The Economic Impact of Natural Resources[§]

Torben K. Mideksa[§§]

July 8, 2012

Abstract

This paper explores the economic impact of natural resource endowment using quantitative comparative case study. Focusing on the Norwegian economy, due to availability of good quality data, the paper thoroughly examines the impact of petroleum endowment. Although the result suggests that the impact varies from year to year, it remains positive and very large. On average, about 20% of the annual GDP per capita increase is due to the endowment of petroleum resources such as oil, natural gas, natural gas liquids, and condensate. Examinations based on sensitivity test, robustness test, dose-response test, and various falsification tests suggest that the finding is robust to alternative explanations. (**JEL Codes:** O13, O14, Q3)

I. Introduction

What is the economic impact of natural resources? The answer to this question has attracted considerable interest through decades of development economics, macroeconomics, resource economics, and political economics research. Most of the existing empirical studies approach this problem by aggregating heterogeneous natural resources and studying the experience of a set of different countries, states, counties, etc. together. Since most economies with the exception of countries such as Israel, Japan, and South Korea possess large endowments of at least one valuable natural resource, it is very hard to interpret the results of many of the existing studies in light of the fundamental question of *how an economy would have evolved*

[§]The funding for this research came from the Center for International Climate and Environmental Research - Oslo (CICERO). The paper is also part of a research project at the *center of Equality, Social Organization, and Performance (*ESOP), at the Department of Economics, University of Oslo. ESOP is supported by the Research Council of Norway. A previous version of this paper that focused only on endowment of oil is titled "The Economic Impact of Oil: the Case of Norway." It was first prepared as a partial fulfillment for the joint course "Environmental Economics and Policy Seminar" given by Robert Stavins and Martin Weitzman at the Harvard Kennedy School and the Economics Department. I am very grateful to both professors being very accessible and inspiring teachers. Moreover, Robert Stavins had provided regular guidance, feedbacks, and encouragement throughout the course year. Anonymous referees and the editor Daniel J. Phaneuf provided numerous comments that have improved the substance and presentation of the paper. The paper has also benefited from the advice of Alberto Abadie and the comments I had from Sinafikeh Asrat, Todd Cherry, Susana Cordeiro Guerra, Cathrine Hagem, Snorre Kvernokk, Jorge Garcia Lopez, Espen Beer Prydz, Håkon Sælen, Steffen Kallbekken, Sigrid Rian, Hege Westskog, and economist colleagues as well as participants of seminars at CICERO and University of Oslo. All remaining mistakes or omissions are mine.
[§§]CICERO, Oslo 1129 Blindern N-0318 Norway. Email: torben_mideksa@post.harvard.edu

in the absence of natural resource endowments. To make progress, this paper takes a radically different approach to the existing literature. Instead of confronting the most complicated task of estimating the expected impact of all natural resources for a given set of economies, it attacks the simpler problem of estimating the impact of a particular set of natural resource endowments for a single economy. By focusing on a single economy and isolating the impact of a particular set of natural resources, it provides a microscopic perspective on the issue and offers complementary evidence to the existing literature.

One would expect, at least theoretically, that the discovery of natural capital would, similar to other sources of increased wealth, raise national welfare and enable nations to consume more goods and services. In the standard growth models, (Solow, 1956; Romer, 1990; and Aghion and Howitt, 1992), new discoveries of natural resource endowments shift the aggregate production possibilities outward by expanding natural capital thereby raising growth for a short period and the level of income for a long period. Sachs (2007) argues that oil revenue can be an important resource for promoting economic development. Indeed, the sustainability literature treats natural resources as wealth in the comprehensive national income accounting. In green national income accounting, for example Weitzman (2003), the discovery of natural resource endowments is interpreted as an increase in the nation's wealth, while activities, such as extraction and use that decrease the stock of this endowment, are interpreted as depreciation of natural capital.

There is some empirical evidence supporting the view that natural resource endowments are economic blessings. Focusing on the world economy at large and using indirect valuation with the World Bank data, Weitzman (1999) concludes that running out of all non-renewable minerals, i.e. crude oil, coal, natural gas, bauxite, copper, iron ore, lead, nickel, phosphate, tin, zinc, gold, and silver would reduce world GDP by about 1% assuming that historical prices *correctly* reflect scarcity values. In a cross-country study focusing on robust determinants of economic growth, Sala-i-Martin et al. (2004) find that the fraction of GDP in mining has a robust positive association with economic growth. The result is consistent with recent empirical works by Brunnschweiler & Bulte (2008)[1] as well as Alexeev and Conard (2009), who also suggest that a large endowment of oil and other mineral resources is positively correlated with long-term average economic well-being.

[1]van der Ploeg and Poelhekke (2010) argue that Brunnschweiler & Bulte (2008) used weak instruments. Using better instrumental variables, they find that the results of Brunnschweiler & Bulte (2008) are not robust. Instead, they find no evidence for a resource curse or blessings similar to Ding and Field (2005).

Nevertheless, another strand of the analytical and empirical literature has emerged with the opposite conclusion: instead of raising wealth, natural resource endowments are economic curses, which trap an economy in lower wealth equilibrium relative to the equilibrium without natural resource endowments. This sub-literature can be classified into at least six major categories on the basis of the main mechanisms of the economic curse.[2]

First, the extra income generated from sale of natural resources appreciates the real exchange rate, and leads to a contraction of the tradable sector. This is the text-book Dutch disease mechanism, which is consistent with no or little change in GDP in response to large windfall gains of natural resources such as natural gas and oil. Second, natural resource windfalls can result in sectoral misallocations and temporary loss of positive externalities of knowledge from missed opportunities for learning by doing in contracting sectors. The endogenous growth model of Matsuyama (1992) and its comparative static results in Rodrik and Rodríguez (2001) suggest that natural resource-driven growth can trap an economy in a dynamically inefficient equilibrium. This is because there could be loss of learning by doing and other knowledge externalities associated with the more dynamic tradable sector. Third, revenues from natural resources may misallocate talent since such revenues are prone to rent seeking. Torvik (2002) shows that with aggregate demand externalities productive entrepreneurs switch to rent seeking in response to a resource boom. Fourth, natural resource windfalls tend to raise corruption. For example, Vicente (2010) uses Cape Verde as comparison unit to São Tomé and Príncipe to show the rise of perceived corruption and the presence of a political oil curse in response to the 1997-1999 natural experiment of Sao Tome and Principe's oil discovery announcements. In a model of multiple equilibriums, Andvig and Moene (1990) show that a temporary increase of corruption can lock an economy into a higher state of corruption. Fifth, the presence of natural resource rents raises the frequency of armed conflict in weak states. Collier and Hoeffler (2004) and Fearon (2005) provide cross-country evidence that there is a positive correlation between resource income and armed conflict. Last, but not least, revenue from natural resource endowments tends to sustain bad policies, which could not have been sustained without windfall income. Mansoorian (1991) observed that resource rich countries tend to accumulate huge debts, which harms their economy both in the short and long run. Manzano and Rigobon (2001) provided a suggestive evidence of this mechanism.

[2] It is beyond the scope of this paper to provide a complete review of the large literature on the resource curse. The coverage here is selective, perhaps idiosyncratic; one is recommended to read Frankel (2010) and van der Ploeg (2011) for a well-articulated, more comprehensive, and up-to-date synthesis of the literature.

There is also a large empirical literature supporting the hypothesis of the natural resources curse. The first empirical evidence is from Sachs and Warner (1995). Hausmann and Rigobon (2002) as well as Sala-i-Martin and Subramanian (2003) argue that that oil and other minerals have a negative impact on growth. Sachs and Warner (2001) argue that "empirical studies have shown that this curse is a reasonably solid fact." van der Ploeg (2011) presents a good synthesis of the literature, which offers suggestive evidence for most mechanisms of the resource curse. At a greater disaggregation level, Papyrakis and Gerlagh (2007) study variations within the US states and observe that resource-rich states tend to have a comparative disadvantage in development relative to resource-poor states. James and Aadland (2011) focus on US counties and test whether the resource curse is present at the county level. Like Papyrakis and Gerlagh (2007) they find that natural resource earnings have a statistically significant negative effect on county-level economic growth. In a similar vein, Caselli and Michaels (2011) investigate the effect of resource windfalls on government behavior using variations in oil output among Brazilian municipalities. They find significant changes in expenditures on urban infrastructure, education, health services, and transfers; but no increase in economic and social outcomes, which are supposed to respond to increased spending on urban infrastructure, education, health services, and transfers. [3]

In general, the theoretical and empirical literatures on the economic impact of natural resource windfalls are divergent. One strand emphasizes the opportunities, i.e. the additional capital stock such a windfall brings to an economy; while the other strand emphasizes various deleterious mechanisms through which income from resource windfalls can reduce sustainable wealth.[4] Some empirical researchers find evidence for economic blessing, others find evidence for economic blessing conditional on having good institutions, and many others find evidence for an economic curse.

However, most empirical studies suffer from deeper analytical problems and the statistical validity of the evidence mentioned in the literature is limited for the following reasons. First, much of the empirical evidence is drawn from cross-country regression results. Cross-country regression based evidence offers opportunities to understand the impact of natural resource capital on national income across many countries. Since such evidence comes

[3] The fact that all the three, more disaggregated, studies point towards a resource curse instead of blessing, is interesting in itself although the evidence is based on the experience of Brazil and the US.

[4] Mehlum et al. (2006) is an exception to the literature. According to Mehlum et al. (2006), these negative mechanisms are effective only conditional on having poor political and economic institution at the time of resource discovery. Mehlum et al. (2006) also present evidence for a positive economic impact of natural resources when the quality of institutions is high.

from datasets with large numbers of observations from different countries, the evidence can be generalized to a broad range of countries. Although evidence from cross-country regressions is useful and, sometimes, the only source for reasonable answers to important questions, the presence of contradicting empirical evidence about the impact of natural resources suggests that such estimates are unlikely to be internally valid. In many cases, controlling for an additional variable changes the sign of the estimate. One cannot be confident that all relevant variables are included, largely because of data limitations. The recommended solution is to apply instrumental variables, though its use is severely constrained due to the difficulty of finding good instruments.

Second, the lack of an appropriate comparison unit poses challenges for impact estimation. Since most cross-country studies on resource curse lack a relevant comparison or control unit that describes how the economy would evolve in the absence of a particular natural capital endowment, it is almost impossible to estimate the impact of natural resources on national income. For example, many attribute Nigeria's poor economic performance to its oil endowment. Sala-i-Martin and Subramanian (2003) write "… all the oil revenues — US$350 billion in total — did not seem to add to the standard of living at all… the main problem affecting the Nigerian economy is the fact that the oil revenues … lower the long-term growth prospects." However, without establishing how the Nigerian economy might have evolved in the absence of the oil endowment, it is difficult to accept that an oil endowment leads to poor economic performance. Although there are clear indicators that Nigeria has not been using its oil revenue in the most efficient way, how can one rule out the possibility that the economy might have performed even worse in the absence of the endowment of oil?

One way of getting around the second problem is to use a comparison unit generated by natural experiments. For example, Vicente (2010) uses natural experiments of the oil discovery announcements in São Tomé and Príncipe in 1997-1999 to show that the perceived corruption has increased, and that the political oil curse is present. However, natural experiments are not a panacea. When natural experiments are found, the evidence they offer often fails to capture general equilibrium effects at the economy level and may miss the main impact of natural resources. General equilibrium effects are relevant because many of the channels through which natural resources affect national welfare work at an aggregate level.[5]

[5] For example, Sachs (2007) argues that windfalls from increases in commodity prices or natural resource discoveries can finance investment that serve as a big push towards industrialization. Macroeconomists argue about whether or not the revenues from natural resource exports affect the nature of fiscal policy, the pattern of

This is why Acemoglu (2010) argues that “counterfactual analysis based on microeconomic data that ignores general equilibrium and political economy issues may lead to misleading conclusions.”

Third, in the absence of sufficient data characterizing the pre-natural resource endowment economy, it is difficult to determine if the alleged performance with the endowment of natural resources could have been driven by pre-existing differences in variables other than natural resource endowment. For example, James and Aadland (2011) compare Wyoming and Maine to establish the presence or absence of the natural resource curse. Wyoming, which depends heavily on natural resources, is alleged to underperform relative to Maine, which has a very low share of income from natural resources, due to the natural resource curse. In the absence of data prior to Wyoming’s discovery and extraction of natural resource endowments, how would one establish that the pre-existing differences of income between Maine and Wyoming were not the driving factors behind the worse economic performance of Wyoming?

Estimating the impact of all natural resources is very complicated due to the inherent difficulty of estimating how an economy might have performed in the absence of the natural resource. There are very few countries without significant endowments of natural resources which can be used as a basis for comparison. The challenge is therefore to establish that economies like Israel, Japan, and South Korea have fared very well precisely because they do not have natural resource endowments.

Given the complexity of the research question the literature poses, this paper takes a step back and approaches the estimation problem from a different and hopefully easier perspective. To simplify the problem, it focuses on the petroleum endowment in Norway due to availability of data and comparable countries suitable for application of the estimation technique this paper adopts. By using good quality data from Norway, which discovered oil in 1971, it studies the impact of petroleum endowment. The results suggest that about 20% of GDP per capita in Norway since the mid-1970s can be attributed to the petroleum endowment.

The rest of this paper proceeds as follows: The next section estimates an optimal comparison unit, which is the main input to arrive at the central results of the paper. Next, the effect of petroleum endowment is estimated and its robustness is explored. A number of

---

equilibrium real exchange rates, etc. that determine the degree of competitiveness of the tradable sector through the Dutch Disease. Political economists underline the deleterious impact of revenue from natural resources.

falsification tests of the main results are presented in the in the third section. The last section presents discussion and concluding remarks.

II. Constructing a Synthetic Norwegian Economy and Estimating the Impact

In a path-breaking comparative case study, Card (1990) studies the effect of the 1980 large influx of Cuban immigration, called the Mariel Boatlift, on the labor market in Miami. The author uses a weighted combination of cities in southern states as a comparison unit for Miami. In another study, Card and Krueger (1994) examine the effect of an increase in New Jersey's minimum wage on employment in fast food restaurants using eastern Pennsylvania as a comparison unit. These comparative case studies approach suggest that measuring impacts requires one estimates the counterfactual outcome in the absence of the intervention from sufficiently similar comparison units.

Despite the insights, there is a key problem with early comparative case studies such as Card (1990): the lack of an empirically rigorous and data-driven way of choosing the best comparison unit when there is more than one option. Although economically similar countries without oil in the pre- and post-treatment period can constitute the donor pool, the selection of a comparison unit is not obvious. A potential solution is to take an “average of all countries” to serve as a comparison unit. This may not be the best answer as some countries are more similar to Norway than others, and, at least in principle, not all countries can have equal weight as a comparison unit. Without a way to get around this choice of the best comparison unit, the impact assessment task is problematic, especially if the selection of a comparison unit is left entirely to researcher’s judgment since it might suffer from, what Rosenbaum (2005) calls, “hidden bias.” However, this challenge is addressed by the synthetic control method of Abadie and Gardeazabal (2003) and Abadie et al. (2010). Their strategy is intuitive: in the same way as Mark Twain’s Tom Sawyer is a combination of different individuals’ characters, the synthetic unit is a convex combination of potential comparison units that often fares better than individual units in reproducing the outcome of the treatment unit in the absence of the treatment.

Since the essence of this exercise is to estimate the impact of petroleum endowment on national income, let us first construct a comparison unit, which involves the construction of a synthetic control unit that resembles the national income of Norway before the petroleum discovery, using potential predictors for GDP per capita of the donor pool. Abadie et al. (2010) describe the analytics and empirical implementation of constructing a synthetic economy in detail. To clarify the reasoning behind the mathematics of generating weights, and the thus the synthetic control unit, suppose J represents the number of countries in the donor pool. The set of countries that constitute the donor pool for the Norwegian economy are non-oil producing OECD nations.[6] To ensure similarity with Norway before the petroleum discovery, the donor pool is limited to the OECD member countries before 1971.[7] Since the best comparison unit is meant to approximate the counterfactual of the unit of interest without the treatment, Abadie et al (2011a) emphasize the importance of limiting the donor pool to units whose outcomes are driven by the same structural process as the outcome of the unit of interest and that are not subject to structural shocks to the outcome variable in the sample period of the study.[8]

Suppose k represents the number of variables that explain GDP per capita. Let X_0 be a $k \times J$ matrix representing the pre-treatment values of determinants of GDP per capita of the donor pool countries, while X_1 is a $k \times 1$ vector of pre-treatment values of determinants of GDP per capita for the treated unit. These variables are: direct resources (i.e. labor force ratio, years of schooling, and the share of private investment in GDP) and some policy indicators (i.e. inflation and openness to international trade). The choice is made partly based on data availability for the donor pool and partly following the economic growth literature, which is summarized in Barro and Sala-i-Martin (2003).

Assuming T^- measures the number of time units before treatment, let Z_0 be a $T^- \times J$ matrix representing the pre-treatment values of GDP per capita of the donor pool countries and Z_1 be a $T^- \times 1$ vector representing the pre-treatment values of GDP per capita for the treated unit. Similarly, assuming T^+ measures the number of time units on and after treatment, let Y_0 be a $T^+ \times J$ matrix representing the post-treatment values of GDP per capita of the donor pool countries and Y_1 be a $T^+ \times 1$vector representing the post-treatment values of GDP per capita for the treated unit.

[6]Unless explicitly stated, the data is from Heston et al (2011) and schooling data is from Barro and Lee (2010).

[7]Australia joined the OECD club on June 7, 1971 - the same year Norway discovered oil. Since Australia's accession to the OECD has nothing to do with Norwegian economy's treatment, this need not be a problem. In the following sections, we conduct sensitivity test by excluding Australia and other countries from the donor pool and show how the main result is sensitive to inclusion or exclusion of such countries from the donor pool.

[8]Test for structural shocks was performed and the results suggest that Ireland and Luxembourg need to be excluded from the donor pool because the income from these economies possesses different structural processes from the rest of the donor pool. Abadie et al (2011a) also exclude these two economies for their donor pool in estimating the economic impact of reunification of Germany.

Suppose also that $\omega = (\omega_1, \omega_2, \dots, \omega_J)'$ is a vector of weights on the donor pool economies in the construction of the synthetic control unit. The vector of optimal weights is defined by $\omega^* = \text{argmin}_{\omega}[X_1 - X_0\,\omega]'\,\upsilon[X_1 - X_0\,\omega]$ subject to the constraints $\omega_i \geq 0$ and $\sum_{i=1}^{J} \omega_i = 1$. υ is any $k \times k$ diagonal matrix with non-negative elements. This implies that the weights need to be chosen to fit the pre-treatment predictors of GDP per capita of the synthetic unit with the treated unit. Since the optimal weight depends upon the diagonal matrix ($\omega^* = \omega(\upsilon)$), the value of υ is chosen to fit the pre-treatment GDP per capita between the treatment and comparison unit.[9]

The weights are generated not only based on pre-treatment similarity of the GDP per capita, but also based on predictors of income such as investment rate, human capital, labor participation, and policy indicators. Thus, the weights are generated through a data driven approach with no personal judgment involved except on the choice of the donor pool, making the process of choosing the best comparison unit from the donor pool rigorous and transparent.

Table 1: Weights assigned to the donor pool in the construction of the synthetic unit.

Country	Weight	Country	Weight
Australia	0.285	Iceland	0.125
Austria	0	Japan	0
Belgium	0.437	Portugal	0
Finland	0	Spain	0
France	0	Sweden	0
Greece	0	Switzerland	.152

The weights that create the best synthetic Norwegian economy from the set of countries given in the donor pool are estimated using the algorithm by Abadie et al. (2011b) and are presented in table-1. The table also offers an interesting result, which may appear counterintuitive. Most Northern European neighbouring countries are given zero weight and the synthetic version of Norway is a convex combination of Australia, Belgium, Iceland, and Switzerland.[10]

[9] Technically, $\upsilon^* = \text{argmin}_{\upsilon \in \upsilon}[Z_1 - Z_0\,\omega(\upsilon)]'\,\upsilon[Z_1 - Z_0\,\omega(\upsilon)]$ subject to the condition that $\|\upsilon^*\| = 1$ and thus the optimal weights are generated according to the rule $\omega^* = \omega(\upsilon^*)$.

[10] Since Denmark and Sweden have been used as a comparison group in the previous literature, for example Larsen (2004), it is necessary to explore why this has happened. This is mainly due to the way that the synthetic control technique works. In generating the synthetic unit, the weights in table 1 are chosen to fit the GDP per capita before the treatment and the set of predictor variables for GDP per capita for the synthetic unit as well as the treatment unit. In this sense, the synthetic economy mimics the real economy not only in terms of GDP per capita but also in terms of the other variables which are potential predictors of GDP per capita. It is necessary also to note that weights assigned on countries of the donor pool change in response to removing (including) an

Once the weights are determined, the synthetic economy's determinants of GDP per capita are reproduced from the pre-treatment values of determinants of GDP per capita of the donor pool X_0 using weights on the donor pool economies ω^*. To present this formally, let ${X_1}^*$ be a $k \times 1$ vector representing the pre-treatment values of determinants of GDP per capita for the synthetic control unit. This value is generated using ω^* and the pre-treatment values of determinants of GDP per capita for the donor pool according to the formula ${X_1}^* = X_0\omega^*$. Using the weights obtained entirely through a data-driven mechanism, the synthetic Norwegian economy is generated and its economic characterics are given in table 2.

Table 2: Pre-treatment characteristics of the synthetic and the actual economy

Variables	Treated Economy	Synthetic Economy
Private investment's share of GDP (%)	30.6387	24.6118
Labor participation ratio	0.4139	0.4247
Average Years of Schooling	7.8258	7.6843
Openness to trade	44.9775	39.5490
Inflation Rate (%)	1.72	0.42

Non-trivial application of the synthetic control requires an acceptable resemblance between the synthetic economy and the "to be treated economy" prior to the point of intervention. According to the results in table 2, almost all characteristics of the Norwegian economy and its synthetic replica are similar. The optimally chosen weights fit different characteristics very well. In fact, with further assessment reflected in figure 1, the overall fit is acceptable for the task at hand.[11]

economy from (to) the donor pool. This is not a problem in itself as long as the ultimate subject of such a study- i.e. estimated treatment effect - is robust to such changes.

[11]A formal hypothesis test of mean difference between the outcome of the synthetic and treated unit suggests that there is no statistical difference between the income of the synthetic and actual Norway, with 0.1% significance level, prior to the intervention.

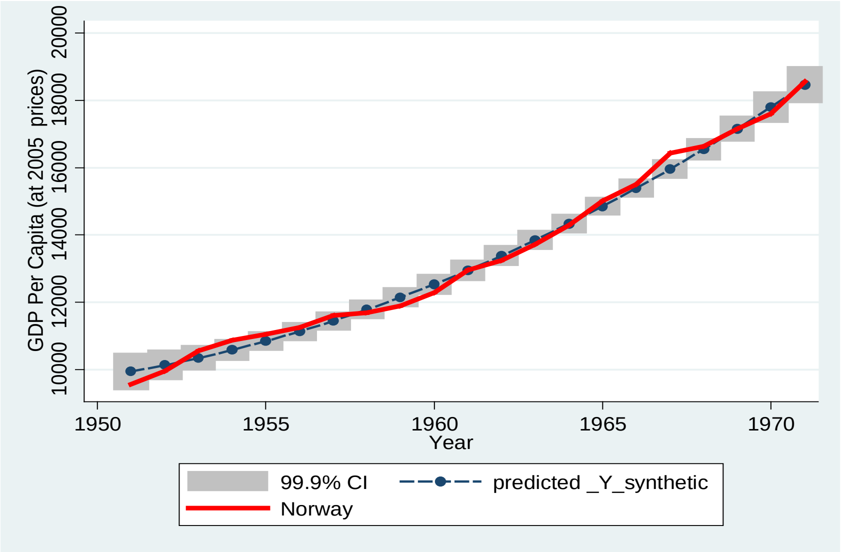

Figure-1: Evolution of GDP per capita of Norway and its synthetic version (with 99.9% confidence interval) Prior to 1971

What is the economic impact of the petroleum endowment? How do the control and the treatment units evolve after the point of treatment? By comparing the evolution of the variable of interest in the control group with that of the synthetic control group, in the spirits of a "before-after" approach, one can obtain a good prediction of the potential impact. Since the comparison group lacks the treatment for the whole period of analysis, the difference in the value of the variable of interest can be attributed to the impact of the treatment. The post treatment values of GDP per capita of the synthetic unit is given by ${Y_1}^* = Y_0\omega^*$ and thus the absolute impact of the treatment for a given time is given by $Y_1 - {Y_1}^*$, whereas the relative impact of the treatment is given by $\frac{Y_1 - {Y_1}^*}{{Y_1}^*}$.

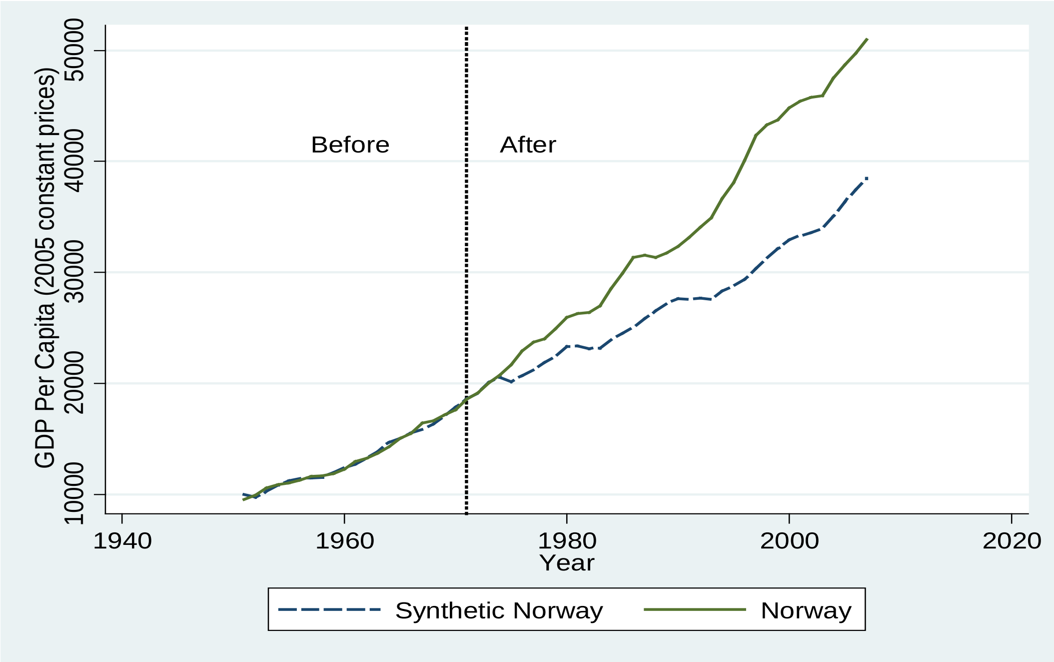

Figure-2 (Panel-A): Evolution of Income in Synthetic Norway and Norway

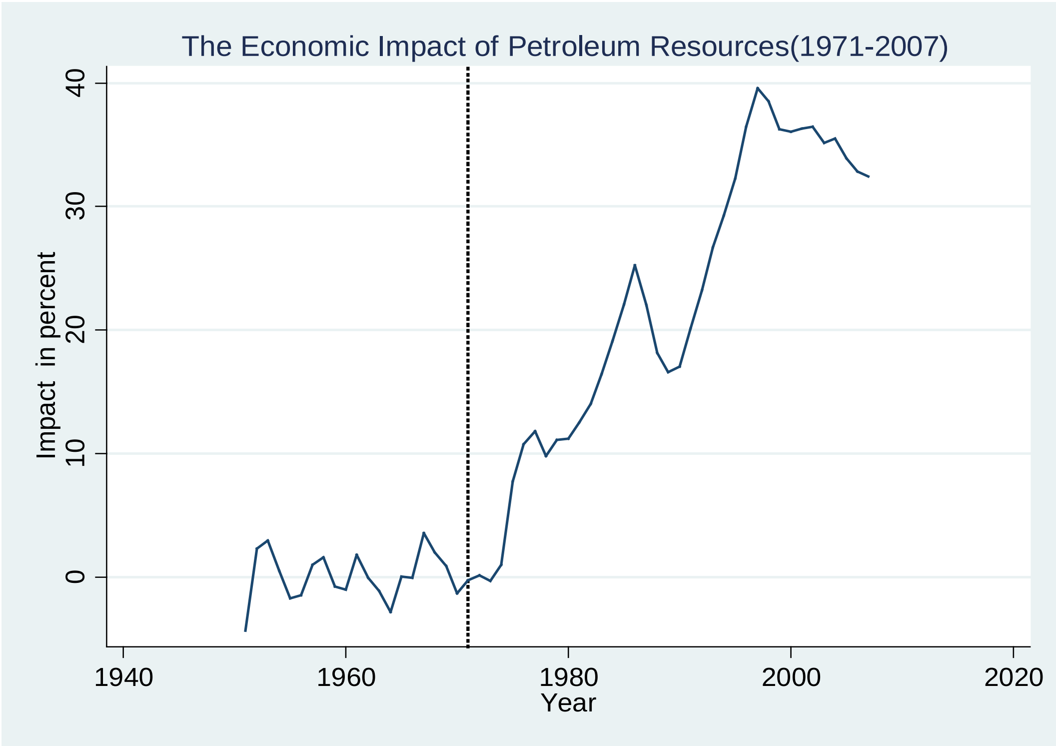

Figure-2(Panel-B): The Estimated Impact Petroleum Endowment on Norwegian Economy

Figure 2 summarizes an answer to the central question of this paper. The panel A plots the evolution of per capita income of the actual and the synthetic unit from 1951 to 2007; whereas panel B plots the percentage difference of the GDP per capita between the treated and the synthetic unit.[12] As can be seen from figure 2 (or footnote 11), the economy and its synthetic counterpart are similar in terms of GDP per capita prior to the treatment. With such a good fit for 18 consecutive years, it is plausible to assume that the synthetic unit can serve as a good comparison unit.

The figure in panel A makes it clear that that the treated economy has perceptibly departed from the synthetic economy once the treatment is applied in 1971. A formal hypothesis test of mean equivalence rejected the equality of the mean of the two trends, after the intervention, with 0.1% significance level. Therefore, endowment of petroleum has a statistically significant effect on per capita GDP.

Table 3: The amount of Petroleum produced and its estimated impact on the national income

Years	Average Treatment Effect/year [Differences in %]	Average Annual Petroleum Production (1000 Sm3)*
1971-1975	1.66	4201.50
1976-1980	10.95	34102.00
1981-1985	16.83	63039.00
1986-1990	19.82	102149.20
1991-1995	26.31	166867.00
1996-2000	37.38	231177.60
2001-2007	34.66	254190.14
1974-2007	23.76	132855.8611

* Petroleum production includes crude oil, natural gas, natural gas liquids, and condensates.

Source: Statistics Norway (http://www.ssb.no/aarbok/tab/tab-385.html).

Since the central question of this paper is quantitative, table 3 presents the quantitative summary of the average impact over time presented in the right panel of figure 3. During 1976-1980, an average of 11% of annual GDP per capita can be attributed to the petroleum endowment. In the following five years, the average impact increases to double digits. Over the entire period, the average impact of petroleum on GDP per capita is roughly 20% per year for an average of 1.3 million (1000 Sm3) of petroleum extracted per day.

The results also suggest that the impact of the petroleum discovery on GDP per capita did not begin until 1974. This is due to at least two important factors. First, the amount of petroleum extracted in the first two years was very small. According to data from the U.S. Energy Information Administration (EIA), the daily extraction was less than 30,000 barrels

[12] The vertical line denotes the year (1971) in which Norway begun extracting petroleum.

per day (BPD).[13] As documented in Austvik (1993), extraction from the first oil field, Ekofisk, was shipped by boat and it did not have an impact until it was connected with the mainland European pipelines of Teesside and Emden in 1975. Thus, the amount of petroleum extraction was lower prior to 1975. Second, according to Norwegian Continental Shelf Journal (2010), the use of the petroleum revenue was limited until the release and adoption of the 1974 White Paper, which outlined how to develop petroleum operations in Norway and how the government can use the revenue. Hence, it is reasonable to expect the impacts of the treatment to lag by a couple of years until the revenue from the sector flows into the economy.[14]

These results open up many questions which this paper cannot address fully. In the rest of this section, the focus is limited to a set of issues related to the robustness of the results, i.e. to what extent the magnitude of estimated impact remains robust to inclusion of additional predictors of GDP per capita, and how sensitive the estimated impact is for sequentially dropping countries with positive weight in the donor pool before proceeding to different falsification tests.

A Robustness Test: A robustness tests involves examining to what extent the reported result is sensitive to using additional predictors of GDP per capita. To this end, a synthetic control unit is generated by controlling for exchange rate, private consumption's share of GDP, government consumption's share of GDP, in addition to the standard variables mentioned in table 2. These are additional measures of international openness and aggregate demand, which might affect GDP per capita at least in the short run. Figure 3 presents the result of these robustness tests.

[13]This number is very low when compared with the amount of extraction by the end of the decade, which was close to 500,000 barrels per day.

[14]The result that the impact of oil did not significantly affected income until 1974 is also established in the literature, using entirely different approaches. For example Larsen (2004) uses structural break approach and finds that there was oil-induced sudden acceleration in 1974. Although it is difficult to tell exactly, the simulation graphs of Bjørnland (1998) using the var-approach suggests that the impact energy boom did not occur immediately.

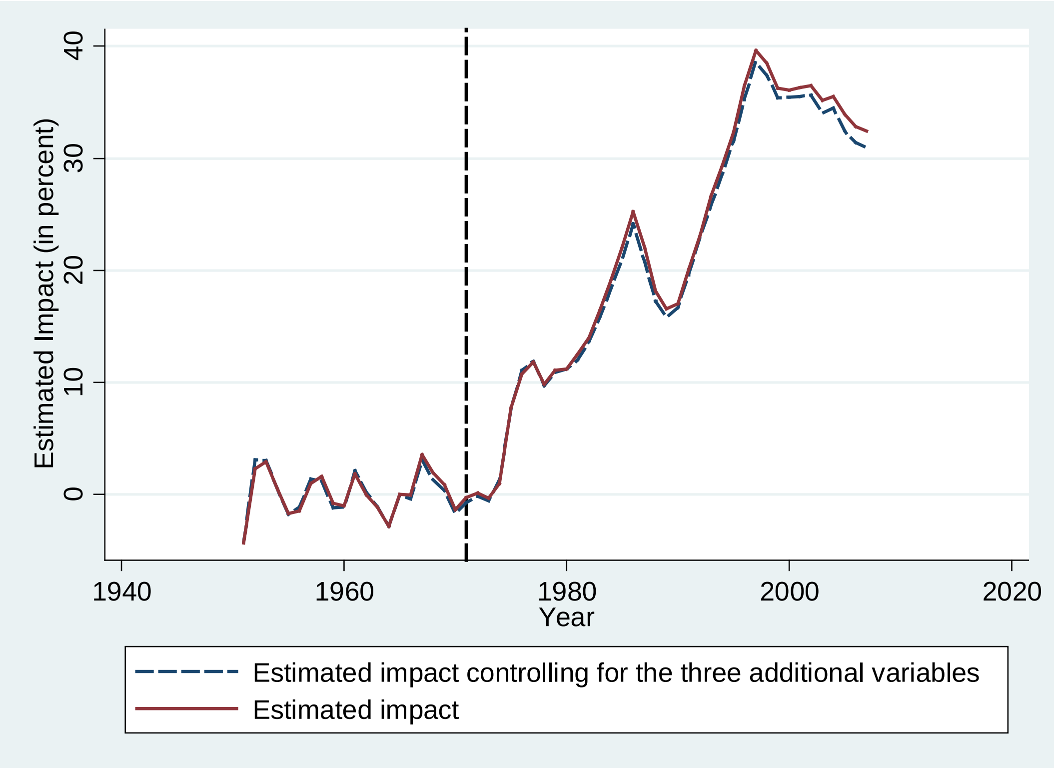

Figure-3: Robustness test of controlling for additional variables on estimated GDP per capita gap.

In general, it is very hard to detect any difference in the pattern of the estimated impacts between the estimated impact reported in figure-2 and the estimated impact for the robustness test. The main message is that the estimated result is surprisingly robust to controlling for these additional indicators.[15] In general, the optimal weights generating the synthetic unit change when conducting robustness test (also sensitivity and robustness tests); but the ultimate variable of interest i.e. estimated impact has not changed much in response to controlling for these additional variables.

Sensitivity Tests: The following sensitivity test focuses on the implications of missing regions or data for the main result. One cannot know, a priori, the potential effect of omission of some countries from the donor pool on the estimated impact. Omission can happen for various reasons such as unavailability of data. For example, Germany is not included in the analysis because of missing data due to the reunification of Germany in 1990. Although Germany may or may not have received positive weights, one might wonder if the omission of potential regions from the donor pool affects the estimated impact. To this end, the paper has conducted a series of tests sequentially removing the main countries used in the construction of the synthetic unit. The results are summarized in figure 4.

[15] The results are also robust to controling initial GDP per capita.

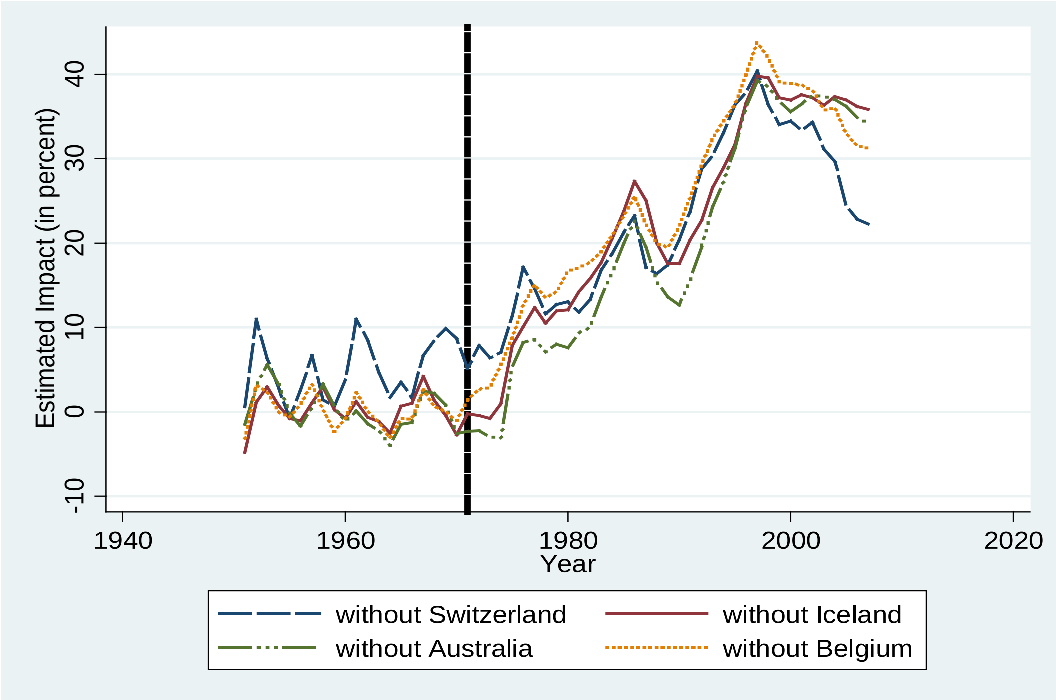

Figure-4: Sensitivity Test of Sequentially Excluding Countries in the Synthetic Control Unit.

Figure 4 plots the treatment effect, in percent, of petroleum discovery given that a particular country is excluded from the donor pool. By comparing the results from figure 4 with that of figure 2, one can observe that the estimated impact is robust to these changes. [16]

III. Some falsification tests

Both the robustness and sensitivity tests suggest that the main result is stable in response to controlling for additional variables or sequentially excluding countries from the donor pool. In this section, I employ dose-response and placebo intervention as a falsification strategy to examine if the result is robust to alternative explanations.

A. Dose-response Test

A potential concern with the previous analysis is that the observed gap between the synthetic and the treated unit could be driven by another factor or event, which occurred in 1974 or the same year that the oil revenue was being used for the first time. Although there was no other known significant intervention (or economically relevant event) in 1974, what amounts to a significant policy is generally unknown (especially given that our understanding of the causes

[16]Excluding Australia from the donor pool provides a synthetic Norway that derives 59%, 17%, and 24% of its weights from Belgium, Iceland, and Switzerland respectively. The synthetic Norway's labor force ratio, years of schooling, the share of private investment in GDP, inflation, and openness to international trade is 0.4307, 7.0721, 25.5334, 0.11%, and 47.32 respectively.

of economic growth is still very limited). Strictly speaking, the fact that one does not know of an intervention that might have happened in 1974, either from the literature or from history, does not suggest that the "mysterious event" cannot be the main driver of the observed impact. If an unknown intervention is the key driver, then the result might have been incorrectly attributed to the country's petroleum resource endowment.

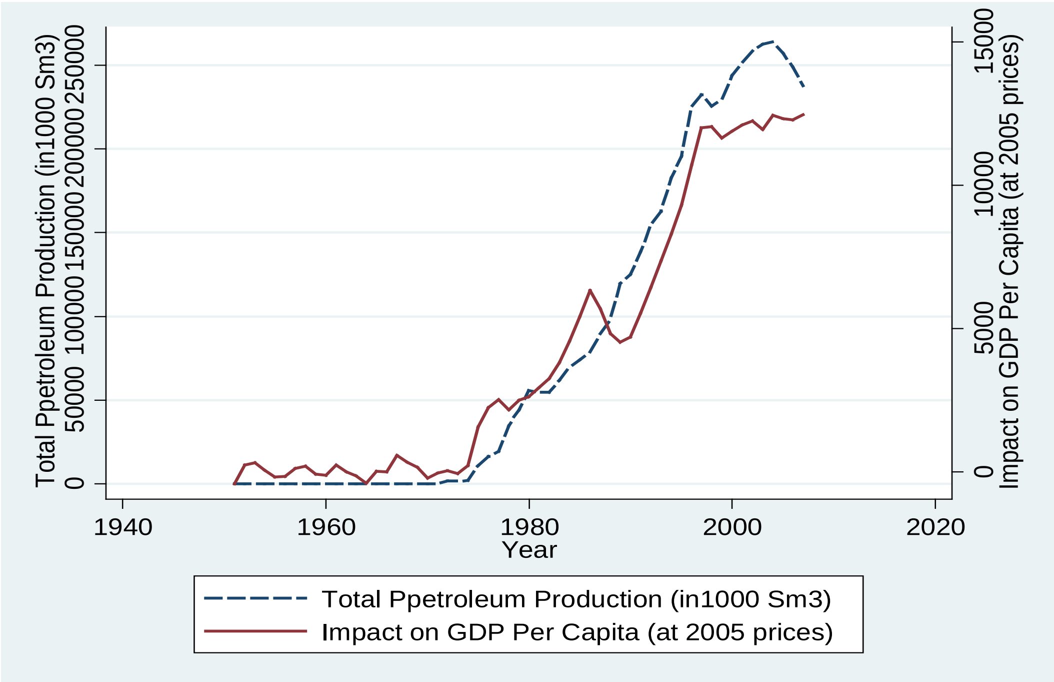

Figure-5: Annual petroleum production and estimated GDP per capita gap, which is the difference between the treated and the synthetic unit's GDP per capita.[17]

A potentially insightful approach attempts to falsify the estimated impact in light of data not used in the estimation. It is essential to note that the quantity of petroleum extracted and exported has never been used in the estimation of the synthetic unit or the treatment effect. In this section, I use this external information to examine if the estimated annual impact is somehow related to the annual quantities of petroleum extracted. By linking the quantity of petroleum extracted (the treatment) with the observed impact on national income (the response), one can examine if the treatment can plausibly explain the alleged response, i.e. the observed impact. If the estimated gap in GDP per capita is driven by something else, which is unrelated to the extraction of petroleum, it is likely that the pattern of the estimated impact

[17] The data for total petroleum production is from a table by Statistics Norway (http://www.ssb.no/aarbok/tab/tab-385.html).

cannot be correlated with the pattern of petroleum production year by year over a long period of time.

To this end, figure 5 plots the impact on GDP per capita and annual petroleum extraction. The x-axis measures years, the left y-axis measures the average amount of petroleum extraction per year, and the right y-axis measures the absolute gap in GDP per capita between the synthetic and treated units. The figure relates the observed gap between the synthetic and treated units with the quantity of petroleum extracted for commercial purpose.

Figure 5 suggests that the estimated GDP gap closely follows the average annual extraction of petroleum and the correlation between the two variables is clearly not random. In fact, the pattern of the estimated impact is strikingly similar to the pattern of petroleum production. Had the observed gap been driven by something else unrelated to petroleum extraction, it would have been less likely that one could observe such a close connection between petroleum production and the observed impact.

Although the quantitative connection between petroleum extraction and estimated impact is not random, it is legitimate to suspect that the reported impact might have been driven by the approach's failure to replicate the actual GDP per capita. To address this concern, the next section examines if the reported impact is driven by the method's inability to reproduce the income trajectory of the treated economy in the absence of petroleum discovery - using placebo tests of fictitious event of resource discovery.

B. Placebo tests

One can conjecture that the gap between the synthetic and the treatment units is caused by the synthetic unit's inability to replicate the treatment unit's outcome. One way to deal with this concern is to use placebo tests using fictitious event of resource discovery as a falsification strategy, i.e. using outcomes known to be unaffected by the treatment as Rosenbaum (2005) suggests. That is, by applying the technique on outcomes that have not received the treatment, the result of fictitious treatment would shed some insight on whether the reported impacts are driven by the method's inability to replicate outcome in the absence of the intervention. If the synthetic unit fails to replicate the observed performance for placebo treatment, then we succeed in falsifying the claim that the estimated gap is driven by the intervention. On the other hand, higher confidence can be attached to the results if it replicates the observed performance in response to the fictitious treatment.

In-time-Placebo: Since Norway did not discover oil before the early 70s, years prior to 1971 can be used to conduct a falsification test of our result. If this paper's application of the synthetic control approach is correct, estimating the impact of a placebo oil discovery before the actual time of discovery would result in no substantive departure between the outcome of the new synthetic unit and the treated unit. To test for these concerns, the study applies a placebo treatment in 1960 and 1965, which are earlier than the assignment of the treatment to the Norwegian economy. Figure 6 plots the result of the falsification exercises, which assign placebo treatments to the Norwegian economy in 1960 and in 1965.

Placebo 1: Fictitious oil discovery in 1960 Placebo 2: Fictitious oil discovery in 1965

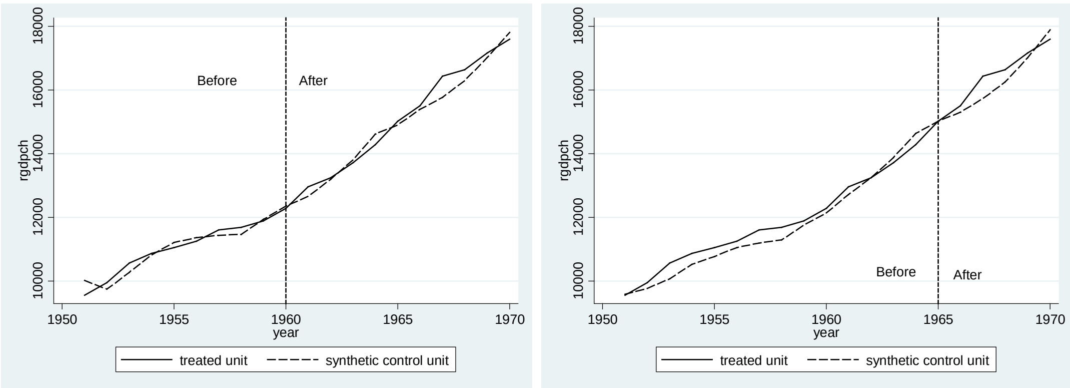

Figure-6: The Impact of Placebo Treatments of oil discovery in 1960 and in 1965 on the Norwegian economy[18]

As can be seen from both panels of figure 6, there is no significant divergence, at least in the sense of what is observed in the presence of real treatment in figure 3, between the synthetic unit and the treated unit in response to fictitious assignment of the treatment in 1960 or 1965. In both cases, the synthetic unit has reproduced the treatment unit. Since there is no perceptible difference between the synthetic economy and the placebo treated economy, i.e. the synthetic unit reproduced the outcome of treated unit, for assignments of a placebo treatment, the observed gap between the outcome of the synthetic and treated Norway as of 1971 cannot be attributed to the method's failure to reproduce the outcome of the treated unit.

Across-space-Placebo: In addition to the falsification test using placebo treatment over time, one can also conduct a falsification exercise using placebo treatment of countries unexposed to the treatment. Since none of the members of the donor pool has oil endowment in the entire time under study, examining the impact of a fictitious treatment of countries in the donor pool would provide a reasonable falsification test to judge whether the estimated impact is large

[18]The optimal weights generating the synthetic unit in the fictitious placebo is different from ones reported in table-1. The data covers only up to 1970 i.e. the time range over which the economy has not received the intervention.

enough relative to impacts obtained for placebo treatment of similar countries without endowment of petroleum. Abadie et al. (2011a) underline that this type of treatment is very stringent in that its outcome would be affected by heterogeneities of shocks of GDP per capita across countries. Nevertheless, the estimated impact of the actual intervention needs to be larger than other country-specific placebo effects for one to assign greater confidence to the alleged impact of the petroleum discovery.

To this end, countries in the donor pool were assigned a fictitious oil discovery in 1971 and the placebo impacts, i.e. the divergence between the synthetic and treatment unit, are obtained for all members of the donor pool. The result of this iterative procedure provides the distribution of estimated impact for countries without petroleum endowment.

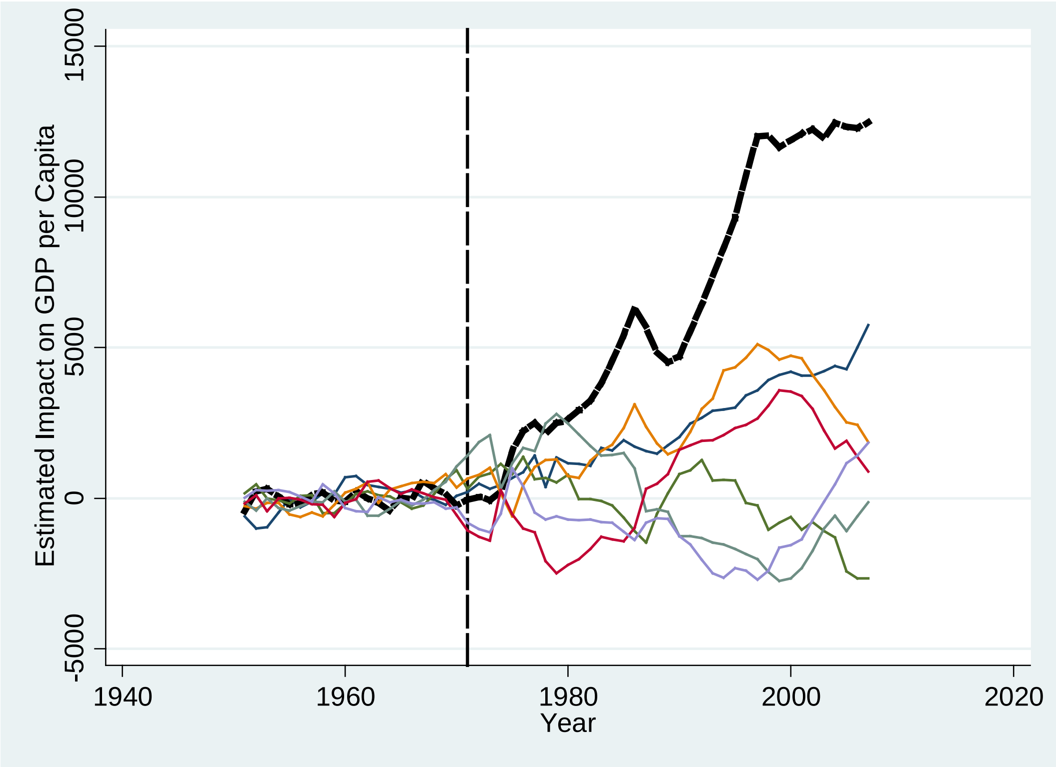

Figure 7: Distribution of absolute impacts of a fictitious treatment in 1971for countries in the donor pool whose synthetic unit is able to reproduce pre-treatment outcome. The bold dash line is the gap for Norway.

Figure 7 illustrates different ways to compare the estimated impact of petroleum and other placebo effects. Not all placebo effects are comparable to the case of Norway. This is because a given synthetic unit cannot form a reasonable basis to make plausible comparison when the synthetic unit is unable to reproduce the observed outcome prior to 1971(i.e. in contrast to the synthetic Norway). In figure 7, comparison is made only among those countries whose synthetic unit is able to reasonably replicate the outcome prior to the time of intervention. The figure makes it clear that the estimated impact of petroleum on the

Norwegian economy is very large relative to the effects of random placebo treatment assigned to countries in the donor pool.

To sum up, the time-placebo test suggests that the synthetic control unit has been able to replicate the actual evolution of national income in the absence of the treatment and the space-placebo test suggests that the relative probability of observing a large estimated impact as much as that of petroleum in Norway, given the random assignment of the placebo-treatments, is very low. All the robustness test, sensitivity test, dose-response test, time-placebo test, and space-placebo test suggest that the main result we observe for Norway could not have come from the inability to reproduce the Norwegian national income in the absence of the treatment. Thus, an economically significant event must have happened in 1971 and the discovery, production, and export of petroleum appear to be the most sensible candidate.

IV. Concluding remarks

This paper's approach has a number of advantages. First, Abadie et al. (2010) argue that the synthetic control technique generalizes the standard Difference-in-Difference impact estimator to the case in which the unobserved effect can be time varying. Hence, it does a better job in reproducing the treatment unit, without treatment, controlling for observed and time varying unobserved effects. They also show that under fairly standard assumptions, the estimated impact is an unbiased estimate of the average treatment effect. Second, a common problem in estimating impacts has to do with extrapolation bias and lack of a rigorous approach for choosing similar comparison unit. Relative to the standard regression approach, safeguards against extrapolation bias is an additional advantage. Relative to the early comparative case studies, transparency and rigor of the choice of the best comparison unit is an extra advantage. Third, by dividing countries into treatment, donor-pool, and non-comparable units, the technique removes the need for a precise measure of the treatment. Since only the unit of interest, and not units in the donor pool, is affected by the treatment, one does not need to have an indicator of the intervention to estimate the impact of the intervention. When precise measure of the intervention is available, one can use that information to examine the pattern of estimated impact with the pattern of the intervention ála dose-response test.

However, it is essential to note that it is not feasible or recommendable to use this approach when one or more of the following conditions are the case. First, one needs to have access to data of the outcome variable and its determinants for both the treated unit and units in the comparison group prior to the intervention to be able to generate the synthetic control unit. For example, one cannot estimate the synthetic control unit for Venezuela to estimate the impact of its petroleum endowment using a dataset that begins in 1950 as Venezuela was extracting and exporting oil long before 1950. This is because one cannot know the nature of the Venezuelan economy in the absence of the oil endowment without data from the pre-intervention period. For the same reason, it is impossible to evaluate the impact of the oil endowment using the synthetic control technique in those countries where extraction of the endowment began before systematic data was available.

Second, sample size might be a problem. Although according to the World Energy Council (2007), countries such as Libya, Nigeria, Norway, Sudan, United Arab Emirates, and the UK have discovered major endowments of oil after 1950, it is not recommendable to estimate the economic impact of oil endowment for Nigeria or United Arab Emirates since they discovered petroleum in 1958 and one is left with only 8 pre-treatment years to estimate the impact for 54 years, as the data set begins in 1950.

Third, it is challenging to estimate the impact of natural resources when a discovery of an endowment is accompanied by other interventions that make it hard to isolate the impact of a particular natural resource. For example, it is challenging to isolate and estimate the impact of oil on Libya's economy as the estimated impact of the endowment might be contaminated with the effect of other factors such as economic and political sanctions. The same comment applies to the case of Sudan since the estimated impact of the endowment might be contaminated by the effect of the long lasting civil war that Sudan was undergoing until recently.[19]

Fourth, the optimal synthetic control group has to satisfy a number of requirements even when data is available for a given detectable and uncontaminated intervention. These include (i) the synthetic unit has to reproduce the treated country's outcome variable, and its determinants, prior to discovery and extraction of the natural resource. Unless the synthetic unit is able to reproduce the variable of interest and its determinants prior to the intervention, it cannot serve as a good comparison unit that is able to reproduce the outcome in the absence of the intervention. (ii) The estimated impact has to pass a number of falsification tests to assess that the alleged impact is not driven by chance or by some other factor.

[19]This would not make the estimation task impossible; but one would need a more elaborate cross-country data base than commonly available.

With these considerations taken into account, this paper attempts to provide a quantitative case study of the economic impact of petroleum endowment. To answer the central question, the paper estimates a synthetic economy whose pre-oil discovery per capita GDP is very similar to Norway's GDP per capita for the period 1953 – 1971, and which differs from the Norwegian GDP per capita after the discovery of oil. The synthetic unit is generated from the pool of OECD countries not endowed with oil, based on the assignment of optimally generated weights. The assessment of the synthetic unit suggests it replicates the GDP per capita from 1953 to 1971, the period before the first major discovery and extraction of petroleum. Comparing the relative performance of the actual unit with the synthetic control unit, this paper suggests that the impact on per capita income is very large. The results indicate that, on average, about 20% of the increase in GDP per capita since 1974 is due to the petroleum endowment. The estimated annual impact correlates well with the total quantity of petroleum extracted.

The paper conducts a number of robustness tests to verify whether the result is driven by some other unknown treatment. The first test is to see whether the results are robust to the inclusion of additional variables. The second test is in the spirit of an "impulse-response" test linking the quantitative change in annual petroleum produced to the estimated annual impact of petroleum extraction. The third test verifies that the observed impact is not driven by the synthetic's unit inability to reproduce the observed outcome by using the time-placebo and space-placebo discovery of petroleum endowment. The fourth test assesses the sensitivity of the estimated impact in response to excluding the countries with positive weight from the donor pool. The results of the quantitative test and falsification tests indicate that the actual output is reproduced in the absence of the real intervention; and, the observed impact is most likely driven by the endowment of petroleum. The robustness and sensitivity tests show that the estimated impact is robust to such concerns.

It is necessary to note that empirical evidence from Norway's experience is interesting but may be exceptional. It is interesting because it shows the enormous opportunities endowments of natural resources entail for improving economic welfare. There are not many policies that are able to deliver a 20% growth in GDP per capita for an economy. This result is remarkable when gauged in light of Lucas's (2003) estimate that the welfare benefits of most ideal monetary and fiscal policies is about 1/20 of 1% of personal consumption. As the discovery of oil took place after many countries had started collecting good quality data, it provides an opportunity to learn from this experience. At the same time, Norway's experience may differ from that of many countries in that its political and economic institutions have remained mostly unchanged over the past decades. The country has been very forward-looking by keeping the revenue from oil invested outside the country for the benefit of future generations instead of consuming it through tax cuts or direct transfers. If indeed the Norwegian experience is exceptional, this raises a set of potentially important research questions: what are the factors behind Norway's exceptionality? Can these factors be replicated in other resource-rich areas? In addition, did other countries (for example the Netherlands and the U.K.) benefit from the discovery and use of their petroleum endowment? These are interesting questions whose answer cannot be extrapolated from the Norwegian experience. A careful and detailed study of the factors behind Norwegian exceptionality and the experience of other countries with resource endowment are left for future research.

References

Abadie, Alberto, Alexis Diamond, Jens Hainmueller (2011a) "Comparative Politics and the Synthetic Control Method" http://www.hks.harvard.edu/fs/aabadie/comparative.pdf

Abadie, Alberto, Alexis Diamond, Jens Hainmueller (2011b). "Synth: An R Package for Synthetic Control Methods in Comparative Case Studies" **Journal of Statistical Software** 42 (13): 1-13.

Abadie, Alberto, Alexis Diamond, Jens Hainmueller (2010) "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program" **Journal of the American Statistical Association**, vol. 105(490): 493-505.

Abadie, Alberto and J. Gardeazabal (2003) "The Economic Costs of Conflict: A Case Study of the Basque Country," **American Economic Review**, and vol. 93(1).

Acemoglu, Daron (2010) "Theory, General Equilibrium, and Political Economy in Development Economics" **Journal of Economic Perspectives**, 24(3): 17–32.

Aghion, Philippe & Howitt, Peter, (1992)"A Model of Growth through Creative Destruction," **Econometrica**, vol. 60(2), pages 323-51.

Alexeev, Michael & Robert Conrad (2009) "The Elusive Curse of Oil," **The Review of Economics and Statistics**, MIT Press, vol. 91(3), 02.

Andvig, Jens Chr. & Moene, Karl Ove (1990)"How corruption may corrupt," **Journal of Economic Behavior & Organization**, vol. 13(1), pages 63-76.

Austvik, Ole Gunnar (1993) "Norwegian Petroleum and European Integration" *Brent Nelsen (editor): "Norway and the European Community; The Political Economy of Integration", Praeger Publisher CT Westport, Connecticut, USA. August 1993. ISBN 0-275-94211-2. http://www.kaldor.no/energy/BREN9211.pdf*

Barro, Robert J. Jong-Wha Lee, A New Data Set of Educational Attainment in the World, 1950–2010, NBER Working Paper No. 15902, 2010

Barro, Robert J. & Xavier Sala-i-Martin, 2003, **Economic Growth**, 2nd Edition, MIT Press Books,The MIT Press, edition 2, volume 1, number 0262025531.

Bjørnland, Hilde C. (1998) "The Economic Effects of North Sea Oil on the Manufacturing Sector" ***Scottish Journal of Political Economy***, 45, 553-585.

Brunnschweiler, Christa N. & Bulte, Erwin H., (2008)"The resource curse revisited and revised: A tale of paradoxes and red herrings," **Journal of Environmental Economics and Management**, vol. 55(3), pages 248-264.

Card, David & Krueger, Alan B, (1994)"Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," **American Economic Review**, vol. 84(4), pages 772-93, September.

Card, David (1990)"The impact of the Mariel boatlift on the Miami labor market," **Industrial and Labor Relations Review**, vol. 43(2), pages 245-257.

Caselli, Francesco, and Guy Michaels (2011) "Do Oil Windfalls Improve Living Standards? Evidence from Brazil." http://personal.lse.ac.uk/casellif/papers/brazil.pdf, *R&R at* **AEJ: Applied Economics**

Collier, Paul, and Anke Hoeffler (2004) "Greed and Grievance in Civil War" **Oxford Economic Papers**, 56(4): 563–95.

Ding, N. and B. Field. (2005) "Natural Resource Abundance and Economic Growth" **Land Economics**, 81, 496-502.

Fearon, James D (2005) "Primary Commodity Exports and Civil War" **Journal of Conflict Resolution**, 49(4): 483–507.

Frankel, Jeffrey A. (2010) "The Natural Resource Curse: A Survey" NBER Working Paper 15836, http://www.nber.org/papers/w15836

Hausman, R and R. Rigobon, (2002) "An Alternative Interpretation of the 'Resource Curse': Theory and Policy Implications" NBER Working Paper Series, WP 9424, Cambridge: National Bureau of Economic Research.

Heston, Alan, Robert Summers and Bettina Aten (2011) "Penn World Table Version 7.0" Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania, 2011.

James, Alex and David Aadland (2011) "The Curse of Natural Resources: An Empirical Investigation of U.S. Counties." **Resource and Energy Economics**, 33(2), 440-453.

Larsen, Erling Røed (2004)"Escaping the Resource Curse and the Dutch Disease? When and Why Norway Caught up with and Forged ahead of Its Neighbors," Discussion Papers 377, Research Department of Statistics Norway http://elsa.berkeley.edu/users/webfac/cbrown/e251_f03/larsen.pdf

Lucas, Robert E., Jr. (2003) "Macroeconomic Priorities" **American Economic Review**, 93(1): 1–14

Mansoorian, Arman (1991) "Resource Discoveries and Excessive' External Borrowing" **Economic Journal**, 101(409): 1497–509.

Manzano, Osmel, and Roberto Rigobon (2001) "Resource Curse or Debt Overhang?" National Bureau of Economic Research Working Paper 8390

Matsuyama, Kiminori (1992) "Agricultural Productivity, Comparative Advantage, and Economic Growth" **Journal of Economic Theory**, 58(2), December, 317-334

Mehlum, H., Moene, K. and Torvik, R. (2006), Institutions and the Resource Curse. **The Economic Journal**, 116: 1–20. doi: 10.1111/j.1468-0297.2006.01045.x

Norwegian Continental Shelf (2010) A Journal from the Norwegian Petroleum Directorate, http://www.npd.no/Global/Norsk/3%20-%20Publikasjoner/Norsk%20sokkel/Nr.2-2010/Norsk%20sokkel-%20eng%20for%20nett.pdf%20-%20Adobe%20Acrobat%20Pro.pdf

Papyrakis, E. and R. Gerlagh (2007) "Resource Abundance and Economic Growth in the U.S." **European Economic Review**, 51(4), 1011-1039.

Rodrik, Dani and Francisco Rodríguez (2001) "Trade Policy and Economic Growth: A Skeptic's Guide to the Cross-National Evidence," **Macroeconomics Annual 2000**, eds. Ben Bernanke and Kenneth S. Rogoff, MIT Press for NBER, Cambridge, MA.

Romer, Paul (1990) "Endogenous Technological Change," **Journal of Political Economy**, Vol 98(#5)

Rosenbaum, Paul (2005), Observational Studies, Encyclopedia of Statistics in Behavioral Science. http://www-stat.wharton.upenn.edu/~rosenbap/BehStatObs.pdf

Sachs, Jeffrey D. Andrew M. Warner, Natural Resource Abundance and Economic Growth NBER Working Papers 5398, National Bureau of Economic Research, 1995.

Sachs, J., and A. Warner (2001) "The Curse of Natural Resources," **European Economic Review** 45:827-838

Sachs, Jeffrey (2007) "How to Handle the Macroeconomics of Oil Wealth," Ch. 7 in **Escaping the Resource Curse**, edited by M. Humphreys, J. Sachs and J. Stiglitz (Columbia University Press: NY), pp. 173-193.

Sala-i-Martin, X., G. Doppelhofer, and R. Miller (2004)"Determinants of Long-Term Growth: A Bayesian Averaging of Classical Estimates (BACE) Approach," **American Economic Review** 94(4):813-835.

Sala-I-Martin, Xavier, and Arvind Subramanian, (2003) "Addressing the Natural Resource Curse: An Illustration from Nigeria." IMF Working Paper WP/03/139

Solow, Robert M (1956) "A Contribution to the Theory of Economic Growth" **The Quarterly Journal of Economics** 70 (1) February, pp 65-94

Torvik, Ragnar (2002) "Natural Resources, Rent Seeking and Welfare." **Journal of Development Economics**, 67(2): 455–70.

van der Ploeg, Frederick (2011) "Natural Resources: Curse or Blessing?" **Journal of Economic Literature**, American Economic Association, vol. 49(2), pages 366-420, June.

van der Ploeg, Frederick, and Steven Poelhekke (2010) "The Pungent Smell of 'Red Herrings': Subsoil Assets, Rents, Volatility and the Resource Curse." **Journal of Environmental Economics and Management**, 60(1): 44–55.

Vicente, Pedro C. (2010) "Does Oil Corrupt? Evidence from a Natural Experiment in West Africa", **Journal of Development Economics**, 92(1), pp. 28-38.

Weitzman, Martin L. (1999) "Pricing the Limits to Growth from Minerals Depletion," **Quarterly Journal of Economics**, 114(2), 691-706.

Weitzman, Martin L. (2003) ***Income, Wealth, and the Maximum Principle***, Harvard University Press

World Energy Council (2007) "2007 Survey of Energy Resources World Energy Council" http://www.worldenergy.org/documents/ser2007_final_online_version_1.pdf