Ishak, Phoebe W.; Gradstein, Mark

**Conference Paper**

# We Don't Need No Education: The Effect of Persistent Income Shocks on Human Capital

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2021: Climate Economics

**Provided in Cooperation with:**
Verein für Socialpolitik / German Economic Association

# We Don't Need No Education: The Effect of Persistent Income Shocks on Human Capital

Mark Gradstein
Ben Gurion University

Phoebe W. Ishak*
Freie Universität Berlin

Februrary 2020

## Abstract

We explore the effects of persistent income shocks on human capital using oil price fluctuations in a large sample of relevant African countries and employing micro data from multiple waves of the Demographic and Health Survey (DHS). Theoretically, such shocks enable human capital investment via the standard income effect; but also crowd it out because of substitutability between natural resource and human capital income sources – so the net outcome can go either way. Our model suggests that the relative strength of the two effects depends on the age at which the shock is experienced and the affected gender. Consistent with these insights, we find that income shocks in early life enhance educational attainment and other derived outcomes; but reduce them if experienced in adolescence, especially for females. These results survive multiple robustness checks, and their broader implications are discussed.

**Keywords:** Human capital, income shocks, natural resources

**JEL codes:** O12, I2

*Contact details: phoebe.w.ishak@fu-berlin.de (corresponding author); grade@bgu.ac.il

# 1 Introduction

Whether or not natural resources are a blessing or a curse has been a long standing subject of debate among economists. Indeed, a large literature has explored existence or the lack thereof of a resource curse: a possibly negative effect of natural resources on measures of economic performance, such as economic growth. This work has generated valuable insights on channels and mechanisms of influence, but overall ambiguous conclusions. In particular, a subset of this literature has examined the effect of natural resources on schooling, both on its own and as a possible channel for the ultimate effect of resource abundance on economic growth. An early important paper Gylfason, 2001, argued for a negative effect, whereby natural resources crowd out human capital investment, and Papyrakis and Gerlagh, 2004, reinforce this conclusion.[1] Some subsequent work, however, Sijns, 2006, Brueckner and Gradstein, 2016, provided contrarian evidence, arguing that income shocks resulting from natural resources are conducive to human capital investment. Even more recently, Abramson and Esposito, 2020, find that traditionally coal rich regions in Europe underinvested in universities in the long term – thus providing further evidence for crowding out of human capital by the abundance of natural resources. Geographically and temporarily closer to the scope of this paper, Ahlerup et al., 2020, detect similar detrimental effect in modern Africa in the context of gold mining. It appears, therefore, that evidence pertaining to the effect of natural resource wealth on human capital is contradictory.

In this paper, we take a more nuanced approach to the issue in order to possibly reconcile the different pieces of existing evidence. Conceptually, we argue that wealth generated through natural resources enables, via the standard income effect, a larger amount of human capital investment in the immediate run. On the other hand, the plausible lack of complementarity between this wealth and human capital implies potential crowding out in the future, so that the overall effect is ambiguous and depends, in particular, on the relative strength of these two elements. The return on human capital investment is a decisive factor in our conceptual framework in determining which of these two dominates. In particular, the higher the return the larger (and positive) the effect of natural resource income on education. A crucial element in the analysis then is the age range in which the child experiences such an income shock. If this happens in early childhood, the income effect dominates because the return on human capital investment is high; but this can be reversed if the shock occurs subsequently, in late childhood/adolescence. This theory plays a role in potentially explaining the wide range of results in existing (especially aggregate cross country) work, which typically ignores the specifics of school age distribution, such as Gylfason, 2001, and Papyrakis and Gerlagh, 2004, on one hand, and Sijns, 2006, and Brueckner and Gradstein, 2016, on the other hand.

To explore empirically the potentially differential impact of resource income

[1] A very partial sample of this literature includes Alexeev and Conrad, 2009, Aragon and Rud, 2013, Auty, 1994, Black et al., 2005, Caselli and Michaels, 2013, Cavalcanti et al., 2019, Gradstein and Klemp, 2020, Papyrakis and Gerlagh, 2007, Sachs and Warner, 2001; Badeeb et al., 2017, and van der Ploeg, 2011, are useful surveys, and Havranek et al., 2016, conduct meta-analysis of hundreds of studies on the issue.

shocks on education depending on the child's age, we utilize the IPUMS-DHS dataset of oil producing African countries, whereby our primary focus is on income shocks generated through world oil price fluctuations. The dataset allows us to link oil price shocks at different ages in childhood to subsequent educational attainment. Although the sample countries are oil producers and net exporters, their share in the world oil output is typically small, so that oil price changes can be safely assumed as exogenous from their vantage point, hence can be interpreted as income shocks. By their nature, these shocks are persistent and can be interpreted as carrying a permanent income effect. Our empirical design differentiates between oil rich and oil poor regions in the sample countries and links outcomes to the differential incidence of oil price shocks by childhood/adolescence age brackets.

Our baseline results, supplemented by a variety of robustness checks, indicate that such shocks have a positive effect on education if experienced in early childhood (ages 0-4), but these shocks are statistically insignificant or are even detrimental for schooling when experienced later on (ages 10-14). These results are both consistent with the large and growing literature on future effects of early life circumstances, reviewed below, and with the work on resource curse, and it has the potential of reconciling the differing findings in these branches of research. We then also explore the role of gender in generating the above results and find that the early childhood positive effect is somewhat larger for boys than for girls, whereas the late childhood negative effect of oil price shocks on education is driven by the far larger effect for girls than for boys. Whereas the positive effect of early childhood shocks on education is well consistent with the established literature, the negative effect in later childhood, especially for girls, is a novel and important finding. It is consistent with the conceptual model, which indicates that, to the extent that returns from schooling and natural resource wealth are substitutes, income shocks in late childhood may result in less schooling. Additionally, we explore derived outcomes, such as households' wealth, women age of first marriage, and the number of children and find that their reaction to income shocks is broadly consistent with our conceptual framework and/or existing literature.

The paper proceeds as follows. The next section contains discussion of the related work. Then section 3 lays out our conceptual framework. Sections 4 and 5 describes the data and empirical strategy. The main results are contained in Section 6, followed in Section 7 by robustness and extension analyses. Finally, Section 8 concludes.

# 2 Related literature

A large volume of recent work documents the importance of early life circumstances for future outcomes and, specifically, for human capital; Almond and Currie, 2011, Almond et al., 2018, are excellent surveys. Adhvaryu et al., 2019, Fenske and Zurimendi, 2017, and Maccini and Yang, 2009, are examples of papers that exploit, as we do, income shocks for identification purposes. Our findings concur with this literature in detecting a positive effect of such shocks in early childhood; Lavy et al., 2016, specifically focus on educational attainment in this regard, whereas the

above papers typically have education as one of their studied outcomes. The paper Fenske and Zurimendi, 2017, is particularly related in using oil price shocks in the context of an African country (Nigeria) for identification.

As our particular income shocks are derived from natural resource prices, our findings in this regard are consistent with Sijns, 2006, and Brueckner and Gradstein, 2016, and differ from Abramson and Esposito, 2020, and Papyrakis and Gerlagh, 2004. One possible difference between this research and those papers is the underlying social and economic environment. We are specifically interested in poor developing countries, whereas the above work also covers developed areas. Additionally, the above work, ignoring the underlying age structure of the population, potentially masks age related incidence of the shocks, which, we argue, is an essential factor generating important heterogeneous effects.

Our findings complement those in Ahlerup et al., 2020, whose focus, like ours, is Africa and, who as we do, exploit changes in mineral (gold) prices for identification. Similar to our findings, these authors find that gold booms during adolescence induce less schooling; they attribute this to opportunities in the mining sector that increase the opportunity cost of schooling. This is inconsistent with our findings being driven by adolescent girls, who are less likely to benefit from work opportunities in the mining sector, see Kotsadam and Tolonen, 2016, for evidence in this regard. Additionally, Ahlerup et al., 2020, abstract form the more general analysis of the age incidence of income shocks.

In this latter regard, this research is more closely related to Shah and Steinberg, 2017, which detects a differential age effect of productivity shocks on educational attainment. Conceptually, however, productivity shocks, which both income and encapsulate substitution effects, are different from pure income shocks; and that paper's instrument for productivity shocks, annual rainfall, is very different from the instrument employed here, resource price shocks. Among other things, one crucial difference is that rainfall is a temporary shock, whereas resource price changes induce a persistent income shock. As will be elaborated more fully through our conceptual framework, this is important for the theoretical argument we make and serves as a primary channel through which age differential effect we empirically identify is induced. Further, child labor – common in the agricultural sector affected by rainfall – is much less prevalent in the oil industry, see Unicef, 2015.[2] We, therefore, do not expect productivity effects to play a major role in our setup.[3] Our contribution, therefore, can be viewed as complementary to Shah and Steinberg, 2017, and different from it both in conceptual and in contextual dimensions.

Finally, our gender related findings are well consistent with and reinforce, using our identification methods existing work, such as Alderman and King, 1998, Kingston, 2005, and Kaul, 2018. These papers typically document existence of a gender bias in educational investments and attainment in developing countries. While this is not the main focus of this paper, we contribute to the issue by docu-

[2]Vast majority of chlid labor in Africa (85 percent) takes place in agriculture and within family units; just less than 4 percent in industry, a fraction of which is in mining such as gold, diamond, copper and cobalt (ILO, 2017).

[3]This is one reason why the focus on oil price shocks – as opposed to price shocks of other minerals, such as gold or diamonds, where child labor is common – is advantageous for our purposes.

menting gender differences in educational attainment responses to early life shocks.

# 3 Conceptual framework

## 3.1 Baseline model

Consider a household consisting of a parent and a child living for two generation-periods. The first period income is equivalent to a windfall of I>0 and is allocated between family consumption, $c_1$, and schooling, $s$, subject to the budget constraint:

$$c_1 + s = I \tag{1}$$

Consistent with the empirical part, we interpret the income windfall as resulting from a natural resource. Stipulating existence of an additional income source in the first period would not change qualitative conclusions.

Schooling generates human capital, $h = H(s), H^{'} > 0, H^{"} < 0$. The return on human capital investment, equivalent to the future wage rate, is $R_i$. Index $i$ corresponds to the child's age within the first generation-period, and we assume that the higher $i$ the lower $R_i$, the rationale being that the younger the child when human capital investment is made, the larger the return – because "learning begets learning", for example, see Almond and Currie, 2011, and Cunha and Heckman, 2007.[4] Thus, an early investment in education has a higher per unit return than a later investment, because of dynamic schooling complementarities.[5]

Second period income is imputed to the child in her adult generation-period and consists of two parts. One is related to the windfall and equals $I$ – which corresponds to a persistent oil price shock.[6] Another part is the wage, return earned on human capital, $R_i H(s)$. In principle, one could consider the two second-period income sources as either complements or substitutes; given that the source of income windfall in our empirical analysis is natural resource, it is common to assume that they are substitutes (as, for example, findings and discussion in Gylfason, 2001, strongly suggest).[7] We then write the second period income as $I + R_i H(s)$. Since the second period is the last one, all income is consumed, so we have:

$$c_{2i} = I + R_i H(s) \tag{2}$$

The parent derives utility from family consumption in period 1 and child's con-

[4]In principle, this mechanism could be incorporated into our model by dividing the first period into two learning sub-periods, in the manner it is done in Cunha and Heckman, 2007. We rely on that work in our reduced form stipulation.

[5]This, in fact, serves as a forceful argument for early schooling interventions, see Cunha and Heckman, 2007.

[6]Introducing a persistence parameter – so that the second period income would be, say, $\gamma I, \gamma > 0$ - would not change qualitative conclusions, although the intensity of persistence would play a quantitative role.

[7]A modicum of complementarity – such as skilled geologists and engineers needed to discovery and exploitation of oil fields or mines – would not change the analysis.

sumption in period 2:

$$U(c_1, c_{2i}) = c_1^{\epsilon} + \delta c_{2i}^{\epsilon}, 0 < \epsilon_1 < 1, \tag{3}$$

where $\delta > 0$ designates time preference – or is related to the extent of parental altruism.

## 3.2 Analysis

Utility maximization with respect to schooling subject to the constraints (1) and (2) yields the following first order condition at the internal equilibrium:[8]

$$FOC = -(I-s)^{\epsilon-1} + \delta R_i (I + R_i H(s))^{\epsilon-1} H' = 0 \tag{4}$$

And the second order condition,

$$\begin{aligned} SOC = &-(1-\epsilon)[(I-s)^{\epsilon-2} + \delta R_i^2 (I + R_i H)^{\epsilon-2} H'] \\ &+[\delta R_i (I + R_i H(s))^{\epsilon-1} H"] < 0 \end{aligned} \tag{5}$$

holds. The derivative of the left hand side in (4) with respect to $I$ is:

$$(1-\epsilon)[(I-s)^{\epsilon-2} - \delta R_i (I + R_i H)^{\epsilon-2} H'] \tag{6}$$

Evaluating this expression at the point where (4) holds, we obtain:

$$\begin{aligned} &(1-\epsilon)(I + R_i H)^{\epsilon-2}[(\delta R_i H')^{(\epsilon-2)/(\epsilon-1)}) - \delta R_i] \\ &= (1-\epsilon)(I + R_i H)^{\epsilon-2} \delta R_i [(\delta R_i)^{1/1-\epsilon} H'^{(\epsilon-2/\epsilon-1)} - 1] \end{aligned} \tag{7}$$

which is positive if and only if the bracketed expression is. Recalling the second order condition, upon totally differentiating (4), this directly leads to the following statement:

$$\begin{aligned} Sign(ds/(dI)) = Sign(-(\delta FOC/\delta I)/(SOC)) = Sign[(\delta R_i)^{1/1-\epsilon} \\ H'^{(\epsilon-2/\epsilon\text{-}1)} - 1] \end{aligned} \tag{8}$$

so that the effect of income windfall on schooling is positive whenever $\delta R_i$ or when the return on human capital is sufficiently high. Recalling our interpretation that this return is an inverse function of a child's age in generation-period 1, we then have that the younger the child's age when experienced a shock, the higher the positive effect of income windfall on schooling.

Note that, in principle, even when $ds/dI < 0$, the second period aggregate income, hence consumption, can nevertheless be an increasing function of the income shock, as $dc_{2i}/dI = 1 + R_i H' ds/dI$, so that the possibly negative effect of income shock on schooling is likely to be offset by the pure income effect of the shock.

[8] It holds whenever $-(I-s)^{\epsilon-1} + \delta R_i (I + R_i H(s))^{\epsilon-1} H' > 0$ and, in particular, whenever returns on human capital investment are high enough; in contrast, when $-(I-s)^{\epsilon-1} + \delta R_i (I + R_i H(s))^{\epsilon-1} H' < 0$, then a corner solution with s=0 is obtained.

### 3.3 Introducing gender

Suppose now that the children differ by gender and let $j$, $j = m, f$, be the gender index. Each household's child is either a boy ($m$) or a girl ($f$). Then the budget constraint is:

$$c_1 + s_{ij} = 1 \tag{9}$$

The return on human capital investment, equivalent to the future wage rate, is $R_{ij}$, with $R_{im} > R_{if}$, so that the return to a male's HC is higher than to a female's HC – because of various gender barriers and social norms. The second-period consumption levels of the children are:

$$c_{2ij} = I + R_{ij}H(s_{ij}) \tag{10}$$

The parent derives utility from family consumption in period 1 and the child's consumption in period 2:

$$U(c_1, c_{2ij}) = c_1^{\epsilon} + \delta_j {c_{2i}}_j^{\epsilon}, \tag{11}$$

It is common in the literature to assume that parents value boys more than girls for a variety of reasons, which would imply that $\delta_m > \delta_f$.

The above analysis then implies that the effect of income windfall on schooling is positive whenever $\delta_j R_i j$ is sufficiently high; particularly, if $(\delta_m R_{im})^{\frac{1}{(1-\epsilon)}} H'(s_{im})^{\frac{(\epsilon-2)}{(\epsilon-1)}} > 1 > (\delta_f R_{if})^{\frac{1}{(1-\epsilon)}} H'(s_{if})^{\frac{(\epsilon-2)}{(\epsilon-1)}}$ - then this effect is positive only for boys.

A few concluding observations are in order. We first note that the above analysis focuses on the demand side, ignoring the supply side. The justification for this is that our main interest is not with how income shocks affect schooling at large but with the differential by age effect. Since marginal costs of schooling are unlikely to vary much by grade, the supply factors are of minor importance in this regard.[9]

Further, the above analysis abstracts from the potentially significant role of government policies. For example, governments typically tax natural resource wealth and use resulting revenues to, among other things, provide public schooling or subsidize privately provided education. While these are important factors, under plausible assumptions, they would not change our qualitative conclusions. To the extent that oil revenues are used as public spending to substitute for households' spending (say, on health, or on social insurance), this would ultimately lead to an increase in disposable income, consistent with the presented model. A support for this is provided in institutional accounts (https://eiti.org/) and academic research (e.g. James, 2015; Crivelli and Gupta, 2014; Ishak and Farzanegan, 2020) indicating that income taxation is reduced in the presence of booming oil revenues.

Additionally, the displayed income shock may stem from natural resource exploitation, as in the empirical analysis below - or from other sources; the only essential elements are that it be unconditional (on, say, school attendance) and

[9]Yet, we do conduct an empirical analysis of the supply of schools below to make sure that it has not been a major factor in our sample.

permanent. In particular, many poverty alleviation income support programs have these features, and so share similarities with income windfall derived from natural resources.[10]

The gist of our argument, that elements of consumption smoothing take place under oil price generated shocks is supported by Hsieh, 2003. In fact, that paper's main objective is precisely to document this fact, in support of the permanent income hypothesis. Although the setup is different (Hsieh, 2003, considers income windfalls to Alaska's residents resulting from Alaska's Permanent Fund), the evidence strongly suggests that households take into full consideration future payments resulting from oil revenues when making current decisions.

Finally, the above analysis, indicating that the effect of natural resource wealth on human capital depends on the age at which its incidence took place has important aggregate implication. In the aggregate, this effect would depend on the age distribution, among other things, which may help explain the different conclusions obtained in existing scholarly work cited in the first two sections of the paper, which ignores this aspect. And the difference in results obtained in specific cases could potentially also be accounted for by such demographic factors.

# 4 Data

## 4.1 Sample

Our sample is focused on oil producing countries in Africa. One reason for this is the relative homogeneity. Another reason is that these countries are well covered, through multiple waves, by our main source of information on outcome variables; this enables us to use repeated cross-section waves from the same environment. A third reason is that for the countries in the sample oil is an important source of revenues, and relatedly, these countries are typically not well diversified. Our data on education, wealth, and individual characteristics come from the individual census records conducted by Demographic and Health Survey (DHS) program. The data is retrieved from Integrated Public Use Microdata Series (IPUMS) International that reports harmonized representative samples. Our analysis is restricted to country survey waves with Global Positioning System (GPS) information on the location of the surveyed households. This is because the geocoded data allows us to assign individuals to their respective oil and non-oil producing regions allocated as explained below. To homogenize the sample, we focus on countries reporting at least one oil-producing region. This leaves us with 65 surveys over the period 1990-2016 for 16 countries: Angola, Cameroon, Democratic Republic of Congo, Ivory Coast, Benin, Ethiopia, Ghana, Madagascar, Namibia, Mozambique, Nigeria, Niger, Senegal, Tanzania, Egypt, and Morocco.

The sampled countries had a population of 757 million people in 2016 representing 62 percent of Africa's population. In assigning oil locations and consequently house-

[10] Albeit, there are also differences in details not modelled here, such as pollution possibly generated by the exploitation of natural resources. We believe, however, that these aspects are of a secondary importance to the issue at hand.

holds, we choose to work on coarser administrative units (ADM 1, or provinces) rather than finer levels (ADM 2, or district). The advantage of this approach is twofold. First, higher administrative levels allow for reducing the measurement error associated with the allocation of oil fields. Second, focusing on provinces can mitigate migration concerns, which is a common phenomenon in periods of high oil prices. Even though migrating from one province to another is possible, it is less frequent as compared migrating from one district to another, especially in the presence delimited ethnic and tribal territories making movement quite restricted. Our sample includes 247 provinces with a mean (median) size of 49 (29) thousands km squared. Figure 1 shows a map of the countries included along with the locations of oil provinces.

It is important to note that in all the countries in our sample, oil producing provinces retain a significant portion of oil revenues, which is accomplished through a variety of means. These include: an explicit subnational sharing formula; ad hoc bargaining procedures between subnational levels of the government; retaining of royalties; specially designed taxes collected by local governments (see Bauer et al., 2016). In fact, for some of oil producing provinces, oil revenues constitute a major source of local governments' income, helping to reduce tax burden on households (the Extractive Industries Transparency Initiative, reports from various years, https://eiti.org) (see section 6.1). Thus, we regard oil producing provinces as a treated and non-oil producing provinces as a control group.

It is also essential to note that the interviews in the DHS modules are conducted for everyone residing in the household where the women are interviewed. Women included in the sample could be married or not; and the men in the sample are not only the women's partners, but could be anyone related to the woman and residing with her in the household (i.e., a brother, a father, or a son). Since initially the DHS focused on women, only later on expanding the scope to include male respondents, as will be seen below, most of the respondents in our sample our women.

## 4.2 Outcomes

*Education.*—Our main outcome variable is individual's educational attainment measured by the number of years of education. To obtain this information, we make use of two types of surveys: Women surveys covering women aged 15-53 years old; and Men surveys including male respondents aged 15-56 years old. Our total sample consists of 580,478 individuals born between 1960 and 2001, with 137,924 observations belonging to male respondents, and 442,554 observations representing female respondents. Note that country coverage differs between the two surveys, particularly for male surveys with Egypt and Morocco not included.

*Wealth.*—To measure household's wealth, we construct a wealth index (WI) using information on household characteristics such as household's possession of consumer durable goods, access to basic services, and housing condition. These indicators are then entered into a factor analysis -using the Principal Component Analysis- from which the first factor is selected to derive the asset weights and consequently the wealth index. The obtained WI is rescaled, so that it ranges from 0 to 100, with 0 representing households having no assets and living in lowest quality housing,

and 100 representing households possessing all assets and living in highest quality housing. The advantage of using our WI over the wealth index provided by the DHS lies in the fact that latter is only calculated at the country level, making it only possible to compare households relative to othe households within a given country. In contrast, the WI can be easily used to compare the households' wealth level across provinces, since it uses the same combination of assets and standard assets' weights to rank households independently of where they live.

*Additional outcomes.*—We also consider additional outcomes, such as women age at first marriage, the number of children, employment and type of occupation, work frequency, and husband's level of education and employment the source of which is the women DHS questionnaires.

## 4.3 Independent variables

The main independent variables are an indicator for oil provinces, international oil prices and their interaction.

Oil provinces are allocated based on the map of world oil deposits from PRIO petroleum dataset (Lujala et al., 2007). Onshore oil deposits were assigned to a given province, if the centroid of the deposit lie within its boundaries. For offshore oil deposits, we first calculated the distance between the centroids of the province and the deposit and assigned the latter to the nearest province. International oil prices are given by the average of Dubai, Brent and Texas prices expressed in real 2010 USD and taken from the World Bank Commodities prices dataset.

*Other variables.*—The DHS contain a set of demographic information for both men and women, which we use to construct birth cohorts and control for individual characteristics. These include age, religion, place of residence (i.e. urban vs. rural) and sex of the household head.

## 4.4 Descriptive statistics

Our sample consists of more than 580,000 individuals interviewed over period from 1990 to 2016. Table 1 report some descriptive statistics of our main variables of interest. Several features are worth mentioning. First, individuals residing in oil regions have on average more years of schooling than their peers in non-oil regions, and the difference is statistically significance. Male (female) respondents in oil regions possess on average 2.5 (2.6) more years of schooling. Second, within both oil and non-oil regions, male respondents have more years of schooling than female respondents, and the difference is larger in non-oil regions. Third, the highest attained educational level for an individual is, on-average, 7.3 years in oil regions, which is equivalent to the completion of primary education plus some post primary education. In non-oil regions, on average individuals do not manage to complete primary education. Similarly, the household wealth tends to be higher in oil regions than in non-oil regions, with more wealth possessions reported by male respondents than female respondents.

# 5 Empirical Strategy

Our conceptual framework proposes two main testable hypotheses: (1) a permanent income shock has differential effects on the educational attainment - and wealth - depending on the age of the child, with positive effects dominating at early life of a child; (2) the positive effects are larger for male children compared to female children. To test these hypotheses, our empirical strategy looks at the effect of oil price shocks at different periods of individual's life and across gender. Our baseline specification takes the following form:

$$Y_{irBt} = \beta(OPS_{Bt} \times OilProvince_r) + \alpha X_{iBt} + \phi_{Bt} + \delta_r + \delta_r \times Bt + \epsilon_{irBt} \quad (12)$$

where the outcome $Y_{irBt}$ is either the years of schooling or, alternatively, the aggregate household's wealth of individual $i$ born in year $B$ and residing in province $r$ during the age interval $t(1, 2, 3)$. We focus on three periods over the individual's life course: (i) pre-school years ($t = 1$, ages 0-4); (ii) primary school years ($t = 2$, 5-9); and (iii) years with completed primary education and some post-primary education ($t = 3$, 10-14), referred sometimes to as adolescence. $OPS_{Bt}$ is the logarithm of the five year moving average of real oil prices. $OilProvince_r$ is a dummy variable that takes a value of 1, if a given province is producing oil. $\phi_{Bt}$ is age interval fixed effects to capture period-varying shocks that are common across individuals in a cohort, and $\delta_r$ is provincial fixed effects to control for time-invariant unobserved heterogeneity at the province level. The $\delta_r \times Bt$ is provincial-specific time trend to account for provincial trends that might be correlated with both educational attainment and oil prices. $X_{iBt}$ is a set of time-varying controls at the individual level including indicators for urban residency, religion, female household head, month of survey, and year of survey.[11] The above equation is estimated on the full sample of individuals and separately for both men and women to reflect potential gender heterogeneous effects. When estimating the full sample, we also for control for gender. Standard errors are clustered at the province level.[12]

Our approach assumes that oil prices are exogenous, which is plausible because, with the exception of Nigeria and Angola, most of our sampled countries are low oil-producing countries contributing less than 1% to total world oil production. For Nigeria and Angola, despite being the largest two oil-producing countries in Africa, their production in 2016 accounted for only 2% of world production each.[13] Furthermore, by interacting oil prices with indicators for oil provinces conditional upon provincial and age-interval fixed effects, we are exploiting differential effects of oil price changes depending on access on oil in the spirit of a difference-in-difference strategy, whereby individuals from the same birth cohort are divided into treatment and control groups, depending on the experience of an oil price shock.

Based on our hypotheses, we expect the sign of $\beta$ to be positive at period $t = 1$,

[11] As annual oil prices are colinear with year fixed effect, the presence of variables capturing the latter component implies that the average effect of the former is accounted for.

[12] Ideally, it would be useful to control for household circumstances of the respondents in their childhood, when being faced with a shock, such as its size or gender composition. Unfortunately, this information is not available in the DHS.

[13] https://www.cia.gov/library/publications/the-world-factbook/rankorder/2241rank.html

or during early childhood years with effect being relatively larger for male children compared to their female peers. For the next two periods, the sign of $\beta$ could be either positive or negative, with the former being smaller in magnitude compared to period 1. If $\beta$ is negative, then we would expect the negative effect to be larger for females.

# 6 Main results

## 6.1 Preliminaries

Before addressing our main hypotheses, we make sure that some of our fundamental assumptions hold true. One of these assumptions is that oil price shocks over designated time intervals are persistent. To this end, consider Figure 2, which describes the behavior of oil prices over time and whose three panels correspond to our 5-year intervals. To substantiate the visual impression that it is persistent, Appendix Table A1 presents results of formal tests for unit roots, which provides support for our assumption.

Then we test whether oil price changes can be interpreted as income changes at a provincial level in our sample. As there is no reliable data on the latter, we resort to estimates, from Lessmann and Seidel's (2017) estimates of provincial income obtained using nighttime light as a proxy. The data is computed based on nighttime lights collected from satellite data provided by the National Oceanic and Atmospheric Administration (NOAA) and is available for the period between 1992 and 2012. We then correlate oil prices with these estimates. The results, displayed in Figure 3, and reinforced through regressions in Appendix Table A2, reassure that, indeed, oil price changes can be interpreted as provincial income change.

## 6.2 Educational attainment

We begin by taking a broad look at the data. To this end, we run an OLS regression on our sample, with the outcome variable being years of schooling attained by resident $i$ in region $r$; and the main explanatory variable being $(OPS_{0-14} \times OilProvince_r)$, where $OPS_{0-14}$ is the logarithm of the 14 year moving average of real oil prices, and $OilProvince_r$ is a dummy variable that takes a value of 1, if a given province is producing oil. Table 2 presents the results. For the entire sample, in columns 1 and 2, they are insignificant when only region, region specific time trend, and age group fixed effects are included as control variables, and just barely significant (at 10% level) when all controls are included, in which case the negative estimated coefficient implies that oil price shocks experienced in childhood reduces the subsequent educational attainment. When the analysis is decomposed by gender, in columns 3-6, it turns out that the negative effect of oil price shocks on educational attainment is driven through the effect on females; the male regression returns insignificant results.

We then proceed with our main analysis, by age groups, as the above analysis potentially masks differences across those. The results of estimating equation 12 are

presented in Table 3. They indicate (see Panel A) that oil price shocks experienced in early childhood enhance subsequent educational attainment, in the entire sample, and separately for females and males. This is consistent with the vast literature on the effect of early childhood income shocks on future outcomes, such as Fenske and Zurimendi, 2017, in the context of Nigeria's oil price shocks. Interestingly, and a new finding relative to the early childhood perspective, oil price shocks experienced between the ages 5-9 have an insignificant effect on subsequent educational attainment for neither gender group, nor for the entire sample, see Panel B.

Even more interesting is the negative effects of such shocks in early adolescence (ages 10-14) on subsequent educational attainment, Panel C. While these results are obtained for each of the gender groups (as well as for the full sample), it is more pronounced for women, with 1% of statistical significance. Not only are these results statistically significant, they are also economically meaningful. Thus, recalling our regression specification, the estimated coefficients, between -0.45 and -0.25, indicate that a one percent increase in the average oil price during the 10-14 age decreases subsequent educational attainment in oil producing regions by a third of the year. A back of the envelope calculation may help to translate these figures into elasticities. A one percent increase in the average oil price can be estimated to be about five percent aggregate increase over the adolescence period. And, since the average of years of schooling in our sample is 5.3, the decrease of a third of the year constitutes some seven percent of this average. With the five percent increase in the oil price results being equivalent to a seven percent decrease in schooling, it then follows that the elasticity of educational attainment of an adolescent individual with respect to an oil price shock is about -1.4 on average.[14]

These results are consistent with our theoretical framework in two ways. First, they indicate that oil wealth increase experienced in adolescence may be detrimental for schooling; and, consistently with the model, they are particularly pronounced for women. Whereas the former is consistent with Ahlerup et al., 2020, who find that gold booms are detrimental for schooling of 12 year old, the latter result differs from that paper's suggestions in detecting a larger effect on female gender.

## 6.3 Wealth

Eventual wealth later in life is our secondary outcome of interest. Our conceptual framework indicates that income shocks have a positive effect on accumulated wealth because of the standard income effect, but either a positive or a negative effect via the schooling channel. This then implies that both effects operate in the same positive direction in early childhood, but their joint outcome can be ambiguous in adolescence, depending on the relative strength of the two effects.

In Table 4 we, therefore, replicate our analysis using wealth as the outcome variable. Panel A in that table confirms the positive effect of early childhood income shocks on subsequent wealth, and Panel B presents insignificant results for the 5-9 age group. Panel C yields negative coefficients for the entire sample as well as for

[14]We have also explored, rather than years of schooling, completion of educational stages, as our outcome variable. The results, reported in the appendix table B1 , note for publication, are qualitatively the same.

each of the two gender groups, indicating that the results with respect to wealth are qualitatively similar to those with respect to schooling. In particular, positive oil shocks in adolescence reduce subsequent wealth. While this finding is also of an independent interest, our above analysis suggests that the reduction in educational attainment is a possible channel for this outcome.[15]

# 7 Robustness and extension

We now carry out several robustness checks. They pertain to various selections of the sample regions and periods; the population demographics; prices of other minerals than oil; and interrelationship between shocks experienced at different ages in childhood/adolescence.[16] We also consider variations on the construction of the wealth index, and explore additional outcomes as an extension.

## 7.1 Countries' and period selection

We first explore the robustness of our results to the sample of countries in the main analysis. To this end, we cut the sample in several ways. In Table 5, columns 1-3, we focus on two biggest oil producing countries in our sample, Nigeria and Angola. We observe that relative to the baseline results, while the results remain qualitatively unchanged, both their statistical and economic significance increase. Thus, in this context, oil price shocks in the lowest (highest) age group positively (negatively) affect future schooling attainment, typically at the 1% statistical significance level. We also replicate, in columns 4-6 of Table 5, our baseline analysis for a group of countries (Angola, Nigeria, Egypt, Congo and Morocco) that have continuously produced oil beginning in 1960 – the earliest year for which we have data from the DHS. Again, like in the case of Nigeria and Angola, the results are only reinforced relative to the baseline analysis.

We then alternatively drop countries with only one oil producing region (which leaves us with 177 regions as opposed to 247 in the full sample). Here, the results (presented in Appendix Table A3, columns 1-3) are not substantively different from those in the main analysis. Likewise, when non-oil producing African countries are added, the results (in Appendix Table A3, columns 4-6) remain qualitatively unchanged, although their statistical significance, not surprisingly, drops. Finally, we randomly reshuffle oil producing and non-oil producing provinces as a placebo test. We find, in Appendix Table A4, that this results in nonsignificant correlations with oil price changes, indicating that the latter are relevant for oil producing provinces.

Oil prices spiked in the post 1973 period, and one may wonder if this structural break may have affected the identified effects. To this end, we replicate our analysis for the post 1973 period – which reduces our sample by more than 15 percent; yet, the results remain essentially the same, see Table 5, columns 7-9.

[15] There may also be additional channels directly related to natural resource curse, as briefly reviewed in the Introduction.

[16] These robustness results are presented for educational attainment as the outcome variable, but they also broadly hold for the wealth index as well.

## 7.2 Demographics

In Table 6, we restrict our sample to respondents being at least 18 years old, the standard high school graduation age. This reduces the sample by almost fifteen percent, but leaves our baseline results almost unchanged qualitatively. We then consider the possibility that migration may have a bearing on our results. To this end, we define a non-migrant as a respondent who has always resided in her current place of residence and introduce an additional control variable – the interaction term between oil price shock and the non-migrant dummy. The results, in Appendix Table A5, are by and large very similar to the baseline results, and the newly introduced control is never significant. More generally, one may be concerned about broader demographic changes that took place in the sample provinces and that could be correlated with oil price shocks. To alleviate those, we correlate the latter with various demographic measures at a provincial level, including migration. We observe, in Appendix Table A6, that none of those (total population, urban vs rural, gender composition, migration flows) displays a significant correlation with lagged oil prices.

## 7.3 Prices of other minerals

Africa's continent is rich with minerals, and one potential concern is that our results might be driven by fluctuations in the prices of those minerals as opposed to that of oil. To alleviate this concern, we introduce in our baseline regression as an additional control variable prices of other minerals interacted with their producing region. We specifically focus on gold, silver, aluminum, copper, lead, nickel, zinc and tin. This choice is dictated by the following two main consideration. For one, a mineral has to be of at least some importance for at least one country in our sample; and the country should not be a major world producer of the mineral so as to be able to affect its price. We then construct the variable, mineral price shock, as follows. For each region in our sample, we select the main mineral produced based on the frequency of the production of this mineral across different mines which is cross-checked by the main minerals that the country is producing.[17]This enables us to construct a dummy variable for each region indicating presence of lack thereof of a main mineral. Multiplying this variable by the mineral's price yields mineral price shock index, which is then constructed similar to the oil price shock. Regression results are shown in Table 7. Comparing with respective columns 2, 4, and 6 in Table 2, we observe that the main coefficients of interest in columns 1, 2, and 3, get marginally reduced (in absolute value), but still remain significant. In particular, one of our main findings, that oil price shocks are detrimental for schooling when experienced in adolescence, remains highly significant.

[17]Minerals are allocated to regions using the Mineral Resources Data System (MRDS) from U.S. Geological Survey (USGS) (https://mrdata.usgs.gov/mrds/).

## 7.4 Controlling for early life shocks and robustness with respect to the definition of age intervals

While our results with respect to the positive effect of early life shocks on subsequent educational attainment and wealth are not too surprising in the light of much existing work on the subject, the negative effect of shocks in adolescence is more novel. To further tease out this result, we now would like to more clearly separate this type of shock from the early life shock.[18] To do so, we run a regression with adolescence shocks, now controlling for early life (ages 0-4) shocks. The results, in Appendix Table A7, while indicating a reduced significance for the males sample, continue to hold. In particularly noteworthy is the highly significant negative effect of the shock in the female sample.

We have also conducted multiple robustness checks with respect to our definition of age brackets, by dividing them up in numerous ways. Appendix Tables B2 and B3, not for publication, report a sample of these exercises, from which it follows that the results are not at all qualitatively sensitive to the way these brackets are defined.

Finally, we consider the supply side, namely, we check whether school availability has changed differentially in oil rich and oil poor provinces in response to oil price shocks.[19] To this end, we employ the Afrobarometer rounds 2-6 (covering the years from 2002-2015),[20] and construct the school availability variable utilizing the following question "Is there a school present in your neighborhood?" Aggregation of these responses at the regional level yields the number of schools we use in Appendix Table B4, not for publication. As can be seen from that table (even rows), school availability does not react differentially to lagged oil price shocks – which is reassuring as it indicates that the supply side is not a major factor in our case.

## 7.5 Modifications of the wealth index

Our construction of the wealth index combines several household amenities and aggregates them via the principal component procedure. We now conduct some robustness exercises with respect to amenities' selection (using the same aggregation procedure throughout). In Appendix Table A8, we add the number of sleeping rooms in the house as yet another attribute; and in Appendix Table A9 we omit two items least correlated with the principal component, namely, radio and bike ownership. As can be seen, the results remain virtually unchanged relative to the baseline analysis in Table 4.

[18] Annual oil prices are autocorrelated, but this autocorrelation is much less pronounced over the span of a decade.

[19] Note that, for our purposes, what primarily matters is the differences in availability of schooling for the different age groups, but this information is not available. However, if aggregate school supply is not differentially reactive to oil price shocks, that would be a solid indication that the supply side is immaterial here.

[20] Not all regions and countries in our sample are covered across all the waves.

### 7.6 Other outcomes

Our main result indicates that early life (adolescence) income shocks positively (negatively) affect female educational attainment. We now consider additional outcomes pertaining to adult female circumstances, specifically, age at first marriage, the number of children, employment and type of occupation, work frequency, and husband's level of education and employment. The results, in Table 8, column 1, indicate that early life (adolescence) income shocks increase (reduce) females' age at first marriage, which is consistent with broad empirical regularities, specifically, the positive association between female schooling and age of marriage as documented in sociological literature, see Saardchom and Lemaire (2005). Further, as follows from Table 8, column 2, early life (adolescence) income shocks reduce (increase) their number of children. Recalling our main result, this finding indicates that early life income shocks enhance women schooling and also reduce their number of children as has been extensively documented in general (Cochrane, 1978); for poor countries (Schultz, 19898); and, specifically, in Africa's context (Osili and Long, 2008). In columns 3 and 4, we check whether early life (adolescence) income shocks affect women's employment status and type of occupation. Results show that these income shocks do not have a statistically significant impact on women's likelihood to be employed; however, the quality of the job matters. Early life (adolescence) income shocks increase (reduce) females' chances to get a skilled job, which is not surprising given that skilled jobs require a higher level of educational attainment. Finally, columns 6-8 show that females who have experienced income shocks in their childhood are more likely to work on all year basis and have a partner with active employment status; in contrast, females who have experienced that in their adolescence years are less likely to work on annual basis and more likely to have a partner who is not working. However, income shocks do not seem to affect the partner's educational level of attainment in a statistically significant manner.

## 8 Conclusion

Whether or not natural resource wealth is a blessing or a curse in general and with respect to human capital accumulation, specifically, has been a long-standing subject of interest for economists. Existing literature has come up with contradictory conclusions in this regard. We contribute to this literature studying how income shocks resulting from oil price fluctuations have affected educational attainment, ultimate household's wealth, and additional derived outcomes, depending on children age of incidence of these shocks and utilizing a large sample of Africa's households. The proposed conceptual framework used to organize thinking about the issue, indicates that incidence timing is crucial, whereby the potentially positive affect of income shocks on educational attainment (and future wealth) diminishes with a child's age, possibly turning to being negative if the income shock is experienced in adolescence, with a more pronounced decline among girls than boys. The mechanism at work is simply the crowding out of educational investment by a resource income windfall.

Our empirical analysis generates results consistent with the above theoretical hypotheses. In particular, and in line with the vast existing work, we document

that income shocks in early childhood (ages 0-4) have a positive effect on subsequent educational attainment and wealth, for both gender groups. More surprisingly, however, we find that income shocks in adolescence (ages 10-14) have a negative effect on both outcomes, especially for girls. These results hold under a variety of robustness checks and they are economically significant. Focusing on adolescent girls who have experienced a positive oil price shock, we further find that they tend to marry earlier and have more children than those who have not experienced such a shock. These findings are consistent with those on educational attainment and with existing literature documenting that education causes women to marry later and to have fewer children.

Summarizing, therefore, this research depicts a convoluted picture of the effect of persistent income windfalls resulting from natural resources on schooling and associated outcomes that crucially depends on the incidence period of such windfalls. Our results may help to reconcile the often contradictory findings pertaining to the effect of natural resource wealth on educational attainment and indicate that the aggregate effect depends on the age distribution of the affected children. Further, such income windfalls have important distributional consequences across the households, depending on the age and gender distribution of the children within a household. Without paying a careful attention to these aspects, the assessment of the effects of income shocks may yield biased results.

While the empirical analysis relies on income shocks stemming from oil price fluctuations, our theoretical model suggests that the basic mechanism at work can be applicable to any persistent (unconditional) income shock that has the potential to crowd out educational investment. This may have policy implications, indicating in particular that a permanent unconditional income transfer may under certain circumstances be an impediment for educational attainment, especially for women, and detrimental even for ultimate household's wealth and related outcomes. This is an important observation for future research as many existing poverty alleviation income support policies rely precisely on this type of income transfers. Hence, the results of this research may inform policy makers as to potential pitfalls of such programs.

# References


Abramson, S. and E. Esposito, 2020. "The Resource Curse in the Long-Run," a mimeo.

Adhvaryu, A., Fenske, J., and Nyshadham, A., 2019, "Early Life Circumstance and Adult Mental Health," Journal of Political Economy, 127.

Ahlerup, P., T Baskaran, A Bigsten, 2020, "Gold mining and education: a long-run resource curse in Africa?" Journal of Development Studies, 56, 1745-62.

Alderman, H. and E.M. King, 1998, "Gender differences in parental investment in education," Structural Change and Economic Dynamics, 9 (4) (1998), 453-468

Alexeev, M. and R. Conrad. 2009. "The Elusive Curse of Oil." Review of Economics and Statistics 91, 586–598.

Almond, D. and J. Currie, 2011, "Human capital development before age five," Handbook of Labor Economics, Ch 15.

Almond, D., J. Currie, and V. Duque, 2018, "Childhood Circumstances and Adult Outcomes: Act II," Journal of Economic Literature, 2018, 56(4), 1360–1446.

Auty RM, 1994, Industrial policy reform in six large newly industrializing countries: the resource curse thesis. World Development, 2, 11–26.

Aragon, F. and P. Rud, 2013, "Natural Resources and Local Communities: Evidence from a Peruvian Gold Mine," American Economic Journal: Economic Policy 5, 1-25.

Arezki, R, P Loungani, R van der Ploeg and T and Venables, 2014, "Understanding International Commodity Price Fluctuations", Journal of International Money and Finance 42, 1-8.

Badeeb RA, Lean HH, Clark J. 2017, "The evolution of the natural resource curse thesis: a critical literature survey. Resources Policy. 51:123–34.

Black, Dan, Terra McKinnish, and Seth Sanders. 2005. "The Economic Impact of the Coal Boom and Bust." Economic Journal, 115 (503): 449–476.

Brueckner, M. and M Gradstein, 2016, Income and Schooling: Evidence from International Oil Price Shocks, Journal of Human Capital 10, 212-234.

Caselli, Francesco, and Guy Michaels (2013). "Do Oil Windfalls Improve Living Standards? Evidence from Brazil," American Economic Journal: Applied Economics, 5, 208-238.

Cavalcanti, T., De Mata, D., and F. Toscani, 2019, "Winning the Oil Lottery: The Impact of Natural Resource Extraction on Growth" Journal of Economic Growth, 24(1): 79-115.

Cochrane, S.H., 1979. Fertility and Education: What Do We Really Know? Johns Hopkins Press, Baltimore.

Crivelli, E., Gupta, S., 2014. Resource blessing, revenue curse? Domestic revenue effort in resource-rich countries. European Journal of Political Economy 35, 88–101.

Cunha F. and J. Heckman (2007), The Technology of Skill Formation. American Economic Review 97(2), 31-47.

Demographic and Health Surveys (DHS), 2020. Available at: https://dhsprogram.com/Data/

Fenske, J. and I. Zurimendi, 2017, "Oil and ethnic inequality in Nigeria," Journal of Economic Growth, 22, 397–420.

Gradstein, M. and M. Klemp, 2020, "Natural Resource Access and Local Economic Growth,", European Economic Review, 127.

Gylfason, T., 2001, "Natural resources, education, and economic development," European Economic Review, 45, 847-859.

Havranek, T., Horbath, R., and A. Zyenalov, 2016, "Natural resources and economic growth: A meta-analysis research," World Development, 88, 134-151.

Hsieh, C.-T., 2003, "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund," American Economic Review, 93, 397-405.

ILO, 2017, "Global estimates of child labour: Results and trends, 2012-2016," Geneva, Available at: https://www.ilo.org/wcmsp5/groups/public/—dgreports/dcomm/documents/publication/wcms_575499.pdf

Ishak, P.W. and M. R. Farzanegan, 2020, "The impact of declining oil rents on tax revenues: Does the shadow economy matter?" Energy Economics, 92.

James, A., 2015. US state fiscal policy and natural resources. American Economic Journal Economic Policy 7 (3), 238–257.

Kaul, T., 2018, "Intra-household allocation of educational expenses: Gender discrimination and investing in the future," World Development, 336-343.

Kingdon, G. G. (2005). Where has all the bias gone? Detecting gender bias in the intrahousehold allocation of educational expenditure. Economic Development and Cultural Change, 53(2), 409-451.

Kotsadam, A. and A. Tolonen, 2016, "African Mining, Gender, and Local Employment," World Development, 83, 325–339.

Lavy, V., A. Schlosser, and A. Shany. 2016, "Out of Africa: Human Capital Consequences of In Utero Conditions." National Bureau of Economic Research Working Paper 21894.

Lessmann, C. and A. Seidel, 2017, "Regional Inequality, Convergence, and its Determinants - A View from Outer Space," European Economic Review, 92(1), 110–132,

Lujala, P., Rød, J. K., and Thieme, N. (2007). "Fighting Over Oil: Introducing a New Dataset," Conflict Management and Peace Science 24(3): 239–256.

Maccini, S. and Yang, D. (2009). "Under the weather: Health, schooling, and economic consequences of early-life rainfall," American Economic Review, 99,1006–1026.

Osili, U. O., and B. T. Long. "Does female schooling reduce fertility? Evidence from Nigeria." Journal of Development Economics 87:1 (2008): 57–75.

Papyrakis, E., and R. Gerlagh (2004), "The resource curse hypothesis and its transmission channels," Journal of Comparative Economics, 181-193.

Papyrakis, E., and R. Gerlagh (2007) "Resource Abundance and Economic Growth in the United States." European Economic Review, 51, 1011-1039.

Saardchom, N. and J. Lemaire (2005) "Causes of Increasing Ages at Marriage: An International Regression Study, Marriage & Family Review 37, 73-97.

Sachs, Jeffrey D., and Andrew M. Warner. 2001. "The Curse of Natural Resources." European Economic Review, 45(4-6): 827–838.

Schultz, T.P., 1998. Demand for children in low income countries. Handbook of Population and Family Economics, Elsevier Press, Amsterdam.

Shah, M. and B.M. Steinberg, 2017, "Drought of Opportunities: Contemporaneous and Long-Term Impacts of Rainfall Shocks on Human Capital Investment", Journal of Political Economy, 125(2), 527-561.

Stijns, J.P., 2006, "Natural resource abundance and human capital accumulation," World Development, 34, 1060-1083.

UNICEF, 2015, "Oil and gas scoping study. Extractive pilot," www.unicef.org/csr/files/Oil_and_Gas_Scoping_Paper_19012015.pdf

van der Ploeg, F., 2011, "Natural Resources: Curse or Blessing?" Journal of Economic Literature, 49, 366-420.

World Bank Commodities prices dataset, 2020. Available at: http://databank.worldbank.org/data/databases/commodity-price-data.

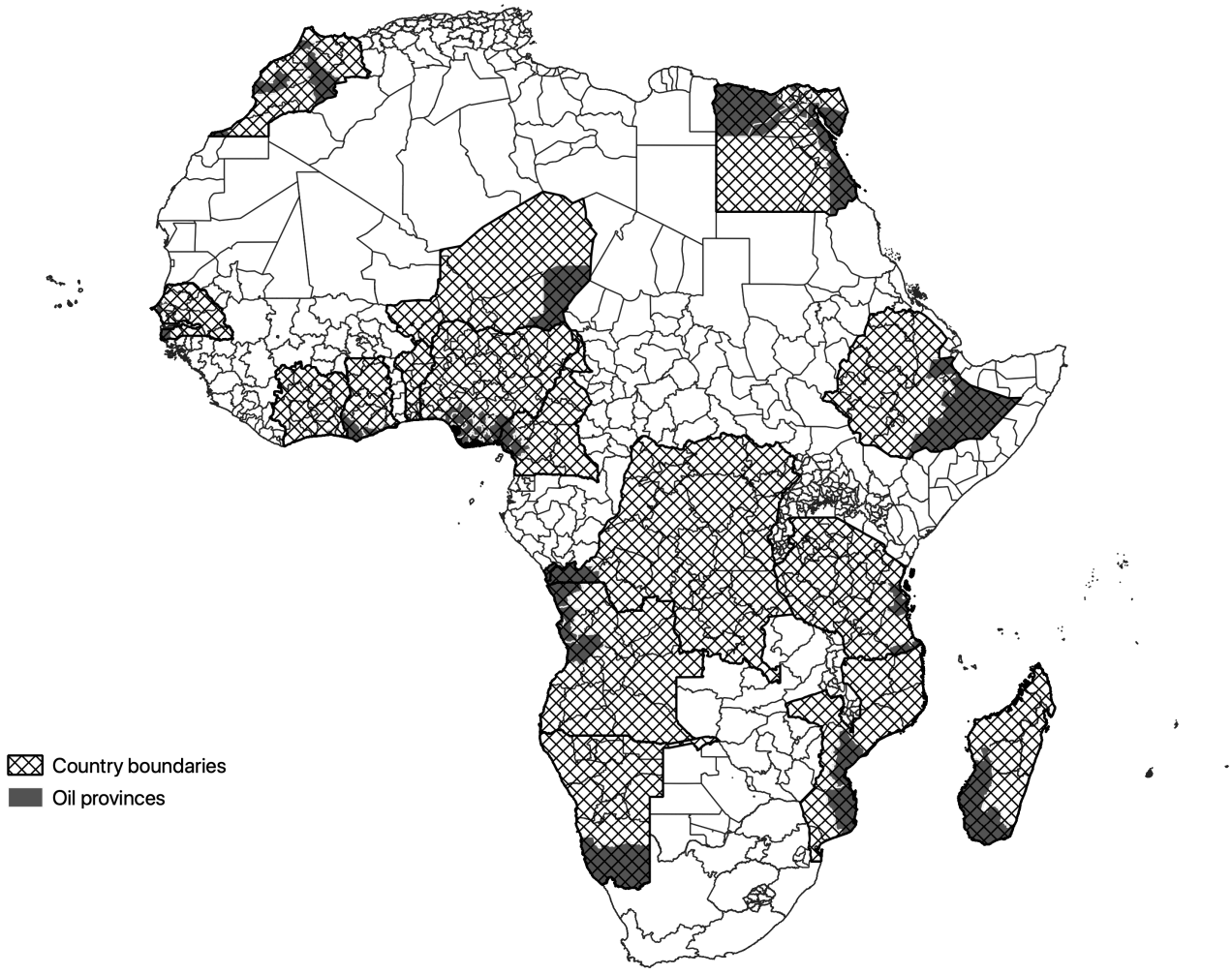



**Figure 1: Location of oil provinces**

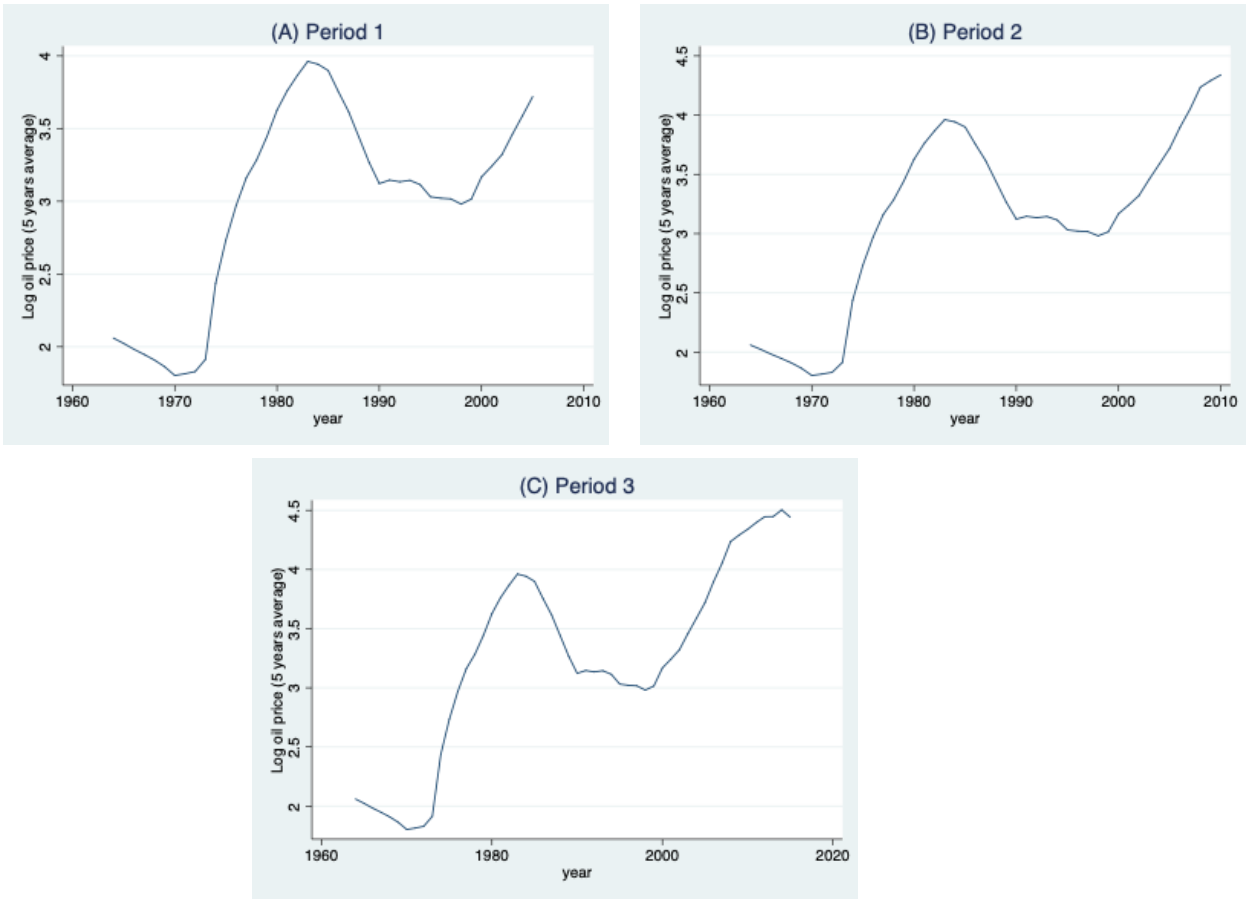
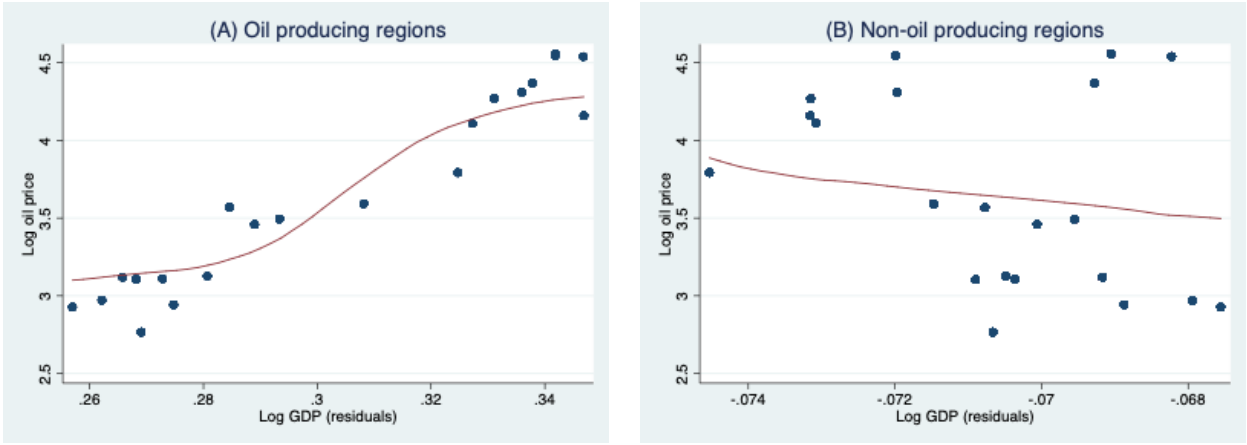



**Figure 2: Log oil price over the 3 periods**




Notes: Both figures are net of province and year fixed effects. The solid line represents the nonparametric local polynomial fit computed using an Epanechnikov kernel.

**Figure 3: Correlation- Log oil prices and average log GDP per capita at the province level**

**Table 1: Summary statistics**

| Variable | N | Mean | SD | Min | Max | Difference |
|---|---|---|---|---|---|---|
| **Education** | | | | | | |
| Oil regions (All sample) | 112,982 | 7.330 | 4.836 | 0 | 24 | *** |
| Oil regions (Male sample) | 27,854 | 8.430 | 4.455 | 0 | 24 | ***[a] |
| Oil regions (Female sample) | 85,128 | 6.970 | 4.901 | 0 | 24 | *** |
| Non-oil regions (All sample) | 467,496 | 4.802 | 4.832 | 0 | 24 | |
| Non-oil regions (Male sample) | 110,070 | 6.032 | 4.801 | 0 | 24 | |
| Non-oil regions (Female sample) | 357,426 | 4.423 | 4.778 | 0 | 24 | |
| **Wealth** | | | | | | |
| Oil regions (All sample) | 92,334 | 30.114 | 24.582 | 0 | 100 | *** |
| Oil regions (Male sample) | 26,799 | 30.600 | 24.241 | 0 | 100 | ***[a] |
| Oil regions (Female sample) | 65,535 | 29.915 | 24.717 | 0 | 100 | *** |
| Non-oil regions (All sample) | 374,420 | 17.496 | 21.313 | 0 | 100 | |
| Non-oil regions (Male sample) | 102,853 | 17.275 | 20.889 | 0 | 100 | |
| Non-oil regions (Female sample) | 271,567 | 17.580 | 21.471 | 0 | 100 | |
| **Log oil price** | | | | | | |
| Period 1 | 580,478 | 3.049 | 0.660 | 1.803 | 3.962 | |
| Period 2 | 580,478 | 3.231 | 0.550 | 1.803 | 4.340 | |
| Period 3 | 580,478 | 3.458 | 0.451 | 2.437 | 4.504 | |

[a] indicates the different between male and female samples is satistically significant at 1% significance level. Difference is based on nonparametric K-sample test on the equality of medians.

**Table 2: The effect of OPS experienced in all age groups together**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Periods (0-14 years old) | | | | | |
| | Education | Education | Education | Education | Education | Education |
| | Full sample | Full sample | Male sample | Male sample | Female sample | Female sample |
| Oil price shock × Oil Province | -0.190 | -0.210* | 0.025 | 0.017 | -0.246* | -0.268** |
| | (0.127) | (0.119) | (0.144) | (0.145) | (0.131) | (0.126) |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 580,478 | 580,478 | 137,924 | 137,924 | 442,554 | 442,554 |
| Number of regions | 247 | 247 | 220 | 220 | 247 | 247 |
| R-squared | 0.320 | 0.420 | 0.297 | 0.387 | 0.350 | 0.431 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |

The dependent variable is the number of years of education. Oil price shock is the ln-14 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table 3: Main education results: the effect of OPS experienced in different age periods**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | | | | |
| | **Education** | **Education** | **Education** | **Education** | **Education** | **Education** |
| | **Full sample** | **Full sample** | **Male sample** | **Male sample** | **Female sample** | **Female sample** |
| Oil price shock × Oil Province | 0.238** | 0.190* | 0.227** | 0.203** | 0.219** | 0.184* |
| | (0.105) | (0.097) | (0.097) | (0.099) | (0.106) | (0.101) |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 580,478 | 580,478 | 137,924 | 137,924 | 442,554 | 442,554 |
| Number of regions | 247 | 247 | 220 | 220 | 247 | 247 |
| R-squared | 0.320 | 0.420 | 0.297 | 0.387 | 0.350 | 0.431 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | | | | |
| Oil price shock × Oil Province | -0.062 | -0.084 | 0.064 | 0.057 | -0.097 | -0.120 |
| | (0.080) | (0.075) | (0.097) | (0.093) | (0.081) | (0.078) |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 580,478 | 580,478 | 137,924 | 137,924 | 442,554 | 442,554 |
| Number of regions | 247 | 247 | 220 | 220 | 247 | 247 |
| R-squared | 0.320 | 0.420 | 0.297 | 0.387 | 0.350 | 0.431 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | | | | |
| Oil price shock × Oil Province | -0.452*** | -0.397*** | -0.269** | -0.240* | -0.472*** | -0.436*** |
| | (0.121) | (0.124) | (0.127) | (0.139) | (0.125) | (0.126) |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 580,478 | 580,478 | 137,924 | 137,924 | 442,554 | 442,554 |
| Number of regions | 247 | 247 | 220 | 220 | 247 | 247 |
| R-squared | 0.320 | 0.420 | 0.297 | 0.387 | 0.350 | 0.431 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |

The dependent variable is the number of years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table 4: The effect of OPS experienced in different age periods on future wealth**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Wealth** | **Wealth** | **Wealth** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.647** | 0.849** | 0.592** |
| | (0.270) | (0.345) | (0.295) |
| Controls | Yes | Yes | Yes |
| Number of observations | 466,754 | 129,652 | 337,102 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.455 | 0.458 | 0.456 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | |
| Oil price shock×Oil Province | 0.042 | 0.208 | -0.001 |
| | (0.242) | (0.408) | (0.252) |
| Controls | Yes | Yes | Yes |
| Number of observations | 466,754 | 129,652 | 337,102 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.455 | 0.457 | 0.456 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.899*** | -0.821* | -0.942*** |
| | (0.314) | (0.426) | (0.328) |
| Controls | Yes | Yes | Yes |
| Number of observations | 466,754 | 129,652 | 337,102 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.455 | 0.457 | 0.456 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the household wealth index constructed as explained in text. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table 5: Robustness for selection of countries and period**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | **Only Angola and Nigeria** | | | **Oil producing countries since 1959** | | | **Post 1973 period** | | |
| | **Panel A. Period 1 (0-4 years old)** | | | | | | | | |
| | **Education** | **Education** | **Education** | **Education** | **Education** | **Education** | **Education** | **Education** | **Education** |
| | **Full sample** | **Male sample** | **Female sample** | **Full sample** | **Male sample** | **Female sample** | **Full sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.356*** | 0.214** | 0.410*** | 0.265*** | 0.246*** | 0.258*** | 0.159* | 0.209* | 0.158* |
| | (0.115) | (0.103) | (0.126) | (0.088) | (0.093) | (0.094) | (0.086) | (0.107) | (0.088) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 134,846 | 37,911 | 96,935 | 237,383 | 49,328 | 188,055 | 479,319 | 114,876 | 364,441 |
| Number of regions | 55 | 55 | 55 | 108 | 81 | 108 | 247 | 220 | 247 |
| R-squared | 0.459 | 0.341 | 0.491 | 0.372 | 0.324 | 0.377 | 0.426 | 0.383 | 0.442 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | | | | | | | |
| Oil price shock $_{O}ilProvince$ | -0.180 | -0.145 | -0.173 | -0.074 | -0.087 | -0.078 | -0.553*** | -0.057 | -0.676*** |
| | (0.117) | (0.132) | (0.126) | (0.090) | (0.122) | (0.095) | (0.180) | (0.202) | (0.181) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 134,846 | 37,911 | 96,935 | 237,383 | 49,328 | 188,055 | 479,319 | 114,876 | 364,441 |
| Number of regions | 55 | 55 | 55 | 108 | 81 | 108 | 247 | 220 | 247 |
| R-squared | 0.459 | 0.341 | 0.491 | 0.372 | 0.324 | 0.377 | 0.426 | 0.383 | 0.442 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | | | | | | | |
| Oil price shock $_{O}ilProvince$ | -0.689*** | -0.385*** | -0.775*** | -0.501*** | -0.363*** | -0.520*** | -0.490*** | -0.252 | -0.556*** |
| | (0.141) | (0.132) | (0.148) | (0.112) | (0.119) | (0.115) | (0.157) | (0.161) | (0.160) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 134,846 | 37,911 | 96,935 | 237,383 | 49,328 | 188,055 | 479,319 | 114,876 | 364,441 |
| Number of regions | 55 | 55 | 55 | 108 | 81 | 108 | 247 | 220 | 247 |
| R-squared | 0.459 | 0.341 | 0.491 | 0.372 | 0.324 | 0.377 | 0.426 | 0.383 | 0.442 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

The dependent variable is the years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table 6: Sample restricted to at least 18 years old**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Education** | **Education** | **Education** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.147* | 0.196** | 0.136 |
| | (0.081) | (0.094) | (0.087) |
| Controls | Yes | Yes | Yes |
| Number of observations | 506,949 | 117,951 | 388,998 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.427 | 0.395 | 0.436 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | |
| Oil price shock × Oil Province | -0.009 | 0.083 | -0.027 |
| | (0.079) | (0.090) | (0.085) |
| Controls | Yes | Yes | Yes |
| Number of observations | 506,949 | 117,951 | 388,998 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.427 | 0.395 | 0.436 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.263*** | -0.215* | -0.276*** |
| | (0.088) | (0.123) | (0.092) |
| Controls | Yes | Yes | Yes |
| Number of observations | 506,949 | 117,951 | 388,998 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.427 | 0.395 | 0.436 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table 7: Controlling for other minerals**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Education** | **Education** | **Education** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.190* | 0.202** | 0.184* |
| | (0.096) | (0.099) | (0.101) |
| Mineral price shock × Mineral Province | 0.021*** | 0.013 | 0.011 |
| | (0.007) | (0.011) | (0.009) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | |
| Oil price shock × Oil Province | -0.084 | 0.057 | -0.120 |
| | (0.075) | (0.093) | (0.078) |
| Mineral price shock × Mineral Province | 0.020** | 0.014 | 0.011 |
| | (0.008) | (0.011) | (0.009) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.431 | 0.420 | 0.387 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.397*** | -0.238* | -0.436*** |
| | (0.124) | (0.139) | (0.126) |
| Mineral price shock × Mineral Province | 0.017** | 0.013 | 0.008 |
| | (0.008) | (0.012) | (0.010) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Mineral price shock is the ln-5 years average of mineral price for period t multiplied by a dummy that take a value of 1 if the region is producing a given mineral. Minerals included are gold, silver, aluminium, copper, lead, nickel, zinc and tin. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table 8: Other outcomes**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | | | | | |
| | **Number of children** | **Age at first marriage** | **Employed** | **Skilled occupation** | **Work all year** | **Husband's education** | **Husband's working** |
| | **Female sample** | **Female sample** | **Female sample** | **Female sample** | **Female sample** | **Female sample** | **Female sample** |
| Oil price shock × Oil Province | -0.124*** | 0.400*** | 0.008 | 0.006** | 0.015** | 0.015 | 0.018** |
| | (0.040) | (0.088) | (0.009) | (0.003) | (0.006) | (0.059) | (0.009) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 442,479 | 336,429 | 441,627 | 441,627 | 233,134 | 318,689 | 328,181 |
| Number of regions | 247 | 220 | 247 | 247 | 220 | 220 | 220 |
| R-squared | 0.589 | 0.242 | 0.264 | 0.050 | 0.199 | 0.393 | 0.222 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | | | | | |
| Oil price shock × Oil Province | -0.068* | 0.243*** | -0.010 | 0.002 | 0.006 | -0.015 | -0.000 |
| | (0.040) | (0.077) | (0.009) | (0.002) | (0.006) | (0.059) | (0.003) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 442,479 | 336,429 | 441,627 | 441,627 | 233,134 | 318,689 | 328,181 |
| Number of regions | 247 | 220 | 247 | 247 | 220 | 220 | 220 |
| R-squared | 0.589 | 0.242 | 0.264 | 0.050 | 0.199 | 0.393 | 0.222 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | | | | | |
| Oil price shock × Oil Province | 0.121** | -0.274*** | -0.017 | -0.008* | -0.017* | -0.071 | -0.031* |
| | (0.047) | (0.075) | (0.011) | (0.004) | (0.010) | (0.060) | (0.016) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 442,479 | 336,429 | 441,627 | 441,627 | 233,134 | 318,689 | 328,181 |
| Number of regions | 247 | 220 | 247 | 247 | 220 | 220 | 220 |
| R-squared | 0.589 | 0.242 | 0.264 | 0.050 | 0.199 | 0.393 | 0.222 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

# Appendix

**Table A1: Unit root testing**

| Variable | Log Oil Prices (Period 1) | | Log Oil Prices (Period 2) | | Log Oil Prices (Period 3) | |
|---|---|---|---|---|---|---|
| | without trend | with trend | without trend | with trend | without trend | with trend |
| Dickey-Fuller | n.s. | n.s. | n.s. | n.s. | n.s. | n.s. |
| Dickey-Fuller-GLS | n.s. | n.s. | n.s. | n.s. | n.s. | n.s. |
| Philipps-Perron | n.s. | n.s. | n.s. | n.s. | n.s. | n.s. |

Note: All tests are based on 5-years average oil prices (log). Abbreviation: n.s., not significant at the 10% level.

**Table A2: Oil prices and GDP per capita at the province level**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Log(GDP per capita) | Log(GDP per capita) | Log(GDP per capita) | Log(GDP per capita) |
| log(oil price), t | 0.326*** | | | |
| | (0.014) | | | |
| log(oil price), t $_{o}ilprovince$ | 0.052** | | | |
| | (0.024) | | | |
| log(oil price), t-1 | | 0.339*** | | |
| | | (0.015) | | |
| log(oil price), t-1 $_{o}ilprovince$ | | 0.053** | | |
| | | (0.025) | | |
| log(oil price), 3-year average | | | 0.364*** | |
| | | | (0.016) | |
| log(oil price), 3-year average$_{o}ilprovince$ | | | 0.056** | |
| | | | (0.027) | |
| log(oil price), 5-year average | | | | 0.356*** |
| | | | | (0.016) |
| log(oil price), 5-year average$_{o}ilprovince$ | | | | 0.059** |
| | | | | (0.028) |
| Number of observations | 4830 | 4830 | 4830 | 4830 |
| Number of regions | 230 | 230 | 230 | 230 |
| R-squared | 0.731 | 0.731 | 0.731 | 0.731 |
| Region FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |

The dependent variable is the log(GDP per capita) for the period 1992-2012. Oil province is a dummy that takes a value of 1 if the region is producing oil. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A3: Further robustness for selection of countries**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Drop countries with only 1 oil region** | | | **Including non-oil producers** | | |
| | **Panel A. Period 1 (0-4 years old)** | | | | | |
| | **Education** | **Education** | **Education** | **Education** | **Education** | **Education** |
| | **Full sample** | **Male sample** | **Female sample** | **Full sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.198** | 0.204* | 0.204** | 0.132 | 0.169* | 0.131 |
| | (0.097) | (0.105) | (0.103) | (0.090) | (0.087) | (0.095) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 430,541 | 99,387 | 331,154 | 1,122,325 | 291,450 | 830,872 |
| Number of regions | 177 | 150 | 177 | 462 | 435 | 462 |
| R-squared | 0.401 | 0.375 | 0.407 | 0.435 | 0.393 | 0.451 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | | | | |
| Oil price shock × Oil Province | -0.088 | 0.034 | -0.106 | -0.058 | -0.029 | -0.064 |
| | (0.076) | (0.096) | (0.081) | (0.073) | (0.092) | (0.074) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 430,541 | 99,387 | 331,154 | 1,122,325 | 291,450 | 830,872 |
| Number of regions | 177 | 150 | 177 | 462 | 435 | 462 |
| R-squared | 0.401 | 0.375 | 0.407 | 0.435 | 0.393 | 0.451 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | | | | |
| Oil price shock × Oil Province | -0.414*** | -0.270* | -0.446*** | -0.278** | -0.289** | -0.288** |
| | (0.135) | (0.158) | (0.137) | (0.117) | (0.127) | (0.121) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 430,541 | 99,387 | 331,154 | 1,122,325 | 291,450 | 830,872 |
| Number of regions | 177 | 150 | 177 | 462 | 435 | 462 |
| R-squared | 0.401 | 0.375 | 0.407 | 0.435 | 0.393 | 0.451 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |

The dependent variable is the years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A4: Placebo test- Reshuffling of oil regions**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Education** | **Education** | **Education** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.007 | 0.011 | 0.005 |
| | (0.004) | (0.008) | (0.005) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 1 (5-9 years old)** | | |
| Oil price shock × Oil Province | 0.001 | -0.003 | 0.002 |
| | (0.004) | (0.008) | (0.005) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 1 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.001 | -0.006 | 0.001 |
| | (0.004) | (0.008) | (0.005) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the number of years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A5: Controlling for migration**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Education** | **Education** | **Education** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.189* | 0.202** | 0.188* |
| | (0.098) | (0.099) | (0.103) |
| Oil price shock × Oil Province × Non-migrants | 0.006 | 0.134 | -0.014 |
| | (0.046) | (0.205) | (0.050) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | |
| Oil price shock × Oil Province | -0.085 | 0.056 | -0.118 |
| | (0.074) | (0.094) | (0.078) |
| Oil price shock × Oil Province × Non-migrants | 0.005 | 0.148 | -0.012 |
| | (0.044) | (0.206) | (0.047) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.398*** | -0.239* | -0.436*** |
| | (0.124) | (0.139) | (0.126) |
| Oil price shock × Oil Province × Non-migrants | -0.010 | 0.109 | -0.028 |
| | (0.043) | (0.200) | (0.046) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the years of education. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Non-migrants is a dummy that takes the value of 1 if the respondant answered that he/she has been always residing in the place of residency. The dummy is also included on its own. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A6: Oil price shocks and population characteristics**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Total poulation** | **Total female** | **Total male** | **Urban population** | **Rural population** | **Migrants** |
| log(oil price), t-1 × oil province | -61.068 | -17.217 | -34.947 | 9.194 | -70.262 | -29.584 |
| | (80.128) | (77.162) | (34.166) | (87.859) | (65.787) | (85.518) |
| Number of observations | 802 | 640 | 584 | 802 | 802 | 802 |
| Number of regions | 217 | 189 | 184 | 217 | 217 | 217 |
| R-squared | 0.964 | 0.951 | 0.977 | 0.960 | 0.957 | 0.963 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Survey-year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Survey-month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-specific time trend | Yes | Yes | Yes | Yes | Yes | Yes |

Oil province is a dummy that takes a value of 1 if the region is producing oil. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A7: Controlling for first period shock**

| | (1) | (2) | (3) |
|---|---|---|---|
| | Period 3 (10-14 years old) | | |
| | Education | Education | Education |
| | Full Sample | Male sample | Female sample |
| Oil price shock × Oil Province | -0.386*** | -0.133 | -0.450*** |
| | (0.118) | (0.149) | (0.119) |
| Oil price shock × Oil Province in Period 1 | 0.016 | 0.141 | -0.018 |
| | (0.093) | (0.102) | (0.099) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580,478 | 137,924 | 442,554 |
| Number of regions | 247 | 247 | 220 |
| R-squared | 0.420 | 0.387 | 0.431 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the number of years of education. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A8: Robustness for wealth index - Adding more components**

| | (1) | (2) | (3) |
|---|---|---|---|
| | Panel A. Period 1 (0-4 years old) | | |
| | Wealth | Wealth | Wealth |
| | Full Sample | Male sample | Female sample |
| Oil price shock × Oil Province | 0.568** | 0.883** | 0.498* |
| | (0.278) | (0.382) | (0.294) |
| Controls | Yes | Yes | Yes |
| Number of observations | 406,691 | 113,467 | 293,224 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.437 | 0.432 | 0.441 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | Panel B. Period 2 (5-9 years old) | | |
| Oil price shock × Oil Province | -0.084 | 0.177 | -0.136 |
| | (0.248) | (0.390) | (0.308) |
| Controls | Yes | Yes | Yes |
| Number of observations | 406,691 | 113,467 | 293,224 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.437 | 0.432 | 0.441 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | Panel C. Period 3 (10-14 years old) | | |
| Oil price shock × Oil Province | -0.921*** | -0.991** | -0.917** |
| | (0.349) | (0.439) | (0.357) |
| Controls | Yes | Yes | Yes |
| Number of observations | 406,691 | 113,467 | 293,224 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.437 | 0.432 | 0.441 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the household wealth index constructed as explained in text. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table A9: Robustness for wealth index - Dropping components**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Wealth** | **Wealth** | **Wealth** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.648** | 0.869** | 0.587* |
| | (0.272) | (0.347) | (0.299) |
| Controls | Yes | Yes | Yes |
| Number of observations | 466,754 | 129,652 | 337,102 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.452 | 0.454 | 0.453 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | |
| Oil price shock × Oil Province | 0.026 | 0.177 | -0.007 |
| | (0.246) | (0.415) | (0.254) |
| Controls | Yes | Yes | Yes |
| Number of observations | 466,754 | 129,652 | 337,102 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.452 | 0.454 | 0.453 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.920*** | -0.871** | -0.948*** |
| | (0.318) | (0.428) | (0.336) |
| Controls | Yes | Yes | Yes |
| Number of observations | 466,754 | 129,652 | 337,102 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.452 | 0.454 | 0.453 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the household wealth index constructed as explained in text. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

# Appendix - Not for publication

**Table B1: Oil price shocks and educational quality**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A. Period 1 (0-4 years old)** | | |
| | **Educational quality** | **Educational quality** | **Educational quality** |
| | **Full Sample** | **Male sample** | **Female sample** |
| Oil price shock × Oil Province | 0.037** | 0.039** | 0.037* |
| | (0.018) | (0.019) | (0.019) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580837 | 138055 | 442782 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.413 | 0.357 | 0.427 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel B. Period 2 (5-9 years old)** | | |
| Oil price shock × Oil Province | -0.009 | 0.018 | -0.016 |
| | (0.013) | (0.016) | (0.014) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580837 | 138055 | 442782 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.413 | 0.357 | 0.427 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |
| | **Panel C. Period 3 (10-14 years old)** | | |
| Oil price shock × Oil Province | -0.070*** | -0.039 | -0.436*** |
| | (0.026) | (0.027) | (0.126) |
| Controls | Yes | Yes | Yes |
| Number of observations | 580837 | 138055 | 442782 |
| Number of regions | 247 | 220 | 247 |
| R-squared | 0.413 | 0.357 | 0.427 |
| Region FE | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes |

The dependent variable is the educational quality measured by the completion of educational stages, with higher values indicating completion of higher educational stages. Oil price shock is the ln-5 years average of oil price for period t multiplied by a dummy that take a value of 1 if the region is producing oil. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table B2: Disaggregating period 2 age bracket into 2 groups: ages (5-7 years old) and ages (8-9 years old)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Period 2 (5-9 years old) | | | | | |
| | Education | Education | Education | Education | Education | Education |
| | Full | Full | Male | Male | Female | Female |
| Oil price shock × Oil Province (5-7 years) | -0.145** | | -0.000 | | -0.181** | |
| | (0.068) | | (0.091) | | (0.071) | |
| Oil price shock × Oil Province (8-9 years) | | 0.025 | | 0.116 | | -0.001 |
| | | (0.073) | | (0.085) | | (0.076) |
| Controls | Yes | Yes | | | | Yes |
| Number of observations | 580,478 | 580,478 | 137,924 | 137,924 | 442,554 | 442,554 |
| Number of regions | 247 | 247 | 220 | 220 | 247 | 247 |
| R-squared | 0.420 | 0.420 | 0.387 | 0.387 | 0.431 | 0.431 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |

The dependent variable is the number of years of education. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table B3: Disaggregating period 3 age bracket into 2 groups: ages (10-12 years old) and ages (13-14 years old)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Period 3 (10-14 years old) | | | | | |
| | Education | Education | Education | Education | Education | Education |
| | Full | Full | Male | Male | Female | Female |
| Oil price shock × Oil Province (10-12 years) | -0.387*** | | -0.264* | | -0.414*** | |
| | (0.133) | | (0.138) | | (0.137) | |
| Oil price shock × Oil Province (13-14 years) | | -0.253*** | | -0.109 | | -0.293*** |
| | | (0.071) | | (0.098) | | (0.073) |
| Controls | Yes | Yes | | | | Yes |
| Number of observations | 580,478 | 580,478 | 137,924 | 137,924 | 442,554 | 442,554 |
| Number of regions | 247 | 247 | 220 | 220 | 247 | 247 |
| R-squared | 0.42 | 0.42 | 0.387 | 0.387 | 0.431 | 0.431 |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age-interval FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region specific-time trend | Yes | Yes | Yes | Yes | Yes | Yes |

The dependent variable is the number of years of education. Controls include year of survey, month of survey, religion, urban residency, and sex of household head. For the full sample, we control for gender. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.

**Table B4: Oil price shocks and number of schools**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Number of schools** | **Number of schools** | **Number of schools** |
| log(oil price), t-1 | 55.415 | | |
| | (33.931) | | |
| log(oil price), t-1 × oil province | -4.468 | | |
| | (18.384) | | |
| log(oil price), 3-year average | | 66.105* | |
| | | (39.875) | |
| log(oil price), 3-year average × oil province | | -3.479 | |
| | | (17.637) | |
| log(oil price), 5-year average | | | 51.333 |
| | | | (31.481) |
| log(oil price), 5-year average × oil province | | | -5.650 |
| | | | (18.633) |
| Number of observations | 628 | 628 | 628 |
| Number of regions | 183 | 183 | 183 |
| R-squared | 0.081 | 0.081 | 0.081 |
| Region FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

The dependent variable is the number of schools covering the years from 2002-2015. Oil province is a dummy that takes a value of 1 if the region is producing oil. The method of estimation is ordinary least squares with Huber-robust standard errors (reported in parentheses) clustered at the region level. Significantly different from zero at *10% significance, **5% significance level, ***1% significance level.