Web Appendix for "Terrorism, Democracy, and Credible Commitment"

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March 30, 2010

Web Appendix

This web appendix includes a series of additional information related to "Terrorism, Democracy, and Credible Commitment." First, we outline the coding rules for the variables used in the quantitative analyses in the paper as well as variables used in sensitivity analyses. Second, we report and discuss a series of robustness checks to make sure that our results are stable when adjusting the specification, employing alternative estimators, and using different operationalizations of the dependent and independent variables. Third, we utilize propensity score matching to pre-process our data and isolate the impact that our key measure has independent of the effects of the other indicators. Using the pre-processed data, we estimate another series of models showing that the results are not dependent upon the assumptions of any particular estimator.

Coding Rules

Baseline Model

Our outcome variable is coded as a count of *domestic terror attacks* from the Global Terrorism Database (2007). It is a composite indicator of all types of terrorist violence committed by a non-state actor against civilians and directed at the government. The measure includes the following types of attacks: assassinations, bombings, facility attacks, hijackings, kidnappings, maimings, assaults, disruption, and arson.

Independent Judiciary is a dichotomous variable, coded 1 if the state has an independent judiciary and 0 if it does not, according to Henisz (2002).

Regime is an index ranging from high autocracy (-10) to high democracy (10) from the Polity project (Marshall and Jaggers, 2001).

$Regime^2$ is a squared measure of the *Regime* index ranging from (0) to (100), created from the Polity project data (Marshall and Jaggers, 2001).

Regime Transition is a dichotomous variable coded 1 if the state is undergoing a regime transition in a given year and 0 if it is not, according to Cheibub and Gandhi (2004).

Capabilities is proxied by the Composite Indicator of National Capabilities (CINC) version 3.02, based on Singer and Stuckey (1972). The index is based on the following six variables: Military Personnel, Military Expenditures, Energy Production, Iron/Steel Production, Nominal Urban Population, and Nominal Total Population.

GDP is a measure of yearly GDP from Fearon and Laitin (2003).

Spatial Terror Lag is constructed as follows: A NTxNT block-diagonal weight matrix records the distance between all states within 1000 km (we obtain the weight matrices from Gleditsch and Ward, 2000). All bilateral distances greater than 1000 km are recorded as zero. The effect of terrorism in the states in the surrounding region are aggregated by multiplying the weight matrices and a temporally lagged, $NTx1$ measure of terrorism in these respective states. This results in a temporally lagged, spatial lag of terrorism in surrounding states that captures the potential diffusion of domestic terrorism from abroad.

Variables for Sensitivity Analysis

Independent Judiciary is a trichotomous variable, coded 2 if the judiciary is independent, 1 if judiciary is somewhat independent or subject to influence from the executive, and 0 if the judiciary is non-independent from Tate and Keith (2007).

Proportional Representation is a dichotomous variable coded 1 if the state has a proportional representation electoral system and 0 if it does not, according to Li (2005).

Majoritarian is a dichotomous variable coded 1 if the state has a majoritarian electoral system and 0 if it does not, according to Li (2005).

Mixed is a dichotomous variable coded 1 if the state has a mixed electoral system and 0 if it does not, according to Li (2005). The excluded reference category for these three dichotomous variables is a nondemocratic system.

Unified Democracy is a composite measure of ten different measures of regime type from Melton, Meserve and Pemstein (N.d.).

Robustness

In this section, we report and briefly discuss the results of additional analyses that test the robustness of our results. We begin by revisiting the baseline model from the main paper and report the results with and without the independent judiciary measure. The first two models in Table 1 report these results (where the second model is identical to the baseline model from the paper). Adding the independent judiciary measure induces only one significant change in the other variables: the measure of GDP per capita, which was originally not statistically significant, becomes significant at the 5% level. The effect of a regime transition appears to be subject to less uncertainty as the p value drops below the 1% threshold of statistical significance. The third model includes a lagged measure of terrorism, which is statistically significant. The result on the independent judiciary attenuates, but remains negative and statistically significant. As discussed in the main paper, we note that a lagged dependent variable may not be appropriate in this context. We estimated other models accounting for history, such as a moving average of past terror counts and an annual average measure, and also found that the key results do not change in any qualitative way.

We conducted a series of additional analyses that tested the sensitivity of our model to the years included as well as the operationalization of the domestic and transnational terrorism measures. Because we are primarily interested in whether the independent judiciary result changes, we report only the incidence rate ratios for the independent judiciary measure across all of the different models in Figure 1. Each dot-line represents the independent judiciary result for a given model; the dot represents the incidence rate ratio estimate and the lines are the 95% confidence intervals. Any confidence interval that crosses 1 would mean that the estimate is not statistically significant.

Domestic Terror	Excluding Ind Judiciary		With Ind Judiciary		With Lagged DV	
	IRR	Std. Er.	IRR	Std. Er.	IRR	Std. Er
Independent Judiciary	–	–	0.365**	0.099	0.668*	0.115
Regime	1.108**	0.018	1.127**	0.018	1.074**	0.013
Regime^2	0.980**	0.005	0.981**	0.005	0.990**	0.003
Regime Transition	1.955*	0.526	2.011**	0.539	1.753	0.513
Capabilities	0.000	0.001	0.000	0.001	0.001	0.003
Population	1.850**	0.233	1.968**	0.250	1.708**	0.145
GDP	1.046	0.037	1.086*	0.045	1.050	0.028
Spatial Lag	1.007**	0.001	1.006**	0.001	1.003**	0.001
Lag of Terrorist Events	–	–	–	–	1.041**	0.007
	LRTEST	X^2=228	LRTEST	X^2=250	LRTEST	X^2=271
	Obs=3615	Countries=153	Obs=3562	Countries=149	Obs=3562	Countries=149

Significance levels : $*$: 5% $**$: 1%

Table 1: Additional Analyses of the Effect of an Independent Judiciary on Domestic Terror Events using Negative Binomial Regression Models, 1970–1997 *The first model excludes independent judiciary and the second model adds it in to illustrate how the results of the model change. The third column includes a lagged dependent variable (see comments in the main paper about our choice of lags.)*

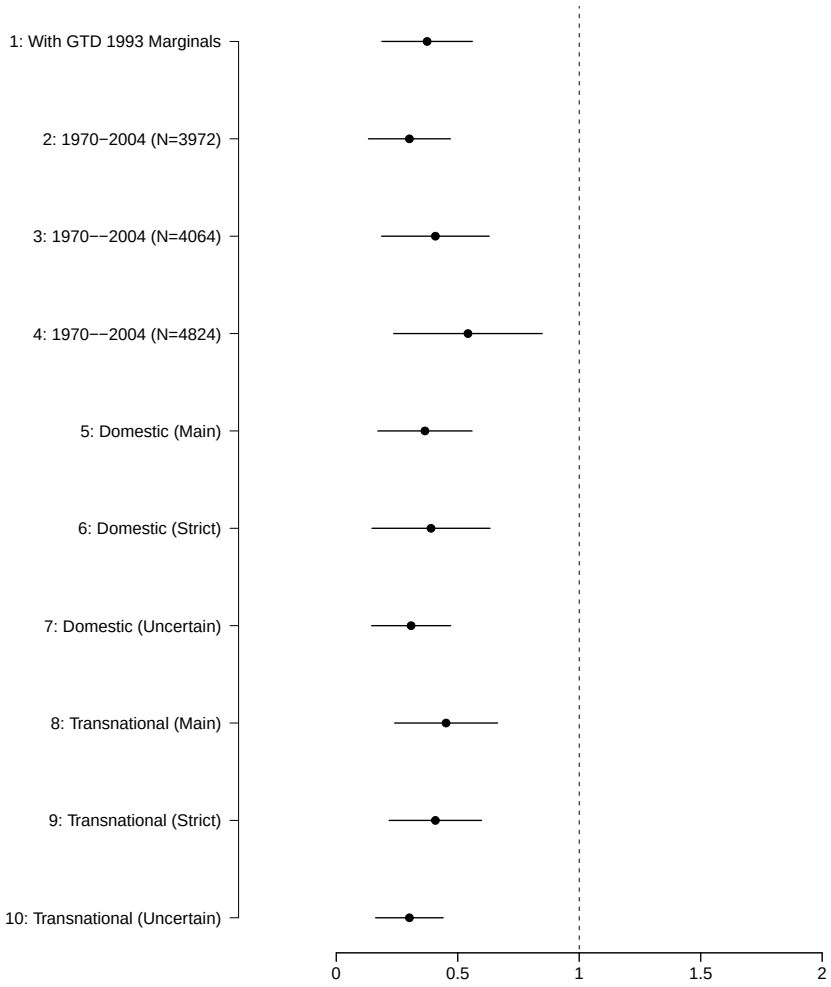

Figure 1: Incidence Rate Ratio Plot for Independent Judiciary Using Additional Data and Different Measures *Each point represents a different regression and the line extending in both directions from the point represents the confidence interval. In this case, none of the confidence intervals for the incidence rate ratios cross 1, which makes all of them statistically significant.*

As the figure shows, none of these additional analyses changes the main result of the paper. In the first model, we added the GTD's marginal estimates for the year 1993 and find that the incidence rate ratio is almost identical to the baseline model (IRR= 0.373 vs. 0.365 in the baseline). Models 2–4 in the figure display the results of adding the years 1998–2004 to the 1970–1997 reported in the baseline analysis. Because of data constraints, we could not obtain consistent data through the year 2004 for all of our variables. Thus, our strategy here was to drop a few control variables with the primary goal of increasing the number of observations on the independent judiciary measure. We were able to increase the number of

observations from 3,562 in the baseline model to 4,824 (a substantial increase) in the fourth model in this figure and find that the results are largely consistent with the baseline model. They appear to attenuate slightly, but the confidence intervals do not cross 1 (what would be the equivalent of a zero effect).

Models 5–7 operationalize the domestic terrorism measure differently than the baseline. The GTD offers information that allows one to distinguish domestic from transnational terrorism, but this information does not always clearly point towards a domestic or transnational coding. Thus, in addition to the main measure reported in the text (#5 here), we coded a strict version of domestic terrorism and an uncertain measure, which is essentially a more lenient measure that could be capturing some transnational terrorism. Although the precise estimates change some, overall the result is very similar across the various models.

Finally, in models 8–10 we explore whether transnational terrorism appears to be fundamentally different from domestic terrorism. Using three different versions of a transnational terrorism variable, we find that the results are quite similar to those for domestic terrorism. If domestic and transnational terrorism follow different causal logics, we would expect the results similarly to be different. It is possible that, indeed, both processes are fundamentally similar. We question this conclusion, however, and instead suggest that transnational terrorism questions need to be modeled quite differently. Because transnational terrorism imply that more than one nationality is involved and that one nationality initiates action against a different target nationality, the process should be modeled using a directed-dyad unit of analysis. The country-year unit of analysis is appropriate for domestic terrorism data, but is likely less suitable for transnational terrorism. In other work (Young and Findley, 2009), we constructed a directed-dyad data set of terrorism so that transnational terrorist behavior can be modeled more accurately. Unfortunately, collecting and organizing the data into directed dyads is laden with difficulties — most notably missing data on perpetrators or targets. Thus, we note the possibility that domestic and transnational processes could be quite similar or different and that future analyses need to address this more clearly.

Finally, we considered whether violence by the state increases or decreases terrorist violence and found that state violence is positively correlated with terrorism and this result is statistically significant (IRR = 2.487 and p= 0.004). This finding is consistent with others who have found a positive relationship between state violence and terrorism (Walsh and Piazza, 2010; Mullins and Young, 2010). In the model including state violence, the result for independent judiciaries is still negative and statistically significant (IRR = 0.589 and p= 0.014).

Zero-Inflated Negative Binomial

Because there are so many zero counts in the data, we also estimate a zero-inflated negative binomial model and include the results in Table 2.[1] Under the "Frequency (Count)" heading in the table, we report the negative binomial portion of the zero-inflated negative binomial model. For each of these results, we report the coefficients, standard errors, and incidence rate ratios. Under the "Probability (Inflate)" heading, we report the logit portion of the model. For both sets of covariates, we also report percent change in the predicted probabilities and odds. For the count equation, this value simply restates the IRR in percent terms.

Having an independent judiciary is again significant and reduces the expected number of terror events by 67.5%. Thus, both the substantive and statistical significance of these findings are not trivial and corroborate the findings from the negative binomial model. The result for independent judiciaries in the inflate portion is not significant, suggesting that the logic applies primarily to the frequency of terrorism.[2]

The regime and regime squared variables both behave as expected and continue to be significant except for the regime squared variable in the inflate equation. We decided to examine this closer to understand this interaction better. Figure 2 shows the marginal effect of the measure of democracy over the range of values for our measure of democracy, the Polity

[1] It is also argued elsewhere that a zero-inflated model is necessary because of two types of zeroes

[2] Recall that the sign on each coefficient in the inflate equation indicates the likelihood of always having no terror. Thus, a negative sign indicates that a state is likely to experience terror.

Covariate	**Base Model**		**Interpretation**	
Frequency (Count)	Coef.	S.E.	IRR	% Δ in E(Count)
Independent Judiciary	-1.068**	0.285	0.344**	-65.6
Regime	0.101**	0.016	1.106**	10.6
Regime^2	-0.014**	0.004	0.986**	-1.4
Regime Transition	0.438†	0.252	1.549†	54.9
Capabilities	-1.617	5.028	0.198	-80.2
Population	0.555**	0.133	1.743**	74.3
GDP	0.009	0.034	1.009	0.9
Spatial Lag	0.004**	0.001	1.004**	0.4
Probability (Inflate)	Coef.	S.E.	OR	% Δ in Odds
Independent Judiciary	-0.296	0.843	0.744	-25.7
Regime	-0.100**	0.023	0.905**	-9.5
Regime^2	0.007	0.006	1.007	0.7
Regime Transition	-0.423	1.017	0.655	-34.5
Capabilities	29.979*	12.875	0.000*	0.0
Population	-0.571**	0.122	0.565**	-43.5
GDP	-0.264†	0.139	0.768†	-23.2
Spatial Lag	-0.179*	0.068	0.836**	-16.4
Observations: N = 3562, Nonzero Obs = 1522, Zero Obs = 2040				
Countries=149, IRR are incidence rate ratios, OR are odds ratios				
Significance levels : † : 10% * : 5% ** : 1%				

Table 2: Estimation of the Effect of an Independent Judiciary on Domestic Terror Events using a Zero-Inflated Negative Binomial Regression Model, 1970–1997 *The bottom of the table provides estimates for the "inflate" portion of the zero-inflated model or the portion that models membership in the "always zero" group. The top of the table presents estimates of the expected influence that each variable has on the frequency of terror events, conditional upon there being at least one attack.*

democracy-autocracy scale.[3] Highly authoritarian states have the smallest marginal effect on expected terror counts. This impact increases until polity reaches roughly 7 than begins to decrease as regimes become highly democratic.[4] The impact that a highly democratic regime has on expected terror counts is roughly the same as a low democratic regime or one with a polity score of around 3 or 4. An important caveat is that low authoritarian states

[3]Similar to an interaction plot, the marginal effect of the key independent variable (Polity) should only be interpreted as conditional on the value of the modifying variable (Polity). See Brambor, Clark and Golder (2006) for a discussion of interpreting interaction terms.

[4]Calculating the minimum point of the curve can be done by estimating $\frac{-b}{2a}$. In our case this means estimating $\frac{-(-0.100)}{2(0.007)}$, which equals 7.143. This is a little confusing as the minimum is actually the maximum when we flip the axes of the graph. Recall, that the signs are switched in the first stage of a ZINB because they represent the impact that the measure has on the likelihood of being in the always zero group. In a traditional logit/probit, the sign indicates the directional impact on the likelihood of experiencing the outcome of interest.

generate less terror than high democracies suggesting that other types of political violence may be more useful in authoritarian regimes in which the state may be acquired by other means such as coups or civil war. With respect to Pape (2003), these findings suggest that democracies are targeted when they are less able to respond, rather than when they are highly responsive. Pape (2003), however, seems correct that most democracies are more likely to generate terror events than most autocracies.

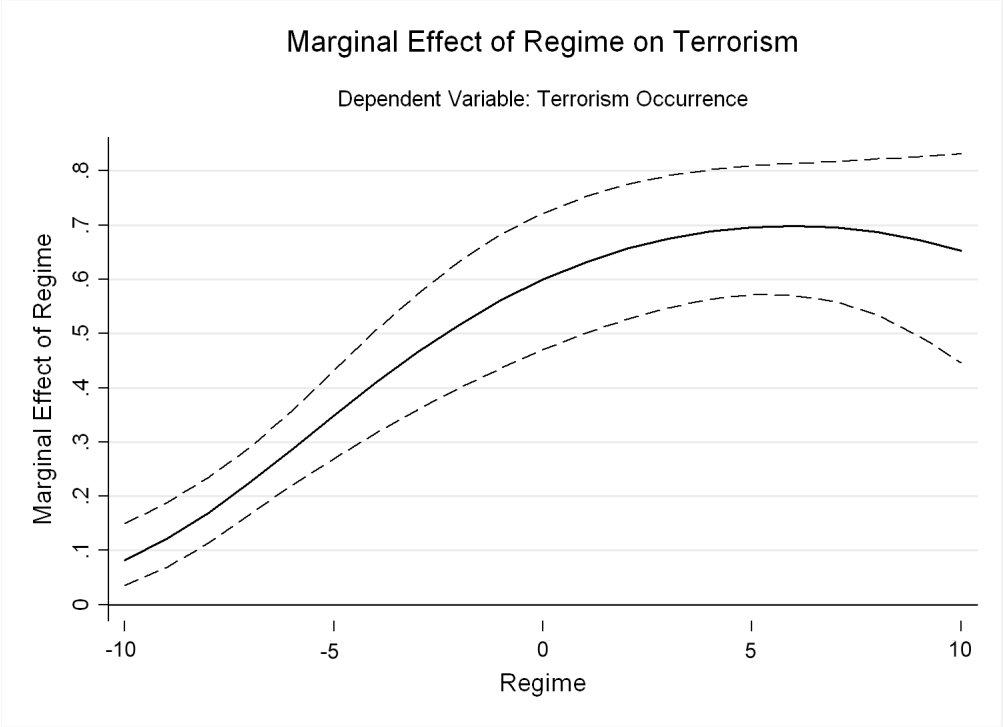

Figure 2: Regime Predicted Probability Interaction Plot

The presence of a regime transition increases expected counts by 53.4%, although this variable is only significant at the 0.1 level in the count equation. Finally, larger populations appear to be correlated with more terror and the likelihood of terror also appears to increase if neighboring states have experienced terror in the previous year. An extra terror attack in a neighboring state increases the expected count of terror attacks in the home state by 0.4%. The inflate portion of the model also supports this. Capabilities and GDP on the other hand have mixed results: the count portion of the model shows no effect, but the inflate portion

shows effects for both. In the inflate portion, capabilities make the incidence of terrorism less likely and higher GDP makes terrorism more likely, the latter result being consistent with the negative binomial models. To ensure that our results are not dependent on any one model specification we now use matching methods to evaluate the effect of independent judiciaries.

Matching

While the results for the measure of credible commitments, independent judiciary, is robust across different model specifications, as Ho et al. (2007) note, our estimates depend on modeling assumptions and different assumptions "can yield different causal inferences" (2). To reduce the dependence that our inferences have on the assumptions of the negative binomial and zero-inflated negative binomial regression models, we *pre-process* the data using propensity score matching techniques. This preprocessing allows us to move towards a more experimental-like condition in which our key independent variable, or *treatment* variable, is more independent of background covariates.[5] Because observational studies like ours lack control over the assignment of observations to treatment conditions (in this case having an independent judiciary), there can be profound differences on observed covariates between the *treatment* group — states that have an independent judiciary — and the *control* group — states that lack an independent judiciary.

For example, states that have independent judiciaries may also tend to be highly democratic or have higher than average levels of capabilities. To pre-process our data, we assign propensity scores to all of the observations in our data that represent the likelihood that an observation has received the treatment. In this case, that means each observation is assigned a probability that it has an independent judiciary based on a logit or probit model with the other covariates included to predict this probability. Observations that have similar propensity scores but vary on whether they received treatment are then *matched* and placed in the data set. Unmatched control and treatment observations are discarded. This allows for the

[5]See Rosenbaum and Rubin (1983) and D'Agostino (1998) for thorough discussions of these methods.

Observations	**Neg. Binom.**			**ZINB**		
	Coef.	S.E.	E(Count) % Δ	Coef.	S.E.	E(Count) % Δ
Matching on: Ind. Judiciary						
Base Model (N=3385)	-1.118	0.336	-67%	-1.192	0.341	-70%
Base Model, Time-Series (N=3024)	-1.808	0.534	-84%	-1.080	0.319	-66%

Table 3: Additional Estimates Based on Matched Data: *All estimates are statistically significant at the 1% level. The first row for the independent judiciary variable reports matching based on the covariates from the base model. The second row uses a time-series cross-sectional matching estimator that first matches within year, then combines each year.*

mean and variances of control variables to be similar between treatment and control groups. By balancing the distributions of the covariates in the model, we can better establish the causal effect of our credible commitment variable.

Because the goal of matching is to reduce the bias between the distributions of the treatment and control groups (i.e., establishing better balance), we chose matching approaches that minimized the bias, or maximized the balance, between groups.[6] To predict propensity scores and preprocess the data, we used the "psmatch2" software for Stata and employed kernel matching. See D'Agostino (1998) and Leuven and Sianesi (2003) for a comparison of various matching methods.[7] We pre-processed the data using the covariates from the baseline model. The results are robust to including more covariates for matching.[8]

The results for the key variables are reported in Table 3. First, we employed different estimators–the negative binomial as well as the zero-inflated negative binomial to ensure that the results hold across different estimators. Next, we used a matching estimator that takes into account the time-series cross-sectional data structure. Since one of the assumptions of propensity score matching is that the units are independent, we matched within each year, then combined each yearly data set into one master file for estimation. This approach allows

[6]The bias between the mean values for variables between treatment and control groups was over 33% in the unmatched sample. After matching, average bias is reduced to 7.6% when matching on judiciary. The variance is also reduced across all matched samples as compared to the unmatched sample.

[7]We also used radial, one to one, and nearest neighbor techniques, but the kernel matching had the largest effect on reducing absolute bias between the matched and unmatched sample.

[8]All replication materials are available on the authors' websites. We trimmed 177 observations, 131 untreated and 46 treated observations decreasing our sample from 3,562 to 3,385.

us to avoid matching a unit, such as Argentina in 1976, with Argentina in 1977.[9]

We find that our models utilizing the pre-processed data show an even stronger effect for the judiciary indicators. Having an independent judiciary reduces the expected count of terror by between 66% and 84%. Taken together with the additional tests, the credible commitment results appear to provide robust support for our hypotheses.

[9]See Young (2008) for a detailed discussion of this approach to matching. Simmons and Hopkins (2005) offer a different approach to the problem of time-series matching. They use 6 year periods as their unit of observation, then match treated and untreated groups within these periods. This approach is reasonable for a single treatment such as signing a treaty, but it can not work in the context of a repeated treatment like an independent judiciary, a state that is an oil exporter, or having an autocratic regime.

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