Oil Discoveries and Democracy

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Abstract

We evaluate the effect of natural resources on political regimes. We use the synthetic control method to compare the evolution of democracy level of countries affected by giant oil discoveries with the weighted democracy level of countries that do not incur the same event and have similar pre-event characteristics. Focusing on 12 countries affected by the peak of oil discovery from the 1970s, we find that the exogenous variation in oil endowment does not have the same effect on all countries. In most of the cases, the event has a negative effect in the long run, but countries with a high level of democracy in the pre-event period are not affected by the peak of oil discoveries. These results support heterogeneity and nonlinearities claimed in the more recent theoretical literature.

Keywords: Natural Resources, Oil discoveries, Democracy, Synthetic Control Method

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> “[…] the problem is often not the lack of natural resources. Many societies with unfavorable conditions do not lack for resources. Well-ordered societies can get on with very little; their wealth lies else-where: in their political and cultural traditions, in their human capital and knowledge, and in their capacity for political and economic organization. Rather, the problem is commonly the nature of the public political culture and the religious and philosophical traditions that underlie its institutions. The great social evils in poorer societies are likely to be oppressive government and corrupt elites and the subjection of women abetted by unreasonable religion […]. (Rawls, 2001: 64)

1. Introduction

Natural resources, such as minerals, oil and gas, constitute for a state a source rent; a government can sell the right to extract the natural resource to a private firm in exchange of royalties or participation in the firm's profits or can decide to set up a state-owned extractive firm and directly enjoy the dividends.

However, there is strong evidence that often a large endowment of natural resources tends to reduce economic growth and is associated with non-democratic regimes. This phenomenon is often called as “resources curse” because the endowment of natural resources tends to constitute a curse rather than a blessing. The key link to explain this phenomenon is the public sector: rents from natural resources are transformed into public expenditure, as a tool used by a politician to remain in power. For example, an offer of employment in the public sector may be done in exchange of a vote, or simply in acceptance of the status quo in a non-democratic regime. So-called *white elephants* projects (expensive, used below capacity, and therefore unsustainable over time) are another example of waste of public monies accrued from extractive resources.[1] This leads to an over-expansion of an inefficient public sector and rents are not used to boost the economy, but allocated in the non-productive sector. Moreover, a growing extractive sector with high profits tends to attract capital, reducing its availability for investments in other industries. First, this lowers funds for other profitable but less politically-linked companies; second, it diminishes diversification and exposes the country to idiosyncratic shocks in the resource-abundant sector. In turn, both may hamper economic growth in the medium term.

[1] Anecdotal evidence of the success and failure in the political economy of natural resources is collected in Collier and Venables (2011). Matsen et al. (2016) define petro populism as the economically excessive use of natural resource revenues to buy political support.

The evidence on the resource curse is not unanimous. This paper aims to take heterogeneity across countries seriously: we perform a data-driven analysis that instead of calculating average effects of natural resource across countries, compares for each country the actual political regime with the counterfactual situation of this country in the absence of a natural resource shock (in our analysis a giant oil discovery). More precisely, we apply the synthetic control method (SCM, Abadie and Gardeazabal, 2003; Abadie et al., 2010) that can deal with endogeneity from omitted variable bias by accounting for the presence of time-varying unobservable confounders. Moreover, it comes with the advantages of transparency (as the weights identify the countries that are used to estimate the counterfactual outcome of the country that discover an oil field) and flexibility (as the set of potential controls can be appropriately restricted to make the underlying country comparisons more sensible).

The choice of the natural resource measure is critical in a study like this, since exogeneity is a pre-requisite for a meaningful claim of causality. Oil production, the typical measure of natural resource abundance, is imperfect since production is non-monotonic over the lifecycle of any oilfield. Therefore, this is a poor indicator oil wealth. Following Tsui (2011), we exploit the exogenous variation in oil endowment to provide evidence that does not suffer from endogeneity problem. In particular, we evaluate the effect of the peak of oil discoveries, defined as the point in time after which the rate of oilfield discoveries begins to decline. We argue that this event is more plausibly exogenous than the first oil discovery since it depends more on geological factors than exploration. Our findings are in line with the literature according to which the effect of natural resources on democracy depends on the quality of institutions (Mehlum et al., 2006; Robinson et al., 2006).

The paper is organized as follows: Section 2 reviews the literature on natural resources and political regimes; in Section 3 presents the methodology, whereas in Section 4 data and some tests on exogeneity are introduced. Section 5 shows the results, whose robustness checks are presented in Section 6. Section 7 concludes.

2. Natural resources and political regimes

This section presents a selective review of the political resource curse problem. In contrast with a first wave of models (Krugman, 1987; and Sachs and Warner, 1999), and those based on rent-seeking (Lane and Tornell, 1996; Torvik, 2002) that implied an unconditional negative relationship between resource abundance and growth, a result that has become fairly standard is that countries with good institutions will be able to use resource rents to increase their economic performance. This because well-developed institutions have enough checks and balances to prevent a politician to take a predatory behavior or to promote unproductive activities and patronage using government expenditure. In contrast, in countries without such mechanisms there is nothing that prevent a self-interested politician to expand the public sector and "bribe voters by offering them well paid but unproductive jobs and inefficient subsidies and tax handouts" (van der Ploeg, 2011). Since resource rents increase for the politician, the value of being in office, rent-seeking will increase and thus institutional quality decreases. Therefore, we can claim that a quality trap exists when resources are discovered: below a certain threshold quality of institutions will decrease whereas above institutions will not be affected or even improve.[2]

Robinson et al. (2006) argue that the political incentives that resource endowments generate are the key to understanding whether or not they are a curse. Politicians tend to over-extract natural resources relative to the efficient extraction path because they discount the future too much. Resource booms, by raising the value of being in power and by providing politicians with more resources that they can use to influence the outcome of elections, increase resource misallocation in the rest of the economy. The overall impact of resource booms on the economy depends on institutions since these determine the extent to which political incentives map into policy outcomes. Countries with institutions that promote accountability and competence will tend to benefit from resource booms since these institutions reduce the perverse political incentives that such booms create. Countries without such institutions may suffer from a resource curse. In Mehlum et al. (2006), the quality of institutions determines whether countries avoid the resource curse or not. The combination of grabber friendly institutions and resource abundance leads to low growth. Producer friendly institutions, however, help countries to take full advantage of their natural resources. These results contrast the claims of Sachs and Warner that institutions are not decisive for the resource curse.

Natural resources make it more difficult for citizens to solve the collective action problem when facing a kleptocrat because they provide rulers with substantial resources to buy off opponents. In Acemoglu et al. (2004) a kleptocrat, who implement highly inefficient

[2] Marchi Adani and Ricciuti (2014) provide evidence on governance quality for African countries. In a more impressionistic way, Robinson et al. (2006: 451) claim: "For every Venezuela and Nigeria, there is a Norway or a Botswana. A satisfactory model should explain why resources seem to induce prosperity in some countries but not others".

economic policies, expropriate the wealth of their citizens, and use the proceeds for his own consumption. The success of a kleptocrat rests, in part, on its ability to use a divide-and-rule' strategy, made possible by weaknesses in the institutions in these societies. Members of society need to cooperate in order to depose a kleptocrat, yet such cooperation may be defused by imposing punitive rates of taxation on any citizen who proposes such a move, and redistributing the benefits to those who need to agree to it. In equilibrium, all citizens are exploited and no one challenges the kleptocrat.

Empirical evidence on the political resource curse has been mainly addressed with the use of panel data, and the quality of institutions has often been proxied by corruption. The link goes from resource dependence to corruption and rent seeking via protection, exclusive licenses to exploit and export resources by the political elite, oligarchs to capture wealth and political power. In a sample of fifty-five countries, resource dependence is indeed strongly associated with a worse corruption perceptions index, which in turn is associated with lower growth (Mauro 1995). Cross-country regressions also suggest that natural resource wealth stimulates corruption among bureaucrats and politicians (Ades and Di Tella 1999). Panel evidence covering ninety-nine countries during 1980-2004 suggests that natural resources only induce corruption in countries that have endured a nondemocratic regime for more than 60 percent of the years since 1956 controlling for income, time-varying common shocks, regional fixed effects, and some other covariates (Bhattacharyya and Hodler, 2010). Collier and Hoeffler (2009) suggest that the combination of high natural resource rents and open democratic systems retards growth unless there are sufficient checks and balances which is not the case in many new resource rich democracies.

A new line of empirical research involves quasi-experimental studies. Vicente (2010) compares changes in perceived corruption in Sao Tome, which had a significant oil discovery announcement in 1997-99, with Cape Verde, which did not find oil, both with similar histories, culture, and political institutions. It uses a dataset of the characteristics of all scholarship applicants during 1995-2005 and tailored household surveys. It finds that corruption increased by close to 10 percent after the announcements of the oil discovery but decreased slightly after 2004. In a regression-discontinuity study not explicitly related with natural resources, Brollo et al. (2013) find that windfall government revenues on Brazilian municipalities, increase corruption and the chances of the incumbent holding on to office, whereas decreases the quality of politicians.

Our analysis follows some insights from Tsui (2011) as far as the choice of oil discoveries as the main variable related with the resource curse is concerned, although his empirical strategy is based on a parametric instrumental variables approach. He argues that oil production, the typical measure of natural resource abundance is noisy. Due to geological constraint, the production rate is non-monotonic over the lifecycle of any oilfield and, hence, production is a poor indicator of the remaining reserves and hence oil wealth. Moreover, as an endogenous flow variable, production also understate the oil wealth of the swing producers who produce below their full capacity. Oil wealth is the capital value of future oil rents and hence a stock variable. To capture oil wealth, oil first has to be found and extracted. Oil exploration is risky business; it is unlikely for the first exploratory borehole in a new area to succeed. Cotet and Tsui (2013) report that with the current technology, the success rate of exploration drilling is still less than half, and historically this has been much smaller. It is therefore plausible to treat oil discoveries as positive oil shocks, whose timing and size of oil discovery are more exogenous than oil production. Moreover, the size of deposit, the quality and other cost-determining oilfield characteristics are exogenous. Tsui (2011) finds that larger oil discoveries are causally linked to slower transitions to democracy, whereas oil discovery has almost no effect for democratic countries. Moreover, the oil effect is larger the higher the oil quality and the lower the exploration and extraction costs. This negative impact of oil wealth does not disproportionately affect large oil producers from the Arab world.

However, we depart from this approach in a fundamental way. Their approach produces average effects of oil discoveries on the level of democracy, whereas ours gives the effect in each treated country. Their approach, therefore, is more general at the cost of hiding differences across countries. Our methodology returns the country-specific effect at the price of concentrating on a few cases. We think that two approaches complement each other.

Our methodology has been applied, in addition to panel difference-in-differences, by Smith (2015) that uses resource discoveries in countries that were not previously resource-rich as a plausibly exogenous source of variation. He finds a positive effect on GDP per capita levels in non-OECD countries, and mixed evidence of the long-run positive effect of resources on productivity, capital formation and education.

3. The Synthetic Control Approach

We apply the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie et al. (2010). The SCM provides quantitative inference in small-sample comparative studies by estimating the counterfactual situation of one or a few aggregate entities in the absence of an event or intervention. The missing counterfactual outcome is given by the weighted outcome of all potential comparison units that best reproduces the characteristics of the case of interest (Abadie et al., 2015). In our case, we compare the democracy level of countries that reach the peak of oil discoveries with the weighted democracy level of countries that do not incur the same event and have similar pre-event characteristics.

To frame the SCM in our context, suppose that there is a balanced panel of $I+1$ countries indexed by i and observed over T years. Among these, country $i=1$ reaches the peak of oil discoveries at time $T_0 < T$ (treaded unit), while the remaining I countries are not affected by giant oil discoveries (donor pool). The effect of the event is given by:

$$\alpha_{1t} = Y_{1t} - Y_{1t}^{N} \qquad [1]$$

where $t > T_0$, Y_{1t} is the observed outcome of country $i=1$ for a post-event period t, and Y_{1t}^{N} is the unobservable potential outcome of country $i=1$, that is the democracy level that would have been observed in the absence of the event. The SCM estimates Y_{1t}^{N} defining a weighted average of the donor pool (synthetic control). Thereby the estimator of α_1 at time t is given by the difference between the outcome of the treated unit and the outcome of the synthetic control at that period:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{i=2}^{I+1} w_i^{*} Y_{it} \qquad [2]$$

The weights w_i^{*} are chosen such that the characteristics (*predictors*) of the treated unit are best reproduced by the characteristics of the synthetic control. More formally, let X_{1k} be the pre-event value of the *k-th* democracy predictor for the treated unit, and let X_{0k} be a $(1 \times I)$ vector of the pre-event values of the same variable *k-th* for the units in the donor pool. Then, the vector W^{*} containing the weights assigned to each control unit is chosen in order to minimise the following summation: [3]

$$\sum_{k=1}^{K} v_k (X_{1k} - X_{0k}W)^2$$

subject to $w_i \geq 0$ and $\sum_{i=2}^{I} w_i$=1.[3] v_k is a weight that reflects the predictive power of the variable k. In the following analysis, we choose the positive semidefinite and diagonal matrix Vusing the data-driven procedure implemented by Abadie and Gardeazabal (2003) and Abadie et al. (2010): Vminimise the mean squared prediction error (MSPE) of the outcome variable in the pre-event period. The MSPE measures the expected squared distance between the outcome of the treated unit and the outcome of the synthetic control in the pre-event period.[4] Thus, the lower is the MSPE, the better the synthetic control resembles the characteristic of the treated unit. In order to achieve lower MSPE, we implement the nested optimisation procedure that searches among all V-matrices and set of W-weights for the best fitting convex combination of the units in the donor pool. Moreover, to make sure we have found the global minimum in the parameter space, we run the nested optimization using three different starting point of V.[5]

This data-driven procedure reduces discretion in the choice of the comparison units and comes with the advantage of transparency since it makes explicit the relative contribution of each unit in the donor pool to the counterfactual outcome. In addition, the SCM allows the unobserved variables affecting the outcome to vary with time. In fact, when the number of pre-event periods is large, only those units that are similar in both observed and unobserved characteristics should produce similar paths of the outcome under scrutiny. Therefore, if the trajectories of the democracy level of the treated unit and the synthetic control are alike over extended years prior to the peak of oil discoveries, a gap in the outcome variable in the following years should be interpreted as produced by the peak itself.

These conclusions cannot be validated by the traditional modes of statistical inference due to the small-sample nature of the data (Rubin, 1990). However, Abadie et al. (2010) provide

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[3] This restriction prevents extrapolation outside the support of the data. See Abadie et al. (2015) for a discussion about its relevance.

[4] $MSPE = \frac{1}{T_0}\sum_{t<T_0}\left(Y_{1t} - \sum_{i=2}^{I+1} w_i^* Y_{it}\right)^2$

[5] The three starting points are the regression based V, the equal V-weights, and a third procedure that uses Stata's maximum likelihood search. The nested optimisation procedure is implemented by the Stata routine *synth* available at: *http://web.stanford.edu/~jhain//synthpage.html*

an alternative model of inference defined as “placebo studies”, and based on the premise that the impact of the event under analysis would be undermined if an estimated effect of similar or greater magnitude were obtained also in cases where the intervention did not take place. In particular, placebo studies consist in applying the SCM to every country in the pool of potential controls. This is meant to assess whether the estimated effect for the treated country is large relative to the effect for a country chosen at random. In this paper, we conduct “in-space placebo tests” that compare the estimated treatment effect for each country that reach the peak of oil discoveries with all the (fake) treatment effects of the control countries, obtained from experiments where each control country is assumed to be affected by the same event in the same year of the treated country. If the estimated effect in the treated country is larger than most of the effects obtained by the (fake) experiments, we can safely conclude that the baseline results are not just driven by random chance. This means that if the path of the post-event level of democracy of our case studies falls well outside the distribution of placebo effects, we will attribute that effect to the peak of oil discoveries.

4. Democracy, predictors, and event periods

We analyse the level of democracy using the Polity IV dataset (Marshall et al., 2014), which provides a 21-point scale ranging from -10 (hereditary monarchy) to +10 (consolidated democracy). In order to scale down the variance and reduce the effect of outliers, we transform the Polity Score to let it lie between 0 and 1, with 1 corresponding to the higher level of democracy.

The set of predictors encompasses those factors that the literature identifies as determinants of democracy. We take into account the relationship between political regimes and economic factors including the log of GDP per capita (*Gdp*).[6] We also include a set of additional variables related to economic development that may predict a country’s democracy level:[7] the index of human capital (*human capital*); the sum of imports and exports over GDP

[6] Several studies corroborate the results of the seminal work of Lipset (1959) according to which economic development consolidates democracy. We use the real GDP on the expenditure side that allows comparing living standards across countries and over time (Feenstra et al. 2015).

[7] See Lipset (1959), Barro (1999), and Acemoglu and Robinson (2006).

(*openness*); the value added of the *mining*,[8] *manufacturing*, and *primary* sectors as percentage of the GDP. In addition, we consider the hostility level of interstate disputes (*hostility*), and the total amount of natural resources rents as percentage of the GDP (*total rents*), in order to control for the possible effects of both conflicts and natural resource rents. Finally, we include the average level of democracy calculated in the 10 years preceding the event under scrutiny.

Following Tsui (2011), we identify the year of the event exploiting the oil production and depletion dataset collected by Campbell (2006). This dataset contains information on the peak year of oil discoveries for the top 65 oil countries. We consider that year as the period in which the event under scrutiny takes place.

The predictors are averaged over a 10-year pre-event period,[9] and the path of the outcome variable is analysed until 2014. Due to data availability, we restrict our analysis to countries affected by the peak in the 1970s or later.[10] We exclude also the developed countries that do not show any variation in the polity score in the time span we consider.[11] Table 1 reports the countries analysed and the year in which they reached the peak of oil discoveries, while Table A.1 lists the events excluded. For each treated unit, the donor pool encompasses all the countries not affected by the event for which data are available. Table 2 provides variables' definitions, sources, and their descriptive statistics.

[Table 1 about here]

[Table 2 about here]

To control whether the characteristics that predict the democracy level are also able to predict the peak of oil discoveries, we run cross sectional linear regressions.[12] The dependent

[8] The value added of the mining sector is obtained subtracting *manufacturing* from the variable "Mining, manufacturing, utilities" taken from the UNCTAD database. The noise of utilities in the measurement of the mining sector is small (Caruso et al., 2014).

[9] Data on *total rents, mining, manufacturing, and primary* are available from 1970. Thus, the time span over which they are averaged is different from the 10-year pre-event period for those countries that reached the peak in the 1970s: Brazil, Cameroon, Chad, India, Malaysia, Mexico, Tunisia, and Vietnam.

[10] Angola, United Arab Emirates, Uzbekistan, and Yemen are excluded due to the lack of pre-event data.

[11] These countries are Denmark, Italy, Netherlands, Norway, and United Kingdom.

[12] Smith (2015) uses linear regressions to show that oil discoveries do not depend on the initial characteristics that may affect future growth.

variable is equal to 1 if the country has reached the peak of oil discoveries since 1970, and 0 otherwise. The predictors are measured at 1970. Table 3 shows the results. All the predictors are insignificant except for *human capital* and *openness*, whose coefficients are both negative and significant at 1% and 10%, respectively. However, when we consider a multivariate regression, only the initial level of human capital is a significant predictor of the peak of oil discoveries. The reason for this unusual result may derive from our sample that does not include developed countries that have reached the oil peak. These countries should present high level of human capital. In any case, this result does not invalidate our analysis since the SCM allows to discard those countries that have pre-event characteristics dissimilar from the treated unit.

[Table 3 about here]

5. Results

As highlighted in the previous sections, the credibility of the SCM hinges on its ability to match the pre-event outcome of the treated country with that of the synthetic control. Table 4 reports the predictor balance and the root mean predicted error (RMSPE) for each of our case studies. The low values of the RMSPE confirm the strengths of the synthetic control estimator. However, the RMSPE is higher than 0.10 for Malaysia, Pakistan and Thailand. Since we consider that magnitude too high to have a good fit between the path of the outcome variable of the treated unit and its synthetic control, we discard these countries in the following discussion.

[Table 4 about here]

Figures from 1 to 12 provide a graphical illustration of the results: panels *a* display the trajectories of the democracy level of each country and their synthetic counterparts, while panels *b* show the gap between the two. Table 5 presents, for each case study, the average effect of oil discoveries calculated by averaging the distance between the outcome of the treated country and the synthetic control every 5 years after the peak of oil discoveries. Table A.2 lists the potential controls and the weight assigned to each country in the synthetic control.

[Table 5 about here]

[Figures 1-12 about here]

The main finding of the analysis is that oil discoveries do not affect all the countries in the same way. Most of the case studies present a negative outcome gap in the long run. Figure 2 shows that the level of democracy of Cameroon is slightly lower than the synthetic control after the peak of oil discoveries. This negative outcome gap increase consistently five years after the peak of oil discoveries. Ten years after this event, the peak of oil discoveries has a negative average effect of 0.16. In 2014 the level of democracy of Cameroon is 0.5 points lower than the level that the country would have reached in the absence of the peak. The path of democracy of Chad presents a jump after the peak (Fig. 3). However, this result is due to the period of anarchy started in 1979, two years after the peak (Collins and Burns, 2013), and classified with 0 by Polity score (0.5 according to our transformed index). After this period, the country always presents a level of democracy lower than the synthetic control. The democracy scores of the Republic of Congo (Fig 5) and Sudan (Fig.10) exceed those of their synthetic controls for a short period (five and four years respectively). Nevertheless, in the long run, the level of democracy of both of them is lower than what would have been observed in the absence of the peak of oil discoveries. Vietnam's democracy score is constant during the post event period. However, the SCM allows estimating that, given the pre-event characteristics of the country, its level of democracy in 2014 would have been 0.63 points higher than the observed level. Kazakhstan has a negative outcome gap in the pre-event period that increases two years after the event, though the magnitude of the effect is not high (-0.15).

For all of those countries, the placebo tests presented in Figure 13 confirm a significant negative effect of oil discoveries on democracy in the decades following the event. On the contrary, oil discoveries affect the level of democracy of Brazil only in the short run (Fig. 1). Indeed, after a drop of the democracy level with respect to the synthetic control, Brazil caught up its counterpart ten years after the peak of oil discoveries. Figure 13a proves the robustness of these results.

[Figure 13 about here]

Mexico and Tunisia do not show clear paths. The peak of oil discoveries seems to arrest the increase of democracy in Mexico that started at the eve of that event. However, as in the case of Tunisia, the country presents also significant negative level of the outcome gap. Instead, we cannot reject the null hypothesis of no effect in two cases: Colombia (fig. 13d) and India (fig. 13g). Indeed, even if for both countries the average effect is negative, their post-event levels of democracy do not fall well outside the distribution of placebo effects. For that reason, we can safely affirm that oil discoveries have no effect on the democracy level of India and Colombia. Interestingly, these are the only two countries with a high level of democracy in the pre-event period (above 0.9).

Finally, a striking case is represented by Gabon (Fig. 6) that is the only country in which the peak of oil discoveries seems to have a positive effect on democracy. However, these results are misleading because, after the even under scrutiny, another shock affected the political institutions of the country. In fact, in the 1990s violent demonstrations and strikes led to political reforms including the transformation of the political system to a multiparty democracy (Collins and Burns, 2013). These events are not captured by the synthetic control, which resemble the characteristics of the treated unit only in the absence of further permanent shocks in the outcome.

6. Robustness checks

In this section, we run a robustness check to test the sensitivity of our main results to changes in the measurement of democracy level. We implement the SCM using the Polyarchy dataset compiled by Vanhanen (2014). This dataset provides an index of democracy given by the combination of its two most important dimensions: the degree of competition (*competition*) and the degree of participation (*participation*). The former is measured by the smaller parties' share of all votes casted in parliamentary or presidential elections, while the latter is measured by the percentage of the population who actually voted in those elections. The combined index of democracy (*democracy*) is obtained by multiplying the two indicators and dividing the product by 100 (Vanhanen, 2000). We estimate the synthetic control using these three variables as outcomes. Table 6 presents, for each indicator, the average effect of oil discoveries calculated every 5 years after the peak of oil discoveries.[13]

[Table 6 about here]

[13] Graphs and placebo tests are omitted to save space, but are available upon request from the authors.

The trends of the outcome gaps show that the results of the previous analysis are robust. In particular, the path of *democracy*-gap replicates almost exactly the one given by *polity*.[14] This is not true for Kazakhstan, whose average effect is positive until ten years after the peak of oil discoveries. However, this discrepancy could be explained by the fact that the synthetic control does not replicate the country in the pre-event period. In fact, the RMSPE is equal to 2.107 for *democracy*. Colombia seems to have a significant, negative outcome gap, but only fourteen years after the event. In addition, in this case, the RMSPE is high. This difference vanishes considering *participation* for which the RMSPE is lower.[15] Another exception is Mexico for which the effect of oil discoveries is negative and significant considering both *democracy* and *competition*. The negative gap starts to decrease five years after the peak. Overall, we can claim that the peak of oil discoveries has at least delayed democratization in Mexico.

7. Conclusions

In this paper, we have undertaken a case-study analysis to evaluate the effect of giant oil discoveries on the political regimes of the affected countries. We used the synthetic control method to estimate the democracy level that would have been observed in the absence of the event. This approach allows to overcome the weaknesses of previous analyses since it can deal with the endogeneity problem and the omitted variable bias.

Overall, this paper confirms the idea that natural resources may be a curse or a blessing for a country, depending on the quality of its institutions (Mehlum et al., 2006; Robinson et al. 2006). In particular, the relationship between natural resources and democracy shows some nonlinearities depending on the initial level of democracy itself. Indeed, only the democracy levels of India and Colombia, which have a democracy score above 0.9, do not change significantly after the peak of discoveries. All other countries, with the exception of Gabon that undertook a period of political reforms after the peak of oil discoveries, are negative affected by the variation in oil endowment. A plausible explanation of these results is that, as the rate of discoveries starts to decline, the incumbents enforce higher entry barriers in order to grab the residual resources. This is prevent in democracies with higher level of executive constraints.

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[14] We cannot assert the same considering *participation*. However, we failed to obtain low value of the RMSPE in most of the cases, as proven by the difference between the treated units and the synthetic controls at t_{0}.

[15] RMSPE= 1.001 for *democracy*; RMSPE= 5.287 for *competition*, RMSPE= 0.395 for *participation*.

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Table 1 – Case studies

Country	*Peak of Oil discoveries*	*Start year synth*
Brazil	1975	1965
Cameroon	1977	1967
Chad	1977	1967
Colombia	1992	1982
Republic of Congo	1984	1974
Gabon	1985	1975
India	1974	1964
Kazakhstan	2000	1991
Malaysia	1973	1963
Mexico	1977	1967
Pakistan	1983	1973
Sudan	1980	1970
Thailand	1981	1971
Tunisia	1971	1961
Vietnam	1975	1965

Table 2 – Variable definitions, sources and descriptive statistics

Variable	*Description*	*Source*	*Mean*	*Standard dev.*	*Min.*	*Max*
Democracy	Transformed Revised Combined Polity IV score (polity2) ranging from 0 (hereditary monarchy) to 1 (consolidated democracy).	Polity IV Project - Center for Systemic Peace	0. 560	0.359	0	1
Gdp	log RGDP^e per capita (at chained PPPs in mil. 2005US$)	Pen World Table 8.1	8. 055	1.160	5.219	11.325
Human capital	Index of human capital per person, based on years of schooling (Barro/Lee, 2012) and returns to education (Psacharopoulos, 1994)	Pen World Table 8.1	2.007	0.627	1.018	3.535
Total rents	Total natural resources rents (% of GDP)	World Development Indicators – World Bank	6.637	10.277	0	83.432
Mining	Value Added by sectors of economic activity, annual, 1970-2013: Mining and utilities (Percentage of Gross Domestic Product)	United Nations Conference on Trade and Development (UNCTAD)	6.607	8.547	0	72.123
Manufacturing	Value Added by sectors of economic activity, annual, 1970-2013: manufacturing (Percentage of Gross Domestic Product)	United Nations Conference on Trade and Development (UNCTAD)	15.666	7.534	0.032	50.180
Primary	Value Added by kind of economic activity, annual, 1970-2013: Agriculture, hunting, forestry, fishing (Percentage of Gross Domestic Product)	United Nations Conference on Trade and Development (UNCTAD)	21.050	15.869	0.034	80.510
Openness	Sum of import and exports over GDP (at constant national 2005 prices)	Pen World Table 8.1	0.691	0.482	0.039	4.605
Hostility	Hostility level of interstate dispute ranging from 0 (no dispute) to 5 (war)	Militarized Interstate Disputes Data v4.1 - Correlates of war	0.853	1.591	0	5

Table 3 – Peak of oil discovery and democracy predictors in 1970

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gdp	-0.030								0.067
	(0.033)								(0.085)
Human capital		-0.150***							-0.344**
		(0.056)							(0.149)
Total rents			0.001						-0.004
			(0.006)						(0.006)
Mining				0.000					-0.000
				(0.006)					(0.008)
Manufacturing					-0.002				-0.000
					(0.003)				(0.006)
Primary						0.000			-0.003
						(0.002)			(0.004)
Openness							-0.153*		-0.134
							(0.078)		(0.101)
Hostility								0.030	0.032
								(0.027)	(0.032)

Notes: The dependent variable is an indicator equal to 1 if the Country has reached the peak of oil discoveries since 1970 and 0 otherwise. Covariates are measured in 1970. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

Table 4 – Predictor balance and RMSPE

Predictor	*Brazil 1975*	*Synthetic Brazil*
Gdp	8.049063	8.494432
Human capital	1.423229	1.592064
Total rents	3.032327	2.624917
Mining	2.817145	4.866887
Manufacturing	29.25174	19.27867
Primary	11.87682	25.98486
Openness	0.1084437	0.27975
Hostility	0.4	3.0722
Average pre-discovery democracy	0.075	0.07599
RMSPE		0.001012
	Cameroon 1977	*Synthetic Cameroon*
Gdp	7.177065	7.141711
Human capital	1.311305	1.384907
Total rents	4.055883	6.277098
Mining	1.17727	1.180156
Manufacturing	14.70772	11.03428
Primary	28.28982	44.37049
Openness	0.2701598	0.2741007
Hostility	0.4	0.406
Average pre-discovery democracy	0.125	0.12755
RMSPE		0.0075677
	Chad 1977	*Synthetic Chad*
Gdp	7.265192	7.25084
Total rents	4.75743	4.746761
Mining	0.9221356	0.921185
Manufacturing	12.31258	12.28897
Primary	41.9175	41.82736
Openness	0.498415	0.4972167
Hostility	0.2	0.1984
Average pre-discovery democracy	0.07	0.07283
RMSPE		0.0336496
	Colombia 1992	*Synthetic Colombia*
Gdp	8.731305	9.189705
Human capital	2.016568	2.145918
Total rents	6.595242	1.345631
Mining	6.283752	4.958925
Manufacturing	18.12654	21.17535
Primary	12.55478	7.596763
Openness	0.2025784	0.2305646
Hostility	1.2	1.2468
Average pre-discovery democracy	0.905	0.904515
RMSPE		0.0011125

Table 4 (continued) – Predictor balance and RMSPE

Predictor	*Republic of Congo 1984*	*Synthetic Republic of Congo*
Gdp	7.536801	7.582842
Human capital	1.667621	1.458849
Total rents	39.64646	21.13613
Mining	28.3415	19.28067
Manufacturing	9.006013	9.808753
Primary	13.46419	16.33363
Openness	1.111467	1.124935
Hostility	0.5	0.4995
Average pre-discovery democracy	0.125	0.12557
RMSPE		0.0149957
	Gabon 1985	*Synthetic Gabon*
Gdp	9.252937	7.839756
Human capital	1.59167	1.627161
Total rents	47.64735	12.65417
Mining	41.641	6.981598
Manufacturing	6.07867	15.45709
Primary	5.053038	27.86907
Openness	1.016262	1.320546
Hostility	0.1	0.1008
Average pre-discovery democracy	0.05	0.0508
RMSPE		0.00063
	India 1974	*Synthetic India*
Gdp	7.055507	8.774614
Human capital	1.223596	2.135507
Total rents	2.316797	2.268751
Mining	2.117753	2.168657
Manufacturing	14.2285	26.663
Primary	42.94529	13.29678
Openness	0.1135095	0.2515587
Hostility	4	0.3451
Average pre-discovery democracy	0.95	0.94994
RMSPE		0.0005722
	Kazakhstan 2000	*Synthetic Kazakhstan*
Gdp	8.751681	8.707081
Human capital	2.708643	2.067864
Total rents	17.93863	9.386802
Mining	12.41103	2.115997
Manufacturing	11.92521	16.18919
Primary	13.41587	21.08155
Openness	1.21744	1.947997
Hostility	0.7777778	0.1145556
Average pre-discovery democracy	0.3222222	0.32225
RMSPE		0.0248456

Table 4 (continued) – Predictor balance and RMSPE

Predictor	*Malaysia 1973*	*Synthetic Malaysia*
Gdp	7.909242	7.99283
Human capital	1.655252	1.69775
Total rents	6.420214	6.41138
Mining	9.681566	9.961237
Manufacturing	14.61374	14.594
Primary	28.48076	15.85045
Openness	0.6859536	0.5896284
Hostility	1.5	0.8885
Average pre-discovery democracy	0.85	0.834385
RMSPE		0.1645738
	Mexico 1977	*Synthetic Mexico*
Gdp	8.904852	8.059699
Human capital	1.65393	1.987598
Total rents	3.48081	3.472643
Mining	8.50995	8.469963
Manufacturing	19.22363	22.49874
Primary	10.6091	23.63679
Openness	0.1206917	0.2188393
Hostility	0	0.5021
Average pre-discovery democracy	0.2	0.1995
RMSPE		0.0001902
	Pakistan 1983	*Synthetic Pakistan*
Gdp	7.341958	7.060531
Human capital	1.3101	1.353643
Total rents	4.194234	1.987898
Mining	4.095575	4.535334
Manufacturing	10.42744	9.951427
Primary	34.40184	33.75569
Openness	0.3474532	0.3627543
Hostility	2.1	2.0756
Average pre-discovery democracy	0.45	0.442475
RMSPE		0.3453261
	Sudan 1980	*Synthetic Sudan*
Gdp	7.152358	7.239045
Human capital	1.137935	1.476076
Total rents	0.0002809	2.006756
Mining	1.892794	2.165256
Manufacturing	8.861436	10.98123
Primary	38.16304	38.15145
Openness	0.1021655	0.4820521
Hostility	1.5	1.5302
Average pre-discovery democracy	0.175	0.17774
RMSPE		0.046698

Table 4 (continued) – Predictor balance and RMSPE

	Thailand 1981	*Synthetic Thailand*
Gdp	7.784089	8.283527
Human capital	1.70597	1.761231
Total rents	2.306225	2.597524
Mining	2.669549	6.407429
Manufacturing	19.71048	18.29137
Primary	25.37276	15.79119
Openness	0.4776597	0.4599142
Hostility	3.9	3.4765
Average pre-discovery democracy	0.435	0.43146
RMSPE		0.1992042

	Tunisia 1971	*Synthetic Tunisia*
Gdp	7.418731	7.490103
Human capital	1.199441	1.417062
Total rents	3.160351	3.295567
Mining	5.51892	1.988342
Manufacturing	8.745414	10.55169
Primary	15.21589	28.22424
Openness	0.6536856	0.4287062
Hostility	0.7	2.2609
Average pre-discovery democracy	0.055	0.056595
RMSPE		0.0142813

	Vietnam 1975	*Synthetic Vietnam*
Gdp	6.545513	7.523913
Human capital	1.739625	1.747607
Total rents	0	1.698035
Mining	3.951625	5.941236
Manufacturing	16.07247	15.72458
Primary	42.62852	34.60567
Openness	0.6227122	0.5732201
Hostility	5	1.3198
Average pre-discovery democracy	0.13	0.135505
RMSPE		0.024323

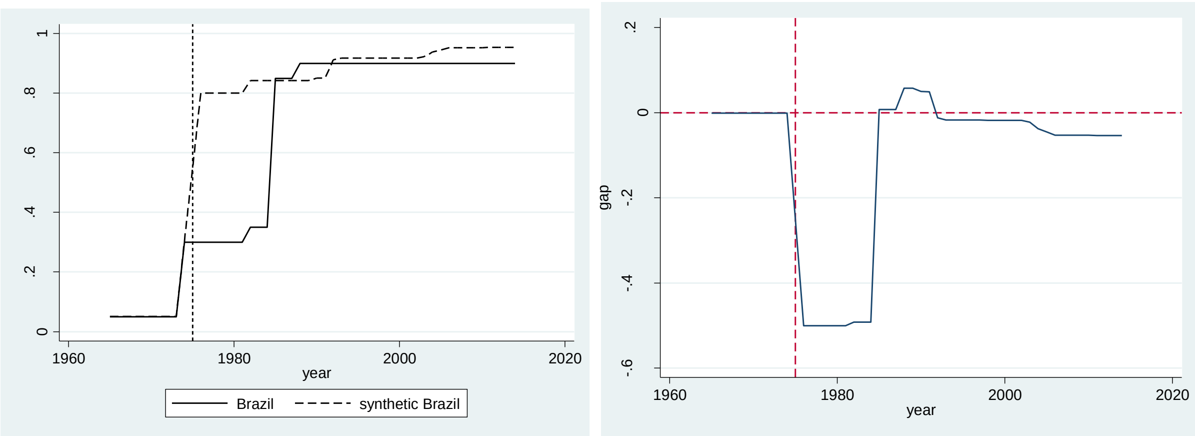
Table 5 – Average effect of the peak of oil discoveries on Polity IV indicator

Country	t_0	t_5	t_{10}	t_{15}	t_{20}	t_{25}	t_{30}
Brazil 1975	-0.251	-0.501	-0.394	0.036	-0.003	-0.018	-0.028
Cameroon 1977	-0.003	-0.047	-0.159	-0.388	-0.431	-0.412	-0.354
Chad 1977	0.061	0.369	0.133	-0.125	-0.228	-0.177	-0.239
Colombia 1992	0.000	-0.060	-0.099	-0.104	-0.113	-0.113	-
Republic of Congo 1984	-0.050	-0.062	0.076	-0.068	-0.256	-0.394	-0.403
Gabon 1985	-0.001	0.029	0.212	0.199	0.196	0.265	0.482
India 1974	0.001	-0.069	-0.063	-0.067	-0.086	-0.036	-0.020
Kazakhstan 2000	-0.022	-0.111	-0.151	-0.151	-	-	-
Mexico 1977	0.151	0.136	0.095	-0.108	-0.128	0.046	0.092
Sudan 1980	-0.047	-0.083	-0.090	-0.163	-0.607	-0.634	-0.659
Tunisia 1971	-0.005	0.029	0.224	-0.495	-0.554	-0.506	-0.395
Vietnam 1975	-0.015	0.000	0.002	-0.045	-0.087	-0.280	-0.267

Table 6 – Average effect of the peak of oil discoveries on Vanhanen's democracy indicators

Country	*indicator*	t_0	t_5	t_{10}	t_{15}	t_{20}	t_{25}	t_{30}
Brazil 1975	Democracy	0.000	-13.411	-14.853	-5.648	0.372	0.733	5.229
	Competition	0.000	-3.078	9.581	10.624	9.703	12.685	14.509
	Participation	-0.981	-1.844	-26.276	-12.368	12.092	14.725	22.829
Cameroon 1977	Democracy	-3.845	-6.663	-6.940	-6.650	-2.884	-12.007	-8.878
	Competition	-1.347	-1.085	-8.257	-3.058	1.598	-34.089	-4.605
	Participation	0.208	7.971	7.513	2.398	2.122	-3.600	-3.104
Chad 1977	Democracy	-5.461	-5.143	-4.765	-6.730	-10.362	-3.604	-2.873
	Competition	-6.767	-6.489	-5.470	-21.258	-33.352	-3.709	10.240
	Participation	-26.900	-25.384	-28.366	-27.397	-5.514	10.169	3.514
Colombia 1992	Democracy	-1.562	-4.373	0.414	-4.502	-7.993	-	-
	Competition	1.693	1.306	0.260	-7.927	-19.607	-	-
	Participation	-2.266	-11.189	-2.618	-8.047	-8.668	-	-
Republic of Congo 1984	Democracy	0.000	-2.927	10.520	3.802	0.242	-6.790	-9.382
	Competition	0.000	-14.193	28.280	9.802	-1.358	-26.439	-37.315
	Participation	-0.421	-0.496	17.558	-21.666	-1.117	13.766	-0.909
Gabon 1985	Democracy	-3.091	0.206	9.740	4.961	3.946	-4.843	-17.275
	Competition	-3.280	2.446	37.020	26.051	23.792	0.731	-41.767
	Participation	0.010	0.146	-1.362	-22.330	-11.963	-16.832	-25.646
India 1974	Democracy	-0.372	1.414	-0.171	-2.194	-4.754	-1.683	-4.023
	Competition	3.114	4.609	1.768	-5.548	-4.287	1.367	-9.207
	Participation	13.752	21.004	1.641	-4.640	6.472	11.376	12.496
Kazakhstan 2000	Democracy	4.658	3.418	-3.171	-	-	-	-
	Competition	6.939	5.990	-10.113	-	-	-	-
	Participation	1.511	-0.753	-3.049	-	-	-	-
Mexico 1977	Democracy	-19.508	-17.357	-11.245	-9.097	-6.149	-2.477	-2.231
	Competition	-42.796	-38.662	-16.475	0.906	-0.624	5.809	10.322
	Participation	-9.974	-5.826	-3.693	-12.637	3.498	1.506	-0.699
Sudan 1980	Democracy	-0.001	-12.316	-14.038	-13.647	-10.430	-14.149	-6.990
	Competition	0.000	0.000	0.000	0.000	0.000	-0.800	7.240
	Participation	1.144	2.190	0.509	-0.236	2.552	3.341	10.366
Tunisia 1971	Democracy	0.012	-0.190	1.121	-4.676	-0.679	-2.124	-13.120
	Competition	-0.189	-0.593	13.474	-29.157	-9.099	-15.596	-25.757
	Participation	0.110	-12.028	-31.423	-42.859	-20.544	-23.541	-44.198
Vietnam 1975	Democracy	0.012	-0.190	1.121	-4.676	-0.679	-2.124	-13.120
	Competition	-0.189	-0.593	13.474	-29.157	-9.099	-15.596	-25.757
	Participation	0.110	-12.028	-31.423	-42.859	-20.544	-23.541	-44.198

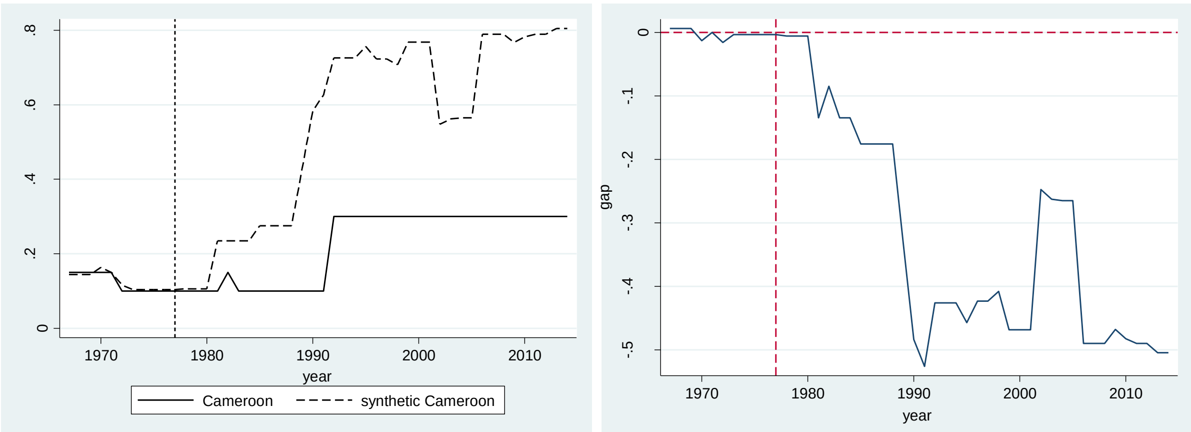
Fig. 1 – Path of democracy, Brazil 1975



a) Brazil vs Synthetic Control

b) Outcome gap

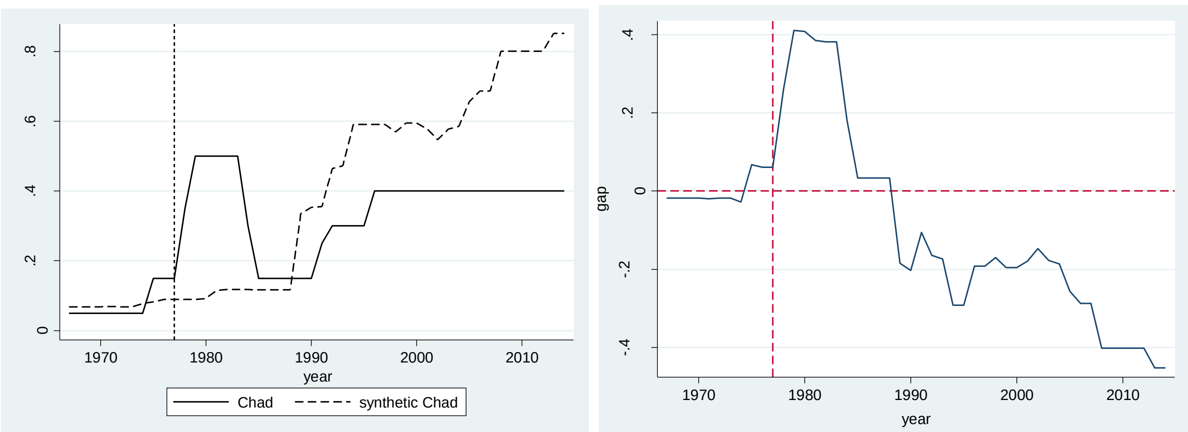
Fig. 2 – Path of democracy, Cameroon 1977



a) Cameroon vs Synthetic Control

b) Outcome gap

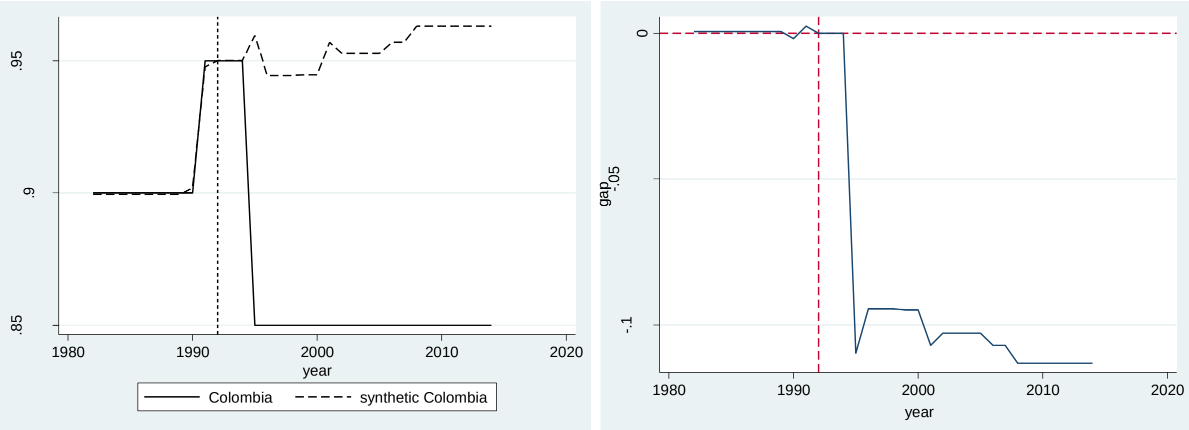
Fig. 3 – Path of democracy, Chad 1977



a) Chad vs Synthetic Control

b) Outcome gap

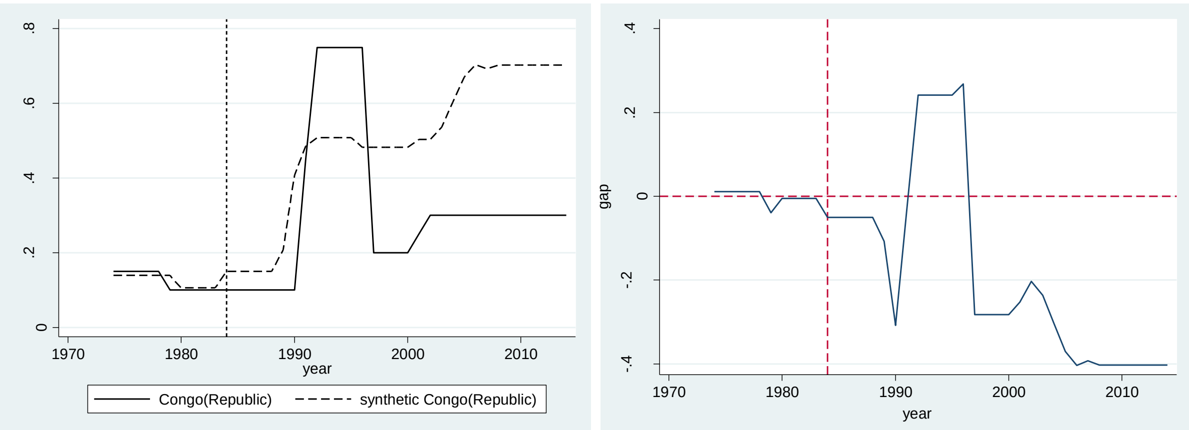
Fig. 4 – Path of democracy, Colombia 1992



a) Colombia vs Synthetic Control

b) Outcome gap

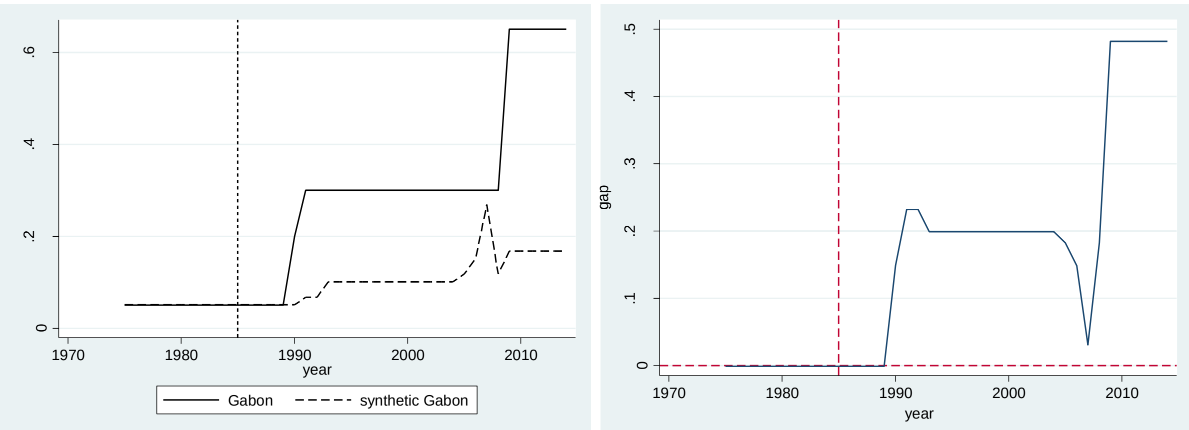
Fig. 5 – Path of democracy, Republic of Congo 1984



a) Republic of Congo vs Synthetic Control

b) Outcome gap

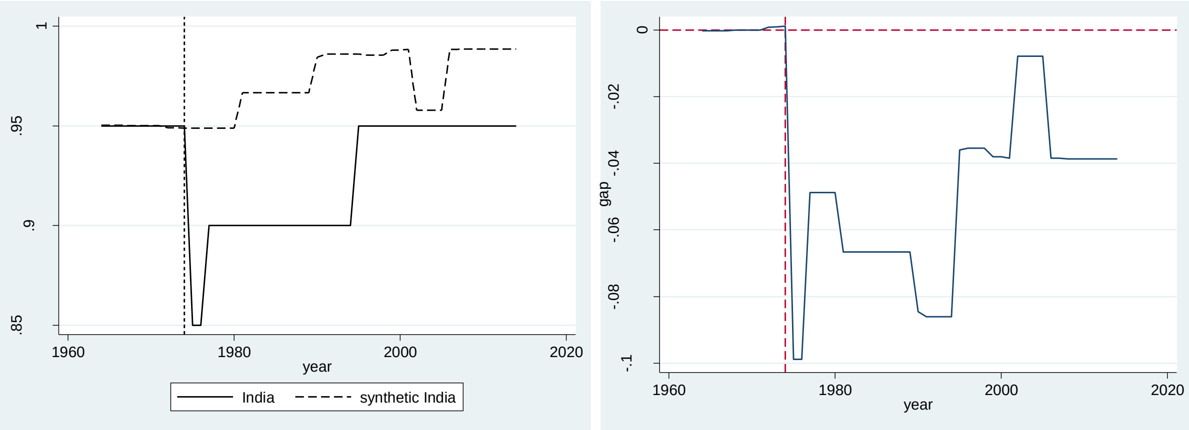
Fig. 6 – Path of democracy, Gabon 1985



a) Gabon vs Synthetic Control

b) Outcome gap

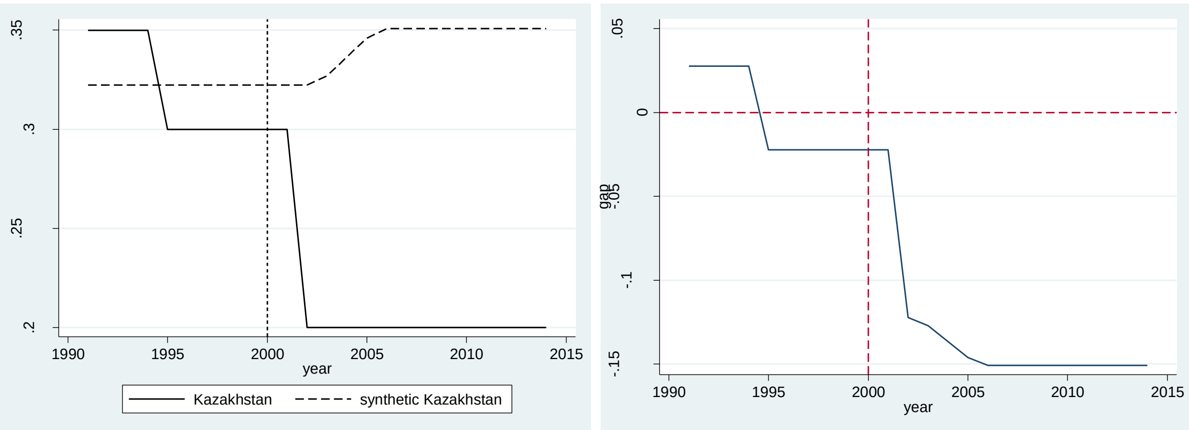
Fig. 7 – Path of democracy, India 1974



a) India vs Synthetic Control

b) Outcome gap

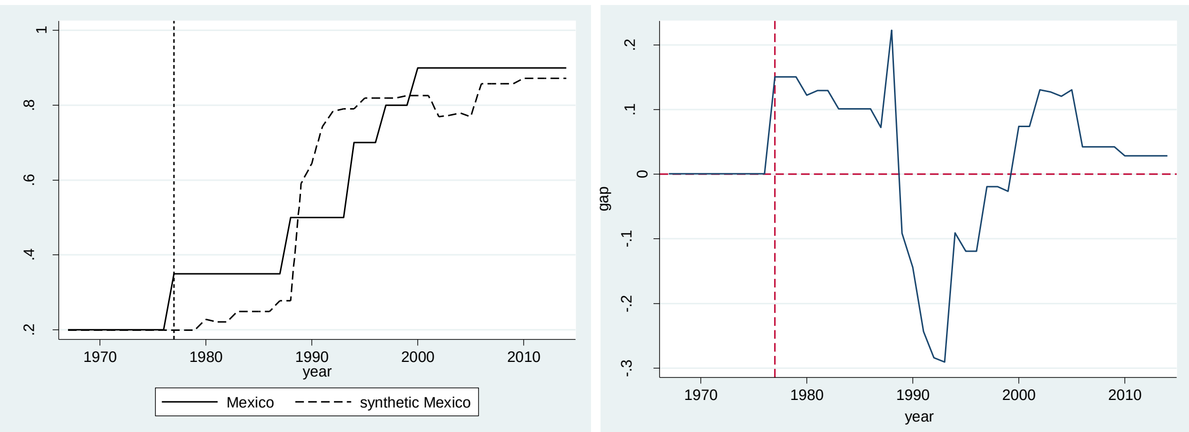
Fig. 8 – Path of democracy, Kazakhstan 2000



a) Kazakhstan vs Synthetic Control

b) Outcome gap

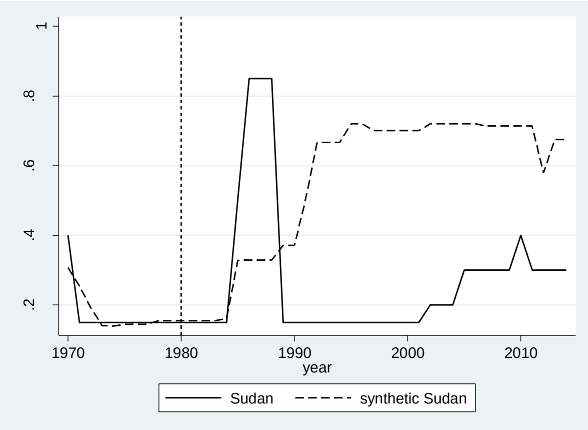
Fig. 9 – Path of democracy, Mexico 1977



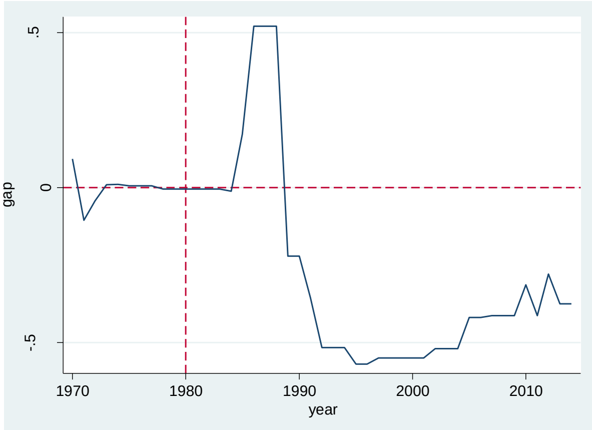
a) Mexico vs Synthetic Control

b) Outcome gap

Fig. 10 – Path of democracy, Sudan 1980

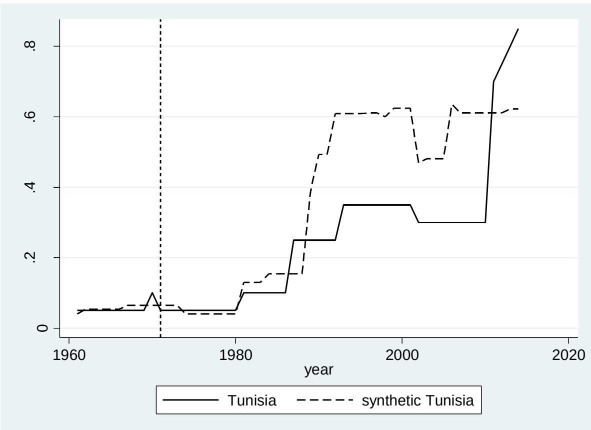


a) Sudan vs Synthetic Control

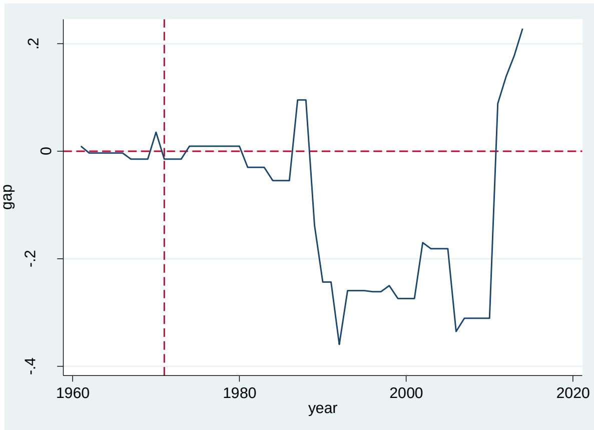


b) Outcome gap

Fig. 11 – Path of democracy, Tunisia 1971

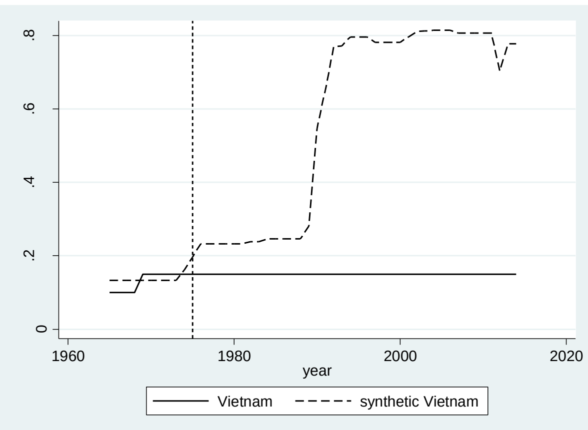


a) Tunisia vs Synthetic Control

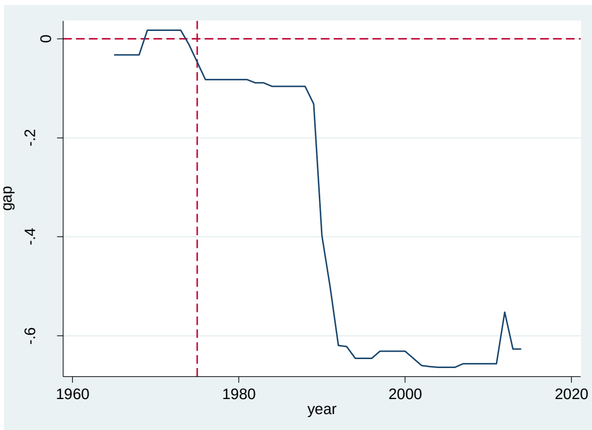


b) Outcome gap

Fig. 12 – Path of democracy, Vietnam 1975

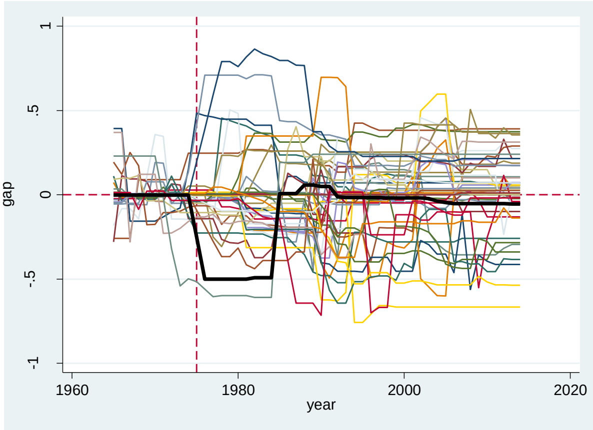


a) Vietnam vs Synthetic Control

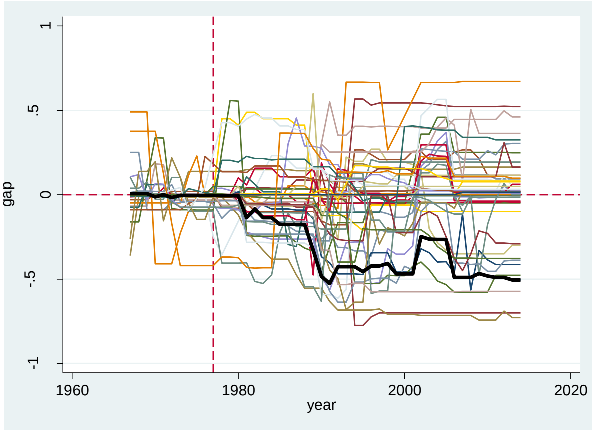


b) Outcome gap

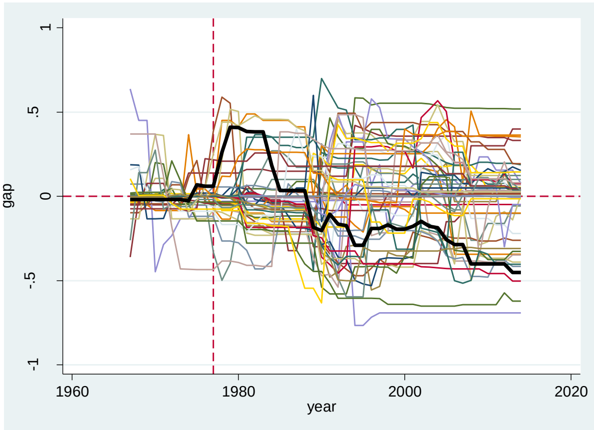
Fig. 13 – Placebo tests



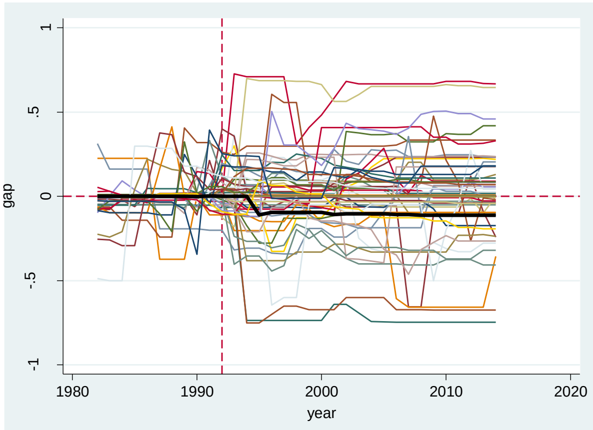
a) Brazil



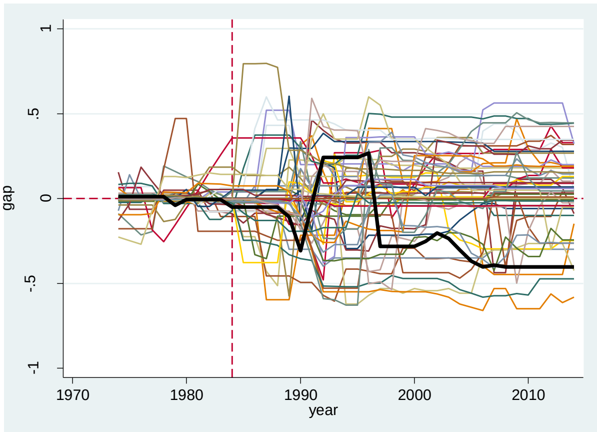
b) Cameroon



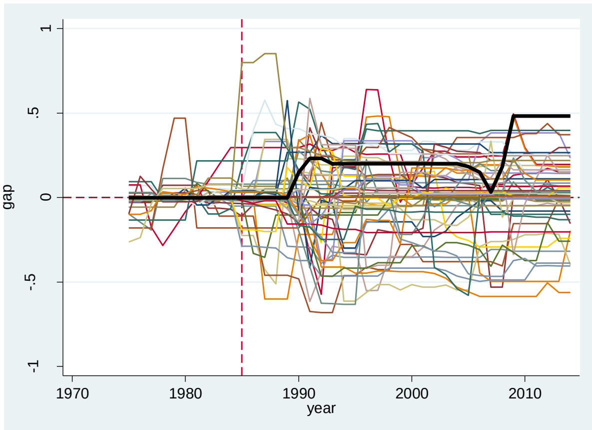
c) Chad



d) Colombia

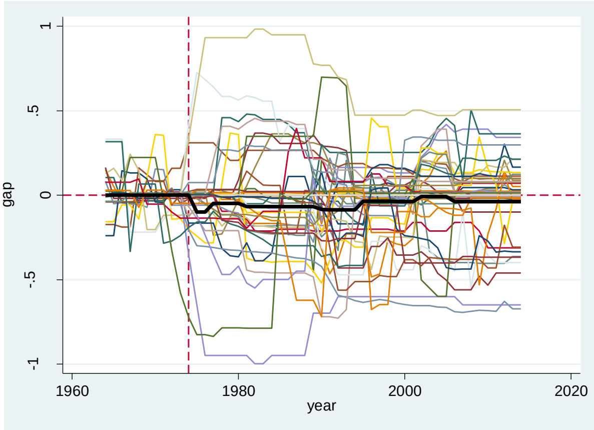


e) Congo

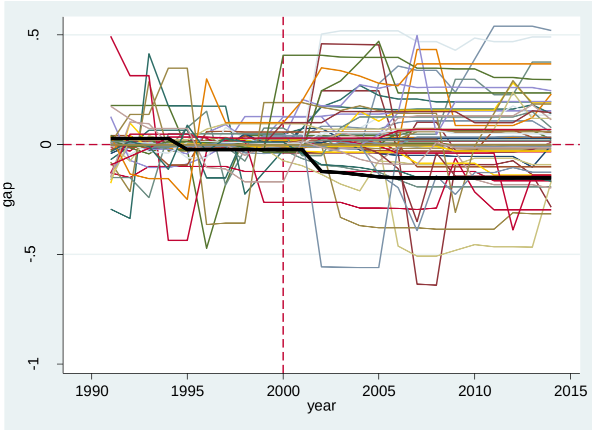


f) Gabon

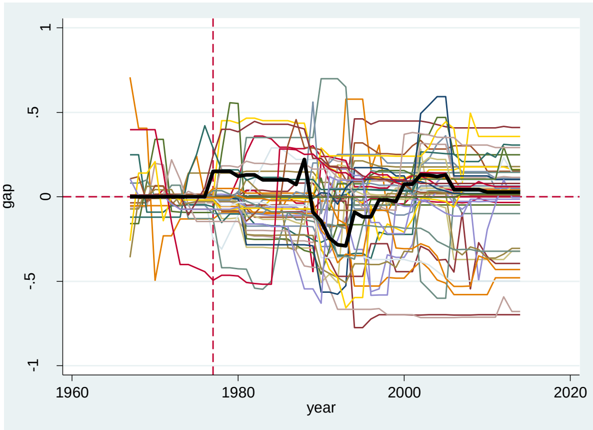
Fig. 13 (continued) – Placebo tests



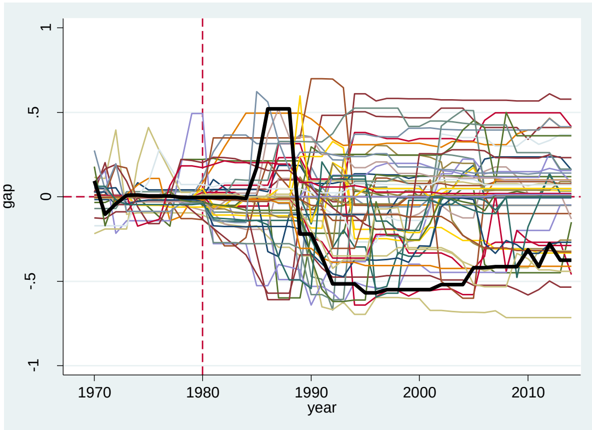
g) India



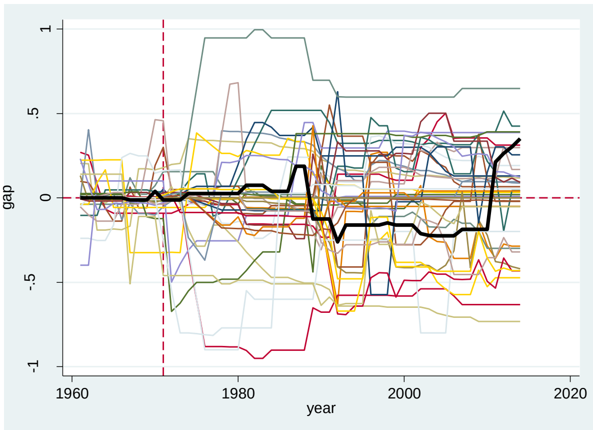
h) Kazakhstan

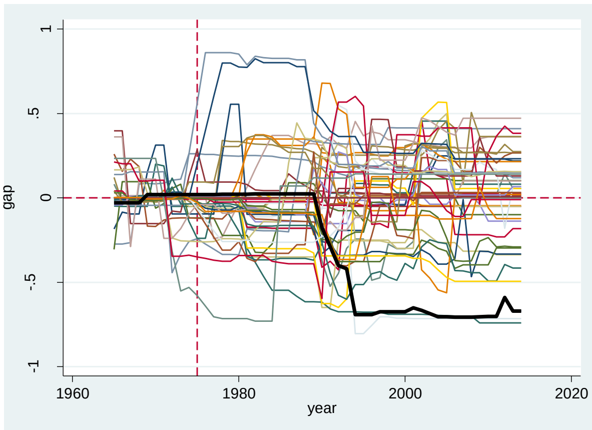


i) Mexico



j) Sudan





k) Tunisia

l) Vietnam

Appendix

Table A.1 – Discoveries excluded from the case studies

Country	*Peak of oil discoveries*	*Country*	*Peak of oil discoveries*
Albania	1928	*Italy*	1981
Algeria	1956	*Kuwait*	1938
Angola	1971	*Libya*	1961
Argentina	1960	*Nigeria*	1967
Australia	1967	*Oman*	1962
Austria	1947	*Peru*	1861
Azerbaijan	1871	*Qatar*	1940
Bahrain	1932	*Romania*	1857
Bolivia	1966	*Russia*	1960
Canada	1958	*Saudi Arabia*	1948
Chile	1960	*Syria*	1966
China	1959	*Trinidad*	1959
Croatia	1950	*Turkey*	1969
Ecuador	1969	*Turkmenistan*	1964
Egypt	1965	*Ukraine*	1962
France	1958	*United Arab Emirates*	1980
Germany	1952	*United States*	1930
Hungary	1964	*Uzbekistan*	1992
Indonesia	1945	*Venezuela*	1941
Iran	1961	*Yemen*	1978

Table A.2 – Country weights in the synthetic control and potential controls

	Brazil
Synthetic control	Central African Republic (0.01), Democratic Republic of the Congo (0 .153), Morocco (0.004), Portugal (0.833)
Potential controls	Belgium, Bulgaria, Burundi, Sri Lanka, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic , Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zambia
	Cameroon
Synthetic control	Benin (0 .075), Nepal (0.368), Niger (0.049), Paraguay (0.301), Tanzania (0.156), Uruguay (0.051)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Morocco, New Zealand, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Togo, Zambia
	Chad
Synthetic control	Bhutan (0.286), Ethiopia (0.007), Honduras (0.027), Malawi (0.169), Nepal (0.052), Paraguay (0.434), Portugal (0.023)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Guinea, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Liberia, Luxembourg, Madagascar, Mali, Mauritania, Mongolia, Morocco, New Zealand, Niger, Philippines, Poland, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Burkina Faso, Uruguay, Zambia
	Colombia
Synthetic control	Nepal (0.007), Spain (0.884), Tanzania (0.048), Zambia (0.061)
Potential controls	Bangladesh, Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Czech Republic, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mauritius, Mongolia, Morocco, Mozambique, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Swaziland, Sweden, Switzerland, Togo, Uganda, Uruguay, Zimbabwe
	Republic of Congo
Synthetic control	Jordan (0.228), Liberia (0.669), Zambia (0.103)
Potential controls	Bangladesh, Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Luxembourg, Malawi, Mali, Mauritania, Mauritius, Mongolia, Morocco, Nepal, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Swaziland, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zimbabwe
	Gabon
Synthetic control	Mauritania (0.336), Singapore (0.001), Swaziland (0.664)
Potential controls	Bangladesh, Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritius, Mongolia, Morocco, Nepal, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, South Africa, Zimbabwe, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zambia

Table A.2 (continued) – Country weights in the synthetic control and potential controls

	India
Synthetic control	Costa Rica (0.429), Japan (0.517), Lao People's Democratic Republic (0.001), Nepal (0.051), Zambia (0.002)
Potential controls	Belgium, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Jordan, Kenya, Republic of Korea, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Morocco, New Zealand, Niger, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay
	Kazakhstan
Synthetic control	Lao People's Democratic Republic (0.349), Liberia (0.095), Singapore (0.556)
Potential controls	Bangladesh, Armenia, Belgium, Botswana, Bulgaria, Burundi, Cambodia, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Czech Republic, Benin, Dominican Republic, El Salvador, Estonia, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Kyrgyzstan, Lesotho, Latvia, Lithuania, Luxembourg, Malawi, Mali, Mauritania, Mauritius, Mongolia, Moldova, Morocco, Mozambique, Namibia, Nepal, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Slovenia, South Africa, Spain, Swaziland, Sweden, Switzerland, Tajikistan, Togo, Uganda, Tanzania, Uruguay, Zambia, Zimbabwe
	Mexico
Synthetic control	Democratic Republic of the Congo (0.067), Japan (0.083), Nepal (0.142), Poland (0.57), Togo (0.137)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Morocco, New Zealand, Niger, Paraguay, Philippines, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Tanzania, Uruguay, Zambia
	Sudan
Synthetic control	Greece (0.014), Jordan (0.126), Mali (0.385), Tanzania (0.267), Uruguay (0.208)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Kenya, Republic of Korea, Lao People's Democratic Republic , Lesotho, Liberia, Luxembourg, Malawi, Mauritania, Mauritius, Mongolia, Morocco, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Swaziland, Sweden, Switzerland, Togo, Zambia, Zimbabwe
	Tunisia
Synthetic control	Jordan (0.485), Mongolia (0.034), Nepal (0.257), Paraguay (0.224)
Potential controls	Belgium, Bulgaria, Central African Republic, Sri Lanka, Congo(Demo), Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Korea(Rep), Lao, Liberia, Luxembourg, Mali, Mauritania, Mongolia, Morocco, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay
	Vietnam
Synthetic control	Bulgaria (0.355), Jordan (0.143), Malawi (0.035), Mali (0.297), Portugal (0.119), Singapore (0.05)
Potential controls	Belgium, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Kenya, Republic of Korea, Lao People's Democratic Republic , Liberia, Luxembourg, Mauritania, Mongolia, Morocco, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Rwanda, Senegal, Sierra Leone, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zambia