On the Clock: Event History Modeling in the Study of Leadership Tenure

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Abstract: While the study of leadership survival across countries and political systems has become a central part of comparative politics, international political economy, and international relations, surprisingly little discussion is dedicated to the challenges in empirically modeling leadership tenure. These models are expected to have unobserved heterogeneity on at least two levels: (1) the leader's country and (2) the nominal regime category in which the leader serves. These problems can result in substantially biased results. Unfortunately, the literature thus far has limited itself to two tools for dealing with this, shared frailty and stratification respectively. This is largely because the event history literature draws heavily from biostatistics, where these are the standard best practices. While these tools are appropriate for a large range of data and are very efficient, they also carry some assumptions that will not always hold, especially for data in comparative politics. This paper suggests the use of several other tools, which are not common practice in event history, to test these assumptions. Scholars in comparative politics can use these tools to verify their results or raise important questions about their variables and measurement. The potential shortcomings of current practice and the utility of these robustness checks are demonstrated through a re-analysis of "selectorate theory" as an explanation of leadership tenure.

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Recently there has been an upswing in the number of studies in comparative politics and international relations that are interested in the factors that influence the tenure of leaders across countries and political systems (Bienen and van de Walle, 1991; Bueno de Mesquita and Siverson, 1995; Bueno de Mesquita, Morrow, Siverson and Smith, 1999 – hereafter BMSS; BMSS, 2003; Chiozza and Goemans, 2004; Colaresi, 2004). The motivations behind these studies are diverse, but all of them are in agreement that understanding the institutional and environmental factors that influence the length of leadership tenure is fundamental to political science in a number of areas.

To analyze leadership survival, the studies mentioned above use a class of estimators usually referred to as event history models (also know as hazard models, survival models, and duration models). Some of these studies have been quite sophisticated, but there remain some significant problems. In particular, unobserved heterogeneity, based on the country which the leader controls and systematic differences in the cost of removal, are often ignored or are explored in a manner that might miss important patterns in the data. These problems are particularly pernicious when trying to draw conclusions across disparate countries and different political systems.

This study looks at the problem of unobserved country-level effects and systematic leadership removal in event history models. It suggests that, although most of the literature recommends a shared frailty/random effects approach, the use of fixed effects to check consistency can be useful for researchers, especially in identifying patterns that are not readily apparent from random events models. Second, it finds that it is important for researchers to take into account the costs of leadership removal, and calls into question comparisons across elected and non-elected leadership tenure, at least without some explicit controls. This also suggests that researchers should be careful of how they construct regime categories. All of this is

demonstrated in a re-analysis of Bueno de Mesquita, Morrow, Siverson and Smith's (1999a) *Journal of Conflict Resolution* article. Here it is demonstrated how failure to take into account country-level effects and removal costs can result in misleading inference.

1. Leadership survival in political science

For political scientists, the understanding of what allows leaders to survive in office is fundamental to the field. Yet it is only recently that we have witnessed the use of large-N, cross-national analysis of the leadership survival question across political systems. Up until the last decade and a half of research, the study of political tenure was mostly confined to particular cases or small comparison groups.

Much of the credit for the recent surge in literature on the topic this lies with the collection and public distribution of Bueno de Mesquita, Morrow, Siverson and Smith's (1999a) list of leader tenure, which provides detailed information on the leader in charge and the time of their installation and removal. This section explores some of this literature and also reviews the importance of leadership tenure in the study of comparative politics, political economy and international relations.

1.1. Comparative Politics

Of all the sub-disciplines in political science, one would expect comparative politics to have been most active in exploring the factors contributing to executive survival or failure. As Laver and Shepsle (1998, p. 28-29) note about the survival of governments in parliamentary democracies, "In practical terms, the survival of any real-world government is of obvious interest to the wide range of actors who must deal with it. These include, among many others: opposition parties, citizens, interest groups, revolutionaries, foreign governments and international organizations." Yet this literature has some substantial limitations.

Part of the reason for this has been the dominance of another set of stability questions --

the stability, or lack thereof, of institutional arrangements (regimes). The literature on democratic stability and instability is large and well-developed (ex. Przeworski and Limongi, 1997; Boix, 2003; O'Donnell, 1996; and Lipset, 1959). This is especially true when discussing the effect of institutions, where debate has centered on the effects of different government arrangements, political party systems, and electoral rules (ex. Lijphart, 1968; Linz, 1990; Bernhard, Nordstrom and Reenock, 2001).

When discussion turns to the survival of a particular executive or leader, the literature becomes much sparser. As should be expected, the most common discussions center around the survival of particular leaders at a particular time (ex. on Malaysia, Nathan, 1998; on Thailand, Snitwongse, 1994; and on North Korea, Ahn, 2002). These discussions are usually dominated by themes of social or economic unrest, and discussions of institutions are usually limited to the case in point.

Within democracies there has emerged some discussion of the durability of governments in parliamentary democracies, although, even here, the first book-length exploration of the topic emerged relatively recently (Warwick, 1994; see also Browne, Frendreis and Geliber, 1984; King et. al, 1990; Laver and Shepsle, 1998; Warwick, 1992). The only book-length cross-country studies to look at leadership survival in both democracies and non-democracies have been those of Bienen and Van de Walle (1991) and BMSS (2003), while the main comparative politics journals have remained almost universally silent on the issue. It is somewhat ironic that the sub-field which has the most logical jurisdiction, and seemingly the most substantive concern, for this question provides so little guidance.

1.2. Political Economy

In political economy, as one would expect, there has been a well-developed literature on the effects of leadership tenure on the economy, but with very little on the causes of longer tenures.

For decades debate has raged about whether democratic or authoritarian regimes are better at promoting economic growth, with no real consensus being achieved (Przeworski and Limongi, 1993; Sirowy and Inkles, 1991). McGuire and Olson's (1996) roving and stationary bandit analogy provides a good entrance into this literature. They argue that, especially in non-democratic states, those leaders with long time-horizons will be those to provide the best conditions for growth. In a democracy, where the controlling interests are those of a majority, those making fiscal decisions not only control government distribution, but also make a market income, which provides even greater incentive for limiting redistribution to the authorities and investing in public goods. The punchline is that "whenever a rational self-interested actor... has an encompassing and stable interests in the domain over which the power is exercised, that actor is led to act in ways that are... consistent with the interests of society and those subject to that power" (p. 73). The expectation from this is that stable leaders perform better than unstable leaders in non-democracies, while the relatively short-lived tenure of democratic leaders only reflects the longer time horizons of the majority they represent.

Others have argued that democracies do a better job providing public services (Lake and Baum, 2001), since competition between political parties means that the state's management (politicians) can be displaced more easily, resulting in a government that acts like a regulated monopoly on the provision of public goods, rather than the relatively unregulated monopolies of authoritarian regimes. Here the underlying assumption is that vulnerability makes leaders more responsive to demands for public goods.

Similarly, BMSS (1999a) find that countries with the largest necessary winning coalitions, and thus the least stable in terms of leadership survival, have the highest growth rates across both democracies and non-democracies (see also Barro, 1997; BMSS. 2003). Those regimes which require the largest coalitions to gain and maintain power encourage greater spending on public goods, as it becomes inefficient, or impossible, to provide private goods to a large enough coalition to guarantee the maintenance of power.

This may seem strange for some scholars who study economic reform, since it has often been suggested that a certain amount of political insulation from the pressures of the electorate for politicians to make the painful reforms that are necessary for economic growth (ex. Przeworski, 1991; Rodrik, 1996). Indeed, debate about whether politicians produce better policy when they are vulnerable or when they are insulated remains one of the most active areas in the political economy literature.

1.3. International Relations

Surprisingly, it has been in international relations that most of the work comparing the stability of executives across regime-types and countries has been done. This interest has come from attempts to explain the "democratic peace" theory (BMSS, 1999b; BMSS, 2004; Bueno de Mesquita, 2007) or to address questions of how war affects leadership survival and how leaders choose to fight (Goemans, 2000; Chiozza and Goemans, 2004; Colaresi, 2004).

BMSS (1999b) have been especially vocal on the use of an institutional understanding of leadership tenure to explain long-standing theoretical shortcomings of democratic peace theory. They argue that a coherent explanation of the phenomena observed under the democratic peace rubric can be found by looking an executive's vulnerability to loosing office. Since democratic leaders are more likely to loose power if they are defeated in a conflict, they are less likely to start wars that they are unlikely to win and will try harder to win the wars they do fight. This makes them unattractive targets for autocrats or other democracies.

What sets BMSS's efforts apart, aside from positing a causal mechanism that explains a wide variety of outcomes, is their development of a theory of leadership stability based on institutional attributes. This theory, commonly referred to as "selectorate theory," posits that two variables, which are comparable across a global sample of countries and institutions, can explain executive survival rates and the vulnerability of regimes to economic crises or failures in conflict. In doing so, they provide a basis for comparisons that impacts all areas of political

science.

2. The use of the Cox proportional hazard model

Since the purpose of studying leadership tenure is to model the probability of a leader surviving or falling at a particular time, these scholars have universally turned to a set of tools referred to as event-history models (also known as hazard, survival or duration models) around which a well-defined set of tools has been developed (see Box-Steffensmeier and Jones, 1997; Box-Steffensmeier and Jones, 2004). Formulated in the field of biostatistics, these models look at the probability of an individual surviving, given a set of covariates (treatments) and time. The primary tool used by these scholars in studying leadership survival is the Cox proportional hazard model. Of the studies cited at the beginning of this article, four of the six use this model. This section gives a very brief overview of the proportional hazard model and the reasons for preferring it to other options in most circumstances.

The Cox proportional hazard model has long been the workhorse for the social sciences (Allison, 1995). The intuition behind the model is that the hazard that the individual or object has of collapse at any particular time *t* is a function of their baseline probability of collapse at that time, along with the effect of the independent variables which the researcher posits. Mathematically, this is expresses as

$$h_i(t)=h_0(t)exp(B'\mathbf{x}) \quad (1)$$

where $h_i(t)$ is the probability of individual *i* experiencing failure at time *t*, $h_0(t)$ is the baseline hazard function and $B'\mathbf{x}$ is the matrix of covariates and regression parameters. The intuitive interpretation of the regression coefficients in this model is the amount of increase or decrease in the baseline hazard that is caused by the independent variable. The model is called "proportional hazard" because the effect of any independent variable is assumed to follow the same pattern over time. This is obviously not always going to be the case, and there are a number of well-known tools for testing and correcting for cases where this does not hold (Box-Steffensmeier and

Zorn, 2001).

The popularity of Cox regression models is primarily due to its flexibility in producing reliable estimates of the impact of the independent variables under a wide variety of underlying patterns of time-dependency. Unlike other well-known models, which parameterize the underlying time-dependence of survival along a particular distribution, the Cox model derives the shape of the baseline hazard directly from the data. This is important for estimation since the parameterization the researcher chooses could substantially impact the inferences that result.

As with any statistical tool, there are some substantial limitations to the Cox model. It is usually not appropriate where the researcher is interested in the underlying relationship between time and survival, as the recovered baseline hazard functions are usually very difficult to interpret substantively (Hjort, 1992, p. 375-376; Royston and Parmer, 2002). It is also slightly less efficient where the baseline hazard can be parameterized, although the differences in estimation of independent variable effects are marginal (Peace and Flora, 1978).

Nevertheless, where the researcher is primarily interested in obtaining consistently accurate estimates of the impact of the covariates, and time-dependence is a statistical nuisance, the Cox model will usually be recommended.

Despite its flexibility, researchers need to adjust the model to deal with issues specific to the empirical context under review. These issues, as they relate to leadership survival, are discussed in the next section.

3. The problem of unobserved heterogeneity

Although individual leaders bring their own talents and resources to their tenure, the tenure of a leader is not wholly independent of that of other leaders. In particular, leaders may be expected to have more fragile, or at least short-lived, terms in certain countries and under particular institutional frameworks. This unobserved, but expected, heterogeneity in event history models can lead to inaccurate inferences. This section explores these two potential areas of

unobserved heterogeneity and the strengths and weaknesses of the suggestions made in the literature for addressing them.

3.1. Country level

Leaders in the same country will face many of the same challenges. In some countries ethnic fragmentation (Horowitz, 1985; Roeder, 1999), poor economic growth (Gasiorowski, 1995), propensity for military involvement in domestic politics (Huntington, 1968), elite norms (ex. Mexico), unmodeled institutions or just idiosyncratic factors can make a long tenure less likely. Yet it is highly unlikely that a researcher will be able to include all such factors in her analysis. This means that the statistical model must be modified to take into account unobserved clustering of leadership tenure on the country level.

The most popular method for dealing with unobserved heterogeneity in the context of event history models is shared frailty. Here the unobserved heterogeneity is modeled as a random variable on the shared characteristic. The standard Cox proportional hazard equation is modified to

$$h_{ij}(t)=h_0(t)\exp(\beta'\mathbf{x}_{ij}+\psi'\mathbf{w}_j) \quad (3)$$

where $\mathbf{w}_j$ are the subgroup frailties, which are assumed to be an independent sample from a distribution with mean 0 and variance 1. When $\psi = 0$, the standard Cox model in equation (1) is obtained. Equation (3) can be re-stated as

$$h_{ij}(t)|(\beta'\mathbf{x}_{ij},v_j)=h_0(t)v_j\exp(\beta'\mathbf{x}_{ij}) \quad (4)$$

where $v_j=\exp(\psi'\mathbf{w}_j)$ denoting the shared frailty of the i observations in the jth group. Here it is usually assumed that the mean of v is 1 and the variance is unknown and equal to some parameter θ. To estimate this, the researcher must define a distribution on v, at which point the expected survivor function is obtained by integrating out of the conditional survivor function the frailty term v (Hougaard, 2000; Box-Steffensmeier and Jones, 2004, p. 146-47). This method of dealing with unobserved heterogeneity is analogous to the random effects model that is used in the study

of panel and time-series cross-sectional (TSCS) designs. The major advantage to this approach is its efficiency. Since the unobserved heterogeneity is assumed to be uncorrelated with the covariates, no information is lost.

Unlike the TSCS context, however, the use of random effects in event history has been relatively uncontroversial. Indeed it is the only method mentioned for dealing with unobserved heterogeneity when the individual cases experience the same outcome in the primary political science book on event history models (Box-Steffensmeier and Jones, 2004), and is also the only method for analyzing country-level heterogeneity used in past studies of leadership tenure (Chiozza and Goemans, 2004; Colaresi, 2004). This is in large part because most of the development of event history models has come from biostatistics, where there are clear clinical controls and the common risks are not measurable. Hougaard (2000, p. 115) gives the following example:

> [A] pair of monozygotic twins have all their genes in common. It is expected that the genes have an influence on the lifetimes of the twins… It is assumed that, conditionally on the gene makeup and the other risk factors, the two twins have independent lifetimes. The problem with this approach is that we do not know the genes, and therefore we need to consider them as random and integrate the effects of the genes out and this creates dependence between lifetimes.

But shared frailty is far from the only method for dealing with unobserved heterogeneity, and, in many ways, it is the least informative in the comparative politics context. While a test of the null hypothesis can be conducted on the random variable to determine the presence of unobserved heterogeneity, it does not give us much information about the source of that heterogeneity. In addition, at least in the TSCS setting, there are those who contend that country-level heterogeneity is almost always an alternative hypothesis in comparative politics and that the assumptions of the random effects variable may mask underlying bias in the estimation (Wilson and Butler, 2007). It is somewhat surprising that there has not been more spillover from discussions in the TSCS literature on this issue, since TSCS and event history are analogous in many ways (Beck, Katz and Tucker, 1998). Hougaard (2000, p. 116) states with regard to frailty

estimation, "[I]t becomes important whether a given explanatory variable is measurable, as it only makes sense to include it in the model if it is measurable." There are some in the TSCS who would say that is exactly the reason for testing country-level controls in comparative politics (Beck, 2001).

The assumption that the random variable is uncorrelated with the covariates means that any unobserved heterogeneity across units within a country that is related to the independent variable is lost. When there is reason to suspect that unit heterogeneity, based on the country the leader is in charge of, will impact the results, there are several methods, similar to fixed effects in the TSCS setting, which may be tried. These methods entail certain costs, especially in efficiency where the analysis involves non- or slow-moving variables, but they are also likely to yield more information on the structure of the data for the researcher's analysis.

Errors in inference can occur at two levels. First, there is likely to be error in hypothesis testing. Because leadership tenure differs systematically between countries, there will be a correlation between the country a leader controls and the standard errors from models of the length of that term, violating the standard regression assumption that errors are evenly distributed. The simplest method for dealing with this is to use robust variance estimates (Lin and Wei, 1989).[1] This assumes that observations are independent across units (or "clusters" – country in this case) but are not independent within those units. They are calculated using a "sandwich" estimate

$$\boldsymbol{V_R}=\boldsymbol{V^{-1}BV^{-1}} \qquad (2)$$

Where $\boldsymbol{V^{-1}}$ is the variance estimate of the standard Cox proportional hazard model and $\boldsymbol{B}$ is a correction factor for intra-cluster correlation.[2]

The second level of error is in the coefficients, where the estimated impact of the

[1] There have been some attempts at developing robust estimation in frailty models using the bootstrap method (see. Jalaluddin and Kosorok, 2000), but none of these methods are currently available in the widely used statistical programs for applied research.

[2] For more information on how B is calculated see Box-Steffensmeier and Zorn (2002, p. 1089-1090) or Greene (1997, p. 504-505).

covariates may be biased upward or downward due to omitted unit effects. To analyze potential bias in the coefficients, three methods can be utilized. One method for estimating with fixed effects is to stratify the baseline hazard based on country. Here again, the idea is to absorb the country-specific differences into the baseline hazards by modifying the Cox model to

$$h_{ij}(t)=h_{0j}(t)exp(B'\mathbf{x}_{ij}) \quad (5)$$

where $h_{0j}(t)$ is the baseline hazard of a leader in country j losing office. Chart 1 look at the recovered baseline cumulative hazard functions of an estimation done using country-specific stratification. The results show quite a bit of difference in the baseline probabilities of leadership collapse among states, which suggests some need to take into account the unit effects stemming from the leader's country. Here again, even though some of the heteroscedasticity problem will be dealt with in the estimation of the coefficients, robust standard errors should be utilized.

(Chart 1 About Here)

A second method is to parameterize the fragility of leadership in the country. Beck, Katz and Tucker (1998, p.1272) suggest doing this in the binary time-series cross-sectional (BTSCS) context by including a count of the number of previous transitions. This, however, runs into a problem where states do not enter the sample at the same time. The coefficient for the covariate may not be accurate since the 20th leadership collapse for an older country may come at the same time as the first for a newer country. Another method to parameterize fragility, which does not run into the same entry problem, is to include a ten to fifteen year moving count of the number of leadership transitions. The downside to this approach is that the choice of time to use is somewhat arbitrary, as there is little theory for how long previous patterns take to dissipate, and this, in itself, may vary by country.

Finally, the method most commonly used in the TSCS context is the pure fixed effects model, whereby dummy variables are included for all of the countries in the sample.[3] Making the

[3] This is also sometimes referred to in the TSCS setting as the least-squares with dummy-variables (LSDV) method (see Stimson, 1985).

equation

$$h_i(t)=h_0(t)exp(B'\mathbf{x}+\gamma'\mathbf{z}_j) \quad (6)$$

where $\gamma'\mathbf{z}_j$ represents the matrix of country dummies and their corresponding coefficient. While often held up as the gold standard for unbiasedness in the TSCS context, fixed effects have numerous problems. Most importantly, they give up any information about the effects of variables between countries. This inefficiency is most severe when dealing with variables that either do not move or move very slowly over time (Plümper and Troeger, 2007). It also requires the inclusion of many, sometimes hundreds, of dummy variables, which may substantially decrease the degrees of freedom in the model.

Yet for all its faults, fixed effects models can be very informative about the structure and relationships between the observed variables and the unobserved country-level effects. The tendency in event history modeling to reflexively turn to random effects without any check of alternative fixed effects specification may cause the researcher to miss important patterns in the data. As such, event history may be able to learn from the TSCS literature on the importance of using fixed effects when testing alternative specification (Wilson and Butler, 2007).

3.2. Regime Type

In addition to country-level effects, leaders implement their policies under a set of institutions which they often inherit from previous periods. As the neo-institutionalists would say, institutions are "sticky," meaning that they change much more slowly than other environmental factors (March and Olsen, 1984). Given this, where they are not explicitly parameterized, some type of control for the institutional context under which the leaders serves would seem appropriate.

The most obvious institutional distinction is between those leaders, whether in a democracy or non-democracy, who can be replaced at regular intervals via election and those

who cannot. In systems of elections, the cost of replacing a leader are extremely low, indeed the mechanism for leadership removal is almost costless on the individual level. This is in stark contrast with any system of government when does not replace its leaders via election. Here the more likely method for leadership removal is rebellion, which entails much higher organization costs and a great deal more risk. A second, and related, institutional distinction is between those leaders who serve in office under term limits. As BMSS (2003) note, these are almost always in systems which have some form of election or easy method of leadership removal.

All of this would suggest that the difference between leadership duration under institutions where replacement is done through election and under institutions where it is done through other means needs to be explicitly incorporated into our models of leadership survival. Chart 2 lends further support to this contention. It shows the recovered baseline cumulative hazard functions for elected versus non-elected leaders. It is clear that the hazard rate increases at a much faster rate for elected leaders, as would be expected from the above analysis.

(Chart 2 About Here)

Three methods are available for researchers to account for the systematic differences between these institutions. The first is to stratify the sample on the basis of elected versus non-elected regimes. Here the equation is similar to that in (5), but the *j* groups are the institutions under which the leader serves, classified as elected or unelected. The second method is to incorporate whether the executive is elected or unelected as an independent variable.

Both of these are appropriate when the researcher believes that the other independent variables in the model have the same effect on the baseline hazard between nominal institutional types. In other words, the assumption implicit in both of these modeling choices is that it is only the baseline hazard, and not the effect of the independent variable that changes between institutional setups, since the reported β' remains constant over $\boldsymbol{x}_{ij}$. If it is not the case, as in the example below, then the researcher should strongly consider modeling the two institutional patterns separately.

4. Empirical context

Table 1 looks at the major studies that explore leadership survival across country and nominal regime category. Of the six major studies, only two attempt to model unobserved heterogeneity on the country level using shared frailty (random effects). These same two are the only ones to model important institutional differences outside of their main variables of interest – one uses stratification and the other uses parameterization. Yet none of these use fixed effects, even as a robustness check. In this section, the above suggestions are implemented using BMSS' (1999a) study of "selectorate theory." It will be demonstrated that using fixed effects to test alternative specification can highlight important patterns in the data that call into question the original findings. It will also be shown how important institutional context is, at least in terms of differentiating between electoral and non-electoral systems.

(Table 1 About Here)

4.1. A short introduction to selectorate theory.

To begin, a short introduction to the logic underlying selectorate theory is appropriate. For the interested reader, BMSS (2002) and BMSS (2003) provide the most comprehensive explanation for how the game theoretic foundations of the theory function.

Each game starts with the executive and an opposition making offers in an attempt to collect a coalition that will give it enough support to keep or take power. This process consists of appealing to two groups. The first group, the selectorate (*S*), consists of all the individuals from which a coalition can be selected. Each of these individuals has an ex-ante affinity to the executive. The current executive must put together a coalition of enough individuals to make a winning coalition (*W*), a group large enough for the executive to take power.

To put this winning coalition together, the executive can use revenues to distribute two types of goods, private and public. Private goods are those which are enjoyed by a particular

individual. Public goods, in contrast, are enjoyed by a large group of people, and they are non-divisible, in that they can not be apportioned by contribution, and non-excludable, in that individuals cannot be prevented from enjoying the benefits (see Hardin, 1982).

In order to minimize costs, the current executive will choose those individuals with the highest affinity for the incumbent. When the size of the winning coalition is small, it is more efficient for leaders to distribute private goods. This is because public goods are not valued as highly by particular individuals (since the benefits are divided among the whole society). When the size of the winning coalition becomes large, however, the distribution of private goods becomes inefficient or impossible because of revenue constraints. This means that larger winning coalitions should result in better provision of public goods.

The goods demanded by members of the selectorate to join the winning coalition depends on both the size of the winning coalition and the size of the selectorate. When a government consists of a small winning coalition and a large selectorate (*W/S* is small), being a member of the winning coalition is highly valuable, and there are a multitude of other individuals who can join the winning coalition if an individual defects. For BMSS, these regimes are best typified by the single party systems of the Soviet Union and China, where the selectorate consists of the entire political party, but through selective nomination procedures and rigged elections, the number of individuals in a winning coalition is relatively small.

In a democracy, both the selectorate and winning coalition are large (*W/S* larger), with the selectorate usually consisting of the entire adult population and a winning coalition often consisting of 50 percent plus one of that population (*W/S* = .51). In this case, it would be difficult to allocate private goods to half of the population while excluding the other half. It also becomes relatively easy for individuals to defect from the winning coalition without concern of their utility decreasing significantly.

In the particular game that BMSS (2002) propose, all actors behave in an optimal manner, and the incumbent is always returned to office. This is because the individual affinity

for the opposition is not revealed until after the decision on leadership has been made. Opposition leaders thus have a problem of credible commitment. They can offer large incentives to encourage defection in the first round of the game, but they can not guarantee that they will continue to do so in the future. Incumbents, on the other hand, have a history of distribution which individuals can rely on to infer their probability of being in the winning coalition in the future.

While the incumbent never looses in the game laid out here, there are significant differences in the resources that an incumbent is able to save after carrying out the promised distribution. Where loyalty is high, such as in small winning coalition/large selectorate states, leaders do not need to make as large of expenditures, both because there is a large pool of individuals from which to choose relatively few positions and because the likelihood of being included in an opposition coalition is relatively small. Where loyalty is not as high, as is the case when the winning coalition is larger and the selectorate smaller, individuals can hold out for larger payoffs, since their ability to threaten defection and their probability of being included in an opposition coalition are higher.

What this means for stability is that executives which have an array of institutions that promote loyalty will have more resources in reserve after each play of the game. It is this reserve that is the mechanism for stability, since it provides resources for dealing with future crises.

4.2. Data

To test the implications of selectorate theory, the replication data made available by BMSS was utilized.[4] Data was analyzed on a yearly basis, in a sample consisting of 2,700 leaders from 191 countries between 1816 and 1999.[5] Table 2 gives an example of the

[4] Data is provided on the internet by the authors: http://www.nyu.edu/gsas/dept/politics/data/bdm2s2/Logic.htm

[5] In the original paper, data only extended to 1990. The dataset was extended to 2000 for their 2003 book. The extra time does affect the observed N, but it does not affect the substantive results. It should also be noted that the replication for the 2003 book utilized monthly information on leadership entry and exit. However, the the independent variables, including Polity III information, do not correspond to

organization of the data, which is based on yearly reporting to allow for time-varying covariates.

(Table 2 About Here)

The dependent variable, leadership survival, was collected by Bueno de Mesquita et. al. (1999a), derived primarily from Spuler, Allen and Sunders (1977) and extended and checked using a number of resources (Langer, 1972; Bienen and van de Walle, 1991; Crystal 1990; and Cook, 1992). In the dataset itself, the leader's name is given, as well as the year and month of their entry and exit from office.

The independent variables were operationalized using the Polity II, III and IV datasets (Jaggers and Gurr, 1996; Marshall and Jaggers, 2002).[6] To measure the size of the selectorate (*S*) the breadth of members of each country's legislature was used. This was coded as 0 if there is no legislature, 1 if the legislature is appointed by heredity, ascription or by the executive, and 2 if they are directly or indirectly selected by popular election.[7]

Size of the winning coalition (*W*) was operationalized using several variables from Polity III. These variables are *Regtype*, *Xrcomp*, *Xropen*, and *Parcomp*. *Regtype* was used to distinguish military from non-military regimes. Military regimes tend to have lower winning coalition size, so regimes with codes not equal to 2 (military/civilian) or 3 (military) were assigned one point. *Xrcomp* looks at the competitiveness of executive recruitment. If the country receives a score greater than or equal to 2, indicating that it is either by election or in the process of transition towards election, than another point was assigned. *Xropen* measures the openness of executive recruitment. Here another point was added to *W* if the country either used a dual system of ascription and election or a system of election for executive recruitment (a score

monthly changes (at least not in the variables used). It also poses a problem as not all institutional variables are time-invariant during a leader's tenure, but using the monthly stset suggested in the replication treats it as though it were. As such, the decision was made to use the yearly format from the 1999 article. Again, this does not change the substantive results, and, in fact, the replication is weaker in the monthly context than in the yearly.

[6] These findings were replicated in a dataset using the Polity IV dataset. Again, the results were relatively invariant, although there is a significant relationship between the advent of the regime change variables (new to Polity IV) and the absence of a legislature post-1965. This will be addressed later.

[7] In BMSS (2003) and in the replication data on the internet, W and S are standardized to a range of 0-1. This was reversed for this analysis for consistency of comparison between these results and BMSS (1999a).

greater than 2). Finally, *Parcomp* looks at the competitiveness of political competition. A point was added if *Parcomp* is coded as 5, meaning that "there are relatively stable and enduring political groups which regularly compete for political influence at the national level" (Marshall and Jaggers, 2002, p. 18). The resulting data ranged from 0 to 4.

These results were analyzed using the Cox proportional hazards setup described above. The coeffients are hazard ratios. These can easily be read as one indicating no change in the baseline hazard rate – the independent variable has no effect. Values above 1 show the percent more likely failure is with a one point increase in the independent variable. For example, if a covariate has a hazard ratio of 1.58, then a one point increase in that covariate results in a 58% greater likelihood of failure. Similarly, values below 1 are interpreted as the reduction in the probability of an event based on a one point increase in the covariate. So a hazard ratio of .42 would suggest that a 1 point increase in the covariate results in a 58% decrease in the likelihood of failure.

4.3. Results

Table 3 looks at the replication of BMSS' (1999a) results. There is some minor difference between the results in the original article and these results, but these are easily explainable by the larger number of years included in the updated replication data and idiosyncratic coding errors that occur in reconstructing the leader change and leader duration variables. The results indicate that, consistent with selectorate theory, larger winning coalitions make leaders less stable, while larger selectorates make them more stable.

(Table 3 About Here)

4.3.1. Controlling for country-level heterogeneity

The next table looks at some alternative methods for dealing with unit heterogeneity based on the country in which the leader's tenure takes place. Model 2 uses the standard shared

frailty model. The inclusion of the shared frailty does change some of the hazard ratios for the variables of interest. It decreases the effect of *W* on the probability of leadership failure by about 20 percent, but it remains statistically significant and still indicates that a one point larger winning coalition makes a leader 20 percent more likely to lose office. The shared frailty increases the impact of *S* by about 3 percent, indicating that a one point increase makes leaders about 10 percent less likely to fail, and strengthens the statistical significance ($p<.011$).

More explicit methods for accounting for country-level heterogeneity tell a very different story, especially on the selectorate size variable. There is an obvious need to take into account problems of heteroscedasticity in the standard errors. Even in the shared frailty model, when the standard errors are saved and then regressed against the country dummy variables, 119 out of 140 are statistically significant on less than a 0.05 level. Model 3 corrects for this using robust clustered standard errors. When the standard errors are corrected, the measure of selectorate size, *S*, ceases to be statistically significant, with a p-value indicating that there is a 25 percent chance the results are simply due to the sample.[8]

Similar results are obtained when other methods of correcting for unit heterogeneity are used. In model 4, which uses stratification based on the country in which the leader's tenure takes place shows a slightly stronger relationship between selectorate size and length of tenure, but the standard errors also increase, leaving the statistical significance nearly unchanged. Parameterizing country-level heterogeneity as the sum of leader turnovers in the previous ten years, *sumchange*, in model 5 has the opposite effect on the hazard ratio, reducing the effect of selectorate size to a little more than a 3 percent increase in the probability of survival for having a legislature. The level of statistical significance also drops, with the p-value now indicating

[8] Ideally there would be an easily to implement program for estimating shared frailty with robust clustered standard errors. I have yet to locate such a program. Stata will not allow the user to specify robust standard errors along with shared frailty. Similarly, in R, "the survival package" allows for robust clustering and shared frailty, but not both (Lumley, 2007). "Frailtypack" reports robust standard errors with its estimation, but these are not clustered (Gonzalez and Rondeau, 2006). In both cases the returned results are similar to those presented from Stata.

almost a 35 percent chance that the observed relationship is due to chance.[9] Finally, using fixed effects estimation produces a similar-sized coefficient to the regular Cox model, but the standard errors are such that there is a 31.2 percent chance that the relationship would be different if another sample were taken.

(Table 4 About Here)

All of the more explicit methods for looking at unobserved heterogeneity produce very different results than the uncorrected Cox model or the shared frailty model. Why is this? One possible explanation is that *S* is a slow mover. In many contexts where the covariate is an institution this intuition would be correct. In this case, however, the ratio of between country to within country variance is only 1.21, which is well short of the 2.8 Plümper and Troeger (2007) identify as being problematic for fixed effects. Another explanation is that there are a lack of a legislature does not reflect the size of the selectorate, but rather the situation in a few, highly unstable states.

A closer look at the data supports this latter interpretation. Of the 11,206 country years of data on *S*, in only 1,660 cases, or about 14.8 percent of the time, does a country have no legislature, and in only 469 cases, or about 4.2 percent of the time, does a country have an appointed legislature. Looking at these cases reveals that they are almost always situations of extreme politics, where either the regular political system has been suspended by military rule or it is a time of "complete collapse of central political authority," either because of foreign intervention, domestic anarchy or a transition period to new institutions (Marshall and Jaggers, 2002, p. 15). In fact, 717 of the cases with no legislature (43 percent) and 64 of the cases with appointed legislatures (13.6 percent) had military regimes, and in 114 cases with no legislature (8.6 percent) and 25 cases with appointed legislatures (5.3 percent) were experiencing a collapse in central authority. In either of these cases, it should be no surprise that leadership tenure would

[9] Some may wonder if this is due to increased collinearity added to the equation by including the sum of leadership changes. However, collinearity diagnostics do not support this argument. The highest VIF score for any of the three variables is 1.37 for W, and the mean VIF is 1.25. This is well short of the traditional threshold of 10 which might raise concerns.

be short. Military regimes tend to be short-lived for a number of reasons: the relatively low likelihood of retribution if they leave office, the relative ease by which their goals are accomplished, lack of legitimacy, internal conflicts with domestic security forces, etc. (see ex. Geddes, 1999; Geddes, 2002; Stepan, 1988). Times of turmoil in central rule are also, almost by definition, going to be periods under which leadership tenure will be unusually short.

Table 5 supports the idea that *S* is really a poor stand-in for these other variables. Including dummy variables that measure whether the military is involved directly in the government, *military_regime*, and whether it is a period of extreme politics according to the standard Polity codes, *polity_interruption*, causes the variable for selectorate size to lose statistical significance, and, even more strikingly, reverses the direction of the relationship. Meanwhile, both of these new variables are strongly statistically significant ($p < .000$), with leaders in military regimes over 300 percent more likely to fail in a particular year, and leaders serving during times of extreme politics over 200 percent more likely to fail.[10] Model 8 demonstrates that these results do not change when using fixed effects estimation with robust standard errors. All of this would lead to the conclusion that the measure of selectorate size is really capturing the extreme instability and/or propensity for military involvement in several states.

(Table 5 About Here)

4.3.2. Controlling for institutional context

As discussed above, whether a leader can be removed through election or not is likely to significantly shape our expectations of leadership tenure. As such, table 6 looks at several different ways of introducing controls for elections into the equation. Model 9 introduces election as a control variable, where a country receives a 1 if it is coded as using election to

[10] Again, there will be those who will attribute these changes to the introduction of collinearity. The VIF scores for these variables, however, remain relatively low, with a maximum VIF of 1.53 for W and a mean VIF of 1.33. Here again, this is well short of the threshold VIF of 10 which raises concern.

decide leadership succession and 0 otherwise. The introduction of this control causes both *W* and *S* to lose statistical significance, and, even more significantly, causes the size of winning coalition to reverse in its relationship to leadership tenure. When controlling for method of succession, leaders are now about 3 percent less likely to fail in larger winning coalition states. Meanwhile, the relationship between election and leadership tenure is very robust and suggest that leaders that can be removed by election are 200 percent more likely to be removed in any particular year.[11]

Stratification by method of succession in model 10 produces similar results. The relationship for both of the selectorate theory variables are now almost indistinguishable from the null hypothesis and winning coalition size continues to reverse direction.

All of this suggests that the primary difference captured by the selectorate variables is the difference between leaders who are elected and those who are not. It is not surprising, however, that leaders in systems that decide succession by regular election would have shorter terms. After all, the costs of removal in such systems are negligible, and many of these systems have term limits, which prevent longer leadership tenures.

The question naturally arises – should systems where succession is decided by election be used in the same model as those that do not? As mentioned above, there is no a-priori reason for believing that the selectorate theory variables will have the same effect in elected regimes as in unelected regimes. Models 11 and 12 provide some support for modeling these two types of regime separately. For both of the selectorate theory variables, the predicted relationship with the length of leadership tenure reverses between succession processes. While substantively this is probably a null issue, given what was found in the previous section, it does serve as a warning for future researchers about assuming that the relationship will be consistent across institutions.

[11] In this model, collinearity is a little more severe, but still not disastrous. The VIF score for W is 3.39 and for election is 2.87, while the average VIF is 2.53. While still below the usual threshold of concern, these numbers indicate a relatively strong correlation between W and election that may be of empirical interest.

(Table 6 About Here)

5. Conclusions

The above discussion and example should have conveyed several important points to the reader. First, while models of leadership survival hold out immense problems for a wide variety of interesting areas in political analysis, they are statistically complex. In particular, comparing the length of leadership tenure across countries and institutional settings raises the specter of unobserved heterogeneity on both levels. While some studies have attempted to take this into account, these problems cannot always be adequately addressed by simple application of the best practices borrowed from other fields.

This leads to the second important point. While the use of shared frailty to account for country-level heterogeneity and stratification for institutional heterogeneity may represent the best and most efficient statistical tools available to the researcher, they also carry significant assumptions and tend not to report potentially useful information to the researcher. If the first commandment of statistical research is to "know thy data," then testing some of these assumptions may prove a useful tool for checking the robustness of the researcher's results and may even lead the researcher to some interesting patterns that would otherwise not be readily apparent. Among these tools is the use of robust standard errors and fixed effects estimation (either through stratification or country controls). While not a recommended solution to heterogeneity problems, because it involves giving up relevant information, these methods can be very informative and can help identify limitations and hidden patterns in the research. It can also be useful in determining the limitations of theories (such as whether they affect electoral and non-electoral system in the same manner) and give rise to important measurement question (such as what institutional categories are important).

Finally, in the context of leadership survival studies, it has been demonstrated that BMSS' (1999a; 2003) results are very fragile in the face of country- and institution-level

heterogeneity. Indeed, by utilizing the tools laid out above, it became apparent that the main variables used in selectorate theory are likely proxies for more basic country and institutional characteristics.

In comparative politics and, to a lesser extent, in international relations, there is a tendency to blindly apply the "best practices" developed elsewhere. What is often forgotten is that a "best practice" usually assumes that the researcher has a clear idea of the data and the relationships at hand. As such, it is often useful for applied researchers to be familiar with and to use multiple tools to explore the patterns in their dataset. While not often discussed, this is no less true for event history than for any other statistical tool in political science.

Tables

Table 1: Previous Studies of Leadership Survival

	Type of Model	Fixed Effects	Random Effects	Electoral Stratification
Bienen and Van De Walle (1991)	Cox	___	___	___
Bueno de Mesquita and Siverson (1995)	Weibull	___	___	___
BMSS (1999)	Cox	___	___	___
BMSS (2003)	Weibull	___	___	___
Chiozza and Goemans (2004)	Cox	___	Shared Frailty (country)	3-level stratified: non-democratic, presidential, parliamentary
Colaresi (2004)	Cox	___	Shared Frailty (country)	4-level variable: autocracy, mixed regime, presidential, parliamentary

Table 2: Data Setup for Proportional Hazard Analysis

Ccode	year	leader_number	leader_change	leader_duration
2	1838	5	0	1
2	1839	5	0	2
2	1840	5	0	3
2	1841	5	1	4
2	1842	6	0	1
2	1843	6	0	2
2	1844	6	0	3
2	1845	6	1	4
2	1846	7	0	1

Table 3: Replication of BMSS Results

	Model 1: replication
W	1.398
	(.000)
S	.928
	(.028)
Subjects	10,766
Failures	1,972

* The reported statistics are hazard ratios, with p-values reported in parentheses.

Table 4: Effects of Selectorate Variables Accounting for Unit Heterogeneity

	Model 2: shared frailty	Model 3: robust cluster	Model 4: stratification, robust cluster	Model 5: parametric effects, robust cluster	Model 6: dummy vars., robust cluster
W	1.202	1.398	1.144	1.077	1.171
	(.000)	(.001)	(.011)	(.079)	(.009)
S	.898	.928	.915	.969	.930
	(.011)	(.251)	(.257)	(.349)	(.312)
Sumchange	___	___	___	1.706	___
				(.000)	
Country	___	___	___	___	not reported
Subjects	10,776	10,776	10,776	10,776	10,776
Failures	1,972	1,972	1,972	1,972	1,972
Clusters	---	191	191	191	191

* The reported statistics are hazard ratios, with p-values reported in parentheses.

Table 5: Effect of Selectorate Variables Accounting for Military Regimes and Periods of Political Interruption

	Model 7: replication w/ additional controls	Model 8: replication w/ additional controls, fixed effects, robust cluster
W	1.772 (.000)	1.513 (.000)
S	1.029 (.244)	1.120 (.211)
military_regime	4.112 (.000)	4.074 (.000)
polity_interruption	3.882 (.000)	3.293 (.000)
Country	___	not reported
Subjects	10,079	10,079
Failures	1,899	1,899
Clusters	---	169

* The reported statistics are hazard ratios, with p-values reported in parentheses.

Table 6: Effects of Selectorate Variables Accounting for Election

	Model 9: controlling for *election*	Model 10: stratification by *election*, robust cluster	Model 11: non-elected regimes only, robust cluster	Model 12: elected regimes only, robust cluster
W	.967 (.202)	.972 (.426)	.855 (.124)	1.221 (.230)
S	.977 (.286)	.975 (.411)	1.046 (.344)	.642 (.003)
election	3.165 (.000)	___	___	___
Subjects	10,083	10,766	6,711	3,372
Failures	1,899	1,972	992	907
Clusters	---	169	147	108

* The reported statistics are hazard ratios, with p-values reported in parentheses.

Charts

Chart 1: Comparison of Baseline Hazards Among States

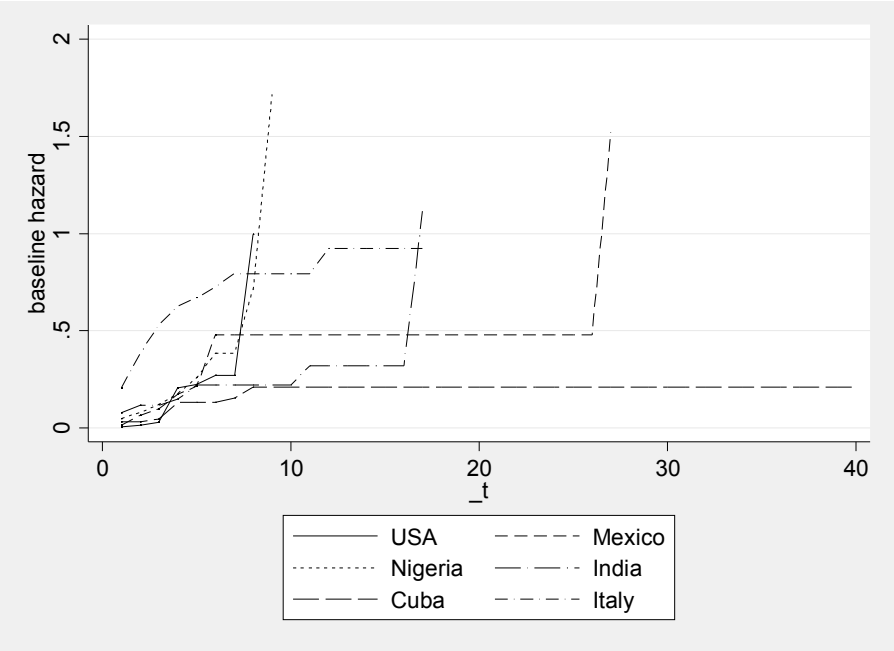
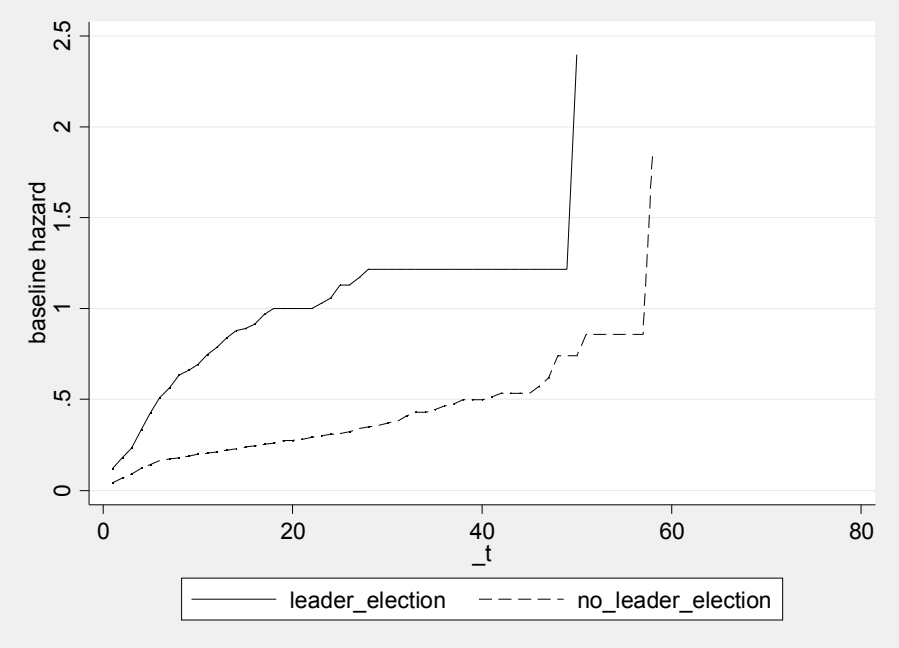


Chart 2: Comparison of Baseline Hazards Between Elected and Non-Elected Governments



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