ECONOMICS

Working Paper Series

No. 01-2020

Oil Revenues vs Domestic Taxation: Deeper insights into the crowding-out effect

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JEL classification: H2, Q33, Q38

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This paper exploits the 2000s commodity price boom to identify the impact of oil revenues on domestic taxation in oil exporting countries. It estimates the average effect of oil revenues on non-resource taxation for 19 oil exporting countries using synthetic control methodology and finds that non-resource tax per capita is on average 14% lower in oil exporting countries because of the 2000s commodity price boom compared to a scenario without price shock. This result confirms the existing literature concerning the resource revenues vs domestic taxation debate. Additional knowledge is derived from the synthetic control method showing that the effect is heterogeneous and occurs only in oil exporting countries with a low level of institutional quality, which are highly oil dependent and prefer the use of tax instruments rather than non-tax instruments. Furthermore, the dynamics of the effect differs in countries with a state-owned oil sector compared to a private-owned oil sector. These findings are new within the debate and contribute to our understanding of the effect of natural resources on domestic taxation. Policy makers concerned by a crowding-out effect should invest the oil dividend to improve their tax administration to avoid the negative consequences accompanying low domestic taxes such as the resulting dependency on a volatile income stream from oil, difficulty in achieving non-fiscal objectives, and lack of positive externalities from taxes such as transparency and better governance.

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1. Introduction

Low levels of government revenues are a major obstacle for development in many countries (Chaudhry 1997). Government revenues average at 17% and 25% of GDP in low- and middle-income countries respectively compared to 32% in high-income countries (Knebelmann 2017). At the same time, natural resources, such as oil, gas, and minerals contribute significantly to the government budget in

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many countries. They generate annually an estimated US$ 4 trillion in economic rents worldwide and the World Bank categorises currently over 50 countries as resource dependent. This massive amount of economic rents could eradicate poverty in those countries, improving the lives of over 1.5 billion people (Barma et al. 2012). Countries ‘blessed’ with an abundance of natural resources could use them to fill the revenue gap and lift resource-rich low- and middle-income countries on a better development trajectory. However, the reality often looks different.

Many scholars argue that resource revenues crowd out other forms of government revenues, especially domestic taxes (Bornhorst, Gupta, and Thornton 2009; Crivelli and Gupta 2014; Mahdavy 1970; Ossowski and Gonzales 2012; Ross 2001; Thomas and Treviño 2013). Focusing on the resource sector and substituting resource revenues for other forms of taxation can be appealing for the government for several reasons. The potential gains in terms of rents is huge, outperforming in many countries the potential gains from traditional taxation (Barma et al. 2012). Collecting revenues from the resource sector is relatively easy because fewer stakeholders are involved. Fewer stakeholders and the resulting low visibility make it easier to hide income if desired (Lei and Michaels 2014). Apart from lower effort, more waste and/or corruption, it could also be argued that governments substitute taxes with resource revenues intentionally to promote economic growth. Lower taxation of companies and individuals lead to lower production costs and higher disposable incomes. The former increases competitiveness of domestic businesses and the latter increases consumption (IMF 2012b).

However, there are also arguments against a crowding-out effect, stating that resource revenues could increase tax income. The most obvious way for resource revenues to increase tax income would be if the government invests the resource revenues directly into tax administration (Besley and Mclaren 1993; Besley and Persson 2009, 2013). Higher wages for tax collectors, better training and technology should improve the tax administration and therefore governments’ tax income. Further, to collect revenues from the resource sector the government needs a sophisticated tax administration division dealing with resource companies. If this division is not operating in isolation, it could be that they have positive spill overs on the rest of the tax administration (Knebelmann 2017).

The way in which resource revenues influence taxes can go both ways and this paper analyses if there is a prevailing direction. Exploiting the 2000s commodity price boom as a positive income shock for oil exporting countries and using comparative case study analysis in the form of the synthetic control method, I find a crowding-out effect of non-resource tax per capita through oil revenues. Due to the 2000s commodity price boom total tax per capita is on average 14% lower in oil exporting countries compared to what total tax per capita would have been without the price boom. The results confirm the finding of other scholars.[2] However, I also show that this effect is heterogeneous by institutional quality, the level of oil dependence, the use of different fiscal instruments, and the ownership structure of the oil sector, which are new findings.

The existence of a crowding-out effect should be a concern for policymakers because oil revenues reliance can impede the planning of a sustainable state budget in several ways. Firstly, the market price of oil is unpredictable which makes revenues volatile (ECB 2004; Loutia, Mellios, and Andriosopoulos 2016). Secondly, the life cycle of oil projects can span over several decades, which increases uncertainty. Fiscal policy decision regarding the project at the time of discovery could turn out to be sub-optimal and renegotiation of former mistakes come with high reputational costs, influencing future projects (Knebelmann 2017). Thirdly, oil is a non-renewable resource that will run out in the near future, hence, consuming the benefits is not sustainable (Auty 1998; Barma et al. 2012).

Reliance on resource revenues can have further adverse political effects by reducing transparency. Political and economic research associate traditional taxation with improvements in transparency and governance, because tax compliance is only sustained through bargains and concessions between citizens and the government (Moore 1966; North 1990; Prichard 2015). Resource revenues do not possess the same beneficial attributes (Ross 2015). This argument represents also the first mechanism of the rentier effect in which according to Ross (2015) resource-rich governments use resource revenues to reduce taxation (taxation effect) to avoid demands from citizens to democratize.[3]

A further negative consequence of a crowding-out effect is that non-financial objectives become more difficult to accomplish. Apart of generating revenues taxes are also a fiscal policy instrument, which can stabilise the economy in turbulent times, redistribute income to reduce inequality, or reduce consumption of goods with negative externalities such as smoking. An underdeveloped tax system fails to provide these policy possibilities (Mahdavy 1970; McLure, Meumark, and Cox 2016).

The empirical literature so far has focused on the question how resource revenues influence tax or revenue effort, which is the tax or revenue to GDP ratio. Bornhorst, Gupta, and Thornton (2009) find in a sample of 30 hydrocarbon-producing countries in the period 1992 to 2005 that a one percentage point increase in hydrocarbon revenues reduces revenue effort by around 0.2 percentage points. Extending the Bornhorst, Gupta, and Thornton (2009) sample to 35 hydrocarbon-producing countries and an extended time period till 2009, Crivelli and Gupta (2014) find that a one percentage point increase in the resource revenues to GDP ratio leads to a 0.3 percentage point decrease in tax effort,

[2] For example: Bornhorst, Gupta, and Thornton 2009; Crivelli and Gupta 2014; Thomas and Treviño 2013; Ossowski and Gonzales 2012.

[3] The other two mechanisms are the spending effect and repression effect through which resource-rich governments increase public spending and suppress formation of political groups to avoid democratization (Ross 2015).

mainly driven by a reduction in revenues from taxes on goods and services. Focusing on geographic sub-samples, Ossowski and Gonzales (2012) and Thomas and Treviño (2013) find similar results for Latin America and Sub-Saharan Africa respectively. In contrast, a recent paper by (Knebelmann 2017) challenges those findings. She uses a sample of 31 oil-rich countries and exploits the 2000s oil price boom as an exogenous shock in resource revenues to measure the causal impact of resource revenues on domestic taxation. She does not find a negative effect of resource revenues on domestic tax effort, rather a weak positive effect.

This paper contributes to the existing literature in several ways. First, I am using a novel methodology, which allows apart from estimating an average treatment effect to analyse sub-samples and make inferences about heterogeneity across countries which was not achieved so far. The synthetic control methodology developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) is based on comparative case studies and allows for a deeper insight into the crowding-out effect. Second, I focus on non-resource tax per capita instead of the non-resource tax to GDP ratio (tax effort). Using a ratio as dependent variable provides useful information, but it is not certain if the effect is triggered by a change in the numerator or denominator.[4] Further, total tax per capita allows seeing the issue from a distinct perspective.[5] Tax effort measures how important taxes are in an economy while per capita values show by how much an individual is affected by an oil price shock. It further allows calculating how much government revenues are foregone because of the price shock. And finally, I am exploiting the 2000s commodity price boom as an exogenous shock to determine the causal relationship between resource revenues and total tax per capita. Only Knebelmann (2017) used an identification strategy relying on exogenous variation of resource revenues.

The identification strategy relies on the stochastic character of the 2000s commodity price boom from the perspective of oi exporting countries. The 2000s commodity price boom describes the rise of many physical commodities in the early 21st century (Helbling 2012). The focus of this paper is on non-renewable natural resources in particular oil and gas, because oil and gas fulfil the requirements of the rentier state theory. Oil and gas create rents and are mostly exported, which generates an external windfall for the government from outside of the domestic economy. This gives the government the possibility to substitute rents for taxes (Beblawi 1990). Between 1999 and 2012 the international oil price increased by 450%. The price hike is associated with increasing demand from emerging economies, low interest rates set by the Federal Reserve resulting in a weak US$ and speculative investments

[4] This could also hold for the tax per capita variable, particularly in countries with a large foreign work force. However, a t-test comparing the average population growth rate in oil exporting countries before and during the 2000s commodity boom did not reveal a significant difference, while average GDP growth is significantly greater during the price boom.

[5] Total tax per capita always refers to non-resource tax per capita throughout this paper if not stated otherwise.

(Carter, Rausser, and Smith 2011; Hamilton 2009, 2011). None of the reasons can be influenced by individual oil exporters in the sample and therefore the price shock qualifies as plausible exogenous allowing to analyse the causal relationship between resource revenues and non-resource tax income. More on the exogeneity and timing of the 2000s commodity price boom will be discussed in section 0.

The working argument is that the non-oil sector of an oil exporting economy is affected in the same way by a price shock as the non-resource sector of a non-oil producing country.[6] The only difference between an oil exporting and a non-oil producing country is that the government in an oil exporting country receives additional funds from the oil sector during the price boom. These additional funds can be saved (increasing reserves), stolen by members of the government (corruption) or transferred to the non-oil sector by increasing public expenditure or by reducing taxes. The latter is the focus of this paper. Under the assumption that non-oil producers are affected in the same way as the non-oil sector in an oil exporting country, they can be used to construct synthetic control countries. The idea of the synthetic control methodology is to create a counterfactual country that behaves in the same manner as the oil producing country if the price shock would not have occurred. Identification and assumptions will be discussed further in section 0.

I find evidence for crowding-out. On average the treatment effect is negative supporting previous findings by indicating that the 2000s commodity price boom and the resulting increase in oil revenues lead to a decrease of about 14% in non-resource tax per capita in oil exporting countries. However, this average effect is heterogeneous, and the synthetic control analysis shows that the tax reducing effect is more prone in highly oil dependent countries with a low level of institutional quality and a preference to extract revenues from the oil sector via tax instruments. The average treatment effect indicates that a person in an oil exporting country paid around US$ 300 less tax each year because of the price boom as compared to a scenario without price shock. For example, Saudi Arabia with an average population of around 24 million, lost US$ 7.3 billion each year. This foregone yearly tax income is only US$ 2.05 bn short of what the Saudi's tax authority expects to generate from the VAT introduced for the first time in Saudi Arabia's history in 2018.[7]

The results survive several robustness checks. Controlling for outliers by excluding the biggest oil exporter and potential swing producer (Saudi Arabia) and the country with the highest per capita taxes (Norway) does not alter the results. Further, the results are robust using different specifications controlling for additional predictor variables.

[6] The non-oil sector of a non-oil producing country is the same as the whole economy.

[7] The Saudi tax administration (General Authority of Zakat and Tax) estimates that the 5% VAT will generate US$ 9.35bn in 2018 (Arabian Business 2018).

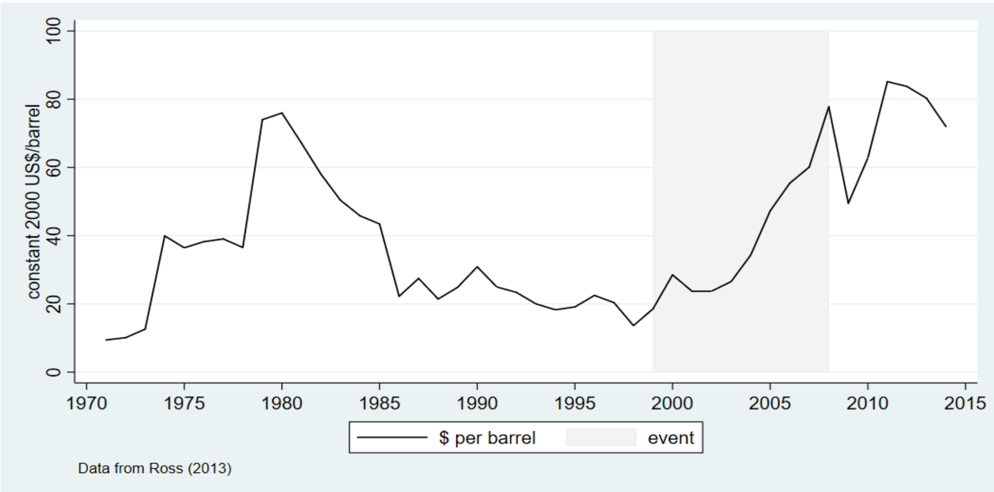
The remainder of the paper is structured as follows: Section 0 describes the 2000s commodity price boom and explains why it can be considered as exogenous in this setting. Section 0 and 0 explains the methodology and data used to estimate the effect. Section 0 discusses the results and section 6 provides robustness checks and section 7 concludes.

2. The 2000s commodity price boom

In this section, I describe the price behaviour of oil throughout the 2000s, the reasons for the commodity price boom as well as the timing and the exogeneity of the 2000s commodity price boom for oil exporting countries.

Following the 1973 and 1979 energy crisis the oil price plummeted from an all-time high above US$ 75 per barrel to US$ 22 in the mid-1980s (see Figure 1). The price stayed low for the following years fluctuating between US$ 18 and US$ 30 per barrel with an average of US$ 22 until 1998. From 1999 on the oil price increased almost continuously up to a new all-time high of about US$ 85 in 2011 and stayed on an elevated level in 2012. Two short interruptions of the price boom occurred between 1999 and 2012. The first, between 2002 and 2003, was because of uncertainties created after the terrorist attack on 9/11 and the invasion of Iraq. The second downturn came because of the Great Recession in 2009. The overall increase between 1999 and 2012 was around 450%.

Figure 1: Oil price 1970-2015



The reasons for the commodity price boom are still discussed and no single cause could be determined. The most prominent arguments include that the price boom was demand driven by high economic growth in China (Hamilton 2009), the monetary policy of the Federal Reserves with low interest rates resulting in depreciation of the US$ (Carter, Rausser, and Smith 2011; Frankel 2008), and that speculative investment played a role (Masters 2008). Most likely it was a mix of all these reasons. Important here to note is that contrary to past commodity price booms there was no significant supply reduction that triggered the price boom. World oil supply was remarkably stable despite events such as hurricanes in the Gulf of Mexico, turmoil in Nigeria and conflict in Iraq (Hamilton 2009). The mentioned reasons - high demand, a weak US$ and speculation - are likely not influenced by oil exporters which makes the period ideal to analyse the causal impact of an oil income shock.

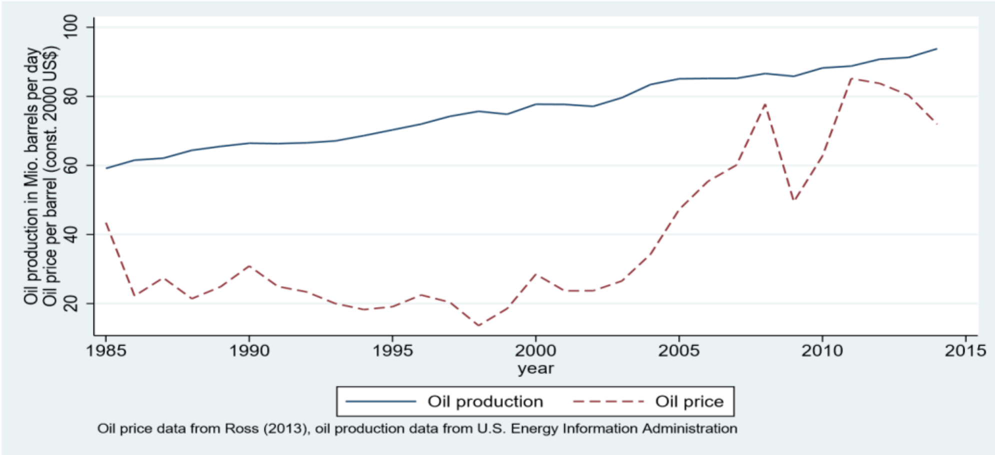
The timing of the commodity price boom is derived by econometric means. I regressed the price for oil on its lagged value for the time between 1961 and 2015 and conduct a Wald-test for each year testing the null hypothesis of no structural break. The Wald statistics identifies structural breaks for the oil price coefficient from 1999 until 2010[8]. The considered treatment period, however, is restricted to cover 1999 to 2007 (see grey area in Figure 1). The reason for the shorter time period is because of an interruption of the price boom in 2008 (Great Recession). Error! Reference source not found. in Appendix A. Tables reports Wald test statistics.

To establish exogeneity in this setting it is necessary to understand that the price shock does not have to be stochastic. It suffices that the treatment assignment is merely orthogonal to the country's characteristics (Liou and Musgrave 2014). This statement translates into two conditions that must be true to capture a causal effect. First, none of the treatment countries influenced the timing or the likelihood of the event and second, no country influenced their assignment to treatment, i.e. did not anticipate it.

An oil producer can influence the timing, likelihood or magnitude of a price shock only through the supply side by changing production levels. Overall, world oil production increased at an almost constant rate over the considered period (see Figure 2). The average oil production growth rate for the sample period is 1.4%, before and after 1999 it was 1.6% and 1.3% respectively. Increasing supply should lead to a fall in prices assuming constant demand. However, oil consumption increased over the same period shifting demand up at a stronger rate than supply leading to a price increase.

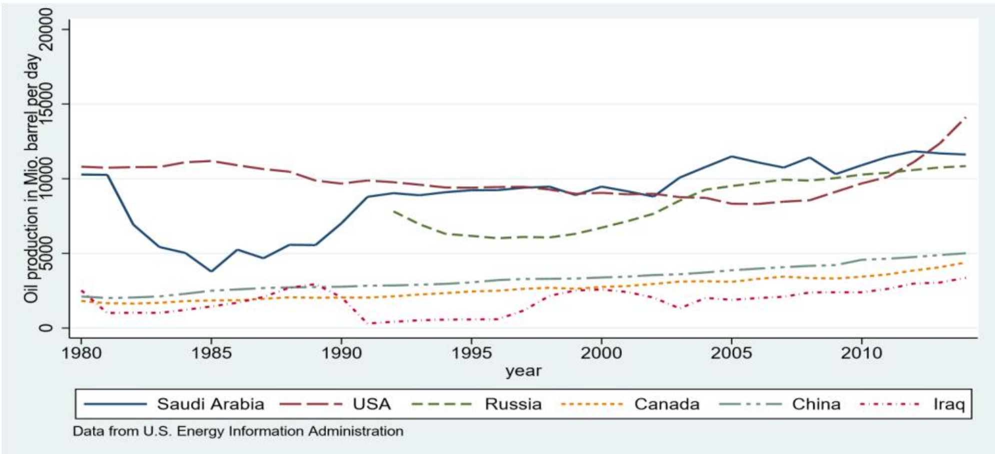
[8] The only exception is 2001. The oil price was affected in 2001 mainly through the 9/11 terrorist attacks but it only slowed down the oil price boom which speed quickly revived in the following years. The p-value in 2001 is 0.115 and only slightly above conventional significance level and will be included in the treatment period to avoid a gap in the sample.

Figure 2: Oil price and world production 1985-2015



For an individual country to be able to influence the oil price it must have a big enough market share. Figure 3 shows oil production of the six biggest oil producers in the world. Out of the six countries, only Saudi Arabia deserves a further discussion because the remaining five are net-oil importer (USA, China) or have no data available (Iraq, Russia, Canada) to be part of the sample.

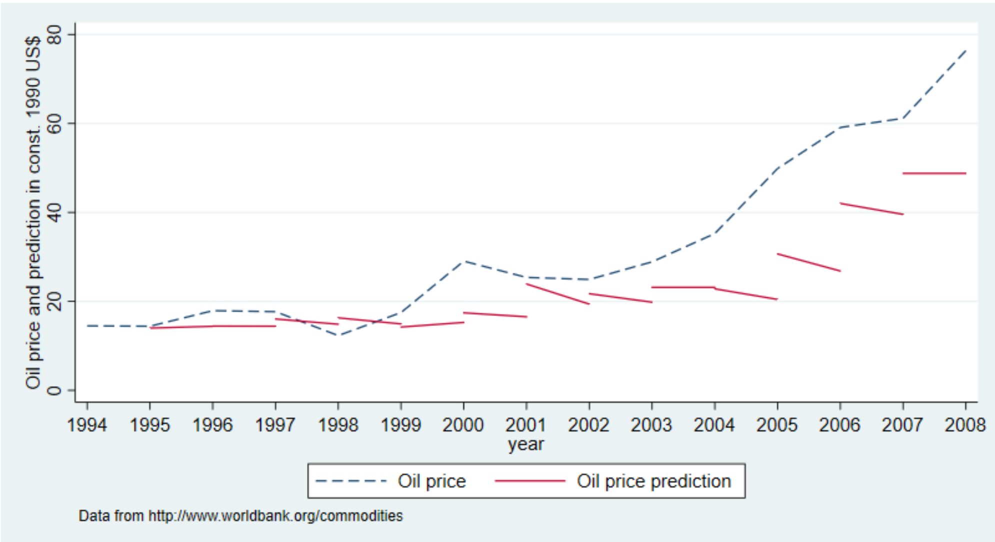
Figure 3: Top six oil producers 1080-2015



Saudi Arabia is considered a swing producer. The favourable oil fields on the Arabic peninsula and estimated reserves of around 260 billion barrels allow Saudi Arabia to change production level at will and influence the oil price (Fattouh and Mahadeva 2013). However, the kingdom is part of OPEC and therefore decisions are made under concessions and discussions with other members. Saudi Arabia's oil production was almost constant from the beginning of the 1990s till 2002 at around 9 million barrels per day. In 2003, production increased to 10 million barrels per day and stayed above this level ever since (see Figure 3). The increase in production should lead to a price decrease but the production increase was not enough, and prices continued to rise. However, given the market power of Saudi Arabia I also test all specifications excluding Saudi Arabia from the sample (see Section 6).

One remaining point before exogeneity can be established is the predictability of the price shock. Did oil exporters anticipate the price shock and adjust their behaviour? This is unlikely because predicting the future oil price is difficult up to the point that financial institutions such as the IMF assume that the oil price follows a random walk, i.e. the best possible prediction of next year's oil price is this year's oil price (ECB 2004). Figure 4 compares the actual oil price with forecasts from World Bank's Pink Sheets (The World Bank 2018). The prediction is always done for the following two years and from 1999 onwards when the price boom started, the prediction is always below the actual oil price. The same is true for other price forecasts.

Figure 4: Oil price and World Bank forecasts



In conclusion, during the 2000s commodity price boom the oil price increased by over 400%, none of the oil exporting countries influenced the price through supply adjustments and it was arguably an unexpected event. Therefore, the oil price boom was plausibly exogenous.

3. Methodology

The 2000s commodity price boom represents a natural experiment and the resulting exogenous variation is explored in this paper to identify the causal effect of a positive resource induced income shock on domestic taxation. A common approach to identify a causal effect with natural experiments is the difference-in-difference (DiD) approach (Angrist and Pischke 2009). DiD estimates a causal effect by contrasting the changes in the pre- and post-treatment period between treatment and control countries. The identifying assumption is that in the absence of the treatment, the outcome variable of the treated and control countries would have followed parallel trends (Abadie 2005; Angrist and Pischke 2009; Ashenfelter 1978; Ashenfelter and Card 1984). The parallel trend assumption is not always plausible and cannot be tested. However, a minimum requirement for a valid DiD should be that the parallel trend assumption holds in the pre-treatment period, which can be tested by interacting the treatment dummy with the time dummies. The interaction term should be zero for the whole pre-treatment period (Angrist and Krueger 1999). This test fails for the sample at hand, hence DiD is not feasible with the available data.

An alternative way, to exploit natural experiments is the synthetic control method (SCM) proposed by Abadie, Diamond, and Hainmueller (2010) and Abadie and Gardeazabal (2003). SCM assumes that in the absence of treatment the expected outcome would have been the same for treatment and control countries conditional on past outcomes and control variables. This is the so called 'independence conditional on past outcomes' assumption (Firpo and Possebom 2015). The advantage of SCM is that it does not rely on parallel trends, allows the effect to vary over time and works in situations with small sample size (Abadie, Diamond, and Hainmueller 2015), which makes it the preferred methodology for this paper.

3.1 Synthetic Control Method

In what follows, I discuss implementation and identifying assumptions of the synthetic control method (SCM).[9] The original SCM framework was designed for cases where treatment occurs only to one country (Abadie and Gardeazabal 2003). It is obvious that the 2000s commodity price boom influenced not just one oil exporting country but all of them. For this reason, I follow Cavallo's extended SCM approach, which allows for multiple treated units at different times (Cavallo et al. 2013).

The basic idea of SCM is to construct a synthetic control country as a weighted average of the available control countries. The weights are generated by a data-driven algorithm to ensure that

[9] For a discussion about the inference of the SCM results see Appendix C. Inference.

covariates and outcomes of the synthetic control country match with the treated country in the pre-treatment period. The weights are then used to predict the outcome variable for the counterfactual of no treatment in the post-treatment period. The resulting synthetic control country can be compared to the actual outcome and the difference represents the treatment effect (Abadie, Diamond, and Hainmueller 2010; Cavallo et al. 2013). A formal discussion follows below.

3.2 Implementation of SCM

Suppose a universe with $(J+1) \in \mathrm{N}$ countries during $T \in \mathrm{N}$ time periods. For now, the intervention (price shock) affects only country 1 (treatment country) and the rest of the countries are unaffected. Intervention starts in $T_0 + 1$ and continuous uninterrupted till T, where $1 \leq T_0 < T$. Let the scalar $Y_{j,t}^N$ be the potential outcome in the absence of the treatment for country $j \in \{1, \dots, J+1\}$ in period $t \in \{1, \dots, T\}$. The scalar $Y_{j,t}^I$ denotes the potential outcome that would be observed if the intervention occurs from $T_0 + 1$ to T in country j at time t. With this notation

$$\alpha_{j,t} = Y_{j,t}^I - Y_{j,t}^N \tag{1}$$

describes the intervention effect for country j in period t. Let $D_{j,t}$ be the intervention dummy assuming the value 1 if country j faces the intervention in period t and value 0 otherwise. Combining this gives the observed outcome for country j in period t by

$$Y_{j,t} = Y_{j,t}^N + a_{j,t} D_{j,t}$$

Because only the first country is affected by the intervention from period $T_0 + 1$ to T, the intervention dummy is defined as:

$$D_{j,t} = \begin{cases} 1, & if\ j = 1\ and\ t > T_0 \\ 0, & otherwise \end{cases}$$

The aim is to estimate the intervention effect $(\alpha_{1,T_0+1}, \dots, \alpha_{1,T})$ for country 1 for each post-intervention period $(T_0 + 1)$. Since $Y_{1,t}^I$ is observable it is only necessary to estimate $Y_{1,t}^N$, the unobserved outcome for country 1 without treatment.

The synthetic control estimator of $Y_{1,t}^N$ can be defined as

$$\hat{Y}_{1,t}^N = \sum_{j=2}^{J+1} \hat{w}_j Y_{j,t}. \tag{2}$$

Suppose a vector of weights $\widehat{W} = [\widehat{w}_2, \ldots, \widehat{w}_{J+1}]'$ with $\widehat{w}_j > 0$ for $j = 2, \ldots, J+1$ and $\sum_{j=2}^{J+1} \widehat{w}_j = 1$ exists so that

$$Y_{1,t} = \sum_{j=2}^{J} \widehat{w}_j Y_{j,t}, \forall t \in \{1, \ldots, T_0\} \ and \ Z_1 = \sum_{j=2}^{J} \widehat{w}_j Z_j \tag{3}$$

holds, then

$$\widehat{\alpha}_{1,t} = Y_{1,t} - \sum_{j=2}^{J} \widehat{w}_j Y_{j,t}, \forall t \in \{T_0 + 1, \ldots, T\} \tag{4}$$

represents the estimated treatment effect for the treated country. The first part of equation (3) states that the weighted average of the pre-treatment outcome of the control countries should perfectly match the pre-treatment outcomes of the treated country. The second part of equation (3) indicates that the weighted average of the control countries' covariates perfectly replicate the covariates of the treated country. These two conditions only hold if the outcome and the covariates of the treated country $(Y_{1,t}, Z_1)$ lie within the convex hull of $[(Y_{2,1}, \ldots, Y_{2,T_0}, Z_2'), \ldots, (Y_{J+1,1}, \ldots, Y_{J+1,T_0}, Z_{J+1}')]$ (Abadie, Diamond, and Hainmueller 2010). This is not often the case but Abadie, Diamond, and Hainmueller (2010) show a way to select $\widehat{W}$ so that equation (3) holds approximately. They propose to minimize the distance between the vector of covariates and outcome variable of the treatment countries and the weighted matrix with the same outcome variable and covariates of each control country in the pre-treatment period using the Euclidian metric (or a re-weighted version of it) (Firpo and Possebom 2015).

In practice, let X_1 be the vector of outcome variable and covariates of the treated country in the pre-treatment period and X_0 the corresponding matrix including the same variables as in X_1 for each control country. The distance between treated outcome and covariates and control outcome and covariates is then given by the vector $\|X_1 - X_0 W\|$ and the vector $\widehat{W}$ is chosen to minimize the distance

$$\|X_1 - X_0 W\| V = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)} \tag{5}$$

where V is a $(K \times K)$ diagonal positive semidefinite matrix whose trace equals one. Intuitively, W is a weighting vector that measures the relative importance of each country in the donor pool[10] and V measures the relative importance of each covariate and the outcome variable.

[10] Donor pool refers to the set of potential control countries, which are used to construct the synthetic control country (Abadie, Diamond, and Hainmueller 2010).

It is standard practice to estimate the root mean square prediction error (RMSPE) as a goodness of fit measure to evaluate the discrepancy between treated and synthetic control outcomes, i.e. how good the first part of equation (3) holds. $RMSPE$ is defined as

$$RMSPE = \left[\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{1,t} - \sum_{j=2}^{J+1}\widehat{w}_j Y_{j,t}\right)^2\right]^{\frac{1}{2}} \tag{6}$$

while the choice of the covariates (Z_i) can be justified by including those variables which better explain the outcome (Y_i) the choice of the inclusion of the pre-treatment outcome variable can influence V and the $RMSPE$. Abadie, Diamond, and Hainmueller (2010) proposes to include all pre-treatment outcome variables, however, this approach was criticised by Kaul et al. (2018) showing that the inclusion of all pre-treatment outcome variables overshadows all other covariates. Following, Ferman, Pinto, and Possebom (2016) the preferred specification is the one with the lowest pre-treatment $RMSPE$. I estimate the pre-treatment $RMSPE$ for different combinations of included outcome variables and on average the lowest $RMSPE$ is achieved if the average of the outcome variable is included (see Table A 2 in the Appendix). In the main specification, I include always the pre-treatment average of the outcome variable to construct the synthetic control country.

So far, the SCM estimator considered in equation (4) only deals with the single treatment case but this approach can be extended to allow for multiple treatment countries (Cavallo et al. 2013). With multiple treatment countries, assume that there are $G \in N$ interventions. For each intervention $g \in \{1, \ldots, G\}$, there are $J^g + 1$ observed countries and denote the country with the intervention as 1^g. In the same manner as in equation (4) define the synthetic control estimator of $\alpha_{1^g,t}$ as

$$\hat{\alpha}_{1^g,t} = \frac{\sum_{g=1}^{G}\hat{\alpha}_{1^g,t}}{G} \tag{7}$$

for each $t \in \{1, \ldots, T\}$.

Intuitively, the extension for multiple treatment countries means that first a synthetic control country is created for each treatment country out of the donor pool. Second, results are derived from the difference between treatment and synthetic control country for each treatment country. Finally, the results are pooled together to calculate the average treatment effect for each post-treatment period $(T_0 + 1, \ldots, T)$.

The explained procedure results in a scaling effect because SCM compares the path of the outcome variable. The country-specific effect will depend on the level of the outcome variable, i.e. the same change in total tax per capita is more important in a country with low total tax per capita. To avoid this scale effect, Cavallo et al. (2013) proposes to normalize the estimates before pooling the country-specific results. Normalization is achieved by setting total tax per capita equal to one for each treatment country in the year before the treatment starts (T_0).

3.3 Identifying Assumptions

The main assumption of the SCM is the 'independence conditional on past outcome' assumption meaning that the weighted average of the control countries replicates the treatment country in the pre-treatment period and behave in the post-treatment period the same way as the treatment country would have in the absence of the treatment (Firpo and Possebom 2015). For this to be true certain assumptions must be made in this setting which will be explained here.

First, it must be established that the event affects only the treatment countries but not the donor pool or that the event affects all countries the same way except for one mechanism. For policy changes this assumption is easier to defend than in the case of the 2000s commodity price boom because all countries in the world are affected by the price boom in one way or another. While only some countries produce oil, all countries in the world consume it. Therefore, the identification in this paper rests on the second part of the assumption, namely that the effect is everywhere the same with one exceptional mechanism.

The exceptional mechanism is that the government in oil exporting countries receive more revenues, which it can use to lower taxes in the non-oil sector. This mechanism, called here 'oil transfer', connects the oil sector with the non-oil sector through the government and does not exist in non-oil producing countries because they do not possess an oil sector. Therefore, the strategy is that by comparing an oil exporting country with a synthetic control country, constructed out of non-oil producing countries, it is possible to capture the 'oil transfer' mechanism.

Let's assume two countries, an oil exporting country and a non-oil producing country. The whole economy in a non-oil producing country consists of a single non-oil sector and the government. Government and economy interact through taxes, regulations, laws, etc. The economy of the oil exporting country can be divided into government, non-oil sector and oil sector. Again, oil and non-oil sector interact with the government through taxes, laws, regulations, etc. Hence, the only difference is the existence of an oil sector in one of the two countries.

A positive price shock now, as it occurred in the 2000s, has several indications for the two countries. First, the non-oil sector in both countries must pay a higher price for oil, which leads to additional revenues for the oil sector. In turn, the oil sector payments to the government in form of taxes, royalties or production sharing agreements increase. This additional government income can now be redistributed to the non-oil sector by the government in the form of lower taxes or increased spending. Without this 'oil transfer' mechanism the impact in the non-oil sector; higher price, inflation, and less economic growth would be the same in both countries because both non-oil sectors must pay higher prices for oil. The only difference is the 'oil transfer' mechanism which only occurs in the oil exporting country. The analysis, therefore, compares the non-oil sector of an oil exporting country with a synthetic control non-oil sector. The synthetic control non-oil sector is constructed out of a weighted average of non-oil sectors and government characteristics from a donor pool of non-oil producing countries.

The 'oil transfer' mechanism consists of all the possibilities a government faces when confronted with additional income from a positive price shock. This means they can use it to spend on public goods, save it, steal it, and finally substitute it for non-oil taxes. The latter is the mechanism of interest in this paper and controls are included for the remaining channels in all specifications.

For the 'independence conditional on past outcome' assumption to hold I have to assume that non-oil sectors in non-oil producing countries are affected in the same way as the non-oil sector in the oil exporting country and the only difference is the 'oil transfer' mechanism through the government. This further implies that spill overs between oil sector and non-oil sector in oil exporting countries do not exist or are negligible. This is important to ensure that the only mechanism captured by SCM is the 'oil transfer' mechanism. In the case of oil exporting countries this assumption is reasonable. The 'enclave' character of the oil industry in combination with its high capital intensity fosters only few linkages to the rest of the economy and contributes little to create employment (Karl 2007).

Finally, two further assumptions must hold. The first is that the effect is everywhere the same, which is given because the oil price increased everywhere the same. Second, spill overs between countries in the outcome variable do not exist. Countries usually are in competition and tax policy can be a determinant for a company on where to settle. However, this tax competition is more prone between oil-poor countries. Nevertheless, if oil-rich countries' tax policies influence non-oil producing countries this would minimize the estimates because the effect is defined as the difference between oil exporters and non-oil producers. If this biases the results, then the true effect would be greater than what is estimated here.

4. Sample, data, and descriptive statistics

Treatment countries and donor pool

The treatment group includes countries who are net oil exporter and produce a significant amount of oil before the 2000s commodity price boom. The treatment countries have to be net exporters because selling the commodity domestically would not qualify as an external windfall following the rentier state theory (Beblawi 1990). Further, they must produce a significant amount of oil before the event to be sure that they are affected by the price boom. Significant amount is defined by producing economic rents from natural resources exceeding 5% of GDP on average in the pre-treatment period.[11] Conditional on data availability I identified 19 countries that comply with the condition mentioned above (see Error! Reference source not found.).

Table 1: Treatment countries

Country	Oil rent (%)	non-res. Tax p.c. (US-$)	Polity2	Ownership	Oil dependency	tax to non-tax ratio
Algeria	9.5	396	-3	State	high	non-tax
Azerbaijan	14.8	144	-7	State	medium	non-tax
Brunei	18.6	1007	0	State	high	tax
Cameroon	5.7	106	-4	Private	medium	non-tax
Ecuador	5.8	288	9	State		non-tax
Egypt	8.8	231	-6	State	medium	non-tax
Gabon	27.7	1718	-4	Private	high	tax
Indonesia	5.1	182	-5	State	medium	tax
Kazakhstan	5.1	609	-4	Private		tax
Kuwait	29.5	583	-7	State	high	non-tax
Libya	22.7	746	-7	State	high	non-tax
Malaysia	4.9	904	3	State	low	tax
Norway	5.2	23300	10	State	low	tax
Saudi Arabia	27.5	416	-10	State	high	non-tax
Syria	19.6	261	-9	State	high	non-tax
Trinidad and Tobago	10.3	1730	10	Private	low	tax
Turkmenistan	33.7	132	-9	State	low	tax
Venezuela	14.7	1024	8	State	medium	tax
Yemen	23.6	100	-2	Private	high	non-tax

Notes: Oil rent is in percent of GDP and values are averages for the pre-treatment period (1989-1998). The remaining variables are measured in 1998, the year prior to the event. Non-resource total tax p.c. is measured in constant 2010 US$ from GRD dataset. Polity2 is the polity index from Marshall and Jaggers (2014). Oil dependency measures the percentage share of government revenues derived from the oil sector (low<20%, medium 20-40%, high>40%). Tax to non-ratio measures oil tax revenues divided by oil non-tax revenues (Tax is a ratio>1, non-tax is a ratio<1).

The pre-treatment period includes, wherever possible, all 10 years prior to the price boom. Because of data issues (gaps) and idiosyncratic shocks (e.g. civil war or conflict) the pre- and post-treatment

[11] Malaysia is also considered a treatment country even so average oil rent is only 4.9%.

period had to be adjusted or missing data has been interpolated for some treatment countries. Table A 3 in Appendix A lists all adjustments.

Each treatment country has an individually assigned donor pool. For a country to be eligible to be part of one or more donor pools it must be from the same region and a non-resource producer.

Restricting each country to the same region ensures that countries have a similar background and are more like each other in economic and cultural aspects. This also avoids interpolation bias which occurs when two extreme countries average out to match the treatment country resulting in the possibility that observed effects simply represents differences in the countries' characteristics (Abadie, Diamond, and Hainmueller 2015). The region restriction had to be lifted for Gabon and Libya. Otherwise, the pre-treatment fit would not have allowed to include them in the analysis.

Donor pool countries must be non-resource producers to comply with the model as explained above. The definition here is widened to resource producers and not merely oil producers, i.e. resource includes mineral and precious mineral producers. This wider definition is applied because the price for minerals and precious minerals also increased significantly in the 2000s and including them in the donor pool would bias the results downward. Furthermore, it must be considered that there are only few countries in the world producing no resources and therefore the definition is adjusted to be relative to the economic size of a country. Non-resource producers are defined as countries generating resource rents less than 1% of GDP to make sure that the amount is negligible and does not represent an important source of government revenues. Donor pools and weights for each treatment country can be seen in Table A 4 in Appendix A.

Data

The dependent variable, *total tax p.c.*, measures total non-resource per capita taxes[12] and is derived from the Government Revenue Dataset (GRD), which was created by the International Centre for Tax and Development (ICTD). GRD provides information about government revenues and its components for most countries of the world for the time period 1980 to 2015 as percentage of GDP (Prichard, Cobham, and Goodall 2014).

The main advantage of the GRD data is that they distinguish between revenues and taxes generated from the resource sector and revenues derived from the remaining economy. Without this distinction, the analysis would be flawed by the fact that resource extracting companies also pay taxes to the government.

[12] Excluding social contribution.

The GRD data are stated in percentage of GDP and to obtain the per capita values the variables were multiplied by GDP figures from the World Economic Outlook measured in constant 2010 US$[13] and divided by population data from the World Development Indicators.[14] *Total tax p.c.* ranges within the treatment countries from less than US$ 100 in Cameroon and Yemen to over US$ 21,000 in Norway (Table 1). Norway is an outlier considering that *total tax p.c.* is 13 times higher than the second highest observed *total tax p.c.* (Trinidad and Tobago US$ 1730). Because of Norway's extreme value a robustness check was carried out excluding Norway from all specifications in section 6.

The predictor variable chosen to construct the synthetic control countries are derived from the identifying assumption explained above. The first variable is the average of the non-resource *total tax p.c.* in the pre-treatment period. The average of the outcome variable rather than a combination of lags was chosen because it resulted in the lowest pre-treatment RMSPE (see Appendix A Table A 2). The second variable is *non-resource GDP p.c.*, which ensures that the non-resource sectors in the treatment and synthetic control country are of similar size. The third set of variables consists of all alternatives a government faces when confronted with additional resource income. They include government spending measured as *capital formation* and *current government expenditure* as well as *reserves* in case the government saves the money and *corruption* to control for stolen funds. Definition and source of each variable used is provided in Table A 5 and Table A 6 shows summary statistics for the whole sample.

Comparing the treatment countries with non-resource producing countries shows that they are on average of a different nature. Table 2 shows descriptive statistics comparing oil exporters with non-resource producers.

The t-statistic shows that citizens in oil exporting countries pay fewer *total tax per capita* and have a lower *non-resource GDP*. The oil exporters have a higher *government expenditure* and *capital formation* and are more *corrupt*, while they receive significantly less *ODA*, have higher *inflation* and are more autocratic. There is no significant difference in terms of *reserves* and *agriculture*.

[13] The World Economic Outlook does not directly provide GDP in constant US$. I followed the proposed way by the WEO to calculate the GDP in constant US$ series (https://www.imf.org/external/pubs/ft/weo/faq.htm#q3a).

[14] WEO also provides population data but WDI are preferred over WEO because they are more precise and cover more countries and years. The exception is Kuwait from 1992 to 1994 where WEO data was available but WDI population data are missing.

Table 2: Descriptive statistics

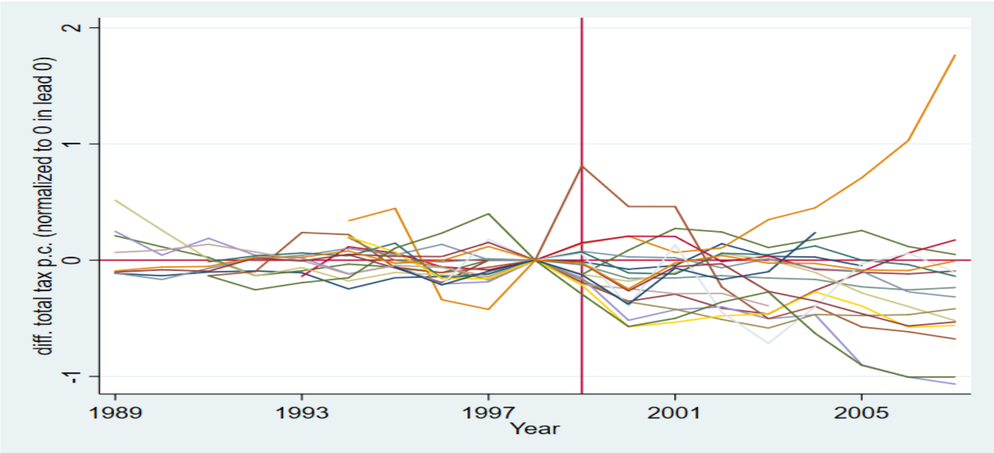
	Non-resource producers	Oil exporters	Diff.	t-stats	p-value
Total tax p.c.	3651.25	1115.97	2535.28	5.756	0.000
Non-resource GDP p.c.	11426.37	4977.02	6449.35	5.998	0.000
Gov. Expenditure	15.84	16.96	-1.12	-1.960	0.050
Capital formation	22.48	23.89	-1.41	-2.083	0.038
Reserves	13.68	13.72	-0.04	-0.033	0.974
Corruption	0.52	-0.43	0.96	5.932	0.000
Agriculture	14.43	12.83	1.59	1.381	0.168
ODA	6.03	1.98	4.05	5.141	0.000
Inflation	11.05	95.23	-84.18	-5.074	0.000
Polilty2	5.04	-2.04	7.08	11.772	0.000

Notes: Means calculated for 10 years prior to the 2000s commodity price boom. Government expenditure, capital formation, reserves, agriculture, and ODA is measured in percent of GDP.

5. Results

Figure 5 shows the results for the synthetic control method. Each line represents one country and shows the difference in non-resource *total tax p.c.* between treatment and synthetic control country over time. The differences are scaled to 0 in the year prior to the treatment and the vertical red line indicates the start year of the commodity price boom, 1999.

Figure 5: Synthetic control method, all countries



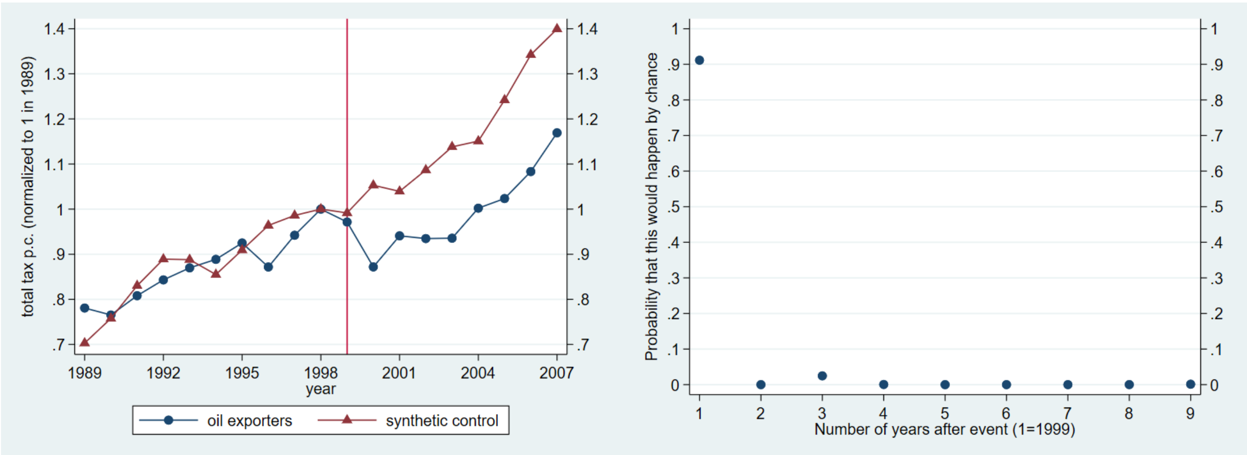
The vast amount of lines in Figure 5 makes it almost impossible to identify a single country but by laying all countries into one figure it is still possible to derive some information. The first indicative result is that more countries have a negative effect, i.e. *total tax p.c.* decreased because of the commodity price boom. The second indicative result is that the effect is heterogeneous. There are as many countries with seemingly no effect as there are with a negative effect and even a few countries seem to have increased taxes, which would be the opposite of a crowding-out effect. The biggest effect is even positive in Azerbaijan.

Average treatment effect

The first indicative result derived from Figure 5, the existence of a negative average treatment effect, is confirmed in Figure 6 (left panel). Figure 6 shows average *total tax p.c.* for the 19 oil exporters and the corresponding synthetic control. The oil exporters and its synthetic counterfactual overlap well in the pre-treatment period and start to diverge in the post-treatment period. *Total tax p.c.* is lower for the whole treatment period for this sub-sample compared to their synthetic counterpart. Tax revenues seem to fall straight after the price boom started, but the downward trend is reversed after two years and *total tax p.c.* starts to recover. In the year 2000, *total tax p.c.* is 13% lower than it was in 1998 and

17% lower than in the synthetic control country in the same year.[15] It takes overall six years for the oil exporting countries to get back to their initial 1998 level of *total tax p.c.* and by this time, they still lack 13% behind the synthetic control country. The p-values indicate that the effect is significant in all years except for year one.

Figure 6: Synthetic control estimates, oil exporting countries



The first indicative result from Figure 5 - negative average treatment effect - is therefore confirmed in the data and in line with the existing literature finding a crowding-out effect (Bornhorst, Gupta, and Thornton 2009; Crivelli and Gupta 2014; Thomas and Treviño 2013). The second indicative result, that the effect is heterogeneous, will be discussed and analysed further below.

I next split the sample according to institutions, ownership structure of the oil sector, oil tax to oil non-tax ratio, and oil dependency of the state budget. See Table 1 for an overview which oil exporting country belongs to which category. The classification is according to 1998 values, the year before the oil boom started.

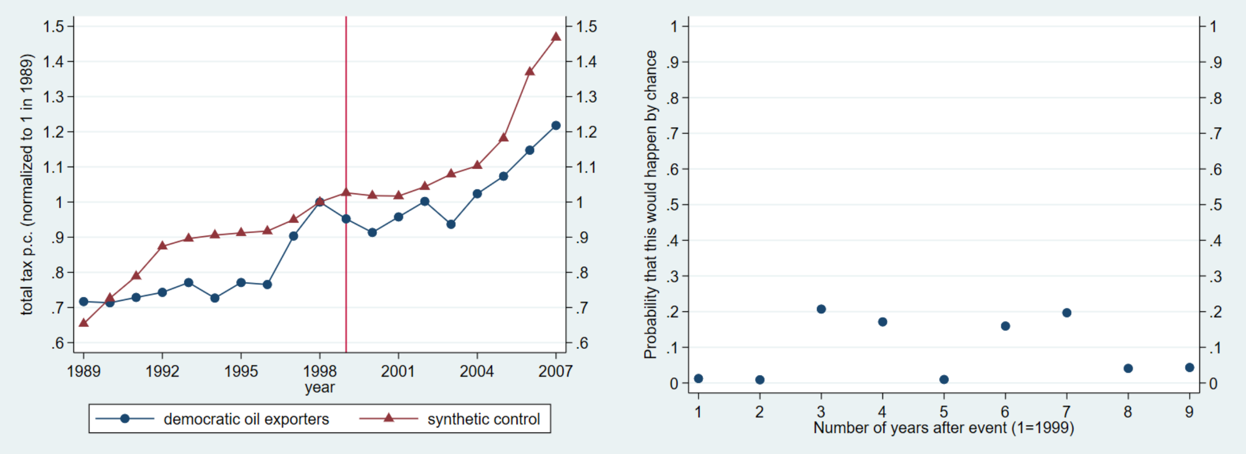
Institutions

Some scholars claim that the adverse effects of natural resources on economic and political outcomes are conditional on the level of institutional quality (Bhattacharyya and Hodler 2010, 2014; Mehlum, Moene, and Torvik 2006; Robinson, Torvik, and Verdier 2006). Therefore, it is worthwhile to explore this potential source of heterogeneity. To test for conditionality, I use the Polity2 score from Marshall, Gurr, and Jaggers (2013) which measures democracy on a 21 points scale ranging from -10 (autocratic institutions) to +10 (democratic institutions). I split the sample into democratic countries (Polity2: +4 - +10), intermediate countries (Polity2: -3 - +3) and autocratic countries (Polity2: -4 - -10).

[15] Predicted values for the synthetic control and actual values for each sub-sample and year are shown in Table A 7 in the appendix and Error! Reference source not found. discussed below shows the difference in percentage terms.

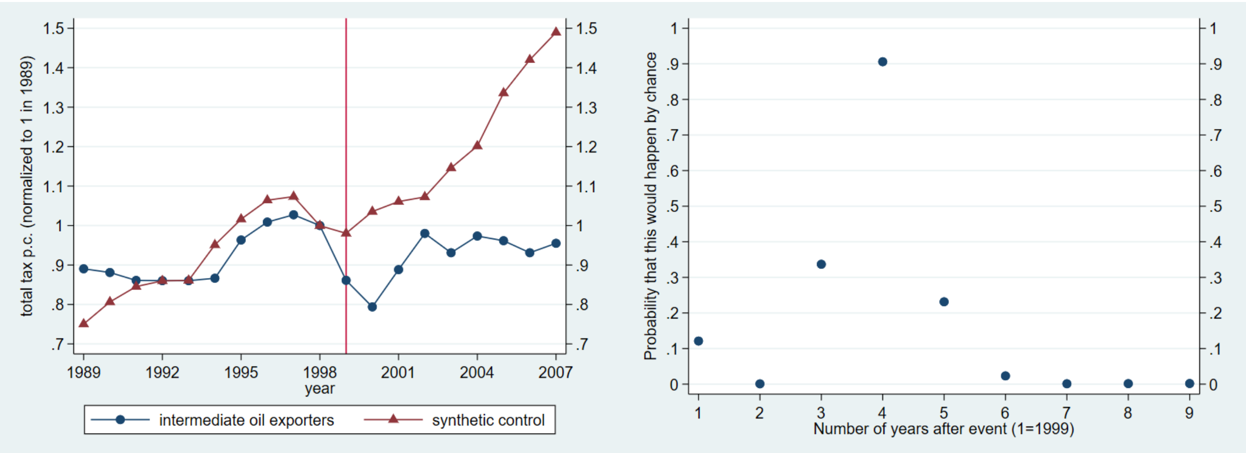
Figure 7 shows that the oil price boom did not influence *total tax p.c.* in democratic countries. Differences between democratic oil exporters and synthetic control are likely to happen by chance, out of the nine p-values four and therefore almost half of them are above the commonly used 10% threshold.

Figure 7: Synthetic control estimates, democratic oil exporting countries



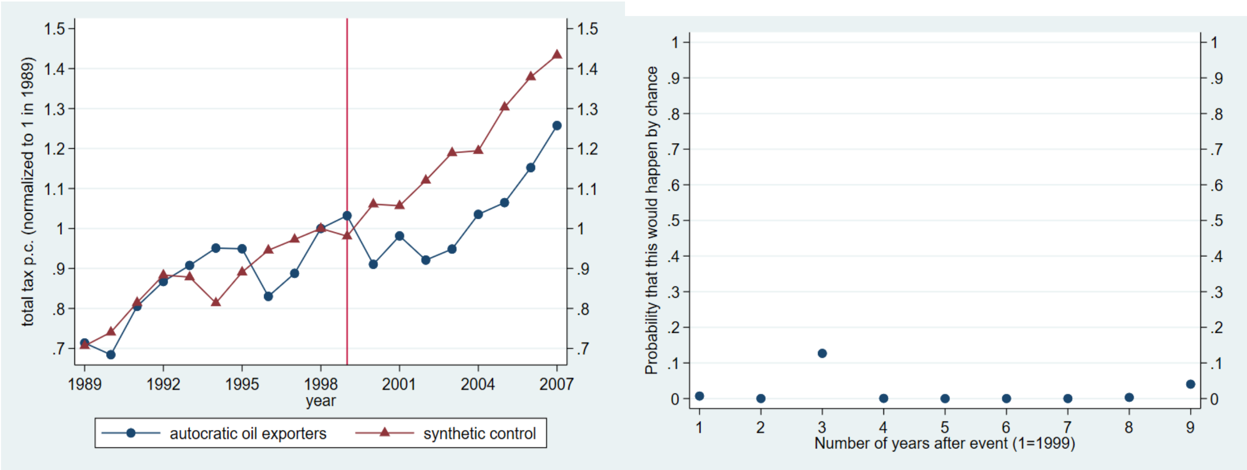
Countries with an intermediate level of institutional quality (Polity2 score between -3 and +3) show mixed results. In Figure 8, the synthetic control estimates for intermediate countries seem to indicate a *total tax p.c.* decreasing effect. In the year 2 and the years 6-9 the p-values also indicate that this effect is significant. However, for the remaining years the effect is insignificant. Therefore, not a clear pattern of a crowding out effect can be observed.

Figure 8: Synthetic control estimates, intermediate oil exporting countries



The case for autocratic countries is clearer. Figure 9 shows the result and the *total tax p.c.* decreasing effect is significant at conventional levels in all years except for year 3 after the event (9/11 terrorist attacks). On average autocratic oil exporting countries collect 13% less *total tax p.c.* than their synthetic counterparts.

Figure 9: Synthetic control estimates, autocratic oil exporting countries



The heterogeneous results for countries with different levels of institutional quality could be explained by Garcia's argument that democratic institutions are more expensive (Garcia and von Haldenwang 2015). Participation, redistribution, and more public goods require more funds and therefore the 2000s oil price boom perhaps was not enough to decrease non-resource *total tax p.c.* in democracies. Further, the results support the argument of a conditional resource curse[16] and under this condition confirms the 'tax effect' argument from Mahdavy (1970) and (Ross 2015a).

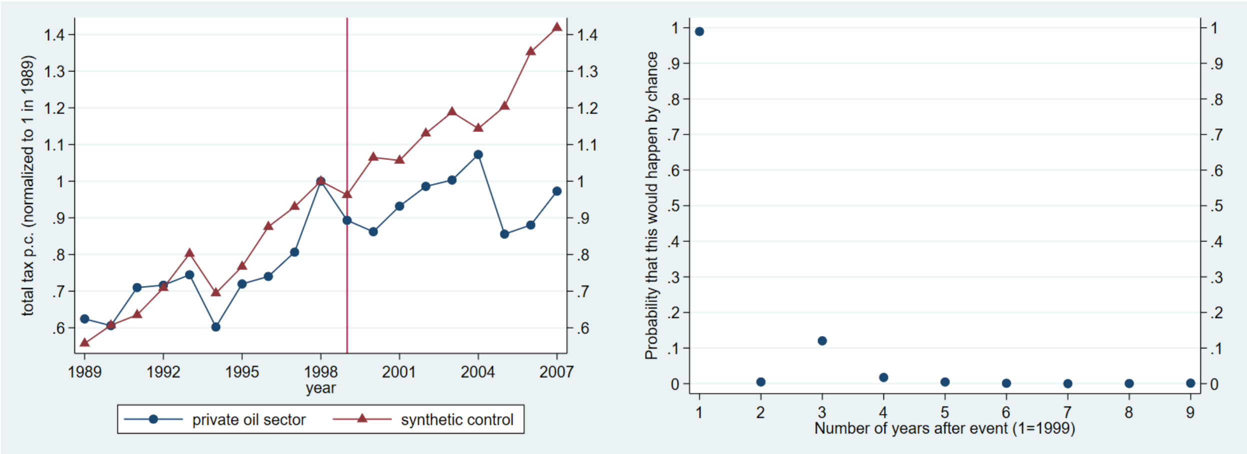
Ownership structure

The ownership structure of the oil sector could also influence taxation of the remaining economy. A private oil sector needs a sophisticated tax administration to extract a fair share. This oil division within the tax administration could have positive spill overs on the rest of the tax administration. The positive spill overs could lead to a more efficient tax administration and to more tax income (Knebelmann 2017). A nationalized oil sector, on the other hand, is often organized under the direct authority of the leader or in a form that the leader has access to its funds. The easy access to oil revenues could lead the government to neglect income from the remaining economy and reduce taxation.

Figure 10 shows the results for countries with a private organized oil sector. Overall a *total tax p.c.* decreasing effect can be observed. However, the effect starts small and becomes stronger after 6-7 years. From 1999 till 2004 the difference is on average 12% and from 2005 till 2007 the oil exporting countries collect on average 32% less *total tax p.c.* compared to their synthetic counterparts.

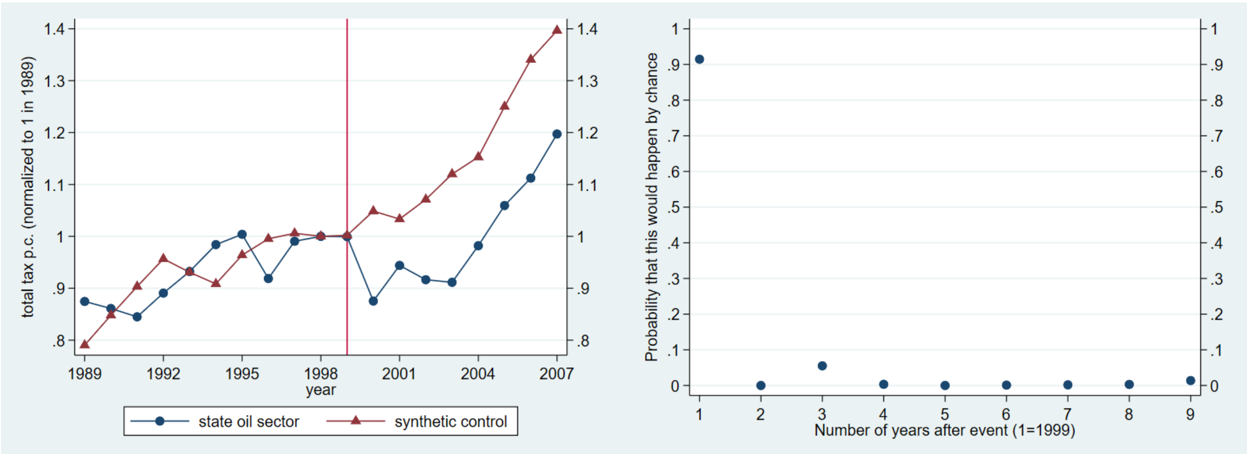
[16] See Liou and Musgrave (2014) for an overview of conditional resource curse research.

Figure 10: Synthetic control estimates, private oil sector



The results for countries with nationalised oil sector show a smaller and partly less significant effect (see Figure 11). Countries with nationalised oil sector collected on average 13% less *total tax p.c.* than their synthetic counterparts.

Figure 11: Synthetic control estimates, nationalised oil sector



The results for private- and state-owned oil sectors are indicative that governments intending to extract more revenues from a private oil company faces more challenges and longer negotiation time to increase their share of the oil revenues, which then can be used to substitute for non-resource taxes. Countries with nationalised oil sector benefit immediately but to a lesser extent perhaps any potential of increasing the government share has been exhausted prior to the windfall.

Fiscal instruments

Another form of government preference that may influence the relationship between oil revenues and *total tax p.c.* could be the instruments chosen to extract revenues from the oil sector. To test for this possible source of heterogeneity I construct a tax to non-tax ratio for each oil exporter in the year before the event. A ratio smaller one indicates that the government extracts more revenues with non-tax instruments while governments with a ratio greater than one use more tax instruments.

The effect in non-tax countries (tax to non-tax ratio <1) is small and mostly insignificant (Figure 12). While tax countries (tax to non-tax ratio >1) have a *total tax p.c.* reducing effect, significant at conventional level for all years (Figure 13). On average tax countries collect 22% less tax than the synthetic control.

Figure 12: Synthetic control estimates, tax to non-tax ratio<1

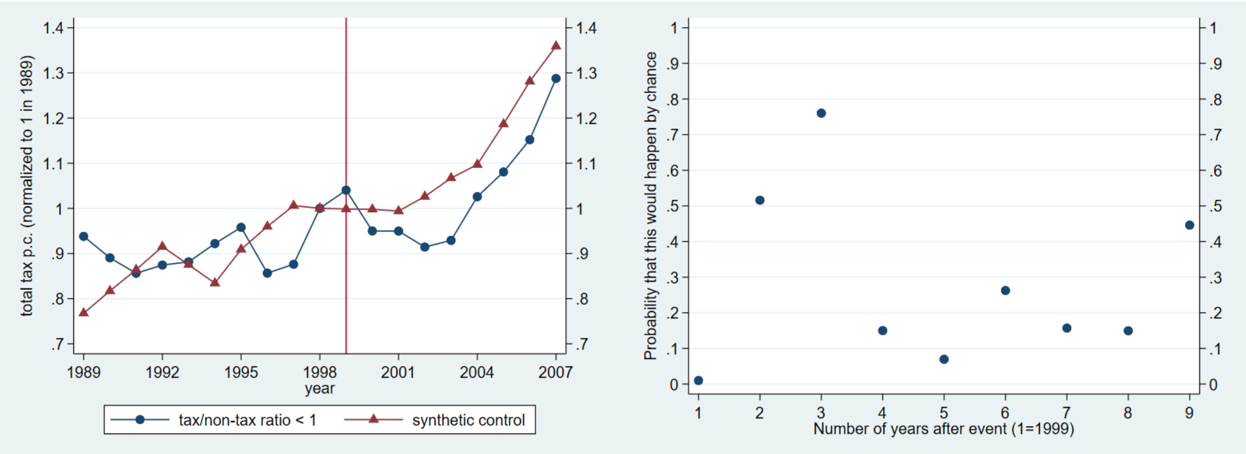
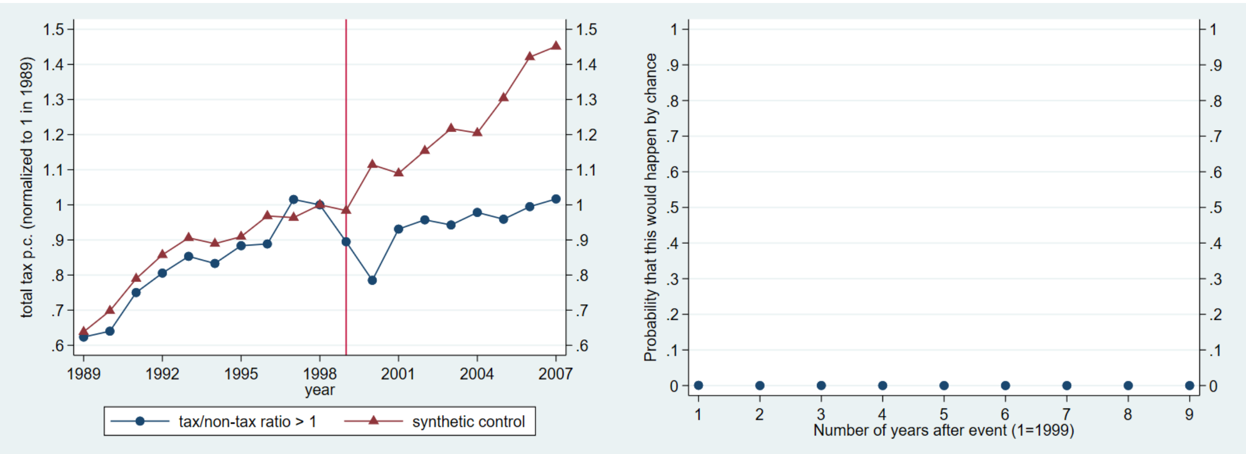


Figure 13: Synthetic control estimates, tax to non-tax ratio>1



The heterogeneity between tax and non-tax countries could be driven by the fact that tax countries collect on average more taxes (US$ 913 per capita)[17] compared to non-tax countries (US$ 327 per

[17] Excluding Norway, with Norway average non-resource *tax per capita* would be US$ 3400.

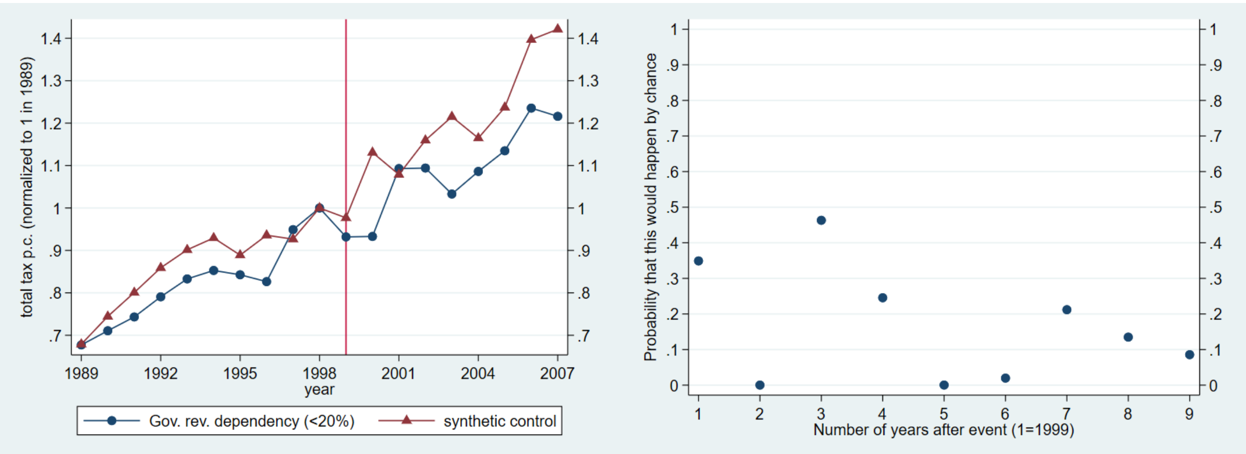
capita).[18] The higher tax income gives tax countries more room to reduce taxes while non-tax countries' possibilities are already exhausted, eventually because they never build them.

Oil dependency

A last type of heterogeneity is the level of oil dependency of the state budget. Oil dependency is also an inverse proxy for tax effort, i.e. more reliance on oil revenues implies less effort in the non-oil sector. The sample is divided into three categories according to the percentage share of oil revenues of total government revenues in the year prior to the event. Countries are categorised as low oil dependent if less than 20% of government revenues are collected from the oil sector, oil revenues for medium-dependent countries range from 20-40% of total government revenues, and high-dependent countries collect more than 40% of total revenues from the oil sector.

Figure 14 and Figure 15 show results for low and medium dependent countries. For most years the estimates are insignificant. Note that for the first time oil exporting countries have a higher *total tax per capita* in the case of medium-dependent countries even if significant in only one year.

Figure 14: Synthetic control estimates, low oil dependence (less than 20%)



[18] Average *total tax p.c.* values refer to the year 1998.

Figure 15: Synthetic control estimates, medium oil dependency (20-40%)

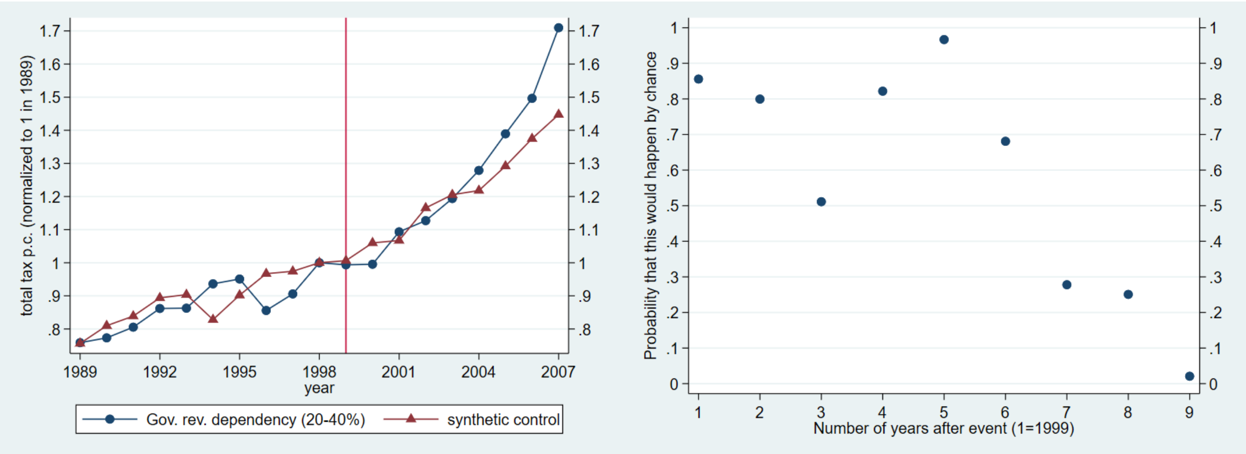
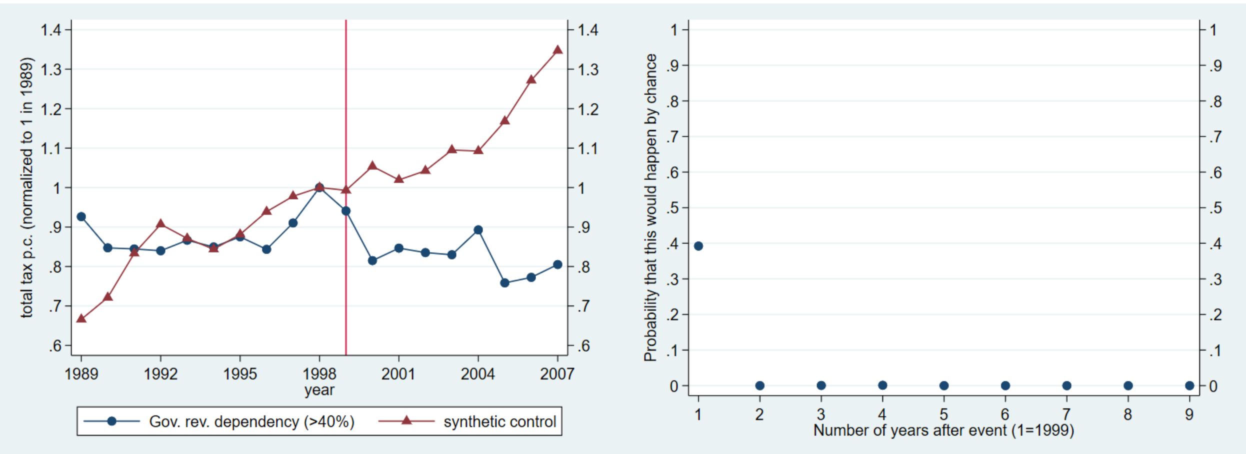


Figure 16 shows the result for high-dependent oil exporting countries. Countries with a state budget deriving more than 40% of its funds from the oil sector show a negative effect, i.e. *total tax p.c.* is on average 25% lower in oil exporting countries due to the 2000s oil price boom.

Figure 16: Synthetic control estimates, high oil dependency (more than 40%)



The results of the heterogeneity analysis seem to indicate that autocratic countries with a focus on tax revenues or a high share of oil revenues in the state budget are confronted with a crowding-out effect. *Total tax p.c.* is lower in those oil exporting countries because of a positive resource income shock.

The size of the crowding-out effect

Finally, I analyse the size of the crowding-out effect in monetary terms. Table 3 shows the percentage and absolute difference between treatment and synthetic control countries for each significant sub-sample.[19] The synthetic control country is chosen as base in each year and the percentage difference shows how many percent the treatment countries' *total tax p.c.* is higher or lower than *total tax p.c.* of the synthetic control.

Column (1) and (2) shows the results for all oil exporting countries and corresponds to the results shown in Figure 6. On average, individuals and companies in oil exporting countries paid 14% less tax each year or US$ 299 per capita. For example, Saudi Arabia with an average population of 24.4 million loses on average US$ 7.3 bn each year. This is only US$ 2.05 bn short of what the Saudi's government expects to generate from the 5% VAT introduced in 2018.

The sub-sample of autocratic countries (column (3) and (4)) shows a smaller absolute effect. The synthetic control countries collect on average US$ 73 (13%) more in *total tax p.c.* than the actual autocratic oil exporting countries. For example, Cameroon, and Syria both with similar population size (17.8 and 18.7 million, respectively) lose US$ 1.3 bn and US$ 1.4 bn on average each year. Comparing oil exporting countries with state or private organized oil sector shows that the overall effect is greater for countries with a state oil sector, US$ 195 against US$ 333 respectively.

The biggest effect can be seen for countries with a tax to non-tax ratio greater than one. Those are countries focusing more on tax instruments to extract money from the resource sector than on non-tax instruments. Overall, individuals and companies in those countries pay on average US$ 937 less in *total tax p.c.* due to the oil price boom.

Concluding, the average yearly increase in oil price of 16% from 1999 to 2007 reduced *total tax per capita* on average between US$ 73 and US$ 937 per capita depending on the sub-sample.

[19] The significant sub-samples are all, autocratic, private, state, tax to non-tax ratio $>$ 1 and highly oil dependent oil exporting countries.

Table 3: Difference in total tax per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	All oil countries		**Autocratic oil countries**		**Private oil sector**		**State oil sector**		**Tax to non-tax ratio>1**		**High oil dependent**		**Oil price**	
Year	diff. in %	diff. in US-$	diff. in %	diff. in US-$	diff. in %	diff. in US-$	diff. in %	diff. in US-$	diff. in %	diff. in US-$	diff. in %	diff. in US-$	in US-$	% change
1999	-2%	-36	5%	24	-7%	-59	0%	-5	-9%	-302	-5%	-34	19	
2000	-17%	-323	-14%	-70	-19%	-173	-17%	-367	-30%	-1119	-23%	-156	29	54%
2001	-9%	-176	-7%	-35	-12%	-106	-9%	-189	-15%	-540	-17%	-113	24	-17%
2002	-14%	-271	-18%	-93	-13%	-123	-14%	-327	-17%	-669	-20%	-136	24	0%
2003	-18%	-361	-20%	-112	-16%	-158	-19%	-441	-23%	-932	-24%	-173	27	12%
2004	-13%	-265	-13%	-74	-6%	-61	-15%	-361	-19%	-770	-18%	-131	34	29%
2005	-18%	-389	-18%	-111	-29%	-297	-15%	-403	-26%	-1174	-35%	-268	47	38%
2006	-19%	-462	-16%	-106	-35%	-403	-17%	-484	-30%	-1448	-39%	-326	55	17%
2007	-16%	-410	-12%	-82	-31%	-380	-14%	-422	-30%	-1477	-40%	-354	60	9%
Sum		-2693		-660		-1759		-2999		-8431		-1691		
Av.	**-14%**	**-299**	**-13%**	**-73**	**-19%**	**-195**	**-13%**	**-333**	**-22%**	**-937**	**-25%**	**-188**		**16%**

Notes: The table shows the difference in *total tax per capita* between oil exporting countries and synthetic control country. The synthetic control country serves as base value and percentage differences are calculated accordingly. The Oil price is in constant 2000 US$ from (Ross 2013)

6. Robustness checks

I conduct a couple of robustness checks dealing with Saudi Arabia, Norway, and additional predictor variables.

I start by excluding Saudi Arabia from the specifications. As discussed in section 2, Saudi Arabia is a swing producer and may have enough market power to influence the oil price. If Saudi Arabia changed oil production in the considered time period influencing the 2000s oil price boom this would lead to endogeneity. Results for the synthetic control estimates excluding Saudi Arabia are shown in Figure B 1 in the appendix and the results survive.

Next, I include further predictor variables to construct the synthetic control. So far, the analysis only included the variables derived from the identifying assumption: *total tax p.c., non-resource GDP, public expenditure, capital formation, reserves,* and *corruption*. These variables cover all the areas of what the government can do with the additional funds derived from the price boom and ensures that the non-resource economy is of the same size. However, other variables could determine how much tax the government can extract from the non-resource sector such as the sectoral composition, foreign aid, or inflation. For this reason, I include *(value added) agriculture* and *ODA*, both measured as percentage of non-resource GDP and *inflation* successively and combined.[20] Figure B 2 in the appendix shows the results, which remain robust.

Finally, I run a last robustness check excluding Norway from the sample. The fact that Norway's *total tax p.c.* is 13 times higher than the second highest *total tax p.c.* in the sample unfolds the question whether Norway is driving the results. Figure B 3 in the appendix presents robust results.

[20] Because of missing data Norway and Turkmenistan are excluded from this robustness check.

7. Conclusion

This paper uses a novel methodology to shed light on the question whether natural resources (oil and gas) crowd out non-resource tax revenues. The synthetic control method (SCM) confirms the existence of a crowding-out effect and further provides evidence about country characteristics that determine this effect. The average treatment effect shows that the 2000s commodity price boom decreased *total tax p.c.* in oil exporting countries by around 14% compared to a scenario without a price boom. This is roughly a US$ 300 lower tax burden per capita.

Apart from the negative average treatment effect, the SCM also shows that the effect differs according to institutional quality, government preferences in terms of tax instruments, and resource dependency. The crowding-out effect was only observed in high oil dependent countries with autocratic institutions and a preference of extracting funds from the oil sector through tax instruments.

The crowding-out effect should be a concern for policy makers for several reasons. Lower taxes increase the dependency on oil revenues which is a volatile income stream impeding budget planning, non-fiscal objectives become more difficult to achieve, and positive externalities from taxes such as transparency and better governance are averted. Therefore, policy makers should consider ways to improve their tax administration to avoid these negative consequences. One possible way would be to invest the oil dividend directly into the tax administration.

Future research could disentangle the tax data even further and analyse the effect on different tax types, e.g. income, VAT, property, direct, indirect, individual or company tax. This is of interest because it can shed light into the behaviour of governments in oil-rich countries. The burden of different taxes is carried by different parts of the population. Lower property tax, for example, benefits the elite while sales tax or VAT is burdened by the entire population and proportionally affects more the poor.

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Appendix A. Tables

Table A 1: Test for structural breaks in the oil price 1991-2010

Structural breaks in the oil price

Year	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
χ^2	0.697	1.093	1.359	1.908	2.484	2.783	2.661	3.427	**6.775**	**6.236**
p-value	0.706	0.579	0.507	0.385	0.289	0.249	0.264	0.180	**0.034**	**0.044**
Year	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
χ^2	4.321	**6.819**	**9.086**	**12.979**	**17.962**	**13.002**	**11.610**	**10.960**	**7.874**	**8.454**
p-value	0.115	**0.033**	**0.011**	**0.002**	**0.000**	**0.002**	**0.003**	**0.004**	**0.020**	**0.015**

Notes: The table shows the results of the Wald test for structural breaks of a regression of oil price on its lagged value. Bold values indicate significance at the 10% level, i.e. the years in which the null of no structural break could not be rejected.

Table A 2: Pre-treatment RMSPE for different specifications

Country	Average	Last one	Last two	Last three
Algeria	0.2087	0.2305	0.2305	0.2286
Azerbaijan	0.3487	0.4926	0.4926	0.4803
Brunei	0.2146	0.1450	0.1450	0.1450
Cameroon	0.0883	0.2295	0.2295	0.1452
Ecuador	0.1000	0.0995	0.0995	0.1009
Egypt	0.0881	0.0912	0.0912	0.1450
Gabon	0.1391	0.1562	0.1562	0.1332
Indonesia	0.0609	0.1057	0.1057	0.0674
Kazakhstan	0.1200	0.1200	0.1200	0.1200
Kuwait	0.1381	0.1257	0.1257	0.1411
Libya	0.1381	0.1759	0.1759	0.1312
Malaysia	0.0739	0.1039	0.1039	0.0971
Norway	0.0097	0.0208	0.0208	0.0107
Saudi Arabia	0.1183	0.1067	0.1067	0.1251
Syria	0.0913	0.2147	0.2147	0.1981
Trinidad and Tobago	0.1335	0.1866	0.1866	0.1688
Turkmenistan	0.1602	0.2073	0.2073	0.1602
Venezuela	0.0936	0.1073	0.1073	0.1025
Yemen	0.0868	0.1064	0.1064	0.1054
Average	0.1269	0.1592	0.1592	0.1477

Notes: Table shows pre-treatment RMSPEs calculated with different lagged outcome variables. Minimum average RMSPE is achieved by including the average of the outcome variable which is used in the analysis.

Table A 3: Treatment countries, treatment periods and data adjustments

	Pre-treatment period		Post-treatment period		Interpolated years
Country	Start	End	Start	End	
Algeria	1989	1998	1999	2012	
Azerbaijan	1994	1998	1999	2012	
Brunei	1991	1998	1999	2012	
Cameroon	1993	1998	1999	2012	2007
Ecuador	1989	1998	1999	2012	1989
Egypt	1989	1998	1999	2010	
Gabon	1989	1998	1999	2012	
Indonesia	1989	1998	1999	2012	
Kazakhstan	1995	1998	1999	2004	
Kuwait	1994	1998	1999	2012	
Libya	1991	1998	1999	2010	
Malaysia	1989	1998	1999	2008	
Norway	1995	1998	1999	2012	
Saudi Arabia	1994	1998	1999	2012	2005-2007
Syria	1991	1998	1999	2007	
Trinidad and Tobago	1989	1998	1999	2005	
Turkmenistan	1996	1998	1999	2008	1999
Venezuela	1994	1998	1999	2012	
Yemen	1989	1998	1999	2003	1989

Notes: Values for interpolated years are calculated by linear interpolation.

Table A 4: Donor pool and weights

	Treatment countries		
Donor Pool	Venezuela	Ecuador	Trinidad and Tobago
Bahamas	0.0820	0.0000	0.2230
Belize	0.0000	0.0000	0.3560
Guatemala	0.0000	0.1660	0.0000
El Salvador	0.0000	0.5770	0.0000
Costa Rica	0.0000	0.0000	0.1790
Panama	0.0000	0.2570	0.0000
Paraguay	0.6390	0.0000	0.0000
Uruguay	0.2790	0.0000	0.2430

	Treatment countries		
Donor Pool	Cameroon	Algeria	Egypt
Gambia	0.0000	0.0000	0.0770
Central African Republic	0.3630	0.0000	0.2460
Uganda	0.0000	0.0000	0.1490
Kenya	0.5020	0.0230	0.0000
Tanzania	0.0000	0.0000	0.0000
Burundi	0.0000	0.0000	0.0000
Rwanda	0.0000	0.0000	0.0000
Mozambique	0.0000	0.0000	0.0000
Malawi	0.0000	0.0000	0.0000
Swaziland	0.0000	0.3320	0.0570
Madagascar	0.1350	0.0000	0.0000
Morocco	0.0000	0.6440	0.4700

	Treatment countries			
Donor Pool	Saudi Arabia	Kuwait	Indonesia	Azerbaijan
Cyprus	0.0000	0.0000	0.0000	0.0000
Turkey	0.0000	0.0000	0.0000	0.0000
Lebanon	0.0000	0.0000	0.0410	0.0000
Jordan	0.4410	0.9260	0.0000	0.2040
Israel	0.0000	0.0000	0.0000	0.0000
South Korea	0.0000	0.0000	0.0000	0.0000
Japan	0.0000	0.0000	0.0000	0.0000
Bhutan	0.0000	0.0710	0.0000	0.0000
Bangladesh	0.0000	0.0000	0.4300	0.1840
Sri Lanka	0.0000	0.0000	0.0000	0.0000
Nepal	0.0000	0.0000	0.1520	0.4200
Thailand	0.0000	0.0000	0.3780	0.0000
Singapore	0.0000	0.0000	0.0000	0.0000
Philippines	0.5590	0.0030	0.0000	0.1920

continuation from Table A 4

	Treatment countries			
Donor Pool	Yemen	Turkmenistan	Kazakhstan	Syria
Cyprus	0.0000	0.0000	0.0000	0.0000
Turkey	0.0000	0.0000	0.1770	0.0420
Lebanon	0.0880	0.0000	0.0420	0.2580
Jordan	0.0320	0.0000	0.0670	0.0000
Israel	0.0000	0.0000	0.0000	0.0000
South Korea	0.0000	0.0000	0.0000	0.0000
Japan	0.0000	0.0000	0.0000	0.0000
Bhutan	0.0000	0.9100	0.0000	0.0000
Bangladesh	0.8530	0.0000	0.0000	0.5330
Sri Lanka	0.0000	0.0000	0.0000	0.0000
Nepal	0.0270	0.0000	0.0000	0.0000
Thailand	0.0000	0.0900	0.0000	0.0100
Singapore	0.0000	0.0000	0.0000	0.0000
Philippines	0.0000	0.0000	0.7140	0.1570

	Treatment countries	
Donor Pool	Malaysia	Brunei
Cyprus	0.0000	0.0090
Turkey	0.0000	0.6150
Lebanon	0.1430	0.0120
Jordan	0.0000	0.0000
Israel	0.0000	0.0000
South Korea	0.4150	0.1870
Japan	0.0000	0.0000
Bhutan	0.3310	0.1780
Bangladesh	0.0000	0.0000
Sri Lanka	0.0000	0.0000
Nepal	0.0000	0.0000
Thailand	0.1110	0.0000
Singapore	0.0000	0.0000
Philippines	0.0000	0.0000

continuation of Table A 4

Donor Pool	Treatment countries Norway
United Kingdom	0.0000
Ireland	0.0000
Netherlands	0.0000
Belgium	0.0000
Luxembourg	0.0000
France	0.0000
Switzerland	0.1310
Spain	0.0000
Portugal	0.0000
Germany	0.0000
Austria	0.0000
Hungary	0.0000
Italy	0.0000
Finland	0.0000
Sweden	0.0000
Denmark	0.8690
Iceland	0.0000

Donor pool	Treatment countries Gabon
Paraguay	0.3820
Italy	0.1020
Kenya	0.4040
South Korea	0.1120

Donor Pool	Treatment countries Libya
Italy	0.0450
Central African Republic	0.1280
Kenya	0.2720
Lebanon	0.0990
Vanuatu	0.4560

Table A 5: Definition and source of variables used in the analysis

Variable	Definition from the source	Source
Total tax per capita	Total tax per capita are total tax revenues (excluding revenues from the resource sector) divided by population. Values are measured in constant 2010 US$.	Author's calculation with data from ICTD GRD for taxes and World Development Indicators for population
ODA (% of non-resource GDP)	Net official development assistance (ODA) consists of disbursements of loans made on concessional terms (net of repayments of principal) and grants by official agencies of the members of the Development Assistance Committee (DAC), by multilateral institutions, and by non-DAC countries to promote economic development and welfare in countries and territories in the DAC list of ODA recipients. It includes loans with a grant element of at least 25 percent (calculated at a rate of discount of 10 percent).	World Development Indicators
GDP per capita	GDP divided by population, converted to constant 2010 US-Dollar, expenditure approach	IMF World Economic Outlook
Population	Total population is based on the de facto definition of population, which counts all residents regardless of legal status or citizenship. The values shown are midyear estimates.	World Development Indicators
Agriculture, value added (% of non-resource GDP)	Agriculture corresponds to ISIC divisions 1-5 and includes forestry, hunting, and fishing, as well as cultivation of crops and livestock production. Value added is the net output of a sector after adding up all outputs and subtracting intermediate inputs. It is calculated without making deductions for depreciation of fabricated assets or depletion and degradation of natural resources. The origin of value added is determined by the International Standard Industrial Classification (ISIC), revision 3. Note: For VAB countries, gross value added at factor cost is used as the denominator.	World Development Indicators

Inflation	Inflation as measured by the annual growth rate of the GDP implicit deflator shows the rate of price change in the economy as a whole. The GDP implicit deflator is the ratio of GDP in current local currency to GDP in constant local currency.	World Development Indicators
Polity2	Combined Polity Score; measuring on a scale from -10 to +10 the polity of a country.	Centre of Systematic Peace
OPEC	Dummy variable equal one for OPEC member countries	www.opec.org
Tax to non-tax ratio	Government revenues from the resource sector derived by taxes divided by government revenues from the resource sector derived by non-tax instruments	Author calculation with data from ICTD GRD
Resource dependency	Resource revenues as percentage of total government revenues.	ICTD GRD
Nationalised resource sector	Dummy equal one if the resource sector is state owned.	Luong and Weinthal (2010)
Government final consumptio n expenditure (% of GDP)	General government final consumption expenditure (formerly general government consumption) includes all government current expenditures for purchases of goods and services (including compensation of employees). It also includes most expenditures on national defence and security but excludes government military expenditures that are part of government capital formation.	World Development Indicators
Gross capital formation (% of GDP)	Gross capital formation (formerly gross domestic investment) consists of outlays on additions to the fixed assets of the economy plus net changes in the level of inventories. Fixed assets include land improvements (fences, ditches, drains, and so on); plant, machinery, and equipment purchase; and the construction of roads, railways, and the like, including schools, offices, hospitals, private residential dwellings, and commercial and industrial buildings. Inventories are stocks of goods held by firms to meet temporary or unexpected fluctuations in production or sales, and "work in progress." According to the 1993 SNA, net acquisitions of valuables are also considered capital formation.	World Development Indicators

Total reserves (includes gold, % of GDP)	Obtained by dividing “Total reserves (includes gold, current US$)” by “GDP (current US$)”. Total reserves comprise holdings of monetary gold, special drawing rights, reserves of IMF members held by the IMF, and holdings of foreign exchange under the control of monetary authorities. The gold component of these reserves is valued at year-end (December 31) London prices. Data are in current U.S. dollars.	Author calculation with data from Word Development Indicators
Corruption	Control of corruption captures perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as "capture" of the state by elites and private interests.	Worldwide governance indicators
Non-resource GDP	Non-resource GDP is calculated from National accounts data calculating value added and GDP from the production side, published by the UN. NRGDP is defined as total value added minus value added in Mining and Utilities (ISIC C and E).	UN, National Accounts Main Aggregates Database

Table A 6: Summary Statistics

	N	Average	Std.	Min	Max
Total tax p.c.	1377	3483.59	5426.05	8.06	28038.94
Non-resource GDP p.c.	1422	10796.83	13948.14	119.32	77243.10
Gov. Expenditure	1404	15.88	5.80	4.05	76.22
Capital formation	1403	22.58	7.19	-0.69	63.04
Reserves	1391	15.42	16.94	0.01	170.57
Corruption	898	0.24	1.15	-1.52	2.47
Agriculture	1422	13.52	12.76	0.05	53.68
ODA	1335	4.63	8.30	-0.67	71.14
Inflation	1412	18.09	129.28	-10.63	3102.40
Polilty2	1286	3.41	7.01	-10.00	10.00
Tax to non-tax ratio dummy	418	0.45	0.50	0.00	1.00
Resource dependency	681	0.19	0.27	0.00	0.93

Notes: *Total tax p.c.*, *non-resource GDP p.c.* are in constant 2010 US$; *Government expenditure, Capital formation, reserves* are in percent of GDP; *agriculture* and *ODA* is in percent of non-resource GDP.

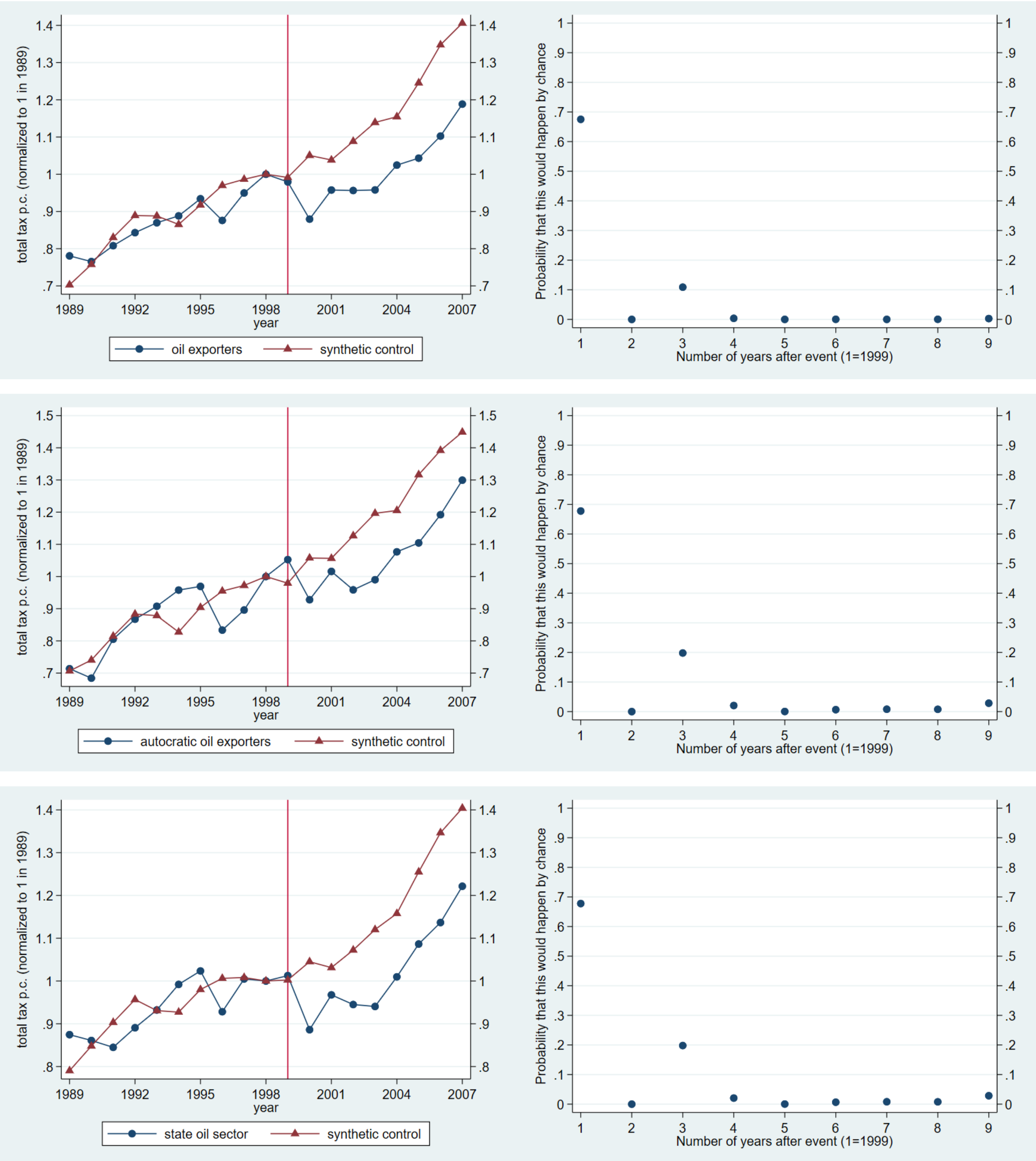
Table A 7: Total tax per capita in treatment and synthetic control countries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All oil countries		**Autocratic oil countries**		**Private oil sector**		**State oil sector**		**Tax to non-tax ratio>1**		**High oil dependent**	
Year	Treatment countries	Synthetic control	Treatment countries	Synthetic control	Treatment countries	Synthetic control	Treatment countries	Synthetic control	Treatment countries	Synthetic control	Treatment countries	Synthetic control
1998	1783.01	1783.20	466.09	466.09	852.58	852.58	2115.30	2115.60	3400.67	3400.67	653.43	653.50
1999	1732.17	1768.02	481.04	457.10	761.65	820.78	2114.02	2119.35	3043.47	3345.62	614.79	648.89
2000	1554.54	1877.36	424.30	494.50	735.11	907.87	1851.53	2218.21	2669.83	3788.69	532.52	688.55
2001	1677.48	1853.26	457.43	492.56	794.55	900.82	1996.82	2185.66	3166.00	3706.17	553.16	666.16
2002	1666.59	1937.67	429.27	522.19	840.56	963.77	1938.52	2265.79	3255.41	3924.12	545.77	681.30
2003	1668.12	2029.17	442.10	554.30	855.27	1013.25	1927.93	2369.27	3205.83	4138.33	542.23	715.57
2004	1786.80	2051.87	482.54	556.75	914.60	975.18	2077.12	2438.48	3327.18	4096.71	583.42	713.95
2005	1824.83	2214.32	496.24	607.43	729.58	1026.47	2240.94	2644.19	3261.49	4435.21	495.65	763.17
2006	1931.54	2393.38	537.06	642.77	750.75	1153.27	2352.78	2836.29	3383.23	4831.68	504.77	831.08
2007	2084.71	2495.11	586.14	668.18	829.49	1209.57	2532.54	2954.27	3457.88	4935.11	526.09	880.27
Av.	**1769.64**	**2068.91**	**481.79**	**555.09**	**801.29**	**996.77**	**2114.69**	**2447.95**	**3196.70**	**4133.52**	**544.26**	**732.10**

Notes: values in constant 2010 US$

Appendix B. Figures

Figure B 1: Robustness check: Excluding Saudi Arabia



continuation of Figure B 1

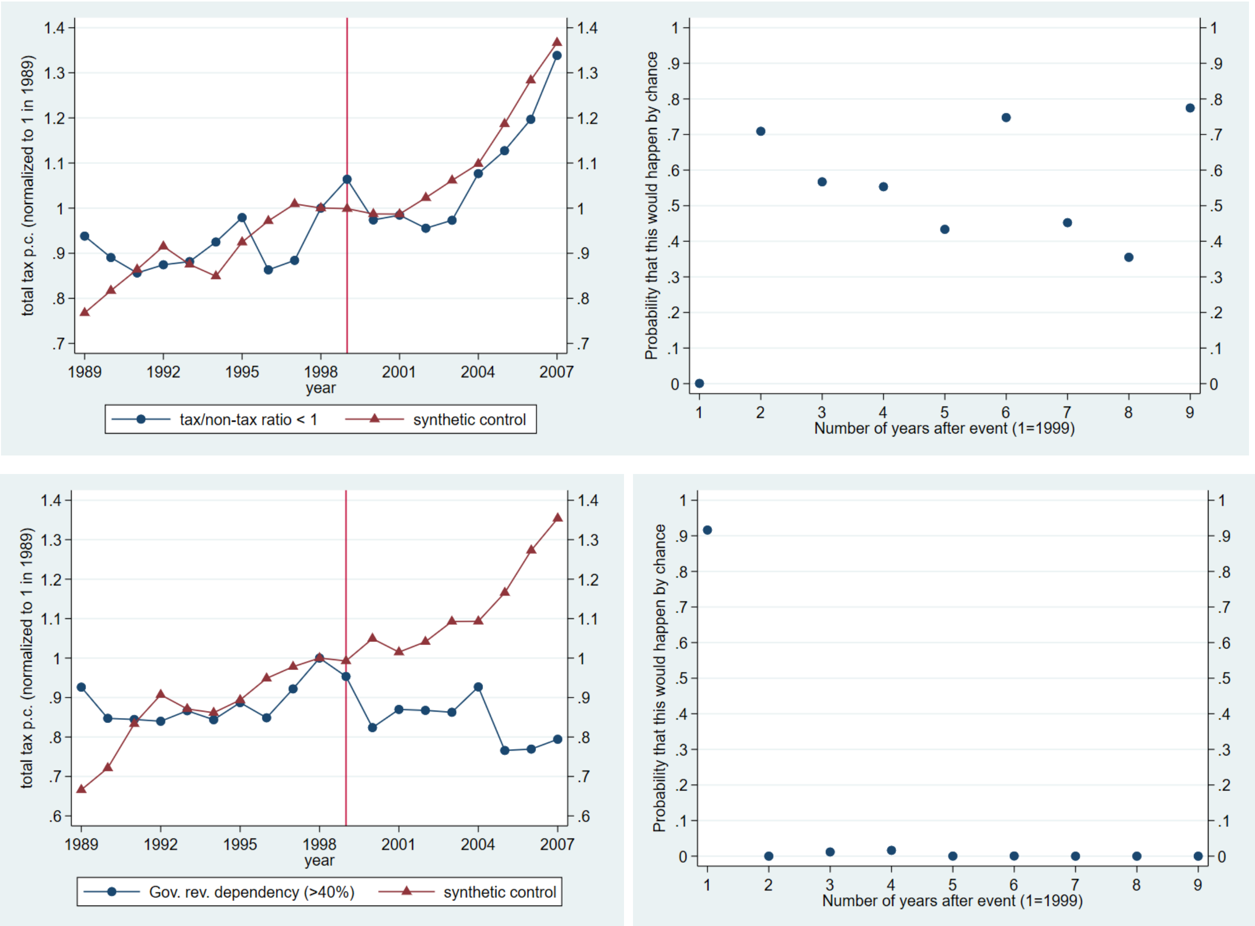
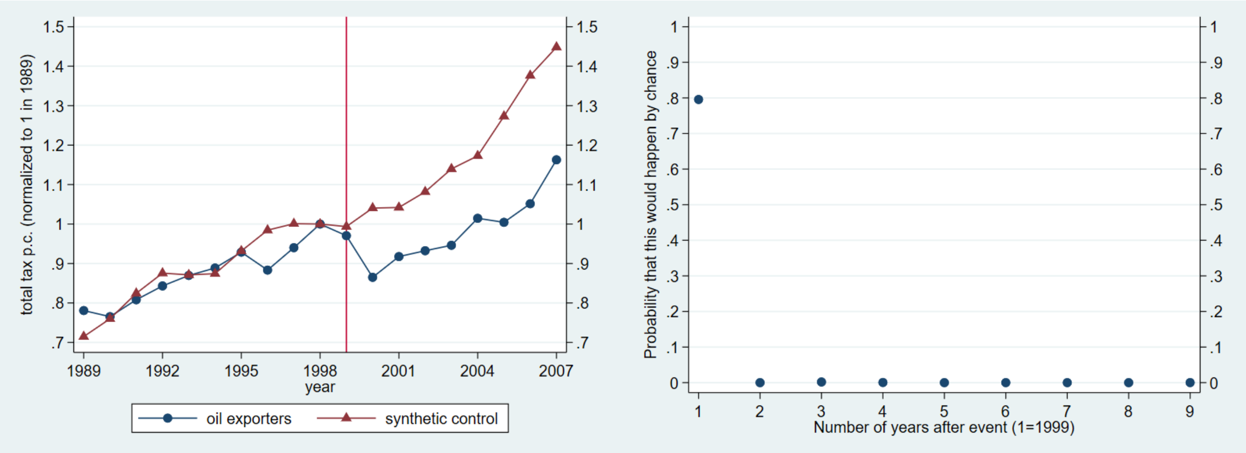
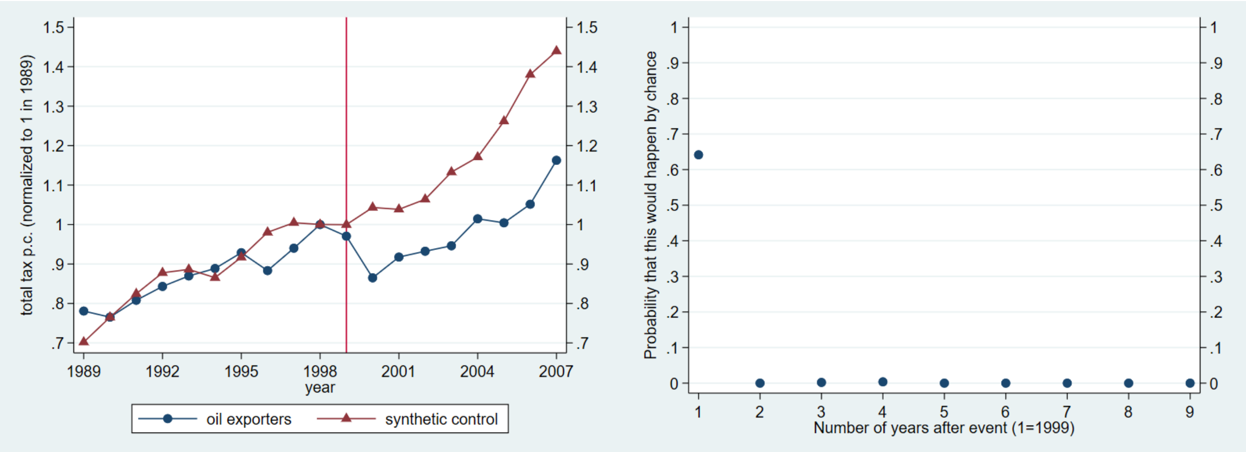


Figure B 2: Robustness check: Including additional predictor variables

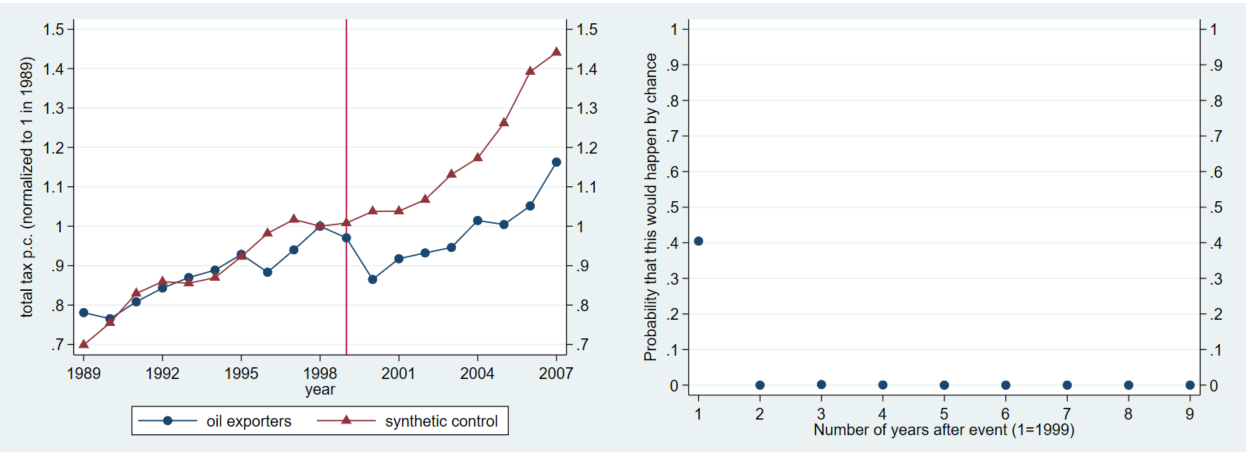
a) Including inflation



b) Including agriculture



c) Including ODA



Continuation of Figure B 2

d) Including all additional predictor variables

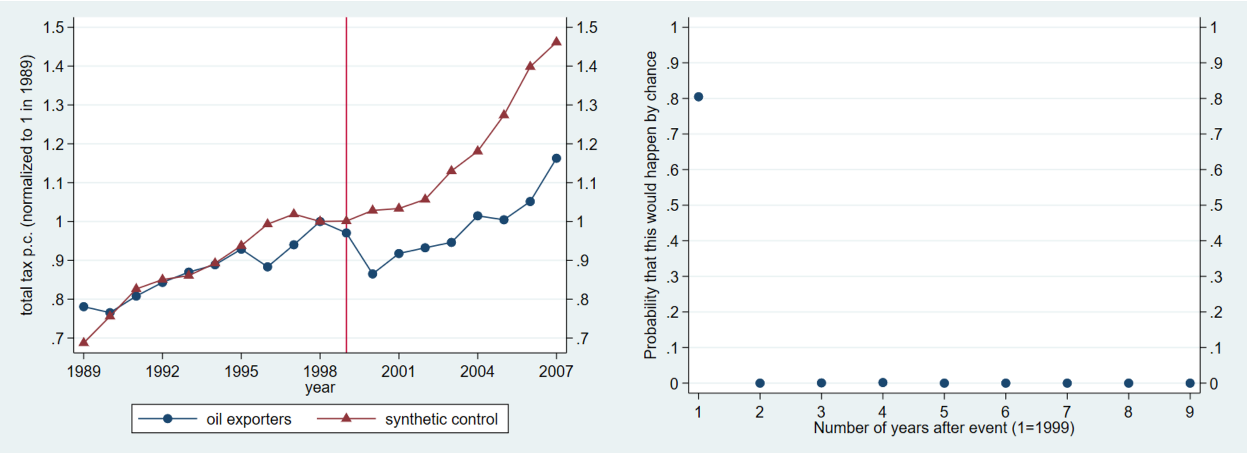
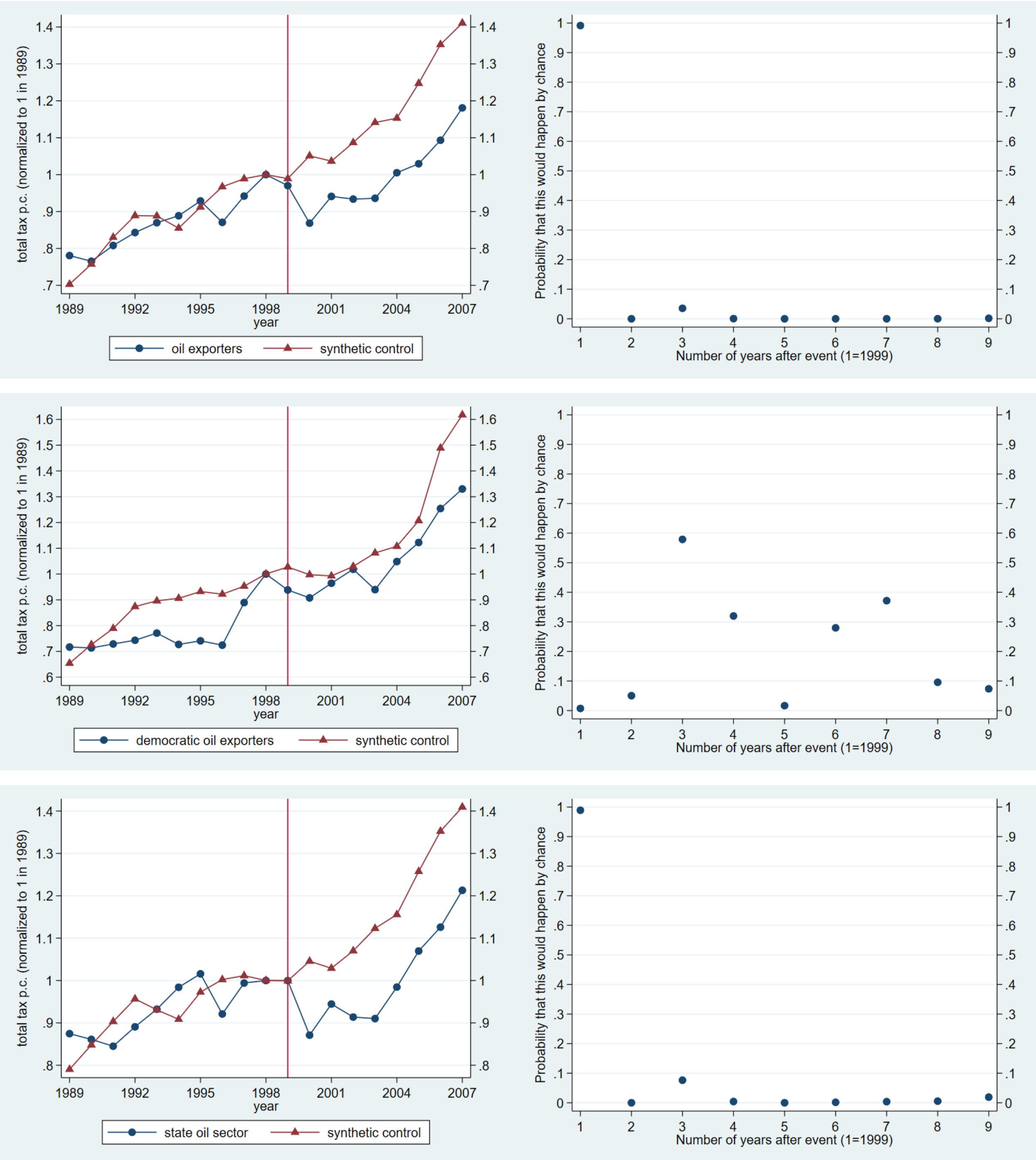
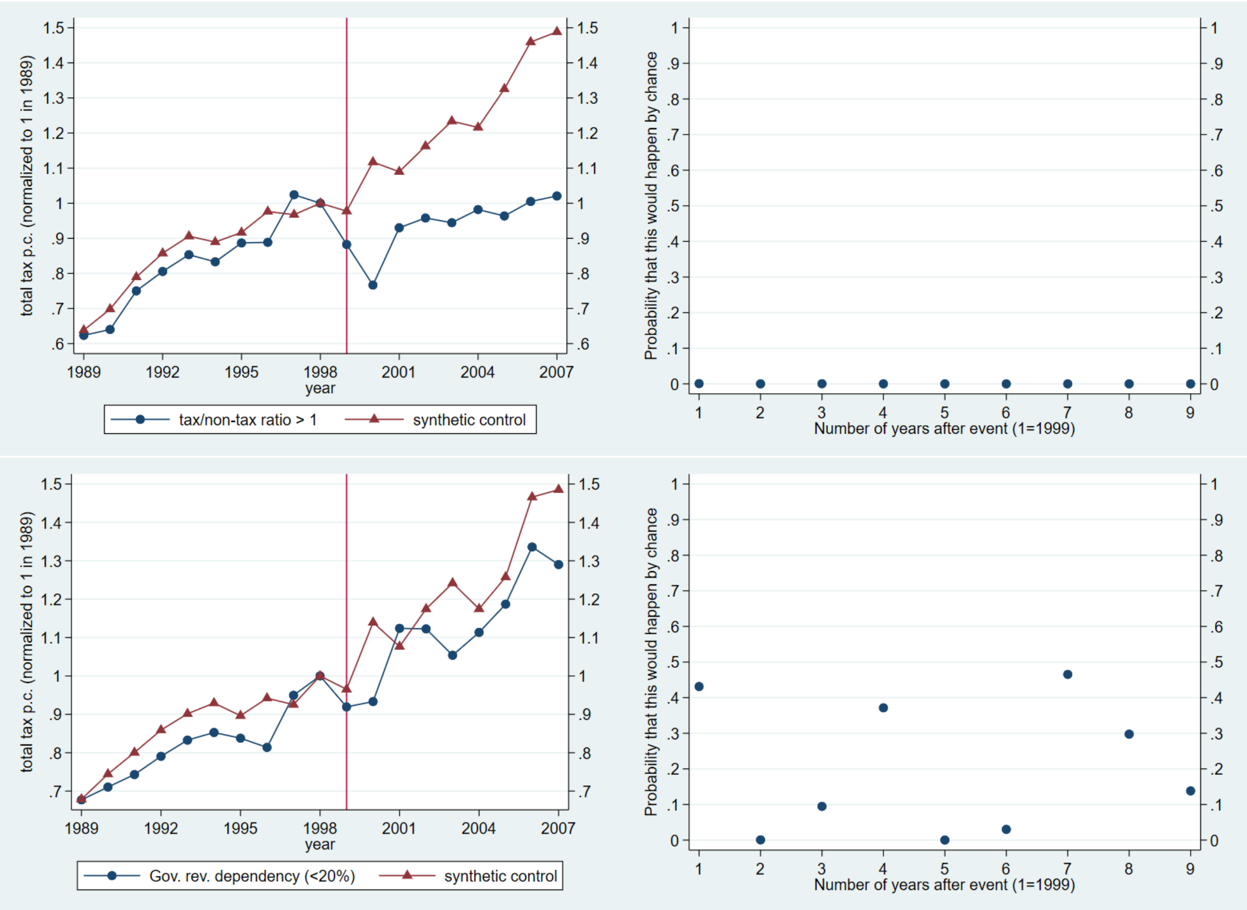


Figure B 3: Robustness check: excluding Norway



continuation of Figure B 3



Appendix C. Inference

Standard errors in common regression techniques usually measure uncertainty about aggregate data. SCM uses aggregate data, hence, uncertainty about them would be 0. However, other forms of uncertainty occur when using aggregate data. Uncertainty in the SCM is derived from ignorance about the synthetic control country's ability to replicate the treatment country in the absence of the treatment. To establish statistical inference Cavallo et al. (2013) proposes to estimate p-values with a procedure similar to permutation tests. These methods allow for valid inference also in settings with few control countries and pre-treatment periods.[21] The idea is to estimate placebo effects and rank them to analyse whether the effect is relatively large compared to a randomly assigned effect. The placebos are derived from estimating the effect for each untreated country (from the donor pool) treating each as if treatment occurred.

The p-values are estimated for each time period according to the following procedure (Cavallo et al. 2013; Firpo and Possebom 2015):

1. For each intervention $g \in \{1, \ldots, G\}$, define which country is assumed to be treated and estimate for each of its control countries an individual placebo effect, $\alpha_{j^g,t}$ as described in equation (4), where $j^g \in \{2, \ldots, J^g + 1\}$.
2. At each post-treatment period, compute every possible placebo average effect by picking a single individual placebo effect, $\alpha_{j^g,t}$ from each intervention g and then taking the average across the G placebos, $\bar{\hat{\alpha}}_{q,j} = \frac{\sum_{g=1}^{G} \hat{\alpha}_{\tilde{j}^g,t}}{G}$, where q indexes placebo estimations and $\tilde{j}^g \in \{2, \ldots, J^g + 1\}$. There are many possible placebo averages given by $Q := \prod_{g=1}^{G} J^g$ and the number quickly grows with G.
3. Rank all placebo average effects and compare in the resulting distribution which rank the actual treatment effect has for each post-treatment period $t \in \{T_0 + 1, \ldots, T\}$.
4. Finally, compute the p-value, $p_t = \frac{\sum_{q-1}^{Q} I[|\bar{\hat{\alpha}}_{q,t}| \geq |\bar{\hat{\alpha}}_{1,t}|]}{Q}$ for each $t \in \{T_0 + 1, \ldots, T\}$ which which shows the probability that the actual treatment effect would have been observed by chance (Cavallo et al. 2013; Firpo and Possebom 2015).

[21] Nevertheless, confidence increases with $N \to \infty$ and/or $T \to \infty$.