1 An Empirical Investigation of the Strategic Use of Debt

- Two Models of Strategic Use of Debt
  - Persson and Svensson (1989): Heterogeneous taste (across political parties) for different levels of public good expenditures (i.e. right: low, left: high)
  - Alesina and Tabellini (1990): Heterogeneous taste (across political parties) for different types of public goods (i.e. defense vs. welfare expenditures)

- Predictions:
  - Both theories predict that right wing governments will run deficits
  - Persson and Svensson predict that left wing governments will raise taxes and run surpluses

  - Alesina and Tabellini predict that left wing governments will spend on welfare programs and run deficits

- Lidbom tries to test:

  1. If debt is used strategically

  2. Which model of strategic debt use is correct

- Empirical Approach

$$Debt_{it} = \alpha + \beta P_{it} + X_{it}\gamma + u_{it}$$

  where P_{it} is the probability of electoral defeat (given by the ex-post vote share) in town i at time t, X_{it} is a vector of variables affecting the level of debt (average income, population size, population density, proportion of elderly, and proportion of young) and $Debt_{it}$ is the debt of a Swedish municipality.

  - Swedish municipalities have elections every 3 years at the same time across municipalities.

  - Test of whether debt is used strategically

- Test of Persson/Svensson vs. Alesina/Tabellini:

$$Debt_{it} = \alpha_1 + \alpha_2 + \beta_1 P_{it} + \beta_2 D_{it} P_{it} + u_{it}$$

  where α_1 is a constant and α_2 is (maybe α_t)??? and D_{it} is a dummy for a left wing incumbent when interacted with

- IV Specification:

$$P_{it} = \alpha_1 + \alpha_2 + M_i + \epsilon_{it}$$

- Findings: Increase in debt with increase in probability of defeat for Right Wing; Decrease with probability of debt for Left Wing

- Problems?

- Why not include lagged debt level?

TABLE 1
FREQUENCY OF GOVERNMENT CHANGES AND AVERAGE VOTE SHARES

Frequency of Government Changes	Number of Municipalities	Average Vote Shares
0	117	62.2
1	28	57.0
2	42	55.9
3	40	53.5
4	29	52.9
5	13	52.8
6	8	52.0
7	0	...

NOTE.—A government change is defined as a change of power between left-wing, right-wing, and undefined governments. The calculation of average vote shares includes only left- and right-wing incumbent governments.

gression as instruments for P_{it}^{*} in the estimation of equation (2).[9] Equation (3) will be estimated with a probit model, that is, $\Pr(P_{it}^{*} = 1) = \Phi(\mathbf{W}_{it}\omega)$, where $\Phi(\cdot)$ is the standard cumulative distribution function, to ensure that the probabilities lie within the 0–1 interval.

The crucial question is then where to find variables to include in $\mathbf{W}_{it}$. To answer this question, we first need to look at municipality election data for the sample period 1974–94. The sample consists of 277 municipalities, and there is a synchronized and fixed election schedule every third year. There were seven elections in the sample period: 1976, 1979, 1982, 1985, 1988, 1991, and 1994. Thus we have a total of 1,939 observations from elections. Table 1 shows the frequency of government changes for the municipalities.[10] The number of government changes is very unequally dispersed among the different municipalities. For example, 117 municipalities (42 percent of the sample) had no change of power (69 had left-wing and 45 right-wing governments), whereas 90 (32 percent of the sample) had three changes or more. Table 1 also shows the average vote share for the incumbent in each group of mu-

[9] Pagan (1984) and Murphy and Topel (1985) show that this instrumental variable approach yields consistent estimates of both the second-stage parameters and the second-step standard errors.

[10] The classification of change of power is compiled from the distribution of seats in local councils, which, because of the proportional representation electoral system, is equivalent to vote shares. The incumbent governments are classified as left-wing, right-wing, or undefined. Left-wing governments include both the Left party and the Social Democratic party. Right-wing governments include three parties or more: the Conservative party, the Centrist party, the Liberal party, the Christian Democratic party (since 1988), and the New Democratic party (1991–94). A government is undefined when neither left-wing nor right-wing parties constitute a majority (50 percent of the seats), and an undefined government is often associated with strong local parties. The undefined government creates a problem because there is no general information about its ideological composition. Using the predictions of the strategic debt models then becomes problematic since they are based on the assumption of the incumbent's preferences (for the level or composition of spending). Because of this, I drop those observations (309) from the debt regression (2). The main results are, however, robust to including them.

TABLE 2
INCUMBENT GOVERNMENTS

	Left-Wing Incumbent	Right-Wing Incumbent
Incumbent defeated, $P^* = 1$	107	194
Incumbent reelected, $P^* = 0$	710	619
Total sum	817	813

nicipalities. Incumbents in those municipalities with no change of power, on average, obtained more than 62 percent of the votes, whereas those that had three or more changes got less than 54 percent. Table 2 presents more disaggregated information about left-wing and right-wing incumbent governments and the number of government changes. The table reveals that a left-wing government held power 817 times and was ousted 107 times, whereas a right-wing government held power 813 times and was ousted 194 times.

The unequal dispersion of government change across municipalities suggests that municipality fixed effects can be used as predictors of the probability of defeat. Thus these fixed effects measure the average frequency of government change and can be interpreted as capturing the latent instability of voters' preferences in a particular municipality. In other words, I assume the distribution of the unobserved variable, the probability of defeat, to have a particular municipality component, which allows me to use municipality dummies as instrumental variables.[11] For these variables to identify a causal effect of the probability of defeat on the level of debt, they must be validly excluded from the debt equation (2). In the empirical analysis, I test for the exclusion of municipality-specific effects from the debt equation; I cannot reject the null hypothesis of no fixed effects.[12] There are two reasons for this identifying

[11] This is similar to Wald's (1940) binary grouping estimator. Durbin (1954) was the first to note the relationship between the instrumental variable with binary instruments and the Wald estimator.

[12] This identifying assumption may be problematic if there are unobserved and unchanging characteristics related to both debt and the probability of defeat. However, using fixed effects would aggravate the bias from measurement errors (Griliches and Hausman 1986). More formally, the bias from using a fixed-effect estimator (with the notation from n. 8) is

$$\text{plim } \beta_{\text{FE}} = \beta - \frac{\beta\sigma_\eta^2}{(1-\rho)(\sigma_p^2 + \sigma_\eta^2)},$$

where $\rho = \text{Cov}(P_{it}, P_{i,t-1})/\sigma_p^2$, whereas the bias from using an OLS estimator is

$$\text{plim } \beta_{\text{OLS}} = \beta + \frac{\text{Cov}(P_{it}^*, \alpha_i) - \beta\sigma_\eta^2}{\sigma_p^2 + \sigma_\eta^2},$$

where α_i is the fixed effect. Hence, there is a trade-off between the bias from using the fixed-effect or the OLS estimator depending on the extent of fixed effects, the extent of measurement errors, and the extent to which P is correlated across time. Since the measurement

TABLE 3
ECONOMIC AND DEMOGRAPHIC VARIABLES

Variable	Mean	Standard Deviation	Minimum	Maximum
Probability of defeat, P^*	.24	.43	0	1
Left-wing incumbent government, $D=1$	.42	.49	0	1
Debt	11,209	5,407	1,061	49,420
Average income	76,022	12,464	35,147	162,799
Population size	30,226	52,978	3,480	692,954
Population density	115	372	.29	3,700
Proportion of elderly (65+)	.18	.04	.03	.29
Proportion of young (0–16)	.21	.03	.13	.37

NOTE.—Debt and average income are per capita figures given in 1991 Swedish kronor.

attribute behavioral significance to any across-municipality correlations in debt that are really due to common national influences.

The dependent variable is public debt measured in per capita terms and at constant prices.[16] There are several measures of debt in the official financial position of municipalities, but I have chosen to work with short- and long-term debt, not including social security liabilities.[17] I made this choice so as to have a comparable measure of debt in the sample period. Table 3 provides summary statistics for the variables in the empirical analysis.

IV. Results

Table 4 shows the effect of the probability of defeat on the level of debt. Column 1 is the OLS regression, using the ex post election outcome as a proxy; column 2 is the instrumental variable approach. Before focusing on the strategic debt hypotheses, I should make some general comments about these regressions. First, the regressions account for about 67 percent of the variation in the level of debt. Second, the main determinant of the level of debt is inherited debt. About 70 percent of the debt is transferred from one election period to the next. The proportion of young, the proportion of elderly, and the population size all have significant and positive effects on the level of debt.

I shall now turn to the test of the strategic debt hypotheses. As ex-

[16] I have used the implicit GDP deflator, expressed in 1991 values. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices. I have also used two other deflators, the consumer price index and a municipality-specific price index, but the results are very similar.

[17] Long-term debts are defined as debts with a maturity of one year or longer; short-term debts have a maturity of up to one year. Data on social security liabilities are available only from 1988.

TABLE 4
IMPACT OF THE PROBABILITY OF DEFEAT ON THE LEVEL OF DEBT (N=1,628)

Variable	Proxy Variable: Ex Post Election Outcomes P^* (1)	Instrumental Variable Method (2)
Socialist incumbent, $D=1$	856	1,097
	(4.54)	(4.90)
Probability of defeat, P	577	1,654
	(2.00)	(3.23)
$D \times P$	−1,953	−2,933
	(−4.27)	(−3.67)
Inherited debt	.74	.73
	(41.65)	(40.62)
Proportion of young 0–15	10,183	13,090
	(1.74)	(2.16)
Proportion of elderly 65+	7,850	9,515
	(2.11)	(2.51)
Average income	−.003	−.002
	(−.30)	(−.16)
Population size	.016	.015
	(7.92)	(7.23)
Population density	−.07	.02
	(−.24)	(.05)
Time effects	yes	yes
R^2	.6680	.6651

NOTE.—The dependent variable is the level of debt. Estimates are based on Swedish municipality data for 1974–94, excluding municipalities that cannot be classified as either left-wing or right-wing. All regressions were run with seven year-specific effects; these coefficients are not reported. Col.1 uses the ex post election outcome as a proxy for the probability of defeat. In col. 2, the probability of defeat is estimated from a probit regression with fixed municipality effects. The fitted probabilities are used as instruments for the ex post election outcome proxy. t-statistics are in parentheses. Instrumental variable standard errors were used in calculating t-statistics for the instrumental variable regression.

plained in Sections II and III, Alesina and Tabellini's hypothesis is $\beta_1 > 0$ and $\beta_2 = 0$, whereas Persson and Svensson's hypothesis is $\beta_1 > 0$, $\beta_2 < 0$, and $\beta_1 + \beta_2 < 0$. The coefficient of the probability of defeat, β_1, is positive and significant at the 5 percent level or better in both regressions.[18] This strongly suggests that a right-wing government accumulates more debt the higher the possibility of its defeat. However, the coefficient in the instrumental variable regression is nearly three times as large as the OLS regression. It thus appears important to correct for measurement errors associated with the proxy for the probability of defeat. Table 4 also reveals quite a substantial difference in the accumulation of debt between right-wing governments with a high probability of defeat compared to those with a low probability. The largest difference is found in the instrumental variable regression. On average, the level of debt is SEK 1,654 per capita higher (which is about 15 percent of the total debt) when an incumbent is certain of being de-

[18] Since both models predict that $\beta_1 > 0$, we can use a one-tail test. The critical value is 1.65 at the 5 percent level.

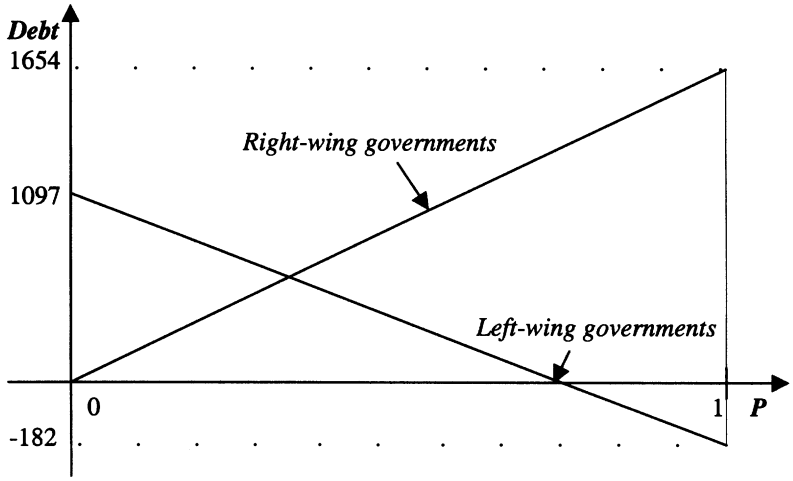


FIG. 1.—Effect of the probability of defeat on debt

feated than when it is certain of remaining in power.[19] The coefficient of the interaction term β_2 is negative and highly significant in both regressions, thus strongly suggesting that right-wing and left-wing governments have different slope coefficients. The slope coefficient for a left-wing government, $\beta_1 + \beta_2$, is negative in both the OLS and the instrumental variable regressions: SEK −1,375 and −1,279 per capita, respectively. This is also confirmed by a formal test: $\beta_1 + \beta_2 = 0$. The null hypothesis is rejected for both regressions.[20] Thus these findings give strong support to Persson and Svensson's model, but not to Alesina and Tabellini's.

Figure 1 gives a picture of the relationship between the level of debt and the probability of defeat for the instrumental variable regression (the intercept for right-wing governments has been normalized to zero). This figure shows too that left- and right-wing governments have different slope coefficients, but also that a left-wing government that is certain of being defeated accumulates slightly less debt than a right-wing government certain of remaining in power. Figure 1 also reveals that a right-wing government that is replaced with certainty has an even higher level of debt than a left-wing government that is certain of remaining in power.

[19] SEK 1,600 per capita is roughly equivalent to $270 per capita (i.e., SEK 6 equals approximately $1.00 in 1991 prices).

[20] The OLS regression: $F(1, 1{,}612) = 15.92$ (P-value .0001); the instrumental variable regression: $F(1, 1{,}612) = 4.80$ (P-value .028).

TABLE I
GUBERNATORIAL ELECTIONS, PARTY AFFILIATION, AND TERM LIMITATIONS
1950–1986

Year	Party in office = 1 if Democrat	Incumbent cannot run = 1 if term limit binds	Incumbent Democrat cannot run	Incumbent Republican cannot run
1950	0.60	0.33	0.25	0.08
1951	0.48	0.31	0.25	0.06
1952	0.48	0.33	0.27	0.06
1953	0.38	0.33	0.21	0.13
1954	0.40	0.31	0.21	0.10
1955	0.56	0.29	0.25	0.04
1956	0.56	0.29	0.25	0.04
1957	0.60	0.38	0.27	0.10
1958	0.60	0.40	0.29	0.10
1959	0.69	0.35	0.29	0.06
1960	0.69	0.35	0.29	0.06
1961	0.69	0.33	0.33	0.00
1962	0.69	0.31	0.31	0.00
1963	0.67	0.38	0.29	0.08
1964	0.67	0.38	0.29	0.08
1965	0.65	0.31	0.25	0.06
1966	0.65	0.33	0.27	0.06
1967	0.48	0.27	0.19	0.08
1968	0.48	0.27	0.19	0.08
1969	0.40	0.27	0.19	0.08
1970	0.35	0.25	0.15	0.10
1971	0.58	0.27	0.19	0.08
1972	0.58	0.27	0.19	0.08
1973	0.60	0.25	0.15	0.10
1974	0.63	0.25	0.15	0.10
1975	0.73	0.33	0.25	0.08
1976	0.73	0.35	0.27	0.08
1977	0.75	0.33	0.27	0.06
1978	0.75	0.35	0.29	0.06
1979	0.65	0.21	0.15	0.06
1980	0.63	0.19	0.13	0.06
1981	0.54	0.23	0.15	0.08
1982	0.52	0.21	0.15	0.06
1983	0.67	0.35	0.23	0.13
1984	0.69	0.35	0.23	0.13
1985	0.67	0.31	0.21	0.10
1986	0.67	0.33	0.21	0.13
Mean	0.60	0.31	0.23	0.08

TABLE II
TERM LIMITATIONS BY STATE, 1950–1986

State law:	
States with no term limits	AZ, AR, CA, CO, CT, ID[a], IL, IA, MA, MI, MN, MT, NH, NY, ND, RI, TX, UT, VT, WA, WI, WY
States limiting governors to 1 term in office	KY, MS, VA[b]
States limiting governors to 2 terms in office	DE[c], NJ, OR
State law changed from no limit to 2-term limit (year of change)	KS (1974), ME (1966), MD (1954), NB (1968), NV (1972), OH (1966), SD (1956)
State law changed from allowing 1 term to allowing 2 terms in office (year of change)	AL (1970), FL (1970), GA (1978), IN (1974), LA (1968), MO (1966)[c], NC (1978)[c], OK (1968), PA (1972), SC (1982), TN (1980), WV (1972)
State law changed from 2-term to 1-term limit (year of change)	NM (1972)

a. No term limitation after 1956.
b. Restriction on terms enacted in VA in 1954.
c. Two-term limit over a lifetime. Enacted in DE (1968), MO (1968), and NC (1978).

Table III provides means and standard deviations of the variables in our analysis, with information provided separately for states that had a term limit at some point from 1950 to 1986 and for states that did not. In those states in which governors' terms are limited by law, the limitation leads to a lame-duck governor in office in roughly half of the years in our sample (51 percent of all years). States with term limits are significantly more likely to be governed by Democrats (66 percent of all years versus 51 percent for states without term limits).

We include as explanatory variables state income per capita, the proportion of the population between the ages of 5 and 17, the proportion of the population over age 65, and state population. States without term limits are significantly larger on average. In addition, these states are significantly wealthier, as measured by income per capita. States without term limits have higher income taxes, corporate taxes, and total taxes per capita[5] than states with term limits and have higher state spending levels as well. Given the

5. Total taxes are the sum of sales, income, and corporate taxes. Total taxes per capita are lower than total state expenditures per capita; the difference is made up primarily by additions to the level of state debt outstanding and by intergovernmental grants received.

TABLE III
STATE POLICY AND ECONOMIC VARIABLES, 1950–1986[a]
(STANDARD DEVIATIONS IN PARENTHESES)

	All states All years	States with term limits	States without term limits
Number of observations	1776	1073	703
Sales tax	276.26	275.60	277.27
	(127.43)	(127.59)	(127.27)
Income tax*	96.93	89.68	108.00
	(110.04)	(105.21)	(116.24)
Corporate tax*	32.43	30.81	34.87
	(29.07)	(25.93)	(33.11)
Total tax*	405.33	395.63	420.14
	(198.00)	(187.97)	(211.67)
State spending*	849.74	811.59	907.97
	(392.60)	(367.88)	(421.23)
Minimum wage* (n = 1769)	1.85	1.59	2.26
	(1.48)	(1.48)	(1.36)
Maximum weekly benefits* (n = 1650)	177.99	162.53	201.83
	(77.99)	(64.66)	(89.93)
State income*	8588.87	8366.10	8928.89
	(2476.72)	(2517.57)	(2374.80)
Proportion elderly (65+) (n = 1728)[b]	0.099	0.099	0.100
	(0.020)	(0.022)	(0.018)
Proportion young (5–17) (n = 1728)	0.238	0.239	0.236
	(0.030)	(0.030)	(0.029)
State population (millions)*	4.080	3.542	4.902
	(4.210)	(2.673)	(5.726)
Party of governor (=1 if Dem)*	0.598	0.656	0.509
	(0.490)	(0.475)	(0.500)
Governor cannot stand for reelection	0.308	0.510	0
	(0.462)	(0.500)	

*Asterisks denote that the mean of this variable is significantly different in states with and without term limits (p-value < 0.01).
a. All taxes, income, and expenditure are per capita in 1982 dollars.
b. Information on proportion elderly and proportion young was not available for 1959.

economic and demographic differences between states with and without term limits, we will control for state-level fixed effects in all of the results presented below. In this way the effect of having a governor in place who cannot run for reelection is identified from the differences in the state's fiscal behavior when an incumbent can run again, and when one cannot. With the stability observed in the states' laws, we are not identifying the effect of term limits primarily from the change in the composition of states that limit terms but from the variation in a state's behavior when the law

TABLE IV

THE IMPACT OF TERM LIMITS ON TAXES, SPENDING, AND MANDATES,[a] 1950–1986

(*t*-STATISTICS IN PARENTHESES)

	Dep var: sales taxes	Dep var: income taxes[b]	Dep var: corporate taxes	Dep var: total taxes	Dep var: state expenditure per cap	Dep var: state minimum wage[c]	Dep var: maximum weekly benefits[d]
Incumbent cannot stand for reelection	7.86	8.74	0.57	6.71	14.38	−0.14	2.25
	(2.58)	(2.54)	(0.67)	(1.56)	(2.10)	(2.57)	(0.83)
State income per capita (1000s)	17.46	9.96	6.60	25.46	3.52	−0.04	8.64
	(4.58)	(2.52)	(5.27)	(4.87)	(0.46)	(0.88)	(3.92)
Proportion state population elderly	980.78	20.68	8.36	695.14	−1143.34	−9.22	−1358.73
	(5.38)	(0.08)	(0.13)	(2.74)	(2.21)	(3.69)	(6.65)
Proportion state population young	229.57	1564.84	221.38	1590.94	1293.53	0.18	646.86
	(2.08)	(9.39)	(5.92)	(9.95)	(4.00)	(0.10)	(6.67)
State population (millions)	−0.99	7.68	2.61	−1.41	−16.70	−0.05	−7.74
	(1.04)	(5.02)	(8.39)	(0.62)	(4.07)	(4.39)	(5.90)
R^2	0.8938	0.8721	0.8253	0.9170	0.9397	0.7619	0.7462
Number of observations	1728	1327	1364	1728	1728	1721	1604

a. See notes to Table III for sample information.
All taxes and income are per capita in 1982 dollars.
All regressions include year and state effects. Huber standard errors were used in calculating *t*-statistics.
b. Income tax regressions are restricted to states that have an income tax. Corporate taxes are treated analogously.
c. State minimum wages are in 1982 dollars.
d. Maximum worker compensation weekly benefits are in 1982 dollars.

We also observe a negative and significant effect of a binding term limit on real state minimum wages. Having a governor in his or her last term in office yields a *reduction* of the real state hourly minimum wage of between $0.12 and $0.14 (equivalent to roughly 8 percent of the mean wage for states with term limits). The effect on maximum weekly workers' compensation benefits for temporary total disability is less robust. Without controls (results not presented), there appears to be a significant positive effect. However, this finding is not robust to the presence of controls for state income and demographics.

In summary, term limits do appear to affect policy choices. We view this as consistent with a model where incumbents care about building political reputations when they can run again for office.

TABLE V

TERM LIMITS, PARTY AFFILIATION, AND FISCAL BEHAVIOR,[a] 1950–1986

(*t*-STATISTICS IN PARENTHESES)

	Dep var: sales taxes	Dep var: income taxes[b]	Dep var: corporate taxes	Dep var: total taxes	Dep var: state expenditure per cap	Dep var: state minimum wage[c]	Dep var: maximum weekly benefits[d]
Democratic incumbent cannot stand for reelection	11.25	9.43	1.86	11.30	17.28	0.03	6.41
	(3.55)	(2.56)	(1.95)	(2.42)	(2.17)	(0.51)	(2.02)
Republican incumbent cannot stand for reelection	−0.21	4.38	−1.61	−4.28	4.91	−0.46	−4.89
	(0.04)	(0.78)	(1.23)	(0.68)	(0.50)	(5.90)	(1.28)
Governor's party (=1 if Democratic)	2.72	8.07	−2.03	4.18	13.39	−0.15	−6.70
	(1.02)	(2.61)	(2.30)	(1.13)	(2.13)	(3.38)	(2.42)
Controls included: income per capita, state population, proportion elderly and young	YES	YES	YES	YES	YES	YES	YES
R^2	0.8942	0.8734	0.8261	0.9175	0.9401	0.7660	0.7474
Number of observations	1728	1327	1364	1728	1728	1721	1604

a. See notes to Table III for sample information.
All taxes and income are per capita in 1982 dollars.
All regressions include year and state effects. Huber standard errors were used in calculating *t*-statistics.
b. Income tax regressions are restricted to states that have an income tax. Corporate taxes are treated analogously.
c. State minimum wages are in 1982 dollars.
d. State maximum worker compensation weekly benefits are in 1982 dollars.

having a Democratic governor on the level of government expenditures, regardless of whether a term limit is faced. Republicans facing term limits do not change state spending levels significantly, consistent with the results observed for taxes.

Republicans in their last term change state policy on minimum wages. This result is much stronger than that presented in Table IV, where all lame ducks were grouped together. When a Republican faces a binding term limit, real minimum wages in the state fall by $0.46 on average. The level effect from having a Democratic incumbent is negative (about $0.15), but there is no additional

TABLE VI

TAXES, EXPENDITURES, AND THE ELECTORAL CYCLE
(STANDARD ERRORS IN PARENTHESES)

	Total state taxes per capita			State expenditure per capita		
Dependent variables:[a]	All governors	Democratic governors only	Dem govs, term-limit states	All govs	Dem govs only	Dem govs, term-limit states
Explanatory variables:						
Election year X governor can run for reelection	529.67 (10.01)	448.52 (26.72)	449.68 (20.11)	1059.41 (16.36)	1025.99 (19.41)	1027.61 (23.58)
Election next year X governor can run for reelection	528.41 (11.13)	442.93 (27.40)	449.89 (21.24)	1058.93 (17.96)	1019.51 (21.20)	1022.17 (25.93)
Election in 2 years X governor can run for reelection	534.26 (9.78)	452.53 (27.41)	451.78 (21.95)	1049.99 (15.78)	1014.46 (21.33)	1005.93 (28.50)
Election in 3 years X governor can run for reelection	524.84 (11.33)	444.75 (27.69)	450.14 (21.73)	1052.35 (18.40)	1022.05 (22.89)	1027.51 (28.95)
Election year X governor cannot run for reelection	541.25 (9.59)	472.43 (27.56)	469.85 (21.23)	1075.08 (15.73)	1045.18 (22.43)	1043.57 (26.50)
Election next year X governor cannot run for reelection	536.60 (9.91)	464.71 (27.74)	463.31 (21.65)	1065.50 (16.16)	1033.85 (23.18)	1034.77 (27.23)
Election in 2 years X governor cannot run for reelection	536.54 (9.29)	466.82 (27.53)	465.29 (21.33)	1072.31 (15.48)	1040.34 (22.17)	1039.59 (26.29)
Election in 3 years X governor cannot run for reelection	533.76 (10.04)	460.59 (27.73)	457.88 (21.57)	1084.45 (16.53)	1053.71 (24.34)	1051.64 (28.74)
F-test: (cycle X can run) = (cycle X cannot run)[b]	1.15 (.3312)	4.04 (.0029)	2.55 (.0383)	2.39 (.0486)	1.87 (.1141)	1.59 (.1742)
F-test: (election year X cannot run) = (election next year X cannot run)	0.57 (.4498)	1.46 (.2265)	1.15 (.2843)	0.90 (.3441)	1.01 (.3163)	0.65 (.4222)
F-test: (election year X cannot run) = (election in 2 years X cannot run)	0.67 (.4132)	0.78 (.3780)	0.58 (.4455)	0.08 (.7788)	0.21 (.6499)	0.15 (.6988)
F-test: (election year X cannot run) = (election in 3 years X cannot run)	1.22 (.2693)	3.44 (.0639)	3.72 (.0544)	0.65 (.4190)	0.47 (.4950)	0.42 (.5185)
State and year indicators	yes	yes	yes	yes	yes	yes
Number of observations	1776	1062	637	1776	1062	637

a. All regressions reported with correction for heteroskedasticity (Huber standard errors).

b. This *F*-test is a joint test of the equality of the following coefficients: (election year X can run) = (election year X cannot run), (election next year X can run) = (election next year X cannot run), (election in 2 years X can run) = (election in 2 years X cannot run), (election in 3 years X can run) = (election in 3 years X cannot run). (*p*-values are printed in parentheses for each F-statistic.)

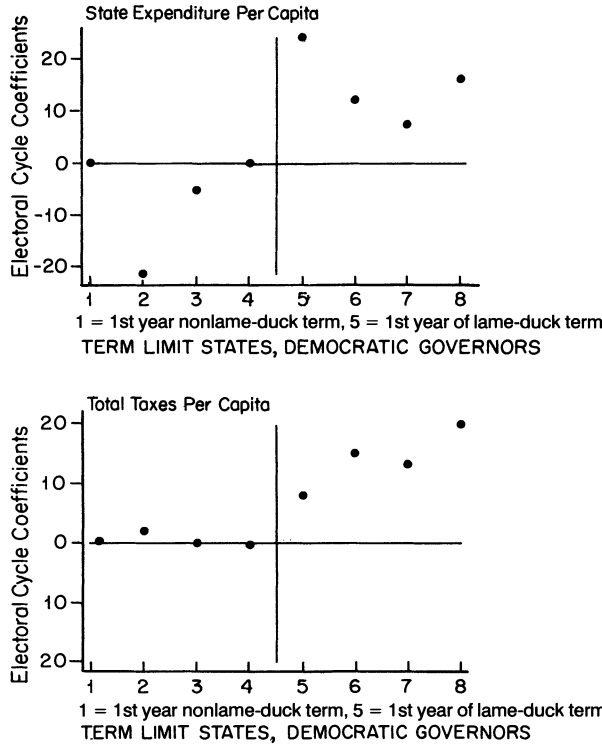


FIGURE I
The Impact of Term Limits on State Spending and Taxation

that a governor is currently in his first (second, third, etc.) year in office, taken from columns 3 and 6 of Table VI. This figure also illustrates a prediction from the model of Section II, if we interpret r as taxes and spending.[11] Governors hold taxes and expenditures low in their first term (providing a high value of r), and voters allow them a second term. At that point the governors care less about putting in effort, resulting in increased taxes and spending.

IV. EXTENSIONS AND DISCUSSION

This section considers some extensions of the earlier results, which cast further light on the interpretation of our findings. First,

11. This assumes a rather pessimistic view in which voters view government spending as valueless.

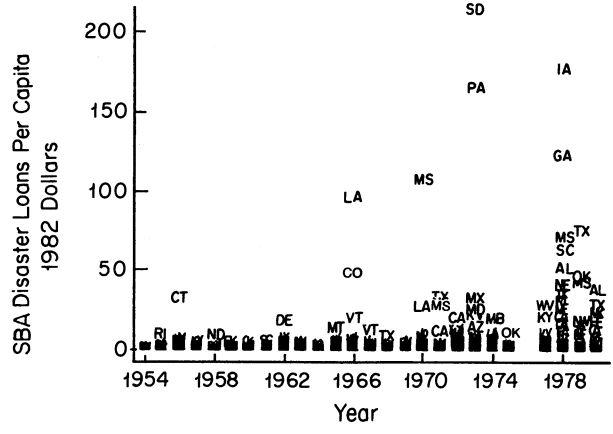


FIGURE II
Disaster Loans 1954–1980

afterward, West Virginia and Ohio were also recognized as disaster areas caught in Agnes' wake. In South Dakota the Rapid Creek flooded Rapid City in June 1972, killing more than 230 people and causing physical damage in excess of $120 million.

Most states, however, received more modest amounts of disaster relief. The disasters underlying even the smaller loans are still potentially large enough to affect the state's needs. For example, if a flood washes away parts of a state's infrastructure, the state may need to mobilize additional resources in order to dig out and rebuild. There are potentially many different ways of using these data to construct measures of whether a state faces a disaster. We choose to do so by constructing a categorical variable that equals one if SBA disaster loans per capita in that year were in the top quartile of disaster loans to all states in all years. There is nothing special about choosing the top quartile, and the results do not appear too sensitive to this choice over a reasonable range.[12] A list of states facing natural disasters is provided in Appendix 3.

Table VII provides a summary of results for the effect of natural disasters on total taxes and total state expenditures.[13] Columns 1 and 4 demonstrate that state taxes and spending increase significantly during a natural disaster, with tax and

12. Our results are robust to choosing a cutoff between the sixtieth to the eightieth percentile of disaster loans to all states in all years.

13. Results are similar if we control also for state income per capita and state population. Results are similar if we regress taxes and spending on an indicator of a disaster last year.

TABLE VII
THE IMPACT OF TERM LIMITS AND NATURAL DISASTERS ON FISCAL BEHAVIOR
(t-STATISTICS IN PARENTHESES)

Dependent variables:[a]	Total state taxes			Expenditure per capita		
Explanatory variables:						
Incumbent cannot run for reelection	13.97 (2.72)	18.55 (3.38)	—	11.85 (1.44)	15.99 (1.86)	—
Democratic governor cannot run	—	—	27.56 (4.61)	—	—	17.59 (1.81)
Republican governor cannot run	—	—	−0.80 (0.11)	—	—	4.28 (0.37)
Natural disaster	12.65 (3.20)	—	—	17.26 (2.57)	—	—
Disaster X incumbent cannot run	—	0.52 (0.08)	—	—	6.29 (0.58)	—
Disaster X incumbent can run	—	17.19 (3.70)	—	—	21.36 (2.72)	—
Disaster X Dem incumbent cannot run	—	—	−4.99 (0.65)	—	—	7.09 (0.58)
Disaster X Rep incumbent cannot run	—	—	14.98 (1.42)	—	—	−3.74 (0.19)
Disaster X Dem incumbent can run	—	—	16.58 (2.87)	—	—	13.15 (1.35)
Disaster X Rep incumbent can run	—	—	18.49 (2.35)	—	—	28.20 (2.30)
Governor's party = Democratic	—	—	−3.48 (0.86)	—	—	9.94 (1.44)
State and year indicators	yes	yes	yes	yes	yes	yes
R^2	.9218	.9221	.9229	.9426	.9426	.9429

a. All taxes and expenditures are in per capita 1982 dollars. Total state taxes are the sum of state sales, income, and corporate taxes. Expenditures per capita are the sum of all state spending. Data are from years 1954 to 1980, with the omission of 1976 (1248 observations in each regression).
All regressions are reported with correction for heteroskedasticity (Huber standard errors).

spending increases in the range of $15 per capita.[14] Columns 2 and 5 demonstrate that it is only governors who may run for reelection who change their behavior in the face of a natural disaster. Lame ducks, who increase taxes and state spending independently of a disaster, do not increase taxes or spending further in response to a disaster. Columns 3 and 6 of Table VII allow Democratic and Republican governors to differ in their responses. It appears that the Democratic lame ducks, that is, those governors who increased spending and taxes in the face of binding term limits, are least

14. Increases in state spending in the face of natural disasters are concentrated in highway and public welfare spending. Additional results are available from the authors.

TABLE VIII
TERM LIMITS, RETIREMENTS, AND CONGRESSIONAL BIDS,[a] 1950–1986
(t-STATISTICS IN PARENTHESES)

	Dep var: total state taxes per cap				Dep var: state expenditure per cap			
Governor cannot stand for reelection	7.97 (1.83)	—	—	8.21 (1.87)	17.98 (2.60)	—	—	18.52 (2.68)
Governor retires and does not run for Congress	—	3.13 (0.59)	—	3.83 (0.72)	—	7.27 (0.75)	—	8.83 (0.92)
Governor retires and does run for Congress	—	—	−9.27 (1.65)	−9.20 (1.64)	—	—	−25.07 (2.50)	−24.91 (2.49)
R^2	.9102	.9101	.9102	.9104	.9374	.9372	.9374	.9377
Number of observations	1776	1776	1776	1776	1776	1776	1776	1776

a. Taxes and income are per capita in 1982 dollars.
All regressions include year and state effects. Huber standard errors were used in calculating t-statistics.

congressional literature, as reviewed, for example, in Lott and Davis [1992]. The absence of a retirement effect is usually attributed to the effects of sorting; i.e., the fact that over time there is sorting with only the good politicians surviving to retirement age (see Lott and Reed [1989]). Such effects could explain the lack of a retirement effect in the gubernatorial data too. As we conjectured, incumbents who will run for Congress at the end of their current gubernatorial term significantly hold taxes and spending down.[16] This is consistent with the results in Peltzman [1992] and Besley and Case [1995] in which voters penalize incumbents who are big taxers and spenders. Besley and Case [1995] build a model in which it is rational for voters to impose these penalties because of an adverse selection effect from higher taxes; the latter are more likely to be set by rent-seeking incumbents. Thus, our finding on governors who run for Congress is quite consonant with the idea that incumbents are trying to build reputations as good political agents.

To summarize, we continue to get positive effects from those

16. Care should be taken in interpreting this coefficient. We cannot measure intentions to run again, only whether the incumbent *actually* ran. There may be a bias toward our finding if only those who hold down taxes are actually able to run, even though many other incumbents may have harbored such intentions.

TABLE IX
THE IMPACT OF TERM LIMITS ON STATE INCOME PER CAPITA,[a] 1950–1986
DEP VAR: LOG (STATE INCOME PER CAPITA)
(*t*-STATISTICS IN PARENTHESES)

Democratic governor (=1)	−0.0011	−0.0011
	(0.28)	(0.35)
Dem gov who cannot run for reelection	−0.0218	−0.0115
	(4.29)	(2.91)
Rep gov who cannot run for reelection	0.0069	−0.0009
	(0.98)	(0.14)
State demographic vars?[b]	no	yes
Year effects?	yes	yes
State effects?	yes	yes
Number of obs	1776	1728
R^2	.9585	.9713

a. Huber standard errors.
b. State population, proportion population elderly, and proportion population young.

who face a binding term limit even when we break out retirements from those who face such limits. However, the results in Table VIII suggest grounds for caution in using the earlier evidence on announced retirements for conjecturing what would happen if a term limit were introduced into Congress.

IV.4. Costs and Benefits of Term Limits

Our analysis so far has been purely positive. However, if a Democratic incumbent who is ineligible to stand for reelection holds taxes and spending down in his first term in office, and raises taxes and spending to a high level in his last term in office, then this suggests an inefficiency. In particular, a distortion in resource mobilization and public good provision may arise if the marginal deadweight loss of taxation is increasing in taxes raised.[17] We would expect this to show up in lower state income per capita when a lame-duck Democratic governor is in office. Table IX presents the results of regressions of log state income per capita on indicators for whether the governor is a Democrat, a lame-duck Democrat, or a lame-duck Republican, together with year indicators, state indicators, and (in column 2) demographic information about the state. States led by Democrats show no difference in state income per capita, while those led by a lame-duck Democrat show a negative and significant effect on income per capita, controlling for

17. That the deadweight loss depends upon the square of the tax rate is a standard proposition in public finance. Barro [1979] exploited this to argue that governments would ideally avoid cyclical changes in taxes.

2 Hit or Miss? The Effects of Assassinations on Institutions and War

- "People that are really very weird can get into sensitive positions and have a tremendous impact on history" - James Danforth Quayle

- Role of individual and of institutions in history: big question

- Estimation Strategies - Fixed Effects? Fixed Effects on Assassination Attempts? Event Study Methodology?

2.1 Data

- Time Series on Leaders: Archigos dataset, v 2.5 (Goemans *et al.*) lists primary leader for each country at each point (dates of rule) in time from 1875 to 2004 (187 countries)

- Assassinations Data: keyword search through New York Times, Washington Post and Wall Street Journal
  - Exclude Coups
  - Exclude Uncovered Plots
  - Limit to cases where weapon was discharged
  - Record weapon used

- Outcome Variables

  - Polity IV Data: Democracy (Polity2 binary variable)

  - COW Data:

    * War (more than 1000 battle deaths from 1816-2002)

    * Log per-capita energy consumption (claim no per capita income measure available pre-1950: Madison data set?)

  - PRIO/Uppsala Data: War (more than 25 battle deaths from 1946-2002)

2.2 Stylized Facts

- Interesting point: decline in assassinations and attempts during WWII. Why? How are leaders for occupied countries classified?

- Numbers of countries change over the sample

2.3 Specifications

- First test identifying assumption of randomness:

$$P\left(success_a\right) = \Phi\left(\gamma_1 + X_a'\gamma_2\right)$$

  also test for mean differences in variables year before event

- Main specification (OLS):

$$y_i = \beta success_i + \gamma X_i + \epsilon_i$$

  - where i is a country-year where an assassination attempt occured and $success$ is a dummy variable equal to 1 if the assassination attempt was successful and zero if it was a failure; X_iT is a vector including weapon dummy fixed effects, fixed effects for number of attempts in a country-year

  - why are these regreessors included? most of them for efficiency? endogeneity?

  - cluster on country
  - Testing
    * Larger sample: use rank test (same as in event study literature)
    * Smaller sample: use Fisher exact test (probability of observing exact or lower joint distribution given the marginal distributions)

- Identifying assumption:

$$E\left(\epsilon_i | X_i, success_i\right) = 0$$

In this case we can interpret β as the casual impact of a successful assassination attempt relative to a failed one on the outcome y_i:

$$\beta = E\left(y_i | X_i, success = 1\right) - E\left(y_i | X_i, success = 0\right)$$

Note that this is different from the impact of assassination relative to no assassination (either a failed attempt or no attempt).

- Problems:
  - Older leaders more likely to die... also different impact across different leaders
  - Countries where attempts are succesful are different or have different impacts of successful assassination (probability not the same in all countries of successful assassination and this is correlated with impact of assassination)
  - Are heterogeneous impacts well identified?

Table 1: Assassinations of Primary National Leaders Since 1875

Country of Leader	Year of Assassination	Name of Leader	Weapon Used
Afghanistan	1919	Habibullah	gun
Afghanistan	1933	Nadir Shah	gun
Algeria	1992	Boudiaf	gun
Austria	1934	Dollfuss	gun
Bulgaria	1943	Boris III	gun
Burundi	1994	Ntaryamira	other
Congo (Brazzaville)	1977	Ngouabi	gun
Congo (Kinshasa)	2001	Kabila	gun
Dominican Republic	1899	Heureaux	gun
Dominican Republic	1911	Caceres	gun
Dominican Republic	1961	Trujillo	gun
Ecuador	1875	Moreno	other
Egypt	1981	Sadat	gun
Greece	1913	George I	gun
Guatemala	1898	Reina Barrios	unknown
Guatemala	1957	Castillo Armas	gun
Haiti	1912	Leconte	explosive device
India	1984	Indira Gandhi	gun
Iran	1896	Nasir Ad-Din	gun
Ireland	1922	Collins	gun
Israel	1995	Rabin	gun
Japan	1921	Hara	knife
Japan	1932	Inukai	gun
Jordan	1951	Abdullah	gun
Korea	1979	Park	gun
Lebanon	1989	Moawad	explosive device
Madagascar	1975	Ratsimandrava	unknown
Mexico	1920	Carranza	unknown
Nepal	2001	Birendra	gun
Nicaragua	1956	Somoza	gun
Pakistan	1951	Khan	gun
Pakistan	1988	Zia	other
Panama	1955	Remon	gun
Paraguay	1877	Gill	unknown
Peru	1933	Sanchez Cerro	gun
Poland	1922	Narutowicz	gun
Portugal	1908	Carlos I	gun
Portugal	1918	Paes	gun
Russia	1881	Alexander II	explosive device
Rwanda	1994	Habyarimana	other
Salvador	1913	Araujo	gun
Saudi Arabia	1975	Faisal	gun
Somalia	1969	Shermarke	gun
South Africa	1966	Verwoerd	knife
Spain	1897	Canovas	gun
Spain	1912	Canalejas	gun
Spain	1921	Dato	gun
Sri Lanka	1959	Bandaranaike	gun
Sri Lanka	1993	Premadasa	explosive device
Sweden	1986	Palme	gun
Togo	1963	Olympio	gun
United States	1881	Garfield	gun
United States	1901	McKinley	gun
United States	1963	Kennedy	gun
Uruguay	1897	Idiarte Borda	gun
Venezuela	1950	Delgado	gun
North Yemen	1977	Al-Hamdi	gun
North Yemen	1978	Al-Ghashmi	explosive device
Yugoslavia	1934	Alexander	gun

Table 2: Assassination Attempts: Summary Statistics

			Probability Leader Killed		Bystander Casualties	
	Obs	Percentage	All Attempts	Serious Attempts	Mean Killed	Mean Wounded
Type of Weapon						
Gun	161	55%	28%	31%	1.0	2.2
Explosive device	91	31%	5%	7%	5.8	18.2
Knife	23	8%	13%	21%	0.3	0.4
Other	19	6%	16%	18%	1.1	0.3
Unknown	10	3%	40%	44%	2.0	1.3
Location						
Abroad	12	4%	25%	30%	3.6	6.5
At home	286	96%	20%	23%	2.4	6.7
Number of Attackers						
Solo	132	59%	24%	29%	0.4	2.5
Group	92	41%	22%	26%	5.6	11.0
Total Attempts	298	n/a	20%	24%	2.4	6.7

Notes: There are 298 total assassination attempts observed and 251 serious attempts. Serious attempts are defined as cases where the weapon was actually used. Note that the location of the attack is observed in every case, but the type of weapon is observed in 288 cases and the number of attackers observed in 224 cases. For some attempts, multiple types of weapons were used, so that the weapon observation counts sum to 304. Attacks with weapons classified as "other" include arson, rocket attacks, stoning, and automobile crashes, among others. Also note that casualties among bystanders are skewed distributions so that the means are much larger than medians.

Table 3: Are successful and failed attempts similar?

Panel A: Pairwise t-tests of sample balance.

Variable	Success	Failure	Difference	Pval on Difference
Democracy dummy	0.362	0.344	0.018	0.80
	(0.064)	(0.035)	(0.072)	
Change in democracy dummy	-0.036	-0.022	-0.013	0.67
	(0.025)	(0.019)	(0.032)	
War dummy	0.263	0.318	-0.055	0.42
	(0.059)	(0.034)	(0.068)	
Change in war	0.036	0.011	0.025	0.71
	(0.058)	(0.034)	(0.067)	
Log energy use per capita	-1.589	-1.740	0.152	0.69
	(0.338)	(0.180)	(0.383)	
Log population	9.034	9.526	-0.492	0.05*
	(0.219)	(0.117)	(0.248)	
Age of leader	55.172	52.777	2.395	0.14
	(1.351)	(0.866)	(1.604)	
Tenure of leader	9.328	7.619	1.709	0.27
	(1.440)	(0.544)	(1.539)	
Num obs	59	194		

Notes: This table reports the means of each listed variable for successes and failures, where each observation is a serious attempt. Standard errors in parentheses. P-values on differences in the mean are from two-sided unpaired t-tests. All variables are examined in the year before the attempt took place. Change variables represent the change from 3 years before the attempt occurred to one year before the attempt occurred. * significant at 10%; ** significant at 5%; *** significant at 1%

Panel B: Multivariate regressions

	(1)	(2)	(3)	(4)
Democracy dummy	0.068	0.063	0.071	0.070
	(0.068)	(0.066)	(0.070)	(0.067)
Change in democracy dummy	-0.039	-0.050	-0.033	-0.036
	(0.100)	(0.103)	(0.104)	(0.109)
War dummy	0.057	0.063	0.061	0.067
	(0.069)	(0.065)	(0.070)	(0.065)
Change in war	-0.024	-0.017	-0.025	-0.013
	(0.077)	(0.083)	(0.076)	(0.083)
Log energy use per capita	0.002	0.001	0.008	0.009
	(0.014)	(0.014)	(0.015)	(0.015)
Log population	-0.027	-0.025	-0.028	-0.032
	(0.021)	(0.021)	(0.021)	(0.020)
Age of leader	0.003	0.003	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Tenure of leader	0.004	0.004	0.005	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Weapon FE	NO	YES	NO	YES
Region FE	NO	NO	YES	YES
Observations	208	208	208	208
P-val of F-test on all listed variables	0.46	0.49	0.46	0.40
P-val of F-test on all listed variables and fixed effects	0.46	0.06*	0.59	0.01***

Notes: This table reports marginal effects from a probit regression, where each observation is a serious attempt and the dependent variable equals 1 for successful assassinations. Robust standard errors in parentheses, adjusted for clustering on country. Weapon FE refers to dummies for each weapon type (gun, knife, explosive, poison, other, unknown), and region FE refers to dummies for each region of the world (Africa, Asia, Middle East / North Africa, Latin America, Eastern Europe, Western Europe / OECD).
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Assassinations and Institutional Change

	(1)	(2)	(3)
	Absolute change in POLITY2 dummy	Directional change in POLITY2 dummy	Percentage of 'regular' leader transitions in next 20 years
Panel A: Average effects			
Success	0.091	0.079	0.111
	(0.047)	(0.051)	(0.057)
Parm p-val	0.06*	0.12	0.06*
Nonparm p-val	0.03**	0.02**	0.18
Obs	221	221	138
Data source	Polity IV	Polity IV	Archigos
Panel B: Split by regime type in year before attempt			
Success × Autocracy		0.131	0.191
		(0.055)	(0.085)
Success × Democracy		-0.012	0.034
		(0.083)	(0.043)
Autoc-Parm p		0.02**	0.03**
Autoc-Nonparm p		0.01***	0.05**
Democ-Parm p		0.89	0.43
Democ-Nonparm p		0.13	0.96
Obs		221	133
Data source	Polity IV	Polity IV	Archigos

Notes: Results from estimating equation (1). Success is a dummy for whether the assassination attempt succeeded. The dependent variable in column (1) is a dummy for whether there was a change from autocracy to democracy or vice versa (change = 1, no change = 0). The dependent variable in column (2) indicates the direction of any change (change to democracy = 1, no change = 0, change to autocracy = -1). The dependent variable in column (3) is the percentage of future leader transitions that are "regular" as opposed to "irregular" (i.e. coups). This measure excludes the transition of the leader in power during the attempt. The sample in all columns is limited to serious attempts. Standard errors and parametric p-values are computed using robust standard errors, adjusted for clustering at the country level; these specifications all include dummies for weapon type and the number of attempts in that year. Non-parametric p-values are computed using Fisher's exact (1935) p-values in columns (1) and (2) and using a Wilcoxon (1945) rank-sum test in column (3). In Panel B, autocracy / democracy is defined by the POLITY2 dummy in the year before the attempt. The main effect for the lagged autocracy variable is also included in the Panel B regressions. Absolute change in POLITY2 dummy is not shown in Panel B as it is mechanically identical to the directional change in POLITY2 dummy once we split by lagged POLITY2 dummy status. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Tenure of leader and duration of effects

	(1)	(2)	(3)	(4)	(5)	(6)
		All leaders			Autocrats only	
	All	Tenure <= 10	Tenure > 10	All	Tenure <= 10	Tenure > 10
Panel A: Directional change in POLITY2 dummy						
1 year out	0.079	0.058	0.129	0.130	0.088	0.214
	(0.051)	(0.051)	(0.125)	(0.057)	(0.069)	(0.110)
Parm p-val	0.12	0.26	0.31	0.03**	0.21	0.06*
Nonparm p-val	0.02**	0.31	0.01***	0.01***	0.10*	0.01***
10 years out	0.046	0.013	0.092	0.190	0.226	0.169
	(0.062)	(0.075)	(0.146)	(0.079)	(0.108)	(0.132)
Parm p-val	0.46	0.86	0.53	0.02**	0.04**	0.21
Nonparm p-val	0.01**	0.12	0.03**	0.05**	0.14	0.05**
20 years out	-0.003	-0.006	0.001	0.023	0.091	0.013
	(0.091)	(0.116)	(0.154)	(0.090)	(0.117)	(0.157)
Parm p-val	0.98	0.96	0.99	0.80	0.44	0.94
Nonparm p-val	0.86	0.78	0.72	0.59	0.79	0.48
Panel B: Percentage of transitions by 'regular' means						
1-10 years out	0.099	0.126	0.087	0.186	0.197	0.102
	(0.077)	(0.089)	(0.243)	(0.113)	(0.145)	(0.255)
Parm p-val	0.21	0.16	0.73	0.11	0.18	0.70
Nonparm p-val	0.35	0.18	0.53	0.16	0.25	0.28
1-20 years out	0.111	0.116	0.274	0.165	0.147	0.306
	(0.057)	(0.063)	(0.181)	(0.095)	(0.113)	(0.227)
Parm p-val	0.06*	0.07*	0.15	0.09*	0.20	0.20
Nonparm p-val	0.18	0.23	0.03	0.05**	0.15	0.03**
11-20 years out	0.119	0.1	0.368	0.208	0.181	0.422
	(0.068)	(0.072)	(0.246)	(0.107)	(0.110)	(0.275)
Parm p-val	0.09*	0.17	0.16	0.06*	0.11	0.15
Nonparm p-val	0.25	0.59	0.04	0.03**	0.16	0.05**

Notes: Each cell reports the coefficient and p-values on "success" from a separate regression of equation (1). Columns (1) and (4) reports results for all leaders, columns (2) and (5) for those with tenure <= 10 years in year before assassination, and columns (3) and (6) for those with tenure > 10 years in year before year of attempt. For the POLITY2 dummy, 1 year out compares the change in polity score 1 year after attempt to 1 year before attempt; 5 years out compares the change in polity score 5 years after attempt to 1 year before attempt, etc. For regular transitions, 1-10 years out calculates the average percentage of leadership transitions that are regular in years 1-10 after the attempt; etc. Standard errors and p-values are as in Table 4. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Assassinations and Conflict: Change 1 Year After Attempt

	(1) Gleditsch-COW Dataset 1875-2002	(2) Gleditsch-COW Dataset 1946-2002	(3) PRIO/Uppsala Dataset 1946-2002
Panel A: Average effects			
Success	-0.072	0.041	0.162
	(0.068)	(0.093)	(0.071)
Parm p-val	0.29	0.66	0.02**
Nonparm p-val	0.57	0.83	0.03**
Obs	223	116	116
Data source	Gleditsch	Gleditsch	PRIO
Panel B: Split by war status in year before attempt			
Success × Intense War	-0.255	-0.103	-0.110
	(0.144)	(0.257)	(0.294)
Success × Moderate War			0.334
			(0.163)
Success × Not At War	-0.024	0.020	0.070
	(0.068)	(0.086)	(0.057)
Intense War-Parm p	0.08*	0.69	0.71
Intense War-Nonparm p	0.13	1.00	0.69
Moderate War-Parm p	N/A	N/A	0.05**
Moderate War-Nonparm p	N/A	N/A	0.13
Not At War-Parm p	0.73	0.82	0.22
Not At War –Nonparm p	0.62	0.71	0.21
Obs	222	116	116
Data source	Gleditsch	Gleditsch	PRIO

Notes: See notes to Table 4. Non-parametric p-values are computed using Fisher's exact tests. In Panel B, at war / not at war is defined by whether the relevant war concept (i.e., the concept used in the dependent variable) is positive in the year before the attempt. The main effect for the lagged war variable is also included in the regression in Panel B. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Alternative specifications

	(1)	(2)	(3)	(4)	(5)
	Absolute change in POLITY2 dummy 1 year out	Directional change in POLITY2 dummy 1 year out		Percentage regular leader transitions 1-20 years out	
	All	All	Autocrats only	All	Autocrats only
Baseline specification	0.091	0.079	0.131	0.111	0.191
(Serious attempts)	(0.047)	(0.051)	(0.055)	(0.057)	(0.085)
Parm p-val	0.06*	0.12	0.02**	0.06*	0.03**
Nonparm p-val	0.03**	0.02**	0.01***	0.18	0.05**
Obs	221	221	142	138	74
Control group: Bystanders	0.078	0.076	0.130	0.151	0.255
Or target wounded	(0.049)	(0.052)	(0.055)	(0.074)	(0.097)
Parm p-val	0.11	0.15	0.02**	0.05**	0.01***
Nonparm p-val	0.07*	0.06*	0.02**	0.13	0.01***
Obs	157	157	103	97	54
Control group: Target	0.081	0.057	0.120	0.182	0.264
Wounded	(0.050)	(0.053)	(0.055)	(0.095)	(0.126)
Parm p-val	0.11	0.28	0.03**	0.06*	0.04**
Nonparm p-val	0.11	0.25	0.12	0.35	0.04**
Obs	104	104	66	68	38
Control group: Any attempt	0.090	0.068	0.132	0.116	0.172
	(0.047)	(0.051)	(0.056)	(0.054)	(0.081)
Parm p-val	0.06*	0.18	0.02**	0.04**	0.04**
Nonparm p-val	0.02**	0.01***	0.01***	0.37	0.10*
Obs	260	260	166	173	94
Solo attempts only	0.073	0.027	0.095	0.144	0.258
	(0.063)	(0.066)	(0.066)	(0.060)	(0.115)
Parm p-val	0.25	0.68	0.15	0.02**	0.03**
Nonparm p-val	0.26	0.41	0.21	0.41	0.11
Obs	100	100	53	65	30
First attempt on leader	0.080	0.048	0.099	0.111	0.206
Serious attempts only	(0.060)	(0.066)	(0.067)	(0.061)	(0.093)
Parm p-val	0.18	0.47	0.14	0.07*	0.03**
Nonparm p-val	0.12	0.11	0.07*	0.41	0.11
Obs	172	172	102	108	52
Adding all Table 3 controls quarter-century FE , and region FE (Serious attempts)	0.081	0.088	0.176	0.192	0.237
	(0.056)	(0.057)	(0.084)	(0.063)	(0.110)
Parm p-val	0.15	0.13	0.04**	0.00***	0.04**
Obs	189	189	115	112	57

Notes: See text. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: What predicts attempts?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Democracy dummy	-0.007*						-0.001
	(0.004)						(0.003)
War dummy		0.028***					0.018***
		(0.006)					(0.006)
Log energy use per			-0.003***				-0.002***
Capita			(0.001)				(0.001)
Log population				0.005***			0.005***
				(0.001)			(0.001)
Age of leader					-0.00022*		-0.00030**
					(0.00012)		(0.00015)
Tenure of leader						-0.00011	-0.00010
						(0.00020)	(0.00024)
Observations	11171	11671	9664	10607	12019	12133	9185
P-value of regression	0.08*	0.00***	0.00***	0.00***	0.08*	0.60	0.00***

Notes: Results are marginal effects from a probit specification. Robust standard errors in parentheses, adjusted for clustering at the country level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 11—Separating Impacts of Successes and Failures on Conflict

	Gleditsch-COW dataset 1875–2002		Gleditsch-COW dataset 1946–2002		PRIO/Uppsala dataset 1946–2002	
	No controls (1)	Adding controls and propensity score stratification (2)	No controls (3)	Adding controls and propensity score stratification (4)	No controls (5)	Adding controls and propensity score stratification (6)
Panel A: Average effects						
Success	−0.069	−0.024	0.035	0.019	0.080	0.076
	(0.060)	(0.049)	(0.075)	(0.068)	(0.062)	(0.061)
Failure	0.001	0.054	−0.022	0.004	−0.056	−0.042
	(0.038)	(0.034)	(0.047)	(0.042)	(0.037)	(0.038)
Success *p*-value	0.25	0.63	0.64	0.79	0.20	0.21
Failure *p*-value	0.98	0.12	0.65	0.92	0.13	0.27
Observations	11,286	11,286	7,183	7,183	7,183	7,183
Data source	Gleditsch	Gleditsch	Gleditsch	Gleditsch	PRIO	PRIO
Panel B: Split by war status in year before attempt						
Success × intense war	−0.248	−0.249	−0.095	−0.106	−0.044	−0.038
	(0.125)	(0.123)	(0.219)	(0.226)	(0.272)	(0.295)
Failure × intense war	0.006	0.011	−0.042	−0.028	0.059	0.071
	(0.063)	(0.060)	(0.081)	(0.084)	(0.072)	(0.075)
Success × moderate war					0.208	0.201
					(0.137)	(0.144)
Failure × moderate war					−0.091	−0.094
					(0.074)	(0.067)
Success × not at war	0.066	0.056	0.074	0.044	0.070	0.043
	(0.051)	(0.050)	(0.066)	(0.067)	(0.055)	(0.056)
Failure × not at war	0.104	0.072	0.049	0.016	0.036	0.007
	(0.043)	(0.039)	(0.041)	(0.040)	(0.035)	(0.035)
Intense war *p*-value—success	0.05**	0.04**	0.67	0.64	0.87	0.90
Intense war *p*-value—failure	0.93	0.85	0.60	0.74	0.42	0.34
Moderate war *p*-value—success					0.13	0.16
Moderate war *p*-value—failure					0.22	0.16
No war *p*-value—success	0.20	0.27	0.27	0.52	0.21	0.44
No war *p*-value—failure	0.02**	0.07*	0.23	0.70	0.32	0.83
Observations	11,286	11,286	7,183	7,183	7,183	7,183
Data source	Gleditsch	Gleditsch	Gleditsch	Gleditsch	PRIO	PRIO

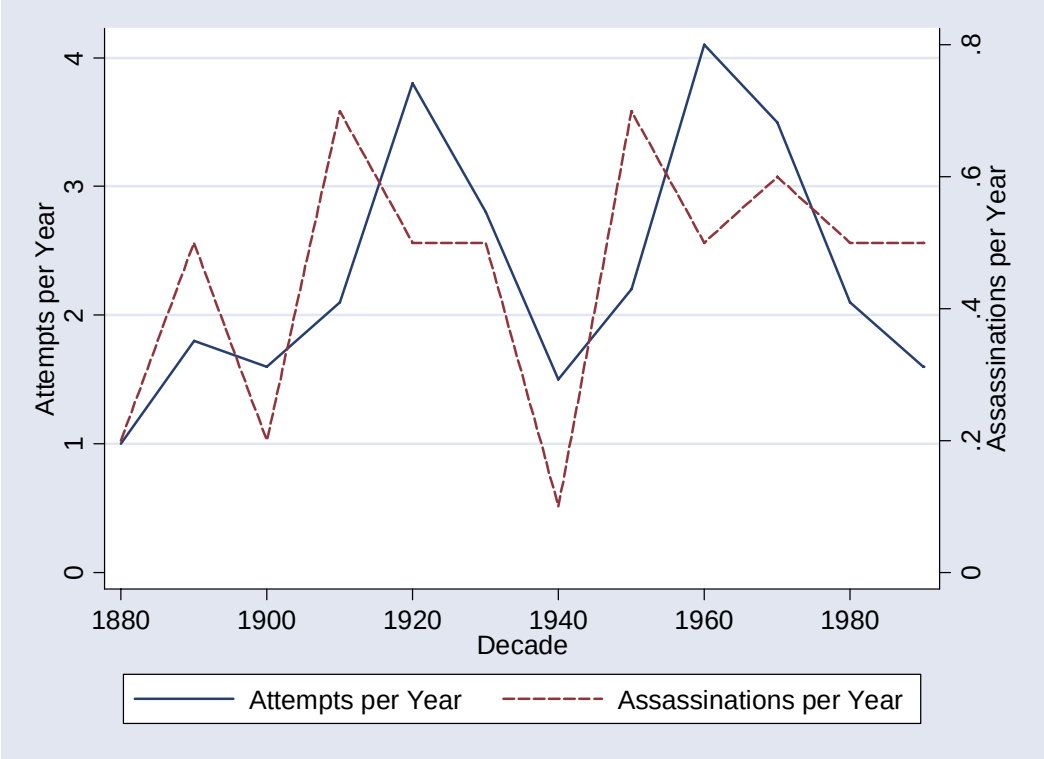
Note: Controls includes all variables shown in Table 9; quarter-century fixed effects; and region fixed effects.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

IV. Interpretations and Implications

Beyond providing an analysis of assassination per se, the facts in this paper inform theories of institutional change and conflict more broadly. We discuss several interpretations and potential implications.

Figure 1: Trends in the Frequency of Assassinations and Assassination Attempts

Panel A: Annual Attempts and Assassinations Worldwide



Panel B: Annual Attempts and Assassinations per Country