

# CROSS-NATIONAL CORRELATES OF TERROR: Empirical Analysis of the Late 20th Century\*

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## **Abstract**

Data limitations have prevented scholars from developing a strong sense of the cross-national covariates of terror. A handful of such studies exist using the ITERATE data set, but those data exclude all domestic terror events, which is to say most such events. Further, those studies suffer from a common weakness in cross-national studies of violence: they focus on the characteristics of polities, economies and societies while ignoring the behavior of governments and dissidents. This study addresses both weaknesses by [1] utilizing a new dataset (the Global Terror Database) and [2] including the behavior of governments and dissidents within the context of the national characteristics that influence their behavior. The empirical domain is all countries in the world from the mid-1970s through the 1990s.

# Introduction

In 1961 Nelson Mandela made the case to the leadership of the African National Congress and the South African Communist Party that peaceful protest had run its course in the struggle against Apartheid. His argument carried the day and he was appointed the Commander of Umkhonto we Sizwe (Spear of the Nation), an underground guerrilla army that began a campaign of terror bombings. Three years later he would describe the argument he made as follows:

we felt that without violence there would be no way open to the African people to succeed in their struggle against the principle of white supremacy. All lawful modes of expressing opposition to this principle had been closed by legislation, and we were placed in a position in which we had either to accept a permanent state of inferiority, or to defy the Government. We chose to defy the law. We first broke the law in a way which avoided any recourse to violence; when this form was legislated against, and then the Government resorted to a show of force to crush opposition to its policies, only then did we decide to answer violence with violence (Mandela, 1964).

This story is useful because it draws one's attention to an issue that receives insufficient attention among researchers studying violent political conflict generally and the use of terror tactics specifically: campaigns of terror arise when dissidents decide that such a campaign will be more effective than alternative options, and when making those decisions dissidents weigh the past and expected future behavior of the government. This is a rather noncontroversial claim—it is difficult to imagine many students of violent political conflict objecting to such a pronouncement. Yet, when one reads the scientific literature on the topic one cannot help but be impressed by the overwhelming tendency of those writing in the literature to focus their attention not on the behavior of dissidents and governments as responses to one one

another's (anticipated) behavior, but instead on the amount of terror or violence a country or city experiences as a function of characteristics of the economy, society or polity. Are the processes that produce terror and violence most likely to be illuminated by an approach that proposes that we study geo-political units and their characteristics? I submit that they are not.

To put the question in a concrete, scientific fashion: How might one specify a model of the co-variables of the number of acts of terror countries would observe annually? Though this is not yet common place, I contend that the place to begin is with the perpetrators of such acts and the behavior that the governments they challenge take toward them. Over ten years ago I made the same argument with respect to both non-violent and violent dissent:

I contend that the parties to these conflicts... pay attention to the behavior of the other parties when forming their own strategic plans. Hence my critique of [earlier] studies is that they are "national attribute" rather than "strategic behavior" models (Moore, 1995, p. 131).

That critique was written on the cusp of the new emphasis among political scientists on strategic interaction that the rise of rational choice would bring to the study of violent conflict. Yet, ironically, this message has not yet had much of an impact on cross-national statistical analyses of violent political conflict as is readily apparent in the considerable literature on the onset of civil war (e.g., Hegre et al., 2001; Sambanis, 2001; Reynal-Querol, 2002; Fearon and Laitin, 2003; Collier and Hoeffler, 2004).<sup>1</sup> In the large-n statistical analyses that

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<sup>1</sup>In a recent review Tarrow (2007) observes the same basic problem, though he (stunningly, in my opinion) misses the key point I have been trying to make for over a decade: the study of strategic interaction will move us forward. Tarrow argues that what is significant about the recent books by Kalyvas (2006) and Weinstein (2007) is their focus on disaggregated cases rather than country-years and the use of multiple methods. To be sure, these are positive qualities, but to emphasize them is to ignore one of the first tenets of research: method is driven by theory (i.e., select an *appropriate* method given your argument and hypotheses). Both Kalyvas and Weinstein develop their theories by training their attention on strategic interaction among governments, dissident groups, and the population. The success of their focus on strategic interaction is the major point to emphasize about these works relative to the large-N civil war literature! Like the Swift Boat Veterans who attacked John Kerry, Tarrow is too busy fighting his generation's old wars to recognize the key feature that makes these works so valuable.

define that literature the *behavior* of governments and dissidents is conspicuously missing. Instead, scholars focus their attention on the characteristics of polities, economies, and societies. Unfortunately, quantitative research on terror exhibits the same weakness (e.g., Lai, 2003; Abadie, 2004; Li, 2005; Piazza, 2006; Lai, 2007; Piazza, 2008).

While these characteristics certainly influence the behavior of governments and dissidents (as the arguments advanced in this literature sometimes imply), it is nonetheless the case that states respond to dissidents and dissidents respond to states (e.g., see Lichbach, 1987; Davenport, 1995; Tilly, 1995; Moore, 2000; Thyne, 2006; Davenport, 2007*a*). As such, this study examines the cross-national variation in the number of terror events as the outcome of a process where dissidents challenging the state use terror as one of many tactics. The decision to produce terror events, I argue, is a function not only of the structural setting of the polity, economy and society, but also of the behavior of the government toward the dissidents, and of other dissident activity.

## **1 A Country Level Model of Terror as a Tactical Choice**

This is not a study of the activity of terrorists. Instead it is a study of the activity of dissidents who use terror as a tactic. I contend that a useful cross-national statistical model of the number of terror events a country experience can be constructed if we consider four items: government coercion, competition among dissident groups, the institutional context in which governments and dissidents compete over policy, and the country-wide socio-economic context that will influence the likelihood of collective dissent against government policies. I raise each issue in turn and explain how it motivates the specification of a statistical model.

## 1.1 Government Coercion

The government's response to the use of terror tactics will, theory suggests, impact its use: tactics that attract coercion will be abandoned, at the margin, for other tactics that can achieve goals and attract less repression (e.g., Lichbach, 1987; Enders and Sandler, 1993).<sup>2</sup> Ideally we would like to develop a cross-national specification that enabled us to measure government response to specific tactics. At present I am unaware of the existence of such data. As discussed above, the task in such a situation is to ask what the theory implies we would (not) observe when examining data aggregated at the country-year.

It is safe to assume that there will be more than one dissident group operating in any country at a given moment in time. Put simply, the argument made above is that coercion works: by punishing acts of terror governments can persuade dissidents to adopt different tactics. I assume that dissident groups respond not only to what government does to their own group, but other groups as well. As such, given sufficiently high levels of coercion, all of the groups may be sufficiently cowed that they only operate underground and take no action to challenge the state. This leads to the expectation that  $T = f(-GC)$ , where  $T$  is a count of the number of terror events, and  $GC$  represents government coercion.

An alternative argument is sometimes advanced. Instead of focusing on the costs that government coercion imposes upon dissident groups, some scholars focus instead upon the grievances that coercion produces among the population (Gurr, 1970; Rasler, 1996, e.g.). These scholars do not deny that coercion is costly. Instead, they argue that coercion is a double edged sword that raises the risk of supporting dissidents who use violence, but also alienates those who might otherwise not have supported the dissidents to such an extent that it is counterproductive. This is especially so to the extent that coercion is meted out

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<sup>2</sup>Note that the argument I am making is distinct from what Goodwin (2006, p. 2034) refers to as the "terrorism is a response to state terrorism" approach to theory construction. The work he describes is a mix of neo-Marxist and neo-Gramscian ideas that can often border on conspiracy theory. As social critique these arguments are provocative, but they are not intended to be scientific research and I do not take them seriously.

nondiscriminately (Mason and Krane, 1989; Mason, 2004; Findley and Young, 2007, e.g.). Wilkinson (2001) in particular argues that counter terrorism is a political, not a military, effort and emphasizes the importance of maintaining and respecting liberal democratic rights and freedoms. This argument produces the opposite expectation:  $T = f(+GC)$ . To account for these competing hypotheses, I thus write:  $T = f(\pm GC)$ .

## 1.2 Compliment or Substitute? Competition among Dissidents

As noted above, dissident groups rarely form in isolation—where one exists, others generally form and compete not only with the government for supporters, but with the other dissident groups. When we study the total number of dissident events in a given country, it is useful to keep this in mind and think carefully about how competition among dissident groups is likely to influence the aggregate data we observe in national statistics. I argue that it will prove useful to divide dissident tactics into two rough categories that have often been used in the literature: non-violent tactics and violent tactics. Acts of terror are, by definition, violent tactics. I further conjecture that when one aggregates dissident activity across groups and time at the country-year level that the number of acts of terror the dissidents in a country will commit in a year will be a positive function of the number of other acts of violent dissent and a negative function of the number of acts of non-violent dissent. The former expectation is motivated by the arguments about outbidding: dissident groups compete with one another for supporters, so when one uses terror, others are likely to follow suit (Brubaker and Laitin, 1998, p. 434, Bloom, 2004). The latter is merely the flip-side. Put another way, I expect that outbidding will produce data aggregated at the national level that exhibits evidence of complementarity among violent acts of dissent and terror, and a pattern of substitution among non-violent acts of dissent and terror. The new specification is:  $T = f(\pm GC, -NV, +V)$ , where  $NV$  is non-violent dissident activity, and  $V$  is violent

dissident activity.<sup>3</sup>

To expand on the point briefly, the argument is that dissident groups employ tactics that are effective (i.e., produce concessions, one of which is lower levels of repression than alternative tactics). When one examines the aggregate data across several groups and a considerable period of time, one should see greater numbers of the types of events that were punished less by the state (Lichbach, 1987; Enders and Sandler, 1993; Moore, 1998). If we assume—as is common—that dissident groups are rational (or purposive) then one can show that individual groups will respond this way. Thus, when we examine the aggregate behavior of several groups over a considerable period of time, we would expect to see evidence of such behavior as groups that do not behave rationally are less likely to survive and thus contribute considerably to our data.

### 1.3 Democratic Institutions as Context

Much of the interest in the cross-national incidence of terror events has focused upon the impact of democratic institutions (see Li, 2005, pp. 278-84). As often happens a plethora of arguments have sprung up, and both positive and negative relationships have been proposed. Further, several different democratic institutions have received attention. The impact of press freedom, greater observance of human rights, constraints on the executive, and different voting rules and party systems' ability to channel social conflict within the political system have all received attention. Li covers all of this ground rather effectively, though I wish to offer an alternative treatment of the causal mechanisms that produces a slightly different

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<sup>3</sup>This is certainly oversimplified. As Lichbach (1987) has theorized generally, and Enders and Sandler (2006) have theorized with respect to acts of terror specifically, when government repression reduces the effectiveness of a tactic dissidents change their tactics. A superior version of this argument, then, will claim that outbidding is likely when acts of terror are successful. Unfortunately, the data with which one has to work when doing cross-national research does not yet permit an evaluation of this more sophisticated argument. That said, as I argue below, if we invoke some reasonable assumptions about what would expect to see at the aggregate level if group competition selected out inefficient groups, then we can nonetheless support this expectation.

specification. I will examine the Li (2005) specification, but will also propose an alternative.

I build on the work by Davenport (2007*b*) and Davenport, Moore and Armstrong (2007) which examines the impact of three democratic institutions on government coercion: voice, veto, and freedom of expression. The debate on the impact of democratic institutions on terror has yet to explicitly recognize that the behavior of two actors in competition is of interest. By building on work that focuses on the impact of democratic institutions on executives and their incentives I am able to make that more readily apparent.

I invoke the standard assumption that executives wish to retain office. As Bueno de Mesquita et al. (2003) have shown, all executives are responsive to the members of their winning coalition (i.e., constituents whose support they need to remain in office). I adopt this basic approach and its implication that rather than invoke a binary distinction between “democratic” and “autocratic” polities (or regimes, if you prefer) we will build more useful theories if we think of polities as having different institutions that can be arrayed over ordinal or continuous scales, each of which has the potential to meaningfully shape the executive’s incentive to adopt certain policies or take certain actions.<sup>4</sup> The task, then, is to evaluate the impact that voice, veto, and freedom of expression are likely to have on an executive who is concerned with retaining office.

By voice I refer to the use of elections to select the executive. More specifically, I am interested in competitive elections where all adult citizens are able to participate (if they choose). This is similar to Dahl’s (1971) two dimensional conceptualization of polyarchy: contestation and participation. The more uniform across all citizens are the costs to enter the race as a candidate, the more competitive is the electoral system. Similarly, the more uniform across all citizens are the costs of voting, the greater is access to participation.

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<sup>4</sup>Note that I am *not* making use of the major implication that Bueno de Mesquita et al. (2003) take from their model: that the larger an executive’s winning coalition:selectorate ratios, the more likely s/he is to adopt policies that provide a large public:private goods ratio. Instead, I am taking from them the more general point that we are better off invoking ordinal/continuous conceptualizations of institutions than binary ones.

Naturally, neither of these concepts have been measured directly, though several scholars have developed alternative means by which to get at these concepts (or so I contend below).

What difference will voice make? Competitive elections are likely to reduce the demand for violent tactics of dissent, including acts of terror. First, competitive elections create a means by which groups can pursue political demands (Gurr, 1970, pp. 304-5). Second, elections delegitimize the use of violence as a tactic to pursue political ends (Gurr, 1970, pp. 183-7).

Veto refers to the number of actors in the political system who can check the executive (Tsebelis, 2002). A political system with zero veto players has an executive with unchecked authority; a polity with many veto players has a number of checks and balances in place to constrain the executive's ability to exercise power. The conventional wisdom about veto players is that the more there are, the more stable policy will be: the status quo will prevail longer the greater the number of veto players. The idea is straight forward: the more actors there are who can block change, the less likely change becomes.

This idea maps nicely onto a model proposed by Gurr (1988), who argues that a state's experience with the use of coercion will greatly influence its future reliance upon coercion for two reasons. First, future leaders will recall past experience, and those ruling states where coercion was used successfully will be more likely to employ it in the future than will those ruling states whose campaigns of coercion were unsuccessful. Second, states that successfully utilize coercion will continue to invest in the bureaucratic structures they created to prosecute coercion. The upshot, according to Gurr, is that in addition to examining whether a countries' political institutions are autocratic or democratic analysts must also examine the success/failure of the state's use of coercion. Put another way, democratic as well as autocratic governments can build strong bureaucracies to capably prosecute coercion.

Gurr's theory is relevant for making use of the veto player theory to study terror as long as it is true that dissidents consider the likely behavior of the state when selecting their

tactics. Tsebelis contends that the more veto players there are the more likely it is that the status quo will persist. Gurr suggests that if we want to know what the status quo policy is likely to be with respect to coercion, we should examine the state's success with coercion in the past. My argument suggests that dissidents respond to the state's coercion policy and are more likely to use terror when the greater is the state's reliance on coercion. Taken as a set these arguments imply that greater a state's success with past use of coercion and the larger the number of veto players, the greater the likelihood—*ceteris paribus*—that dissidents in that state will use terror to press their claims.

Finally, the extent to which the state respects the right to freedom of expression may also affect the extent to which the dissidents turn to terror as a tactic. It is widely observed that terror requires an audience, and that the media perform the function of providing information to the public (e.g., Wilkinson, 2001, chap. 9). That is, terror is less effective if the public is unaware of it. This suggests that countries that respect freedom of the press are more likely to experience terror events, all other things held constant, because the tactic is more likely to be effective than it would be in a similar state that practiced press censorship.

The arguments made above imply the following additions to the specification is:  $T = f(\pm GC, -NV, +V, -Vc, +Vt, +CB, +(Vt \times CB), +FE)$ , where  $Vc$  indicates voice,  $Vt$  is the number of veto players,  $CB$  represents the size of the coercive bureaucracy, and  $FE$  represents freedom of expression.

## 1.4 The Socio-Economic Context

I have argued that no work to date with which I am familiar proposes an interactive specification where dissident and government behavior influence one the other. Instead, other studies propose that the number of acts of terror will be a function of a number of characteristics of states and the economies and societies they govern. To this point I have sketched an argument about the interaction of dissidents and states within the context of political

institutions. We can further build the argument by turning to the studies that populate the literature and use them to round out our specification by placing the behavior of dissidents and states, acting within political institutions, in a broader economic and societal context. I turn to the list of variables from Phases II and III of what is now called the Political Instability Task Force (PITF), formerly known as the State Failure Task Force (Esty et al., 1998; Goldstone et al., 2000).<sup>5</sup> Though their model was created to explain the likelihood of state collapse I contend that there are two reasons it is useful for identifying a set of usual suspects for the study of terror events. First, the task force assembled the largest set of variables that might plausibly covary with violent political conflict of which this author is aware. They then proceeded to determine which of those hundreds of variables were most strongly associated with state collapse. Second, their findings demonstrate that these three variables have remarkably robust relationships with state failure. Consistent with the argument I have made above, I conceive of state failure as a partial outcome of competition between states and dissidents (in most cases other states will also play a role). In this sense, the task force has identified the structural characteristics in which dissidents are most likely to succeed. Because I have argued that terror is one tactic that dissidents might select, it seems reasonable to conjecture that the variables that most strongly and robustly correlate with state failure are useful candidates as usual suspects for the structural characteristics in which terror events are most likely, all other things equal. This argument leads to the following specification:  $T = f(\pm GC, -NV, +V, -Vc, +Vt, +CB, +(Vt \times CB), +FE, PITF)$ , where *PITF* indicates the three variables reported in Phases II and III of the Political Instability Task Force project.

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<sup>5</sup>These reports found that the probability of state failure was most highly predicted by a remarkably parsimonious model with only three variables: infant mortality, trade openness, and type of polity. More recent efforts by the task force—known as Phase IV and Phase V—have produced more nuanced models, but have adopted considerably more sophisticated statistical modeling techniques (Bates et al., 2003; Goldstone et al., 2005). From the perspective of this effort, where I am searching for a list of “usual suspects,” the earlier parsimonious model is preferred as the Phase IV and Phase V models pursue very specific tasks.

## 2 Statistical Analyses

A split population (aka zero-inflated) model makes most sense as terror events are rare and there are a number of countries that experience zero events over the time period we are studying (1980-1997).<sup>6</sup> In a similar study Li (2005) chose to drop those cases from his sample. Rather than exclude those cases I model the process by using a zero-inflated negative binomial (zinb) regression model.<sup>7</sup> Long (1997, chap. 8.5) provides a brief introduction to these models, and Zorn (1998) is a useful reference.

### 2.1 Measuring the Concepts

To measure the number of terror events observed in a given country in a given year I use the Global Terrorism Database (GTD)—available through the ICPSR (<http://www.icpsr.umich.edu/cocoon/ICPSR/STUDY/04586.xml>).<sup>8</sup> Specifically, I used the GTD events data to produce an event count of all types of terror events in each country-year.<sup>9</sup> Most studies of terror events have used the ITERATE data. I use GTD because the ITERATE data code only transnational events, which they define as events where the perpetrators and victims are from different countries. Due to this emphasis, the ITERATE data exclude the entire category of “domestic terror” such as the Oklahoma City bombing, the 1986 Olympics bombing in Atlanta, and so on. The GTD codes all terror events regardless of the nationality of the perpetrators and targets and is thus allows one to create a more valid variable for studying the production of terror events.

That said, one shortcoming of the GTD data is that it is missing data for the year 1993

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<sup>6</sup>Though the Global Terrorism Data is available from 1970-1997 the sample used here is restricted to the period 1980-1997 because the CIRI data are only available back to 1980.

<sup>7</sup>I estimated the models using the `zinb` routine in `Stata 10.0`, and the `fitstat` package (Long and Freese, 2000) to generate the information criteria scores reported below.

<sup>8</sup>See the website at <http://www.start.umd.edu/data/gtd/> for a description of the larger project from which these data are developed.

<sup>9</sup>The GTD allows one to break down terror events by a number of types. This analysis groups all types of events.

(it covers the years 1970-97; LaFree and Dugan, 2007). To produce data for that year I used Amelia, a multiple imputation program. More specifically, I used the one and two year lagged and lead values of the GTD data, and the contemporaneous, and one and two year lagged and lead values of the events from the ITERATE dataset to impute the missing values for GTD, 1993. This procedure produced five new datasets, and I calculated the mean score for each country across the imputed data and rounded the value to the nearest integer. I then merged those 1993 data into the GTD data from ICPSR, thus producing a complete data set for 1970-1997.<sup>10</sup>

I use variables from the Cross National Time Series Archive (Banks, 2001) to measure violent and non-violent dissent.<sup>11</sup> There are two measures of non-violent tactics available: anti-government demonstrations<sup>12</sup> and general strikes.<sup>13</sup> Banks' data has three measures of violent tactics: riots,<sup>14</sup> assassinations,<sup>15</sup> and acts of guerrilla warfare.<sup>16</sup>

To measure government coercion I use the CIRI project's physical integrity rights index (Cingranelli and Richards, 2004). The *Short Description of Variables* file the CIRI project describes this index as follows:

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<sup>10</sup>To assess the validity of the imputed data I turned to the replication dataset for Li (2005) and calculated the bivariate correlation between each of his independent variables and the GTD data for 1993. I then compared those correlations to the bivariate correlations between his ITERATE variable and those for my GTD variable. This is a concordance validity analysis. I was moderately surprised at how similar were the correlations: in every case the sign and statistical significance are the same, and in most cases the size of the correlation is very similar.

<sup>11</sup>More specifically, I use the domestic conflict event data which Banks codes from the daily files of the *New York Times*.

<sup>12</sup>Banks defines these as "Any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority, excluding demonstrations of a distinctly anti-foreign nature." The source for this, and the following, variable definitions is the *Cross-National Time Series Data Archive Variable Location and Variable Descriptions* electronic file.

<sup>13</sup>General strikes are "Any strike of 1,000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority."

<sup>14</sup>Riots are "Any violent demonstration or clash of more than 100 citizens involving the use of physical force."

<sup>15</sup>The project defines assassinations as "Any politically motivated murder or attempted murder of a high government official or politician."

<sup>16</sup>Acts of guerrilla warfare are "Any armed activity, sabotage, or bombings carried on by independent bands of citizens or irregular forces and aimed at the overthrow of the present regime."

Table 1: Measures of Democratic Institutions

Concept	Variable	Source
Voice	acldreg	Alvarez et al. (1996)
Veto (Govt Constraint)	Xconst	Polity IV
	PolConIII, PolConV, j	Henisz (2000)
Free Expression	Free Speech	Cingranelli and Richards (2004)

[PHYSINT] Physical Integrity Rights Index:

This is an additive index constructed from the Torture, Extrajudicial Killing, Political Imprisonment, and Disappearance indicators. It ranges from 0 (no government respect for these four rights) to 8 (full government respect for these four rights). Details on its construction and use can be found in: David L. Cingranelli and David L. Richards. 1999. "Measuring the Level, Pattern, and Sequence of Government Respect for Physical Integrity Rights." *International Studies Quarterly*, Vol 43.2: 407-18.

The operational indicators for the democratic institutions are listed in the table below.

The Alvarez et al. (1996) measure of democracy, as updated by Cheibub and Gandhi (2004), is a binary indicator of whether competitive elections were used to select the executive. I use two data sources to find measures for the veto concept: Polity IV (Marshall and Jaggers, 2005) and the Political Constraints Data (Henisz, 2000).

To examine the hypothesis implied by Gurr's (1988) garrison state argument I use a measure of the size of the state's coercive bureaucracy. This is a proxy for the successful use of coercion that Gurr is really interested in, but it is readily available (i.e., does not require a substantial data collection effort), and thus provides a useful first look to determine whether the hypothesis generates support that might warrant data collection. The justification for the proxy status is as follows: Gurr's successful use argument suggests that states that are successful will continue to invest in the coercive capacity of the state. As such, *ceteris paribus*, the greater the frequency of successful use of state coercion, the larger will be the

state's coercive apparatus.

A measure of the number of people employed by the military, police, and prison systems would be ideal, but cross-national data on police and prison personnel are very spotty. Data on military personnel are available, however, and I use the Correlates of War project's National Material Capabilities (version 3.02) data (Singer, Bremer and Stuckey, 1972; Singer, 1988). I then interact that variable with the measures of veto to evaluate the hypothesis that

Finally, I need to measure three PITF variables: infant mortality, trade openness, and type of polity. For infant mortality I use the data described in Abouharb and Kimball (2007). I turn to the Penn World Table (6.2) to measure trade openness, which is defined as the ratio of the sum of imports and exports to gross domestic product in real terms (Heston, Summers and Aten, 2006). The Polity IV project is the source for my measure of polity type (Marshall and Jaggers, 2005). I took the difference of the `democ` and `autoc` variables. Because I have my own arguments about the impact of distinct democratic institutions, I not only explore the impact of the summary measure of polity type, but also explore the components listed above.

## 2.2 Statistical Results

I estimated the parameters and associated statistics for several models before settling on the one reported below. I first estimated a model that included only the PITF variables, and established that both trade openness and democracy, though not infant mortality, influence the number of annual terror events a country experienced between 1980 and 1997. I then added the government and dissident behavior variables and found that some of them had the expected effect, while others did not (more on this below). Finally, I added the political institution variables and found that voice (elections) and freedom of expression have an impact. These preliminary estimates demonstrated that there was sufficient support for the

components of the specification to continue to a complete model.

Having briefly described that work I now turn my attention to the results from the final model. I have thus far waved my hands at the issue of specifying covariates for the two equations in the zinb model. By way of background, the zinb model has one equation that estimates the probability that a country-year observes zero acts of terror. The important point here is that there are two types of zeros: those associated with country-years that—given covariate values—might have produced a terror event and those country-years that—given covariate values—will never produce a terror event. The second equation is an event count model that estimates the expected number of terror events conditioned on the expectation that the country year will produce at least one such event. That it is a negative binomial reflects my assumption that the probability of multiple events in any given country year is not independent.

An important debate exists in the literature on the number of terror events across different types of polities, and Li (2005) and Drakos and Gofas (2006) provide excellent discussions. The substantive question is whether democracies, which embrace freedom of the press, invite greater levels of terror (as discussed above) or whether it simply appears that this is so because events are rather likely to be reported in a democracy (with a free press), but may not be reported in a country that censors the media. This raises a methodological concern as the data might be biased: it is unclear whether a country that exhibits a count of zero events but censors the media has zero events because dissidents recognize that terror will be relatively ineffective, or if there are zero events recorded because the government successfully censored the reporting of the events that did occur. Both scenarios are plausible, and the data cannot help us sort it out.

It turns out that zero inflated (aka sample selection) models were constructed to address precisely this sort of problem. I use the first (logit) equation in the zinb model to specify those countries that are zero because there really were no events (dissidents used different

tactics) from those that did experience events, but successfully censored their reporting. This discussion strongly suggests that I should include freedom of expression in this equation. I also include the PITF measures, the other political institution measures (to establish that freedom of expression, not a different institution, is operating), the preceding year's value of respect for human rights, the binary measure of whether there were any terror events in the country the year prior, and a series of dummy variables for each major world region.

Table 2 reports the results from the zinb regression model. I report coefficient estimates for the logit portion of the model (which estimates the probability of observing zero events) and incident response rates for the negative binomial portion of the model (which estimates the number of observed events given at least one such event). The incident-rate ratio (IRR) which is equal to the exponentiated value of the coefficient estimate. An IRR value of one indicates that the independent variable has no impact on the number of observed events. A value less than one indicates a negative relationship, and a value greater than one represents a positive impact on the number of events.

I begin with the coercive behavior of the state, as measured by the last year's value of respect for the physical integrity of the person. The IRR of 0.89 suggests that respect for integrity rights reduces the rate of terror events: every additional point on the CIRI scale—which ranges from 0 to 8—is associated with an 11% decrease in the number of terror events. This is a substantially large effect and suggests that respect for the integrity of the person (freedom from torture, extrajudicial killing, political imprisonment, and disappearance) is strongly associated with lower levels of terror. Put another way, coercion that involves violating rights tends to spur, not deter, the use of terror. This finding is inconsistent with the “repression imposes costs” perspective advanced by Lichbach (1987); Moore (1998), and strongly supports the case made by Wilkinson (2001), among others, that democracies must not abandon their liberal safeguards of freedom when challenged by dissidents that use terror.

Table 2: Number of Terror Events, 1980-97

Regressor	Zero Events (Logit) Coefficient Estimate	Event Count (NB) Incidence-Rate Ratio
<i>Physical Integrity Rights<sub>t-1</sub></i>	0.116 (0.072)	0.893***
<i>Assassinations</i>	—	1.099***
<i>Guerrilla Attacks</i>	—	1.127
<i>Riots</i>	—	1.011
<i>General Strikes</i>	—	1.067
<i>Anti-Government Demonstrations</i>	—	1.056*
<i>Voice (Elections)</i>	0.294 (0.414)	0.733**
<i>Legislative Veto</i>	—	1.127
<i>Judicial Veto</i>	—	0.713**
<i>Freedom of Expression</i>	-0.356* (0.193)	0.560***
<i>Military Personnel/Pop<sub>t-5</sub></i>	-1.588*** (0.219)	5.62e+08**
<i>Jud Veto * MilPer/Pop<sub>t-5</sub></i>	—	6.38e+09**
<i>Infant Mortality Rate</i>	0.002 (1.082)	0.998
<i>Trade Openness</i>	-36.048*** (1.480)	0.996**

Table continued on next page...

Table 3: Number of Terror Events, 1980-97 Continued

Regressor	Zero Events (Logit) Coefficient Estimate	Event Count (NB) Incidence-Rate Ratio
<i>log of Population</i>	—	1.189***
<i># of Terror Events<sub>t-1</sub></i>	—	1.010***
<i>Dummy # of Terror Events<sub>t-1</sub></i>	-22.048*** (3.073)	—
<i>Total Veto Players</i>	-1.116 (0.684)	—
<i>African Country</i>	0.120 (0.466)	0.883
<i>American Country</i>	0.381 (0.442)	1.014
<i>Asian Country</i>	0.966* (0.503)	1.002
<i>European Country</i>	0.306 (0.493)	0.941
<i>Constant</i>	0.496 (0.685)	—
<i>Observations</i>	Zero: 634	Non-zero: 1,099

\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$  (two-tailed).  
Zero Inflated Negative Binomial Regression Model.  
Standard errors are given in parentheses.  
Pseudo Log Likelihood: -4900;  $\alpha$ : 1.459 (0.069)

Turning to the argument about “outbidding” among dissident groups leading to evidence that other violent tactics are compliments to terror and non-violent tactics are substitutes, note that only two of the dissident behavior variables produced statistically significant coefficient estimates: Assassinations and Anti-Government Demonstrations. Both are associated with increased numbers of terror events, thus I fail to find support for the substitution hypothesis, but the positive relationship for Assassinations is consistent with the substitute hypothesis. The size of the effects are substantial: for each additional assassination the results lead one to expect a 10 percent increase in the expected number terror events, and each additional demonstration is associated with an expected six percent increase.

What of the liberal democratic institutions? The presence of meaningful elections (voice) is associated with 27% fewer expected terror events, a rather substantial impact. I had available several measures of veto (separation of power), yet only one of them—judicial veto—exhibits any influence. To be more specific, the presence partisan composition of the legislature; a summary measure of legislative, judicial, and federal veto players; and the Polity IV projects executive constraints variable all fail to produce a statistically significant coefficient estimate when I included them in a model. Thus, the data fail to support the argument advanced by Li (2005) that separation of powers tie the hand of the executive and thus make liberal democracies attractive targets for terror tactics. They also fail to support the hypothesis I advanced combined the insights of Tsebelis (2002) and Gurr (1988): though they are not reported in the table, none of the interaction terms of military personnel per capita five years prior and these measures of veto produced statistically significant sets of coefficients. Further, as I explain in the following paragraph, the one veto variable that did exhibit an impact has the opposite impact of that expected by my argument.

Judicial veto, military personnel per capita five years prior, and the interaction term of the product of the two all produce statistically significant coefficient estimates.<sup>17</sup> The

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<sup>17</sup>As Braumoeller (2004) and Brambor, Clark and Golder (2006) explain, a fully proper analysis of inter-

hypothesis implied by my conjunction of the Tsebelis (2002) and Gurr (1988) arguments anticipates that all three variables will have a positive impact on the number of terror events. Yet table 2 records that while military personnel per capita five years prior, and the interaction term have IRR values above one, and thus a positive impact, the IRR for judicial veto is less than one, indicating a negative relationship. More specifically, in countries where the courts are unwilling to issue rulings that challenge the government the proportion of the population that served in the military five years prior is strongly positively associated with the expected number of terror events.<sup>18</sup> This is quite consistent with the conventional wisdom that terror is the ultimate asymmetric warfare weapon (i.e., the weapon of the weak): larger militaries make guerrilla struggle and other forms of violent and non-violent protest less likely to produce success, thus making terror a relatively more appealing tactical choice.

In addition, when judges are willing to challenge government, but the military is large, there is still a very strong positive impact the expected number of terror events.<sup>19</sup> However, and this is the most interesting part, when a country has courts that will veto executive decisions *and* no military personnel (Botswana and Iceland meet this condition), the number of expected terror events *decreases* 29%! To determine how small a country's military must be, relative to its population, for the existence of an independent judiciary to lead to a reduction (rather than increase) in the expected number of terror events would require a more complex analysis, as referenced in the footnote above. But the back-of-the-envelope examination of the IRRs in table 2 provide the intriguing, and as yet unreported, finding that independent courts exercise a non-trivial and interesting impact on the number of

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action terms is more complex than this discussion implies. Nevertheless, the discussion here captures the broad scope of the effects. More specific analysis is possible, but not necessary for my purposes.

<sup>18</sup>The IRR value of 56.2 million is misleading because that is based on a shift from zero people in the military to the entire population in the military, and the empirical range of the variable is from zero to 20% of the population serving. Nevertheless, the effect is very large: in the absence of an independent judiciary that will challenge the executive, larger militaries invite considerably larger amounts of terror.

<sup>19</sup>Again, the IRR value of 638 million is not meaningful as the range from zero to one for the variable is way out of sample. Nonetheless, the effect is both positive and very large.

terror events a country is likely to experience. Like the finding described above about the impact of respect for personal integrity rights on the reduction of expected terror events, this result strongly supports the arguments advanced so eloquently by Wilkinson (2001), among others, regarding the importance of liberal democratic institutions for fighting terror. While legislatures have no discernible impact, according to these data, the combination of an independent judiciary and a small military, relative to the population, reduces the expected number of terror events. As Wilkinson (2001) argues, reliance on police, rather than troops, to combat terror is needed to get this effect.

Finally, the results in table 2 indicate that freedom of expression has a very large negative impact: the IRR of 0.36 leads us to expect a whopping 64% reduction in the expected number of terror events for a country with a partly free press relative to one that censors strongly, and another 64% for a country with a fully free press compared to one with a partly free press. This is a very large effect indeed. Further, note that this is the opposite impact of that expected by those who argue that democracies will seem to attract more terror because the press will report them. Importantly, the results do support that argument: the estimate of -0.36 in the logit equation of the zinb model indicates that the greater the extent to which the country protects freedom of expression, the less likely the country is to exhibit zero terror events. To eliminate the awkward double negative: the negative estimate shows that countries that protect freedom of expression are more likely to observe at least one terror event. The IRR of 0.36 in the negative binomial equation, on the other hand, shows that greater protection of freedom of expression is associated with considerably fewer terror events given that there is at least one.

To provide a summary about the impact of liberal democratic institutions, all three measures have the expected impact on the number of terror events. These results demonstrate the value of distinguishing among different liberal democratic institutions. As Gleditsch and Ward (1997) and Davenport (2007*b*), among others, emphasize, the study of the impact of

democratic institutions on political behavior has focused too strongly on single, summary indicators of democracy. These results demonstrate the value of distinguishing among different types of liberal democratic institutions.

Those are the major findings of interest from the analysis. Before turning my attention to implications, however, I wish to briefly note a few other points. First, a country's openness to trade both increases its likelihood of experiencing at least one terror event *and* decreases the expected number of such events given that at least one occurs. In other words, like freedom of expression, openness to trade makes a country more vulnerable to experiencing at least one terror event, but among the set of countries that experience terror, trade openness reduces the expected total. The other PITF variable, infant mortality rate, has no impact at all. Infant mortality is widely regarded as one of the best cross national measures of poverty, and despite the popular belief that the use of terror by dissidents is associated with terror, the fact that this is false is so widely known among experts that it is banal. So this finding is not in the least surprising. Second, the lagged value of the total number of terror events also has a positive impact, as anticipated: on average yesterday's terror begets more terror today. Finally, the regional dummies, on the other hand, do not have much of an impact: Asian countries tend to have a lower likelihood of observing at least one terror event than countries in the Middle East, but other than that knowing the region from which a country comes is not useful for anticipating the likelihood of a terror event, nor the number of such events should at least one occur.

### **3 Implications**

What are the key take away points this inquiry has to offer? On a personal note the lack of support for my own hypothesis about substitution is mildly disappointing. Like all scientists I am loathe to throw out an appealing argument that has served well in other contexts, but

this initial effort suggests that terror and non-violent protest are not substitutes. I plan to probe that relationship in the future.

The primary take away message, in my opinion, is that this study provides strong statistical support for the arguments advanced by Wilkinson (2001), among others, that liberal democracies are ill served by [1] heeding arguments that the threat to the state posed by terror attacks justifies necessitates the adoption of temporary policies that weaken protections of liberty, and [2] turning to the military, rather than the police, as the primary bureaucracy that should fight the dissidents. All of the findings that come to bear on these debates are unequivocal with respect to both our ability to reject the likelihood that they are due to chance and in regards to their substantive import in terms of changing the expected number of terror events. And Wilkinson and other champions of liberal democratic institutions and policing dissidents who use terror receive support from each of them: respect for personal integrity rights; the combination of judicial veto and a relatively small military; and the protection of freedom of expression are all strongly associated with lower expected numbers of terror events.

Lastly, turning my attention to theory construction, while I failed to find support for substitution, the broader case I make for studying terror as a tactic chosen by dissidents in response to the behavior of both the government and other dissidents, and doing so within the context of political and socio-economic context, is strongly supported by this inquiry. Not only do the results show that there dissidents respond strongly to government behavior, and their own past behavior, they also show that there is, as I expected, an interesting complimentary relationship between terror and some other violent tactics. The vast majority of person hours invested in scientific inquiry to these questions has focused exclusively upon political and socio-economic context and ignored the strategic interactions among dissidents and governments that produce these types of events. I am optimistic that studies such as this one will persuade future researchers to invest more time in developing theories, research

designs, and the relevant data to better unravel the patterns of strategic interaction that exist within those political and socio-economic contexts.

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