The Democracy Dilemma. Aid, Power-Sharing Cabinets, and Post-Conflict Democratization

Supplementary Online Appendix

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1 List of Countries and Sample Construction

I apply three criteria for the selection of post-conflict country-years, based on Kreutz' conflict termination dataset (Kreutz 2010) (1) The country must have experienced at least one conflict termination between 1990 and 2010. Civil conflict termination is observed "when an active year is followed by a year in which there are fewer than 25 battle-related deaths" (Kreutz 2010, 244). (2) The country must have experienced a war termination of one of the following types: Peace Agreement, Ceasefire, Ceasefire with conflict regulation, or Victory by one side. These definitions are based on Kreutz' typology of conflict termination who includes other outcomes and low activity in addition to the four types just listed. I exclude the categories "other outcomes" and "low activity" because it is unclear whether the underlying conflict has actually ended or is simply "frozen". (3) The country must have experienced at least one period of two consecutive years of post-conflict peace. This is to allow for a minimum of time for political reforms to take place and to exclude conflicts that have consecutive annual outbreaks and stops of conflict.

Table 1. List of Countries

Country	Years obs.	$\varnothing \frac{Aid}{GDP}$	Has PS?	$\varnothing$ Polity IV	$\varnothing$ FH
Angola	2005-2006	1.03	Yes	-2	2.5
Azerbaijan	1996-2000, 2006-2010	2.45	No	-6.8	2.6
Bangladesh	1993-1997	3.02	Yes	6	4.9
Bosnia and Herzegovina	1996-2000	18.61	Yes	.	3.1
Burundi	2009-2010	29.09	No	6	3.2
Cambodia	1999-2003	13.18	No	2	2.3
Central African Republic	2003-2005, 2007-2008	8.62	No	-1	2.8
Comoros	1990-1994, 1998-2002	12.67	Yes	2.5	3.6
Congo	1994-1996, 2000-2001, 2003-2007	8.12	No	-1.6	3.4
Cote D'Ivoire	2005-2009	3.73	Yes	0	2.1
Croatia	1996-2000	0.76	No	-1.2	4.3
Democratic Republic of Congo	2002-2005, 2009-2010	24.38	Yes	3	2
Djibouti	1995-1998, 2000-2004	13.26	Yes	-1.7	2.9
El Salvador	1992-1996	5.1	No	7	5
Georgia	1994-1998, 2005-2007, 2009-2010	6	No	5.6	4.2
Guatemala	1996-2000	1.97	No	8	4.5
Guinea-Bissau	2000-2004	23.9	No	2.6	3.5
Haiti	1992-1996, 2005-2009	10.4	No	3	2.7
Indonesia	1993-1996, 2006-2010	0.98	No	1.3	3.8
Lebanon	1991-1995	1.99	No	.	2.8
Lesotho	1999-2003	9.62	No	5.6	4.6
Liberia	1991-1995, 2004-2008	42.1	Yes	2.6	2.9
Macedonia	2002-2006	4.9	Yes	9	5
Mali	1991-1993, 1995-1999	12.89	Yes	5.8	5.1
Mexico	1997-2001	0.22	No	6.8	4.9
Moldova	1993-1997	5.24	No	7	4
Mozambique	1993-1997	36.12	No	2.8	4
Nepal	2007-2010	5.38	Yes	6	3.9
Nicaragua	1991-1995	16.97	No	6.4	4.1
Niger	1998-2002, 2009-2010	12.01	Yes	2	3.4
Nigeria	2005-2008	2.57	No	4	3.9
Panama	1990-1994	2.92	No	8.2	5
Papua New Guinea	1997-2001	13.89	No	4	5.4
Paraguay	1990-1994	1.32	No	5	4.8
Peru	2000-2004	1.5	No	8.2	5.4
Rumania	1990-1994	0.98	No	5	3.6
Russia (Soviet Union)	1997-1998	2.04	No	3	4.2
Rwanda	2003-2007	23.78	No	-3	2.5
Senegal	2004-2008	9	No	7.6	5.4
Serbia	1992-1996, 2000-2004	6.71	No	0	3.6
Sierra Leone	2001-2005	29.37	No	4.4	4.2
Somalia	1997-2000, 2003-2005	6.64	No	0	1.3
Tajikistan	1999-2003	16.53	No	-1.4	2.2
Trinidad and Tobago	1991-1995	0.48	No	9	6.8
Uzbekistan	2001-2003, 2005-2009	1.67	No	-9	1.2
Venezuela	1993-1997	0.29	No	8	5.2

Note: A total of 46 countries. Freedom House scores have been inverted to increase comparability between Polity and FH (i.e. higher values indicate a higher level of democracy). An "." indicates missing data.

2 Description of Covariates

Below, I provide a description of and rationale for including the covariates:

- *GDP per capita*. GDP/pc controls for long-standing arguments in the literature suggesting that economic development positively affects political development (Lipset 1959; Robinson, Torvik, and Verdier 2006). Further, the GDP per capita variable also accounts for the bias that poorer countries (as measured by GDP per capita) receive more aid (Hoeffler and Outram 2011). Data is taken from the United Nations, based on IMF estimates and is measured at current prices (United Nations 2015).

- *Population size*. A classic argument in the literature is that larger populations make democratic governance more difficult: the higher the number of individual preferences that need to be aggregated by the political system, the more difficult it is to take them efficiently into account (Remmer 2010, 280). At the same time, less populous countries also receive more aid, on average Alesina and Dollar 2000; Hoeffler and Outram 2011. To account for any confounding relationship between population size and aid allocation, I therefore include the natural log of population size into my empirical models. Data is taken from the World Bank (World Bank 2015b).

- *Nonstate violence* I include a measure of nonstate violence that occurs within the country. Although my sample is constructed on the basis of the absence of violent conflict between one or more rebel groups and the government, in many instances violence between groups (without direct government involvement) continues. If nonstate armed conflict continues during the country-year under observation, I expect the chances of political reforms to decline. Data for the existence of nonstate violence is taken from the UCDP nonstate violence data set (Sundberg, Eck, and Kreutz 2012). I use the 2.5-2014 version of the dataset which defines nonstate armed conflict as "the use of armed force between two organized armed groups, neither of which is the government of a state, which results in at least 25 battle-related deaths in a year" (Pettersson 2014, 2).

- *Natural Resource Rents*. To rule out that the relationship between aid, power-sharing, and political development is driven by natural resource rents, I therefore control for state income from natural resources. I include a measure of rents from natural resources as per cent of GDP. Data is taken from the World Bank (World Bank 2015a). The World Bank includes the following sources in its calculation of natural resources rents: The sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents (World Bank 2015a). Since World Bank data is incomplete for some country-years, I carry forward the last known observation to avoid missing variables.[1]

- *Regime Type*. Including the level of a post-conflict country's level of democratic development serves three purposes: First, it allows me to control for regime type effects. Political development, i.e. reforms that allow *inter alia* for elections, independent judiciaries, public goods provision might be more easily conducted in countries which already have a certain amount of political freedom. Second, the inclusive nature of power-sharing governments might be reflected in the measurement of a country's institutions. By including a measure of the level of political development, I can separate out the effects of power-sharing as opposed the effects of regime type. Third, more democratic countries typically receive more aid, since donors are reluctant to deliver aid in the hands of blatant dictators (at least since the end of the Cold

1. The only country in which no data is available for natural resource exports is Somalia. I set Somalia's natural resource rents to zero in order to prevent the country from dropping out due to missing values. However, estimation results do not change with or without Somalia.

War, see Dunning 2004; Bermeo 2016). Also, including the level of regime type functions as a baseline adjustment, so that the resulting coefficients can be interpreted as a change in the dependent variable, if the dependent variable is measured in the future (Allison 1990). Consequently, I include a measure of a country's *Polity* score at year t to control for regime type effects on political development. Since some countries have missing Polity scores (Bosnia for instance), I replace Polity scores with Freedom House scores in the models that use Election Quality, Judicial Independence and Particularistic Spending as dependent variable (Freedom House 2013).

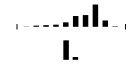
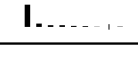
- *Conflict Intensity* More violent conflicts may have detrimental effects on post-conflict political development, but might simultaneously determine whether a power-sharing arrangement will be established. To rule out that any effect of power-sharing on political development does not simply reflect extremely deadly conflicts that were ended through a power-sharing arrangement, I include a dummy for conflict intensity. This variable also serves the purpose of controlling for a potential aid allocation bias: countries with more severe conflicts are likely to receive more aid, given the higher rates of destruction of infrastructure and the greater humanitarian needs of countries with more severe conflicts. The dummy variable takes a one when the conflict in any of the 10 years prior to the first year of post-conflict peace has exceeded 1000 battle-related deaths. Data for conflict intensity is taken from the Uppsala Conflict Data Program (Themnér and Wallensteen 2012).

3 Summary Statistics and Descriptive Evidence

3.1 Summary Statistics

Table 2. Summary Statistics

Cross-Section Time-Series Sample

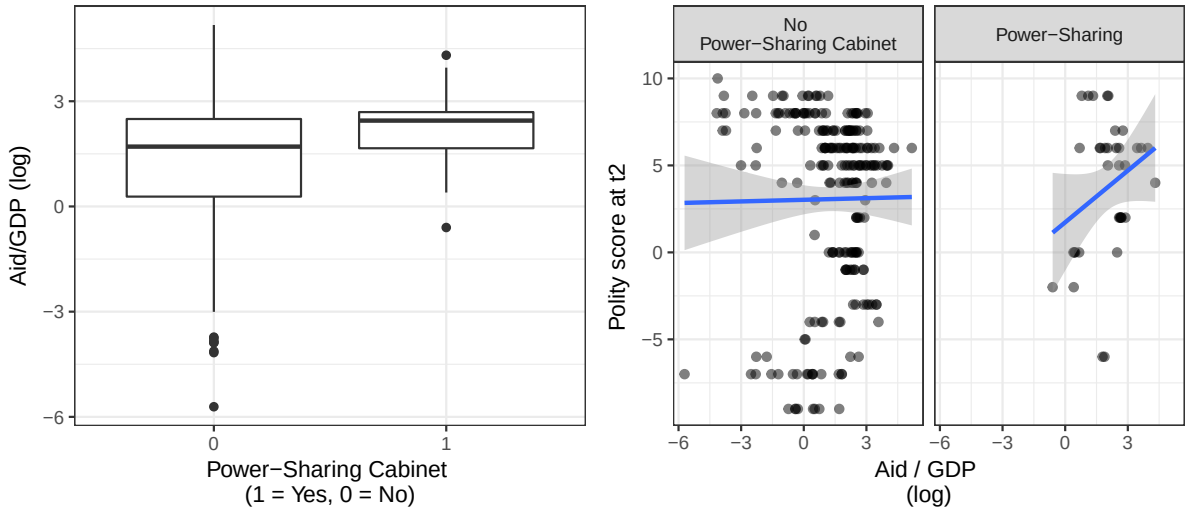
	Obs	Min	Max	Mean	Std.Dev	Median	Distribution
Polity	263	-9.0	10.0	3.2	5.1	5.0	
Freedom House	272	1.0	7.0	3.8	1.4	4.0	
Judicial Independence (V-Dem)	273	0.0	0.9	0.4	0.2	0.5	
Judicial Independence (LJI)	273	0.0	0.9	0.3	0.2	0.3	
Particularistic Spending (V-Dem)	273	-3.0	1.7	0.0	1.0	0.1	
Political Corruption (V-Dem)	273	0.2	0.9	0.7	0.2	0.8	
Power-Sharing (cabinet)	273	0.0	17.0	0.6	2.2	0.0	
Aid/GDP	273	0.0	176.7	10.2	15.5	6.6	
Nonstate Conflict	273	0.0	1.0	0.1	0.3	0.0	
Conflict Intensity	273	0.0	1.0	0.3	0.5	0.0	
GDP p/c	273	4.4	8.8	6.7	1.0	6.6	
Population	273	12.9	19.3	15.9	1.4	15.8	
Natural Resource Rents	273	0.0	82.6	10.9	16.2	3.4	

Election Sample

	Obs	Min	Max	Mean	Std.Dev	Median	Distribution
NELDA Election Quality	145	3.0	9.0	7.2	1.7	8.0	
V-Dem Clean Election Index	144	0.1	0.9	0.5	0.2	0.5	
Power-Sharing (cabinet)	145	0.0	18.0	0.8	2.9	0.0	
Aid/GDP	142	0.0	54.1	10.4	13.0	7.3	
Nonstate Conflict	145	0.0	1.0	0.1	0.2	0.0	
Conflict Intensity	145	0.0	1.0	0.2	0.4	0.0	
GDP p/c	142	5.0	8.7	6.7	1.0	6.7	
Population	145	12.9	19.3	15.8	1.3	15.8	
Natural Resource Rents	145	0.0	51.2	7.9	12.1	2.3	

3.2 Descriptive Statistics

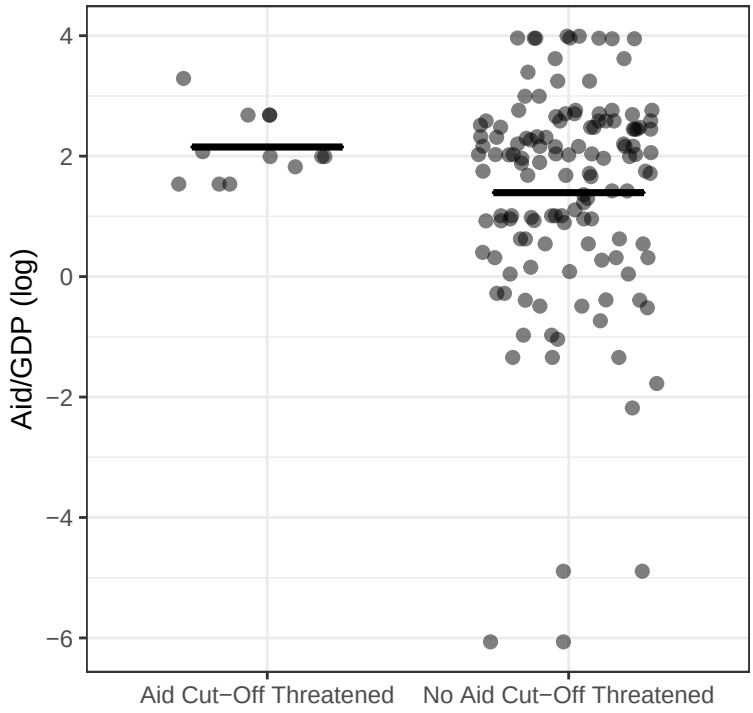
Figure 1. Power-Sharing, Foreign Aid, and Democracy



The left panel of Figure 1 shows that post-conflict countries with power-sharing cabinets in place received on average substantially more aid than non-power-sharing country-years. The right panel of Figure 1 illustrates the effects of this linkage on the relationship between aid, power-sharing and democracy: in country-years without power-sharing cabinets, the assocation between aid and democracy is flat. In country-years *with* power-sharing cabinets, we observe a clear, positive association between the level of aid income and democracy. This descriptive pattern is consistent with my expectation that power-sharing and aid jointly predict positive democracy scores.

3.3 Aid Volume and Aid Cut-Offs

Figure 2. Aid Volume and Aid Cut-Offs



Note: The bold horizontal lines represent distribution means.

Table 3. 2SLS and Matching Results

	Democracy (Polity)		Election Quality		Rule of Law		Public Goods	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Power-Sharing (binary)	−0.26	−2.01**	−0.60***	−0.16*	0.13**	0.11*	0.81***	1.02***
	(0.58)	(0.92)	(0.15)	(0.09)	(0.05)	(0.06)	(0.29)	(0.30)
Aid / GDP (instrumented)	0.76**		−0.02		0.00		−0.14	
	(0.33)		(0.02)		(0.01)		(0.12)	
Aid / GDP (log)		−0.36		−0.00		−0.01		0.07
		(0.25)		(0.04)		(0.02)		(0.11)
Power-Sharing (binary) * Aid (instrumented)	0.51**		0.32***		−0.05**		−0.21*	
	(0.26)		(0.06)		(0.02)		(0.11)	
Power-Sharing (binary) * Aid		1.15***		0.13***		−0.05**		−0.32***
		(0.36)		(0.04)		(0.02)		(0.11)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Kleibergen Paap-rk F-Statistic	37.86		17.17		40.32		40.23	
Num. obs.	261	108	141	72	270	108	271	108

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: Odd-numbered columns are results from 2SLS models, even-numbered columns show matching results. Robust standard errors clustered on country reported in brackets. Year FEs drop out of the matching models due to collinearity with remaining observations in the reduced sample.

4 Endogeneity

Foreign aid is not randomly allocated. Thus, the results might be driven by idiosyncratic allocation preferences, such as donor preferences for more democratic countries. The 2SLS approach addresses such form of unobserved endogeneity. I employ an instrumental variable proposed by Harding and Venables that exploits the donor-recipient-specific long-term aid relationship and year-specific total aid budget variation on the part of donors (excluding the aid to the recipient) (Harding and Venables 2010). Specifically, I multiply the long-run average of aid flows to a recipient between 1980 and 2010 by a donor's average aid budget for all countries excluding the recipient. This instrumental variables captures the "push" factors of aid, that is variation in the supply-side of aid.[2] Thus, this instrument exogenously determines aid flows to a recipient, but is unlikely to be correlated with short-term political events.

The odd-numbered models in Table 3 display the 2SLS models and show substantively similar results to the findings reported in the main text. The coefficients of the interaction terms are in the expected directions and precisely estimated. The coefficient of the interaction term for model 1 that uses Polity scores as the dependent variable (0.51) is similar in size to its counterpart in **??** (0.66). IV diagnostics indicate sufficient instrument strength with F values between 17 and 40.

Power-sharing, too, is not randomly distributed across post-conflict countries. I address this form of endogeneity by using a matching approach. An additional benefit is that matching guards against model dependency, such as functional form misspecification (Ho et al. 2006). I match on all covariates also employed in the OLS analyses reported above. Particularly relevant are conflict intensity, foreign aid, and democracy, since high-intensity conflicts, international pressure, and prior experience with democracy are all potential predictors of the establishment (and continuation) of power-sharing governments. To mitigate post-treatment bias, I measure these variables' values at the beginning of each period. I perform nearest-neighbor matching, using a Mahalanobis distance with a 2-to-1 ratio. Matching diagnostics indicate that this procedure leads to a substantial reduction in the covariate imbalance that exists in the original sample.[3] To account for residual variation in the matching models, I include the covariates from the OLS models for the models on the matched

2. See below for a technical discussion of the construction of the IV.
3. See below for detailed matching diagnostics.

sample as well (Ho et al. 2006, 215).

The even-numbered models in Table 3 display the results from OLS models estimated on the matched data. The coefficients of the interaction terms between the power-sharing and foreign aid variable are substantively consistent with their counterparts in Table 1 in the main text: in the context of power-sharing executives, foreign aid drives limited democratization in the form of clean elections, but simultaneous deterioration in the rule of law and public goods provision.

While the matching and 2SLS models give me some added leverage in supporting a causal interpretation of the findings, there are also caveats to keep in mind. One is that I treat the endogeneity of aid and power-sharing as two independent econometric problems, while it is, in fact, one interconnected problem. It would be preferable to use an IV strategy to account for the endogeneity of power-sharing as well to achieve a clean identification of the interaction of the two instrumented variables. Yet it is difficult, if not impossible, to find a valid instrument for power-sharing that is not also related to democracy. While the alternative approach of matching is a second-best strategy, it cannot—unlike an IV strategy—guard against bias from unobserved covariates. Its additional benefit therefore mainly comes from its robustness to model dependency. Thus, I prefer to interpret the results of Table 3 as representing the plausible range of effects that can be estimated from the data, rather than representing true average treatment effects (conditional on the respective power-sharing/aid variable).

4.1 Using Exogenous Aid Budget Fluctuations as Instrumental Variable

The basic idea underlying the 2SLS approach is to find one or several variables that determine the allocation of aid, but are uncorrelated with the initiation of political reforms. This eliminates the bias of the original *Aid* variable that might result from reverse causality or selection problems and produces consistent estimates for *Aid*. I employ an instrument proposed by Harding and Venables (2010) that exploits the donor-recipient-specific long-term aid relationship and year-specific total budget variation by donors.[4] This instrument captures the "push" factors of aid, that is variation in the supply-side of aid.

Harding and Venables' aid instrument takes the following form:

$$z_{idt} = s_{id} \sum_{i=1}^{N} a_{dt} \tag{1}$$

s_{id} represents a long-term share of foreign aid by donor d to country i over the period 1980 to 2010. a_{dt} gives us all bilateral aid by donor d to all countries in my sample except recipient country i in year t. Multiplying s_{id} with $\sum_{i=1}^{N} a_{dt}$ gives an estimate of exogenous aid flows to country i in year t. The idea behind this is instrument is as follows: the time-variation in a_{dt} is a function of the overall aid budget of donor d in year t. Since a donor's overall aid budget is likely to be driven by domestic and international political considerations independent from the prospect of political reforms in recipient country i, the variation in a_t should be exogenous to a change in democracy scores. Similarly, since s_{id} is a long-term average of bilateral aid flows between donor and recipient, it should also be exogenous to short-term variation in democracy scores. Consequently, we can compute z_{idt} as a time-varying estimate of bilateral aid flows by donor d to country i in year t.

To arrive at an exogenous estimate of the *total* annual aid flows to i, I compute Z_{it}, the sum of z_{idt} for the 30 largest donors in my sample:

$$Z_{it} = \sum_{d=1}^{30} z_{idt} \tag{2}$$

4. For another application of this instrumental variable, see Hodler and Raschky (2014).

The two-stage least-squares equations then take the following form:

$$Aid_{i,t} = X_{i,t} + Z_{it} + \mu_{it} \tag{3}$$

$$\begin{aligned} Democracy\ Indicators_{it_2} = & \hat{Aid}/GDP + \\ & \hat{Aid}/GDP * Power\text{-}Sharing + \\ & X + \epsilon \end{aligned} \tag{4}$$

In the first stage, I regress *Aid* on all the explanatory variables X in the base specification plus the exogenous instrument Z_{it} as given by Equation 1 and Equation 2.[5] In the second stage, I use the predicted values for Aid, $\hat{Aid}$, generate the natural log of the respective ratio of $\hat{Aid}$ to GDP and interact the variable with *Power-sharing*.

5. The *Power-sharing* variable is included in X.

4.2 Matching Diagnostics

Figure 3. Matching Diagnostics

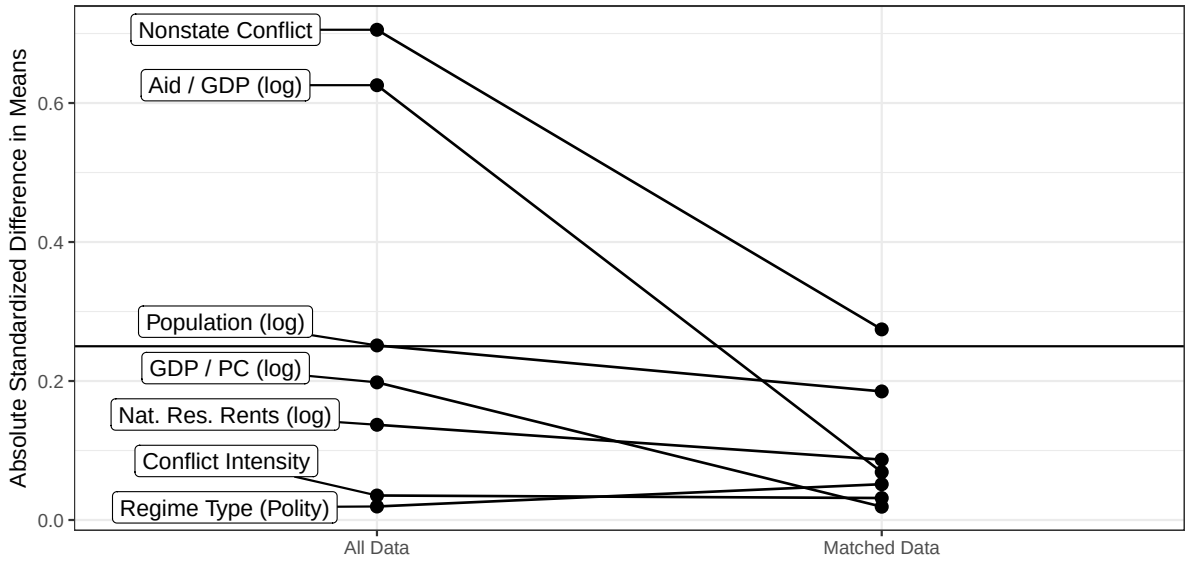


Table 4. Matching Diagnostics: Imbalance Reduction

Before Matching

	Mean Treated	Mean Control	P-Value T-Test	P-Value K-S Test
Aid / GDP (log)	2.08	1.43	0.00	0.01
GDP / PC (log)	6.37	6.54	0.29	0.14
Population (log)	15.51	15.91	0.16	0.00
Conflict Intensity	0.25	0.27	0.85	
Nonstate Conflict	0.47	0.12	0.00	
Natural Resource Rents (log)	1.62	1.79	0.44	0.11
Regime Type (Polity)	1.83	1.74	0.92	0.24

After Matching

	Mean Treated	Mean Control	P-Value T-Test	P-Value K-S Test
Aid / GDP (log)	2.08	2.01	0.74	0.42
GDP / PC (log)	6.37	6.39	0.93	0.13
Population (log)	15.51	15.81	0.34	0.01
Conflict Intensity	0.25	0.24	0.88	
Nonstate Conflict	0.47	0.33	0.17	
Natural Resource Rents (log)	1.62	1.73	0.68	0.39
Regime Type (Polity)	1.83	2.08	0.80	0.48

Note: The p-value of the t-test test against the Null Hypothesis of difference in means between the distributions (i.e. the covariate distributions in the treated and control group). A statistically significant p-value indicates there a difference in means. The p-value for the Kolmogorov-Smirnov test tests against the Null hypothesis that the data from two samples come from the same underlying distribution. A statistically significant p-value for the K-S test indicates that the data in the treated and control comes from two different distributions. Generally, for matching non-significant p-values indicate better common support for the covariates. The Kolmogorov-Smirnov test statistic cannot be computed for dummy variables which is why the K-S p-value columns for conflict intensity and nonstate conflict are empty.

5 Alternative Explanations: UN Peacekeeping and Ethnic Fractionalization

Table 5. Power-Sharing, Foreign Aid and Post-Conflict Democratic Development: Alternative Explanations

	Democracy (Polity)		Election Quality		Rule of Law		Public Goods	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Power-Sharing (binary)	−0.38		−0.13		0.15***		0.83***	
	(0.62)		(0.18)		(0.04)		(0.28)	
Power-Sharing (No. of seats)		−0.07		−0.05*		0.03***		0.09
		(0.12)		(0.03)		(0.01)		(0.06)
Aid / GDP (log)	0.05	0.08	−0.03	−0.03	−0.02*	−0.02*	−0.02	−0.01
	(0.18)	(0.18)	(0.02)	(0.02)	(0.01)	(0.01)	(0.07)	(0.08)
Power-Sharing (binary) * Aid	0.61**		0.11		−0.05***		−0.24**	
	(0.25)		(0.07)		(0.01)		(0.12)	
Power-Sharing (No. of seats) * Aid		0.07*		0.02**		−0.01***		−0.03*
		(0.04)		(0.01)		(0.00)		(0.02)
GDP p/c (log)	−0.02	−0.01	−0.03	−0.03	−0.02	−0.02	−0.38***	−0.37***
	(0.30)	(0.30)	(0.03)	(0.04)	(0.02)	(0.02)	(0.12)	(0.13)
Population (log)	0.15	0.11	0.03	0.03	−0.02	−0.02*	0.03	0.03
	(0.13)	(0.13)	(0.02)	(0.02)	(0.01)	(0.01)	(0.10)	(0.10)
Conflict Intensity	0.01	−0.08	−0.16**	−0.14	0.03	0.02	0.18	0.16
	(0.44)	(0.44)	(0.08)	(0.09)	(0.03)	(0.03)	(0.22)	(0.22)
Non-State Violence	−0.78	−0.87	−0.29***	−0.30***	0.03	0.03	−0.91**	−0.90**
	(0.55)	(0.56)	(0.05)	(0.05)	(0.03)	(0.03)	(0.44)	(0.45)
Nat. Res. Rents	−0.03*	−0.03**	−0.01***	−0.01***	−0.00**	−0.00**	−0.00	−0.00
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Regime Type	0.82***	0.82***	0.11***	0.10***	0.10***	0.10***	0.39***	0.38***
	(0.07)	(0.07)	(0.02)	(0.02)	(0.01)	(0.01)	(0.10)	(0.10)
Ethnic Frac.	0.15	0.46	0.27***	0.34***	−0.10	−0.11	0.15	0.23
	(0.80)	(0.77)	(0.09)	(0.09)	(0.07)	(0.07)	(0.58)	(0.57)
UN PKO	0.65	0.55	0.14**	0.11	−0.00	−0.01	−0.29	−0.29
	(0.54)	(0.55)	(0.07)	(0.07)	(0.03)	(0.03)	(0.25)	(0.25)
Countries	46	46	41	41	46	46	46	46
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Num. obs.	263	263	141	141	272	272	273	273
Adj. R^2 (full model)	0.77	0.76	0.66	0.65	0.60	0.60	0.34	0.33

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: OLS results with robust standard errors clustered on country reported in brackets. In Models 1, 3, and 4 the unit of observation is the post-conflic country-year. In Model 2, the unit of observation is the post-conflict election round. Dependent variable (DV) Model 1: Polity 2 scores; DV Model 2: NELDA Election Quality Score, higher values indicate cleaner elections. DV Model 3 DV: Linzer and Staton Judicial Independence Score, higher values indicate stronger rule of law; DV Model 4: V-Dem's Public vs. Particularistic Spending score, higher values indicate higher spending on public goods (see text for references and further details on codings).

- Data on UN peace operations is taken from Doyle and Sambanis (2006), updated by Hegre, Hultman, and Nygard (2011).
- Data on Ethnic Fractionalization comes from Alesina et al. (2003).

6 Probing the Aid Conditionality Assumption

Table 6. Probing Conditionality Assumption

	(1) Aid Cut-Offs	(2) Chinese Aid
Aid Cut-Off	0.05	
	(0.10)	
Chinese Aid / GDP (log)		0.39
		(11.11)
Power-Sharing (No. of seats)	0.03**	0.07
	(0.01)	(0.11)
Power-Sharing (No. of seats) * Aid Cut-Off	0.16**	
	(0.06)	
Power-Sharing (No. of seats) * Chinese Aid		4.66
		(5.18)
Aid / GDP (log)	−0.03	0.58
	(0.02)	(0.37)
GDP p/c (log)	−0.03	−0.66
	(0.04)	(0.67)
Population (log)	0.02	0.72***
	(0.02)	(0.25)
Conflict Intensity	−0.10	−1.21
	(0.10)	(0.76)
Non-State Violence	−0.25***	−1.50*
	(0.06)	(0.77)
Nat. Res. Rents	−0.01***	−0.02
	(0.00)	(0.02)
Regime Type	0.10***	0.53***
	(0.02)	(0.14)
Countries	41	46
Year FE	Yes	Yes
Num. obs.	141	115
Adj. R^2 (full model)	0.53	0.54

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: OLS results with robust standard errors clustered on country reported in brackets. Dependent variable in Model 1 is the V-Dem Clean Election Index; in Model 2: Polity 2 scores. Unit of observation is the election round in Model 1 and country-year in Model 2.

7 Additional Robustness Checks

7.1 Alternative Dependent Variables

The following variables are used in the robustness checks with alternative dependent variables (Table 8):

- *Unified Democracy Scores*. The unified democracy scores represent an aggregation of 10 existing democracy indices. It theoretically ranges from negative infinity to positive infinity, with most values between -2.5 (not democratic) and +2.5 (very democratic). See Pemstein, Meserve, and Melton (2010) for a detailed description.
- *XPOLITY*. Employs the violence-corrected XPOLITY scores by Vreeland (2008) to account for biased coding in the Polity scores.
- *Interregnum Correction*. Employs the corrected Polity scores by Plümper and Neumayer (2010) that account for potential bias in Polity during periods of transition.
- *NELDA Election Quality Indicator*. I use information on different elections in the NELDA dataset to construct an alternative index for election quality. Table 7 compares the NELDA and the V-Dem election quality indicators on how well they each capture the concept of the election cycle (Bishop and Hoeffler 2016; Norris 2015). The table lists each variable, together with the question the original NELDA data collectors asked in determining the variables' value. The NELDA variables are recorded as dummy variables,with a "1" indicating a "yes"-answer to the question. I add all dummies to create a 10-point (0-9) scaled ordinal variable. I invert the order of the scale so that higher values indicate cleaner elections, matching the coding direction of the V-Dem indicator.
- *Judicial Constraints (V-Dem)*. V-Dem's *v2x_jucon* variable measures judicial constraints on the executive. It represents the aggregated expert opinion of answers to the question "To what extent does the executive respect the constitution and comply with court rulings, and to what extent is the judiciary able to act in an independent fashion?" The measure is rescaled to an interval scaled variable. See Coppedge et al. (2015).
- *Political Corruption (V-Dem)*. V-Dem's *v2x_corr* variable aggregates expert opinion on the question "how pervasive is corruption?" in a given year. The index is the average of the indicator public sector corruption index (v2x_pubcorr); executive corruption index (v2x_execorr); the indicator for legislative corruption (v2lgcrrpt); and the indicator for judicial corruption (v2jucorrdc) and transformed to an interval scaled variable. See Coppedge et al. (2015).

Table 7. Election Quality: Components and Indicators

Electoral quality component	NELDA	V-Dem Clean Elections Index
Legal Framework	*nelda4* ("Was more than one party legal?")	-
Electoral Management Bodies (EMB)	-	*v2elembaut* ("Does the Election Management Body (EMB) have autonomy from government to apply election laws and administrative rules impartially in national elections?"); *v2elembcap* ("Does the EMB have sufficient staff and resources to administer a well-run national election?")
Electoral Rights	*nelda11* ("Were there concerns that elections were not free and fair?"); *nelda33* ("Was there significant violence involving civilian deaths immediately before, during, or after the election?")	*v2elpeace* ("In this national election, was the campaign period, election day, and post-election process free from other types of violence related to the conduct of the election and the campaigns (but not conducted by the government and its agents)?") *v2elfrfair* ("Taking all aspects of the pre-election period, election day, and the post-election process into account, would you consider this national election to be free and fair?")
Voter Register	-	*v2elrgstry* ("In this national election, was there a reasonably accurate voter registry in place and was it used?")
Ballot Access	*nelda13* ("Were opposition leaders prevented from running?")	*v2elfrfair*
Campaign Process	*nelda15* ("Was there opposition harassment?")	*v2elvotbuy* (" this national election, was there evidence of vote and/or turnout buying?"); *v2elintim*
Media Access	*nelda16* ("In the run-up to the election, were there allegations of media bias in favor of the incumbent?")	-
Voting Process	*nelda29* ("Were there riots and protests before or after the election?");	*v2elirreg* ("In this national election, was there evidence of other intentional irregularities by incumbent and/or opposition parties, and/or vote fraud?"); *v2elpeace*
Role of Officials	*nelda32* ("Were results that did not favor the incumbent canceled?")	*v2elirreg*
Counting of Votes	*nelda47* ("Were there allegations of vote fraud (by Western observers)?")	*v2elirreg*
Indicator	10-point ordinal scale (0-9)	Interval Composite Index (0-1)

Table 8. Power-Sharing, Foreign Aid and Post-Conflict Democratic Development: Technical Robustness Checks

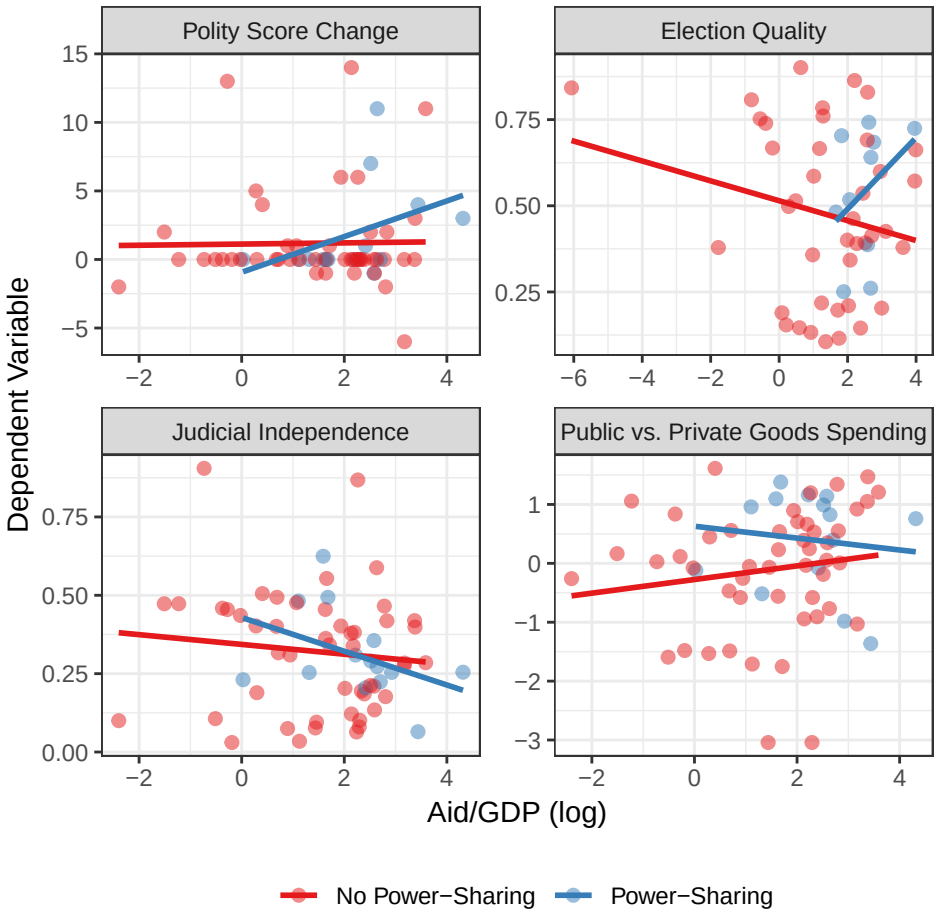
	Democracy (UDS)		Democracy (XPOLITY)		Democracy (Interreg.)		Election Quality (NELDA)		Rule of Law (V-Dem)		Public Goods (V-Dem/Corruption)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constituent Terms												
Power-Sharing (binary)	−0.02		−0.26		−0.19		−1.91		0.27***		0.02	
	(0.06)		(0.33)		(0.51)		(1.39)		(0.10)		(0.07)	
Power-Sharing (No. of seats)		−0.01		−0.02		−0.00		−0.02		0.05***		0.01
		(0.01)		(0.05)		(0.08)		(0.16)		(0.02)		(0.02)
Aid / GDP (log)	−0.00	0.00	0.05	0.06	0.05	0.07	−0.22	−0.17	−0.02**	−0.02**	−0.00	−0.00
	(0.02)	(0.02)	(0.13)	(0.13)	(0.18)	(0.18)	(0.15)	(0.16)	(0.01)	(0.01)	(0.01)	(0.01)
Interaction Term												
Power-Sharing (binary) * Aid	0.03		0.35**		0.50**		1.01*		−0.06		−0.01	
	(0.03)		(0.14)		(0.22)		(0.57)		(0.04)		(0.02)	
Power-Sharing (No. of seats) * Aid		0.01*		0.04		0.05*		0.04		−0.01**		−0.00
		(0.00)		(0.02)		(0.03)		(0.05)		(0.00)		(0.00)
Controls												
GDP p/c (log)	−0.00	−0.00	−0.05	−0.04	−0.10	−0.08	−0.10	−0.08	−0.06*	−0.05	−0.03	−0.03
	(0.03)	(0.02)	(0.25)	(0.25)	(0.33)	(0.34)	(0.27)	(0.29)	(0.03)	(0.03)	(0.02)	(0.02)
Population (log)	0.02	0.02	0.09	0.06	0.08	0.04	0.14	0.14	0.01	0.00	0.02	0.02
	(0.01)	(0.01)	(0.10)	(0.10)	(0.12)	(0.12)	(0.20)	(0.19)	(0.02)	(0.02)	(0.01)	(0.01)
Conflict Intensity	0.03	0.02	0.03	−0.01	0.14	0.07	−0.27	−0.42	0.00	−0.01	−0.04	−0.04
	(0.03)	(0.03)	(0.40)	(0.40)	(0.53)	(0.53)	(0.68)	(0.75)	(0.05)	(0.05)	(0.04)	(0.04)
Non-State Violence	−0.13***	−0.13***	−0.41	−0.43	−0.34	−0.37	−2.31**	−2.33**	0.00	0.01	0.03	0.03
	(0.04)	(0.04)	(0.47)	(0.48)	(0.57)	(0.59)	(0.92)	(0.89)	(0.05)	(0.05)	(0.03)	(0.03)
Nat. Res. Rents	−0.00	−0.00*	−0.02	−0.02	−0.03	−0.03	−0.01	−0.01	−0.00	−0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
UDS	0.87***	0.86***										
	(0.05)	(0.05)										
XPOLITY			0.82***	0.81***								
			(0.06)	(0.07)								
Polity2 (Interregnum)					0.82***	0.82***						
					(0.07)	(0.07)						
Regime Type							0.30**	0.25*	0.14***	0.13***	−0.06***	−0.06***
							(0.14)	(0.14)	(0.01)	(0.01)	(0.01)	(0.01)
Num. obs.	273	273	254	254	254	254	142	142	273	273	273	273
Adj. R^2 (full model)	0.84	0.84	0.76	0.76	0.78	0.78	0.40	0.37	0.55	0.55	0.40	0.40

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: OLS results with robust standard errors clustered on country reported in brackets.

7.2 Cross-Section

Figure 4. Power-Sharing, Foreign Aid, and Democracy + Components: Cross-Sectional Variation

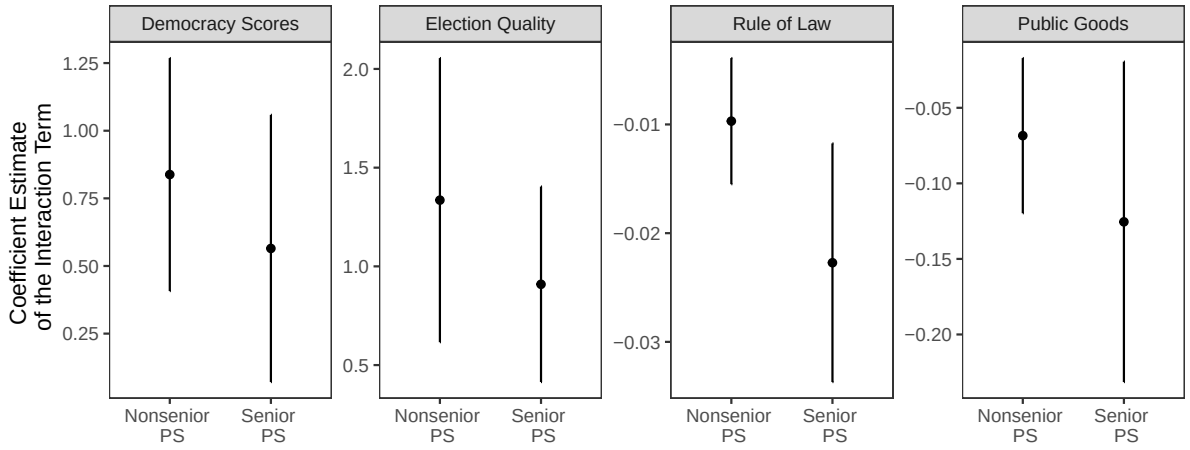


7.3 Variation by Type of Cabinet-Level Power-Sharing

To illustrate how different types of cabinet-level power-sharing generate differential access to state resources (and thus differently drive the rent-seeking dilemma), I use the PSED data to code whether rebels held senior-level positions (e.g. vice presidents, foreign minister, defense minister) as opposed to nonsenior position (e.g. tourism ministry). If my argument about rent-seeking induced by power-sharing is correct, the positive effect on democracy should be less pronounced (Polity scores and Election quality) while the negative effects should be stronger (limits on the rule of law and particularistic spending) in cases with senior-level power-sharing, since these position enable more direct access to state resources.

Figure 5 provides evidence in support of this expectation. The plot shows the coefficient of the interaction term between *Power-Sharing* and *Aid* as in the main results table in the manuscript, but replaces the *Powe-Sharing* variable with *Senior* and *Nonsenior-Power-Sharing* instead. The coefficient with the senior-level power-sharing variable is less positive in the case of the Polity score and election quality and more negative in the case of rule of law and particularistic spending.

Figure 5. Power-Sharing: Senior vs. Nonsenior Cabinet Positions



7.4 Logged vs. Not Logged Aid

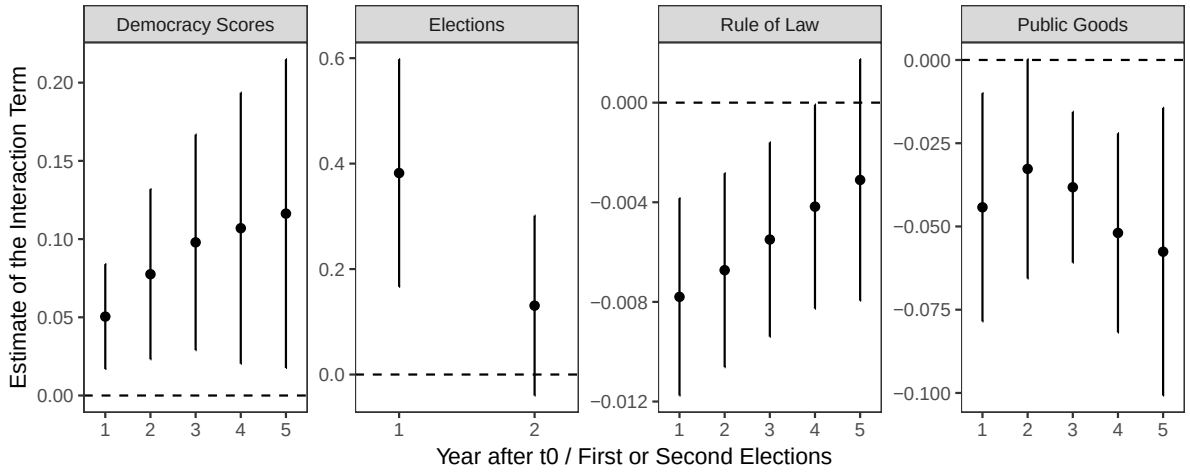
Table 9. Logged vs. Not Logged Aid

	(1) Aid logged	(2) Aid not logged
Power-Sharing (binary)	−0.43	0.41
	(0.53)	(0.43)
Power-Sharing (binary) * Aid	0.66***	
	(0.22)	
Power-Sharing (binary) * Aid / GDP (not log-transformed)		0.04**
		(0.02)
Aid / GDP (log)	0.06	
	(0.17)	
Aid / GDP (not log-transformed)		0.02*
		(0.01)
GDP p/c (log)	−0.04	−0.01
	(0.32)	(0.29)
Population (log)	0.14	0.14
	(0.14)	(0.13)
Conflict Intensity	0.13	0.01
	(0.46)	(0.42)
Non-State Violence	−0.76	−0.69
	(0.55)	(0.54)
Nat. Res. Rents	−0.03	−0.03**
	(0.02)	(0.02)
Regime Type	0.82***	0.82***
	(0.07)	(0.07)
Countries	46	46
Year FE	Yes	Yes
Num. obs.	263	263
Adj. R^2 (full model)	0.77	0.77
Adj. R^2 (proj model)	0.75	0.75

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

7.5 Effects over Time

Figure 6. Effects over Time



Note: The plot displays the coefficient of the interaction term with different temporal leads of the dependent variable.

7.6 Decade Fixed Effects

Table 10. Decade Fixed Effects

	Polity	Election Quality	Jud. Independence	Part. Spending
Power-Sharing (binary)	−0.84	−0.24**	0.12***	0.87***
	(0.61)	(0.12)	(0.04)	(0.29)
Aid / GDP (log)	0.03	−0.02	−0.02**	−0.03
	(0.17)	(0.02)	(0.01)	(0.07)
Power-Sharing (binary) * Aid	0.75***	0.18***	−0.05***	−0.26**
	(0.24)	(0.04)	(0.01)	(0.13)
GDP p/c (log)	−0.02	0.01	−0.01	−0.39***
	(0.32)	(0.02)	(0.02)	(0.11)
Population (log)	0.05	0.03*	−0.02*	0.05
	(0.11)	(0.02)	(0.01)	(0.09)
Conflict Intensity	0.23	0.02	0.02	0.12
	(0.46)	(0.06)	(0.04)	(0.21)
Non-State Violence	−0.42	−0.24***	0.01	−0.91**
	(0.51)	(0.06)	(0.04)	(0.42)
Nat. Res. Rents	−0.03*	−0.01***	−0.00**	−0.00
	(0.02)	(0.00)	(0.00)	(0.01)
Regime Type	0.82***	0.09***	0.10***	0.40***
	(0.07)	(0.02)	(0.01)	(0.08)
Countries	46	41	46	46
Decade FE	Yes	Yes	Yes	Yes
Num. obs.	263	141	272	273
Adj. R^2 (full model)	0.76	0.53	0.58	0.37
Adj. R^2 (proj model)	0.76	0.52	0.57	0.37

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

7.7 Election Promise

Table 11. Election Promise

	Polity	Election Quality	Jud. Independence	Part. Spending
Power-Sharing (binary)	−0.55	−0.23**	0.14***	0.87***
	(0.62)	(0.11)	(0.04)	(0.31)
Aid / GDP (log)	0.07	−0.04*	−0.02*	−0.03
	(0.17)	(0.02)	(0.01)	(0.07)
Power-Sharing (binary) * Aid	0.62**	0.16***	−0.06***	−0.27**
	(0.25)	(0.04)	(0.01)	(0.13)
GDP p/c (log)	0.13	−0.00	−0.01	−0.38***
	(0.34)	(0.03)	(0.03)	(0.13)
Population (log)	0.15	0.03	−0.02*	0.04
	(0.13)	(0.02)	(0.01)	(0.10)
Conflict Intensity	−0.15	−0.13	0.02	0.09
	(0.44)	(0.08)	(0.04)	(0.26)
Non-State Violence	−0.75	−0.23***	0.02	−0.91**
	(0.52)	(0.06)	(0.04)	(0.42)
Nat. Res. Rents	−0.04**	−0.01***	−0.00***	−0.00
	(0.01)	(0.00)	(0.00)	(0.01)
Regime Type	0.81***	0.10***	0.10***	0.41***
	(0.07)	(0.02)	(0.01)	(0.09)
Election Promise in PA	0.50**	0.09***	0.01	0.05
	(0.20)	(0.02)	(0.02)	(0.16)
Countries	46	41	46	46
Decade FE	Yes	Yes	Yes	Yes
Num. obs.	263	141	272	273
Adj. R^2 (full model)	0.77	0.65	0.59	0.33
Adj. R^2 (proj model)	0.76	0.54	0.56	0.32

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

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