When Does Oil Harm Child Mortality?

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Supplemental Material

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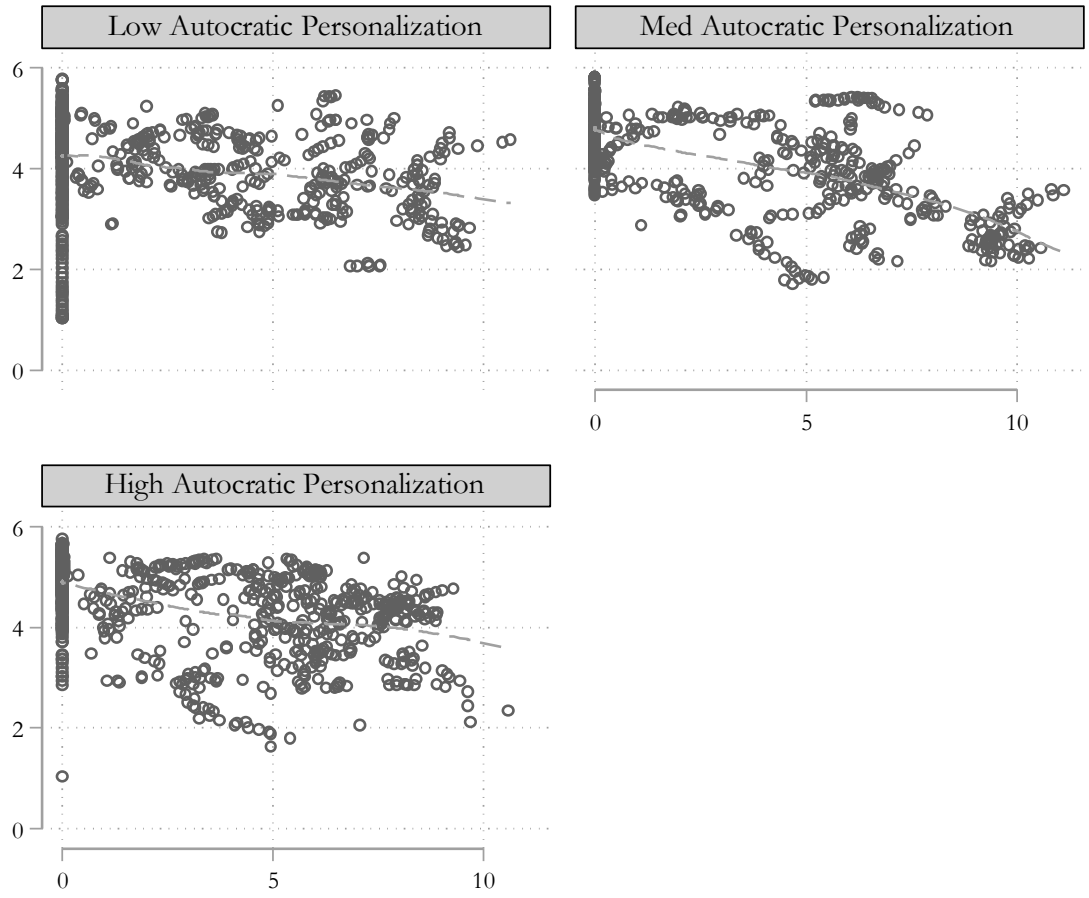
1. Examination of non-linear conditional marginal effects

We apply Hainmueller, Mummolo, and Xu's (2019) approach to test for non-linearity in our estimate of the conditional marginal effect of *oil income* on *child mortality*. We first follow their diagnostic approach and examine the scatterplot between the key independent variable *oil income* and the dependent variable *(log) child mortality* in three sub-samples from the main model estimation sample. These sub-samples correspond to the lowest, middle, and highest 1/3 of observed values of the moderating variable *autocratic personalization*. Each graph plots a lowess fit line.

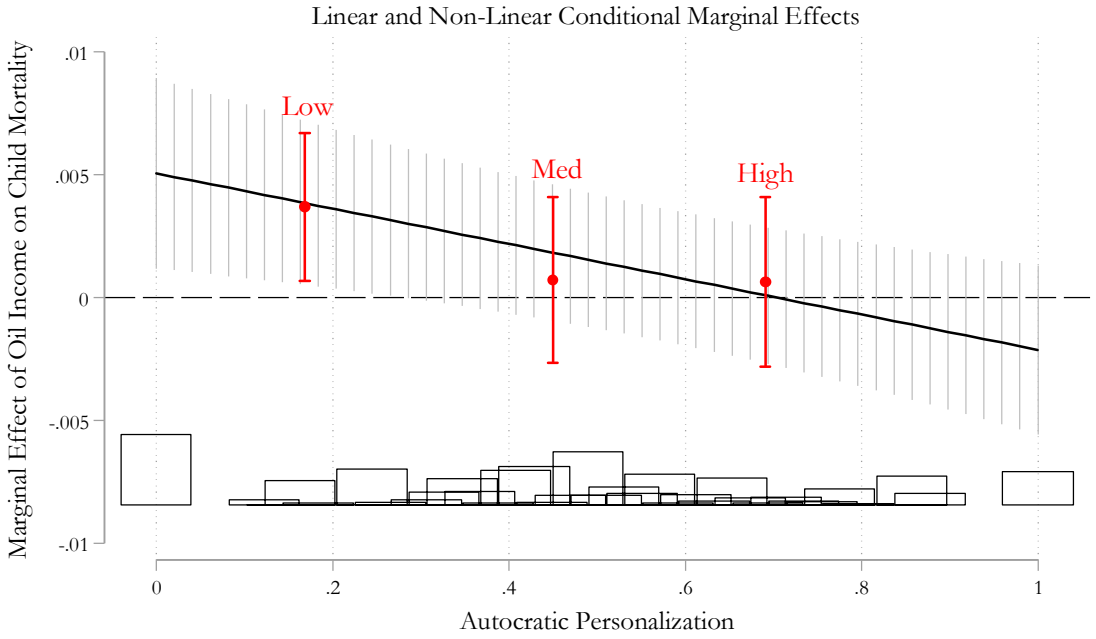
Figure 1: Diagnostic Scatterplots of *Oil Income* and *Child Mortality*, by levels of *Autocratic Personalization*



Visually inspecting the panels suggests that a linear model is appropriate in each sub-sample, however the presence of fixed effects in the data means that we also supplement this with the authors' binning estimator which breaks the continuous moderating variable into bins, representing the terciles used to construct the sub-samples in Figure 1. Figure 2 reports the results of this binning estimator (represented as the whisker plots in red), along with the estimated linear interaction effect assumed to change at a constant rate with the moderating variable *autocratic personalization* (i.e., the marginal effect

we present in Figure 3 in the main manuscript), represented as the solid black line with the light gray confidence interval.

Figure 2: Linear and Non-Linear Conditional Marginal Effects Compared



Linear marginal effect plotted in black; Non-linear marginal effect calculated from Hainmueller, Mummolo, and Xu's (2019) binning estimator plotted in red.

The results from the binning estimator support our assumption of a linear interaction effect. Each estimate falls close to the estimate linear conditional marginal effect. Importantly, the substantive conclusions we draw from this exercise are identical to those we discuss in the main manuscript. Specifically, *oil income* has a positive and significant effect on *child mortality* when autocrats have low levels of *autocratic personalization*, or short time horizons.

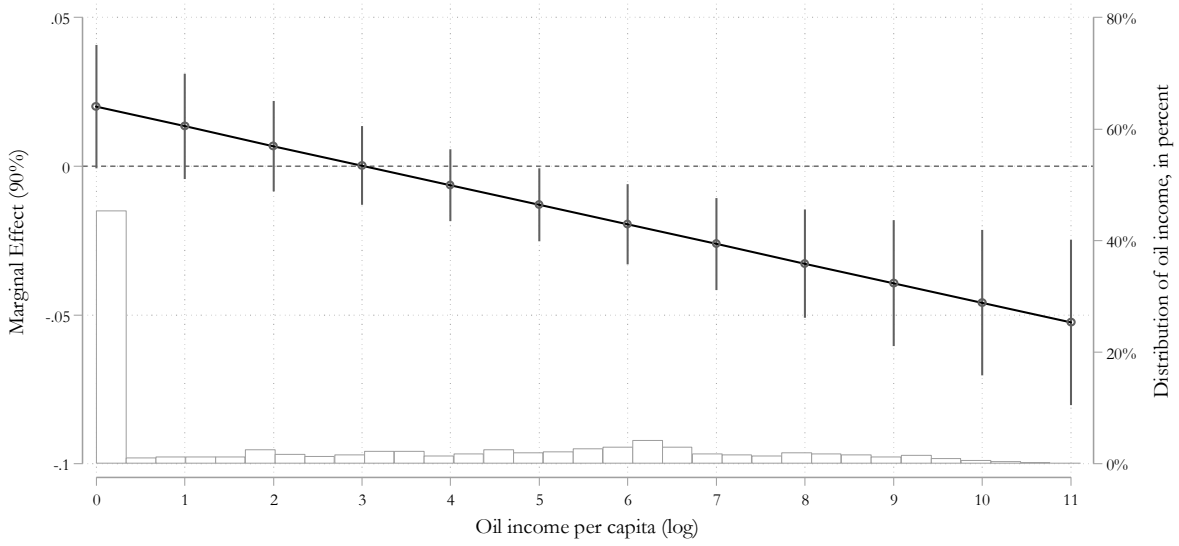
2. Symmetrical interpretation of interaction effect *autocratic personalization (x) oil income*

Figure 3 examines our key interaction effect *autocratic personalization (x) oil income* symmetrically; that is, it examines the marginal effect of *autocratic personalization* on *(log) child mortality* across the range of observed values of *oil income*.

The coefficient plot in Figure 2 in the main manuscript suggests that the effect of *autocratic personalization* is not statistically significant when *oil income* is zero. This is reflected on the left side of the marginal effect plot below, where the confidence intervals of the linear marginal effect includes zero. We interpret this as demonstrating that, for non-oil producers and even moderate-sized oil producers, higher levels of *autocratic personalization* have no impact on the observed rates of child mortality. In other words, for this large share of autocracies, tilting the balance of power toward the leader is unlikely to improve rates of child mortality, while increasing the likelihood of other harmful outcomes of autocratic personalization, such as Frantz et al.'s (2020) demonstration that such regimes are more likely to use repression.

However, at higher levels of *oil income*, the effect of *autocratic personalization* becomes negative and statistically significant. One interpretation is that amongst large oil producers, an increase in leader's security is associated with an improvement in rates of child mortality. Nevertheless, drawing firm conclusions is complicated given the relative paucity of observations at these levels of *oil income*.

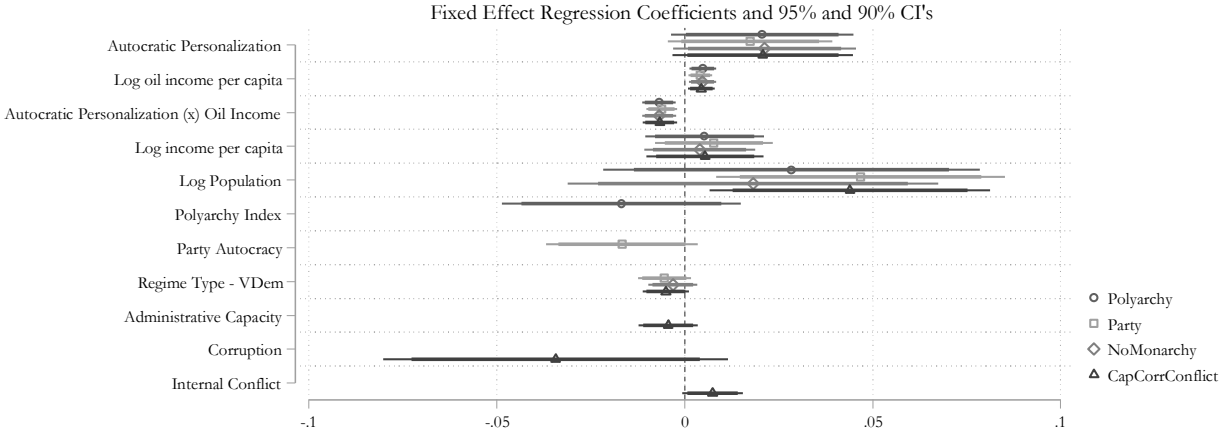
Figure 3: Conditional Marginal Effect of *Autocratic Personalization* on *Child Mortality*, across levels of *Oil Income*



3. Robustness of main model to specification changes

We evaluate the robustness of our main result to four separate model specification changes. First, we replace the ordinal measure of regime type with VDem's *polyarchy index* (Coppedge et al. 2019). Second, we include a measure of *party-based autocracies* (data from Geddes et al. 2014). Third, we exclude monarchies from the sample, again relying on Geddes et al. (2014). Fourth, we include additional measures of *administrative capacity* and *corruption,* from VDem (Coppedge et al. 2019), and a binary measure of whether there was internal or internationalized internal conflict in the year (Themnér and Wallensteen 2013). Our results are robust to each of these model specification changes. Figure 5 presents the estimated regression coefficients from these models, while the individual panels in Figure 6 presents plots of the estimated linear marginal effects of *oil income* for each specification.

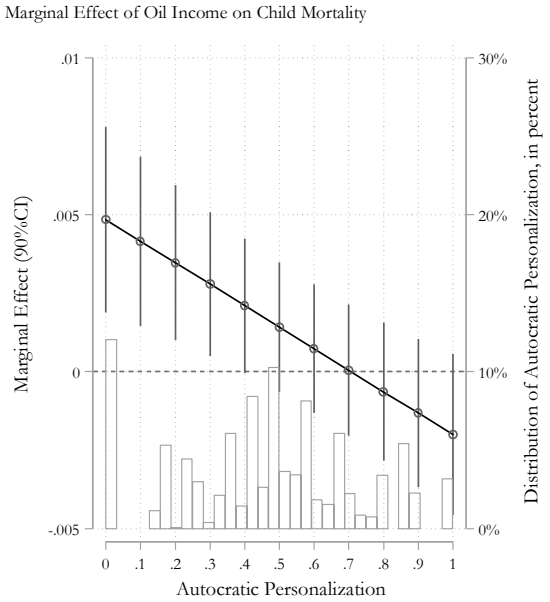
Figure 5: Fixed Effect Regression Coefficients Evaluating Model Robustness to Specification Changes



DV is (log) Child Mortality. Sample includes all autocratic regime years 1980-2010.
All IV's lagged one year. Lagged DV, constant, year, and country fixed effects included
but not reported. N/clusters = 2,135/103 for models Party and Capacity/Corruption/Conflict;
2,021/99 for model Polyarchy; 1,903/103 for model NoMonarchy. Standard errors clustered on country.
See text for full sample, variable, and model description.

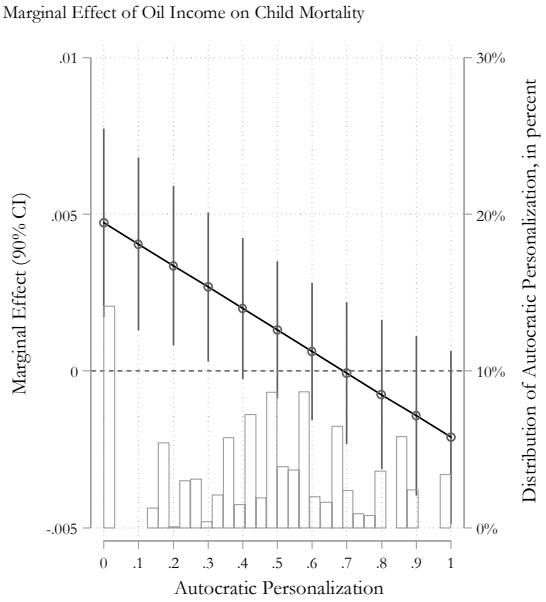
Figure 6: Linear Conditional Marginal Effects from Fixed Effect Regressions Reported in Figure 5

a) Uses VDem *polyarchy index*



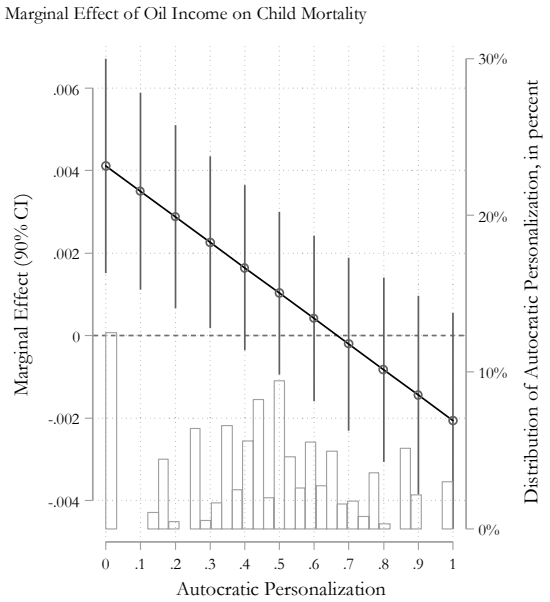
Regime type measured as VDem polyarchy

c) Excludes monarchies from sample



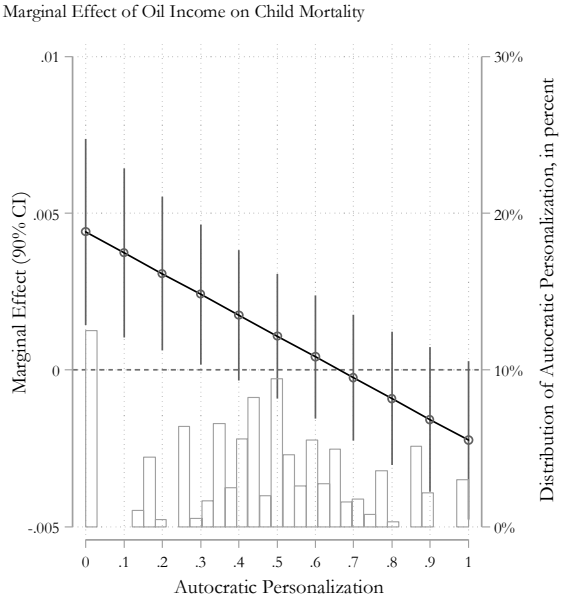
Drop Monarchies

b) Controls for party-based autocracies



Control for Party Autocracies

d) Controls for administrative capacity, corruption, and conflict

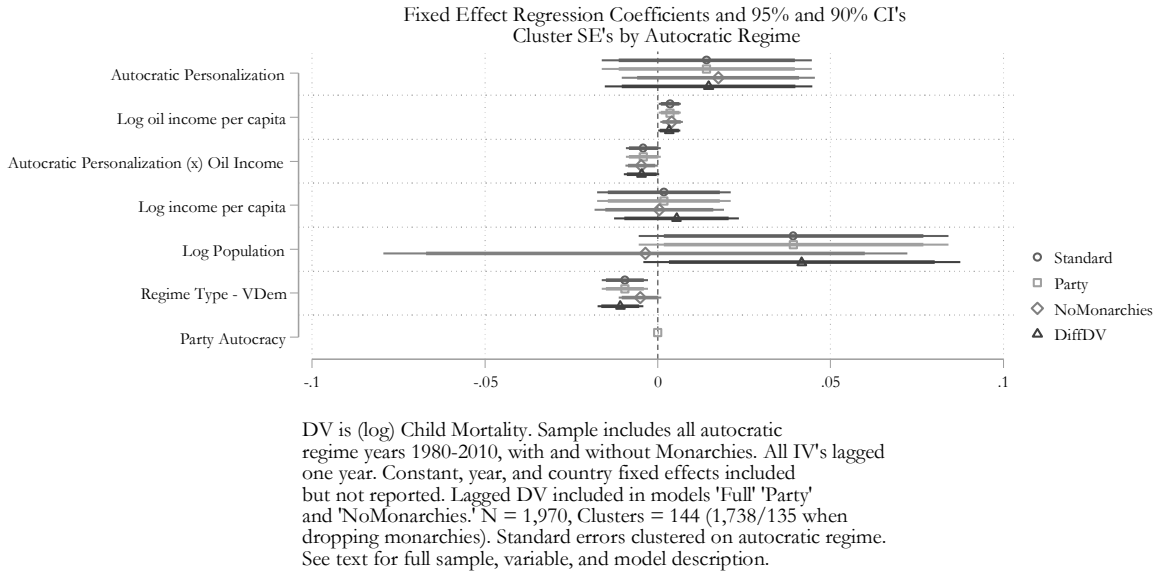


Control for Corruption, Administrative Capacity, & Conflict

4. Robustness to clustering standard errors on autocratic regimes

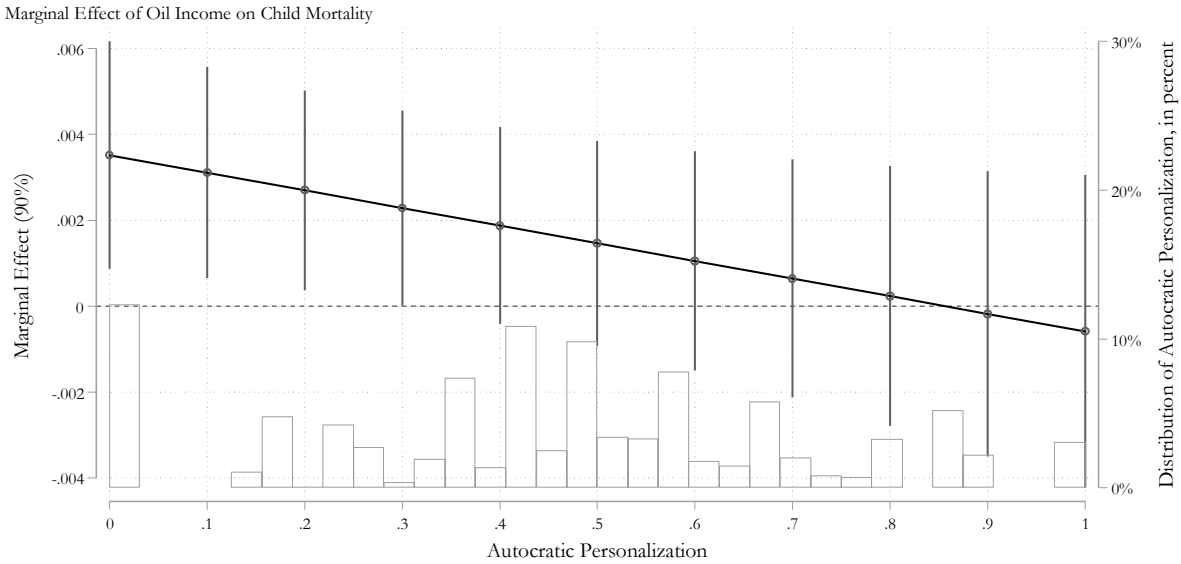
Geddes et al.'s (2018) measure of autocratic personalization varies by year and autocratic regime. Our results are robust to clustering standard errors by autocratic regime, instead of country as reported in the main manuscript. Figure 7 reports the coefficient plots for 4 separate models: the standard model presented in the main text, and alternatives controlling for party autocracies, excluding monarchies, and calculating the dependent variable as a one year change in rates of child mortality (see item #7 below). The results are consistent across all model permutations. For the sake of clarity, we limit our presentation of marginal effects (Figure 8) to those based on the standard model only.

Figure 7: Fixed Effect Regression Coefficients, Clustering on Autocratic Regime



DV is (log) Child Mortality. Sample includes all autocratic regime years 1980-2010, with and without Monarchies. All IV's lagged one year. Constant, year, and country fixed effects included but not reported. Lagged DV included in models 'Full' 'Party' and 'NoMonarchies.' N = 1,970, Clusters = 144 (1,738/135 when dropping monarchies). Standard errors clustered on autocratic regime. See text for full sample, variable, and model description.

Figure 8: Linear Conditional Marginal Effects, Clustering by Autocratic Regime

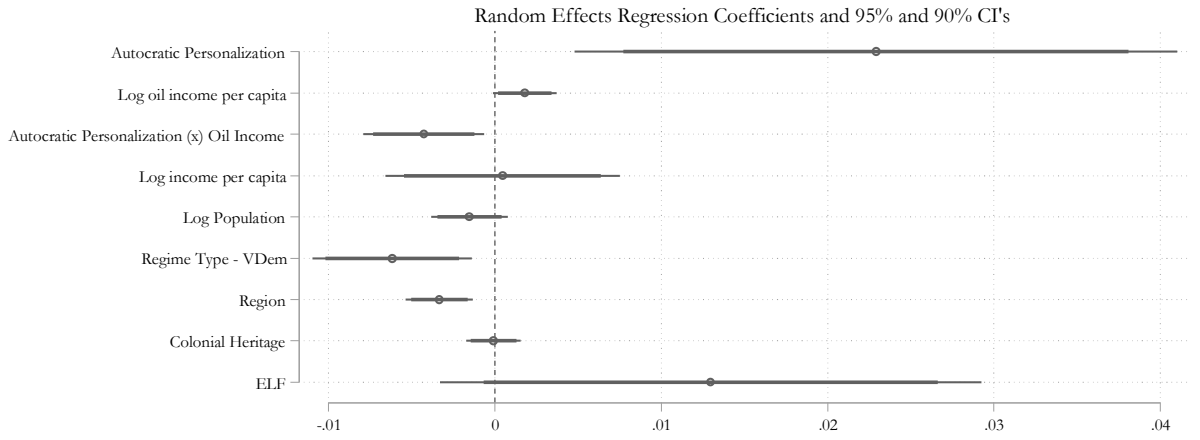


5. Assessing the impact of the sluggish moderating variable *Autocratic Personalization*

Our measure of time horizons (*autocratic personalization*) varies by country and year, though changes within panels relatively slowly. The inclusion of fixed effects in our preferred specification may absorb the impact of this slowly changing variable. To assess the impact of this measure and our modeling choices, we conduct two additional robustness analyses.

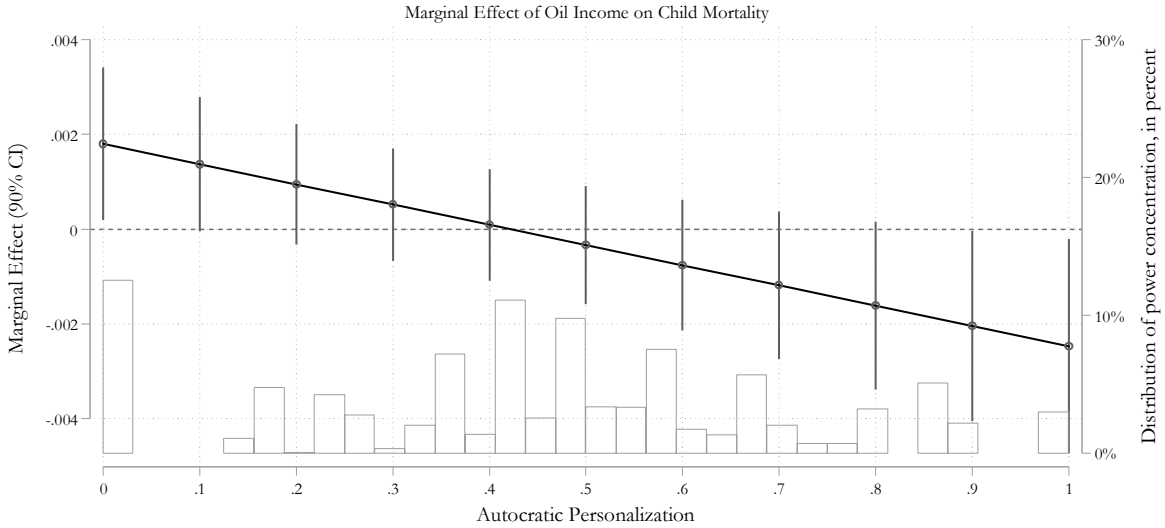
First, we re-estimate our main specification using random effects, but adding controls for three separate fixed factors: colonial heritage (Hadenius and Toerell 2007), region, and a measure of ethno-linguistic fractionalization, or ELF (Alesina et al. 2003). Figure 9 presents the random effects coefficient estimates, and Figure 10 reproduces the relevant conditional marginal effect. The central results are consistent with those we report in the main manuscript; *oil income* has a positive and significant impact on *child mortality* when time horizons are short. The random effects coefficients do indicate that an increase in leaders' time horizons has a positive and significant impact on *child mortality*, though only when *oil income* takes a value zero. Overall, the results are consistent with our argument that oil harms population health when time horizons are short, and that an increase in time horizons can offset the negative effect.

Figure 9: Random Effects Regression Coefficients



DV is (log) Child Mortality. Sample includes all autocratic regime years 1980-2010.
Model replaces country-specific fixed effects with random effects and a measure of region, colonial heritage, and ELF. Lagged DV, constant, and year effects included in each model but not reported.
N/clusters = 2,024/101. Standard errors clustered on country. See text for full sample, variable, and description.

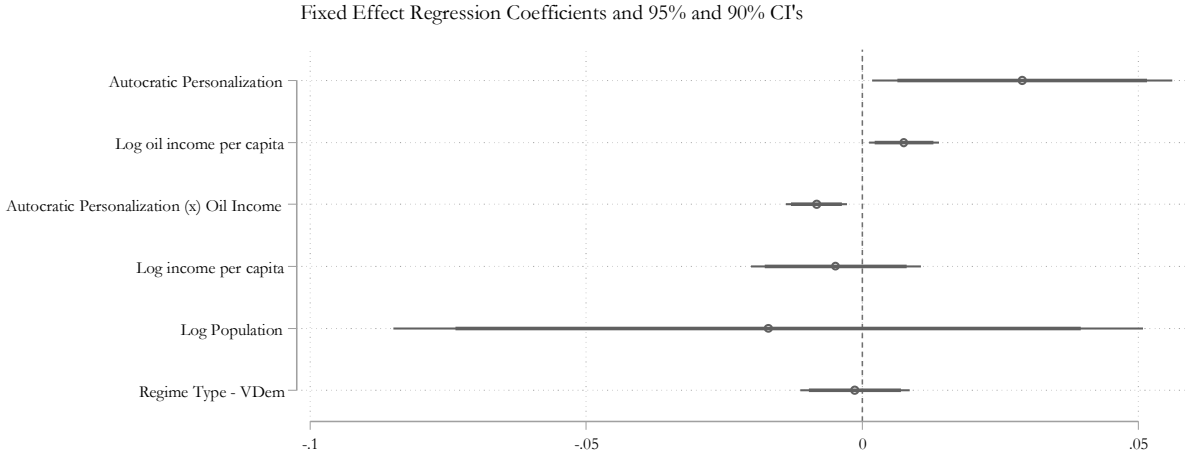
Figure 10: Linear Conditional Marginal Effect of Oil Income, based on estimates from Random Effects regression model



Random Effects model, controlling for region, colonial heritage, and ELF

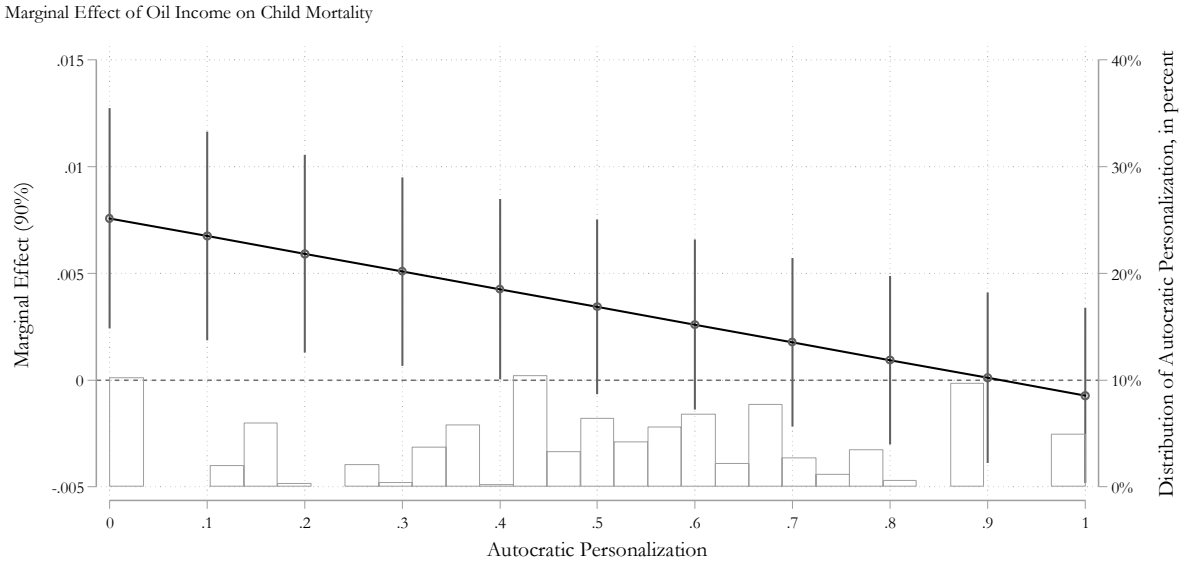
Second, we re-estimate the fixed effects regression, while limiting the sample to the panels where there is considerable within-panel variation in the sluggish variable *autocratic personalization*. We define inclusion in this sample as all panels where the standard deviation of *autocratic personalization* falls above the median. In essence, this limits the sample to the half of all observations where the variable is least sluggish. Figure 11 reports the coefficient estimates of this model, and Figure 12 reports the key conditional marginal effect. Our main conclusions remain unaltered by this specification change.

Figure 11: Coefficient Estimates for High Variability in *Autocratic Personalization* Sample



DV is (log) Child Mortality. Sample is autocratic regimes (1980-2010) whose within-panel variability in terms of autocratic personalization is in the top half of the sample distribution. All IV's lagged one year. Lagged DV, constant, year, and country fixed effects included but not reported. N = 1,066, Clusters = 50. Standard errors clustered on country. See text for full sample, variable, and model description.

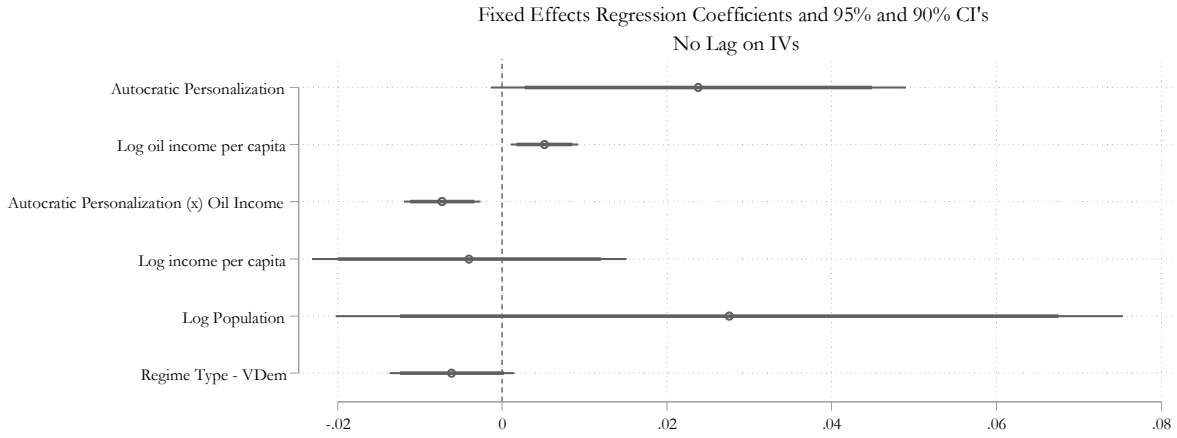
Figure 12: Linear Conditional Marginal Effect of *Oil Income*, High Variability in *Autocratic Personalization* Sample



6. Robustness of main model to dropping temporal lag on independent variables

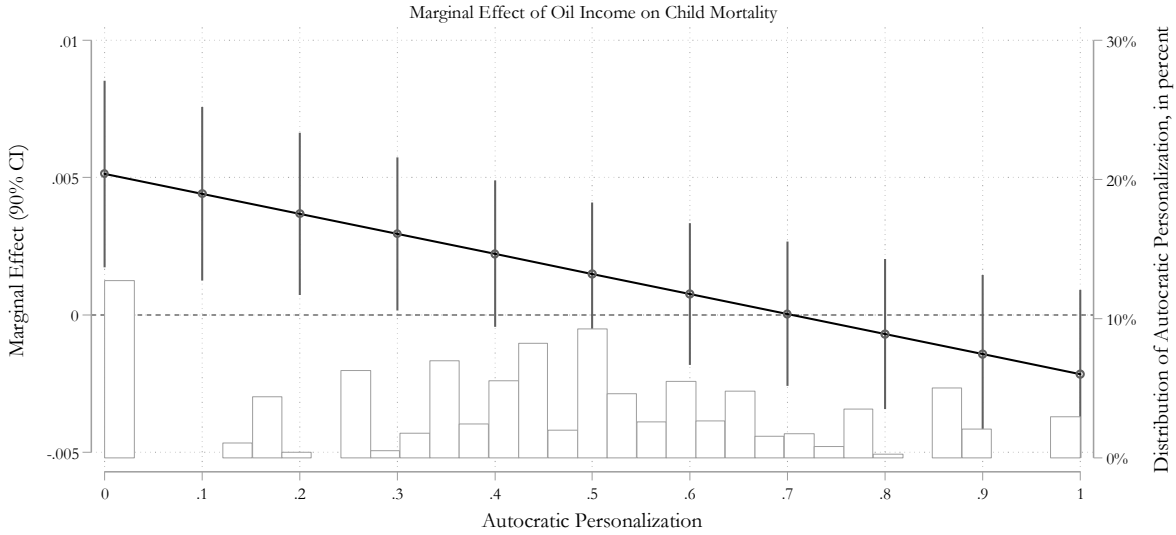
Bellemare, Masaki, and Pepinsky (2017) critique the use of lagged explanatory variables in response to concerns about endogeneity. Our inclusion of lagged independent variables is principally to ensure that such factors are measured temporally prior to observation of the dependent variable, as opposed to the "lag identification" approach these authors critique (949). Nevertheless, we re-estimate our main specification without the one-year lag on the independent variables (though we continue to include the lagged dependent variable to address autocorrelation). Figures 13 and 14 present the regression coefficients and marginal effects plot from this exercise. The results are substantively identical to those we report in the main manuscript.

Figure 13: Regression Coefficients without one-year lag structure



DV is (log) Child Mortality. Sample includes all autocratic regime years 1980-2010. Model drops the one-year lag on all independent variables reported in main text. Lagged DV, constant, and year effects included in each model but not reported. N/clusters = 2,050/101. Standard errors clustered on country. See text for full sample, variable, and description.

Figure 14: Linear Conditional Marginal Effect of *Oil Income*, Based on Model without the one-year lag structure on explanatory variables



Fixed Effects Model, no lag on IVs

7. Robustness to calculating dependent variable as one year change in (log) child mortality

Our results are robust to differencing the dependent variable so that each year's value reflects the change in (log) child mortality from the prior calendar year. We estimate these differenced DV models with and without a lagged dependent variable, and the results remain consistent. Figure 15 reports the coefficient estimates from these two fixed effect regressions. Figure 16 present the linear conditional marginal effects plot, based on the regression model without the lagged dependent variable.

Figure 15: Regression Coefficients from one-year Differenced Dependent Variable

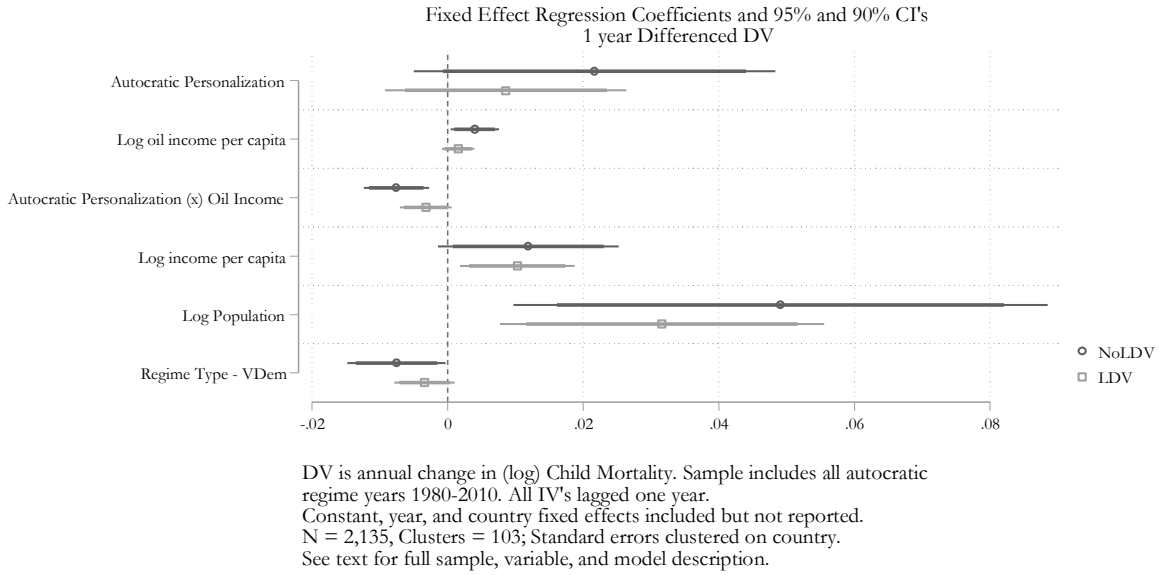
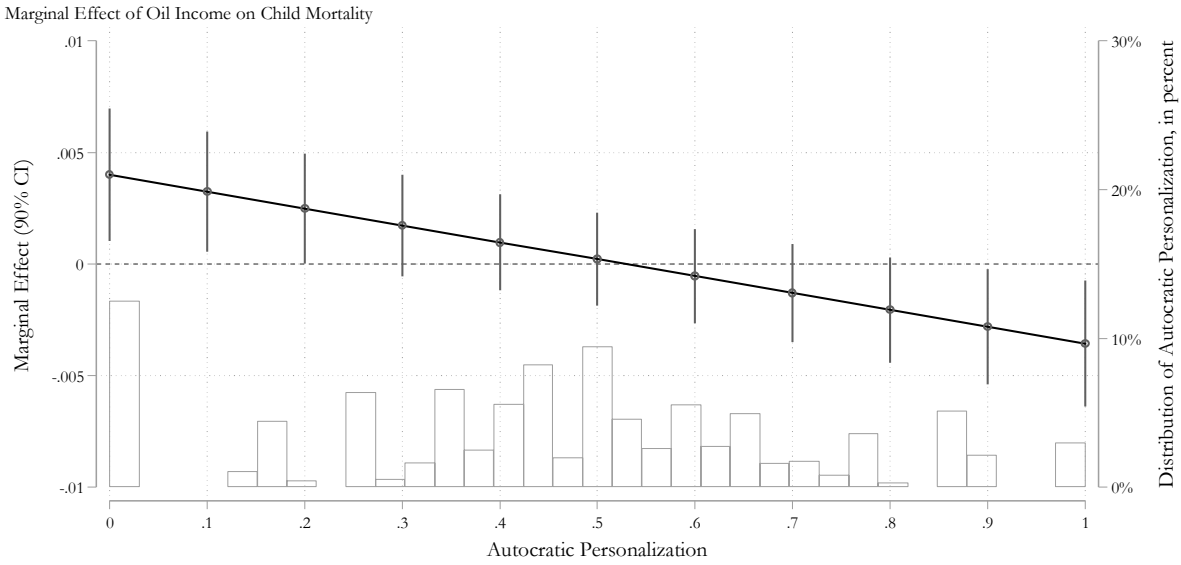


Figure 16: Linear Conditional Marginal Effect of *Oil Income* on one-year differenced dependent variable



8. Robustness to including measure of *Autocratic Legislature* and discussion

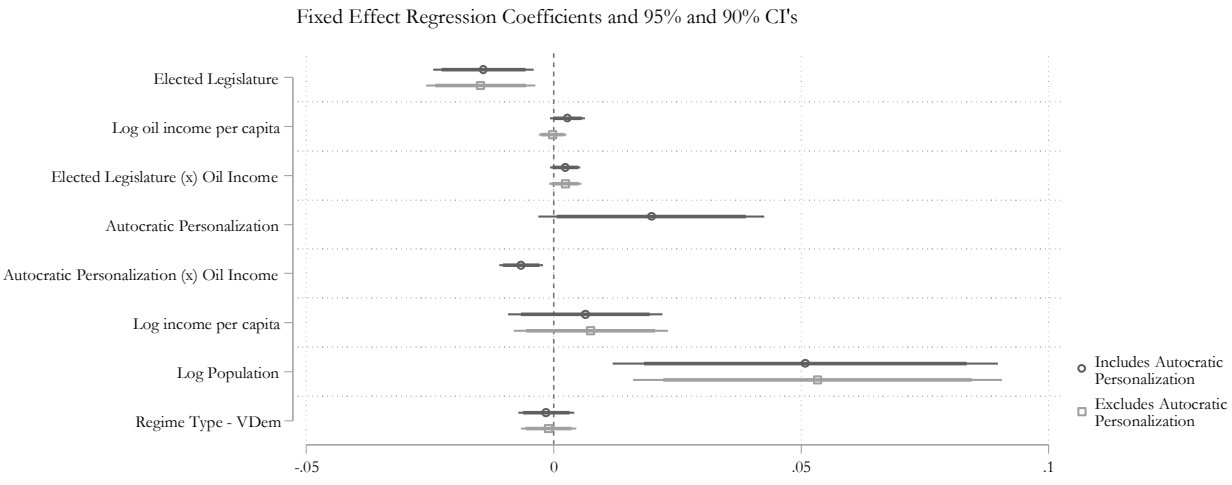
The main manuscript describes our approach to measuring time horizons with the variable *autocratic personalization*, including a discussion of how this differs from alternative traits of authoritarian rule, including whether formal political institutions like parties and legislatures exist. The presence of such institutions can solve several dilemmas of authoritarian rule, especially commitment problems between dictators and regime insiders. Studies regularly demonstrate that such institutions are associated with more stable autocratic rule and longer tenures (e.g. Boix and Svolik 2013; Magaloni 2008).

We argue and demonstrate in the paper that *autocratic personalization* measures a conceptually and empirically distinct aspect of authoritarian politics, and that a high degree of *autocratic personalization* can lengthen leaders' time horizons regardless of the presence of formal institutions such as legislatures. This fact is at the core of our theoretical and empirical argument that oil income is harmful for child mortality only when leaders' power vis-à-vis other regime insiders is low.

In this section, we focus on these formal institutions and assess whether autocratic legislatures similarly condition the impact of oil income on child mortality rates. If autocratic legislatures' impact on regime stability functions in the same way, we would expect the impact of oil income on child mortality to be particularly harmful when these institutions were absent. That is, the absence of such power-sharing institutions would reduce regime stability, encourage the use of oil rents to build leaders' support coalitions, and consequently divert needed funds from improving public health.

We evaluate this argument by creating a dummy variable distinguishing autocracies with competitively elected legislatures (1) from those without (0), drawing on data from Cheibub, Gandhi, and Vreeland (2008). We then re-estimate our main regression model while including this variable *elected legislature* and the interaction term *elected legislature (x) oil income*. For further analysis, we also estimate the model while excluding the variables *autocratic personalization* and the interactive term *autocratic personalization (x) oil income*. Figure 17 reports the estimated regression coefficients for each.

Figure 17



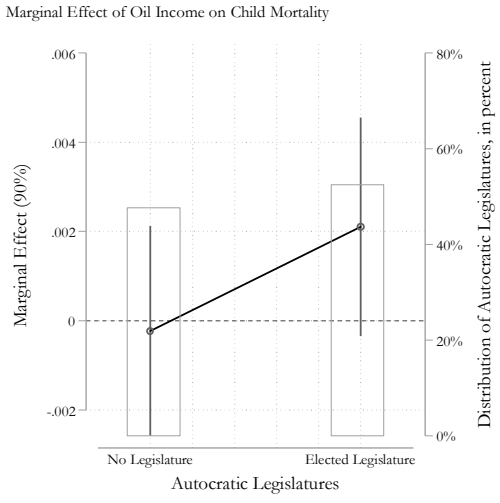
DV is (log) Child Mortality. Sample includes all autocratic regime years 1980-2009. All IV's lagged one year. Lagged DV, constant, year, and country fixed effects included but not reported. N = 2,024, Clusters = 101. Standard errors clustered on country. See text for full sample, variable, and model description.

In both models, *elected legislature* has a negative and statistically significant coefficient, but the coefficient for the interactive term is not distinguishable from zero. We calculate conditional marginal effects to provide further substantive discussion of the results.

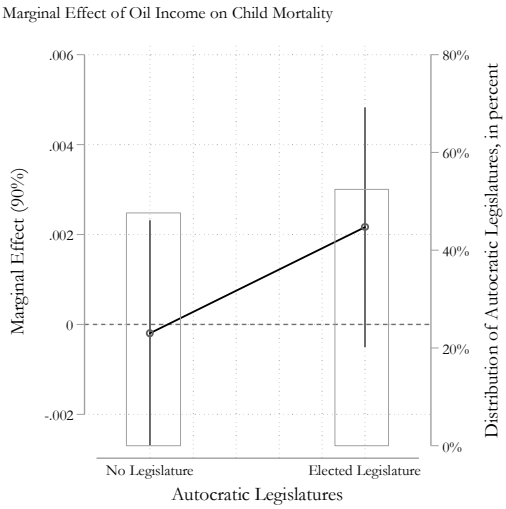
First, Figure 18 (panels A and B) report the marginal effect of *oil income* on child mortality rates for autocracies with and without a competitively elected legislature. Panel A corresponds to the regression model that retains our original theoretical variables of interest *autocratic personalization* and the interactive term, while Panel B corresponds to the model omitting these variables. Across both, the results suggest that the presence of elected legislatures in autocracies does not condition the impact of oil rents on child mortality. The estimated marginal effects have overlapping confidence intervals, such that they are not distinguishable from one another. Though autocratic legislatures may solve other important dilemmas of authoritarian rule more broadly, their presence has no impact on when oil income harms child mortality rates.

Figure 18: Marginal Effects Plots

A) Model including *autocratic personalization* variables



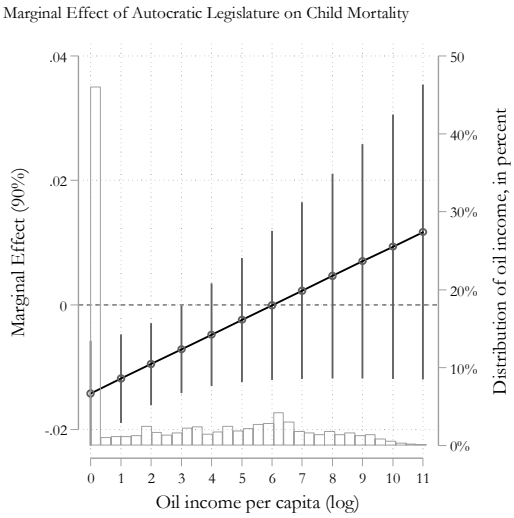
B) Model excluding *autocratic personalization* variables



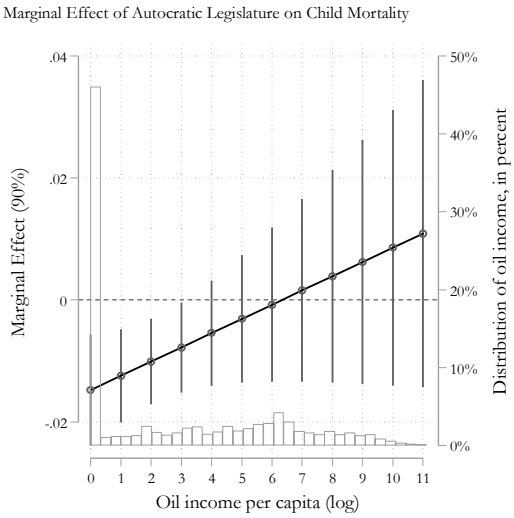
We also estimate symmetrical marginal effects; that is, the marginal effect of *elected legislatures* on child mortality, across observed levels of *oil income*, with all other model covariates held to mean values. The negative and statistically significant regression coefficient for *elected legislatures* in Figure 17 suggests that such institutions reduce rates of child mortality in non-oil producing countries (that is, when *oil income* is 0). Plotting the marginal effects (Figure 19) further clarifies this impact.

Figure 19: Marginal Effects Plots

A) Model including *autocratic personalization*



B) Model excluding *autocratic personalization*



Each plot shows that autocracies with competitively elected legislatures outperform those without when it comes to reducing rates of child mortality – the marginal effect on the left side of the plots is negative and statistically significant. However, this effect is strongest in non-oil producers and rapidly dissipates, until the beneficial effects of this institution evaporate at relatively modest levels of oil production. One interpretation is that the unique politics of oil producing autocracies render the effects of these otherwise beneficial institutions moot.

Fully understanding the effect of formal institutions in oil producing autocracies is beyond the scope of this paper, although we offer a preliminary observation as to why legislatures only reduce child mortality in the absence of oil income. For instance, Boix and Svolik (2013) argue that formal legislatures solve the monitoring problem in dictatorships, where regime insiders are never quite sure if the dictator is withholding information or lying about the size of resources his support coalition can expect to share in. However, one defining feature of the oil industry is its' relative secrecy (Ross 2012, 59-62). It is possible that the information asymmetries generated are simply too large to be overcome through formal institutions, and thus a principle mechanism linking the presence of elected legislatures to a host of superior outcomes in autocracies breaks down in large oil producers. We leave further exploration of this finding to future research.

9. Descriptive Statistics

Our universe of cases is all autocracies from the years 1980-2010. We define our specific sample using data on regime type from Geddes, Wright, and Frantz (2018). These authors report 2,290 authoritarian country-years during this period, representing 109 different countries. Missing data for other variables limits our main regression to a sample size of 2,135 authoritarian country-years, representing 103 different countries.

The descriptive statistics table (Figure 20) below reports the number of observations, along with mean values, standard deviations, and minimum and maximum values for all variables that appear in our main model and the supplemental materials. The final column reports the share of total possible authoritarian country-years (i.e. 2,290) that contain missing values for the specific variable, and are thus excluded from the analysis. The main manuscript and the prior sections of this supplemental materials file describe all sources for the variables.

Figure 20 – Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max	Percent Missing from Total Eligible Sample
Child Mortality (log)	2,174	4.29	0.91	1.03	5.82	5.07
Child Mortality (log), annual change	2,166	-0.03	0.03	-0.29	0.44	5.41
Autocratic Personalization	2,290	0.46	0.28	0.00	1.00	0
Oil Income per capita (log)	2,228	2.86	3.20	0	11.10	2.71
Income per capita (log)	2,128	7.43	1.29	4.87	11.65	7.07
Population (log)	2,227	16.20	1.35	13.32	21.01	2.75
Regime Type (Ordinal)	2,255	0.57	0.63	0	3	1.53
Year	2,290	-	-	1980	2010	0
Polyarchy Index	2,124	0.25	0.15	0.02	0.82	7.25
Party Autocracy	2,290	0.48	0.50	0.00	1.00	0
Monarchy	2,290	0.10	0.30	0.00	1.00	0
Rigorous and Impartial Public Administration	2,269	-0.54	1.09	-3.22	2.84	0.92
Corruption Index	2,269	0.64	0.24	0.02	0.97	0.92
Internal Conflict Dummy	2,290	0.28	0.45	0	1	0
Colonial Legacy	2,234	4.04	2.52	0	8	2.45
Ethnic Fractionalization	2,193	0.54	0.25	0.00	0.93	4.24
Autocratic Legislature Dummy	2,143	0.49	0.50	0.00	1.00	6.42

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