Online Appendices
Competitive Intervention, Protracted Conflict, and the Global Prevalence of Civil War

Noel Anderson
Department of Political Science
University of Toronto

Appendices

A Variable Summary Statistics 2

B 25 Battle Death Threshold Results 3

C Two-Year Intermittency Rule Results 7

D A Note on the Temporal Coverage of the Dataset 10

E Addressing Reverse Causality Concerns 12

F A Note on Case Selection, Data, and Sources 13

A Variable Summary Statistics

Variables, models 1-6	**Obs**	**Mean**	**SD**	**Min**	**Max**
Competitive intervention	955	0.346	0.476	0	1
Cold War	955	0.534	0.499	0	1
GDP per capita (logged)	955	8.011	1.061	5.875	10.842
Democracy	955	0.264	0.441	0	1
Oil producer	955	0.587	0.493	0	1
Population size (logged)	955	17.196	1.431	13.708	20.912
Mountainous terrain (logged)	955	2.797	1.129	0	4.407
Ethnic conflict	955	0.694	0.461	0	1
Secessionist conflict	955	0.459	0.499	0	1
Multiparty conflict	955	0.270	0.444	0	1
UN peacekeeping	955	0.076	0.266	0	1
Rebel territorial control	955	0.511	0.500	0	1
Parity	955	0.073	0.261	0	1
Rebels stronger	955	0.012	0.107	0	1
Additional variables, models 7-11					
Competitive intervention (incl. alleged)	955	0.364	0.482	0	1
No support	955	0.260	0.439	0	1
Government only	955	0.264	0.441	0	1
Rebels only	955	0.130	0.336	0	1
US/Soviet competitive intervention	955	0.063	0.243	0	1
Non-superpower competitive intervention	955	0.283	0.451	0	1

Table A.1: Variable summary statistics

B 25 Battle Death Threshold Results

The dataset analyzed in the accompanying article includes all civil wars fought between 1975 and 2009 with at least 25 battle-related deaths per year and a minimum of 1000 cumulative battle-related deaths over the course of their full duration. The latter criteria serves to minimize concerns over heterogeneity of cases inherent in a low 25 fatality threshold and ensures that cases are sufficiently alike to merit meaningful comparison in empirical analyses. Indeed, one methodological problem with the UCDP/PRIO Armed Conflict Dataset (on which the accompanying article draws) is its inclusion of arguably dissimilar forms of political violence—for example, coups and civil wars (see Anderson and Worsnop, forthcoming). Precisely because we would not expect competitive interventions to afflict coups, including these cases in empirical analyses risks overstating the substantive effect of competitive intervention on civil war duration. Nonetheless, it is valuable to probe empirical findings at different thresholds to confirm that results align with theoretical predictions. To those ends, this appendix reanalyzes the empirical models reported in the accompanying article using a 25 battle-related deaths per year threshold with no cumulative requirement.

Table A.2 replicates Table 2 of the accompanying article. As expected, results show a marginal increase in the substantive effect of the competitive intervention variable in models 2-6 and a strengthening of its statistical significance across all models. In numbers, competitive intervention is shown to decrease the hazard of civil war termination by an average 59 (model 5) to 68 (model 4) percent relative to conflicts that were not experiencing competitive intervention.

Estimates for the controls are largely in line with those reported in the accompanying article, though regime type is no longer statistically significant. Oil producing states are found to experience shorter conflicts on average, while mountainous terrain is associated with longer conflicts in the larger sample. The variable proxying for strong rebels is estimated to be both substantively and statistically significant, but caution is warranted in interpreting this result: the low 25 battle death threshold brings with it the inclusion of a number of coups in the dataset, many of which are coded as having stronger rebels. This coding reflects the fact that most coups are undertaken by disaffected members of a state's own military forces. Consequently, the results reported for the strong rebels variable are likely driven by cases of coups, most of which are very short intrastate conflicts.

Table A.3 replicates Table 3 of the accompanying article. Here again, results are consistent for the competitive intervention variables, both with respect to the inclusion of alleged (unsubstantiated) support in the construction of the competitive intervention variable and in regards to the effect of competitive intervention relative to one-sided support to government or rebel forces. The Cold War indicator is found to be weakly statistically significant in models 7-9. However, once competitive intervention is disaggregated into its superpower and non-superpower varieties, the effect of the Cold War once again becomes indistinguishable from zero (models 10-11).

The substantive effects of the disaggregated competitive intervention variables are likewise consistent with the accompanying article. In numbers, superpower proxy war is found to decrease the hazard of conflict termination by over 80 percent relative to conflicts that were not experiencing a competitive intervention, on average. This result also holds when employing Firth penalized likelihood estimation to account for concerns over monotone likelihood. Likewise, non-superpower competitive interventions are found to decrease the hazard of conflict termination by an average 63 percent relative to conflicts that were not experiencing a competitive

intervention. Once again, these results are consistent with the accompanying article.

In sum, empirical results align with theoretical predictions when employing case selection criteria that both include and exclude a 1000 cumulative battle-related deaths requirement. Competitive intervention is shown to have a substantively and statistically significant relationship with civil war duration, regardless of the threshold criteria employed.

Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
COMPETITIVE INTERVENTION		-0.967***	-0.920***	-1.135***	-0.896***	-1.087***
		(0.176)	(0.184)	(0.193)	(0.175)	(0.184)
		0.380	0.399	0.321	0.408	0.337
COLD WAR	-0.449***		-0.146	-0.192	-0.219	-0.241
	(0.123)		(0.135)	(0.150)	(0.134)	(0.151)
	0.638		0.864	0.825	0.804	0.786
GDP PER CAPITA	-0.021			-0.060		-0.076
	(0.066)			(0.070)		(0.073)
	0.979			0.942		0.927
DEMOCRACY	-0.338			-0.190		-0.250
	(0.210)			(0.214)		(0.206)
	0.713			0.827		0.779
OIL PRODUCER	0.327***			0.384***		0.414***
	(0.125)			(0.142)		(0.143)
	1.387			1.468		1.514
POPULATION SIZE	-0.144***			-0.238***		-0.259***
	(0.055)			(0.053)		(0.060)
	0.866			0.788		0.772
MOUNTAINOUS TERRAIN	-0.120**			-0.094*		-0.114**
	(0.055)			(0.051)		(0.054)
	0.887			0.910		0.892
ETHNIC CONFLICT	-0.140				-0.106	-0.230
	(0.164)				(0.164)	(0.169)
	0.869				0.900	0.794
SECESSIONIST CONFLICT	0.203				-0.060	0.238
	(0.156)				(0.143)	(0.155)
	1.225				0.942	1.269
MULTIPARTY CONFLICT	-1.084***				-0.863***	-0.947***
	(0.205)				(0.204)	(0.215)
	0.338				0.422	0.388
UN PEACEKEEPING OPERATION	0.056				0.386**	0.234
	(0.192)				(0.185)	(0.191)
	1.058				1.470	1.263
REBEL TERRITORIAL CONTROL	-0.265*				-0.054	-0.232
	(0.139)				(0.148)	(0.141)
	0.767				0.948	0.793
PARITY	0.138				0.162	-0.026
	(0.209)				(0.193)	(0.225)
	1.148				1.176	0.975
REBELS STRONGER	1.070***				1.158***	1.032***
	(0.396)				(0.427)	(0.373)
	2.915				3.183	2.807
Conflict Episodes	271	263	263	260	260	260
Terminations	242	234	234	231	231	231
Observations	1321	1296	1296	1293	1293	1293

Table A.2: Cox model estimates. The table lists variable coefficients, with robust standard errors clustered on country in parentheses, followed by hazard ratios (e^{β}). *p $\leq$ .10; **p $\leq$ .05; ***p $\leq$ .01

Variables	Model 7 (incl. alleged)	Model 8 (CI vs. reb)	Model 9 (CI vs. gov)	Model 10 (disaggregated)	Model 11 (Firth)
COMPETITIVE INTERVENTION	-1.022***	-0.928***	-0.888***		
	(0.184)	(0.217)	(0.201)		
	0.360	0.395	0.412		
NO SUPPORT		0.489***	0.529**		
		(0.138)	(0.222)		
		1.631	1.698		
GOVERNMENT ONLY		-0.051			
		(0.191)			
		0.950			
REBELS ONLY			0.025		
			(0.190)		
			1.026		
US/SOVIET COMPETITIVE INTERVENTION				-2.239***	-1.841***
				(0.786)	(0.843)
				0.107	0.159
NON-SUPERPOWER COMPETITIVE INTERVENTION				-1.017***	-0.995***
				(0.190)	(0.209)
				0.362	0.370
COLD WAR	-0.262*	-0.275*	-0.274*	-0.222	-0.226
	(0.151)	(0.159)	(0.159)	(0.151)	(0.151)
	0.770	0.759	0.761	0.801	0.798
OTHER CONTROLS INCLUDED	Yes	Yes	Yes	Yes	Yes
Conflict Episodes	260	260	260	260	260
Terminations	231	231	231	231	231
Observations	1293	1293	1293	1293	1293

Table A.3: Cox model estimates. Models 7-10 report variable coefficients, with robust standard errors clustered on country in parentheses, followed by hazard ratios (e^{β}). Model 11 reports a Cox model with Firth penalized likelihood estimates. *p $\leq$ .10; **p $\leq$ .05; ***p $\leq$ .01

C Two-Year Intermittency Rule Results

The dataset analyzed in the accompanying article measures civil war duration using start and end dates provided in the UCDP/PRIO Armed Conflict Dataset, which defines armed conflict as "a contested incompatibility that concerns government or territory or both, where the use of armed force between two parties results in at least 25 battle-related deaths in a calendar year. Of these two parties, at least one has to be the government of a state" (Gleditsch et al. 2002; Themnér and Wallensteen 2014, 541). A conflict's start date is recorded once the UCDP/PRIO criteria are met; it is considered terminated once it ceases to meet this criteria for one full year.

Notably, however, some conflicts in the dataset drop below the 25 annual battle-death threshold for just one year, only to reignite again the following year. While the accompanying article is most interested in understanding the determinants of sustained military conflict, it is reasonable to ask whether a one-year lull in fighting is sufficient to merit coding a conflict as "terminated." To strike a balance between treating brief, one-year lulls in fighting as terminations and treating distinct onsets of armed violence as the same conflict, a number of scholars employ a two-year intermittency rule—that is, a conflict is considered terminated only when it has failed to meet inclusion criteria for two full years.[1] This appendix reanalyzes the empirical models reported in the accompanying article using this two-year intermittency criteria to examine the robustness of the results to this alternative coding rule.

Table A.4 replicates Table 2 of the accompanying article. The table shows that empirical results are consistent with theoretical predictions regardless of whether a one- or two-year intermittency rule is used to identify conflict terminations. In fact, the substantive effect of the competitive intervention variable in models 2-6 is marginally strengthened when using the two-year rule, and it remains statistically significant across all models. Likewise, the results identified for the control variables are akin to those reported in the accompanying article, with the lone exception that the relationship between regime type and conflict duration is rendered somewhat more tenuous.

Table A.5 replicates Table 3 of the accompanying article. Here again, the table shows that results are consistent when using both the one- and two-year intermittency rules. In terms of both statistical and substantive significance, the results are essentially unchanged across all models. This underscores the robust relationship between competitive intervention and civil war duration and confirms that the findings reported in the accompanying article are insensitive to alternative measures of conflict termination.

1. See, for example, Fearon 2004; Cunningham, Gleditsch, and Salehyan 2009; Cunningham 2011; Wucherpfennig et al. 2012.

Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
COMPETITIVE INTERVENTION		-1.016***	-0.936***	-1.255***	-1.079***	-1.294***
		(0.313)	(0.330)	(0.337)	(0.306)	(0.329)
		0.362	0.392	0.285	0.340	0.274
COLD WAR	-0.584**		-0.300	-0.400	-0.187	-0.353
	(0.270)		(0.289)	(0.293)	(0.275)	(0.275)
	0.558		0.741	0.670	0.829	0.703
GDP PER CAPITA	0.148			0.024		0.040
	(0.155)			(0.159)		(0.164)
	1.160			1.024		1.041
DEMOCRACY	-0.720*			-0.716*		-0.572
	(0.428)			(0.372)		(0.377)
	0.487			0.489		0.564
OIL PRODUCER	0.320			0.518*		0.525
	(0.322)			(0.313)		(0.374)
	1.377			1.678		1.690
POPULATION SIZE	-0.262**			-0.390***		-0.411***
	(0.129)			(0.121)		(0.146)
	0.770			0.677		0.663
MOUNTAINOUS TERRAIN	-0.070			-0.034		-0.046
	(0.095)			(0.086)		(0.088)
	0.932			0.967		0.955
ETHNIC CONFLICT	-0.064				0.213	-0.064
	(0.316)				(0.285)	(0.303)
	0.938				1.238	0.938
SECESSIONIST CONFLICT	0.214				-0.146	0.318
	(0.323)				(0.264)	(0.326)
	1.239				0.864	1.375
MULTIPARTY CONFLICT	-0.828***				-0.579*	-0.723***
	(0.300)				(0.298)	(0.277)
	0.437				0.560	0.485
UN PEACEKEEPING OPERATION	0.559				1.125***	0.581
	(0.383)				(0.357)	(0.438)
	1.750				3.080	1.787
REBEL TERRITORIAL CONTROL	0.127				0.368*	0.145
	(0.249)				(0.221)	(0.258)
	1.136				1.445	1.156
PARITY	0.120				0.019	0.167
	(0.383)				(0.396)	(0.399)
	1.127				1.019	1.182
REBELS STRONGER	0.741				0.562	0.934
	(0.968)				(0.797)	(0.863)
	2.098				1.754	2.545
Conflict Episodes	95	95	95	95	95	95
Terminations	73	73	73	73	73	73
Observations	1048	1048	1048	1048	1048	1048

Table A.4: Cox model estimates. The table lists variable coefficients, with robust standard errors clustered on country in parentheses, followed by hazard ratios (e^{β}). *p $\leq$ .10; **p $\leq$ .05; ***p $\leq$ .01

Variables	Model 7 (incl. alleged)	Model 8 (CI vs. reb)	Model 9 (CI vs. gov)	Model 10 (disaggregated)	Model 11 (Firth)
COMPETITIVE INTERVENTION	-1.196***	-1.584***	-1.279***		
	(0.367)	(0.360)	(0.362)		
	0.302	0.205	0.278		
NO SUPPORT		-0.469	-0.165		
		(0.322)	(0.285)		
		0.625	0.848		
GOVERNMENT ONLY		-0.305			
		(0.351)			
		0.737			
REBELS ONLY			0.305		
			(0.351)		
			1.356		
US/SOVIET COMPETITIVE INTERVENTION				-1.907**	-1.524**
				(0.787)	(0.891)
				0.149	0.218
NON-SUPERPOWER COMPETITIVE INTERVENTION				-1.212***	-1.163***
				(0.360)	(0.352)
				0.298	0.312
COLD WAR	-0.367	-0.360	-0.360	-0.328	-0.322
	(0.283)	(0.275)	(0.275)	(0.283)	(0.271)
	0.693	0.698	0.698	0.721	0.725
OTHER CONTROLS INCLUDED	Yes	Yes	Yes	Yes	Yes
Conflict Episodes	95	95	95	95	95
Terminations	73	73	73	73	73
Observations	1048	1048	1048	1048	1048

Table A.5: Cox model estimates. Models 7-10 report variable coefficients, with robust standard errors clustered on country in parentheses, followed by hazard ratios (e^{β}). Model 11 reports a Cox model with Firth penalized likelihood estimates. *p $\leq$ .10; **p $\leq$ .05; ***p $\leq$ .01

D A Note on the Temporal Coverage of the Dataset

The dataset analyzed in the accompanying article examines all civil wars fought between 1975-2009. The temporal scope of the dataset is restricted to 1975-2009 due to data constraints. While the inclusion of the entire Cold War period could provide additional statistical power by increasing the dataset's sample size, the restricted coverage does not bias the article's empirical findings. This appendix addresses a number of potential concerns to explain why this is the case.

First, one concern might be that the time period under analysis is overly favorable to the argument that is developed in the article, providing too easy of a test of the hypotheses. However, the arguments the article develops at the micro- and macro- levels of analysis are intended to be generalizable across time, to include both Cold War *and* post-Cold War civil wars, as well as superpower *and* non-superpower competitive interventions alike. In that sense, the micro- and macro-level arguments and empirics are agnostic to time period. Notably, this generalizability is borne out in the quantitative results.

A second possible concern, however, might be focused on the systemic level of analysis. The article argues that the Cold War should be associated with a greater prevalence of competitive intervention. Here, we might be concerned that precisely because superpower proxy wars were prevalent during the Cold War, the results are largely driven by this subset of competitive intervention. This concern is addressed in Models 10 and 11 (Table 2), which show that Cold War-era superpower competition does not undercut the generalizability of the argument about competitive intervention. Even when we disaggregate competitive intervention into its superpower and non-superpower varieties, the latter variable remains statistically and substantively significant. This demonstrates that the negative consequences of competitive intervention are not being driven solely by US/Soviet proxy war; competitive intervention by less powerful states remains a significant predictor of longer wars, again in both the Cold War and post-Cold War periods.

Finally, a third possible concern relates to Cold War era conflicts that began prior to the period of observation and continued into the period of observation. For example, the civil war in Guatemala is coded as meeting the UCDP/PRIO Armed Conflict Dataset's criteria beginning in 1965 and persisting until 1995. This suggests there is an entire decade of "missing" Cold War era conflict-years in the accompanying article's dataset, which begins in 1975. However, this is not the case: one strength of a duration model statistical design is that this concern can be explicitly accounted for in the model. In particular, by treating wars that began prior to 1 January 1975 as left-censored, the model ensures that the time dependence of wars that began prior to the start of the observation period is accounted for.

More generally, the dataset is quite balanced in terms of the number of years of coverage between the Cold War and post-Cold War periods, with 16 years of observation in the former period and 19 years of observation in the latter period. Relatedly, the number of observations on the two sides of the Cold War/post-Cold War split are also reasonably balanced, with 510 Cold War era conflict-year observations, and 445 post-Cold War era conflict-year observations.

Finally, the Cold War years of observation that are included in the dataset are in many ways a continuation (rather than a change) of geopolitical competition between the superpowers. Even during the period of détente, roughly lasting 1968-1975, there was no slow down in military transfers to clients. Westad (2007, 197), for example, highlights that while the costs of the Vietnam war helped spur the American desire for détente, US policymakers remained committed to the establishment of regional "policemen" states, supported by US arms and money, that would have

the responsibility of keeping communism contained in their region. Similarly on the Soviet side, détente was seen as an opportunity to establish the foundations for a more activist approach to the Third World. Arguments for activism were based on both opportunism (ex. US defeat in Vietnam, growth of national bourgeoisie in former colonies, etc.) and growing capacity (in the form of military infrastructure, the development of a fleet of long-distance transport planes, and the growth of the Soviet navy) (Westad 2007, 202-206). In short, the period captured in the dataset analyzed in the accompanying article is reflective of trends in superpower competition throughout the Cold War.

E Addressing Reverse Causality Concerns

A possible critique of the findings reported in the accompanying article might be that there is a selection effect driving the results. In particular, it may not be the case that competitive interventions are prolonging civil wars; rather, it might be argued, longer civil wars tend to disproportionately attract competitive interventions. In the latter case, the link between competitive intervention and longer conflict would be a spurious one.

There are two responses to this critique. First, the time-series cross-national data structure of the dataset alleviates this concern by allowing the competitive intervention variable to vary across years of a civil war. The models reported in the accompanying article compare the duration effect of a given year of competitive intervention with the baseline duration dependence of a year without competitive intervention. As a result, even if it were true that only long conflicts attracted competitive interventions, the model would account for this in the estimates it derives.

Second, it can be empirically demonstrated that the majority of competitive interventions begin in the early years of a civil war. Figure A.1 plots the year of competitive intervention onset for civil wars that were afflicted by this form of intervention in the dataset (note that left-censored conflicts are not included owing to their incomplete observation history). The figure reveals that in the majority (66 percent) of cases, the onset of a competitive intervention occurred in the first year of a conflict. In 93 percent of cases, the onset of a competitive intervention occurred by the third year of a conflict. In other words, it is not the case that longer civil wars tend to disproportionately attract competitive intervention; rather, competitive intervention tends to generate longer conflicts.

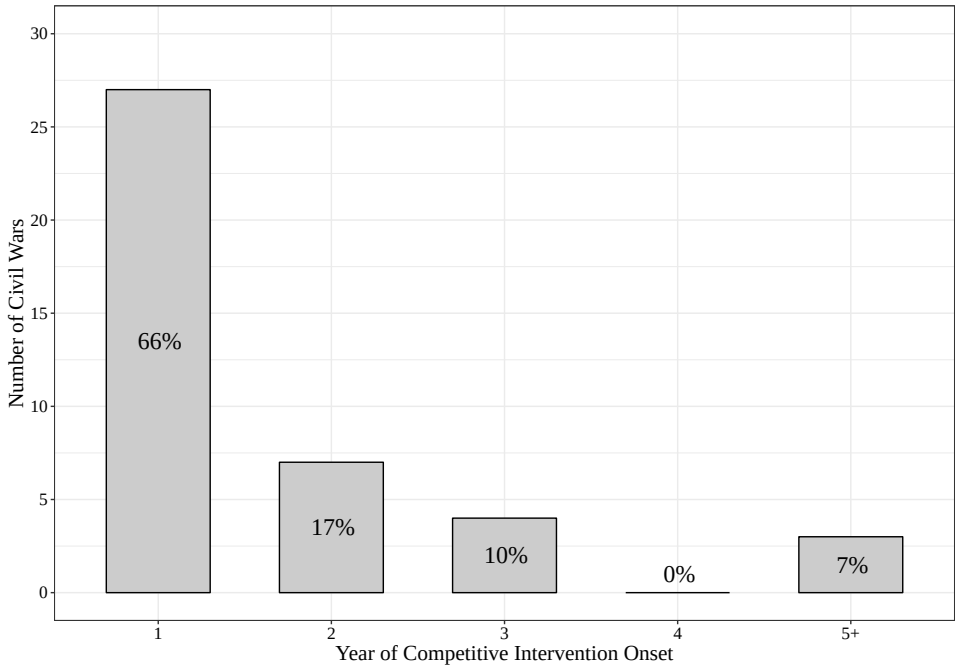

Figure A.1: Year of competitive intervention onset for civil wars that were afflicted by competitive intervention at any time during their duration

F A Note on Case Selection, Data, and Sources

The accompanying article presents a within-case analysis of the Angolan civil war (1975-1991) to examine the observable implications of my theory and to verify the processes that link competitive intervention to protracted conflict. The Angolan case provides a "least-likely" test for the theory for three reasons.[2] First, the conflict was disturbingly violent, inflicting hundreds of thousands of deaths, displacing millions, and decimating the country's economic, political, and social institutions (UN Inter-Agency Task Force 1989, 24-27). The identification of strategic restraint in the interactions of competitive interveners despite the intensity of violence in the country demonstrates the powerful conditioning effect of escalation fears.

Second, intervener behaviors were not limited by strict resource constraints. The US and USSR were global superpowers; South Africa was a regional hegemon; and Cuba profited from the largess of the Soviet Union, which supplied both the MPLA and the Cubans deployed to Angola. Consequently, decisions to continually invest in the conflict at a level which generated stalemate, rather than decisive military advantages, confound analysis unless one accounts for escalation fears among competitive interveners.

Finally, the interveners were strongly motivated to see their side win the civil war. For the US and the USSR, Angola represented one of the "hot" frontlines of the Cold War given its vast natural resource wealth, its strategic air and naval facilities, and its significance for the balance of power in southern Africa.[3] For the South Africans, the outcome of the conflict was of existential significance due to deep-seated fears that a MPLA victory would mark the beginning of the end of the apartheid regime, representing the first domino in a cascade which would first liberate Namibia (then South West Africa) before propelling Black Nationalism to the seat of power in Pretoria.[4] And while the strategic interests of the Cubans are the subject of dispute—some stress Castro's ideological conviction to national liberation, others suggest the Cubans served at the behest of the Soviet Union (cf. Gleijeses 2002, 379; Mumford 2013, 62)—no one can doubt the significance of the scale of Cuba's intervention: the tiny island nation maintained a deployment of tens of thousands for over a decade, peaking at 55,000 troops in 1988.[5] Thus, the interests at stake for interveners in the Angolan civil war cannot be dismissed as inconsequential; winning the war *mattered*. To understand why interveners limited the scope of their support to domestic combatants requires an explicit account of the strategic dilemmas inherent in competitive intervention.

To examine how competitive intervention manifest on the Angolan battlefield, I draw on

2. On the virtues of "least-likely" tests, see Gerring and Seawright (2007, 115-119).

3. The consequences of American neutrality in the conflict were reviewed in a National Security Council background paper in 1975, which stressed that a MPLA victory risked increased Soviet influence in Southern Africa, not least through access to Angolan ports and airfields. See NSC (1975, 3). This assessment was correct: the Soviets sought to monitor shipping lanes around the Cape of Good Hope and into the South Atlantic. See Shubin (2008, 72).

4. For South African military commanders like Major General (ret.) Gert Opperman, for example, Angola was the lynchpin of South African security: Pretoria would have to "do as much as possible to keep the enemy away from the Namibian border [...] if we couldn't do that, Namibia would be the next to fall. And if Namibia fell, the enemy would have got very close to Cape Town through the Northern Cape, and to Pretoria/Johannesburg through Botswana, because they would have been the next dominos to fall in the process" (Interview, Major General (ret.) Gert Opperman, June 2014, Pretoria). For a similar view from former South African Foreign Minister Pik Botha, see Besoek van Mnr Clark aan Suid-Afrika en Suidwes-Afrika: 10-13 Junie 1981 (23 June 1981, 25).

5. By war's end, some 337,033 Cuban soldiers would serve in the country. See Gleijeses (2013, 519).

an extensive set of primary and secondary materials. These include semi-structured interviews with former military commanders, political leaders, and diplomats in South Africa, conducted in person and by telephone over the summer and fall of 2014; archival documents and declassified intelligence materials collected from the United States and South Africa; and a growing number of memoirs, personal accounts, and biographies published by political and military veterans of the Angolan conflict (see, for example, Stockwell 1978; Davis 1978; Crocker 1992; Kissinger 1999; Nujoma 2001; Shubin 2008; Geldenhuys 2008; Papenfus 2010; Shubin and Tokarev 2011; Geldenhuys 2012; de Vries 2013). For access to Cuban primary source material, I rely on the work of historian Piero Gleijeses who, to date, remains the only foreign scholar to have been allowed access to the Cuban archives for the post-1959 period. Gleijeses (2002, 2013) has published two lengthy works, *Conflicting Missions* and *Visions of Freedom*, which draw on thousands of Cuban documents and thousands of pages of conversations between Fidel Castro and his closest aids and foreign leaders.

Triangulation of data and sources is key to assessing potential bias in research materials. In the accompanying article's case study, I have sought to triangulate in two ways. First, I draw on a heterogeneous set of primary and secondary materials. The persuasiveness of a given piece of evidence is strengthened when it is confirmed across multiple types of sources—for example, when interviewees recall events that are also noted in declassified intelligence reports written during the war and reiterated in independent participant memoirs published in the interim. Thus, for each example of strategic restraint discussed in the study, I have sought to include references to a diverse set of independent materials to cross-check the veracity of a given piece of evidence. Second, I draw on materials provided by both sides of the war. Confidence in a given piece of evidence is improved when opposing sides concur on a particular event or outcome. Thus, in each section of the case study, I have sought to include Cuban/Soviet sources as well as South African/American sources to legitimate claims.

By way of example, consider the discussion of geographic focal points that were used to control escalation during the war. This section draws on details of the Cuban defensive line provided by Gleijeses (2013). As noted above, Gleijeses is the only foreign scholar to have been provided access to the Cuban archives for the post-1959 period, and thus while his work represents a tremendous contribution to scholarship in general and history specifically, some might voice concerns that there is no way of knowing whether the sources he selected for publication (or for that matter, was provided access to) are biased in some way. Gleijeses' work also tends toward considerable admiration for Fidel Castro's regime, which has led some to question whether his account is partial to a Cuban narrative. For these reasons, I triangulate his account with additional primary sources—namely, interviews with retired South African military generals. In interviews conducted during the fieldwork stage of this project, it was confirmed to me that Cuban troops did indeed remain behind the defensive line for the majority of the war, while South African forces constrained their own operations to south-central Angola. Available participant memoirs further reinforce confidence in this evidence,[6] as do secondary sources (George 2005).

To be sure, triangulation is no panacea, but by including a diverse set of independent primary and secondary source materials that originate from different sides of the civil war, there is stronger confidence in empirical claims.

6. Breytenbach (2002, 247), for example, writes that SADF control was established in "Cunene Province east of the Cunene River and as far north as Cassinga."

References

Anderson, Noel, and Alec Worsnop. Forthcoming. "Fatality Thresholds, Causal Heterogeneity, and Civil War Research: Reconsidering the Link Between Narcotics and Conflict." *Political Science Research and Methods.*

Besoek van Mnr Clark aan Suid-Afrika en Suidwes-Afrika: 10-13 Junie 1981. 23 June 1981. Digital Innovation South Africa, South Africa Department of Foreign Affairs.

Breytenbach, Jan. 2002. *The Buffalo Soldiers: The Story of South Africa's 32-Battalion, 1975-1993.* Alberton: Galago.

Crocker, Chester. 1992. *High Noon in Southern Africa: Making Peace in a Rough Neighborhood.* Johannesburg: Jonathan Ball Publishers.

Cunningham, David E. 2011. *Barriers to Peace in Civil War.* Cambridge: Cambridge University Press.

Cunningham, David E., Kristian Skrede Gleditsch, and Idean Salehyan. 2009. "It Takes Two: A Dyadic Analysis of Civil War Duration and Outcome." *Journal of Conflict Resolution* 53 (4): 570–597.

Davis, Nathaniel. 1978. "The Angola Decision of 1975: A Personal Memoir." *Foreign Affairs* 57 (1): 109–124.

De Vries, Roland. 2013. *Eye of the Firestorm: The Namibian-Angolan-South African Border War - Memoirs of a Military Commander.* Johannesburg: Naledi.

Fearon, James D. 2004. "Why Do Some Civil Wars Last So Much Longer than Others?" *Journal of Peace Research* 41 (3): 275–301.

Geldenhuys, Jannie. 2008. *At The Front: A General's Account Of South Africa's Border War.* Johannesburg: Jonathan Ball Publishers.

———, ed. 2012. *We Were There: Winning the War for Southern Africa.* Pretoria: Kraal Uitgewers.

George, Edward. 2005. *The Cuban Intervention in Angola, 1965-1991: From Che Guevara to Cuito Cuanavale.* New York: Frank Cass.

Gerring, John, and Jason Seawright. 2007. "Techniques for Choosing Cases." In *Case Study Research: Principles and Practices,* edited by John Gerring. New York, NY: Cambridge University Press.

Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand. 2002. "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research* 39 (5): 615–637.

Gleijeses, Piero. 2002. *Conflicting Missions: Havana, Washington, and Africa, 1959-1976.* Chapel Hill: University of North Carolina Press.

———. 2013. *Visions of Freedom: Havana, Washington, Pretoria, and the Struggle for Southern Africa, 1976-1991.* Chapel Hill: University of North Carolina Press.

Kissinger, Henry. 1999. *Years of Renewal.* New York, NY: Simon & Schuster.

Mumford, Andrew. 2013. *Proxy Warfare.* Cambridge: Polity Press.

NSC. 1975. *Meeting Minutes (27 June).* National Security Adviser's National Security Council Meetings File, Box 2.

Nujoma, Sam. 2001. *Where Others Wavered: The Autobiography of Sam Nujoma.* London: Panaf Books.

Papenfus, Theresa. 2010. *Pik Botha and His Times.* Pretoria: Litera Publications.

Shubin, Gennady, and Andrei Tokarev, eds. 2011. *Bush War: The Road to Cuito Cuanavale.* Johannesburg: Jacana.

Shubin, Vladimir. 2008. *The Hot "Cold War": The USSR in Southern Africa.* London: Pluto Press.

Stockwell, John. 1978. *In Search of Enemies: A CIA Story.* New York: W.W. Norton.

Themnér, Lotta, and Peter Wallensteen. 2014. "Armed Conflicts, 1946-2013." *Journal of Peace Research* 51 (4): 541–554.

UN Inter-Agency Task Force. 1989. *South African Destablization: The Economic Cost of Frontline Resistance to Apartheid.* New York: United Nations.

Westad, Odd Arne. 2007. *The Global Cold War: Third World Interventions and the Making of Our Times.* Cambridge: Cambridge University Press.

Wucherpfennig, Julian, Nils W. Metternich, Lars-Erik Cederman, and Kristian Skrede Gleditsch. 2012. "Ethnicity, the State, and the Duration of Civil War." *World Politics* 64 (1): 79–115.