legacy of armed conflict than in countries without such a conflict legacy at the commencement of oil income*

The findings of Rustad and Binningsbø (2012) may be supportive of this notion. While they study the duration of post-conflict peace, not armed conflict onset, they find that conflicts with natural resource links (in particular conflicts motivated by incompatibility over natural resource distribution) are more likely to resume than conflicts without such links. This may indicate preliminary support to the notion that distributive issues related to natural resources

[41] Collier and Hoeffler (2004) model such conflict-specific capital to gradually vane, measured by time since the last conflict.

may be particularly salient motive for continuing fighting in settings where conflict structures are present. Although Rustad and Binningsbø study natural resources in general, petroleum resources may be a highly relevant in this regard as oil exploitation generates exceptionally high values concentrated on few hands. Pressures for redistribution should in other words be particularly salient in the case of petroleum resources.

The notion that organizational capital inherited from previous conflict is a particularly important source of rebel capacity and determinant of insurgency finds support in a recent study by Daly (2012). Studying determinants of violence in Colombian municipalities, she finds that past mobilization is the main predictor of rebellion, and more importantly so than poverty, rough terrain and lootable natural resources. On the basis of data from 274,428 municipality-months, she finds that regions where previous mobilization has taken place are six times more likely to experience rebellion than regions without past mobilization. The mechanism, she suggests, is that organizational legacies from previous conflict ease mobilization for later militarized collective action (Daly, 2012, p. 477).

A few qualifications to the proposition that past armed conflict may ease mobilization for renewed conflict following oil may be noted. The first is that the nearness in time of the previous conflict is likely not unimportant. Conflict-specific capital such as organizational infrastructure may be expected to decline with time after the end of the previous conflict, as pointed out by Collier and Hoeffler (2004).

Second, the outcome of the previous conflict may distinguish whether the condition of multiple sovereignty persist in the post-conflict phase or not, and thereby be indicative of the regenerative potential. This is the main argument of Mason et al. (2011, p. 173). A negotiated settlement may preserve the condition of multiple sovereignties better than if the previous conflict ended with a military victory, or a total extermination of one side, such as in the last phases of the civil war in Sri Lanka in 2009. For the purpose of this study, it will suffice to expect that the condition of multiple sovereignties will more easily be revived in countries where a recent armed conflict has taken place compared with countries without such a recent conflict history. The occurrence of conflict is a manifestation that the legitimacy of the government is contested, and that society hosts a latent support base sufficient to sustain insurgency.

Finally, continuity between past mobilization and renewed conflict does not theoretically predicate that it is the identical rebel organization that is reincarnated following the onset of oil income. As emphasized by Daly (2012, p.477), insurgencies often draw on existing social structures and networks to overcome collective action and commitment problems, in which previously mobilized militant networks may be exceptionally suited for co-optation. For this reason, I hypothesize that the recent presence of an armed organization may add to the feasibility of renewed mobilization following oil, and whether or not this is the same rebel organization or a new one is regarded unimportant.

Table 3.2 below sums up the main parts of the theoretical argument and affiliated hypotheses; before I move on to develop the empirical procedure in Chapter 4.

Table 3.2: Summary of Hypotheses and Corresponding Explanations

Hypothesis	**Explanation**
H1: The conflict-inducing effect of oil income will be higher in countries with low bureaucratic control than in countries with high bureaucratic control at the commencement of oil income **H2:** The conflict-inducing effect of oil income may be lower in countries with high quality of public service provision than in countries with low quality of public service provision at the commencement of oil income	Countries' institutional legacy may condition the conflict-inducing effect of oil
H3: Oil income increases conflict risk more severely in countries with a recent legacy of armed conflict than in countries without such a conflict legacy at the commencement of oil income	Countries' legacy of armed conflict may condition the conflict-inducing effect of oil

Overarching explanation: *Differences in exposure to armed conflict among oil income countries may be explained by differences in these countries' institutional and societal starting points as oil income ensues.*

4 Research Design

This chapter presents the empirical procedure and quantitative research design developed for testing my proposition that institutional legacy and conflict legacy mediates the conflict-inducing effect of oil. The purpose is to make this procedure transparent and replicable, expose challenges, and to discuss and justify important methodological choices (King et al., 1994). The chapter is organized as follows. First, I describe the main properties of the empirical data used for the analysis, including structure of the dataset and the units of analysis. Second, I discuss the operationalization of independent variables and control variables, and reflect on issues relating to the validity and reliability of these measures.[42] Third, I present logistic analysis as the statistical model employed to test the hypotheses. Fourth, I discuss the main methodological challenges of the empirical procedure and how they are addressed in order to enhance the possibility of making valid inferences from results.

4.1 Data Structure and Unit of Analysis

This section describes the structure of the data which is utilized in the empirical analysis of my research question. The dataset used in the analysis is a time-series cross-section (TSCS) structure, with the country-year (eg. Algeria 1967) as the unit of analysis. The data includes yearly observations on relevant variables for all countries listed as independent members of the international system as classified by Gleditsch and Ward (1999).[43] The years 1961-2007 is the time-period covered in the analysis of the dependent variable, as this is the period for which I have data on all operationalized independent- and control variables. The dataset also includes country-years from 1946 to 1960 in order to measure and determine the sequence of countries' first oil income and conflict legacy.

[42] While there many different definitions of the concepts of validity and reliability, I here use them in the following "common" sense: validity referring to the ability of the operationally defined variable to capture the theoretically defined concept they are intended to measure (Adcock & Collier, 2001). Deficiency in the validity of measures may lead to systematic measurement error and biased results. Reliability, on the other hand, refers to the accuracy of the measurement of the operational variables, and measures' ability to yield consistent observations when repeated. Problems with reliability lead to unsystematic/random measurement errors.

[43] There is one exception to this rule: non-independent countries that had incidence of extra-systemic conflict within their borders prior to independence have country-years from the first incidence of such conflict and up to independence included in the frame. This is to facilitate the construction of other variables ("conflict prior to oil" variables), and as these country-years contain no other data they are not included in any regression analyses.

4.2 Operationalizations

This section describes how the theoretical concepts discussed in the theory section, which make up dependent and independent variables in my study, are made empirically measurable through the specification of indicators. The validity of measures are pursued by careful attention to the correspondence between theoretically defined concepts and the operationally defined indicators (Adcock & Collier, 2001). In the following, I account for the choice of indicators of the dependent and the independent variables, and present the set of control variables included in the analysis. The sources of data on these indicators are also presented.

4.2.1 Dependent Variable: Internal Armed Conflict Onset

My research question addresses the relationship between oil, various mediating variables, and risk of domestic armed conflict onset. The dependent variable is thus onset of internal armed conflict. This calls for a binary outcome measure of this variable: domestic armed conflict may (1) or may not (0) commence in a given country-year.[44]

While there are several available data-compilations with information on domestic conflict onset,[45] I utilize conflict data from UCDP/PRIO Onset of Intrastate Armed Conflict Dataset 1946-2011 (Gleditsch et al., 2002; Themnér & Wallensteen, 2012).[46] This dataset follows the Gleditsch and Ward (1999) classification of independent states, and contains yearly observations of onset of internal armed conflict for each country between 1946 and 2011. The dataset includes cases based on UCDP/PRIO's standard definition of armed conflict,[47] and

[44] The choice of studying armed conflict *onset* is primarily for the purpose of comparability with other relevant studies in the field, as they predominantly focus on the relationship between oil and conflict onset risk (an exception being studies focusing on duration of conflict).

[45] The most commonly used data-compilations include UCDP/PRIO Armed Conflict Data (various versions); Fearon and Laitin (2003) data on civil wars; Correlates of War data; as well as Sambanis (2004) conflict data. The main differences in coding rules across these data-compilations include the amount of violence specified as criterion for inclusion; whether or not civilian killings are included in this count, and time-coverage. The number of onsets and country-years with domestic armed conflict vary notably according to which coding procedure is followed, as do time-period for study, which makes it hard to compare results from studies using one set of data from another (Hegre & Sambanis, 2006).

[46] This is the UCDP/PRIO Armed Conflict Dataset v.4-2012 in a country-year version, structured for quantitative analysis. Downloadable from: www.pcr.uu.se/research/ucdp/datasets/onset_of_intrastate_armed_conflict/

[47] UCDP/PRIO employs the following definition of conflict: "[…..] a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths." For more detailed information on the operationalization of various elements of the definition, I refer to the codebook (Themnér, 2012), and the list of definitions at the UCDP website: http://www.pcr.uu.se/research/ucdp/definitions/

only includes cases of internal armed conflict, that is, cases where conflict takes place between a government and one or several internal organized groups (Themnér, 2012, p. 9).[48] Any armed confrontation between a government and an internal opposition group that reaches a threshold of 25 battle-related deaths in a calendar year is included in the dataset. It is however a criterion that the internal opposition group is formally organized in the sense that it has announced a name for their group, and has stated an incompatibility with the government (Themnér, 2012, p. 2).

More specifically, my dependent variable is coded 1 if an internal armed conflict begins in a given country-year, and 0 if otherwise. Consecutive conflict years are coded as 0 to allow for the possible onset of additional conflicts within the same country.[49] If a conflict begins again after two consecutive years of peace, it is treated as a new onset event. This coding rule is somewhat arbitrary; it is not always easy to say what is an end of conflict and what is merely a phase of low activity. It is however a widely applied rule also in studies that I want to be able to compare my results to (eg. de Soysa & Neumayer, 2007; Lujala, 2010; Ross, 2012), so it makes sense to apply the same rule for the sake of comparability. The impact of prior conflict will nonetheless be controlled for in other ways.

The choice of UCDP/PRIO's conflict data as indicator of internal armed conflict is justified with regard to both the measures' content validity and its reliability. Regarding content validity; the operational definition corresponds closely with the theoretical conceptualization of internal armed conflict presented in the introduction. Compared with other data-compilations it has the advantage that it uses the relatively low threshold of 25 battle-deaths, which means that it offers more fine-grained data on the phenomena of interest than datasets employing higher thresholds for inclusion.[50]

[48] Both cases of internal armed conflict without intervention from other states and cases with intervention from other states are included (referring to type 3 and 4 in UCDP's typology of conflict). Cases of colonial- and interstate armed conflict (type 1 and 2 in UCDP's typology) are excluded from this dataset.

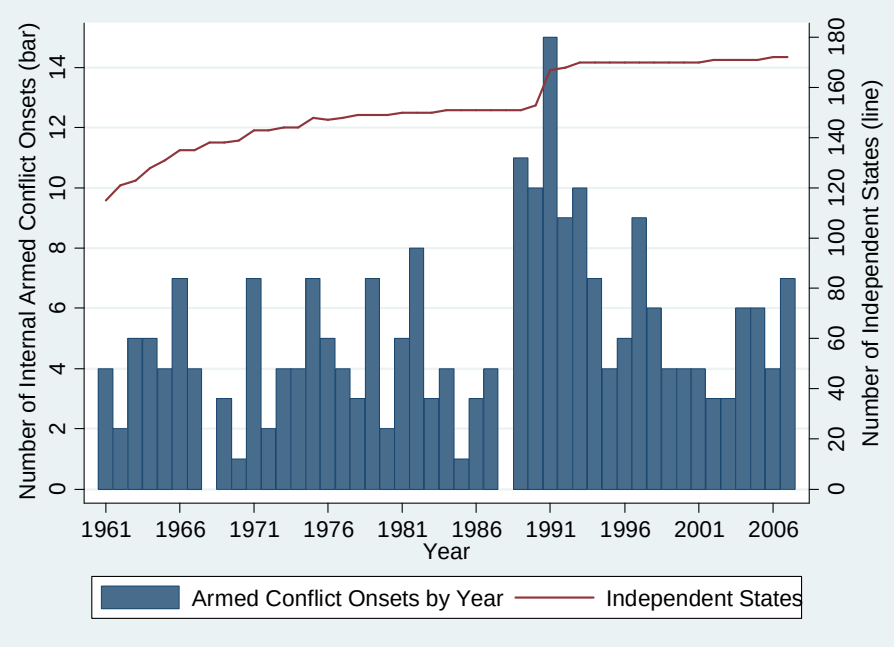
[49] This coding rule is also fair given that dropping consecutive conflict years from the sample would artificially increase the mean of the dependent variable in particular for countries that have experienced multiple onsets, possibly making these more influential in the data (Fearon, 2005, p. 488).

[50] At the same time, the battle-related criterion means that incidences of large-scaled one-sided violence, like massacres, genocide and communal violence, may not be included in the count. This may seem somewhat at odds with a common expectation of what should be considered armed conflict. Yet, the measure ensures strict attention to the phenomena of interest, which entails instrumental use of armed force in the struggle over incompatible political objectives between a government and a contending rebel organization.

Regarding reliability, the UCDP/PRIO data is superior in the sense that applied coding rules are consistent (across countries and years), well specified and transparent. This has been a problematic issue in particular concerning the correlates of war data (Sambanis, 2004, p. 817).[51] The UCDP/PRIO conflict data is also a highly reputed source, and widely applied in research in the field.[52] Another beneficial feature is that this data covers a long time span.

For the period 1961-2007 the data covers a total of 7169 country-years. A total of 235 onsets of internal armed conflict within the period give a mean of approximately .033, which may be interpreted as countries' average annual risk of armed conflict of about 3.3 % within this period.

Figure 4.1: Number of Internal Armed Conflict Onsets and Independent States by Year



[51] A minor, yet noteworthy drawback to the reliability is that the temporal unit of the calendar year imposes some degree of arbitrariness in the measurement of cases: if for instance 25 battle deaths occurred within the period December 1975-November 1976, but not within January 1976 to December 1976, the value of the dependent variable in 1976 will nonetheless be measured as zero although the threshold was reached within a time period of a year in the first case.

[52] For documentation, see: http://www.pcr.uu.se/research/ucdp/publications/Publications_using_UCDP_data/

4.2.2 Independent Variables

4.2.2.1 Petroleum Resources

Two different indicators of the main independent variable are employed in this thesis, *log oil income per capita* and *nationalized oil income per capita*. The coming section describes how these indicators are constructed.

Log oil income per capita

As the primary indicator of the main independent variable: oil, I choose to rely on the measure provided by Ross (2012, p. 16). His oil variable, *log oil income per capita* is measured as the total value of oil and gas production, divided by a country's population.[53] The measure is log-transformed to reduce the impact of extreme values of oil income. Compared to other available measures this measure is preferable for the following reasons:

First, this measure avoids a number of sources of bias inherent in other measures. A commonly used measure is the value of a country's export earnings from oil divided by its GDP per capita (e.g. Collier & Hoeffler, 1998; Sachs & Warner, 1995). Another commonly used measure is fuel exports as percentage of total exports, sometimes in a dummy format based on some cutoff point (e.g. Fearon, 2005; Fearon & Laitin, 2003). Both types of measures consider resource *exports* only, which is problematic. It is not theoretically justified why only exports should count, and not rents from domestic fuel sales.[54] Another problem is that the focus on exports introduces a potential bias upwards for poorer countries, which tend to export more of their produced oil and consume less domestically than richer countries. Dividing exports by GDP per capita introduces a similar bias, as countries with lower GDP in the denominator will have a larger oil exports to GDP ratio than countries with higher GDP. The problem is then to distinguish which variable is to blame for the effect on conflict risk.

[53] Ross' (2012) data-sources for constructing this measure are the following: The World Bank (figures on oil and gas production from 1970-2001), BP Statistical Review of World Energy (figures on oil and gas production after 2001), US Geological Survey's Mineral Yearbook (figures on oil and gas production before 1970). Data on oil and gas prices are obtained from BP Statistical Review. For further details see Ross (2012, p. 17).

[54] These measures also include re-exports, which has the implication that some countries that have never produced oil domestically stand out as remarkable oil exporters (Humphreys, 2005, p. 522).

Ross' oil measure avoids this problem altogether by dividing production figures (not exports) by the country's population (not GDP per capita).[55]

Second, this oil measure is more precise in the sense that it takes into account the varying value of oil on the world market. This is an improvement compared with e.g. Humphreys (2005, p. 523) oil measure, which considers only the quantity of oil produced. If it is the size of oil revenue that matters, fluctuating values of the commodity is an important aspect to include.[56] Similarly, compared with dummy-indicators of oil production, Ross' continuous measure provides more precise measurement, providing the possibility to consider quantity as an aspect related to the risk-inducing effect of oil.[57]

Nationalized log oil income per capita

A potential weakness of the *log oil income per capita* measure is that although it may be a precise indicator of the value of oil and gas produced in a country in a given year, it may not be a reliable indicator of the size of income from this production to the government of the country.[58] As emphasized by Andersen and Ross (2013), up to the late 1960s, most of the oil rents generated by production in non-western countries typically benefited a few large international oil companies, the so-called "seven sisters".[59] Not before a wave of oil nationalizations occurred in the sector between the late 1960s and mid 1970s did most governments of oil producing countries gain control of the rents. This may be an important aspect to consider when measuring oil, as an implicit assumption of the state-centered causal explanations is that the point when massive revenue starts flowing into *government* holdings is where the detriment starts.

In order to be able to distinguish between oil income seized by external companies and oil income seized by governments, I construct a new variable from Ross (2012) oil income measure: *nationalized oil income per capita.* This is the value of *oil income per capita* from the first year following a country's nationalization of its oil industry. Information on year of

[55] Another potential problem of dividing by GDP per capita is that oil itself often make up a substantial share of GDP per capita.

[56] Eg. in January 1999 the oil prize $10 a barrel, while in June 2008 it was $145 a barrel (Ross, 2012, p. 10).

[57] Unfortunately, Ross does not specify what type of monetary unit is used for value calculations.

[58] As Ross (2012, p. 17) points out, reliable information on this is very hard to obtain, as this sector has been marked by great discretion over revenue flows over the years and governments of many countries have had incentives to conceal information on their petroleum earnings.

[59] The "seven sisters" comprised the Anglo-Persian Oil Company; Gulf Oil, Standard Oil of California; Texaco; Royal Dutch Shell; Standard Oil of New Jersey, and Standard Oil Company of New York.

oil nationalizations is obtained from Guriev et al. (2011), that provide data on "all nationalizations of foreign-owned oil companies around the world during 1960-2006" (Ibid., p. 4). This data provides year of nationalization by company rather than by country, meaning several incidences of nationalizations may occur within the same country, at different years. I operationalize a country's year of nationalization as the year of the first nationalization of a foreign oil company in that country. Although this may not imply that the government of the country seizes control of *all* oil rents generated by extraction of oil within that country from that year, it is a plausible indicator that it gains control over substantially *more* rents from that year, compared to the time preceding the first nationalization. In my opinion, this may be a better approximation of oil rents to the government than the variable which does not take into account whether or not nationalizations in the oil industry have occurred.

Guriev et al. (2011) provide data on nationalizations for 42 countries. Since there are 100 different countries with oil income in the Ross (2012) data, countries lacking information on oil nationalization need to be handled. It could be that they have nationalized their oil industry prior to the time period covered by Guriev et al. (2011), or it could be that they have never nationalized. These two possibilities have quite different implications. The best approach to handle this would be to investigate each case qualitatively, but for me, time does not permit so. Instead of dropping these countries from the data, countries missing information on nationalization are treated as if they have nationalized their income from the initial year of oil income. Thus, *nationalized oil income* is oil income per capita for countries from the first year following an oil nationalization in the country, or from the first year of oil income if data on nationalization is missing.

The arbitrariness of using first year of oil income as year of nationalization for countries missing such information is a clear limitation to the reliability of this measure. However, it may also be argued that compared with other studies, which either do not take nationalization into account at all or like Andersen and Ross (2014, p. 14) simply use a dummy for years 1981-2006 based on the assumption that within this period nationalization has occurred

everywhere, my measure may approximate the actual year of nationalization more accurately and thus enhance estimation.[60]

A list of oil countries, with year of first oil income and year of first nationalization is provided in Table A.1 in Appendix A.

4.2.2.2 Institutional Legacy

A main contention of this thesis that in order to understand why oil induces conflict in some countries, but not in others, we need to consider countries' divergent starting points. One crucial aspect to consider is difference in countries' initial institutional capacity. The theory section above has specified this claim further, and hypothesized that the effect of oil income on conflict risk is conditional on countries' initial institutional capacity in terms of *bureaucratic control* and *quality of public service provision*. In the following, I describe how these two aspects of institutional capacity are operationalized, as well as sources of data for the measures.

Legacy of Bureaucratic Control: *The Bureaucratic Quality Index*

As a measure of a country's level of bureaucratic control in a given year, I rely on a measure of bureaucratic quality developed by Hegre and Nygård (2014, p. 9). Their "bureaucratic quality index" covers country-years between 1960 and 2009, and is normalized to have mean of zero and a standard deviation of 1, so that low values indicate poor bureaucratic quality and high values indicate good bureaucratic quality. To investigate the hypothesis that the impact of oil is conditional on countries' initial level of bureaucratic control, I construct a variable *initial bureaucratic control*, which reflects the country's value on the bureaucratic quality index the first year of oil income following Ross' oil data.[61]

[60] For instance, for Burma, which nationalized their oil industry in 1962, employing an indicator of nationalization fixed for all countries to the period after 1980 lead to quite imprecise measures on which to base the test of the relationship between the dependent and independent variable. Burma is of course not the only example in this regard: according to my data there are 13 countries that nationalize during the 1960ies; and 25 countries during the 1970ies, of which the main bulk during the first half of the decade.

[61] Ross' oil income data go back to 1960, although some countries started to produce oil long before that and thus may have had oil income earlier (see overview in Table A.1 in Appendix A). For all these countries, bureaucratic legacy is nonetheless operationalized as their 1960 score on the bureaucratic quality index, as this is the first available data.

To facilitate comparison with non-oil countries, I operationalize non-oil countries' bureaucratic legacy as their value on the bureaucratic quality index for the year 1960, as this is the most frequent initial year of oil income among oil countries.[62]

The bureaucratic quality index is constructed from two separate indicators: (1) a bureaucratic quality indicator from the International Country Risk Guide (ICRG) and (2) a government effectiveness indicator from Worldwide Governance Indicators (WGI) (Hegre & Nygård, 2014, p. 9).[63] These indicators measure the following:

The ICRG *bureaucratic quality* indicates the institutional strength and quality of the bureaucracy in a country, in terms of its ability to facilitate continuation of policy and day-to day administrative functions, also when governments change.[64] The WGI *government effectiveness* captures "perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government's commitment to such policies" (Kaufmann et al., 2010, p. 4).

As indicated by the operational definitions above, the primary focus of the sub-indicators of the bureaucratic quality index is on the *quality* and resilience of bureaucratic institutions and public service provision. It may be argued that the indicators insufficiently reflect the aspect of bureaucratic institutions' geographical reach and extension of state control into rural areas, which is a central aspect of the concept of bureaucratic control used by Fearon and Laitin (2003, p. 80). However, while *quality* rather than *geographic dispersion* of public institutions is primarily what the measure aims at, the latter is also partially incorporated by the measure:

[62] Since non-oil countries have no first year of oil income, the selection of such a reference point for measuring bureaucratic legacy becomes somewhat arbitrary. Choosing 1960 may not be completely justified given that among oil countries, the reference point for measuring initial institutions varies between 1960 and 2005, and for more than half of these countries at some arbitrary point between 1961 and 2003 (see Table A.2 Appendix A). Ideally, my analysis would consider multiple reference points for non-oil countries to see how this affects results, but this is not feasible for me. Notwithstanding the problem of arbitrariness of the selected reference point, it is desirable to also include non-oil countries in the comparison to control for the potential impact of institutional legacy in the absence of oil.

[63] Original ICRG data available for purchase at http://www.prsgroup.com/, while WGI data is available at http://data.worldbank.org/data-catalog/worldwide-governance-indicators.

[64] High scores are given to countries where "the bureaucracy has the strength and expertise to govern without drastic changes in policy or interruptions in government services"; and where the bureaucracy is "autonomous from political pressure and have an established mechanism for recruitment and training"(PRS Group, 2014, p. 7).

Of the many sub-indicators used to construct the WGI government effectiveness indicator, several of them capture aspects of public infrastructure (World Bank, 2014, p. 1).[65]

In sum, this indicator may be among the best available measures of bureaucratic quality, constructed from reputable data sources with the best coverage over countries and years. Yet, it is not primarily an indicator of bureaucratic reach and "disciplining" of rural areas, and this affects what inferences may be made from analysis' results. The measure justifies inferences related to the mediating effect of bureaucratic quality, but may not fully justify inferences on the mediating effect of bureaucratic control throughout territory; both of which are however constituting elements of bureaucratic control.

Another qualification pertaining to content validity must be made. In the theory section, I distinguish between bureaucratic control and quality of public service provision as different aspects of institutional legacy. Yet, the operationalized variables may not justify full separation of these aspects, because the measures are partly overlapping; they capture some of the same stuff. In particular, the bureaucratic quality index partly draws on data on quality of public service via the WGI indicator. Thus, they may not be viewed as indicators of fully distinct aspects, rather as alternative indicators of the same phenomena, institutional legacy.

The original ICRG bureaucratic quality indicator covers the years 1984-2009, and the WGI government effectiveness indicator reaches back to 1996. In order to reduce missing data, and to extend the time period covered by their composite bureaucratic quality index, the authors have performed multiple imputation (Hegre & Nygård, 2014, p. 9). Multiple imputation entails using all information available in the dataset to estimate values for observations with missing information. As the empirical indicators which the bureaucratic quality index is based on only have observations back to 1984 while data on the variable for years before that are imputed, this means that a considerable portion of the data on which I base my test of H1 are estimated rather than observed. This is a somewhat problematic issue, which I will discuss thoroughly in the section on methodological challenges.

[65] Examples of sub-indicators focusing on infrastructure includes: satisfaction with roads and highways, satisfaction with public transportation service, electricity grids, sanitation and drinking water, and more (World Bank, 2014, p. 1).

Table 4.1 below provides an overview of oil countries' scores on the variable *initial bureaucratic control* for the reader to inspect. I provide this here to show that although a share of these are estimated rather than observed values, they do not generally appear as unsound estimates of how countries rank in relation to each other on this variable. Apparent flaws to the face validity of values do however occur, such as Angola being ranked in the 60th to 80th percentile in 1975. For a full list of countries' scores on the initial bureaucratic quality variable, including 1960 scores for non-oil countries, see Table A.2 in Appendix A.

Table 4.1: Distribution of Oil Countries Across Percentiles of Initial Bureaucratic Control

Percentiles	*Country (Year of Measurement)*		
1-20 Poorest Initial Bureaucratic Control	Afghanistan (1967) Benin (1980) Chad (2003) Chile (1960) Congo, Dem. Rep. (Zaire) (1970) Egypt (1960) Equatorial Guinea (1994)	Ghana (1978) Guatemala (1976) Iran (1960) Libya (1961) Philippines (1979) Rwanda (1970) Sudan (1993) Surinam (1986)	Syria (1968) Turkmenistan (1991) Uzbekistan (1991) Vietnam, Dem.Rep. (1981) Yemen (Arab Rep.) (1986)
21-40	Azerbaijan (1991) Belarus (1991) Cambodia (2000) Cameroon (1977) China (1960) Congo(1960) Cote D'Ivoire (1978)	Ecuador (1960) Gabon (1960) Georgia (1992) Iraq (1960) KyrgyzRepublic (1991) Lithuania (1992) Mexico (1960)	Nigeria (1960) Qatar (1971) Rumania (1960) Tajikistan (1991) Tunisia (1966) Turkey (1960)
41-60	Algeria (1962) Bangladesh (1971) Bolivia (1960) Croatia (1991) Cuba (1960) Kazakhstan (1991) Kuwait (1961)	Malaysia (1960) Myanmar (1960) Oman (1964) Pakistan (1960) Peru (1960) Poland (1960) Russia (Soviet U)(1960)	Serbia (Yug.) (1960) Thailand (1963) Trinidad and Tobago (1962) Ukraine (1991) United Arab Em. (1971) United Kingdom (1960)
61-80	Albania (1960) Angola (1975) Argentina (1960) Bahrain (1971) Barbados (1966) Brazil (1960) Colombia (1960)	Czech Republic (1993) India (1960) Indonesia (1960) Jordan (1986) Morocco (1969) New Zealand (1960) Papua N. Guinea (1992)	Saudi Arabia (1960) Slovakia (1993) Slovenia (1991) Spain (1966) Taiwan (1960) Venezuela (1960)
81-100 Best Initial Bureaucratic Control	Australia (1961) Austria (1960) Belgium (1970) Brunei (1984) Bulgaria (1960) Canada (1960) Denmark (1972)	France (1960) German Fed. Rep. (1960) Greece (1981) Hungary (1960) Ireland (1978) Israel (1960) Italy (1960)	Japan (1960) Netherlands (1960) Norway (1971) Sweden (1978) Switzerland (1985) United States of America (1960)

Initial bureaucratic control is measured at first year of oil income, or in 1960 for countries with oil income prior to 1960, operationalized as countries' scores on the "bureaucratic quality index" this year (Hegre & Nygård, 2014). Information on year of first oil income is from Ross (2012), complemented by information in Petrodata (Lujala et al., 2007)

Legacy of Public Service Provision: *Education Attainment Ratio*

I proxy the quality of countries' public service provision their performance in providing education to its people. General provision of education requires considerable organizational and implementational capacity on the part of the state. Therefore, states' efficiency in providing education may be seen as a plausible indicator of its' capacity to formulate and implement public policy (Barakat & Urdal, 2009).[66]

For data on countries' education provision, I use a measure of male secondary education attainment developed by Hegre et al. (2013, p. 11). This variable reflects the "proportion of males aged 20-24 with secondary or higher education of all males aged 20-24". The measure is carefully constructed on the basis of several reputed sources to expand the number of observations across countries and years.[67] The original variable covers the years 1970-2009, but I rely on the identical variable from Hegre and Nygård (2014), which has been extended back to 1960 by multiple imputation.

As with the foregoing measure of institutional legacy, I create a variable *initial education* to facilitate test of the hypothesis that the impact of oil is conditional on initial quality of public services. Initial education reflects the country's value on the education attainment variable the first year of oil income, or the value of education in 1970 if the first year of oil income is 1970 or before.[68] To facilitate comparison with non-oil countries, I operationalize non-oil countries' initial education as the value of education attainment in 1970.

A threat to the validity of using education attainment as an indicator of the quality of a country's public service provision is that education may also be provided by non-governmental organizations or international organizations that operate independently from the state. However, I find it questionable whether NGO activities may greatly affect the general education level in a country. Moreover, focusing on secondary education may be a way to circumvent this problem as NGOs largely involve in primary education provision.

[66] To recall, the theoretical concept that this is supposed to reflect is the level of reciprocity and responsiveness of state to society, or the relative "attachment" between a state and its' populace, which are central ideas in the "government detachment" explanation of oil and armed conflict.

[67] I refer to Hegre et al. (2013, pp. 11-12) for details on sources and methods for construction of the variable.

[68] I consider it better to use the 1970 value of education as an approximation of initial education for countries with first oil income in or before 1970 since education data in 1970 is observed, while education data for earlier years are estimated. The same also applies to initial education for non-oil countries.

Another issue is that my measure of education only considers education attainment among males. While this may be interpreted as a shortcoming to the measures' validity, I will claim the opposite. If female education attainment was to be included in the measure, observations would also reflect gender attitudes in a country, which is a somewhat different theoretical concept than the quality of public service provision. Attainment of secondary education among females could plausibly reflect a country's gender attitudes more than the capacity of the state to provide education. If a state is able to provide education to males, but not to females, it may not be the states' capacity to provide education that fails, rather prevailing gender norms of that society. By focusing on male education only possible disturbance from gender attitudes may be avoided.

4.2.2.3 Conflict Legacy

In order to test the hypothesis that the effect of oil income on conflict risk is mediated by a country's recent experience of armed conflict *prior* to oil, I construct the variable *pre-oil conflict.* This is a dichotomous variable indicating whether (1) or not (0) a country experienced armed conflict on its territory during any of the 10 years leading up to the first year of oil income.[69] For measuring the incidence of conflict within a country during the pre-oil period, I use the UCDP/PRIO Armed Conflict Dataset v.4-2012 (Gleditsch et al., 2002; Themnér & Wallensteen, 2012), and include all types of conflict that include the use of force by a non-governmental formally organized group within a country's territory.[70]

Since my oil income data starts 1960, I assume that countries with first oil income in 1960 may also have had oil income *before* 1960. For these countries I use the year of first production of oil and gas according to PETRODATA v.1.2 (Lujala et al., 2007) as year of first oil income.

For countries with 1945 as first year of production, I am unable to measure whether they had an armed conflict during the 10 years leading up to 1945 as the conflict data only extends back to 1946. Including these countries in the analysis therefore introduces uncertainty to

[69] For instance, for Oman, with first year of oil income 1964, the variable "previous conflict" indicates whether armed conflict occurred within the country's territory during the ten-year period 1955-1964.

[70] Following UCDP/PRIO typology, this applies to type 1 ("extra-systemic" (colonial)), 3 ("internal") and 4 ("internationalized internal"). Extra-systemic conflicts are coded such that they are measured as an incidence in the country where they geographically occurred, not as an incidence of the colonial power which is the default coding.

results. Excluding them from the analysis leads to fewer observations and reduced comparability with other studies. As it is unclear what is worse, I manage these countries in two different ways in the analysis. First, I include them, but treat them as if they had *no* armed conflict prior to oil. Second, I run an analysis where these countries are excluded entirely, and compare results to the analysis where they are included.[71]

I also construct a secondary version of conflict legacy: *pre-nationalization conflict* indicates whether (1) or not (0) armed conflict occurred on a country's territory during the ten years leading up to the first year of nationalized oil income.

Two remarks on the reliability of these measures may be noted. First, the selected reference points for determining the period of measurement of previous conflict (year of first oil income and year of first nationalized oil income) may be criticized as somewhat arbitrary. This is because present theory is not definite on the issue of *when* oil starts to induce conflict: Is it when oil is discovered (as implicit in the honeypot and booty-futures financing mechanisms), when production commence (as in extraction related grievance mechanism), or when oil rents start to flow into governments accounts (as in state-centered mechanisms)? Due to different explanations of the relationship many possible reference points may be relevant. Yet, I find the two selected reference points defendable for the following reason: so far, there has been no quantitative empirical support that discovered reserves induce conflict,[72] and both production start and income is proxied by the measure of first year of oil income applied here. Moreover, the reference point using first year of nationalized oil income approximates the point in time which following state-centered mechanisms should be the point when the conflict-inducing effect of oil ensues.

A second remark relating to the reliability of the indicator is that measuring conflict prior to oil in a period of ten years is a somewhat arbitrarily selected time frame. While the choice may be justified by the notion that the effect of a prior conflict may be expected to decline over time due to a gradual reduction of conflict-specific capital (Collier & Hoeffler, 2004), it is not easy to justify why ten years is preferable to nine or eleven or some other frame. For now I will simply state that the arbitrariness of the selected time frame is a weak spot of the

[71] See Table C.1 and C.2 in Appendix C for an overview of the legacy of armed conflict prior to oil income and oil nationalization for oil countries included in the sample. Countries with unknown pre-oil conflict legacy due to early commencement of oil production and -income are in a separate category.

[72] Except the finding that petroleum reserves may prolong ongoing conflict when located within the conflict zone (Lujala, 2010).

measure, but at least it is applied consistently across cases. Eventual findings may indicate whether further inquiries with alternative operationalizations should be pursued.

4.2.3 Control Variables

In order to reduce the problem of omitted variable bias, my baseline model includes a set of control variables. These are variables that have been identified as robust and sizable determinants of armed conflict onset in previous inquiries. They are included not only for being robust determinants, but also because they are theoretically relevant and thus may introduce bias in other explanatory variables if omitted. I do however attempt to restrict the number of control variables, and the choice of including a variable is guided (while not always determined) by the variables' contribution to improve the model's predictive ability. As controls I include a country's GDP per capita, population size, regime type and instability, and regional affiliation. I also control for temporal and spatial dependence of armed conflict onset. In the following, I account for why these variables are included, and provide information on operationalizations and sources of data of the variables.

GDP per Capita

Countries' income level, or GDP per capita, stands out as one of the most robust determinants of armed conflict (Hegre & Sambanis, 2006). It is a catch-all measure which has been used to proxy diverse theoretical concepts, most prominently the economic opportunity cost of participating in rebellion (Collier & Hoeffler, 2004) and state capacity to prevent insurgency (Fearon & Laitin, 2003). The GDP per capita variable used here is taken from a dataset provided by Hegre and Nygård (2014). The variable is calculated on the basis of data from Maddison (2007) the World Bank (2011); and Gleditsch (2002), and measured in international Geary-Khamis dollars for comparability across national currencies. The variable is log transformed, and it is lagged one year to reduce endogenity.

Population Size

Another highly robust finding is that populous countries are exposed to greater risk of armed conflict than countries with smaller population (Raleigh & Hegre, 2009). Large populations may be more difficult to control and contain a larger number of potential rebel recruits (Fearon & Laitin, 2003, p. 81). The variable on countries' population size is obtained from the Hegre and Nygård (2014) replication data, and based on original data from World Population

Prospects 2006 (United Nations, 2007) produced by the United Nation Population Division. The variable is log transformed in order to reduce the impact of very large populations.[73]

Regime Type and Transition

The relationship between regime type and conflict risk has been subject to extensive scrutiny and debate in recent years. In theory, the relationship is straightforward: semi-democracies and regimes in transition are the most conflict prone regime types as they engender both motivations and opportunities of rebellion, while autocracies and democracies are less at risk of internal conflict.[74] In the empirical field, there has been a move from a near consensus around findings in support of these notions (Fearon & Laitin, 2003; Hegre et al., 2001), to noteworthy criticism concerning the applied measures (Vreeland, 2008), leading up to more improved indicators, but also more complex findings on relationships between regime type and armed conflict onset.[75]

While democratic institutions per se may be ineffective in reducing conflict risk, there are nonetheless good theoretical expectations that different regime types may affiliate with different exposure to internal armed conflict (Gleditsch et al., 2009). Considering the plausible interrelationship between oil rents and regime (Andersen & Ross, 2013; Ross, 2012), a variable for regime type should be included in order to reduce this potential source of omitted variable bias.

For information on regime type and transition, I rely on data provided by Gates et al. (2006).[76] The nominal variable "*regime type*" is based on the so-called SIP (Scalar Index of Polities) scale, and indicates whether the country was an inconsistent regime (0), a regime in transition

[73] Countries' population size is also incorporated in the *oil income per capita* measure (and nationalized oil income per capita) and in the *GDP per capita* measure.

[74] If armed conflict is seen as a product of rebels' motive and opportunity, democracies should associate with lower probability of armed conflict based on the assumption that democracy reduces motivation for rebellion by providing non-violent channels for competition for political objectives (Gleditsch et al., 2009, p. 162). Autocratic regimes should associate with low probability of conflict as opportunity of rebellion is constrained by effective repression. In mixed and transiting regimes, where non-violent political channels are partly barred, and repression only partly applied, the mix of opportunity and motive produce a higher risk of armed conflict.

[75] For instance, findings by Hegre (2003) and Collier and Rohner (2008) indicate that the effect of democracy may be contingent on countries' economic status; and Buhaug (2006) finds that democracies are more prone to territorial conflicts than the other regime types, while democratization may place countries at risk of conflict over government.

[76] I find this the most appropriate source of data on regime type as other data sources such as the Polity index and Freedom house data are coded with reference to political violence, implying an apparent problem of endogeneity (Vreeland, 2008, p. 414).

(1), an autocracy (2), or a democracy (3) in the observed country-year.[77] The variable is lagged one year to mitigate reverse causality. In the empirical analysis, this variable transformed to a set of categorical dummy variables, where inconsistent regimes are used as the reference-category.

Rough Terrain

I include a variable indicating the percentage of a country's territory covered by mountains, as this has been found to be a robust determinant of civil war (Fearon & Laitin, 2003; Hegre & Sambanis, 2006) and may proxy availability of rebel sanctuaries. I rely on data from Tollefsen et al. (2012) for this measure, which has better country coverage than the indicator used by Fearon and Laitin (2003). The variable is log transformed. The variable may be regarded time-invariant within the analyzed time period and therefore only make sense to include in cross-country comparison.[78]

Regional Affiliation

Internal armed conflict is not uniformly distributed across the globe, some geographical regions are more conflict prone than others. To reduce the potential of bias from this, a number of regional control variables were added in the basis model. None of them were significant, and I decided to include only the one regional control which improved the explanatory ability of the model; the Middle-East and Northern-Africa region.[79] This is also a theoretically relevant region when it comes to oil-producing countries: the entire field of rentier-state theory builds on studies of oil countries in the Middle-East region. The variable is dichotomous, (1) if the country belongs to the Middle-East North-Africa region, (0) if not.

Temporal Dependence: Time since Conflict

As emphasized by Beck et al. (1998), observations in binary dependent variables in TSCS data are likely to be temporally dependent, which violates the assumption of independent observations and may lead to incorrect parameter estimates. Applied to my analysis, this

[77] The original SIP scale is a continuous variable assigning each country-year a value between 0 and 1, based on features of the country's regime that year, and where "ideal" autocratic regimes are found near the low end of the scale while "ideal" democratic regimes are found near the high end of the scale. Regimes in transition are coded as missing. For further information on the coding rules of the SIP scale I refer to Gates et al. (2006, p. 898). The regime type variable which I use in my analysis codes country-years with a SIP score between 0 and 0.2 as autocracies, SIP scores between 0.8 and 1 as democracies, SIP scores > 0.2 & <0.8 as inconsistent regimes, and missing SIP scores as transitioning regimes.

[78] I refer to Tollefsen (2012, p. 10) for more information on the coding of this variable.

[79] This investigation was conducted by a series of likelihood ratio tests of the baseline model with particular regional dummies versus models without the particular region dummy.

means that an outbreak of conflict in one year may increase the likelihood of conflict outbreak in subsequent years. Another condition that may introduce temporal dependence of observations is a country's recent liberation. In order minimize the potential bias in estimates introduced by a previous outbreak of conflict or independence; a parameter for the impact of this on conflict risk should be included in the baseline model.

There are several solutions to this problem available in the literature. I rely on a procedure suggested by Raknerud and Hegre (1997), and model the expectation that the impact of the previous conflict will decline over time, expressed by the following decay function: 2^(-time since last onset or independence/α), where "α" refers to the half-life parameter.[80] After testing and comparing the log-likelihood of the baseline model using different half-life parameters, I choose the parameter which predicts the data best, which is 10 years.[81]

Spatial Dependence: Neighborhood Conflict

Several studies demonstrate that armed conflict in adjacent territories have a spill-over effect across borders which increases the risk of armed conflict within a country (Gleditsch, 2007; Hegre & Sambanis, 2006). This contagion effect is more richly described in qualitative works, illuminating many transnational aspects of armed conflict.[82] In my analysis, this challenges the assumption that observations of the dependent variable are independently and identically distributed, and may lead to incorrect parameter estimates. I address the potential dependency between armed conflict onset and neighborhood conflict by adding a control variable indicating whether (1) or not (0) there was armed conflict in the neighborhood during the year prior to the year of observation of the dependent variable.[83]

[80] In more sophisticated approaches to handle this problem, the impact of proximity of independence and proximity of conflict are estimated separately (see e.g. Hegre et al., 2001, p. 37). Yet combining the estimate of the impact of proximity of conflict and independence is common, and involves little practical difference to separate estimates.

[81] This is the procedure pursued by Buhaug (2006, p. 699) and Fjelde (2009, p. 206). Increasing the half-life parameter above 10 years (20, 50 and 100) yielded nearly identical log likelihoods, and had no impact on the estimates of other variables.

[82] Dokken (2008, pp. 55-77) note the following contagion aspects of conflicts in Africa: strategic alliances between rebel groups and political elites of neighboring countries, trade-networks and recycling of small arms and light weapons, cross-border mercenaries and militarized refugees, "ordinary" refugee flows, and natural resource trade.

[83] A country's neighborhood is defined as all countries that share a border with the country, defined as "having less than 100 km between any points of their territories" (Hegre et al., 2013, p. 9).

Table 4.2 below summarizes the indicators and data sources of dependent-, independent- and control variables of this analysis. Descriptive statistics are presented in Table 4.3 and Table 4.4 on the next page.

Table 4.2: Overview of Variables, Indicators and Sources

	Variable Name	*Data source(s)*
Dependent variable	Internal Armed Conflict Onset	UCDP/PRIO Onset of Intrastate Armed Conflict Dataset v.4-2012
Independent variable		
Petroleum Resources	Log Oil Income per Capita	Ross (2012).
	Nationalized Log Oil Income per Capita	Nationalizations data: Guriev et al. (2011); Oil data: Ross (2012)
Mediating variables		
Institutional Legacy	Initial Bureaucratic Control	Hegre and Nygård (2014). Original sources: ICRG Bureaucratic Quality and WGI Government Effectiveness.
	Initial Education	Hegre et al. (2013); Hegre and Nygård (2014). Original sources:Lutz et al. (2007); Barro (2000); Samir et al. (2010)
Conflict Legacy	Pre-Oil Conflict	UCDP/PRIO Armed Conflict Dataset v.4-2012. Oil Data: Lujala et al. (2007); Ross (2012). Nationalizations: Guriev et al. (2011).
	(Pre-Nationalization Conflict)	
Control variables	Log GDP per Capita	Hegre and Nygård (2014). Original sources: Maddison (2007) the World Bank (2011); and Gleditsch (2002).
	Log Population Size	Hegre and Nygård (2014). Original source: World Population Prospects 2006 (United Nations 2007).
	Regime Type and Transition (Inconsistent, Transition, Autocracy, Democracy)	Gates et al. (2006)
	Log Rough Terrain	Tollefsen et al. (2012)
	Middle-East and North-Africa	Ross (2012)
	Proximity of Conflict	Estimated following Raknerud and Hegre (1997)
	Neighborhood Conflict	Hegre and Nygård (2014)

Table 4.3: Descriptive Statistics for Continuous Variables 1961-2007

	N	Mean	SD	Min	Max	5th pctile	95th pctile	Missing
Ln Oil Income/cap $_{t-1}$	6847	2,164	2,820	0	11,147	0	7,871	322
Ln Nationalized Oil/Cap $_{t-1}$	6847	1,974	2,746	0	11,147	0	7,743	322
Initial Bureaucratic Control	7125	0,134	0,866	-1,879	2,084	-0,999	1,807	44
Initial Education Attainment	7085	0,461	0,292	0,022	1	0,075	0,993	84
Ln GDP/cap $_{t-1}$	6939	8,003	1,121	5,330	10,905	6,361	9,840	230
Ln Population	7087	8,864	1,677	4,690	14,100	5,863	11,599	82
Ln Rough Terrain	7122	2,368	1,587	-3,571	4,565	-0,383	4,337	47
Proximity of Conflict	7127	0,128	0,253	0	1	0	0,707	42

Table 4.4: Frequency Tables for Categorical Variables 1961-2007

Onset of Internal Armed Conflict			Frequency	Percent
		0	6,892	96.14
		1	235	3.28
		.	42	0.59
		Total	7,169	100.00
Pre-Oil Income Conflict			Frequency	Percent
		0	6,537	91.18
		1	632	8.82
		Total	7,169	100.00
Pre-Nationalization Conflict			Frequency	Percent
		0	5,748	80.18
		1	1,421	19.82
		Total	7,169	100.00
Regime Type and Transition $_{t-1}$			Frequency	Percent
	Inconsistent	0	1,325	18.48
	Transition	1	623	8.69
	Autocratic	2	2,818	39.31
	Democratic	3	2,338	32.61
		.	65	0.91
		Total	7,169	100.00
Middle East and North Africa			Frequency	Percent
		0	6,341	88.45
		1	828	11.55
		Total	7,169	100.00
Neighborhood Conflict $_{t-1}$			Frequency	Percent
		0	2,806	39.14
		1	4,281	59.72
		.	82	1.14
		Total	7,169	100.00

$_{t-1}$Variables lagged one year

4.3 Statistical Model

As the dependent variable in this analysis is dichotomous, I will apply logistic regression as this is a statistical model well suited for estimation of binary outcome variables (Stock & Watson, 2012, pp. 423-447). Two commonly noted benefits of using the logistic model compared with a linear probability model are that logistic regression always yields estimated probabilities of an event occurring within the 0-1 range,[84] and the stretched-S functional form better captures the non-linear nature of the population regression function.[85]

In my analysis, the logistic model estimates a function for the *log odds* that the value of the dependent variable equals one; that is, the log odds of an armed conflict onset. Coefficients of independent variables indicate the expected change in log odds of conflict onset associated with a one-unit change in the independent variable. As change in likelihood of an event occurring expressed in log odds is not intuitively accessible, I will report results in odds ratios.[86] Expressed in odds ratios, the value of a coefficient indicates the multiplicative increase in odds of armed conflict onset for a one-unit change in the dependent variable. Coefficient values above 1 indicate an increase in odds (and probability) of armed conflict onset, while values below 1 indicate a decrease in odds (and probability) of conflict onset.

As armed conflict onsets rarely occur (internal armed conflict commenced in only 235 of the 7169 country-years included in the analysis), standard logistic estimation may lead to underestimation of the probability of such rare events (King & Zeng, 2001). I therefore considered using rare events logit as a procedure for correcting for this, but found that results were largely the same regardless of model employed. I prefer ordinary logit as this model allows for interaction terms and comparison of log likelihoods.

In order to make analysis results sensible, I will supplement regression tables with calculations of predicted probabilities for meaningful values of independent variables and

[84] A linear probability model may yield estimates that are outside the 0-1 range, which is meaningless when the coefficient is to be interpreted as the estimated probability of an event occurring.

[85] Logistic regression is not the only statistical model with these benefits, however. The probit model has nearly identical properties as the logistic model, and the models produce approximately identical estimates in equal analyses (Stock & Watson, 2012, p. 435).

[86] Odds ratios express the probability that Y=1, divided by the probability that Y=0. Log odds are natural logarithms of the odds.

calculations of expected changes in probability for meaningful changes in independent variables.[87]

4.4 Methodological Challenges

This section addresses a number of methodological challenges that all represent threats to the validity of inferences made from the analysis results. Some have been briefly mentioned above, but here I will discuss them in a more structured manner in terms of what problems they introduce and what is done to alleviate them.

Omitted Variable Bias

Omitted variable bias becomes an issue when a variable which is excluded from the analysis is correlated both with the dependent variable and one or several independent variables. This may lead to misleading estimates of impact of the explanatory variable(s) on the dependent variable, and is always a potential problem in regression analysis (Stock & Watson, 2012, p. 221). The inclusion of control variables identified as important conflict determinants in other studies reduces this problem, and the intercept also incorporates effects from omitted variables. Omitted variable bias will also be addressed by testing the robustness of results by using an OLS linear probability model with country fixed effects.[88]

Imputed Data

An important methodological dilemma of this thesis is that it relies on data which to a notable extent has been estimated by multiple imputation in order to overcome the problem of missing observations and extend coverage back in time. This issue is particularly relevant in the test of the hypothesis that the conflict-inducing effect of oil is mediated by a country's *bureaucratic legacy*, as nearly all of the observations on this variable will reflect estimated, not empirically observed values.[89] As noted in the description of this variable, it is based on replication data

[87] All statistical analyses, graphed results and calculations of substantive results are conducted in Stata 13.

[88] Using country-fixed effects is a way of controlling for country-specific omitted variable bias. Country-fixed effects in logistic analysis may not be appropriate, however, as countries with no variation in the dependent variable will be excluded from the analysis (Beck & Katz, 2001).

[89] This is because initial bureaucratic quality is either measured at the time when the country started to earn oil income, or 1960 for non-oil countries, which are predominantly points in time before observed data on the operationalized independent variable exist.

from the Hegre and Nygård (2014) *bureaucratic quality index*, which is constructed on the basis of observational data from 1984, while data back to 1960 are estimated.[90]

The main drawback of relying on this measure is that the hypothesis is tested on a considerable amount of estimated, not observed data. This obviously challenges the general rule that in order to produce reliable knowledge about causal relationships in real-world phenomena, theoretical expectations need to be evaluated against empirical observations. Yet, when it comes to bureaucratic quality, country-wide observational data on this prior to mid-1980 does not exist. This does however not mean that bureaucratic quality was a non-existent phenomenon prior to 1980. What it does mean is that to be able to bring this condition into the analysis, I have to use some approximation of it.

Accepting this, I find the use of the bureaucratic quality index defendable for the following reasons: First, it is constructed on the basis on two of the best existing data sources of bureaucratic quality with the best coverage in countries and years: the ICRG and WGI data material.[91] Second, the measure has been developed by highly reputed scholars in the field, that developed it for their own purposes, and their resultant work has been accepted for publication in the journal of peace research, which is a peer-reviewed journal.

Third, it may be seen as a more sophisticated measure for capturing an institutional aspect central in state-centered explanations of the resource curse than any alternative measures employed so far in the scholarly debate. Scholars investigating the topic so far have either used *no* exogenous measure for institutional dimensions implicit in their arguments,[92] or quite *distant* proxies for states' institutional capacity,[93] or they have employed *fixed* measures of

[90] Other variables that I have obtained from the Hegre and Nygård (2014) replication data have also undergone multiple imputation in order to reduce missingness, including the education attainment variable (years 1960 to 1969), and also the GDP per capita variable. The replication data represent the mean of imputed values from repeated imputation of variables, and could thus be interpreted as the "best guess" for the true value of the missing observation.

[91] In a recent review of construct validity of measures used in the state capacity debate, Hendrix (2010, p. 283) finds that measures capturing bureaucratic quality are among the most valid indicators of state capacity compared with alternatives.

[92] In the analyses by Fearon and Laitin (2003)and Fearon (2005), oil exports is presented as a default indicator of weak state capacity. In de Soysa and Neumayer (2007), the conflict-inducing effect of non-lootable rents is explained as a result of their impact on state capacity, while no exogenous measures of such capacity are employed.

[93] Humphreys (2005) shows that conflict risk increases with *past* oil production, using this as a proxy of the link between oil and weak state capacity.

institutional capacity often measured at a later point in time than the phenomena they wish to explain, hence disregarding the logic that cause must precede effect.[94]

Using initial education attainment ratio as an alternative indicator of institutional legacy offers the possibility of convergent validation of the measure (Adcock & Collier, 2001, p. 540). Insofar as countries' scores on alternative indicators of the concept of institutional legacy are empirically related and produce similar results in analysis, this may be taken to support that the indicators capture the same underlying theoretical concept. The correlation coefficient between the bureaucratic quality measure and the education attainment measure is 0.6, which indicates a strong positive relationship and thus a high degree of convergence between the measures. The correlation coefficient for initial bureaucratic quality and initial education is 0.55, which also verifies a substantial convergence among the measures.

While I have used these two indicators of institutional legacy, they are clearly not the only ones possible; and different proxies of institutional legacy could potentially be derived from the theoretical concept. Replicating the analysis with several alternative indicators could thus provide further indication of the strength of the hypothesized relationship.

Simultaneous Causality

A main threat to the validity of inferences on the nature of causal relationships based on results from statistical analysis is that it may be difficult to determine which direction the causal relationship takes (Lund, 2002). While the empirical design assumes that the causal effect runs from the independent variables to the dependent variable, armed conflict may simultaneously affect several of the independent variables. In my analysis, armed conflict may be expected to affect oil income, bureaucratic quality, education attainment, GDP per capita and also regime type. In order to reduce the problem of endogeneity, these explanatory variables are lagged one year, so as to comply with the logic that cause(s) precede effect in time. Although this is a widely applied way of coping with the problem of simultaneous causality, it may not eliminate the problem altogether.

[94] In their study of the mediating effect of institutional quality on the resource curse, Mehlum et al. (2006) measure institutional quality at a point in time towards the end of the period for which they measure the dependent variable. The outcome variable, GDP growth, indicates the average GDP per capita growth between 1965 and 1990, while the purported mediating variable; institutional quality; is measured at one point in time in 1982 (Mehlum et al., 2006, pp. 13, 16). The problem of this type of design is that it makes no attempt to ascertain that cause comes before effect, which is necessary for their causal explanation to hold.

Non-Independent Observations

An assumption underlying statistical inference is that observations are independently and identically distributed, so that the value of Y_2 is independent from Y_1. This assumption is likely to be violated in the case of the dependent variable, as the likelihood of an internal armed conflict onset may be partly dependent on previous onsets in the same country and in neighbor countries. As discussed above, including a decay function for previous onset of armed conflict in the same country as well as a control variable for armed conflict (lagged) in an adjacent country aims to reduce the problem of such dependencies.

Autocorrelation and Heteroscedasticity

Some common problems associated with TSCS data structures will be mentioned as they have implications for the analysis. First, TSCS data is often characterized by temporal and spatial autocorrelation and panel-level heteroscedasticity.[95] In OLS estimation, these issues violate the assumption that error terms are uncorrelated and have the same variance (Kennedy, 2003, 8.1-8.4). The same issues may be problematic in logistic regression, which is the statistical model used here, and lead to biased and misleading parameter estimates and incorrect standard errors. Panel-level heteroscedasticity will here be addressed by clustering standard errors on country.[96] Temporal autocorrelation will be addressed by controlling for previous conflict.

Chapter 5 on the coming page proceeds with results from the empirical analysis.

[95] Temporal autocorrelation refers to correlation between error terms across time, for instance within a panel. An example could be that the error term for armed conflict onset in Malawi in 1978 is likely to be correlated with Malawi in 1977. Spatial autocorrelation occurs where error terms are correlated across panels within a time period. Heteroscedasticity occurs when error terms do not have constant variance across panels, for instance if Malawi repeatedly had larger variance in error terms than Cambodia throughout a time-series.

[96] Clustered standard errors allow errors to have an arbitrary correlation pattern within a cluster, but assume that the errors are uncorrelated between clusters.

5 Results

This chapter presents the results from the quantitative analysis of the hypotheses outlined in the theory chapter. These hypotheses proposed that the conflict-inducing effect of oil income may be conditioned by

- the extent of initial bureaucratic control in a country (H1)
- the quality of initial public service provision (measured by initial education) (H2)
- a country's legacy of armed conflict at the commencement of oil income (H3)

Before embarking on the test of hypotheses, I will briefly review results from the baseline model used in all further regressions. Next, results from the analyses of H1, H2 and H3 will be presented. For each hypothesis, I present results from regression models conducted on two different samples: including a sample of 100 oil countries, and a "full sample" of 170 oil and non-oil countries.[97] All coefficients in regression tables are exponentiated and reported in odds ratios rather than log odds for more intuitive interpretation.

In order to aid evaluation of size and significance of interaction terms, regression tables are supplemented with graphed results of the conditional effect of oil income on risk of onset for representative values of the hypothesized mediating variables, while holding other variables constant at values representative for oil-countries at large.

To further facilitate interpretation of the results in substantive terms, I graph predicted probabilities of armed conflict onset across the range of values of oil income, conditional on values of interest of mediating variables. As the values of control variables also greatly matter for predicted probabilities, I draw on two real-world cases of aspiring petroleum countries: Tanzania and Uganda, and fix control variables to values representative of each country in 2007. This enables grasping the conditional nature of the impact of oil income on conflict risk, dependent on initial institutional legacy and conflict legacy; given present conditions of two conceivable cases.

[97] An exception to this is H3, for which the analysis of the sub-sample of oil countries is conducted by a bivariate analysis. Since the research objective both addresses what explains divergent experiences of armed conflict among oil countries, and what conditions the general conflict-inducing effect of oil, I find it justified to study the hypotheses both a within a sample of oil countries, and within a sample of all oil and non-oil countries.

In order to validate the obtained findings, the regression analyses are supplemented with a systematic evaluation of models' performance and robustness.

The main findings are as follows:

- Results indicate partial support of the proposition initial bureaucratic control mediates the effect of oil income on conflict risk (H1). At lower values of initial bureaucratic control, the effect of oil on conflict risk is high (relatively speaking) and significant, while this effect is gradually vanes and becomes inseparable from zero at higher values of initial bureaucratic control. As this conditional effect is not consistent across the entire range of initial bureaucratic control, the interaction coefficient does not achieve overall significance. This interpretation is unchanged upon removal of influential outliers.
- The proposition initial education attainment mediates the effect of oil income on conflict risk (H2) receives partial to full support. At lower initial education attainment ratios the effect of oil on conflict risk is relatively high and significant, while for higher initial education attainment ratios, the effect of oil income on conflict risk is reduced and becomes inseparable from zero. This conditional effect is significant when 5 influential deviant country-years are excluded from the analysis of 6695 country-years between 1961 and 2007.
- Results do not support the proposition that countries' legacy of armed conflict mediates the conflict-inducing effect of oil income (H3). Yet, there is significant evidence that conflict legacy distinguishes oil countries' risk of conflict onset independently from size of oil income. Whether or not this strong effect of conflict legacy is entirely independent from the presence of oil remains unsettled due to a limitation of the research design.
- Interacting oil income with variables such as initial bureaucratic control, initial education and conflict legacy only slightly adds to the ability to predict observations correctly, as indicated by comparison of ROC-curves. This improvement of prediction comes at the expense of more complex models, as indicated by the models' AIC.

5.1 Statistical Analysis Results

5.1.1 Baseline Model

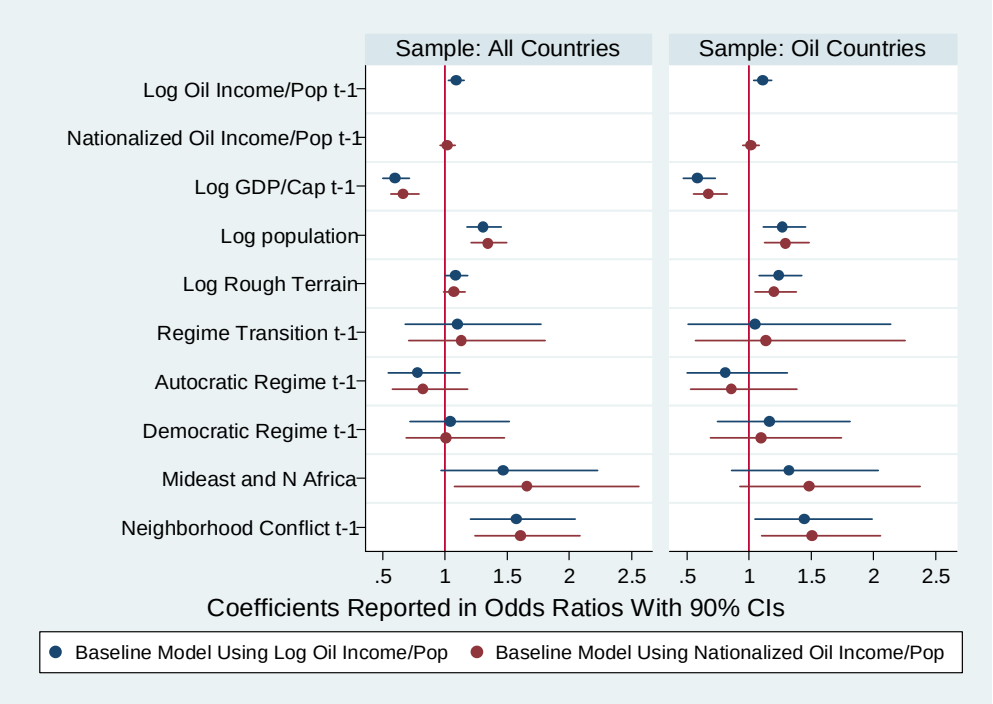
This section will briefly review results from the baseline regression models which are used throughout the analysis. Since the baseline models do not test any of the hypotheses, I have placed tabular results in Table B.1 in Appendix B. Yet, it is of interest to know how the two specifications of the main independent variable, oil, and the control variables behave when regressed on the sample of oil countries and on the full sample of countries.

Figure 5.1 ahead provides a coefficient plot of the baseline models, regressed on the full sample of countries (left plot) and on the subsample of oil countries (right plot). Models using *log oil income/pop* as the measure of oil (blue color) are plotted against the identical model using *nationalized log oil income/pop* as the operationalization of oil income (red color). All coefficients are exponentiated, and indicate the multiplicative impact of a unit increase in the independent variable on the *odds* of internal armed conflict onset in a country-year.[98] When reading the figure it must be kept in mind that independent and control variables vary in terms of scales of measurement, some are measured in dichotomous categories while others are at continuous with different ranges; meaning that the sizes of coefficients, or the magnitude of variables' impact, may not be readily compared across variables of different scales.

As indicated by the graph, the models using *log oil income/pop* as the measure of oil replicates the general finding that oil income increases the likelihood of armed conflict onset. The coefficient of log oil income/pop is significant at the 95% level and of a similar size both when the model is regressed on the subsample of oil countries and when regressed on the full sample of countries. The same is not true when *nationalized log oil income/pop* is employed as the measure of oil: In neither sample is the coefficient for nationalized oil separable from zero. This is somewhat surprising in light of state-centered explanations of the oil-conflict relationship which build on the premise that oil rents seized by governments is the source of the trouble.

[98] Expressed in odds ratios, coefficient values above 1 indicate that an increase in the value of the independent variable is associated with an increase in the odds (and likelihood) of conflict onset, while coefficients below 1 indicates a reductive impact on the odds (and likelihood) of onset for the given variable. Coefficients exactly at 1 would indicate perfectly null impact on the odds of conflict onset for the variable, and unless the confidence interval of the coefficient excludes the value 1 for the given level of significance the null hypothesis of zero impact of the variable may not be rejected.

Figure 5.1: Coefficients Plot of Baseline Models



The left plot graphs results from the baseline models when regressed on the full sample of 170 countries. The right plot graphs results from the identical models when regressed on the sample of 100 oil countries. Results in tabular form are available in Table B.1 in Appendix B. As the coefficient for proximity of conflict is very large compared to the rest and disturbs the graphs scale, it is left out from the figure along with the intercept, although both are included in the regression. Coefficients for regime variables are estimated with inconsistent regimes as reference category.

Coefficients of control variables behave as expected. Increases in GDP per capita relate to significantly lower odds of onset, while larger populations, conflict in the neighborhood and proximate conflict on own territory affiliate with significantly higher odds of onset in all models. The coefficient for rough terrain is larger and more significant in models regressed on the sample of oil countries, indicating that rougher terrain significantly relates to higher conflict risk in particular among oil countries. Being a MENA country also increases the odds of armed conflict, albeit only significantly so in the regressions of the full sample when using nationalized oil income as the measure of oil. None of the regime coefficients are significant, although they suggest that autocracies are somewhat less conflict prone, while regime transitions and democracies are somewhat more conflict prone (and slightly more so in the subset of oil countries than in the full sample), than inconsistent regimes.

5.1.2 Effects of Oil Income Given Initial Bureaucratic Control

The first hypothesis proposes that the conflict-inducing effect of oil may be stronger in countries with low initial bureaucratic control than in countries with high initial bureaucratic control. I test this hypothesis by adding an interaction term between log oil income per capita and initial bureaucratic control to the baseline model.[99] I first conduct the analysis on a subset of oil countries and next on the full sample of countries.

Findings indicate partial support for the hypothesis. As evident in Table 5.1 on the next page, none of the interaction terms of oil income and initial bureaucratic control are statistically significant (see model 3 and 6). Yet, to inspect the possibility that the coefficient for oil income may differ across the range of initial bureaucratic control, I plot the estimated impact of an x-unit increase in oil income on conflict risk for different values of initial bureaucratic control, holding other variables constant. Plotted results do provide support that there is a difference in the significance of the effect of oil on conflict risk dependent on whether initial bureaucratic control is low or high.

The three columns to the left in Table 5.1 are models regressed on a subset of oil countries, while the three columns to the right are models regressed on the full sample of countries. Model 1 is the baseline model with *log oil income* as independent variable, included for comparison. In model 2, *initial bureaucratic control* is added as a single term, to inspect the independent effect of this variable. The coefficient below 1 indicates that higher scores on initial bureaucratic control relate to lower odds of conflict onset, but the finding is far from significant (p-value 0.84). In model 3 an interaction term between initial bureaucratic control and oil income is introduced. The individual coefficient of oil income remains unchanged in size and significance, which may be interpreted such that when the value of initial bureaucratic control is at 0 (slightly below the mean value of 0.13), increases in oil income significantly increases the odds of conflict. The individual coefficient of initial bureaucratic control changes direction, but remains insignificant. The interaction term coefficient of 0.97 indicates that when both oil income and initial bureaucratic control increase in value, this is related to a decrease in odds of conflict onset. The interaction term is not significant, however (p-value 0.41). Control variables remain largely unchanged across these models.

[99] Since only *log oil income/pop* was found to significantly relate to onset, I will base the inspection of the conditioning impact of institutional legacy on this measure of oil.

Table 5.1: Interacting Oil Income and Bureaucratic Legacy. Logistic Regression of the Onset of Armed Conflict 1961-2007

	Model (1) *Oil Countries*	*Model (2)* *Oil Countries*	*Model (3)* *Oil Countries*	*Model (4)* *All Countries*	*Model (5)* *All Countries*	*Model (6)* *All Countries*
Log Oil Income/Pop $_{t-1}$	1.108**	1.108**	1.108**	1.090**	1.090**	1.089**
	(0.010)	(0.011)	(0.010)	(0.013)	(0.014)	(0.014)
Initial Bureaucratic Control		0.967	1.068		0.939	0.953
		(0.835)	(0.751)		(0.595)	(0.724)
Initial Bureaucratic Control * Oil Income/Pop $_{t-1}$			0.970			0.993
			(0.408)			(0.838)
Log GDP/Cap $_{t-1}$	0.585***	0.591***	0.589***	0.599***	0.608***	0.609***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log population	1.271***	1.275***	1.267***	1.309***	1.315***	1.314***
	(0.003)	(0.003)	(0.005)	(0.000)	(0.000)	(0.000)
Log Rough Terrain	1.240***	1.239**	1.235**	1.089*	1.090*	1.089*
	(0.010)	(0.011)	(0.012)	(0.091)	(0.086)	(0.086)
Inconsistent Regime $_{t-1}$ (Ref. Cat.)	-	-	-	-	-	-
Regime Transition $_{t-1}$	1.046	1.044	1.059	1.099	1.102	1.103
	(0.918)	(0.921)	(0.896)	(0.744)	(0.739)	(0.738)
Autocratic Regime $_{t-1}$	0.810	0.812	0.816	0.783	0.784	0.784
	(0.470)	(0.471)	(0.482)	(0.264)	(0.266)	(0.267)
Democratic Regime $_{t-1}$	1.164	1.185	1.187	1.045	1.071	1.074
	(0.570)	(0.540)	(0.536)	(0.846)	(0.764)	(0.757)
Mideast and N Africa	1.322	1.313	1.270	1.469	1.448	1.438
	(0.289)	(0.290)	(0.357)	(0.128)	(0.137)	(0.145)
Neighborhood Conflict $_{t-1}$	1.443*	1.431*	1.424*	1.572***	1.558***	1.556***
	(0.061)	(0.083)	(0.084)	(0.005)	(0.006)	(0.006)
Proximity of Conflict	3.661***	3.674***	3.661***	2.689***	2.707***	2.706***
Observations	3999	3999	3999	6741	6741	6741
Countries	100	100	100	170	170	170
Onsets	152	152	152	222	222	222
Log Likelihood	-578.503	-578.478	-578.251	-886.978	-886.826	-886.811
AIC	1179.006	1180.955	1182.501	1795.956	1797.652	1799.621

Exponentiated coefficients; *p*-values in parentheses; $_{t-1}$ variable lagged one year; SE clustered on country * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The three columns to the right repeat this procedure on the full sample of countries. Findings are similar. The single coefficient of initial bureaucratic control in model 5 remains insignificant, although the p-value indicates that the estimate is somewhat more precise than among only oil countries. When the interaction term is added in model 6, the individual coefficient for oil income remains largely unaltered in size and significance. The confidence of estimated interaction effects in model 6 is further reduced (p-value 0.84), and the size is further neutralized, approaching no impact.

Although tabled results appear to disappoint the proposition, graphed results are somewhat more supportive. Figure 5.2 and 5.3 below plots the conditional effect of a unit increase in oil income for different values of initial bureaucratic control. Figure 5.2 graphs results from model 3 regressed on the subset of oil countries, while Figure 5.3 graphs results from model 6 regressed on the full sample of countries. To facilitate comparison under equal terms, values of control variables are held constant at representative values for oil income countries in 1961.[100] Both plots show estimates with 90% confidence intervals, and effects are reported as the effects of a unit increase in log oil income/pop on *probability*, not odds, of armed conflict onset. The distribution of observations of initial bureaucratic control is indicated by reference to percentiles on the x axis ($_{p10}$= 10th percentile).

Both graphs indicate that for lower values of initial bureaucratic control, the impact of oil on conflict risk is higher, while this impact is reduced and not significantly different from zero for higher initial bureaucratic control. Comparing the plots, the conditioning impact of initial bureaucratic control on the effect of oil is larger in the subset of oil countries than in the full sample, as indicated by a steeper slope and a higher impact of oil income for the lowest initial bureaucratic quality. Figure 5.2 shows that among oil countries, an increase in oil income significantly increases conflict risk for values of initial bureaucratic control between -1 to about .6, a range within which observations between the 10th and 50th percentile of initial bureaucratic control are located.[101] For values higher than approximately .6 of initial bureaucratic control (values shared by observations in the upper quartile of the data), the effect of oil is reduced and is not significantly separable from zero.

[100] As the focus of this thesis is on the role played by initial conditions for the conflict-inducing effect of oil I find it appropriate to use representative values (mean, median or mode) on control variables from the first year for which information is available, which is 1961. The same values on control variables are used for estimating conditional effects of oil throughout the rest of the chapter, except for the estimates of predicted probabilities where another set of fixed values for control variables are employed.

[101] For values below -1 of initial bureaucratic control, the effect of oil is highest, but not significant.

Figure 5.2: Graphed Results of Model 3. Sample Oil Countries

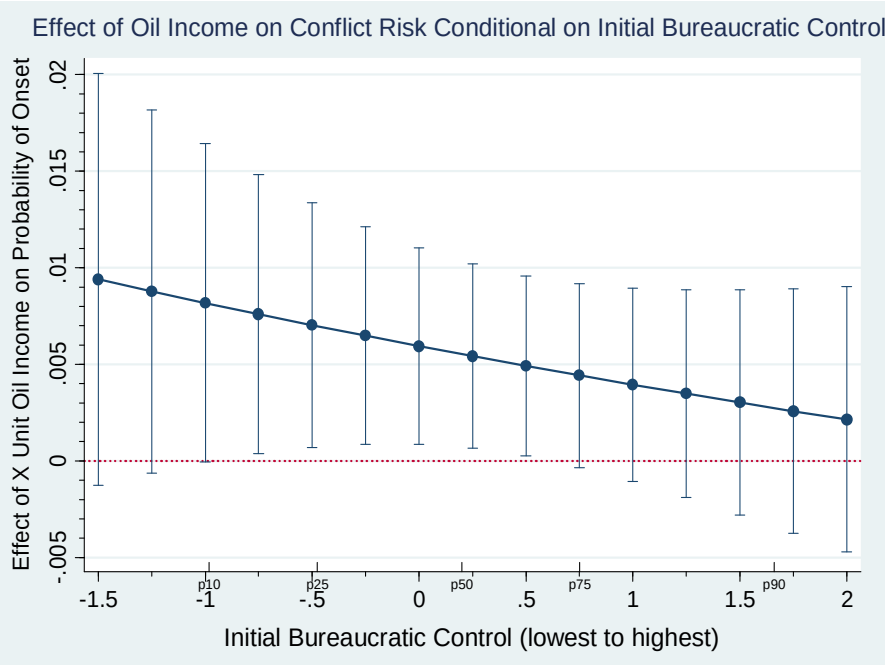
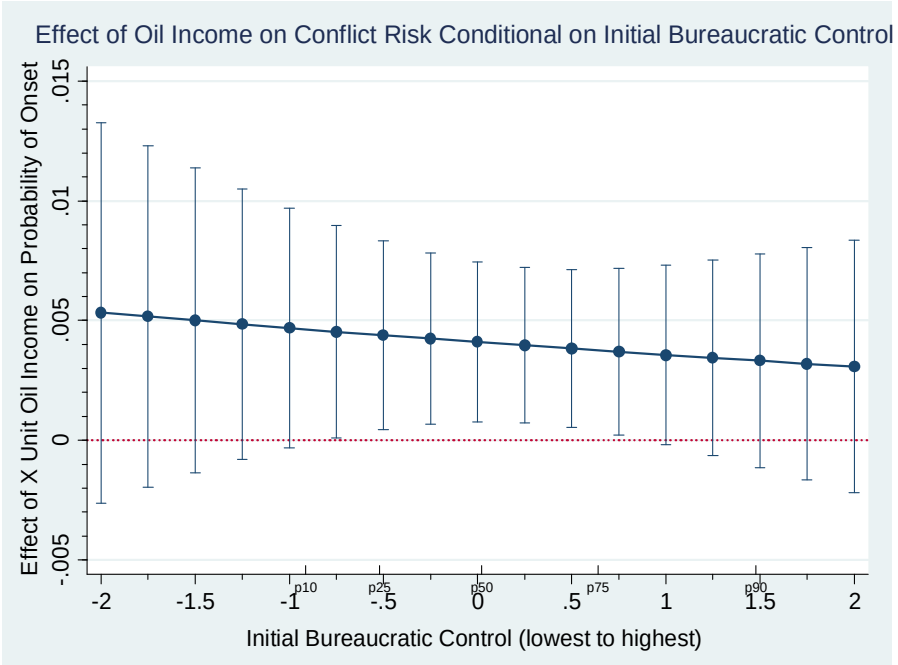


Figure 5.3: Graphed Results of Model 6. Sample All Countries



Both graphs show the conditional effect of oil income for different values of initial bureaucratic control, with 90% confidence intervals. Percentiles of observed values of initial bureaucratic control (for respective samples) are indicated on the x-axis (p10-p90). Other variables are set to mean, median or most frequent values of oil income countries in 1961.

The results from the full sample of countries (Figure 5.3) appear less convincing, but do point in the direction that the conflict-inducing effect of oil is offset by higher levels of initial bureaucratic control. The size of the conditioning impact is however reduced in terms that higher values of initial bureaucratic control involves less of a reduction in the effect of oil income on conflict risk than it did in the sample of oil countries. Here, the conflict-inducing effect of oil income is significant for values of initial bureaucratic control between about -.7 to about .9, gradually decreasing for higher levels of initial bureaucratic control. However, it appears that among both oil and non-oil countries, only very good initial bureaucratic institutions are able to offset the conflict-inducing effect of oil, as only observations above the 80^{th} -90^{th} percentile lay within the range where the effect is lowest and confidence intervals overlap zero. In this plot as well, values of initial bureaucratic control below -.75 are affiliated with the highest impact of oil income on conflict risk, but the estimates are not significant.

In sum, these findings offer partial support of H1, which proposes that the conflict-inducing effect of oil income is conditioned by initial bureaucratic control. Both in the sample of oil countries and in the full sample, the trend in the data is that oil increases conflict risk more severely for lower values of initial bureaucratic control, while the effect is gradually offset by higher initial bureaucratic control. Although interaction terms in the table are void of significance, the finding that at certain lower percentiles of initial bureaucratic control the effect of oil income is significantly different from zero, while the effect of oil is reduced and becomes insignificant at the highest values of initial bureaucratic control, may be interpreted such that under certain circumstances, there is a significant difference in the effect of oil conditioned by initial bureaucratic control.

5.1.3 Effects of Oil Income Given Initial Education Attainment

Hypothesis two proposes that the effect of oil income may be offset in countries with high initial quality of public service provision, here operationalized by initial education attainment. As with the former hypothesis, I conduct the test by adding an interaction term between oil income and initial education first in a regression conducted on a subsample of oil countries, next on the full sample of countries. Results are presented in Table 5.2 on the next page. The three columns to the left present the analyses of the subset of oil countries, while the three columns to the right present the analyses of the full sample of countries.

Table 5.2: Interacting Oil Income and Education Legacy. Logistic Regression of the Onset of Armed Conflict 1961- 2007

	Model (7) *Oil Countries*	*Model (8)* *Oil Countries*	*Model (9)* *Oil Countries*	*Model (10)* *All Countries*	*Model (11)* *All Countries*	*Model (12)* *All Countries*
Log Oil Income/Pop $_{t-1}$	1.103**	1.088**	1.143**	1.088**	1.087**	1.115**
	(0.016)	(0.039)	(0.045)	(0.016)	(0.015)	(0.044)
Initial Education		0.462*	0.719		0.453**	0.546
		(0.066)	(0.592)		(0.039)	(0.187)
Initial Education * Oil Income/Pop $_{t-1}$			0.893			0.937
			(0.428)			(0.584)
Log GDP/Cap $_{t-1}$	0.598***	0.677***	0.672***	0.603***	0.686***	0.683***
	(0.000)	(0.004)	(0.003)	(0.000)	(0.001)	(0.001)
Log population	1.276***	1.266***	1.267***	1.310***	1.315***	1.311***
	(0.003)	(0.004)	(0.004)	(0.000)	(0.000)	(0.000)
Log Rough Terrain	1.239**	1.234**	1.233**	1.089*	1.093*	1.093*
	(0.010)	(0.014)	(0.013)	(0.087)	(0.066)	(0.069)
Inconsistent Regime $_{t-1}$ (Ref. Cat.)	-	-	-	-	-	-
Regime Transition $_{t-1}$	1.153	1.108	1.117	1.102	1.096	1.091
	(0.745)	(0.814)	(0.802)	(0.737)	(0.754)	(0.766)
Autocratic Regime $_{t-1}$	0.789	0.775	0.774	0.788	0.785	0.783
	(0.422)	(0.386)	(0.382)	(0.277)	(0.269)	(0.260)
Democratic Regime $_{t-1}$	1.130	1.096	1.086	1.040	1.040	1.032
	(0.647)	(0.736)	(0.759)	(0.864)	(0.866)	(0.892)
Mideast and N Africa	1.325	1.167	1.132	1.462	1.305	1.273
	(0.286)	(0.558)	(0.628)	(0.133)	(0.287)	(0.337)
Neighborhood Conflict $_{t-1}$	1.445*	1.438*	1.451*	1.574***	1.562***	1.568***
	(0.062)	(0.061)	(0.059)	(0.005)	(0.005)	(0.005)
Proximity of Conflict	3.708***	3.632***	3.608***	2.671***	2.645***	2.632***
Observations	3910	3910	3910	6695	6695	6695
Countries	98	98	98	169	169	169
Onsets	150	150	150	222	222	222
Log Likelihood	-569.134	-567.623	-567.317	-886.360	-884.272	-884.110
AIC	1160.268	1159.246	1160.635	1794.721	1792.545	1794.221

Exponentiated coefficients; p-values in parentheses; $_{t-1}$ variable lagged one year; SE clustered on country* p < 0.10, ** p < 0.05, *** p < 0.01

Again, findings indicate partial support of the hypothesis. The table conveys that none of the interaction terms between oil income and initial education achieve significance. Yet, when plotting conditional effects of oil for the low to high range of initial education, there is a clear trend that at higher levels of initial education, the effect of oil on conflict risk is offset. The conditioning impact is greatest when studying the subset of oil countries.

Model 7 is the baseline model for oil countries included for comparison.[102] Model 8 includes the variable *initial education* as a single term. The coefficient indicates that higher levels of initial education relates to significantly lower odds of armed conflict onset with 90% confidence. When adding the interaction term in model 9, the coefficient for oil increases somewhat in size and remains significant at the 95% level, and indicate that for initial education attainment ratio of 0, a one unit increase in oil income multiplies the odds of armed conflict by 1.143.[103] The single term of initial education indicates that higher levels of initial education when oil income is zero relate to lower odds of armed conflict. The interaction coefficient of 0.89 indicates that when an increase in oil income is combined with an increase in initial education, the impact of oil on conflict risk changes direction. Yet, neither the interaction term nor the single term of initial education achieves significance.

Results from the analysis conducted on the full sample of countries (model 10-12) are largely similar. Model 10 is the baseline model regressed on all countries included for comparison.[104] When controlling for initial education level in model 11, the coefficient indicates that higher levels of initial education level significantly relates to a lower odds of conflict among countries in general. In model 12, the single coefficient of oil income remains significantly above 1, while both the single term of education along with the interaction term drop below 1 and indicate a reductive impact on odds of conflict, both failing to achieve significance.

Although interaction term coefficients are void of stars, graphed results below do support that the initial level of education conditions the effect of oil income. Figure 5.4 and 5.5 graphs estimates of how the impact of an x unit increase in oil income on probability of onset changes across the range of initial education attainment. Figure 5.4 is based on model 9, regressed on only oil countries, while Figure 5.5 is based on model 12 regressed on the full sample of countries.

[102] Model 7 is identical to model 1 in Table 5.1, only excluding two countries due to missing education data.
[103] Since 0 is outside the range of initial education, this estimate is not substantially meaningful.
[104] Model 10 is identical to model 4 in Table 5.1, only excluding one country due to missing education data.

Figure 5.4: Graphed Results of Model 9. Sample: Oil Countries

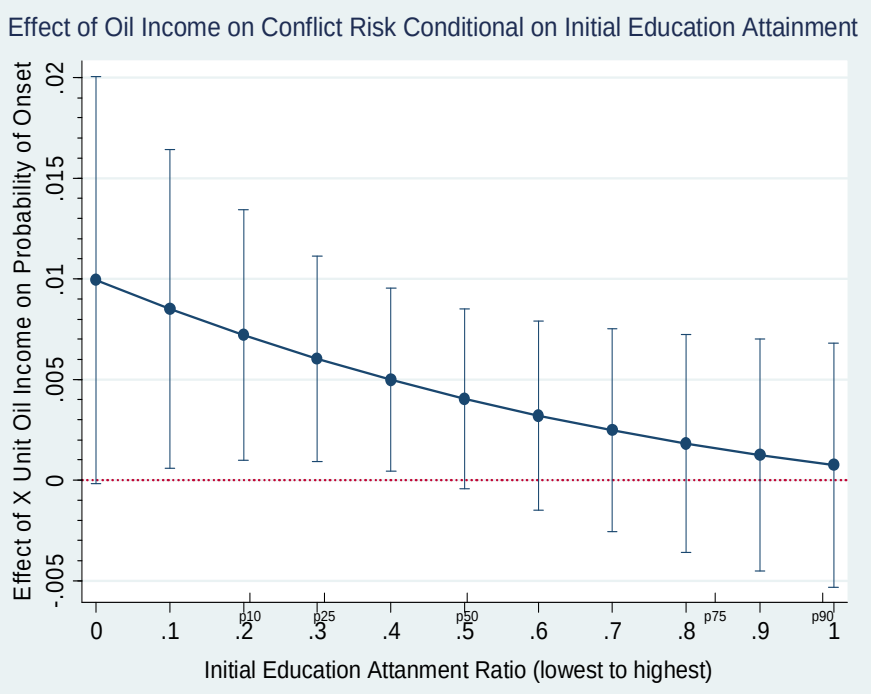
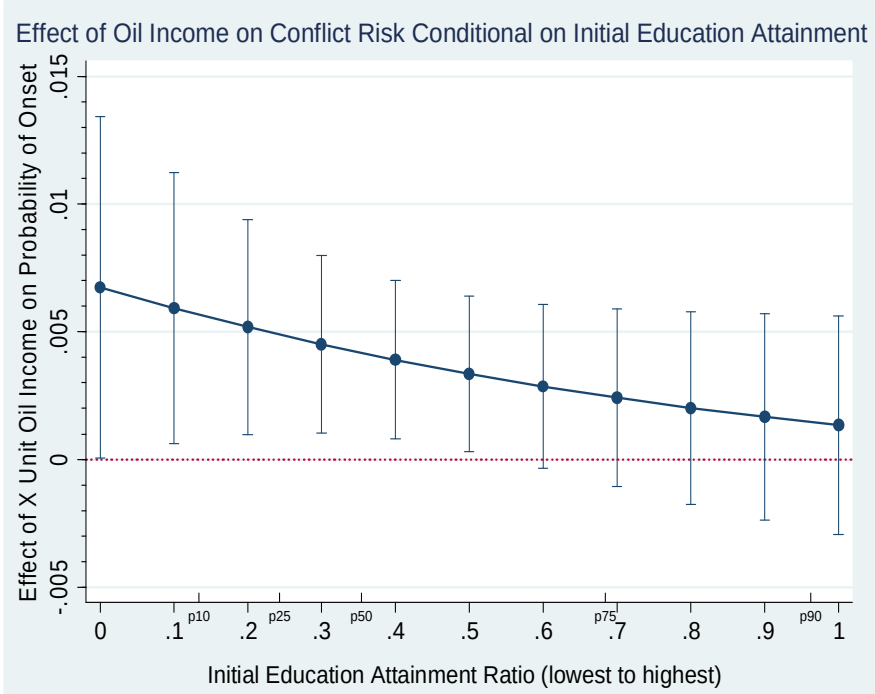


Figure 5.5: Graphed Results of Model 12. Sample: All Countries



Both graphs show the conditional effect of oil income for different values of initial education attainment, with 90% confidence intervals. Effects are reported in probabilities. Percentiles of observed values of initial education attainment (for respective samples) are indicated on the x-axis (p10-p90). Other variables are set to mean, median or most frequent values of oil income countries in 1961.

Plotted results tell the same story whether analysis is conducted on the subset of oil countries or on the full sample of countries. The effect of a unit increase in oil income on probability of conflict onset is highest for lowest initial education attainment, while this effect gradually diminishes for higher values of initial education. Up to an initial education attainment ratio of about .45 (among oil countries), or up to an initial education ratio of about .55 (among the full set of countries), oil significantly increases the probability of onset, while for initial education ratios above these points this conflict-inducing effect of oil is no longer significant. Again, the size of the conditioning impact is larger when analyzed on the subset of oil countries than among the full set of countries, as visible from the greater absolute difference in effect at the extremes of initial education in the upper graph.

5.1.4 Effects of Oil Income Given Conflict Legacy

To test the hypothesis that the conflict-inducing effect of oil income depends on countries' legacy of armed conflict during the years leading up to first oil income, I add an interaction term between oil and conflict legacy to the baseline model. The logistic analysis is conducted on the full set of countries. Several different specifications of the interaction are employed. First; I interact the "ordinary" oil measure *log oil income per capita* with *pre-oil conflict* legacy.[105] As many countries started to earn oil income before 1946, I am not able to determine whether these "early" oil countries had a conflict legacy prior to oil or not. In the main analysis, I treat these countries as if they had no conflict legacy prior to oil. Yet, upon closer inspection, results are clearly sensitive to how these countries are handled, and I supplement graphed results from the main analysis with graphs showing how this conditional relationship is affected when countries with an unknown conflict legacy are treated as a separate category. The results from the logistic analysis of the full set of countries are complemented with a bivariate analysis conducted on the subset of countries with oil income.

The hypothesis has also been tested by a different specification, namely whether the effect of *nationalized oil income* may be conditional on *pre-nationalization conflict* legacy. As results

[105] That is, a dummy variable indicating whether or not the country had an incidence of armed conflict during the ten years leading up to oil first income.

are largely similar for both specifications, results from this test will not be presented in the main text, but is available in Appendix D.[106]

Results do *not* support the hypothesis that the conflict-inducing effect of oil income is conditioned by countries' conflict legacy. When countries with an unknown conflict legacy are treated as a separate category, having or not having a conflict legacy is of negligible difference for the conflict-inducing effect of oil.

Conflict legacy prior to oil is nevertheless of significant importance for countries' propensity to conflict. The bivariate analysis complement this story: Among countries with oil income or nationalized oil income, a having or not having a legacy of armed conflict significantly differentiates the likelihood of armed conflict onset among these countries.

Results from the logistic analysis are presented in Table 5.3 on the next page. Model 13 is the baseline model, identical to model 4 in previous tables, only excluding 10 years prior to first oil income as they are used for assessing conflict legacy. When controlling for *pre-oil conflict* singly in model 14, the coefficient for oil remains largely unaltered in size and significance. The coefficient of 1.23 indicates that conflict legacy increases odds of onset, but the finding is not significant. When inserting an interaction term in model 15, both single coefficients for oil income and pre-oil conflict are positive and significant, but the interaction coefficient is negative and insignificant. For this coding of conflict legacy, the single coefficient of oil income indicates that when conflict legacy is zero, an increase in oil income significantly increases odds of onset. The single, significant coefficient of pre-oil conflict indicates that when oil income is zero, odds of onset for countries *with* conflict legacy are 1.53 times higher than for countries with no conflict legacy.[107] The interaction coefficient of 0.92 surprisingly indicates that for countries *with* pre-oil conflict, increases in oil income relate to lower odds of onset, albeit not significantly (p-value 0.24).

A problem with this puzzling finding that is that among countries with pre-oil conflict coded as zero, a large share of them have an unknown conflict legacy (33 of the 85 countries coded as having no pre-oil conflict actually have an unknown legacy, see Table C.1 Appendix C).

[106] Results from the analysis of nationalized oil income and conflict legacy actually fall slightly more in line with expectations of the hypothesis, but short of significance, they are inconsequential for the final interpretation of findings.

[107] The combination of zero oil income and pre-oil conflict of 1 is rare, but does occur in the data.

Table 5.3: Interacting Oil Income and Conflict Legacy.
Logistic Regression of Armed Conflict Onset 1961-2007.

	Model (13) *All Countries*	*Model (14)* *All Countries*	*Model (15)* *All Countries*
Log Oil Income/Pop $_{t-1}$	1.091**	1.086**	1.101**
	(0.015)	(0.023)	(0.011)
Pre-Oil Conflict Legacy		1.225	1.533*
		(0.374)	(0.087)
Pre-Oil Conflict Legacy* Oil Income/Pop $_{t-1}$			0.921
			(0.242)
Log GDP/Cap $_{t-1}$	0.591***	0.600***	0.592***
	(0.000)	(0.000)	(0.000)
Log population	1.313***	1.319***	1.313***
	(0.000)	(0.000)	(0.000)
Log Rough Terrain	1.082	1.083	1.087
	(0.116)	(0.120)	(0.106)
Regime Transition $_{t-1}$ (0/1)	1.125	1.093	1.061
	(0.703)	(0.778)	(0.852)
Autocratic Regime $_{t-1}$ (0/1)	0.827	0.822	0.819
	(0.394)	(0.385)	(0.372)
Democratic Regime $_{t-1}$ (0/1)	1.078	1.067	1.064
	(0.746)	(0.781)	(0.790)
Mideast and N Africa	1.437	1.409	1.440
	(0.177)	(0.222)	(0.185)
Neighborhood Conflict $_{t-1}$	1.466**	1.460**	1.436**
	(0.021)	(0.024)	(0.030)
Proximity of Conflict	2.713***	2.691***	2.671***
Observations	6456	6456	6456
Log Likelihood	-856.519	-856.103	-855.567
AIC	1735.037	1736.206	1737.133
Countries	170	170	170
Onsets	215	215	215

Exponentiated coefficients; *p*-values in ( ); Inconsistent Regime$_{t-1}$ as reference category for regime dummies; SE clustered on country * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In model 13-15 ten years prior to year of first oil income are censored as they are endogenous to pre-oil conflict legacy.

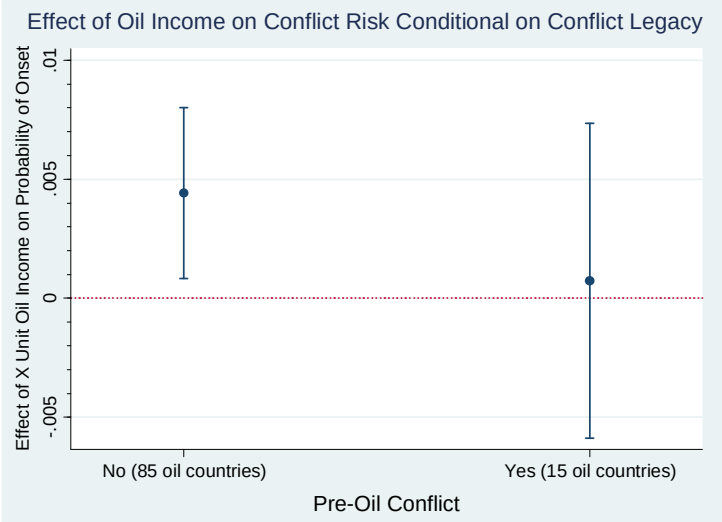
To see what happens if these countries are separated from the group of countries with no conflict legacy, I therefore rerun model 15, but treat countries with an unknown conflict legacy as a separate category of conflict legacy in the interaction term.[108]

When comparing plotted results, it is clear that the observed higher effect of oil on conflict risk among countries with no conflict legacy (Figure 5.6) is driven up by an even higher (albeit not significant) oil effect among the countries with an *unknown* conflict legacy (Figure

[108] Tabular results from model 15b are available in Table B.2 in Appendix B.

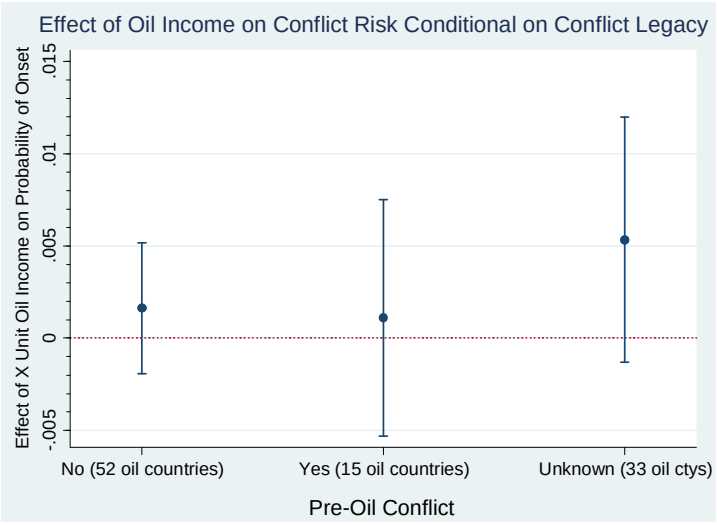
5.7). Among countries with a *measured* pre-oil conflict legacy, the effect of oil income on conflict risk is of nearly identical and rather low size, and both insignificant.

Figure 5.6: Model 15. Conditional Effects of Oil Given Conflict Legacy (0, 1)



Both graphs show the conditional effect of oil income for different categories of conflict legacy, with 90% confidence intervals. Other variables are set to mean, median or most frequent values of oil income countries in 1961. Figure 5.7 is based on the same model as Figure 5.6, except that countries with an unknown conflict legacy are interacted with oil income as a separate category instead of being included in the category of countries with no conflict legacy.

Figure 5.7: Model 15b. Conditional Effect of Oil Given Conflict Legacy (0, 1 or Unknown)



Even if none of these findings support the hypothesis that countries' conflict legacy mediates the effect of *oil income* on conflict risk; conflict legacy prior to oil income does seem to have an independent effect on conflict risk, as indicated by the large and significant coefficient of

the single term of conflict legacy in model 15.[109] Results from a bivariate analysis of the relationship between conflict legacy and armed conflict onset conducted on a subsample of oil countries underscores that differences in oil countries' exposure to armed conflict is significantly determined by their legacy of armed conflict prior to oil. Table 5.4 below shows the frequency of oil countries with at least one conflict onset and no onset, ordered according to whether they had conflict legacy prior to oil.

Table 5.4: Bivariate Analysis: Conflict Legacy and Conflict Onset Among Countries With First Oil Income After 1946

		No. Countries with **Onset** of Armed Conflict Following Oil Income		*Total:*
		At least 1 onset	No onset	
No. Countries with Conflict Legacy Prior to First Oil Income	Yes	11 (73.3%)	4 (26.7%)	15 (100%)
	No	20 (38.5%)	32 (61.5%)	52 (100%)
Total:		31 (46.3%)	36 (53.7%)	67 (100%)

Pearson chi2(1) = 5.695 Pr = 0.017

The table shows that among 15 countries with conflict legacy prior to first oil income, 11 (or 73.3%) of them experienced at least one new onset following oil. Only 4 countries with a conflict legacy escaped new onset following oil, but it does not mean that they escaped conflict altogether: Sudan, Guatemala and Papua New Guinea are among these 4 and had continuous ongoing conflicts both prior and following first oil income. Among 52 countries without a conflict legacy prior to first oil income, 20 of them (or 38.5%) experienced at least one onset. In other words, oil countries *with* a conflict legacy were nearly 35 percentage points more likely to experience new conflict following oil than countries without a conflict legacy.[110] The probability of observing this difference if there was no relationship between conflict legacy and conflict onset among oil countries would be very low (Pr= 0.017).[111]

[109] This also becomes apparent when predicted probabilities are calculated in section 5.2 ahead.

[110] This corresponds to a relative difference of about 190%.

[111] A list identifying countries within each category is provided in Table C.3 in Appendix C. An alternative table showing the relationship between conflict legacy and *incidence* of armed conflict as dependent variable is also provided here (Table C.4).

To sum up, the bivariate table demonstrates that countries' conflict legacy *prior to* oil income significantly differentiates oil countries likelihood of experiencing armed conflict following oil income. Yet, the findings from the logistic regression analysis conducted on the full sample of countries do not provide support of the hypothesis that countries' conflict legacy mediates the effect of oil on conflict risk among countries at large.

5.2 Scenarios: Petroleum and Conflict Risk in Tanzania and Uganda

The findings presented above indicated that both countries' initial bureaucratic control and initial education attainment may have a distinguishing impact on the effect of oil on their risk of armed conflict onset in subsequent years. Also, conflict legacy proved to importantly affect oil countries' onset risk, albeit in a direct manner rather than by mediating the effect of oil income. Yet, results presented so far say little about *how much* having robust or poor institutions or conflict legacy at the onset of oil income matters for real-world countries' conflict propensity as their oil revenue grows.

Tanzania and Uganda are two African countries that are now in the initial phases of petroleum exploration and production. How much more dangerous would a certain amount of oil income be, if these countries have fragile initial institutions, compared to if they had robust initial institutions? How important may their divergent conflict legacy be for their risk of new onsets following oil? This section attempts to make findings obtained in the analysis more appreciable by providing a visual approximation of how much countries' institutional legacy and conflict legacy may distinguish the effect of oil income on predicted probabilities of onset.

When calculating predicted probabilities, the values assigned to control variables also greatly matter for the resulting estimate. I control for the impact of other variables by setting them at values representative of the two countries mentioned above, Tanzania and Uganda in 2007.[112] These countries are of particular interest for two main reasons: First, they represent two soon-to-be petroleum producing countries in East Africa, a region which has been highlighted to harbor most promising oil and gas discoveries in the world in present years (Deloitte, 2013, p.

[112] This may offer more substance to estimates than fixing control variables to their mean values, which may not be representative of any real-world country.

2). Second, the two countries display some variation over critical characteristics such as income level, stability and size. Simply put, Tanzania is a large, poor, populous, relatively peaceful country (which is also mountainous), while Uganda is a smaller, somewhat less poor, conflict-ravaged neighbor with less mountainous terrain. In combination, the two countries offer the possibility to visualize the implications of different institutional and societal starting points for the relationship between size of oil income and probability of onset across countries with these characteristics.

Graphed predictions using representative values for Tanzania 2007 on control variables are presented and discussed in the main text. Since estimated probabilities are very similar also when shifting control variables to values representative of Uganda in 2007, these graphs will not be further discussed in the text, but they are included for inspection in Appendix E.

All graphs present the predicted probability of internal armed conflict onset on the y-axis for a representative range of size of oil income per capita measured in dollars (from 1st to approximately 99th percentile of observations of country years with oil income above zero) on the x-axis. The percentiles of observed oil income per capita are indicated by a p on the x-axis. Estimates for the conditional relationship between oil income and probability of onset are provided for the 10th and the 90th percentile of observations on initial institutions (bureaucratic control and education attainment), and for conflict legacy of 0, 1 and unknown. All estimated probabilities are reported with 90% level confidence intervals.[113]

5.2.1 The Mediating Role of Initial Bureaucratic Control

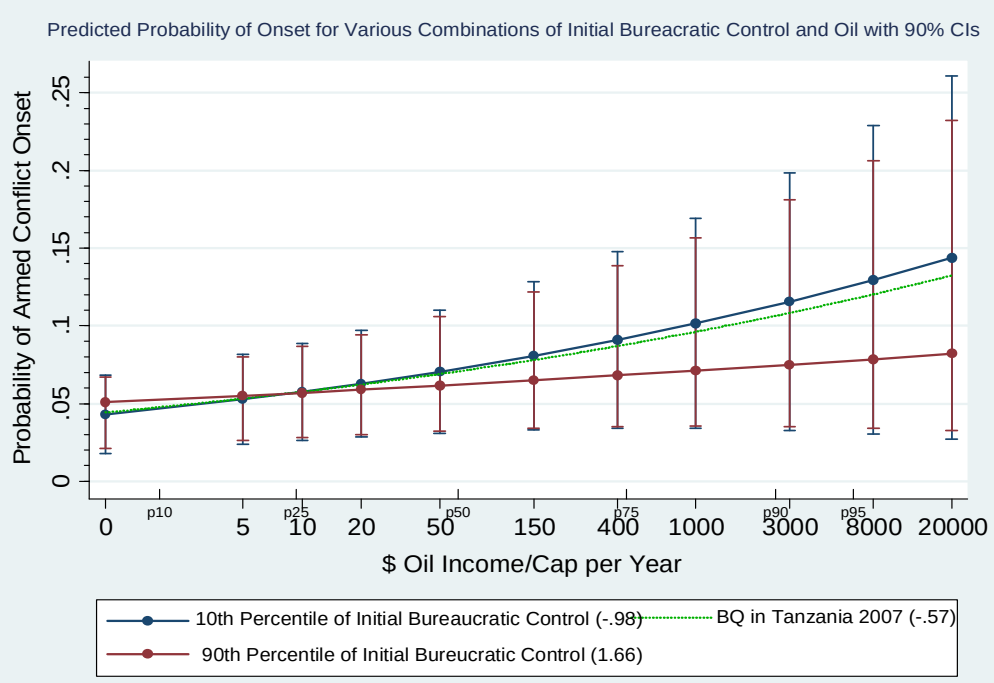
Figure 5.8 below graphs the predicted probabilities of onset for increasing oil income per capita conditional on fragile (10th percentile) or robust (90th percentile) initial bureaucratic control. The estimate is based on results from model 3, fixing control variables on values representative of Tanzania in 2007. The blue line shows the predicted probability of onset for increasing values of oil income per capita, when initial bureaucratic control is set to represent the lowest 10th percentile of observations on this variable. The red line expresses the predictions of onset for increasing values of oil when initial bureaucratic control is at the 90th percentile of observations on this variable. The green line projects how growing oil income

[113] All estimates of marginal effects and predictive margins have been conducted using Stata 13 Margins commands.

would affect probability of onset given bureaucratic control at the level observed in Tanzania in 2007.[114]

As indicated by the difference in steepness of the red and blue curves, higher levels of oil income in countries with poorest initial bureaucratic control (blue line) more dramatically increases conflict risk than in it would in countries with robust initial bureaucratic control (red line). The slight incline of the red line indicates that even for a high level of initial bureaucratic control, higher oil income affiliate with higher probability of onset, but recalling results from Figure 5.2, this effect was not close to significance.[115]

Figure 5.8: Tanzania. Predicted Probability of Armed Conflict for Increasing Oil Income at the 10th and 90th Percentile of Initial Bureaucratic Control.



Predicted probabilities of onset with 90% confidence intervals. Other variables set to representative values for Tanzania in 2007. Estimates based on model 3 (oil countries).

[114] A more precise estimate of the impact of growing oil income on conflict risk given values representative of Tanzania would probably be less dramatic than what this graph indicates, as growing oil income would increase GDP per capita and thereby reduce estimated conflict risk. This inflexibility is admittedly a drawback of the estimate. At the same time, a country with a large population like Tanzania may be less likely reach very high levels of oil income per capita, so that the differentiating impact of initial institutions may never become as great as indicated towards the right side of the graph.

[115] The effect of oil on conflict risk at the 10th percentile of initial bureaucratic control also barely achieved significance at the 90% level, while at the 25th and 50th percentile there was a more clear and significant incremental effect of oil on conflict risk.

For a country with the characteristics of Tanzania in 2007 and initial bureaucratic control at the level of the frailest 10th percentile, an increase from zero oil income to 400 $ per capita annually would increase the predicted annual probability of onset by about 5 percentage points from approximately 4% to 9%. If initial bureaucratic control were at the robust 90th percentile, prognoses would be better: the same increase in oil income would increase the predicted probability of onset by 2 percentage points (from 5% to 7%). For higher levels of oil income per capita, the difference becomes greater.[116] In any event, large and overlapping confidence intervals of the predicated probabilities of onset mean that these plots may only describe a somewhat uncertain trend.[117]

5.2.2 The Distinguishing Impact of Initial Education

Figure 5.9 on the next page shows predicted probabilities of onset for increasing levels of oil income conditional on initial education level at the 10th and 90th percentile of all observations among oil countries, with other variables held at Tanzania-2007 values.

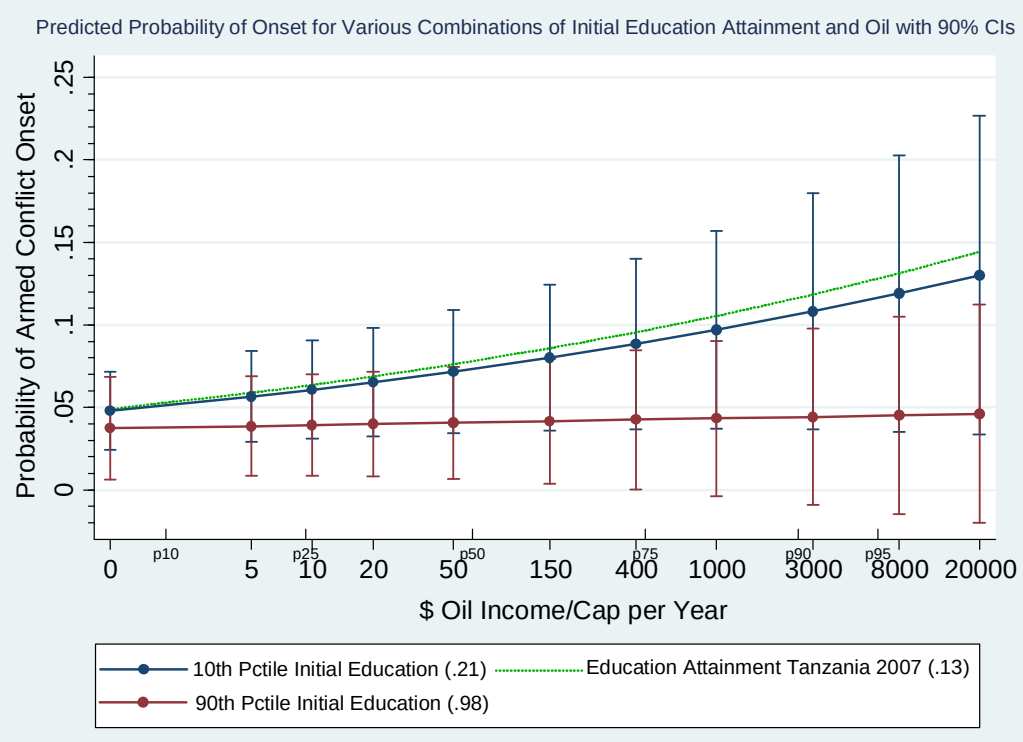
The distinguishing impact of initial education appears unambiguous. At the 90th percentile of initial education (red line), increasing oil income has virtually no impact on the predicted probability of armed conflict onset. The difference between minimum and maximum of oil income at this level relates to a less than 1 percentage point increase in predicted probability of onset; but from Figure 5.4 it may be recalled that this small effect is insignificant.

At the 10th percentile of initial education (blue line), higher levels of oil income gradually increases probability of onset. For a country with the characteristics of Tanzania in 2007 and initial education at the 10th percentile of all oil countries, an annual probability of onset of 5% for zero oil income is estimated to increase to about 7.5% for oil income of about 50$/cap annually, which is a bit less than the median oil income among oil countries. Recalling from Figure 5.4 the effect of oil on conflict risk on this level of initial education was significant. An oil income of about 400$/cap (a little less than the size of oil income at the 75th percentile) at

[116] But estimates also become more problematic at high levels of oil income due to the unlikely combination of a very high value of oil income and low GDP per capita, which is default in the baseline calculation.
[117] The only thing that can be said with 90% confidence is that higher oil income per capita in a country with poor initial bureaucratic control does increase conflict risk, while we can't be as certain that higher oil income in a country with robust initial institutions does the same.

this level of initial education further yields predicted risk of onset of about 9%.[118] The green line indicates that oil income in Tanzania might be even riskier, with an education attainment ratio of .13 in 2007 the country scores below the 10th percentile of initial education among oil countries at large.

Figure 5.9: Tanzania. Predicted Probability of Armed Conflict for Increasing Oil Income at the 10th and 90th Percentile of Initial Education Attainment.



Predicted probabilities of onset with 90% confidence intervals. Other variables set to representative values for Tanzania in 2007. Estimates based on model 9 (oil countries).

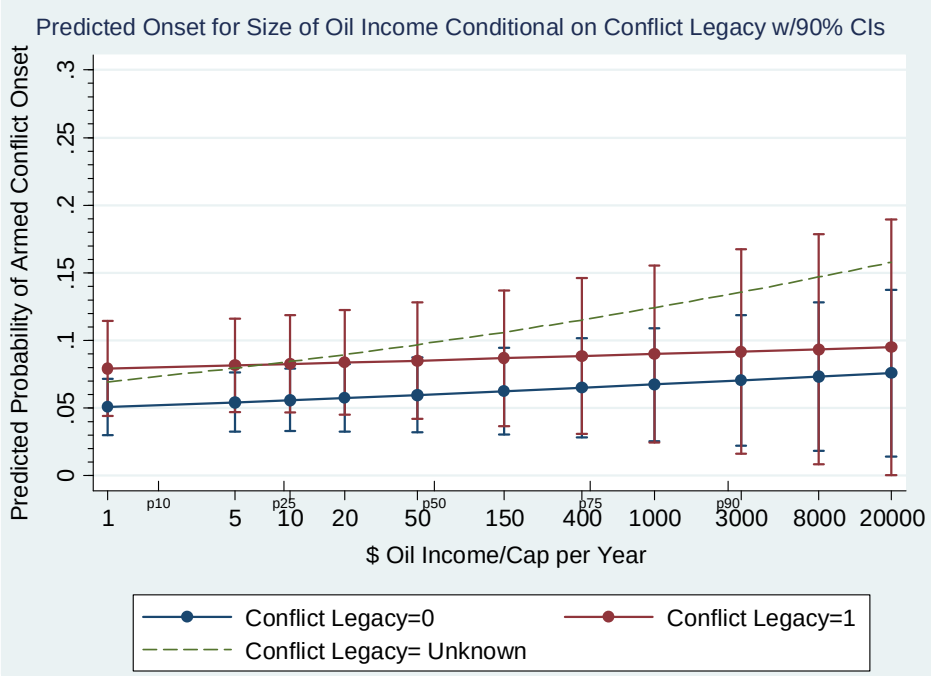
Again, large and partly overlapping confidence intervals of the predicted probabilities imply that estimates indicate a trend with a considerable degree of uncertainty. Yet, what we may state with confidence is that at the lower scale of initial education, increases in oil income does relate to increases in probability of onset, whereas among countries at the high end of initial education, increases in oil income may dubiously relate to a tiny increase in conflict risk.

[118] Again, predictions would most likely appear less dramatic if the model also took into account that increases in oil income would also yield increases in GDP per capita, which would counteract the size of the effect of increasing oil on predictions according to my model.

5.2.3 The Added Risk of Conflict Legacy

Figure 5.10 visualizes the predicted probability of armed conflict onset for increasing levels of oil income per capita, dependent on conflict legacy prior to oil income. Countries with an unknown conflict legacy are treated as a separate category, as they were found to drive the results for countries with no conflict legacy when included in this category (see Figure 5.7).

Figure 5.10: Tanzania. Predicted Probability of Armed Conflict for Increasing Oil Income, With or Without Conflict Legacy



Predicted probabilities of onset with 90% confidence intervals. Other variables set to representative values for Tanzania in 2007. Estimates based on model 15b in Appendix B.

The graph shows that conflict legacy is a condition of distinguishing importance for later probability of onset across the entire range of oil income. Restricting attention to the estimates based on observations with a known conflict legacy prior to oil, predicted probability of onset is higher if the country *has* a conflict legacy (red line) than if it has *no* conflict legacy prior to oil (blue line), for any value of oil income.

At the minimum value of oil income per capita, a country like Tanzania in 2007 would be estimated to have a 5% probability of armed conflict onset if it had no legacy of armed conflict prior to oil; whereas the probability would be estimated to 8% at the same level of oil income if the country had a pre-oil conflict legacy. Increasing the size of oil income slightly adds to the probability of onset both if the country has a conflict legacy prior to oil or not, but

recalling Figure 5.7, none of the coefficients of oil income were significant for these categories.[119]

In sum, the graph illustrates that conflict legacy may not have a distinguishing impact on the effect of increases in oil income on probability of onset. Yet, conflict legacy does appear to be a differentiating condition for oil countries' propensity to new onsets: For any value of oil income, having a pre-oil conflict legacy puts the country at higher risk of new onset than if it had no conflict legacy.

5.3 Models' Performance and Robustness

5.3.1 Predictive Power and Goodness of Fit

The hypotheses presented in this thesis all seek to enhance explanations of the relationship between oil income and armed conflict. Therefore, it is also worth to consider whether adding variables such as initial bureaucratic control, initial education and conflict legacy adds to the predictive power of the models. I will concentrate the evaluation of models' predictive power and fit to the main models of interest, namely those with interaction terms as they are used for the test of hypotheses. The predictive power of the models *with* interaction terms will be compared to the predictive power of the same models *without* the interaction terms, when these are regressed on an identical sample. I evaluate predictive power by addressing the Akaike Information Criterion (AIC), and assess goodness of fit by inspecting the Receiver Operating Characteristic (ROC) curves of the models.

The AIC indicates the relative quality of a model for a given set of data. Understood as a measure of the distance between the fitted likelihood function of a model and the unknown true likelihood function of the data, a lower AIC is better as it indicates that the model is "closer to the truth" (Kennedy, 2003, p. 101). AIC takes into account the trade-off between goodness of fit and models' complexity, meaning more complex models of equal fit will be punished with a higher AIC.

[119] It appears to be most interesting if country has an unknown conflict legacy (i.e. belong to the category of "early" oil countries that started to earn oil income prior to 1946). The green line indicates that if the country belongs to this category the probability of armed conflict onset increases nearly linearly for increasing values of oil income. It seems that there may be countries in this category that mainly drive the results for the relationship between oil and conflict onset, irrespective of the condition under scrutiny here.

Comparing the AIC of models with interaction terms to the same models without interaction terms reveals that adding interaction terms does little to aid models' performance. When initial bureaucratic quality is interacted with oil, this only serves to slightly increase the AIC compared to the same models without the interaction term (see AIC of model 3 versus 1, and of model 6 versus 4 in Table 5.1). The same is true for the model that interacts oil income with conflict legacy using the ordinary log oil income measure (see AIC of model 15 versus 13 in Table 5.3). The models interacting initial education with oil income achieve a nearly identical AIC as the same models without the interaction terms (see AIC of model 9 versus 7, and of model 12 versus 10 in Table 5.2). The general lack of improvement of AICs may be interpreted as a sign that the interaction terms generally add to the complexity of the models, without "compensating" this added complexity in terms of improving fit.

Comparing ROC-curves of these models supplements this impression. The ROC-plot visualizes the relationship between the fraction of true positives (i.e. the number of correctly predicted onsets divided by the total number of onsets) and the fraction of false positives (i.e. the number of incorrectly predicted onsets of armed conflict divided by the total number of non-onsets) at different thresholds.

The size of the area between the x-axis and the ROC-curve (the "area under the curve", or AUC) indicates the predictive power of the model, as it indicates the how well the model predicts observations compared with that of a random model (AUC of 0.5) and a perfectly predicting model (AUC of 1.0).

Figure 5.11 on the next page plots the ROC-curves of models testing the conditioning impact of initial bureaucratic control on the effect of oil income, against models with only oil income as an independent variable. Figure 5.12 repeats the same for models testing the conditioning impact of initial education on the effect of oil income, against models with only oil income as an independent variable. Figure 5.13 combines ROC-plots of the model testing the conditioning impact of conflict legacy on the effect of oil income against the simpler model without the interaction term.

AUCs for each model are in parentheses, and models on the same horizontal line in the legend are tested on the identical sample of countries and may thus be compared.

Figure 5.11: ROC-Plots Oil and Initial Bureaucratic Quality

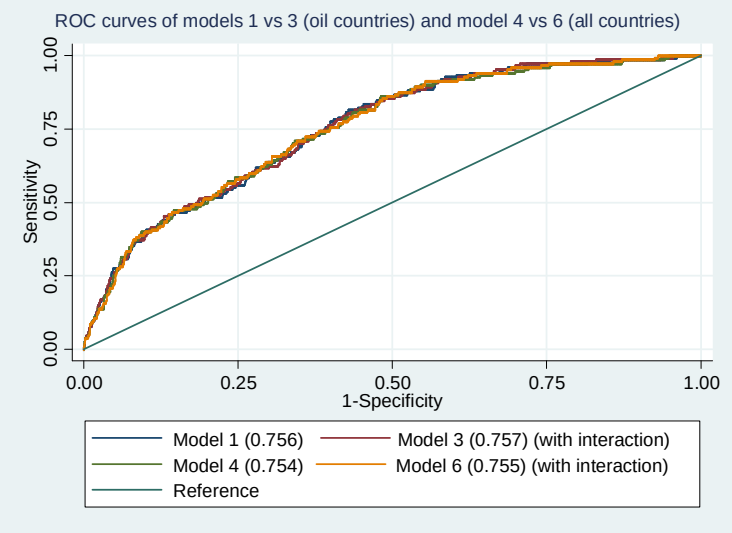
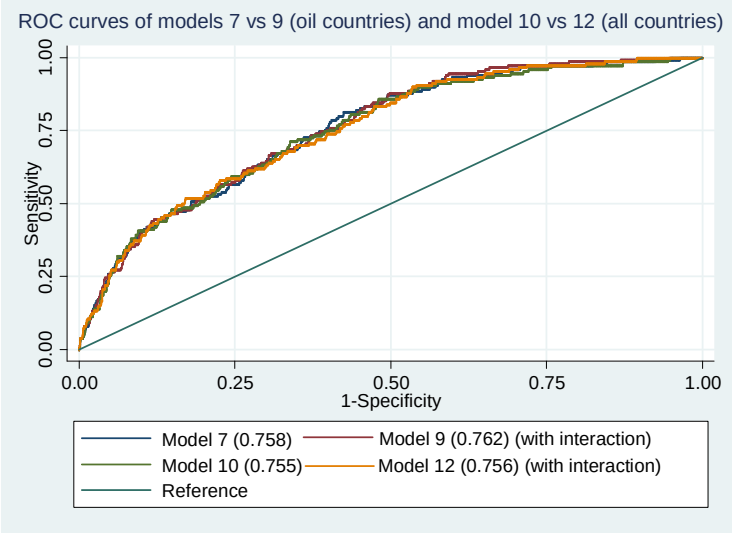


Figure 5.12: ROC-Plots Oil and Initial Education



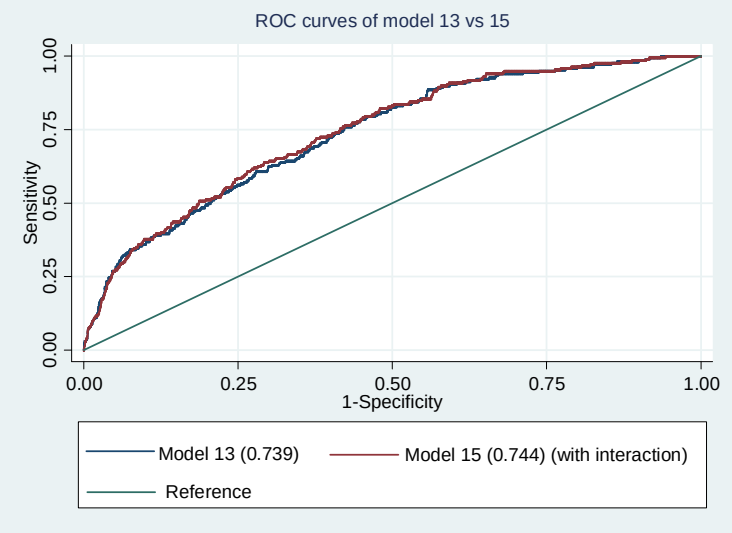


Figure 5.13: ROC-Plots Oil and Conflict Legacy

The figures show ROC-curves for models testing the conditional relationship of oil income on conflict risk versus the more simple models with only oil income as a single independent variable. Area under the curve for each model in parentheses. Models on the same horizontal line in legends are tested on the identical sample of countries and may thus be compared, in order to assess whether the more complex models with interaction terms are able to enhance the ability to predict observations correctly.

Comparing the ROC-curves, all of the models *with* interaction terms have a marginally larger area under the curve compared to the same models without interaction terms. This means that adding the interaction terms slightly adds to the ability to correctly predict onsets compared to models without the interaction terms. Yet, as indicated by the general lack of improvement of AICs, this slight improvement in predicting observations comes at the expense of a more complex model of explaining the same observations.

5.3.2 Robustness of Results

In order for the presented analysis to be valid, my models have to satisfy the assumptions underlying logistic regression. If these assumptions are not met, problems such as biased coefficient estimates, inflated standard errors of coefficient estimates may arise; which could lead to invalid statistical inferences regarding support or non-support of hypotheses. In the following, I will assess the potential problems of influential observations, multicollinearity, and omitted variable bias.

Influential Observations

Logistic regression estimates are highly sensitive to outliers and influential data points. Sometimes deviant observations have great impact on coefficient estimates, in the sense that excluding the observation from the regression would importantly change the parameter estimate (Pregibon, 1981, p. 713, Fig. 3).[120] In order to ascertain that obtained estimates are fit to represent the main share of observations, and not driven by a few influential deviant observations, it is necessary to inspect how results are affected by the removal of such influential observations.

In order to identify such influential observations I utilize the Pregibon (1981) $\Delta\hat{\beta}$ influence statistic. The statistic summarizes the change incurred by deleting the i^{th} observation in the model's fitted regression function (Long & Freese, 2006, p. 151). For each of the models employed to test H1, H2 and H3, I calculate observations' $\Delta\hat{\beta}$ influence statistic, and plot this

[120] Indeed, Pregibon (1981, p. 706) points out that an observation's residual from the fitted line is important for detecting poorly predicted/fitted observations, but observations with smaller residuals may exert more leverage on a model's fit than observations with the largest residuals.

against observations' id (see Figure F.1-F.3 in Appendix F). I then repeat the regressions, only excluding the most influential observations as identified in the graphs.[121]

In order to recognize how the removal of influential observations affects the estimates obtained in the original analysis, I graph the conditional effects of oil income across hypothesized mediating variables for each model. Graphed results from models run without influential outliers are placed alongside graphed results from the same models in the original analysis. The graphs are available for inspection in Appendix F. Following this procedure, I find the following:

- For models testing H1 (Figure F.5 and F.6), removal of the five most influential observations either has no impact (model 3) or make obtained results slightly more supportive (model 6) of the proposition that countries' initial bureaucratic control mediates the effect of oil income on conflict risk. In other words, results obtained from the analysis of this proposition are not driven by a few influential observations: Rather, we may be confident that they are representative of the main share of observations.[122]
- For models testing H2 (model 9 and 12), removal of the five most influential observations make results *more* supportive of the hypothesis (Figure F.7 and F.8): The size of the conditioning effect becomes substantially larger for both models. Moreover, the effect of oil conditional on initial education becomes significant at the 90% level when the 5 most influential country-years are removed from the full sample of 6695 country-years in model 12.[123]
- For the model 15b testing H3, removal of the five most influential observations imposes no notable change on estimates. Conflict legacy continues to be of no distinguishing impact for the effect of oil income. Only among countries with an unknown conflict legacy does the effect of oil income become significant (Figure F.4).

In sum, this analysis of influential outliers corroborates that the findings obtained in the original analysis are consistent, and not an outcome driven by a few deviant cases. Moreover, the effect of oil conditional on initial education becomes significant (p-value 0.058) upon

[121] There is no strict rule regarding how extreme an observation must be to justify exclusion: the only rule of thumb is that the distance between the observation and the main bulk of observations is large compared to others.
[122] I also tried lower thresholds for excluding outliers, which only served to change results in favor of the hypothesis by enlarging the size of the conditional effect of oil income, albeit not accompanied by significance.
[123] In analysis of oil countries (model 9), the effect of oil conditional on initial education becomes significant at the 90% level if the 19 most influential country-years are removed from the sample of 3910 country-years.

removal of the five most deviant observations from the full sample of oil and non-oil and countries (model 12). This means that support for H2 is further reinforced when the assumption of no large outliers is addressed. For the other hypotheses, interpretation of results remains unchanged.

Multicollinearity

A potential threat to the precision of estimates is if there is high correlation among some of the independent- and control variables, that is, if there is imperfect multicollinearity present among two or more regressors in a model (Stock & Watson, 2012, p. 241).[124] The consequence of this in my analysis would be that the coefficient for at least one of the regressors affected by multicollinearity would by imprecisely estimated. With higher degree of multicollinearity, coefficient estimates become unstable, and standard errors as well as confidence intervals expand. The risk of accepting a false null hypothesis (type-II error) increases.

In order to assess whether multicollinearity poses a problem to the obtained estimates, I use the *variance inflation factor* (VIF) command after estimating each model. The VIF-value of a variable indicates how much the variance of an estimated regression coefficient is increased due to collinearity. A rule of thumb is that variables with VIF-values above 5 are somewhat problematic, while VIF-values above 10 are unacceptably high.

Following this procedure, I find that multicollinearity does not seem to be a critical problem for the robustness of obtained results. VIF-values of explanatory- and control variables in models testing H1 and H2 are generally low (Table F.1 and F.3 Appendix F), while the interaction term in models testing H3 introduce some degree of multicollinearity between the explanatory variables (values ranging between 3.1-7.6 for key independent variables, see Table F.2). This is not unexpected since constitutive terms will be correlated with interactions, and it means that standard errors of these variables are higher than if these were not correlated. As they all remain below the critical VIF-value of 10, I interpret this such that multicollinearity does not threaten the validity of these findings.[125]

[124] This is contrast perfect multicollinearity, which occurs if a regressor is a perfect linear function of other regressors.

[125] This assessment also seems reasonable given that these models have yielded very similar results across different samples, and across exclusion and inclusion of influential outliers.

Omitted Variable Bias

The selection of control variables was guided by the principle of including variables that, if excluded, would represent probable sources of omitted variable bias. Yet, influence from omitted variables is difficult to guard completely against, and in particular, unobserved differences at the country-level could produce biased estimates and invalid inferences. A procedure for correcting for this would be to employ country-fixed effects, which entails giving each country an own intercept to control for country-specific effects.

There is however certain issues that preclude employing fixed effects in my analysis. In a logit model, fixed effects would throw out all countries that were in peace during the whole period (Beck & Katz, 2001).[126] Given my research objective, which is to sort out why some oil countries remain in peace, while others become marred in conflict, it is unsound to discard information from oil countries that have remained peaceful throughout the period.

Applying OLS with fixed effects as a linear probability model could have been a solution as it does not throw out countries that remained in peace from the estimate. Yet, on testing this, another problem was revealed: Since several of the constituent variables in interaction terms are time-invariant (initial bureaucratic control, initial education, conflict legacy), this prevents estimation of these interaction terms. This is likely because coefficients of interaction terms indicate the effect on conflict risk when both constituent terms increase, which does not occur when estimated only within units.

For these reasons, I am not able to control for omitted country-specific factors that are determinants of conflict onset at the same time as they correlate with other independent variables. This can't be ruled out as a source of uncertainty in estimates.

5.4 Summary of Findings

This chapter has empirically evaluated three hypotheses formulated in the theory chapter on conditioning factors in the relationship between oil income and armed conflict risk. Each of the three hypothesized conditional relationships has been inspected among oil countries in particular, and among oil and non-oil countries in general, within the period 1961-2007. Table 5.5 ahead summarizes the main findings of the empirical investigation of the hypotheses.

[126] This is because fixed effects models estimates effects on changes within units; and if the country remained in peace there would be no change in the dependent variable to estimate such effects from.

Table 5.5: Summary of Results From the Empirical Tests of Hypotheses

Hypothesis	Empirical Support?
H1: *The conflict-inducing effect of oil income will be higher in countries with low bureaucratic control than in countries with high bureaucratic control at the commencement of oil income*	Partially supported. At lower values of initial bureaucratic control, the effect of oil on conflict risk is higher, and the effect declines for higher values of initial bureaucratic control. Overall, the interaction coefficient for oil income and initial bureaucratic control does not achieve significance. Yet; within certain ranges of initial bureaucratic control there is a difference in the significance of the effect of oil on conflict risk between lower and higher values of initial bureaucratic control. The finding is unaltered by removal of influential outliers.
H2: *The conflict-inducing effect of oil income may be lower in countries with high quality of public service provision than in countries with low quality of public service provision at the commencement of oil income*	Partial to full support. For the indicator of public service provision employed here; education, the analysis shows that for higher initial education ratios, oil income does not significantly increase the risk of onset. At lower initial education ratios; the effect of oil income on conflict risk is relatively higher and significant. The interaction coefficient for oil income and initial bureaucratic control does not achieve significance in the primary analysis, but when 5 observations with undue influence on estimates are excluded from the full sample of 6695 country-years, the conditional effect becomes significant at the 90% level.
H3: *Oil income increases conflict risk more severely in countries with a recent legacy of armed conflict than in countries without such a conflict legacy at the commencement of oil income*	Not supported. When countries with an unknown conflict legacy prior to first oil income are treated as a separate category, having or not having a conflict legacy is inconsequential for the effect of oil income. Yet, conflict legacy is in itself an important distinguishing condition for oil countries' risk of armed conflict onset. For any size of oil income, countries with a conflict legacy prior to oil are at a higher risk of onset than countries with no conflict legacy. This finding is robust when outliers are removed, and the same trend applies when studying the relationship between nationalized oil income, pre-nationalization conflict legacy and conflict onset.

5.4.1 Discussion

As the table above has synthesized the main findings of the empirical analysis, I will not repeat them in detail here. Rather, I will in the following address a few issues of relevance for the further interpretation of these findings.

First, the results obtained in this analysis only partially supported the hypothesis that initial bureaucratic control conditions the effect of oil on conflict risk. Are we from this right to infer that initial bureaucratic control is only moderately important for the effect of oil on conflict risk? A justified complaint which may be raised against this interpretation is that the operational measure of bureaucratic control applied in this analysis only reflects one side of the concept it is meant to gauge.

As discussed in the operationalization chapter, the operational indicator specifically captures the quality and resilience aspect of bureaucratic control. There is no way to ascertain that observations of this variable in country-years also reflect the *geographic dispersion* of such institutions. The geographic outreach and extension of state control into rural areas is an important aspect of the concept of bureaucratic control used by Fearon and Laitin (2003), as well as in conceptualizations of the sources of states' coercive strength (Fjelde & de Soysa, 2009). The potential role of this aspect of bureaucratic control may be underestimated in this analysis because of the chosen indicator insufficiently incorporates this feature. The suggestive findings attained in this analysis warrant further exploration of the conditioning role of bureaucratic control, using indicators more precisely targeting the geographic extension of state institutions into rural areas.

Throughout the analysis, initial bureaucratic control and initial education attainment are used as if to indicate diverse aspects of institutional capacity. Results indicated that education attainment somewhat more confidently conditioned the effect of oil income on conflict risk. This difference in effect does however not warrant the inference that public service provision (or accommodative/co-optative capacity) more importantly conditions the effect of oil income than bureaucratic control (or coercive/regulatory capacity). As was discussed in the research design chapter, the operationally defined indicators measure overlapping phenomena, since the bureaucratic quality index also incorporate quality of public service provision. The correct interpretation must therefore be that the variables *initial bureaucratic control* and *initial education attainment* represent alternative indicators of the larger theoretical concept

institutional legacy. The partial to complete support provided by these two alternative proxies of institutional legacy may be taken as encouraging support of the notion that countries' institutional legacy is of importance for the subsequent effect of oil on conflict risk.

The findings obtained in the analysis of H3 suggest that the proposition that conflict legacy may be distinguishing for the effect of oil must be rejected. At the same time, pre-oil conflict legacy was found to have a notable and significant individual effect on conflict risk

There are two main drawbacks of the data and design applied to test H3, which limit the ability to draw definite inferences on the basis of the findings. A first notable challenge is that year of first oil income for a considerable share of oil countries are prior to the period during which conflict data is available. As the conflict legacy of these countries remains unsettled, this means that the proposition is not fully tested.

Secondly, the variables pre-oil- (and pre-nationalization) conflict legacy are operationalized such that they by definition incorporate the presence of oil income (or nationalized oil income). This means that there could be omitted variable bias from oil in the estimated separate effect of conflict legacy. Even if size of oil income is controlled for in the log oil income measure, the mere promise of oil in a country may also trigger violent struggle. As we have no observations of pre-oil conflict or pre-nationalization conflict equal to one for countries that have never had oil, there is a difficulty of separating the effect of "only" conflict legacy from the potential added effect of oil. In other words, inferring that the individual effects of pre-oil and pre-nationalization conflict legacy are completely independent of the presence of oil may not be correct.

Finally, the finding that *nationalized oil income* does not significantly increase conflict risk is in itself worth a few thoughts. This non-finding could be regarded surprising in light of state-centered explanations of the oil-conflict relationship which build on the premise that oil rents seized by governments is the source of the trouble. If this was true, the measure gauging oil rents available to governments following nationalization should be expected to be more conflict-inducing than the ordinary oil income measure which does not distinguish between rents captured by foreign firms or by governments. In contrast, the finding may pretty well support that the mechanisms suggested by state-centered theories are not inevitable: bureaucratic decay, detached governments and increased conflict risk are not necessary consequences of large oil income into government coffers.

6 Conclusion

This thesis has explored conditioning circumstances in the relationship between petroleum resources and internal armed conflict. While the conflict-proneness of oil countries has received considerable scholarly attention in recent years, the question of what mechanisms mainly underlies and generates this occurrence remains disputed. Previous contributions have emphasized *opposite* paths: state-centered and rebel-centered causal routes. Moreover, none of the explanations resolve the puzzle of why the curse of armed conflict befalls some oil countries, while others emerge unharmed.

Attempting to fill in missing pieces of the causal narrative, I suggested the perspective that differences in oil countries' initial starting points are worth considering. The precise objective of this thesis has been to explore countries' *institutional legacy* and *conflict legacy* as two potential conditioning factors of the effect of oil income on armed conflict risk.

In my line of argumentation, countries' institutional legacy is expected to condition whether the scenarios portrayed by state-centered and rebel-centered mechanisms are likely to play out. State-centered mechanisms in previous literature describe how oil income *inhibits* the origin and growth of state institutional capacity facilitative of societal peace, including bureaucratic control and reciprocity of state-society relations.[127] These causal arguments say nothing about what happens when such institutional capacities are already in place. I argue that the scenarios portrayed by state-centered mechanisms are plausible among countries with a fragile institutional starting point, but less plausible among countries with a robust institutional starting point. In countries where robust institutional capacity (in terms of bureaucratic control and reciprocity of state-society relations) has already been established, they are persevered by norms, practices, formal regulations, peoples' expectations, people employed, physical buildings, etc; which may not be expected to wither by the onset of oil rents. In short, I argue that the fragility or robustness of countries institutional legacy at the commencement of oil income is likely to anticipate the subsequent institutional trajectory, with differential implications for conflict risk.

[127] This is ostensibly because such institutional capacities originate from government's dependency on extracting taxes from the population. Availability of "easy" rents from petroleum alleviates governments' reliance on tax, and thereby precludes the development of institutional capacity that sustains the ability to tax.

Conflict legacy, on the other hand, was suggested to represent an unusual aid for rebel capacity of revolt following oil income. As oil-financed regimes control unique assets which may be used to repress or buy off potential opposition, the ability of challengers to organize rebellion against such regimes may be seen as a paradox. The proposition of this thesis was that in countries' with a recent history of armed conflict, organizational legacies and conflict-specific capital persists in the post-conflict phase which may be expected to ease new mobilization following oil income to the government.

Observable implications of these arguments were tested by quantitative analysis of data from a total of 170 oil and non-oil countries in the years 1961 to 2007.

Main Findings

For two operational indicators of institutional legacy, initial bureaucratic control and initial education attainment, findings indicated partial support of the argument that institutional legacy conditions the effect of oil income. Across both measures and several models, there was an unfailing trend that the effect of oil income on conflict risk was higher for poorer performance of initial institutions, while this effect was steadily reduced for higher performance of initial institutions. Under specific conditions, both initial bureaucratic control and initial education attainment significantly distinguished the effect of oil on conflict risk: within certain lower intervals of institutional indicators, the effect of oil income on conflict risk was relatively higher and separable from zero, while this effect declined and became insignificantly different from zero at higher intervals of initial institutions.

Robustness tests revealed that these findings were not driven by influential outliers or problematic due to high multicollinearity. Excluding a low number of influential outliers only served to strengthen the findings' support of this proposition.

With regard to the second suggested mediating factor, *conflict legacy*, obtained results did not support that this condition saliently differentiates the effect of increases in oil income on conflict risk. The estimated effect of oil income on conflict risk was largely equal among countries with and without a settled conflict legacy prior to oil.[128]

On the other hand, conflict legacy was found to have a significant *individual* impact, irrespective of size of oil income. As such, this finding does propose one answer to the

[128] The consistency of this finding was also corroborated by robustness tests.

problem of the differential experience of armed conflict among oil countries, as underscored by results obtained in the bivariate analysis. Yet, that having a conflict legacy put countries at risk of renewed conflict is an already established "truth" in peace research, which my finding only adds unsurprising evidence of. Most important for my argument, is that the condition does not resolve the puzzle of what makes oil income conflict-inducing among countries general.

Qualifications

Two caveats to the definiteness of the interpretation on the role of conflict legacy were noted. First, the conflict legacy of many countries which started to earn oil income prior to 1946 remains unsettled. If these countries' pre-oil conflict legacy was settled, this could possibly alter results. As found in my analysis, the effect of oil income on conflict risk was highest precisely within this group.[129] A second caveat is that there might be omitted variable bias from oil in the "conflict legacy" variable.[130] Some of its strong individual effect may be due to the inherency of oil in the measure. After all, the effect of oil on conflict risk may only be due to the actual size of oil income; the mere *promise* of oil has also been found to stir violent political struggle.[131] While there may be ways to solve this problem, for this thesis it will have to suffice to emphasize that this limitation in the design leaves some uncertainty in the inference that conflict legacy is of no distinguishing importance for the effect of oil on conflict risk.

Implications of This Study and Road Ahead

Taken together, the findings of this analysis suggest some interesting implications for the understanding of what factors condition the relationship between oil income and armed conflict. Most importantly, countries' institutional legacy should not be rejected as a distinguishing condition for the effect of oil on conflict risk. The finding that nationalized oil income does not significantly increase the risk of conflict further underscore that institutional decay and increased danger of conflict is not an unavoidable path in the wake of large rents to

[129] When a few outliers were removed, the effect of oil on conflict risk among these countries remained relatively higher and achieved significance.

[130] This is because it is operationalized as "incidence of armed conflict during ten years prior to first oil income", and therefore is contingent on oil income: For countries with no first year of oil income it is not measured. It is not certain that conflict legacy is equally conflict-inducing in the absence of oil.

[131] This effect has been seen for instance in Chad, where oil stirred political and violent conflict many years before extraction commenced (Gould & Winters, 2011; Humphreys, 2005).

government assets. The notion that differences in institutional starting points may give rise to divergent institutional trajectories, with differential implications for conflict risk, receives encouraging, albeit not ideal support from these findings.

Several further avenues may be worth exploring in relation to countries' institutional legacy as a conditioning circumstance. First of all, identifying and employing more precise indicators of bureaucratic control, which also takes in the *geographic dispersion* of state institutions into peripheral regions, is desirable in order to enhance the test of this argument. Secondly, identifying and employing alternative indicators of institutional capacity may be desirable. The concept institutional capacity comprises several partly overlapping aspects for which there may be only imperfect observational data available. If using different sorts of indicators of the same underlying concept yield results that point in the same direction, this could be one approach to ascertain its' role.

My argument on the distinguishing role of institutional legacy rests on the premise that divergent institutional starting points give rise to divergent institutional trajectories following oil.[132] While findings yield preliminary support of the overall argument, the assumption that fragile initial institutions will remain fragile in the wake of oil, while robust initial institutions will persist, remains an untested part of the argument. Further analysis is required for this aspect is to be ascertained. If true, this seems to give emerging African oil producers with fragile bureaucratic control and low performance in public service provision gloomy outlooks for the future. Yet, there is no definiteness that the obtained findings which are representative of country-years between 1961 and 2007 are generalizable as conflict forecasts for future oil income countries.

Awareness of the problems associated with large resource rents may be better than in earlier years, and steps are currently taken in the attempt to mitigate adverse effects in new oil nations. Efforts are made to prepare future oil economies' ability to manage oil revenues (Dovi, 2013) and increase transparency in the resource sector such as the Extractive Industries Transparency Initiative (EITI). Examining the interrelationships between such restorative interventions, quality of institutions, and ability to manage political pressures that arise in the wake of petroleum production remains a promising agenda for future research.

[132] In essence: In countries with fragile initial institutions, oil income will inhibit their improvement by alleviating the need to tax, and these countries will be more susceptible to armed civil conflict. In countries with robust initial institutions, oil income may not induce their decay, and no heightened risk of conflict is expected.

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Appendix A

Table A.1: List of Countries with Year of First Oil Income, Nationalization and Number of Onsets

Country	First oil income	First nationalization	Onsets During Years of Oil Income (1961-2007)	Onsets During Years of Nationalized Oil (1961-2007)
Albania	1945		0	0
Argentina	1945	1963	2	1
Austria	1945		0	0
Bolivia	1945	1969	1	0
Brazil	1945		0	0
Canada	1945		0	0
China	1945		0	0
Colombia	1945	1972	1	0
Cuba	1945		1	1
Ecuador	1945	1969	0	0
Egypt	1945	1961	1	1
France	1945		1	1
German Federal Republic	1945		0	0
Hungary	1945		0	0
India	1945	1975	14	12
Indonesia	1945	1960	9	9
Iran (Persia)	1945	1973	8	7
Iraq	1945	1961	8	7
Japan	1945		0	0
Malaysia	1945	1973	3	2
Mexico	1945		1	1
Myanmar (Burma)	1945	1962	13	12
Netherlands	1945		0	0
New Zealand	1945		0	0
Pakistan	1945	1974	6	3
Peru	1945	1968	3	1
Poland	1945		0	0
Rumania	1945		1	1
Russia (Soviet Union)	1945	2006	5	-
Saudi Arabia	1945	1972	1	1
United Kingdom	1945		2	2
United States of America	1945		1	1
Venezuela	1945	1971	3	2
Chile	1950		1	1
Italy/Sardinia	1952		0	0
Serbia (Yugoslavia)	1952		1	1
Bulgaria	1954		0	0
Israel	1956		3	3
Congo	1957	1974	3	3
Gabon	1957	1973	1	0
Nigeria	1958	1971	3	1
Taiwan	1960		0	0
Turkey (Ottoman Empire)	1960		3	3

Country	First oil income	First nationalization	Onsets During Years of Oil Income (1961-2007)	Onsets During Years of Nationalized Oil (1961-2007)
Kuwait	1961	1972	0	0
Libya	1961	1969	0	0
Algeria	1962	1962	1	1
Trinidad and Tobago	1962	1969	1	1
Thailand	1963		2	2
Oman	1964	1972	1	0
Barbados	1966		0	0
Spain	1966		3	3
Tunisia	1966		1	1
Afghanistan	1967		1	1
Syria	1968		1	1
Morocco	1969	1975	2	0
Belgium	1970		0	0
Congo, Dem. Rep. (Zaire)	1970		4	3
Rwanda	1970		1	1
Bahrain	1971	1974	0	0
Bangladesh	1971	1975	1	0
Norway	1971		0	0
Qatar	1971	1972	0	0
United Arab Emirates	1971	1971	0	0
Denmark	1972		0	0
Angola	1975	1976	5	4
Guatemala	1976		0	0
Cameroon	1977		1	1
Cote D'Ivoire	1978		0	0
Ghana	1978	1974	1	1
Ireland	1978		0	0
Sweden	1978		0	0
Philippines	1979	1973	1	1
Benin	1980		0	0
Greece	1981		0	0
Vietnam (Dem. Rep.)	1981		0	0
Brunei	1984		0	0
Switzerland	1985		0	0
Jordan	1986		0	0
Surinam	1986		1	1
Yemen (Arab Rep.)	1986	1969	1	1
Azerbaijan	1991		2	2
Belarus (Byelorussia)	1991		0	0
Croatia	1991		1	1
Kazakhstan	1991		0	0
Kyrgyz Republic	1991		0	0
Slovenia	1991		0	0
Tajikistan	1991		1	1
Turkmenistan	1991		0	0
Ukraine	1991		0	0
Uzbekistan	1991		2	2

Country	First oil income	First nationalization	Onsets During Years of Oil Income (1961-2007)	Onsets During Years of Nationalized Oil (1961-2007)
Georgia	1992		1	1
Lithuania	1992		0	0
Papua New Guinea	1992		0	0
Czech Republic	1993		0	0
Slovakia	1993		0	0
Sudan	1993	1976	0	0
Equatorial Guinea	1994		0	0
Cambodia (Kampuchea)	2000	1968	0	0
Chad	2003	2006	1	-

SOURCES: Year of first oil income is based on the Ross (2012) data, but for countries with oil income in 1960 year of first income is replaced with first recorded year of oil production according to *PETRODATA v.1.2.* Year of first nationalization is based on Guriev et.al. (2011). Onsets from UCDP/PRIO Armed Conflict Dataset v.4-2012.

Table A.2: Countries' Score on Initial Bureaucratic Control (BQ)

Non-Oil Countries (Measured 1960)		**Oil Countries**	
Country	*Initial BQ*	*Country*	*Initial BQ*
Dominican Republic	-1,88	Equatorial Guinea (1994)	-1,53
Djibouti	-1,35	Syria (1968)	-1,34
Guinea-Bissau	-0,99	Yemen, Arab Rep.(1986)	-1,30
Comoros	-0,91	Surinam (1986)	-1,07
Korea, Republic of	-0,91	Ghana (1978)	-1,07
Central African Republic	-0,87	Vietnam, Dem. Rep. (1981)	-1,04
Niger	-0,84	Sudan (1993)	-1,00
Jamaica	-0,79	Uzbekistan (1991)	-0,99
Laos	-0,71	Afghanistan (1967)	-0,98
Guinea	-0,69	Guatemala (1976)	-0,98
Armenia	-0,69	Rwanda (1970)	-0,97
Sri Lanka (Ceylon)	-0,69	Benin (1980)	-0,95
Macedonia (FYROM)	-0,69	Libya (1961)	-0,95
Bosnia-Herzegovina	-0,68	Chad (2003)	-0,91
Uganda	-0,66	Congo, Dem. Rep. (Zaire) (1970)	-0,91
Guyana	-0,62	Turkmenistan (1991)	-0,84
Mali	-0,62	Chile (1960)	-0,74
Mozambique	-0,62	Iran (1960)	-0,68
Somalia	-0,61	Egypt (1960)	-0,63
Haiti	-0,58	Philippines (1979)	-0,63
Moldova	-0,52	Tajikistan (1991)	-0,62
East Timor	-0,51	Mexico (1960)	-0,61
Korea, P. Rep.	-0,49	Nigeria (1960)	-0,55
Burundi	-0,49	Azerbaijan (1991)	-0,52
Malawi	-0,47	Belarus (1991)	-0,50
Eritrea	-0,44	Congo (1960)	-0,48
Senegal	-0,42	Cameroon (1977)	-0,47
Zimbabwe (Rhodesia)	-0,41	Cambodia (2000)	-0,44
Fiji	-0,37	China (1960)	-0,27
Nepal	-0,37	Rumania (1960)	-0,26
Sierra Leone	-0,37	Georgia (1992)	-0,22
Burkina Faso	-0,28	Gabon (1960)	-0,20
Kenya	-0,26	Tunisia (1966)	-0,19
Namibia	-0,23	Kyrgyz Republic (1991)	-0,18
Paraguay	-0,22	Qatar (1971)	-0,14
Mauritania	-0,22	Iraq (1960)	-0,13
Tanzania	-0,18	Lithuania (1992)	-0,12
Finland	-0,17	Cote D'Ivoire (1978)	-0,12
Lesotho	-0,13	Turkey (1960)	-0,11
Uruguay	-0,13		
Madagascar	-0,11		
Botswana	-0,07		
Lebanon	-0,05		
Zambia	-0,04		

Non-Oil Countries (Measured 1960)	
Country	*Initial BQ*
Liberia	-0,02
Montenegro	0,01
El Salvador	0,02
Maldives	0,03
Honduras	0,08
Mongolia	0,15
Costa Rica	0,15
Panama	0,20
Nicaragua	0,22
Gambia	0,25
Swaziland	0,31
South Africa	0,36
Togo	0,44
Estonia	0,50
Portugal	0,54
Bhutan	0,59
Belize	0,61
Singapore	0,61
Latvia	0,69
Czechoslovakia	0,77
Malta	0,97
Ethiopia	1,01
Cyprus	1,06
German Dem. Rep.	1,48
Mauritius	1,51
Solomon Islands	1,57
Iceland	1,63
Cape Verde	1,76
Luxembourg	1,98
Bahamas	2,08

Oil Countries	
Country	*Initial BQ*
Ecuador (1960)	-0,06
Bangladesh (1971)	-0,04
Oman (1964)	-0,02
Pakistan (1960)	0,01
Kazakhstan (1991)	0,02
Russia (Soviet Union) (1960)	0,02
Malaysia (1960)	0,03
Trinidad and Tobago (1962)	0,05
Peru (1960)	0,09
Bolivia (1960)	0,16
Kuwait (1961)	0,18
Ukraine (1991)	0,20
Serbia (Yugoslavia) (1960)	0,20
Algeria (1962)	0,23
Poland (1960)	0,23
Myanmar (Burma) (1960)	0,26
United Arab Emirates (1971)	0,26
Cuba (1960)	0,33
Croatia (1991)	0,35
Thailand (1963)	0,38
United Kingdom (1960)	0,41
Brazil (1960)	0,43
Colombia (1960)	0,46
Venezuela (1960)	0,52
New Zealand (1960)	0,56
Saudi Arabia (1960)	0,61
Morocco (1969)	0,62
Spain (1966)	0,62
Angola (1975)	0,63
Indonesia(1960)	0,64
Jordan (1986)	0,65
Taiwan (1960)	0,65
Bahrain (1971)	0,65
Barbados (1966)	0,70
Argentina (1960)	0,73
Albania (1960)	0,74
Slovakia (1993)	0,75
Czech Republic (1993)	0,77
India (1960)	0,82
Slovenia (1991)	0,85
Papua New Guinea (1992)	0,85

Oil Countries	*Initial BQ*
Country	1,22
Ireland (1978)	1,24
Denmark (1972)	1,25
Israel (1960)	1,27
German Fed. Rep. (1960)	1,30
Belgium (1970)	1,32
Sweden (1978)	1,35
Italy (1960)	1,35
Hungary (1960)	1,44
Austria (1960)	1,64
Bulgaria (1960)	1,66
France (1960)	1,69
Norway (1971)	1,74
Japan (1960)	1,81
Canada (1960)	1,90
Greece (1981)	1,93
Australia (1961)	1,97
Netherlands (1960)	2,01
United States of America (1960)	2,05
Switzerland (1985)	1,17
Brunei (1984)	

SOURCES: Bureaucratic Quality Index by Hegre and Nygård (20014), measured at first year of oil income, alternatively 1960. Year of first oil income is based on the Ross (2012) data, but for countries with oil income in 1960 year of first income is replaced with first recorded year of oil production according to *PETRODATA v.1.2.*

Appendix B

Table B.1: Baselinemodel. Logistic Regression of the Onset of Internal Armed Conflict Onset 1961-2007

	Model (A) *All Countries*	*Model (B)* *All Countries*	*Model (C)* *All Countries*	*Model (D)* *Oil Countries*	*Model (E)* *Oil Countries*	*Model (F)* *Oil Countries*
Log Oil Income/Pop$_{t-1}$		1.090**			1.108**	
		(0.013)			(0.010)	
Nationalized Oil Income/Pop $_{t-1}$			1.019			1.013
			(0.616)			(0.735)
Log GDP/Cap $_{t-1}$	0.682***	0.599***	0.666***	0.689***	0.585***	0.676***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Log population	1.358***	1.309***	1.348***	1.295***	1.271***	1.292***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.003)	(0.002)
Log Rough Terrain	1.071	1.089*	1.074	1.198**	1.240***	1.202**
	(0.157)	(0.091)	(0.138)	(0.032)	(0.010)	(0.027)
Inconsistent Regime$_{t-1}$	-	-	-	-	-	-
Regime Transition $_{t-1}$	1.147	1.099	1.134	1.150	1.046	1.132
	(0.628)	(0.744)	(0.657)	(0.737)	(0.918)	(0.767)
Autocratic Regime $_{t-1}$	0.840	0.783	0.826	0.867	0.810	0.857
	(0.430)	(0.264)	(0.382)	(0.632)	(0.470)	(0.598)
Democratic Regime $_{t-1}$	1.007	1.045	1.011	1.089	1.164	1.095
	(0.977)	(0.846)	(0.963)	(0.758)	(0.570)	(0.748)
Mideast and N Africa	1.713**	1.469	1.662*	1.507	1.322	1.483
	(0.040)	(0.128)	(0.053)	(0.155)	(0.289)	(0.169)
Neighborhood Conflict $_{t-1}$	1.618***	1.572***	1.609***	1.517**	1.443*	1.507**
	(0.003)	(0.005)	(0.003)	(0.029)	(0.061)	(0.031)
Proximity of Conflict	2.846***	2.689***	2.828***	3.964***	3.661***	3.944***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	6741	6741	6741	3999	3999	3999
Countries	170	170	170	100	100	100
Onsets	222	222	222	152	152	152
Log Likelihood	-889.756	-886.978	-889.625	-581.347	-578.503	-581.290
AIC	1799.512	1795.956	1801.249	1182.694	1179.006	1184.580

Exponentiated coefficients; *p*-values in parentheses; $_{t-1}$ variable lagged one year; SE clustered on country * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Interacting Conflict Legacy - Logistic Regression. Countries With Unknown Conflict Legacy Interacted as Separate Category

	Model (15b) All Countries
Log Oil Income/Pop $_{t-1}$	1.044
	(0.410)
Pre-Oil Conflict Legacy	1.603*
	(0.056)
Unknown Conflict Legacy	1.386
	(0.499)
Pre-Oil Conflict Legacy*	0.977
Oil Income/Pop $_{t-1}$	(0.764)
Unknown Conflict Legacy*	1.052
Oil Income/Pop $_{t-1}$	(0.572)
Log GDP/Cap $_{t-1}$	0.578***
	(0.000)
Log population	1.246***
	(0.003)
Log Rough Terrain	1.065
	(0.195)
Regime Transition $_{t-1}$	1.053
	(0.869)
Autocratic Regime $_{t-1}$	0.788
	(0.261)
Democratic Regime $_{t-1}$	1.019
	(0.936)
Mideast and N Africa	1.380
	(0.214)
Neighborhood Conflict $_{t-1}$	1.485**
	(0.022)
Proximity of Conflict	2.493***
	(0.000)
Observations	6456
Log Likelihood	-853.040
AIC	1736.079
Countries	170
Onsets	215

Exponentiated coefficients; *p*-values in parentheses *variable lagged one year. SE clustered on country* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Ten years prior to year of first oil income are censored as they are endogenous to pre-oil conflict legacy.

Appendix C

Table C.1: Conflict Legacy of Oil Income Countries

Oil countries *with* armed conflict during 10 years prior to first oil income	Oil countries *without* armed conflict during 10 years prior to first oil income		Oil countries with unknown conflict legacy (first oil income prior to 1946)
Israel	Gabon	Brunei	Argentina
Algeria	Nigeria	Switzerland	Bolivia
Oman	Morocco	Jordan	Colombia
Syria	Bangladesh	Surinam	India
Congo, Dem. Rep. (Zaire)	Chile	Belarus	Indonesia
Angola	Italy/Sardinia	Croatia	Iran
Guatemala	Serbia (Yugoslavia)	Kazakhstan	Iraq
Philippines	Bulgaria	Kyrgyz Republic	Malaysia
Yemen, Arab Rep.	Congo	Slovenia	Myanmar
Azerbaijan	Taiwan	Tajikistan	Pakistan
Georgia	Turkey	Turkmenistan	Peru
Papua New Guinea	Australia	Ukraine	Russia (Soviet Union)
Sudan	Kuwait	Uzbekistan	Venezuela
Cambodia	Libya	Lithuania	Albania
Chad	Trinidad and Tobago	Czech Republic	Austria
	Thailand	Slovakia	Brazil
	Barbados	Equatorial Guinea	Canada
	Spain	Norway	China
	Tunisia	Qatar	Cuba
	Afghanistan	United Arab Emirates	Ecuador
	Belgium	Denmark	Egypt
	Rwanda	Cameroon	France
	Bahrain	Cote D'Ivoire	German Fed. Rep.
	Benin	Ghana	Hungary
	Greece	Ireland	Japan
	Vietnam, Dem. Rep.	Sweden	Mexico
			Netherlands
			New Zealand
			Poland
			Rumania
			Saudi Arabia
			United Kingdom
			United States of America
Total: 15	*Total: 52*		*Total: 33*

Sources: Year of first oil income from Ross (2012), supplemented by PETRODATA . Conflict data: PRIO/UCDP Armed Conflict Data v4-2012.

Table C.2: Legacy of Armed Conflict Prior to Nationalization

Oil countries *with* armed conflict during 10 years prior to oil nationalization	Oil countries *without* armed conflict during 10 years prior to nationalization		Oil countries with unknown conflict legacy prior to nationalization
Algeria	Afghanistan	Kyrgyz Republic	Albania
Angola	Australia	Libya	Austria
Argentina	Bahrain	Lithuania	Brazil
Azerbaijan	Barbados	Norway	Canada
Bangladesh	Belarus (Byelorussia)	Qatar	China
Bolivia	Belgium	Rwanda	Cuba
Cambodia (Kampuchea)	Benin	Saudi Arabia	France
Chad	Brunei	Serbia (Yugoslavia)	German Fed. Rep.
Colombia	Bulgaria	Slovakia	Hungary
Congo, DR (Zaire)	Cameroon	Slovenia	Japan
Gabon	Chile	Spain	Mexico
Georgia	Congo	Surinam	Netherlands
Guatemala	Cote D'Ivoire	Sweden	New Zealand
India	Croatia	Switzerland	Poland
Indonesia	Czech Republic	Taiwan	Rumania
Iran (Persia)	Denmark	Tajikistan	United Kingdom
Iraq	Ecuador	Thailand	United States of America
Israel	Egypt	Trinidad and Tobago	
Malaysia	Equatorial Guinea	Tunisia	
Morocco	Ghana	Turkey (Ottoman Empire)	
Myanmar (Burma)	Greece	Turkmenistan	
Nigeria	Ireland	Ukraine	
Oman	Italy/Sardinia	United Arab Emirates	
Pakistan	Jordan	Uzbekistan	
Papua New Guinea	Kazakhstan	Vietnam, Dem. Rep.	
Peru	Kuwait		
Philippines			
Russia (Soviet Union)			
Sudan			
Syria			
Venezuela			
Yemen (Arab Rep.)			
Total: 32	*Total: 51*		*Total: 17*

Sources: Data on year of first nationalization from Guriev et al.(2011). If missing data on nationalization, year of first oil income from Ross (2012) is employed, supplemented by PETRODATA if first year of oil income is 1960. Conflict data: PRIO/UCDP Armed Conflict Data v4-2012.

Table C.3: Country Overview: Conflict Legacy and Onset Following Oil Income

Countries with conflict legacy and conflict onset following oil	*Countries with conflict legacy, but no new onset following oil.*	*Countries with no conflict legacy but conflict onset following oil income*	*Countries with no conflict legacy and no conflict onset or incidence following oil income*
Georgia Azerbaijan Chad Congo, D.R. (Zaire) Angola Algeria Syria Israel Yemen (Arab Rep.) Oman Philippines	Cambodia Guatemala* Papua New Guinea* Sudan* Countries with * have continuous conflict prior to and following oil income	Afghanistan Bangladesh Cameroon Chile Congo Croatia Gabon Ghana Morocco Nigeria Rwanda Serbia (Yugoslavia) Spain Surinam Tajikistan Thailand Trinidad and Tobago Tunisia Turkey (Ottoman Empire) Uzbekistan	Australia Bahrain Barbados Belarus (Byelorussia) Belgium Benin Brunei Bulgaria Cote D'Ivoire Czech Republic Denmark Equatorial Guinea Greece Ireland Italy/Sardinia Jordan Kazakhstan Kuwait Kyrgyz Republic Libya Lithuania Norway Qatar Slovakia Slovenia Sweden Switzerland Taiwan Turkmenistan Ukraine United Arab Emirates Vietnam, Dem. Rep.

Table C.4: Bivariate Analysis: Conflict Legacy and Incidence of Conflict Among Countries With First Oil Income After 1946

		No. Countries with Incidence of Armed Conflict Following Oil Income		*Total:*
		At least 1 incidence	No incidence	
No. Countries with Conflict Legacy Prior to First Oil Income	Yes	14 (93.3%)	1 (6.7%)	15 (100%)
	No	20 (38.5%)	32 (61.5%)	52 (100%)
Total:		34 (50.7%)	33 (49.3%)	67 (100%)

Pearson chi2(1) = 14.0241 Pr = 0.000

Appendix D

Table D.1: Interacting Nationalized Oil Income and Conflict Legacy - Logistic Regression of Armed Conflict Onset 1961-2007. All Countries Included in Estimates.

	Model (16) Nationalized Oil Income/Pop	*Model (17) Nationalized Oil Income/Pop*	*Model (18) Nationalized Oil Income/Pop*
Nationalized Oil Income/Pop $_{t-1}$	1.054	1.005	0.972
	(0.199)	(0.911)	(0.638)
Pre-Nationalization Conflict		2.088***	1.805**
		(0.001)	(0.045)
Pre-Nationalization Conflict * Nationalized Oil Income $_{t-1}$			1.063
			(0.405)
Log GDP/Cap $_{t-1}$	0.645***	0.695***	0.703***
	(0.000)	(0.002)	(0.003)
Log population	1.331***	1.264***	1.276***
	(0.000)	(0.001)	(0.001)
Log Rough Terrain	1.062	1.069	1.064
	(0.276)	(0.289)	(0.317)
Regime Transition $_{t-1}$ (0/1)	1.229	1.208	1.238
	(0.512)	(0.557)	(0.504)
Autocratic Regime $_{t-1}$ (0/1)	0.904	0.904	0.912
	(0.659)	(0.643)	(0.674)
Democratic Regime $_{t-1}$ (0/1)	1.082	0.981	0.990
	(0.749)	(0.938)	(0.967)
Mideast and N Africa	1.343	1.165	1.154
	(0.344)	(0.624)	(0.633)
Neighborhood Conflict $_{t-1}$	1.764***	1.684***	1.698***
	(0.002)	(0.006)	(0.005)
Proximity of Conflict	2.922***	2.237***	2.194***
Observations	6272	6272	6272
Log Likelihood	-786.915	-779.007	-778.584
AIC	1595.830	1582.013	1583.167
Countries	170	170	170
Onsets	194	194	194

Exponentiated coefficients; *p*-values in ( ); Inconsistent Regime$_{t-1}$ as reference category for regime dummies; SE clustered on country * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Ten years prior to nationalization year are censored as they are endogenous to conflict legacy variables.

Figure D.1: Model 18. Effect of Nationalized Oil Given Conflict Legacy

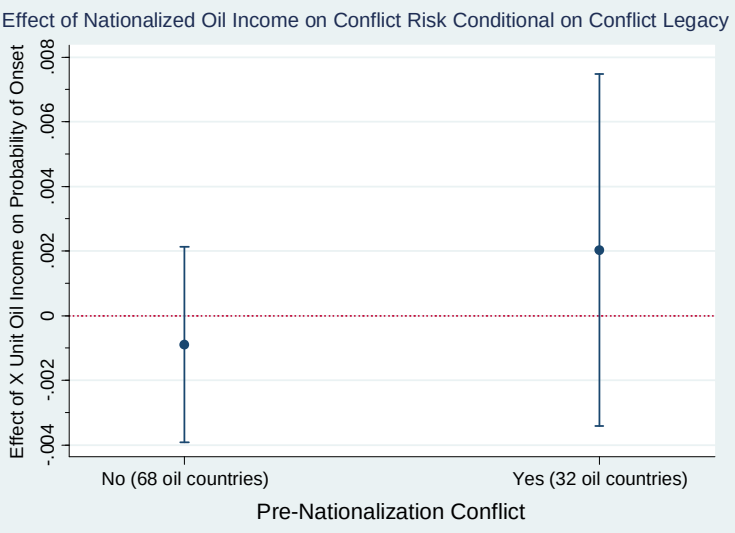


Figure D.1 and D.2 shows the conditional effect of nationalized oil income for different categories of conflict legacy, with 90% confidence intervals. Other variables are set to mean, median or most frequent values of oil income countries in 1961.

Figure D.2: Effect of Nationalized Oil Given Conflict Legacy (0, 1 or Unknown)

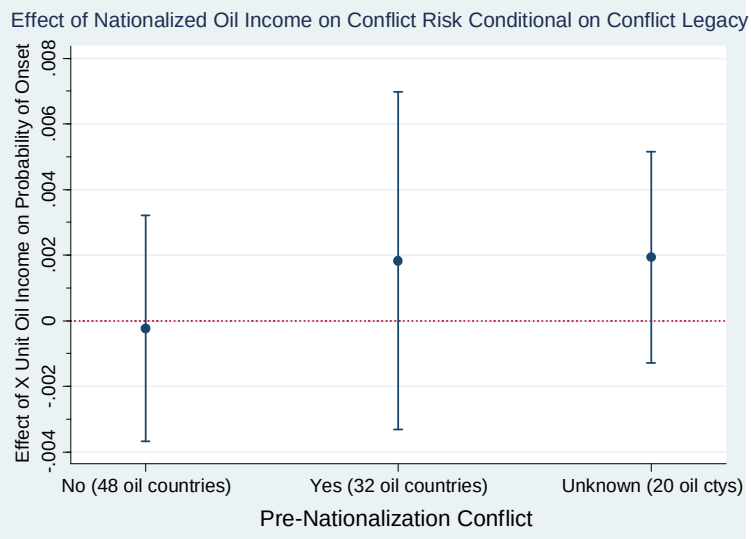


Table D.2: Bivariate Analysis: Conflict Legacy and Conflict Onset Among Countries With First Nationalized Oil Income After 1946

		No. Countries with **Onset** of Armed Conflict Following Nationalized Oil Income		*Total:*
		At least 1 onset	No onset	
No. Countries with Conflict Legacy Prior to First Nationalization	Yes	20 (66.7%)	10 (33.3%)	30 (100%)
	No	18 (35.3%)	33 (64.7%)	51 (100%)
Total:		38 (46.9%)	43 (53.1%)	81 (100%)

Pearson chi2(1) = 7.465 Pr = 0.006

Table D.3: Country Overview: Conflict Legacy and Onset Following Nationalized Oil Income

Countries with conflict legacy and conflict onset following nationalized oil income	*Countries with conflict legacy, but no new onset following nationalized oil income*	*Countries with no conflict legacy but conflict onset following nationalized oil income*	*Countries with no conflict legacy and no conflict following nationalized oil income*
Algeria	Bolivia	Afghanistan	Australia
Angola	Cambodia	Cameroon	Bahrain
Argentina	Gabon	Chile	Barbados
Azerbaijan	Colombia*	Congo	Belarus (Byelorussia)
Congo, DR (Zaire)	Bangladesh*	Croatia	Belgium
Georgia	Guatemala*	Egypt	Benin
India	Morocco*	Ghana	Brunei
Indonesia	Oman*	Rwanda	Bulgaria
Iran (Persia)	Papua New Guinea*	Saudi Arabia	Cote D'Ivoire
Iraq	Sudan*	Serbia (Yugoslavia)	Czech Republic
Israel		Spain	Denmark
Malaysia		Surinam	Ecuador
Myanmar (Burma)		Tajikistan	Equatorial Guinea
Nigeria		Thailand	Greece
Pakistan		Trinidad and Tobago	Ireland
Peru		Tunisia	Italy/Sardinia
Syria		Turkey	Jordan
Venezuela		Uzbekistan	Kazakhstan
Philippines			Kuwait
Yemen (Arab Rep.)			Kyrgyz Republic
			Libya
			Lithuania
			Norway
			Qatar
			Slovakia
			Slovenia
			Sweden
			Switzerland
	Countries with * have continuous conflict prior to and following nationalized oil income		Taiwan
			Turkmenistan
			Ukraine
			United Arab Emirates
			Vietnam, Dem. Rep.

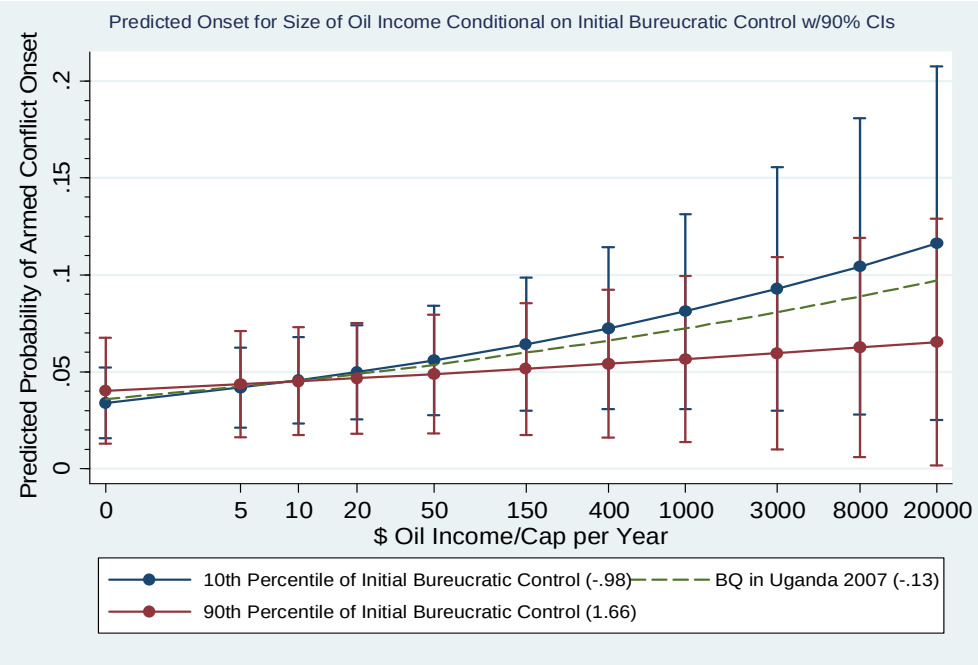
Table D.4: Bivariate Analysis: Conflict Legacy and Incidence of Conflict Among Countries With First Nationalized Oil Income After 1946

		No. Countries with **Incidence** of Armed Conflict Following Nationalized Oil		*Total:*
		At least 1 incidence	No incidence	
No. Countries with Conflict Legacy Prior to First Nationalization	Yes	27 (90 %)	3 (10%)	30 (100%)
	No	18 (35.3%)	33 (64.7%)	51 (100%)
Total:		45 (55.6%)	36 (44.4 %)	81 (100%)

Pearson chi2(1) = 22.8944 Pr = 0.000

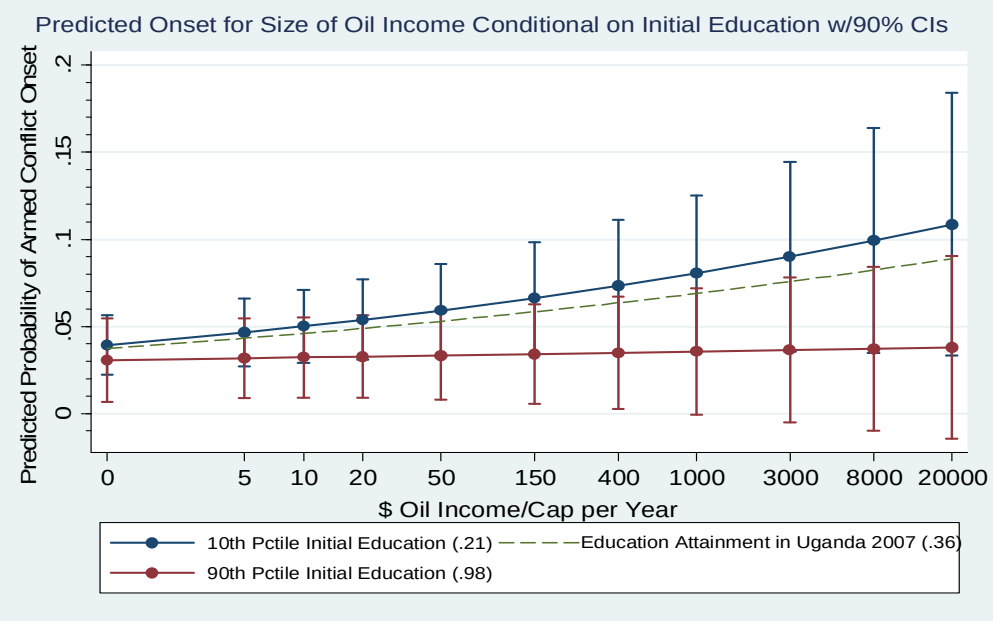
Appendix E

Figure E.1: Uganda. Predicted Probability of Armed Conflict for Increasing Oil Income at 10th and 90th Percentile of Initial Bureaucratic Control



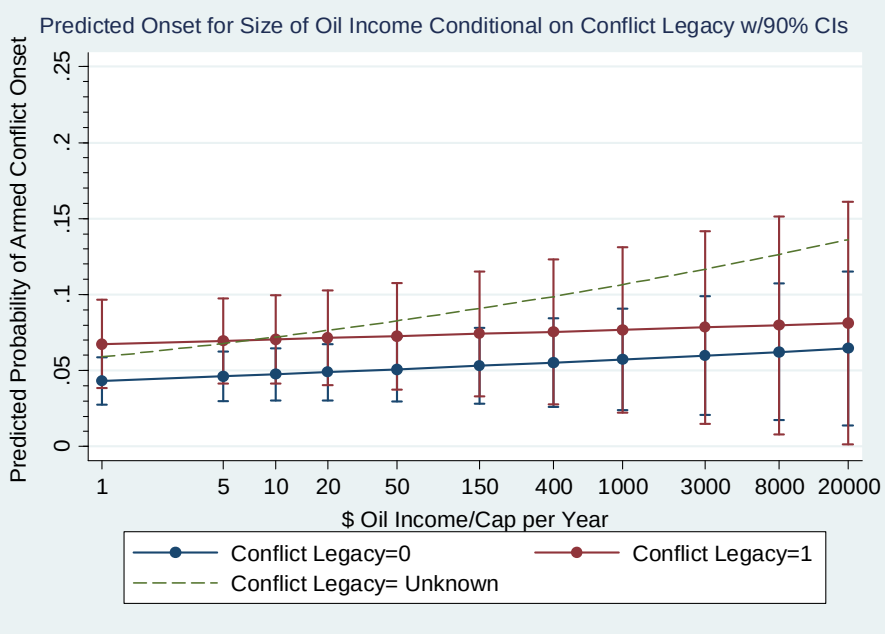
Predicted probabilities with 90% confidence intervals. Other variables set to representative values for Uganda in 2007. Estimates based on model 3 (oil countries).

Figure E.2: Uganda. Predicted Probability of Armed Conflict for Increasing Oil Income at 10th and 90th Percentile of Initial Education



Other variables set to representative of Uganda in 2007. Based on model 9 (oil countries). 90% CIs.

Figure E.3: Uganda. Predicted Probability of Armed Conflict for Increasing Values of Oil Income Conditional on Conflict Legacy.



Predicted probabilities with 90% confidence intervals. Other variables set to representative values for Uganda in 2007. Countries with an unknown conflict legacy are treated as a separate category. Estimates based on model 15b.

Appendix F

Figure F.1: Influential Observations in Regressions Testing H1

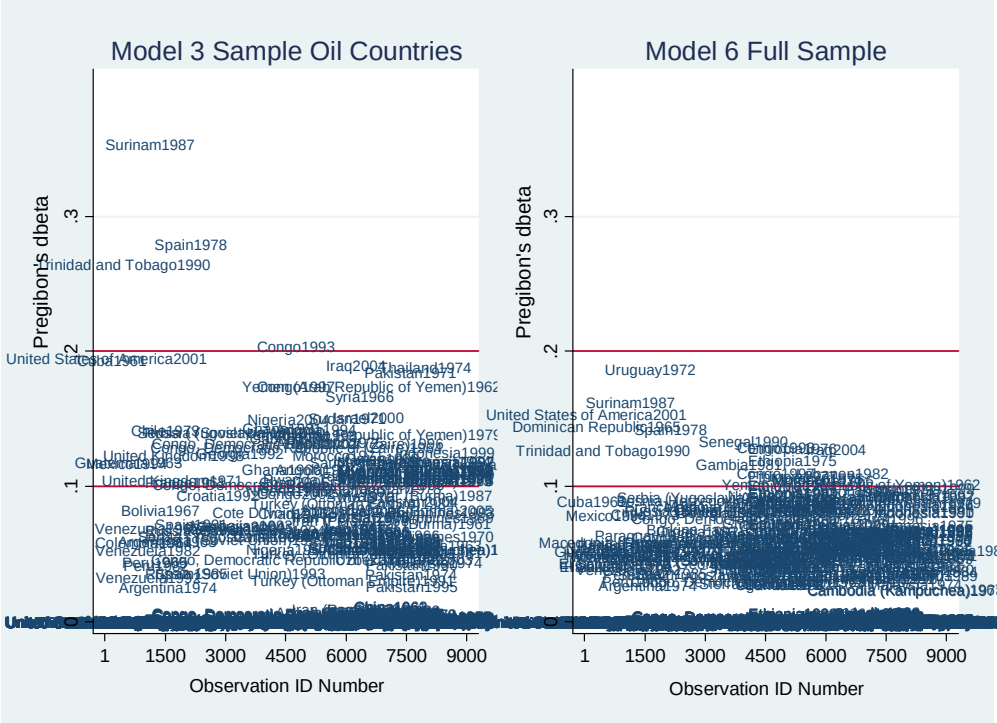


Figure F.2: Influential Observations in Regressions Testing H2

Figure F.3: Influential Observations in Regressions Testing H3 (Model 15b to the Left)

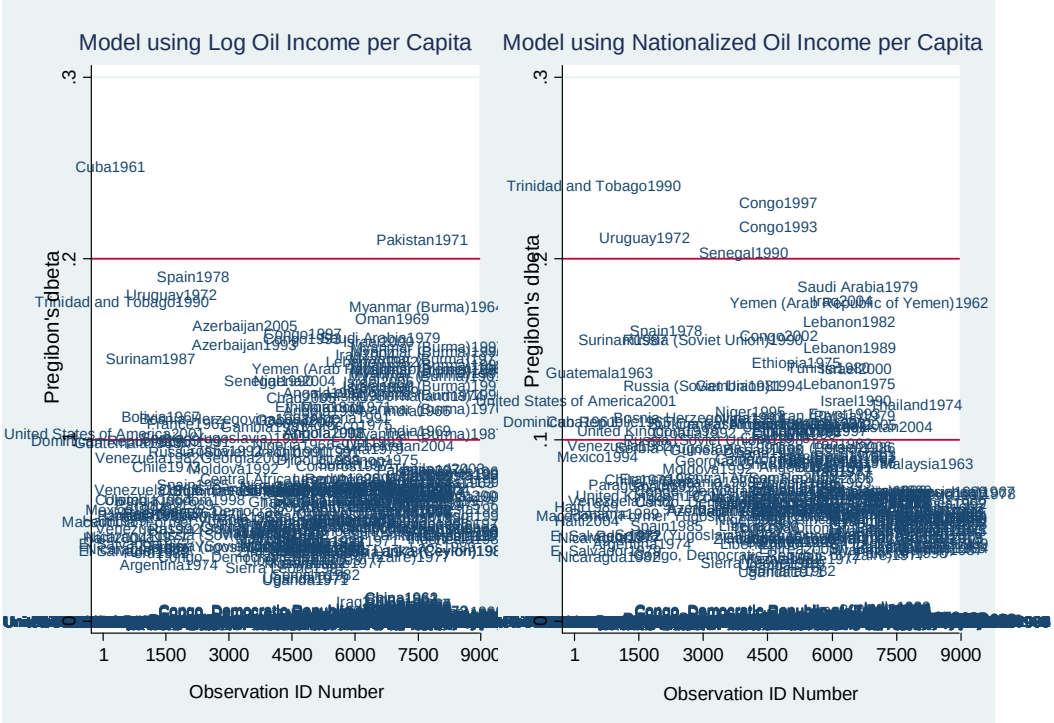
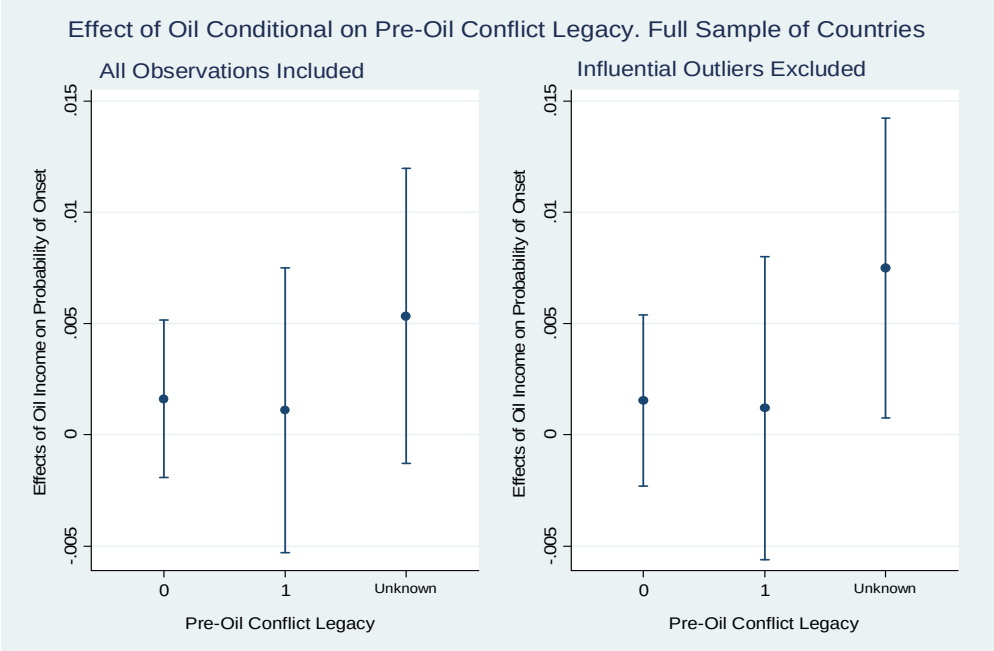
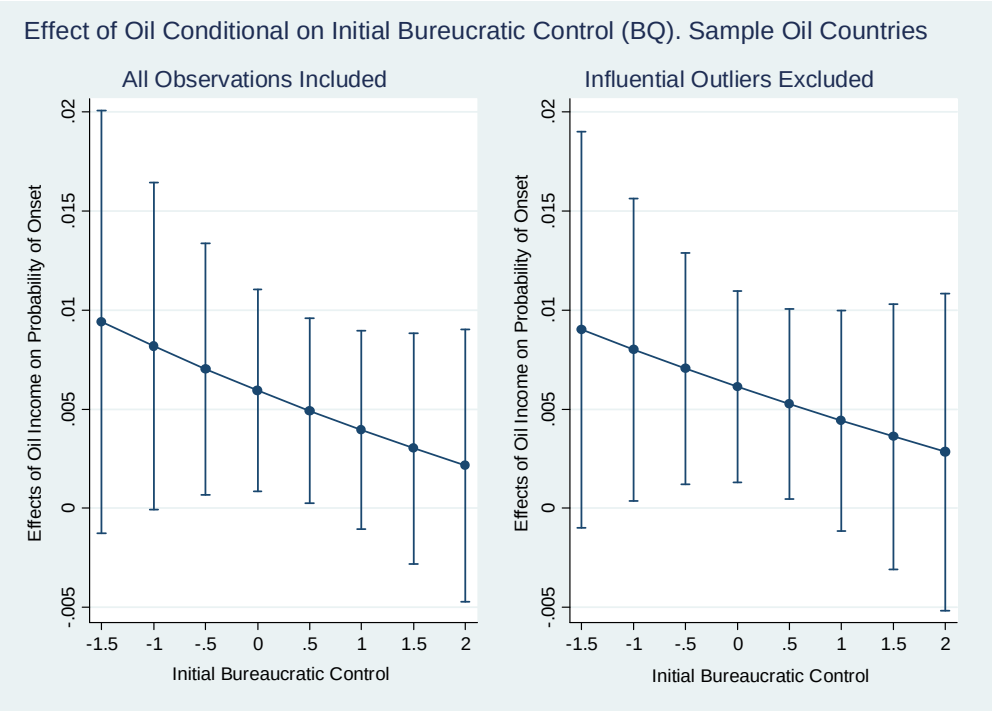


Figure F.4: Graphed Results of Model 15b, With and Without Influential Outliers



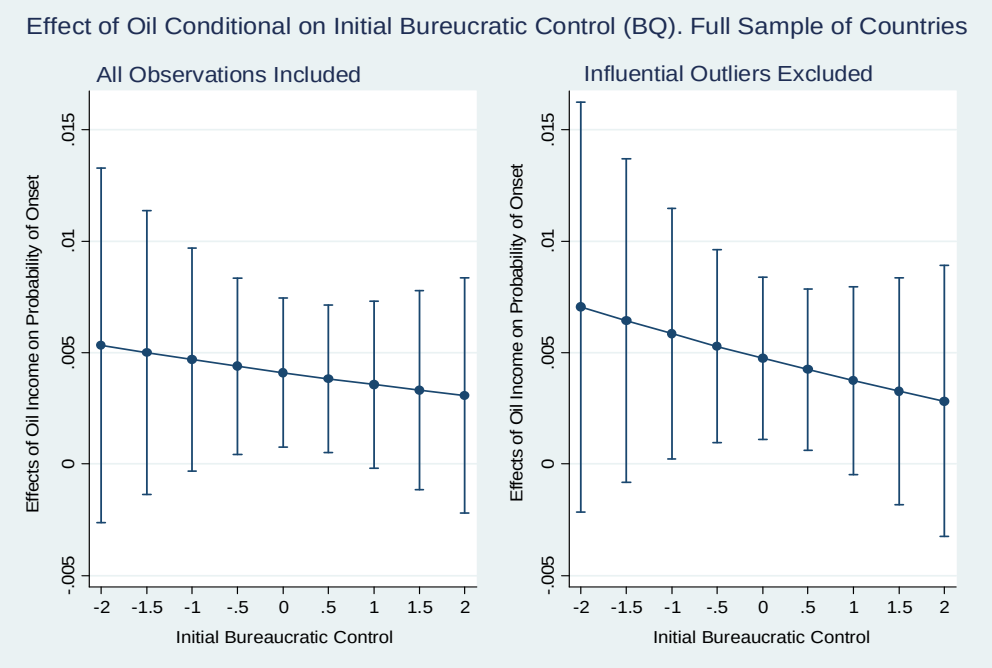
The graph to the left identical to previously graphed results from model 15b. Both graphs present estimates with 90% confidence level. In the right hand graph, the following 5 outlier observations are excluded: Cuba 1961, Trinidad and Tobago 1990, Uruguay 1972, Spain 1978, Pakistan 1971.

Figure F.5: Graphed Results of Model 3, With and Without Influential Outliers



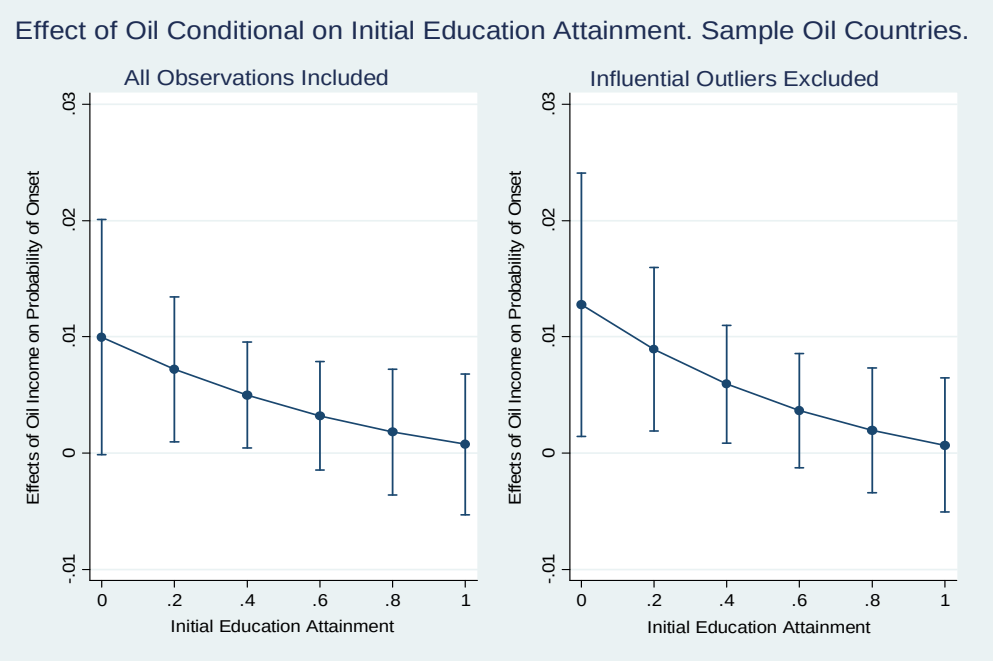
The graph to the left is identical to previously graphed results from model 3. Both graphs present estimates with 90% confidence level. In the right hand graph, the following 6 outlier observations are excluded: United States of America 2001, Cuba 1961, Trinidad and Tobago 1990, Surinam 1987, Spain 1978, Congo 1993.

Figure F.6: Graphed Results of Model 6, With and Without Influential Outliers



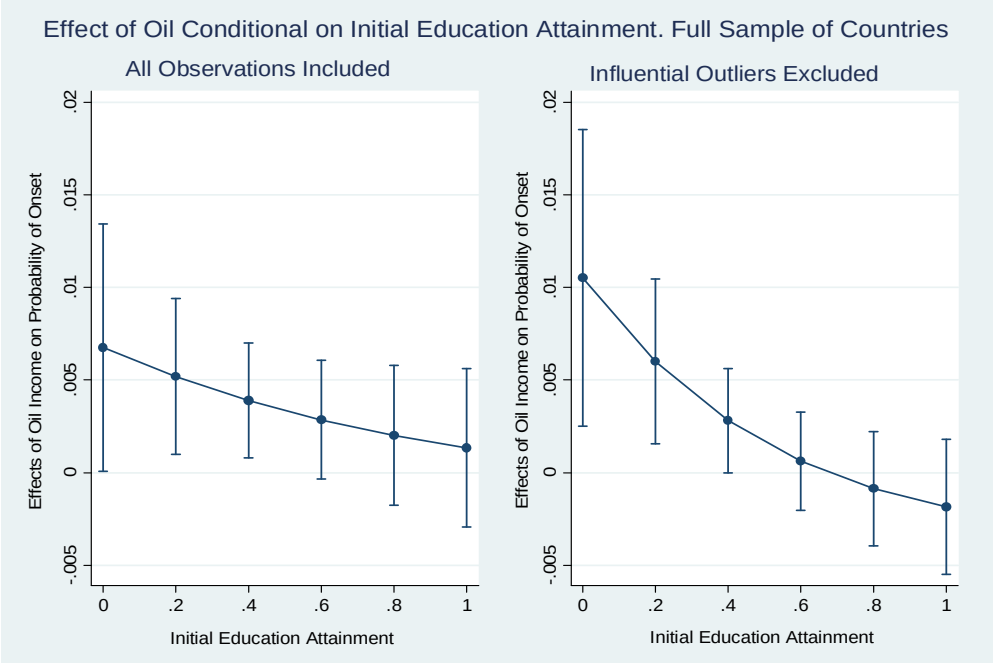
The graph to the left identical to previously graphed results from model 6. Both graphs present estimates with 90% confidence level. In the right hand graph, the following 5 outlier observations are excluded: United States of America 2001, Dominican Republic 1965, Surinam 1987, Uruguay 1972, Spain 1978.

Figure F.7: Graphed Results of Model 9, With and Without Influential Outliers



The graph to the left identical to previously graphed results from model 9. Both graphs present estimates with 90% confidence level. In the right hand graph, the following 5 outlier observations are excluded: Surinam 1987, Spain 1978, Serbia 1991, Russia (Soviet Union) 1990, Russia 1994.

Figure F.8: Graphed Results of Model 12, With and Without Influential Outliers



The graph to the left identical to previously graphed results from model 12. Both graphs present estimates with 90% confidence level. In the right hand graph, the following 5 outlier observations are excluded: Russia (Soviet Union) 1990, Russia 1994, Russia 1999, Azerbaijan 1993, Azerbaijan 2005.

Multicollinearity Assessment

VIF-values are obtained by the Stata *collin* postestimation command. VIF-values of variables in models important for the test of H1, H2 and H3 are reported.

Table F.1: VIF-Values for Variables in Model 3 and 6 (Testing H1)

Variable	**VIF-Value**	
	Model 3 (Oil Countries)	*Model 6 (All Countries)*
Log Oil Income/Pop $_{t-1}$	1.69	1.76
Initial Bureaucratic Control (BQ)	3.80	2.35
Log Oil Income*Initial BQ	2.54	1.82
Log GDP/Cap $_{t-1}$	2.60	2.23
Log population	1.34	1.27
Log Rough Terrain	1.20	1.14
Regime Type $_{t-1}$	1.32	1.21
Mideast and N Africa	1.34	1.36
Neighborhood Conflict $_{t-1}$	1.17	1.15
Proximity of Conflict	1.24	1.20

Table F.2: VIF-Values for Variables in Model 9 and 12 (Testing H2)

Variable	**VIF-Value**	
	Model 9 (Oil Countries)	*Model 12 (All Countries)*
Log Oil Income/Pop $_{t-1}$	6.81	6.33
Initial Education	4.40	3.09
Log Oil Income*Initial Edu.	7.64	6.60
Log GDP/Cap $_{t-1}$	2.54	2.74
Log population	1.32	1.28
Log Rough Terrain	1.19	1.14
Regime Type $_{t-1}$	1.25	1.17
Mideast and N Africa	1.40	1.41
Neighborhood Conflict $_{t-1}$	1.18	1.16
Proximity of Conflict	1.23	1.20

Table F.3: VIF-Values for Variables in Model 15 and 15b (Testing H3)

Variable	**VIF-Value**	**VIF-Value**
	Model 15 (All Countries)	*Model 15b (All Countries)*
Log Oil Income/Pop $_{t-1}$	1.75	2.28
Conflict Legacy	1.90	4.54
Log Oil Income*Conflict Legacy	2.01	5.07
Log GDP/Cap $_{t-1}$	1.66	1.64
Log population	1.27	1.58
Log Rough Terrain	1.14	1.15
Regime Type $_{t-1}$	1.17	1.18
Mideast and N Africa	1.34	1.30
Neighborhood Conflict $_{t-1}$	1.16	1.16
Proximity of Conflict	1.22	1.21

Replication Data

Replication data and do-file is available at: http://bit.ly/1kmxW4D