Supplementary Material

Autocratic Elections: Stabilizing Tool or Force for Change?

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A Overview

This online appendix describes and presents descriptive statistics and various robustness tests that are not reported in tables in the paper. It also includes a number of extensions that expand on our main result. This main result can be summarized as follows: Elections make autocratic regimes more likely to break down in the short term, but not in the long term; many specifications even suggest that elections go together with increased autocratic regime stability in the long term. The additional tests reported below clearly show that the short-term result is very robust. Further, the extensions presented at the end of this appendix also indicate that the mechanisms discussed in the paper are likely to be those generating this observed relationship. Importantly, the result is not changed much when accounting for the various plausible alternative explanations that we identify. When taken together, elections seem to destabilize autocratic regimes in the short term because they serve as focal points for anti-regime collective action, but they might stabilize such regimes by allowing them to improve their capacities for co-optation and repression in the longer term.

In Section A.1, we present descriptive statistics for the variables used in our core models, including bivariate correlations and histograms detailing the distribution of autocratic regimes for instance according to the timing of breakdown after elections are held. This section also includes the list of the regime breakdowns – as coded by Geddes, Wright and Frantz (2014) – included in our analysis.

In Section A.2, we report salient robustness tests that are only mentioned and not reported in tables or figures in the paper. This section is further divided into sub-sections:

First, we report models incorporating different types of autocratic elections and models designed to assess multi-collinearity issues. *Second*, we test models with alternative (plausible) sets of control variables, for example models that include the Geddes et al. autocratic regime type dummies as regressors.

Third, we report a number of models that make different assumptions when specifying the shape of short- and long-term effects, including models using differently specified decay functions (and other functions) to capture different operationalizations of what constitutes "short-term" and "long-term" effects as well as Generalized Additive Models (GAMs). *Fourth*, we report models that exclude particular sets of observations that may unduly influence the results, or at least our interpretation of these results. *Fifth*, we investigate how the results hold up in Cox proportional hazard survival models that do not depend on the same assumptions about functional form (as our baseline logit models) and duration dependence.

In Section A.3, we conduct another set of tests dealing with the repeated nature of elections and related potential challenges with capturing their long-term effects. We propose a set of model specifications that, in alternative ways, address this issue, for instance modelling the cumulative number of elections held under a particular regime.

In Section A.4, we evaluate the models that center on whether our correlations reflect causal effects by treating elections as endogenous. This section contains the second-stage results for IV-probit models treating the long-term variables as endogenous, and the first-stage results for all the main IV probit models. The section also shows results for alternative IV-probit, 2SLS and biprobit specifications briefly mentioned, but not reported in tables, in the paper.

In Section A.5, we further discuss the potential endogeneity issues, and the assumptions underlying our IV models. We follow up by conducting causal sensitivity tests to investigate how extensive endogeneity issues must be for our main results to collapse.

In Section A.6, we investigate whether plausible alternative mechanisms (to those we propose in the paper) contribute strongly to the observed relationship between elections and autocratic regime breakdown. We mainly do so by investigating whether the results are affected by excluding/including varying sets of cases where these alternative mechanisms are most likely to be at work. To name one example, we test models excluding observations where leaders stepped down prior to/did not run in the autocratic elections. To name a second example, we investigate what happens to our results when excluding all regime breakdowns that subsequently spurred democratization.

A.1 Descriptive statistics

Table A.1 reports descriptive statistics for the variables entering our baseline models. The table shows these statistics for the aggregated sample of 3893 observations in Model 1, Table 1 of the paper, but also breaks down the statistics for the observations that do and do not experience regime breakdown. One of the most notable difference between these two groups of observations, for our purpose, is the large share of regime failure observations that occurred in election years (35%) versus the much lower

share for autocratic country-years that did not experience regime breakdown (8%). The table also, for example, shows that regime breakdown country-years have much lower average GDP per capita growth (-0.13 versus 1.72) and are less dependent on natural resources (4.36 versus 7.35) than country-years without breakdowns.

Table A.2 reports the pairwise correlation coefficients between this set of variables for the same 3893 observations. One concern with our tests would be that our decay functions proxying for the short-term and long-term effects of elections in the baseline models would be strongly correlated, thus producing very sensitive results. However, the correlation between ElecShortTerm and ElecLongterm is not too high, at 0.73 (we discuss models addressing potential issues of multi-collinearity more in the next section). The correlation between having an election year and having had elections in the past 5 years is even lower (0.38), allowing for the separation of short-term and long-term effects.

Table A.3 lists all the 199 regime breakdown observations in the baseline model, as well as 22 additional breakdowns from a parsimonious model without controls at the bottom of the table, alphabetically sorted after country name. While some countries, such as Spain and Somalia, only have experienced one autocratic breakdown in this sample, which extends back to 1946, others have observed multiple breakdowns, with Bolivia experiencing the highest number (8) of breakdowns. The table also displays the number of years that have passed since the last executive election (which we use for our baseline models) and years that have passed since the last election of any kind registered in the NELDA dataset (which we employ for robustness tests).

Figure A.1 displays a time series plot with number of regime breakdowns, by year, over the entire sample of autocratic regimes in Model 1, Table 1. While there is substantial year-by-year variation, there is no clear, discernible trend from the plot, although there were a large number of breakdowns around the end of the Cold War. A "typical year" in the post-WWII period contained between 1 and 5 breakdowns, with the minimum number of autocratic regime breakdowns being 1 (happened 15 times) and the maximum being 11 (happened twice).

Figure A.2 shows a histogram with the frequency for autocratic regime observations along the y-axis, and how many years have passed since the last executive election along the x-axis. Figure A.3 shows an equivalent histogram, but only for the observations experiencing regime breakdown. The density of both distributions are clearly left-skewed, with a long right tail, but the two distributions differ markedly in one aspect: While there is a more gradual drop in the frequency when moving along the x-axis for the former histogram, the latter provides a very steep drop from the election year to the following year(s). This is in line with our expectation that it is the immediate aftermath of elections that tends to be the most conducive to autocratic regime breakdown. Figures A.4 and A.5 show similar histograms when we group observations after the number of years that have passed since the last election of any kind (as

registered in NELDA). While there are naturally more observations close in time to elections, in general, for these figures, comparing them shows the same pattern as that described above: The distribution for regime breakdowns is clearly more left-skewed than the distribution for all observations. In particular, the share of all observations that are election years is 21.7 percent, whereas the share of regime breakdowns occurring in election years is 49.8 percent.

Table A.1: Descriptive statistics for variables entering baseline models

Variable	Mean	Standard deviation	Minimum	Maximum
		All observations (n=3893)		
Regime failure	0.05	0.22	0	1
ElecShortTerm	0.18	0.31	0	1
ElecLongTerm	0.51	0.35	0	1
Election year	0.10	0.30	0	1
Election past 5 years	0.43	0.49	0	1
Region polity	0.36	0.17	0.12	0.97
Ln GDP per capita	7.77	0.96	5.79	11.08
GDP per capita growth	1.62	8.10	-68.94	151.06
Military size	0.79	0.95	0	7.69
Resource dependence	7.20	13.91	0	100
Duration	19.83	26.80	1	263
Duration^2	1111.26	5728.01	1	69169
Duration^3	157832.40	1410158	1	18200000
		Only obs. without regime failure (n=3694)		
Regime failure	0	0.00	0	0
ElecShortTerm	0.17	0.29	0	1
ElecLongTerm	0.50	0.35	0	1
Election year	0.08	0.28	0	1
Election past 5 years	0.41	0.49	0	1
Region polity	0.36	0.17	0.12	0.95
Ln GDP per capita	7.78	0.97	5.79	11.08
GDP per capita growth	1.72	7.77	-68.94	151.06
Military size	0.81	0.96	0	7.69
Resource dependence	7.35	13.99	0	100
Duration 20.09	27.20	1	263	
Duration^2	1143.26	5870.84	1	69169
Duration^3	164820.90	1447081.00	1	18200000
		Only obs. with regime failure (n=199)		
Regime failure	1	0.00	1	1
ElecShortTerm	0.40	0.45	0	1
ElecLongTerm	0.67	0.35	0.01	1
Election year	0.35	0.48	0	1
Election past 5 years	0.62	0.49	0	1
Region polity	0.44	0.16	0.18	0.97
Ln GDP per capita	7.56	0.84	5.84	9.86
GDP per capita growth	-0.13	12.56	-47.41	107.53
Military size	0.50	0.57	0	3.46
Resource dependence	4.36	11.92	0	100
Duration	14.87	17.25	1	105
Duration^2	517.18	1304.74	1	11025
Duration^3	28106.99	114332.50	1	1157625

Note: The statistics are reported for the country-year observations entering Model 1, Table 1 in the paper.

Table A.2: Bivariate correlation coefficients for variables entering baseline models

	Reg. fail.	ElST	ElLT	El. yr	El. past 5	Reg. Pol.	Ln GDP pc	GDP pc gr.	Mil. size	Res. dep.	Dur.	Dur.^{2}	Dur.^{3}
Regime failure	1												
ElecShortTerm	0.17	1											
ElecLongTerm	0.11	0.73	1										
Election year	0.20	0.88	0.47	1									
Election past 5 years	0.09	0.63	0.84	0.38	1								
Region Polity	0.11	0.14	0.17	0.10	0.19	1							
Ln GDP p.c.	-0.05	-0.06	-0.18	-0.02	-0.08	0.07	1						
GDP p.c. growth	-0.05	-0.01	-0.01	0.00	-0.03	0.01	0.09	1					
Military size	-0.07	-0.19	-0.31	-0.11	-0.28	-0.11	0.23	0.05	1				
Resource dependence	-0.05	-0.06	-0.12	-0.04	-0.09	-0.23	0.32	-0.01	0.23	1			
Duration	-0.04	-0.12	-0.30	-0.06	-0.14	-0.06	0.30	0.00	0.12	0.20	1		
Duration^2	-0.02	-0.07	-0.18	-0.04	-0.10	-0.07	0.20	0.00	0.07	0.20	0.88	1	
Duration^3	-0.02	-0.06	-0.14	-0.03	-0.09	-0.07	0.17	0.00	0.07	0.19	0.82	0.99	1

Note: The coefficients are pairwise correlation coefficients, calculated for for the 3893 observations entering Model 1, Table 1 in the paper.

Table A.3: List of all regime failure observations in baseline model (199; top) and additional regime failure observations in model without covariates (30; bottom), with number of years since last executive election followed by year since last election (of any kind) in parenthesis

Afghanistan–1973 (34; 4)	Afghanistan–1978 (39; 9)	Afghanistan–1992 (53; 4)
Albania–1991 (52; 0)	Algeria–1992 (4; 1)	Argentina–1946 (0; 0)
Argentina–1955 (1; 1)	Argentina–1958 (0; 0)	Argentina–1966 (3; 1)
Argentina–1973 (0; 0)	Argentina–1983 (0; 0)	Armenia–1998 (0; 0)
Azerbaijan–1992 (0; 0)	Bangladesh–1975 (4; 2)	Bangladesh–1982 (1; 1)
Bangladesh–1990 (4; 2)	Belarus–1994 (0; 0)	Benin–1963 (3; 3)
Benin–1965 (1; 1)	Benin–1967 (3; 3)	Benin–1969 (1; 1)
Benin–1970 (0; 0)	Benin–1990 (20; 1)	Bolivia–1946 (7; 7)
Bolivia–1951 (0; 0)	Bolivia–1952 (1; 1)	Bolivia–1964 (0; 0)
Bolivia–1969 (3; 3)	Bolivia–1971 (5; 5)	Bolivia–1979 (0; 0)
Bolivia–1982 (2; 2)	Brazil–1985 (25; 3)	Bulgaria–1990 (44; 0)
Burkina Faso–1966 (1; 1)	Burkina Faso–1980 (2; 2)	Burkina Faso–1982 (4; 4)
Burkina Faso–1987 (9; 9)	Burundi–1966 (4; 1)	Burundi–1987 (3; 3)
Burundi–1993 (0; 0)	Burundi–2003 (10; 10)	Cambodia–1970 (17; 4)
Cambodia–1975 (3; 2)	Cambodia–1979 (7; 3)	Cameroon–1983 (3; 0)
Cen African Rep–1965 (1; 1)	Cen African Rep–1979 (15; 15)	Cen African Rep–1981 (0; 0)
Cen African Rep–1993 (0; 0)	Chad–1975 (6; 6)	Chad–1979 (10; 10)
Chad–1990 (21; 0)	Chile–1989 (0; 0)	Colombia–1953 (4; 0)
Colombia–1958 (0; 0)	Congo-Brz–1963 (2; 0)	Congo-Brz–1968 (7; 5)
Congo-Brz–1991 (30; 2)	Congo/Zaire–1997 (13; 10)	Costa Rica–1949 (0; 0)
Cuba–1959 (5; 1)	Czechoslovakia–1989 (50; 3)	Dominican Rep–1962 (0; 0)
Dominican Rep–1965 (3; 3)	Dominican Rep–1978 (0; 0)	Ecuador–1947 (8; 8)
Ecuador–1966 (6; 4)	Ecuador–1972 (4; 4)	Ecuador–1979 (0; 0)
Egypt–1952 (13; 2)	El Salvador–1948 (3; 3)	El Salvador–1982 (0; 0)
El Salvador–1994 (0; 0)	Ethiopia–1974 (35; 1)	Ethiopia–1991 (4; 4)
Gambia–1994 (2; 2)	Georgia–1992 (0; 0)	Georgia–2003 (3; 0)
Ghana–1966 (6; 6)	Ghana–1969 (9; 0)	Ghana–1979 (0; 0)
Ghana–2000 (0; 0)	Greece–1974 (35; 0)	Guatemala–1958 (0; 0)
Guatemala–1963 (5; 2)	Guatemala–1966 (0; 0)	Guatemala–1970 (0; 0)
Guatemala–1985 (0; 0)	Guatemala–1995 (0; 0)	Guinea–1984(2; 2)
Guinea Bissau–1980 (6; 6)	Guinea Bissau–1999 (0; 0)	Guinea Bissau–2003 (3; 3)
Haiti–1946 (0; 0)	Haiti–1956 (6; 6)	Haiti–1986 (0; 0)
Haiti–1988 (0; 0)	Haiti–1990 (0; 0)	Haiti–1994 (4; 3)
Honduras–1956 (0; 0)	Honduras–1971 (0; 0)	Honduras–1981 (0; 0)
Hungary–1990 (51; 0)	Indonesia–1966 (11; 11)	Indonesia–1999 (44; 0)
Iran–1979 (0; 0)	Iraq–1958 (19; 0)	Iraq–1963 (24; 5)
Iraq–1968 (29; 10)	Iraq–1979 (40; 21)	Iraq–2003 (8; 3)
Ivory Coast–1999 (4; 4)	Ivory Coast–2000 (0; 0)	Kenya–2002 (0; 0)
Korea South–1960 (0; 0)	Korea South–1987 (0; 0)	Laos–1960 (9; 0)
Laos–1962 (11; 2)	Lesotho–1986 (21; 16)	Lesotho–1993 (28; 0)
Liberia–1980 (5; 5)	Liberia–1990 (5; 5)	Liberia–2003 (6; 6)
Libya–1969 (18; 4)	Madagascar–1972 (0; 0)	Madagascar–1975 (3; 3)
Madagascar–1993 (0; 0)	Malawi–1994 (0; 0)	Mali–1968 (8; 4)
Mali–1991 (6; 3)	Mauritania–1978 (2; 2)	Mexico–2000 (0; 0)
Mongolia–1993 (0; 0)	Myanmar–1960 (12; 0)	Myanmar–1988 (40; 2)
Nepal–1951 (12; 12)	Nepal–1991 (52; 0)	Nicaragua–1979 (5; 5)
Nicaragua–1990 (0; 0)	Niger–1974 (4; 4)	Niger–1991 (2; 2)
Niger–1999 (0; 0)	Nigeria–1979 (0; 0)	Nigeria–1993 (0; 0)
Nigeria–1999 (0; 0)	Pakistan–1977 (30; 0)	Pakistan–1988 (41; 0)
Panama–1951 (3; 3)	Panama–1955 (3; 3)	Panama–1982 (2; 2)
Panama–1989 (0; 0)	Paraguay–1948 (0; 0)	Paraguay–1954 (0; 0)
Paraguay–1993 (0; 0)	Peru–1956 (0; 0)	Peru–1963 (0; 0)
Peru–1980 (0; 0)	Peru–2000 (0; 0)	Philippines–1986 (0; 0)
Poland–1989 (50; 0)	Portugal–1974 (16; 1)	Romania–1989 (41; 4)
Rwanda–1973 (4; 4)	Rwanda–1994 (6; 6)	Senegal–2000 (0; 0)
Sierra Leone–1968 (7; 1)	Sierra Leone–1992 (7; 6)	Sierra Leone–1996 (0; 0)
Sierra Leone–1998 (2; 2)	Somalia–1991 (5; 5)	South Africa–1994 (55; 0)
Spain–1976 (37; 37)	Sri Lanka–1994 (0; 0)	Sudan–1964 (8; 6)
Sudan–1985 (2; 2)	Sudan–1986 (3; 0)	Syria–1947 (8; 0)
Syria–1951 (2; 2)	Syria–1954 (1; 0)	Syria–1958 (5; 4)
Syria–1963 (10; 9)	Taiwan–2000 (0; 0)	Thailand–1947 (8; 1)
Thailand–1957 (18; 0)	Thailand–1973 (34; 4)	Thailand–1988 (49; 0)
Thailand–1992 (53; 0)	Togo–1963 (0; 0)	Turkey–1950 (11; 0)
Turkey–1960 (21; 3)	Turkey–1961 (22; 0)	Turkey–1983 (1; 0)
Uganda–1971 (9; 9)	Uganda–1979 (17; 17)	Uganda–1985 (23; 5)
Uruguay–1984 (0; 0)	Venezuela–1958 (0; 0)	Yugoslavia–1990 (45; 32)
Zambia–1991 (0; 0)		
Extra failures from model without covariates (Model 1, Table A.9)		
Afghanistan–2001 (62; 13)	Bangladesh–2008 (22; 0)	East Germany–1990 (51; 0)
Guinea–2008 (5; 5)	Haiti–2004 (4; 4)	Kyrgyzstan–2005 (0; 0)
Mauritania–2005 (2; 2)	Mauritania–2007 (0; 0)	Nepal–2006 (67; 7)
Pakistan–1958 (11; 11)	Pakistan–1971 (24; 1)	Pakistan–2008 (61; 0)
Serbia–2000 (0; 0)	Soviet Union–1991 (0; 0)	Thailand-2007 (68; 0)
South Vietnam–1963 (2; 0)	South Vietnam–1975 (4; 2)	Yemen–1962 (23; 23)
Yemen–1967 (28; 28)	Yemen–1974 (35; 3)	Yemen–1978 (39; 7)
South Yemen–1990 (23; 4)		

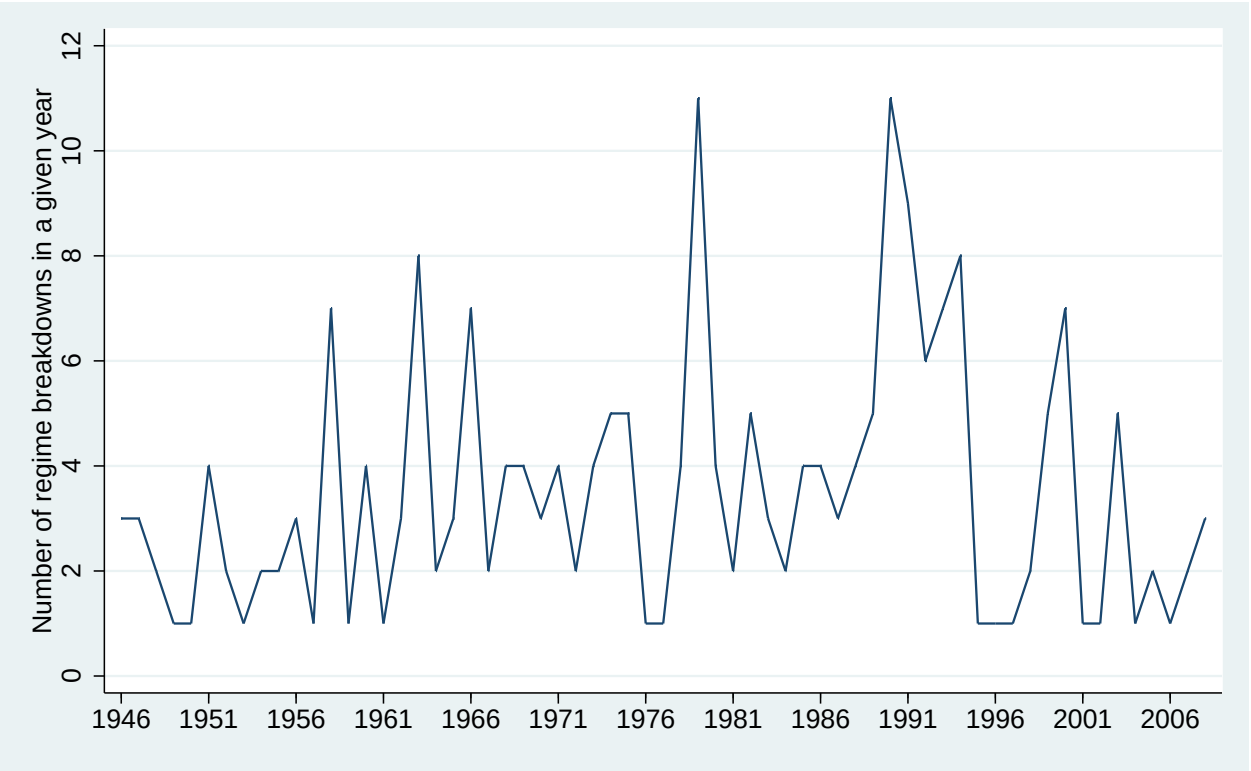


Figure A.1: Time series plot with number of autocratic regime breakdowns per year, for the sample of Model 1, Table 1.

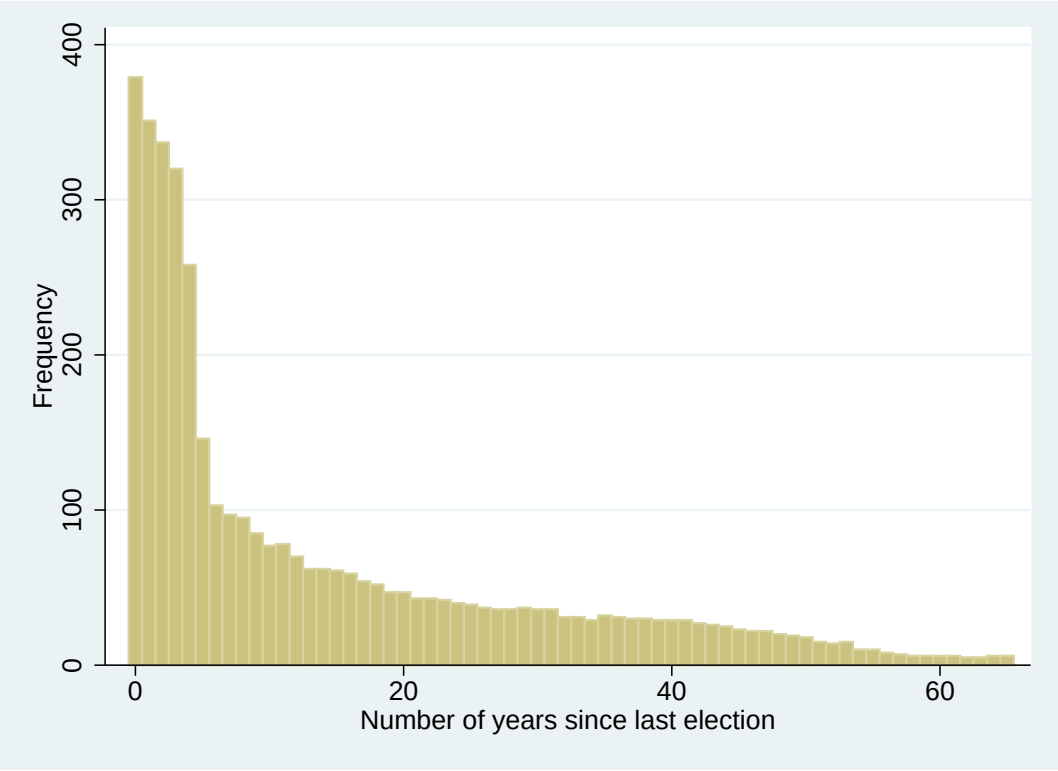


Figure A.2: Histogram with number of all autocratic regime observations (y-axis), from the sample of Model 1, Table 1, after how many years have passed since last executive election (x-axis).

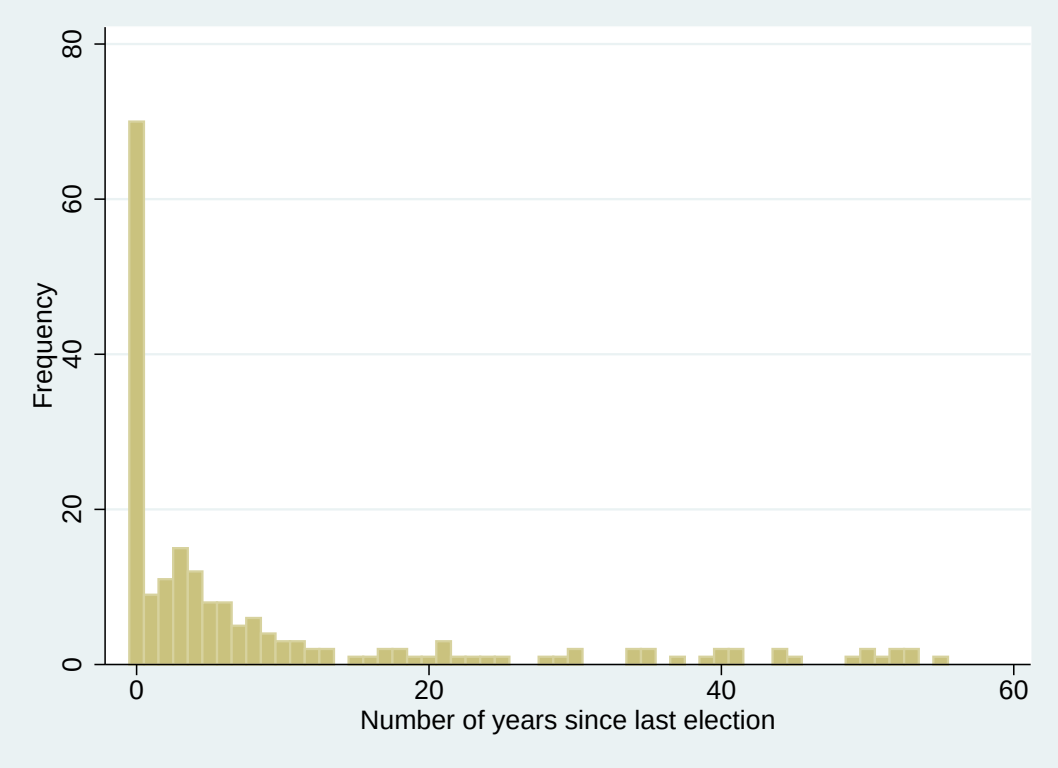


Figure A.3: Histogram with number of regime failures (y-axis), from the sample of Model 1, Table 1, after how many years have passed since last executive election (x-axis).

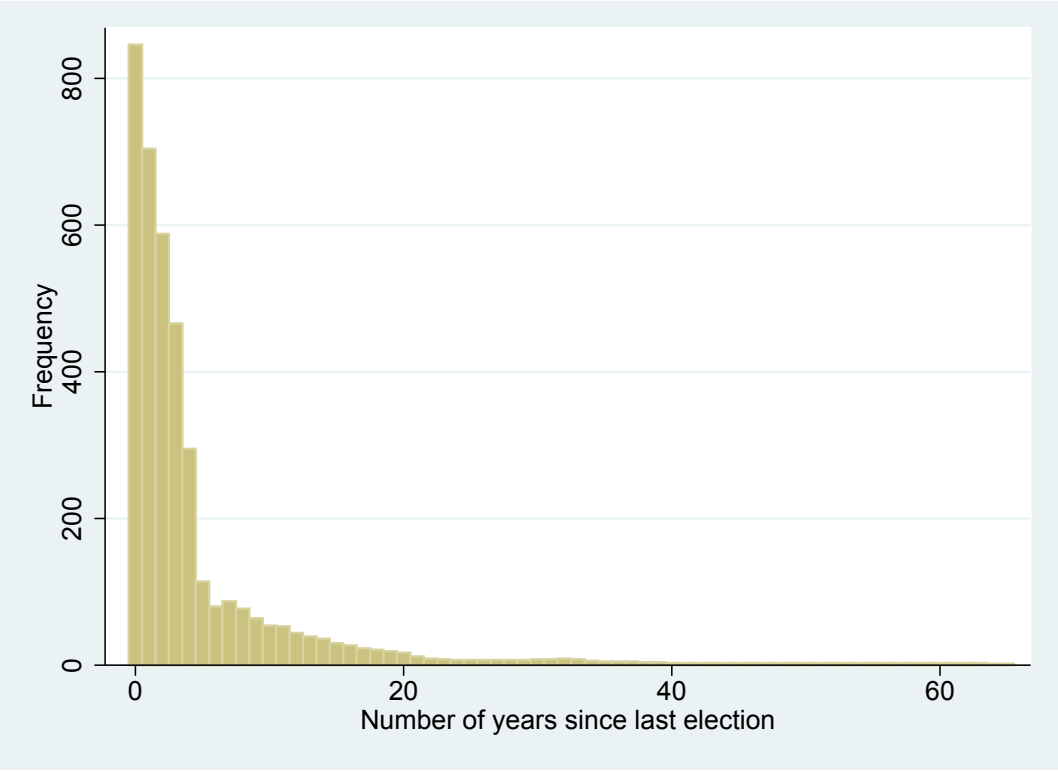


Figure A.4: Histogram with number of all autocratic regime observations (y-axis), from the sample of Model 1, Table 1, after how many years have passed since last election (of any kind; x-axis).

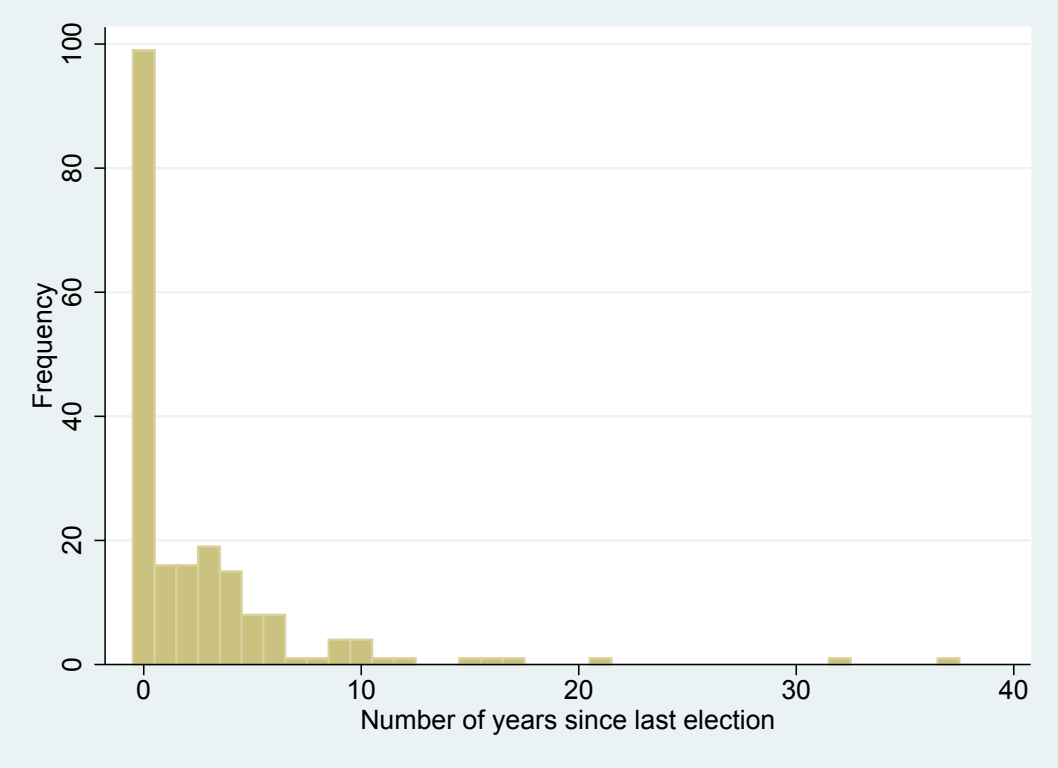


Figure A.5: Histogram with number of regime failures (y-axis), from the sample of Model 1, Table 1, after how many years have passed since last election (of any kind; x-axis).

A.2 Core robustness tests

This section investigates whether the main results presented in the paper hold when we make plausible changes to our specifications. For instance, we experiment with including additional relevant control variables, using different decay function specifications, and employing an alternative operationalization of regime change. We also test models that are more parsimonious, to see if our results are not artifacts of 'garbage can regression' problems such as highly correlated control variables inducing arbitrary sign flips (e.g., Achen, 2002), and check for outlier sensitivity and for the exclusion of particular cases. However, we start by investigating whether our results are sensitive to the inclusion of different types of elections.

A.2.1 Including/excluding different types of elections, and models assessing potential multi-collinearity issues

We primarily focus on executive elections in our core analysis. As we discuss in the paper, this is, in part, due to the expectation that executive elections more strongly carry our theorized mechanisms. However, this is also due to potential issues with including mid-term elections and separating the short-term and long-term effects when using our baseline modeling strategy. In order to properly separate the short-term from the long-term effect of elections, we need at least a handful of years to pass between elections. If elections are held every second year, it is naturally difficult to gauge the long-term effect by investigating the time that has passed since the last one. However, we return to different ways of measuring the long-term effect that alleviate this issue in Appendix Section A.3, and, before that, we also try out a number of alternative functional form specifications for the short- and long-term effects in Section A.2.3.

We initially expected models including also non-executive elections to yield less precise results, despite increasing the total number of elections. To check this, and investigate whether the main results hinge on the choice to exclude non-executive elections, we re-ran our models when including all NELDA coded elections. Table A.4 shows the replication of Table 1 of the paper when including all elections. However, including all elections, as it turns out, has little substantive effect on our overall findings – especially concerning the short-term effect – and typically alters the estimated effects and standard errors by only a little, although it should be noted that *ElecLongTerm* is weakened in Models 1 and 3, Table A.4. The fact that the results are somewhat weaker for the long term effect in Table A.4 is, however, possibly due to the above-discussed methodological issue; this specification makes it very hard to discern any long term effect of elections since they occur so frequently in many autocracies holding both executive and non-executive elections. Nevertheless, in the extensive Model 5 both the long- and short-term effects are significant at 1% and just as strong as those we present in the paper.

We noted in the paper that many of the regimes that hold executive elections also hold legislative elec-

tions simultaneously, meaning that part of our baseline estimates should reflect effects of non-executive elections as well. However, many regimes, such as a number of communist regimes and Suharto's Indonesia, do/did not hold executive elections but do/did hold legislative elections. In such regimes, the above-discussed methodological issue of numerous elections making it hard to identify any long-term effect is not as pertinent, and including non-executive elections is a defensible choice (although it introduces an asymmetry by including non-executive elections only for some countries). To make sure that our results do not hinge on the omission of non-executive elections in these regimes we therefore also tested models including them (only) for the regimes that do not hold executive elections (alongside executive elections for the regimes that do have such elections). These models are reported in Table A.5, and the results are by and large substantively identical to the baseline results presented in the paper. This suggests that the somewhat weaker results for the long-term effect presented in Table A.4 could mainly stem from the methodological issue of capturing long-term effects with this particular design in the presence of multiple elections that are held close to each other (in countries holding executive and non-executive elections in different years).

Further, Figure A.6 shows the results from a GAM model also addressing the issues discussed just above. It is identical to the GAM reported in the main paper (in Figure 2), but we here include legislative elections for those regimes that *only* hold such elections according to NELDA. Substantially we find very similar results, but, interestingly, the negative short term effect drops more quickly in this model – the GAM reaches the zero line about 1.5 years earlier than in the model reported in Figure 2 in the paper.

Next we further investigate the issue of multi-collinearity. The short- and long-term decay functions are quite strongly, though not very strongly, correlated (.73 in the baseline sample). This could potentially affect our results, although we note that, theoretically, collinearity does not induce biased coefficients, but rather increase the size of the standard errors.

While we discuss below why we deem these models highly problematic, Tables A.6 and A.7 contains a series of specifications where we alternate which of the 'time-since-election' terms we include. The tables includes models with our three baseline sets of controls, the first table using decay functions and the second using the dummy variable set up. Further, the first three models in each table only contain *ElecShortTerm*/Election year while the three last only contain *ElecLongTerm*/Election in the last 5 years. All the short-term coefficients are substantively identical to the results we report in the paper in terms of sign and significance, though they are somewhat smaller in size.

The three *ElecLongTerm* models, in contrast, are very different. These now report an increased risk of regime failure. At first glance this might appear to run counter to our main finding. However, there is a very plausible methodological explanation for this: The long-term models all now suffer from strong omitted variable bias (whereas the short-term models do not to the same extent, as the short-term

decay function and election year dummy do not capture effects other than right after the election), and this drives the change in sign for the long-term coefficients. Since we clearly demonstrate that holding an election increases the short-term risk of regime failure, this must be controlled for, otherwise the long-term coefficients will pick up both the short- and long-term effects: Remember that also the long-term decay function is at its strongest right after the election, it only dissipates more slowly than the short-term. (To be clear, this set-up is constructed so that it follows the theoretical argument: The mechanisms described as generating the long-term effect will expectedly also be initiated fairly quickly after the election, *but* what makes them "long-term" mechanisms is that they should linger on for quite a few years).

To remedy this, while at the same time alleviating concerns about collinearity, the models in Table A.8 therefore include the long-term decay function but now combined with the dummy variable marking election years. This leaves *ElecLongTerm* to capture the entire effect of the election outside of that associated with the election year. The correlation between these two variables is not very strong, .45. Indeed, in these models the long-term effect closely resembles that of our baseline models, being positively signed and statistically significant at least at the 5% level in two of the three models. We take this as evidence that multicollinearity is likely not what is driving the results of our baseline models.

Table A.4: Full Table 1 replicated when all types of autocratic elections are included

	Regime failure					
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to election / 1	1.620***		1.651***		2.328***	
	(0.311)		(0.317)		(0.385)	
Proximity to election / 8	−0.591		−0.864		−2.495***	
	(0.570)		(0.591)		(0.743)	
Election		1.259***		1.239***		1.473***
		(0.163)		(0.165)		(0.201)
Election 5 year		0.274		0.168		−0.487
		(0.238)		(0.242)		(0.305)
Region Polity	2.577***	2.589***	4.374***	4.376***	3.421***	3.398***
	(0.456)	(0.459)	(0.930)	(0.934)	(1.138)	(1.141)
ln(GDP per capita)	−0.267***	−0.278***	−0.563***	−0.569***	−0.759***	−0.768***
	(0.100)	(0.101)	(0.139)	(0.140)	(0.168)	(0.170)
GDP Growth	−0.028***	−0.027***	−0.023**	−0.023**	−0.001	−0.001
	(0.009)	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)
Military size	−0.311**	−0.298**	−0.247*	−0.244*	−0.327*	−0.343*
	(0.126)	(0.126)	(0.144)	(0.144)	(0.190)	(0.191)
Resource dependence	−0.005	−0.005	0.001	0.001	0.009	0.010
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Duration	−0.049***	−0.050***	−0.033**	−0.034**	−0.036*	−0.032*
	(0.015)	(0.015)	(0.016)	(0.016)	(0.019)	(0.019)
Duration^2	0.001**	0.001**	0.001*	0.001*	0.001*	0.001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2					2.446***	2.369***
					(0.350)	(0.350)
Constant	−1.512**	−1.799**	0.461	0.008	2.808*	1.834
	(0.757)	(0.701)	(1.171)	(1.136)	(1.445)	(1.406)
Region dummies		*Y*	*Y*	*Y*	*Y*	*Y*
Decade dummies		*Y*	*Y*	*Y*	*Y*	*Y*
Observations	3,893	3,893	3,893	3,893	3,710	3,710
Log Likelihood	-711.063	-700.727	-693.808	-684.057	-501.723	-495.351
Akaike Inf. Crit.	1,444.126	1,423.454	1,433.616	1,414.114	1,051.447	1,038.703

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.5: Table 1 replicated when non-executive elections are included, but only for autocratic regimes that did not hold any executive election. (For regimes holding at least one executive election, only executive elections are included.)

	Regime failure					
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to election / 1	2.282***		2.317***		2.531***	
	(0.300)		(0.307)		(0.367)	
Proximity to election / 8	−1.292***		−1.515***		−2.168***	
	(0.421)		(0.440)		(0.560)	
Election		1.689***		1.687***		1.750***
		(0.182)		(0.185)		(0.217)
Election 5 year		−0.108		−0.224		−0.495*
		(0.196)		(0.201)		(0.255)
Region Polity	2.676***	2.700***	4.524***	4.566***	3.678***	3.659***
	(0.459)	(0.465)	(0.932)	(0.936)	(1.138)	(1.140)
ln(GDP per capita)	−0.247**	−0.247**	−0.566***	−0.572***	−0.793***	−0.797***
	(0.099)	(0.100)	(0.140)	(0.140)	(0.171)	(0.171)
GDP Growth	−0.030***	−0.031***	−0.024***	−0.026***	−0.002	−0.004
	(0.009)	(0.009)	(0.009)	(0.010)	(0.012)	(0.012)
Military size	−0.313**	−0.288**	−0.255*	−0.244*	−0.329*	−0.317*
	(0.131)	(0.130)	(0.147)	(0.148)	(0.191)	(0.190)
Resource dependence	−0.006	−0.005	−0.0003	0.001	0.008	0.010
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Duration	−0.050***	−0.047***	−0.035**	−0.031*	−0.040**	−0.034*
	(0.015)	(0.016)	(0.016)	(0.016)	(0.019)	(0.019)
Duration^2	0.001***	0.001**	0.001**	0.001*	0.001*	0.001
	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Duration^3	−0.00000*	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2					2.405***	2.337***
				(0.343)	(0.344)	
Constant	−1.311*	−1.810***	0.747	0.181	2.730*	1.949
	(0.719)	(0.693)	(1.146)	(1.134)	(1.425)	(1.402)
Region dummies		*Y*	*Y*	*Y*	*Y*	*Y*
Decade dummies		*Y*	*Y*	*Y*	*Y*	*Y*
Observations	3,893	3,893	3,893	3,893	3,710	3,710
Log Likelihood	-698.749	-689.631	-680.946	-672.462	-496.068	-489.536
Akaike Inf. Crit.	1,419.498	1,401.263	1,407.892	1,390.924	1,040.137	1,027.072

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Figure A.6: Time since an election and the effect of elections on regime failure, estimated from an alternative GAM model where non-executive elections are included, but only for autocratic regimes that did not hold any executive election. (For regimes holding at least one executive election, only executive elections are included.)s

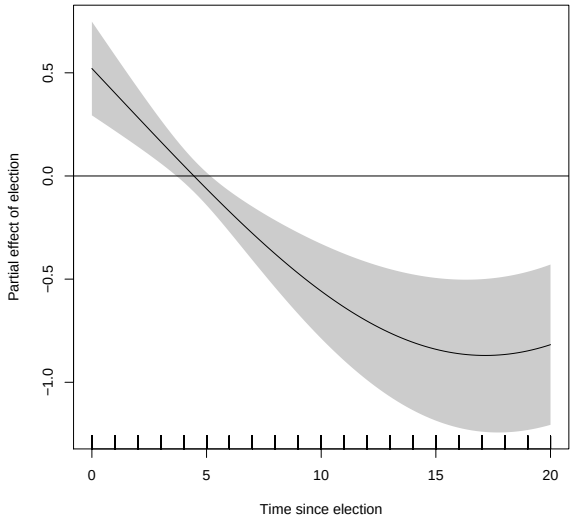


Table A.6: Baseline models excluding either the short-term or the long-term decay functions

	Regime failure					
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to election / 1	1.526***	1.407***	1.316***			
	(0.188)	(0.198)	(0.235)			
Proximity to election / 8				1.021***	0.799***	0.455
				(0.256)	(0.288)	(0.359)
Region Polity	2.493***	4.576***	3.622***	2.607***	4.760***	3.578***
	(0.461)	(0.935)	(1.139)	(0.455)	(0.927)	(1.120)
ln(GDP per capita)	−0.234**	−0.533***	−0.723***	−0.212**	−0.540***	−0.744***
	(0.100)	(0.141)	(0.170)	(0.099)	(0.140)	(0.168)
GDP Growth	−0.030***	−0.026***	−0.003	−0.028***	−0.023**	−0.002
	(0.009)	(0.010)	(0.012)	(0.009)	(0.009)	(0.012)
Military size	−0.213*	−0.215	−0.288	−0.225*	−0.242*	−0.317*
	(0.122)	(0.142)	(0.184)	(0.123)	(0.141)	(0.183)
Resource dependence	−0.007	−0.001	0.008	−0.006	0.001	0.011
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Duration	−0.044**	−0.026	−0.027	−0.038**	−0.020	−0.023
	(0.017)	(0.016)	(0.018)	(0.017)	(0.016)	(0.018)
Duration^2	0.001*	0.001	0.001	0.001	0.0004	0.0004
	(0.0005)	(0.0004)	(0.0004)	(0.0005)	(0.0003)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2			2.419***			2.658***
			(0.339)			(0.334)
Constant	−2.001***	−0.396	0.921	−2.442***	−0.485	1.276
	(0.701)	(1.138)	(1.392)	(0.703)	(1.140)	(1.417)
Decades N	*Y*	*Y*	*N*	*Y*	*Y*	
Regions N	*Y*	*Y*	*N*	*Y*	*Y*	
Observations	3,893	3,893	3,710	3,893	3,893	3,710
Log Likelihood	-708.218	-693.856	-508.977	-729.639	-713.262	-522.795
Akaike Inf. Crit.	1,436.436	1,431.712	1,063.953	1,479.278	1,470.525	1,091.589

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.7: Baseline models excluding either election year dummy or election in the last 5 years dummy

	Regime failure					
	(1)	(2)	(3)	(4)	(5)	(6)
Election	1.644***	1.550***	1.488***			
	(0.166)	(0.172)	(0.199)			
Election 5 year				0.532***	0.337*	0.158
				(0.160)	(0.177)	(0.224)
Region Polity	2.560***	4.674***	3.731***	2.589***	4.908***	3.641***
	(0.463)	(0.938)	(1.150)	(0.455)	(0.924)	(1.120)
ln(GDP per capita)	−0.232**	−0.528***	−0.730***	−0.217**	−0.527***	−0.751***
	(0.101)	(0.142)	(0.171)	(0.099)	(0.139)	(0.168)
GDP Growth	−0.031***	−0.027***	−0.005	−0.027***	−0.022**	−0.001
	(0.009)	(0.010)	(0.012)	(0.009)	(0.009)	(0.012)
Military size	−0.248**	−0.232	−0.301	−0.253**	−0.255*	−0.329*
	(0.124)	(0.144)	(0.187)	(0.126)	(0.144)	(0.185)
Resource dependence	−0.007	−0.001	0.008	−0.006	0.001	0.011
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Duration	−0.050**	−0.030*	−0.032*	−0.046***	−0.025	−0.026
	(0.023)	(0.018)	(0.019)	(0.016)	(0.016)	(0.018)
Duration^2	0.001	0.001	0.001	0.001*	0.0005	0.0005
	(0.001)	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00001)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2			2.405***			2.710***
			(0.339)			(0.330)
Constant	−1.871***	−0.337	1.063	−1.973***	−0.222	1.559
	(0.718)	(1.145)	(1.397)	(0.688)	(1.128)	(1.395)
Decades N	*Y*	*Y*	*N*	*Y*	*Y*	
Regions N	*Y*	*Y*	*N*	*Y*	*Y*	
Observations	3,893	3,893	3,710	3,893	3,893	3,710
Log Likelihood	-695.708	-681.101	-498.363	-732.343	-715.355	-523.356
Akaike Inf. Crit.	1,411.417	1,406.201	1,042.726	1,484.686	1,474.710	1,092.712

Notes *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.8: Models investigating potential multi-collinearity issues by pairing long-term decay function with election year dummy

	Regime failure		
	(1)	(2)	(3)
Election	1.838***	1.894***	2.022***
	(0.224)	(0.232)	(0.273)
Proximity to election / 8	−0.410	−0.821**	−1.380***
	(0.308)	(0.352)	(0.440)
Region Polity	2.618***	4.847***	3.979***
	(0.464)	(0.938)	(1.150)
ln(GDP per capita)	−0.224**	−0.520***	−0.776***
	(0.101)	(0.141)	(0.172)
GDP Growth	−0.031***	−0.026***	−0.004
	(0.009)	(0.009)	(0.012)
Military size	−0.291**	−0.273*	−0.361*
	(0.132)	(0.149)	(0.196)
Resource dependence	−0.006	−0.0001	0.009
	(0.008)	(0.008)	(0.008)
Duration	−0.052**	−0.036**	−0.043**
	(0.023)	(0.017)	(0.019)
Duration^2	0.001	0.001*	0.001*
	(0.001)	(0.0004)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000
	(0.00001)	(0.00000)	(0.00000)
SIP 2			2.522***
			(0.339)
Constant	−1.692**	0.140	2.339
	(0.726)	(1.158)	(1.457)
Decades	*N*	*Y*	*Y*
Regions	*N*	*Y*	*Y*
Observations	3,893	3,893	3,710
Log Likelihood	-694.824	-678.368	-493.387
Akaike Inf. Crit.	1,411.648	1,402.735	1,034.774

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

A.2.2 Testing alternative control variable sets

Table A.9 presents different versions of our parsimonious baseline model in Table 1 of the paper, but with changing control variable sets. In Model 1, we simply run a model without any covariates. This model actually retains a short-term coefficient significant at the 1% level (further tests show that the short-term results hold also when excluding the 554 additional observations in this model relative to the baseline, see Appendix Table A.11), whereas the long-term coefficient retains its sign but loses significance. In Models 2–4 we sequentially exclude the duration controls (Model 2), military size (Model 3) and resource dependence (Model 4), to see whether our baseline results hinge on including these controls, which could possibly induce post-treatment bias. They do not, although the long-term effect is slightly weakened when excluding the duration controls.

Further, we tested models adding extra controls, to check for potential omitted variable bias (although it should be noted that several of these controls, such as foreign aid and public spending, are likely endogenous to holding elections, thus potentially introducing post-treatment bias). Model 5 includes a control for urbanization, which, e.g., Wallace (2013) shows can be dangerous for dictators.[1] Model 6 includes a control for aid as a fraction of GDP, since aid is often given conditional on elections, and affects regime breakdown (Hariri, 2015; Wright, 2009).[2] Model 7 includes public spending as a control, since increasing such spending provides a possible alternative way in which the regime may co-opt the opposition, other than elections, and may thus affect breakdown. Model 8 controls for trade-openness, which some studies indicate leads to democratization (Eichengreen and Leblang, 2008). The results are very robust also when including the various extra controls.

Finally, Model 9 in Table A.9 utilizes a different coding of regime-transitions than our preferred measure drawing on the Geddes et al data. More specifically, it employs the *RegTrans* variable from the Polity dataset (Marshall, n.d.). This variable (mainly) captures substantial (≥ 3 points) changes on the Polity index, which extends from -10 to +10. *RegTrans* may thus exclude some instances of regime changes, captured as transitions by the GWF data, between different regimes that are about equally autocratic, while it may include political (de-)liberalization within instances that GWF classify as the same regime. The results turn out to be very similar when using this alternative coding, with p-values below 0.01 for both the (destabilizing) short-term and (stabilizing) long-term effects.

The wider literature on autocratic regime breakdown and democratization have highlighted how different regime types, such as monarchies, military regimes and party-based regimes, differ in terms of duration and resistance to various regime threats (e.g., Geddes, 1999; Hadenius and Teorell, 2007). Thus,

[1] The urbanization data is from the World Development Indicators (World Bank, 2013), and so are the public spending and trade openness data used below.

[2] The aid data is taken from Nielsen, Findley, Davis, Candland and Nielson (2011).

we wanted to check whether our results were driven by different such regime types both having different propensities to hold elections and to breaking down. Results for models controlling for autocratic regime type (from Geddes, Wright and Frantz (2014), with dominant party regimes as reference category) – including region and decade dummies, but excluding the SIP measure – were reported in Table 2 of the paper. To further check whether our results are robust, Table A.10 includes controls for authoritarian regime type for our alternative specifications. In Models 1 and 2, we add dummies for authoritarian regime type to our most parsimonious baseline models, while models 3-4 add these dummies to the more extensive models with region and decade dummies and the SIP measure controlling for differential levels of democracy. Our main results do not change much when controlling for such differences in autocratic regime types, and the models recover the destabilizing short-term and stabilizing long-term effects of elections. As for the other models, the short-term effect is particularly robust.

When changing the control variable set, the sample often also changes as a result of different patterns of missing data on the different controls. To check whether the variation in results in the models reported in Table 1 of the paper stem from the different control strategies *or* from changes to the sample, we include Table A.11. In addition to models without controls (1–2), Models 3–6 in this table contains the four most parsimonious baseline models in Table 1 (i.e., Models 1–4, Table 1), but now run on the 3710 observations that are included in Models 5–6 in Table 1. Again, the strong result for the short-term effect is retained across models, showing that it is not sensitive to sample changes either. A comparison of point estimates between Models 3–6 in Table A.11 and Models 1–4, Table 1 shows that the long-term result is not very sensitive to sample changes either. Indeed, the one main difference between the different baseline models of the paper – the large change in significance for the 5-year dummy in the more extensive model – is mainly due to the control for democracy level, and not due to the changes in sample (as can be seen from the insignificance of the 5-year dummy in Table A.11 on the reduced sample).

Table A.9: Core logit models with changing control variable sets

	Regime failure								RegTrans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Proximity to election / 1	1.832***	1.967***	2.059***	2.093***	2.104***	2.379***	2.201***	2.169***	2.768***
	(0.281)	(0.298)	(0.302)	(0.300)	(0.304)	(0.403)	(0.323)	(0.323)	(0.290)
Proximity to election / 8	−0.213	−0.684*	−0.746**	−0.982***	−0.936**	−0.916*	−0.987**	−0.918**	−1.468***
	(0.316)	(0.355)	(0.362)	(0.362)	(0.375)	(0.491)	(0.403)	(0.401)	(0.358)
Region Polity		2.605***	2.573***	2.680***	2.497***	2.760***	2.498***	2.536***	1.378***
		(0.453)	(0.448)	(0.440)	(0.478)	(0.621)	(0.502)	(0.501)	(0.430)
ln(GDP per capita)		−0.267***	−0.279***	−0.247***	−0.261**	−0.338**	−0.242**	−0.266**	−0.006
		(0.098)	(0.097)	(0.095)	(0.123)	(0.145)	(0.116)	(0.114)	(0.089)
GDP Growth		−0.031***	−0.031***	−0.027***	−0.029***	−0.053***	−0.030***	−0.032***	−0.002
		(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)	(0.009)
Military size		−0.314**		−0.288**	−0.276**	−0.419**	−0.187	−0.188	−0.199*
		(0.131)		(0.119)	(0.137)	(0.201)	(0.147)	(0.147)	(0.109)
Resource dependence		−0.007	−0.008		−0.008	−0.0004	−0.001	−0.004	−0.001
		(0.008)	(0.008)		(0.008)	(0.009)	(0.008)	(0.008)	(0.006)
Duration			−0.054***	−0.049***	−0.049***	−0.017	−0.037*	−0.037*	−0.089***
			(0.016)	(0.016)	(0.015)	(0.040)	(0.022)	(0.019)	(0.022)
Duration^2			0.001***	0.001**	0.001**	0.001	0.001	0.001	0.002**
			(0.0004)	(0.0004)	(0.0003)	(0.001)	(0.001)	(0.001)	(0.001)
Duration^3			−0.00000*	−0.00000	−0.00000	−0.00001	−0.00000	−0.00000	−0.00001
			(0.00000)	(0.00000)	(0.00000)	(0.00001)	(0.00001)	(0.00000)	(0.00001)
Urbanization					0.003				
					(0.006)				
Aid / GDP						−3.665			
						(3.855)			
Public spending							0.007		
							(0.005)		
Trade openness								−0.001	
								(0.002)	
Constant	−3.313***	−1.799**	−1.423**	−1.470**	−1.369	−1.280	−2.130**	−1.414*	−2.183***
	(0.148)	(0.708)	(0.703)	(0.688)	(0.852)	(1.140)	(1.008)	(0.793)	(0.665)
Observations	4,467	3,893	3,939	4,054	3,853	2,239	3,216	3,216	3,710
Log Likelihood	-836.467	-711.062	-713.587	-733.782	-704.618	-383.345	-602.942	-603.486	-789.535
Akaike Inf. Crit.	1,678.933	1,438.124	1,447.174	1,487.564	1,433.236	790.689	1,229.883	1,230.972	1,601.069

Note: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable in Models 1–8, and RegTrans based on Polity IV data in Model 9.

Table A.10: Additional logit models controlling for type of autocratic regime

	Regime failure			
	(1)	(2)	(3)	(4)
Proximity to election / 1	1.965***		2.232***	
	(0.307)		(0.382)	
Proximity to election / 8	−0.683*		−1.748***	
	(0.385)		(0.548)	
Election		1.676***		1.681***
		(0.200)		(0.238)
Election 5 year		−0.083		−0.535**
		(0.199)		(0.269)
Region Polity	2.005***	2.053***	4.105***	3.996***
	(0.489)	(0.494)	(1.156)	(1.157)
ln(GDP per capita)	−0.238**	−0.239**	−0.739***	−0.720***
	(0.103)	(0.104)	(0.175)	(0.175)
GDP per capita Growth	−0.031***	−0.032***	−0.003	−0.006
	(0.010)	(0.010)	(0.012)	(0.013)
Military size	−0.220	−0.214	−0.344*	−0.325
	(0.136)	(0.134)	(0.205)	(0.200)
Resource dependence	−0.005	−0.005	0.009	0.009
	(0.008)	(0.008)	(0.009)	(0.010)
SIP2			2.414***	2.409***
			(0.349)	(0.349)
Duration	−0.022	−0.025	−0.0003	0.007
	(0.022)	(0.023)	(0.021)	(0.020)
Duration^2	0.001	0.001	0.0003	0.0002
	(0.001)	(0.001)	(0.0004)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00001)	(0.00001)	(0.00000)	(0.00000)
Personalist	1.317	1.208	−12.306	−12.513
	(1.111)	(1.111)	(673.066)	(670.739)
Military	2.121*	2.004*	−11.438	−11.621
	(1.110)	(1.111)	(673.066)	(670.739)
Monarchy	0.544	0.464	−13.400	−13.600
	(1.041)	(1.044)	(673.066)	(670.739)
Constant	−2.950**	−2.989**	13.920	13.377
	(1.290)	(1.294)	(673.068)	(670.740)
Regions	No	No	Yes	Yes
Decades	No	No	Yes	Yes
Observations	3,893	3,893	3,710	3,710
Log Likelihood	-680.352	-671.349	-487.031	-480.693
Akaike Inf. Crit.	1,390.704	1,372.698	1,030.062	1,017.386

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.11: Models without controls and parsimonious baseline logit models run on identical sample as the more extensive baseline models in the paper (3710 obs).

	Regime failure					
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to election / 1	2.057***		2.409***		2.542***	
	(0.338)		(0.351)		(0.369)	
Proximity to election / 8	−0.129		−1.152**		−1.750***	
	(0.407)		(0.449)		(0.518)	
Election		1.832***		1.853***		1.852***
		(0.220)		(0.224)		(0.228)
Election 5 year		0.214		−0.146		−0.375
		(0.218)		(0.230)		(0.249)
Region Polity			2.520***	2.563***	5.724***	5.655***
			(0.538)	(0.547)	(1.105)	(1.106)
ln(GDP per capita)			−0.192*	−0.196*	−0.534***	−0.554***
			(0.116)	(0.118)	(0.166)	(0.167)
GDP Growth			−0.007	−0.008	−0.001	−0.003
			(0.012)	(0.012)	(0.011)	(0.011)
Military size			−0.379**	−0.352**	−0.368*	−0.353*
			(0.167)	(0.163)	(0.192)	(0.187)
Resource dependence			−0.0005	−0.0005	0.006	0.007
			(0.008)	(0.008)	(0.008)	(0.008)
Duration			−0.069***	−0.068***	−0.046**	−0.039**
			(0.018)	(0.026)	(0.019)	(0.019)
Duration^2			0.001***	0.001	0.001**	0.001*
			(0.0004)	(0.001)	(0.0004)	(0.0004)
Duration^3			−0.00000	−0.00000	−0.00000*	−0.00000
			(0.00000)	(0.00001)	(0.00000)	(0.00000)
Constant	−3.710***	−3.686***	−1.901**	−2.223***	0.033	−0.473
	(0.202)	(0.140)	(0.828)	(0.833)	(1.376)	(1.367)
Decades					*Yes*	*Yes*
Regions					*Yes*	*Yes*
Observations	3,710	3,710	3,710	3,710	3,710	3,710
Log Likelihood	-575.926	-568.726	-547.710	-540.536	-526.825	-520.593
Akaike Inf. Crit.	1,157.852	1,143.452	1,117.420	1,103.072	1,099.650	1,087.185

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

A.2.3 Different specifications of the short-term and long-term effects

Our baseline models rely on decay functions with different half-life parameters to model the temporally dependent effects of elections. Decay functions are generally given by $N_t = N_{t-1}2^{(\frac{-t}{\tau})}$, where t measures time (here since the last election), and τ is the average time it takes for the effect to halve, i.e. the 'half-life' parameter. These functions are standard in physics and biology to describe effects that dissipate over time, but they have also been used in political science (e.g., Hegre, Ellingsen, Gates and Gleditsch, 2001). In our baseline set-up, we set the half-life parameters to 1 and 8. Nonetheless, we want to make sure that our core results do not hinge on the specific parametrization. We therefore run a number of sensitivity tests where we vary the rate at which the effect dissipates.

This speaks, more broadly, to potential concerns that our results are sensitive to particular specifications of the short-term and long-term effects, although the GAM results shown in the paper makes this a less likely explanation of our baseline results. Table A.12 displays the core models when we vary the half-life parameters in the decay functions (from 0.5–2 years for the short-term effect, and from 4–16 years for the long-term effect). The results, both for the long-term and short-term, are robust to making these changes.

We also tested a quite different functional specification, simply assuming that the short-term and long-term effects operate only within a given time window (Table A.13 presents models where we assume that this window is 2 years for the short-term effect, and 10, 15 or 20 years for the long-term effect). These models further assume that the strength of these effects decline at a constant rate, before disappearing at the end of their respective time windows. These models corroborate our baseline results, identifying positive and significant short-term coefficients and negative and significant long-term coefficients on breakdown.

Numerous other functional form specifications modelling the short- and long-term effects can, of course, be designed (and we further test very different ways of operationalizing the long-term effect in Section A.3). This is why we have highlighted the interesting results provided by our flexible GAMs, which basically let the data determine the temporal effect pattern of elections on regime breakdown. Below, we present some further discussion and results from alternative GAMs.

While the GAMs impose very few restrictions on the functional form of the relationship under study, they demand that one specifies the degrees of freedom (i.e., the degree of 'wiggliness') allowed, before letting the data determine the form of the relationship within those restrictions. While we have no particular reason to believe the GAMs presented in the paper should be very sensitive, we want to make sure that the core results do not depend on the degrees of freedom imposed and other specification choices related to controls or elections included, and we therefore estimated a series of alternative GAMs.

But first, in the paper we opted for a graphical presentation of the core finding in the GAM (see Figure 2). Table A.14 thus reports the parameter estimates for the GAM discussed in the paper. Regarding robustness tests, Figure A.7 shows a GAM also including decade and region dummies as controls, but which is otherwise similar to that in Figure 2 of the paper. The results turn out to be robust to this change, replicating the pattern observed in the core model. Further, the GAM results are not sensitive to the choice of only including executive elections: Table A.15 shows the same model estimated on all types of elections. Figure A.8 shows the effect of time since election from this model, including all elections, graphically, now including all types of election. Both the estimated results reported in the Table and the graphical representation in the Figure closely resemble the results from the core model that we display and discuss in the paper (although the estimated break-even point where the long-term effect dominates the short-term appears earlier, after about 4 years, when also including non-executive elections).

Further, Figure A.9 shows a series of GAM where we vary the degrees of freedom allowed. As noted, we allowed 4 degrees of freedom in the baseline GAM. Here we vary the degrees of freedom from 5 to 8.[3] By varying the degrees of freedom we allow the effect of time since election on regime failure to *change direction*, i.e., to varyingly increase or decrease, fewer of more times. But, this also comes at a loss of efficiency and greater risk of over-fitting to the data. As these graphs show, varying the degrees of freedom has no substantial implications, although the models with more degrees of freedom indicate that the risk of regime failure following an election drops somewhat faster initially, and there is more uncertainty associated with the very long term for 7 and 8 degrees of freedom. In sum, however, varying the degrees of freedom does not substantially alter our core results from the baseline GAM, although the fact that the short-term destabilizing effect disappears more quickly when we increase the degrees of freedom suggests that we *might* overestimate the persistence of the short-term effect in our baseline.

Finally, we report results from another very flexible, but more conventional, model: Table A.16 shows results for a version of our baseline logit models that includes dummies for the election year as well as dummies for *each of the first 10 years after the election year*. The reference category is thus observations that are *more than* 10 years removed from an election year, including observations for countries that have not held autocratic elections. The point estimates of these models also suggest that the destabilizing effect of elections on autocratic regimes operate mainly in the first few years following an election, but only the election year is significant and stands out in terms of statistical significance. Hence, the steep change from the election year to the next is more in line with the most flexible GAMs discussed above, suggesting again that the point estimates in our baselines might overestimate the persistence of the destabilizing short-term effect.

[3]We do not report GAMs with fewer degrees of freedom since these simply are more linear versions of the baseline,

Table A.12: Core logit models with different decay-function specifications

	Regime failure			
	(1)	(2)	(3)	(4)
Proximity to election / 0.5	1.942***			
	(0.249)			
Proximity to election / 1		2.603***		1.925***
		(0.414)		(0.260)
Proximity to election / 2			2.572***	
			(0.444)	
Proximity to election / 4		−1.353***		
		(0.457)		
Proximity to election / 8	−0.606*		−1.617***	
	(0.326)		(0.512)	
Proximity to election / 16				−0.904**
				(0.394)
Region Polity	2.599***	2.608***	2.532***	2.542***
	(0.463)	(0.462)	(0.459)	(0.461)
ln(GDP per capita)	−0.225**	−0.220**	−0.232**	−0.231**
	(0.101)	(0.100)	(0.100)	(0.100)
GDP Growth	−0.031***	−0.030***	−0.029***	−0.029***
	(0.009)	(0.009)	(0.009)	(0.009)
Military size	−0.292**	−0.300**	−0.268**	−0.274**
	(0.132)	(0.132)	(0.129)	(0.130)
Resource dependence	−0.006	−0.006	−0.006	−0.007
	(0.008)	(0.008)	(0.008)	(0.008)
Duration	−0.051***	−0.050***	−0.048***	−0.049***
	(0.019)	(0.017)	(0.015)	(0.015)
Duration^2	0.001*	0.001*	0.001***	0.001**
	(0.001)	(0.0005)	(0.0003)	(0.0003)
Duration^3	−0.00000	−0.00000	−0.00000*	−0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Constant	−1.648**	−1.692**	−1.499**	−1.407*
	(0.718)	(0.703)	(0.716)	(0.739)
Observations	3,893	3,893	3,893	3,893
Log Likelihood	-698.526	-703.756	-712.451	-705.653
Akaike Inf. Crit.	1,419.052	1,429.513	1,446.901	1,433.305

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.13: Core logit models with different functional form specifications on short-term and long-term effects of elections: Effects dissipate at constant rate, within given time interval, before disappearing (short-term disappears after two years, and long-term after 10, 15 and 20 years in various models).

	Regime failure		
	Long-term lasts 10 years	Long-term lasts 15 years	Long-term lasts 20 years
Short-term effect elections	1.953****	1.715****	1.678****
	(0.36)	(0.31)	(0.29)
Long-term effect elections	-1.066**	-0.815*	-0.876**
	(0.45)	(0.43)	(0.44)
Region Polity	3.912***	3.958***	4.023***
	(1.23)	(1.24)	(1.24)
ln(GDP per capita)	-0.880****	-0.880****	-0.883****
	(0.21)	(0.21)	(0.21)
GDP p.c. Growth	0.000	0.000	0.001
	(0.01)	(0.01)	(0.01)
Military size	-0.462**	-0.446*	-0.442*
	(0.23)	(0.23)	(0.23)
Resource dependence	0.009	0.008	0.008
	(0.01)	(0.01)	(0.01)
Duration	-0.017	-0.017	-0.018
	(0.02)	(0.02)	(0.02)
Duration^2	0.001	0.001	0.001
	(0.00)	(0.00)	(0.00)
Duration^3	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)
SIP 2	2.945****	2.922****	2.911****
	(0.41)	(0.41)	(0.41)
Region dummies	Y	Y	Y
Decade dummies	Y	Y	Y
N	3649	3649	3649
Countries	111	111	111

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

without making any substantive differences.

Table A.14: Parameter estimates for core GAM model

	Dependent variable:	
	Regime failure	
Region Polity	2.511^{***}	(0.455)
ln(GDP per capita)	-0.232^{**}	(0.099)
GDP growth	-0.027^{***}	(0.009)
Military size	-0.237^{*}	(0.123)
Resource dependence	-0.007	(0.008)
Duration	-0.043^{***}	(0.015)
Duration^2	0.001^{**}	(0.0003)
Duration^3	-0.00000^{*}	(0.00000)
Constant	-1.671^{**}	(0.692)
Smooth estimates	**EDF**	**Chi.sq**
Time since election	3.00^{***}	(21.42)
Observations	3,893	
Adjusted R^2	0.031	
Log Likelihood	-738.098	
UBRE	-0.621	
Note:	*p<0.1; **p<0.05; ***p<0.01	

Figure A.7: Time since an election and the effect on regime failure, estimated from an alternative GAM model where also region and decade dummies are added as controls

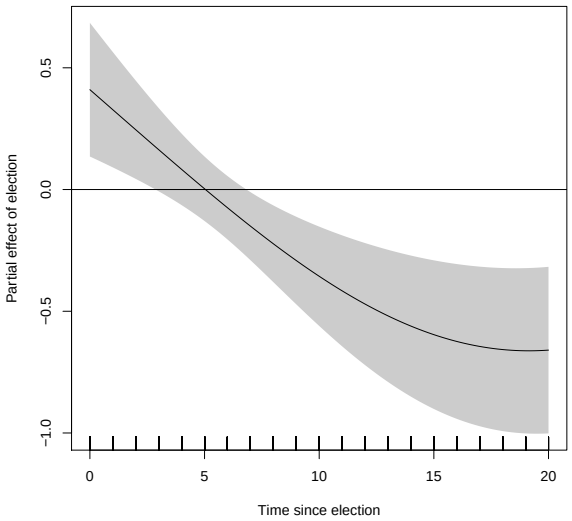


Table A.15: Parameter estimates for core GAM model with all elections (executive and non-executive) included

	Dependent variable:	
	Regime failure	
Region Polity	2.700^{***}	(0.454)
ln(GDP per capita)	-0.270^{***}	(0.101)
GDP growth	-0.028^{***}	(0.009)
Military size	-0.288^{*}	(0.122)
Resource dependence	-0.005	(0.008)
Duration	-0.048^{***}	(0.015)
Duration^2	0.001^{**}	(0.0003)
Duration^3	-0.00000^{*}	(0.00000)
Constant	-1.466^{**}	(0.708)
Smooth estimates	**EDF**	**Chi.sq**
Time since election	3.00^{***}	(20.50)
Observations	3,893	
Adjusted R^2	0.031	
Log Likelihood	-736.455	
UBRE	-0.622	
Note:	*p<0.1; **p<0.05; ***p<0.01	

Figure A.8: Years since election and the effect on regime failure, estimated from a GAM model with 4 degrees of freedom for years, including both executive and non-executive elections since election

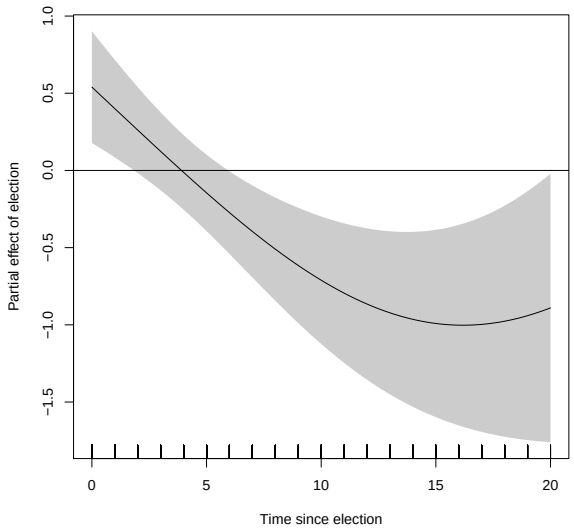


Figure A.9: Time since an election and the effect of elections on regime failure, estimates from GAMs with various degrees of freedom

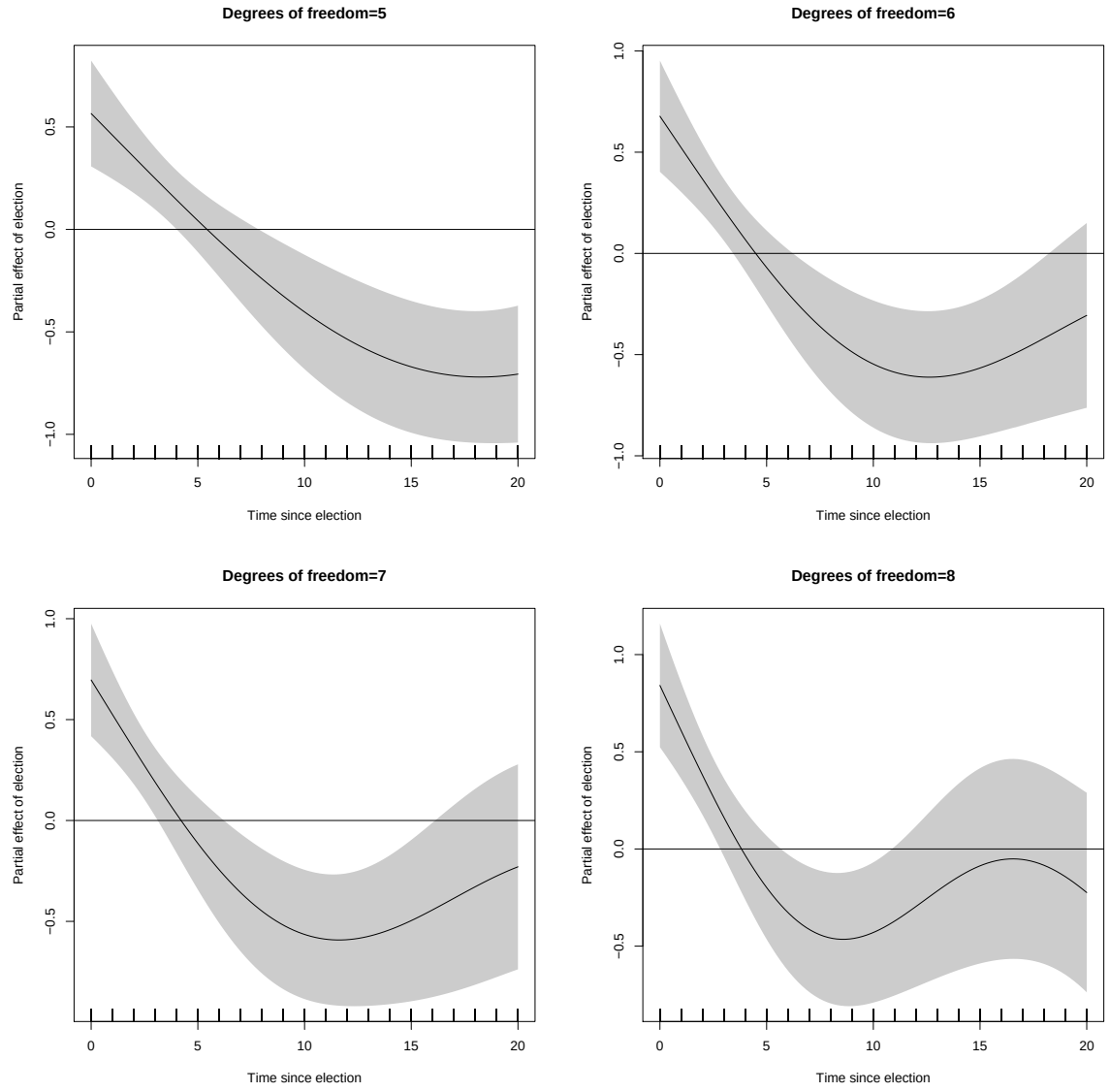


Table A.16: Flexible logit models with dummies for years passed since last election (up to 10 years since last election; reference category is more than 10 years since last election).

	Regime failure		
	(1)	(2)	(3)
Election	1.703***	1.615***	1.583***
	(0.177)	(0.182)	(0.213)
Election 1 year	0.237	0.095	0.040
	(0.245)	(0.252)	(0.291)
Election 2 year	0.055	−0.092	−0.055
	(0.263)	(0.269)	(0.304)
Election 3 year	0.107	−0.032	−0.183
	(0.264)	(0.270)	(0.319)
Election 4 year	0.261	0.162	0.296
	(0.233)	(0.237)	(0.266)
Election 5 year	−0.368	−0.452*	−0.837**
	(0.258)	(0.260)	(0.335)
Election 6 year	−0.060	−0.166	−0.077
	(0.250)	(0.257)	(0.292)
Election 7 year	0.080	0.012	−0.009
	(0.262)	(0.267)	(0.311)
Election 8 year	−0.202	−0.255	−0.226
	(0.283)	(0.288)	(0.331)
Election 9 year	0.111	0.087	0.009
	(0.264)	(0.269)	(0.331)
Election 10 year	0.245	0.204	0.273
	(0.250)	(0.256)	(0.301)
Region Polity	2.492***	4.720***	3.924***
	(0.472)	(0.944)	(1.158)
ln(GDP per capita)	−0.245**	−0.533***	−0.747***
	(0.103)	(0.142)	(0.174)
GDP Growth	−0.031***	−0.027***	−0.005
	(0.010)	(0.010)	(0.012)
Military size	−0.231*	−0.239*	−0.311*
	(0.126)	(0.145)	(0.187)
Resource dependence	−0.007	−0.001	0.007
	(0.008)	(0.008)	(0.009)
Duration	−0.048**	−0.029*	−0.030
	(0.023)	(0.017)	(0.019)
Duration^2	0.001	0.001	0.001
	(0.001)	(0.0004)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000
	(0.00001)	(0.00000)	(0.00000)
SIP 2			2.385***
			(0.342)
Constant	−1.832**	−0.275	1.170
	(0.723)	(1.161)	(1.443)
Region dummies	*N*	*Y*	*Y*
Decade dummies	*N*	*Y*	*Y*
Observations	3,893	3,893	3,710
Log Likelihood	-692.800	-678.448	-493.388
Akaike Inf. Crit.	1,425.600	1,420.896	1,052.775

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

A.2.4 Problematic cases and outlier sensitivity

As we discuss in the paper, coding the start and end dates of political regimes in a reliable manner is certainly a difficult endeavor, and disputable choices are likely to occur. One very attentive reviewer pointed to one particular instance of the regime coding, namely the case of Guatemala-1982 discussed in the introduction of our paper: Geddes et al do not count this coup-induced change of leadership as a regime change, whereas our account suggests it maybe should. Nonetheless, Table A.17 shows that our results are not altered when recoding Guatemala-1982 as a regime change

Another reviewer correctly noted that in some of our cases, in-depth knowledge of the countries' histories clearly suggest that elections were not playing an independent causal role in bringing down the regime (although they coincided in time). as discussed in the paper, this does not mean that elections can not play a strong, causal role in many other instances. Nonetheless, to ensure that our results are not driven by the inclusion of notable cases, Table A.17 reports results where we drop the two cases highlighted by the reviewer, namely the Argentinean military regime ending in 1983 and Chilean regime under Pinochet ending in 1989. Dropping these cases – either one of them at the time, or both simultaneously as in Models 3 and 4 of Table A.17 – does not affect our results either.

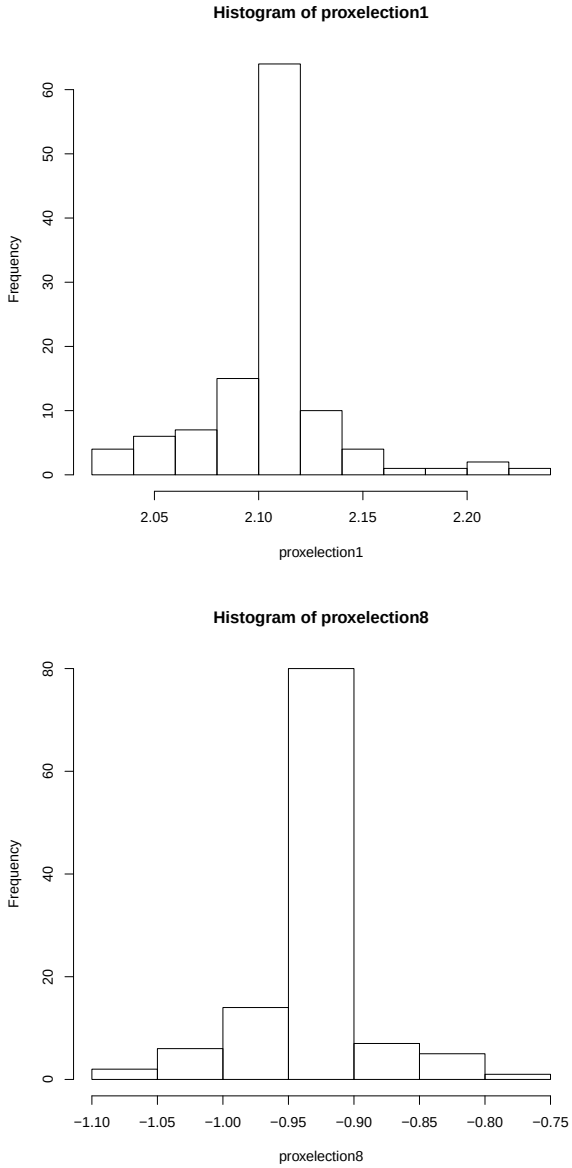
More generally, a concern – given the limited number of regime breakdowns (199 in our baseline sample) – could be that our results are driven by the experiences of particular countries or particular regime breakdown observations. However, further tests that we have conducted ensure us that this is not the case. For instance, Figure A.10 shows results from Jacknife estimations where we exclude individual countries. The figure reports the parameter estimates for $ElecShortTerm$ (top panel) and $ElecLongTerm$ (bottom panel) and shows that the estimates are fairly robust to excluding any particular country.

Table A.17: Baseline models re-coding Guatemala-1982 as regime change (Models 1 and 2), and excluding Argentinean (76–83) and Chilean (73–89) regimes (Models 3 and 4)

	Regime failure			
	(1)	(2)	(3)	(4)
Proximity to election / 1	2.444***		2.374***	
	(0.376)		(0.377)	
Proximity to election / 8	−2.056***		−2.038***	
	(0.527)		(0.528)	
Election		1.734***		1.684***
		(0.233)		(0.235)
Election 5 year		−0.528**		−0.524**
		(0.260)		(0.261)
Region Polity	3.934***	3.762***	3.849***	3.684***
	(1.138)	(1.137)	(1.151)	(1.149)
ln(GDP per capita)	−0.768***	−0.758***	−0.779***	−0.766***
	(0.170)	(0.172)	(0.171)	(0.173)
GDP Growth	−0.003	−0.005	−0.003	−0.005
	(0.012)	(0.012)	(0.012)	(0.012)
Military size	−0.357*	−0.338*	−0.354*	−0.333*
	(0.195)	(0.189)	(0.195)	(0.188)
Resource dependence	0.009	0.009	0.009	0.009
	(0.008)	(0.009)	(0.008)	(0.008)
Duration	−0.041**	−0.033*	−0.042**	−0.033*
	(0.019)	(0.019)	(0.019)	(0.019)
Duration^2	0.001*	0.001	0.001*	0.001*
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2	2.459***	2.396***	2.444***	2.376***
	(0.338)	(0.337)	(0.341)	(0.340)
Constant	2.451*	1.633	2.587*	1.739
	(1.452)	(1.428)	(1.460)	(1.435)
Region dummies	*Y*	*Y*	*Y*	*Y*
Decade dummies	*Y*	*Y*	*Y*	*Y*
Observations	3,710	3,710	3,687	3,687
Log Likelihood	-503.649	-498.370	-499.203	-494.310
Akaike Inf. Crit.	1,055.297	1,044.741	1,046.407	1,036.620

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Figure A.10: Coefficient histogram from Jacknife estimations where we remove individual countries one by one



A.2.5 Cox survival models

To further ensure that our results do not depend on our choice of model (logit), or specification of temporal dependence (time since regime change, and time since regime change squared and cubed), we run the same analyses with a Cox proportional hazard model instead of a logit. This model makes no assumptions about temporal dependence. Table A.18 presents these results for such models with the equivalent set of controls to that of the baseline models (1 and 2) of Table 1 in the paper, a model including decade and region dummies (3), and a model including the GWF autocratic regime type dummies (4). These results are qualitatively also very similar to the results found when using the logit model. Whereas the long-term effects are not statistically significant at conventional levels in Models 2 and 4, the short-term effect is very robust, in the sense that *ElecShortTerm* and the election year dummy always maintain very low p-values.

Table A.18: Cox Survival Model on autocratic regime failure

	(1)	(2)	(3)	(4)
ElecShortTerm	1.836***		1.834***	1.696***
	(0.287)		(0.297)	(0.287)
ElecLongTerm	-0.849*		-1.079**	-0.680
	(0.368)		(0.413)	(0.367)
Election year		1.358***		
		(0.185)		
Election last 5 years		0.0527		
		(0.189)		
Region polity	2.393***	2.374***	4.530***	1.691***
	(0.443)	(0.447)	(0.873)	(0.465)
ln(GDP per captia)	-0.209*	-0.211*	-0.436**	-0.218*
	(0.0986)	(0.0987)	(0.135)	(0.101)
GDP per capita growth	-0.0290***	-0.0303***	-0.0240**	-0.0298***
	(0.00844)	(0.00853)	(0.00843)	(0.00871)
Military size	-0.287*	-0.246*	-0.257	-0.241
	(0.127)	(0.124)	(0.140)	(0.131)
Resource dependence	-0.00194	-0.00195	0.00184	0.000746
	(0.00748)	(0.00755)	(0.00756)	(0.00769)
Party				0.256
				(1.086)
Personalist				1.016
				(1.104)
Military				1.751
				(1.105)
Monarchy				-0.135
				(1.038)
Decades	No	No	Yes	No
Regions	No	No	Yes	No
AIC	1755.5	1743.3	1745.8	1709.9
ll	-870.8	-864.7	-853.9	-843.9
N	3893	3893	3893	3893
Regime failures	199	199	199	199

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors in parentheses

A.3 Repeated elections

In our baseline modeling strategy, we try to separate long- and short-term effects of elections by using decay functions and GAMs. However, as we discuss in the paper, there are some (long-term) mechanisms that may not be very well captured by such an approach. Since most regimes hold repeated elections, the "long-term" effect can usually not exceed the number of years that pass between repeated elections. Although this is implied in the model, it may very well be that the long-term effect does not disappear when there is a new election. (Assuming this would imply, for example, that the difference in breakdown risk, *ceteris paribus*, would be the same for a country that held its first and only election 4 years ago, and a country that held its last election four years ago after previously having held 10 previous elections.) In short, in our baseline models the long-term effect of elections does not cumulate, but is "canceled out" by each new election. In the following set of models, in Table A.19, we make different alterations to relax this assumption.

At the outset, we note that two of the specifications pertaining to alternative long-term measures are already discussed in the paper, but reported (in Table 2) for models including decade and region dummies. Hence, Table A.19 shows these alternative strategies embedded in a different model excluding region and decade dummies. However, the more important difference from the models presented in Table 2 of the paper relates to the simultaneous inclusion of *ElecLongTerm* and the alternative measures of long-term effects. One interpretation of this is that we are simultaneously estimating the long-term effect of time having passed since the last election, and the even longer-term effect related to the regime's electoral-institutional history. As we discuss in the paper, these arguably map on to somewhat different mechanisms addressed in our theoretical discussion.

First, we include a variable that is, simply, the number of previous elections held in the same regime in Model 1. This variable could have a negative coefficient, capturing the cumulative risk-reducing effects of having held many previous elections, on top of the long-term effect of the previously held election. As expected, the impact of the short-term variable is not altered by including this election count variable; elections are still destabilizing in the short term. More interestingly, also the stabilizing long-term effect of the previous election is basically unchanged when adding the new variable, and, perhaps surprisingly, the number of previous elections held is not found to have an *independent* impact on probability of regime change (notably holding time since last election constant). This contrasts with the statistically significant result reported for the number of elections in Model 2, Table 2 of the paper, which omits *ElecLongTerm*.

We thereafter try out a specification (Model 2) where we, in addition to modelling the short-term and long-term effects of the last election, control for wether the election is the first election held by the

regime. First elections may, for different reasons, have coincided with a period of political instability for many regimes. They have, for example, often taken place right after obtaining national independence. Yet, the first-election dummy fails to reach conventional levels of significance, and, more importantly, controlling for it does not affect our core results. Third, we test a model (Model 3) that tries to explicitly distinguish the short-term effects stemming from election events from the effects of having electoral institutions, which theoretically corresponds quite well to our notion of the long-term effect of institutions. We do so by using a fairly high threshold for the number of years (7 years) since an election was last held within a regime as a proxy for whether the regime has or no longer has "operating" electoral institutions. In this model, ElecLongTerm maintains its size, but is no longer significant due to the increased standard errors, whereas the new control is also negative, as expected, although also statistically insignificant. However, the short-term effect of elections is robust also to this adjustment. Fourth, to make sure that the core results are not simply driven by regimes that never hold elections, Model 4 reports results when only including regimes that have held at least one election. This sheds about 1000 observations from the sample, but the results for both the short-term and long-term effects are robust.

Table A.19: Repeated elections

	Regime failure			
	(1)	(2)	(3)	(4)
Proximity to election / 1	2.113***	2.167***	2.157***	2.195***
	(0.304)	(0.323)	(0.324)	(0.322)
Proximity to election / 8	−0.992**	−0.957**	−1.214	−1.303***
	(0.408)	(0.377)	(0.740)	(0.475)
Region Polity	2.530***	2.558***	2.555***	2.272***
	(0.473)	(0.462)	(0.462)	(0.525)
ln(GDP per capita)	−0.228**	−0.227**	−0.229**	−0.207*
	(0.100)	(0.100)	(0.100)	(0.115)
GDP Growth	−0.030***	−0.030***	−0.030***	−0.025**
	(0.009)	(0.009)	(0.009)	(0.010)
Military size	−0.283**	−0.282**	−0.282**	−0.274*
	(0.131)	(0.131)	(0.131)	(0.142)
Resource dependence	−0.006	−0.006	−0.006	−0.005
	(0.008)	(0.008)	(0.008)	(0.008)
Duration	−0.051***	−0.051***	−0.050***	−0.055*
	(0.016)	(0.016)	(0.016)	(0.031)
Duration^2	0.001**	0.001**	0.001**	0.001
	(0.0004)	(0.0004)	(0.0003)	(0.001)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00001
	(0.00000)	(0.00000)	(0.00000)	(0.00001)
Sum elections	0.013			
	(0.035)			
First election		−0.187		
		(0.348)		
7 years			−0.192	
			(0.434)	
Constant	−1.545**	−1.565**	−1.326	−1.309
	(0.721)	(0.714)	(0.923)	(0.824)
Observations	3,893	3,893	3,893	2,849
Log Likelihood	-705.065	-704.986	-705.033	-592.511
Akaike Inf. Crit.	1,434.129	1,433.971	1,434.067	1,207.022

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

A.4 Additional results for models treating elections as endogenous

This section shows additional results from our instrumental variable model estimations. Table A.20 shows second-stage IV-probit results with the long-term variables treated as endogenous (where we mainly rely on the longer-term spatial lag instruments, given that we want to instrument for the long-term effect of elections). These point estimates show a similar pattern as the reduced form estimates in terms of consistently having the expected sign. Nevertheless, we cannot conclude on any causal long-term effects as the long-term coefficients are statistically insignificant even at the 10% level. The standard errors are large, and the Sargan tests reject the null-hypothesis that the exclusion restriction holds (suggesting that we shouldn't put too much faith in these long-term IV models). We should note that the estimated long-term effect falls short of statistical significance also in most other specifications that we tested. Some IV-probit models that we tested – for example some models using only the share of neighboring autocracies with an election year – do, however, report a statistically significant effect. But, given the lack of robustness and poor theoretical fit of the neighboring *election year* variable as an instrument for the long-term effect, we do not report such alternative IV models. In sum, we cannot conclude on any causal long-term effect of elections on breakdown, and hope that future research will come up with better identification strategies on this relationship.

Table A.21 shows the first-stage results for the IV-Probit models reported in Table 3 of the paper, where the short-term variables are treated as endogenous. Table A.22 shows the first-stage regressions for the models in Table A.20 where the long-term variables are endogenous.

As discussed in the paper, we follow standard practice by conducting the diagnostic tests on structurally equivalent FE2SLS models, due to the lack of such tests for IV-Probit. The Sargan tests for many of the short-term models that we tested suggest that we cannot reject the null that the overidentification restrictions are valid (especially in the models where we control for the "alternative channel" of diffusion of instability). The F-statistics for the excluded instruments are, as also discussed in the paper, only moderate, indicating that there could be some weak-instrument bias when we employ these instruments for the short-term variables. However, comparing our F-values with the Stock-Yogo weak ID test critical values suggest that the IV-probit models should be substantially less biased than models treating elections as exogenous; the weakest of our first-stage F statistics suggest around $10-20\%$ bias from consistent estimates and *towards* the (biased) OLS results, and most of our first-stage F-statistics are at least fairly sizeable. Furthermore, most coefficients from the IV-probit models are consistently (far) larger in size than (the highly significant) coefficients from single-equation Probit models.

Hence, even if the *point estimates* should be affected by weak-instrument bias, the results reported in Table 3 of the paper do provide evidence of the hypothesized de-stabilizing short-term causal effect

of elections (although we again remind that we should be careful with decisively concluding, due to the strong assumptions associated with such models).

We conducted several robustness tests on the short-term models, using alternative estimation techniques, alternative control-variable sets, and alternative instrument sets. In Table A.23 we report FE2SLS models that are otherwise similar to the IV-probit models reported in Table 3 of the paper. IV-probit is known for being sensitive. Despite 2SLS being a linear model, many scholars therefore prefer this estimation technique to IV-Probit even for dichotomous dependent variables (such as regime breakdown) (see, e.g., Angrist and Pischke, 2009). Indeed, Table A.23 show that the FE2SLS models actually yield somewhat stronger results on the short-term effect of elections than the IV-Probit models.

Appendix Table A.24 reports more parsimonious IV-Probit models, using the same sets of instruments as in Table 3 of the paper. More specifically, these models drop the region and decade dummies. The results from Table 3 are quite stable to this change, and the results are even a bit stronger when excluding these controls.

However, the statistical significance of the short-term effect does depend on the exact sub-set of instruments included, as is illustrated by Appendix Table A.25. These models add the dummy variable capturing whether at least one autocracy in the neighborhood had an election year to the two instruments used in the most trustworthy models of Table 3 in the paper (i.e., Models 7–10). The coefficients basically retain their sign and size in these models, but the statistical significance is weakened (three of the four models display a p-value for the short-term variables lower than .10 but higher than .05). As we make clear in the paper, the high uncertainty associated with many of the IV-Probit estimates suggest that we should not conclude decisively on a clear causal effect, based on this design, but the weight of the evidence still suggest to us that such a causal effect is more likely than not.

Finally, A.26 displays the results from the biprobit models mentioned in the paper, modelling, respectively, Election Year (Model 1) and Election last 5 years (Model 2) as endogenous. These models yield both the expected destabilizing effect of elections in the short term and the stabilizing effect in the long term.

Table A.20: Second-stage IV-probit results, with Election last 5 years or ElecLongTerm as endogenous variables.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Election past 5 years	-0.215		-0.339		-0.580		-0.723		-0.507	
	(1.08)		(0.65)		(0.69)		(0.79)		(0.83)	
ElecLongTerm		-0.769		-1.366		-1.591		-1.904		-1.303
		(2.25)		(1.36)		(1.50)		(1.71)		(2.08)
Election year	0.887*		0.941***		1.060***		1.099***		1.028***	
	(0.50)		(0.30)		(0.32)		(0.32)		(0.38)	
ElecShortTerm		1.186		1.589*		1.760*		1.945*		1.563
		(1.54)		(0.91)		(1.02)		(1.12)		(1.44)
Region Polity	2.551***	2.542***	2.573***	2.542***	2.423***	2.327***	2.572***	2.497***	2.421***	2.349***
	(0.51)	(0.49)	(0.49)	(0.48)	(0.52)	(0.53)	(0.48)	(0.50)	(0.52)	(0.53)
ln(GDP per capita)	-0.248***	-0.248***	-0.253***	-0.248***	-0.268***	-0.252***	-0.264***	-0.248***	-0.266***	-0.254***
	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)
GDP per capita growth	-0.014***	-0.014***	-0.014***	-0.013***	-0.014***	-0.014***	-0.013***	-0.013***	-0.014***	-0.014***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Military size	-0.141	-0.138*	-0.149*	-0.145*	-0.138*	-0.127*	-0.167**	-0.149**	-0.134	-0.124
	(0.10)	(0.08)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.08)
Resource dependence	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration	-0.021**	-0.023**	-0.021**	-0.024***	-0.030*	-0.035**	-0.018*	-0.025***	-0.030*	-0.035**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Duration^2	0.000*	0.000**	0.000*	0.000**	0.001	0.001	0.000	0.000**	0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^3	-0.000	-0.000*	-0.000	-0.000*	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighboring instability					0.702***	0.681**			0.712***	0.707***
					(0.26)	(0.27)			(0.26)	(0.27)
Neighb. instab. past 5 yrs					0.119	0.118			0.113	0.105
					(0.14)	(0.13)			(0.14)	(0.15)
Region dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	3434	3434	3434	3434	3264	3264	3434	3434	3264	3264
Countries	112	112	112	112	112	112	112	112	112	112
Sargan p-value	–	–	.01	.01	.12	.13	.01	.01	.01	.01
Cragg-Donald Wald F stat	28.6	16.3	8.7	6.2	7.4	4.7	22.3	15.1	19.4	10.9

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses.
IV-probit models; second-stage regressions with Election year and ElecShortTerm as endogenous independent variables and regime failure as dependent variable.
All models include share of neighboring autocracies with election last 5 years, as instrument. Models 3–10 also include share of autocracies globally with election year as instrument.
Models 3–10 also include share of autocracies globally with election last 5 years as instrument.
Models 3–6 further include five extra instruments tapping neighboring and global environment in terms of autocratic elections.
First-stage regressions are reported in Table A 21.

Table A.21: First-stage IV-probit results, with Election year or ElecShortTerm as endogenous variables

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	El. year	ElecShortTerm	El. year	ElecShortTerm	El. year	ElecShortTerm	El. year	ElecShortTerm	El. year	ElecShortTerm
Election past 5 yrs	0.218***		0.210***		0.209***		0.218***		0.208***	
	(0.01)		(0.01)		(0.01)		(0.01)		(0.01)	
ElecLongTerm		0.732***		0.716***		0.702***		0.730***		0.701***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Region Polity	0.107**	0.055	0.083	0.048	0.074	0.057	0.077	0.034	0.073	0.066
	(0.05)	(0.04)	(0.06)	(0.04)	(0.06)	(0.04)	(0.05)	(0.04)	(0.06)	(0.04)
ln(GDP per capita)	-0.003	-0.007	-0.005	-0.008	-0.004	-0.009*	-0.003	-0.007	-0.004	-0.008*
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)
GDP per capita growth	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Military size	-0.000	-0.004	-0.002	-0.005	-0.002	-0.005	-0.001	-0.005	-0.000	-0.003
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)
Resource dependence	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration	0.000	0.004***	0.001	0.003***	-0.000	0.002***	0.000	0.003***	-0.000	0.003***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^2	-0.000	-0.000**	-0.000	-0.000*	-0.000	-0.000	-0.000	-0.000**	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^3	0.000	0.000*	0.000	0.000	0.000	0.000	0.000	0.000*	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighboring instability					0.016	0.026			0.021	0.028
					(0.04)	(0.03)			(0.04)	(0.03)
Neighb. instab. past 5 yrs					-0.000	-0.012			0.000	-0.008
					(0.02)	(0.01)			(0.02)	(0.01)
Share neighb. dict. w. elect. year	0.098***	0.063***	0.119***	0.103***	0.111**	0.098***	0.060**	0.038*	0.032	0.011
	(0.03)	(0.02)	(0.05)	(0.03)	(0.05)	(0.04)	(0.03)	(0.02)	(0.03)	(0.02)
Share dict. globally w. elect. year			1.809***	1.397***	1.707***	1.341***	1.859***	1.289***	1.701***	1.233***
			(0.28)	(0.21)	(0.28)	(0.21)	(0.25)	(0.19)	(0.26)	(0.19)
Number elections neighborhood			-0.057***	-0.049***	-0.055***	-0.049***				
			(0.01)	(0.01)	(0.02)	(0.01)				
At least one election neighb.			0.049**	0.032*	0.043*	0.031*				
			(0.02)	(0.02)	(0.02)	(0.02)				
Number dictatorships neighb.			-0.044*	-0.042**	-0.035	-0.049**				
			(0.02)	(0.02)	(0.03)	(0.02)				
Share neighb. dict. w. el. last 5 yrs			-0.117	-0.102*	-0.089	-0.081				
			(0.08)	(0.06)	(0.08)	(0.06)				
Share glob. dict. w. elect. last 5 yrs			-0.882	0.026	-0.451	0.466				
			(0.96)	(0.72)	(0.97)	(0.73)				
Region dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	3782	3782	3434	3434	3264	3264	3782	3782	3264	3264
Countries	112	112	112	112	112	112	112	112	112	112
Cragg-Donald Wald F stat	11.5	9.0	12.0	10.7	9.4	8.9	32.9	27.0	21.4	18.0

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses.
Notes: IV-probit models; first-stage regressions with Election year and ElecShortTerm as dependent variables (regime failure is dependent in second stage).
All models include share of neighboring autocracies with election year as instrument. Models 3–10 also include share of autocracies globally with election year as instrument.
Models 3–6 further include five extra instruments tapping neighboring and global environment in terms of autocratic elections.
Second-stage regressions are reported in Table A 20.

Table A.22: First-stage IV-probit results, with Election past 5 years or ElecLongTerm as endogenous variables

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	El. 5 years	ElecLongTerm	El. 5 years	ElecLongTerm	El. 5 years	ElecLongTerm	El. 5 years	ElecLongTerm	El. 5 years	ElecLongTerm
Election year	0.461***		0.463***		0.492***		0.465***		0.491***	
	(0.02)		(0.02)		(0.03)		(0.02)		(0.03)	
ElecShortTerm		0.687***		0.691***		0.711***		0.688***		0.706***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Region Polity	0.178**	0.039	0.053	0.012	0.053	-0.004	0.157*	0.031	0.113	-0.002
	(0.08)	(0.04)	(0.08)	(0.04)	(0.09)	(0.04)	(0.08)	(0.04)	(0.09)	(0.04)
ln(GDP per capita)	-0.039***	-0.005	-0.037***	-0.004	-0.039***	-0.004	-0.041***	-0.006	-0.042***	-0.005
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
GDP per capita growth	0.001	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.001	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Military size	-0.059***	-0.014***	-0.060***	-0.013***	-0.058***	-0.013***	-0.058***	-0.014***	-0.057***	-0.014***
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)
Resource dependence	0.001	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.001	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration	0.006***	-0.002***	0.006***	-0.002***	0.006***	-0.002***	0.006***	-0.002***	0.006***	-0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^2	-0.000***	-0.000	-0.000***	-0.000*	-0.000***	-0.000**	-0.000***	-0.000	-0.000***	-0.000*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^3	0.000***	0.000**	0.000***	0.000**	0.000***	0.000***	0.000***	0.000**	0.000***	0.000***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Share neighb. dict. w. el. last 5 yrs	0.537***	0.255***	0.585***	0.259***	0.661***	0.265***	0.465***	0.225***	0.589***	0.253***
	(0.11)	(0.06)	(0.12)	(0.06)	(0.13)	(0.06)	(0.12)	(0.06)	(0.12)	(0.06)
Number elections neighborhood			0.040*	0.036***	0.038	0.036***				
			(0.02)	(0.01)	(0.02)	(0.01)				
At least one election neighb.			-0.031	-0.016	-0.027	-0.016				
			(0.03)	(0.02)	(0.03)	(0.02)				
Number dictatorships neighb.			-0.173***	-0.030*	-0.149***	-0.009				
			(0.03)	(0.02)	(0.04)	(0.02)				
Share neighb. dict. w. elect. year			-0.018	-0.058	-0.019	-0.058				
			(0.07)	(0.03)	(0.07)	(0.04)				
Share dict. globally w. elect. year			-0.480	-0.688***	-0.440	-0.664***				
			(0.42)	(0.20)	(0.43)	(0.21)				
Share glob. dict. w. elect. last 5 yrs			4.449***	1.141	4.108***	0.746	5.258***	2.077***	4.670***	1.557**
			(1.44)	(0.71)	(1.49)	(0.73)	(1.33)	(0.65)	(1.39)	(0.69)
Neighboring instability					-0.027	-0.023			-0.054	-0.032
					(0.06)	(0.03)			(0.06)	(0.03)
Neighb. instab. past 5 yrs					0.053**	0.029**			0.089***	0.036***
					(0.03)	(0.01)			(0.02)	(0.01)
Region dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	3434	3434	3434	3434	3264	3264	3434	3434	3264	3264
Countries	112	112	112	112	112	112	112	112	112	112
Cragg-Donald Wald F stat	28.6	16.3	8.7	6.2	7.4	4.7	22.3	15.1	19.4	10.9

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses.
IV-probit models; first-stage regressions with Election past 5 years and ElecLongTerm as dependent variables (regime failure is dependent in second stage).
All models include share of neighboring autocracies with election past 5 years as instrument. Models 3–10 also include share of autocracies globally with election past 5 years as instrument.
Models 3–6 further include five extra instruments tapping neighboring and global environment in terms of autocratic elections.
Second-stage regressions are reported in Table 20.

Table A.23: Second-stage FE2SLS results, with Election year or ElecShortTerm as endogenous variables.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Election year	0.499**		0.192**		0.206**		0.262***		0.231*	
	(0.25)		(0.09)		(0.10)		(0.10)		(0.12)	
ElecShortTerm		0.764*		0.231*		0.262*		0.376**		0.309
		(0.42)		(0.13)		(0.14)		(0.15)		(0.19)
Election past 5 years	-0.069		0.005		0.002		-0.017		-0.003	
	(0.06)		(0.02)		(0.02)		(0.02)		(0.03)	
ElecLongTerm		-0.642		-0.102		-0.130		-0.266*		-0.177
		(0.41)		(0.13)		(0.14)		(0.15)		(0.19)
Region Polity	0.304***	0.314***	0.420***	0.424***	0.363***	0.359***	0.354***	0.358***	0.357***	0.350***
	(0.08)	(0.08)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)
ln(GDP per capita)	-0.034**	-0.047***	-0.041***	-0.043***	-0.036**	-0.038**	-0.033**	-0.039***	-0.036**	-0.039**
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
GDP per capita growth	-0.001**	-0.001*	-0.001***	-0.001***	-0.002***	-0.002***	-0.001**	-0.001**	-0.002***	-0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Military size	-0.017*	-0.019**	-0.018**	-0.019**	-0.014	-0.015	-0.018**	-0.018**	-0.015	-0.016*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Resource dependence	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration	0.001	-0.002	0.001	0.001	0.002	0.001	0.001	0.000	0.002	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^3	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighboring instability					0.123***	0.120***			0.122***	0.119***
					(0.03)	(0.03)			(0.03)	(0.03)
Neighb. instab. past 5 yrs					0.016	0.016			0.016	0.017
					(0.01)	(0.01)			(0.01)	(0.01)
Country dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	3781	3781	3434	3434	3264	3264	3781	3781	3264	3264
Countries	112	112	112	112	112	112	112	112	112	112
Sargan p-value	–	–	.01	.01	.15	.16	.25	.26	.68	.64
Cragg-Donald Wald F stat	11.5	9.0	12.0	10.7	9.4	8.9	32.9	27.0	21.4	18.0

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses.
FE2SLS models; second-stage regressions with Election year and ElecShortTerm as endogenous independent variables and regime failure as dependent variable.
All models include share of neighboring autocracies with election year as instrument. Models 3–10 also include share of autocracies globally with election year as instrument.
Models 3–6 further include five extra instruments tapping neighboring and global environment in terms of autocratic elections.

Table A.24: Second-stage IV-probit results, with Election year or ElecShortTerm as endogenous variables. Models excluding decade and region dummies

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Election year	2.301		2.219***		2.125***		1.940***		2.191**	
	(1.41)		(0.62)		(0.78)		(0.75)		(0.88)	
ElecShortTerm		4.197***		2.085**		1.859*		2.656***		2.968***
		(1.38)		(0.90)		(1.04)		(0.99)		(1.08)
Election past 5 years	-0.375		-0.360**		-0.348*		-0.287		-0.362*	
	(0.36)		(0.17)		(0.19)		(0.20)		(0.21)	
ElecLongTerm		-2.717**		-1.113*		-0.982		-1.585**		-1.736**
		(1.06)		(0.64)		(0.71)		(0.72)		(0.75)
Region Polity	0.990***	0.699	1.028***	1.126***	0.872***	0.932***	1.057***	1.050***	0.867***	0.826***
	(0.38)	(0.55)	(0.26)	(0.25)	(0.28)	(0.27)	(0.26)	(0.26)	(0.29)	(0.28)
ln(GDP per capita)	-0.115**	-0.104*	-0.116**	-0.126***	-0.142***	-0.151***	-0.121***	-0.126***	-0.142***	-0.146***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
GDP per capita growth	-0.011***	-0.008	-0.014***	-0.015***	-0.014***	-0.015***	-0.012***	-0.011***	-0.014***	-0.014***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Military size	-0.096*	-0.119**	-0.112*	-0.139**	-0.103*	-0.126**	-0.099*	-0.128**	-0.102*	-0.129**
	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Resource dependence	-0.002	-0.002	-0.002	-0.002	-0.001	-0.001	-0.002	-0.002	-0.001	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration	-0.023*	-0.031***	-0.028***	-0.034***	-0.039**	-0.043***	-0.024**	-0.030***	-0.039**	-0.045***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Duration^2	0.000	0.000*	0.001*	0.001***	0.001*	0.001*	0.000	0.000	0.001*	0.001**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Duration^3	-0.000	-0.000	-0.000	-0.000*	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighboring instability					0.672***	0.730***			0.674**	0.626**
					(0.26)	(0.26)			(0.26)	(0.27)
Neighb. instab. past 5 yrs					0.134	0.161			0.132	0.174
					(0.11)	(0.12)			(0.11)	(0.11)
N	3782	3782	3434	3434	3264	3264	3782	3782	3264	3264
Countries	112	112	112	112	112	112	112	112	112	112

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses.
IV-probit models; second-stage regressions with Election year and ElecShortTerm as endogenous independent variables and regime failure as dependent variable.
All models include share of neighboring autocracies with election year as instrument. Models 3–10 also include share of autocracies globally with election year as instrument.
Models 3–6 further include five extra instruments tapping neighboring and global environment in terms of autocratic elections.

Table A.25: Second-stage IV-probit results, with Election year or ElecShortTerm as endogenous variables. Models employing alternative set of (three) instruments

Model	(1)	(2)	(3)	(4)
Election year	1.529*		1.826*	
	(0.89)		(1.07)	
ElecShortTerm		1.970		2.499*
		(1.26)		(1.43)
Election past 5 years	-0.284		-0.326	
	(0.22)		(0.25)	
ElecLongTerm		-1.360		-1.597
		(0.94)		(1.04)
Region Polity	2.265***	2.334***	2.205***	2.135***
	(0.49)	(0.49)	(0.59)	(0.61)
ln(GDP per capita)	-0.265***	-0.264***	-0.237***	-0.227***
	(0.07)	(0.07)	(0.07)	(0.08)
GDP per capita growth	-0.011**	-0.010**	-0.014***	-0.014***
	(0.00)	(0.00)	(0.00)	(0.00)
Military size	-0.105	-0.105	-0.109	-0.105
	(0.07)	(0.07)	(0.07)	(0.07)
Resource dependence	0.000	0.000	0.001	0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Duration	-0.015	-0.019**	-0.032**	-0.036**
	(0.01)	(0.01)	(0.02)	(0.02)
Duration^2	0.000	0.000**	0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)
Duration^3	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Neighboring instability			0.674**	0.644**
			(0.26)	(0.27)
Neighb. instab. past 5 yrs			0.079	0.092
			(0.12)	(0.12)
Country dummies	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y
N	3782	3782	3264	3264
Countries	112	112	112	112
Sargan p-value	.05	.03	.45	.41
Cragg-Donald Wald F stat	22.8	19.6	15.0	13.6

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses.
IV-probit models; second-stage regressions with Election year and ElecShortTerm
as endogenous independent variables and regime failure as dependent variable.
All models include the following instruments: share of neighboring autocracies with election year;
at least one autocracy in neighborhood with election year; share of autocracies globally with election year.

Table A.26: Biprobit results

	(1)	(2)
Dep. variable	Regime failure	Regime failure
Election year	2.278***	1.064***
	(0.273)	(0.122)
Election last 5 years	-0.263*	-0.879**
	(0.104)	(0.313)
Region polity	2.206***	2.544***
	(0.479)	(0.486)
Ln GDP per captia	-0.210**	-0.251***
	(0.0673)	(0.0677)
GDP p.c. growth	-0.0132**	-0.0132**
	(0.00439)	(0.00442)
Military size	-0.103	-0.110
	(0.0694)	(0.0714)
Resource dependence	0.00199	0.00172
	(0.00322)	(0.00351)
Regime duration	-0.0227	-0.0224*
	(0.0124)	(0.00894)
Regime duration^2	0.000475	0.000439*
	(0.000413)	(0.000216)
Regime duration^3	-0.00000214	-0.00000163
	(0.00000351)	(0.00000147)
Region dummies	Y	Y
Decade dummies	Y	Y
Dep. variable	Election year	Election last 5 years
Election last 5 years	6.028	
	(975.1)	
Election year		7.511
		(1531.2)
Number elections neighborhood	-0.212	0.168*
	(0.111)	(0.0837)
At least one election neighb.	0.409*	-0.0248
	(0.161)	(0.126)
Number dictatorships neighb.	-0.0243	-0.392**
	(0.163)	(0.128)
Share neighb. dict. w. elect. year	-0.516	-0.305
	(0.326)	(0.266)
Share neighb. dict. w. elect. last 5 yrs	-0.188	1.167**
	(0.562)	(0.430)
Share dict. globally w. elect. last 5 yrs	-23.26**	20.52***
	(7.609)	(5.182)
Region dummies	Y	Y
Decade dummies	Y	Y
Fisher's Z transf. of correl. between regr.	-1.114**	0.473*
	(0.357)	(0.228)
AIC	2673.2	4236.7
ll	-1292.6	-2074.3
N	3263	3263

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.5 Causal sensitivity tests

While we have opted for an instrumental variables strategy to identify the treatment effect(s) of elections, this is no panacea. The instruments only yield a moderately strong first stage, and we can not be sure that the exclusion restriction is satisfied; elections in the neighborhood – even when conditional on instability in the neighborhood – could affect regime-breakdown through other pathways (than inducing elections in the country of interest) that we have not been able to identify, and thus control for. Hence, while the exogeneity assumption in the non-IV models is problematic, the IV models also come with debatable assumptions.

To further address concerns relating to endogeneity biases driving our main result, we perform a set of causal sensitivity tests, drawing on the toolkit presented in Blackwell (2014). These tests gauge the sensitivity of estimates to assumptions of ignorability (i.e., no omitted confounders), which corresponds to the assumption of no endogeneity. Hence, these tests conceptualize endogeneity as an omitted variable problem, and probe how big this problem must be in order to make the estimated effect disappear. More specifically, Blackwell's approach estimates the coefficient of interest under different (simulated) scenarios where (in reality unobserved) variables that display different correlations with the treatment and outcome are included in the regression. This allows us to gauge *how large the confounding from unobservables must be for our estimate to be indistinguishable from zero.*

Below, we plot the results from such exercises, applied both to the short- and long-term effects of elections. Since the causal sensitivity tests presented and programmed by Blackwell (2014) assume a binary treatment, we use the models where the long- and short-term effects of elections are operationalized by the two dummy variables.

Figure A.11 shows the estimated coefficient for the short-term effect – here taken from Model 6, Table 1 in the paper – under different levels of confounding, parametrized as the proportion of the explained joint variance between treatment and outcome. The signs of the confounding parameter describe the direction of the bias (positive or negative selection). This display shows that the short-term effect of elections is quite robust to potential confounding from unobservables. If the confounding is negative – meaning that more unstable regimes are *less* likely to hold elections – then the estimated coefficient (assuming no confounding) is actually biased downwards. In other words, we would be underestimating the short-term destabilizing effect of elections. This can be seen by following the estimated coefficient as it moves rightwards, as negative confounding from an unobservable increases. Furthermore, the figure shows that the estimated effect is also quite robust to positive selection on unobservables. As can be seen from the figure, an unobserved confounder would have to explain roughly 50% of the joint variance in treatment (election year) and outcome (regime-breakdown) for the estimated effect to be indistinguishable from

zero. This is a powerful result; while the existence of such an unobserved confounder (some omitted variable that makes unstable regimes that much more likely to hold elections) is certainly possible, we find this very unlikely.

Figure A.11: Sensitivity to confounding from unobservables (Short-term)

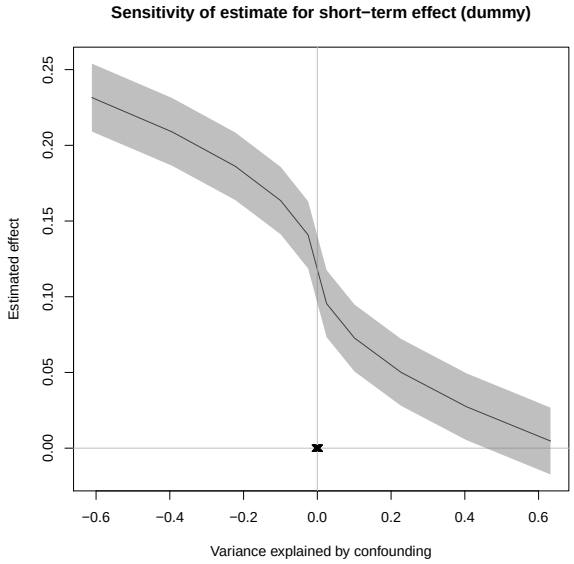
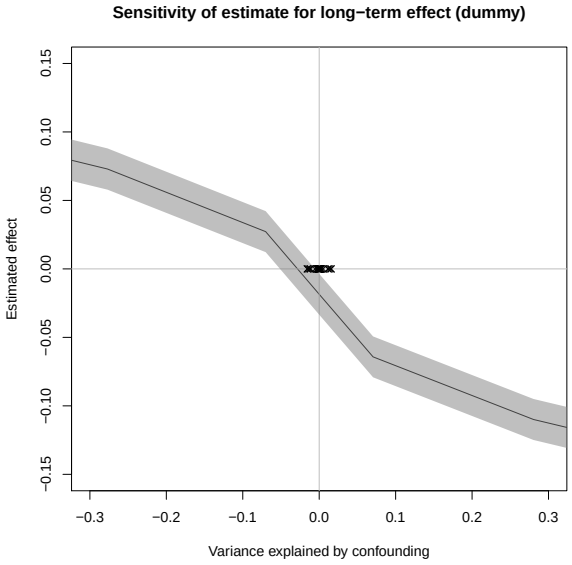


Figure A.12 performs a similar analysis for the long-term effect of elections. This shows two things: First, the estimated coefficient is negative, implying a stabilizing effect on autocratic regimes, although it is not as strong as the short-term coefficient. Second, it requires very little positive confounding to pull this estimate in a negative direction, suggesting that positive selection bias might quickly yield even more strongly negative effects on regime breakdown (i.e., strengthening the conclusion that elections stabilize autocratic regimes in the long term). Furthermore, a moderate amount of negatively signed confounding would result in a positive coefficient for the 5-year election dummy. Hence, we should be most worried about negative selection into this 'treatment status'. In other words, if more unstable regimes are far less likely to hold elections because of some factor that we have not controlled for, this could lead to conclusions of a regime-stabilizing long-term effect of autocratic elections being wrong.[4]

To summarize, the sensitivity analyses show that the short-term effect is fairly robust to confounding from unobservables, both when we are considering so-called positive and negative selection. The long-term effect, on the other hand, is less certain. While it is plausible that the selection is either negative or close to zero – given that we have no clear priors as to what shape the selection, if present, might take –

[4]We should note that the 5-year election dummy is the least robust among our baseline long-term measures in other tests. Hence, if we could have modelled continuous treatments, we would speculate that *ElecLongTerm* would be at least somewhat less sensitive also in these tests.

Figure A.12: Sensitivity to confounding from unobservables (Short-term)



negatively signed confounding from unobservables would yield a destabilizing long-term effect of elections on autocratic regime survival. Hence, we are less certain about our theorized stabilizing long-term causal effect of elections, but we can be more confident about the destabilizing short-term causal effect.

A.6 Checking whether alternative mechanisms drive our results

In this section we investigate whether alternative mechanisms than the ones we postulate are driving the results. To investigate this, we mostly proceed by excluding cases that arguably instantiate a given such alternative mechanism, and run the analyses again on the remaining cases. If our proposed explanation is generating the pattern we observe, the results should be largely unaffected by dropping such cases.

First, as we discuss in the paper, one potential mechanism that could be operating, and that seemingly runs counter to our theoretical story, is that some autocratic incumbents may voluntarily opt not to run in the elections and let the opposition take power (given a win). In these cases, the regime-change is not a result of election-induced instability, but the election just serves as a convenient vehicle for regime-orchestrated liberalization. If this dynamic is driving our short-term results, then our argument may not hold up (but, see our discussion below on this issue). To probe this possibility, we estimate models where we recode or exclude all cases where the incumbent does not run in the election, employing coding of such instances embedded in the dataset by Geddes, Wright and Frantz (2014). Examples of such cases are Nigeria in 1999/00, Burundi in 2003/04 and Guatemala in 1985/86. Models run on samples that are circumscribed in this way are presented in Tables A.27 (recoding the dependent variable *only* for the year experiencing such breakdowns; Model 3 in this table is equivalent to Model 5 in Table 2 of the paper) and A.28 (excluding all observations for regimes ending in such breakdowns). As is clear from these tables, our short-term results (and our long-term results) are not driven by this dynamic, increasing our confidence that our account is appropriate.

As a final note on this issue, we remind of the possibility that at least some of the cases dropped in these tests *could* be relevant for our theoretical argument after all: *If* incumbents opt not to run in elections because they fear mass mobilization following a manipulated election – and possibly being removed either by a subsequent revolution or an election-induced coup – the incumbent is basically stepping down because of the very mechanisms that we are highlighting (although he or she does so in anticipation of them). Nonetheless, we conclude this discussion by noting that our interpretation of the results and conclusions do not hinge on this, as our short-term result holds when excluding all such questionable cases.

Table A.29 displays models pertaining to various other alternative explanations of our main results, and some extensions.

First, our results may simply be affected by us combining observations both from regimes that hold and those that do not hold elections. We therefore re-run our baseline models only for regimes that have held at least one election. Results for these cases are reported in column 1 (this result was also discussed in Section A.3); excluding regimes that never hold elections does not affect the results, neither for the

short- nor for the long-term coefficients.

Second, as also discussed in Section A.3, it may be that the first election a regime holds (for example a post-independence or post-conflict election) takes place in a very peculiar context, where the regime in question is particularly vulnerable. Thus, election years may be correlated with breakdown simply because these first elections are held in very volatile contexts. In Section A.3., we introduced a first-election dummy, but in Model 2, Table A.29 we simply exclude all observations prior to the second election being held in the NELDA dataset. Our results are actually strengthened, particularly for the long-term effect, when we make this adjustment and re-estimate our models on the remaining 1503 observations.

Third, as an extension of the above we investigate whether our results are driven by particularly fragile regimes. If the regimes that have held recent elections and "die" are very fragile regimes that only live to hold one (or two) elections; a stabilizing long-term coefficient might not be due to there being an actual long-term effect, as hypothesized. Such a coefficient could rather stem from only the stable regimes being left in the sample a few years after having passed the "first election threshold". We make some headway towards adjusting for this through controlling for regime duration, but this may be insufficient. Therefore, we exclude all regimes that are 4 years or younger in Model 3, and only run the analysis on the 3077 observations from mature and purportedly more stable regimes. However, our results, both for the short-term and long-term coefficients, are retained when we make this adjustment to the sample.

Fourth, sometimes the election occurred under a previous, different regime than the one we investigate the breakdown for. In these cases, the mechanisms we postulate are arguably less relevant. We therefore run models where we only include the cases where the last election occurred under the current regime (i.e., if time since election $<$ regime duration). The result of these tests can be found in Model 4, and they clearly suggest that our core results are not driven by elections taking place in different regimes than those we measure the breakdown and survival of. If anything, the results are actually slightly strengthened when making this adjustment.

Fifth, as discussed, authoritarian regime parties and autocrats sometimes lose (even manipulated) elections (see, e.g., Levitsky and Way, 2010), and may also sometimes opt to leave office as a consequence – for instance, because they fear that doing otherwise could spur revolutionary or coup attempts. This would contribute to a correlation between regime breakdown and being in an election year. A leader stepping down because of an electoral defeat often entails not only regime breakdown, but also a democratization brought about by elections. While related – at least if the autocrat leaves because of anticipated anti-regime collective action – this could perhaps be construed as a different mechanism than what we propose in the paper. To investigate this, we exclude all cases of elections where the incumbent

stepped down as a result of the election, using the information available in the NELDA dataset. The results of this robustness test is shown in column 5. The results are somewhat weaker, but still in the same direction and clearly different from zero for the short-term coefficient.

Sixth, we investigate whether our results may be affected by (repeated) elections ultimately leading to democratization, for instance through the processes described in Lindberg (2006). Lindberg proposes that experience with elections brings about democracy through fostering democratic values, improving civil liberties, and through creating political organizations that are conducive to democracy. If Lindberg is correct, this would work in the opposite direction of our proposed long-term stabilizing effects of elections on autocratic regime. While elections may allow autocratic regimes to improve their co-optive and repressive capacities, in general, they may also spur norm and behavioral changes in the citizenry that increase the risk of one particular type of regime change, namely democratization. Hence, we expect that our long-term stabilizing effect should actually increase if we could partial out the Lindberg-mechanism. However, *some* of the short-term destabilizing effects we find might actually also be ascribed to that mechanism rather than the collective-action mechanism we propose. Countries with more recent election will also be the countries with a higher frequency of regular elections, and these countries will be the most exposed to the processes described by Lindberg.

To investigate whether our results are affected by this, we first add a control variable for the cumulative number of elections a regime has held in the past (Model 6): Repeated elections should be positively correlated with the probability of democratization according to the Lindberg argument (although repeated elections may mainly affect gradual liberalization within regimes rather than boosting probabilities of democratic transitions, see Bogaards, 2013; Lindberg, 2013), and controlling for this might affect our estimates of the short- and long-term effects of an election. More specifically, one could expect that the long-term coefficient, in particular, should be strengthened once accounting for this proposed autocracy-destabilizing mechanism related to elections inducing liberalization. However, our results remain substantively the same (the point estimate for the long-term coefficient observes only a minimal change, from -0.931 to -0.992, compared to the baseline) when making this adjustment. However, to further investigate this issue, we provide a more direct test by excluding all cases of democratization (Model 7) from being counted as instances of regime changes.

Many instances of democratization – for instance those resulting from wide-spread anti-regime protests following elections – fit well with our theoretical argument, suggesting that excluding such cases might be a questionable operation if the purpose is to test hypotheses derived from our argument. Nonetheless, we tested such specifications, and our core results are more mixed for models excluding democratization cases. For instance, when running the baseline model including region and decade dummies, *ElecShort-Term* retains the expected sign but just turns statistically insignificant at conventional levels with a

t-value of -1.5. This is perhaps to be expected, simply from the fact that we are left with far fewer instances of regime change in the data when not counting those leading to democratization. However, as Model 7 shows, this is very different when employing a model that omits region and decade dummies. While we note that this may introduce omitted variable bias, it also introduces more degrees of freedom and allows for more precise estimation. Indeed, for such a model the results are clearly stronger when not counting democratization experiences as autocratic regime changes. That is, when only considering how autocratic elections influence the probability of an autocratic regime breaking down and being replaced by another autocracy, we see an even stronger short-term destabilizing effect of elections and a far clearer stabilizing effect. *If* we are to trust the parsimonious specification, it suggests that our baseline results – while still strong – might actually be attenuated by the kind of countervailing mechanisms proposed by Lindberg, related to autocratic elections having different impacts on democratization experiences more in particular.

Finally, as an extension we tested models investigating the relationship between autocratic elections and probability of democratization more directly, using a dummy recording (only) instances of democratization based on the GWF data. Very interestingly, we find indications of the same relationships when democratization rather than autocratic regime breakdown more generally is employed as the dependent variable. We first simply re-ran our baseline model on democratization (Model 8). The point estimates in this model indicate that autocratic elections are associated with a much increased probability of democratization in their immediate aftermath, but a reduced probability over the longer term. However, the results from Model 8 must be interpreted with caution, due to over-fitting (resulting from there being far fewer instances of democratization than total regime breakdowns). In order to avoid over-fitting and obtain more credible results, we therefore also tested a more parsimonious model dropping the set of duration controls. This model (Model 9) also identifies the above-described pattern; autocratic elections are related to increased probabilities of democratization in their immediate aftermath, but they are also related to reduced prospects of democratization in the longer term.

Table A.27: Table 1 from paper excluding regime failures in which no incumbent runs in competitive election won by opponent; 1946–2008

	Regime failure							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proximity to election / 1	1.466***		1.512***		1.698***		1.671***	
	(0.323)		(0.334)		(0.391)		(0.394)	
Proximity to election / 8	−0.442		−0.776*		−1.279**		−1.764**	
	(0.388)		(0.441)		(0.541)		(0.744)	
Election		1.305***		1.279***		1.329***		1.138***
		(0.211)		(0.213)		(0.247)		(0.209)
Election 5 year		0.022		−0.115		−0.334		−0.364
		(0.198)		(0.215)		(0.265)		(0.304)
Region Polity	2.364***	2.360***	4.502***	4.466***	4.359***	4.262***	4.030***	4.012***
	(0.489)	(0.494)	(0.965)	(0.964)	(1.176)	(1.176)	(1.176)	(1.179)
ln(GDP per capita)	−0.263**	−0.265**	−0.516***	−0.517***	−0.659***	−0.654***	−0.661***	−0.662***
	(0.107)	(0.108)	(0.146)	(0.147)	(0.175)	(0.176)	(0.173)	(0.174)
GDP Growth	−0.033***	−0.034***	−0.029***	−0.030***	−0.005	−0.006	−0.003	−0.003
	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)	(0.012)	(0.012)
Military size	−0.267**	−0.256*	−0.277*	−0.273*	−0.337*	−0.330*	−0.327*	−0.339*
	(0.136)	(0.134)	(0.155)	(0.154)	(0.198)	(0.194)	(0.194)	(0.195)
Resource dependence	−0.012	−0.013	−0.006	−0.006	0.002	0.001	0.002	0.002
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Duration	−0.034	−0.037	−0.016	−0.014	−0.025	−0.020	−0.022	−0.019
	(0.022)	(0.024)	(0.017)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Duration^2	0.001	0.001	0.0004	0.0004	0.0005	0.0004	0.0004	0.0004
	(0.001)	(0.001)	(0.0004)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Duration^3	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000	−0.00000
	(0.00001)	(0.00001)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2					1.644***	1.588***	1.628***	1.536***
					(0.374)	(0.373)	(0.384)	(0.384)
Constant	−1.633**	−1.728**	−0.105	−0.404	1.028	0.527	1.456	0.726
	(0.769)	(0.758)	(1.207)	(1.201)	(1.496)	(1.471)	(1.485)	(1.442)
Region dummies			*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Decade dummies			*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Observations	3,893	3,893	3,893	3,893	3,710	3,710	3,710	3,710
Log Likelihood	-647.253	-640.747	-634.725	-628.802	-475.810	-471.614	-475.689	-470.965
Akaike Inf. Crit.	1,316.506	1,303.494	1,315.450	1,303.604	999.620	991.229	999.378	989.930

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.28: Table 1 from paper excluding all regime-years for regimes that end (fail) with no incumbent running in competitive election won by opponent; 1946–2008

	Regime failure							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proximity to election / 1	5.519***		5.930***		6.337***		6.337***	
	(0.723)		(0.775)		(1.037)		(1.037)	
Proximity to election / 8	−3.753***		−4.857***		−6.279***		−6.279***	
	(0.850)		(0.984)		(1.325)		(1.325)	
Election		3.496***		3.514***		3.339***		3.339***
		(0.451)		(0.461)		(0.563)		(0.563)
Election 5 year		−0.927*		−1.151**		−1.841**		−1.841**
		(0.484)		(0.523)		(0.723)		(0.723)
Region Polity	3.587***	3.712***	6.459***	6.281***	0.151	−0.903	0.151	−0.903
	(0.762)	(0.776)	(1.615)	(1.608)	(2.284)	(2.210)	(2.284)	(2.210)
ln(GDP per capita)	−0.132	−0.139	−0.492*	−0.487*	−1.722***	−1.592***	−1.722***	−1.592***
	(0.162)	(0.160)	(0.261)	(0.258)	(0.383)	(0.368)	(0.383)	(0.368)
GDP Growth	−0.011	−0.014	−0.004	−0.009	0.009	−0.002	0.009	−0.002
	(0.020)	(0.020)	(0.020)	(0.020)	(0.031)	(0.030)	(0.031)	(0.030)
Military size	−0.229	−0.193	−0.186	−0.215	−0.137	−0.248	−0.137	−0.248
	(0.221)	(0.208)	(0.260)	(0.248)	(0.366)	(0.355)	(0.366)	(0.355)
Resource dependence	0.004	0.004	0.010	0.011	0.037***	0.041***	0.037***	0.041***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)	(0.013)	(0.013)
Duration	−0.090*	−0.088*	−0.070	−0.067	0.007	0.033	0.007	0.033
	(0.050)	(0.050)	(0.054)	(0.055)	(0.039)	(0.039)	(0.039)	(0.039)
Duration^2	0.002	0.002	0.001	0.002	−0.0002	−0.0004	−0.0002	−0.0004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Duration^3	−0.00001	−0.00001	−0.00001	−0.00001	0.00000	0.00000	0.00000	0.00000
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
SIP 2					8.454***	8.152***	8.454***	8.152***
					(0.980)	(0.959)	(0.980)	(0.959)
Constant	−3.002**	−3.859***	−0.047	−1.656	9.694***	7.080**	9.694***	7.080**
	(1.248)	(1.215)	(2.221)	(2.155)	(3.350)	(3.194)	(3.350)	(3.194)
Region dummies			*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Decade dummies			*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Observations	2,394	2,394	2,394	2,394	2,302	2,302	2,302	2,302
Log Likelihood	-233.894	-233.608	-220.296	-222.305	-121.746	-126.807	-121.746	-126.807
Akaike Inf. Crit.	489.787	489.216	486.592	490.611	291.491	301.614	291.491	301.614

Notes: *p<0.1; **p<0.05; ***p<0.01
Logit regressions with Geddes-Wright-Frantz (GWF; 2014) regime failure as dependent variable.

Table A.29: Checking for alternative explanations (Models 1–7), and extending our analysis to consider effects on democratization (Models 8–9)

	Regime failure							Democratization	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Proximity to election / 1	2.195***	2.831***	2.406***	2.548***	1.687***	2.113***	3.710***	4.037***	3.875***
	(0.322)	(0.473)	(0.374)	(0.365)	(0.392)	(0.304)	(0.760)	(0.506)	(0.496)
Proximity to election / 8	−1.303***	−2.209**	−1.304***	−1.478**	−0.626	−0.992**	−2.301**	−2.704***	−2.410***
	(0.475)	(0.979)	(0.443)	(0.610)	(0.493)	(0.408)	(0.947)	(0.616)	(0.587)
Region Polity	2.272***	2.638***	2.963***	2.642***	1.975***	2.530***	4.411***	4.471***	4.454***
	(0.525)	(0.675)	(0.529)	(0.550)	(0.623)	(0.473)	(1.041)	(0.654)	(0.641)
ln(GDP per capita)	−0.207*	−0.090	−0.217*	−0.271**	−0.271**	−0.228**	−0.290	−0.170	−0.184
	(0.115)	(0.154)	(0.116)	(0.120)	(0.135)	(0.100)	(0.254)	(0.150)	(0.145)
GDP Growth	−0.025**	−0.050***	−0.046***	−0.027**	−0.005	−0.030***	−0.001	−0.024*	−0.027*
	(0.010)	(0.017)	(0.012)	(0.012)	(0.013)	(0.009)	(0.023)	(0.014)	(0.014)
Military size	−0.274*	−0.443*	−0.391**	−0.301*	−0.443**	−0.283**	−0.510	−0.205	−0.227
	(0.142)	(0.227)	(0.161)	(0.157)	(0.197)	(0.131)	(0.364)	(0.182)	(0.184)
Resource dependence	−0.005	−0.006	0.004	−0.007	−0.005	−0.006	−0.011	−0.004	−0.004
	(0.008)	(0.012)	(0.008)	(0.009)	(0.010)	(0.008)	(0.023)	(0.012)	(0.012)
Duration	−0.055*	−0.094**	−0.047**	−0.036**	−0.056***	−0.051***	−0.182**	−0.077**	
	(0.031)	(0.043)	(0.021)	(0.017)	(0.020)	(0.016)	(0.088)	(0.038)	
Duration^2	0.001	0.003*	0.001**	0.001**	0.001**	0.001**	0.008*	0.002	
	(0.001)	(0.002)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.004)	(0.001)	
Duration^3	−0.00001	−0.00002	−0.00000	−0.00000	−0.00000	−0.00000	−0.0001	−0.00001	
	(0.00001)	(0.00001)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.0001)	(0.00001)	
Sum elections						0.013			
						(0.035)			
Constant	−1.309	−1.663	−1.732**	−1.232	−1.367	−1.545**	−2.927	−3.396***	−3.922***
	(0.824)	(1.214)	(0.835)	(0.976)	(0.945)	(0.721)	(1.849)	(1.093)	(1.056)
Observations	2,849	1,503	3,077	2,600	3,554	3,893	3,554	3,893	3,893
Log Likelihood	-592.511	-316.486	-493.363	-472.533	-459.248	-705.065	-164.177	-349.746	-353.001
Akaike Inf. Crit.	1,207.022	654.972	1,008.726	967.066	940.496	1,434.129	350.353	721.491	722.002

Notes: *p<0.1; **p<0.05; ***p<0.01
Model 1 excludes all regimes that do not hold elections.
Model 2 excludes all observations prior to second election.
Model 3 excludes all regimes that were 4 years or younger.
Model 4 excludes all observations where last election was held under a previous regime.
Model 5 excludes all observations where leader stepped down as direct result of election.
Model 6 includes a control measuring the cumulative number of elections under the regime.
Model 7 does not count instances of democratization (from GWF) as regime breakdowns.
Model 8 replicates the baseline, but employs a dummy measuring democratic transitions (from GWF) as dependent variable; please note that issues with overfitting make results less credible.
Model 9 employs a dummy variable measuring democratic transitions (from GWF) as dependent variable, dropping the duration variables from the baseline to avoid overfitting issues.

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