# Online Appendix

How Budgets Shape Power Sharing in Autocracies

## Contents

# A Data and Sample

## A.1 Budget Data

**Table A.1:** Correlation across budget series (logged).

| | PWT | CNTS | ICTD |
|---|---|---|---|
| PWT | 1 | 0.913 | 0.949 |
| CNTS | 0.913 | 1 | 0.949 |
| ICTD | 0.949 | 0.949 | 1 |

PWT: Penn World Tables, Govt. Consumption

CNTS: Cross-National Time-Series, Govt. Revenue

ICTD: Intl. Centre for Tax and Dev., Tax Revenue

## A.2 Sample

**Table A.2:** Missingness due to listwise deletion

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Americas | -0.09 | | | -0.13 |
| | (0.12) | | | (0.11) |
| Asia | -0.09 | | | -0.10 |
| | (0.06) | | | (0.08) |
| Europe | -0.07 | | | -0.12 |
| | (0.11) | | | (0.12) |
| Year | | -0.00 | | -0.00 |
| | | (0.00) | | (0.00) |
| Polity | | | -0.01 | -0.01 |
| | | | (0.01) | (0.01) |
| EPR Groups | | | | -0.00 |
| | | | | (0.00) |
| Oil Producer | | | | 0.03 |
| | | | | (0.07) |
| N | 3,168 | 3,168 | 3,168 | 3,168 |

We regress a dummy variable denoting that an administration-year observation is missing on regional dummies, politiy scores, the number of EPR groups, and whether the country is an oil producer. Standard errors are clustered on administration.

**Table A.3:** Summary statistics.

| Variable | N | Mean | SD | Min | q25 | q50 | q75 | Max |
|---|---|---|---|---|---|---|---|---|
| B | 2807 | 22.22 | 1.74 | 16.75 | 21.02 | 22.03 | 23.46 | 28.33 |
| $C_t = 0; a_t = 0$ | 2807 | 0.58 | 0.49 | 0 | 0 | 1 | 1 | 1 |
| $C_t = 0; a_t = i$ | 2807 | 0.01 | 0.1 | 0 | 0 | 0 | 0 | 1 |
| $C_t = 1; a_t = 0$ | 2807 | 0.4 | 0.49 | 0 | 0 | 0 | 1 | 1 |
| $C_t = 1; a_t = e$ | 2807 | 0.01 | 0.09 | 0 | 0 | 0 | 0 | 1 |
| Irregular Transition | 2782 | 0.04 | 0.21 | 0 | 0 | 0 | 0 | 1 |
| Leader Death | 2782 | 0.01 | 0.12 | 0 | 0 | 0 | 0 | 1 |
| First Year in Office | 2807 | 1976.47 | 13.49 | 1960 | 1964 | 1975 | 1986 | 2012 |
| Military Pedigree | 2699 | 0.51 | 0.5 | 0 | 0 | 1 | 1 | 1 |
| EPR Groups | 2807 | 5.47 | 5.32 | 2 | 3 | 4 | 6 | 37 |
| Start Age | 2782 | 46.09 | 11.49 | 17 | 38 | 45 | 54 | 78 |
| Oil Producer | 2807 | 0.45 | 0.5 | 0 | 0 | 0 | 1 | 1 |

**Table A.4:** Unconstrained autocrats excluded due to EPR.

| | Country | No. Admin. Excluded | Average Population (mil.) |
|---|---|---|---|
| **Admin. Missing from EPR** | | | |
| 1 | Fiji | 5 | 0.8 |
| 2 | Comoros | 4 | 0.3 |
| 3 | Qatar | 3 | 0.3 |
| 4 | Suriname | 2 | 0.4 |
| 5 | Romania | 2 | 19.3 |
| 6 | Equatorial Guinea | 2 | 0.2 |
| 7 | Oman | 2 | 0.7 |
| 8 | Kosovo | 1 | NA |
| **Only 1 Group in EPR** | | | |
| 9 | Haiti | 9 | 5.7 |
| 10 | Burkina Faso | 8 | 7.2 |
| 11 | Dominican Republic | 5 | 4.0 |
| 12 | Swaziland | 4 | 0.7 |
| 13 | Republic of Korea | 4 | 29.7 |
| 14 | Portugal | 3 | 8.7 |
| 15 | Democratic People's Republic of Korea | 3 | NA |
| 16 | Lesotho | 2 | 1.6 |
| 17 | Tunisia | 2 | 5.8 |
| 18 | United Arab Emirates | 2 | 1.3 |
| 19 | Somalia | 1 | NA |
| **Totals** | | | |
| | Total Excluded | 64 | 86.6 |
| | Total Included | 360 | 2,355.0 |

**Table A.5:** Correlates of exclusion due to EPR.

| | (1) | (2) | (3) |
|---|---|---|---|
| First Year in Office | -0.00 | -0.00 | -0.00 |
| | (0.00) | (0.00) | (0.00) |
| Polity | 0.01 | 0.01 | 0.01 |
| | (0.01) | (0.01) | (0.01) |
| Oil Producer | 0.17 | 0.16 | 0.16 |
| | (0.03) | (0.03) | (0.04) |
| Start Age | | 0.00 | 0.00 |
| | | (0.00) | (0.00) |
| Military Pedigree | | | 0.05 |
| | | | (0.04) |
| Administrations | 424 | 422 | 384 |

We create an indicator for whether an administration includes multiple politically relevant groups in the EPR. We then regress that indicator on other administration-specific covariates to assess how missingness in the EPR affects selection into our sample.

## A.3 Alternative Codings of Leader's Actions and States

**Baseline** For states, $C_l^t = 0$ if and only if we observe that leader $l$'s country in year $t$ has a dominant group in government as recorded in the EPR data. For actions, $a_l^t = e$ if the previous year has an inclusive state ($C_l^{t-1} = 1$) and the number of groups in power decreases in year $t$. Likewise, $a_l^t = i$ if the previous year has an exclusive state ($C_l^{t-1} = 0$) and the number of groups in power increase in year $t$. In all other cases, $a_l^t = \varnothing$.

**Excluding Partial Purges** A ruling coalition starts as exclusive ($C_l^t = 0$) if it is initially dominated by a single group and inclusive otherwise. We then define inclusion ($a_l^t = i$) as adding another group as a junior or senior partner in government. This addition would change the subsequent state to inclusive ($C_l^t = 1$). If a coalition is in an inclusive state, the leader can exclude members by reducing the number of groups in government ($a_l^t = e$), changing the state in the next year to exclusive. While rare, adding groups from an already inclusive state or subtracting groups from an exclusive state are considered as maintenance of the status-quo ($a_l^t = \varnothing$).

**Dominant** For $t = 1$, $C_l^1 = 0$ if and only if we observe that leader $l$'s country in year $t$ has a dominant group government as recorded in the EPR data. A group is dominant if it holds the elite positions of government even though there may be token members from other groups that do not affect decision making. If there is no dominant group, then $C_l^1 = 1$. For $t > 1$, $a_l^t = \varnothing$ if there is no change in the country's dominant group status, i.e., there was a (no) dominant group in both $t$ and $t-1$. $a_l^t = e$ if there was a switch from no dominant group to a dominant group between $t$ and $t-1$. For inclusion, $a_l^t = i$ if there was a switch from dominant group to no dominant group between $t$ and $t-1$. The remaining states are coded following $C_l^{t+1} = \mathbf{I}(a_l^t, C_l^t)$.

Table A.14 shows how structural estimates differ using these different coding schemes.

## A.4 Comparing the EPR to other Datasets on Power Sharing

We compare our measures of power sharing from the EPR to datasets compiled by Arriola, Devaro and Meng (2021) and Francois, Rainer and Trebbi (2015).

First, Arriola, Devaro and Meng (2021) compile data for African countries from 1990–2016 on whether opposition politicians secure a cabinet post. Their focus is on election outcomes, so they only provide this measure for election years; they have, on average, just under four observations per country. Only 2 percent of the observations in our sample appear in this dataset; only 9 percent of the administrations in our sample have at least one observation in these data. In 83 percent of the cases in which Arriola, Devaro and Meng (2021) code opposition representation in the cabinet, the EPR agrees that multiple groups are represented in government. We show in Table A.6 that their measure positively correlates with the EPR's variables for whether multiple distinct groups are included in government, how many groups are included, and the absence of a dominant or monopoly group. We have very little statistical power (just 28 clusters), but the regression coefficient in column 3 is significant at the ten-percent level.

**Table A.6:** Associations between different power-sharing measures.

| EPR Variable: | Multiple Included | | Number Included | | No Dominant Group | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 𝟙(Opposition in Cabinet) | 0.22 | | 1.19 | | 0.10 | |
| (Arriola, Devaro and Meng 2021) | (0.18) | | (0.68) | | (0.16) | |
| Groups in "Top" Posts | | 0.08 | | 0.25 | | 0.07 |
| (Francois, Rainer and Trebbi 2015) | | (0.03) | | (0.12) | | (0.03) |
| Clusters | 28 | 57 | 28 | 57 | 28 | 57 |
| N | 56 | 484 | 56 | 484 | 56 | 484 |

Notes: We regress the EPR power-sharing measures (column) on the other measures from the literature (row). Standard errors clustered on administration. We drop Tanzania in even-numbered models, as the very high level of ethnic diversity recorded in Francois, Rainer and Trebbi (2015) does not cross the country's salient political cleavage. In 2000, Francois, Rainer and Trebbi (2015) record 13 different ethnic groups represented in top cabinet posts in Tanzania — the most of any country in their sample. By contrast, Arriola, Devaro and Meng (2021) code no opposition representation in the cabinet, which corroborates the EPR coding that "Mainland Africans" (through the CCM Party) were a politically dominant group.

Second, Francois, Rainer and Trebbi (2015) provide data on the ethnic affiliation of ministers in 15 African countries from independence through 2004. Only 19 percent of the observation in our sample appear in this dataset. In 76 percent of the cases in which they record multiple ethnic groups holding "top" cabinet posts (president, prime minister, defense, state, treasury, justice), the EPR agrees that multiple groups are represented in government.[1] We exclude Tanzania in this comparison, as the very high level of ethnic diversity recorded in Francois, Rainer and Trebbi (2015)

[1] Francois, Rainer and Trebbi (2015) use a more inclusive definition of top posts, which includes ministers whose portfolios relate to "economic" affairs. These ministers often lead more peripheral ministries (e.g., fisheries, forestry, foreign investment), so we exclude them from our coding of top posts.

does not cross the country's salient political cleavage. Unlike Francois, Rainer and Trebbi (2015), we are interested in the inclusion of political opponents, not ethnic representation per se. We show in Table A.6 that the number of distinct groups in top cabinet posts is significantly and positively associated with power-sharing measures from the EPR.[2]

One possible reason for the positive correlation between our measure of power sharing and those in Arriola, Devaro and Meng (2021) and Francois, Rainer and Trebbi (2015) is that the EPR measures rather big changes in the composition of leaders' cabinets. It records whether or not a group is a partner in government, not the proportion of government positions controlled by each group. This should provide reassurance that our coding of leaders' actions are capturing meaningful changes in power sharing over time and is not due to measurement error. Furthermore, in Appendix Section A.3, we describe a coarser coding of leaders' action using only changes in whether the group in power is a dominant group or not. We show that our estimates of leaders' payoff parameters do not change using this alternative coding in Appendix D. Finally, we reestimate the model using data from Francois, Rainer and Trebbi (2015) and present the analysis in Appendix Section F

[2]In Francois, Rainer and Trebbi's (2015) data, ministers can be multi-ethnic. Suppose we have two ministers, and one member is group A and the other is 2/3 group A and 1/3 group B. For our purposes, we must decide whether those ministers hail from the same group. To avoid overstating the diversity of cabinets, we would treat these two ministers as sharing a common ethnic identity (group A) and, thus, regard this two-member cabinet as ethnically homogeneous.

# B Transition Probabilities

## B.1 Covariates

We include additional covariates when estimating the transition probabilities (Equation 6). These reduce confounding by conditioning on features that affect leaders' actions, the budget, and their survival. (Country fixed effects absorb any static differences across countries.) The Archigos data enable us to code the leader's age at the start of their administration, as well as the first year of their tenure. Older leaders might have reduced survival probabilities. Stationarity in our model excludes measures that vary over time within administrations. Yet, we capture changes over time that affect survival (e.g., in medical technologies) by including each leader's first year in office. Using data from Ellis, Horowitz and Stam (2015), we code whether the leader has a military background, as this might enable the leader to more effectively wield coercive power and repress rivals.[3] As our coding of leaders' actions depends on their decisions to include or exclude other ethnic groups from their ruling coalitions, we condition on the number of ethnic groups. Finally, a large literature on the resource curse relates oil wealth to authoritarian survival Ross (see 2015, for a recent review). We use data from Ross and Mahdavi (2015) to determine if a country is an oil producer during a leader's time in office.

[3] Alternatively, military leaders might be inclined to "return to the barracks," wanting merely to secure order rather than extend their tenure (Geddes 2003).

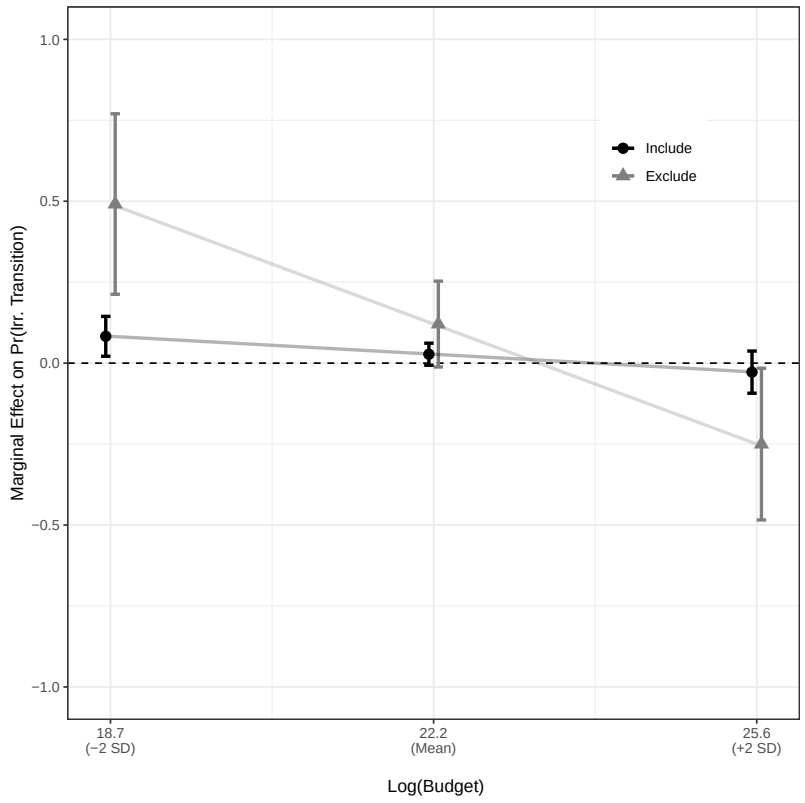
## B.2 Regression Estimates

**Table A.7:** Irregular leader removal.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $B_l^t \equiv$ Log(Budget) | 0.02 | 0.02 | 0.02 | 0.02 | 0.03 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| $\mathbf{I}_l^t \equiv$ Included | 0.30 | 0.31 | 0.33 | 0.37 | 0.39 |
| | (0.20) | (0.19) | (0.20) | (0.21) | (0.21) |
| $\mathbf{E}_l^t \equiv$ Excluded | 2.42 | 2.38 | 2.49 | 2.53 | 2.52 |
| | (0.85) | (0.87) | (0.86) | (0.86) | (0.89) |
| $\mathbf{I}_l^t \cdot B_l^t$ | -0.01 | -0.01 | -0.01 | -0.02 | -0.02 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| $\mathbf{E}_l^t \cdot B_l^t$ | -0.10 | -0.10 | -0.11 | -0.11 | -0.11 |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| First Year in Office | -0.00 | -0.00 | -0.00 | -0.00 | -0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Military Pedigree | | | -0.05 | -0.05 | -0.03 |
| | | | (0.01) | (0.01) | (0.01) |
| EPR Groups | | | | -0.01 | -0.01 |
| | | | | (0.01) | (0.01) |
| Start Age | | | | | 0.00 |
| | | | | | (0.00) |
| Oil Producer | | | | | -0.00 |
| | | | | | (0.03) |
| N | 2674 | 2674 | 2674 | 2674 | 2674 |
| Country Fixed Effects | 87 | 87 | 87 | 87 | 87 |
| Year Fixed Effects | 0 | 54 | 0 | 0 | 0 |

Models 1–5: linear probability models with country fixed effects per Equation 6. Time-varying covariates lag the outcome by one year. Standard errors clustered on administration.

**Figure A.1:** Marginal effect of leader's actions on Pr(irregular transition)




Marginal effects (and confidence intervals for $\alpha = 0.1$) of including an excluded group or excluding an included group on the probability of an irregular leadership transition when the budget (logged) is at its mean or $\pm 2$ standard deviations. Predictions use estimates from model 5 in Table A.7.

**Table A.8:** Leader death.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $B_l^t \equiv$ Log(Budget) | 0.00 | -0.02 | 0.00 | 0.00 | 0.00 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| $\mathbf{I}_l^t \equiv$ Included | -0.08 | -0.15 | -0.08 | -0.08 | -0.08 |
| | (0.12) | (0.12) | (0.13) | (0.13) | (0.12) |
| $\mathbf{E}_l^t \equiv$ Excluded (E) | -0.07 | -0.16 | -0.05 | -0.06 | -0.06 |
| | (0.11) | (0.13) | (0.12) | (0.12) | (0.12) |
| $\mathbf{I}_l^t \cdot B_l^t$ | 0.00 | 0.01 | 0.00 | 0.00 | 0.00 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| $\mathbf{E}_l^t \cdot B_l^t$ | 0.00 | 0.01 | 0.00 | 0.00 | 0.00 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| First Year in Office | -0.00 | -0.00 | -0.00 | -0.00 | -0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Military Pedigree | | | -0.01 | -0.01 | -0.00 |
| | | | (0.01) | (0.01) | (0.01) |
| EPR Groups | | | | 0.00 | 0.00 |
| | | | | (0.01) | (0.01) |
| Start Age | | | | | 0.00 |
| | | | | | (0.00) |
| Oil Producer | | | | | -0.01 |
| | | | | | (0.02) |
| N | 2674 | 2674 | 2674 | 2674 | 2674 |
| Country Fixed Effects | 87 | 87 | 87 | 87 | 87 |
| Year Fixed Effects | 0 | 54 | 0 | 0 | 0 |

Models 1–5: linear probability models with country fixed effects per Equation 6. Time-varying covariates lag the outcome by one year. Standard errors clustered on administration.

**Table A.9:** Budget.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $B_l^t \equiv$ Log(Budget) | 0.94 | 0.93 | 0.94 | 0.94 | 0.94 |
| | (0.01) | (0.02) | (0.01) | (0.01) | (0.01) |
| $\mathbf{I}_l^t \equiv$ Included | -0.48 | -0.44 | -0.47 | -0.43 | -0.44 |
| | (0.29) | (0.28) | (0.29) | (0.29) | (0.29) |
| $\mathbf{E}_l^t \equiv$ Excluded | -0.52 | -0.55 | -0.52 | -0.47 | -0.47 |
| | (0.46) | (0.45) | (0.46) | (0.45) | (0.45) |
| $\mathbf{I}_l^t \cdot B_l^t$ | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| $\mathbf{E}_l^t \cdot B_l^t$ | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| First Year in Office | 0.00 | -0.00 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Military Pedigree | | | -0.00 | -0.00 | -0.00 |
| | | | (0.01) | (0.01) | (0.01) |
| EPR Groups | | | | -0.01 | -0.01 |
| | | | | (0.01) | (0.01) |
| Start Age | | | | | -0.00 |
| | | | | | (0.00) |
| Oil Producer | | | | | -0.02 |
| | | | | | (0.02) |
| N | 2674 | 2674 | 2674 | 2674 | 2674 |
| Country Fixed Effects | 87 | 87 | 87 | 87 | 87 |
| Year Fixed Effects | 0 | 54 | 0 | 0 | 0 |

Models 1–5: linear models with country fixed effects per Equation 6. Time-varying covariates lag the outcome by one year. Standard errors clustered on administration.

## B.3 Predicted Values

**Table A.10:** Predicted transition probabilities.

| | | Budget Level ($B_l^t$) | | | | |
|---|---|---|---|---|---|---|
| | | $-\sigma_B$ | $-\sigma_B/2$ | $\overline{B}$ | $+\sigma_B/2$ | $+\sigma_B$ |
| | | 20.46 | 21.28 | 22.11 | 23.14 | 23.96 |
| Probability of Leader Survival: $g_l(a_l^t, C_l^t, B_l^t)$ | | | | | | |
| Included | $\mathbf{I}_l^t$ | 0.94 | 0.93 | 0.92 | 0.90 | 0.89 |
| Excluded | $\mathbf{E}_l^t$ | 0.69 | 0.75 | 0.82 | 0.91 | 0.97 |
| Maintained | $a_l^t = \varnothing, C_l^t = 0$ | 0.98 | 0.97 | 0.94 | 0.92 | 0.89 |
| Expected Future Budget: $E[B_l^{t+1} \mid a_l^t, C_l^t, B_l^t]$ | | | | | | |
| | $\mathbf{I}_l^t$ | 20.63 | 21.42 | 22.21 | 23.20 | 23.99 |
| | $\mathbf{E}_l^t$ | 20.56 | 21.35 | 22.14 | 23.13 | 23.92 |
| | $a_l^t = \varnothing, C_l^t = 0$ | 20.61 | 21.39 | 22.16 | 23.13 | 23.90 |

Using the first-stage regressions in Table 4 and the definitions of $g_l$ and $f_l$ in Section , we show how leader survival ($g_l$) and next year's budget ($B_l^{t+1}$) evolve after the autocrat chooses action $a_l^t$ in state $s_l^t = (C_l^t, B_l^t)$. The columns denote the current budget level, where the values represent the mean ($\overline{B}$) and plus/minus a half or full standard deviation ($\sigma_B$). The table uses the same background characteristics as in Figure 3: the autocrat has unlimited authority, has a military background, entered office in the mid-1970s at the age of 45, and rules a country with no oil and no past civil wars.

## B.4 Robustness: Including Time-Varying Covariates

**Table A.11:** Transition probabilities estimated with time-varying covariates.

| Outcomes measured in $t+1$: | Irregular Removal (1) | Death (2) | Budget (3) | Irregular Removal (4) | Death (5) | Budget (6) |
|---|---|---|---|---|---|---|
| $B_l^t \equiv$ Log(Budget) | 0.03 | 0.00 | 0.93 | 0.02 | -0.02 | 0.92 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) |
| $\mathbf{I}_l^t \equiv$ Included | 0.34 | 0.01 | -0.57 | 0.27 | -0.09 | -0.57 |
| | (0.23) | (0.13) | (0.32) | (0.23) | (0.14) | (0.31) |
| $\mathbf{E}_l^t \equiv$ Excluded | 2.54 | -0.01 | -0.68 | 2.41 | -0.13 | -0.74 |
| | (0.95) | (0.13) | (0.46) | (0.97) | (0.14) | (0.49) |
| $\mathbf{I}_l^t \cdot B_l^t$ | -0.01 | -0.00 | 0.03 | -0.01 | 0.00 | 0.03 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| $\mathbf{E}_l^t \cdot B_l^t$ | -0.11 | -0.00 | 0.03 | -0.10 | 0.01 | 0.03 |
| | (0.04) | (0.01) | (0.02) | (0.04) | (0.01) | (0.02) |
| p-value from test $H_0$: $\{\gamma_1 = 0, \ldots, \gamma_5 = 0\}$ | 0.02 | 0.39 | 0.00 | 0.06 | 0.11 | 0.00 |
| Additional Controls: {First Year in Office, Start Age, Military Pedigree, EPR Groups, Oil Producer} | | | | | | |
| Country Fixed Effects | 87 | 87 | 87 | 87 | 87 | 87 |
| Year Fixed Effects | 0 | 0 | 0 | 45 | 45 | 45 |
| N | 2,459 | 2,459 | 2,459 | 2,459 | 2,459 | 2,459 |

Models 1–6: linear regression models with country fixed effects. Models 4–6 include year fixed effects. Models with irregular leader transitions and leader death as the dependent variable are linear probability models. The budget and leader action variables lag the dependent variable by one year. All models include covariates for the leader's first year in office, their age when assuming power, whether they have a military pedigree, the number of EPR groups in the country, and whether the country produces oil. These covariates are permitted to vary over time and missingness leads to a slight reduction in our sample size. We omit coefficients on these controls to conserve space. Standard errors are clustered on administration.

## B.5 Robustness: Using Giant Oilfield Discoveries an an Exogenous Budget Shock

Lei and Michaels (2014) argue that the discovery of giant oilfields (encompassing 500 million barrels of ultimate recoverable reserves) generates a major resource windfall. Moreover, they show that "the timing of giant oilfield discoveries is plausibly exogenous, at least in the short-medium run" after conditioning on country and year fixed effects (140). Using this exogenous variation, Lei and Michaels estimate the causal effects of these giant oilfield discoveries, finding that oil production increases by 35-50 percentage points in the 4-10 years after discovery; oil exports increase 20-50 percent within 6-10 years; and government spending increases by 4-6 percent over the subsequent decade.

While Lei and Michaels focus on the reduced form relationship between giant oilfield discoveries and internal conflict (their main dependent variable), both their formal model and empirical strategy indicate that they view such discoveries as an instrument for government resource revenue: "giant oilfield discoveries increase oil revenues, generating windfall income for the incumbent" (139). We are similarly interested in identifying the effect of government budget shocks, though our focus is on how this interacts with leaders' actions to determine their probabilities of surviving in power.

**Table A.12:** Effects of giant oilfield discoveries on oil production and budgets.

| | Log(Oil & Gas Production) | | | Log(Budget) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Discovery in $t-4$ | 0.21 | | | 0.15 | | |
| | (0.11) | | | (0.07) | | |
| Discovery from $t-2$ to $t-6$ | | 0.26 | | | 0.15 | |
| | | (0.15) | | | (0.06) | |
| Discovery from $t-4$ to $t-6$ | | | 0.24 | | | 0.21 |
| | | | (0.12) | | | (0.07) |
| Country Fixed Effects | 52 | 52 | 52 | 87 | 87 | 87 |
| Year Fixed Effects | 48 | 48 | 48 | 48 | 49 | 49 |
| N | 1,222 | 1,233 | 1,222 | 2,514 | 2,552 | 2,539 |

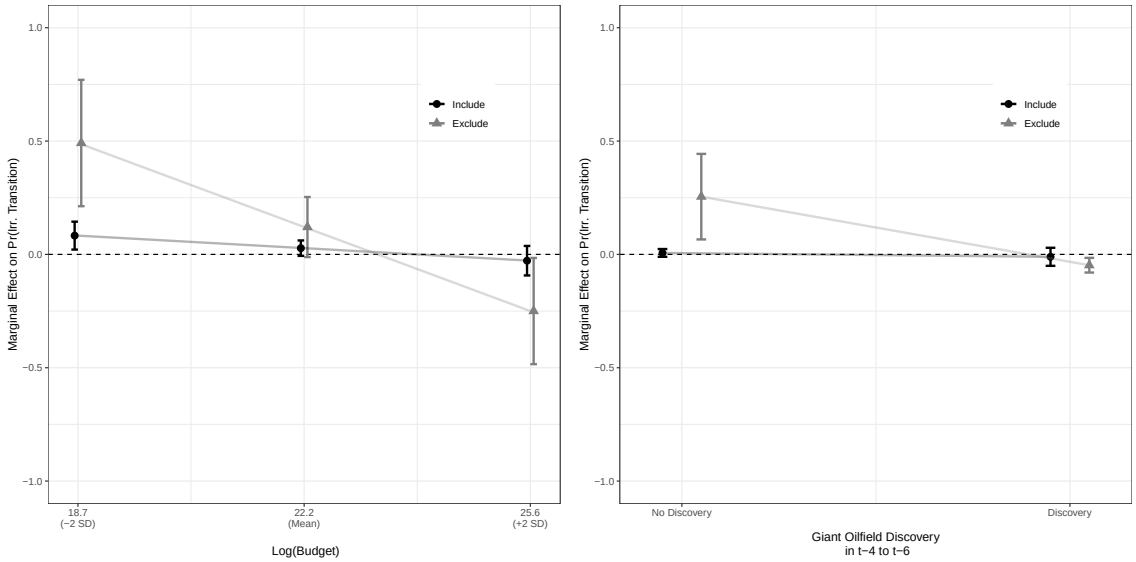
Notes: Standard errors clustered on administration.

We use Lei and Michaels's (2014) replication data but restrict attention to the administrations that overlap with our sample. Employing the authors' preferred specification, we first estimate in Table A.12 the effect of giant oilfield discoveries on oil and gas production per capita (logged) and our measure of government budgets (logged). Looking at columns 4-6, we find that recent oil discoveries increase our measure of governments' budgets by 15 to 20 percent.

Like Lei and Michaels (2014), we next estimate the reduced form relationship. We focus on the relationship between giant oilfield discoveries and irregular leadership transitions, reestimating Equation 6, but substituting an indicator for past oil discoveries for our budget measure $B$. In

Figure A.2, we reproduce Figure A.1 (left) and then show the marginal effects of excluding potential rivals and inclusion for leaders who do and do not enjoy a recent giant oilfield discovery (right).

**Figure A.2:** Marginal effect of leader's actions on Pr(irregular transition).




**(a)** Log(Budget) **(b)** Giant Oilfield Discoveries

The marginal effects follow the same pattern. While giant oilfield discoveries generate substantial budget increases, they do not generate a two-standard-deviation budget increase. Hence, the more modest magnitudes using this alternative empirical strategy.

# C Reduced-form Evidence that Budgets Affect Power Sharing

In Table A.13, we show that power sharing is more likely and inclusive in oil-producing autocracies as our budget measure increases. These associations are robust to the inclusion of year fixed effects and the leader-specific controls used in Table 4.

**Table A.13:** Reduced-form relationship between budgets and power sharing.

| | Included ($\mathbf{I}_l^t$) | | No Dominant Group | | # Included Groups | |
|---|---|---|---|---|---|---|
| Log(Budget) (B) | 0.11 | 0.14 | 0.12 | 0.15 | 0.29 | 0.34 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.07) | (0.08) |
| Additional Controls: | {First Year in Office, Start Age, Military Pedigree, EPR Groups} | | | | | |
| N | 1,212 | 1,212 | 1,212 | 1,212 | 1,212 | 1,212 |
| Country Fixed Effects | 44 | 44 | 44 | 44 | 44 | 44 |
| Year Fixed Effects | 0 | 54 | 0 | 54 | 0 | 54 |

Notes: Sample restricted to oil-producing countries. Standard errors clustered on administration.

# D Leader's Payoffs with Different Action Codings

Section A.3 describes two alternative codings of the leaders' actions. Table A.14 reports the resulting payoff estimates using these alternative codings, where we rerun both the first- and second-stage estimation procedures using the alternative codings.

**Table A.14:** Estimates of leaders' payoff parameters with alternative codings.

| Leader's Utility: | | $u_l(a_l^t, s_l^t; \theta) = B_l^t + x_l \cdot \beta + \rho \cdot \mathbf{I}(a_l^t, C_l^t) + \mathbf{E}(a_l^t) \cdot x_l \cdot \kappa$ | | |
|---|---|---|---|---|
| | | Baseline | Excl. Partial Purges | Dominant |
| Office Benefits ($\beta$) | Constant | -3.60 | -3.70 | -5.23 |
| | | (0.03) | (0.03) | (0.04) |
| | Unlimited Authority | -0.05 | 0.39 | -0.00 |
| | | (0.04) | (0.05) | (0.04) |
| | Military Pedigree | -0.70 | -1.36 | -0.00 |
| | | (0.04) | (0.05) | (0.04) |
| | Oil Producer | -0.82 | -0.69 | -0.31 |
| | | (0.04) | (0.05) | (0.02) |
| | Cum. Civil Wars | -0.30 | -0.69 | -1.22 |
| | | (0.01) | (0.02) | (0.02) |
| | Exports | 0.23 | -0.03 | 0.54 |
| | | (0.02) | (0.02) | (0.02) |
| Inclusion Cost ($\rho$) | | -0.98 | -1.14 | -1.25 |
| | | (0.00) | (0.00) | (0.00) |
| Exclusion Cost ($\kappa$) | Constant | -9.95 | -11.21 | -12.76 |
| | | (0.25) | (0.27) | (0.20) |
| | Unlimited Authority | 1.17 | 1.51 | 1.91 |
| | | (0.29) | (0.29) | (0.26) |
| | Military Pedigree | 0.64 | 0.73 | 0.65 |
| | | (0.25) | (0.28) | (0.23) |
| | Oil Producer | 0.14 | 0.65 | 0.13 |
| | | (0.25) | (0.20) | (0.17) |
| | Cum. Civil Wars | 0.12 | 0.02 | -0.44 |
| | | (0.11) | (0.09) | (0.09) |
| | Exports | -0.11 | -0.16 | -0.68 |
| | | (0.13) | (0.13) | (0.11) |
| | Log Likelihood | -261.59 | -210.98 | -187.38 |
| | Administrations | 303 | 303 | 303 |

Standard errors based on outer-product of gradients. Alternative codings of the action and state variables are described in Section A.3.

# E Leader's Payoffs with Different Sample Criteria

As described above, our baseline analysis focuses on leaders in countries (i) with polity2 scores of weakly less than 5, (ii) included in the Autocracies of the World database (AoW), and (iii) that impose at most slight to moderate limitation on executive authority.[4] Because we use the EPR data to code power sharing, we also require that countries have more than one ethnic group. We relax these sample criteria along two dimensions and reestimate model; we rerun both the first- and second-stage estimation procedures using alternative sample criteria. First, we drop the democracy requirements from (i) and (ii), i.e., the country has a polity2 score of less than 5 and is included in AoW. Second, we relax the executive constraints requirement and include countries in an intermediate category between limited constraints and substantial constraints (i.e., the country has an executive constraint measure from Polity of less than or equal to four). Table A.15 compares the estimates of the leaders' payoff parameters across our three samples. It shows that the point estimates have similar magnitudes and directions regardless of the specific sample criteria. The one exception is how unlimited authority affects the leader's office benefits, but this effect was not significant at conventional levels in the baseline model.

[4] That is, we require that a country's executive constraint measure from Polity is less than or equal to three.

**Table A.15:** Estimates of leaders' payoff parameters with different sample criteria.

| Leader's Utility: | | $u_l(a_l^t, s_l^t; \theta) = B_l^t + x_l \cdot \beta + \rho \cdot \mathbf{I}(a_l^t, C_l^t) + \mathbf{E}(a_l^t) \cdot x_l \cdot \kappa$ | | |
|---|---|---|---|---|
| | | Baseline | Drop AoW & Polity | Relax unconstrained criteria |
| Office Benefits ($\beta$) | Constant | -3.60 | -3.43 | -2.96 |
| | | (0.03) | (0.03) | (0.02) |
| | Unlimited Authority | -0.05 | -0.56 | 1.29 |
| | | (0.04) | (0.04) | (0.03) |
| | Military Pedigree | -0.70 | -0.35 | -1.90 |
| | | (0.04) | (0.04) | (0.03) |
| | Oil Producer | -0.82 | -0.63 | -3.03 |
| | | (0.04) | (0.05) | (0.03) |
| | Cum. Civil Wars | -0.30 | -0.35 | -0.43 |
| | | (0.01) | (0.01) | (0.01) |
| | Exports | 0.23 | 0.14 | 0.39 |
| | | (0.02) | (0.01) | (0.02) |
| Inclusion Cost ($\rho$) | | -0.98 | -0.99 | -1.05 |
| | | (0.00) | (0.00) | (0.00) |
| Exclusion Cost ($\kappa$) | Constant | -9.95 | -9.86 | -9.61 |
| | | (0.25) | (0.27) | (0.23) |
| | Unlimited Authority | 1.17 | 1.10 | 2.35 |
| | | (0.29) | (0.31) | (0.30) |
| | Military Pedigree | 0.64 | 0.65 | -0.49 |
| | | (0.25) | (0.29) | (0.24) |
| | Oil Producer | 0.14 | -0.08 | -1.06 |
| | | (0.25) | (0.27) | (0.23) |
| | Cum. Civil Wars | 0.12 | 0.12 | -0.05 |
| | | (0.11) | (0.14) | (0.09) |
| | Exports | -0.11 | -0.06 | -0.06 |
| | | (0.13) | (0.13) | (0.14) |
| | Log Likelihood | -261.59 | -264.41 | -281.08 |
| | Administrations | 303 | 326 | 315 |

Standard errors based on outer-product of gradients.

# F Leader's Payoffs and Substantive Effects with Different Data

In Appendix Section A.4, we describe how our coding of the EPR data correlates with data from Francois, Rainer and Trebbi (2015, FRT hereafter). In this Appendix section, we reestimate our structural model using the sample and data from FRT, re-running both the first- and second-stage estimation procedures. Table A.16 presents estimates of the leader's payoff parameters across three models. The baseline model presents the estimates from Table 5 in the main text. In model 1, we use the original EPR coding, but we subset the observations to the countries and years included in the FRT data and reestimate the model. In model 2, we use the FRT data to code leaders' actions and then reestimate the model.

Comparing the models reveals two takeaways.[5] First, the coefficient estimates in models 1 and 2 are mostly in the same direction — nine out of 13 estimates have the same sign. Second, inclusion costs and the constant associated with exclusion costs are substantially smaller when using the FRT data (model 2) compared to the EPR data (model 1). In model 2, $\rho$ is positive although the estimate is small and not precise. This difference likely arises because the EPR data focuses on "politically relevant" ethnic groups, where a group is politically relevant if "at least one political organization has claimed to represent its interests at the national level or if its members are subjected to state-led political discrimination" (Cederman, Min and Wimmer 2012, 99). The EPR focuses on politically salient cleavages and, as such, combines groups that are separately enumerated in the FRT data, as in the example of Tanzania described in Section A.4. As another example, the EPR combines several smaller, politically aligned groups in Idi Amin's Uganda as "South-Westerners," rather than separately coding whether the Ankole, Banyoro, Toro, and Banyarwanda were partners in government.[6] For this reason, there are smaller, more finely delineated groups in the FRT data and, thus, there appear to be more fluctuations in power sharing over time (even if the these changes do not cross salient political cleavages). All else equal, more observed variation in power sharing implies that leaders have lower costs to purging groups from government and face lower inclusion costs.

Finally, we also explore the robustness of our substantive predictions using the newly estimated models. To do this, we fix an initial coalition type $C_l \in \{0,1\}$ and plot the sample-average probability of a power-sharing change, i.e., $\frac{1}{L}\sum_{l=1}^{L} \Pr(a_l \neq \varnothing; (B_l, C_l), V_l)$, for each budget level $B_l$ between the mean budget level plus and minus one standard deviation.[7] Figure A.3 shows the substantive effects for model 1 and Figure A.4 shows the effects for model 2. These should be compared to Figure 3 in the main text. Broadly, the results show similar patterns. Leaders are most likely to include an excluded opposition with a larger budgets, and leaders are most likely to exclude an in-

[5] Models 1 and 2 have a different number of administrations. In our data, we record three administrations that last a single year and do not appear in the FRT data (Benin, 1969; Republic of Congo, 1969; Democratic Republic of Congo, 1960).

[6] During this time period, the EPR also combines Uganda's Madi, Lugbara, and Alur ethnic groups into the "Far North-West Nilers."

[7] We use the mean plus/minus one standard deviation because our sample size is smaller in this analysis, so we do not want to extrapolate to the extreme levels of the state space with few observations.

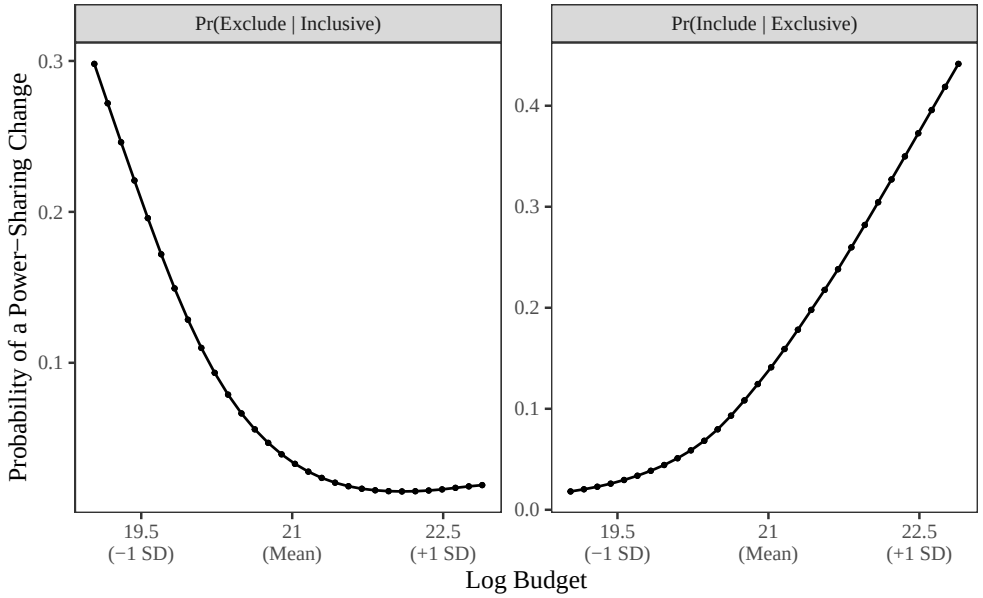
**Table A.16:** Estimates of leaders' payoff parameters with FRT sample and data

| Leader's Utility: | | $u_l(a_l^t, s_l^t; \theta) = B_l^t + x_l \cdot \beta + \rho \cdot \mathbf{I}(a_l^t, C_l^t) + \mathbf{E}(a_l^t) \cdot x_l \cdot \kappa$ | | |
|---|---|---|---|---|
| | | Baseline | Model 1 | Model 2 |
| Office Benefits ($\beta$) | Constant | -3.60 | -1.92 | -1.05 |
| | | (0.03) | (0.26) | (1.34) |
| | Unlimited Authority | -0.05 | -1.51 | -2.02 |
| | | (0.04) | (0.14) | (2.15) |
| | Military Pedigree | -0.82 | -2.85 | 0.22 |
| | | (0.04) | (0.10) | (1.83) |
| | Oil Producer | -0.82 | -0.47 | -5.59 |
| | | (0.04) | (0.11) | (1.67) |
| | Cum. Civil Wars | -0.30 | -1.06 | -1.76 |
| | | (0.01) | (0.05) | (1.02) |
| | Exports | 0.23 | 1.03 | 0.17 |
| | | (0.02) | (0.06) | (0.66) |
| Inclusion Cost ($\rho$) | | -0.98 | -0.86 | 0.13 |
| | | (0.00) | (0.07) | 0.43 |
| Exclusion Cost ($\kappa$) | Constant | -9.95 | -8.57 | -1.46 |
| | | (0.25) | (1.19) | (1.27) |
| | Unlimited Authority | 1.17 | -1.35 | 0.23 |
| | | (0.29) | (1.06) | (0.96) |
| | Military Pedigree | 0.64 | 1.71 | 0.67 |
| | | (0.25) | (1.12) | (1.22) |
| | Oil Producer | 0.14 | 1.95 | -0.48 |
| | | (0.25) | (0.86) | (1.28) |
| | Cum. Civil Wars | 0.12 | -1.85 | -0.37 |
| | | (0.11) | (0.86) | (0.82) |
| | Exports | -0.11 | 0.76 | 0.04 |
| | | (0.13) | (0.90) | (0.45) |
| | Log Likelihood | -261.59 | -47.60 | -255.51 |
| | Administrations | 303 | 60 | 57 |

Standard errors based on outer-product of gradients.

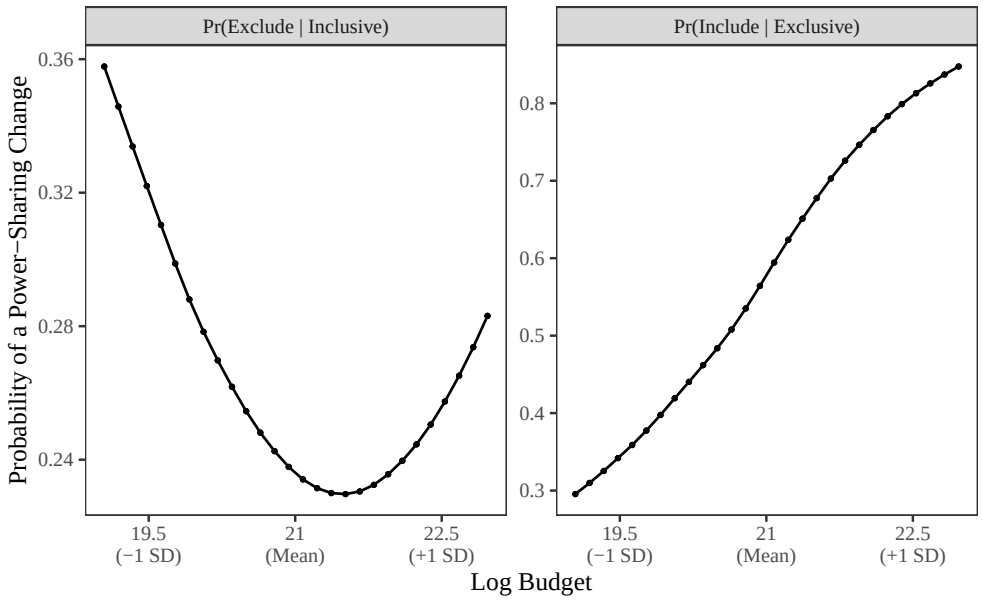
cluded opposition with smaller budgets. One difference emerges, however. Namely, the probability of excluding an included opposition has a potentially non-monotonic relationship with the leader's budget. Specifically, leaders may be the least likely to exclude at mean budget levels, although we hesitate to over interpret this result given the small number of administrations in the sample.

**Figure A.3:** Effect of budget levels on power sharing using model 1 from Table A.16




Sample-average predicted probability that the leader excludes an included opposition (**left**) and includes an excluded opposition (**right**).

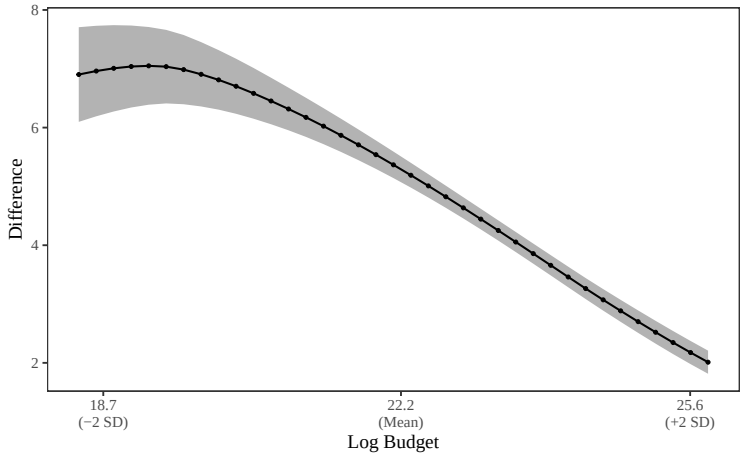
**Figure A.4:** Effect of budget levels on power sharing using model 2 from Table A.16




Sample-average predicted probability that the leader excludes an included opposition (**left**) and includes an excluded opposition (**right**).

# G Additional Figures

**Figure A.5:** Difference between $V_l(B_l, C_l = 0) - V_l(B_l, C_l = 1)$.




All variables, $z_l$ and $x_l$ are held at their sample medians, and the shaded area denotes the 90% confidence intervals from a country-level jackknife.

**Figure A.6:** Budget implications of commodity boom in Africa.

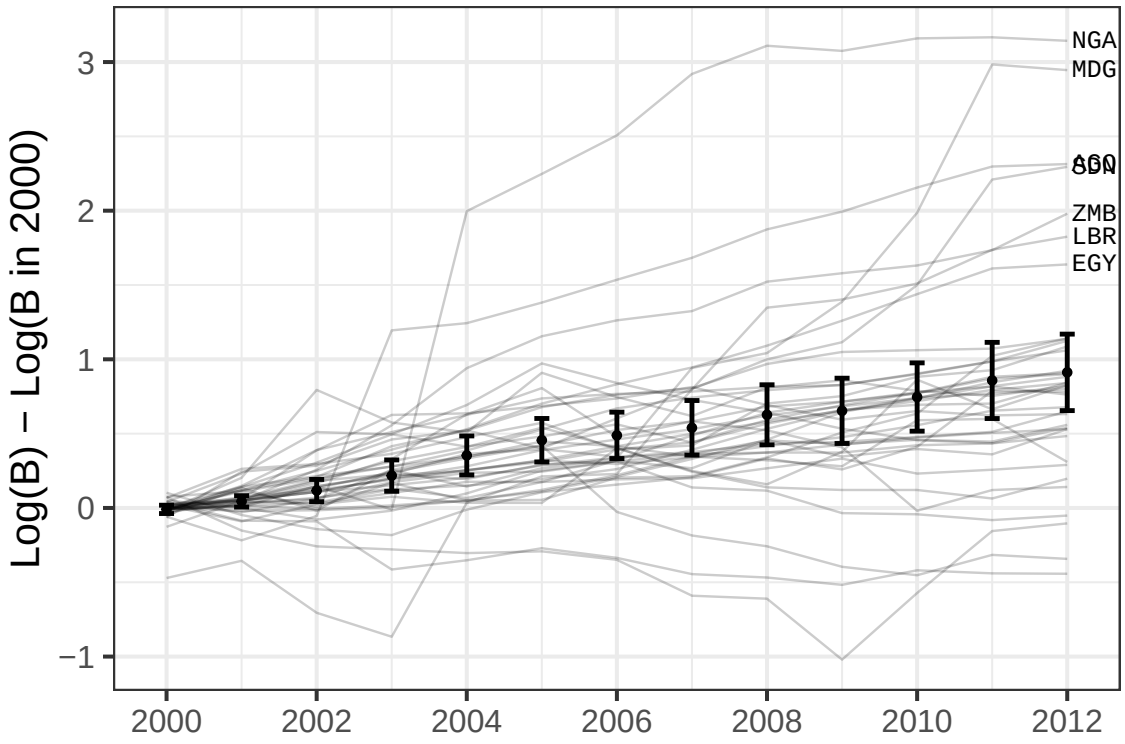
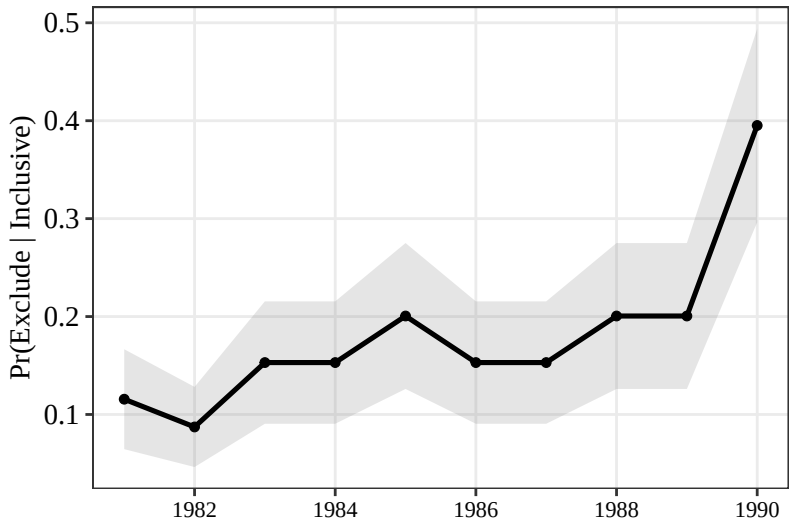


**Figure A.7:** In-sample Predictions for Liberia.




Y-axis is the predicted probability that the leader purges an included group. All $x_l$ and $z_l$ variables are set using values from Liberia.